THE CHEMICAL CAREER OF WILLIAM PROUT

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The two portraits of Prout are reproduced by the kind permission of the Treasurer of the Royal College of Physicians. Acknowledgement is made to the Director General of the Ordnance Survey for the reproduction of part of the 2½ inch Gloucestershire sheet map, ST78, showing the village of Horton. I am greatly indebted to the following people who have at one time or another answered my inquiries, offered me facilities, or helped me through discussions: The Librarians of the Chemical Society, Edinburgh University, Gloucester Public Library, the Royal College of Physicians, the Royal Institution, the Royal Society, the Royal Society of Medicine, and of the Wellcome Historical Medical Library; the Inter-library loans staff, past and present, of Leicester University Library; Cécile Desbarests of McGill University; C.H. Josten; D.M. Knight; the late Professor J.R. Partington; David Piper of the National Portrait Gallery; D. Talbot Rice of Edinburgh University; R. Storey of the National Register of Archives; and above all, Lt.Col. Peter Warner, and my Supervisors, past and present, J.E. McGuire, R.S. McGowan and A.G. Keller.
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INTRODUCTION

Nearly thirty years after the death of the English physician and chemist, William Prout, Dr. William Munk recorded in his biographical Roll of the Royal College of Physicians: "I am not aware that any full and searching estimate of Dr. Prout's merits as a philosopher and chemist has yet appeared". Until recently Munk's remark remained true, for no attempt to write a definitive study of Prout's life and work had ever been published, and Munk's own sympathetic account, significantly longer than his other biographical sketches, remained the most readily available source of knowledge. Such neglect is surprising, for Prout's name is a familiar one in textbooks of chemistry and physics associated with the unitary hypothesis that the chemical elements possess atomic weights which are integral multiples of the atomic weight of hydrogen. He is otherwise featured in a minor way in histories of chemistry and medicine as the discoverer of hydrochloric acid in gastric juice, and as an early organic analyst. Although Prout's hypothesis and its fate has attracted several historians, no attempt has ever been made to show the

1. W. Munk, Roll of the Royal College of Physicians, 3 vols., London, 1878, vol. 3, p. 110. In future this work will be cited as "Munk, Roll".

2. W.H. Brock, "The Life and Work of William Prout", Medical History, 9, 101-126, 1965; a copy is provided in the Appendices to this thesis. This article was a much condensed version of Chapters 1, 3, 4 and 5.

connections, if any, between his unitary hypothesis and his other work in the fields of chemistry, biochemistry, and the practice of medicine.

In 1961 I made a preliminary survey of Prout's papers and books to see whether his work would bear the intensive research out of which a thesis could emerge. It soon became clear that Prout's work in various scientific fields was unified by a particular philosophy of matter, a molecular theory; and that his commitment to it explained the direction taken by his various researches. It also emerged that the neglect of Prout's work had already set in long before he died in 1850. There are at least two reasons for this. First, Prout's chemical career was essentially finished by 1834 when he published his Bridgewater Treatise; thereafter he devoted himself almost entirely to his medical practice, and the revision of his two books. Second, as these revisions show fairly clearly, Prout bothered little to keep abreast of the rapid new developments that were taking place in chemistry and biochemistry; although much of his research had foreshadowed that of Liebig and his school, he found himself and his work eclipsed by their achievements in the 1830s and 1840s. When he died, both the Chemical Society and the Royal Society ignored his death, and it was left to the physicians to pay tribute to him in the chief medical journals of the day. (4)

4. Prout had never bothered to join the Chemical Society so perhaps this justifies their silence. But he had been a loyal and useful member of the Royal Society; their silence may be connected with the death of Gay Lussac, who also died in 1850 and received a long eulogy. If this is so, it is ironic that a French chemist should take precedence over an English Fellow.
At this point the reader may be wondering whether the intensive study of an admittedly minor nineteenth-century chemist is justified. To this my reply would contain the following ingredients.

1. Although now considered to be a minor figure, Prout commanded a good deal of respect in scientific circles of the early nineteenth-century both as an experimentalist and as a theorist. 2. His contribution to the concept of the unity of matter played a dominant role in the theory of elements and the fortunes of the atomic theory through the nineteenth-century and the early part of this century. The discovery of manuscript material sheds fresh light on this subject.

3. His molecular theory, and his independent derivation of Avogadro's hypothesis, reveal a deep concern with the problems of explaining chemical events. 4. His biochemistry and its relations with the fortunes of Liebig's work throw important light on early organic and animal chemistry. 5. Finally, the proof of the pudding must be in the eating; it is hoped that this thesis itself will show the reader that Prout's ideas and their relationship with those of his contemporaries are of great interest for the historian of chemistry; and that in the final analysis they do much to illuminate the activities of major figures of the first half of the nineteenth-century such as Dalton, Davy, Dumas and Liebig.

Since no attempt has been made to examine Prout as a physician, the thesis has been deliberately entitled a study of Prout's chemical career. However, in actual fact much incidental light has been shed on his medical practice. The study is divided in two parts. In the first section, after a brief biography, Prout's contributions to experimental science are considered. A discussion of his development
of apparatuses and techniques for organic analysis is followed
by surveys of his experimental work in the field of physiological
chemistry: urine chemistry, the problem of sensation, respiration,
and digestion. The first part ends with a short chapter on Prout's
interest in barometry. The second part is concerned with Prout as
a theorist, and the transition is made through a study of his Bridge-
water Treatise and his support for natural theology and vitalism. A
detailed discussion of Prout's hypothesis from 1800 to 1850 is follow-
ed by a final chapter which explores his molecular theory and attempts
to link together his theoretical ideas with his experimental work in
the field of animal chemistry. An attempt is also made to find reasons
for Liebig's success, and Prout's failure, to establish the science
of biochemistry. Some conclusions are drawn in a brief epilogue.

During the preparation of this study I was fortunate enough to
On the death of his aunt, Miss D.A. Nicol, in 1963, he kindly made
available to me a large amount of Prout manuscript material which he
had inherited. Free use of the most important elements of this mat-
erial has been made, and some transcriptions are given in Appendices.
PART I

PROUT THE EXPERIMENTALIST
Chapter One: William Prout (1785-1850)

"He was an example of a man gifted by nature with high intellectual endowments improving these endowments by constant study, investigation and reflection. An amount of professional labour, such as would have wearied many men was daily performed by him; and from this he turned for relaxation to arduous chemical and mechanical researches. His mind was of that rare quality which is ever open to the reception of truth, and which steadily pursues that object, undismayed by difficulties, and indifferent alike to ridicule and neglect." (1)

The subject of these striking remarks, William Prout, was born on the 15 January 1785 at the little village of Horton, between Chipping Sodbury and Hawkesbury, in Gloucestershire. The present village has altered little since the nineteenth-century, and it still "nestles easily in a fold of the hills which gird it round on three sides, but on the fourth slope steeply down, affording a lovely panoramic view of the fertile vale of Gloucester which stretches far away on either hand." (2) The standard biographical notices of Prout all speak of his family as having lived on their own property at Horton for many generations. However, all these notices are based on one of two obituaries of Prout whose writers,

1. Medical Times, 1,17,1850.

so it would appear, were guilty of exaggeration. The Lords of the Manor at Horton from the time of Edward VI until the 1780s were the great Catholic family of the Pastons, but during Prout's youth the Manor had passed into private and uninterested hands.

In 1599, a certain Lawrence Allway purchased from the Pastons a parcel of land within Hawkesbury parish adjacent to the Horton parish boundary known as Chalkly or Chalkleys; and this land remained in the Allway family until the eighteenth-century. To it was added, in 1652, the "copy-holder" of Allway in Horton parish leased from the Pastons who were then suffering debts from heavy recusancy fines. Finally, a later descendent, William Allway, being without issue, left both Allways and Chalkley in 1723 to his nephew Richard Bennett whose sister, Elizabeth, was married to a Daniel Prout of Chipping Sodbury. This Daniel Prout was the great grandfather of William Prout.


5. For information concerning Prout's genealogy I am indebted to the private files of Miss Dorothy Ada Nicol. After her death in 1963, Lt.Col. P.N.H. Warner kindly made these files available to me.
THE ALLWAYS, BENNETTS AND PROUTS OF HORTON

Lawrence ALLWAY - Mary ?
( ? - 1665)

Charles ALLWAY
(1642-1646)  
William ALLWAY
(1644- ?)  
James ALLWAY
(1645- ?)  
Jane ALLWAY
(1647- ?)  
Margaret ALLWAY
(1649 - ?)

Elizabeth ALLWAY - William BENNETT

William ALLWAY
( ? -1723)

Elizabeth BENNETT - Daniel PROUT
(1660-1730)

Richard BENNETT

Mary PROUT

William BENNETT
( ? - 1777)

Heir, 2nd cousin
John Proout

Richard BENNETT
( ? -1786)

Nebuchadnezzar PROUT - Martha HALE

John PROUT (1745-1820)
THE HORTON PROUTS

Daniel PROUT (1660 - 1730)  Elizabeth BENNETT (? - 1730)

Nebuchadnezzar PROUT  -  Martha HALE

John PROUT  Arthur PROUT  William PROUT  James PROUT  Gabriel PROUT  Ephraim PROUT  Hannah PROUT  Robert
(1745 - 1820)  (1752 - 1819)  (1746-1824)  (1750-?)  (1752-?)  (1756-1819)  (1758-?)
m
Sarah SLATER
(see infra Appendix 3)

Hannah (LIMBRICK?)
(1756 - ?)

William PROUT  Robert PROUT  John PROUT
(1785 - 1850)  (1786 - 1862)  (1788 - died infancy)
m

Agnes ADAM (1793 - 1863)

Christine PROUT  John William PROUT  Alexander PROUT  Walter Robert PROUT  Thomas Jones PROUT  Elizabeth PROUT  Agnes
(1817-1881)  (1818-1854)  (1821-1857)  (1823-1909)  (1825-1918)  (1826-1878)
The Prouts, who perhaps originated from Flemish refugee stock, were a local family; the surname is to be found frequently in various forms in the registers of the neighbouring parishes of Horton, Hawkesbury, the Sudburys, and Wickwar (6); and in the Horton Rental Book of 1665-1741. (7) Through their intermarriage with the Bennet(t)s they appear to have raised their station from that of peasant labourers or wool workers to that of yeoman farmers.

In 1739 Richard Bennett's son, William Bennett, purchased a freehold in Horton called Gilbert Ridings, and on his death in 1777, he left the three land parcels of Chalkley, Gilbert Ridings and Allways to his second cousin, John Prout, the father of William Prout. John Prout was a son of Nebuchadnezzar Prout of Tungroves (a copyholder of Horton) and Martha Hale; and a grandson of Elizabeth Bennett and Daniel Prout whose fortunate legal guardianship of William Bennett had led to this establishment of family fortunes towards the end of the eighteenth-century. However, because of the intricacies of the family tree, various legacies were left by William Bennett (6), and since the lease on Allways fell back to the Lord of the Manor in 1788, John Prout hardly

6. Cf. Wickwar's "Prout's Charity": "Arthur Prout of Cromhall in 1712, gave by will a close of ground called Millcroft, but now Cook's Leaze, and £20 to be placed out at interest, for the purposes of buying and giving a cloth coat each to five poor men, and a cloth coat or gown to such five poor women of the Parish", Gloucestershire Notes & Queries, 4, 210, 1890.

7. Ms. bound in parchment, 7 by 15½ inches, purchased by Miss Nicol and now in the possession of Lt. Col. Warner.

8. The original legacies are in the Nicol-Warner files.
inherited a fortune. But perhaps the inheritance was sufficient to prevent him from emigrating to America to join two of his brothers who had sent back to him fascinating reports of their near success in realising a fortune in the real estate upon which Washington City was being built. (9) So John Prou特 remained a Gloucestershire farmer and married a local Horton girl whose Christian name was Hannah. They had three children, the eldest being William Prou特. (10) The youngest child, John, born in 1788, died within two months of his birth, while Robert Prou特, born 19 November 1786, carried on with the farming tradition at Horton until his death in 1862.

William and Robert's father, who died picturesquely in 1820 "in consequence of having run a thorn into his hand which occasioned a locked jaw" (11), left the Horton estate (Chalkley and Gilbert Ridings) to his eldest son. The latter, however, now a distinguished London physician, evidently had no time or inclination for a country life; he therefore sold his rights to his bachelor brother Robert.

9. Their surviving letters are of great interest. The original letters are in the possession of Lt.Col. J.W. Nicol of Ballogie, D.S.O., and my examination of them has been confined to copies made by Miss Nicol. It is to be hoped that eventually they may be published by a student of American history. Cf. National Register of Archives of Scotland, List 0060. The American Prou特 line continues.


THE VILLAGE OF HORTON IN GLOUCESTERSHIRE

(Shaded areas show land owned by the Prouts in the 19-century, and by their descendants today)
On the latter's death in 1862, the small estate passed to William Prout's eldest son, John William Prout. The land is now owned by the Warner family - Prout's great great grandsons, but all that remains today is a freehold of some 35 acres of land which is inconveniently dispersed within the parishes of Horton and Hawkesbury, and which is used for agricultural purposes by two tenant farmers. (13) (See map of Horton).

Like so many other nineteenth-century physicians of humble origins, William Prout's early education was almost negligible. Although he learned to read and write at a local Dame school at Wickwar, (14) and later attended a charity school at Badminton, his elementary education had ceased before he reached the age of thirteen. (15) There had been an extremely good library at Horton Manor House, but it is not known whether the library remained there after the Pastons left in 1768; in any case it

12. The other surviving son, Thomas Jones Prout, who also inherited, sold his interest to John.


would be conjectural to suggest that Prout would have had access to it. From the age of thirteen to seventeen nothing is known of Prout's activities; his two principal obituarists were quite in the dark concerning this period, and we can only conjecture that he must have worked with his father on the land.

At the age of seventeen, however, Prout suddenly became critically aware of his educational deficiencies, and with an awakened interest in mechanical things, mathematics and music, he determined to engage upon some sort of systematic learning. With this aim he left home for some eighteen months between 1802 and 1804 to join a private Academy at Sherston in Wiltshire run by the Rev. John Turner, Vicar of Horton, and of Luckington in Wiltshire. Here he acquired the rudiments of Latin and Greek—essential languages for a university course, whether or not he yet had that ambition. However, he soon returned home, either dissatisfied with his own progress, or with the standards of Turner's Academy, since some time in 1805 or 1806 he took the extraordinary step of advertising in a local newspaper for advice on the prospects for further learning for an ill-educated twenty-year old.

16. The Catalogue of the (Horton) Library, 1758, bound Ms., 7½ by 12 inches, with Paston bookplate, in possession of Lt.Col. Warner. This was purchased at an auction by Miss Nicol on the chance that it would shed light on her Gloucester ancestors. It does not do this, but it throws important light upon the contents of a Catholic family's library.

17. For Turner, see Gents. Mag., 1848, ii, 215. Sherston is only a few miles from Horton.

18. Edinb. Med. Surg. J., 76, 127, 1851. I have not been able to trace this advertisement in either the Gloucester Journal or Felix Farley's Bristol Journal.
A reply came from another clergyman, the Rev. Thomas Jones (1758-1812) who ran a classical seminary at Redland, Bristol. (19) Jones had been educated at Cambridge and Dublin, and had been Vicar of two Devonshire parishes before he opened his "classical seminary for young gentlemen" at the turn of the century. Jones was later described as a liberal and generous man, and in view of Prout's obvious admiration for him hallowed by the christening of his youngest son with the names Thomas Jones, it may be significant that,

Dr. Jones had himself experienced the difficulty of emerging from obscurity and comparative indigence, to distinction and competence: and to those who had engaged in the same arduous struggle, his advice and assistance were always accessible. Many were indebted to him for the first step in the progress of their advancement, and therefore we may hope that many will love his memory, as long as recollection shall hold its seat in their bosoms. (20)

Prout spent two happy and formative years with Jones. In return for his tuition he taught the younger pupils of the school, and stimulated by a pupil's curiosity over the exciting new events that were then taking place in electrochemistry, Prout himself began to form what was to become a lifelong passion for the science of chemistry. It was Jones who suggested that Prout should become a doctor and who recommended him to enter the University of Edinburgh.


for this purpose. (Oxford and Cambridge were naturally out of the question as Prout's social status was too low). Thus in 1608, at the mature age of twenty-three, Prout went up to Edinburgh armed with a letter of introduction to Jones's old teaching friend, Dr. Alexander Adam, the Rector of the Edinburgh High School. (21)

He remained diligently in Edinburgh for the three year period except for visits to the country villages of Duddingston and Morningside during the summer vacations. These were sufficiently close to the university (we are told) (22) to ensure full use of the library; however, we may suspect that Agnes, Adams's elder daughter by a second marriage, was an additional Edinburgh attraction.

It has been frequently pointed out that a Scottish medical education at the beginning of the nineteenth-century was then the best that could be had in Britain, in fact "so much the better as to have been an example for imitation". (23) Edinburgh University Medical School during this period was at the zenith of its fame.


The reputation had been raised to an unparalleled height by the Monros, by Black, by Cullen, by the Gregorys, the Homes, the Rutherfords, the Hamiltons, the Bells; by Barclay, Gordon, and many others; whilst the general fame of the University had been sustained by such names as those of Robertson, Blair, Hutton, Dugold Stewart, Playfair, Leslie, Brown, and a phalanx of eminent theologians, philosophers, and literati. (24)

Unfortunately, nothing is known of Prout's days as a medical student, for like the vast majority of undergraduates, he left no mark upon his university, or on the medical societies of Edinburgh. (25) On the 10 August 1809 he read an elaborate paper, "Sketch of the History of Physic" to an unknown medical audience. (26) This essay surveys various medical systems from the times of primitive societies until the end of the sixteenth-century in an ambitious but fairly competent manner; and Prout indicated at the end that he was prepared to continue the survey to the present day on some future occasion.

Two other manuscript essays have also survived from the Edinburgh period; a Latin composition on hearing of 1810, "Dissertation de Sonis et Actions harmonia Auris humana"; (27) and the very interesting and important essay on sensation, "De Facultate Sensiendi". (28)

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25. Personal communication from Dr. Douglas Guthrie.

26. Ms. of 97pp., dated Edinburgh, July 1809, and described as "Read August 10, 1809". Since there is no record of this essay in the files of the Harveian Society, the Aesculapian Society, or the Royal Medical Society of Edinburgh, the audience can only have been some other unknown society.

27. Edinburgh, 1810, 29pp., probably a class essay. See Appendix 9.

28. Edinburgh, 1810, 26pp., in English. For full discussion see infra, Chapter 4 and transcription in Appendix 6.
His teachers at Edinburgh included Monro tertius on anatomy and surgery, Thomas Hope on chemistry and pharmacy, James Home on dietetics and materia medica, Andrew Duncan jr. on the theory of medicine (physiology), including its history, (29) and James Hamilton on midwifery. (30) In addition Prout would have attended clinical lectures on the patients at the Royal Infirmary. Among his contemporaries as medical students were Marshall Hall, John Davy, Henry Holland, and the man who became one of his closest friends, John Elliotson. (31) Prout graduated M.D. on 24 June 1811, (32) with a straight-forward and unoriginal academic dissertation on fevers which proceeded by way of definitions, symptoms, causes, pathology, prognosis, and treatment. (33)

After graduation, Prout left Edinburgh, and took rooms off Leicester Square in London where, until he gained the Licentiate of the Royal College of Physicians to practise medicine on the 22 December 1812, (34) he walked the wards of the united hospitals

29. There are several pencil notes in the ms. of Proout's History of Physic referring to Duncan's statements which conflict with, or expanded, points in Proout's lecture.


34. Munk, Roll, 3,109; the examination was conducted orally in latin,
of St. Thomas's and Guy's; and had his first two scientific papers published in the *London Medical and Physical Journal*. Armed with his licence he set up a practice at 4 Arundel Street, just off the Strand, and it was here in 1813 that he began his active career as a chemist and physiologist. The Scots chemist Thomas Thomson, on his return from a tour of Sweden in 1812, had begun to publish an important monthly journal, the *Annals of Philosophy*, and it is from early issues of this periodical that we learn that in 1814 Prout delivered a course of lectures at his home, "the attendance on which though small was select, and so highly was he already esteemed that his audience constantly included Sir Astley Cooper." (35) Prout's attendance at Guy's hospital would have brought him into contact with the great surgeon, and also with Alexander Marcelet, lecturer in chemistry at Guy's, who was a pioneer animal chemist and intimate friend of the Swedish chemist Berzelius. Two published advertisements for Prout's lectures read:

Dr. Prout intends in the course of the winter, to deliver a series of lectures on Animal Chemistry. The object of these will be to give a connected view of all the principal facts belonging to this department of chemistry, and to apply them, as far as the present state of our knowledge will permit, to the explanation of the phenomena of organic action. (36)

35. Munk, *Roll*, 3, 109, based his statement on *Med. Times*, 1, 15, 1850, which said Prout's course was in 1813. Either Prout gave two annual series of lectures or, more likely, the 1813 announcement of the lectures was confused by the obituarist with the 1814 realisation.

Dr. Prout will commence a series of lectures on Animal Chemistry on Friday, February 18 [1814], at half past eight in the evening. These lectures will be given at his residence, 4 Arundel Street, Strand, and will be continued weekly at the same hour. (37)

Lecture courses in private houses were not uncommon at this time, although accounts of them were rarely published. There was even an "Animal Chemistry Club" or "Society for the Promotion of Animal Chemistry" whose members, including Brodie, Home, Davy, Babington, Brande and Clift, met in each other's homes once a month. However, by 1813 this had "degenerated into a mere dining club", and in any case its members were exclusively Fellows of the Royal Society so that Prout could have been admitted only under exceptional circumstances.

A pile of Prout's lecture notes has survived. They are almost indecipherable, but it is sufficiently clear that Prout first offered his audience a general statement concerning his chemical philosophy before he reviewed in detail the chemistry of the blood, respiration, digestion, and urine. Some of this material formed the basis for his only extensive paper, that on sanguification, published in 1816; and from references in his other later publications it can be gathered that he first revealed his method for the preparation of pure urea.

38. Sir B.C. Brodie Jr. (ed.), Autobiography of the late Sir Benjamin Brodie, 2nd ed., 1865, pp. 88,91-2. This "society" is currently under investigation by Mr. N.G. Coley as part of his study of the development of animal chemistry.
39. See infra chapter 7. The manuscripts are in the possession of Lt. Col. F.E.H. Warner. For some transcriptions, see infra Appendix 7.
at one of these sessions, as well as the details of some of the organic analyses that he had started to make.

The year 1814 was an extremely busy and eventful one for Prout since, besides his lecture course which would have brought him forward prominently in the circle of London scientists, he was elected a Fellow of the flourishing Medical and Chirurgical Society, he was married, he visited Paris, he continued analysing vegetable and animal materials, and he performed his duties as a professional physician.

His election to the Medical and Chirurgical Society, whose prominent members included Astley Cooper and Alexander Marcot, took place on the 10 May, and his association with it continued for many years. He read several papers to its members, served on its Council from 1817 to 1819, and acted as a Vice-President in 1823 and from 1833 to 1835. (40)

Prout was married on 22 September 1814 at St. John's Church, Smith Square, Westminster, to Agnes Adam (1793-1863) with whom he had fallen in love as a student. (41) No description of Agnes survives, but she evidently shared her husband's interest in painting. (42) Europe was momentarily at peace, so the Prouts were able to pay a

40. See Officers' lists, Medico-Chirurgical Society Transactions, 8,1817; 18,1833; 19,xli,1835. His connection with the Society lapsed after 1835.

41. Gents.Mag., October 1814, p. 392. Miss D.A. Nicol, with an eye for detail, noted that the wedding service was conducted by the Rev. David Longlands.

42. An oil painting of Lock Katerine and Ben Venue in the Trossachs of Scotland which was painted by Agnes before her marriage is in the possession of the Nicol of Ballogie.
honeymoon visit to Paris where, like Humphry Davy before them, they were able to have a private view of the paintings and artistic treasures which Napoleon had collected during his campaigns with the aid of the traveller Baron Denon. A Medical Times writer rhapsodises over this episode: "We can imagine the delight with which a man so refined, and so open to the appreciation of the beautiful as that of William Prout — no mean draughtsman himself, — wandered through the noble salons of the Louvre." (43) Prout's watercolour of Horton Church painted in 1804 has survived and clearly indicates a delicate artistic imagination. (44) He also enjoyed having paintings about him for the same unknown writer reports that Prout's consulting rooms were hung with several canvases. Later in life he made friends with the artist Thomas Phillips whose son was to make a posthumous copy of a Prout portrait made by John Hayes. (44)

On their return to England the Prouts settled at Southampton Street, Bloomsbury, where a daughter, Christina, was born to them in 1815. The child only survived a few months, but there were six further children. A son, John William, who became a wealthy lawyer, was born in 1817; a second son, Alexander Adam, who like his father became a doctor, was born in 1818; Walter Robert, born in 1820, lost his life as a Major during the Indian Mutiny; a fourth son, Thomas Jones, was born in 1823 and became a clergyman and classics don at

43. Med. Times, 1,15,1850. Davy's reaction (30 October 1813) had been one of cool indifference according to J.A. Paris, Life of Sir Humphry Davy, 1831, p. 268.

44. For portraits of Prout see infra Appendix. 1.
Christ Church Oxford. There were also two daughters, Elizabeth, born in 1825, and Agnes, born in 1826. The house where the family resided from 1821 to 1850 (40 Sackville Street, Piccadilly) is no longer standing and has been replaced by car showrooms. (45)

There was a short-lived medical journal called The Annals of Medicine and Surgery; or Records of the Occurring Improvements and Discoveries in Medicine and Surgery and the Immediately Connected Arts and Sciences which appeared quarterly during 1816 and 1817. The editors are not definitely known, although there is a suggestion by one of Prout's obituarists that they were Prout and his friend John Elliotson.

It has been said that this journal was conducted by Dr. Elliotson and Dr. Prout; but the correctness of this statement we have no means of ascertaining. (46)

Unfortunately, none of Prout's writings, published or unpublished, makes any allusion to the editorship, and although both he and Elliotson wrote for the journal, there seems to be no internal evidence that they acted in an editorial capacity. (47) Indeed, from later references to the journal by Elliotson, at a time when anonymity would have lost all its point, he referred impersonally to "The editors of a medical review." (48)

45. For fuller details concerning the careers of Prout's children, see infra Appendix 2.
47. See my query concerning the editorship (to which there have been no replies), Medical History, 8, 291-3, 1964 (infra, Appendix 10).
Two honours marked Prout's success as a scientist and physician. In 1818 he sought election to the Royal Society through his friend Alexander Marcet. This proposal was countersigned by Wollaston, and a group of medical Fellows, Henry Warburton, Charles Koenig, Peter Mark Roget, Leigh Thomas, Gilbert Blane and Mathew Baillie. (49)
The Fellowship duly came to him in March 1819. Prout served the Royal Society well by acting on its Council from 1826 to 1828, and in helping to supervise the construction of the Society's Standard Barometer in 1835. In 1827 he was honoured by the Society's Copley medal for one of his papers.

The final accolade to Prout's skill in Physics came on the 25 June 1829 with his election to a Fellowship of the Royal College of Physicians. As one of the four youngest doctors of the College, he was invited to deliver the Gulstonian lectures in 1831. His three lectures on "The Application of Chemistry to Physiology, Pathology and Practice" (51) were summarised in the Medical Gazette where they

49. "William Prout, M.D., of Southampton Street, Bloomsbury, author of several chemical and physiological papers, one of which was printed in the last volume of the Royal Society, we, the undersigned, do from our own personal knowledge, recommend him as worthy of that honour, and as likely to become a very valuable and useful member." This notice was suspended 10 December 1818 and the election succeeded 11 March 1819. Cf. Med.Times,1,15-16, 1850.

50. Membership of Council was offered him again in 1832, but declined; see Prout's letter to J.G. Children, 22 October 1832, Royal Society Manuscripts, M22. 50 and Minutes of Council, vol.XI,p.329,1832.

gave rise to an extremely acrimonious dispute with another fellow of the College, Alexander Wilson Philip. (52) Personal abuse and invective were still common weapons in controversies, yet later commentators have tended to agree with Munk that "it is to be regretted that the discussion ... provoked more discourtesy than should ever be shown by great improvers of science." (53) Annoyed by Prout's remarks on physiologists' lack of chemical insight, Philip catalogued his own physiological achievements at great length and detail. A diplomatic reply from Prout was followed by Philip's accusation that Prout was rude and careless with words; Prout retorted that Philip was too fond of wordy disputes, fault-finding, and of expounding the "same thrice-told tale" of his own achievements; inevitably Philip replied in stronger terms that Prout's language and polemical techniques were those of a pamphleteer. And so this dreary dispute rolled on until finally Prout had the sense to withdraw from the argument altogether and allow Philip the egotistical last word.

The few facts we have presented are virtually all that is known of Prout's life; for although Agnes survived her husband by thirteen years, unlike many Victorian widows of distinguished men, she did

52. Ibid., 8,641-52(Philip), 705-7(Prout), 737-40(Philip), 769-70 (Prout), 770-6(Philip), 801-2(Philip), 802-4(Prout), 843-4(Philip), 1831; ibid., 9,38-46(Prout), 69-79(Philip), 1831-2.

not publish any loving study of her husband's career. Only rarely
too does a scientific contemporary mention Prout's name, and then
never to reveal any personal facet of his life. Since none of
Prout's personal correspondence has survived (54) the remainder
of his life after 1814 has to be summarised by the generalisation
that he became a very successful physician who specialised in
urinary and digestive complaints; the details of his life from 1814
until his death then become the details of his scientific books
and papers, and of his scientific influence.

The intense earache which he suffered as a youth (55) seems
to have been a symptom of the deafness that afflicted Prout long
before his death. It caused him to withdraw from, and even shun,
scientific society - to its loss, and perhaps even more to his.

Thus there is no mention of him at the Royal College of Physicians
after 1831 (56), or at the Royal Society after 1841 (57), or at the
British Association after 1839 (58); and he did not join the Chemical
Society when it was founded in 1841. Deafness must have been

54. There are an unknown quantity of letters and notebooks, etc., in
Scotland which I have not been allowed to examine. It is to be
hoped that one day a bona fide student will gain permission to
For Prout's relations with William Lewis, see infra, Appendix, 5.


56. The Gulstonian Lectures, given in June 1831.

57. After this year Prout's name does not appear on membership of the
Physics and Meteorological Committee, *Royal Society Minutes of
Council*, 1,254,1832-46.

58. *British Association Reports*, 8,xxix, 1839; his name disappeared
from the list of subscribers to the Association after 1837.
especially tragic for him since he had a great love of music. At some time in his life he built an organ "which he played with great skill; several anthems were also composed by him" (59) - presumably strictly for family consumption.

He fell ill during the cholera outbreak of 1848, and became much worse in the summer of the following year. An autumn excursion into the country did not improve matters, but emaciated, he returned to London to continue with his practice. His health grew steadily worse during the Spring of 1850; finally, when the President of the Royal Society, the surgeon Sir Benjamin Brodie, called to see Prout on the 9 April, Prout told him that he knew he was dying. The end came the same day; the cause was apparently gangrene of the lung following a burst abscess. Curiously, he had requested that no post mortem should be made.

Prout is buried somewhere in the vast acreage of the "new" Kensal Green cemetery, and there is a memorial tablet in Horton village church which simply records:

Sacred to the Memory of William Prout, M.D., F.R.S.
Born in this parish 15 January 1785
Died in London 9 April 1850
Scintillulam contulit

Prout was an early riser who did much of his research and writing before he breakfasted at 7.0 a.m.; the remainder of the day was devoted to his patients. "Besides his extensive town practice, scarcely a day passed that boxes and parcels did not arrive from

the country, and the analysis of their contents, together with the necessary correspondence, consumed no small portion of each day." (59) There are also references by his obituarists to an extensive foreign correspondence, but no trace of this has come to light.

In stature he was a slim man of medium height. The following description fits the two portraits (44) which are reproduced in this thesis.

His head was nobly developed, and his intellectual qualities strongly marked; (60) the hair soft and snowy white. His features were delicately chiselled, eyes brilliant, complexion very pale, but the expression of his countenance combined benevolence with great intelligence. There was a blandness in his manner which inspired confidence, and set the most nervous patient at ease. He always dressed with scrupulous neatness, usually in black, with gaiters, or silk stockings. (61)

A notable estimate of Prout's character has been quoted at the beginning of this chapter. He undoubtedly impressed those with whom he came into contact. Thus Thomas Thomson thought very highly of him, and William Charles Henry, who disagreed profoundly with Prout's interpretation of the atomic theory, admitted that like Wollaston and Davy, Prout possessed "a taste for extreme exactitude" and an "unrivalled manual expertness" never achieved by Dalton. (62)

60. A phrenological reference indicating perhaps that Elliotson wrote this obituary.

61. Med.Times, 1,17,1850; quoted Munk, Roll, 3,113. Cf. the Lancet's remark that he was "the beau ideal of the venerable physician", op.cit. April 13,1850.

The influence of his genius will be felt to the remotest generations, wherever chemistry, in its varied and important applications to physiology, pathology and therapeutics, to health, disease, and remedies, shall be studied. It is an honour to our profession and country to have produced such a man; it should be felt as an universal regret that his career of usefulness and investigation has closed. He has died, however, full of years as of honours, and "the silver chord" was not "loosed" until he had raised in his works an imperishable monument of his fame. (63)

The truth or falsehood of this assessment will become apparent in the following pages.

63. Lancet, April 13, 1850, p. 449.
WILLIAM PROUT

From a painting by H.M. Paget in the Roy. Coll. Physicians
WILLIAM PROUT

From painting by H.W. Phillips Jr. in Roy. Coll. Physicians
Chapter Two: Analytical Techniques

The analysis of chemical substances has been advanced by chemists at two levels: chemists have tried to resolve substances into either their "proximate" or immediate principles (e.g. the resolution of a carbonate into an oxide and carbon dioxide), or into their ultimate elements (e.g. the analysis of lime into the elements calcium, carbon and oxygen). Although both these kinds of analysis may be pursued qualitatively or quantitatively, the analysis into proximate principles is usually associated with qualitative ends, while elementary analysis, which usually succeeds proximate analysis, is associated with quantitative procedures.

Historically, in the divisions of chemical science known as vegetable and animal chemistry, the techniques of proximate analysis were developed from the seventeenth-century onwards and first systematised by Fourcroy and his colleague Vauquelin, and finally by Chevreul. (1)

However, after the acceptance of Lavoisier's antiphlogistic chemistry and the appearance of the atomic theory, the methods of elementary analysis which were due to Lavoisier, Saussure, Gay Lussac and

M. E. Chevreul, Considérations Générales sur l'Analyse Organique, Paris, 1824; for the distinction between immediate and elementary analysis, see p. 6.
There is no standard account of the development of organic analysis. Besides the original sources cited, I have found helpful:
H. Kopp, Geschichte der Chemie, Brunswick, 1843-7, vol. 4, passim;
(although based on Kopp this is excellent); A. J. Berry, From Classical to Modern Chemistry, 1954, p. 143ff.
Thenard, Prout, and Liebig, proved themselves more important for the development of organic chemistry and biochemistry.

Although Prout used both kinds of analysis, he was particularly associated with the quantitative determination of elementary composition. His development as a chemical analyst took place during the decade 1815 to 1825, and by the end of this period he had established for himself a European reputation as an accurate and authoritative experimentalist. In this chapter we shall consider the contributions which Prout made to the development of gravimetric analytical apparatus for the purpose of elementary organic analysis. Then, in the following two chapters, some of his achievements in qualitative and quantitative analysis in the fields of the chemistry of urine and digestion will be considered.

Quantitative organic analysis for the determination of elementary composition began in 1784 with Lavoisier's technique of direct oxygen combustions by means of which he analysed alcohol, wax, olive-oil and sugar. (2) The carbon dioxide formed in the combustion was absorbed in caustic potash and the water formed calculated by weight differences. However, this method was cumbersome, dangerous, and inaccurate, and Lavoisier's early death prevented him from devising any improvements. Although some successes using Lavoisier's techniques in a modified form were recorded by the Swiss chemist, Nicolas de Saussure, (3) it was Gay Lussac and his collaborator


3. J. de Physique, 64, 316, 1807; Annals of Philosophy, 4, 34, 1814.
Fig. 1

GAY LUSSAC AND THENARD'S ORGANIC
COMBUSTION APPARATUS

(Recherches Physico-Chimiques, vol. 2, endplate)
Thenard who first introduced in 1810 a technique which was capable of producing reliable, and therefore significant, results. (4)

In their method (Fig. 1), the organic substance to be analysed was mixed into a paste with the oxidising agent potassium chlorate and heated in pellet form in a vertical tube by means of a spirit lamp. The products of combustion, oxygen, carbon dioxide, water and (when present) nitrogen, were collected over mercury in a special apparatus, and their volumes measured. The volume of oxygen produced from a fixed weight of chlorate was first determined in a control experiment, and the difference between the potential volume of oxygen released by the chlorate, and the volume of excess oxygen found after combustion, could be assumed to have formed water (after allowing for the formation of carbon dioxide), and hence the quantity of hydrogen in the original substance could be determined. The amount of carbon contained in an organic compound could be simply estimated from the volume of carbon dioxide formed since the composition of this gas was well known to chemists. Finally, nitrogen could only be determined after the other gaseous products had been removed, and in fact the method was inappropriate for animal materials since a complex mixture of nitrogen oxides, which defeated the exercise, was very easily formed.

Although the technique was time-consuming, dangerous, and dependent on the dexterity of the operator for its accuracy, (5) the French chemists applied it with skill and fair accuracy to the


5. For Berzelius's criticisms, see Ann. Phil., 4, 402, 1814. An improved furnace apparatus was described by Thenard in his Traité de Chimie,
Fig. 7 is not part of Berzelius's apparatus.

analysis of a variety of animal and vegetable substances including fibrin, albumen, sugars and vegetable acids. One of Gay Lussac's and Thenard's more interesting theoretical conclusions was that the proportions of hydrogen and oxygen in sugars and mucilaginous substances were similar to the proportions of these elements in water. Such substances, they suggested, could be regarded therefore as hydrates of carbon. Much later Prout adopted this division of organic substances in the family he called saccharinicus, while the word carbohydrate was not introduced until 1844 by Carl Schmidt.

After 1814 organic analysts (including Prout) adopted Berzelius's simple horizontal arrangement of the French chlorate method. This had the advantage that elaborate calculations were avoided since carbon and hydrogen were weighed directly as carbon dioxide and water by absorption in caustic potash and calcium chloride. Moreover, the danger of explosions, which were only too frequent with the French apparatus, was completely removed since combustion was effected by a charcoal fire which was confined by a moveable screen. The screen was moved slowly along the whole length of the tube so that the rate of combustion was carefully controlled by heating only one section of the tube at a time. These features, with modifications, were adopted by Prout in his first analytical publication.

5. (cont.) 3rd ed., 1821, vol. 4, p. 188 and Plate XXXII.
and in this state they will dissolve, and retain water with
stability. The mode, therefore, adopted by Berzelius was
inadequate to separate the whole of the water formed; an
account for the small quantity obtained by him on our
acid. The remark, however, if founded in truth, applies
to all the substances analysed by him. In a quaternary
carbon and azote are perhaps the elements whose quanti-
most easily arrive at a just knowledge of.

ARTICLE IV.

Description of an Instrument to measure and register the
Fall of the Tide throughout the whole Flow and Estuary.

The parts of this instrument which are devoted to me-
height of the water consists of a copper tube placed in the
sea or river in a vertical position, and provided with a
filling its bore, at the same time that it is freely at liberty
to fall upon the surface of the water, which is admitted into
end of the tube by a small opening, or by a pipe, and which
will preserve the same level as the external water of the sea
prevent the float being affected by the undulations of the

A small line is attached to the float, and carried up to
roller, round which it makes several turns; and the line's
weight, being wrapped upon the axis of the wheel on one
side of the centre, will cause the wheel to turn one way
the float rises or falls upon the surface of the water. This
motion is communicated by wheel-work to a second
ylinder, upon the surface of which a sheet of paper is

The registering part of the instrument is an eight-day
clock, which at every ten minutes lets fall a small hemp
mark on the sheet of paper wrapped upon the cylinder.
sequence, this sheet will be covered with a succession of
the intervals between them will show on a reduced scale
of rise or fall of the water during the interval, ten minutes
elapsed between the different marks made by the clock.

The general action of the machine being understood, the
construction will be explained by the drawing, in which
fig. 2, represents the whole machine mounted upon a tripod
ported upon three feet screws a, by means of which
adjusted that the clock will beat correctly, or in other
escape of the teeth of the swing-wheel will take place
stances from the perpendicular on the opposite sides; the
ports a mahogany table B, which is represented on a line
figures 3 and 4, the first being a side view, and the other
Upon this table are erected standards C D E for the sur

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One source of inaccuracy in Gay Lussac's and Thenard's method as Berzelius pointed out, was the absence of any vigorous drying of the organic substance under analysis. The presence of water vapour might lead to the over-estimation of hydrogen and oxygen. To overcome this Berzelius recommended the use of hygroscopic sulphuric acid to dry substances before they were analysed. (9) In 1815 Prout described a piece of equipment of his own design in which substances were dried by sulphuric acid in a vacuum at 212° F. (10) (Fig. 3) and thereafter exsiccation became standard practice among analysts. (11)

In his discussion of the chlorate method Prout recommended the use of an "apparatus somewhat similar to Berzelius", (12) but he suggested that it was best to make separate combustions for the estimation of individual elements rather than attempt to analyse a

9. Ann.Phil., 4, 402, 406-9, 1814, "I introduced the dried substance into a flagon, the mouth of which I covered with paper. The flagon I plunged two-thirds into a sand-bath, which I had prepared, of a large porcelain mortar filled with sand, which I had previously raised to the heat of 212° or 230°. I put this hot mortar under the receiver of an air-pump, along with a quantity of concentrated sulphuric acid, and immediately pumped the air out of the receiver."

10. Ann.Phil., 6, 269-73, 1815; Plate XXXIX, fig. 1 at p. 273.


12. Ann.Phil., 6, 172, 1815. He thought that Robert Forrett's method using mercurous oxide in an iron tube, although successfully used in the analysis of cyanides, was not for wider exploitation. Forrett used a spirit lamp and found carbon dioxide by potash absorption, Phil.Trans., 1815, 220-30.
substance with a single combustion (as with Berzelius), even though
this made the whole procedure very laborious. Berzelius, he thought,
had overlooked the fact that any water vapour produced during the
combustion could interfere with the other gaseous products and hence
produce a hydrogen estimation above the true one. (13) Prout did
not state explicitly how this error could be overcome, but in later
papers he always advised against the direct weighing of water and
recommended that hydrogen should be estimated by difference. Liebig,
on the other hand, was to agree with Berzelius, and weigh the water
directly.

In the gravimetric analysis of organic compounds Prout demonstrate
ed how Wollaston's equivalent slide-rule, or Synoptical Scale
of Equivalents, could be modified so as to produce the empirical
formula of a substance, assuming (as Berzelius had declared after
much hesitation in 1814) that organic substances obeyed laws of
definite proportions. (14) The number of atoms of each element con-
tained in a ternary or quaternary compound was easily discovered,

13. Prout specifically picked on the case of oxalic acid which
Berzelius had estimated as $C_{12}H_2O_{18}$. If this was the anhydride
then no hydrogen should have been found. In a letter to Marceet,
Berzelius said that he dried gases through tubes of calcium
chloride before analysis. "Je souhaiterais avant que l'on me fet
des objections contre la justesse de mes resultats, que l'on
voudrait se convaincre par des experiences, faites avec les memes
soins que les miennes, si les objections sont fondes ou non", 8 Dec. 1815, H.G. Soderbaum (Ed.), Jons Berzelius Bref, 6 vols.,
started on the wrong footing with Berzelius, see infra, Chap. 3.

14. Ann.Phil., 6,273,1815. There is a manuscript copy of this article
among the Prout papers of 3pp. Berzelius's opinions are consid-
ered in Chapter 3, infra.
he wrote, provided that the proportions of at least two of the elements were known. Also of advantage, but not absolutely necessary, was a knowledge of the atomic (i.e., molecular) weight of the substance. If the scale of a manufactured Wollaston slide-rule was pasted over with a new logarithmic organic scale bearing only "multiples of an atom of oxygen, hydrogen and carbon, from one to ten; and of azote, from one to four or five, or more", the instrument could be adapted for organic estimations. Prout gave two worked illustrations by way of instruction. For example:

Suppose we had the weight of a particle of a ternary compound to be 46.5, oxygen being 10, and that 46.5 parts of it contained 15.15 carbon, 1.34 hydrogen, and consequently 30.01 oxygen. To find the number of atoms of each of these elements, we have only to place 10 on the slide opposite oxygen, and then opposite each of the numbers respectively we have the numbers of atoms of each element required. Thus opposite 15.15 carbon, we have 2 carbon; opposite 1.34 hydrogen, 1 hydrogen; and opposite 30.01 oxygen, 3 oxygen. Such a compound, then, will consist of three atoms oxygen, two atoms carbon, and one atom hydrogen. (16)

Wollaston slide rules were made by all the London instrument makers, but I have not found any evidence to suggest that Prout's rule was made professionally. It would in any case have been a very simple modification to effect, and although I have found no record that Prout's suggestion was noticed by his contemporaries, it must have been one which occurred to several of them.

When Gay Lussac introduced analysis by copper oxide in 1815 (17) Prout quickly adapted the new method to a charcoal furnace apparatus


16. Ibid., p. 270. For some reason Prout thought it improbable that any organic solid substance would contain more than six atoms of carbon, or even as many as four; or that more than two atoms of nitrogen ever entered into combination, Ibid., p. 271.
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which he had developed. This apparatus, which was an adaptation of Berzelius's chlorate apparatus with the combustion products collected vertically instead of horizontally, was first described in 1817 before the Medical and Chirurgical Society. (18) (Fig. 4) Prout's main reason for using copper oxide was that the chlorate technique had never been satisfactory for the analysis of animal materials since it never produced a simple oxidation product of nitrogen. He had therefore been very impressed by Gay Lussac's demonstration that copper oxide could be made hot enough to oxidise hydrogen and carbon, and yet not affect nitrogen. (19)

The new apparatus consisted of a sturdy glass tube of 1/4 inch bore and 1 foot in length which was filled with about 4 grains of the organic substance mixed with an equal amount of copper oxide. Thin copper sheeting was wrapped around the tube to prevent accidents due to expansion, and undue heating of the oxide. (20) The tube was placed horizontally inside an iron charcoal furnace which (as in Berzelius's chlorate method) possessed a moveable partition


18. Med.Chir.Trans., 8, 532, 1817 (with plate); also in Ann.Phil., 11, 352, 1818, "It need hardly be observed, except in justice to Berzelius, that the apparatus here described in only a simple modification of that used by him for a similar purpose." Berzelius continued to use potassium chlorate and salt until several years later.

19. High temperatures had to be avoided.

20. Cf. Berzelius's apparatus where iron sheeting was used.
"by shifting which the fire can be gradually applied to the whole

tube in succession." (21) Another tube filled with calcium chloride
to absorb water led the combustion products to a mercury gasometer.

In general, however, separate experiments were made for
determining the quantities of water and of gases formed, and
in the latter case a small bent tube, to convey the gas to the
gasometer, was connected at once to the tube, A, at the point
E by means of the caoutchouc tube G. (22)

Thus, despite the advantages of the copper oxide method, in the
interests of accuracy, Prout continued to estimate the elements in
separate combustions; he also repeated the analyses many times "with
every possible attention to those circumstances which might influence
the results." All the substances had been dried in his vacuum
apparatus beforehand, and the weighings were made with a delicate
balance whose weights had been specially calibrated for Prout from
platinum standards made by Troughton.

Apart from the gasometer (which had been personally calibrated
by Prout) the layout of the apparatus was identical to that of
Berzelius. However, the apparatus described by Prout in 1820 was
very different; and if prototypes are to be looked for, it was a
return to the layout of Gay Lussac and Thenard. (23) (Fig. 5) The
copper oxide combustion was retained, but Prout argued that his new
technique was "susceptible of far greater precision, and is much less

22. Ibid., p.526.
A full description (slightly different from Prout's) was given
by William Henry, Elements of Experimental Chemistry, 9th ed.,
London, 1823, vol. 2, pp.167-9. The diagram is also different
(Fig.6).
Fig. 5
FRONT'S ORGANIC SPIRIT LAMP COMBUSTION APPARATUS OF 1820
(Ann.Phil., 15, 190, 1820)

Fig. 2
Plate CII

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Fig. 3

Fig. 4

Prepared for D. Thomson, Snr., for Baldwin and Dolch, 3ury, publisher, 1820.
troublesome to use than any that has hitherto been recommended for the analysis of organized substances." (24) The major changes were that the combustion was affected by a spirit lamp instead of a charcoal furnace, (25) and that the layout was vertical instead of horizontal. The design of this "spirit-lamp combustion apparatus" was very ingenious.

4 grains of the substance to be analysed, mixed with 4 grains of copper oxide, were placed in the glass tube, G, which was some 10 inches long and 1/5 to 1/6 inch in bore. This tube was attached vertically through a cork stopper into a brass collar, C, at the base of a wooden dish, H. A circular spirit-lamp, F, which was allowed to envelop this tube, could be moved up or down by counterpoised weights, MM, attached to a pulley system. The gaseous combustion products were collected (as in the earlier apparatus) in a graduated tube, K, by mercury displacement. The gasometer, K, was enclosed by another glass tube, I, which could be fitted to the base of the wooden dish by a brass screw thread. Ingeniously, this external tube could be used to equalise "the heights of the mercury

24. Ann.Phil., 15,192,1820. See the brief but favourable notice by Thomson, ibid., 16,3-4,1820; also ibid., 16,218,1820, where Thomson revealed that Prout had once worked on the ferrocyanides.

25. Prout said much later that the idea of using a spirit lamp instead of a furnace came from Porrett, Phil.Trans., 1827, p.360; however, a lamp had been used before this by Gay Lussac and Thenard.
on the inside and outside of the tube, \( K \), and thus to supersede the necessity of calculation. 

A vertically adjustable circular plate, \( N \), prevented the spirit lamp, \( F \), from heating the mercury dish.

The apparatus was used as follows. The lamp \( F \) was raised "to the upper part of the tube, \( G \) (two or three inches of which at this part is filled with pure oxide of copper only), and there permitted to stay till the tube becomes red-hot. When this is the case, it is depressed a little, and another portion of the tube similarly heated, and so on, till the whole of the tube has been heated in succession, when the operation is completed. The gaseous products are then analyzed in the usual manner, if the substance submitted to the operation has contained azote; if not, the whole of the gas (except a very minute quantity) will be carbonic acid." (27) Hydrogen could be estimated in a number of ways.

1. The best and most accurate way, Prout suggested, was to weigh the tube \( G \) before and after the combustion. The loss in weight could be used to calculate the hydrogen content provided the quantity of gases produced from the same weight of the analysed substance had been made previously. Presumably corrections for water vapour would have been applied, though Prout does not mention these.


2. Water could be collected in the special tube (fig. 4 of Fig. 5) instead of the gasometer.

3. Porrett's method: (28) "The quantity of gases being ascertained as usual, the oxide of copper employed in the experiment is to be put in sulphuric acid. The portion of the oxide which has been reduced is thus obtained in a metallic state, and consequently the quantity of oxygen which has been expended may be thus ascertained." (29) Since both copper and its oxide dissolve in sulphuric acid, the estimation would not have proved very accurate.

Of the three methods, the first was the best thought Prout; the second method was open to error, and the third was time-consuming.

When specific gravity measurements were required, a special glass mercury gasometer (Fig. 5, no. 3) could be fitted to the combustion tube, and the dish and the graduated tube, K, removed. By way of improvement Prout suggested that it would have been simpler to have avoided the need for the wooden dish, H, the external tube, I, and the specific gravity device by providing "an oblong vessel furnished at one end with a deep well for equalising the height of the mercury on both sides of the graduated tube, K." (29) This improvement was evidently affected by the optician and instrument maker, Tuther of Holborn, who seems to have manufactured Prout's apparatus on a small scale. It was this version of the apparatus that was described by William Henry in 1823. (30) (Fig. 6)

Two square upright pillars are morticed into a square tray (a a at the bottom of the figure) about 3-4ths of an inch deep, and are fixed at the top by brass screws into a flat shelf of wood, 3 in. broad at each end, and 5 in the middle, in which is an oval slit or hole 4½ inches long and 1½ wide, distant 1½ inches from the right hand extremity of the shelf. Below this is another shelf, b b, which is moveable by a rack and pinion worked by a small handle, as shown at f. Into a shallow cavity in this shelf is fixed a cistern of copper covered with hard varnish.

Fig. 6: PROUT'S SPIRIT LAMP COMBUSTION APPARATUS IN THE VERSION BY TUTHOR OF HOLBORN

In this later version the shelf, bb, and the spirit lamp could be worked by a rack and pinion mechanism instead of by a pulley system. Instead of a wooden trough the shelf was fitted with a copper cistern which contained a deep mercury well, d, used for equating the levels of the mercury within and without the graduated tube, h, by immersion of the latter within the well. The outer vessel of Prout's prototype was thereby avoided. Otherwise, the instruments were used in an identical manner.

Henry pointed out that since the apparatus allowed only part of the combustion tube to be heated at a time, this was a disadvantage when liquids, or solids which yielded liquid products, were analysed. To overcome this Henry recommended that a multiwick spirit lamp that had been devised by the artisan J. T. Cooper of Lambeth should be somehow amalgamated with Prout's apparatus. It would, however, have been difficult to accomplish this without arranging the combustion tube horizontally and this would have led (as Henry recognised) to other difficulties. Nevertheless, such a lamp was adopted by Prout in his final apparatus.

French and German translations of Prout's paper as well as praise from Berzelius for ingenuity made the spirit lamp apparatus known throughout Europe and America. Prout claimed that his


technique was a great improvement on his own and other chemists' charcoal furnace methods, and that substances analysed with it seemed to contain more carbon than with the old charcoal technique. Nevertheless, Prout did not remain satisfied for long once he had recognised some of the awkward properties of copper oxide, and eventually, by 1827, he had made a partial return to Lavoisier's direct oxidation procedure with a costly and elaborate apparatus.

The disadvantages of copper oxide oxidation in organic analysis only slowly dawned on Prout and other chemists.

1. In the estimation of hydrogen, water was either collected directly and weighed (Berzelius and later, Liebig), or calculated from the weight loss of the analysed substance (Ure in 1822, and Prout). As we have seen Prout thought that the latter procedure was the best; but whichever method was adopted all extraneous water had to be carefully removed from the apparatus since copper oxide was hygroscopic. Although this had occurred to Gay Lussac and Thenard and to Prout "at a very early period", he awarded Andrew Ure the honour of solving the problem. Ure allowed samples of copper oxide to reach hygroscopic equilibrium in the atmosphere and then heated them to estimate the amount of water absorbed. He then used these figures as the proportionate error to be deducted in a combustion analysis. If the hygroscopic nature of copper oxide had been the only problem, Ure's analytical procedure could

have produced very accurate results.

2. However, when the oxide was heated to estimate the amount of absorbed water, Ure had found a "still more troublesome property", namely that it also absorbed air. Prout estimated that when 200 grains of pure dry warm copper oxide were exposed to the atmosphere they gained from 0.02 to 0.05 grains in weight within 10 to 15 minutes. Of this increase (only half of which was acquired before the oxide was cold) from about \( \frac{1}{2} \) to \( \frac{1}{4} \) was due to air, and the remainder was due to water. The air absorption was so variable that it was difficult to overcome.

3. As part of his analytical technique Prout had always recommended that in order to ensure complete combustion, more than one exposure to heat was necessary, and that each time the mixture had to be removed from the combustion tube for retituration.\(^36\) Unfortunately, when this was done it was usually found on the second heating that the reactants had gained weight instead of losing it because the copper oxide had partly reoxidised during its brief exposure to the atmosphere.

With these criticisms in mind Prout concluded that the traditional copper oxide method made complete accuracy impossible.

To conquer these, every means that could be thought of, as likely to succeed, were tried, but without effect, and I was obliged to relinquish the matter in despair, and endeavour to

\(36. \text{Phil. Trans.}, 1827, p. 361n.\)
Fig. 7: PROUT'S APPARATUS FOR ORGANIC ANALYSIS (1827)
contrive some other mode of analysis that should be free from these difficulties altogether. (37)

Prout's solution was to devise an apparatus in which direct oxygen combustions could be made in conjunction with the use of copper oxide. A description of this complicated instrument, (38) which was probably constructed by John Newman, can hardly be made more economically than in Prout's own words. (39) (See Fig. 7)

AB is a platform, two feet square, surrounded by a ledge about 2½ inches high, for preserving any mercury that may chance to fall about, and furnished with four adjusting screws (of which two, cc, are sectional views), by means of which it may be placed perfectly horizontally. Into this platform ... are fixed perpendicularly four square pillars, DE, DE, about four feet and a half high, at the top of which is placed another small platform, FF, about four inches wide, and which may be fixed or removed at pleasure by means of the brass pins, ab, ab. II are glass tubes graduated with the utmost care to hundredths of a cubic inch, and which are cemented at bottom into semicircular iron tubes enclosed in the blocks KK (as represented by the dotted lines). These iron tubes project a little below the wood at the lower part, where they are furnished with iron stop-cocks, SS, for drawing off the mercury when it may be necessary. Into the other end of these semi-circular tubes are likewise cemented the glass tubes LL (of smaller dimensions, and a little larger than the tubes II), and forming with them, when taken together, inverted syphons. The smaller tubes, LL, are represented as surmounted by funnels, RR, furnished with stopcocks, the object of which is to permit the mercury to flow into them with any velocity that may be required. On the tops of the larger tubes, II, are cemented the vertical stopcocks, MM, of which fig. 2 is a sectional view on a larger scale. ... the cup, a, is filled with oil, and ... the plug, b, which is square at the upper part, and adapted to a key, is furnished with a shoulder, on which the screw-cap, c, rests, and by means of which it may be tightened at pleasure. (40)


38. Berzelius, Traité, vol. 5, 1831, p. 33, "Plus tard Prout a décrit un autre appareil, qui est très compliqué."


40. In a note Prout added that the syphons, II, could be easily
FIG. 8: DETAIL FROM PROUT'S APPARATUS (1827)

(Phil.Trans., 1827, Plate XV)
The spirit lamp, \( q \), could be adjusted in position from the combustion tube, \( P \). "Fig. 3 (of Fig. 8) is an enlarged view of this lamp; it consists of two reservoirs, \( de \), for holding the spirit, connected together by means of the tube, \( f \), into which are placed, at the distance from one another of about \( \frac{1}{3} \) of an inch, a number of vertical burners, \( gg \), \&c. about \( \frac{1}{12} \) of an inch in diameter, and \( \frac{2}{3} \) inch long, and made as thin as possible, with the view of preventing the conduction of heat. These burners are each furnished with a few threads of cotton, and are bent a little like the teeth of a saw, in order that their flame may envelop the tube, \( P \), more completely. ... The tube, \( P \), is of green or bottle glass, moderately stout, and about \( \frac{1}{5} \) of an inch internal diameter. It is fixed between the horizontal parts of the vertical cocks, \( MM \), so as to be perfectly air-tight, and when required, the whole, or any part of it, may be heated by means of the lamp, \( q \), at the pleasure of the operator. (37)

The syphon gasometers, \( IL \), \( IL \), were filled with mercury and a quantity of oxygen (produced from potassium chlorate) was introduced into one of the tubes, \( I \), through one of the cocks, \( M \), as shown in no. 4 of Fig. 8. Once the oxygen had come to room temperature, "the exact quantity of the gas is to be accurately noticed, as well as the state of the barometer and thermometer at the same time. The tube, \( P \), containing the substance to be analysed, is then to be firmly fixed between the cocks \( MM \), and subjected to heat, during which the oxygen gas is to be transferred from one syphon to another, through the red hot tube, with any velocity that may be required, and which may be regulated by means of one of the stopcocks of the funnels, \( RR \), and the stopcock, \( S \), of the opposite syphon. (41)

40. (cont.) removed and replaced by others of a different capacity. "Those of a larger size have balls near the top, as represented by the dotted lines, and may contain as much as 20 cubic inches of gas." Prout advised that both legs of the syphons should be calibrated in order to overcome any effects of capillary attraction from tubes of slightly different calibres.

41. Phil. Trans., 1827, pp. 364-5. The apparatus of Fig. 8, no. 5 was simply a device to remove and analyse the products from the tube I.
The simple eudiometric assumption upon which this impressive instrument was based was that only one of three things could occur when an organic substance containing only carbon, hydrogen and oxygen was burned.\(^{(42)}\)

1. If the volume of oxygen remained constant in both siphons then the hydrogen and oxygen contained in the substance must have existed in the same proportions as in water. This reasoning is based on the empirical knowledge that there is no change in volume when carbon is burned in oxygen to form carbon dioxide. In this case, therefore, a complete estimation could be made by merely noting the amount of carbon dioxide which was formed.

2. If the volume of oxygen increased in the second siphon then there must have been a greater amount of oxygen in the substance than in water. In this case a complete estimation followed if the increase in the volume of oxygen, as well as the volume of carbon dioxide which was formed, was noted.

3. The final possibility was a decrease in the volume of oxygen in the siphon. This implied that the amount of hydrogen present in the substance was greater than in water. In this case the oxygen decrease, as well as the amount of carbon dioxide formed, had to be noted.

It is also worth noticing that the apparatus was of general use in gas analysis; for example, oxides could be analysed by reduction with hydrogen.

\(^{42}\) Phil.Trans., 1827, p.362.
However, even this new technique was not without its faults. Prout experienced some difficulty in ensuring a complete oxidation of the organic substance. In order to overcome this he used a booster oxidising agent in the combustion tube of either cupric or cuprous oxide. (When cupric oxide was used, oxygen was only admitted to the combustion tube after some preliminary heating of the tube). Of course, the need for a booster destroyed the simplicity of the volumetric estimation, and as Berzelius recognised, it made the technique something of a viscous circle. Nevertheless, Prout claimed that the merits of his new apparatus were threefold.

1. The errors due to the absorption of water and air by copper oxide were completely neutralised, and the corrections for any water vapour present in the products of combustion could be calculated from published tables.

2. A complete combustion was achieved.

3. The apparatus avoided the trouble of collecting and estimating hydrogen by weighing as water, and depended upon simple volumetric estimations.

Yet the disadvantages of the apparatus are fairly obvious. It was expensive to build and a large quantity of expensive mercury was required for its operation; it took even longer to use than the

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43. *Jahresbericht*, 8,242-4,1829: "Untersuchungen von dieser Art sind von grosser Wichtigkeit und verdienen alle Aufmerksamkeit, aber ihr Resultat muss nicht so gleich als richtig angesehen werden."

44. Prout wrote of publishing his own set of such tables, *Phil.Trans.*, 1827, p. 367n. Although this work was begun (infra, chap. 5), no tables were ever published.
traditional time-consuming copper oxide method; its accuracy, despite Prout's claims, was open to question (for example, the copper might have reoxidised and yet the gaseous volumes remained constant even though really there had been an excess of hydrogen present); and, critically important, the apparatus as described could not be used for the determination of nitrogen. Presumably Prout intended to make some provision for the latter defect; however, all his published analyses with this instrument were compounds of carbon, hydrogen and oxygen.

It was for this apparatus and the attendant analyses of sugars that Prout was awarded the Royal Society's highest honour— the Copley medal— in 1827. In the words of Davies Gilbert, then President of the Royal Society, the new analytical technique promised not merely to disentangle any one particular combination, but to afford an insight into all the products created by living chemistry. They [i.e. the Council of the Royal Society] have hastened, therefore, to stamp with their highest mark of approbation, as well as the mode of analysis itself as the specimens of what, in the hands of Dr. Prout, it has already performed; and not doubting, but that by the exertion of such talents, such ingenuity, and such labour, their satisfaction will from year to year be continually increased. (45)

The irony was that Prout published no more analyses after 1827! His career up until then reveals him as a chemist searching for the Analysts' Philosopher's Stone—a perfect technique of organic analysis. In one sense this was achieved by Liebig in 1830(46) by a simple

45. Philosophical Magazine, (2)3,61-2,1828.
46. Poggendorff’s Annalen der Physik und Chemie, 21,1,1834. The estimation of nitrogen was perfected by Dumas, Journal de Pharmacie et de Chimie, 20,129,1834.
Fig. 9: LIEBIG'S ORGANIC COMBUSTION APPARATUS OF 1830

(Annalen der Physik u. Chemie, 21, (1831), Taf. 1, endplate)
method which included an ironic return to the charcoal furnace of the early analysts, and the estimation of hydrogen by the direct weighing of water. (Fig. 9) Unfortunately, on the basis of his experience with the charcoal furnace and the unique use of copper oxide, Prout completely rejected the accuracy of Liebig's method.

Liebig's analytic apparatus was in effect tried by me nearly twenty years ago, and for rude approximations it answers very well; but it is not, in my opinion at all adapted for obtaining very accurate results. (47)

Although he removed this critical reference to Liebig from the final edition (1840) of his textbook on the urine, it is clear from this remark that until then he failed to appreciate the need for simplicity as well as accuracy in organic analysis. In fact for very accurate analysis a compromise is made between the techniques of Prout and Liebig. Although the simple apparatus and method of estimation of Liebig is used, a current of air, or oxygen, is usually blown over the heated copper oxide as in Prout's procedure. (48) But it appears that Prout's apparatus continued to be used in England for several years after the introduction of Liebig's method. (49)


48. E.g. T. M. Lowry and A. C. Cavall, Intermediate Chemistry, rev. 2nd ed., London, 1941, p. 448. The method seems due to Hess, British Association Reports, 6, 1839; and it was first used seriously by Dumas and Stas, Annales de chimie, 1, 1839.

Prout had the analyst's patience and good sense. He always insisted that no results should ever be recorded until the chemist had completely mastered his apparatus and fully considered whether the nature of the substance under analysis made a modification of the usual technique necessary. Repeated observations were advisable, just as in astronomy. But the most important analytical principle, and one which he continually emphasised, was that every possible attempt had to be made to analyse substances in their purest states.

Too much attention is paid in histories of chemistry to the confusions and difficulties over nomenclature and atomic weights in the development of nineteenth-century chemistry, and not enough consideration is given to the tremendous technical difficulties with which the early analysts wrestled. But the intellectual problems of organic chemistry would never have been solved without the dogged search for adequate foundations that was made by analytical chemists. Prout spent twelve years of his life looking for an apparatus and technique which would provide accurate analyses of organic materials, but he was never satisfied that he had solved the problem. Although Andrew Ure could write optimistically that his own analytical technique allowed him to complete six combustions a day, (50) we can be sure that Prout worked at a much slower pace when it is remembered that he had to combine his chemical hobby with the heavy professional duties of a London specialist. Prout's friend

50. Phil.Trans., 1822, p. 460. Ure's claim seems very unlikely since even Liebig, in recommending the time saved by his new method, only made a claim for 400 analyses a year, Annalen der Pharmacie, 36, 193a, 1830 (an editorial note to a paper by Hess). Berzelius's method had taken 1½ to 2 hours, Ann.Phil., 4, 405, 1814.
Charles Daubeny aptly described the situation when he wrote:

The greater part of Dr. Prout's analyses were made with an apparatus of his own which, however ingenious it might be, was far more difficult to use, and required for its success many more precautions than that at present in the hands of chemists, and hence the precision to which he attained is the greater subject for commendation. Add to which, that these delicate investigations were carried on by him, unassisted, amid constant interruptions, at intervals snatched from the daily demands made upon his time by professional engagements. (51)

51. *Edinburgh New Philosophical Journal*, 53,99,1852. It is not clear whether Daubeny was thinking of the apparatus of 1820 or of 1827, but his comment applies equally well to both.
"I am engaged in the investigation of the laws which (molecular weights) obey - laws which appear to regulate not only the operations of the animal economy, but the whole material world." (1)

In a letter published in 1822, Prout wrote that he had been inspired to enter into the field of organic chemistry by the hope that a discovery of the laws which governed the combination of carbon, oxygen, hydrogen and nitrogen in organic compounds would lead to "an insight into the laws which regulate the union of other elementary principles". He had therefore

"set to work, and after a very great labour, and no trifling expense in apparatus, &c., succeeded, as I supposed, in analyzing more or less perfectly almost every well-defined and crystallized organic substance that I could procure. A few of my earlier results were published, perhaps prematurely, but the great mass, as is well known to several of my friends, still remain by me, nor have I for various reasons, the least inclination to publish them at present." (2)

The analyses released by him for publication in 1827 were relatively few in number, and it appears therefore that little of this analytical mass was subsequently published. (3) Our conclusion must be that Prout was a perfectionist who was unprepared to publish

1. Medico-Chirurgical Society Transactions, 9,484,1818.
3. They have not been located in manuscript form.
authoritative analyses until he was absolutely certain in his own mind that his technique was accurate and hardly open to improvement. It must be admitted, however, that Prout's refusal to publish many of his results was old-fashioned and out of keeping with nineteenth-century, and of course, modern methods of scientific communication. In the end, it lessened his fame, and robbed him of the essential stimulus which comes from mutual scientific criticism.

Although a belief that chemical phenomena were the basis of physiological processes was quite common among early nineteenth-century chemists —certainly among those who practised vegetable and animal chemistry— it was not always recognised by physiologists. Part of Prout's aim, and achievement, was to alter this state of affairs; what he began, Liebig completed. But chemists were not able to speculate about the details of physiological chemistry until they first had a knowledge of elementary compositions. This knowledge, moreover, had to be both accurate and extensive. The fact that Prout spent much time in the search for accurate methods and that he published relatively few analyses compared with Liebig, did not, however, prevent him from applying his knowledge to physiology and medicine. A review of his work in the field of urine chemistry will make this clear.

Prout was already well-known in London scientific circles as an animal chemist when he first came to general scientific attention as an organic analyst with the publication of five short papers on the proximate analysis of various substances in 1815. Most of these papers, and several later short analytical papers, were
ephemeral and inconclusive, and do not justify any general
discussion. (4) For example, Prout attempted the proximate
analysis "of the colouring matter, or ink, ejected by the cuttle-
fish". (5) His conclusions were solely negative, e.g. that it
contained no albumen; the sepia's ink, melanin, is a polymer of
indigo - 5:6 - quinone, and much too complex for analysis at this

4. The two papers of 1815 not discussed in the text are:
from Astley Cooper in April 1814. Prout shows a thorough acquain-
tance with simple chemical tests and reagents but he was unable
to reach any satisfactory conclusions.

Offered as a correction to Vauquelin and Buniva who had claimed
to have found a crystalline amniotic acid in the amniotic fluid
of a cow. (Annales de chimie, 33,269,1800). Prout could find
nothing in this fluid (from a cow in early gestation) which
agreed with their analysis and he conjectured that their disagree-
ment with him might be due to the different ages of their
respective samples. Later, Laissagne made successive comparative
analyses between the amniotic and allantoic liquors and renamed
Vauquelin's principle allantoic acid after he had demonstrated
that it was present in the allantoic, but not the amniotic fluids.
He made no reference to Prout (Ann.chim., 17,295,1821) and this
led E.W.B. (rayley) to write extravagantly that there was no
problem "because the authority of Dr. Prout, by whom the
confirmatory evidence has been furnished is so valuable upon a
point of this nature, on account of his minute acquaintance with
animal fluids, and his practical skill in their examination,"
Philosophical Magazine, 1,320,1832. Brayley believed that
Vauquelin and Buniva might have really analysed a mixture of the
amniotic and allantoic fluids rather than the allantoic liquid
as Laissagne had charitably suggested. The episode illustrates
the strength of Prout's reputation during the 1830s. According
to Turner, Elements of Chemistry, 4th ed., 1835, pp.679,930,
Prout analysed the amniotic fluid of a pregnant woman. I cannot
find that this was published by Prout. See also J.R. Partington,

5. Ann.Phil.,5,417-20,1815 (note design argument at end) and
W. Buckland, infra, p.70. Cf. H.M. Fox and G. Vevers,
period. Yet all five papers reveal Prout's acquaintance with, and concern for the improvement of, chemical tests and analytical reagents, or his general skill and ability as an analyst which was frequently remarked on by contemporaries. For instance, when the first Professor of Chemistry at London University, Edward Turner, began his elaborate programme of analyses in order to check Thomas Thomson's values for the atomic weights, he used silver chloride as a standard. However, Prout, "whose remarks are always pertinent, being founded on careful observation", pointed out to Turner that "pure" silver chloride usually gave out hydrogen chloride when it was dried, and that if this was not completely removed it would interfere with Turner's estimation of the atomic weight of chlorine. (In fact Turner had carefully dried his silver chloride even though he was unaware of the presence of the hydrogen chloride gas until he was told by Prout). (6) It was a concern for details like this, of course, that was to play a major role in the testing of Prout's hypothesis throughout the nineteenth-century.

Of major importance, however, were Prout's series of analytical papers relating to the chemistry of urine and digestion which may be said to have begun with the analysis of a snake's excreta in 1815 and ended with the analysis of fossil faeces in 1829. The practical

6. "...a doubt has been raised by my friend Dr. Prout, whose remarks are always pertinent, being founded on careful observation. Chloride of silver, as Dr. Prout informs me, invariably gives out muriatic acid at a certain stage of drying; and he suggests whether this loss may not be sufficient to influence the atomic weight of chlorine," Philosophical Transactions of the Royal Society, 1833, p. 534 and emphasised in Phil. Mag., (3), 4, 397, 1834.
nature of the researches in urine chemistry and their relation to
Prout's medical practice will now be discussed; the chemistry of
digestion will be discussed in the following chapter.

A good example of Prout's qualitative skill is his analysis
of a snake's urine in 1815. A sample of the urinary faeces of a
young boa constrictor on exhibition near the Exeter Change which
had been given to Prout by a Mr. Lean (7) was dissolved in hydro-
chloric acid and the insoluble portion shown to be uric acid by what
is now recognised as "the urease test". An ammonium oxalate test
devised by an otherwise unknown "Mr. Wilson" (8) provided the
additional information that the soluble portion was mainly calcium
phosphate and ammonium chloride. To his obvious surprise, Prout
found from quantitative estimation that the excrement was almost
pure uric acid (90.16%) (9), and he wondered whether this
extraordinary excretion was a pathological condition brought about

7. *Ann. Phil.*, 5,413-6,1815. The animal which was examined by Prout,
was some 10 feet long and fed about once every month on a rabbit.
A.W. Hofmann, otherwise generally a reliable historian, wrote
that Prout in 1815 "when a youth only in his 19th year" discovered
uric acid. See his *Life Work of Liebig*, Faraday lecture, London,
1876, p.84.

8. "oxalate of ammonia will often throw down lime when in a state of
union with animal matter, together with the animal matter itself,
when other tests will scarcely indicate its presence...For this
interesting observation I am indebted to Mr. Wilson."

9. | Component                     | Percentage |
<table>
<thead>
<tr>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>uric acid</td>
<td>90.16%</td>
</tr>
<tr>
<td>potash</td>
<td>3.45</td>
</tr>
<tr>
<td>ammonia</td>
<td>1.70</td>
</tr>
<tr>
<td>potassium sulphate and traces of sodium chloride</td>
<td>0.95</td>
</tr>
<tr>
<td>calcium phosphate, calcium</td>
<td>0.80</td>
</tr>
<tr>
<td>carbonate, magnesia</td>
<td>2.94</td>
</tr>
<tr>
<td>mucus and colouring</td>
<td>100.00 %</td>
</tr>
</tbody>
</table>

This is a good result.
by the serpent's unnatural captivity. This was the first demonstration that uric acid and not urea was excreted by reptiles. Prout recalled, however, that Wollaston had found uric acid in bird-droppings and that according to Berzelius "the excrements of a bird called guano, brought from the South American Sea Islands" contained large amounts of uric acid. (10) A few years later Prout's colleague John Davy, who had gone out to Ceylon, confirmed that uric acid was a constituent of the solid urine of snakes and lizards in their natural habitat, (11) and Prout himself confirmed it with a chameleon, crocodile and other reptiles. (12)

This analysis by Prout was but part of a more general programme of research connected with urine and digestion, and for some years he apparently continued (copied by other chemists) to use the boa constrictor's urine as a source of pure uric acid. (13)


11. Phil.Trans., 1818, p. 303, dated Ceylon, 25 March 1817. Although frustrated by his two predecessors, Davy's cousin, Edmund Davy, independently published detailed analyses in the manner of Prout, Phil.Mag., 54,303-6,1819. It is notable also that Vauquelin published an identical result, Ann.chim., 21,440,1822.

12. Chamaeleonis vulgaris, Ann.Phil., 15,471,1820, "Its composition was precisely the same as that of the urinary excrement of the boa constrictor and lizard tribe, as previously ascertained by Dr. Davy and myself." Crocodile, Transactions of Geological Society, 3,238,(1829)1834.

13. Pertington mentions that uric acid was obtained from zoos in Hamburg and commanded high prices, History, vol.4, p. 333.
Naturally he also examined the constrictor’s faeces although he did not publish the results. Samples were, however, presented to his friend Alexander Marcet who found they did not contain uric acid but calcium phosphate. Marcet concluded that the faeces "were nothing but the undigested residue of the food of the animal". (14) When the great geologist, William Buckland, wished to confirm his hypothesis that the coprolites found at Lyme Regis were a fossil faeces, Prout was a good choice for the position of geochemist. In 1829 Prout confirmed for Buckland that coprolites were basically calcium phosphate mixed with smaller amounts of compounds of lime, carbon, iron and sulphur, and he agreed that they resembled physically and chemically the faeces of living animals. (15)

"The above composition seems to prove beyond a doubt the animal origin of these bodies, or, in other words, that their basis is bone. The question is, by what means bone can be made to assume the appearance presented by them. That mere time, and the circumstances to which they have been exposed, are not sufficient to account for these changes, seems to be proved by the fact, that many of the specimens contain fragments of bone possessing its original characteristic structure. We must therefore seek for some other explanation; and your opinion [i.e. Buckland’s] that they are of faecal origin, or of the nature of album graecum, seems to me to offer a very satisfactory solution, and to account at once for their


chemical composition, for their form, and for their mechanical structure, which can hardly be explained on any other supposition. (16)

Prout then recalled that reptiles and birds, as he had previously demonstrated, excreted a solid urine composed of ammonium urate. But since this "is comparatively a destructible substance, it could hardly be expected to be met with in the present instance under circumstances so unfavourable for its preservation." (17) In more favourable circumstances, however, uric acid might be preserved, as in guano, and it was not too much to be expected that Buckland might one day discover uric acid in antediluvian deposits.

In the present case Buckland had found abundant supplies of coprolites wherever there were fossil remains of Icthyosaurus, and both Prout and Buckland noticed bones of smaller fish within the faeces. These smaller bones were stained by a "bright jet-black colour". Here Prout's previous experiments with the ink of the cuttlefish in 1815 came in useful for this colouring matter was identical to the fossil cuttle-fish ink bogs which were also to be found.

16. "The analysis of the fossil faeces of Icthyosaurus and other animals", Trans.Geol.Soc., 3,237-8,(1829)1834. Read 3 April 1829, this forms a letter to Buckland whose "On the Discovery of Coprolites, or Fossil Faeces, in the Lias at Lyme Regis, and in other Formations" was read 6 February 1829, loc.cit., pp.223-36; abstract Proc.Geol.Soc., 1,139,(1829)1834. For some other references to geochemical analyses by Prout, see Buckland's Bridgewater Treatise, Geology and Mineralogy, 2 vols., 1837, vol.2. Later Henslow found some 56-61% calcium phosphate in coprolites, British Association Reports, 14,51,1845. The agriculturalist, Lawes, patented the preparation of superphosphate fertilisers from coprolites in 1842, cf. Partington, History, vol.4, p.313. Although Lawes was a pupil of Prout's friend Daubeney, I have not been able to find any causative connection.

in the lias at Lyme Regis. This was conclusive evidence that "the
Ichthyosaurs fed largely upon the Sepiae of those ancient seas."

In view of this geochemical interest, and the evident knowledge
of contemporary geology which Prout displayed in his Bridgewater
Treatise, and the eminence of Buckland, it is somewhat surprising
that Prout was never elected to the Geological Society. (18)

Prout exhibited pure urea at his lectures on Animal Chemistry
in 1814 (19) and later, in two lectures to the London Medical and
Chirurgical Society on the 24 June 1817 and 1818, he presented the
results of his analyses of the primary constituents of healthy and

16. Private communication from Librarian Geological Society. The
biographical notice in Penny Cyclopaedia, Second Supplement, 1858,
p. 541, devotes as much space to this geological work as any other
topic.

of Gravel, Calculus and other Diseases, London, 1821, p. 9. (cited
by Edition as "Inquiry"). This is confirmed from manuscript
lecture notes. Prout's method was praised for its ingenuity and
became the standard technique described in chemical textbooks,
e.g. E. Turner, Elements of Chemistry, 4th Ed., 1834, p. 871.
Fresh urine was first evaporated to a smooth consistency; after
it had cooled, pure concentrated nitric acid was added until the
solution became a dark crystalline mass. This was then neutral-
lised with potassium carbonate or soda, and the mixture evaporated
in order to separate off the potassium nitrate by crystallisation.
The urea which remained was further purified by mixing it into a
paste with animal charcoal. The urea was then washed out with
distilled water, concentrated by evaporation, and allowed to
crystallise. More urea could be extracted from the charcoal by
washing it in alcohol, and the charcoal procedure could be
repeated many times to ensure absolute purity.
diseased urine. The first paper \( (20) \) was divided into two sections: the first part, which will be considered now, was analytical and theoretical; the second part, which will be considered later in the context of his urine textbook, was devoted to the medical treatment of diseased urine.

Prout's first account of the constituents of urine was confined to urea, "saccharine matter" and lithic acid (uric acid) because the other constituents such as phosphates and oxalic acid had proved intractable. After he had carefully described the chief physical and chemical properties of urea, Prout reported on its analysis using copper oxide in the charcoal furnace apparatus. For atomic weights he used those he had calculated in the anonymous paper of 1819, \( (21) \)


21. Ann.Phil.,6,32,1815. \( H = 1.25; C = 7.5; N = 17.5; O = 10 \) (i.e. \( H = 1; C = 6; N = 14; O = 8 \)). In the second paper, however, Prout stated "I wish to remark, that the numbers representing the weights of the atoms may be placed very differently according as different views are taken of the subject. I have not yet fully made up my mind upon this part of the inquiry", Med.Chir.Trans., 9,483n,1818, my stress.
and the resultant value for the proportional number of urea, 37.5, was uniformly adopted by other chemists; thus Wöhler used it in his paper on the synthesis of urea in 1827. (22) (See example)(p.8)

Other analyses of urea nitrate, uric acid, sugar, diabetic sugar and sugar of milk (lactose) were also given. However, Prout could find so little difference in composition between the three sugars that he concluded:

I am inclined to think the primary and simple saccharine principle is composed of one atom of each element, and that the varieties in its external characters are to be attributed to the influence of the presence of minute portions of foreign matters, analogous, for example, to what occurs in the mineral called aragonite. (23)

Prout was to elaborate this suggestion in 1827 into the cumbrously-titled concept of merorganization which will be discussed in the next chapter, and in sections on vital and molecular theory. (Chapters 4, 6 and 8)

The chemical section of the paper on urine was concluded with a number of important generalisations.

22. Wöhler, Annalen der Physik und Chemie, 12,253,1828. He actually referred to Prout's second paper (1818) where the same proportional weight was given, although C = 20% instead of 19.99%. Liebig confirmed Prout's analysis of urea for Wöhler's satisfaction, Aus Justus Liebig und Friederich Wöhlers Briefwechsel, ed. A. W. Hofmann and R. Schwarz, Weinheim/Bergstr., 1958, p. 11, and favourably referred to it before the British Association in 1837, Brit. Ass. Reports, 6,39,1837.

23. Med.Chir.Trans., 8,538,1817. Aragonite has two crystalline forms; which differ only in their water content; but at this period it was thought to contain a small amount of strontium.
1. The atomic theory or theory of definite proportions, holds good in all these instances. A circumstance which renders it probable, that this will afterwards be found to be the case in all substances capable of crystallizing or forming crystalline compounds both in the vegetable and animal kingdoms.

2. The above compounds appear to be formed by the union of more simple compounds, as urea of carburetted hydrogen and nitrous oxide, lithic acid of cyanogen and water, &c.; circumstances which render almost certain that their artificial formation falls within the limits of common chemistry.

The possibility of synthesis followed from the early radical theory of Gay Lussac. If organic compounds were built up from simple radicals or groups of atoms, then synthesis appeared possible, at least in principle. Although this was not the reasoning which lay behind Wöhler's experiments in 1827, it was the reasoning which led Prout to make several attempts to synthesise urea from ethane and nitric acid at this period. None of these attempts was successful.

Urea was first analysed by myself thirty years ago; and from its composition I was satisfied that it might be formed artificially. I made numerous attempts to form it, but did not succeed; and the honour of forming the first organic compound artificially is due to Wöhler. (24)

24. W. Prout, On Stomach and Urinary Diseases, (cited by edition as "On Stomach"), 5th ed. 1848, p.530. This is somewhat ungracious since in 1831 (Gulstonian lectures), Prout had publicly doubted Wöhler's achievement, Medical Times, 8, 390, 1831.
Berzelius had authorised in 1814 that the laws of definite proportions, as understood by him in inorganic chemistry, were not obeyed by organic substances. Berzelius's two rules of combination for inorganic compounds were:

1. When two elementary bodies combine, they unite in such proportions, that one volume of the gas of one combines with an equal volume of the gas of the other, or with two or with three volumes of that gas, without any intermediate fractions; and in these combinations one of the elements ought always to be considered as unity.

A corollary to this was that inorganic bodies of the first order of composition were always binary compounds.

2. When two bodies, each containing oxygen, combine, the oxygen in the one is always a multiple of that in the other by a whole number.

A more general form of the second rule was that when substances which contained a common electronegative element (like oxygen or sulphur) combined together, the ratio of the one to the other was always a whole number.

Although the second rule in its specific form referring to oxygen was obeyed by organic bodies, the first rule, and its corollary, was not obeyed. Organic compounds of the first order always contained more than two elements, and the only form of rule was,

When three or more elementary bodies, of which oxygen is always one, combine so as to produce a ternary, quaternary, &c. oxide, a certain number of atoms or volumes of one of the elements combines with a certain number of atoms or volumes of each of the others; but it is not necessary that any one of these elements should be considered as unity. (26)


26. Ibid., p. 325. At this date the known hydrocarbons were regarded as inorganic substances.
Thus, for Berzelius, the exclusive principle of organic formation was that compounds "of the first order contain more than two elements, none of which must of necessity be considered as unity." On the other hand, when first order organic substances combined with first order inorganic compounds, the ordinary laws of definite proportions were obeyed.

Prout's first conclusion was therefore essentially a confirmation of Berzelius's opinion. However, since 1814, Berzelius had had doubts concerning the applicability of any kind of rule of definite proportions to first order organic compounds, and in 1818 he wrote that "dans la nature organique, les degrés de combinaison sont presque à l'infini, et n'ont aucune analogie avec ceux qu'offre la nature inorganique." (27) The only analogy was that both inorganic and organic substances with the same composition had the same properties - but even this analogy was to be lost with the discovery of isomerism. Berzelius remarked that organic compounds were principally composed of oxygen, carbon and hydrogen, and that "leur atomes peuvent se combiner dans toutes les proportions sans que l'un d'eux y joue nécessairement le rôle de l'unité." (28) Hence, when Prout published his conclusions in 1817, he was unknowingly opposing Berzelius who now maintained that no analogy could be drawn between the laws of definite proportions which governed the composition of inorganic compounds and those which governed the composition of

27. Essai sur la théorie des proportions chimique, Paris, 1819, p.41. (The Essai was extracted from vol.3 of Lärbock i kemien, Stockholm, 1818).

organic substances. Therefore, when Marcet informed Berzelius of
Prout's conclusions, Berzelius was critical and sceptical.

As a result of these remarks Marcet forwarded a copy of Prout's
paper ("qui me paraît avoir du mérite") to Berzelius on 10
February 1818 and defended his friend in a covering letter. However,
Berzelius was unimpressed by Marcet's claim that Prout's experiments
had been made "avec beaucoup de soin et de bonne foi" and he replied
that "les résultats obtenus par le Dr. Prout sur la composition des
trois substances animales ne méritent certainement pas la confiance
que vous y mettez. Prout ne s'est point aperçu des difficultés
attachées à cette matière, et il a trop confiance en sa manière
d'appliquer les proportions définies." Once more Marcet sprang
to Prout's defence: "en vérité, vous ne lui rendez par justice.
C'est un bon travailleur et un homme vrai et modeste - mais sans
doute il peut se tromper comme tout d'autres. Cependant, je le
crois assez exact dans ce qu'il fait."

30. Ibid., p. 165.
31. Ibid., p. 173.
Although Berzelius made no reply to this letter it seems likely that he was thinking of Prout's two conclusions, as well as the work of Gay Lussac, when in his *Essai Sur la Théorie des Proportions* published in Swedish in the year following the controversy, he wrote:

> quelque chimistes ont envisagé la composition des corps organique d'une manière différente de celle que je viens d'exposer, d'après mes propres expériences. Elle a été représentée par les combinaisons inorganique binaire qui peuvent résulter de leurs éléments. Ainsi, l'un a trouve que la composition...de sucre, par un volume d'eau en gaz combiné avec un volume de carbone à l'état de gaz.” (33)

But how could the enormous number of organic compounds be explained in terms of such simple compositions? Even if Gay Lussac's binary hypothesis was justified in the case of alcohol and ether, it could never be justified for sugar which existed in three species "dont les différences, tant en propriétés spécifiques qu'en composition, resteraient inexplicables, d'après cette manière d'envisager la composition organique." If chemists adopted such a system they might fall into error by adapting their analyses to these preconceived views. Possibly there were different "species" of organic compounds to be found in different animals. Thus fibrin, albumen, and urea might vary in some small way from one animal to another and yet be a single species within their genus. Indeed fibrin of a cow's blood seemed to be slightly different from human fibrin. So Berzelius's point was that chemists should not be too hasty in the application of rules of chemical proportions to organic compounds, for it might turn out to be as harmful to the progress of organic

---

chemistry as it had been beneficial to the progress of inorganic chemistry. Until it had been shown that urea samples showed no significant variation, Prout's work was not to be taken very seriously. (34)

Finally, Berzelius levelled the objection that such a radical theory was opposed to his own electrochemical system in which oxygen was absolutely electronegative; for he now wished to tentatively extend his system of electrochemical dualism to organic compounds. This extension could only be made on the grounds that organic substances were oxides of radicals composed from carbon, hydrogen and nitrogen, and not if they were interpreted as compounds of simple inorganic radicals.

The peculiar distinction exists that in inorganic nature all oxidized bodies have a simple radical, whereas on the other hand all organic substances consist of an oxide with compound radicals. In the case of plant substances the radical generally consists of carbon and hydrogen, and in the case of animal substances of carbon, hydrogen and nitrogen. (35)

Prout's support for the French system of radicals, \((WX)(YZ)\), where up to two of the elements \(W, X, Y,\) and \(Z\) may be oxygen, was therefore distinctly opposed to the electrochemical dualism of Berzelius, + (XYZ)0, where \(X, Y\) and \(Z\) are elements other than oxygen.

THE ANALYSIS OF UREA - An example of Prout's analytical calculations
(from Med.Chir.Trans., 8,536,1817)

60°F. Pressure 29.5 inches  H = 1.25;  C = 7.5;  O = 10;  N = 17.5

4 grains of urea burned with copper oxide gave 2.45 grains water
6.3 cubic inches carbon dioxide
6.3 cubic inches nitrogen

hydrogen estimation

proportional number of water (H₂O) is 1.25 + 10 = 11.25

11.25 grains of water contain 1.25 grains of hydrogen

\[
2.45 = \frac{1.25 \times 2.45}{11.25} = 0.266 \text{ grains H}
\]

carbon estimation

100 cubic inches carbon dioxide weigh 46.596 grains (from 1815 paper)

\[
6.3 \times 46.596 = 296.52 \text{ grains} = 2.9 \text{ grains}
\]

proportional number of carbon dioxide is 7.5 + 20 = 27.5

27.5 grains of carbon dioxide contain 7.5 grains of carbon

\[
2.9 = \frac{7.5 \times 2.9}{27.5} = 0.799 \text{ grains C}
\]

nitrogen estimation

100 cubic inches nitrogen weigh 29.652 grains (from 1815 paper)

\[
6.3 \times 29.652 = 186.6 \text{ grains}
\]

Hence the analysis gives:

hydrogen 0.266
carbon 0.799
nitrogen 1.866
oxygen 1.066 by difference

That is,

\[
\begin{align*}
\text{H} & \quad 6.66 \% \\
\text{C} & \quad 19.99 \\
\text{O} & \quad 26.66 \\
\text{N} & \quad 46.66 \\
100.00 \% & \quad \text{by difference}
\end{align*}
\]

This leads to an equivalent formula \( \text{CONH}_2 \) where \( O = 10 \).

Any errors in calculation are no doubt due to Prout's use of a Wollaston slide rule.
After stating his third conclusion Prout noticed:

The remarkable relation found to subsist between urea and sugar, seems to explain in a very satisfactory manner the phenomena of diabetes, which may in fact be considered to consist in a deprived secretion of urea. Thus the weight of the atom of sugar (18.75) is just half that of urea (37.5); the absolute quantity of hydrogen in a given weight of both is equal, while the absolute quantities of carbon and oxygen in a given weight are precisely twice those of urea. (36)

Such analyses seemed to afford Prout with "glimpses of laws that will hereafter be found to influence the whole system of Nature's operations." The Pythagoreanism, or fixation with chemical arithmetic which is to be first found in Prout's anonymous paper in 1815 (Chapter 7), reappears in this third conclusion, and it was amplified in the second paper on the chemical constituents of urine in 1818. (37)

ANALYSES OF 1817

<table>
<thead>
<tr>
<th>Elements</th>
<th>UREA</th>
<th>SUGAR</th>
<th>URIC ACID</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>atom</td>
<td>%</td>
<td>atom</td>
</tr>
<tr>
<td>hydrogen (1.25)</td>
<td>2.5</td>
<td>6.66</td>
<td>1.25</td>
</tr>
<tr>
<td>carbon (7.5)</td>
<td>7.5</td>
<td>19.99</td>
<td>7.5</td>
</tr>
<tr>
<td>oxygen (10)</td>
<td>10</td>
<td>26.66</td>
<td>10</td>
</tr>
<tr>
<td>nitrogen (17.5)</td>
<td>17.5</td>
<td>46.66</td>
<td>-</td>
</tr>
<tr>
<td></td>
<td>37.5</td>
<td>100</td>
<td>18.75</td>
</tr>
</tbody>
</table>

36. Med.Chir.Trans., 8, 541, 1817. There was also a fourth medical conclusion.

The purpose of his investigations was to show the relation between the different constituents of urine and the albumen of the blood. Prout analysed albumen from the blood serum of one of John Elliotson's diabetic patients, and although it could not be assigned a rational composition, the percentage composition was, in agreement with the analysis of Gay Lussac and Thenard, the same as for the albumen of healthy blood. However, the urine of this diabetic had a nitrogen content some 20% less than normal urine. Here the significance of Prout's third generalisation (1817) becomes plain. "Diabetes is a depraved secretion of urea" in the sense that urea was perhaps excreted as two molecules of sugar rather than as urea; such an explanation, of course, made the transformation of nitrogen necessary.

Since the first paper Prout had found many imperfections in his analytical method and he therefore went some way towards justifying Berzelius's criticism. "A more extensive experience in the analysis of organic substances has made me acquainted with various circumstances which I never suspected, and which are of the utmost importance in this most difficult branch of practical chemistry. Even yet I have not surmounted all the difficulties, so as to be able to succeed beyond a doubt in any one given experiment." (38) This was a

38. *Ed.Chir.Trans.*, 9,478,1818. He also noted that the analyses of albumen and oxalic acid gave him atomic proportions which were irreconcilable with Berzelian atomic theory, and appeared to contradict his first conclusion, viz. albumen, $\text{C}_4\text{H}_6\text{O}_6\text{N}$ and oxalic acid, $\text{C}_2\text{H}_2\text{O}_4\text{O}_7$, *ibid.*, p.479. Since he was addressing a medical audience Prout did not enlarge upon this chemical technicality.
serious admission, yet it did not prevent him from tabulating the
corrected analyses and drawing his readers' attention to "the extra-
ordinary relations that exist among the ...numbers." (39)

ANALYSES OF 1818

<table>
<thead>
<tr>
<th>100 parts contain</th>
<th>H</th>
<th>C</th>
<th>O</th>
<th>N</th>
<th>weight of 1 atom</th>
</tr>
</thead>
<tbody>
<tr>
<td>sugar</td>
<td>6.66</td>
<td>40.00</td>
<td>53.33</td>
<td>-</td>
<td>16.75</td>
</tr>
<tr>
<td>urea</td>
<td>6.66</td>
<td>20.00</td>
<td>26.66</td>
<td>46.66</td>
<td>37.50</td>
</tr>
<tr>
<td>lithic acid</td>
<td>2.22</td>
<td>40.00</td>
<td>26.66</td>
<td>31.11</td>
<td>56.25</td>
</tr>
<tr>
<td>(uric acid)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>cystic oxide</td>
<td>5.00</td>
<td>30.00</td>
<td>53.33</td>
<td>11.66</td>
<td>75.00</td>
</tr>
<tr>
<td>(cystine)</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>oxalic acid</td>
<td>4.44</td>
<td>20.00</td>
<td>75.55</td>
<td>-</td>
<td>112.5</td>
</tr>
<tr>
<td>albumen (from blood serum and egg white)</td>
<td>7.77</td>
<td>50.00</td>
<td>26.66</td>
<td>15.55</td>
<td>112.5</td>
</tr>
</tbody>
</table>

It would appear from this table that the molecular weights of sugar, urea, uric acid, cystine (where the presence of sulphur was missed) and oxalic acid formed an arithmetical series, or were related by multiples of the weight of sugar. This relationship appeared even more significant if albumen from blood serum was included in the series. Here was the beginning of the metamorphosis theme of Prout's later molecular and physiological studies, wherein molecules underwent reduction and completion (depolymerisation and polymerisation) to the designs of organic agents. (Chapter 6)

39. Med.Chir.Trans., 9,453-4,1818. Compare the table of 1817 (loc. cit.5,540,1817). The only significant change is uric acid where nitrogen had been over estimated and consequently the amount of carbon dioxide under estimated; the quantity of water was also over estimated. A "want of analogy" in molecular weights had led Prout to repeat the uric acid analysis. cf. Med. Chir.Trans., 9,478,1818.
Prout’s work with urine, for which he designed a special hydrometer for use in clinical practice, (40) led him to the discovery of a substance which Wollaston and he named purpuric acid. This research formed the subject of Prout’s first communication to the Royal Society to whom it was read by Wollaston on 11 June 1818.(41) No doubt his intention was to gain election to the Society, and this event rapidly followed the paper’s publication. (Chapter 1)

The purple colour produced in the reaction between uric and dilute nitric acids had been first described by Scheele, (42) the discoverer of uric acid, in 1776. As we have seen, the reaction had been used to characterise the uric acid of a reptile's faeces by Prout in 1815, and he now ascribed the colour to an ammonium salt of an unknown acid -to be called purpuric acid "from its remarkable property of forming compounds ... of a red or purple colour." (43)

40. "Description of an Instrument for ascertaining the Specific Gravity of the Urine in Diabetes and other Diseases", Ann.Phil., 25(=9), 334-5, 1825. See also, Inquiry, 2nd ed., 1825, p. 318. The sole peculiarity of this "urinometer" was the scale which ran from (100)0 to (10)60 with a few clinical points marked on the verso, e.g. "W (opposite 0 on the scale side) is the point about which the instrument stands in pure water. HS or healthy standard is the mean point about which healthy urine usually ranges. The portion of the scale marked diabetes is that to which the instrument rises in diabetic affections." Ann.Phil., loc.cit., p. 335. A diagram was given. Prout remarked later that it was in general use among practitioners, On Stomach, 5th ed., 1848, p. 160n. "It is now so common and so generally used that the merit of introducing it is entirely forgotten", obituary, Edinb.Med.Surg.J., 76, 161, 1851.

41. "Description of the acid principle prepared from lithio or uric acid", Phil.Trans., 1818, pp. 420-8.


43. Phil.Trans., 1818, p. 421.
The free acid proved to be a fine yellowish powder, although as Prout came to realise with the publication by Liebig and Wöhler of their work on uric acid derivatives, it was not the free acid which was prepared in this manner, but an ammonium compound of a purine base which Liebig named murexid or murexide. In the enlarged editions of his treatise on the urine, Prout followed Liebig and Wöhler's account of these purine derivatives, but since he refused to use chemical formulae, he held many reservations.

44. Uric acid was digested in dilute nitric acid, and the excess acid neutralised with ammonia. After concentrating by evaporation, dark red granular crystals of ammonium purpurate were formed. To obtain the free acid, the purpurate was dissolved in potash and heated until all the colour disappeared. This solution was then neutralised with sulphuric acid, and allowed to crystallise. Analysis with copper oxide gave $\text{H}_4\text{N}_5\text{O}_6$; $\text{C}_{27.22}\%$; $\text{O}_{36.36}\%$; $\text{N}_{31.31}\%$, i.e. $\text{C}_2\text{H}_6\text{N}_2\text{O}_2$. (1815 values for atomic weights). Later Prout discredited this analysis, but never published a revision, Med.Chir.Trans., 9,481,1818.

45. Annalen der Pharmacie, 26,319-38,1838. The names for the derivatives were Wöhler's. Purpuric acid is $\text{C}_6\text{H}_4\text{N}_6\text{O}_5$, whose ammonium decomposition product is murexide, $\text{C}_8\text{H}_6\text{N}_6\text{O}_3$; it is unstable and rapidly hydrolys to form alloxan, $\text{C}_4\text{H}_2\text{N}_0\text{O}_4$; which is acidic in its enol form, and uramil, $\text{C}_4\text{H}_3\text{N}_0\text{O}_4$. Uramil was named murexane by Liebig who incorrectly supposed that it was the same as Prout's purpuric acid.

46. Prout principally objected to Wöhler's choice of names. "The erythric acid is called, by Liebig (sic, Wöhler), alloxan; the crystals formed by dilute nitric acid alloxantin, for what reason I am unable to assign." "I must protest also against the barbarism of the terms ..." allantoin, cyamelid, uramil, etc.; these were all "extraordinary" names. See On Stomach, 5th ed., 1848, pp. 540-1.
that he had not been aware of the existence of the two distinct products "both of which were necessary to the formation of what I termed the purpurate of ammonia" (47); nevertheless, wisely as events proved, Prout retained the term purpuric acid for murexane with the implication that the former was distinct from the latter.

The remainder of Prout's paper was devoted to a description of a variety of inorganic purpurates, and from the brilliance of their colours he was led to speculate that such salts might be the basis of many animal and vegetable colours. If this were the case, then they could prove to be of commercial and artistic value. A small murexide dye industry was developed during the 1850s in Manchester, but it ceased upon the introduction of the aniline dyes. (48)

The inherent complexity of purine chemistry led to a dispute among chemists over the interpretation of Prout's work. Earlier in 1618 the Italian chemist Brugnatelli had described the preparation of an erythric acid by the action of concentrated nitric acid on uric acid. (49) A year later, Vauquelin criticised Prout's Royal Society paper (50) and confessed that even after he had carefully followed Prout's directions, he had been unable to prepare purpuric acid. In fact, Vauquelin probably obtained alloxantin, C₇H₆N₂O₈, but, rather unfairly, he suggested that Prout had really prepared erythric acid and therefore only repeated the work of Brugnatelli.

47. Ibid., p.539.


49.Giornale di Fisica, 1,38,117,1818 (not seen); English version, Phil.Mag., 52,30-47,1818; French (for Vauquelin), Ann.chim.,8,201,1818.
Although this was chronologically possible, it is clear that Prout had not seen the Italian’s paper until it appeared in English in July 1818, by which time he had already discussed his own work at the Royal Society. Prout promptly sprang to his own defence by pointing out that erythric acid had different properties from purpuric acid; perhaps erythric acid was "a quadruple salt, formed by the union of supernitrate and superpurpurate of ammonia (viz. a hydrogen salt); or [it] may be a simple compound of nitric and purpuric acid." (51)

In fact, Brugnatelli’s acid was almost certainly alloxan, \( \text{C}_4\text{H}_2\text{N}_2\text{O}_4 \) which, as Liebig eventually demonstrated, formed murexide when it was reacted with alloxantin in ammonia. In defending his earlier statements on purpuric acid, Prout stated that he had already recognised the presence of three distinct products in the reaction between uric and nitric acids, but he had not had the time to investigate them. Finally, in order to convince Vauquelin of the accuracy

50. Journal de physique, 38,456-9,1819; Prout saw this trans., Quarterly J. of Science, 8,157-9,1819.

of his account of purpuric acid, he sent the Frenchman samples of
purpuric acid, ammonium purpurate, and pure uric acid from the boa
constrictor. Vauquelin's reaction is not recorded, though it appears
remarkable that he made claim to have discovered the preparation of
pure uric acid from a snake's excreta in 1822. (52)

Another new acid (53) was announced by Prout in 1822 when
Alexander Marcet read a paper to the Medical and Chirurgical Society
on "a singular variety of urine which turned black soon after being
discharged." (54) Marcet had obtained the sample from the physician,
William Babington, in December 1814; the patient had been a male
child of 17 months who was clearly suffering from the metabolic dis-
ease, alkaptonuria. Marcet had lost contact with the child, but sam-
ple of its urine had been kept and only recently passed to Prout for
analysis. The latter reported that he could find no urea or uric
acid in this urine, and that the black colour was due to an unknown
principle combined with ammonia which he proposed to call appropri-
tely, melanic acid. As Partington has pointed out, this was homogenous

52. Ann.chim., 21,440-2,1822; Prout attributed Vauquelin's lack of
success to using impure uric acid prepared from human calculi.
Note that the 7th ed. of Turner's Elements, 1842, edited by
Liebig and Gregory, attributed the preparation of pure uric
acid to Vauquelin, p. 759.

53. Prout was less successful in the analysis of a horse's urine in
which he missed the identification of hippuric acid, Ann.Phil.,
16,150-1,1820. He erroneously thought it to contain a large
quantity of urea —so accounting for the ammoniacal odour of
stables. Hippuric acid was identified in horse's urine by
Liebig, Annaalen d.Physik, 17,389,1829. See also, Prout, On
Stomach, 5th ed., 1848, p. 239.

54. Med.Chir.Trans., 12,37-43,1823, read 5 March 1822, Prout pp.43-5,
reported Ann.Phil., 20,71,1822. No quantitative analysis was
given. For full literature see Partington, History, vol. 3,
acid (2,5-dihydroxyphenylacetic acid). The term melanic acid did not pass into chemical literature, and there is no indication that Prout saw any analogy between the acid and the ink of the sepia, melanin, which he had previously examined. (5)

Prout's growing reputation as a physician who specialised in urinary diseases inevitably led him to the chemical analysis of various urinary calculi. In 1819 he discovered ammonium urate in large quantities in a few calculi. The existence of such calculi, although originally demonstrated by Vauquelin and Fourcroy, had been denied by more recent authorities like Brande, Henry and Marcet. (55) When such a stone was extracted by the surgeon Henry Cline from a boy aged two in St. Thomas's Hospital during April 1819, Elliotson passed it to Prout for analysis. Prout's detailed description and identification agreed with Fourcroy's (whom he quoted in French), but he believed the calculus to be very rare. (56) Since he had only met with one other example, again in a young boy, he concluded that such stones were peculiar to children before puberty.

All Prout's early work on urine chemistry and pathology was elaborated into a little book published in 1821 which "established his reputation as a chemist and practical physician". (57) This


57. Munk, Roll., 3,109.
Inquiry into the Nature and Treatment of Gravel, Calculus and other Diseases of the Urinary Organs (58) was probably one of the first nineteenth-century medical textbooks to deal with disease from a chemical viewpoint. (59) An instant clinical success, the merit of this book is well-measured by the fact that it went into French and German translations - a rare honour at a time of continental supremacy in medicine. (60)

The Inquiry opened with a chemical comparison between the constituents of human blood and urine which served to establish a norm from which diseased urine deviated. It also permitted Frout, in a later edition, to draw the significant metabolic conclusion that urea was formed in the blood and subsequently secreted through the kidneys by an organising agent. (61)


60. The translations were: Traité de la gravelle, du calcul vesical et des fonction des organes urinaires, trans.Ch. L. Morgue, Paris, 1822.
Untersuchung über das Wesen und die Behandlung des Harngrisses, Hornsteins und anderen Krankheiten, die mit einer gestorten Thatigkeit der Hornverzeuge zusammenhängen, Weimar, 1823.

61. On Stomach, 5th ed. 1848, p.529, but also in 3rd ed. 1840. This was first demonstrated by Christison, Edinb.Med.Surg.J., 32,262, 1829. Frout says that he had first detected urea in the blood in 1816 but never published the result; he again found it in 1826 in a sample of blood given him by Bright, Bright's Medical Reports, 1,84n,1827. See On Stomach, 5th ed. 1848, p.531n.
<table>
<thead>
<tr>
<th>Blood contains</th>
<th>Urine contains</th>
</tr>
</thead>
<tbody>
<tr>
<td>water</td>
<td>water</td>
</tr>
<tr>
<td>albumen, fibrin</td>
<td>albumen, fibrin</td>
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<tr>
<td>red particles</td>
<td>red particles</td>
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<tr>
<td></td>
<td>urea</td>
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<tr>
<td></td>
<td>lithic acid</td>
</tr>
<tr>
<td></td>
<td>(uric acid)</td>
</tr>
<tr>
<td>laetic acid and</td>
<td>lactic acid, etc.</td>
</tr>
<tr>
<td>accompanying animal matters</td>
<td></td>
</tr>
<tr>
<td></td>
<td>mucus of bladder</td>
</tr>
<tr>
<td></td>
<td>pus</td>
</tr>
<tr>
<td></td>
<td>sulphur, phosphorus,</td>
</tr>
<tr>
<td></td>
<td>muriatic acid</td>
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<tr>
<td></td>
<td>fluorine?</td>
</tr>
<tr>
<td>potash, soda, lime, magnesia, silex?</td>
<td>potash, soda, lime, ammonia</td>
</tr>
<tr>
<td></td>
<td>magnesia, silex?</td>
</tr>
</tbody>
</table>

62. Inquiry, 1821, p.6, the 3rd. and 4th eds. have a more elaborate table which includes the comparison with bile. Not in 5th ed.
Although from this table we can see that Prout had noticed the presence of albumen in diseased urine, until Richard Bright correlated the presence of albumen in urine with dropsy and renal diseases in 1627, it was not thought that albuminuria had anything to do with the kidneys. (63) Prout, therefore, did not offer his readers any particular cause for albuminuria, or adopt any useful treatment. (64) However, from the second edition onwards, Prout always distinguished between chylous and serous albuminous urine; the former kind of diseased urine contained an albumen which resembled the incipient albumen of chyle (chyluria), and the latter an albumen identical with that found in the blood serum (albuminuria). "Distinctly defined instances of both these varieties" were extremely rare and more usually the urine was found to contain albuminous matters of both a chylous and serous character. In the final edition of his urine textbook Prout renamed the former chylo-serous urine and differentiated it from the serous urine familiar from the work of Bright. (65)

In fact both the symptoms of chyluria and albuminuria are well-defined, and while the latter frequently occurs independently of the former, chyluria (which is usually caused by a lesion in the

64. Inquiry, 1821, p. 42.
65. On Stomach, 5th ed., 1048, p.111. "The albumen in the urine has been distinguished by Dr. Prout as serous and chylous; if the latter be ever observed, it should be described", Seventh Annual Report of the Registrar General of Births, Deaths and Marriage in England for 1843, 1844, London, 1846, p. 264, Statistical Nosology, Urinary Organs, Disease 95, Granular Disease of Kidneys.
lymphatic system which allows fats to pass into the urine) is not
dependent on kidney lesions. But albumen will almost certainly pass
along with fats into the urine in chyluria so that Prout's term
chylitic-serous urine is quite appropriate. (66) Prout correctly
noticed the intermittent character of chyluria; that it was more
common in the tropics than in Europe; and that it was not necessarily
connected with an organic lesion of the kidneys. (67) The parasitic
nature of chyluria, and its connection with the lymphatic system was
only established at the end of the nineteenth-century.

In his discussion of the diseases of urine, Prout adopted a
classification based upon the solubility of the chemical substances
found in diseased urine. Soluble principles such as urea, sugar
and albumen could be morbidly deranged in quality and quantity;
insoluble principles led to the appearance of gravels and calculi.
In order to aid diagnosis Prout published a routine for testing the
urine which could be easily performed by the general practitioner
by the patient's bedside. The advantages and importance of such a
procedure need hardly be stressed. Among these tests was one for
detecting excess urea - a symptom of chronic nephritis.

The mode which I commonly use to detect an excess, is to put
a little of the urine into a watch glass, and add to it carefully
nearly an equal quantity of pure nitric acid, in such a manner

66. H.B. Jones, Phil.Trans., 1850, pp. 651-66; J.L. Thudichum,
67. Prout, Gulstonian lectures, Medical Gazette, 5, 389, 1831;
that the acid shall subside to the lower part of the glass, from its greater specific gravity, and allow the urine to float above it. If spontaneous crystallization takes place (urea nitrate), an excess of urea is indicated; and the difference of excess can be inferred, near enough for practical purposes, by the greater or less time which elapses before crystallization takes place, which time may vary from a few minutes to two or three hours." (66)

Detailed "practical rules for determining the nature of the Affection and its appropriate Remedies, from the properties of the Urine, and other symptoms" (69) were published in the second edition of the treatise in 1625. In order to detect variation there had to be a standard technique for the examination of a patient's urine;

I prefer a transparent cylindrical vessel, such as a common phial, of not less than one inch, nor more than two inches in diameter, and from six to eight inches long. In such a vessel all the sensible properties, both of the urine and its deposits, can be distinctly ascertained. (70)

Prout listed the necessary apparatus as follows: red and blue litmus paper; turmeric paper; watch glass or thin platinum vessel of the same shape (for evaporation or detection of excess urea); two small discs of plate glass for differentiating pus from mucus by Thomas Young's optical test; specific gravity bottle or portable hydrometer; blowpipe and forceps for tests on gravel and calculi;

70. Inquiry, 2nd ed., 1825, p. 266.
test tubes; stoppered phials containing solutions of reagents such as ammonia, potash and nitric acid. All these materials "can be readily packed into a small portable case, or pocket book, and will be sufficient, by the aid of a common taper, or candle, to perform all the experiments on the urine, and urinary productions, that are commonly necessary." (71)

Urine examined first thing in the morning would chiefly determine the kidney's action; urine examined after a meal might reveal digestive derangements. The physician was required to note fluctuations in the amount of urine excreted, its colour or transparency, its specific gravity (the average range was 1.01 to 1.015) and its acidity and alkalinity. For acidity, Prout used ordinary litmus and a delicate chemical test for detection of uric acid.

A very delicate test for the presence of a free acid in the urine, is the precipitation of the lithic acid from it in the solid state, and the quantity of free acid present may be commonly judged of pretty nearly from the time required to produce this effect, and the quantity of lithic acid precipitated." (72)

It is interesting to notice that he did not recommend a murexide test here. The presence of urinary sediments, such as the urates and alkaline earth phosphates could be detected by the colour of the urine (yellow, pink and red), and the first edition of the treatise carried a hand-painted endpiece designed, somewhat unsuccessfully, to show the typical sedimentation colours of uric acid diathesis.

71. Ibid., p.318, not in later editions, presumably because this had passed into general practice by 1840.
Finally, Prout also appears to have examined urinary deposits under the microscope from about 1820 onwards; however, he only recommended the general adoption of microscopy in 1843 after the appearance of the achromatic microscope. (73)

Urine analysis has become an extremely important and simple part of medical diagnosis; to a very large extent it was Prout who brought back uroscopy into general medicine upon a strictly scientific basis, and stripped of its medieval mysticism.

I cannot too strongly impress upon my readers the necessity of frequently examining the state of the urine. Those who wish to know anything respecting the deranged operation of the urinary organs must submit to this drudgery, or be content to remain ignorant. ... Patients should be also directed to make general observations upon this subject themselves. In particular, they should be directed to keep two or three large wine or ale glasses in their bedroom, and observe the state of the urine at different times of the day, especially in the morning and evening, and likewise to note the changes which it undergoes by standing for some time. (74)

73. On Stomach, 4th ed., 1843 and 5th ed., 1848, "Description of the Plates". "I was well acquainted with the late Dr. Goring, the early and great improver of the microscope among us, who first drew my attention to this instrument nearly thirty years ago [c. 1820]. I possessed one of the best of the old construction that could be procured, and was in the habit of constantly applying it to urinary sediments and other matters long before it was generally employed for such purposes in this country. Hence almost all the microscopic phenomena of urinary sediments now so familiar, were well known to me. The instrument I employed, however, was far inferior to those now in use, and hence, ... it very imperfectly or altogether failed to show many phenomena of importance", 5th ed., 1848, p. 63.

C.R. Goring (1792-1840) was the author of, Microscopic Illustrations, 1829; Microscopic Cabinet, 1832. In both of these he collaborated with the optician, Andrew Pritchard (1804-1882). See the respective notices of these men in Dictionary of National Biography.

in the medical section of the previously discussed paper on the constituents of urine read to the Medico-Chirurgical Society in 1817 (20), Prout had mentioned that he had practised uroscopy for several years before meeting Dr. Charles Scudamore "who I found entertained similar views and had prosecuted the subject much further than I had done." (75) Scudamore gave Prout morbid urine samples from his own patients, and the free discussion with an older expert must have been invaluable to Prout. Unlike Scudamore and Marcret, Prout felt unable to suggest any chemical remedy for the stone; the only satisfactory solution was the knife, for "when a calculus is once formed, a further enlargement is probably a common chemical process, and will proceed whether the urine be healthy or not, for all the urine naturally contains the ingredients most commonly met with in calculi." (76) Of chemical remedies he was completely sceptical, both because of their potentially dangerous side-effects, and because "the object of the chemical practitioner is at best but of a secondary description, namely to prevent the effects of diseases rather than to remove it [sic]." (74)

Such comments are still applicable today for, unfortunately, the detailed causes of stone formation remain unknown and the only cure is still the surgeon's knife.

76. Ibid., p.549.
An Appendix to the Inquiry included some statistics for the mortality rates during the operation for the stone. These were based on the published reports of the Norwich and Norfolk hospitals, and for the region of Bristol. Alexander Marret had given the fatalities below the age of puberty for the year 1816 as 1 in 18, and for adults 1 in 42. From this Prout deduced that the mortality for the whole Norwich group was 1 in 113/8, that is, he took the mean value between the two proportions (224). Combined with the Norfolk and Bristol statistics this led him to a figure for the national mortality rate of 1 in 74. However, this figure seemed too low to John Yelloly, a founder of the Medico-Chirurgical Society and a Physician attached to the Norwich hospital. (77) Yelloly accused Prout of false reasoning: Prout had arrived at the proportion 1 in 113/8 for the Norwich group on the concealed assumption that the total numbers of dead children and adults were the same. Since this was not the case, a true mean could only be obtained by dividing the total number of lithotomies (506) by the number of fatalities (70). In this way Yelloly deduced a figure of 1 in 74 for Norwich, and hence a national figure of 1 in 62. Prout immediately rejected this criticism (78) and suggested that he had deliberately chosen a Norwich mean which fitted the facts suggested by the "present improved state of surgery". The only data related to the year 1816, so that Yelloly's ratio of 1 in 62 "seems when all the circumstances are duly considered, to

exceed the truth " Prout’s figure of 1 in 74, admittedly deduced from less rigorous principles, happened to coincide more closely with surgical facts. In any case, Prout pointed out, the data were misleading because London, where two-fifths of the total lithotomies in Great Britain were performed, was excluded from the statistics.

Our only interest in this unimportant controversy is to notice that it would have been prevented if Prout had properly explained his analysis of the statistics in the first instance. Terseness was a perennial fault with Prout and it frequently gave rise to misunderstanding.

Prout’s determination "to confine his attention chiefly to practical points" (79) remained paramount in all five editions of his book. He furthermore confined himself to "illustrations" and left the establishment of his conclusions to medical colleagues. Since his object was the truth, he wrote, "whoever will direct him to this object, where he has failed to reach it, will be esteemed a friend". A second edition appeared in 1825 (80) and this included a fine pull-out sheet of coloured engravings to illustrate the known types of urinary calculi, (81) and new material on the more important diseases of the kidneys and bladder. (82)

79. Inquiry, 1821, Preface.
81. These engravings by (Francis?) Lunn were made from material in the Museum of the Royal College of Surgeons; apparently they could be separately purchased as A Synoptical View of Urinary Calculi, undated (1825?), 9 inches by 12 inches, copy Royal College of Physicians. These illustrations were retained in later editions.
For the sake of completeness we shall mention here that in 1840 Prout published a third edition of his treatise under the title, *On the Nature and Treatment of Stomach and Urinary Diseases* (83). Although it is termed a new edition of the earlier *Inquiry*, it would be more correctly described as a new book. Munk thought that its publication marked an era in the history of animal chemistry, while Charles Daubeny noted that Prout had "abstained as much as possible from ... speculations, and [had] evinced an exemplary caution in confining his practical deductions strictly within their legitimate limits, at the same time that he [had] displayed a profound sagacity in the discrimination and treatment of the diseases which fell within his province." (84) However, more critically, from the point of view of the historian of science, the book's interest is diminished by Prout's practical bias, his lack of theoretical discussion, and controversy. Indeed, with the two further editions which followed in 1843 and 1848, (85) even contemporary reviewers criticised Prout for not examining and explaining some of the theoretical issues involved.

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82. This material was not claimed as original, and was principally drawn from the works of S.T. von Soemmering.

83. London, 1840, 483 pp., Preface dated September 1840; an extra set of plates drawn by Prout showed urinary deposits viewed through a compound microscope. In the 4th ed., 1843, several of these plates were taken from P.F.O. Rayer, *Traité des maladies des reins*, 3 vols., Paris, 1839-41, which contained many references to Prout's work.


in physiology. (86) However, we should not forget that it was Prout's deliberate intention to avoid controversy, and that such a practical emphasis may well have been welcomed by the contemporary general practitioner. Prout paid little attention to his critics and he made it clear that the only criticism he would listen to would have to come from "the experienced chemical pathologist." (87)

Today the most interesting and important section of the book is the introductory first part which contains a succinct and original non-Liebigian account of the physiology of the digestive and urinary systems; much of it, as we shall see, a specialised version of material previously presented by Prout in the third book of his Bridgewater Treatise. Yet, in the two remaining editions, Prout very unwisely placed this biochemical introduction at the end of the volume as a third book, as if to still further emphasise the work's practical nature. (88) It was, however, impossible to understand the first two books, which employed his special vocabulary and classification, without referring to this section. It was as if Liebig were to have published his Animal Chemistry with all the speculative chemical equations at the end of his book.

In this chapter we have been concerned to review some of Prout's analytical results in the field of urine chemistry between the years 1814 and 1840 which followed from his efforts to discover an accurate

86. E.g. British & Foreign Medical Review, 11, 336, 1841; see also infra, Chapter 8.


method of organic analysis. In this field he made a number of
significant discoveries for both biochemistry and organic chemistry:
the discovery of ammonium urate in the urine of the boa constrictor
eventually led to the generalisation that birds and reptiles differed
from mammals in that the end product of protein metabolism was uric
acid and not urea; the discovery of purpuric acid might be said to
have laid the foundations for the investigation of purine and its
derivatives, and on a personal level it led directly to Prout's
election to the Royal Society. This work was never divorced from
his chosen career as a London physician and it led him to rehabilitate
uroscopy as a diagnostic technique. Despite his obsession with
chemistry, Prout was always sober enough not to believe that urinary
diseases could be cured by chemotherapy alone. Instead of chemical,
and perhaps harmful remedies, Prout approached the bedside as an
Hippocratic physician and emphasised the pathological importance of
urine in diagnosis by recommending simple chemical procedures which
could be used by any practitioner for analysing the urine. His ex-
perience with the urine was presented concisely and systematically in
book form in 1821, and this gave medical practitioners a complete
guide to the whole subject and made the author the first authority in
London on diseases of the urine. As we shall discover in the next
chapter, while he worked on urine chemistry he was at the same time
investigating the chemistry of digestion and assimilation. In 1840,
Prout welded together these two fields of research into a massive
textbook which served as the guide to normal and pathological metabol-
ism until it was replaced by the more powerful insights and resources
However, Prout was not concerned throughout his scientific life with the mere accumulation of chemical facts; always he sought for the laws which governed "not only the operations of the animal economy, but the whole material world." The Daltonian atomic theory, as modified by Berzelius, seemed to promise to rationalise the complexities of organic chemistry, and already in the papers of 1817 and 1818 it can be seen that Prout not only felt that synthesis was possible, but that the ultimate explanation of physiological chemistry might be a molecular theory of transmutation. This theory will be examined in the final chapter when it will be shown to have been the source of all Prout's chemical work. With this in mind we turn to consider the other principal subject of his researches, the physiology of digestion.
Chapter Four: Physiology

1. Sensation

Among the Prout manuscripts which became available to me during the preparation of this thesis there is a clumsily-written but important undergraduate essay which not only provided Prout some material for his first published paper, but which also throws interesting new light on the origins of "Prout's Hypothesis".

*De Facultate Sentienti* is a quarto manuscript of 26 pages, dated Edinburgh 1610, in which Prout's concern is "to deliver my own opinion only, respecting the curious and interesting subject of sensation, without paying much attention to what has been said by others on it." (1) From the manner in which he addresses "my professors" in this essay, it would appear to have been a memoir written to satisfy the demands of the medical course. It has nothing to do with the dissertation submitted for the degree of M.D. in the following year, and the best modern analogy would be the undergraduate "project essay" which is set in many of our university departments. (2)

1. *De Facultate Sentienti*, Edinburgi, 1610, unsigned but in Prout's handwriting. The essay is in English apart from the title-page which is laid out like a dissertation. The pages are sewn together with binder's thread, and the verses used for the side-notes and emendations. Since the pagination does not commence until the third sheet I have assigned these the values i to iii. For a transcription, see infra, Appendix 6.

2. The coda runs "I shall now therefore put a period to it, sincerely hoping that upon the present occasion it will meet with the approbation of my Professors", p.23. I am grateful to Mr. S.M. Simpson, Department of Manuscripts, Edinburgh University Library, for
An abstract of this metaphysical and physiological essay will now be given.

With respect to the subject I am aware that it is purely metaphysical and for that I should offer an apology were I not convinced, that my professors will pardon me when they consider how closely metaphysics & physiology are connected. (3)

Prout began by saying that the nature of the vital principle which organizes vegetable and animal principles is still an inscrutable mystery since there is nothing else in nature with which we can compare it, either in quality or degree. Therefore "we are obliged to conclude that it is a principle sui generis or first principle" and define it indirectly from its observed effects. (4)

Prout never departed from this opinion.

The living principle is that principle which when combined with matter has apparently the power of imparting to it one or more of the three following properties viz vegetation, sensation, and capacity for knowledge, according to the different modes or degrees in which it combines with it. (5)

2. /continued...making a search for a record of this essay on my behalf. Arising from his negative search it occurred to me that the essay might have been a prize-essay submitted to an Edinburgh medical society. However, neither the Royal Medical Society, nor the Harveyan Society offered prize compositions on Sensation during the period 1808-1812. See Edinburgh Medical and Surgical Journal, vols.4, 1808 to 7, 1811. The only possible conclusion is that Prout's essay is a departmental composition or compulsory class exercise, several examples of which exist in Edinburgh University library for earlier and later periods than 1808-1811.

3. Sentlendi, p.ii. The relevance to Prout's hypothesis will be examined in chapter 7.

4. Ibid., p.ii. For Prout's vitalism see infra, chapter 6.

5. Sentlendi, p.ii.
These three properties, or categories, correspond to the three Aristotelian souls, vegetable, animal and intellectual, which exist together in man alone, and Prout terms their respective combinations with matter, vegetative, sensitive and intellectual combinations.

We may note in passing that this classification is the metaphysical foundation for Prout's nutritive hierarchy. (Chapter 8) The function of the vegetative combination is never mentioned in the essay, and we can only suppose that it was introduced in order to square with traditional physiology; otherwise Prout could find no analogical use for it. The intellectual combination, which is only found in man, is especially important since it has the power of acquiring knowledge "through the medium of a property of the sensitive combination termed sensation." (6)

What is the nature and subject of this knowledge, and how is it acquired by the intellectual combination? Prout suggests that the capacity of the intellectual combination for acquiring knowledge is compounded from three Lockean properties of this combination: perception, memory and reason. If sensation is preliminarily defined as the "property of the sensitive combination which is called into action when matter is brought into contact with any part of the sensitive combination or body of a sentient being" (6), then perception may be defined as "that faculty of the intellectual combination which furnishes the basis ...or the materials through the medium of sensation with which memory & reason operate in the formation of knowledge." Memory therefore becomes the faculty of

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the intellectual combination which recalls without perception what
it has previously conceived; and reason another faculty through
which the intellectual combination can select and apply perceptions
which have been retained by the memory.

But what is matter? that is, what is our knowledge of matter?
Prout takes his definition of matter from the Philosophical
Arrangements, 1775, of the 18-century classicist, James Harris. (7)

Matter is that elementary constituent in composite substances
which appertains in common to them all without distinguishing
them from one another. (8)

By this definition all matter is of one substance, the primary
matter which had been distinguished by early Greek philosophers as

or "matter which has a capacity for becoming many things before it
actually becomes any of them." (9) Prout suggests there is an
analogy between the first matter and the uncombined living principle,
and a further analogy between the secondary matter and the first
combination of the vital principle with matter (the intellectual
combination) since if perception could take place any way other

7. James Harris (1709-1780), classicist, grammarian and pedant. He
moved in the circle of Samuel Johnson who respected his scholar-
ship but thought him a "coxcomb" and "a prig and a bad prig".
Leslie Stephens stated that "Harris's books are dry and technical,
but have a certain interest from his adherence to the Aristotelian
philosophy during the period of Locke's supremacy," Dict.Nat.Biog.,
London 1891, vol.25, pp.7-8. See also, Boswell, Life of Johnson,
passim. There is a collected edition of Harris's works by his son,
Lord Malmesbury, 2 vols., London, 1801, but I have used a copy of
Phil.Arr. in London University Library.


9. Sentienai, p.3 ; Harris, p.72 foot (x).
than through sensation, this is the only condition of matter which could be perceived. A third condition of matter is the physical matter which is endowed with the primary qualities of extension and hardness. Physical matter is analogous to, and adapted to, the sensitive combination, and it induces sensation. Prout emphasises that none of these kinds of matter has a real existence in nature; that is, at the empirical or chemical level, none of these categories can be isolated. We can only arrive at a knowledge of these categories by processes of abstraction and analogy.

There does exist in nature, however, an aggregated matter which enables the intellectual combination, through its faculties of reason and memory, to deduce a knowledge of such primary conditions of matter. In fact, Prout's whole purpose in making these distinctions "is merely to endeavour to render probable by their means the unity of matter." (10)

The \( \nu \nu \) or secondary matter, is then redefined by Prout as "matter in its aggregate state", by which he means such empirical materials as wood, stone, water, air and, presumably, the chemists' elements. Secondary aggregated matter may exist in five phases or physical states which depend upon the state of aggregation: solid, liquid, aeriform (gas), aetheriform and luciferous. This semi-empirical classification now becomes the basis for much analogical reasoning.

10. Sentieendi, p. 3, my stress. For the bearing on Prout's Hypothesis, see infra, chapter 7.
How does the secondary aggregated matter differ from the final form of primary physical matter? Prout argues somewhat incoherently that the sole difference lies in a sensible secondary quality called roughness which is added to the primary sensible qualities of extension and hardness. Perhaps the difficulty in following Prout in this step arises from the use of the terms primary and secondary qualities in the context of Aristotelian physics. However, in this he was merely following Harris who, although an Aristotelian, had briefly mentioned the Lockean language of primary and secondary qualities. (11) Prout seems to have conceived that extension and hardness were perceptual qualities of matter itself (i.e. primary qualities); while roughness was a secondary quality whose perception depended on the state of aggregation of matter. (12) Only after this preliminary metaphysical analysis is Prout able to discuss the manner in which the intellectual combination determines the existence of matter by means of sensation.

Matter excites sensation in the sensitive combination through the primary, but relative, qualities of extension and hardness. When a finger is drawn along the edge of a table a series of perceptions is received through sensation, and the faculties of

11. Harris, p. 88.

12. "As for Roughness, Smoothness, Hardness, Softness, tho' they may be said perhaps to penetrate farther than the surface, yet are they, to man's sensation at least, so many qualities superficial", Harris, p. 171.
memory and reason enable comparisons to be drawn between these sensations and other previously experienced sensations so that it becomes known that matter exists "at every point between the angles of the table and hence that these two angles could not be the same angle." (13) Without the aid of memory and reason, perception alone could not lead us to a knowledge of the extension or of the hardness of the table. (14) But without sensation there would be no knowledge at all.

When matter makes contact with any part of the sensitive combination of a sentient being, a sensation will be experienced by it, and perception in the intellectual combination will follow "though in different degrees of intensity &c. according to the acquisiteness [sic] &c. of the organization of the part in which contact takes place". (15) The succession of contact followed by perception, is extremely rapid, and the process is easily, but erroneously, believed to be one of cause and effect.

the philosopher however is aware of the contrary. reason tells him that these must necessarily be only the two extremes of perhaps a long series of cause & effect of the nature of which he is extremely ignorant but which are as yet absolutely wanting to connect the two extremes." (15)

Future research might uncover some of these links in the chain.


14. Compare: "We never experience a pure sensation, for all our thoughts and feelings are coloured by previous experience and by associations which come involuntarily to mind", R.W. Moncrieff, The Chemical Senses, London, 1951, p.75.

Prout's opinions on the mechanism of sensation are advanced in the form of two propositions.

1. Since sensation is an effect (or property) produced by the combination of the living principle with matter, and since the cause of the effect is simply matter which is really only of one kind, it follows by analogy "that sensation is but of one kind also." Furthermore, since there are different kinds of secondary aggregated matter, sensation forms "the basis or substratum of all those varieties of it which apparently take place when different conditions &c of matter are brought in contact with different parts of the bodies of sentient beings." In fact there are five conditions of matter; therefore, by analogy, there will be five kinds or modifications of sensation: touch, taste, smell, hearing, and vision.

2. Matter in its aggregate solid form is made up of secondary aggregated particles which are formed from the primary physical particles (extension and hardness) combined with the secondary sensible quality of roughness. But roughness is distinguished by the modification of sensation, touch, and hence (by analogy) all other forms of sensation and their variations in individual intensity will also depend upon roughness. Now the degree of roughness is dependent upon the sizes of the aggregated particles of secondary matter, and therefore Prout concludes, tastes, smells, sounds and colours all ultimately depend "upon the different sizes, &c. of the aggregated particles of the same matter." (16)

It will be noticed that this was also the conclusion of traditional corpuscular philosophy.

Some parts of the body like the lips, glans penis, and the finger-tips are more sensitive than others, while other parts are especially contrived so that "the contact of external aggregated matter ... is effected through the media of certain mechanical apparatus." (17) These apparatuses, the tongue, ear, eye and nose, are so special that the sensations produced are dramatically modified "and appear altogether different from sensation in its usual form" as experienced through the skin or the lips. For example, the eye is adapted to the subtle form of aggregated matter called light which, by contact with the bare optic nerve, "produces that modification of sensation termed seeing." (18) It will be noticed that Prout does not sufficiently differentiate between general sensory perceptors, like the mucous membranes, which are susceptible to chemical irritation, and the more sophisticated localized receptors which he tabulates as follows: (19)

18. Ibid., p.10.
19. Ibid., p.11. For the difference between chemical sensibility and smell and taste, see Moncrieff, op.cit., ref.14, p.172.
condition of matter     part of body affected     senses     sensation
aggregated solid       fingertips                    touch     feeling
liquids                 tongue                        taste     savour
seriform                nose                          smell     odour
atheriform              ears                          hearing   sound
luciform                eyes                          vision     colour

Prout makes seven comments on this table.

1. Analogy leads to the following conclusion. Since physical matter
   if it could exist alone could produce sensations and perceptions
   through its sensible qualities of extension and hardness, "these
   must necessarily be composite... that is to say they must consist
   of sensation and perception of matter combined with certain
   qualities corresponding to those which exist in conjunction with
   the matter itself & these qualities of sensation & perception I
   shall term quantity & intensity supposing the first to correspond
   with extension & the second with hardness, &c." This was badly
   expressed: "these" must refer to sensations and perceptions.

2. Roughness - if the secondary particles of aggregated matter are
   all equal in size, the sensation of roughness is uniform; if they
   are different in size, the roughness is irregular. Furthermore,
   "this modification of sensation like all others is varied in two
   ways: by the original sensible qualities of physical matter, viz.
   in quantity & intensity ... the quantity of the modified sensation
   corresponds with the quantities of surface acting at the same
   time in its production, while its intensity corresponds with the

20. Sentendi, p.10 verso.
degree of roughness (whether greater or less) of the superficies of the same matter acting at the same time in producing it." (21)
The modern analysis of touch is more complex for we now recognise three receptors, touch, pain and temperature.

3. Gustation is only excited by matter in the liquid state in which the cohesion of the secondary particles is much less than in the solid state. Taste is appreciated by the tongue and "varies in quantity & intensity ... corresponding to the quantity of matter acting at the same time, the latter to the different tastes."

4. Olfaction is produced by the action of the aeriform aggregation of matter on the nose. In this state the particles are too small to be appreciated by either the body or the tongue as touch or taste. Smell is also varied in quantity and intensity corresponding to the quantity of matter acting at the same time, and to different odours.

5. Hearing is only excited in the special organs, the ears, by a very subtle aggregated matter which may or may not be the Aether of the physicists. (22) Hearing varies in quantity and intensity, but these are more usually termed loudness or acuteness (sic) and gravity.

6. Vision is only excited in the eyes by the most subtle form of all aggregated matter, light. "Vision varies in quantity & intensity the latter answering to the variety of colors the former to their brightness." (23)

22. Ibid., p.13.
7. In a note Prout suggests the probability that the nerves which serve the various sensitive instruments, the tongue, nose, ear, etc., are also specially adapted to respond only to a specific state of matter, "but if we may judge from the analogy of the unity &c of the acting matter this adaptation must consist only in different degrees of fineness &c of organization in them." (24)

Prout's concern is obviously to reduce all sensory mechanisms to a unity, and in an explicit recapitulation he states:

In order to set forth the (speculation - del) probability of the unity of matter as distinctly as possible we have begun by endeavouring to trace its identity through all its primary conditions from the of the ancients down to its physical state, or that condition of it in which it becomes the object of sensation from its being vested with sensible qualities. Respecting this nature of the unity of matter, I may here observe that it was adapted by most of the ancient philosophers and by many of the moderns among the latter of whom was I believe the immortal Newton himself. the speculations still are by many accounts visionary, but when we reflect upon the astonishing discoveries that have been made in chemistry and the progress it is still making, who will say that at some future day they will not be realized at least with matter in its secondary or aggregated condition. (25)

Although the programme of chemical reduction was to be placed upon an experimental basis by Prout a few years after he wrote these words, his programme to reduce the number of sensations has never been realised. In searching for analogies Prout overlooked significant differences, and he often fell into the simplest pitfalls of analogical reasoning. Nowadays it is usual to recognise that the

"five senses" have many differences and distinctions. Indeed, some physiologists advocate as many as twenty to thirty distinct sensations and classify them on the basis of stimulus, sensation, sensory mechanism and type of response. Of course, where the similarities in sensory mechanism override the differences - namely in the cases of taste and smell - Prout's reduction holds good. However, the investigation of these chemical senses has been by experimentation and not solely by speculative analogical thought.

In the present context Prout is not concerned with speculations about the decomposition of the elements, so, dismissing them, he returns to his immediate subject which is to justify the assumption that sensation is really only of one kind. He repeats that touch, taste, smell, hearing, and vision are only modifications of the same original sensation which are caused by "differences of subtlety &c in the aggregated particles of the same secondary or aggregate condition of matter, which are enabled to act on the sensitive combination by means only of certain apparatus adapted to the mechanical properties of those varieties of aggregated matter." (27)

Various analogies between the different kinds of sensation are next considered. Although roughness and taste may seem to be very different, they are analogous in the following respects: both may vary in intensity and quality within certain fixed (though admittedly undetermined) limits. "Perhaps", Prout suggests in a

26. Moncrieff, Chemical Senses, p. 54.

27. Sentiments, p. 16.
fit of Pythagorean enthusiasm, "the varieties of all the modifications of sensation are caused by varieties in the particles of matter producing them whose ratios with respect to each other are contained within 1 and $\frac{1}{2}$ only, that is to say that they are the extremes of the ratios of the particles of matter producing such varieties." (28) The mathematical reasoning, which is not carefully explained, is taken from the analogy of sound and it leads Prout to the suggestion that there may be at least three "primitive tastes" which correspond to the three principal sounds (the octave or fundamental, the third, and the fifth), and the three principal colours, red, yellow and blue. The few other experienced tastes might then be supposed to be caused by particles whose ratios with respect to each other were intermediate between 1 : 1, 4 : 5 and 2 : 3. (29) Wisely, Prout does not attempt to firmly identify the three principal tastes beyond the suggestion that they may be acid, alkaline and bitter. It is intriguing to notice that modern physiologists agree with Prout, but on experimental grounds, that most tastes are blendings of a few primary tastes of which the four usually recognised are sweet, sour, salt and bitter. (30)


29. Prout's attempt to invent ratios of intensity in some degree anticipates Weber's law that the change in stimulus required to produce a noticeable difference in sensation is proportional to the stimulus, $\frac{\Delta S}{S} = \text{constant}$, where $S$ is the stimulus, and $\Delta S$ is the change in stimulus.

Similar analogies could be worked out between tasting and smelling.

Prout adopts the view that sound is conducted by vibrations of an aether, not the air, and that hearing is produced by the striking of successive aetherial waves against the auditory nerves. The sensation of hearing varies with the size, shape and nature of the bodies which produce the sounds; an irregular rough surface produces irregular rough sounds because the aggregate particles of aether vary in size from the very large to the very small. If all the particles are of one size, then the sensation produced will be smooth and uniform, and vary only in gravity (i.e. pitch). By the analogy of light and the musical octave, Prout is led to postulate the existence of seven primitive sounds.

When one undula is performed in the same time as any two others the octave is produced by such two undulae. Now the times of these undulae are as the sizes, &c of their causes and hence there are seven points between the two extremes of the octave including the first which are more marked than the rest on account of the simplicity of their ratios &c and which thus constitute the seven primitive sounds gradually rising above each other in intensity of which the principle, third and fifth are the chief. (31)

Loudness is dependent on the number of vibrations which act on the ear in the same time, and on the force with which they strike the ear in the auditory nerve. (32)

32. "As the undulations causing sound move according to the law of a pendulum in the arch of a cycloid (vide Newton Princip. Lib. 2. Prop. 47 &c) those causing the same sound must necessarily be completed in the same time be the space what it will passed over by them. hence the greater the space the greater will be the momentum with which they fall upon the ear," ibid., p.19. See Newton, Principia, Cajori ed., 1934, p.375.
Following an analogy drawn by Newton in his *Opticks,* it seemed likely that the seven primitive sounds corresponded with the seven primitive colours. Newton had described how he had measured the band widths of the spectral colours and found that if the base of the spectrum, $GM$, was extended to $X$, so that $GM = MX = \frac{1}{2}$, then,

$$\frac{G\lambda}{GX} = \frac{8}{9} \quad \text{(violet)}, \quad \frac{G\nu}{GX} = \frac{5}{6} \quad \text{(indigo)}, \quad \frac{G\sigma}{GX} = \frac{3}{4} \quad \text{(blue)},$$

$$\frac{G\varepsilon}{GX} = \frac{2}{3} \quad \text{(green)}, \quad \frac{G\chi}{GX} = \frac{3}{5} \quad \text{(yellow)}, \quad \frac{G\iota}{GX} = \frac{9}{16} \quad \text{(orange)},$$

$$\frac{GM}{GX} = \frac{1}{2}.$$

Accordingly, the boundaries of the different colours were "in proportion to one another, as the numbers, 1, 8, 5, 3, 2, 3, 9, 1, and so ... represent the Chords of the Key, and of a Tone, a third

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Minor, a fourth, a fifth, a sixth Major, a seventh and an eighth above that Key." (34) Newton had been particularly struck by this analogy between the spectrum and the musical scale since he mentioned it on several occasions. Prout seems to have read Newton's clearer account of the analogy in the posthumously published *Lectiones Opticae* as he referred to the fact, not mentioned in the *Opticks*, that the breadths of the orange and indigo bands were approximately equal to only a half of the other colours. (35) In his Optical Lectures Newton had suggested that indigo and orange were analogous to the semitones, and the other five colours to the tones, of a musical scale. As Houstoun (36) and others have pointed out, the resultant symmetrical scale is the Dorian mode on D, DEFG ABOD.

Prout was captivated by this analogy and suggested that such a scale might be used by composers with advantage. "Indeed, if my memory does not deceive me I have read of strains having been occasionally introduced into their composition in this mode by some


- Music scale frequencies: 360, 320, 300, 270, 240, 216, 202\(\frac{2}{3}\), 180
- Light measures: 360, 321, 303, 270, 240, 214, 202, 180

See also, *Newton's Correspondence*, vol. 1, Cambridge, 1959, pp. 376-7, letter to Oldenburg, 7 December 1675.

celebrated masters with very fine effect." (37) Red, yellow and blue were the colours which stood out in the spectrum and they corresponded with the fundamental, third and fifth of the octave. The sensation of colour arose from the mixture of these three primary colours. (38) In view of the recent revival of interest in blind people who claim to distinguish colour by touch, it is notable that Prout quotes this phenomenon as a possibility since colour, like touch, is dependent on the state of the superficies of aggregated matter.

In fact, of course, Newton and Prout were completely deceived. There is no correlation between the light spectrum and musical scales, and Newton was misled by the character of his glass prism. The widths of spectral bands depend upon the characteristics of the prism itself, and in any case, Newton's measurements of these widths are so inaccurate as to suggest that he had already conceived the analogy between light and sound before the measurements were made. This is one possible reason why Newton abandoned any attempt to overcome chromatic aberration. (39) Although the methods for removing chromatic aberration had been known for over sixty years, Prout clearly did not appreciate the experimental fallacy behind Newton's

37. Sentiendi, p. 19. The analogy has been exploited by composers like Alexander Scriabin whose "Keyboard of Light" was to be played during his "Prometheus - Poem of Fire", 1909-10.

38. The modern theory of colour sensation opts for red, green and blue; and a separate black and white vision in poor light.

analogy in 1810. If he had measured the spectral band widths for himself, Prout would have been unable to find any analogy with musical frequency intervals. Instead, committed to the discovery of analogies between different sensations, he was glad to accept, uncritically, the support of the illustrious Newton.

Finally Prout exploits analogies between audition and vision just considered, and gustation and olfaction. Perhaps, he speculated, the three principal sounds and colours correspond with three principal tastes and smells! Fortuitously the modern theory of gustation and olfaction supports such reductions. (40)

What advantages were there in Prout's analysis of sensation? Would it not be just as reasonable to treat the five sensations as distinct phenomena? Prout believed that such a fivefold analysis would not be as simple; whereas his thesis had the merit of concentrating:

our attention upon one object, and [it] teaches us to believe that in all the instances above mentioned we see only modifications of that object and not new ones, the comparison &c of these may induce us to form a more just notion of its properties and thus at some future day lead us to the discovery of its real nature, independently of this however the beautiful simplicity which the idea presents of the operations of the Author of Nature, and the coincidence which in this respect it points out between this and the rest of his works cannot fail to recommend it to the attention. And I may here observe that it was chiefly this that led me to prosecute the subject on the present occasion. (41)

In addition, Prout's principal reasons, economy of thought and the simplicity of nature, were supported by James Harris's view of the


41. Sentirenda, pp. 21-2.
unity of matter and life.

And thus we may affirm that these three, that is to say, EXTENSION, FIGURE, and ORGANIZATION, are the three ORIGINAL FORMS to BODY PHYSICAL or NATURAL, Figure having respect to its EXTERNAL; Organization to its INTERNAL; and Extension being common both to one and to the other. 'Tis more than probable that from the Variation in these universal, and, as I may say, PRIMARY FORMS, arise most of those SECONDARY FORMS usually called QUALITIES SENSIBLE, because they are the proper Objects of all those Senses which are the proper Objects of our several Senses. Such are Roughness and Smoothness, Hardness and Softness, the tribes of Colours and Odours, not to mention those Powers of Character more subtle, the Powers Electric, Magnetic, Medicinal, &c. (42)

One difficulty remained. How could the same kind of matter produce roughness to the touch, or taste to the tongue? Protr was unable to give a specific answer to this although he pointed out that it was clear from the behaviour of chemical elements that different arrangements and combinations could produce strikingly different properties. Why should not the same be true of sensation?

The same variety of matter in innumerable instances is capable of producing by admixture or combination with another variety of matter in different proportions &c. only modifications equally striking in their apparent forms & qualities as for example oxygen and nitrogen. (43)

It need hardly be added that this is "Protr's Hypothesis" which was to be placed upon an experimental basis by its author, in an atomic context, five years later. (Chapter 7)

42. Harris, Philosophical Arrangements, pp. 88–9.

It is on this point that Prout concludes his essay, but it
is followed by some important notes on caloric, taste, smell and
flavour. Heat is so different from any other sensation that Prout
thinks that it scarcely warrants that term. A certain amount of
free caloric is present in sentient beings as animal heat in order
both to maintain the combination between matter and the vital prin-
ciple, and maintain the animal's temperature through the action of
the vascular system. (44) It seemed likely that free caloric main-
tained the fluidity of body fluids and served as a reservoir "to be
always ready to enter into combination as latent caloric with the
nervous system through the means of which alone it can act upon the
vascular one &c." What the physiologist called the sensation of
heat was therefore the presence in the body of a greater amount of
free caloric than was necessary to maintain the functions of life.
It was not a true sensation since the heat existed in every part of
the body, whereas true sensation, Prout defined, acted only by
external contact. (45)

De Faculatate Sentienti is an extraordinary and excellent
example of the power and the pitfalls of analogical reasoning
which is unguided by experiment. Humphry Davy was to write only a
few years later that:

The substitution of analogy for fact is the bane of chemical
philosophy, the legitimate use of analogy is to connect facts
together, and to guide to new experiments. (46)

44. Sentienti, p. 23.

45. Prout also proposed a mechanism for the sensations of cold, the
warming up of the body after chilliness, and the effects of the
external application or removal of heat, ibid., p. 22.

46. Journal of Science and Arts, 1, 268, 1816.
Prout achieved an intriguing generalisation about the nature and etiology of sensation in his essay, but at the cost of going far beyond the limited empirical evidence, and over-emphasising superficial or accidental similarities, and missing the significant differences between sensations. Although Prout was never to develop these ideas on the chemical and unitary basis of sensation, the quest for a simple chemical picture of the world was to remain with him throughout his chemical career.

With these remarks in mind it is perhaps significant that the remaining notes on taste, smell and flavour which closed the essay are those which have best stood the course of time; for the basis of his discussion is firmly empirical. These notes are also important since they formed the basis of his first publication in 1812.

Taste was always confused with flavour, which physiologists believed to be the principle in a substance which excited taste, until Prout argued correctly that taste and flavour were different sensations.

Taste is that modification of sensation which is caused by the contact of certain substances soluble in water or saliva with the tongue, the nostrils being at the same time closed and the tongue not being in contact with any other part of the mouth. (47)

For the first time in the essay Prout mentions experiments which support this definition of gustation. If substances like nutmeg

47. Sentieni, p. 24. The published definition is briefer:

Taste is that sensation which is produced by substances when brought into contact with the tongue under certain circumstances, the nostrils being at the same time closed.

See Medical and Physical Journal, 28, 457, 1812.
were placed upon the tongue and the nostrils closed or plugged, then only a slight pungency was experienced. (48) This was in fact the experimental justification for Prout's contention expressed in the main essay that the number of tastes was limited to acid, alkaline, bitter "and perhaps one or two more." (49) Since substances which excited taste usually also excited sensations when placed in contact with parts of the body stripped of the cutis, Prout concluded, incorrectly, that all substances which excited taste were stimulants. (50) Moreover this fitted in with Prout's conception that taste was similar to touch, and yet the most limited and imperfect of all sensations. What is usually taken for taste, says Prout, is flavour which is really a combination of taste with smell. People with colds who lose their sense of smell (anosmia) also lose their sense of "taste," i.e. flavour; and people born without a tongue could legitimately claim to "taste". (51)

Smell is that modification of compound sensation which is excited by various states of matter either in an aeriform state or in a state of extremely fine mechanical division when these are drawn in the air through the nose." (52)

48. "It is now many years ago since I heard it asserted 'that a nutmeg has no taste', and witnessed a position so apparently paradoxical proved to be correct. The experiment was directed to be made with the nostrils closed, and in this case it was found to be impossible to distinguish, by what is properly called the taste, whether a portion of nutmeg, or any other substance, was introduced into the mouth," Med.Phys.J., p.459.


50. As has been noticed already Prout did not differentiate between chemical sensations like taste and smell, and chemical irritation.


52. Ibid., p.458.
The sensory mechanism of the nose, like the tongue, was chemical or galvanic. Flavour was another modification of sensation produced by the union of taste and smell. "Substances in general have the strongest flavor that are volatile or partly soluble in air as well as water..." (53) In his published paper Prout gave flavour the rigorous definition which as since passed into scientific literature.

Flavor is that sensation which is produced when substances under certain circumstances are introduced into the mouth, the nostrils being at the same time open." (51)

Olfaction was independent of gustation, but not of flavour. Olfaction could be influenced by strong flavours, as when vinegar is held in the mouth and masks the odour of ammonia held to the nose. (54) The seat of flavour was more extensive than that of taste and included the palate, fauces, the rear of the nose, the pharynx and the upper esophagus.

As previously stated, these notes on taste, smell and flavour were published anonymously by Prout in 1812. (56) Later they were noticed by his close friend John Elliotson who had been a student with him at Edinburgh, and published in Elliotson's translation of Blumenbach's Institutions of Physiology. (57)


54. The reverse experiment did not work, he explained, because ammonia had little flavour due to its great solubility in water.

55. A short note on hearing in the essay is unimportant. (p.26).


57. J.F. Blumenbach, The Institutions of Physiology, trans. from Latin by John Elliotson, 3rd. ed., London, 1820, p.162; 4th ed. (Elements of Physiology), 1828, p.239. The 1st and 2nd eds. of 1815 and 1817 were not available to me, but they probably contain this reference to Prout.
In this way Prout's analysis passed into the general literature of the physiology of sensation even though, curiously, his name has never been attached to it.

In the published account no hint of the original discussion on the nature of sensation was given; instead Prout added some remarks on the necessary conditions for the perception of these chemical sensations. For taste, the tongue had to be moist and the rapid substance soluble in water or saliva. Metals (which were insoluble) excited taste because they were easily oxidised into soluble oxides. The sensation of taste was therefore chemical or electrical in character.

Taste appears to be universally produced by, or during the moment of, chemical action, especially when this is effected, as perhaps it is in every instance, by galvanic agency." (58)

A third criterion for gustation was motion, for "if we apply any substance to the tongue in such a manner that it may be at rest, and its action circumscribed, the sense of taste will be very imperfectly, if at all, produced." Once the substance was rolled about the tongue or the mouth, the taste became distinct. This condition continues to be demanded by modern physiologists since the motion of a substance over the tongue or around the mouth serves both to increase the rate of solution in saliva and to overcome the adaptation (i.e. fatigue) of the chemical taste receptors. (59)

Olfaction similarly required a moist nasal membrane, the solubility of the odorous substance in air, and the motion of the air through the nostrils. These conditions also remain

59; Moncrieff, op.cit., p.73 and chap.5 passim.
accepted by modern physiology. Provided the air or gas which contained the odourous substance did not decompose it, its chemical nature was immaterial. If inspiration was momentarily suspended, the sensation of smell ceased.

Since flavour was compounded of taste and smell, the same three conditions for its production were necessary. For this reason the most highly flavoured substances were those which were soluble in both water and air "and hence all substances containing a volatile oil, as the various spices, aromatics, &c. have this power in an eminent degree, though they excite comparatively very little taste." (61)

In this anonymous paper, Prout made only one significant correction to his earlier account of flavour. Whereas in 1810 he had suggested that inspiration of air through the nostrils was not absolutely necessary for the experience of the sensation of flavour ("as flavor is equally strong during expiration and inspiration"), in 1812 he criticised this earlier view. Instead he thought that "during the act of flavoring ... there is a slow expiration of the air through the nostrils, and at the same time generally a slow inspiration through the mouth." (61) Prout's point was presumably that substances volatilised in the mouth would find their way into

60. Moncrieff, op.cit., p.90.
61. Med.Phys.J., 28,460,1812. In his discussion of flavour, Prout wrote mysteriously of an eminent gentleman who had first drawn his attention to the subject and of another equally eminent gentleman who had agreed with Prout's interpretation.
the nasal cavities while moving in the direction of expired air. However, it seems to me to be mechanically difficult, if not impossible, to simultaneously expire air through the nostrils and inspire air through the mouth. Since olfaction occurs both when air is breathed in and out of the nose, flavour will also be experienced under these conditions. Therefore, Prout's unpublished opinion was in fact strictly correct. Even though "owing to the lower path of the outward air odour is usually noticed during inspiration, ... the many odours of food are mostly experienced through the vapours finding their way from the mouth and throat to the olfactory region during inspiration." (63) This must also be true of flavour, but Prout's condition of a simultaneous "slow inspiration through the mouth" will not do.

2. Respiration

In a review of physiological progress published in 1820, "a friend" of Thomson examined the suggestion that teeth were analogous to hair, nails and horns, and continued: (64)

A similar opinion was advanced in 1811 by Dr. Prout, who at that time drew up the sketch of a paper, the object of which was to prove that the teeth are to be considered as appendages to the integuments, and to be classed with horns, nails, &c. The opinion was principally founded upon extensive anatomical inquiries, showing the analogy between the formation of the teeth, and horns, feathers, &c., and partly also upon physiological reasonings. The paper was never published, owing to reasons which need not be mentioned, but the opinion was stated to many of the author's friends at the time, and he intends at some future opportunity to lay the subject before the public in extended form.

63. Moncrieff, Chemical Senses, p. 90.

64. Ann. Phil., 16, 113, 1820, my stress. The essay was also mentioned by Elliotson in Blumenbach, Elements of Physiology, 1826, p. 387.
Needless to say, no paper on this subject was ever published by Prout, and no draft of any such essay has been found among the Prout papers. As we have seen, apart from the medical dissertation of 1611, he did not publish anything until December 1612 when he had settled in London. That Prout's early interests were anatomical, however, is confirmed by a second paper which was of a histochemical character. (65)

Two facets of his character are shown in this publication: the ability to utilise chemical knowledge in a different scientific field; and his inability, through lack of time, to explore in greater detail the new synthesis that he had indicated. In this paper on anatomical injection what was to become his perennial complaint was immediately pressed before the reader in a letter which preaced his article.

Being engaged in a subject which at present occupies the greatest portion of my time, I have been induced to publish the following observations, with the hope that someone who may have greater abilities and opportunities than myself may pursue them.

The art of Anatomical Injection, which dated back to the seventeenth-century work of Wren, Swammerdam, de Graaf and Ruysch, had by the middle of the eighteenth-century become something of a collective mania among anatomists. As Cole remarked in his essay on the subject, "For a time it monopolized attention, and its importance was so grossly exaggerated as to countenance the belief that all problems of anatomy might be solved by its means." (66) Prout, who would have


been introduced to the art of injection with mercury, wax, and various coloured fluids as a medical student, thought that no improvements had been made since the time of Ruysch. His own contribution, in which he was quite original, was to simply precipitate coloured chemicals inside the vessels of morbid specimens. According to Cole:

"After the Hunters, no important development in injection methods is to be recorded until the introduction of the soluble form of Prussian blue, and carmine gelatine, and other precipitates as colouring matters, in the 19-century." (67) Cole attributed these developments to L. Doyère and A. de Quatrefages (1841) and Strauss-Durckheim (1843). It seems clear, therefore, that Prout's paper was ignored by contemporaries.

After many trials in which Prout learned to avoid solutions which damaged the animal flesh or in which the precipitation tended to block the distribution of the solutions, he obtained excellent results from the precipitation of Prussian Blue from saturated solutions of potassium ferrocyanide and ferric sulphate. Another successful agent was barium chloride with sodium sulphate leading to a white precipitate that was especially suitable for filling transparent parts; less perfect was the precipitation of a carmine colour from stannous chloride and cochineal. All injections were made through a syringe after the solutions had been first heated up to 100°F ; the order in which the solutions were injected seemed immaterial.

67. Cole, op.cit., p. 335. Great use of ferrocyanide precipitation in viva was made by Bernard.
In an investigation of the vascular nature of the ox's eye, Prout found that "the vessels of all parts of this organ appear to communicate freely with one another; the part least connected with the rest is the retina, and this is supplied by its own proper artery." He successfully stained the cornea, the lens capsule (which he misinterpreted as vascular in character), and concluded that the hyaloid membrane "in the adult state at least ... derives all its vessels from the great arterial communication situated a little behind the ciliary ligament, and not from the retina, as usually stated." Although Prout promised to continue his research into the anatomy of the eye he never did so; for his attentions were already totally absorbed by biochemical research. His first paper in the field of human physiology and biochemistry appeared in Thomson's Annals of Philosophy in the same year, 1813.

Prout's intention in these "Observations on the Quantity of Carbonic Acid Emitted from the Lungs during Respiration, at different Times, and under different Circumstances" was to determine both whether the power or capability of the lungs to form the carbon dioxide exhaled in human breath was constant throughout the day, and constant for the individual. According to Lavoisier neither was the case. Prout's elaborate analyses were made on himself with the aid of a breathing apparatus that was much less cumbersome to use than that previously employed by Allan and Pepys, and similar to a modern spirometer.

68. He sent this proof to Astley Cooper whose reaction is not recorded. The lens is fibrous, not vascular.

69. W. Allen and W. Pepys, Phil.Trans., 1808, pp. 249-31. They had concluded that the volume of carbon dioxide produced in the lungs was equal to the volume of oxygen consumed.
Acid Gas [Nov.

his height, and hence
his maximum energy;
e influence, must now
and if the hypothesis
it is perhaps easy to
which it presents, and,
on if carbonic acid
under its immediate

of the sun, or minor
ous system, we know
part of our frames
To refer; therefore, an
is in other words to
I consider it better to
to error myself, and
at in the present state
ctical. It is possible
lish may depend upon
ilar to myself, and
doubt they are liable
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are no doubt of their
ld be premature on a
of a single indivi-
till I have searched
the observations of
etter myself they will
ent points in phy-
ements were made is ex-
ry perhaps be learned.
]. Its whole capacity
0; of these 2 cubic
accurately graduated
it is filled with water,
ir is screwed on, as
in the stand; and by
permitted to run out

As heat, light, electricity,
ately and energetically
their periodical changes,
act only by placing it
mon chemical process,
to be understood.

On the Carbonic Acid Gas respired from the Lunges.

Corrected by R. Long. 2.2.3.42, 342, 1813.
The capacity of the vessel (see Fig. 10) from the stopcocks G to H was exactly 25 cubic inches of water at 60°F.; and that of the slender tube, which was graduated in tenths of a cubic inch, 2 cub. inches.

The apparatus was filled with water from G to H and the respiratory bladder, C, which had a capacity of 300 cub. inches was filled with respired air and screwed on at G. This air, which contained carbon dioxide, was admitted to the main vessel by releasing the water through B; it was allowed to reach room temperature before analysis. The stopcock, G, was then closed, and an elastic gum bottle, D, containing caustic potash screwed on to the bottom of the apparatus at H. This solution was then introduced into the main vessel through H and shaken with the air in order to absorb the carbon dioxide. "It is then returned to the bottle, and the stopcock, H, secured. ... the cock, H, is again opened under water in the vessel, E, which rising shows the quantity of carbon dioxide contained in the analysed air, care being taken to raise or depress the instrument till the water stands exactly at the same height on the inside as the outside of the stem." (70)

A blind analysis for the carbon dioxide absorbed by the water was made using mercury, and a correction factor of one-tenth percent applied to every reading. Prout's procedure was exact and thorough. In each experiment, thermometer, barometer and pulse readings were taken, and six deep but uniform expirations were made into the bladder after inspiration of the air through the nostrils. It should be noted, however, that he did not determine the quantity of air or oxygen inspired, and that he expressed his results as percentages of

All carbon dioxide in the expired air (viz. for every 100 cubic inches of air expired, so many cubic inches of carbon dioxide were also expired). His results, despite the loving care shown in his procedures, were therefore of no quantitative value in the determination of the total amount of carbon dioxide exhaled. Yet viewed qualitatively his results, and his interpretations of them, are of considerable interest.

Throughout this research Prout placed himself on a strict regimen "which consisted in keeping myself as nearly as possible in the same state in every respect." He adhered to this "arduous" discipline for nearly three weeks during August 1813, "making the experiments every hour, and sometimes oftener, during the day, and occasionally during the night also." There seemed to be a consistent pattern in his results which he formulated as a law of nature:

The quantity of oxygen gas consumed, and consequently of carbonic acid gas formed, during respiration, is not uniformly the same during the 24 hours, but is always greater at one and the same part of the day than at any other, that is to say, its maximum occurs between 10.0 a.m. and 2.0 p.m., or generally between 11.0 a.m. and 1.0 p.m.; and its minimum commences about 8.30 p.m., and continues nearly uniform till about 3.30 a.m. (71)

No such law will be found in modern textbooks of biochemistry for Prout was partly misled by his regimen and general ignorance of cellular chemistry; and otherwise, he tended to underrate the influence of foodstuffs, or rather, the importance of noting the times of meals and fasts. The correlation of all the possible variables

71. Ann. Phil., 2,330, 1813. The quantity of carbon dioxide remained stationary at night at 3.3%, but after 3.30 a.m. it rose steadily to as much as 4.1% at noon, and returned to 3.3% by 8.30 p.m. The mean value (equals half the sum of the day and night averages) was 3.45%.
which may influence metabolism only began to be completely unravelled in the magnificent research programme of the physiologist and nutritionist Edward Smith some forty-five years later. (72) In comparison Smith found that the variations in the quantities of carbon dioxide expired:

during the day were extremely great, so that the maximum and minimum quantities of carbon dioxide usually differed to the extent of more than half the latter. ... These variations were due to food, and were of such a nature that an increase began directly after a meal and progressed to a maximum, after which they declined gradually to a minimum until the following meal. (73)

Smith found that there was a maximum quantity some two hours after a meal, and a corresponding minimum before meals, and he noticed the existence of "a state of the system which is nearly uniform under all the circumstances of the day, when the body is uninfluenced by exertion or the primary processes of digestion, and which may be called the basal or normal state." (74) Obviously Prout's absolute minimum for the early hours of the morning represents Smith's basal state, or a basic metabolic rate as it would now be called; while his noon maximum was probably due to a midday dinner. Unfortunately, since unlike Smith, Prout did not tabulate his mealtimes, it is impossible to completely interpret all of his results. Initially Prout thought

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72. "Experimental Inquiries into the Chemical and other Phenomena of Respiration and their Modifications by various Physical Agencies", Phil.Trans., 1859, pp. 681-714; "On the Action of Foods upon Respiration during the primary processes of Digestion", ibid., pp. 715-42. Smith noted that Prout's results were valueless. A biography of Smith is expected from Carleton B. Chapman of the University of Texas.

73. Phil.Trans., 1859, p. 698.

74. Ibid., p. 700.
that the intake of food had some bearing on carbon dioxide output, since he stated that he had kept himself "as regular as possible, with respect to food, &c., and only partook of the most simple, in order that it might not interfere with the results." (75) But in a later paper he declared that although "some are of the opinion that there is more carbonic acid given off a few hours after eating ... I have not myself observed this circumstance." (76) Food, he thought, did little more than keep the output at the standard for the time of day. Prout was therefore obliged to look elsewhere for an explanation of his First Law.

The diurnal regularity of the phenomena led him to speculate that "the presence and absence of the sun alone regulate these variations". (77) This itself was not entirely unreasonable, as Smith also noticed, (78) for there is a seasonal variation in the metabolic rate. Indeed, Prout went on to investigate this in a second paper. (79) But notice that Prout felt that the sun alone was responsible for such variations: Prout's speculation was of a very general hypothetical character. (80) Thus whereas for Smith variations from a basal

75. Ann.Phil., 2,335,1813.

76. Ann.Phil., 13,69-70,1819. Nevertheless, in 1813 he had detected a frequent slight depression of carbon dioxide output between 6.0 a.m. and 8.0 a.m.; which he put down to abstinence from food, or a result of his Second Law (infra), ibid., 2,332,1813.


78. Phil.Trans., 1859, p. 702.


80. In fact his results are all explicable in terms of the influence of pH CO₂ on the oxygen dissociation of haemoglobin; and of nervous stimuli, as well as external factors such as temperature.
norm had to be explained, for Prout variations from both an inferior and superior norm were to be explained. Yet Prout was too good an observer to have failed to notice that the total quantity of carbon dioxide expired varied from day to day, and that his own average output differed from the outputs that had been published by other observers. (31) For example, on one occasion his own output maximum rose to 4.9% from the usual 4.1%, and his minimum rose to 4.15% from the usual 3.3%. There had been no change in his health, but a decided fall in the barometer. "Generally speaking", he concluded from this, "I think I have found the quantity of carbon dioxide expired increase during the sinking of the barometer." (32) This pressure factor was confirmed later by Vierordt and Smith. (72) However, Prout went further, and in order no doubt to correlate this fact with his theories of matter, he ascribed the whole effect without further explanation to a highly concentrated electrical state of the atmosphere.

Other variations seemed to follow a period of fasting, especially between 6.0 and 8.0 a.m. —presumably prior to Prout's breakfast. At first he correctly assigned their cause to abstinence; then he changed his mind and explained them in terms of a Second Law:

81. Prout assembled and published several values from the literature which were all greater than his own since, he believed, they had all been estimated at the time of the natural maximum. (There is considerable variation in the respiratory quotient of individuals). Full references, Ann.Phil., 2,333,1813.

82. Ann.Phil., 2,331n,1813; also ibid., 4,335n,1814.
PROUT'S GRAPH SHOWING THE HOURLY FLUCTUATIONS OF CARBON DIOXIDE OUTPUT (Ann. Phil., 2, 352, 1813)
Whenever the quantity of oxygen gas consumed, and conse-
quence of carbonic acid gas formed, has been by any cause
increased or raised above the natural standard of the
period, it is subsequently as much decreased below that
standard, and vice versa. (83) (See Fig. 11, Plate XII)

This apparent oscillation naturally followed from Law I in which
Prout had failed to appreciate the possibility of a normal lower
standard state from which the deviations occurred.

His further experimental work on the causes of the increases (84)
or decreases in carbon dioxide output, although on a small scale, was
no less thorough than Smith's, and only vitiated by his use of per-
centages. It included the examination of the effects of exercise,
eating and drinking (alcohol and tea), and the "depressing passions"
(i.e. emotional states conducive to yawning, sighing, or deep inspira-
ations). Exercise, which he failed to express in reproducible quanti-
tative terms, caused him considerable variations of carbon dioxide
output according to its nature, degree and duration. Although violent
exercise (running or a long brisk walk) brought about short dramatic
decreases of carbon dioxide from the norm for the time of day, moder-
ate exercise such as ordinary walking or speaking appeared "always at
first to increase its quantity; but after having been continued for a
certain time, it ceases to produce the effect; and if prolonged to
induce fatigue, the quantity is diminished." Abstinence from food, or
fatigue, also lowered carbon dioxide output; but Prout made an


84. Increases were few in number and always transient, as opposed to
the numerous cases of less-transient decreases, ibid., p. 335.
insufficient number of experiments to fully realise the effects of foodstuffs both in sustaining the system to the minimum basal line and of causing the observed increases in output above the basal line of short duration. Any kind of alcoholic drink "uniformly lessen[ed], in a greater or less degree, the quantity of carbonic acid gas elicited, according to the quantity and circumstances under which it (was) taken." (85)

Very acutely, he noticed the effect which alcohol had on a full stomach: a period of low carbon dioxide output, followed by a period of increase accompanied by much yawning, as if "the system is then freeing itself of the retained carbon." The function of yawning was therefore "to counteract the effects of that approaching state of the system which favours the retention of carbon and induces sleep. We yawn and stretch ourselves in the morning after waking to dispose of the carbon which has been retained during that act." (86) Anxiety and solicitude also produced decreases, and Prout warned the experimentalist who would repeat his work "of the absolute necessity there is of keeping the body and mind in a state of the greatest possible uniformity." If not, erroneous and discordant results would be produced "as they were for the first day or two with me; so much so indeed, that I was almost ready to give up the matter in despair, and to imagine that there was nothing like uniformity in it." In his eagerness to show a friend (probably Elliotson) his noon maximum, Prout obtained

86. Ibid., p. 337a.
The demonstration by the young surgeon Benjamin Brodie that respiration was dependent on the brain and nervous system, led Prout to suggest that the numbers he had obtained were "a sort of relative measure of the degree of energy of the nervous system; but whether it be a measure of the energy of this system as it operates through the medium of the blood, or the muscular apparatus of circulation or respiration, or partly through all the three, I am unable to decide." Prout's numbers were of course not an absolute measure of metabolism, and it was left to Liebig's pupils to demonstrate the significance of the respiratory quotient as a "measure of the energy of a system."

Unable to decide the full significance of the percentage numbers, Prout preferred to speculate in a more general manner concerning the cyclical pattern of life which, he was convinced, was connected with the daily cycle of the sun.

For according as this 'great source of life and heat' is present or absent, are organized beings, in general, either awake and active or asleep and inactive. Now at noon this powerful agent of nature is at his height, and hence at this time may be supposed to exert his maximum energy; whatever, therefore, is under his immediate influence, must now be supposed to be affected more strongly; and if the hypothesis be correct, that (the) nervous action is so, it is perhaps easy to account for ... (the fact) of the greatest elicitation of carbonic acid at noon, respiration being considered as under its immediate influence. (69)

87. Phil.Trans., 1811, p. 36ff; 1812, p. 37ff.
88. Ann.Phil., 4, 336-1814; also as draft among Prout papers; the word 'through' replaces 'on' in an addendum to the published paper.
89. Ann.Phil., 2, 341-2, 1813. This refers to the degree of carbon dioxide formation, not to its actual formation which Prout knew to be chemical.

(Ann. Phil., 4, 333, 1814)
In 1614 Prout returned to this solar hypothesis, and in an elaborate graphical plate (Fig. 12, Plate XXIV) he attempted to show that an increase in exhaled carbon dioxide "always uniformly occurred soon after the commencement of [dawn] twilight, and before sunrise, throughout the year." (90) This increase was greatest when the nights were longest, and it uniformly decreased as the nights grew shorter; "a circumstance which, however, appears to have been chiefly owing to a diminution having taken place in the usual minimum quantity towards the morning, either probably from the fatigue of watching or from drowsiness." Armed with this new information, Prout hastened to modify his First Law to read:

The quantity of oxygen gas consumed, and consequently of carbonic acid formed during respiration, is not uniformly the same during the 24 hours, but is always greater at one and the same part of the day than at any other, that is to say, its maximum occurs between 10.0 a.m. and 2.0 p.m., or generally between 11.0 a.m. and 1.0 p.m.; and its minimum commences about the ending of evening twilight and continues nearly uniform until the beginning of the dawn twilight. (90)

This generalisation is not very useful since Prout does not say how much air was inspired, or relate the change of season to the total daily gram weight output of carbon dioxide. But it did imply that less carbon dioxide was expelled during the winter months than in the summer. This was not confirmed by later experimentalists who, on the contrary, recorded a greater excretion of carbon in the winter than in the summer months. According to Smith, "the advancing hot season

90. Ann. Phil., 4, 333, 1814. "These experiments were made by staying up the whole of the night" on several occasions between the winter and summer solstices of 1813-14. Smith suggested later that research was needed on the possible influence of light intensity through the year, Phil. Trans., 1859, p. 708.
lessered all the vital and mechanical changes of respiration, viz.,
the quantity of carbonic acid expired and of air inspired, the rate
and depth of respiration, the quantity of vapour exhaled, and the
cooling of the body. These include lessened muscular and vascular
actions and chemical changes. With the return of the cold season the
quantities increased." (91) However, Smith found that a decrease of
carbon dioxide output occurred about the termination of the evening
twilight. Prout's seasonal correlation is probably true, but it is
meaningless unless measurements are made of the total quantities of
air inspired and carbon dioxide expired. Percentage formulations
will not do.

Prout's promise to consider the effects of "other articles of
the materia medica and alimentaria" (92) on carbon dioxide output
were partly fulfilled by the Scots physician and chemist, Andrew
Fyfe junior, in an Edinburgh dissertation De copia acidi carbonici
e pulmonibus inter respirandum evoluti. (93) Prout learned of the
Scotsman's experiments from Fyfe's brother who was in London during
1813. The two men communicated and exchanged results. By November 1813
Prout had Fyfe's results to hand, and he summarised them in Thomson's
Annals in the following year. Fyfe's own daily output of 8.5% was

91. Smith, Phil.Trans., 1859, p. 708.
93. Edinburgh M.D. dissertation, 1814, 23pp. There is a copy with a
dedication to Prout among the Prout papers. Fyfe (1792-1861)
mentioned Prout's work in his essay, pp. 16, 20-3, but he did not
extend the research in the several editions of his textbook,
Elements of Chemistry, Edinburgh, 1827, etc.; e.g. p. 295 of this
first edition.
reduced by a vegetarian diet to 4.5%, and showed erratic variations with a meat diet. (94) He had also found that mercury and dilute nitric acid reduced the carbon dioxide output of a syphilis patient to 3%. Prout confirmed the observation upon a syphilitic in Guy's Hospital who was under the supervision of Astley Cooper. He was pleased to be able to endorse Fyfe's results, but he was critical concerning the insensitivity of Fyfe's apparatus compared with his own, and he appealed for other workers to repeat their experiments. However, apart from Berzelius, (95) their contemporaries were little interested in such physiological experiments, and apart from the isolated experiments of Charles Coathupe in 1639, whose inaccurate results were diametrically opposed to Prout's, (96) his work had to await the clarification of the role of respiration and its relation to general metabolism made by Liebig; before the final repetition and extension of his work by Edward Smith in the 1850s.

3. Digestion 1614-1827

One of the more interesting papers to be published in the short-lived Annals of Medicine and Surgery (supra, Chapter 1) was Prout's "Inquiry into the Origin and Properties of the Blood". (97) Only three


96. Phil.Mag., (3)14,401-14,1839. Coathupe also used percentages. He noted down the times of his meals but concluded that the "carbonic acid gas produced by respiration ... is less during the period of active digestion, [and] that it increases with increased abstinence from food."

parts of this general review (much of it based upon his Animal Chemistry lectures) were published in 1616, and because of the journal's limited circulation Prout had the incomplete paper republished in a slightly modified and abbreviated form in Thomson's Annals in 1819. (98) The missing section, which had entailed a long investigation of the chicken's egg, was only finally presented, as an independent paper, to the Royal Society in 1822. (99) Prout's announcement of the discovery of hydrochloric acid in animal gastric juice followed in the autumn of 1823. But Prout had not suddenly started to work on the chemistry of digestion in 1823, for, as the paper on sanguification shows clearly, he had been involved in the problems of digestion as far back as 1616 when he had argued that "blood begins to be formed, or developed from the food, in all its parts from the first moment of its entrance into the duodenum, or even, perhaps, from the first moment of digestion, and that it gradually becomes more perfect as it passes through the different stages to which it is subjected, till its formation be completed in the sanguiferous tubes, when it represents an aqueous solution of the principal textures and other parts of the animal body to which it belongs." (100) Since at this time he divided the process of blood formation into the four stages: digestion (confined to the stomach); chymification (confined to the


duodenum); chylification (confined to the lacteals); sanguification proper (confined to the blood vessels), we may conclude that his concern to provide a chemical natural history of blood formation led him to a study of digestion as part of this larger programme of physiological research. In fact the programme probably began during the lecture period of 1814 for he seems to have had some encouragement from both Alexander Marceau and Astley Cooper.

Although Prout reviewed each of the four stages of sanguification in turn, here we shall be principally concerned with his ideas on digestion.\(^{101}\) The acidic contents of a rabbit's stomach were examined some two hours after feeding and shown to contain "traces of alkaline muriate, with slight traces of an alkaline phosphate and sulphate; also of various earthy salts, as the sulphate, phosphate and carbonate of lime." \(^{102}\) A similar acidity—a well authenticated observation in the literature—\(^{103}\) and analysis were made on the gastric fluids of a pigeon, tench and mackerel. Since the heterogeneous nature of the stomach fluids had caused so much confusion in the past, Prout proposed to divide and identify the potential contents into saliva, the mucous coat and exhalents of the stomach, and the actual gastric juice whose identity was "unknown, it never having been attained in a separate state." Yet it was clearly "some volatile acid from its effects on litmus paper. ... I considered it in the

\(^{101}\) A long preliminary section on vitalism will be examined in Chapter 6, infra. The 1816 version also carried an "historical sketch of the progress of our knowledge respecting the blood." Both of these parts were deleted in the version of 1819.

\(^{102}\) Ann.Phil., 13, 13, 1819; the contents of the stomach were strained through a linen cloth; the filtrate reddened litmus and coagulated milk, but did not contain albumen.
pigeon as carbonic. There appears, however, to be occasionally another acid which is of a much more permanent nature, and it is probably the phosphoric acid." (104) It is clear, therefore, that at this period, 1616 to 1619, Prout was far from the identification of the gastric juice as hydrochloric acid; in fact, by 1620, he had come to Berzelius's opinion that the active principle was lactic acid. (105)

In his study of the "evolution" of the blood, Prout had analysed the chymes (106) of several animals, including dogs (107) and rabbits.


106. This usually means the semi-solid acidic contents of the digesting stomach. Prout, however, applied the term solely to the contents of the small intestine from the duodenum to a region where the aliment began to show traces of "albuminization". See Ann.Phil., 13, 13n, 1819.

107. The chymes and chyles of two dogs fed on vegetable and meat diets respectively, had been given to Prout by Astley Cooper who wanted such analyses made before his lectures to the Royal College of Surgeons in 1615. Cooper seems also to have assigned the same tasks to Marlet. (See C. Soudamore, A Treatise on Nature and Cure of Gout, 2nd ed., London, 1817, p. 309). In a letter to Berzelius, Marlet wrote:

"J'ai en dernier lieu examiné le chyme d'un animal nourri exclusivement de matière végétale, et j'y ai trouvé abondance d'albumen par tous les tests ordinaires, savoir chaleur, acide, alcohol, oxyuriate de mercure, etc. Mais un jeune Docteur (le Dr Prout) qui, soit dit à ma honte, vous avait mieux étudié que moi, objecte que ces tests ne sont pas décisifs et que vous recommandez l'acide acetique et le prussiate de potasse pour découvrir l'albumen.

(Berzelius, Breif, vol. 1, part III, p. 123). Berzelius replied that he was pleased to know Prout had read his organic memoir of 1814 (Ibid., pp. 132-3)."
an ox, trench, mackerel and pigeon fed on vegetable diets in
order "to ascertain if the chyme exhibited any traces of the albuminious contents of the blood" which the food contents had not. He used Berzelius's test for albumen which involved the addition of potassium ferrocyanide to an acetic acid solution of the substance; a white precipitate indicated the presence of albumen. Despite the changes which obviously took place in the chyme after it had passed through the duodenum and mixed with the bile and pancreatic juices, Prout could not at first detect any albumen. In view of Becarri's demonstration in 1742 that vegetable substances contained albumen, Prout's experimental results are extraordinary, and they puzzled later investigators like Tiedemann and Gmelin.

It took the genius of Bernard to notice the analogy between the changes which took place in the duodenal chyme and the saponification of fats, and to ask what was the chemical cause of this change. Yet Prout, like Spallanzani, felt that the changes which took place within the alimentary tract were purely and simply chemical (even though they be controlled ultimately by vital agencies), and hence reproducible under laboratory conditions.

I tried to produce these changes out of the body, and with this view mixed a portion of the fluid obtained from the contents of the stomach of the rabbit, ... with a portion of the bile of the same animal. A distinct precipitation took place, and the mixture became neutral; but although I thought that the resulting fluid was more of an albuminuous nature, yet the formation of a perfect albuminous principle was doubtful; probably the presence of the pancreatic juice was necessary to complete the formation of this principle. (108)

When Prout continued the analyses of the contents of the alimentary tract through the large intestine, caecum and rectum, he found that the albumen vanished altogether.

More successfully, he detected its presence in the serum of chyle taken from the thoracic duct. When the serum from which fibrin had been separated was boiled with acetic acid, a copious white precipitate was formed. Ordinary albumen did not behave in this way thought Prout. But if the precipitate was filtered off and treated with ferrocyanide in acetic acid, the albumen test was satisfied. (109) Since the material did not behave in quite the same way as ordinary albumen, Prout labelled it an incipient albumen, and after further inspection he decided that

the proportions of albuminous matter, and especially of fibrin, is much less; or at least their principles exist in a much less perfect state in the chyle as immediately taken up from the intestines, than as exists in the thoracic duct, and about to enter the sanguiferous system.

Either the albumen was formed at one particular moment and site, or as was more probable, there was a gradual "completion" of the principle as it passed through the lacteal system.

In conformity with this view ... I have ventured to call by the name of incipient albumen a peculiar principle uniformly found in the chyle of the mammalia, and which appears to decrease in quantity as the two albuminous principles increase. (110)

109. The acetic acid neutralises the alkaline albumen which is precipitated. Cf. W.T.Brande, Phil.Trans., 1812, p.95.

Prout was vague concerning the exact chemical nature of this 
incipient albumen; it was probably a mixture of albumproteins. Brande
and others had commented on its resemblance to milk, and both Prout
and Vauquelin seized upon its resemblance to the fatty matter of brain.
At one time Prout had therefore speculated that the incipient albumen
might be circulated to the brain to form "the cerebral and nervous
substance" - a throwback to Galenic physiology. It is more than likely
that Prout's concentration upon analytical techniques for the estimation
of carbon, oxygen and hydrogen, and their improvement, prevented
him from making a thorough investigation of the chemistry of albumino-
ous substances. In any case there was no satisfactory technique for
nitrogen analysis until 1834. (111)

Prout's remarks on the detection of albumen in duodenal chyme
were misunderstood by Tiedemann and Gmelin in their work on digestion
published in 1826. (112) They had understood him to mean that albumen
was not to be found in the duodenum even after the chyme had mixed
with the bile and pancreatic juices, and absurdly, that Prout had been
unable to detect any albumen in the stomach after a meat diet. Perhaps
the misunderstanding was due to careless reading of the German trans-
lation of Prout's paper by Meinecke, (113) and confusion over Prout's
negative result with the vegetable diet. The two Germans pointed out

111. J.B. Dumas, J. de Pharmacie, 20, 129, 1834; the more familiar test
of Lassaigne was designed in 1843.

112. F. Tiedemann and L. Gmelin, Recherches experimentales, physiolog-
in 1826).

that they had shown pancreatic juice contained much albumen; therefore it would be most surprising not to find albumen in the duodenum. Prout found no difficulty in defending his earlier position.

When my paper was published in 1819, I did not know the composition of the pancreatic juice, as was then stated, (114) and I regret that I do not know so much about it yet as I could wish. I believe, however, that it contains albumen, and consequently admit that some of the albumen found in the duodenum may be derived from this source, though it is still my decided belief that by far the greater proportion found there under the circumstances I have mentioned is derived from the food, and is actually developed on the spot during the series of changes that take place, and in which the bile and pancreatic juice play an important part. (115)

Tiedemann and Gmelin referred back to Prout's paper and gave a considerate reply; they admitted completely representing him, and returned the praise he had placed upon their work. (116)

It seemed impracticable to separate the fourth stage of actual sanguification from another physiological process, respiration; for Prout, in common with other physiologists, erroneously believed that "the chyle from the thoracic duct proceeds into the sanguiferous system, mixes with the general mass of circulating fluids, and almost immediately passes through the lungs, where it is exposed to the air, and appears to undergo the final process, and to be converted into blood." (117) In turn, the problem of respiration, and therefore of

114. "The properties of the pancreatic juice I never could satisfactorily ascertain; but it has usually been considered as analogous to the saliva; and if this opinion be correct, it may be safely considered as containing no albumen", Ann.Phil., 13, 272-3, 1819.

115. Ann.Phil., 26, 409-10, 1826; protein breakdown does occur in the duodenum and jejun. Prout gave substantially the same account in 1834.

116. Phil.Mag., (2) 4, 4, 1828.

117. Ann.Med. & Surgery, 1, 147, 1816; Ann.Phil., 13, 265, 1819. The white particles which could be seen in chyle probably became red when exposed to air in the lungs.
sanguification, was associated with the problem of the origin of animal heat. These were problems shrouded in too much obscurity for Prout to shed much light upon them. All that he was able to do was break down the whole subject into a series of separate research problems, and review the questions that had been answered or remained unanswered.

Respiration appeared to be an identical process in all animals acting through the conversion of carbon and oxygen into carbon dioxide via the blood or an analogous fluid. Davy had convincingly demonstrated that gases other than oxygen could not be respired with the preservation of life. But was respiration the same "in degree in different classes of animals compared with one another, or in different animals of the same class?" Comparative experiments on animals had not been made, while among animals of a single species, there had been only the unrepeated experiments of Prout and Fyfe. They had also been the only experimentalists to ask whether the pattern of respiration varied in degree at different times of the day and season. Was the blood as a whole, or only in part, concerned with respiration? Was it true, as Berzelius believed, that the colouring matter of the blood contained the principle from which the carbon was oxidised during respiration? In any case, what was the real function and purpose of respiration? Prout's own speculative opinion, related to his theory of matter, was that it activated the electrical powers of

118. Ann.Phil., 13, 266, 1819. Prout remarked that the question of nitrogen respiration was sub judice. A few experimentalists had noted an apparent absorption of nitrogen during respiration; others that the quantity of nitrogen expired was greater than that inspired; others that it remained constant. See E. Turner, Elements of Chemistry, 4th ed., 1833, pp. 306-7. In fact the volume of nitrogen expired is slightly greater because of the partial pressure effect of room observation.
an organism, "and we may even go so far as to suppose that the
colouring principle represents the most oxydizable metal, in the
galvanic battery ... and that the carbonic acid represents the met­
allic oxide formed during its action." (119) The act of respiration
converted chyle into blood by the removal of unwanted carbon, and
animal heat was released in the process. Prout sided with those
physiologists who thought that carbon was released in the lungs to
combine with atmospheric oxygen rather than that carbon dioxide it­
sel f was released from the blood; but by 1834 he had changed his
mind. (120) The precise connections between animal heat, respiration
and the assimilation of food were dismissed with the dispiriting
reflection that "from the vital character of the processes, we shall
probably ever remain ignorant of their precise nature." It always
remained a failing of Prout's scientific method compared with Liebig's
that he believed his ignorance was perpetually veiled by Isis, and
that it was an argument for vitalism. (See Chapter 6)

The experiments on "the evolution, &c., of the blood of the
chick in ovo" were not completed in time for inclusion in the paper
of 1816. Because of his work on urine chemistry, Prout was still not
in a position to publish any results when the sanguification paper
was republished in 1819. The chick embryo investigation had meanwhile
broadened in scope, and eventually, in 1822, it was presented to the
Royal Society as "Experiments on the Changes which take place in the

118. (cont.) pressure effect of oxygen absorption.
fixed principles of the egg during incubation". (121) This was one of the first examples of chemical embryology - a field of research which has only produced significant results in this century.

Prout found that an incubating egg lost about a sixth part of its weight by evaporation of water through its shell; this loss was some eight times greater than from an egg placed under non-incubatory conditions.

**ANALYSIS OF AN IDEAL (STANDARD) EGG**

- weight 1000 grains; weight of shell and membrane 106.9 grains;
- weight of white 604.2 grains; weight of yolk 288.9 grains.

<table>
<thead>
<tr>
<th></th>
<th>sulphuric acid</th>
<th>phosphoric acid</th>
<th>chlorine</th>
<th>carbonates of potash, soda</th>
<th>carbonates of lime, magnesia</th>
</tr>
</thead>
<tbody>
<tr>
<td>albumen</td>
<td>.29</td>
<td>.45</td>
<td>.94</td>
<td>2.92</td>
<td>.30</td>
</tr>
<tr>
<td>yolk</td>
<td>.21</td>
<td>3.56</td>
<td>.39</td>
<td>.50</td>
<td>.68</td>
</tr>
<tr>
<td>total</td>
<td>.50</td>
<td>4.01</td>
<td>1.33</td>
<td>3.42</td>
<td>.98</td>
</tr>
</tbody>
</table>

In eggs examined analytically over an incubation period of three weeks, Prout found that an exchange of chemical substances took place between the yolk and a part of the white. Initially the yolk (which was very difficult to analyse since combustion brought about the oxidation of phosphorus to phosphoric acid, and the inhibition of complete combustion) lost a portion of its oily matter to the white causing it to curd like milk. Reciprocally, the albumen lost portions.

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121. Phil. Trans., 1622, 377-400, Read 16 May 1622; rep. Ann. Phil., 21(5),100-111,1823, with small correction to original paper. Prout examined the eggs of a chicken, duck and turkey.
of its watery and saline parts to the yolk which increased in size. As the incubation proceeded, the watery and saline parts migrated back to the white from the yolk which consequently shrunk in size. In particular, Prout detected a loss of phosphorus from the yolk to the white where it formed the calcium phosphate essential for the growth of the chick's bones. But the overall amount of phosphorus in the embryo remained constant. On the other hand, where had the calcium come from? For according to Prout's analyses there was a deficiency of calcium in the new-laid egg.

The only possible sources, therefore, ... are from the shell, or transmutation of other principles.

He felt that it would be impossible for an analyst to demonstrate that the shell provided the lime because of the wide variations in shell weight, and the impossibility of analysing the shell before incubation. This was, of course, only impossible from the vitalist's viewpoint. Furthermore, there appeared to be an absence of vascular connections between the shell and the membrane putanalis which inscribed it; it seemed impossible for calcium to pass from the shell to the membrane. Although (as will be shown in Chapter 6) in 1816 Prout had strongly denied the possibility of a creation of elements by animal organisms, he now proposed for serious consideration the possibility of a synthesis of the "element" calcium. He carefully guarded himself against ridicule:

I by no means wish to be understood to assert, that the earth is not derived from the shell; because, in this case, the only alternative left me is to assert that it is formed by transmutation from other matter; an assertion, which I confess myself not bold enough to make in the present state of our
knowledge, however strongly I may be inclined to believe that, within certain limits, this power is to be ranked among the capabilities of the vital energies. (122)

As we shall see, Prout did not believe in the ultimate nature of the so-called elements, so that for him, and several other chemists and vitalist physiologists, such organic transmutations were legitimate speculations. Sir Gilbert Blane took Prout to mean that elements were created in the body by a vital agency, (123) but this was refuted by Alfred Taylor in 1849. (124) Prout’s anonymous Edinburgh obituarist made a positivist moral point over this episode: “The explanation which, in the light of twenty-six years chemical analysis has been able to furnish, is most serviceable in impressing the lesson, that, in circumstances in which we are unable to explain all phenomena in living bodies, it is most prudent to wait and suspend judgement until the accumulation of further facts ... comes to give the elucidation which was previously wanted.” (125) Berzelius, who represented the multiple-element chemists and physiologists, held no brief for transmutations and avoided mentioning Prout’s idea in his fullsome


123. G. Blane, Elements of Medical Logic, London, 1825, p. 70, cited by Taylor, ref. 124. This edition was not available to me. The 1st ed. of 1819 (Logick) is very vitalistic, but it does not, of course, contain this reference to Prout.

124. A. Taylor, Guy’s Hospital Medical Reports, 13(=6), 141-8, 1849.

summary of Prout's paper.

Den eigentlich chemischen Theil dieser versuche halte ich für eine der wichtigsten untersuchungen in der Tierchemie, welche in verflossenen Jahre zu unseren Kenntniss gelangt sind, und glaube, sie daher hier nicht übergehen zu dürfen. (126)

This paper on the egg was a pioneering programme of research in chemical embryology; but despite Berzelius's words its fate was for it to be forgotten in the advent of superior analytical techniques, and the development of colloid chemistry.

By 1822, Prout was so completely in command of his material that (having just completed his treatise on urinary diseases) he was able to plan and write a book on digestion: Observations on the Functions of the Digestive Organs, especially those of the Stomach and Liver; with practical Remarks on the Treatment of some of the Diseases to which these Organs are liable. A prospectus published in Thomson's Annals in February 1823 announced that the book would:

comprise the results of an experimental inquiry into the nature of some of the more important chemical changes which take place during the digestion and assimilation of the food. The practical remarks will principally relate to the proper adjustment and use of remedies, and to the pernicious effects liable to be produced in delicate habits by the constant operations of various slowly acting causes, especially impure or hard waters: illustrated by analyses of the principal waters in common use in the metropolis and its vicinity. (127)

Although this book was actually in press, it was never published; however, much of its intended contents undoubtedly found their way

126. Jahres Bericht, 4, 239-45, 1825. ("I believe that the purely chemical part of this research has been one of the most important inquiries in animal chemistry which have come to my notice during the past year; it ought not to be ignored.")

into the Bridgewater Treatise of 1634, and the enlarged edition of
his urinary textbook of 1640. John Elliotson and Herbert Mayo both
quoted from the unpublished book. (128)

The explanation given later by Prout for the non-publication
of this work was that his discovery of hydrochloric acid in the
gastric juice during the autumn of 1623 so thoroughly disorganised
his assembled material and ideas on the subject of stomach digestion
that he felt obliged to withdraw the book. That this drastic action
was necessary appears surprising in view of the small extent to
which hydrochloric acid figured in his later theory of digestion, and
it may be wondered why the book could not have appeared, suitably
modified, the year after the discovery. The answer is clearly suggest­
ed by the anonymous writer of the Edinburgh Medical and Surgical
Journal's obituary notice of Prout who gave three different, and
therefore additional reasons for non-publication. (129)

1. The first edition of Prout's urine textbook had proved extremely
successful. It had the distinction of being one of the first
English medical treatises of the nineteenth-century which merited
translation into French. The publisher's stock was exhausted and
a new edition was demanded. This second edition, with several
modifications from the first edition, appeared in June 1625.

128. J. F. Blumenbach, Elements of Physiology, trans. J. Elliotson,
4th ed., London, 1625, pp. 310-2. The same passage was copied
from Elliotson by H. Mayo, Outlines of Physiology, 3rd ed.,

2. As a direct result of the publication of the urine textbook, and his growing fame as a urinary specialist, his medical practice increased considerably. Consequently, the time for his own research and writing was drastically decreased. When Berzelius politely inquired of his friend Marcat for news of London chemists, including Prout, Marcat replied significantly: "Prout est bon, mais il est malheureusement médecin, et ses recherches sont trop interrompues."(130)

3. Such little private time for research as he had at the end of 1823 was occupied by the analyses of wines for his Edinburgh friend, Dr. Alexander Henderson.(131)

The letter's History of Wines Ancient and Modern, well-known to wine-bibbers and book-collectors, appeared in 1824.(132) Henderson acknowledged Prout's help in several places in the book, and his analyses of some eighteen wines were published as an appendix.(133)

133. E.g. op.cit., p. 323, gypsum when used in the fermentation of must, remained suspended in the wine, as shown by Prout's analysis of a 40 year old Pazarote.
   p. 342, a simple chemical test to demonstrate the adulteration of wines by colouring matter.
   p. 359, Prout had provided Henderson with a concise account of must fermentation adapted from Ure's Nicholson's Chemical Dictionary.
Appendix II, p. 361, Prout "who obligingly undertook them at my request" used slow distillation until about 2/3 of a known quantity of wine had lost its spirit to a receiver. The quantity and specific gravity of the distillate were then compared with Gilpin's Tables (Phil.Trans., 1794, p. 382).
These three reasons, coupled with the discovery of hydrochloric acid in gastric juice, would undoubtedly have led to the postponement and abandonment of the book on digestion. And if the book was initially only postponed, a decisive factor for abandoning it would have been the publication by Tiedemann and Gmelin of their excellent study of digestion in 1826.\(^{(112)}\)

Prout's discovery of hydrochloric acid in gastric juice was announced to the Royal Society on 11 December 1823. Now recognised as "a classic of scientific reasoning", the published account in the Philosophical Transactions of 1824 was extremely terse,\(^{(134)}\) so that whereas most chemists and physiologists appear to have agreed with Prout's identification -especially after the spectacular confirmation from Beaumont in America- there were a few who were prepared to argue over the validity of Prout's analytical deductions. Immediate challenges came from Leurest and Lassaigne in France, who claimed to have found free lactic acid in the stomach, and from Tiedemann and Gmelin in Germany, who had detected free acetic, butyric and hydrochloric acids. Both parties were engaged on the problems of digestion at the same time as Prout, and the resulting controversy, which has been altogether ignored by historians of medicine, enables a more detailed account of Prout's discovery to be made than is possible from the short account he gave to the Royal Society.

As we have seen, Prout had been intermittently engaged in the chemistry of digestion since at least 1816. Between 1816 and 1819 his uncertain opinion had been that the active acid of the gastric juice

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\(^{(134)}\) Phil.Trans., 1824, 45-9. It is only 5 pp. in length, well below the length of his other papers in this journal.
was the phosphoric, mixed perhaps with some carbonic acid. But by 1820, when he actually identified free hydrochloric acid in the gastric fluid of a dyspeptic patient, (135) he had been conditioned by reading Berzelius into believing that the active principle was lactic acid. Any hydrochloric acid which he found he dismissed as pathological, or as due to his own bad analysis. It seems very likely that he intended to present in his book on digestion some sort of analytical proof that the acid was lactic acid and no other. Consequently, he wrote later, my "inquiry was conducted in a much more rigorous and elaborate manner than it probably otherwise would have been." Eventually, "after a series of the most complete evidence that perhaps was ever brought to bear on a chemical point, I was obliged to conclude, is [sig, in] opposition to my preconceived notion, that the acid was the muriatic and no other." (136)

Now the misunderstanding between Proct, Tiedemann and Gmelin, and to a lesser extent with Leuret and Lassaigne, arose because Proct did not publish any account of his preliminary experiments or mention his previous commitment to lactic acid; but instead, in the manner of a mathematician offering a proof of a theorem, he offered a demonstration to his readers that the acid was hydrochloric acid, and a value for the quantity of free hydrochloric acid present in gastric juice. Proct's methodology was here very sound, and he showed a keen sense for the principles of chemical analysis:

135. Phil.Nag., (2)4,121a,1828.
136. Ibid., p.120.
The mere determination of the existence of a principle in any compound, without its quantity being at the same time ascertained, is often unsatisfactory; at least the determination of the latter point corroborates the former in no small degree; for before the quantity of a substance can be ascertained, it must be obtained per se, or in some well-determined state of combination, circumstances necessarily implying a much more complete and satisfactory investigation than that by mere tests alone. (136)

He had spent much time devising a method for the characterisation of gastric juice which would satisfy both these criteria, and he had been able to describe his results to the Royal Society economically, without detailing his many starts and failures. (137) To his objectors he replied:

If it be objected that these preliminary experiments ought to have been given, I can only say that I did not at that time think this necessary, nor do I now. The muriatic acid was not a new substance, nor one difficult to be identified, besides, such a preliminary inquiry seemed to be sufficiently indicated by the method proposed; for who would ever think of proposing a formal method of analysis, involving the quantity of substances, without determining beforehand what those substances were? Further, my paper was intended to be little more than a simple announcement of an important fact which, before it could be established, I well knew must be corroborated by other experience than mine; and lastly, something must be ascribed to a sort of innate antipathy to long-winded dissertation which is too apt to cause me to err on the side of brevity. (136)

For his demonstration that the free acid of gastric juice was hydrochloric, and that the salts present in it were alkaline chlorides, Prout digested the contents of a rabbit’s stomach in distilled water, and divided the resultant clear liquid into four parts. The first portion was used to determine the quantity of hydrochloric acid (i.e.

137. E.g. he found that distillation of gastric fluid (a method used later by Children and Tiedemann and Gmelin) only proved the presence of hydrochloric acid, and could not be used to determine its quantity.
chloride) present in the fixed alkaline chlorides, by precipitation with silver nitrate (1). Since the solution was previously evaporated to dryness, the volatile ammonium chloride and free hydrogen chloride were determined separately as follows. A second portion was supersaturated with potash to neutralise the free hydrochloric acid, and the total quantity of chlorides, including ammonium chloride, were found by precipitating with silver nitrate (2). In their treatise on digestion published in 1825, Leuret and lassaigne voiced their disagreement with Prout, even though they had repeated his experiments as described. They challenged his accuracy over this one analytical step: neutralisation with excess potassium hydroxide would (they suggested) have led to many side products; pure silver chloride would not have been precipitated. Prout's calculation for free hydrochloric acid was therefore invalid. In reply Prout was quick to point out that "the merest tyro" would have obviated this defect, and he retorted sarcastically "I wish these gentlemen to know (what every chemist might have taken for granted when it was stated that the experiments 'were made in the usual manner', that the excess of potash was always supersaturated with nitric acid before the nitrate of silver was employed."

138. The two Frenchmen favoured the presence of lactic acid, which they claimed to have detected. Although they also found hydrochloric acid, they believed this to be a side-product of digestion. See F. Leuret et F.L. Lassaigne, Recherches physiologiques et chimiques pour servir à l'histoire de la Digestion, Paris, 1825, pp. 112-6, "Nous avons obtenu une suite de résultats à peu-près d'accord avec ceux qui avaient été annoncés [by Prout], mais que ne peuvent nous permettre d'en déduire, comme on le verra plus bas, l'existence de l'acide muriatique libre". (p. 116) They mis-spelled Prout's name as Proust, but apologised in an Errata, p. 223.
The third portion was used to determine directly the quantity of free acid by volumetric analysis using a solution of potash of known strength (3). The combination of these results (1, 2, 3) determined the amount of chlorine that was combined with ammonia. (Result of portion (2) less sum of (1) and (3).) This last result was independently checked by evaporating to dryness the neutralised third solution in order to volatilise the ammonium chloride. The amount of chloride combined with fixed alkalies was then determined with silver nitrate (3A). (Quantity of ammonium chloride equals portion (2) less (3A).) In a later paper Prout stated that he had omitted to mention at that point in his original account that the solute remained neutral after the evaporation. This, he argued in 1828, would not have not been the case if any organic combustible acid had also been present in gastric juice, for then the carbon dioxide released would have produced some acidity.

The purpose of the fourth portion of gastric fluid was in fact to test for the presence of other mineral acids, particularly sulphuric or phosphoric acids; he was unable to detect their presence.

Prout's analyses demonstrated that there was a considerable quantity of unsaturated hydrochloric acid present in the stomachs of rabbits, horses, calves and dogs, during digestion. The examination of the fluids ejected by dyspeptics confirmed its presence in human gastric juice. No ammonium chloride was found in the human fluid, except in the case of a dyspeptic who had found solice in drinking an ammonium potion!
### Analysis of Gastric Juice

1. **Animal gastric juice**
   - Chloride combined with fixed alkali (1) grains: 0.12 0.95 1.71
   - Chloride combined with ammonia (2) - [(3) + (1)]
   - Free hydrochloric acid (3) grains: 1.59 2.22 2.72

   Between 40 and 56% of the total chloride was hydrochloric acid.

2. **Human gastric juice**
   - Between 23 and 28% of the total chloride was hydrochloric acid.

   In one pint of human gastric juice:
   - Chloride combined with fixed alkali: 12.11 12.40 11.25
   - Chloride combined with ammonia: - - 5.39
   - Free hydrochloric acid: 17.24 17.03 20.92

   *(M) dyspeptic who drank ammonia*

The presence of free hydrochloric acid in the stomach was quickly confirmed by J.G. Children, the Secretary of the Royal Society, in 1824, from the observation of a dyspeptic and sceptical friend. (139)

It was also confirmed independently by Tiedemann and Gmelin in February 1824. However, the German physiologist and chemist, who together were more than Prout's equal in the field of the chemistry of digestion, were not satisfied that hydrochloric acid was the only acid present in gastric juice. In 1823 the French Academy of Sciences had proposed as a topic for an essay competition, the nature of digestion in

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different classes of animals. None of the candidates entirely satisfied the Academy, though honourable mention was made of Leuret and Lassaigne, and Tiedemann and Gmelin who "have made a great number of experiments, and have attained remarkable results. For this reason, and in consideration of the expensive nature of the researches in which they engaged, the Academy have adjudged to each [party] the sum of 1500 francs." Leuret and Lassaigne accepted, but the Germans were offended by the Academy's offer with its implication that their work was no better than the Frenchmen's; they therefore published their work independently in German.\(^\text{140}\) The whole matter was an academic scandal—especially after the publication of the respective researches revealed their comparative merits to a wider circle. Prout was quite clear about the respective merits of the French and German parties: "[I have a] high opinion of MM. Tiedemann and Gmelin's volume. Having gone over most of the ground traversed by these gentlemen, I am well aware of the labour and difficulty of the march; and though we may differ in some minor particulars, which is not to be wondered at, I am satisfied, as far as we go together, with the general accuracy of their observations. With respect to MM. Leuret and Lassaigne's book, I am sorry that I cannot express the same sentiments; indeed as a work it does not appear to me to be at all comparable with that of the German philosophers."\(^\text{141}\)

\(^{140}\) Cf. the remarks of Tiedemann and Gmelin in the Preface to the French ed. of their book; and their Appendix which lists numerous details in which they (correctly) differed from Leuret and Lassaigne. (Title at ref. 112)

\(^{141}\) This was also Eliotson's opinion; J.F. Blumenbach, *Elements of Physiology*, 4th ed., 1820, p. 325.
The German investigators had admonished Prout for seeming to deny the presence of any other acid in gastric juice; for they had detected both acetic and butyric acids in the stomach as well as hydrochloric acid. But this was a misunderstanding caused by the original terseness of Prout's paper to the Royal Society in which he had only been concerned to demonstrate the presence of hydrochloric acid, and not necessarily to deny the occasional presence of other acids. By 1828 Prout was prepared to concede to Tiedemann and Gmelin that other organic acids were sometimes present, "though I confess I believed, and do still, that the muriatic acid occurs naturally more frequently, and in greater abundance in that organ than any other acid." It was probable, he reasoned, that other acids were derived from the food rather than by the stomach itself, but he did not suggest how this might be tested. (142)

The Germans replied that Prout's original words had led them to their interpretation, (143) and ignoring Prout's reply concerning his methodological procedure, they wondered how his determination of the quantity of hydrochloric acid was to be interpreted unless he had believed all other acids were absent. For, they reasoned, if acetic acid had been present it would have also been neutralised by potash, and consequently the free hydrochloric acid would have been over-estimated. If Prout claimed accuracy and planning for his method, then

142. Phil.Mag., (2)4, 123, 1828.

143. Prout had said: "The experiments ... seemed to preclude the possibility of the presence of any destructible acid, and the only known fixed acids likely to be present were the sulphuric and phosphoric", Phil.Trans., 1824, p. 47. Note that Prout said nothing of any organic acids which might be produced from food.
he must have implicitly claimed the absence of acetic acid. And yet now, Prout admitted that he had found acetic acid in gastric juice. "It follows in consequence, that his method of determining the quantity of free muriatic acid in the stomach cannot be relied upon." (144) The chemical arguments for these analytical criticisms are summarised in a Table.

Although the criticisms are reasonable ones, as Prout remarked, they gave little credit to his chemical acumen.

Messrs Tiedemann and Gmelin will, I trust, give me credit when I assert that I was perfectly aware of all the chemical objections they have raised, and many more to the same effect; and never should have thought of applying the method in question in a new case when the nature of the acid was unknown, and particularly in the case of a destructible acid in conjunction with the muriate of ammonia. (145)

Since in the particular samples of the gastric juice which he had analysed in 1823 no organic acids had been detected, his method was perfectly correct. If other acids were found to be present in other samples, his analyst's experience would inevitably lead him to a modification of the quantitative method in use in the estimation of hydrochloric acid. Our judgement must be that Prout had never intended to deny the occasional presence of other acids in the stomach, it being understood by him subsequently that such acids were derived from foodstuffs or, as he added in 1828, derived from derangement of the stomach and the processes of assimilation. (146)


145. Phil.Mag., (2)4,121n,1828.

146. Ibid., p.123. Food which contained acetates might produce acetic acid in the presence of hydrochloric acid. The presence of organic acids in the stomach caused great confusion in the confirmation of Prout's work.
SUMMARY OF CHEMICAL CRITICISMS

A. Prout's three analyses assuming hydrochloric acid present, but acetic acid absent.

1. Estimate fixed alkali chlorides.

\[ \text{NaCl, NH}_4\text{Cl, HCl } \xrightarrow{ \text{heat} } \text{NaCl} + \text{NH}_3 \uparrow + \text{HCl} \uparrow \]

estimate free NaCl with AgNO_3 \(\rightarrow\) ppt. AgCl

2. Estimate total chloride.

\[ \text{NaCl, NH}_4\text{Cl, HCl } + \text{KOH } \xrightarrow{ \text{heat} } \text{NaCl} + \text{KCl} + \text{NH}_3 \uparrow + \text{H}_2\text{O} \uparrow \]

solution stays neutral

\[ (\text{KOH } + \text{NH}_4\text{Cl } \rightarrow \text{NH}_3 \uparrow + \text{H}_2\text{O} + \text{HCl} ) \]
\[ (\text{KOH } + \text{HCl } \rightarrow \text{KCl } + \text{H}_2\text{O} ) \]

estimate NaCl, KCl with AgNO_3 \(\rightarrow\) ppt. AgCl

3. Estimate free HCl with KOH \(\rightarrow\) KCl + \text{H}_2\text{O}

B. Tiedemann and Gmelin's objection; if acetic acid is present in reaction 2, then

\[ \text{HAc } + \text{KOH } \rightarrow \text{KAc } + \text{H}_2\text{O} \]
\[ \text{NH}_4\text{Cl } + \text{KAc } \rightarrow \text{KCl } + \text{NH}_4\text{Ac} \]

the nett effect is that less, or even no \text{NH}_4\text{Cl} will sublime.

in reaction 3,

\[ \text{HCl } + \text{HAc } + \text{KOH } \rightarrow \text{KAc } + \text{KCl} \]

where the effect is that more KOH is used, so leading to the over-estimation of HCl.

Hence

(i) hydrochloric acid is over-estimated.

(ii) chloride combined with ammonia is under-estimated, or none is found at all (as in Prout's human gastric juice.)
Tiedemann and Gmelin made no further reply and doubtless conceded victory to Prout on this as on other points of their criticism. The whole dispute was carried out with great courtesy and mutual admiration—in striking contrast to Prout's angry dispute with Wilson Philip a few years later. (Chapter 1).

Strong support for Prout's discovery of hydrochloric acid in the stomach came from the observations of William Beaumont in America; his work, however, was never cited by Prout. In 1835, Braconnet confirmed the presence of hydrochloric acid and the absence of lactic acid in the gastric juice of a dog. Nevertheless, a persistent group of sceptics, who included the young physiologist Claude Bernard, continued to suggest that the only free acid was lactic acid and that hydrochloric acid was only produced by interaction with food chlorides—the very process by which Prout believed lactic and acetic acids were principally formed. Sometimes these critics showed a surprising lack of analytical skill; one obstinate opponent was Robert D. Thomson, Lecturer in Chemistry at Glasgow before becoming Lecturer at St. Thomas's Hospital in London, and a nephew of Thomas Thomson. He attacked Prout, and Tiedemann and Gmelin before the British Association in 1839, and in several lengthy articles. Mildly, Prout noticed in his clinical textbook that Thomson's methods were not to be relied

147. W. Beaumont, Experiments and Observations on the Gastric Juice, 1833 (Dover reprint 1959). Analyses of gastric juice which confirmed the presence of HCl were made for Beaumont (who was not a chemist) by Dunglison of Virginia Univ., and Benjamin Silliman of Yale. Berzelius, to whom a sample was sent, was unable to record an analysis. Beaumont was not familiar with Prout's work (only mention, p. 82).

on. Kasich has noted the way in which contemporary textbooks were usually quite undogmatic; if they did not explicitly deny the presence of hydrochloric acid, or deny that it was the active acid of the gastric juice, they always listed it with other organic acids.Only after 1852, with the publication of the "classic monograph" of Bidder and Schmidt, was there little doubt that hydrochloric acid, which was naturally produced from the glandular cells of the gastric mucosa, was the only free acid.

Yet it seems possible that Prout deferred to the criticism of 'over-generalisation' with which his study was greeted, for to a large extent he played down the presence of hydrochloric acid in the stomach in his later writings. Although he had said that his discovery "was one of those leading facts that open up an entire new field of inquiry," he usually considered chlorine as the active principle of stomach digestion even after the isolation of pepsin by Schwann in 1836.

149. A.H. Kasich, op.cit., ref. 103. R.D. Thomson, Brit. Ass. Reports, 3, 58, 1839. He evaporated gastric juice in open vessels, and naturally found no HCl in the residues. Later he fell into the error of Leuret and Lassaigne, Phil. Mag., (3)26, 416-20, 1845. Even a good analyst like Bence Jones was hesitant: "The gastric juice is a highly acid fluid. What acid it is has not been determined. Hydrochloric, phosphoric, acetic, lactic, and butyric acids have each been said to exist in the gastric juice", Lancet, 1850, i, p. 415. The final irony is surely Prout's editor, J.W. Griffith, who footnoted that the free acid was more likely lactic acid than hydrochloric acid. See Prout, Chemistry, 4th ed., 1855, p. 345.


151. Phil. Mag., (2)4, 123, 1828.

152. Annalen der Physik u. Chemie, 38, 358-68, 1836. Schwann recognised that hydrochloric acid was necessary for the activation of pepsin.

* Prout's Bridgewater Treatise is cited by edition as "Prout, Chemisty."
Chlorine and hydrochloric acid were, he suggested, produced from
the sodium chloride of the blood by galvanic action. The basic oxide
released produced the alkalinity of the blood, and after its excretion
from the bile, reformed sodium chloride in the duodenum.

We have in the principal digestive organs, a kind of galvanic
apparatus, of which the mucous membrane of the stomach, and
perhaps that of the intestinal canal generally, may be considered as the acid or positive pole; while the hepatic
system may, on the same view, be considered as the alkaline
or negative pole. (153)

This matter will be discussed in greater detail in Chapter 8.

The Copley Medal winning paper of 1827, "On the Ultimate Analysis
of Simple Alimentary Substances", was the last of Prout's papers on
organic analysis, and it was an introduction to the theory of digestion
which he explained at greater length in the Bridgewater Treatise in
1834. The paper was announced as the first of a series of three essays
in which he would in turn discuss the composition and assimilation of
the three foodstuffs which he had classified as the saccharinous (the
carbohydrates), the oleaginous or oily (the fats), and the albuminous
(the proteins). Prout had evidently drawn up this classification for
his unpublished book on digestion in 1823.

In the fourth edition of his influential translation of Blumen-
bech's Physiology, John Elliotson gave a long quotation from his "not
less excellent than distinguished friend's" book which, since it
explained the nutritional origin of Prout's classification in detail,

153. Prout, Chemistry, 1834, pp.496-7 and On Stomach, 5th ed., 1848,
pp.476-2. In the latter Prout spoke of a "modified electricity",
"for we do not believe that electricity, precisely as we are
acquainted with it, and as existing in inanimate nature, can be
thus employed." See also, H.F. Baxter, Phil.Trans., 1843, p.243.
and was the source for continental knowledge of Prout's ideas, will bear extensive quotation.

Observing that milk, the only article actually furnished and intended by nature as food, was essentially composed of three ingredients, viz. saccharine, oily, and curdy, or albuminous matter, I was by degrees led to the conclusion that all the alimentary matters employed by man and the more perfect animals, might, in fact, be reduced to the same three general heads; hence I determined to submit them to a rigorous examination in the first place, and ascertain, if possible, their general relations and analogies. ... The characteristic property of saccharine bodies is that they are composed simply of carbon united to oxygen and hydrogen in the proportions in which they form water; the proportions of carbon varying in different instances from about 30 to 50 per cent. The other two families consist of compound bases (of which carbon constitutes the chief element) likewise mixed with and modified by water, and the proportion of carbon in oily bodies, which stand at the extreme of the scale in this respect, varies from about 60 to 80 per cent; hence, considering carbon as indicating the degree of nutrition, which, in some respects may be fairly done, the oils may be regarded in general as the most nutritious class of bodies; and the general conclusion from the whole is, that substances naturally containing less than 30 or more than 50 per cent of carbon are not well, if at all, adapted for aliment.

Man tried to copy Nature's great aliment in his own cookery.

He dissatisfied with the productions spontaneously furnished by nature, calls from every source, and, by the power of his reason, or, rather, his instinct, forms in every possible manner, and under every disguise, the same great alimentary compound. This, after all his cooking and art, how much soever he may be inclined to disbelieve it, is the sole object of his labour, and the more nearly his results approach to this, the more nearly they approach perfection. Thus, from the earliest times, instinct has taught him to add oil or butter to farinaceous substances, such as bread, and which are naturally defective in this principle. The same instinct has taught him to fatten animals, with the view of procuring the oleaginous in conjunction with the albuminous principle, which he finally consumes, for the most part in conjunction with saccharine matter, in the form of bread or vegetables. Even in the utmost refinements of his luxury and in his choicest delicacies, the same great principle is attended to, and his sugar and flour,
his eggs and butter, in all their various forms and combinations, are nothing more nor less than disguised imitations of the great alimentary prototype milk, as presented to him by nature. (154)

This whole passage illustrates Prout's considerable powers as a dietician. Yet as a chemist, he never found the time to analyse all three classes of aliment, (155) and only one paper on saccharine foods was ever published in 1827. (156) Confusion had arisen in the analyses of sugars, he suggested in this paper, because there were several kinds and series of sugars, ranging from the strong and perfect cane sugar, to the low and poorly-defined sugar of honey (glucose). He published a large number of analyses of sugars and vegetable acids in this paper.

In an attempt to explain the organization of aliments into the animal body, Prout borrowed an idea from the discovery by John Herschel in 1824 of the effects of minute portions of 'impurity' on the electrical states of substances. (157) Prout suggested that in order for a substance to pass from a crystalline inorganic state to an amorphous


The passage is followed closely by Prout in his Chemistry, 1834, pp.477-80. In his second Gulstonian lecture in which he also mentioned milk as the alimentary prototype, he cited Eliotson-Blumenbach without revealing that it was a self-reference, Med. Gazette, 8,324,1830-1. See supra, ref.126.

155. In other contexts he wrote of four aliments, adding water. Later Jonathan Pereira added eight others in his Treatise of Food and Diet, 1843.


organic state, the presence and admixture of "foreign or extraneous parts" was absolutely necessary. This conception obviously dates from Prout's work on the sugars in 1817-8. (Chapter 3) These foreign bodies, which were elements other than the traditional organic elements of carbon, hydrogen, oxygen and nitrogen, or the addition or subtraction of water, \(^{(158)}\) were not present in definite proportions, but equally diffused through the substance which otherwise retained its composition. Any substance so affected, or merorganized (the term, protorganized appears in the abstract), \(^{(159)}\) not only lost its power of crystallisation, but it also completely altered its chemical properties; in the language of Berzelius, it had isomerised.

By these incidental matters ... the ordinary chemical properties of the essential elements of the organized living structure are variously modified, in particular, that the essential elements are hindered from assuming a regular crystallized form.

For example, starch, Prout suggested, should be considered as merorganized sugar; their analyses were very similar, but their properties and forms very different. In fact starch is a chain polymer of repeat-glucose units, and in this sense, it may be viewed as glucose or sucrose.

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158. "Any substance may be supposed capable of performing the part of a merorganizing body; but ... water appears to constitute the first and chief, at least in organized substances", Phil.Trans., 1827, p. 375.

159. Proc.Roy.Soc., 2, 324, 1815-30. Also Prout, letter to Herschel, undated, "I enclose the abstract and drawing belonging to my paper..." (Roy.Soc. Herschel Letters, HS. 14, 192). It seems that Prout wrote his own abstract and for some reason used the word protorganized instead of merorganized which he says was suggested by [Rev. Francis?] Lunn, Phil.Trans., 1827, p. 375.
molecules held together by the elements of water. Prout remarked on the ease with which merorganized substances lost any water not essential to their unit of composition when heated below 212° F.

Although Charles Daubeny thought that merorganization was an attractive way of explaining some kinds of isomerism, no other chemist or physiologist adopted the word, and after 1831 Prout abandoned the word, but not the concept. (See Chapter 8)

Although it was essentially the recognition of both the necessity for trace elements in the chemistry of organization, and a realization of the tremendous importance of water in the chemistry of living processes, Prout fell a victim to the complication, or unwise depiction of these substances as "foreign" or "extraneous". Other chemists came gradually to recognise their importance in animal economy; but they recognised that it was simpler not to view them as "impurities", but as substances which entered into composition in small, but nevertheless definite, proportions.

The Copley paper "On Ultimate Analysis" effectively linked together for the first time Prout's preoccupations with accurate analysis and animal chemistry, and (in view of his suppressed book) anticipated the fuller public expression of his ideas on digestion which he

160. C. Daubeny, Introduction to the Atomic Theory, Oxford, 1831, pp. 79-82. He noted the similarity to the medical doctrine of homoeopathy, and thought merorganization might explain the many varieties of mineral waters. Prout agreed with him in his letter of 1831, quoted ibid., p. 132.

161. A.W. Philip thought the word "particularly objectionable", Med. Gazette, 8, 77, 1831-2. Note also the "merorganized lucubrations of Dr. Prout", Dublin Journal of Medical Science, 1, 66, 1832. The words merorganic, anorganic and teleorganic were used by G.H. Lewis, Comte's Philosophy of the Sciences, 1853, p. 150.
published in 1834. (Chapter 8). The paper, apart from the
speculation concerning merorganized substances, was firmly grounded
in experiment and gave the chemical world details of a new method of
analysis (Chapter 2), a new classification of nutrients, and accurate
analyses of many saccharinous materials. Prout had obviously come a
long way from the undergraduate extravagant analogies of De Facultate
Sentendi. Since then he had established himself as an expert on
urinary and digestive complaints, as an accurate and very conscientious
analyst, and as a chemist who was especially interested in metabolism.
His two papers on respiration had been pioneering attempts to achieve
precision in the subject of the living body to quantitative analysis.
The difficulties which attended this, and the fact that his work was
done only on a limited scale and unrelated by him to digestion and
excretion, diminished the value of his work which in any case could
not be properly understood until the intricacies of cellular chemistry
began to be unravelled towards the end of the century. Again, his
work on urine and digestion between 1814 and 1827 provided him with
a firm experimental foundation upon which to erect a theory of meta-
bolism. His research on the egg had been an entirely original study
of the actual chemical, as opposed to anatomical, changes which took
place during incubation. Again, this pioneering attempt to apply
chemistry to the living body was filled with difficulties of inter-
pretation that could not be properly made until after the development
of physical chemistry. Finally, Prout’s identification of hydrochloric
acid in the gastric juice was a sensational discovery, made with great
analytical skill, which solved a very old problem.
Yet throughout these experimental studies in chemical physiology
Prout was not content to merely publish practical results. Speculat-
ive hints of a very general kind were continually dropped by him: the
sun and the cycle of the seasons directly influenced the rhythm of
living processes (1814); elementary analyses revealed a numerological
pattern which might be the key to the understanding of the mutual
relationships and transformations in the living body (1817-8); the
possibility that the living body could under exceptional circumstances
synthesize certain elements (1816, 1822); the electrical nature of
some of the basic chemical processes performed in the living animal
(1823); and finally, the idea that traces of elements mixed with in-
organic or organic substances might be the sole difference between
them and organized substances (1817 and 1827). How these general ideas
fitted in with Prout's unitary and molecular theory of matter will be
explained in the final two chapters of this thesis. Meanwhile, there
is one other aspect of Prout's experimental interests which has to
be considered: barometry; for this too, when closely examined, will
be found to have bearing on his theoretical interests.
Chapter Five: Barometry

At some time prior to 1830, Prout designed and constructed an expensive and accurate barometer which was described by the physicist and meteorologist James Forbes, as "one of the finest philosophical instruments I have ever had the pleasure of seeing." (1) Prout's son, the Rev. Thomas Jones Prout, presented this barometer to the Oxford Museum in 1860, but unfortunately it can no longer be found there. (2) In 1831, the newly-established Meteorological Committee of the Royal Society recommended to Council that an improved Standard Barometer should be built for the use of the Royal Society. The Council duly resolved in December of that year "that Dr. Prout and Mr. Daniell be requested to superintend the completion of the Standard Barometer, with a view to its being put up in the Library of the Society, and that the Secretary Peter Roget write to inform these gentlemen of their request." (3) Daniell was well-known as a meteorologist, but the choice of Prout can only be ascribed to the knowledge that he had recently constructed a new barometer.


After consultation with Prout, Daniell replied to Council as follows.

Dr. Prout has requested me to state our joint opinion, that, as the instrument which we propose to erect will not be moveable, and must be completed upon the spot where it is ultimately to remain, it will be advisable to wait till the Council shall have come to some ultimate determination with regard to the meteorological instruments in Council, and the place in which it may be advisable to make the Observations. We are strongly of the opinion that no place at present connected with the Library of the Society can be properly made available to the purpose..." (4)

Prout and Daniell assigned the task of building the new barometer to John Frederick Newman, "a superlative workman with a flair for designing barometers" (5), and the instrument-maker who had built Daniell's previous Standard Barometer in 1821 (6) and Prout's own instrument. A few years later the amateur astronomer Francis Baily was added to the team in order "to superintend the graduation of the scale of the barometer." (7)

The instrument, which was completed in 1836, was really two separate barometers enclosed in a single cistern; the two independent tubes were made from flint and crown glass respectfully "with a view to ascertain whether, at the end of any given period, the one may have

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had any greater chemical effect on the mercury than the other, and thus affected the results."

(3) The scale, which was set off in tenths of an inch by Baily from a standard scale belonging to the Royal Astronomical Society, was attached to a brass rod which passed between the two tubes and ended in a fine agate point. A rack and pinion mechanism moved the rod and scale so that the point just touched the surface of the mercury when observed through an eyepiece let into the cistern. As with most Newman barometers, the thermometer was rather unwisely placed with its bulb in the mercury and so did not record the air temperature. (9) Prout filled the two tubes with mercury which had been elaborately purified by heating under a vacuum. (10)

The Royal Society's instrument may be compared with the very brief description of Prout's own barometer which he gave to the British Association in 1832. In the following quotation similar features are underlined.


10. Phil.Trans. 1837, p.432. The specific gravity was 13.581 at 62°F. and 30 inches pressure. In his description of the barometer, Baily curiously only credits Prout for estimating the specific gravity of mercury and for filling the tubes.
The internal diameter of the tube is .573 inch, and it is
guarded at [the] bottom by platina, in the manner recommended
by Mr. Daniell. The distance between the upper and lower
surfaces of the mercury is determined by a brass rod ending
at the bottom in a fine point, and having the usual scale
affixed to the top. The rod is moveable by a screw, so that
the lower point can always be brought in contact with the
surface of the mercury in the cistern, while the scale at the
upper end of the rod marks, with the utmost precision, the
exact distance between the upper and lower mercurial surfaces,
without the necessity of further adjustment or calculation...

It will be noticed that Prout's scale was set off in 1/1000 of an
inch and not 1/10 of an inch as in the Royal Society's instrument.*
Prout stated that he had used an old standard length "formerly
belonging to Mr. Cavendish" to calibrate his scale. It is known
that Cavendish's apparatus passed to John Newman. (12) The upper
part of Prout's instrument, therefore, was common to the large
moveable scale barometer of Newman as exemplified by the Royal
Society's instrument. (13) Dr. Knowles Middleton informs me,
however, that it is unlikely that Prout's cistern was exactly the
same as Newman's simple iron cistern which was only described in
1833 and almost certainly used in the Royal Society's barometer. (14)
On the other hand, it seems possible to me that Prout's barometer,
which was built by Newman, could have been the prototype of the iron

12. G. Wilson, Life of the Honourable Henry Cavendish, London,
1851, p. 475.
cistern instrument. In fact, since both instruments have disappeared, their resemblances and differences are largely conjectural. (15)

On the completion of the Royal Society's barometer, Newman's bill evidently shocked the Council for they referred it for comment to Prout who clearly thought sufficiently highly of Newman's skill to reply,

I find from Newman's books that about £10s was received by Mr. N. last year [1835] on account of the R.S. Barometer. This sum was for the whole of the work then completed, including, as I understand, the brass frame and all the work, in short, not charged in the present bill. In the present bill £19-10- was charged on account of the Barometer; the rest of the charges are for various articles, some of which, as you will see by the bill, were obtained so long ago as 1832. The payment of this portion of his present bill was put off, Mr. N. informed me, last year by desire of Mr. Lubbock... On the whole, I confess, I do not think the charges unreasonable, considering the length of time the instrument has been on his hands. The changes (I hope improvements) that have been made from time to time; and the great care that has been taken to render it as complete as possible is [quite??] superb.

For my own part, it has always given me the greatest pleasure to render every assistance in my power; and [I hope??] the instrument, now completed, will be worthy of the R.S." (16)

From published financial statements the total cost of this barometer appears to have been nearly £70. It seems likely that Prout's instrument was as expensive. As Munk noted, following an anonymous obituary of Prout, "in pursuing his scientific investigations, and especially those on the atmosphere, expense was not regarded by

15. The Royal Society's barometer was possibly destroyed when the society moved from Somerset House to Burlington House in 1856. A new standard barometer was built in 1854 and housed at Kew Gardens.

Dr. Prout, and much of his apparatus was of the most elaborate and costly character." (17)

It may be reasonably wondered why a doctor and organic chemist like Prout should want an elaborate barometer. Munk's statement is correct, for Prout's barometer was designed for a large-scale research into the density of air and the common gases. In 1827 he remarked how he had realised for some time that a source of error in his organic analyses was the value which he took for the density of air.

I have long suspected the perfect accuracy of this datum as settled fifty years ago by Sir G. Schuckburgh, and have been accustomed for some time past to make an allowance for it; but I was induced to undertake a series of experiments on the subject, which I hope shortly to lay before the public." (18)

The results of this comprehensive programme were not published until 1832, and then only in part. Clearly, if Prout's results were to be accurate, a careful consideration of instruments would have been an essential preliminary. The involved paper (19) on the specific gravity of air, and the expansion of air, which he read to the British Association at Oxford in 1832 indicates the great pains which he went to in order to ensure precision. In these experiments

ibid., 4,30,1837-43 (1837), "Newman for Barometer, £32-19-3".
The total is £65-6-3.

18. Sir George Shuckburgh (sic) Evelyn, Phil.Trans., 1777, p.513, obtained the value 30.5 grains per 100 cub.in. air. See also Nicholson's J., 4,35,1803. (Quotation from Phil.Trans., 1827,p.870).

made in 1831 and 1832, Prout used the new barometer (which he did not illustrate or describe in detail), the standard thermometer belonging to Daniell, specially calibrated weights from the instrument-maker, Robinson, and a special balance constructed by Robinson which was "mounted with a counterpoise of glass, as nearly as possible of the same size and weight as the balloon in which the air is weighed, by which all errors from buoyancy, &c, are completely obviated." This was probably the first time a buoyancy correction had been applied in physical measurements. (20) Obviously the concern for details like this took a good deal of planning; the daily experiments were not begun until May 1830 when atmospheric air freed from any carbon dioxide was weighed in both dry and moist states. (21)

Prout described his technique as follows.

The air to be weighed was first passed through lime water, into a large bell glass receiver, where it was permitted to remain for six or eight hours, with the view of separating the carbonic acid present. One portion of it was then introduced into a similar apparatus filled with the strongest

20. cf. W.A. Miller's remark, "Regnault, in his elaborate researches (Ann.de chim. (3)14,211,1845), has reduced the number of corrections ordinarily required, by counterbalancing the globe in which the gas is to be weighed by a second globe of equal size, made of the same glass; a practice which had been previously adopted by Prout, in his careful investigations on the density of the atmosphere", W.A. Miller, Elements of Chemistry, Part I (Chemical Physics), 5th.ed., 1872, p.281. (Noted by Partington, History, 4, p.228).

21. Expansion of air, May 1830-August 1831, nearly 1000 experiments; Weight of air at 32°F., 16 December-5 February 1832, about 87 experiments (since ice was necessary, this research could only be made in the winter months).
sulphuric acid [for dry air], while another portion was conveyed into a similar apparatus filled with distilled water. [for moist air] With these two fluids the different portions of air were permitted to remain in contact for at least twelve hours... A known quantity of air in each state, as determined by a very simple gasometer, was then introduced into the weighing balloon, and its weight carefully determined, with all the necessary precautions. In weighing air at 32° [F.], an apparatus on the same principle was employed, but so constructed, that the whole gasometer would be surrounded by ice for some hours before the air was weighed." (22)

His average result from 87 experiments for the density of dry air was very accurate: 100 cubic inches of air at 32°F., 30 inches pressure, London latitude, weighed 32.7958 (+ 0.0507) grains. (23)

But a strange event had occurred on 9 February 1832.

... on which day the weight of the air was 32.8218: and it is remarkable that after this period, during the whole time that the experiments were continued, the air almost uniformly possessed a weight above the usual standard; so that... the mean of the 42 observations after this crisis [32.8018], exceeds the 44 preceding it [32.7900, sic] by no less than 0.0118 grains. (24)

Prout immediately rebutted any suggestions that this was due to his carelessness.

The apparatus employed, and the care taken were the same throughout, and there can be no doubt that the difference, whatever it depends on, really existed, and did not arise from error of experiment." (24)


23. This value converts to 1.2962 grams per litre at 15.5°C., 76 cm. pressure. The present value is 1.2928 grams per litre at N.T.P. Compare Shuckburgh (1777), 30.5 grains per 100 cub.in. at 60°F., 30 inches pressure; and Regnault (1847), 32.58684 grains per 100 cub.in. at 60°F., 29.92 inches pressure.

It is difficult to see a reason for this anomaly, and as Berzelius remarked when he reported on Prout's results, if a lesser analyst had produced such an anomalous result, it would not have deserved any consideration. Surely we must agree with Berzelius that since Prout had taken such great pains over his technique, and because he had such a high reputation for accurate analysis, that the event in question must have happened. Naturally Prout, with his typical flare for generalisation, speculated whether the anomalous reading was in some way connected with the start of the cholera epidemic of 1832. If some heavy body were diffused through the atmosphere — perhaps the agent of the disease — this would account for the increased density of the air. Prout remained fond of this speculation since he repeated it in his Bridgewater Treatise and in the later editions of his clinical textbook. Daubeny thought it a fine example of Prout's "power of generalization" and recalled with what scientific caution he had proposed it.

25. Berzelius, Jahres Bericht, 13, 52, 1834.

26. See Chemistry, 351-3 and further discussion, infra, chapter 3. According to the Roy. Soc. Meteorological Journal, the wind changed direction from S.W. to N.N.W. between the 9th and 10th Feb. 1832. There was a light fog and rain on the morning of the 9th, and it was overcast and hazy on the 10th. There was no radical pressure variation, 30.389 to 30.395 inches.

Although he was understood to have continued the meteorological researches alluded to during the whole period of the cholera in 1832, he delayed their publication until they could be still further corroborated. Unfortunately, when the cholera broke out a second time in 1848, his health was too enfeebled to allow of his undertaking, in addition to a large medical practice, a similar course of laborious investigations, so as to satisfy his own scrupulous mind as to their truth." (28)

Oddly, the phenomenon was reinvestigated, and confirmed, by R. D. Thomson during the 1854 cholera season. (29) Prout also noticed fluctuations in density about the period of a new or full moon; "whether this arises from aerial tides has not been satisfactorily determined". Later in the Bridgewater Treatise he took it for granted that the moon could influence the weather.

In another train of argument Prout reasoned that there should be some variation in the density of air due to wind direction. Air which had travelled over the whole extent of London from the East to his house in Piccadilly (an east wind) would probably have had considerable quantities of oxygen removed from it and replenished by carbon dioxide. Since Prout removed carbon dioxide from the air before making his measurements, it followed that air from the east would "be necessarily found lighter". On the other hand, no such impoverishment should be found in a west wind arriving at London from the country. From his results, if the west wind contained a positive amount of oxygen, the differences between the weights of the airs were as follows: (30)


30. Brit.Ass.Reports, 1,575-4,1851-2(1852) Winds between S.W. and N.W., S.E. and S.W., N.E. and N.W. were classed as West, East, South and North winds respectively.
mean of 47 observations

<table>
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<th>Observation</th>
<th>Weight (grains of 100 cub.in.)</th>
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<tbody>
<tr>
<td>WEST wind</td>
<td>23</td>
<td>32.7964</td>
</tr>
<tr>
<td>&quot;</td>
<td>39</td>
<td>32.7944</td>
</tr>
<tr>
<td>&quot;</td>
<td>21</td>
<td>32.7943</td>
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**difference (West positive)**

<table>
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<th>Comparison</th>
<th>Difference</th>
<th>Weight</th>
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<tbody>
<tr>
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<td></td>
</tr>
<tr>
<td>South and North</td>
<td>0.0002</td>
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<tr>
<td>West and South</td>
<td>0.0023</td>
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<td>West and North</td>
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These differences, although small, were nevertheless significant, he claimed, for they confirmed that the density of air was affected by the quarter of the wind. This argument, although correct in principle, could hardly have been significantly demonstrated without further experiments and chemical tests, especially when the differences involved were no greater than the likely experimental errors. Prout was not unaware of errors.

It may be proper to observe, that many of the minute differences in the weights of the air at different times are more apparent than real, and depend upon the sluggishness of the mercurial barometer, which prevents it from being an exact measure of the movements of the lighter and more mobile fluids. (30)

In another series of experiments on the variation of air density...
with temperature, Prout showed that the mercury and air thermometers were not strictly complementary between 32° and 212°F.

"as at present generally supposed, but that there is a gradually increasing difference from 32° upwards, and amounting at 72° to upwards of $\frac{1}{5}$ths of a degree, the mercurial thermometer being in advance; (that is to say, 62° on the mercurial scale coincides almost exactly with $61\frac{1}{2}$° on the air scale)."

This significant observation that the Gay Lussac-Charles-Dalton law of expansion was not strictly accurate, was ignored by Prout's contemporaries, and no further study of the expansion of gases was made by a physicist until Regnault published his researches in 1847.

As a result of nearly a thousand determinations of air density between May 1830 and August 1831, Prout found that 100 cubic inches of air at 60° F., 30 inches pressure, weighed 31.0117 grains, and 32.7900 (sic) grains at 32° F. instead of the 32.8206 grains.

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31. "On the weight of the atmospheric air at other temperatures, and the law of its expansion by heat", Brit. Ass. Reports, 1, 574-5, 1831-2 (1832). Here Prout confirmed the findings of Dalton (1801) who was not, however, mentioned. For Dalton's results, which were less accurate than Gay Lussac's, see Partington, History, vol. 3, p. 770. W.C. Henry remarked: "Even at temperatures between 32° and 212° Fahr., in which interval Dulong and Petit believed that the air and mercurial thermometers expand equally, I was informed by that emphatically exact observer, Dr. Prout, that he had detected notable differences between the expansions of air and mercury; differences which necessarily attain their maximum at a station equidistant between the freezing and boiling points. These experiments are still inedit." However, "Dalton with his usual sagacity had anticipated such results", Life and Scientific Researches of John Dalton, 1854, p. 38, my stress.

calculated from Gay Lussac's formula. (33) These studies showed, Prout stressed, that air should no longer be taken as a standard for the specific gravities of gases; this was "as improper as sea water would be for fluids."

This conclusion had important implications for his own unitary theory of matter which at that time, as we shall see, was under the analytical scrutiny of Edward Turner. (Chapter 7) This is undoubtedly the reason why the Chemistry Committee of the British Association allocated money to Dalton and Prout for further research into the densities of hydrogen, oxygen, nitrogen and carbon dioxide at the Cambridge meeting in 1833. (34) Unfortunately nothing is known of this intriguing collaboration (if it ever took place), or of that between Thomas Clarke and Prout for the same investigation made in 1839. (35) It is quite clear from the British Association

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33. Note that he took the value 32.79 rather than the average value of 32.7958 determined in 1832. Gay Lussac's coefficient of expansion for air had been 0.00375; Dalton's 0.00391. Prout's value for this coefficient reduces to $\frac{32.79 - 31.0117}{31.0117 \times 15.5}$, i.e. 0.003636. This agrees well with Regnault's value, 0.0036706, and the modern value, 0.00366.

34. Brit. Ass. Reports, 2, xxxvi, 1833. See Johnston's statement, ibid., 1, 420, 1831-2 (1832), concerning chemists' ignorance of gaseous densities. It is noteworthy that the results of Prout's research on gaseous densities were keenly awaited by Turner, Phil. Trans., 1833, p. 539.

abstract of Prout's paper on air that his experiments were intended to be part of a long programme of research, using accurately calibrated apparatus, into the densities of air and all the common gases, and the effects of vapour pressure on gases. (36) Unfortunately, despite this auspicious start, the results of his further studies were never published, and nothing has been found in manuscript form.

These experiments on barometry may also reflect an interest by Prout in the relatively young science of meteorology. Although Prout served on the Meteorological Committee of the Royal Society from 1831 until 1841 (37) there is no evidence that he was a member of the Meteorological Society which existed in London from 1823 until 1841. (38) The reader of the long second part of Prout's Bridgewater treatise, Chemistry, Meteorology and the Function of Digestion, will discover a well-digested account of meteorological physics which is based upon the writings of the pioneer meteorologists, Luke Howard, John Leslie, Pouillet, and Daniell, as well as the

35. He did not discuss any of the results with moist air in 1832.

37. Roy.Soc.Minutes of Council (manuscript), XI,290, 1828-32; printed, vol.1, pp. 2, 180, 254 etc. 1832-46. From 1839 the committee was known as the "Committee of Physics, including Meteorology".

38. The Assistant Secretary of the Royal Meteorological Society, Mr. J.H. Willink, informs me that there is no record of Prout's name on the Council or as a Member of the then Meteorological Society.
more general writings of Humboldt and John Herschel. (39)

To Prout, the meteorologist was concerned with the present conditions of the earth, and began his work where that of the geologist ended. Meteorologists were particularly concerned to discover "the influences of heat and light, and of the energies allied to them; to study the laws of the distribution and change of these important agents in the production of climate; to trace in short, the effects of these wonderful principles upon the earth, the ocean, and the atmosphere and all the infinite variety of phenomena dependent upon them." (40) In the Bridgewater treatise Prout discussed in a systematic and clear manner the relations between the land, sea and atmosphere; the measurement of heat and light and their propagation; the temperature of the earth and climatic isotherms; the dependence of climate on season, geography and the atmosphere; evaporation and condensation, clouds and rain; and finally, the adaptation (i.e. design) of life to climate. We shall make no attempt to review this meteorological physics, but it is worth calling attention to two points:

39. L. Howard, article "Meteorology" in *Encyclopaedia Metropolitana*. (Despite an index I have been unable to trace this article in the Encyclopaedia).


Prout's *Meteorology* is 234 pp. in length compared with the *Chemistry*, 177 pp., *Digestion*, 139 pp.


41. Some further remarks will be made in the section on Natural Theology, *infra* chapter 6.
1. In his discussion of the means by which heat was propagated through a body or from one body to another, Prout introduced the term *convection* to describe the conduction of heat by rising warm air. (42) Although the recognition of this phenomenon has been ascribed to Rumford (43), Prout was the first person to use this term. Nevertheless, it is by no means clear how the word came into general use among physicists after the publication of the Bridgewater treatise in 1834. Since the word was used in 1843 by J. F. Daniell, a friend of Prout, and a man who had definitely read the *Chemistry*, I think it is very likely that it was through him that the word convection gained currency. (44) Prout made considerable use of the idea of convection in his meteorology since,

> A portion of water or of air heated above, or cooled below the surrounding portions, expands or contracts in magnitude, and thus becomes specifically lighter or heavier, rises or sinks accordingly; carrying with it, the newly acquired temperature, whatever that temperature may be." (45)

42. *Chemistry*, Book I, p. 65, from *convection*, a carrying or conveying.


The general currents of the atmosphere, as Daniell had described, were familiarly experienced as winds (46) and, Prout thought, they also gave rise to clouds. (47)

2. In a review of the effects "of the occasional presence of foreign bodies in the atmosphere", (48), Prout showed a concern to link them with diseases and epidemics. The "dry fogs" of the 1780s had been associated with an influenza epidemic (49), while Berzelius had described the awful effects of inhaling a minute quantity of hydrogen selenide (50). Small quantities of substances like selenium ejected from volcanoes or from the earth during an earthquake might conceivably produce epidemics, Prout thought. Even more perturbing, however, was the possibility that such foreign substances might be diffused through the air in a state of solution by convection currents and be completely imperceptible to the senses. This thought gave him the opportunity to mention once again the anomalous air density result of 1832 and its possible connection with the outbreak of cholera. (51)

If this were the case, then cholera was probably a variety of malaria which Prout, in company with the majority of nineteenth-century physicians, believed to be due to noxious exhalations which arose

47. Ibid., p. 319.
48. Ibid., p. 345.
49. Ibid., p. 348.
50. Ibid., p. 349.
51. Ibid., pp. 351-3.
from marshy areas or from the stinking areas of towns inhabited by
the poor. Although the molecular theory which was developed by Prout
in the Chemistry was ignored in the section on Meteorology, it is
not difficult to see that the speculation involved in this pyrogenic
theory of disease was implicitly related to his idea of merorganization.
(Chapter 8)

It can be seen, therefore, that Prout's experimental work on
barometry, apart from its own intrinsic merit and interest, was
related to theoretical views which we have yet to consider. Barometric
measurements were specifically made to render more accurate his
organic analyses, and the values of specific gravities which bore
on his hypotheses of the unity of matter and the integral multiple
weights of the elements. A singular anomalous reading of the density
of air was immediately fitted into his general molecular scheme and
offered as a possible explanation of the cholera epidemic which
physicians seemed powerless to prevent. These points will be
considered again in chapters 7 and 8. But before that we must pause
to consider both how Prout came to write a Bridgewater Treatise on
chemistry, meteorology and digestion, and how he fared, not in the
role of experimentalist or theorist, but as a Natural Theologian.
1. The Bridgewater Treatises

The argument for the existence of God from "the wonder of His works" using the model of Newtonian scientific methodology formed the basis of Natural Theology. This was a subject of considerable popularity during the eighteenth-century, and despite its logical disproof by Hume, it survived in Great Britain into the nineteenth-century until the theory of evolution demolished its empirical foundations. (1) A steady stream of learned and popular literature on the subject was published during the one hundred years after Newton's death in 1727. These treatises are well-epitomized by Archdeacon William Paley's very successful Natural Theology which appeared in 1802; not only did this book become a set-book at Oxford and Cambridge, but it also became indelibly impressed upon the mind of the young Charles Darwin. (2) Paley, who was committed to the Mechanical World picture of the English Newtonian tradition - the Sovereign Order of Nature - reacted strongly against the atheistic 'blind chance' of the French savants and of such English radicals as Erasmus Darwin. His arguments were drawn mainly from examples of organic contrivances, that is, he argued that the various species of

animals and plants were examples of designed artifacts. Paley paid little attention to the premises of the argument for design, and he ignored the devastating criticism of Natural Theology which had been published by Hume. This is not surprising, for the Christian religion was the status quo of an ordered, decent society, and as Paley recognised, the alternative appeared to be the blind chance, atheism and anarchy of the French materialists and revolutionaries.

There were some twenty editions of Paley's manual before 1820, and a further ten appeared before the end of the century. The book became the Bible of Nature for many scrupulously religious scientist, especially physicians who, despite their often barbaric education, usually became models of ethical and religious propriety. It is not a coincidence that four of the eight Bridgewater authors were practicing physicians.

In his book Genesis and Geology, the eminent American historian Charles Gillispie distinguished between the religious attitudes of three groups of British scientists who were prominent during the 1820s and 1830s. First, there was a minor collection of

5. The British Museum catalogue lists 31 eds. to 1679.
6. e.g. Charlotte Hall, the devoted wife of Marshall Hall, portrays her husband usually engrossed in Paley whilst his carriage conveyed him upon his rounds, Memoirs of Marshall Hall, London, 1861, p. 58.
7. Four were in holy orders, three were geologists (and four of them were christened William!)
pure scientists who expressed no public opinion concerning any connection between science and religion. This was not a conscious attitude on their part, or a rigid polemical ideology that science and religion were separate realms of experience. The latter was only a later nineteenth-century development. Individually, this group includes very devout members, e.g. Dalton, a Quaker, and Faraday, a Sandemanian. It was not possible, however, for a British geologist, whose science was developing very rapidly at that time, to separate his private religious feelings from his public science; so that whereas geologists are not characteristic of Gillispie's first division of scientists, they are characteristic of his other two parties. The second group followed uniformitarian geology: Science was a means of investigating God's divine plan for the universe, and science could have a religious function; but attempts to reconcile the results of scientific research (specifically geological research) with the biblical narrative were a waste of effort. The latter approach was more characteristic of the third group who followed catastrophist geology: Science was again a means of investigating God's divine plan for the universe, and science revealed that the principal end of the creation was the anthropomorphic one. But geological research revealed an actively participating Deity and not the passive God of the uniformitarians. Thus, for the uniformitarians, God had created an initial set of conditions and had passively permitted the universe to naturally become what it is at
present; (8) for the catastrophist, God intervened and actively participated in the development of the universe by supernaturally adapting means to a benevolent end. The dispute over 'Genesis and Geology' which took place between the scientists of the second and third groups (the clergy can be ignored) was consequently over both the interpretation of the geological record and over whether this record revealed a providential deity or not.

It was in this environment that the Bridgewater Treatises came to be written, and it is interesting to notice that all the authors belong to Gillispie's third group; they believe in a catastrophic geology, a providential deity, a God who created the universe for man, and a God who supervised its operation with this purpose continually in view. How Prout followed this pattern in his book on natural theology will appear in what follows. But first, why and how was Prout chosen to write a Bridgewater Treatise?

The Bridgewater Treatises were sponsored by the will, dated 1825, of the eighth and final Earl of Bridgewater, The Reverend Francis Henry Egerton (1756-1829). The son of a bishop, Egerton was an eccentric clergyman who "assiduously neglected his parish" (9) for studies of French and Italian literature, family history, and natural theology. (10) In his will he bequeathed £8000 to the


10. See notice in Dict.Nat.Biography. He wrote a Treatise on Natural Theology.
Royal Society (11) as a payment to the person or persons chosen by its President who would "write, print and publish, one thousand copies of a work on the Power, Wisdom and Goodness of God, as manifested in the Creation; illustrating such work by all reasonable arguments, as for instance the variety and formation of God's creatures in the animal, vegetable and mineral kingdoms; the effect of digestion and thereby of conversion; the construction of the hand of man, and an infinite variety of other arguments." (12)

These terms placed a considerable burden of responsibility on Davies Gilbert, a personal friend of Sir Humphry Davy, who had succeeded the latter as President of the Royal Society in 1827. Gilbert's responsibility was made more difficult by the fact that during 1829 and 1830 he was the centre of the controversy over the power structure of the Royal Society between the Scientists and the Gentlemen Amateurs. (13) Prout was on the side of the scientists and he favoured the continuation of Gilbert as President.

11. The money was invested by trustees in 3½% Bank Annuities. This legacy is not the only example of an attempt during the nineteenth-century by a religious zealot to reward in cash anyone who would prove the existence of God from design. Compare the terms of Mrs Hannah Acton (1838) to the Royal Institution in memory of her husband. £1000 was invested, and 100 guineas was to be awarded septennially "as a reward or prize to the person who, in the judgement of the committee or managers ... of the (Royal) Institution, should have been the Author of the best essay illustrative of the wisdom and beneficience of the Almighty, in such department of science, as the committee or managers should in their discretion have selected." Quoted in Preface to Chemistry as Exemplifying the Wisdom and Beneficence of God, London, 1844, by the chemist George Fownes, who was the first Actonian prize-winner.

12. See notice at the front of any copy of the Bridgewater Treatises.

"My dear Sir", he wrote to Gilbert,

I cannot help expressing my regret that you are about to retire from the Royal Society; indeed you have been so shamefully used that I do not wonder at it, but still as I have said, I regret the circumstance very much, particularly on account of the Society itself which I fear will be the sufferer. As to your proposed successor I know nothing; public report represents him as a strong political partizan; as a man of no science, and moreover in such a state of health as to be generally unable during the winter to venture out in the evening. If all this & more to the same effect be true, I cannot conceive an individual worse adapted for the Presidency of the R.S. & I have no hesitation in saying that, on these grounds, I as a member, shall most certainly oppose his election. These I believe are the sentiments of a very large proportion of the members, perhaps indeed of most of them, except perhaps the intrigueurs who, I fear are at the bottom of this, and of all the cabal that has lately existed in the Society & which has so disgusted me that I feel a very strong inclination to retire from it altogether. Still hoping that this great evil will not be inflicted on the Society & that you will long continue its President. (14)

However, Gilbert's position finally became intolerable and he resigned from the Presidency in November 1630 in favour of the non-scientist, the Duke of Sussex. Gilbert, therefore, did not have a direct hand in the final printing and publication of the Bridge-water Treatises between 1633 and 1636; this function was directed by the Society's brilliant Secretary, Peter Roget -himself one of the chosen authors. (15) However, before his resignation, Gilbert coordinated the initial search for eight suitable authors. His apparently personal decision that the legacy should be broken down into eight divisions did not go uncriticised and it became used as

14. Letter 8 October 1630, in library of the Royal Institution of Cornwall; I am grateful to the Curator, Mr. H.L. Douch for copying out this letter for me.

yet another excuse for dissatisfaction with Gilbert's presidency.
He had, however, wisely aligned both the Archbishop of Canterbury,
William Howley, and the Bishop of London, C.J. Blomfield, onto his
side, and invited them to suggest names and even to negotiate terms
with writers of their own choice. (16) In this manner, Charles Bell
was selected for Anatomy "including of course Lord Bridgewater's
favorite topic the Human Hand" (17); Roget for Physiology and
Comparative Anatomy; William Buckland for Geology and Mineralogy;
John Kidd for the Adaption of Man's Environment to his Physical
Condition; and William Chalmers on the Adaption of Man's
Environment to his Moral and Mental State.

The collection of correspondence connected with the Bridgewater
authors that was published privately by Gilbert's nephew, John Enys,
later in the century (16) shows that at first Gilbert did not
think in terms of a separate chemical treatise but one which would
include a discussion of the imponderable fluids, light and heat. (18)
The name of David Brewster was proposed and seriously considered,
but prompted by the Bishop of London (19), by 2 September 1830,

16. Cf. John D. Enys (ed.) Correspondence Regarding the Appointment of
the Writers of the Bridgewater Treatises Between Davies Gilbert
and Others, privately printed, Penryn, Cornwall, J. Gill & Son,
1877, pp. 32. I am grateful to Reverend Enys for allowing me to
read this very rare booklet. It is hereafter cited as "Enys".

18. Enys, pp. 9, 10, 11.
19. Enys, p. 11, Blomfield to Gilbert, "I agree with you in thinking
that Light might be the subject of a distinct treatise, provided
that it embraces Optics. Is chemistry to form a branch? If not
Heat should be coupled with Light. (The imponderable fluids
were still then part of Chemistry).
Gilbert instead suggested a treatise "Chemistry chiefly in reference to the Etherial or Imponderable Fluids or especially to Light, including Optics with the recent discoveries of Polarization". This treatise was to be written by either Prout or Brewster. Blomfield then advised Gilbert that Prout was "a first-rate chemist", and "I should think would do whatever he undertook with great ability", but he spoiled the effect by admitting that "I don't know what sort of writer Dr. Prout is; never having seen his publications." (20) He concluded his advice by emphasising that the literary merits of Prout and Brewster were an important factor in deciding between them. In fact there was little to choose between their respective literary gifts and scientific experience, and how the choice was ultimately made cannot be known; for with Gilbert's resignation, the task of organisation seems to have passed entirely into the hands of Roget. (21) It may, however, be conjectured that, since Prout was well-known personally to Roget and Gilbert through his Council membership of the Royal Society, and above-all because he was an expert on the subject of digestion which had been specifically mentioned in Bridgewater's bequest, Brewster was passed over in favour of Prout. A letter from Blomfield to Gilbert of 18 November 1830 named Kidd and Buckland as his own choice, and Whewell and Chalmers as the choice of Canterbury. Gilbert added to this letter in his own hand the names of Roget, Bell, Kirby

20. Enys, p. 16.

21. Roget did keep Gilbert informed of developments. There was also difficulty over the choice of an author for natural history. For some reason the naturalist Knapp was suddenly rejected in favour of the octogenarian, William Kirby.
During the winter of 1631 scandalous rumours began to appear in the press concerning the Bridgewater legacy. Buckland was appalled and appealed to Gilbert for a public statement from him concerning the chosen authors. Gilbert obliged with a suitable statement in March 1631. Meanwhile, Roget had difficulty finding a publisher. Murray accepted the series, then backed down; Longmans refused to publish an initial limited edition of a thousand copies. At last, at a meeting of the nominees in London on 11 October 1632, the final allocations were decided and contracts signed with the publisher William Pickering. Lord Bridgewater's desire for professional a posteriori demonstration of the existence of a benevolent deity was finally satisfied by the publication of the eight "strange and deadly" volumes intermittently between 1633 and 1636.


23. E.g. Literary Gazette and Journal of Belles Lettres, Arts, Sciences, no. 733, p. 86, 5 February 1631. The authors were here spoken of as "competitors for the legacy".

24. Announced at a meeting of the Royal Society at the end of February or the beginning of March 1631, and published Phil.Mag., (2) 9, 200, 1631. (Enys, p. 21-4).

25. Roget to Gilbert, Enys, p. 17.

2. Proudt's Bridgewater Treatise

Proudt's Bridgewater Treatise, The Chemistry, Meteorology and the Function of Digestion considered with reference to Natural Theology appeared in the Spring of 1834 (Prefaced 3 February) and a second edition was found necessary during the same year (Prefaced 7 June). (27) As we have seen, Proudt was well qualified to write with distinction and originality on the apparently unrelated topics of chemistry and the digestive process; and with distinction, if not originality on meteorology. Almost all the reviews were full of praise, although by 1854, when the agnostic physicist, John Tyndall, was asked to edit a posthumous edition of Proudt's book for the popular Bohn Library series, he commented sarcastically in his private journal:

I should have thought more highly of Dr. Proudt had I not read his book. Certainly if no better Deity than this can be purchased for the eight thousand pounds of the Earl of Bridgewater, it is a dear bargain. It is very evident that Dr. Proudt would never have written such a book through the spontaneous promptings of his own spirit; it was written for

27. The Dedication was to Davies Gilbert. If the terms of the will were strictly followed the first edition would have been limited to a 1000 copies, or perhaps even to only 125 copies (i.e. 8 volumes at 125 copies each). My own copy is a 1st. ed. with fine binding, but library copies are more usually 2nd. eds. in publisher's cloth.
money, and lacks even common scientific depth, not to speak of religious inspiration." (28)

I think that Tyndall was unfair in his estimate of Prout's scientific competence, for many of Prout's pages had simply dated. Tyndall was one of the more outspoken Victorian materialists, and his real dislike was for the Design Argument and perhaps also Prout's vitalism. Possibly there was a grain of truth in the financial sneer, though this was a common criticism which all the Bridgewater authors had to face. As "expense was not regarded by Dr. Prout" (29) in acquiring analytical apparatus, and as he had a wife and six children to support, and if the report that he was charitable with regard to patients' fees is not just "obituary sentiment", then who is Tyndall to begrudge Prout the unsolicited Bridgewater legacy? Nor is there any reason to doubt Prout's religious sincerity or belief in the design argument. (30)

28. Private Journal in Royal Institution library, 19 November, 1854. Quoted A. S. Eve and C. H. Creasey, Life and Work of John Tyndall, 1945, p. 56, with an incorrect date, 10 June 1854. The Athanaeum's sarcasm is also worth quoting. "To dismember Meteorology from Geology - the one involving causes of which the other presents the effects - in order to make it the link between Chemistry and Digestion, was the work of no ordinary mind; and to separate Digestion from Physiology - a part from the whole - and again place Physiology deprived of the Hand, in opposition to the Physical Nature of Man, allowed to retain his hands, evinced most uncommon tact in classification. The ingenuity with which Dr. Prout has connected his subjects, does not render their combination a bit the less ridiculous." loc. cit., vol. 58, p. 349, col. 1, 1854. (cf. also Edinb. Rev. 58, 422, 1853-4).


30. According to Miss Ada Nicoll's notes, Prout had a pew in the gallery near the pulpit of the fashionable physicians' church of St. James in Piccadilly, just opposite Sackville Street.
In his Introduction to the Chemistry, Prout discussed the three classes of objects which could be utilised in the argument for design. (31)

1. "Those objects regarding which, the reasoning of man coincides with the reasoning evinced by the Creator". For example, clothing is designed and used by man for warmth just as fur and hair serve the same purpose in animals. Man is therefore able to reason that animal fur and hair are designed by a Creator for the purpose of providing warmth. Sometimes in this form of argument man is only partly able to trace God's intentions "as in various phenomena amenable to the laws of quantity."

2. "Those objects, in which, man sees no more than the preliminaries and the results, or the end and design accomplished; without being able to trace through their details, the means of that accomplishment." All the phenomena of chemistry fell into this second class, while at the other extreme were

3. "Those objects, in which, design is inferred, but in which the design, as well as the means by which it is accomplished, are alike concealed; as in the existence of fixed stars, of comets, of organized life."

Chemistry, Prout thought, showed obvious marks of design, but the actual mechanism by which this design was produced could not, unlike the phenomena and objects of class 1, be completely understood by the human mind. Yet the whole burden of Prout's Chemistry was to show how macrochemical phenomena could be reduced to the beautiful

design of a molecular theory of matter. The total effect, as an Athenaeum reviewer noticed, (32) was to reduce chemistry to the design argument of class 1, and this implied that if Prout's molecular theory proved to be incorrect, then his whole argument for the existence of God would be compromised. In fact, however, Prout had cautioned the reader that his molecular hypothesis was only introduced by way of illustration and that its details were not essential to the design argument. In the second edition, to prevent further misunderstanding, Prout stated firmly "that no argument illustrative of design has been founded on the supposed molecular arrangements which he has given; and that the reality of design in the phenomena of chemistry is no more affected by the truth or falsehood of its imaginary incidents." (33) As we shall find, however, it is clear that Prout sincerely believed that his molecular opinions were correct and "calculated, sooner or later, to bring chemical action under the dominion of the laws of quantity." (34)

The theological argument and content of Prout's Chemistry followed the pattern of its companion volumes: examples of apparent utility and design in both the inorganic and organic kingdoms were, by cumulative effort, made into an argument for the unity of design and

32. Athenaeum, 58, 349, 1834.
33. Chemistry, 2nd ed., 1834, "To Reader".
34. Ibid., 1st ed., 1834, "To Reader".
purposeful beneficence of a Superior Chemist. (35) Prout nowhere offered a justification of analogical reasoning. In fact, as Hume had argued, the use of analogical reasoning in theology has no logical validity for, unlike science, in theology hypotheses cannot be tested either directly or by their fulfilment of predictions based on the original hypotheses. Although the use of analogical reasoning in scientific discourse is an invaluable aid to the formulation of hypotheses, these always have to be tested against experience. However, this condition cannot be fulfilled in theology for the design analogy does not produce (in Hume's requirement) a constant conjunction between the hypothetical first cause, God, and his effects, the materials of the World. We cannot confirm the existence of a World Designer by watching him make one. Nevertheless, as Hurlbutt has pointed out (in order to account for the longevity of natural theology after Hume) the design argument is psychologically persuasive because of "previously acquired and emotionally grounded prejudices in its favor". (36) In this latter respect Prout's treatise was obviously successful, as a few examples of arguments, both naive and sophisticated, will show.

Although "the excellent Paley" had remarked that chemistry could not provide the same sort of rigorous argument for a Deity that was provided by mechanics, Prout maintained that "the very imperfections

35. "The argument of design is necessarily cumulative; that is to say, it is made up of many similar arguments. To avoid repetition therefore, the illustration of principles rather than of details, has been studied; and the application of particular facts to the argument, has been often left to the Reader," Chemistry, 1834, To the Reader.

and difficulties of chemistry" gave it some advantages over mechanics.

When the Deity ... operates through the medium of mechanism, he appears almost too obviously to limit his powers within the trammels of necessity; but when he operates through the medium of chemistry, the laws of which are less obvious, and indeed for the most part unknown to us, his operations have much more the character of those of a free agent, and, in many instances also, appear of a higher order and are more striking and wonderful. (37)

In other words, since chemistry was an empirical science and very much the science of secondary qualities, (38) it was very difficult to discover the causes of chemical phenomena. God was therefore revealed as a free agent in all his omnipotence, and not as a God who restricted his power (as in mechanics) through easily recognised laws of nature. For, closely following Paley, Prout subscribed to the opinion that the so-called "laws of nature" were limitations which God had prescribed to his power. (39)

In the actual discussion of the known laws and phenomena of chemistry all theological argument is dismissed, and the treatise becomes an advanced chemistry textbook which deals with the atomic-molecular theory. (40) Eventually, of course, a return is made to the design argument: matter could not have always existed in its present molecular

37. Chemistry, 1834, p. 11; i.e. a theistic rather than a deistic God.
38. Ibid., p. 8.
39. Ibid., p. 16.
40. Cf. George Wilson's comment that Prout's treatise was of more value as a chemistry textbook than for its theology, Religio Chemici, Cambridge, 1862, p. 1.
form, and since it could not have attained its (designed) form by chance, "it must have been the work of a voluntary and intelligent Being." (41) Prout's programme is clearly similar to Boyle's attempt in the seventeenth-century to remove the atheistic stigma from Epicurean atomism.

And what a still more sublime idea is this calculated to convey to us of the wisdom and power of that Being who contrived and made the whole! When and where, do we naturally exclaim, did this Being exist? Whence his wisdom, and whence his power? There is, there can be, but one answer to these enquiries. The Being who contrived and made all these things must have pre-existed from Eternity—must have been omniscient—must have been omnipotent—MUST HAVE BEEN GOD! (42)

This pattern of theological rhetoric and argument alternating with long passages of straightforward scientific fact or speculation, is repeated throughout the treatise. Thus, in the fourth chapter, which deals with the 54 known elements, a description of their properties is balanced by a consideration of their function (i.e. their final cause) as part of the fabric of creation. One important example will be sufficient. The French physician Jean Coindet introduced the use of iodine for the treatment of goitre into French hospitals in 1820; but Prout claimed that its use for this purpose had been discovered by him in 1816 "after having previously ascertained by experiments on himself that it was not poisonous in small doses. ... The employment of the compounds of iodine in medicine was at that time made no secret; and so early in 1819, the remedy was adopted in St. Thomas's Hospital by Dr. Elliotson, at the author's suggestion". (43)

41. Chemistry, 1834, p. 86.
42. Ibid., p. 91.
Proust could have been embarrassed by the absence of any obvious function for the majority of the elements; but taking a tip from Paley, he asked "who can tell what design is latent under apparent incongruities?" (44) The logical implication of a muddled Designer was thereby avoided. Of course the fact that many elements were of an injurious or poisonous nature could not be so lightly dismissed. Whereas "the properties of oxygen stamp it as an element and subordinate agent of the most important kind; while the numberless contrivances which are observable in nature, to secure, or evade, or modify its operations, are most extraordinary, and exhibit some of the most marked and unequivocal evidences of design on the part of the great contriver," (45) the same could not be said of chlorine which was fatal to life in its free state. However, Proust noted triumphantly, chlorine had been designed to be of the greatest use for life when compounded with other elements. He refers to common salt and the hydrochloric acid of the stomach. This avoids the logically necessary conclusion from analogy that evil poisons have been designed by an evil Designer.

In this manner Proust developed the specious argument of Paley's eighteenth-century optimism that evil was perhaps essential for the greater good of the world. Poisons had been introduced by the Creator not because he was unable to prevent their existence, "but on purpose and designedly to display his power." (46) Proust also proposed a

44. Chemistry, 1834, p. 155.
45. Ibid., p. 97.
46. Ibid., pp. 155, 170.
"scientific" reason, namely that the "incompatible properties of the elements ... in some way contribute to the perfection of the compounds ...; and the only grounds upon which such incompatibility seems to admit of explanation is, that it results necessarily from those limitations which the Deity has thought proper to prescribe to his power, and to which he always most rigidly adheres." (47) It had been the properties of compounds rather than the properties of the elements which God had designed "without reference to the secondary properties of the elements themselves, which were left to be determined as the more general laws of matter might decide." (48)

The Deity induced adaptations in matter by subtle changes of quality and quantity. If the quality (i.e. the composition) of the air or sea had been different, or if it were to alter, then the whole economy of nature would be drastically changed; if the quantity or ratio of land to sea had been different, or if it were to alter, or if the quantities of hydrogen and oxygen present in water were any different, then "the whole order of Nature would be subverted, and the whole of the present arrangements be involved in ruin." (49) It is interesting to notice that Prout suggested that such differences might have been responsible for the past upheavals in the Earth's history. Prout was a catastrophist:

47. Chemistry, 1834, p.169.

48. Ibid., p.169, my italics. This argument horrified George Wilson, op.cit., ref.40, pp.27,46.

49. Chemistry, 1834, p.158.
Organized beings ... are at least as fixed and permanent in their nature, as the state of equilibrium in which they have been placed; and consequently, no new plants or animals can, as the world now exists, be imagined to be produced without a new and specific act of creation; or at least, without an entire change of equilibrium.

[The changes that the world had undergone] appear to be of two distinct orders which have alternated with one another in succession. The first ... seem to have been of a slow and gradual kind ... and existing for a considerable period. The changes of the second order ... have been violent, sudden and disruptive, of comparatively short duration, and differing exceedingly in degree and extent. ... The standard of organization seems to have been progressively raised after each convulsion. The last general catastrophe of the disruptive order was evidently a deluge.

This is the catastrophic progressionism of Sedgwick. (50)

The Meteorology contains very little teleological argument. Colour is proposed as an example of God's benevolence, (51) for if snow had been black, absorption of light and heat would have led to severe flooding from the melting snow. Fortunately, white snow absorbs a certain portion of light and of heat (by a beautiful provision more as the angle of incidence increases?) while so much light is reflected as is useful and no more. (52)

The anomalous expansion of water was so important "that it is doubtful whether the present order of nature could have existed without it", (53) for (in the manner of Cleopatra's nose) if it were not for the comparative lightness of ice, it would have formed from the bottom of water instead of from the surface, and therefore been the last part to melt. Life in lakes and ponds would have been impossible. Frout actually considered this argument as the most telling example of design

51. Ibid., p. 235.
52. Ibid., p. 241.
53. Ibid., p. 248.
in the whole of nature, especially if it was considered in con-
junction with the constant homogeneity of air which was critical
for the existence of terrestrial life. (54)

In order that...water might not be frozen; and that air might
not become irrespirable; laws must be infringed—and THEY
ARE INFRINGED; infringed too, precisely where their infringe-
ment, both in kind and degree, is indispensibly necessary to
organic existence. (55)

As for climatic effects which appeared to be solely destructive and
evil, Prout defended them like Paley on the grounds of either the
unsearchable ways of God, or general benefit:

A hamlet is laid waste; a few individuals may perish; but the
general result is good; the atmosphere is purified; and the
pestilence with all its trains of evil disappear. (56)

Even deadly malaria and cholera were evidently designed by God to
stimulate man's reasoning powers to discover the antidote, and to urge
him to industry through the conversion of marsh to fertile land. But
generally, Prout preferred more tractable topics like proofs of
benevolent successes, rather than of benevolent failures.

It is of course easy to pick out passages like these from any
treatise on natural theology and hold them up for twentieth-century
ridicule. We must not forget that in a non-evolutionary, provident-
ialist and anthropomorphic conceptual system in which every creature
and substance is thought to be designed to play a role subservient to
man, all apparent divergences and exceptions to the fine adaptation

55. Ibid., p. 360.
56. Ibid., p. 362.
of means to an end, have to be explained in terms of that system. So to atheists for whom adaptations were merely the result of immanent "necessary and eternal laws of nature"; and to deists who argued that design could not be proved, Prout replied as follows. The so-called laws of nature were founded on either reason and necessity, or on experience. But it was almost impossible to demonstrate any laws of the first kind. The atheist could neither prove his laws of nature were necessary, nor could he prove them eternal for, Prout argued empirically and catastrophically, experience showed that different laws of nature had prevailed in different ages of the world.

Hence as these laws cannot be proved to have a necessary existence, or to have existed from eternity as they now are, it becomes more than probable that they have had a beginning; and thus the inference of a pre-existent Law-maker, and all its consequences are at once inevitable. (57)

This argument clearly demonstrates that it was necessary to adopt some kind of uniformitarianism before the designs of natural theology could be converted into the adaptations of evolutionism.

The deist's argument could only be neutralised. As an empiricist Prout was willing to admit that since we could only understand nature through experience, no a priori demonstration of design (and therefore presumably of the existence of God) could be given. But equally, he argued, no a priori demonstration that the world had not been designed had yet been given. We should notice that Prout does not say that such a demonstration is impossible, and also that he makes no reference to the arguments of Hume.

The design argument makes almost no appearance in the Chemistry of Digestion until the final recapitulation (58) where Prout maintains that all the facts he had presented were "obviously demonstrative of design". The important mutual relations between the inorganic, vegetable and animal kingdoms were a general scheme laid down by Providence. The balanced association of the chemical processes of digestion and respiration with the anatomical structures of digestion and circulation of the blood could only be explained "on the supposition that a will exists somewhere; and also a power to execute that will." (59)

However, if Design is not prominently argued in the third book of Prout's Bridgewater Treatise, it is replaced by vitalism, which could be described as microcosmical natural theology. This will be a convenient place, therefore, to discuss Prout's vitalism and its relation to his chemical career.

3. Prout's Vitalism

In common with the majority of chemists and physiologists in the first half of the nineteenth-century, Prout believed that the chemistry of organized substances was intrinsically different from that or "unorganized" materials. (60)


59. Ibid., p. 541.

Their form is altogether different, and instead of being bounded by straight lines and angles, it is almost universally made up of some variety or combination of curves. (61)

Such differences were to be explained by a principle of organization or living or vital principle which was present in all organized materials. This governing power which had been "endued by the Creator with a faculty little short of Intelligence" (62) created, from ordinary inorganic elements and agents, organic substances which were in states of combination different from those found in the mineral kingdom, and which "perhaps also, consist of ultimate and refined forms of matter, which do not naturally, and perhaps cannot exist separately under the present constitution of the universe." (63) There was a continuous battle between the chemical forces and elementary substances of the inorganic kingdom, and the organizing force of living organisms. The equilibrium of living systems could only be maintained by "the constant and unremitting agency of the vital principle." If the vital agent failed either naturally through age, or from sudden exhaustion, then death resulted and "the brutæ tellus prevails, and speedily restores the incarcerated atoms to their original state of existence." It will be seen that Prout here partly adopted the vitalism of the French physiologist Xavier Bichat, who had offered the definition of life as "l'ensemble des fonctions qui résistent à la mort." (64)


However, Prout did not follow Bichat in believing that organisation could not be discussed in terms of inorganic forces and substances.

Apart from the unpublished *De Facultate Sentienti*, Prout's first physiological essay with a vitalist flavour was the paper on sanguification which was published in 1816. In this, as in the *Sentienti*, Prout suggested that the character of the vital principle could never be completely understood since it was impossible to study it by a comparison of its quality or quantity with any other known object. Prout seems here to use the phrase *vital principle* to mean a cause of life and organisation, not that it is some ponderable entity. But at other times, especially in his final words on the subject, he appears to intend it to be an imponderable or incorporeal agent. This ambiguity is common in the literature of the period, and is merely a reflection of chemists' and physiologists' puzzlement at the phenomena of organized life. But Prout believed that the investigation of life by the chemist was not entirely a hopeless proposition, for the animal chemist could investigate the means employed by the vital agency, and by the analysis of its general field of activity, map out "what it can, or what it cannot, or rather what it does, or what it does not, accomplish." (65) This positive attitude was of course to be the key

65. *Ann. Med. & Surgery*, 1,13,1816. In 1810 Prout wrote that the nature of the vital principle "is still and perhaps will ever remain an inscrutable mystery for we know things only by comparing them with others to which they have a resemblance in quality or property, but the principle of life has not its like in nature with which we can compare it and hence we are obliged to conclude that it is a principle sui generis or first principle", *Sentienti*, p. ii. A similar outline was made in *Chemistry*, 1834, p.429. Note also the lectures of 1814, *infra*, Appendix 7.
to further progress in biochemistry and physiology since it allowed the investigator complete freedom of research.

In his own "mapping", Prout described two negative features and one positive characteristic of the vital principle.

1. "The vital principle does not create material elements, or change one element into another." (66) In other words, Prout believed that the principle of the conservation of matter, or the conservation of the elements, was obeyed in organic chemistry. Nevertheless, we have already noticed in Chapter 4 that Prout sometimes felt obliged to override this negative characteristic of the vital principle and replace it with the possibility that under extreme conditions elements might be transformed one into the other. (67) But if we look closely enough at his statement in 1816, we find that there is no inconsistency because, (as suggested by the Prout protyle hypothesis which was published in the same year but in a different context), the so-called elements of the chemist were not necessarily the ultimate principles of chemistry; following Humphry Davy, these "elements" were simply undecomposed bodies.

The astonishing discoveries of modern chemistry have reduced the number of elements to comparatively few, and many of those formerly considered as simple substances, are now known to be

67. Supra, Chapter 4, p.159. "Yet, while it is thus denied that organized bodies possess the power, either to create or to change ..., it has been admitted to be exceedingly probable, that the organic agent is, within certain limits, qualified to compose and decompose many substances which are now viewed as elements", Chemistry, 1834, p.432.
compounds of two, three, and even more different principles, as for example bone earth, or phosphate of lime. But we cannot suppose, that even yet we are arrived at the limits of our knowledge. Chemistry is still progressive, and as we extend its limits, we diminish the number of our elements, so that even those substances which at this time we consider as simple, may, in the course of a few years, be demonstrated to be compounds. (68)

Since it was well known that plants and animals died if they were fed indifferently, if the living principle possessed "a transmuting power, it [the living thing] could subsist on any species of matter foreign to itself." However, this was not possible. For this reason Prout preferred to believe that normally the living principle did not exert any creative or synthetic power.

2. "The vital principle does not combine elements in such a manner, that the result or compound shall differ in its properties from those which it would possess if its elements were combined by any other agent. (69)

This extremely important suggestion again made for a positive scientific approach to animal chemistry. "Thus the phosphate of lime ... the carbonate of lime, the sulphuric acid, the carbonic acid, &c. formed by the processes of organization in animal bodies, differ in no respect from those formed by the chemist." (70) Chemical laws were invariant whatever the means, inorganic agent or vital agent, by which they were effected.

68. Ann.Med.& Surgery, 1,13,1816. Note that this remark was published in March 1816 only one month after the second anonymous paper which contained the protyle hypothesis. The idea is substantially repeated, Chemistry, 1834, p.431.


One and one will make two, whether the author of the addition be an idot (sic) or a philosopher, or even the Deity himself, and the same must be said of a particle of oxygen and a particle of hydrogen, which, when they unite must make a particle of water, whether the combination takes place by the agency of common chemistry, or by that of the vital principle in organized beings. (70)

This meant that the chemist was free to apply his knowledge and techniques to the field of organized matter.

3. "The vital principle operates upon inorganic elements and by means of inorganic agents." A vital agent directed and coordinated the inorganic forces of caloric, light, electricity and "that power or principle (if it be different from the electric fluid) which is the cause of chemical action" (71) towards the organization of the inorganic elements carbon, oxygen, hydrogen and nitrogen. (72) Since all these subordinate inorganic agents and bodies were found associated with life, it may be wondered why, in that case, Prout thought vitalism necessary at all. The fundamental reason was the direction and government of the processes of life.

It is perfectly ascertained that (heat, light, electricity, &c.), out of an unorganized body, and left entirely to themselves, never would or could unite, either in virtue of their own properties, or from accident, so as to form any plant or animal however insignificant. Are we not then compelled to infer, that within a plant or animal, there exists a principle or agent superior to those whose operations we witness in the inorganic? (73)

71. Ann. Med. & Surgery, 1,13,1816. That is, inorganic agents were subordinate to the vital agent.

72. He also added lime, magnesia, phosphorus, sulphur and iron.

Whereas today a biologist or biochemist will explain that the processes of life are directed by enzymes or the genetic code, the early nineteenth-century chemist and physiologist, who was without these concepts, explained that the vital agency commanded, directed, and when necessary, modified, the actions of subordinate inorganic powers which in turn acted upon inert materials according to the known laws of physics and chemistry. Inevitably there were many different positions over the role of vitalism in explanations of living processes. At one extreme physiologists like Bichat asserted that because life was such a completely different phenomenon from any inorganic or "unorganized" phenomenon, the organized could never be explained in terms of inorganic materials or forces. In its extreme form this meant that chemistry was a useless science to the physiologist whose discipline was concerned with biological laws of a fundamentally different order from the laws of physics and chemistry. Such a viewpoint was strenuously opposed by the early practitioners of animal chemistry, Pourcroy, Marcet, Bostock, Prout and Liebig, who argued that since life (and organization) deployed inorganic elements like carbon, hydrogen, oxygen and nitrogen in its handiwork, the ordinary rules of chemical affinity were also deployed in living processes. They did not exclude vitalistic explanations, for they conceived that a vital agency (whether material or a force was left unresolved) commanded, directed, and when necessary modified the actions of inorganic agencies like heat, light and electricity, which in turn acted upon the inert elements according to the known laws of physics and chemistry. Very few chemists bothered to discuss their use of vitalistic explanations in any detail.
Historians of chemistry have not always noticed that the majority of vitalists, or rather chemical vitalists, discussed the behaviour of organized substances, and not organic substances; hence, living in an age when there is a clear-cut, non vitalist distinction between inorganic and organic chemistry (viz. organic chemistry is the chemistry of the carbon compounds), they have been most troubled and concerned about the question of synthesis, and whether the synthesis of a substance like urea in 1828 destroyed the vitalists' cause.\(^{74}\)

This is not to deny, however, that some chemists did assert dogmatically that even organic synthesis was impossible—as impossible as inorganic synthesis was possible; but such chemists are to be found mainly in the first two decades of the nineteenth-century when the problems of purifying and analysing organic substances seemed to make them a class apart from mineral substances, and make their obedience of the laws of definite proportions seem an open question.

Historically, syntheses like that of urea did not destroy chemical vitalism; if this were the case one might well wonder how Prout could have speculated about the possibility of its synthesis in 1817 and yet have remained a vitalist all his life. (Chapter 3) The truth is that chemists like Prout were deploying the language of vitalism in order to explain the behaviour of organized bodies rather than of organic substances. The distinction is crucial, for whereas an organized body like a cat, or a tree, or a stomach in vivo, is living

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74. For pertinent remarks on the synthesis of urea, see T.C. Lipmann, *loc.cit.*, ref. 60. The historical tradition seems to have originated with M. Berthelot, see Jacques, *loc.cit.*, ref. 60, p. 37.
and vital, an organic body like sugar, or urea, or even albumen, which is a constituent of those organized bodies, is as lifeless as a mass of zinc oxide. As Thomas Thomson wrote in 1820:

Notwithstanding the imperfection of our present knowledge, I see no reason why we should despair of being able hereafter to account for many of those processes which puzzle us at present, and even of being able to form artificially various substances, both animal and vegetable, which we cannot do at present. (7b)

Organic substances, made from the same elements found in inorganic chemistry, were the chemical constituents of organized bodies, and it was necessary to suppose the existence of a vital force in the latter in order to account for their formation and stability.

Therefore, Prout was quite in order to speculate and even attempt the synthesis of organic materials like sugar and urea; but this had no bearing on his vitalism since the synthesis of life itself was by definition out of the question. The vital force might have more efficient ways of producing organic compounds, but there was no reason to think that the chemist would not be able to repeat some of these syntheses in the laboratory. But the synthesis of life—of organized materials—seemed quite out of the question.

The philosophy of chemistry will draw the conclusion that the production in our laboratories of all organic compounds, as long as they are not a part of an organism, must be seen as not merely probable but as certain. (76)

So wrote Liebig and Wöhler in 1836 in their brilliant experimental


paper on the products of nitrogen metabolism. Their prediction began to be verified in earnest during the 1840s with Kolbe's work on methods of organic synthesis.

Philosophically it may be argued that little was achieved by the postulation of a vital agency; but as Everett Mendelssohn has pointed out, historians must live with it and see how it functioned within the chemical and physiological models used by nineteenth-century scientists. (77) Methodologically, the concept of vitalism proved useful both as an explanatory hypothesis, and as a factor which effectively demarcated one science with one set of problems from another with a slightly different set of problems. In this case, the science of biochemistry was enabled to emerge from physiology and animal and vegetable chemistry.

It will be clear then, that if Prout had been asked why the vital agent did not act directly upon inorganic elements, or why the subordinate inorganic agencies were necessary, he would have answered that it would be unscientific to have reasoned otherwise. For it would "give to the vital principle a power little short of a creative one"; (a denial of Prout's first characteristic of the vital principle), and it would be rather like asking a potter to convert clay into porcelain "without the aid of tools, of the intermediate processes of tempering, baking, &c." (78) Moreover, decay after death was a protracted affair,


whereas if the vital agent acted directly on the inorganic elements, then the instant it ceased to operate would see the instantaneous decomposition of the whole organism.

The organization of inorganic materials took place at the minute terminations of the parts, or "machines", of living things where comminution occurred.

In these minute extremities it is evident, that the principles operated upon, must be separated as it were into atoms, each one of which will thus be capable of being acted upon or not, independently of the rest, and consequently combined with, or separated from others, in any manner which the economy of the being may require. (79)

Prout was here thinking of the formation of the blood in particular, for where the arteries terminated, so also, he believed, did the nervous and absorbent (lymphatic) systems. If, as seemed likely, the vital agent acted through the nervous system by exploiting the inorganic agency of electricity, then the "synthesis" or organization of blood and other materials, and the separation of unwanted materials through the lymphatic system, could well take place at this microscopic level. Prout's notion shows some similarity with the later idea of the cellular laboratory.

In 1816 Prout went so far as to assert that in principle even "organized substances ... might be formed artificially if we could bring together the elements composing them precisely in the same manner, in which they are brought together by the organic process." (80)

79. Ann. Med. & Surgery, 1,16,1816. The idea was first proposed in the lectures of 1814, see infra, Appendix 7. By 1845, the minute extremities had become cells, Chemistry, 3rd ed., 1845, pp. 389, 407.

In terms of the previous analysis, Prout clearly was referring to more than the synthesis of such organic substances as urea; he implies that it might one day be possible to synthesise organic molecules of considerable complexity like the heterogeneous fluid, blood. This was a much more advanced opinion than that of any of his contemporaries, and is closer to the later opinion which Liebig expressed as follows:

The power ... to effect transformations, does not belong to the vital principle; each transformation is owing to a disturbance in the attraction of the elements of a compound, and is consequently a purely chemical process. ... We are able to form in our laboratories formic acid, oxalic acid, urea, and the crystalline substances existing in the liquid of the allantois of the cow, all products it is said, of the vital principle. We see, therefore, that this mysterious principle has many relations in common with chemical forces, and that the latter can indeed replace it. (81)

However, Prout never again expressed himself in such mechanistic terms. Instead he made a significant, and retrograde, change in his chemical vitalism in later publications.

He does not appear to have made any further pronouncement upon the issue of vitalism between 1816 and 1831 when, during the Gulstonian Lectures to the Royal College of Physicians, he began to speak of organic agents rather than a singular organic agent, or separate vegetable and animal living principles. These three lectures on "The Application of Chemistry to Physiology, Pathology and Practice" were a continuation of the theme of the 1827 Copley paper, and they also anticipated the discussion in the third book of Prout's Bridgewater

In his lectures Prout anticipated Liebig's propaganda with the argument that physiologists paid too much attention to mechanical and metaphysical expalntions in biology and medicine, and none to chemical explanations. Biology and medicine demanded a chemical approach, he stressed. However, it is significant that Liebig also demanded that the chemical approach should be quantitative. Prout ascribed the lack of progress in animal chemistry as due both to the intrinsic difficulty of the subject and to the incompetence of the pure inorganic chemist who began to work in the unfamiliar field of biological chemistry. The solution of this problem was for physiologists to become chemists, for they would be well acquainted with the ordinary

82. The Gulstonian lectures were delivered in June 1831, and abstracted Medical Gazette, 8,257-65,321-7,385-91,1831. Prout was contracted to write the Bridgewater Treatise between October 1830 and January 1831.


84. This was a repetition of an earnest plea made in 1816: "It must be confessed, that hitherto physiology has not been so much benefited by the application of chemistry, as might have been expected. This has been owing in a great measure, to the cultivators of what is termed animal chemistry, having been generally mere chemists, whose object was chiefly to multiply useless experiments, & to form vague analogies between organic & inorganic substances. Hence the rational physiologist has either been disgusted by chemical explanations, & rejected them altogether, or the speculative theorist ignorant of the degree in which they ought to be appreciated, has employed them in constructing his airy fabrics. Chemistry, however, in the hands of the physiologist, who knows how to avail himself of its means, will, doubtless, prove one of the most powerful instruments he can possess; but it is capable of being employed to advantage by no one but the physiologist", Ann.Med.& Surgery, 1,289,1816, my stress.
phenomena of the living state so that if they were also trained in chemistry, then they might discover the properties, composition and the conditions of formation and change "which the mere chemist is apt to overlock, or knows not how to appreciate even if he observes them." (85) This argument was rejected by the physiologist Wilson Philip in his long acrimonious correspondence with Prout in the pages of the Medical Gazette. (86) It must be doubted whether Prout's arguments had any immediate effect in England even though Henry Bence Jones, a pupil of Liebig, later stated that Prout "first established the true connexion between chemistry and medical practice." (87)

Prout continued his Gulstonian lecture with the warning that the chemical physiologist should, after admitting the existence of an organizing principle, be a positivist with respect to life itself and avoid all speculation concerning either the nature of life or the vital agency. (88) This was rather like Galileo and Newton's attitude towards the nature of gravitation. The only legitimate scientific approach to the phenomenon of life was the empirical examination of what living organisms did or did not accomplish. In this way it was found that the elements of organized substances were the same as those of inorganic materials. Yet it was unnecessary and bad methodology to

85. Medical Gazette, 8,258,1831.
86. Supra, Chapter 1, p. 25.
88. Medical Gazette, 8,258,1831.
assume from this that an "organic principle" held these elements together when the ordinary observed (sic) chemical affinities of the elements for one another was a sufficient and simpler explanation. At this level at least, vitalism was superfluous. Sugar, an organic product, had been found to contain only the elements carbon, hydrogen and oxygen, but it was absurd to contend as some physiologists did that the power which held these elements together was a vital principle of the plant when the known elective affinities of these elements for one another could be invoked. Prout's methodological assumptions here are the simplicity and uniformity of nature.

How then were the obvious differences between inorganic and organic substances to be explained? Prout offered the concept of merorganization, first mooted by him in 1817, as a partial solution. Using the analogy of the remarkable changes which could be induced in some inorganic substances, such as iron, by the presence of traces of "impurities", he suggested that the earthy and saline bodies which usually accompanied organic molecules were not accidental heterogeneous impurities, but necessary ingredients. Organization could not proceed without them since they affected the "energy state" of the complex even though they did not appear to enter into the organic body in any definite proportion. Prout thought the experiments of John Herschel upon the electrical effects of impurities meant that

89. Medical Gazette, 8,258,1831; see also supra, Chapter 3, p. 74.

they acted similarly in organic molecules and perhaps by operating interstitially, such impurities prevented the organic molecules from assuming the shape of inorganic crystalline forms. In this respect it was significant "that many of those minute foreign substances which Mr. Herschel found to exert most energy in his experiments, are precisely those most usually occurring in organized bodies, such as sulphur, phosphorus, magnesium, calcium, iron, &c." These materials operated by being interposed, as it were, between the essential elementary atoms of organized substances, & thus prevent them from assuming the crystallized form, in which state they would be totally unfit for the purposes of the economy of living organized beings. (91)

In the third Book of his Bridgewater Treatise, these matters are referred to at some length (92) with little change in either words or implication from the introduction to the 1816 paper on sanguification and the Gulstonian lectures. The only possible way to explain life was to assume the existence of an agency "different from and superior to, that which operates among inorganic matters". (93) Prout distinguished the peculiar living forces which were common to organized bodies from the inorganic agencies like electricity and heat by the term organic agents. Organized bodies were composed from the elements carbon, hydrogen, oxygen and nitrogen, and frequently minute quantities of incidental elements which drastically modified the ordinary chemical properties of the four essential elements and their agents, and

91. Medical Gazette, 8,260,1831.


"apparently furnish to the organic agent new powers utterly beyond our comprehension; which powers the organic agent has been endowed with the ability to control, and direct, in any manner that, from the exigencies of the living organized being, may become requisite." (94) An example of such a power would be the separation of "the molecules of bodies, considered at present as elementary, into more refined forms of matter (submolecules?)". (94) The relevance of this kind of vitalism to Prout's biochemistry will be considered in the final chapter.

The exact nature of organic agents was unknown, and it was only possible to describe what they could or could not do. Since ordinary inorganic elements and agents "either in virtue of their own properties, or from accident" could never form a living plant or animal:

are we not compelled to infer, that within a plant or animal, there exists a principle or agent superior to those whose operations we witness in the inorganic world; and which agents moreover possess, under certain restraints, the power of controlling and directing the operations of these inferior agents? (95)

The existence of only one such agent would be inadequate to account for the tremendous variety of plants and animals, and for this reason Prout postulated a large number of such agents. Distinct orders of vital powers had been proposed by the German physiologist J.F. Blumenbach towards the end of the eighteenth-century, (96) and the idea of a hierarchy of vitalistic powers was implicit in Prout's Sentienoi of

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These agents could not create matter or transform one ultimate element into another, but they could compose or decompose "many substances which are now viewed as elements". (97) When organic agents combined elements together the resulting organic compounds did not differ from compounds made by chemists from inorganic agents—where this was in fact possible—since God never acted in opposition to his own laws of nature.

Organic agents acted in two ways, either by "peculiarity of composition and of structure", or by virtue of the manner in which this peculiarity of composition and structure was produced. The first method referred to mere organization in which the presence of small portions of interstitial materials accounted for different chemical behaviour caused by a difference of structure. "For though similarity of composition does not necessarily imply similarity of structure; yet similarity of structure perhaps, without exception, indicates similarity, or, at least, analogy of composition." (98) The second method by which this composition and structure of organized substances was produced was peculiar to organized substances. In inorganic chemistry the chemist worked with molecules in the mass, whereas it seemed that the organic agent "having an apparatus of extreme minuteness, is enabled to operate on each individual molecule separately; and thus, according to the object designed, to exclude some molecules, and to bring others into contact." (99) From this analysis it is clear that

98. Ibid., p. 435.
99. Ibid., p. 436.
the functions of Prout's agents are very similar to the faculties of Galen and the archai of van Helmont; so that he required an organizing agent for each apparatus of metabolism, the stomach, the liver, the kidney, and so on.

The organic agent, in its simplest state, may be viewed as a power which so controls certain inorganic matters, as to form them into an apparatus, by which it arranges and organizes other matters, and thus effects its ulterior purpose. (100)

Where the control of one agent ended, that of another higher, more complex and effective, agent began; and this process continued with ever increasing powers of organization until "the perfection of organized existence" was obtained. In the creation of such endless diverse organic agents, God revealed his attribute of infinity.

After reading Prout's Gulstonian lectures, the Manchester surgeon John Roberton, expressed dismay over the manner in which Prout insisted that an organic agent existed in every individual. He could not believe that such an hypothesis was necessary, "and besides (it) is calculated, by its mysticalness, to retard or discourage the study of this science." (101) The positivist distinction drawn by Roberton between inorganic and organized substances was that the chemist could predict the course of events, or reactions, in the former, but not in the latter where there was a dependence of part and process upon the whole which had no analogy with inorganic chemistry. Roberton was not trying to avoid vitalism (which was a legitimate hypothesis), but

100. Chemistry, 1834, p. 437. He added that the idea of successive creation of organic agents of increasing complexity was in accord with geology. As an afterthought, he savaged Lamarck, p. 478.

101. Medical Gazette, 8, 745, 1831. This point was also made by Comte's English disciple, George Henry Lewis, Comte's Philosophy of the Sciences, London, 1853, p. 29.
he thought Prout's brand of vitalism raised more questions than it
answered. What happened to the vital agencies after death? What
kind of "intelligence" did these agents possess? Vitalism, like
attraction or affinity, Roberton declared, was only a metaphysical
expression to describe a set of facts which did nothing to explain
their ultimate causes.

The whole which we know of vital action is, that it consists
in a most complex series of changes, which we cannot in any
case imitate; occurring according to a certain order for
each species of being, and subject to modification from an
infinite variety of circumstances. We are further compelled
to believe, by the highest moral evidence, that each species
of organic being has a determinate form and peculiar prop-
erties bestowed upon it by the Supreme Intelligence, in other
words that the WILL of the Deity was the direct cause of the
first of each kind of animal and vegetable. Hence it follows
that a living organised being ... constitutes a link in the
chain of its own species, the Deity standing in the relation
of direct cause to the first in the series, which He endowed
with properties fitting it to propagate the second; and so
the species has been, and is continued. (102)

Thus, for Roberton, organic substances were not intended for manip-
ulation by men, and in opposition to the scientific vitalism of
Prout, he proposed a theological vitalism in which the only cause was
God.

However, Prout (who was in the middle of his controversy with
Wilson Philip) refused to be drawn into any argument over vitalism.
His opinion remained unchanged in the third edition of his textbook on the urine and all of the subsequent editions, (103) although he admitted that historically there had been several hypotheses concerning the vital functions upon "which the peculiarities of organized bodies depend." These were:

1. The time-honoured belief in "independent existing vital principles or agents, superior to, and capable of controlling and directing, the forces operating in inorganic matters." This was Proust's own belief.

2. The suggestion that vitality was not independent of the common properties of matter, but a property superadded to them. This was the position adopted by Liebig and Berthelot, and the later synthetists.

As chemistry progressed in the nineteenth-century, the need for vitalistic models was reduced until only explanations of the creation of life retained them. As far as chemistry was concerned it disappeared completely once the attempt to differentiate between organic and organized bodies disappeared, and the separate disciplines of organic chemistry and biochemistry emerged. Since this second form of vitalism (if strictly speaking it can be called this) maintained that the vital force itself was open to chemical investigation, Proust had to reject it. To him it was equivalent to saying that material forces (i.e. in the role of vital forces) possessed the intelligence to select, bring and combine together the primary elements and elaborate them into the "wonderful mechanism" of an organized being. But:

material forces fulfil the will of the Creator in organic processes, without any knowledge or will of their own; and are mere brute forces, which left to themselves, so far from

forming organized beings, the moment the vitality ceases, are actively employed in destroying organization. (104)

3. The suggestion that the simplest form of vitality (irritability) was the "result of certain aggregations of inorganic matters." This property when acted on by other powers produced more complex phenomena such as sensation and the whole web of actions which were denominated life. This position had been adopted by the physiologist John Fletcher who had said:

It is not Life then, but only a necessary condition of life, namely irritability or vitality which is the result of organism; and when we speak of organized matter we mean, not that it is endowed with life — any more than any inorganic matter is endowed with combustion or sensible motion — but only that it possesses a property which, when acted on by appropriate powers, is competent to give rise to that series of actions in which Life consists. (105)

Prout found this as untenable as the former since it overlooked the element of choice or will found in organization to preserve existence or restore to health. Laws of matter, or of development were incompatible with choice or will.

In the third edition of his Chemistry (1845), Prout forgot his own remarks concerning the inadvisability of speculating about the nature of the vital agency. (106) It is possible that this deliberate amplification was in answer to Robertson.

An "organic agent" was an "intelligent agent" or "conscious being possessing knowledge, will, and power"; (107) it was to be distinguished from an "intelligent being" which described a knowledgeable

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being without will or power. God had vested his mediating power through these delegated agents which were neither material things or forces, nor in any sense vital forces; they were immaterial conscious beings whose knowledge, will and power varied from those confined to mere organization of elements (pure organic agents), through animal agents, to the intellectual agents peculiar to man.\(^\text{108}\) The pure organic agents organized matter into irritable cellular forms; the higher animal agents controlled the lower agents and moulded the cells into functional forms; and in man alone, the intellectual agent controlled the organization of the rational human form.

If it were objected that Prout was unable to state the exact nature and property of an organic agent, or its specific site in the animal body, he would reply that his inability to answer was not evasion but because these were improper questions to ask about immaterial things. Nevertheless, he was prepared to answer the question concerning their existence separate from matter on the "death" of an organized being, "promising that what we advance is not to be considered as doctrines either of philosophy or of religion, but as assumptions not opposed to either, and fairly deducible from natural phenomena."\(^\text{109}\) Here the distinction between an intelligent being and an intelligent agent came into use, and the deduction was made from the experience that although the character of our organization changes dramatically from youth to old age, nevertheless our identities or personalities remain constant. This is a debateable point, but it

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109. Ibid., p.403.
enabled Prout to argue that an immaterial something (the soul?), independent of all material changes, had remained constant. Such were organic agents. On separating from matter these agents became intelligent beings, but Prout carefully avoided suggesting what happened to them after this beyond implying that pure organic and animal agents whose intelligence was limited to this earth, passed an existence commensurate with this world.

But the intellectual agent in man, soaring as it does to other worlds, and recognizing the existence and attributes of the Deity Himself, even by the aid of an imperfect and mortal machinery; may surely be presumed to be immortal. (110)

We need not be concerned as to whether these beings were created by God when matter was first made, or only when the condition of the Earth favoured their separate creation. Nor should we be troubled about the question of reproduction of organic beings. These beings, thought Prout, were located inside the cells of the body. As God willed, they became organic agents. Thus, evolution was entirely ruled out, and development was asserted to have taken place entirely according to God’s interference and Divine Plan to show intelligent agents his wisdom and power. This explains the need for catastrophism, for by limiting the existence of beings as agents, and continually creating new ones, God could demonstrate his wisdom and power to the maximum number of beings.

It is clear that Prout’s vitalism became more theological and metaphysical as he grew older. Whereas at first, as a young man, he had been content to describe organic agents as faculties little short

of intelligence, at the end of his life he asserted that they were "conscious beings" possessed with "knowledge, will, and power". This sounds dangerously like polytheism, a doctrine which would have shocked Archbishop Howley and Bishop Blomfield who had probably carefully vetted the Bridgewater authors for doctrinal deviations. "We must confess", wrote one hostile critic a propos this "ugly excrescence" to an otherwise brilliant book, "our inability to discern the essential difference between this figment, and the mythical notions of the ancients respecting their deities or semi-deities, which we presume that Dr. Prout would condemn as absurd." (111)

Yet, as Prout pointed out, his hypothesis of organic agents did not prevent the scientist from investigating organic phenomena. (112) It was an explanation of the simplest kind, he claimed, which avoided the difficulties implied by the assumption of material forces, vital forces, or laws of development. (113)

As we should expect, of course, in the Bridgewater Treatise Prout's vitalism was closely related to the design argument. For example, he was able to comment upon "the wonderful adaptation of the elements and the agents of organic nature to each other." (114) If carbon, nitrogen and water had had different chemical properties, then

111. British & Foreign Medical Review, 21,123,1846.
113. In ibid., p.398, Prout mentioned a fourth hypothesis that vitality was due to certain "laws of development" which had been given to matter.
114. Ibid., p.439.
the present organic agents would have been unable to function and adapt the properties of these molecules to organization. Fortunately, the Deity had not designed matters differently, although on other planets, different designs might have been affected. (115) The properties of the element carbon were truly remarkable, and God had "displayed a greater stretch of his power, in accommodating to such an extraordinary variety of changes, a material so unpromising and so refractory as charcoal", than in the creation of the human mind itself.

To Him, however, all things are alike easy of accomplishment; and He, doubtless, has willed these and other proofs of His omnipotence, in order to convince us of this truth, - that the Creator of the mind, could alone have created the matter with which the mind is associated. (116)

In this manner Prout fulfilled the conditions of the Bridgewater legacy to the Royal Society. His book combined together, in a delectable manner, the argument for the existence of God from Design with the sciences of chemistry, meteorology and metabolic physiology. In the course of this argument, Prout revealed for the first time in detail, as we shall see shortly, his own molecular speculations, and he attempted to reduce the phenomena of inorganic, vegetable and organic chemistry to the properties of molecules and agents. In animal chemistry, where he was specifically concerned in the Bridgewater

115. "In the different planets" (where temperatures were very different from those on earth) "may not the living principle be attached to different elements, more or less fixed or volatile, as the distance of the planet from the sun may require?" Chemistry, 3rd ed., 1845, p. 441n.

116. Ibid., p. 443.
Treatise with the problems of digestion, the resulting vitalism was a curious mixture of contemporary organic chemistry with old-fashioned "faculty type physiology". Thus, in the manner of the Greek physician Galen who, when required to account for a bodily function or process, postulated a "faculty of alteration", Prout introduced organizing faculties, or "organic agents". Inevitably there had to be a great number of these agents—as many as there were vegetable or animal functions or processes—, and the resulting complex picture was curiously at odds with his molecular speculations which were always aimed at the reduction or unification of entities. However, this kind of vitalism did not interfere with Prout's programme to investigate the Chemistry of Life; perhaps (as Claude Bernard maintained of his own views later in the century) it had the merit of making it clear that physiology and biochemistry had peculiar problems that differed from those of the physical sciences.

In a very sympathetic account of Galen, Lester King has written:

The 'faculties' emphasise that biology is dynamic; that vital phenomena, involving real change and temporal sequence, can be studied as functional units; that description and analysis represent the first stage of understanding ... the 'faculties' indicate that the scientist may probe for biological and physical similarities among phenomena; that a broad synoptic viewpoint may be of value, even though specific details may be incorrect. (117)

If we transfer this quotation to Prout by substituting organic agent for faculty, we obtain a sound appraisal of Prout's chemically orientated vitalism. However, by endowing his agents with "intelligence" and suggesting they were immaterial conscious beings, Prout seems

to have been unnecessarily extravagant. And his pessimistic opinion that the nature of the vital agency, or life, would never be completely understood is a weak and unjustified viewpoint. Ironically, it was by exploiting the method which Prout had recommended, the mapping out of what the vital agency did, that permitted the freedom of research in animal chemistry that eventually led to the disappearance of vitalism. In his own generalisations concerning the abilities of the vital agents, Prout strictly adhered to the laws of physics and chemistry with the single and important exception that he permitted the composition and decomposition of the so-called chemical elements. From hindsight, the organic agents are seen to have many analogies with the later enzymes, and even the genetic instruction book. This need cause no surprise, for in the absence of physical knowledge concerning enzymes, and before the development of cytology, vitalism was the simplest possible explanation of organization. But although the explanatory functions of organic agents and enzymes, or the genetic code, are similar and sometimes identical, the evolutionary context in which the latter explanations are made has completely replaced the providential connotations of vitalism.

Far from preventing or inhibiting Prout from the investigation of the chemistry of digestion, we conclude that vitalism allowed him to follow the dynamics of the digestion and assimilation of foodstuffs in a temporal sequence through the stomach and intestines, and to make conjectures concerning their chemical and vital transformations into blood or the fabric of the animal body, or the decomposition of the organism and the elimination of waste-products. In a word, Prout's vitalism permitted the investigation of metabolism. However, this
programme would only be successful when amalgamated with the tools of a quantitative and symbolic chemistry. In this step lay Prout's failure and Liebig's success.
PART II

PROUT THE THEORIST
Chapter Seven: Prout's Hypothesis

In a more enlightened period, we have extended our enquiries and multiplied the number of the elements; the last task will be to simplify; and by a closer examination of nature, to learn from what small store of primitive materials, all that we behold and wonder at was created. (1)

The philosophical view that the world of appearances may be reduced to one real invariant (Monism) was first examined by the pre-Socratic Ionian thinkers. Empirically, they identified the basic material, or protyle, from which the world of appearances was constructed with a variety of different substances such as water, mist, fire, or more abstractly, with an intangible apeiron. Such ideas accepted the phenomena of change and the multiplicity of things without any attempt to explain how they came about. Parmenides produced the logical dilemma of pre-Socratic thought: how could change (including what we would call chemical change) and the variety of material things be explained in terms of one invariant, or element? Historians of Greek thought have recognised that three ways out of this philosophical cul-de-sac were tried. Two of these ways may be described as roads which led to a corpuscular physics, the other a road which led to a non-corpuscular or qualitative physics.

The medical philosopher, Empedocles, argued that change and multiplicity should be explained in terms of several basic roots or corpuscular elements which were themselves invariant; i.e. they persisted unchanged through a chemical process which was therefore really the association or separation of these elements. His qualitatively-

1. K. Chenevix, Phil.Trans., 1803, p. 320.
different elements, four in number, Earth, Air, Fire and Water, were supposed to be omnipresent in both the reactants and products of a change.

The atomists, on the other hand, retained the idea of a single basic stuff (i.e. one element), but they sub-divided it into an indefinite number of indivisible and invariant particles called atoms which were only distinguishable by their geometrical primary qualities. Atomism may therefore be described as a unitary theory of matter in which a singular element is divided into indivisible parts; these parts, which can only be distinguished by their size, shape and motion, persist through change. Chemical change is reduced to the rearrangement of these atoms in space (the void).

Although the connections between these two Greek corpuscular theories of matter and Dalton's atomic theory are only superficial and indirect, (2) it is nevertheless useful to view Dalton's theory, qua matter theory, as an amalgamation of these two earlier corpuscular philosophies. Chemical change according to Dalton was explained in terms of a number of qualitatively-different (Empedoclean) and quantitatively-different (Democritan) atoms. However, these atoms were no longer omnipresent elements, but simply the smallest parts of Lavoisier's undecomposed bodies.

It was Aristotle who first effectively challenged corpuscular explanations of change by denying that it was due to the co-mingling

2. Thus, atomism was an a priori essay put forward to resolve a philosophical problem; Dalton's chemical atomism was proposed as a scientific hypothesis in order to resolve a set of physico-experimental phenomena.
and separation of invariant primordial units of matter. Still faced with the Parmenidean problem, he effectively overcame it with his teleologically orientated doctrines of matter and form, potentiality and actuality. It should be noted, however, that Aristotle retained a first matter which persisted through change and "carried" the old and new forms through the transformation (i.e. transmutation). This explanation of chemical change, which he never fully developed, was debased over the centuries, especially by the alchemists who treated it as a theoretical basis for their experimentation. The idea became current that the first matter was preparable, and that spectacular and financially-rewarding transformations could be performed by exchanges of preparable forms. Moreover, since Aristotle retained the four Empedoclean elements, (3) the distinctions between the Empedoclean and Aristotelian matter theories became somewhat blurred, and the philosophers in the seventeenth-century, who revived a quantitative corpuscular physics, included both those who insisted on a universal substratum of matter (or one element), and those who believed in the existence of several qualitatively-different substrata. From the seventeenth-century onwards, through the influence of Boyle and Newton, physicists tended to talk in terms of one universal matter, while chemists adhered to multi-element theories. However, as a result of the critical analysis of the theory of matter which Boyle made in the Sceptical Chymist, chemists no longer believed that these elements were omnipresent. Instead, through the influence of Stahl, elements

3. Modified to fit Aristotle's doctrine of matter and form so that Earth had the forms or qualities of cold and dry, Water those of cold and moist, Air moist and hot, Fire hot and dry.
came to be treated pragmatically as those substances which the chemist could not decompose. This received powerful support from Lavoisier in 1789 when he advised chemists against speculations concerning the ultimate constitution of matter, and supported a purely pragmatic definition of the element.

All that can be said upon the number and nature of elements is, in my opinion, confined to discussions entirely of a metaphysical nature. The subject only furnishes us with indefinite problems, which may be solved in a thousand different ways, not one of which, in all probability, is consistent with nature. I shall, therefore, only add, that if, by the term \textit{elements}, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable we know nothing at all about them; but, if we apply the term \textit{elements} or \textit{principles of bodies}, to express our idea of the last point which analysis is capable of reaching, we must admit, as elements, all the substances into which we are able to reduce bodies by decomposition. Not that we are entitled to affirm, that these substances which we consider as simple, may not themselves be compounded of two, or even of a greater number of more simple principles; but since these principles cannot be separated, or rather since we have not hitherto discovered the means of separating them, they act with regard to us as simple substances, and we ought never to suppose them compounded until experiment and observation have proved them to be so. (4)

By amalgamating corpuscular physics with Lavoisier's pragmatic chemistry, Dalton was in many ways closer in spirit to Empedocles and pre-Boylan chemistry than to the atomism of Democritus. Yet there were many nineteenth-century exponents of a corpuscular chemistry who were closer to Democritus; these adopted the atomism of a universal matter and developed a molecular theory. This school, while it was unable to share the empirical advantages enjoyed by the multi-element viewpoint, nevertheless enjoyed the encouragement of conceptual simplicity and the prospect of a physical, quantitative, and mathematical

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chemistry.

The theory of the elements in the nineteenth-century became a dialogue between the school of multi-element chemists and the various schools of reductionists; and since Dalton had built the multi-element viewpoint into his atomic theory, reductionists to some extent inevitably felt obliged to reject simple Daltonian atomism. From the foregoing analysis, however, we can see that in doing this they were not rejecting atomism, or a corpuscular physics; they were rejecting an Empedoclean physics for a Democritan molecular physics.

In the forms known as "Prout's Hypothesis", this viewpoint is attributed to William Prout. However, Prout correctly described it as "an opinion not altogether new", and for many years historians of chemistry have demonstrated that its immediate intellectual ancestry lay in the writings of Humphry Davy and Thomas Thomson who had both wanted to reduce and simplify the growing number of elements endowed to chemists by Lavoisier and the labours of Davy himself. The issue is actually a little more complicated, and made more interesting by the fact that Prout's undergraduate essay, On the Faculty of Sensation, shows that he had an Aristotelian-like commitment to a belief in prime matter.

Therefore, before the respective claims of Davy and Thomson to have originated Prout's hypothesis can be reviewed, the new evidence

revealed by this manuscript source must be discussed. (6)

An abstract of Prout's essay has been given in Chapter four, and here, in order to promote the discussion, we shall only recapitulate the relevant material on matter.

The vital principle, Prout suggested, combined with ordinary inert matter in varying degrees and so produced the characteristics of vegetable, animal and human life. Prout took his definition of matter from the eighteenth-century authority on Aristotle, James Harris.

Matter is that elementary constituent of composite substances which appertains in common to them all without distinguishing them from one another. (7)

Consequently, by this Aristotelian definition, all matter was one, or made of the primary matter which had been denominated by early Greek philosophers as \( \nu \varphi \xi \varepsilon m \varphi \nu \varphi \). Yet this matter obviously existed in many conditions or forms of secondary matter, the \( \nu \varphi \eta r \), or "matter which has a capacity for becoming many things before it actually becomes any of them." (8) A third condition of matter was physical, extended and hard. (9) None of these conditions of matter necessarily really existed in nature, for knowledge of them was derived by abstraction and analogy from the secondary or aggregated

6. A preliminary announcement of the bearings of Sentiiendi on the origins of Prout's Hypothesis was made by me to the XI Congrès International D'Histoire Des Science at Warsaw on 26 August 1965 as "Some Unpublished Papers of William Prout Relating to Matter Theory."

7. Sentiiendi, p. 2, quoting J. Harris, Philosophical Arrangements, 1775, p. 63.

8. Sentiiendi, p. 3; Harris, p. 72 footnote (e).

9. Sentiiendi, p. 3. Prout ignored Harris's and the Greeks' other divisions of matter: the second matter in body devoid of quality, solid three dimensional matter, and abstract (mathematical) matter, Harris, p. 86.
forms of matter with which we are so familiar through our senses of sight and touch. Prout's only purpose in making these distinctions was "merely to endeavour to render probable by their means the unity of matter."

The \( \phi \rho \tau \lambda \), or secondary matter, was redefined by Prout in a more scientific manner as "matter in its aggregate state", and by this he referred to such substances as wood, stones, water, air, and Lavoisier's chemical elements. Secondary matter could exist in one of five different states of aggregation, solid, liquid, aeriform, atheriform and luciform. It differed from the primary physical form of matter by possessing the quality of roughness.

**Inferred**

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| extended, hard and rough |

As we saw, the essay was largely concerned to argue how the intellectual combination (which was only to be found in man) derived a knowledge of material things through sensation; and it was an attempt through analogical reasoning to reduce all sensational mechanisms to a unity.

In order to set forth the probability of the unity of matter as distinctly as possible we have begun by endeavouring to trace its identity through all its primary conditions to the \( \tau \eta \nu \theta \varepsilon \chi \omega \lambda \mu \nu \varepsilon \nu \nu \nu \) of the ancients down to its physical state, or that condition of it in which it becomes the object of sensation from its being vested with sensible qualities. Respecting the nature of the unity of matter, I may here observe that it was adopted by most of the ancient philosophers and by many of the moderns among the latter of whom was I
believe the immortal Newton himself, the speculations still are by many accounts visionary, but when we reflect upon the astonishing discoveries that have been made in chemistry and the progress it is still making, who will say that at some future day they will not be realised at least with matter in its secondary or aggregated condition. (10)

Besides a physiological and psychological programme then, this was equally an explicit commitment to a chemical programme in which the elements were to be reduced to one kind of matter.

The disordered manuscript notes of Prout's lectures on animal chemistry which he gave privately in London in 1814 (Chapter 1), show that he continued his commitment to the unity of matter; likewise he continued to use the Aristotelian-Harris language of "active and "passive" agencies. (11) "The objects of nature", he wrote in his notes,

may be divided into elements and agents or the passive and active principles. (Corresponding to the pos. and neg.) Ancient and modern conjectures on the nature of the ultimate passive and active principles. General coincidence (of) these in the notion of their being but of one kind -i.e. that their (sic) is but one active principle in nature from which all others are deduced and modified. (12)

If this hypothesis were correct, then the passive primary matter was organized by an active agent into the various elements, compounds, organized beings and higher active agents. Consequently,

ordinary (?) or compound elements & agents can differ from one another in degree only, i.e. according as the active or passive


12. Manuscript of first lecture, labelled "Synopsis", fol.1, see infra, Appendix 7.
principles predominate in their composition & secondly, that the more active principles can act over all the less active ones.

In other words, every natural object was relatively passive (as an element) to certain other objects (agents), but active (as an agent) towards others (elements).

In the same lecture Prout referred to a diagram which is not to be found on the manuscript; however, the following diagram is found on a page of the draft for the same lecture. (13)

Primary Elements & Agents

Prout explained that in this table (or one like it):

The objects of nature, either as actually known to us, & known by their effects only I divide into two great classes which are denominated elements & agents, between these two I do not pretend to draw a distinct line, but suppose them to run into each other or in other words that every element is more or less an agent & v.v. those however which from their characters being stronger marked appear better than others to deserve these appellations are placed on the extremities of the scale. (14)


14. Lecture 1, "Synopsis", fol. 3; also in other drafts. See infra, Appendix 7, p.502.
In the division of mineral chemistry, hydrogen was the primary element or passive agent which was converted by the active agency of galvanic electricity, or its modification (whatever that might be), into the other chemical elements and agents of mineral chemistry. The other agents were the familiar imponderables, caloric, light and magnetism, and by implication, the vegetable and animal organizing principles.

Metal, of which hydrogen is considered the most characteristic & pure is placed as the primary element - & galvanism with its modification as the ultimate agent - between which two lie all the other elements & agents of this division. (14)

Although oxygen is not mentioned in the text, the diagram (which seems to have been modified for the actual lecture) placed oxygen on the same level as hydrogen, and it is not clear whether Prout supposed that galvanic electricity first acted on hydrogen to produce oxygen, and then that these two elements reacted with the galvanic agency to produce all the other elements; or whether he supposed that oxygen was another primary element which was perhaps as characteristic of the non-metals as hydrogen was of the metals. This ambiguous position over the role of oxygen is also to be found in Prout's first anonymous paper in 1815; it may well reflect Davy's uncertainty over the same question. However, it may be suggested that since Prout speaks in the text of the development of carbon and nitrogen from hydrogen and oxygen, and yet he placed these four elements on the same horizontal line, that he believed that oxygen was also developed from hydrogen. This would be in keeping with his belief that the number of elements was "doubtless much more numerous than the simplicity of natures operations requires."
The characteristic primary element of vegetable chemistry was carbon, and the characteristic agent a special vegetable living principle. Since all the resources of inorganic mineral chemistry were also included in this division, the vegetable agency was free to act not only on carbon, but also upon the mineral imponderables, caloric, light, electricity, etc., as well as the ordinary chemical elements. Finally, the characteristic primary element of animal chemistry was nitrogen which was organised by a characteristic animal living principle. Here all the resources of both the mineral and vegetable kingdoms of chemistry were also included.

Proust's lecture notes are unfortunately incomplete, but the general picture is fairly clear. On the grounds of the simplicity of nature, Proust suggested to his audience that an animal principle organized nitrogen and the elements and agents of the other two lower kingdoms into animal substances; the vegetable principle organized carbon and the elements and agents of the mineral kingdom into vegetable substances (and he noted the objection that some vegetable substances had been found to contain nitrogen); and finally, the foundation stone for this simple hierarchy was the mineral kingdom based solely on galvanic electricity and hydrogen. (15)

15. This hierarchy of life appeared to be confirmed by geologists' findings. The order in which the earth had developed seemed to indicate the absence of fossil life in the oldest granites, and its presence in only the newest rocks. "Geological facts show that mineral substances are the oldest materials existing in our globe & hence render it probable that they existed at a period when vegetable & animals were not..." (from manuscript random sheet).
In one sense this audacious classification may be regarded as a pedagogic device upon which Prout based his lectures. Yet in view of his earlier essay *On Sensation*, and the famous anonymous papers which he published in 1815 and 1816 in Thomson's *Annals of Philosophy*, it is clear that Prout genuinely believed in the complexity of Lavoisier's and Davy's elements. Nevertheless, in his lecture he qualified himself by stating that "the ocean of hypothesis" was to be carefully avoided. Enough has been said, however, to prove that Prout was committed to the unity of matter while he was a student at Edinburgh, and before the publication of Davy's *Elements of Chemical Philosophy* in 1812 publicised the reductionists' cause. The manuscripts from the lecture period of 1814 confirm - what has never been seriously doubted by historians of chemistry - that Prout had read Davy's textbook, since several references are to be found to it. There is therefore no reason to think that this new manuscript evidence in any way diminishes the influence which Davy exerted upon the development of "Prout's Hypothesis". Rather, I believe, it reinforces the impact which Davy could have had on Prout who was already a convinced reductionist.

**The influence of Davy**

Robert Siefried has pointed out in a series of articles that Lavoisier's pragmatic definition of the element was intellectually disquietening since it left chemists with the problem of deciding whether a chemically-undeecomposable substance was a real element, or
whether time would show that it was compound. (16) Humphry Davy experienced this dilemma in a most acute form, and he consequently refrained from speaking of "elements" at all, and preferred the non-committal phrase, _undecomposed bodies_. Like Richard Chenevix (who was quoted at the head of this chapter), Davy felt instinctively that there were too many "elements", and he hoped for an explanation of Dalton's laws of definite and multiple proportions in the discovery of bodies which really were un-decomposable rather than with an atomic hypothesis that was bound up with the present limitations of chemical analysis in its acceptance of a multiplicity of elements. (17)

Ironically, of course, Davy suffered the embarrassment of isolating many new Lavoisieran elements; yet he remained true to his sceptical reductionist principles and accepted his discoveries as possessing the same ontological status as the other undecomposed bodies. Whether sodium, potassium, the alkaline earth metals, or chlorine and iodine really were _elements_ remained to be seen.

During Davy's "period of perplexities", 1807-8 (to quote the apt phrase of Joshua Gregory (18), the confusions of his experiments with sulphur, phosphorus and tellurium in which he found them to

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"contain" hydrogen, lent him much support to his reductionist commitment. (19) From 1809 to 1812, Davy made several public pronouncements of his belief in the ultimate simplicity of nature, and these must have awakened considerable interest. In a lecture at the Royal Institution in 1809, he speculated on the probability that substances which we at present conceive to consist of different species of matter may ultimately be referred to different proportions of similar species, and in this way the science of the composition of bodies may be materially simplified. (20)

Two years later he publicly discussed the analogous properties of the metals and suggested that they all contained a common elementary principle, or principles. These suggestions reached an even wider public through the appearance of his Elements of Chemical Philosophy in 1812. In this Davy wrote:

A series of proportions may be formed in which the metals may be supposed composed of hydrogen, and another substance in definite quantities; and in this hypothesis, the lightest would contain the largest quantity of hydrogen, and possess as they are found to possess, the strongest attraction for oxygen and chlorine. (21)

19. Siegfried has called these experiments the experimental basis of Prout's hypothesis, J. Chem. Education, 33,263,1956. In a letter to Davy, Edward Clarke (Professor of Mineralogy at Cambridge) said that he had mentioned the hydrogen content of Davy's metals to Cambridge audiences. (Letter of 23 Feb. 1811 in library of Royal Institution).

20. The Collected Works of Sir Humphry Davy, Bart., ed. by J. Davy, 9 vols., London, 1839-40. See vol. 8, p. 323. In the same year he wrote to the botanist Thomas Knight, "I have come to a conclusion ... that water is the basis of all the gases, and that oxygen, hydrogen, nitrogen, ammonia, nitrous acid, &c, are merely electrical forms of water, which probably, according to the θέρατον, is αψίδα, is the only matter without power, and capable, according as it receives power, or change in its electricity, of assuming the various forms hitherto considered as elementary", J. Davy, Fragmentary Remains Literary and Scientific of Sir Humphry Davy, Bart, London, 1858, p. 129.

In an "Advertisement" for a projected second edition of his Elements which never appeared, Davy drew attention to his adoption of integral proportional weights, and added:

I have usually given whole numbers, taking away or adding fractional parts, that they may be more easily retained in the memory. When the number was gained from experiments in which a loss might be supposed, I have added fractional parts, so as to make a whole number. (22)

Here Davy gave the impression that his sole motivation in adopting whole numbers was to be in keeping with "an elementary book devoted to the general truths and methods of the science." Certainly Davy did not try to link this pedagogic simplification in any way with the speculative simplification in the seventh division of the book where he unfolded his methodological commitment to the reduction in the number of elements. Prout, who as we have seen was already committed to a belief in a prime matter, did make such a linkage, and he must have drawn encouragement from Davy's remark that:

It is contrary to the usual order of things, that events so harmonious as those of the system of the Earth, should depend on such diversified agents, as are supposed to exist in our artificial arrangements; and there is reason to anticipate a great reduction in the number of the undecomposed bodies. (23)

or that,

Matter may ultimately be found to be the same in essence, differing only in the arrangements of its particles; or two or three simple substances may produce all the varieties of compound bodies. (24)

23. Ibid., p. 42.
24. Ibid., p. 132.
and especially Davy's remark that,

We know nothing of the true elements belonging to Nature; but as far as we can reason from the relations of the properties of matter, hydrogen is the substance which approaches nearest to what the elements may be supposed to be. ... After hydrogen, oxygen partakes most of the elementary character. (25)

This last sentiment was expressed in a similar manner by Prout only two years later.

Davy's mature public opinion was, therefore, that all inflammable bodies contained hydrogen and that oxygen was probably the basis of substances with other properties. However, the exact role of oxygen was left ambiguous. Different electrical states, or different arrangements of the same matter were conceived to constitute different chemical species, just as ice, water and steam were all the same material in different physical states. Even if this hypothesis were shown to be true, the facts and doctrines of chemistry would remain largely unaltered. "The only change in the science would be, that those substances now considered as primary elements must be considered as secondary; but the numbers representing them would be the same, and they would probably be all found to be produced by the additions of some simple numbers or fractional parts." (26)

Stated positively, this is of course Prout's hypothesis, and it also foreshadows Prout's later suggestion that fractional multiples of the primary element might be involved. It was along Davy's "promising


path of enquiry" (27) that Prout wandered in 1815.

The influence of Thomas Thomson and John Miers

It was not Humphry Davy, however, whom Prout specifically cited as having first noticed the "law of multiples", but the protagonist of Daltonian atomism, Thomas Thomson, who, in the very first edition of his popular System of Chemistry in 1802, had shown a concern for the simplification of the number of elements endowed to chemistry by Lavoisier.

As the term simple substance in chemistry means nothing more than a body whose component parts are unknown, it cannot be doubted that, as the science advances towards perfection, many of those bodies which we consider at present as simple will be decomposed; and most probably a new set of simple bodies will come into view of which we are at present ignorant. These may be decomposed in their turn, and new simple bodies discovered; till at last, when the science reaches the highest point of perfection, those really simple and elementary bodies will come into view of which all substances are ultimately composed. When this happens ... the number of simple substances will probably be much smaller than at present. Indeed it has been the opinion of many distinguished philosophers in all ages, that there is only one kind of matter; and that the differences which we perceive between the bodies depend upon the variety in the figure, size and density of the primary atoms when grouped together. (28)

In an M.Sc. thesis published in 1955, W. F. Millard cited this passage and remarked that it was not surprising that Thomson became a "staunch supporter of Prout's hypothesis in later years". (29)


There seems to have been some confusion among historians as to what exactly Prout's hypothesis was. In fact there were two hypotheses: one, that atomic weights, or specific gravities were integral multiples of the atomic weight or specific gravity of hydrogen; and two, that hydrogen was the prime matter, or less specifically, that there were only one or two ultimate elements from which all the known chemical elements were constructed. It seems to me to be very important to always make this distinction since a chemist could adopt one of these hypotheses without necessarily adopting the other. In order to avoid confusion, I shall always refer to the former hypothesis as "the integral multiple weights hypothesis", or more simply, the "multiples hypothesis"; and to the latter as "the protyle hypothesis" or "unitary hypothesis".

Although in the early statement of Thomson's quoted by Millard, he clearly supported a reductionist thesis, Thomson was never to use Prout's multiples hypothesis as a support for reductionism. As we shall see, Thomson's support for "Prout's hypothesis" was limited to the first empirically-testable part of the hypothesis, namely the integral multiple weights hypothesis, and I can find no evidence that he ever wrote in favour of Prout's protyle hypothesis.

In the second of an important series of papers "On the Daltonian Theory of Definite Proportions in Chemical Combinations" published in 1813 in his own journal, Thomson drew attention to the fact

There are eight atoms of simple bodies whose weights are denoted by whole numbers; namely

1. oxygen 1  
2. sulphur 2  
3. potassium 5  
4. arsenic 6  
5. copper 8  
6. tungsten 8  
7. uranium 12  
8. mercury 25

An atom of phosphorus is ten times as heavy as an atom of hydrogen. None of the other atoms appear to be multiples of 0.132 [hydrogen]; so that if we pitch upon hydrogen for our unit, the weights of all the atoms will be fractional quantities except that of phosphorus alone.

Thomson drew no generalisation from these integers (apart from the erroneous one that there was no connection between specific gravity and atomic weight) and he appears to have looked upon them only as an additional argument for the oxygen scale \((O=1)\) instead of the Daltonian hydrogen scale \((H=1)\). Nevertheless, it is certain that Prout read this comment since he specifically referred to Thomson's observation of the commensurateness in atomic weights of some of the metals.\(^{31}\)

It is possible that Thomson's use of oxygen as a basis for the scale of atomic weights (which was also supported by Wollaston and Berzelius) may have been a further reason, apart from the influence of Davy, why Prout queried "Is the other factor oxygen?" in his first anonymous paper. Prout could have drawn encouragement from Thomson's work and observations upon atomic weights because he was aware that Thomson shared his and Davy's belief in the simplicity of the world.

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\(^{31}\) Prout's Hypothesis, Alembic Club Reprint, no. 20, Edinburgh, 1932, pp. 36-7.
and the complexity of the accepted elements. Once Prout had taken the initiative in 1815 and 1816, Thomson became (as we shall see) his intellectual agent. It is worth emphasising again, however, that Thomson only supported and made propaganda for Prout's integral multiples hypothesis; he held no brief for the protyle hypothesis as formulated by Prout.

There was one other possible influence upon the development of the protyle hypothesis which, as far as I know, has never been noticed before. This was the erroneous work of John Miers on the composition of nitrogen. (32)

The analogy between the basic properties of ammonia, soda and potash, and the decomposition of the latter two substances by Davy in 1807, led many chemists to speculate that nitrogen was a compound. At one time Davy believed that nitrogen was an oxide of an unknown element, "nitrioum" or "ammonium"; while, because of the pivotal importance of oxygen in his system, Berzelius did not relinquish his belief that nitrogen was an oxide of an unknown element until 1820. Even Thomson, in his discussion of the nature of nitrogen, wrote that "azote is a compound body can scarcely be doubted. That it contains oxygen is probable, from its little combustibility." (33) Nevertheless, he concluded in the manner of Lavoisier, that nitrogen should remain an element "till some fortunate experimenter succeed in showing us the constituents of azote."

33. Ibid., 3,139,1814.
In a theoretical paper published in Thomson's *Annals* a few months later in 1814, a young jeweller's assistant, John Miers, attempted to reduce the elements to hydrogen and oxygen on the analogy that these two elements were the components of water, nitric acid, nitrogen and ammonia (*sic*).

It is needless to point out the several others [i.e. analogies] that must occur to those who investigate the subject, as the whole vegetable and animal world present such numberless instances of wonderful arrangements of the most complex materials formed of a few primary elements by the most simple means that could have been devised. (35)

Water was a binary compound of hydrogen and oxygen; nitrogen, Miers suggested, might be composed of six atoms of hydrogen and one of oxygen. In modern notation,

water \[ HO \]
nitrogen \[ N = H_6O \]
nitric acid \[ NO_5 = H_6O_6 \]
nitrous acid \[ NO_3 = H_6O_4 \]
nitrous gas (nitric oxide) \[ NO_2 = H_6O_3 \]
nitrous oxide \[ NO = H_6O_2 \]
ammonia \[ NH_3 = H_9O \]
ammonium \[ NH_4 = H_{18}O \]

These speculative compositions agreed with the experimentally determined compositions of these substances; and if it could be shown

34. Miers (1769-1879) later became a very distinguished botanist and Fellow of the Royal Society. From 1838 until his death he worked on the botanical collections which he had acquired during his years in South America, 1816-38. He was an opponent of evolution. See, *Proc.Roy.Soc.*, 29, xxii-iii, 1879, and *Dict.Nat.Biography*.

experimentally that nitrogen was a compound of hydrogen and oxygen, this would lend considerable support to reductionist considerations:

As it is seen that, by the union of the simple elements with two kinds of compound atoms of a double series, the one formed of a particle of oxygen with one of hydrogen, the other of a particle of oxygen with six of hydrogen, so great a variety of compounds may be generated, a question naturally arises, why may not the atoms of oxygen and hydrogen be capable of uniting in more than these two proportions, and why may not other kinds of matter, at present deemed simple, have also atoms of the same order, but of different members of the same two sorts of elementary atoms? (36)

The suggestion that two of the common elements like hydrogen and oxygen, or carbon and oxygen, combined together to form all the known chemical elements, became a fairly common variety of reductionist speculation. (37) Miers envisaged molecular units of hydrogen and oxygen which resisted chemical decomposition and which were the known "elements" such as nitrogen, chlorine, boron, etc. The "happy state of simplicity" which would result from the verification of this speculation was a challenge which chemists were not to ignore.

The field is now open for all who feel interested in this enchanting pursuit; the extent of research is boundless beyond conception; and there may probably be gained by the beautiful system of atomic combination a more certain and accurate view into the secret operations of nature than has been obtained by all the valuable discoveries that have enriched the science of chemistry of late years. ... To others far more competent must be left the prosecution of this important task. (38)


37. E.g. D. Low, An Enquiry into the simple bodies of Chemistry, 1844 (see infra); T. Carnelley, Chemical News, 53,157,169 and esp. 197-200,1866.

38. Ann.Phil., 3,371-2,1814. Miers did caution that it was perhaps too early for such speculations and, ironically, he advised chemists to first collect their empirical data in the manner of such masters as Thomson.
It was Prout and Thomson who took up this challenge.

From the foregoing it will have been seen that there are points of contact between Miers and Prout: both were concerned with the simplicity that would result from a reduction in the number of elements. Miers built his speculations on the prime elements, hydrogen and oxygen, which had been singled out by Davy in his textbook. Similarly, in his first anonymous paper, Prout referred to both these elements as the units for arithmetical relationships. Miers adopted, in a clear manner, a molecular theory of matter, and unlike Davy he associated the reductionist hypothesis with proportions by weight. Both these steps were also taken by Prout. Finally, there is a more personal point of contact. Miers lived with his father in the Strand and, until the end of 1814, Prout was his neighbour in Arundel Street, just off the Strand. The possibility that Miers attended Prout's lectures in 1814 cannot be proved, but since, like Michael Faraday, he was a young man intent on self-improvement and on an education in the sciences, his attendance at a local series of science lectures seems to me to be highly probable.

It can be demonstrated that Prout had read Miers' papers. In the second half of 1814, Miers published the details of experiments which purported to prove that nitrogen was a compound of hydrogen and oxygen. He began with water, and on the basis that nitrogen (according to his speculations) contained more hydrogen than oxygen ($H_2O$), he tried to remove the oxygen from water and transform it into nitrogen by passing

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steam and hydrogen sulphide through a hot copper tube. Miers obtained a number of very conflicting results including, in one case, the production of ordinary atmospheric air, and in another, the production of a "sulphureted azotic gas" which caused a black precipitate to form when it was passed into caustic potash. In both these cases Miers claimed that his suspicions of the compound nature of nitrogen had been given a high degree of probability; but as Thomson commented in his Annual Report on the progress of Chemistry in 1814, Miers' results were too inconsistent to prove anything.\(^{(40)}\)

Many years later, in 1820, Prout pointed out that the so-called "pure potash" used by analytical chemists usually contained small quantities of silver, and sometimes lead, iron or copper. This could be shown easily by passing hydrogen sulphide through potash.

The presence of these metals in the alkalies has doubtless often misled chemists by inducing unnatural appearances. I may mention one striking instance. Some years ago, Mr Miers announced that he had discovered a gas having the property of precipitating potash and soda black. The gas was a mixture containing sulphuretted hydrogen, and the precipitates arose from metallic impregnation in the alkaline solutions employed. \(^{(41)}\)

Since Prout stated that his discovery of the impure nature of common potash had been made "many years ago", it appears likely that he made it as a direct result of reading Miers' papers and repeating the experiments. If this is the case, then Miers' speculations assume some significance, and might go some way towards explaining the diffident manner in which Prout announced the protyle hypothesis in 1815-6.

\(^{(40)}\) Ann. Phil., 5, 14-5, 1815.

\(^{(41)}\) Ibid., "Pure potash", 16, 150, 1820.
We have now completed a survey of some of the speculations of Prout, Davy, Thomson and Miers which were proposed before the publication of Prout's first anonymous paper in 1815. We concluded from the evidence of the undergraduate essay, De Facultate Sentendi, that Prout's belief in the unity of matter was developed from a non-atomic context, the qualitative physics of Aristotle, in which there was a prime matter that gave identity to a world of changing forms. The evidence of the confused manuscripts of his lecture notes on animal chemistry which were delivered in 1814 showed that he retained this commitment to the unity of matter after he had learned of the atomic theory; by which time he would also have been aware of the speculations and suggestions of Davy, Thomson, and possibly also those of John Miers. It is not known when Prout first came to know of Dalton's atomic theory. His chemistry teacher at Edinburgh was Thomas Hope, an opponent of Dalton's theory, and although Dalton had lectured on his theory at Edinburgh in 1807, it seems unlikely that Prout could have become familiar with Dalton's ideas during his own stay there as a medical student, 1808-11. In none of the manuscript material for the period 1810 to 1814 is there any mention of the atomic theory. We cannot tell whether he read Dalton's textbook during this period, or whether he reflected on the brief references to Dalton's theory made by Thomson in his System of Chemistry. Even

42. J.R. Partington, History, 3, p. 797.
43. A New System of Chemical Philosophy, Manchester, 1808-10; the atomic theory was explained in the part published in 1808.
if he had read these sources there is little reason to suppose
that he would have grasped the significance of the atomic theory
until the publication of Thomson's and Berzelius's appraisals of the
theory, together with an appraisal of Gay Lussac's law of combining
volumes, in 1813.\textsuperscript{45} After all, Thomson made a point of exclaiming
in 1813 how little Dalton's theory had been studied in England.\textsuperscript{46}
From Thomson, and to a lesser extent Berzelius, Prout would have
acquired a knowledge of the importance of atomic weights and specific
gravities. It was the publications of Thomson and Berzelius, together
with the publication of Wollaston's table of Synoptic Equivalents,\textsuperscript{47}
which undoubtedly led to Prout's paper on specific gravities in which
the integral multiple weights hypothesis was first formulated. These
mathematical relations, which were to attract the attentions of
Thomson, were for Prout, as he revealed in 1816, an indication of the
unity of matter to which, privately, he had been committed for several
years. We shall now examine these two famous papers in detail.

Prout's Anonymous Papers on Specific Gravity

Prout's first anonymous paper appeared in Thomson's Annals of
Philosophy for November 1815 with the innocent title, "On the relation
between the Specific Gravities of Bodies in the Gaseous State and the
Weights of their Atoms."\textsuperscript{48} As we shall notice more fully in the

\begin{itemize}
\item \textsuperscript{45} Ann.Phil., 2,32,1813 (Thomson); \textit{ibid.}, p.443 (Berzelius).
\item \textsuperscript{46} \textit{Ibid.}, 1,146,1813.
\item \textsuperscript{47} Phil.Trans., 1814, p.1.
\item \textsuperscript{48} Ann.Phil., 6,321-30,1815; Prout's Hypothesis, Alembic Club
Reprint, Edinburgh, 1932, pp.25-37 (for convenience, all quotations will be taken from this source).
\end{itemize}
next chapter, to single out the unitary hypothesis from this paper (where in any case it is not explicitly mentioned) and its sequel in 1816, is to misjudge Prout's purpose which was explicitly indicated in the title; namely, a study of the relation between the combining weights of substances, and the combining volumes of the same substances in the gaseous state. He may well have undertaken the task after reading Berzelius's recommendation in the conclusion of his long essay on the "cause of Chemical Proportions" (49) that research into "the relation between the specific gravity of a compound body and the contraction which its elements undergo in combining" would be of great value in the development of the atomic theory.

Prout's claim that his observations were "chiefly founded on the doctrine of volumes as first generalised by M. Gay Lussac; and which, as far as the author is aware at least, is now universally admitted by chemists", (50) suggests that he was unaware of Dalton's rejection of the law of volumes in 1810. (51) If, as D.M. Knight has recently suggested, (52) Prout's paper represented a compromise between the positions adopted by Berzelius and Dalton in their respective treatments of the atomic theory, (53) he would hardly have been unaware of this.

50. Prout's Hypothesis, p. 25.
52. Knight, loc. cit., ref. 5, p. 206.
This fact, together with Prout's treatment of air as a chemical compound, leads me to think that Prout knew very little of Dalton's own opinions at this time. It would probably be more true to say, therefore, that Prout saw himself as an arbitrator, not between Dalton and Berzelius, but between the gravimetric atomic theory as expounded by Thomson and Berzelius on the one hand, and Gay Lussac's volumetric relations on the other; just as Avogadro had attempted in 1811 (54) and Berzelius himself in 1813. (55)

Prout began his paper with a calculation of the specific gravities of the elementary gases oxygen, nitrogen, hydrogen and chlorine. The values for oxygen and nitrogen were obtained from the surprising assumption, contrary to Dalton's views, that atmospheric air was a chemical compound, even though there was no condensation of volume when 4 volumes of nitrogen were "combined" with 1 volume of oxygen in the formation of air. He found a justification for this assumption in the constant composition of air, and claimed that it was an original view. However, the notion that air was a chemical compound had been widely accepted at the beginning of the nineteenth-century, and it had been strongly rejected by Dalton. This seems to be an indication of Prout's lack of training in chemistry, or a familiarity with its


55. Ann.Phil., 2,359,1813. In 1821, a mathematician named Charles Sylvester claimed priority over Prout: "The relation of the specific gravity to the weight of the atom I pointed out to Mr. Dalton, Dr. Henry, and to Dr. Thomson, long before the account of it was published by Dr. Prout in Thomson's Annals", Ann.Phil., 18(2),213,1821.
Since the volume ratio of nitrogen to oxygen was 4:1, and Wollaston's equivalent weights for these gases were respectively 17.5 and 10, Prout judged that air was a compound of 2 atoms of nitrogen and 1 atom of oxygen. (i.e. $N_2O$, or 77.77% N to 22.22% O). Then, by simple algebra, he calculated the specific gravities of the two gases and found them to be in good agreement with experimental values. (Air = 1).

<table>
<thead>
<tr>
<th></th>
<th>calculated</th>
<th>experiment</th>
</tr>
</thead>
<tbody>
<tr>
<td>nitrogen</td>
<td>0.972</td>
<td>0.969 (Biot &amp; Arago)</td>
</tr>
<tr>
<td>oxygen</td>
<td>1.111</td>
<td>1.104 (Thomson)</td>
</tr>
</tbody>
</table>

But how did Prout arrive at, or justify, the opinion that air contained 2 atoms of nitrogen and 1 atom of oxygen? He must have worked back from the experimentally determined specific gravities which indicated that the atom of nitrogen was specifically lighter than the atom of oxygen. How could this be resolved with Wollaston's atomic weights of nitrogen and oxygen unless either the nitrogen atom was split during its combination with oxygen, so that $N_2 = 17.5$, or the volume occupied by an oxygen atom was exactly half the volume occupied by...

56. The compound nature of air was entertained by both Thomson (System of Chemistry, Edinburgh, 1802, vol. 3, p. 271) and by Berthollet. For Dalton's views see Partington, History, vol. 3, p. 765ff. Dalton's attack on the 'air is a compound school' was renewed in 1817, Ann.Phil., 9,183,1817, and this is the only indication that he had ever read Prout's anonymous paper. As late as 1836 Thomson continued to support the view that air was a compound, $N_2O$; see his Records of General Science, 3,185,1836, where Prout's arguments of 1815 are given without acknowledgement.
an atom of nitrogen, so that, in modern terms, \( O = 3 \)?

The former interpretation, if correct, would have required that air contained 2 compound atoms of nitrogen to 1 simple atom of oxygen. I.e. in volumes,

\[
2(N_2) : 1(0) \\
or \quad 4N : 1(0) \\
or even \quad 4N : 1(O_2) \quad \text{where 1 atom of oxygen occupied } \frac{1}{2} \text{ a volume.}
\]

However, none of this is explicitly stated, and the only justification for this interpretation is the fact that Prout did "split" atoms many years later, and at the same time refer to this paper of 1815 as containing the notion of a molecular theory, even though it had not been at all clearly stated. It was certainly an understatement for Prout to admit that his ideas had not been clearly presented; in fact the whole effect of his anonymous paper is one of intellectual confusion. It is for this reason that I believe that a more favourable interpretation of this passage is that Prout treated the oxygen atom as exceptional, namely, that whereas other gaseous atoms occupied one volume, oxygen occupied only half a volume. This was certainly how Thomson understood this passage. Later, at some unknown date, Prout saw that a simpler, more uniform system would be produced if atoms

57. The problems of reconciling gaseous combination with gravimetric composition had confronted (i) Dalton, who had solved them by the rejection of the law of combining volumes, and (ii) by Berzelius who set up a principle that equal volumes of the simple gases under the same physical conditions contained equal numbers of atoms. Prout implicitly adopted Berzelius's position except for his treatment of oxygen, where he followed Thomson. See text.

58. Phil.Trans., 1827, p.354; Phil.Mag., (3)5,132,1834.
(ie. molecules) were conceived to be at least diatomic. But this
would have entailed the adoption of the principle that equal volumes
of gases under the same conditions contained the same number of
particles—a principle which Dalton rejected, and neither Berzelius
nor Thomson were able to accept.

Thus, in a significant footnote, Prout pointed out that since:

one volume of hydrogen combines with only half a volume of
oxygen, but with a whole volume of iodine ... the ratio in
volume ... between oxygen and iodine is as $\frac{1}{2}$ to 1, and the
ratio in weight is as 1 to 15.5. Now .5555, the density of
half a volume of oxygen, multiplied by 15.5, gives 8.6111, and
8.6111 + .06944 = 124. Or generally, to find the
sp.gr. of any substance in a state of gas, we have only to
multiply half the sp.gr. of oxygen by the weight of the atom
of the substances with respect to oxygen. (59)

Prout's relationship,

$$\text{sp.gr. of gas } X = \frac{\text{sp.gr. of oxygen}}{2} \times \frac{\text{atomic weight of } X}{\text{atomic weight of oxygen}}$$

(is air = 1)

is analogous to the relationship,

$$\text{vapour density of gas } X = \frac{\text{molecular weight of } X}{\text{molecular weight of } H}$$

(H = 1)

or,

$$\text{Mol. weight of } X = "\text{twice the vapour density" of } X.$$

Prout's first paper continues, however, to present further problems
and signs of confused and hasty composition. It must therefore be
seriously doubted whether he realised the full significance of his
calculations at that time.

59. Prout's Hypothesis, p. 29n, my stress.
His determination of the specific gravity of hydrogen was of the greatest contemporary importance since, being the lightest known gas, its specific gravity was extremely difficult to determine experimentally. (60) Prout proposed that the traditional method of direct weighing of the hydrogen should be abandoned and replaced by a calculation based upon the specific gravity of a denser compound into which it entered in a known proportion, and whose specific gravity could be accurately determined. Ammonia was ideal for this purpose, and using Gay Lussac's observation that ammonia consisted of 3 volumes of hydrogen and 1 volume of nitrogen condensed into 2 volumes, he calculated algebraically that: (61)

If, \( 3h + 1N = 2 \) ammonia,

\[ \text{sp. gr. ammonia} = \frac{3}{2} \text{sp. gr. hydrogen} \times \frac{1}{2} \text{sp. gr. nitrogen} \]

And if \( x \) is the sp. gr. of hydrogen, \( \frac{3x + 0.9722}{2} = 0.5902 \)

Hence, \( x = \frac{1.1804 - 0.9722}{3} = 0.0694. \)

(The specific gravity of ammonia had been determined by Davy as 0.59016, and by Biot and Arago as 0.59; Prout arbitrarily took the average of 0.5902). Prout's calculated value of 0.0694 for the specific gravity of hydrogen was a good deal less than Thomson's experimental value of 0.073. (62) Prout was naturally jubilant when Dulong and Berzelius

60. "The specific gravity of hydrogen, on account of its great levity, and the obstinacy with which it retains water, has always been considered as the most difficult to take of any other gas", Prout's Hypothesis, p. 26.

61. Note that Prout was attacked by Ure and Brande for using algebra, quarterly J. Science, 13, 322-4, 1822.

62. The modern value is 0.06952 at N.T.P. The values for ammonia and atmospheric nitrogen are 0.5963 and 0.9722.
made a new experimental determination of the specific gravity of hydrogen in 1818 which confirmed his theoretical value. \((0.069)\) (63)

It is interesting, incidentally, to notice the effect which this new experimental determination had on Dulong himself. When asked by Berzelius what he thought of Prout's multiples hypothesis, he commented:

> Je vous dirai franchement que lorsque le mémoire du docteur Prout parut, je ne peux pas défendre l'impression qu'il me fit, quoique assurément ses arguments ne fussent pas de nature à entraîner la conviction. Je fus le seul des rédacteurs des Annales de Physique et de Chimie, dont je faisais alors partie, qui ne traita pas ce mémoire de vision cornue. J'en donnais un extrait dans le premier volume de ce recueil. (64)

> Depuis je n'ai pas perdu de vue cette idée originale. Lorsque nous avons déterminé de nouveau les densités de plusieurs gaz, je me suis empressé de vérifier son exactitude. Ainsi que vous l'avez remarqué, les nombres de ceux que Prout avait donnés. C'est surtout dans la vue de décider cette question que je vous engageais à faire l'analyse de l'air, à la precision du millième, afin de connaître le rapport des densités de l'oxygène et de l'azote. (65)

63. Ann.Phil., 18(=2),48-50,1821. See Marcet's letter to Berzelius, 29 March 1819, "Prout est enchanté de vos résultats sur l'atome d'hydrogène; il était arrivé par le calcul à des conclusions tout à fait semblables", Berzelius Bref, vol.1, iii, p.168. Thomson was also jubilant: "It would have been wonderful indeed if [Prout's value had been immediately accepted] and if some of those chemical understrappers who are unable to think with precision, or indeed to think for themselves at all, had not come forward with their sneers, as if it were an unpardonable sin to deviate in any respect from the ipse dixit of those individuals whom they have thought proper to set up as the gods of their idolatry", Ann.Phil., 13,xxvii,1819.

Stas remarked that: "Right or wrong [the rare penetration of Prout] permitted him to divine the specific weight of hydrogen forty years before it was determined experimentally with any degree of accuracy", Prout's Hypothesis, p.42.

64. Ann.Chim.,1,41-16,1816. It is more an abstract than a translation; Prout's second paper was never translated.

65. Berzelius, Bref, vol.2, iv, p.36. It seems extraordinary that Dulong had not discussed this matter with Berzelius before.
Dulong was referring here to Proct's comparison between the calculated values for the specific gravities of hydrogen, oxygen and nitrogen, and his comment that "the specific gravity of oxygen as obtained is just 16 times that of hydrogen as now ascertained, and the specific gravity of azote just 14 times." (66) If, as Berzelius had suggested, equal volumes of gases under the same physical conditions contained equal numbers of atoms, then (on the scale, 1 vol. H = 1) equal volumes of oxygen weighed 16, and of nitrogen, 14. In other words, the atoms of oxygen and azote weighed respectively 16 and 14. However, as we have seen, Proct preferred at this time to reason that the oxygen atom only occupied half a volume, so that its atomic weight was only 8.

A similar calculation and adjustment was made for chlorine on the basis of the experimentally determined specific gravity of hydrogen chloride gas. Davy's, and Biot and Arago's values for hydrogen chloride were arbitrarily adjusted from 1.278 to 1.2845 upon the grounds that the lower value was judged "erroneous in the same proportion that we found the specific gravity of oxygen and azote to be above, (which though not rigidly accurate), may yet be fairly done, since the experiments were conducted in a similar manner." (66) The calculated specific gravity of chlorine was in this way found to be "very nearly" 2.5, in close agreement with Thomson's experimental value of 2.483. In fact, the original value for hydrogen chloride, 1.278 (Davy) would have given Proct a figure, 2.4866, which was in even closer agreement

(66) Proct's Hypothesis, p. 27, my stress. The actual figures as calculated should be 16.01 and 14.008.
to Thomson's value; however, it would have had the disadvantage that on the scale $H = 1$, it would have given $C_1 = 35.83$, which was ambiguously far from an integral number. On the other hand, the adjusted figure for hydrogen chloride used by Prout gave a value of 36.02 for chlorine, or "exactly 36 times that of hydrogen". (67)

So much for calculations prearranged to conform to an hypothesis! It is quite obvious that Prout deliberately chose values for the specific gravities of substances which would produce integral numbers on the hydrogen scale; and he can only have done this because he felt that these numbers had some sort of physical significance. Since Prout's paper was so liberally sprinkled with such dubious qualifying phrases as "just", "does not differ much from", "exactly", and even the ultimate absurdity, "correct, or nearly so", (68) it is surprising that hardly any of the bitter criticism which was thrown at Thomson was applied to Prout.

In a second part of his paper, Prout extended his treatment to the solid elements, iodine, carbon, sulphur, phosphorus, calcium, sodium, iron, zinc, potassium, and barium. This extension is remarkably similar to the extrapolation which Avogadro had made to the applicability of his hypothesis. (69) Most of Prout's estimations involved quantitative analyses of his own devising, and

67. Prout's Hypothesis, p.28, my stress.

68. This has been the subject of comment by J. Kendall, "Adventures of an Hypothesis", Proc.Royal Soc.Edinburgh, 63A, 1-2, 1949-52, "No element, it is obvious, could fail to fit into a scheme of such flexibility."

they were not without originality and skill, despite Thomson's feeling that they were open to various inaccuracies. (70) However, the procedure still remained flexible, and convenient specific gravity values were always chosen. For example, Gay Lussac's value for the atomic weight of iodine was 156.21 (0 = 10), but Prout considered this too high and lowered it to 155. This gave a specific gravity of 8.61025 which Prout printed as 8.611111 (sic), or 124.06 times the specific gravity of hydrogen; in Prout's terms, "exactly 124". (71)

It is obvious, therefore, that although according to the title of his paper Prout promised to calculate specific gravities from atomic weights, in fact he chose atomic weights which would produce integral specific gravities when compared with hydrogen. The net effect was the correction of atomic weights by means of an integral weights hypothesis. In the case of his own experiments which "were made with the greatest possible attention to accuracy, and most of them were many times repeated with almost precisely the same results," Prout was clearly justified in making such adjustments since he knew the experimental error involved. (72) Equally clearly, he was not justified in adjusting the experimental results of other chemists.


71. However, the value 124.06 is based upon 8.61025, and the figure 8.611111 is presumably a misprint. Carbon was calculated as 0.4166 or "exactly 12 times that of hydrogen". This should obviously be 6, as was printed in Table 1. It is another example of the numerous errors scattered through the paper.

72. Prout's Hypothesis, p. 31.
All the results were summarised in a Table which showed the calculated gaseous specific gravities \((H = 1)\), atomic weights \((1 \text{ volume } H = 1)\) \((73)\), atomic weights \((0 = 10)\) from calculation, the calculated specific gravities \((\text{air} = 1)\), the experimental specific gravities \((\text{air} = 1)\), the weights in grains of 100 cubic inches of the substances at 30 inches pressure and 60°F. \((74)\), and finally, the experimentally determined grain weights of 100 cubic inches. Two other Tables exhibited "the proportions, both in volume and weight, in which they [the elements] unite with oxygen and hydrogen." \((72)\) However, by extending his calculations to compound gases Prout introduced (like Dumas later) a confusion of nomenclature since he omitted to qualify the word atom by the admittedly paradoxical description compound. Worse still, by not clearly stating the principles of his molecular philosophy, Prout muddled together atoms and volumes.

(The) table also exhibits one or two striking examples of the errors that have arisen from not clearly understanding the relation between the doctrine of volumes and of atoms. Thus ammonia has been stated to be composed of one atom of azote and three of hydrogen, whereas it is evidently composed of one atom of azote and only 1.5 of hydrogen, which are condensed into two volumes, equal therefore to one atom. \((75)\)

73. Misprinted in 1815 as 2 volumes \(H = 1\); the second paper was written ostensibly to correct this. See note 88.

74. Obtained by multiplying the specific gravity \((H = 1)\) by 100 and dividing the result by 47.21435 \((\text{sic, the volume occupied by 1 grain of air})\).

75. Prout's Hypothesis, p.36. For some remarks on the confused nomenclature in nineteenth-century atomic theory see the M.Sc. thesis of N.G. Coley, ref. 69.
Prout's argument appears to be that since according to Gay Lussac

1 vol. nitrogen + 3 vols. hydrogen = 2 vols. ammonia

that the two volumes of ammonia had to be replaced by one compound
atom of ammonia; hence the three volumes of hydrogen had been distri-
buted or condensed into one atom of ammonia. Prout's weight equa-
tion was, therefore,

1 atom nitrogen + 1.5 atoms hydrogen = 1 compound atom ammonia.

Nevertheless, in the corrected Table published by Prout in 1816, the
weight equation was given as

1 atom nitrogen + 3 atoms hydrogen = 1 compound atom ammonia

and no reason for this particular correction was given.

In a final Table Prout grouped together the specific gravities
of the common metals as calculated from Berzelius's atomic weights. (76)

Significantly, all of them were integral like the common gases and
non-metals of the first Table. (77) Prout commented that:

all the elementary numbers, hydrogen being considered as 1,
are divisible by 4, except carbon, azote, and barytium
[i.e. barium], and these are divisible by 2, appearing
therefore to indicate that they are modified by a higher
number than that of unity or hydrogen. (78) Is the other
number 16, or oxygen? And are all substances compounded
of these two elements? (79)

76. In a published note, Ann. Phil., 6,472,1815, Prout explained
why these atomic weights were slightly different from those of
Berzelius; "their having been founded on the deductions of
others, (principally of Dr. Thomson) from the experiments of
Berzelius, and not upon that chemist's own deductions." See
Prout's Hypothesis, p. 37.

77. The relevant note to Table IV was misprinted as the note to
Table III -another example of Prout's carelessness.

78. Prout overlooked chromium = 18 which is divisible by 2, another
carelessness.

The significance, if any, of Prout's divisors 2 and 4 is obscure, and one wonders what the contemporary reader made of the expression "modified by". Possibly, if we allow that Prout might have been already in possession of the series hypothesis (infra), then these divisors would have represented sub-molecular units. This would be equivalent to saying that molecules comprised two or four particles. I believe this to be the correct interpretation, but since none of Prout's contemporaries could have known this, they must have found this passage incomprehensible. Even if by the phrase, "elementary numbers", Prout referred to both the atomic weights and the specific gravities on the hydrogen scale, there seems to be nothing in the tables to have justified Prout's query whether oxygen was the other Urstoff. This makes it highly probable that he was referring implicitly, or obliquely, to Davy's, or possibly Miers' speculations. It should be noted, however, that there was no explicit formulation of the protyle hypothesis in this paper; this followed in the second paper.

Instead, Prout picked out for principal comment the integral values of the atomic weights on the hydrogen scale; he noted with satisfaction that: "Substances in general of the same weight appear to combine readily, and somewhat resemble one another in their nature." (79)

Ernst von Meyer, in his History of Chemistry, accused Prout of arbitrarily altering the numerical values of atomic weights "so that they should not merely be whole numbers, but should also show regular differences among each other." (80) This seems to be true, since a

number of atomic weight values were in arithmetical series.

<table>
<thead>
<tr>
<th>Ca</th>
<th>Na</th>
<th>Fe</th>
<th>Zn</th>
<th>Cl</th>
<th>K</th>
</tr>
</thead>
<tbody>
<tr>
<td>20</td>
<td>24</td>
<td>28</td>
<td>32</td>
<td>36</td>
<td>40</td>
</tr>
</tbody>
</table>

No doubt privately Prout thought of "missing" elements, just as Mendeleeff was to do. This also underlines the fact that Prout, like Thomson, was searching for a periodic law as well as a theory of the elements.

Prout closed his paper with a final enigmatic generalisation that "all the gases, after having been dried as much as possible, still contain water, the quantity of which, supposing the present views are correct, may be ascertained with the greatest accuracy." This conclusion did not follow from the tables; it is little wonder that Dulong, and later Kopp, found this remark extremely puzzling.

Verbosity can be a great failing in scientific literature, but equally, extreme tersity can lead to obscurity. It could be argued that Prout was playing the role of Thales; all things are water, and how much water they are can be estimated. But a more likely explanation is that Prout saw that the amount of water vapour in gases could be estimated by some sort of correction factor once the ideal specific gravities had been calculated.

81. In a penultimate generalisation, Prout observed that oxygen appeared not to enter into compounds in the ratio of 2 volumes or 4 atoms. This fortuitous observation was due solely to his premise that oxygen only occupied half a volume.

It is a relief to turn away from these obscurities and fail-
ures of presentation to Prout's short correction of 1816 which, though
still not entirely free from ambiguity, is a model of clarity in
comparison with the first paper. The muddle over atoms and volumes
was resolved when Prout proposed that the advantage of relating or
correlating atoms with volumes, by the assumption that one volume of
hydrogen was occupied by one atom of hydrogen, was that,

the specific gravities of most, or perhaps all, elementary
substances (hydrogen being 1) will either exactly coincide
with, or be some multiple of, the weights of their atoms. (84)

If, on the other hand, the assumption was made that one volume of
oxygen was occupied by one atom of oxygen (viz. 2 vols. H = 1 atom), the
relation would not be so simple, for then "the weights of the atoms
of most elementary substances, except oxygen, will be double that of
their specific gravities with respect to hydrogen." Unknown to Prout,
this assumption had been proposed already by Avogadro; it was to be
made by Prout in 1834 and is equivalent to the classical chemical
relation:

molecular weight = 2 (vapour density)

This relationship appears to have been the source of Prout's error in
the Table headings published in 1815. (85)

However, in the 1816 correction Prout remained orthodox and
employed the first, simpler relation to demonstrate how the specific


84. Prout's Hypothesis, p. 40.

85. "Weight of atom, 2 vols. of Hydro. being 1", ibid., p. 32. This
was corrected to read "weight of atom, 1 vol. hydrogen being 1"
"upon which latter supposition the tables were actually
constructed, ibid., p. 38-9.
gravities of elements in the gaseous state could be rapidly calculated by means of the Wollaston slide rule. The protyle hypothesis followed immediately:

If the view we have ventured to advance be correct, we may almost consider the \( \frac{1}{16} \) of the ancients to be realised in hydrogen; an opinion, by the by, not altogether new. If we actually consider this to be the case, and further consider the specific gravities of bodies in their gaseous state to represent the number of volumes condensed into one; or, in other words, the numbers of the absolute weight of a single volume of the first matter \( \frac{1}{16} \) which they contain, which is extremely probable, multiples in weight must always indicate multiples in volume, and vice versa; and the specific gravities, or absolute weights of all bodies in a gaseous state, must be multiples of the specific gravity or absolute weight of the first matter \( \frac{1}{16} \), because all bodies in a gaseous state which unite with one another unite with reference to their volume. (84)

That such an opinion was not altogether new was, as we have seen, a reference to Davy, and perhaps to his own private thoughts. It should be noticed that Prout no longer referred to the multiple of oxygen; unless the guarded qualification that hydrogen was almost the protyle of the ancients was an oblique reference to other primordial units of matter. However, this is equally likely to be a guarded reference to the suggestion which Prout allowed to be published in 1831 that the ultimate unit of matter might be a sub-multiple of hydrogen. (86)

Until then, Prout's contemporaries understood him to mean that the absolute value of the specific gravity represented the number of volumes of hydrogen which had condensed together to form a particular "element".

86. G. Daubeney, An Introduction to the Atomic Theory, Oxford, 1831, p. 129; see infra.
where, if \( n = 6 \), the element was carbon

if \( n = 14 \), the element was nitrogen, etc,

and \( x \) is an integer which until 1831 was taken to be 1.

Although Thomas Thomson was the first to reveal Prout's identity in print in the May issue of his journal in 1816,\(^{(87)}\) he had said earlier that he had a good idea of the anonymous author's identity.\(^{(88)}\) This would not have been difficult for him since as editor of the Annals he would have been familiar with Prout's erratic handwriting from Prout's previous submissions for publication. Prout himself drew attention to his authorship in the June issue of the Annals of Medicine and Surgery\(^{(89)}\) in the same year, and not in 1817, as is usually stated.\(^{(90)}\) This suggests that he personally gave Thomson permission to reveal his identity.

It is intriguing to notice how after his initial diffidence about the authorship, Prout became quite proud of these papers since he frequently (and sometimes unnecessarily) referred to them in the course of his later publications.\(^{(91)}\) The reasons for his anonymity remain obscure; the 1815 paper was neither his first publication, nor his first venture into chemistry, and he was already well known in London.

\(^{87}\) Ann.Phil., 7,343,1816.

\(^{88}\) Ibid., p. 17.

\(^{89}\) Ann.Med.& Surgery, 1,150,1816.


\(^{91}\) E.g., Phil.Trans., 1827, p. 354; Phil.Mag., (3)5,133,1834.
as a physician and animal chemist. It was Stas who said: "Prout had so little faith in the exactness of his hypothesis that he published it under the veil of anonymity". (92) But in his lectures in 1814 Prout had evidently not hesitated to reveal his commitment to the belief in the unity of matter, and in any case this was a fairly common belief. It would appear, therefore, that the anonymity was connected with his contribution to the atomic theory, rather than the speculation concerning the complexity of the elements. Perhaps he felt unqualified to make a contribution to the atomic theory because it appeared to be the research territory of the theoreticians and experimentalists, Thomas Thomson and Jöns Berzelius. He certainly sufficiently distrusted his own proficiency as an experimentalist (where he actually excelled) that he took the bulk of his experimental data from "superior(s) to himself in chemical experiments and fame." (93)

The protyle hypothesis cannot be a sufficient reason for the anonymity since it did not figure explicitly in the first paper, and if it had not been for the errors in this paper, presumably Prout would not have published the hypothesis at all at this date.

The fate of Prout's Hypotheses, 1816 to 1850

As we have just seen, Prout announced publicly in 1816 that since he had demonstrated, largely by calculation, that the atomic weights on the hydrogen scale were whole numbers, possibly all the so-called elements were really polymers of hydrogen. The simplicity for which many natural philosophers had been searching since Lavoisier

92. Prout's Hypothesis, p. 42.
93. Ibid., p. 25.
had defined the concept of the element was over; the embarrassing status of Lavoisier's elements was resolved; back to Thales, everything was hydrogen!

Such an hypothesis was obviously exciting, and from our twentieth-century hindsight we realise that there was more than a hint of truth in the idea. Potentially, the hypothesis could be tested, and therefore, after a slow beginning, catalysed by Thomson, it inevitably stimulated analytical work. (94) For if chemists could show by exact and improved methods of analysis that atomic weights were integral multiples of the atomic weight of hydrogen within the limits of experimental error, then this would provide a means for correcting atomic weight values and support (but not prove) the view that everything was hydrogen. Proof that elements with integral atomic weights were made of hydrogen could only be given if these elements were decomposed with the evolution of hydrogen. (95) Once more we notice

94. Note that it was only after Thomson had publicised the multiple weights hypothesis. "It is a very remarkable circumstance", wrote Harry Rainy in 1826, "that though more than ten years have now elapsed since Dr. Prout's paper was published ... no chemists, except Dr. Thomson, should have engaged in any experimental researches on a subject so highly important", Ann.Phil., 27(=11), 193, 1826. The more conventional view appears to be due to J.F.W. Johnston: "The simplicity of this relation drew the immediate attention of chemists; for assuming it to be a law of nature, it was seen that could we accurately determine the true weight of hydrogen, we might at once correct all other atomic weights, and if not obtain for each substance the precise combining ratio, at least bring very close together the extreme limits of error", Brit.Ass.Reports, 1,415-6,1831/2 (1832). Nevertheless, Rainy was correct; it was the publication of Thomson's First Principles in 1823 that was the real stimulus.

95. Oddly, until the advent of the spectroscope, there appear to have been no chemists who followed this particular programme.
the importance of clearly distinguishing between the integral multiple weights hypothesis and the protyle hypothesis; the latter was a world-view or conceptual system that was really quite independent of the empirical verification or falsification of the former. One other fact also becomes clear. If Prout's protyle hypothesis were found to be true, then the status of Dalton's multi-element atomic theory would be threatened. An atomic theory of matter would still be possible, but the simple theory which Dalton had proposed would have to give way to the older corpuscular physics in the form of a molecular theory of matter. As we have already suggested, inevitably what happened in the nineteenth-century was that a dialogue took place between the school of multi-element chemists and the school of reductionists; and one result of this dialogue was the adoption of a molecular theory during the 1850s and 1860s.

What was the effect, we must now ask, of new improved chemical experimentation upon this dialogue? Although the story has been related several times before, (96) we must repeat the salient points together with new details in order to provide a context for Prout's molecular theory. Since the story is a long one, we shall restrict it by ending in 1850, the year of Prout's death.

A very general answer may be given first. Analysts tended to support Prout in Great Britain and America where the principal spokesman was Thomas Thomson, but reject it on the continent where the principal spokesman was Berzelius. (Like all generalisations, this one

96. See the works cited in ref. 5. No detailed history of atomic weight determinations has yet appeared in English.
provides a notable exception - Jean Baptiste Dumas). Essentially no public pronouncement on the controversy was made by Prout until 1827, and to all intents and purposes he appeared to have retired into the field of biochemistry. That he had not entirely lost interest in the matter will appear in the sequel.

The work of Thomas Thomson

In his anxiety to monopolize the honour of finishing the fabric of Dr. Prout [Thomson], unhappily for his fame, deranged the whole edifice, and instead of fixing the key-stone, has actually laboured unwittingly to displace it. (97)

In the paper which revealed Prout's identity and clarified his somewhat obscure remarks, Thomson (98) seized upon the suggestion that "the specific gravity of any body [on the air = 1 scale] may be obtained by multiplying the weight of the atom [on O = 1 scale] by half the specific gravity of oxygen gas." He noted that this relationship indicated that:

"Atomic" weight = 2 (specific gravity)

and that gaseous substances could be classified into three groups according to the relationship between "atomic" weight and specific gravity, on the scale 0=1 for both weight and volume. (It should be understood that by "atomic weights" Thomson included "compound atomic weights", that is "molecular weights"). In the first group, the values were identical; in the second group, the "atomic" weight was double

97. Andrew Ure, Quart.J. Science and Arts, 20,126,1826.

that of the specific gravity; and in the third group, the "atomic" weights were four times as great.

The weight of the atom is either equal to the specific gravity of the gas, or twice that weight, or four times that weight. (99)

<table>
<thead>
<tr>
<th>Group I</th>
<th>Sp.Gr. 0 = 1</th>
<th>At.Wt. 0 = 1</th>
</tr>
</thead>
<tbody>
<tr>
<td>oxygen</td>
<td>1.000</td>
<td>1.000</td>
</tr>
<tr>
<td>ethane</td>
<td>0.876</td>
<td>0.875</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Group II</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>phosgene</td>
<td>3.095</td>
<td>6.190</td>
</tr>
<tr>
<td>chlorine</td>
<td>2.250</td>
<td>4.500</td>
</tr>
<tr>
<td>cyanogen</td>
<td>1.621</td>
<td>3.242</td>
</tr>
<tr>
<td>carbon dioxide</td>
<td>1.374</td>
<td>2.750</td>
</tr>
<tr>
<td>hydrogen sulphide</td>
<td>1.062</td>
<td>2.124</td>
</tr>
<tr>
<td>sulphur</td>
<td>1.000</td>
<td>2.000</td>
</tr>
<tr>
<td>nitrogen</td>
<td>0.875</td>
<td>1.750</td>
</tr>
<tr>
<td>carbon monoxide</td>
<td>0.475</td>
<td>1.750</td>
</tr>
<tr>
<td>steam</td>
<td>0.5625</td>
<td>1.125</td>
</tr>
<tr>
<td>carbon</td>
<td>0.375</td>
<td>0.750</td>
</tr>
<tr>
<td>hydrogen</td>
<td>0.0625</td>
<td>0.125</td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th>Group III</th>
<th></th>
<th></th>
</tr>
</thead>
<tbody>
<tr>
<td>hydrogen iodide</td>
<td>3.986</td>
<td>15.944</td>
</tr>
<tr>
<td>hydrogen chloride</td>
<td>1.1557</td>
<td>4.623</td>
</tr>
<tr>
<td>ammonia</td>
<td>0.53125</td>
<td>2.125</td>
</tr>
</tbody>
</table>

These relationships are so good that it would be difficult not to suspect that there has been an adjustment of figures. In fact, in transforming from specific gravities on the scale air = 1, to 0 = 1, Thomson manipulated the final decimals occasionally so that there was complete accord with the known molecular weights. However, the differences are so small that it would be extremely unfair to imply that these alterations were unjustified. On this basis, therefore, it

seemed to Thomson that the atomic weights of the elements (with the exception of oxygen) were, as Prout had implicitly stated, double their specific gravities, or volumes.

Just as Thomson had taken up the flag of Daltonian atomism in 1813, he now took up Prout's integral multiples hypothesis; and with the object of verifying it, he engaged upon an arduous programme of analyses from 1818 to 1823. That verification was Thomson's sole aim is made clear from several of his remarks during the course of these researches. The fact that so many atomic weights do fall close to integral values was, of course, an apparent experimental confirmation of his commitment. The few outstanding exceptions to integral weights, like chlorine and copper, could only present a problem to Thomson's followers when analytical techniques had been made more rigorous and exact; even then a way out of this problem had been already suggested by Prout.

In 1817 Thomson published a new edition of his textbook in which he included hypothetical atomic weights that supported Prout's integral multiples hypothesis. The chemists of the Royal Institution school, who continually opposed Thomson personally for some real or imagined slight to their hero Sir Humphry Davy, led by Thomas Brande, attacked him from the pages of their journal. Bluntly, they accused both Prout and Thomson of rounding-off their experimental values.


101. J. Science and Arts, 4, 299-321, 1818. The review is in the same abusive style as the review of Berzelius's An Attempt to Establish a pure Scientific System of Mineralogy, in ibid., 1, 228-41, 1816. Since this was by the young J.F. Daniell (Berzelius, Dref, vol. 1, p. 144), he may also have been the abuser of Thomson who believed, however, that it was by the editor, Thomas Brande.
Thomson's citation of Prout's results for the specific gravities of oxygen, hydrogen and nitrogen in his textbook without giving the reasoning which lay behind their adoption, led the reviewer to examine Prout's original papers. After suggesting that Prout begged the question over the chemical constitution of the atmosphere, the reviewer argued that the reasoning of Prout and Thomson was circular.

It is curious to observe the cross application of the calculations of the two doctors. Dr. Prout deduces the specific gravities of oxygen and azote from the weights of their atoms 10 and 17.5, allowed by Dr. Thomson; and Dr. Thomson calculates the weights of the atoms from the specific gravities determined by Dr. Prout. (102)

Prout's argument from "theoretical considerations" for the specific gravity of hydrogen was pretentious, he continued, since all it amounted to really was the random adjustment of experimental findings. In any case, the accuracy of the ammonia method was suspect.

To us it appears that the sources of error in calculating from a body composed of three volumes are much more numerous (taking more particularly into consideration the nature of that body) than any which could arise from the old method, of direct weighing (103)

The reviewer also had sport offering contradictory quotations from Prout and Thomson, but most of his criticisms were only debating points. Finally, he noted the hypothesis of multiple weights, but rejected it as unconvincing with the valid comment that "we always have a jealousy of these just numbers". (103) The many changes which Thomson had introduced into atomic weight values between 1813 and 1817 were strong additional reasons for caution in the adoption of any more new values.

102. J. Science & Arts, 4,312,1818.

103. Ibid., p. 313.
This review has been mentioned at length since it was one of the few criticisms of Thomson which was also addressed at Prou. In the following year, 1818, Thomson published a new list of atomic weights to three decimal places on the scale, $0 = 1$.\(^{(104)}\) It contained several whole numbers, and several rounded fractions that were \(\frac{1}{8}\)th multiples of the atomic weight of hydrogen (\(8 \times 0.125 = 1.000\)).

Although these numbers could be interpreted as deliberate deceptions by Thomson, it is more likely that he believed that experimental errors should not be "allowed to countervail a general law of so much simplicity, and supported by so many probabilities."\(^{(105)}\) Surely all atomic weights (including molecular weights) were integral multiples of the atomic weight of hydrogen.\(^{(106)}\) In this paper Thomson accepted Prou's theoretical value for the specific gravity of hydrogen, and in the following year he obtained an experimental confirmation of it by the direct weighing of pure hydrogen which he had prepared from sublimed zinc and distilled sulphuric acid. No corrections for the presence of water vapour in the hydrogen and the air were recorded.\(^{(107)}\)

\(^{104}.\) *Ann.Phil.*, 12,338-50,436-41,1818; cf. "I have very slightly modified Gay Lussac's numbers to make an atom of iodine a multiple of .125, *ibid.*, p. 339a. Examples of values: 
\[H = 0.125, \text{Cl} = 4.5, \text{I} = 15.625, \text{C} = 0.75.\]


\(^{106}.\) "I suspect that all the atoms, if accurately ascertained, would be multiples of hydrogen; 3.663 (cobalt) not being a multiple of .125, I take 3.625, which is the nearest multiple as the weight of an atom of cobalt", *Ann.Phil.*, 12,345 note w,1818. Also, "Nature delights in simplicity. Hence I am led to expect simple numbers for the weights of the atoms of the simple bodies", *ibid.*, 16,17,1820.

\(^{107}.\) The experimental value was 0.06933 compared to Prou's 0.0694, *Ann.Phil.*, 14,65-6,1819. The method used was described more fully, *ibid.*, 15,323-3,1820.
Further new specific gravity measurements and chemical analyses were published by Thomson in 1820 and 1821. (108) He took the opportunity to publicly express a wish that Prout would continue with his work on atomic weights.

Dr. Prout had the merit of discovering that even the experiments of Biot and Arago gave the specific gravity of hydrogen gas above the truth and [had] the sagacity to determine the true specific gravity without any original experiments of his own; but merely by a close and ingenious comparison of the experiments of others. This is a degree of skill that places its possessor in a more elevated rank than a mere experimenter, and induces me to prognosticate with confidence that if Dr. Prout persevere in the career which he has begun with so much ardour, the science of chemistry will be indebted to him for discoveries of a far higher and more important kind than have hitherto been made. (109)

This fine compliment is perhaps the origin of the tradition that Prout was only a theoretician, and not an experimentalist; (110) for Thomson appeared to say that Prout, unlike Berzelius who "trusted entirely to experiment", (111) was completely above experiment. This was an exaggeration of course, but perhaps he was trying to be provocative: chemistry, a qualitative science, needed quantitative minds. Indeed, the interest of analytical minds like those of Prout and the young John Herschel in the phenomena of chemistry was of the greatest importance, thought Thomson; and they would reap "a rich harvest of

108. Ann.Phil., 16,161-77,241-68,1820 ; ibid., 17,241,1821 ; ibid., 18,120,1821. "It is a fundamental law in chemistry that all the atomic weights of bodies are multiples of the atom of hydrogen", ibid., 16,265,1820.

109. Ibid., 16,166-7,1820.

110. The evidence was briefly considered by O.T. Benfey, J.Chem. Education, 29,78-81,1952.

discovery and reknown, if they choose to devote to this delightful
science the requisite degree of attention, and combine their experi-
mental skill with their mathematical acquirements." (109)

Remarks such as these were bound to antagonise Berzelius who was
already growing his disapproval of Thomson in private correspondence.
But Thomson went even further, and specifically objected to the number
of decimal values which Berzelius had given in his list of atomic
weights; both on the valid grounds that such accuracy was unattainable,
and the invalid one that nature was simple and not given to such com-
lications. (113) Prout's integral multiples law, Thomson declared,
was the "third great step in our investigation of the atomic theory" (114)
and Berzelius had paid the penalty of inaccuracy for ignoring it. His
lack of precision was entirely due to the fact that he had no such
theoretical criterion to guide his experimental findings. (115) Thomson,
on the other hand, believed that he had developed a foolproof and
accurate experimental technique for the verification of Prout's hyp-
othesis.

The first major public attack on Thomson's support for the integral
multiples hypothesis came not from Berzelius, but from the Glasgow
chemist, Andrew Ure, in 1821. By all contemporary accounts Ure was an

112. E.g. Berzelius to Gaspard de la Rive, 23 July 1821, "Thomson has

113. "The long train of decimals attending almost everyone of these
numbers is sufficient of itself to render their accuracy sus-
picious. Nature is not wont to indulge in such complicated
relations", Ann.Phil., 17(=1), 242, 1821.

114. Ibid., 16, 329, 1820.

115. Ibid., 17(=1), 3, 1821.
unpleasant character; but he was a competent chemist who had seen the wisdom of adopting the hydrogen scale of equivalents, and of adopting a single volume of hydrogen as the equivalent of the atom. He had even appeared to favour Prout’s protyle hypothesis, which he ascribed to Davy. (116) Yet he was no friend to Thomson; in a long savage review of the sixth edition of Thomson’s System; (117) written for Brande at the Royal Institution, Ure deplored the hypothetical character of Thomson’s reasoning.

Whenever he begins to generalise, his technical decision (sic) of manner leaves him, and, to the surprise of the readers of those clear [experimental] details, which he had merely transcribed from experimental chemists, he becomes obscure and contradictory. To this defect a more serious fault has been added; and which, progressively gaining force, has of late grown almost intolerable; we mean, the preference of hypothesis to facts on innumerable occasions, so that it is difficult for the experienced chemist, and impossible for the tyro, to distinguish between them in his works. (118)

Ure’s review quickly degenerated into personal abuse, but above all he castigated Thomson for the suggestion that chemists had worked without any guiding principle. Thomson’s book was filled with errors on every page which were “occasioned chiefly by the incessant twisting, stretching, and curtailing of experimental results, to suit some fantastical atomical” guiding principle. (119)

Ure, and his editor, Brande, had nothing to say about Prout on this occasion, but they did suggest that Thomson’s division of the

116. “On Equivalents”, Phil. Mag., 57, 95-116, 1821, reprinted from his edition of Nicholson’s Dictionary of Chemistry. At this period Ure was at the Andersonian Institution in Glasgow.


118. Quart. J. Science, Literature & Arts, 11, 119-71, 1821; see p. 121.

119. Ibid., p. 171, Ure’s stress.
atomic weight-specific gravity relationship into three classes of substances was a trivial effect due solely to the fact that chemists had adopted a dual standard of oxygen for weights, but air for specific gravities. This is quite true, but without a molecular theory of chemical combination Thomson was surely right to have treated it as a possibly significant series of relationships.

Thomson was much too busy with his experiments to reply to Ure and Brande's "uncandid review" for nearly a year, when he published a long and detailed defence—chiefly of his personal character. Although the review had been written by Ure, Thomson directed his spleen against Brande whom he believed had reviewed the fifth edition of his textbook in a similarly vindictive manner. Thomson avoided any reference to the accusation that he had "adjusted" his experimental results, and instead defended his experimental accuracy on the modest grounds that lack of dexterity had forced him "to look out for a method in which no dexterity was required". As a result he had adopted the method of preparing equivalent solutions which, when reacted together, exactly neutralised each other.

His reply occupied 35 pages; inevitably it was followed a few months later by another 20-page assault from the Royal Institution which was again a dreary attack upon Thomson's integrity. These personal controversies are arid reading for the historian; yet it is

120. Ann.Phil., 1(=3), 241-75, 1822. "Every reader will participate in my astonishment, and agree with me that a more uncandid review has scarcely ever appeared, and that it fixes an indelible stigma on the editor and the author", p.275.

possible that they may have brought Thomson a certain amount of sympathy. If this is the case, then they may also have hindered the serious experimental appraisal of Thomson's work. Some support for Thomson certainly came from the new editor of the *Annals of Philosophy*, Richard Phillips. In 1824, Phillips published a table of equivalent weights which were all integral on the hydrogen scale. But it should also be noted, in mitigation of Brande's opposition to Thomson, that in 1823 he published a comprehensive table of integral equivalents \( (H = 1) \) for the use of his students at the Royal Institution. However, Brande offered no theoretical discussion, and since it seems unlikely that he had suddenly adopted the integral multiples hypothesis, the probable explanation is the simplicity of the numbers for the sake of his students' memories.

At first Thomson had intended to publish his results periodically in the *Annals of Philosophy*, but he soon decided against this in favour of the magnum-opus which he published in two volumes in 1825, *An Attempt to Establish the First Principles of Chemistry by Experiment*. He claimed that this book was based upon "thousands" of accurate experiments which had been made "in the College laboratory of Glasgow by myself or my pupils under my immediate direction and superintendence." The *First Principles* was impressively dedicated to the

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122. *Ann.Phil.*, 23(=7), 185-97, 1824, based on the tables of Thomson, Henry, and (diplomatically?) Brande.


124. 2 vols., London, 1825, cited as "First Principles". I have used the copy which Thomson sent to Faraday, in the Royal Institution.

founders and developers of the atomic theory, Dalton, Gay Lussac, Davy, Berzelius, Wollaston and William Prout. Ostensibly, the book was directed at University students of medicine. Thomson hoped "to reduce the whole doctrine of atoms to the utmost degree of simplicity and accuracy, (126) but he warned the reader against bias because his results did not "exactly coincide with the analytical results of other chemists". The reason why Berzelius's results were so different, Thomson suggested, was because he had usually worked from the metals themselves, and not from their salts. It was far better to work from neutral salts which could be rigorously purified, claimed Thomson. In the case of zinc, Berzelius, who had worked from the contaminated metal (argued Thomson), obtained the atomic weight 4.03225 (0 = 1); whereas Thomson, who had worked from purified zinc sulphate, obtained the rounded decimal, 4.25. (127) Unfortunately for Thomson, it turned out that this analysis was incorrect.

Thomson's results were offered as a direct challenge to those of Berzelius whose atomic weights, he declared, were only "approximate to the truth"(128) and only rarely correct. Berzelius's atomic theory was "so complicated and intricate, that it would be surprising if it were a true representation of what takes place in nature." This statement confirms the impression that Thomson's methodological assumption was the principle of economy.

127. Berzelius, needless to say, was unimpressed. See Berzelius *Bref*, vol. 2, iv, p. 61, letter to Dulong.
In an historical Introduction based upon an earlier paper, Thomson praised Prout's "admirable" anonymous article of 1815 "for a degree of sagacity that has seldom been exceeded in chemical investigation, and shows clearly that the author, if he chose, might rise to the highest eminence as a chemical philosopher." (129) It was to Prout that Thomson ascribed the observation (which was really his own interpretation on the oxygen scale) that:

all ... atomic weights are multiples of the atomic weight of hydrogen; indeed, that all of them are multiples of twice hydrogen, or 0.25, and most of them of 4 hydrogen, or 0.5. He also observed, that in general the specific gravity of the body in the gaseous state, may be obtained by multiplying its atomic weight by 0.5555, or half the specific gravity of oxygen gas; because the oxygen atom is represented by half a volume, but that of most other substances by a whole volume. (130)

He had rapidly convinced himself that Prout was right, and the integral multiple weights law therefore gave analytical chemists a new tool with which to determine atomic weights.

For every substance, of which I could procure a sufficient quantity to enable me to examine it fully, has been found not only a multiple of the atomic weight of hydrogen, but if we accept [sic] a few compounds into which a single or odd atom of hydrogen enters, they are all multiples of 0.25 or of two atoms of hydrogen. (131)

Thomson altogether avoided any reference to Prout's other hypothesis that the elements were compound bodies made of hydrogen. His

129. First Principles, vol. 1, p. 23, and "It was this admirable paper that satisfied me that new analytical investigations were still necessary to determine the atomic weights of bodies with perfect accuracy", p. 25. Note also supra, ref. 109.


131. Ibid., p. 26. He also tried to find a law that governed the amounts of water of crystallisation in salts; but to no avail.
only reference to the homogeneity of the elements was made in the context of the Boscovichean force atom.

With respect to the notion entertained by Boscovich, that the ultimate atoms of bodies are homogeneous, we are incapable at present of deciding whether it be well or ill founded. It is not likely that any of these ultimate elements has ever come under our inspection. All our simple bodies are most probably compounds. It is possible that the ultimate elements of bodies may be very few—it is even conceivable that they may be reduced to two; but in what way all the variety of bodies with which we are acquainted, could be produced from one single kind of elementary body or atom, I cannot, for my own part, form any conception. (132)

This is an interesting statement, for it shows that although Thomson was firmly committed to the principle of the simplicity of nature, and equally firmly that he was in favour of the existence of mathematical relationships between the atomic-molecular weights of chemical substances, he refused to speculate about the unity of matter, or about the candidates for such a theory. If he had agreed with Prout that hydrogen, or hydrogen and oxygen were primary elements, he would undoubtedly have said so; instead he only went so far as to state that if chemical elements were complex, it was unlikely that any of the "ultimate elements" from which they were constructed were known to the chemist. (133) Thomson made no deduction that since atomic weights were multiples of the weight of the hydrogen atom, or of the oxygen atom, that one or both might be ultimate elements. His attitude is quite different from that of Prout's, and it once again points to the necessity of dichotomising the so-called "Prout's Hypothesis". Thomson's position was not unlike that of Mendeleeff's and


133. Compare Anaximander's attitude to Thales-type elements; and Prout, Chemistry, 1st ed., 1834, p. 129.
Lothar Meyer's fifty years later when faced with the implication that the relationship between the atomic weights described by their Periodic law were an indication of the unity of matter. (But whereas Thomson would, like Lothar Meyer, have accepted the implication, but avoided speculation, Mendeleeff actually rejected it completely).

The great bulk of Thomson's book was concerned with the experimental determinations and calculations of the specific gravities and atomic weights of all the known elements. The careful reader will find a certain amount of evidence that Thomson adjusted a few results to conform with the integral multiple weights law. For example:

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4.2319 \text{ is the (experimentally determined) weight of an atom of fluoboric acid. The law of Dr Proult, which will be found to hold in the atomic weights of all bodies, shows us that this number is a very little too high. The true atomic weight is undoubtedly 4.25. (134)}
\]

By means of such adjustments Thomson was able to uncover a number of remarkable mathematical relations between the various atomic and molecular weights of substances. Although none of these relations is significant, they show what Thomson meant and understood by the "First Principles of Chemistry", and clarify the meaning of his belief that a mathematical or quantitative basis lay behind the qualitative properties of substances. The relations were presented in the form of a series of generalisations in the final chapter. (135)

1. Hydrogen was the lightest known element with an atomic weight of 0.125 \((0 = 1)\).

2. The atomic weights of all other elements were multiples of 0.25, or two atoms of hydrogen. This meant that all atomic weights were

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either whole numbers or multiples of the quarter decimals, 0.25, 0.5, and 0.75. Thomson did not draw any conclusion from these relations that hydrogen was the prime matter.

3. Of the 117 atomic (and molecular) weights which he had determined, only 5 were multiples of a single hydrogen atom; 37 were multiples of two hydrogen atoms (0.25: 11 acids, 11 bases, and 15 elements); 25 were multiples of four atoms of hydrogen (0.5: 6 acids, 8 bases, 2 combustion supporters, 3 combustibles that produced acids, and 6 that were alkalifiable); 50 were multiples of one atom of oxygen, that is integral weights, and 18 of these were elements.

It will be observed that in this third generalisation, Thomson introduced oxygen as a unit of comparison as well as hydrogen. This was continued in further generalisations until the following broad principle was reached.

13. There are five or six of the simple bodies which we have found to combine both with 1 atom and 1½ atoms of oxygen. ... I have sometimes thought that the anomaly might be obviated by admitting, that oxygen in reality has an atomic weight amounting to 0.5 instead of 1. (136)

Prout had probably already taken this step by 1825, for Thomson's proposition leads to a molecular theory of matter. If the oxygen atom were 0.5, so that two atoms formed a molecular unit of oxygen then, as Thomson pointed out,

not only those simple bodies whose atomic weights are whole numbers, but those likewise, whose weights and in 0.5 are multiples of oxygen. These constitute the whole of the supporters of combustion, and all the acidifiable and intermediate combustibles, except three: namely, carbon, arsenic, and tungsten. Of the alkalifiable combustibles, amounting to 29, 17 would be multiples of oxygen, leaving altogether, 15

simple bodies which are multiples of 2 atoms of hydrogen. Thus 31 simple bodies would be multiples of oxygen, and 15 multiples of 2 atoms of hydrogen. (137)

From this statement it is clear that Thomson hoped to discover rules of affinity from such numerical principles—principles which were the real basis for chemists' gross classifications of substances by their chemical properties. However, it would be difficult to avoid concluding from this analysis that the physical basis for these relations, and chemical affinities, was that matter was made from hydrogen and oxygen.

In a final burst of Pythagoreanism, which clearly foreshadows the deductions of Dumas, and the transmutation speculations of some of the less well-known chemists, Thomson pointed out some of the extraordinary arithmetical relations between the elements. For example:

- carbon = 0.75
- phosphorus = 0.75 x 2 = 1.5
- sodium = 1.5 x 2 = 3.0

And if one made the explicit assumption that the elements were homogeneous, then:

- phosphorus = 2 carbon
- sodium = 4 carbon
- molybdenum = 8 carbon

etc.

However, Thomson was not explicit concerning this assumption; he merely noted down these relationships without commenting on their physical cause. Yet although he avoided any reference to Prout's protyle hypothesis, it would have been a naive reader who was unable

to read into this numerology what Prout had already hinted at, and what Dumas was to make explicit—namely, support for the idea that chemical elements were compound. Despite my earlier qualification then, it appears that like Miers and Dumas, Thomson wondered whether the ultimate units of chemical phenomena were hydrogen and oxygen. But he never made any explicit reference to this possibility.

What sort of reception was given to this extraordinary book? Apart from the inevitable disapproval of the Davian school of chemists at the Royal Institution, Thomson received a good English press. Unfortunately, there is no record of Prout's impressions, but it seems likely that while he approved of Thomson's general purpose and was delighted with his demonstration of the law of multiples, he had some reservations about many of the analyses. (138) There is no doubt that the opinion of the majority of British chemists was voiced by Phillips in his review of the *First Principles*. Thomson's method, he wrote,

> appears to us liable to exception in very few cases; the work must form a part of every chemical library, and will be referred to as a standard by those who wish to acquire information as to the atomic weights of bodies, or to a knowledge of the experimental means of ascertaining them. (139)

Thomson's book did become a standard among British chemists; Prout was able to say at the beginning of 1827 that his law of integral multiples appeared to have become generally adopted by chemists. (140)


The one exception noted by Prout was Andrew Ure who had written a polemical review of Thomson's book for the Royal Institution's journal. (141)

Ure had disagreed fundamentally with Thomson's whole approach. The real aim of chemistry was not, as Thomson seemed to think, to determine the relative weights of substances, but to determine the properties and qualitative relations and affinities between elements and compounds.

To view with Dr. Thomson, the first principles of chemistry as consisting in an examination of weights and measures, is too narrow and debases the science into an affair of addition and subtraction. This arithmetical process is, no doubt, a valuable accessory; but can never compete either in interest or utility with the knowledge of the chemical affinities, from whose play, the countless diversities of composition and unceasing successions of form in the material system, are derived. (142)

Experimentally, Ure thought that Thomson's method of precipitation left much to be desired, "and hence he has often presented us with results, tallying well with the atomic theory, and with Berzelius [sic], which he states as his own, though they could never have been derived from his narrated experiments." (143) All this followed because Thomson had tried to prove the validity of Prout's integral multiple weights hypothesis which Ure admitted was an interesting and plausible idea. But the opportunity for sarcasm was too good to miss.


143. Ibid., p.115.
Thomson sublimes ordinary zinc in an earthen retort, dissolves a given weight of it in nitric acid, and then expels the acid, by heating the nitrate to redness in a green glass retort. In this way, he obtains at once, to the minutest fraction, every thing which Dr. Prout's atomic multiples require, viz., 5.25 grams of oxide, from 4.25 grams of metal. This felicity of coincidence between his experiments and his theoretical aim is so usual with Dr. Thomson, and with him alone, as to excite no surprise in our minds. (144)

Finally, Ure reiterated his support for the hydrogen scale which avoided numbers less than unity in the scale of equivalents, and brought about a general coincidence between atomic weights and gaseous specific gravities. Such a system had the support of Davy, Dalton, Prout, William Henry and Richard Phillips.

Thomson, although he could hardly have expected favourable treatment from Ure, made an immediate reply. Apart from some well-hammered abuse of Ure for his continuous campaign against him, he made an honest and serious attempt to defend his belief in quantitative research.

The whole of chemistry, so far as it is entitled to the name of science consists in the accurate measurement of quantities. (145)

Events were to prove Thomson right even though the detailed attempt made by him was to prove worthless.

Ure's "severe animadversions" on Thomson's First Principles were sympathetically noted by Phillips who redressed the balance of criticism by printing an extract from Benjamin Silliman's glowing and uncritical American review:

144. *Quart.J.Science*, 20,122,1825.

There is nothing, the offspring of the present age, which, so far as we are informed, surpasses this Attempt to Establish the First Principles of Chemistry by Experiment. The vast amount of labour performed—the patient and persevering repetition of tedious and often difficult processes, frequently to the eighth or tenth time—the consummate skill discovered in devising and executing the experiments, and the surprising coincidence of the results of analysis with the deductions of theory, excite our astonishment, and prove beyond a question that chemistry, if not founded on intuitive, is built on demonstrative truth. (146)

However, the reception of the First Principles was not confined to either fulsome praise or rhetorical abuse; far more serious for Thomson's reputation as a highly credited, or highly discredited, chemist was the attack made on him by a friend, the Glaswegian physician, Harry Rainy. (147) Rainy's criticisms, which were made on experimental grounds, were similar to, and continued those made by the Irish physician, James Apjohn, in 1622; (148) and they foreshadowed the searching experimental appraisal of Thomson's work that was published by Edward Turner in 1633.

In April 1622, Thomson had published a reply to a friend's private criticism (probably Rainy's) that he had not corrected for the presence of water vapour when he published an account of the determination of the specific gravity of hydrogen in 1620. (149) Thomson argued in reply that although he had said nothing about the presence of water vapour in hydrogen in the published account of his experiments, he had been fully aware of such "an obvious source of inaccuracy". He then

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147. See notice of Rainy (1792-1876) in *Dict.Nat.Biography*.


149. *Ann.Phil.*, 16,165-70,1820; note also *ibid.*, 14,65-6,1819.
stated that in practice the error due to the presence of water vapour was so small that it could be ignored, provided that both the air and the hydrogen used in the estimation were deliberately saturated with water vapour. For, in this case, both the volumes of moist air and moist hydrogen would weigh more than if dry; if the calculations were made only to the fourth decimal place then the two errors involved would more or less cancel each other out. This being the case, Thomson argued, his and Prout's value of 0.0694 for the specific gravity of dry hydrogen was accurate.

This argument was rejected by Apjohn in May 1822; he suggested that, on the contrary, hydrogen was the only permanent gas which (at ordinary temperatures) had its density increased by the presence of water vapour. (150) It followed that although Thomson's method of weighing gases which had been previously saturated with water was a good one, and generally applicable, it was necessary to apply a correction for the presence of vapour in the case of hydrogen since the error was increased, not decreased. Apjohn's principle was correct, but the formula he published for the estimation of the specific gravity of a dry gas from the value for the specific gravity of a moist gas, contained an erroneous factor. Since he also associated himself with Thomson's arch-enemy, Andrew Ure, this possibly explains why Thomson took no notice. (151)


Thomson's account of the specific gravity of hydrogen in the *First Principles* attracted the attention of his friend Harry Rainy who hastened to report in the August issue of the *Annals of Philosophy* (152) that Thomson had made a bad error by under-estimating the quantity of water vapour in his hydrogen sample. Rainy had made his own careful experiments and discovered that the Prout-Thomson value for the specific gravity of dry hydrogen was incorrect for any given temperature and pressure; he proposed a vapour correction factor which had the significant effect of reducing the specific gravity of hydrogen from Prout's and Thomson's 0.0694 to 0.0673. This gave an H:O ratio of 1:16.54 instead of 1:16.

Although Rainy's H:O ratio is not correct since it was based upon Thomson's erroneous value for the specific gravity of oxygen, his result was of great importance since it led him to discuss Prout's integral weights hypothesis. In this first paper, he suggested that the truth of the hypothesis was an open question that was dependent upon the experimentalist's skill.

If Dr. Thomson's experiment is correct (and of this we can scarcely doubt from the care and attention with which it is performed), it disproves the hypothesis that the specific gravities of all the gases are multiples by integer numbers of the specific gravity of hydrogen. It is true that 16.54 does not differ from 16 by more than about 1/32 of the whole, and that a very slight change in the number adopted for the specific gravity of hydrogen would account for the difference; but this merely shows how difficult it is to make any experiment sufficiently accurate to decide on the truth of the hypothesis. (153)


Here Rainy hit the nail on the head, and Thomson seemed to have been placed in the awkward position of either admitting that his experiments were at fault, or that his experiments were right, but that Prout's hypothesis was wrong. However, Thomson was unperturbed. He replied to Rainy in a friendly fashion\(^{(154)}\) that he was certain that his own experimental results with hydrogen were all quite accurate. He admitted that there had been an error over the correction for the presence of water vapour, but this error had not been in the direction claimed by Rainy. Far from under-estimating the amount of water vapour in hydrogen, Thomson believed that he had over-estimated it. He agreed with Rainy's formula for the conversion of the specific gravity of a moist gas into a dry one, and used his own water vapour pressure data to obtain a specific gravity ratio of \(H : O :: 1.0036 : 16\), or \(1 : 15.941\), which he thought was close enough to \(1 : 16\). However, Thomson had not been altogether satisfied by this, and in order to overcome the objections raised by the uncertainties involved in correcting the specific gravities of gases in the moist state for the presence of vapour, he had made an entirely new set of experiments in which he weighed dry hydrogen. The drying procedure was very thorough: pure hydrogen was passed through about 37 inches of calcium chloride tubing, and the amount of water absorbed was used as an additional check on the calculations. The results of these measurements were a new triumph for Thomson, even though the \(H : O\) ratio proved to be \(1.0077 : 16\), or \(1 : 15.87\). Nevertheless, he claimed, within the

\[154. \text{Ann.Phil., 26(=10),} 352-60,1825. \] Thomson thought Rainy's paper was written with "perspicuity, accuracy and modesty".
experimental errors, this ratio was sufficiently close to the integral ratio, 1:16.

Rainy thought otherwise. Far from clearing away his doubts, Thomson's new measurements only strengthened them. In March of the following year, 1626, Rainy replied more forcefully and critically than in his first paper that Thomson had made errors not only in his old experimental routine with moist hydrogen, but also in the new direct determination of the specific gravity of dry hydrogen. (155)

On the basis of some of his own experiments on vapour pressure whose results he found to be different from the published tables of Dalton and Ure, he once more rejected Thomson's old, but amended H:O ratio of 1:15.941. As for the new determination with dry hydrogen, he thought that it had a number of technically unsound features; but more significant from our point of view, he questioned whether (even on the assumption that the ratio was correct) Thomson's interpretation that the difference from H:O::1:16 of 0.12 in the new ratio H:O::1:15.87 was immaterial.

The deviation from an integer is 0.12, or about 1/8, which seems small, but we must remember that the utmost possible deviation is 0.5, for were the experimental results to vary from 16 by more than 0.5, it must approach some other integer. The variation from the theoretic result in this experiment, is actually about 1/2 of the greatest possible variation. Dr. Thomson would probably reply, that the greatest possible variation would be 1 and not 0.5, if we compare the atomic weights of oxygen and hydrogen, instead of their specific gravities; but even admitting this, the deviation from the theory is still about 1/8 of the greatest possible deviation. All numbers which have large ratios to any given number must of course nearly coincide with integral multiples of that number. (156)

156. Ibid., p. 190.
The larger the ratio the more inevitable became the coincidence that the ratio should reduce to whole numbers. Since the atomic weight of hydrogen was so small, and since it could be taken as the relative weight \((H = 1)\) of a system, then "the atomic weights of other bodies must nearly be integers". Surely Thomson would not have so readily admitted the integral multiple weights hypothesis if the basis for comparison had been chlorine, 36. Rainy's attitude fore-shadowed that taken by Stas; atomic weights may approach integral numbers, but this approach need not have any deeper significance. His attitude perhaps also reflects that of the multi-element school that the integral weights hypothesis was bound up with a particular and simple world picture.

To Rainy's mind the only satisfactory test of the integral multiple weights hypothesis would be to estimate the weights of atoms which did "not bear such a large proportion to that of hydrogen". If it could be demonstrated that the atomic weights of carbon, oxygen and nitrogen were integers, then and only then, would there be "strong reasons for believing that the other atoms are also integers." \(^{157}\)

This was a sound and sensible empirical attitude. Unfortunately, although it sounded simple enough, the experiments demanded by it were not easy to design, and when they were made the results were not so easy to interpret as Rainy had hoped.

In the meantime, before these experiments were made, Thomson's results were the best available to chemists, thought Rainy. But he emphasised that using them was not the same thing as believing them.

\(^{157}\) Ann.Phil., 27(=11),191,1826.
to be true. Indeed, he specifically warned Thomson against the
delusion that because British chemists were apparently adopting his
values for the specific gravities of hydrogen and oxygen, that this
was an absolute belief.

It will have been seen that Thomson got himself into a contra-
dictory position with Rainy. In 1622 he had denied that water vapour
made any material difference to the specific gravity of hydrogen; yet
in 1626 he conceded an influence. Because of this concession Rainy
believed that Thomson should also concede that his experimental results
were hostile to Prout's multiple weights hypothesis, and he advised
that:

The subject evidently requires further elucidation and I am
sure Dr. Thomson is too candid to object to its being fully
discussed, however clear the evidence may appear to his own
mind. (156)

In the event, however, the further elucidation came from another
chemist who was then studying in Scotland, Edward Turner.

Rainy's achievement was to show that Thomson's specific gravity
methods were open to scrutiny and doubt, and that he had by no means
established the correctness of the integral weights hypothesis which
he had set out to prove. Rainy's case would have been stronger if he
had also tackled the value for the specific gravity of oxygen. Like
Andrew Ure, Rainy also came out explicitly and accused Thomson of
some fabrication.

Dr. Prout proposed his views, as a probable conjecture, not
inconsistent with the established facts of chemistry, and
therefore, deserving of fuller inquiry. Dr. Thomson adopted
them as completely proved, and seems to think that they must

be admitted to be true, if they cannot be proved to be false. He accordingly introduced them into his System, and did not scruple in any instance to modify experimental results so as to correspond with them. (159)

Since Thomson's System of Chemistry was such a widely-read textbook, Rainy thought that Thomson had done a grave disservice to chemistry by publishing experimental results in the First Principles that had been "vitiated by modifying them to suit an hypothesis, which, whether it be true or false, will tend to retard the science, if adopted prematurely and on insufficient evidence." (159) Yet, just like Turner only a few years later, Rainy placed on record his cautious opinion that nothing he had written disproved Prout's integral weights hypothesis. It was an hypothesis still open to experimental inquiry.

To this Thomson made no public reply, and Rainy does not seem to have made any further contributions to the debate, or indeed to the science of chemistry. However, as we have seen in an earlier chapter, the reappraisal of Prout's integral weights hypothesis by improved methods of specific gravity measurement was evidently begun by Prout himself; but with what results we do not know. (Chapter 5) In any case, the next appraisal of the hypothesis was to be concerned with Thomson's other techniques, those of quantitative analysis.

It has been suggested that the full force of Rainy's attack was weakened by the respect and friendly admiration which he felt for Thomson, and the fact that Thomson's tremendous propaganda for the adoption of Daltonian atomism in Great Britain had been accomplished only at the cost that Berzelius's work was less well known. (160)

think that this is only partly correct. Rainy's criticism was as well-mannered as Turner's was to be, and all the more potentially effective for being so compared with the gross polemics of Andrew Ure, and to some extent, Berzelius. Rainy was ineffective, however, because unlike Turner he left it for others to decide on the truth of Frout's hypothesis. It must also be noted that throughout the period 1815 to 1825 Berzelius's work was fairly-well publicised in Great Britain, largely through the efforts of Thomson, and later Richard Phillips. Through the strength of his own journalism, however, Thomson managed to persuade a large section of British chemists to follow his own interpretation of the atomic theory, and he seems to have shared the suspicions of the Royal Institution clique that Berzelius's work was a mystification rather than a clarification of chemical phenomena. In this respect it is undoubtedly significant that Berzelius's EEssai (1819), which clarified his intentions, was never translated into English. On the other hand, it must be emphasised that Berzelius's values for the atomic weights were well-known to British workers. In the same year that Thomson published the First Principles, J.C. Children published "a summary view of the atomic theory according to the hypothesis adopted by M. Berzelius", and Phillips printed a list of Berzelius's non-integral atomic weights transcribed from the EEssai. (161) In the space of a few months, therefore, readers of the annales of Philosophy were able to compare the atomic weights of Phillips, Berzelius and Thomson. It remains a fact,

however, that British textbook writers of this decade seem to have preferred the weights of Thomson or Brande to those of Berzelius.

In 1826 Berzelius published a new series of atomic weights based upon the new criteria of specific heats and isomorphism as well as his old empirical rules. (162) No integral values were recorded by him and he made no reference to Prout's hypothesis. We know his opinion of it, however, from the devastating reference to Thomson's First Principles which he made in his Jahres Bericht for 1826, published in 1827. (163)

This work belongs to those few productions which science will derive no advantage whatsoever. Much of the experimental part, even of the fundamental experiments, appears to have been made at the writing desk; and the greatest civility which his contemporaries can show its author, is to forget that it was ever published.

Berzelius's famous accusation of quackery was grossly unfair; as Phillips remarked, it went beyond the bounds of just criticism. Berzelius was wrong to "arraign the character of an individual, who may be actuated by motives and principles as pure as his own." (164) Thomson was an honest and sincere man, said Phillips, and if any deception had taken place, then Thomson had deceived himself more than anyone else.


164. Phil. Mag., (2) 4, 452-3, 1828.
It is possible that, misled by a favourite hypothesis, he
may, like many before him, have been too eager in seizing
facts favourable to his views, and too tardy in perceiving
those that are unfavourable. (165)

Despite Phillip's defence, by 1626 Thomson's supporters had
begun to have doubts. Berzelius had argued in the German edition of
his chemistry textbook that since Thomson's analysis of the reagent
barium chloride was inaccurate, and the resultant value of the atomic
weight of barium quite erroneous, (166) most of his other atomic
weight estimations were compromised. Even Phillips admitted that it
appeared as if something had gone wrong; it was Thomson's duty to
chemistry to defend his reputation as an analyst, or admit and
rectify any mistakes that he might have made.

Unfortunately Thomson never chose to make any explicit retraction. Instead he replied in kind to the "foul aspersions of the
Stockholm Professor", defended his integrity, and completely avoided
the question of the accuracy of his results. (167) He revealed that
in 1625 he had sent a copy of his book to Berzelius who had replied
by letter that Thomson had made an elementary error with the precipitation of zinc carbonate in one of his first experiments. (168)
Berzelius had actually said a good deal more, namely, (in Partington's
happy phrase (169) ) that Thomson had altered good experiments (Ber-
zelius's) to make them agree with bad ones (Thomson's) and that

165. Phil. Mag., (2)4, 453, 1826. This is the most charitable explanation. For Berzelius's regrets, see Partington, History, 4, p.226.


167. Phil. Mag., (2)5, 217, March 1829.


And to his close friend Dulong, Berzelius confided:

J'ai voulu m'assurer de prétendu poids multiple des autres corps de celui de l'hydrogène; mais la chose est d'une difficulté énorme, et pour le moment le plus grande nombre de cas est contraire à cette idée. J'ai répété les expériences principales de Thomson. Quel charlatan! -aucun des résultats qu'il donne n'est exact; en se servant des poids atomiques prescrits par lui pour faire des précipitations, on obtient des liquides qui, étant filtrés, donnent de nouveaux précipités avec l'un des sels employés. (171)

Berzelius seems to have clearly appreciated the basic source of Thomson's errors before Turner.

In defence against Berzelius's remarks which had received such publicity from Phillips' translation, Thomson foolishly accused Berzelius of altering his own atomic weights of 1819 in the light of the First Principles, and publishing them as his own in 1820.

I am uncharitable enough to believe, that it was in order to prevent his countrymen and the Germans from being aware of the benefit he derived from my labours, that his attack upon me was made. I had touched his selfish feelings, and disturbed those dreams of chemical sovereignty in which he had been evidently indulging. (172)

With this preposterous charge Thomson remained steadfast in his belief in the truth of Irout's integral weights hypothesis, and in the accuracy of his own atomic weights. The only changes he was prepared to make were all consistent with Prout's hypothesis; (173) and he dismissed Berzelius's non-integral value for the atomic weight of barium.

171. Ibid., vol. 2, iv, pp. 70-1 (September 1826).
172. Phil. Mag., (2)5, 220, 1829.
173. E.g. he raised chromium from 2.5 to 4, and phosphoric acid from 3.5 to 4.5, Phil. Trans., 1827, p. 159.
He continued to have difficulty with the analysis of oxalic acid. In the *First Principles* Thomson had deduced that oxalic acid crystals contained over half their weight of water, but just before publication, Prout "of whose accuracy and information I entertain a very high opinion", (174) had written to him stating that he found much less water in the crystals than Thomson. The latter repeated his analysis, but obtained the same result. After the publication of his book, Prout wrote again to Thomson to say that he also stood by his analysis. (175) This analytical discrepancy between Thomson and Prout shows the great difficulty the former experienced in completely removing water of crystallisation from the salts which he analysed. It also suggests that Prout may have had some reservations concerning Thomson's other analyses before the critical onslaughts of Berzelius and Turner were published. Later, Thomson said Prout was right. (176)

Thomson's reputation never completely recovered from Berzelius's attack, for although his work had been founded on experiment and not done at the writing desk except in the sense that he had preconceived ideas about the kind of result he should obtain, and he was honest enough to admit this whereas other scientists of the period took trouble to disguise their precommitments, it became apparent in 1828 that many of his experiments were inaccurate, or based upon false assumptions. As J.W. Mallet pointed out at the end of the last

175. *Phil. Mag.*, (2)5, 22–3, 1829. Later, Pridaux tried to resolve the dispute with the suggestion that there were two kinds of crystal; but otherwise he sided with Prout, *ibid.*, (2)6, 166, 1829.
From the incorrect assumption that air was a chemical compound, Thomson deduced incorrect densities for hydrogen and oxygen and chlorine, which led to incorrect values for the combining weights of his principal analytical reagents, hydrochloric acid and barium chloride. Since the majority of his determinations hinged upon the precipitation of barium sulphate until the solute was neutral to barium chloride, numerical error was introduced into all estimations that were based upon the use of barium chloride. In addition, and this was not known by Thomson, or even by Berzelius, the heavy precipitate of barium sulphate always carried down with it some of the soluble reagents and the solute, thus rendering useless all Thomson's analytical care. The method of precipitation was suspect in any case since, as Berzelius pointed out, none of the preliminary experiments, in which the equivalent quantities of two pure salts were estimated, were recorded in the First Principles. Mallet concluded that:

Thomson must have been satisfied with only very moderate accordance between the figures of individual experiments and have accepted the mean of these as exact, without thought for the large probable error involved -then, reducing his figures to correspond with the standard assumed, namely oxygen as unity, and expressing the results in grains he had but small quantities to work with in the final experiments which he does report, and the absolute errors are seemingly but very small.

It is ironic that, because of an internal cancellation of inaccuracies, Thomson's values are in many cases better than those of Berzelius when compared with modern values. (178)

My concern to trace the fortunes of Prout's hypotheses through the work of Thomson has led to Prout's overshadowment. This is historically correct, for until 1831, Prout took no part in the controversy over his work. The reader must therefore be patient if Prout is once again placed in the background while the work of his friend Edward Turner is briefly reviewed.

The Work of Edward Turner

Edward Turner (1796-1837) studied medicine at Edinburgh University where he later lectured on chemistry before he became the first Professor of Chemistry at the new University of London in 1828. He began his career as a disciple of Thomson and went on record that he recommended:

the careful study of his recent admirable treatise on the First Principles of Chemistry to every one who feels an interest in the science; nor [can I fail to express] my admiration of the profound sagacity, unwearied industry, and great experimental talent displayed throughout the whole of that work. (180)

He made no reference to Prout's protyle hypothesis; but he appears to have been convinced of the multiple weights hypothesis, while positivist towards the hypothesis of atoms. The essay which contained these remarks was expanded in 1827 into one of the best of nineteenth-century textbooks, the Elements of Chemistry. (181)

This book was even acclaimed, despite its allegiance to "Apollo

179. The competition for the Chair was intense; it was declined by Faraday and Thomson, and possibly by Prout. See H. Terrey, Annals of Science, 2,143,1937.


181. 8 editions: 1827, 1828, 1831, 1833, 1834; and 3 posthumous.
Before his transition to London, Turner was led through his mineralogical interests to investigate the atomic weight of manganese which Thomson had estimated by the precipitation reaction between manganese sulphate and barium chloride. (183) Now Turner had seen Berzelius's criticism of Thomson's use of barium chloride, and his correction of the atomic weight of the reagent, (166) so out of concern to make his own experiments with manganese as accurate as possible, he had quickly repeated Berzelius's analysis and found it confirmed. Disconcerted, he drew Thomson's attention to it and urged him to check his experiments "without delay; since an error in the atomic weight of barium will at once vitiate an extensive series of his most elaborate analyses." As we have already see, Thomson replied in 1829 that he saw no reason to doubt the accuracy of his analysis. (167)

Turner gave little sign in 1828 that he would devote the next five years of his life to the redeterminations of the most important atomic weights. But he undoubtedly made this decision in view of the large discrepancies between the atomic weights of Thomson and Berzelius. In order to make certain that the task was worthwhile he spent some time on an accurate analysis of barium chloride; but because of his teaching appointment in London, he was unable to publish the result until 1829. (184)

182. Quart.J.Science, (2)2, 60-7, 1827.
184. Phil.Trans., 1829, pp. 291-9; reprinted because of its importance, Phil.Mag., (2)8, 180-8, 1830.
In this important paper, Turner rejected Thomson's claim that when 106 parts of barium chloride solution (70Ba and 36Cl) were mixed with 88 parts of potassium sulphate solution, a perfect neutralisation occurred so that no barium chloride or potassium sulphate could be detected in the filtrate; he agreed instead with Berzelius that 2% of the barium chloride was to be found in the filtrate. Thomson had made two mistakes, suggested Turner. First, he had probably used an impure sample of barium chloride; second, he and all analysts, including Berzelius, had been unaware that when the two salts were mixed together, some potassium sulphate clung to the barium sulphate precipitate and caused an error in the barium estimation. By using specially prepared barium chloride, and carefully washing the precipitate, Turner found in favour of Berzelius's analysis.

<table>
<thead>
<tr>
<th></th>
<th>Thomson</th>
<th>Berzelius</th>
<th>Turner</th>
</tr>
</thead>
<tbody>
<tr>
<td>Ba</td>
<td>66.037</td>
<td>65.926</td>
<td>65.984</td>
</tr>
<tr>
<td>Cl</td>
<td>33.963</td>
<td>34.074</td>
<td>34.016</td>
</tr>
</tbody>
</table>

Turner carefully abstained from any conclusion concerning the atomic weight of barium until that of chlorine (upon which it depended) could be checked. It had become clear to him that a good deal of careful experimental work would be necessary in order to verify or refute Prout's integral weights hypothesis. Consequently, in the third edition of his textbook, published in 1831, he retained Thomson's values for the atomic weights and explained to Berzelius, who had thought this an inconsistent policy, that:
At a distance, you cannot fully appreciate my position. Many here are prejudiced in favour of Thomson's equivalents merely in consequence of their simplicity, and I saw clearly that I had no chance of producing conviction in others until my experiments had been considerably extended. (185) Turner had placed himself in the delicate position of "umpire between two of the greatest of living chemists"! (186)

Meanwhile, a provincial chemist from the Plymouth Institution, John Prideaux, who had planned to publish a table of equivalents based upon Thomson's values, had grown tired of waiting for Turner's promised reappraisal; he therefore offered his own experimental check upon Thomson in 1830. (187) Prideaux found that barium chloride and zinc sulphate neutralised one another in exactly the proportions stated by Thomson, and he therefore expressed confidence in the remainder of Thomson's values. It appears that he probably obtained similar results to Thomson because of a defective drying procedure. (188) He clearly had not appreciated Turner's warning concerning the precipitation method. However, even Prideaux expressed doubts concerning Thomson's value for the atomic weight of nitrogen, but modestly concluded that "whilst the ablest chemists are at variance on such simple experiments, we must be content with approximations." (189) It

187. Phil.Mag., (2)7, 276, April 1830.
189. Phil.Mag., (2)7, 273, 1830.
is not surprising to find that Prideaux was willing to adopt a compromise between Thomson and Berzelius; he suggested that the sensible procedure would not be to dismiss Thomson’s concise and facile numbers, but to average them with those of Berzelius.

<table>
<thead>
<tr>
<th></th>
<th>Thomson ($0 = 1$)</th>
<th>Berzelius ($0 = 100$)</th>
<th>Prideaux ($0 = 1$)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Al</td>
<td>1.25</td>
<td>171.167</td>
<td>1.2</td>
</tr>
<tr>
<td>As</td>
<td>4.75</td>
<td>470.042</td>
<td>4.73</td>
</tr>
<tr>
<td>N</td>
<td>1.75</td>
<td>177.036</td>
<td>1.76</td>
</tr>
<tr>
<td>Ba</td>
<td>6.75</td>
<td>836.886</td>
<td>6.66</td>
</tr>
<tr>
<td>C</td>
<td>0.75</td>
<td>76.437</td>
<td>0.76</td>
</tr>
<tr>
<td>Cl</td>
<td>4.5</td>
<td>442.65</td>
<td>4.46</td>
</tr>
<tr>
<td>O</td>
<td>1.0</td>
<td>100.00</td>
<td>1.0</td>
</tr>
<tr>
<td>Ag</td>
<td>13.75</td>
<td>1351.6</td>
<td>13.63</td>
</tr>
</tbody>
</table>

These average figures entail an abandonment of any attempt to reveal a law of integral multiples.

In September 1831, the first meeting of the British Association for the Advancement of Science was held at York, and an ad hoc Chemistry Committee resolved that it was of the utmost importance that Chemists should be enabled, by the most accurate experiments, to agree in the relative weights of the several elements, Hydrogen, Oxygen, and Azote, or what amounts to the same thing, that the specific gravity of the three gases should be ascertained in such a way as would insure the reasonable assent of all competent and unprejudiced judges. (190)

In the following June, 1832, at the meeting held in Oxford, Turner presented a report of his investigations of the atomic weights of lead, silver, chlorine and bromine which he published in full in the *Philosophical Magazine*. (191) Since the adoption of the integral multiple weights hypothesis had taken nearly all its proof from


Thomson's First Principles, Turner said, "I turned to that work with the view of putting some of the statements, contained in it, to the test of careful experiment." (192) He reminded his audience that he had shown that the king-pin of Thomson's analytical structure, the analysis of barium chloride, was in material error. Although Thomson had not acknowledged his error in the reply to Berzelius in 1829, Turner was gratified to note that Thomson had quietly lowered the atomic weight of barium from 70 to 68 in a new edition of his System published in 1831. (193) In this, although he avoided any discussion of the integral weights hypothesis (perhaps because of Turner's investigations) Thomson's atomic weights remained integral on the hydrogen scale.

In his 1832 paper, Turner gave only a few of his results, and the full experimental details were reserved for the ears of the Royal Society in the following year. He drew three conclusions from his experiments: (194)

1. The atomic weights commonly used by British chemists have been adopted without due inquiry, and several of the most important ones are erroneous.

2. The hypothesis, that all equivalents are multiples by a whole number of the equivalent of hydrogen, is inconsistent with the present state of chemical knowledge, being at variance with experiment.

3. The subjoined equivalents are very nearly correct:

<table>
<thead>
<tr>
<th>Element</th>
<th>Equivalent</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lead</td>
<td>103.5</td>
</tr>
<tr>
<td>Silver</td>
<td>108</td>
</tr>
<tr>
<td>Barium</td>
<td>68.7</td>
</tr>
<tr>
<td>Nitrogen</td>
<td>14</td>
</tr>
<tr>
<td>Chlorine</td>
<td>35.45</td>
</tr>
</tbody>
</table>

192. Phil. Mag., (3)1,109,1832.


194. Phil. Mag., (3)1,112,1832.
As a result of this Oxford paper, the Chemistry Committee of
the British Association urged Turner to extend his work on atomic
weights. (195) Through references by Berzelius, Staś and others to
Turner's researches, the impression has grown up that this work was
sponsored by the British Association from the very beginning. As can
be seen from the foregoing account, however, this sponsorship came
only in 1832 when Turner's work was approaching completion; after
1833 he published nothing on the subject.

At the same Oxford meeting in 1832, Prout described his measure-
ments of the specific gravity of air, and the Chemistry Committee (of
which he was a member (196) ) asked him, together with John Dalton,
to make fresh experimental studies of the specific gravities of oxy-
gen, hydrogen, and carbon dioxide. (197) The initiative for these
experiments came from Dalton, as may be gathered from a previously
unpublished letter from Daubeny to Prout, 27 October 1831.

I wish you would agree (??) to meet at Oxford next July, when
we propose receiving the British Association lately organized
at York. Dalton wants much to have the specific gravity of
Hydrogen and other fundamental points in the Atomic Theory
settled by a sort of Chemical Committee, and as he promises
to be of the party at Oxford next year it strikes me that a
better time could not be chosen for such an undertaking. My
laboratory, such as it is, could be placed at your service,
though I had rather leave the decision of a point requiring
such delicate manipulation to more skilful hands. (198)

There is no evidence, however, that the intended collaboration between

196. Ibid., pp. 113 and 116.
197. Ibid., p. 116. See also Apjohn, ibid., p. 575 and Chapter 5 supra.
198. Letter 27 October 1831, bound in Royal Institution's copy of
Prout and Dalton ever took place; the results of the later award of £40 to Prout and Thomas Clark of Aberdeen for the specific gravity measurements in 1839 are equally hidden. (199)

This flurry of interest in atomic weights, specific gravities, and the multiple weights hypothesis was also reflected in the masterful Report on the Present State of Chemistry made to the Association by James F.W. Johnston, a pupil of Thomson’s who had also studied with Berzelius. (200) In his report, Johnston mentioned the disagreement and confusion among chemists concerning the relation between atomic weights and volume weights. According to the molecular theories of Ampère and Dumas, he said, equal volumes of all bodies in the gaseous state at the same temperature and pressure contained the same number of atoms; but Berzelius and most continental chemists disagreed with this and accepted only that equal volumes of the permanent gases contained equal numbers of atoms. The latter was a simpler rule and it avoided the anomalies which had made Dumas despair of the atomic theory. But the British school of chemists, Johnston noted, had adopted a third relationship which was identical with that of Berzelius except that oxygen was made to contain twice as many atoms as an equal volume of any other permanent gas. The most obvious consequence of this rule was to give water the formula \( \text{H}_2\text{O} \) instead of Berzelius’s \( \text{H}_2\text{O} \). "This opinion", which was due to Thomson, "involves a departure from the supposed simplicity of nature, for which


200. Ibid., 1, 420-1, 1831-2(1832).
there is, *a priori*, no sufficient reason", declared Johnston. Such a rule had the singular merit that it produced simpler formulae for oxygen compounds but, Johnston warned, a good deal further research was needed to see whether it was the true relationship, or only a convention. As we shall find in the next chapter, although Prout had adopted Thomson's system in his anonymous papers, by 1832 he had adopted a molecular system similar to that of Avogadro.

Whatever the relationship was between specific gravity and atomic weight, Johnston recognised that specific gravity measurements were as much a bone of contention among chemists as atomic weights. To correct the latter by means of the former would therefore probably only lead to a magnification of errors. Clearly separate experimental programmes would have to be undertaken in order to determine accurate values for both quantities, and their mutual relationship would follow from this. It was for this reason, explained Johnston, that the Chemistry Committee of the British Association had made its two recommendations. Finally, Johnston advised that the case for or against the Prout-Thomson law of multiple weights remained unproven.

In 1833 Johnston could, perhaps, have been more dogmatic. For in his Oxford paper Turner had only stated that the atomic weights which he presented were "nearly correct", and that the integral weights hypothesis was "inconsistent with the present state of chemical knowledge". These points were elaborated in a paper read to the Royal Society on 16 May 1833. (201) In some ways, however, Turner

appeared less certain of his conclusions in this paper. For example, he went back on some of his quite accurate earlier results; and though he offered specific results, his individual determinations were often discrepant within the range demanded by Prout's law.

<table>
<thead>
<tr>
<th>Element</th>
<th>Thomson (1825)</th>
<th>Turner (1832)</th>
<th>Turner (1833) (202)</th>
<th>Berzelius</th>
</tr>
</thead>
<tbody>
<tr>
<td>Pb</td>
<td>104</td>
<td>103.5</td>
<td>103.6</td>
<td>103.5598</td>
</tr>
<tr>
<td>Ag</td>
<td>110</td>
<td>108.0</td>
<td>108.0</td>
<td>108.1285</td>
</tr>
<tr>
<td>Ba</td>
<td>70</td>
<td>68.7</td>
<td>68.7</td>
<td>68.5504</td>
</tr>
<tr>
<td>Cl</td>
<td>36</td>
<td>35.5</td>
<td>35.42</td>
<td>35.412</td>
</tr>
<tr>
<td>N</td>
<td>14</td>
<td>14.0</td>
<td>14.15</td>
<td>14.1628</td>
</tr>
<tr>
<td>Hg</td>
<td></td>
<td>202.6</td>
<td>202.5315</td>
<td></td>
</tr>
<tr>
<td>S</td>
<td></td>
<td>16.09</td>
<td>16.0932</td>
<td></td>
</tr>
</tbody>
</table>

His values for silver ranged from 107.92 to 108.08, which led to the slightly embarrassing average integer, 108. He warned that his value for nitrogen was in doubt because any error in the determinations of silver, lead or barium would have been multiplied fivefold in his estimation for nitrogen. To overcome this chemists would have to "give preference to the more direct method, founded on an exact determination of the densities of oxygen and nitrogen gases, such as we anticipate from the labours of Dr. Prout." (203) This seems to be a clear indication that Prout continued with his work on specific gravities.

Despite various discrepancies, Turner's values were closer to those of Berzelius than to Thomson's; in these circumstances Turner declared that:


203. Ibid., p. 539.
Dr. Prout's hypothesis, as advocated by Dr. Thomson, -that all atomic weights are simple multiples of that of hydrogen, -can no longer be maintained.

Integral numbers might still be used as convenient approximations for "medical men, students, and manufacturers", but otherwise chemists must not delude themselves that it was a scientific truth.

However, Turner wanted to salvage something.

Let me not however be misunderstood: I mean simply to affirm that the experiments by which it has been attempted to prove the truth of this hypothesis are inaccurate. I may go further, and declare it to be not only unsupported by evidence, but to be at variance with the most exact analytical researches which have been conducted. I deny not that some simple relation subsists among atomic weights, and that their ratios may possibly be expressed by some simple series of numbers; but at present no one has assigned any physical cause for the existence of such a relation; no such relation has hitherto been discovered; nor, as appears to me, has analytic chemistry attained that degree of perfection which can justify any one in finally asserting or denying its existence.

Turner has sometimes been criticised for this rather wavering conclusion; but coming as it did from a man who took a strictly positivistic line towards the atomic theory, this empiricism was only to be expected. Moreover, Turner was surely right, for his point was only that since analytical chemistry was open to further improvements, it would be wrong of chemists to make unqualified positive or negative guesses about mathematical relationships between the elements. Yet it is doubtful if Turner would have been satisfied by a Periodic law, for he also declared that he required a "physical cause" -that is, a theory of the elements that would account for any such quantitative relationships. But this only came with the electronic theory of...
the atom. Turner evidently did not feel that Prout's protyle
suggestion was anything more than a speculation, and a speculation
that an empiricist should avoid.

It is clear from Turner's remarks that he did not think that
his work proved that there was no relationship between the atomic
weights of the elements. All he had done was to reject the form of
Prout's hypothesis upheld by Thomson. Hence it was possible for
chemists to continue their support for various forms of Prout's
multiple weights hypothesis, and of course its corollary, the protyle
hypothesis. No chemist could avoid noticing, for example, that Turn-
er's and Berzelius's values for the atomic weights of the elements on
the hydrogen scale were still curiously close to whole numbers, or
simple fractions like $\frac{1}{2}$ or $\frac{1}{3}$. Although Thomson refrained from commen-
ting upon the multiple weights hypothesis in his History of Chem-
istry (205) published in 1830, he seems to have concluded that Turner
had merely subscribed to the views of his continental enemies. In 1836
he continued to uphold the validity of the hypothesis even though he
no longer used Prout's name in this connection. (206) This seems to
have been Thomson's last comment on the subject; he lived on until
1852, and it would be interesting to have known his reaction to the
work of Dumas and Stas in 1840.

name is only mentioned in connection with the rule for specific
gravity calculations, vol. 2, p. 300; the multiple weights
hypothesis is, however, ascribed to Dalton (sic) without any

206. Records of General Science, 3,179-191,251-3,1830, e.g. "I con-
ceive I have proved to the satisfaction of the most squeamish
chemist, that the atomic weight of sulphur is 2. And that
Berzelius's number, and of course Dr. Turner's is erroneous,
exceeding the truth by rather more than a half per cent", p. 189.
Two publications in the *Philosophical Transactions* for 1839 illustrate the problem of falsifying Prout's multiple weights hypothesis. In the first, Frederick Penny made a careful series of analyses using techniques that foreshadowed those used by Stas. He found in favour of Berzelius and Turner.

The estimates in general use among British chemists are not the strict representatives of chemical truth, founded on experiment, and... the favourite hypothesis, of all equivalents being simple multiples of Hydrogen, is no longer tenable. (207)

Thus, even six years after Turner's great paper it is clear that British chemists were still using Thomson's simple values even for accurate work, and that the integral multiple weights hypothesis was still maintained by them. This is also clear from a paper published by Richard Phillips in the same year. Phillips - friend of Prout and a continued supporter of Thomson - reached the opposite conclusion to Penny; Turner's results were inaccurate and "no material, and scarcely even any appreciable error can arise from considering the equivalents of hydrogen, oxygen, azote, and chlorine, as 1, 8, 14 and 36 respectively."

(208)

**Penny's Table**

<table>
<thead>
<tr>
<th></th>
<th>Thompson (H=1)</th>
<th>Turner</th>
<th>Penny (1839)</th>
</tr>
</thead>
<tbody>
<tr>
<td>O</td>
<td>8</td>
<td>8</td>
<td>8</td>
</tr>
<tr>
<td>C1</td>
<td>36</td>
<td>35.42</td>
<td>35.45</td>
</tr>
<tr>
<td>N</td>
<td>14</td>
<td>14.15</td>
<td>14.02</td>
</tr>
<tr>
<td>K</td>
<td>40</td>
<td>39.15</td>
<td>39.08</td>
</tr>
<tr>
<td>Na</td>
<td>24</td>
<td>23.3</td>
<td>23.05</td>
</tr>
<tr>
<td>Ag</td>
<td>110</td>
<td>108.0</td>
<td>107.97</td>
</tr>
</tbody>
</table>

207. *Phil.Trans.*, 1839, 13-33 (p.32)

208. *Phil.Trans.*, 1839, 35-8 (p.37). Thomson's values were still being used in 1845 in both Great Britain and America; cf. *Encyclopaedia Metropolitana*, vol.4, 1845, Article "Chemistry", and Berzelius, *Pref.*, 3, vii, 249.
Even if one agreed with Berzelius, Turner and Penny in the rejection of Thomson's values, it was still possible to support a multiple weights hypothesis. In his own publications before 1625, Thomson had always implied that the basis of a law of multiples could be a fractional unit of the weight of hydrogen. This was also Prout's idea, and in 1631 he proposed that this might have some sort of physical basis. In this year the Professor of Chemistry at Oxford, Charles Daubeny, prepared his *Introduction to the Atomic Theory* (209) for publication, and he sent the proof sheets to Prout for his comments. The latter obliged with a long letter (printed by Daubeny as an Appendix) in which he suggested that there was no sufficient reason "why bodies still lower in the scale than hydrogen (similarly related to one another, however, as well as to hydrogen) may not exist, of which other bodies may be multiples without being actually multiples of the intermediate hydrogen." (210) This speculation that the protyle might be half, a quarter, or some other fraction of the hydrogen atom, was Prout's first public pronouncement on the protyle hypothesis since 1616. It was to be taken up by Marignac, Mauméne, Pelouze, and above all by Dumas; but it was to be firmly rejected by Stas.

This new suggestion from Prout prompts the questions: how did Prout view the fortunes of his hypotheses during the period we have reviewed? And more generally, what support did the protyle or


reductionist hypothesis receive during the period 1816 to 1839? As we have recognised, the latter question was bound up with the status of the chemical elements.

Since Prout has left no recorded comment concerning his attitude towards the controversies over atomic weights, anything I say will be necessarily conjectural. I think that it is almost certain, however, in view of Prout's letter to Daubeny in 1831, that his commitment to the unity of matter was strong enough to withstand any criticism of Thomson's atomic weights. Like most British chemists he probably felt at first that Thomson was right; in 1827 he noted with pleasure that the law of multiples had been adopted by Thomson and most other British chemists. (140) But the fact that he disagreed with Thomson over the analysis of oxalic acid, and the fact that he helped Turner with his analysis of silver chloride, suggests that he was willing to agree with Turner's strictures on Thomson's methods, and also agree with him that the simple law of multiples maintained by Thomson was erroneous. This would have been the point of his new speculation offered to Daubeny. Indeed, in the Gulstonian lectures which Prout delivered to the Royal College of Physicians in 1831, he wondered whether:

the science of chemistry had not been rather retarded by it [Dalton's theory] than advanced; for to suit the imaginary standards of this bed of Procrustes, real results, I fear, have been too often extended or compressed beyond all legitimate bounds, and thus truth sacrificed to error. (211)

This could refer to Thomson. It is clear from the Bridgewater Treatise

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that Prout never abandoned his own belief in the unity of matter, or that there was a mathematical relationship between the elements.

(Chapter 8)

As far as his contemporaries are concerned, there was surprisingly little comment on the question of the status of the chemical elements between 1816 and the establishment of the phenomena of isomorphism and isomerism, and the development of the organic radical theory in the 1830s. Ludwig Meinecke, Professor of Technology at Halle University, who translated several of Prout's papers into German for Schweigger's Journal, published a series of papers on specific gravities and stoichiometry between 1816 and 1819. He may have been influenced by Prout's first paper of 1815, though he later denied this and suggested plausibly that they had both independently derived their ideas from the work of Dalton and Gay Lussac.

Meinecke adopted a round number (runder zahl) to represent the weight of a portion (Antheile) of each element; this number was deduced from the analyses of other chemists but so chosen as to be a multiple of either the portion of oxygen, or of hydrogen. He attributed the belief that all stoichiometric magnitudes were integral multiples of hydrogen to Dalton, but like Prout he pointed out that the

212. Gilbert's Annalen der Physik, (2)24, 159-75, 1816; Schweigger's J. für Chemie und Physik, 22, 137, 1818; 27, 39, 1819. For references to other papers, see Prout's Hypothesis, pp. 7-13 and J.R. Partington, History, 4, p. 224.


214. This has caused confusion among historians who have sometimes thought Meinecke attributed the protyle hypothesis to Dalton. Since atoms were indivisible for Dalton, this would have been impossible; Meinecke did not make this mistake. See editorial discussion in Prout's Hypothesis, and C.T. Benfey, J. Chemical Education, 29, 79, 1952.
multiple relations suggested that Daltonian atomism needed modification: there were not many elements.

Even if one does not accept the atomistic views which are the basis for Dalton's rule, one must nevertheless think it curious that most of the stoichiometric numbers are exactly divisible by the number for hydrogen, and all these numbers are nearly so divisible. Although it must not be concluded from this that all substances contain hydrogen as a fundamental element, it has to be assumed that every substance contains a specific proportion of the principle that particularly characterises hydrogen (namely, combustibility, affinity for oxygen, phlogiston, negative electricity) and since every new experience in chemistry shows with increasing clarity that there is a great simplicity in chemical compounds, one may venture the hypothesis that the specific degrees of combustibility (oxidisability, negative electricity), which characterise the simple substances, can be expressed by masses of the hydrogen value; or, in other words, that since the stoichiometric numbers are actually only derived from their behaviour to oxygen, all stoichiometric numbers of the simple substances have to be divisible by the value for hydrogen. (215)

Consequently, there had to be a simple relation between specific gravities of gases and their stoichiometric values. (216) Meinecke,
then, thought that the measurable properties of the elements and compounds suggested either that the "elements" were graded polymers of hydrogen, or that the elements were mixtures of hydrogen and some other principles.

Despite the large number of papers he wrote on the subject, together with priority disputes, Meinecke's work attracted little attention, and his suicide in 1823 prevented him from entering into the later Thomson-Turner discussions. Although much attention was paid to Naturphilosophie in Germany, his ideas do not appear to have sparked off any interest in the possible light which chemistry could shed on the unity of nature. Instead that interest seems to have been the preoccupation of British chemists.

In his rejected Royal Society paper on the kinetic theory of gases, John Herapath pictured heat as the motion of hard elastic atoms of various shapes and sizes. Clearly this was a corpuscular theory of matter; not unnaturally therefore, he supported "one of the sublimest ideas of the ancients."

There is but one kind of matter, from the different sizes, figures, and arrangements of whose primitive parts, arises all the beautiful variety of colour, hardness and softness, solidity and fluidity, opacity and transparency &c which is observed in the production of Nature." (217)

Herapath believed, on the grounds of simplicity alone, that probability was "strongly in favour of the ancient idea". However, chemists paid little attention to these physical fantasies of Herapath even though he went on to develop a molecular theory (similar to the forgotten Avogadro's) which encompassed chemical reactions as well as the

217. Ann.Phil., 17 (=1), 231, 1821; see also ibid., 7, 58-9, 1816 where he had made the same point.
phenomena more familiarly described as kinetic. In this analysis he allowed the particles of the so-called simple bodies to decompose during chemical change. (218)

Faraday, who was in many ways a faithful disciple of Davy, expressed dissatisfaction with the great number of metals in 1818 (219). He also, like Davy, adopted a Boscovichian force-atom which supposed a homogeneity, or unity of matter. (220) The alteration of force fields explained not only chemical change, but raised the possibility of the transmutation of elements. Later in the century, Faraday became very interested in Dumas's speculations about the elements, but it is not known whether he had any intercourse, social or scientific, with William Prout.

Apart from these two well-known scientists, and odd references by one or two other obscurer figures, the interest of chemists seems to have been entirely absorbed by the problems of atomic weight determinations, analysis, and the Prout-Thomson multiple weights hypothesis. The question of the simplicity or complexity of the chemical elements was always in the background however. With the appearance of fresh arguments based upon new analogies drawn from the organic radical theory, isomorphism, isomerism, atomic heats, and above all the redetermination of the atomic weight of carbon by Dumas and Stas in 1840, Prout's protyle hypothesis and the status of the chemical elements, once again became a talking point among chemists. (221)

At the beginning of 1840 bad health sent Dumas to take the waters at Aix where he met the young Belgian chemist, Jean Servais Stas. Together they began to redetermine the atomic weight of carbon which had proved necessary because of the anomalies in the analyses of naphthalene when Berzelius's value $C = 76.42 \ (O = 100)$ was used. The experiments of Dumas and Stas were continued in Paris with the greatest care by burning pure carbon compounds in oxygen. There was no doubt about the result: Berzelius's old value was too great by at least 2%, and it would have to be reduced to $C = 73$.

In a letter to the French Academy of Sciences Dumas pointed out that this value had been the theoretical one deduced by Prout in 1815 ($C = 6, \ H = 1$). Had Prout been right in other cases?

Si, comme le pense le Dr. Prout, et comme cela parait maintenant fort probable, tous les poids atomiques sont des multiples de celui de l'hydrogène par des nombres entiers, il y aurait bien des choses à rectifier dans les poids atomiques actuellement admis. (223)

All atomic weights should be redetermined immediately, both for the sake of inorganic and organic analyses, and because of the light the new results might shed on the true nature of substances which had been thought simple.

While not all chemists could share Dumas and Stas' enthusiasm for the hint of a new chemical and world viewpoint, their experimental result was rapidly vindicated. Already, in 1839, the English chemist George Fownes, working in Liebig's laboratory, had reduced


the value of carbon's equivalent, \((224)\) and the need to make some reduction was confirmed by Liebig and Redtenbacher in 1841. \((225)\)

In the same year the Frenchmen's result was completely vindicated by the Germans Erdmann and Marchand, \((226)\) and by the Swiss analyst, Marignac, \((227)\) who showed that Liebig and Redtenbacher's method was inaccurate. But because of their personality disputes with Dumas over the theory of organic chemistry, neither Liebig nor Berzelius was able to adopt the French result with its implication of Multiplenfieber. \((228)\) Yet Berzelius was sensible enough to realise that his own value for carbon was at fault. In a new determination made by physical methods he found \(C = 73.12\). \((229)\)

Just as Prout's original papers, or rather those of Thomson, had split chemists into two camps, so did this redetermination of the atomic weight of carbon. Throughout the 1840s, therefore, there was a bustle of analytical activity in which improved methods of accuracy, such as buoyancy corrections, and the determination of excess solutions after volumetric endpoints, were devised as Dumas, Stas, Erdmann

\begin{itemize}
  \item \textit{Phil. Mag.}, (3)15,62,1839.
  \item \textit{Annalen d.Chemie u. Pharmacie}, 38,113-40,1841; \textit{Phil. Mag.}, (3)19, 210-28,1841; \(C = 75.854\).
  \item \textit{J.für Praktische Chemie}, 23,159,1841; \textit{Phil. Mag.}, (3)18,332, 1841; \(C = 75.07\).
  \item J.R. Partington, \textit{History}, 4, p. 230.
  \item Liebig's comment, \textit{Annalen d.Chemie u. Pharmacie}, 38,195-216,1841.
  \item \textit{Jahres Bericht}, 23,26-30,1844.
\end{itemize}
and Marchand, Pelouze, Mauméne, and Marignac buckled down to the redetermination of atomic weights. Oddly, this was almost exclusively a continental activity, and British chemists, though intensely interested in the results, preferred to speculate rather than investigate.

Erdmann and Marchand, who made their determinations between 1841 and 1852, were in favour of the integral multiples hypothesis, and the latter chemist used integral weights in his biochemistry textbook, *Lehrbuch der Physiologischen Chemie*, in 1844. (230) Dumas himself immediately went on to redetermine the ratio between hydrogen and oxygen, and concluded that \( 0 = 8 \) (\( H = 1 \)) within the limits of experimental error. (231) The result was confirmed by Erdmann and Marchand. (232) Dumas now felt convinced that Prout had glimpsed some great general truth.

In considering water to be composed of 1 hydrogen for 8 oxygen, a chemist will never be exposed to an error in his experiments or calculations ... all the atomic weights are in need of attentive revision; without adopting or rejecting the opinions of Doctor Prout, I am forced to allow that they are generally in accordance with my experiments. (233)

Prout's hypotheses were once again open questions.

Both Dumas and Stas began their work with a bias towards the integral multiple weights hypothesis and the unity of matter; but from


231. *Comptes Rendus*, 14, 537-47, 1842. Berzelius had given \( 0 = 16.016 \) (\( H = 1 \)).


1840 onwards their paths diverged. In view of Dumas' commitment to a molecular theory of matter, his feeling for the complexity of the chemical elements, and that there must be mathematical relations between them, is not surprising. (234) In particular he pointed out that specific heats and isomorphism were evidence for the complexity of the elements. "In my youth", Stas reported to Leo Baekeland,

I was an ardent believer in the unity of matter as expounded by Prout. I was so well convinced about his theory that I became eager to furnish additional proofs by redetermining more accurately the atomic weights of those elements where the atomic numbers were not an even multiple of hydrogen. I simply imagined that more careful determinations would have eliminated these irregularities. But the more I eliminated any errors of experimentation, so much the more did my results contradict my dearest hopes. Finally, I had to admit that I was beaten and had spent the most important part of my life in killing my first love as a theory. (235)

Stas, in Belgium, spent twenty years from 1840 to 1860 working towards this conclusion; but Dumas, in France, spent seventeen years from 1840 to 1856 working towards the opposite conclusion that Prout's law of multiples was true—in a modified form. (236) Unfortunately, Prout did not live to read these contradictory views.

During the 1840s there was also renewed speculation in Great Britain concerning the possibility of transmutation. In 1844, David Low, Professor of Chemistry at Edinburgh, published a very successful Inquiry into the Nature of the Simple Bodies of Chemistry in which he endeavoured to show that:


It is not necessary, in our inquiries into the phenomena of chemical actions, and the laws which determine them, to assume the existence of many elements, distinct in their corpuscular constitution, from one another, and from the bodies which experiment has determined to be composed of more than one element or member. (237)

Low felt as unhappy as Davy once had about the apparent complexity of the chemical world, and like him supposed that analogical arguments implied that:

If bodies which we term simple, present the same general physical properties, and exert the same chemical actions as those which we term compound, and pass into the compound bodies in their characters and functions, the merely negative evidence, that we are unable to decompose them by overcoming their chemical affinities, should not invalidate the conclusions, that both kinds of bodies are to be placed in the same class of natural products, and cannot be separated the one from the other by so wide a chasm as a distinct corpuscular constitution. (238)

This was sober reasoning in the Davy-Prout tradition. But Low went on to speculate extravagantly that all the chemists' elements were compounds of just hydrogen and carbon—the elements with the lowest atomic weights. There was no reason, he thought, why even these two elements should not turn out to be composed of a single matter which was polarised as m+ and m-.

Richard Phillips, who otherwise supported the multiple weights relationship (supra), wrote a very sarcastic review of Low's book; (239) but Low replied reasonably enough that his ideas concerning the complexity of the elements were not new. He had merely "ventured to

238. Ibid., 2nd ed., 1848, p. 2.
239. Phil. Mag., (3)24,296,1844.
enunciate the proposition somewhat more precisely" than, for example, Davy; Low did not mention Prout. Some experimental evidence was cited by Low, and by his fellow countrymen, Rigg (240) and Samuel Brown. (241) Such evidence, which is reminiscent of Miers' work on nitrogen earlier, was both inaccurately done and inaccurately interpreted; as such, it was often refuted. (242)

Even Berzelius was drawn into the new debate over the nature of the elements and their arithmetical relations. In 1823 he had published a discussion of the integral multiples hypothesis and admitted that it might be true for the lighter elements, though he warned that Thomson's experiments published in the Annals of Philosophy were not to be trusted.

The fact that at present there is not a single reason, either chemical or physical, for believing this to be the case, does not preclude its possibility.

Even so, he pointed out, there were the glaring anomalies of carbon and chlorine. (243)

Thomson's work soon put paid to any benevolent feelings which Berzelius might have had towards Prout's hypothesis. His demand for rigorous analytical standards meant that adjustments to atomic weights were to be made only on the basis of experiment, and not on the

240. R. Rigg, Experimental Researches shewing Carbon to be a Compound Body made by Plants, London, 1844.


242. G. Wilson and J.C. Brown, ibid., pp. 547-559, read 1844; Phil. Mag., (3) 19, 295, 1841. For a fuller discussion of the "celtic movement", see D.M. Knight, loc. cit., ref. 5.

preconceptions of Prout's integral "law" -or Multiplenfieber, as he sarcastically called it. Turner's work had apparently proved Berzelius right; but then came Dumas and Stas's adjustment of the weight of carbon, together with the fresh determinations of Erdmann and Marchand, and Marignac; analysts who were all committed to a multiples relationship and a unitary theory of matter, but who were respected by Berzelius. Berzelius felt bound to speak out. Therefore he welcomed the inquiry as to his opinion of Prout's hypothesis which was made in 1844 by the American, Benjamin Silliman junior. (244)

Berzelius's masterful reply was published in the American Journal of Science in 1845. (245) In it he reiterated his belief that experiment, and only experiment, could decide the issue. When Prout had "imagined" his hypothesis originally, it had seemed that atomic weight determinations could never attain great accuracy, but that they suggested that within the limits of unavoidable experimental error, atomic weights were multiples of the atomic weight of hydrogen. Even so, argued Berzelius, this extrapolation had not been justified even then in all instances. Prout, he said mistakenly, had not expressed any idea on the cause of the multiples; but this, if it really existed, could only be that hydrogen was the ultimate primary matter. Where was the evidence for this though? Had transmutations of the elements ever been observed? At the most chemists had found elements with the same atomic weights; and Berzelius felt that this phenomenon was evidence against a polymer hypothesis rather than for it.

Thomson's experimental work, although quite uncritical, had led to the adoption of the integral multiples hypothesis in many quarters, and the idea had persisted despite Turner's work. Dumas had published atomic weights for carbon, oxygen, hydrogen, nitrogen and calcium which supported the hypothesis, and the incomparably exact work of Marignac had shown that other elements approached multiples of half the atomic weight of hydrogen. However, to say that the elements were exact multiples of \( \frac{0}{5} \) would be to go beyond the limits of experimental error. Even Marignac had been forced to conclude that chlorine was an exception to any such law. (246) Berzelius's own atomic weights, though some had been made over thirty years before, provided innumerable exceptions to Prout's relation. Yet Berzelius was prepared to admit that some kind of arithmetical law might be found to hold between the platinum metals.

Berzelius's American article shows that he believed firmly in the reality of many elements, and that he felt that the fact that many atomic weights approached integral or half-integral values was largely coincidental. In those cases where elements had very similar properties, such relationships might well prove the case, though only experiment could prove them. Berzelius's opinions seem very similar to the conclusions that Stas published in 1860. (247) They were reiterated in the final French edition of his textbook where he warned that unitary theories of matter were speculative and undemonstrated. (248) His final words on the subject are contained in the

\[246. \text{Berzelius, Bref, vol. 3, vii, p. 214.} \]
\[247. \text{Prout's Hypothesis, p. 41.} \]
fourth volume of this translation published in 1847. Speculation on the whole subject had to cease, otherwise:

Elle pourrait facilement conduire à des hypothèses dont la fausseté ne se trahirait peut-être que tard. Je pense donc que, dans aucun cas, lorsque le poids atomiques d'un corps simple se rapproche du multiple d'un autre corps, on ne doit rendre le nombre, donné par l'expérience, égal à ce multiple. Ceci ne devra se faire qu'à une époque du développement de la science, où l'on pourra répondre sûrement de l'exactitude de ces rapports. (249)

Despite their lip-service to Baconism, few British chemists were this empirical. Daniell noted in 1843 that "the more accurate our analyses become, the nearer appears to be the coincidence of facts with [Prout's] theory."

The notion is doubtless founded upon a sense of that sublime symmetry and simplicity which, the more we inquire the more we find pervading all the works of creation; and when we recollect that in the most perfect of all the sciences, the Laws of Kepler themselves, which have been so amply confirmed by the triumphant progress of Astronomy, were derived from similar views of the geometric harmony of Nature, we are inclined not to reject this view of numerical harmony in the composition of bodies, until actually proved to be inconsistent with the positive results of accurate experiment. (250)

There is a refreshing intellectual honesty about the writings of those who supported Prout.

By 1850 then, the experimentalists' conclusions from their atomic weight determinations was that Prout's integral multiples hypothesis did not hold in all instances. (251) At the same time, Marignac,

Mauméné, and to some extent, Pelouze, attempted to save the phenomena by following up Prout's earlier suggestion that the prime matter could be something less than the hydrogen atom by proposing that a unit of 0.5 might solve the persistent anomaly of chlorine.\(^{252}\)

Thomas Graham, who believed in the unity of matter,\(^{253}\) was very excited by these suggestions, and he concluded in 1850 that:

> It appears to be definitely settled that the equivalents of the elements are not, without exception, multiples of the equivalent of hydrogen. The number for chlorine (35.5) is conclusive against that hypothesis. At the same time, the accurate determinations of the equivalents of chlorine, silver, and potassium, by Mauméné, lend positive support to the opinion that these and all other equivalents are multiples of half the equivalent of hydrogen. So do the recent determinations of carbon and hydrogen in reference to oxygen, and those of nitrogen, sodium, iron and calcium. The number for lead also, upon the determination of which extraordinary pains have been bestowed by Berzelius at different times, namely 103.56, is favourable to the same view. \(^{254}\)

Thus despite the opposition of Berzelius, the last decade of Prout's life saw a speculative and experimental renaissance of Prout's Hypothesis. Not surprisingly, Prout was delighted that his views on the relations between the elements seemed vindicated.\(^{255}\) However,


it is not part of may plan to trace the fate of Prout's Hypotheses after 1850. Instead I shall turn to examine the molecular theory that Prout developed after 1816. The events which have been described in this Chapter have all been examined implicitly within the context of the simple atomic theory as propagated by Dalton, Thomson and Berzelius. But Prout's belief in the unity of matter led him to reject simple Daltonian atomism for a corpuscular physics. The molecular, or polarity, theory which resulted from this was the unifying theory by which Prout hoped to explain all chemical and biochemical phenomena.
Chapter Eight: Prout's Molecular Theory and Biochemistry

Chemistry forms the connecting link between that kind of knowledge which is founded on quantity; and those kinds of knowledge which rest solely on experience. (1)

The analytical chemist John Mallet, who wrote an excellent study of nineteenth-century atomic weight determinations for the Chemical Society in 1893, was (I believe) the first person to suggest that the real significance of Prout's anonymous papers of 1815 and 1816 was that, without realising it, Prout had based his work on the hypothesis of Avogadro. Mallet believed that

an originally incidental remark of Prout arrested the attention of Thomson, and he and Prout, charmed by the extreme simplicity of the volume relations of substances in the gaseous state (as demonstrated by Gay Lussac) - relations really dependent on the numbers of molecules concerned in reactions - thought that they had discovered a like simplicity between the weights of reacting molecules (or atoms, these two being then confounded with each other). (2)

Such may be true of Thomson, but as has been shown in the last chapter, Prout himself played the multiple weights hypothesis, and the protyle hypothesis, very quietly, and left it to others (notably Thomson and Turner) to test their validity. Moreover, Mallet seems to have been

unaware that Prout at some stage in his career was able to understand that the regularities of Gay Lussac's law of combining volumes depended upon the number of molecules involved in a reaction. For, as Andrew Meldrum first pointed out, (3) Prout formally announced and supported Avogadro's hypothesis in his Bridgewater Treatise published in 1834.

However, it seems to me, as I implied in the last chapter, that Mallet was probably correct in suggesting that Prout did not appreciate Avogadro's hypothesis when he first published his views on the relation between specific gravity and atomic weight. On the other hand, I believe that Prout's commitment to the unity of matter entailed viewing chemical change from a molecular viewpoint, and this in turn prepared him to adopt the Avogadro law as a deduction from the gas laws. It may be argued that the reverse is equally probable, namely that the deduction of Avogadro's law from the gas laws would have prepared Prout to split Daltonian atoms whenever necessary. If this were the case, then he should have found no difficulty in moving to a position in which all the elements were molecular conglomerations of the hydrogen atom, or some simpler species. This latter suggestion was made by me in a published article, (4) but I no longer believe it tenable in view of the new evidence that Prout supported a protyle hypothesis as an undergraduate before he knew of either Dalton's atomic theory, or Gay Lussac's gaseous laws of combination. The confused state of Prout's anonymous papers now lead me to suppose

that the first suggestion is more probable: Prout only deduced Avogadro's law after the publication of his anonymous articles. This may have occurred at any time between 1816 and 1833, but in view of Prout's remarks concerning the molecular law which will be discussed in a moment, and his mathematical speculations in the urine papers of 1817 and 1818 (Chapter 3), it appears likely that he made the deduction very soon after March 1816.

After 1816, Prout devoted all his research attention to biochemistry, and later he recalled that his views on the relation between specific gravity and chemical proportions:

led me to others which I was exceedingly anxious to verify; and as I was interested ... in the composition of organic substances, it struck me that by submitting these substances to analysis, I might not only obtain a knowledge of their composition, but by investigating the laws which regulate the union of the elements, hydrogen, carbon, oxygen and azote, be able to obtain an insight into the laws which regulate the union of other elementary principles. (5)

The result was a highly speculative matter theory which "after twenty years of close attention and no ordinary labour, we have been induced to consider as the most simple and consistent with the phenomena." (6)

Prout published two slightly different versions of this molecular theory in the first and third editions of his Bridgewater Treatise, 1834 and 1845 respectively. (7) The later version was given in

much fuller detail and it is undoubtedly a development of the 1834 thesis as well as an elaboration of it. In addition a draft of the long 1845 version, which dates from 1837 and offers some additional details, has survived in manuscript. (d) A composite version of this theory will now be described, and this will be followed by an attempt to link the theory with the account of Prout's chemical and physiological work which has been presented in earlier chapters.

Since the time of Newton, Prout explained, natural philosophers had recognised the operation of two antagonistic forces in their analysis of reciprocal motions; an attractive centripetal or gravitating force, and a repulsive, centrifugal or opposing force. Both forces belonged inherently to every atom and aggregation of matter within the universe, and the force of repulsion was usually recognised as the "inertia" of matter, i.e. its resistance to the attractive force. Prout noted that the alternative "Dynamic Physics" of Kant and the Naturphilosophen (whom he did not identify) "while it does not exclude Newton's principles; may, from its more general character, be said to comprehend or include them." (9) In this mechanical system, the repulsive force resulted from the motion of matter which was conceived to possess an inherent tendency to revolve


on its axis with a velocity inversely proportional to the mass. The attractive force, on the other hand, was directly proportional to the mass, and exceeded the repulsive force by an amount that was recognised as the attractive force of gravitation. It was this system which Prout adopted in his own brand of molecular physics where he also pursued another aspect of the Kant-Schelling Naturphilosophie programme, the unification of forces. (10) In Prout's case this comprised the reduction of heat, light, electricity and magnetism (the classical imponderable fluids) to attractive and repulsive forces produced by the axial rotation of minute masses of matter. His most likely mentors in Naturphilosophie are Davy and Oersted, (11) but he always placed a higher value on empiricism as a guide to understanding nature, rather than on intuition.

At the molecular level, thought Prout, a large number of hypotheses had been proposed concerning the nature of molecular forces and their interaction. The commonest hypothesis was an extrapolation of Newtonian macrophysics to microphysics. The molecular forces were thought to be composed of two antagonistic forces, one attractive, the other repulsive, which were either identical with the forces of macrophysics, or simple modifications of them. However, this simple molecular theory had not been able to explain successfully the phenomenon of cohesion, or the phenomena of heat and light which

10. "All natural philosophy consists ... in the reduction of given forces in appearances diverse, to a small number of forces, adequate to the explanation of the effects of the former, beyond which our Reason cannot proceed," Kant, Met. Anfangs., Bax trans., p.212.

had had to be explained by additional hypotheses such as that light was the wave motion of an ethereal medium. But a brave attempt had been made by the Italian physicist, Ottaviano Mosotti, in 1836 to derive a mathematical theory of molecular forces that would include cohesion and electricity. (12)

Following the efforts of Davy and Berzelius, who had both considered that the ultimate cause of chemical combination was electrical, any molecular theory of electricity had to be seriously considered. Mosotti had adopted the single electric fluid of Franklin and Aepinus and combined it with a variety of Boscovichean atomism. "I have supposed", Mosotti wrote,

that a number of material molecules are plunged into a boundless aether, and that these molecules and the atoms of the aether are subject to the action of forces required by the theory of Aepinus; and then endeavoured to ascertain the conditions of equilibrium of the aether and the molecules. Considering the aether as a continuous mass, and the molecules as isolated bodies, I found that if the latter be spherical, they are surrounded by an atmosphere the density of which decreases according to a function of the distance which contains an exponential factor. ... Proceeding in the next place to the condition of equilibrium of the molecules, I observed that, for a first approximation ..., the reciprocal action of two molecules and of their surrounding atmospheres is independent of the presence of the others, and possesses all the characteristics of molecular action. At first it is repulsive, and contains an exponential factor which is capable of making it decrease very rapidly; it vanishes soon after, and at this distance two molecules would be as much indisposed to approach more nearly as they would be to recede further from each other; so that they would remain in a state of steady equilibrium. At a greater distance the molecules would attract each other, and their attraction would increase with their distance up to a certain point, at which it would attain a maximum; beyond this point it would diminish, and at

a sensible distance would decrease directly as the product of their mass, and inversely as the square of their distances. (13)

\[ \text{Mosotti's force function} \]
\[ \text{Boscovich's force function} \]

It will be noticed that Mosotti's force diagram is very similar to part of the more familiar Boscovich force curve. Mosotti's molecules were self-repulsive, like the aetherial particles, but less than the latter, so that the molecules and aether molecules attracted each other with different intensities. The resultant was an excess of attraction over repulsion which was identical with gravitational force. (14)

However, Prout felt unable to recommend the adoption of either the Newtonian theory, or that of Mosotti, or that of Boscovich, both because he thought they did not provide a unified theory of physical and chemical phenomena, and because they were so complicated as to render them "utterly unworthy to be ascribed to the Deity, whose primary laws are all simple, general, and comprehensive." (16)

15. Prout did not discuss Boscovich's theory at any length, but several asides make it clear that he would never accept point atoms.
Prout required instead a strict adherence to the God-given principle of the uniformity of nature. There could not be two primary sets of laws, one for microphysics, the other for macrophysics. His own preference, as he put it, was for the mechanical (i.e. dynamical) hypothesis of macrophysics and microphysics.

viz., that two distinct antagonistic forces were originally imparted by the Deity to all matter; that one of these forces is in its nature attractive, and increases as the quantity of matter increases; that the other of these forces is exerted by matter in motion on itself (or axis); is of a repulsive nature, and increases in intensity (like the motion through which it is exerted) as the quantity of matter decreases; and finally, that the operations of the microcosm, or world imperceptible to our senses, are precisely identical with the operations of the macrocosm, or sensible universe. (16)

This was Prout's Polarity Theory.

Matter was composed of spherically-shaped molecules which possessed a natural "tendency to revolve on their axes" with a velocity inversely proportional to their molecular weight. God had created two antagonistic forces between molecules: an attractive centripetal force proportional to the mass, and a repulsive force inversely proportional to mass which resulted directly from the resultant motion. Prout agreed with Mosotti that the force of gravitation was probably the excess difference between these forces, for if it were not for "the opposing centrifugal force, the whole of the matter in the Universe would instantaneously rush together in one mass." (17)

Like Berzelius, Prout explained cohesion and chemical affinity in terms of polarisation. (18) Polarity, caused by the "motions of


two contiguous molecules on their axes", was of two kinds: homogeneous polarity, or cohesive attraction or repulsion between similar molecules; and heterogeneous polarity, or chemical attraction or repulsion between dissimilar molecules. In the two 1834 editions of the Chemistry Prout only described his ideas concerning homogeneous molecular arrangements, i.e. what we would describe today as the physical or molecular forces between the molecules of a homogeneous material.

In the solid crystalline state, Prout conjectured, identical spheres which each possessed a polar, or chemical, axis that was electrical in nature, and various magnetic cohesive axes, united together in three-dimensional agglomerations, Davy's statement that "the laws of definite proportions and the electrical polarities of bodies, seem to be intimately related", (19) was combined by Prout with the recent work on electromagnetism. Oersted's fundamental experiment (20) which had established the existence of magnetic forces distributed in a circle in the neighbourhood of an electric current, was treated as a model of the situation at molecular level. The chemical axis of a molecule was analogous to the wires carrying the electric current. But in the case of the wires, as Ampère had shown, (21) in consequence of the magnetic forces which were also present, similar wires were attracted or repelled according to the

direction of the current. These effects were analogous to the action of cohesive axes in the molecule. Among molecules, attraction occurred if the equatorial motions were in opposite directions, for their identical velocities would be dampened and a static union produced; repulsion occurred if the equatorial motions were in the same direction. (22) It seemed "very probable, nay almost inevitable ... that the electric polarities correspond with the supposed chemical polarities, and the magnetic with the cohesive polarities of our molecule." (23)

Prout clarified his model by referring to the following diagrams.

The identical spheres

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M  M'  M''  M'''
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in which Ee is the electrical (chemical) axis, Mm, M'm', etc. are equatorial magnetic (cohesive) axes, can stabilise their mutual rotatory forces by aggregating into a cubic mass, or some other derivative solid. (24)

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23. Chemistry, 1st ed., 1834, p.44.
This was analogous to Ampère's electromagnetic experiment in which positive electricity flowed in a wire connecting the copper and zinc plates of a battery, and negative electricity from zinc to copper, while another electrical (magnetic) current ran in spirals round the wires such that the north pole of a magnet moved from right to left.

When two sets of such wires were placed side by side:

- led to magnetic attraction; a situation paralleled by cohesive attraction, where the equatorial poles move in opposite directions, leading to static aggregation.
- led to magnetic repulsion; a situation paralleled by molecular repulsion (or divulsion) due to the equatorial poles revolving in the same direction.

Prout's polarity hypothesis was a model which easily accommodated changes of phase. In the first published version of the hypothesis, he treated heat as a polarisable fluid, caloric. As in Dalton's atomic theory, he supposed that caloric formed an atmosphere around a particle, and inasmuch as the overall volume of the molecule was increased, the forces of magnetic cohesion were weakened so that a rotation of the molecules about their axes of polarity easily occurred. This "loosening of the bonds" explained both the production of, and the different physical properties of, liquid and gaseous
states of matter. However, in the more detailed later version of his hypothesis, Prout described "the molecules of the imponderable fluids light and heat ... as immeasurably smaller, and to move with inconceivably greater velocity than the molecules of any ponderable substance." (25) This idea seems to have followed from Davy's suggestion in 1812 that

It seems possible to account for all the phenomena of heat, if it be supposed that in solids the particles are in a constant state of vibratory motion, the particles of the hottest bodies moving with the greatest velocity, and through the greatest space; that in fluids and elastic fluids, besides the vibratory motion, which must be conceived greatest in the last, the particles have a motion round their own axes, with different velocities, the particles of elastic fluids moving with the greatest quickness; and that in ethereal substances the particles move round their own axes, and separate from each other, penetrating in right lines through space. (i.e. self-repulsive) Temperature may be conceived to depend upon the velocities of the vibrations; increase of capacity on the motion being performed in greater space; and the diminution of temperature during the conversion of solids into fluids or gases, may be explained on the idea of the loss of vibratory motion, in consequence of the revolution of particles round their axes, at the moment when the body becomes fluid or aeriform, or from the loss of rapidity of vibration, in consequence of the motion of the particles through greater space. (26)

Prout's theory then, was a compromise between Davy's kinetic theory and the old imponderable theory of caloric. At the same time it extended Davy's idea of molecular rotations and vibrations to produce electric and magnetic polarities. Thus, in Prout's fully developed molecular theory, heat was no longer an ethereal atmosphere which surrounded an atom, but a beam of swiftly rotating particles which directly interfered with the normal velocities, and hence polarities and orientation of ponderable molecules.


On this basis, if the equatorial motions of contiguous rotating atoms became reversed by the action of these heat particles, then a gaseous self-repulsive molecule was produced. If the two atomic axes only suffered displacement through a right-angle, an equilibrium condition was produced in which contiguous atoms had neither a tendency to unite or separate. This state constituted the liquid phase. (27)

Again it seems possible that Prout could have developed these ideas from hints dropped by Davy, and in this case, Berzelius. Davy had suggested in the Elements of Chemical Philosophy that:

in solids the attractive force predominates over the repulsive; in fluids, and in elastic fluids (gases) they may be regarded as different states of equilibrium; and in ethereal substances (imponderables) the repulsive must be considered as predominant over, and destroying the attractive force. (28)


28. Davy, Collected Works, vol. 4, p. 56. After 1840 Prout could also have found support from John Davy's publication of his brother's "Dialogue on the Chemical Elements" which had not been previously included in Davy's Consolations in Travel, London, 1830. "All that is necessary for the doctrine of corpuscular philosophy is to suppose the molecules which we are not able to decompose, spherical molecules; and that by the arrangement of spherical molecules, regular solids are formed; and that the molecules have certain attractive and repulsive powers which correspond to negative and positive electricity", Collected Works, vol. 9, p. 388.
viz. Prout: solids $A > R$; liquids $A = R$;
Davy: $A > R$, $A = R$
Prout: gases $R > A$; imponderables $R > A$
Davy: $A = R$, $R > A$

where $A =$ attractive force, $R =$ repulsive force.

Berzelius had also suggested a similar analysis, although he was not as explicit as either Davy or Prout.

Si l'on voulait expliquer l'état gazeux, en disant que les atomes se tournent mutuellement leurs pôles analogues, et se repoussent ainsi de tous côtés; et l'état liquide, en supposant à leurs axes une petite inclinaison l'on serait obligé de trouver une nouvelle circonstance par l'effet de laquelle leurs axes seraient maintenus dans une position mutuelle, que leur polarité doit tendre constamment à leur faire quitter. (29)

However, the chemist was principally concerned with the interaction between different molecules. In heterogeneous polarity, unlike contiguous molecules were in rotation. Since such molecules differed in weight and volume, they differed also in angular velocity; their interactions were, therefore, slightly different from those of homogeneous contiguous molecules. If two different contiguous molecules were similarly orientated, they could not become mutually stationary and cohere, because their equatorial rotations were disparate. Instead static cohesion could only take place if their axes were in a horizontal plane, viz.

Yet this cohesive state could never be continuous since "two dissimilar molecules may unite statically (only) at those moments when the motions of the two molecules are coincident; e.g. two molecules, the one moving twice as fast as the other, may unite at every revolution of the slower molecule, which will be coincident with every second revolution of the quicker." (30) This analysis enabled Prout to suggest that the most stable chemical combinations should be formed between substances which were most simply related by weight. This hides the assumption that atomic or molecular weights are simply related, i.e. Prout's multiple weights hypothesis. For if the atomic weights of the elements are whole numbers those numbers will be (presumably) proportional to the angular velocities of their molecules. For example, hydrogen and oxygen form the stable combinations $\text{H}_2\text{O}$ and $\text{H}_2\text{O}_2$ because the velocity ratios $1:8$ and $1:16$ are reasonably simple. None of this analysis was made explicit by Prout, and it would be unfair perhaps to suggest that velocity ratios like $1:8$ and $1:16$ are not of the simplest kind; or that this explanation of stable combinations is a case of post hoc ergo propter hoc. For, at least in principle, Prout's molecular model has the makings of a good explanatory and predictive theory providing measurements of molecular angular velocity can be made independently of atomic weights.

Finally, Prout explained that if two heterogeneous molecules had their chemical axes reversed, there would be no possibility of any kind of union since they would be prevented at the equators by similar

rotational directions, and at the poles by opposing velocities or polarities.

One possibility remained, however, namely that the dissimilar molecules might be so arranged that their axes were at an angle. In this case an equilibrium might be possible, especially if the axes were at right-angles. Such a combination "is very unstable, and favourable for change." (30)

Prout did not mention the conditions which might produce such a union, but presumably the agency of heat would have been required to distort the axes.

It is clear that in Prout's theory homogeneous and heterogeneous union are very similar mechanisms. In fact reduction was possible, he concluded.

Heterogeneous or Chemical Union is only a particular case of Homogeneous Statical Union; and that Heterogeneous or Chemical Repulsion is the same, or rather results from the same cause as Homogeneous Divulsion.

From this he drew the important conclusion that single molecules could not exist by themselves, but that they could only exist in at least binary groups. This had significant consequences for Prout's interpretation of organic chemistry.
But Prout wanted to go still further towards the unification of forces. Light, electricity, gravitation and chemical affinity, he argued, were all manifestations of an attractive force; while heat, magnetism and cohesion all arose from a repulsive force. The differences between these individual phenomena were only apparent and arose simply from "the differences in magnitude and consequent different intensities of action among molecules." This is rather vague but once again, although this was never explicitly stated by him, it seems clear that a full mathematical development of his theory was dependent upon accurate values for atomic and molecular weights.

As for the experimental macro-phenomena of sensible electricity, electrolysis, and magnetism, Prout thought that these arose from the operations of various grades of molecules: electricity from the very smallest molecules, electrolysis from molecules of a higher order, and so on. The immediate cause of such phenomena was the separation of contiguous molecules so that they moved "together in the same direction." When groups of molecules moved together in opposite directions, positive and negative polarities were produced whose intensities were of two kinds.

the intensity depending on the greater or less velocity of the molecules; and the intensity depending on the greater or less separation of the poles of contiguous molecules; ... the quantity of polarity is proportional to the number of pairs of molecules moving together at the same time and in the same direction. (32)


In the manuscript version of his theory Prout was a little more explicit. Since molecules were at least binary, when a group of molecules moved in one direction, an equal number were forced to move in an opposite direction. Such movements could only take place in the liquid and gaseous states for the symmetrical molecules of solids were rigid and immoveable.

![Diagram of normal and polarised homogeneous gas]

Sensible static electricity could arise from the joint motions of any kind of molecule, ponderable or imponderable, but since the intensity of polarisation depended on the size and velocity of molecules, the sensible electricity produced by the motions of the imponderable fluids heat and light were much more intense than anything produced by the motions of say oxygen or water. Heat and light, or rather the minute swiftly rotating molecules whose interaction with other molecules was perceived as heat and light, were therefore probably the principal constituents of static electricity and magnetism respectively. Galvanic electricity, on the other hand, seemed to "depend on the intensely excited action (by ordinary electricity) of the molecules of oxygen & hydrogen, &c." (33) Thus, just as the

33. Manuscript version, p.10; see infra, Appendix 8.
interaction of the smallest particles with larger molecules produced heat, light, magnetism or electricity, these new states might in turn through further interaction with the ponderable elements produce the phenomenon of electrolysis.

This model also permitted a simple explanation of the phenomena of static electrical attraction and repulsion. When a polarised condition had been induced in a substance it underwent a threefold stress.

1. There was a natural tendency for the molecules to return to equilibrium by recombining with contiguous molecules in motion, and this was supported by the inherent electrical or chemical attractions within the molecules.

2. When groups of imponderable molecules, or highly excited ponderable molecules moved together in a certain direction, cohesion was impossible; therefore, by the mutual interference of their motions "the molecules naturally repel each other & separate by a sort of centrifugal force." (34) Similarly electrified bodies would repel each other because their motions were similar. This is not a bad explanation, but it raises the question, why are electrified bodies stable?

3. Because of the first tendency, the isolated molecules will try to separate or induce a favourable contiguous molecule. Contiguous molecules will be induced to reorientate "into an opposite or reverse state of motion, & thus give origin to opposite, or what

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34. Manuscript version, p.12.
are denominated, returning collateral currents, & these currents again to others, &c.." (34) This is electric induction.

No explanation of magnetostatics was given, but since magnetism was the sensible effect of the equatorial rotations of the previously discussed polarising motions of molecules, it inevitably accompanied the sensible phenomenon of electricity. (35) A magnetic needle arranged its poles transversely "the north pole being to the right or left according as the motion of electricity is in this or that direction."

But a magnetic needle only exhibited polarity when it was a chord of the circulating magnetic current; if it was a diameter it would revolve with it.

Although William Whewell dismissed Prout's model as an arbitrary speculation, it had one important consequence for Prout's molecular theory of chemistry. In his treatment of the change of phase, solid to liquid to gas, Prout placed the cohesive axes into positions of extreme repulsion. In this manner he was able to derive the familiar Daltonian self-repulsive gas particle; a model which agreed with the experimentally known facts of the homogeneity of the atmosphere, the phenomenon of gaseous diffusion, and the well-established gas laws. But Prout went further than Dalton by noticing that the gas laws suggested that "all gaseous bodies under the same pressure and temperature contain equal numbers of self-repulsive molecules." This is Avogadro's hypothesis.

Although Prout did not deduce the equal numbers of molecules hypothesis from his polarity model, this model was nevertheless an effective explanation and justification for it since it explained the experimentally known gas laws. For:

every molecules of matter, when it is in the gaseous state, and subjected to similar pressure and temperature, may, without reference to its other properties, be supposed to be in circumstances exactly similar and consequently liable to be affected in an exactly similar manner by all further increments of heat. (36)

It is intriguing to notice that Avogadro had not deduced his hypothesis in this way from the identical behaviour of all gases under changes of temperature and pressure, but purely from a consideration


of Gay Lussac's law of combining volumes. (39) There seems to be no particular reason to doubt Prout's statement that he arrived at the molecular law "long before he was aware of the essays on the subject by Messrs. Avogadro, Ampère [sic], and Dumas." (40) He does not appear to have studied the English translation of Ampère's molecular theory which appeared in the Philosophical Magazine in 1815, (41) and he stated that he learned of Ampère's suggestion only through Johnston's review of the state of chemistry in 1832. (42) But since Johnston's review did not mention Ampère, but Dumas's Traité de Chimie applique aux Arts, it appears that Prout must have studied this book (where Ampère is mentioned) after reading Johnston. Prout also cited Michael Donovan's rejection of the hypothesis as the source of his knowledge concerning Avogadro. (43) Gaudin's theory was not noticed by him at all. (44)

Later, in the section of his Chemistry devoted to the laws of chemical combination, Prout argued that the equal numbers of molecules hypothesis also followed logically from Gay Lussac's law of

42. Brit. Ass. Reports, 1, 420-1, 1831-2(1832).
combining volumes which "established that bodies, in their gaseous state, combine both chemically and cohesively with reference to their volumes." (45) The polarity hypothesis implied that homogeneous cohesion could only take place between a binary system of molecules at the very least; and so like Avogadro, Prout noted that in the formation of steam from hydrogen and oxygen, "every self-repulsive molecule has been divided into two, and consequently must have originally consisted of at least two elementary molecules, somehow or other associated so as to have formed only one self-repulsive molecule." (46) It is ironic that Prout, like Avogadro, avoided the term atom, and used the same terminology as him, viz. elementary molecule instead of atom. (47) Other common gases, like chlorine and nitrogen, were also binary molecules "united to each other cohesively and acting as a single one." (48)

The conclusion that the molecules of elementary bodies in the gaseous state were binary was, of course, very important and significant. For on this assumption Prout overcame the traditional electrochemical objection to Avogadro's hypothesis that similar parts would repel one another, and therefore never cohere. However, the model

46. Ibid., p. 123.
48. Chemistry, 1st ed., 1834, pp. 124-5. He also allowed for the possibility that similar molecules might combine by their chemical axes. This was most likely to occur, he thought, in the crystals of elements. On the other hand, dissimilar molecules might occasionally combine cohesively.
which justified this assumption was too much for a Daltonian to follow, as Prout's opponent William Charles Henry was quick to point out. For it seemed to a Daltonian that caloric brought about a change of state not on the individual atom (as in Dalton's original theory) but on a bi-molecule, or arbitrary group of atoms. As an orthodox Daltonian, Henry believed that each atom attracted caloric to itself and in this way exerted a self-repulsive force. Quoting Laplace in support, Henry argued that he could not see how such interatomic forces could be resolved with the formation of stable, yet self-repulsive groups of atoms. According to orthodox theory, Prout's molecules would simply disintegrate!

The contrary hypothesis of Dr. Prout involves the anomaly of supposing heat to have a combining affinity for two or more atoms, while it is destitute of such affinity for single atoms; and also that of supposing two atoms to have relations towards two atoms, or three towards three, which do not obtain between single atoms. (49)

This was a powerful argument against Prout, and it could only be avoided by the adoption of a different heat model, such as the one used by Prout in the later version of his theory. Avogadro's hypothesis was first generally adopted by physicists who derived it from a mechanical, kinetic theory of heat.

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49. Phil. Mag., (3)5, 37-8, 1834. Coley, loc. cit., p.141 has pointed out that this criticism held no difficulties for Avogadro who believed that each atom contributed to the specific heat of a molecule. In his Fisico de Corpi Ponderabili, Turin, 1837-41, vol. 4, Avogadro recognised that Henry's remarks, though perhaps justified against Prout's particular version of the molecular theory, were not against the molecular hypothesis itself.
It was shown in Chapter six that Prout's Bridgewater Treatise was favourably received. (50) There was some comment that he appeared to argue for design on the dubious basis of his hypothetical molecular system, or that the book would have been better had its author not been tempted to publish these speculations by the vivifying influence of the Bridgewater legacy.(51) The only serious critical discussion of the book's chemical innovations was made by Dalton's biographer. (52) This is, therefore, of some interest.

Henry singled out for attack the two propositions (1) that equal volumes of gases under the same conditions contained the same numbers of molecules, and (2) that the gaseous self-repulsive molecules did not represent the ultimate atom, but were composed from many of them. Henry's first criticism was that these conceptions or opinions were not original ones. "(They) occurred about the same time to


52. The only other contemporary scientific references to the theory which I have found are: (1) the previously mentioned citation by Whewell (ref.36); (2) an oblique citation by Brander who refers the reader who would know more about the obscurer parts of molecular philosophy to Prout's treatise, W.T. Brander, A Manual of Chemistry, 6th ed., 2 vols., 1848, vol.1, p.143; (3) E. Turner, Elements of Chemistry (ed. Liebig and Gregory), 7th ed., London, 1842, p.170, mentions briefly that Prout held similar ideas to Ampère; (5) C. Daubeney, An Introduction to the Atomic Theory, Oxford, 1850.

With the exception of Whewell, all these references were to Prout's Avogadro-type molecular theory, and not to the polarity hypothesis.
MM Ampère and Avogadro. It was published by the former, so early as the year 1614. (53) It was subsequently revived by Dumas, and has recently been maintained and illustrated by his pupil M. Gaudin. (44) Dr. Prout has arrived at the same conception without being aware that it had been previously entertained by others. (54)

In reply Prout repudiated the implication that he had been beaten to the post by other chemists, by claiming that his 1815 anonymous paper had been founded on this molecular hypothesis, "understood but not expressed", and that "from that time to the present I have seen no reason to doubt its truth." (55) Only a mathematical demonstration that it was incorrect would convince Prout that he was wrong, and he reiterated his Guldstonian lecture statement that there were several details of Daltonian atomism that he found impossible to accept. Indeed, he had always thought Dalton's work on atoms much less satisfactory and complete than his work on gases.

The atomic theory of Dalton by connecting chemistry with quantity was undoubtedly the greatest step that has been made in modern times; but ... My notion of the atomic theory is, and always has been, that it does not represent a just view of the laws which regulate the union of natural bodies, and consequently that it is inapplicable both to organic and inorganic chemistry. The light in which I have been always accustomed to consider it has been very analogous to that in which I believe most botanists now consider the Linnaean system; namely, as a conventional artifice, exceedingly convenient for many purposes, but which does not represent nature. On the continent, the modification of Dalton's views, proposed by Berzelius, is generally adopted, but this I fear, is still more imperfect than our own. (56)

53. From this remark it seems Henry had only heard of Avogadro from Ampère's brief reference to him at the end of his first paper, Ann.chim., 90, 45, 1814.

54. Phil.Mag., (3)5, 33, 1834.

55. Ibid., p.132. "A mass of curious evidence exists in favour of the hypothesis, (which evidence, if no one else does, I may be induced at some time to arrange and publish)...."

56. Medical Gazette, 8, 262, 1831.
Daubeney had thought when he read this in 1831 that it read like a censure motion against Dalton, but Prout had hastened to inform him that he had a great respect for Dalton. (57) He subscribed to Dalton's postulates, as far as they went, he said, but he was not satisfied that they went far enough. In other words it was Dalton's naive atomism that Prout was against, and he would have replaced it by his own more complex molecular theory which was based on volume relations, and in particular on the Avogadro law. In this respect, Berzelius's volume theory was as bad as Dalton's, for it too did not go far enough. Nevertheless, at an elementary level, or first approximation, Dalton's system would probably continue to be used. The danger here was, however, that atomists would continue to fall "to the temptation of adopting the results of their experiments to the standard set forth by the theory of definite proportions"; that is, by arbitrarily restricting the types of combination to prevent the division of the "atom" in a chemical reaction. This had been the failing of the three giants of atomism, Dalton, Thomson and Berzelius.

Of course, it was precisely because Prout believed the equal volumes - equal numbers of molecules hypothesis that Henry felt morally obliged to attack him and argue that, although proposition (2) followed logically from the first proposition, the latter was an incorrect assumption. The burden of proof therefore lay in Henry's ability to demolish Avogadro's hypothesis. Although Henry's objections possessed only negative force, they are worth considering.

He claimed first that the Boyle-Mariotte law did not warrant the equal volumes-equal number of molecules hypothesis, as Prout had claimed in the Chemistry, for Boyle's law had been previously derived by Newton "from the law of variation observed by the repulsive forces which actuate the molecules of elastic fluids, not from the numerical aggregation of atoms in space." (58) To understand this argument it must be recalled that Dalton had placed much emphasis on Newton's (misunderstood) derivation of Boyle's law in the Principia. (59) Henry's point was that since Newton had derived Boyle's law from a gas model which was independent of the number of molecules, Prout could not derive any information about the numbers of molecules in a unit volume from Boyle's law. In any case, he continued quite correctly, neither Boyle's nor Charles's law, nor the law of heat capacities was completely accurate. Prout's molecular theory was, therefore, first not strictly derived from the facts, and second, it led to consequences that were inconsistent with basic (Daltonian) atomism.

We are compelled, by the rules of philosophising, to recur to the simple and beautiful conception of the indivisibility of the atom, taught by the illustrious author of the atomic system. To Henry then, it seemed much more likely that the mutually repulsive "molecules" of gases were the ultimate atoms.

Unfortunately, Prout declined to reply to Henry's critique at any length or depth. (60) His reputation in England and on the

58. Phil. Mag., (3)5, 34, 1834.
60. Phil. Mag., (3)5, 132-3, 1834.
Continent as a chemist and physician was considerable, and a technical defence of his ideas in an important journal like the Philosophical Magazine would, without a doubt, have been very influential.

To all intents and purposes then, Prout reached the same conclusions about the relations between combining weights and volumes as Avogadro before him; however, like Ampère to some extent, Prout did not build into his molecular hypothesis any rule of simplicity. We have seen already that Prout described gaseous molecules as composed of at least two parts. Avogadro had said as much: of several consistent molecular formulae, we should choose the simplest one. e.g.

\[ 2 \text{H}_2 + \text{O}_2 \rightarrow 2 \text{H}_2\text{O} \]  the simplest

\[ 2 \text{H}_3 + \text{O}_2 \rightarrow 2 \text{H}_3\text{O} \]

\[ 2 \text{H}_4 + \text{O}_4 \rightarrow 2 \text{H}_4\text{O}_2 \]

But Prout believed otherwise. The polarity theory, and possibly the influence of Davy, who had written out proportional weights as multiples of fixed lower units, (61) led him inexorably to complex molecules composed of a uniform matter.

The self-repulsive molecules of oxygen and hydrogen are at least double; but the probability is that they are in reality much more compounded, as the following observations will show. The self-repulsive molecule of water, on entering into combination, is often found to be divided into two, or three (perhaps more) parts. Now as we cannot admit the division of the ultimate molecule, or atom; we must of course conclude, that the molecules of oxygen and of hydrogen, are much more compounded ... and must each of them contain at least three components, or submolecules. Hence the self-repulsive molecules (viz. three of oxygen, and six of hydrogen) which we may suppose to be associated, in the first place, the hydrogen with the oxygen chemically; and afterwards the three submolecules of water with one another

61. E.g., Phil.Trans., 1811, p.18.
cohesively, so as to constitute one spherical molecule. (62)

At the back of Prout's mind here, and following logically from a unitary theory of matter, was the series theory of proportional weights which he had briefly mentioned in the Gulstonian lectures in 1831. There, in his polemic against Dalton, he had argued that the numbers conventionally labelled atomic or equivalent weights:

appear to me often nothing more than one term of a natural series peculiar to each body, and determining its combination. Thus 9, the number assumed to represent the combining weight of water, is to be considered only as one term of the series 3; 6; 9; 12; 15; &c., in all which proportions (and perhaps in still lower submultiples of them) this fluid enters into combination, perhaps as often in the proportion 9, especially in the organic kingdom. Chemists have already a glimpse of this important fact when they speak of bodies uniting to others in the proportion of two, three or more atoms, which, in fact, are nothing more than the terms of a natural series such as that alluded to. (63)

In other words, since the equivalent weight of water is 9, it only enters into reactions or combinations, in sub and super multiples of three equivalents. Stability is given to the molecule by the homogeneous forces. By implication elements also have a natural series which determines their combining powers. The water series, 3; 6; 9; 12, was followed in combinations with carbon (e.g. in the saccharines), but other series were possible when water combined with substances which had a different combining series.


$$2H_2 + O_2 \rightarrow 2H_2O \rightarrow 2\left[3(H_2O)\right]$$

63. Medical Gazette, 8,263,1831; Chemistry, 1st ed., 1834, p. 139.
Thus in a natural group or family as the saccharine group ... by adhering to a single number, as 9, for water, we should be led to fractions of atoms without end, but by considering the carbon as associated with different proportions of water, in terms of the above series (as experiment indicates to be the case) all these absurdities are avoided, and at the same time the existence of a beautiful law is indicated. (63)

Daubeny was inclined to think that this was a needless complication of the law of definite proportions; but since Prout's presupposition of homogeneous matter was different from Dalton's position, this was not the whole story. (64)

In the Chemistry Prout insisted that his molecular hypothesis was the simplest and most consistent explanation of a wide range of phenomena. It was more than worthy of serious consideration since it had the additional merits of suggesting several problems for further investigation.

1. Did submolecules have the same chemical properties as molecules? For example, did the submolecules which united cohesively to form an oxygen molecule possess the same properties as the molecules itself? Prout was inclined to think that any differences would be of degree and not of kind; that is specific rather than generic differences, since the only effect of cohesion, he thought, might be to produce a

64. C. Daubeney, Introduction to the Atomic Theory, 1831, p. 44. In his letter to Daubeny (ibid., p. 130) Prout wrote: "in uniting with bodies having different combining series, the aqueous series itself may become modified or different -and hereby hangs, if I am not mistaken, a very curious tale". Needless to say, Prout never revealed this tale to the world, except to tease Daubeny still further by suggesting that the forms of plants might depend on the oxygen series, 2; 4; 6; 8, and an identical series of carbon and water based on 3.
slight modification of the chemical polarities. There was, fortunately, a clear analogy in the behaviour of the homologous paraffin series where:

the sensible properties of all these compounds, though resembling each other in some respects, are yet specifically different; and as they are all composed of the same gaseous body in different proportions, these differences must be considered rather as the result of cohesive than of chemical union. (65)

2. The latter conclusion favoured a viewpoint from which the molecules of all elements were polymers of simpler species. It is odd, however, to find that the Proutian unitary hypothesis was expounded by its author with great caution and diffidence in the Bridgewater Treatise. Perhaps the explanation for this is that it is a reflection of Turner's reinvestigation of atomic weights; yet the unity of matter is no more stressed in the third edition of the Chemistry published in 1845 in spite of the new experimental evidence in its favour. Despite this caution it is nevertheless clear that Prout still believed in the unity of matter.

Although we have rendered it exceedingly probable that the molecules of bodies considered at present elementary, are immediately compounded of many others more or less resembling them; yet it is obvious that there must be a point at which these and other elements exist in a primary and ultimate form, and beyond which, if they can be supposed to be subdivided, they must be something altogether different. (66)

Like Davy in his Elements of Chemical Philosophy therefore, Prout seems to have accepted the reality of more than one type of ultimate submolecule which in character resembled the imponderables heat and

light, not only by their extreme tenuity, but in other characters also; and this very intensity of property and character may be reasonably considered as one, if not the principal reason, why they are incapable of existing in a detached form.

In some ways this may seem as a remarkable forecast of the properties of atomic particles and excited atoms, but it follows clearly enough from the polarity theory. Moreover, these excited and swiftly-rotating sub-molecules, thought Prout, were probably the means by which metabolic processes were performed in living systems. It also followed from the theory that the synthesis of the inorganic elements was at least a possibility; but if it took place at all, Prout conjectured, the most likely occasion was during metabolism. Prout's appeal to the possibility that calcium was synthesised by a growing chick within the incubating egg is at once recalled to mind. (67)

3. If the number of ultimate elements was but a few, the extreme variety of observed chemical species could be considered to arise by simple aggregation and chemical combination by volume. The complex rotatory motions of these particles would give rise to chemical and cohesive forces.

For no sooner is a new compound molecule formed by an assemblage of similar molecules, then it may be supposed to be capable of combining with other molecules chemically, and of thus entering into a long and novel series of combinations; while these combinations again in their turn may be imagined to lead to others, and so on, till the variety becomes extreme. (68)

In basic conception this is like the old seventeenth-century corpuscular philosophy of Boyle and Newton in which a basic particulate matter


is hierarchically arranged in corpuscular aggregates of ever increasing complexity. The weights of the resultant molecular complex were, as Prout realised, bound to be related to one another by multiples. He therefore wished to retain some form of the integral multiple weights hypothesis. Hierarchical complexes also explained the similarities between elements which fell into natural groups of "families"; their possession of similar properties was a direct result of common structures - a concept which modern chemistry has completely vindicated. (69)

4. The single cohesive force which Prout had adopted to explain crystallisation could also be used to account for isomorphism; since "when the molecules of different bodies are of the same size (or rather of the same weight), they may be naturally supposed capable of associating themselves into the same form; and if they happen to be mixed together, they may even enter indiscriminately into the same crystal." (70) This is a correct explanation for isomorphism. In his Gulstonian lectures Prout had recorded the opinion that the principles of Dalton and Berzelius were totally incapable of explaining the phenomena of isomorphism and isomerism; and in his letter to Daubeny of the same year (1831), Prout claimed to have hit upon their true explanation in terms of weight and size relationships in 1815.

So long ago as 1815, I was led to infer that relation in weight might indicate a relation also in size among the atoms of


70. ibid., p.131; 3rd ed., 1845, p.139; a near coincidence of forms produced plesiomorphism.
bodies; and that many of those striking and curious analogies in property, form, &c. which I thought I observed among bodies atomically related, might depend upon one or other of these circumstances. (71)

I think that Prout was being completely honest here, for he did make brief remarks in the 1815 paper on the relationship between atomic weight and chemical properties. "Substances in general of the same weight appear to combine readily, and somewhat resemble one another in their nature." (72) According to the polarity hypothesis, different elements were of different sizes and polar intensities; weight therefore was correlated with size. This suggests that Prout may have derived the polarity theory and molecular law of Avogadro during the period 1815 to 1818.

Prout's explanation of isomorphism suggested to him that when molecules in a solution did not possess the necessary shapes and angular velocities for satisfactory cohesion, they probably made up the required shape, or acquired the right velocity, by attaching themselves to other kinds of molecules. This completion was the role of water of crystallisation which, for example, enabled molecules to readily combine together and form symmetrical crystals. But other molecules could also act as completing agents, and Prout thought that this was an explanation for many types of isomerism, which he called

71. Daubeny, *Introduction to Atomic Theory*, 1831, p.131-2; see anon, *Dublin Journal of Medical & Chemical Science*, 1,67,1832, for an attack on Prout's "ostentatious modesty" in claiming by implication to have discovered both isomorphism and isomerism before Mitscherlich and Berzelius.

merorganization. In his biochemistry Prout developed this idea to describe the completion of organic molecules by minute quantities of interstitial "impurities". In particular, Prout suggested, much of the great variety of substances in organic chemistry was due to the presence of minute quantities of strongly self-repulsive foreign molecules equally dispersed among the more ordinary molecules which strongly modified their molecular arrangements and their observed chemical and physiological behaviour. (73)

Prout's final elaboration of his discussion of weight was to demonstrate that molecular weights were proportional to the specific gravities (vapour densities) of the molecules. (74) In this way the molecular law which had been implicit in his 1815 paper was finally and clearly presented. But, he noted unhappily, because one of the elements of water was used as a standard for atomic weights, a considerable confusion had arisen in chemistry because of the actual differences between volume and combining weights.

As a mere matter of convenience it is certainly preferable to consider the two volumes of hydrogen as one atom, (to use the language of Mr. Dalton), in which case oxygen will be 8, and water, 9; but a strictly philosophical arrangement ... would require that the volume in all instances should be made the molecular unit; in which case the relative weights of the self-repulsive molecules of hydrogen and oxygen will be as 1 to 16. (75)

73. Chemistry, 1st ed., 1834, pp. 425-6. See supra, Chapter 4, p. 178; Chapter 5, p. 199; Chapter 6, p. 236.


It is rather unfortunate that Prout was prepared to compromise in this way, for although he correctly perceived that a 2-volume system was a better scientific basis for atomic and molecular weights, like Gerhardt later, he bowed to convention and adopted 1-volume "equivalents" for the remainder of his chemical treatise. Of course a book of Christian apologetics was hardly the place to have introduced a new system of atomic weights. But to some extent this compromise was forced on Prout by his own molecular theory; for given his hypothesis concerning the series of combining weights, and his commitment to the unity of matter and its polymerisation, any atomic weight system was necessarily arbitrary.

These numbers certainly do not represent nature; for as we have already stated, a strictly philosophical arrangement can be rationally founded only upon the volumes of bodies in the gaseous state, in which case some common volume in all instances should be considered as the molecular unity. Now, as in most instances, this molecular unity seems capable of sub-division, of course the number made to represent it can hardly ever be supposed to be a prime number. Hence, as combining molecules of bodies exist both below and above the molecular unity, they may often (perhaps always) be represented by a series. (76)

It was in this way that Prout failed to take the lead in the introduction of atomic weight values. This lead would have been a step of major importance, not only for the development of nineteenth-century chemistry (for it was a step hardly agreed upon by chemists before 1860), but for Prout's own interpretation of the organic chemistry of living systems. Although Prout's views on atoms and molecules were discussed favourably by Charles Daubeny in the second edition of

his Introduction to the Atomic Theory, this did not appear until the year of Prout's death, 1650, by which time Avogadro's law had already begun to receive its reappraisal from Dumas's pupils Laurent and Gerhardt, and was to be supported in England by Alexander Williamson after his return from France in the same year.

The molecular theory and biochemistry

In previous chapters we have seen how, between the years 1615 and 1623, Prout devoted much of his attention to the biochemistry of digestion. The publication of a book on this subject was interrupted by, among other things, the discovery of hydrochloric acid in the gastric juice of animals in the summer of 1623. From Prout's remarks it seems evident that this discovery opened up an entirely new world of research for him, and made him intensify his attempts to apply the molecular theory which he had developed to the physiological problems of digestion and metabolism. In this pioneering field, Prout attempted unsuccessfully without the aid of chemical formulae what Liebig and his pupils began to accomplish with success during the last decade of Prout's life.

It seemed to Prout that his analyses had revealed the existence of arithmetical relations between the various members of the three alimentary bodies which he had described in 1627, the saccharinous, albuminous and oleaginous bodies. These relationships were

77. It appears, however, that they were discussed in his Supplement to the Introduction to the Atomic Theory, London, 1840, 62 pp. This pamphlet was not available to me.

78. Phil.Trans., 1827, p. 357.
especially clear among members of the saccharine family, \(^{(79)}\) or vegetable aliments, which were all composed from carbon and the elements of water, and which all formed oxalic acid or its analogues with nitric acid. Some of these materials like sugar, vinegar, and lactic acid existed in crystalline forms; others like starch, lignin and the gums were uncrystallisable and, in Prout's language, more organized. He firmly rejected Liebig's belief that the saccharine aliments were used solely to produce animal heat, \(^{(80)}\) an opinion with which Liebig was later forced to agree. Prout believed that saccharinous substances were transformed into both fats and albuminous substances, but this raised the problem of the source of extra nitrogen. Albuminous substances or "animal aliments" were never found in crystalline forms except when drastically modified, e.g. uric acid. All albuminous substances contained nitrogen but Prout never went beyond this to venture an opinion concerning their chemical constitution except to dismiss Mülder's proteine hypothesis for its crudity. \(^{(81)}\)

He did not deny that a common molecule could be recovered from albumen, fibrin, etc., but "that a substance obtained like proteine by the rude and disorganizing processes of common chemistry, should be that common proximate element; or that such a substance should ever be employed at all in vital operations without undergoing the preliminary assimilating process, is more than at present we are disposed to admit." All parts of an animal's body contained albumen and gelatine which


could be easily separated by boiling with water. Gelatine was the least organized kind of albumen and in many ways the analogue of the saccharine principles in plants for it could be broken down into sugar just as starch could. (32)

Although Prout stated that he had made many analyses of oleaginous substances (83) no such analyses were ever published by him. He believed, however, that they were binary compounds of ethane (olefiant gas) and water. Oils were usually divisible into two portions, a stearine, and an oleine.

Most organized substances contained at least three primary elements; but, Prout insisted from his polarity theory, they must be dualistically arranged. Thus in the sugar series, the binary molecular aggregates were carbon and water, and in the oleaginous family, ethane and water. The spherically-arranged forces of homogeneity were extremely strong in organized substances, and this was an addition effect due to their intensity in the primary elements carbon, oxygen and nitrogen. These intensities explained the peculiar ability of carbon to form very large supermolecules of "atomic" weight 12, 18, 24, etc.

What then was the biochemical significance of the arithmetical relationships revealed by the analysis of members of the alimentary families? Sugar cane was composed of 9 atoms of carbon and 8 atoms of water, each associated by cohesive forces into two supermolecules

82. Gelatine lacks several amino acids that are characteristic of proteins.

weighing \((9 \times 6) = 54\) and \((8 \times 9) = 72\) respectively. (84)

<table>
<thead>
<tr>
<th></th>
<th>(C)</th>
<th>Water</th>
</tr>
</thead>
<tbody>
<tr>
<td>Lignin</td>
<td>54</td>
<td>54</td>
</tr>
<tr>
<td>Cane sugar, wheat starch</td>
<td>54</td>
<td>72</td>
</tr>
<tr>
<td>Sugar of honey, arrowroot</td>
<td>54</td>
<td>108</td>
</tr>
<tr>
<td>Absolute vinegar</td>
<td>24</td>
<td>27</td>
</tr>
<tr>
<td>Crystalline vinegar</td>
<td>24</td>
<td>36</td>
</tr>
</tbody>
</table>

The above analytical table which was published by Prout in 1834 suggested to him that the carbon proportion (or supermolecule) remained constant in saccharines while the proportions of water varied. Vinegar belonged to a different carbon series, and therefore exhibited different properties from the sugars.

Since water was the only variable within a given carbon saccharine series, it could be called appropriately the modifying supermolecule of the sugars. But the chemist was neither able to convert cane sugar into honey sugar by adding the requisite amount of water, nor able to perform the reverse procedure without decomposing the honey sugar. Therefore, Prout concluded, water must be present in a state of "essential union" in these sugars.

He thought that similar relations could be established in the fatty or oleaginous family where the basic unit was olefiant gas \((\text{CH}_2)\),

84. Chemistry, 1st ed., 1834, p. 482. Note that Prout used the "unphilosophical" weights, \(C = 6, O = 8\). In the 3rd ed., 1845, p. 382, he altered the supermolecules to \((24 \times 6) = 144\) carbon, and \((28 \times 9) = 252\) water.
and in gelatinous or albuminous substances. But it was a great weakness of Prout's biochemistry that he should have been so vague concerning the detailed chemistry of these families; in fact he never produced any examples of their arithmetical relationships.

After he had established the existence of the series relationships among saccharinous substances Prout introduced the terminology of the sugar refiner: all strong or high organic substances had constituent supermolecules of a less-complex kind than those of weak or low substances.

in the strong, fixed and solid oils or fats, ... the modifying molecule of water is very small, perhaps in some oleaginous bodies, is even a submolecule. Whereas, in alcohol, which is the weakest condition of the oily principle, the weight of the modifying supermolecule of water is more than half that of the olefiant gas, and alcohol is perfectly soluble in water. (85)

But weak substances possessed less intense homogeneous forces than were found in strong substances in which these forces had become saturated. (86)

The conversion of a strong compound into a weak compound was to be called reduction (i.e. the addition of water); and the reverse process whereby water was removed, completion. Two generalisations followed immediately, namely that the greater the molecular weight, the more easily was a substance decomposed, for the homogeneity was low; and the greater the amount of water contained (i.e. the greater the weakness), the greater was the solubility. (87)

87. Chemistry, 1st ed., 1834, p.486; this correlates with the number of -OH groups.
weak substances $\xrightarrow{\text{COMPLETION}}$ strong substances

(complex, high molecular weight, quite soluble, potentially unstable)

(less complex, low molecular weights, quite stable)

On first acquaintance it might be thought that Prout misapplied the two terms completion and reduction, for they are usually taken to imply, on the one hand bringing or raising to entirety or fulfilment, and on the other hand lowering to the simplest. However, Prout's usage is immediately clarified by its application to the biochemistry of digestion.

In the stomach substances were reduced to their weakest forms by the combined action of hydrochloric acid and other watery secretions. This was a purely chemical process, but as was remarked in chapter four, increasingly Prout came to look upon hydrochloric acid as a pathological disturbance and by-product of chlorine, the real "powerful influence" that brought about reduction. The chlorine or hydrochloric acid was derived from sodium chloride in the blood which also produced the alkalinity of the bile secretions. The mechanism by which this separation was effected was by the electrolysis of blood.

We have in the principal digestive organs, a kind of galvanic apparatus, of which the mucous membrane of the stomach, and perhaps that of the intestinal canal generally, may be considered as the acid or positive pole; while the hepatic system may, on the same view, be considered as the alkaline or negative pole. (88)

Reduction was the principal chemical feature of digestion, or primary assimilation, as Prout preferred to call it, in contrast to secondary assimilation which included both the processes of tissue

formation from the blood (formative process) and the destruction and removal of unwanted parts from the animal system (destructive process).

Secondary assimilation was, therefore, equivalent to what Liebig called the "metamorphosis of tissues", and in Prout's view necessitated the existence of disorganizing agents. The blood itself was formed from chyle and lymph produced during primary and secondary processes of assimilation respectively.

To some extent reduction was performed artificially by human beings by cookery, but "unfortunately, cooks are seldom chemists ... hence their labour is most frequently employed, not in rendering wholesome articles of food more digestible ... but in making unwholesome things palatable." (89) Since the reducing function of the stomach was easily deranged it was of the greatest importance for the dietician to understand this function. For if the function was weak it would be foolish to give a patient heavy, meaty solids instead of pulpy foods; if the function was intense, as in diabetes, then solid, hard, but nutritious, foods were called for. (90) These nutritional points, as well as the statement already quoted in Chapter four that milk was the great alimentary prototype on which all diets and cookery should be modelled, were repeated by Liebig, who somehow gained the credit for them.

Two other functions of the stomach were to convert (we should say synthesise) one class of aliments into another when necessary in order to produce a chyle of uniform content (conversion), and the

90. Ibid., p. 494.
organization and vitalization of the food materials. Whereas conversion was a purely chemical process, Prout held steadfast to the belief that the other process was vital. (91) But his belief in chemical conversion meant that several years before Liebig and Dumas he had suggested that sugars could be converted into fats, and even into albumenes; and fats into albumenes, and vice versa. (92) The question of the possible conversion of carbohydrates into fats became a very controversial issue during the 1830s; it was supported by Liebig's German school, but rejected by the French school of animal chemists. The matter was eventually settled in favour of Prout and Liebig during the early 1840s. Not until 1843 was sugar detected in the blood, and Bernard's discovery of glycogen was not made until 1856.

As far as the ability of the animal body to convert one kind of foodstuff into another then, Prout's molecular and unitarian theory of matter led him to a correct assessment. However, it also led him to expect that under certain unspecified conditions the synthesis of absent elements (like nitrogen) might take place. (93) This was an assumption that Liebig, and to a lesser extent Dumas, rejected. Usually, Prout thought, the nitrogen came from a nitrogeneous source that was already present in the blood and which was secreted into the

91. Cf. Brit.& Foreign Medical Review, 11,332,1841, where the reviewer asked why the organizing power of the stomach could not also be considered chemical.


duodenum during primary assimilation. The non-nitrogenous part of this secretion was then extracted from the blood by the liver or stomach as lactic acid. Prout offered no chemical evidence whatsoever in support of this conjectural mechanism.

In the duodenum the chyme from the stomach was mixed with bile and pancreatic juices, and its acidity neutralised by the reformation of sodium and potassium chlorides. Although Prout had observed that the bile induced a precipitation of the fluid contents of the duodenum, Prout along with Dumas and Liebig saw no reason to believe that fats were in any way chemically altered in the small intestine. Not until 1849 was it shown by Bernard that pancreatic juice was indispensable for fat absorption. Prout thought that the excremental portion of the food was separated out in the duodenum, and the remainder of the chyle absorbed by the lacteal system.

Once the chyle entered the lacteals, the opposite process of completion began whereby the strong, and now transformed and vitalised aliments were passed into the general blood stream, while the excess water was passed to the lungs for release during the respiratory process. Prout offered no evidence that water was transpired from the lungs in this way; in fact this suggestion, which had been made originally by Lavoisier and Fourcroy, had been disproved by Allen and Pepys in 1808. But Prout did not follow Lavoisier's

96. Phil.Trans., 1808, pp. 249-81.
other suggestion that the carbon dioxide exhaled from the lungs was a product of the oxidation of carbon in the lungs. Instead, he ascribed the carbon dioxide to the reduction of albumen in the tissues to form the gelatine that was found in connective tissues like the skin. (97) In order to explain the presence of albuminous substances in the "absorbents"—viz., the tissues served by the blood stream, Prout had to postulate that gelatine was reconverted back into albumen. (98) He failed to mention that carbon would be necessary for this reverse process to occur; and Frederick Holmes has argued that Prout failed to understand that since the same amount of carbon would be needed for albumification as for gelatification, there would be none left to be exhaled. However, Prout does seem to have been aware of this defect since later he qualified that only certain (unspecified) forms of albuminous substances could form gelatine, and that there was only one source of carbon dioxide production. (99) He was also uncertain whether carbon dioxide production was correlated with the production of animal heat, and unlike Liebig, he did not argue that heat was the central function of animal metabolism.

Clearly the greater part of Prout's metabolic system was more ingenious than experimentally clear and detailed. In any case it must be remembered that he did not believe that the processes of reduction

97. Chemistry, 1st ed., 1834, p. 524; 3rd ed., 1845, p. 536, where he added that gelatine contained 3 to 4% less carbon than other albuminous substances.


and completion were simply chemical; vitalizing, or organic, agents were supposed to be distributed in both the stomach and lacteals. (Chapter 6)

The general vagueness, or qualitativeness, of Prout's account of primary assimilation is also found in his treatment of secondary assimilation, even though this was given in more detail in his later clinical treatise On Stomach and Urinary Diseases in 1640. According to Prout, as has already been stated, secondary assimilation was both formative and destructive. These two processes were conducted by the hydrolysis of active molecules into either a new and important metabolic principle, or (more commonly) into two principles, one of which was excremental. When two products were formed the molecules were complementary. Sometimes, especially in derangements, three excrementious products might be formed. For example:

- albumen + water → gelatine + hydrated carbon dioxide (normal process)
- gelatine + water → ammonium carbonate (abnormal process)
- gelatine + water → saccharine + urea (normal process)

Gelatification or gelatinification was a formative process by which the albuminous substances within the blood stream became the solid gelatinous tissues that formed the foundation for all other animal tissues. (100) It took place in the capillary blood vessels and

100. On Stomach, 5th ed., 1848, p. 484.
was the direct cause of the transformation of arterial into
venous blood. Albumification or albuminification was the other
formative process whereby water was eliminated and albuminous sub-
stances within the blood were converted into solid albuminous tissues.
The process included the **fibrification** of fluid fibrin into the solid
muscular fibrine. In contradiction to Liebig, Prout supposed that
both these processes were sources of animal heat, **(101)** but in neither
case did he present any experimental evidence to support his account
of these fundamental changes. Nor did he have anything to say by way
of explaining other formative processes like the synthesis of bone
and hair, or the production of internal secretions like saliva and
semen.

In destructive secondary assimilation, tissues were **unmade** and
converted either into new useful materials, or into simple and
usually crystallisable excretion products. These processes were very
delicate and easily went wrong, leading to pathological conditions.
For example, gelatinous tissues were normally resolved into the
complementary principles of urea and lactic acid which were then ex-
creted through the kidneys or the skin. **(102)** Sometimes, however,
other abnormal decomposition products were formed, or the normal

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101. Liebig divided foodstuffs into (1) plastic, or tissue-forming,
(2) respiratory. Albuminous substances were plastic; hence he
concluded incorrectly that nitrogenous substances were not
heat producing. See *infra*, p. 441.

102. Liebig had said that lactic acid was never found in healthy
urine, but Prout denied this.
products were produced in excessive amounts; this was then
indicative of disease. The products of the destruction of albuminous
tissues were rather problematic. Uric acid was certainly one product,
though it was most usually to be found excreted in the form of ammon-
ium urate. No evidence was given by Prout for the production of urea
and uric acid from the degradation of gelatinous and albuminous
tissues. On this theory, one would expect muscular exercise to destroy
albuminous tissues faster than gelatinous, and that urine would con-
tain more uric acid than urea. But among mammals, the contrary is
found.

As far as pathology was concerned, it can be seen that Prout's
nosology included the following possibilities:

A. Primary mal-assimilation

(usually not serious)  (1) stomach  (a) malfunction of the
         reducing power (most
         important)

(b) malfunction of the
         converting power

(c) malfunction of the
         vitalizing power

(2) duodenum  - the additive effects
         of malfunction (1)

(3) lacteals  (a) the effects of (1)
             and (2)

(b) malfunction of
             completion

(c) malfunction of the
     vitalizing power
B. Secondary mal-assimilation

(usually more serious) (4) formative (a) malfunction of
    gelatification
    (b) malfunction of
    albumification
    (c) malfunction of
    other processes,
    especially lymph
    formation

(5) destructive (a) gelatinous tissues
    (b) albuminous tissues
    (c) other destructive
    processes

In the final edition of his pathology textbook on digestive and
urinary diseases, Prout gave a remarkable summary of his beliefs
concerning the relationship between alimentary families, their meta-
bolism, and their connection with the liver and kidneys. (103) The
saccharines, he supposed, were the link in nature between the unorg-
anized and the organized; but they were not fully organized if they
only consisted of carbon and water. In order to organize the sacchar-
inous substances minute traces of other elements such as nitrogen and
phosphorus were required. The synthesis of saccharines took place in
plants either by the direct combination of carbon with water, or by

the reaction between carbon dioxide and water, with the extraction of oxygen. Higher organized syntheses also took place; these involved the production of albumenes from saccharines, nitrogen and phosphorus. Oleaginous materials were then produced by the vital powers of plants and animals from both the saccharines and albumenes. The reverse processes of degradation commonly occurred during secondary assimilation.

**PRAUT'S CYCLE**

\[ C + H + O \rightarrow \text{crystalline sugars} \]

**SYNTHESIS**

organized saccharines, starches, lignin, etc.

**DEGRADATION**

albumenes

oleaginous substances

The synthesis of saccharinous substances was not an exclusive function of plants for, in a manner reminiscent of Greek physiology, animals were also capable of combining carbon with water, or if not able to begin at this low point in the scale, at least able to combine the organized saccharine principles with nitrogen and other elements so as to form albuminous substances. This function, or faculty, was performed by the liver through the separation or synthesis of a nitrogenous bilary principle. However, in the normal circumstances of an adequate diet, little call was made on this faculty
by a carnivorous animal; but in times of necessity, or in herbivorous animals, it might be more frequently called into action. (104) Prout summarised the theory of nutrition which was to be the basis of his pathology as follows.

Water, the basis of all organisation, is first combined with carbon to form the primary organised compound the saccharine radical; that the saccharine radical by undergoing certain changes is converted on the one hand into the oleaginous radical; and on the other, by undergoing certain changes and involving azote, is converted into the albuminous radical or principle. Consequently, that the albuminous principle on account of its involving the inferior radicals, must in all its ulterior changes be more or less influenced by the presence of these radicals. (105)

An attempt has been made to summarise Prout's theory of metabolism in a flow diagram. (p. 415)

Despite the general vagueness of Prout's scheme, he was explicit enough concerning the practical consequences of his analysis.

1. Design. The adaptation of anatomical structures to chemical mechanisms could only be explained as the deliberate act of a Designer. (see Chapter 6)

2. The carbon dioxide cycle, and hierarchy of cuisine obligée. (106) Plants and animals were mutually dependent on one another. Plants supplied nourishment to animals who, in turn, supplied them with the carbon dioxide which would otherwise be fatal to animal life if it were allowed to accumulate in the atmosphere. (107)

104. Prout believed that atmospheric nitrogen was used directly by vegetable eaters; he overlooked Boussingault's disproof of this, Ann. Chim., 12,153-67,1844.


3. Dietetics. The least organized substances like sugars and alcohol were bound to be more difficult to assimilate than purely amylaceous substances. (108) Most of these substances were not to be found in nature and had been prepared artificially by man; perhaps this was an indication of their undesirability as foodstuffs.

Man has been over-officious; and has studied the gratification of his palate, rather than followed the dictates of his reason.

The dyspeptic patient was singularly foolish if he sat down "to a luxurious modern banquet, composed of sugar, and oil, and albumen, in every state and combination, except those best adopted for food."

The gastronomic ingenuities of the continental cooks were rarely nutritious, and their use of pure sugar and oil was positively dangerous to persons with weak digestions. Yet the French, unlike the English, were sensible enough to pay more attention to reduction cookery. In general,

Reason is too little followed, the indulgence of the palate is the sole object; so that the organs of digestion already enfeebled, and incapacitated for the assimilation, even of the most proper nourishment, are daily oppressed with a task for which they are altogether unequal. (109)

Worse still, Prout concluded, such disabilities would be inherited and eventually lead in the fullness of time to the extinction of whole families.

4. Mechanism. The organic agents organized the processes of primary and secondary assimilation through the ganglionic nerves via the

109. Ibid., p. 510.
SUMMARY OF PRACT'S THEORY OF METABOLISM

**Primary Assimilation**
(reduction, conversion, vitalisation)

1. **Fats, oils, albumenes**
   - Reduction (+H₂O)
   - Liver to uniform chyme

2. **Primaries**
   - Cl to uniform chyme

3. **Secundaries**
   - Secondary assimilation (formative, destructive)

4. **Lungs**
   - Loss of carbon in

5. **Blood**
   - + CO₂
   - Heat

6. **Secretions**
   - Lymph for blood

7. **Excretions**
   - Skin, tissues, etc.

8. **Fats and other tissues**

9. **Cholesterol**
10. **Inorganic salts**
11. **Phosphates, etc.**

**Destructive Assimilation**

- Uric acid
- Ammonium urate
- Cystine
- Xanthine
- Hippuric acid

- Oxalic acid
- Lactic acid (kidney, skin)
- Ammonium carbonate
- Urea (kidney)

**SECRETIONS**
- Stomach
- Duodenum
- Pancreatic juices
- Bile

**ALBUMINIFICATION**

- Lacteals
- Completion (-H₂O)
- + albumen
- Lymph
agency of galvanism (electrolysis) and the other inorganic agents, heat and light. (110) Respiration was probably the function by which these electrical processes were generated or excited. (111) All such phenomena, with the exception of the vital agents, were capable of explanation within the terms laid down by Prout's molecular, or polarity, theory. He emphatically denied that vitalism could be rejected, or that the vital agents or nervous powers could be simply electricity. Even admitting that electricity, properly directed, could change the proximate elements of the food into those of chyle; can we imagine this principle to vary spontaneously its mode of operation, so as to produce the same chyle from every sort of aliment—that electricity is an intelligent agency acting with a certain object? (112)

Nevertheless, galvanism was a very useful model with which to explain the functions of the liver and kidneys since these organs could be pictured as the negative and positive poles of all the "actions going on throughout the organic system." (113)

The neutral and alkalescent character of the bile, and the oxygenated and acidulous character of the urine, show that the general character of the actions going on in the liver and the kidneys are directly opposed to each other—in short, that the general action of the liver is negative, the general action of the kidneys of a positive character; and that one of these two important organs thus antagonistically related to each other, cannot be deranged without deranging the other. (114)

111. Annals Medicine & Surgery, 1,156,1816; supra, Chapter 4, p. 156.
113. On Stomach, 5th ed., 1848, p. 572 n.; see Chapter 4, supra, p. 176 where in the context of hydrochloric acid production, Prout said that the stomach was positive, the liver negative.
At a molecular level the liver and hepatic system removed from the circulating portal blood any unassimilated or superfluous fats, extraneous saccharinous materials, and nitrogen-deficient substances which had been produced during primary assimilation. The kidneys, on the other hand, removed from renal blood all unassimilated, superfluous and simple albuminous materials and their associated inorganic substances. The tasks of these organs was therefore one of selection, and when necessary modification by synthesis or degradation. Changes wrought in the liver were usually of a synthetic or organizing nature, whereas changes produced in the kidneys were always of a disorganizing kind. If either of these functions was impaired, pathological conditions were rapidly produced; and Prout, showing more presence than knowledge justified by experiment, suggested that the liver was very "deeply involved in Diabetes". (115) Because of the delicate electrical or polar balance between the alimentary, hepatic and renal systems, the impairment of one of these electrical systems almost invariably led in the end to derangements in the other two systems.

Since milk, the alimentary prototype, contained all four natural aliments, water, saccharinous, albuminous and oleaginous, as well as certain important mineral matters, Prout recognised that the chemical basis of pathology could be approached by considering the normal assimilation and excretion of each class of aliment. This

approach was the burden of his revised textbook on stomach and urinary diseases.

Water was used in human metabolism both as a solvent or diluent for certain chemical processes, and more intimately in the association with organized principles in reduction. In the former type of function, water was directly absorbed into the blood from the stomach and gut, and used to maintain the fluidity of the blood, or as a solvent for various noxious products that were excreted as perspiration from the skin and as urine from the kidneys. Associated water was usually taken into the body with the food, or as combined hydrogen and oxygen. The ability of the stomach to absorb water directly was easily deranged, and the presence of excess water in the stomach led to imperfect digestion of the food, much discomfort and distress. Such sufferers were advised by Prout to avoid pulpy foods, or drinking with their meals.

It was possible to differentiate the urine produced by primary and secondary assimilation by examining urine before and after a meal. Urine collected immediately before breakfast was representative of secondary destructive assimilation (urinas sanguinis), and its abnormalities shed valuable light on pathological states such as nephritides and diabetes. (116) (See Chapter 3)

116. The opposite state, a deficiency of water absorption, was little understood according to Prout.

117. Diabetes was rigorously defined by Prout as "a disease in which a saccharine state of the urine is the characteristic symptom", On Stomach, 5th ed., 1848, p. 24. Earlier physicians had used the term diabetes to cover any urinary complaint in which there was diuresis.
Prouit devoted most of his attention to the pathology of saccharine assimilation and excretion. Unfortunately, his ignorance of glycojen, and the exact function of the liver, invalidated most of his discussion within a few years of his death. Since he believed that normal healthy blood did not contain sugar, he supposed that the saccharine principles had to be completely transformed during their primary assimilation into oleaginous and albuminous substances. If the stomach or other digestive organs were unable to accomplish this very important function, diabetes resulted. However, since in diabetes, if anything, reduction was enhanced, it was only the converting and organizing functions that were abnormal. A similar explanation was given for the production of other abnormal substances like oxalic acid which were usually symptoms of a future calculus. Sugar might sometimes also be produced during secondary assimilation when gelatine was produced from albumen, and vice versa. This only happened during the most advanced states of diabetes. More commonly, lactic acid was produced which immediately predisposed a patient to urinary complaints. The best treatment was a diet containing the minimum of saccharinous or farinaceous materials; but one which was carefully blended with vegetable roughage in order to remove the dangers of constipation. (118)

The derangement of saccharinous assimilation seemed to Prouit to be altogether more easy to acquire, and therefore more common, than that of any other principle because of their low organization and

118. He gave a recipe of bran, eggs and milk as a substitute for bread, On Stomach, 5th ed., 1848, p.44.
vital character. Some of this tendency was hereditary or dietary, or a question of temperament, sex and age, or due to exposure to cold and damp, or to malaria. He had been struck by the association of malaria with oxalic acid diathesis in 1832 at about the same time that the correlation with asiatic cholera was first made. (119)

The atmospheric or pythogenic theory of disease was widely supported by nineteenth-century physicians who were faced with unsanitary conditions and repeated attacks of smallpox and cholera. The atmosphere was conceived to be charged with the exhalations from fermentations and organic decompositions; these exhalations produced diseases and epidemics. In Prout's extended conception of merorganized bodies, the exhalations were thought of as rapidly rotating particles which were able to penetrate living organisms and interfere with their states of organization. (A virus analogy immediately springs to mind). Although a particulate "germ" theory, it was not the same as the germ theory of disease since Prout said nothing about contagion. However, like all atmospheric theories of disease it explained the prevalence of disease in marshy areas and undrained unsanitary areas of towns. Although this explanation proved too simple, it at least suggested that a fundamental step in preventive medicine would be sanitary reform.

The tobacco vice had a particularly bad effect on saccharine assimilation thought Prout.

Although confessedly one of the most virulent poisons in nature; yet such is the fascinating influence of this

noxious weed, that mankind resort to it in every mode they can devise, to ensure its stupifying and pernicious agency. (120)

And he went on to paint a vivid picture of the tobacco smoker with cachectic looks, dark, greenish-yellow tinted blood, malignant diseases of the stomach and liver, and cancer of the lips.

Any malfunction of the assimilation of albuminous substances was also best discovered from an examination of the urine for an excess or deficiency of urea, the presence of albuminous material, the presence of uric acid and its compounds, or the presence of cystine, xanthine and hippuric acid. All these materials might form, or help form, concretions. Such disorders were influenced by heredity, poor diets, intemperance, and worry; but like Liebig later, Prout thought they were more frequently due to malfunction of the processes of secondary assimilation, especially the transformation of gelatine into urea and saccharinous substances. He suspected that malfunction in the production of urea was often a forerunner of diabetes, and hence a very important symptom.

Much less was known about the derangements of oleaginous assimilation, Prout admitted, but clear examples were obesity and leanness, and the malfunctions which produced gall-stones. Once more Prout suggested inheritance as a culprit for these derangements. Climate and locality were aggravating causes. He recognised that fat acted as an insulator, even suggesting, contrary to the opinions of Liebig and Dumas, that fats were burned during secondary assimilation

to produce animal heat. (121) The other main function of fats seemed to be something to do with nervous and cerebral tissues in which they were associated with large quantities of phosphorus. The quantity of fatty matter in animals appeared to be correlated with their vitality ("nervous energy") since leanness was often accompanied by the presence of large amounts of phosphorus in the urine. Prout hazarded a rather futile prediction that "the oleaginous principle is much more deeply implicated, not only in the operations of organic life, but in those higher operations connected with the animal functions, than is commonly supposed." (122) Weakly, he concluded that if this were true then the study of oleaginous pathology would prove of great importance since it might lead to a knowledge of the causes of the most deadly diseases.

The incidental inorganic matters which were also found in food-stuffs or in the products of assimilation, also produced diseases, or could be symptomatic of them. Signs of illness were associated with either their insolubility (as with the magnesium, calcium and ammonium phosphates), or with their solubility (as with soda, potash and ammonia in alkaline urine). Phosphorus was associated with the nerves and nervous action; this probably explained why nervous illnesses were frequently accompanied by an excess of phosphates in the urine. (123) This conclusion was experimentally confirmed by Henry

122. Ibid., p. 276. If he were younger, said Prout, he would write a book about it.
123. Magnesia was associated with albuminous tissues, and lime with gelatinous tissues, he thought.
It will have been seen from the foregoing that Prout's theory of the nature of matter was intimately related by him to his activities in the field of biochemistry. His theory inspired him to develop an explanation of the major living processes of digestion and assimilation; and these schemes in turn provided him with an explanation of the digestive and urinary diseases in which he specialised as a physician. Prout's scientific career was a complete unity: in the discovery of the ultimate nature of matter lay part of the explanation of life and disease.

Yet both his molecular theory and his theory of digestion and assimilation were almost entirely ignored, and only a few features of his ideas on nutrition and pathology were adopted by his contemporaries. Why was this so? An answer may emerge by comparing Prout's methods and ideas with those of his younger rival, Justus Liebig.

**Liebig and Prout**

It must be said at once that Prout himself was much to blame for the neglect and inefficacy of his metabolic and molecular studies. Despite his unorthodoxy towards Dalton's atomic theory he was inherently a conservative man, and he suffered the chagrin of living to see Liebig and other continental chemists and physiologists build a new science on many of the principles which he had stated or foreshadowed. Had he lived only ten more years he would have suffered

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the further chagrin of hearing Cannizzaro disclose the relationship between atomic and molecular weights which he had outlined in the Bridgewater Treatise, and the over-confident dismissal of the unitary hypothesis by the skilful Stas. The brilliant polemical journalist, Thomas Wakley, in a Lancet editorial of 1844, made a personal attack on Prout's inertia and conservatism that went directly to the heart of the matter. (125)

The decade 1833-1845 brought the German chemist Justus Liebig into international prominence. His tours of England during 1837 and 1842, and his specially-invited Reports to the British Association on agriculture and animal chemistry, made him particularly well-known in this country; and led to the opening of the Royal College of Science in 1845. Many people hoped in 1844 that Liebig would take over the direction of the College when it was eventually opened, but Liebig recommended instead the appointment of his pupil, A.W. Hofmann. When J.F. Daniell, Professor of Chemistry at King's College London, died suddenly in 1845, serious efforts were made to woo Liebig into the vacant chair. These efforts were made most notably by the President of the Royal Society, Sir Benjamin Brodie. (126) These attempts failed because the Governors of King's felt that the religious constitution of the College forbade the appointment of a Lutheran professor. The chair went to Daniell's assistant, William Allen Miller.

Wakley was one of the reformers who felt that British chemistry was not to be measured against the tremendous improvements and discoveries which had been made by continental chemists in recent years, and that Liebig's appointment to an English chair of Chemistry would stimulate the teaching and study of the subject in this country. A patriot asked Wakley why Britain needed a Liebig when it already had a distinguished chemist in Prout. That was just the trouble, argued Wakley:

Many individuals hold Dr. Prout to be the first of British organic chemists; but several of the doctrines he espouses are opposed to those which are now taught in the continental schools that possess the highest repute.

One had only to read the 1843 edition of Prout's *Stomach and Urinary Diseases* to see this. For instance: Prout had ignored the discovery of pepsin (made in 1836) and relegated the views of Schwann and Müller to a brief footnote; he had old-fashionedy stated that exact compositions of albuminous substances could not be given, and he still regarded them, as well as saccharinous and oleaginous substances as compounds of just four elements "mororganized" in the living body by minute portions of certain unspecified "accidental minerals"; he had completely ignored Mülter's *proteins*; he


128. "We are aware that credit is due to Dr. Prout for having at an early period, when chemists regarded the saline and earthy residues of their analyses as so much useless refuse, contended against the error, and maintained that those constituents played unknown but important parts in the living economy. Yet, after other chemists have most cleverly demonstrated the truth of this, why retain a term (incidental) which is calculated to induce a misconception in the minds of students and others as to the true interpretation of numerous chemical facts", *Lancet*, 1844, p.407.
more or less ignored Liebig's arguments for the progressive changes of organic compounds in the living state, questioned the accuracy of the analyses upon which such views were based, and ignored the value of chemical formulae and equations in such discussions; finally, Liebig and Prout differed over nutrition and pathology in numerous details, the fundamental difference being that Liebig and Wöhler based all their studies upon the concept of tissue oxidation, whereas Prout never referred to the action of oxygen on the tissues.

This was a formidable list of complaints, all of which, with certain reservations to be mentioned, were justified. Wakley was in no doubt as to the reasons for Prout's failure.

1. Prout had declined to use chemical formulae which he scorned as unphilosophical expedients because they did not represent true elementary compositions.

(We) affirm that had Dr. Prout himself paid more regard to them, we verily believe he would never have placed reliance upon several false analogies, and would have suppressed from the world some hypotheses which are unsubstantiated either by facts or arguments: for notwithstanding that we are told in the preface to the 4th edition on Stomach and Renal Diseases that the work is practical, and precludes all controversy—we will venture to say that there is no "standard" work of the present day which contains more hypotheses or more matter of controversy. (129)

This is rhetorical overstatement, for Liebig's Animal Chemistry was a far more controversial book than Prout's textbook which, in any case, was addressed to the practising physician and not to the

129. Lancet, 1844, p. 489; note the similar point made by British & Foreign Medical Review, 11, 336, 1841.

academic chemist. However, Wakley had hit upon an extremely important point, namely Prout's rejection of contemporary chemical notation. This will be examined more closely in a moment.

2. Prout deserved his reputation, but as an historic figure, for his work on gastric juice and the alimentary principles. No doubt continental chemists had begun where Prout left off. But why had it been left to them to make the recent advances in animal chemistry? Because "Dr. Prout's name and authority exercises an influence that is detrimental" to the teaching and progress of chemistry in Great Britain. Science always declined when

the authority of those who, having earned a reputation for themselves, cast unfounded doubts upon the labours of others, neglect and repudiate, without sufficient cause, the methods followed by their competitors, and deny them that honour to which they are justly entitled by their discoveries. We regret to find Dr. Prout in this category. (131)

Ignoring the exaggeration of Wakley's remarks, it must be remembered that by the 1840s Prout was almost the sole older practitioner of animal chemistry who was still alive in Great Britain. (132) He was the only important chemist surviving and publishing from the earlier brilliant generation of Davy and Wollaston. Dalton was a dying man, Thomson was old and weary in Scotland, Bostock was to die in a cholera epidemic in 1846 and both he and Hatchett had long ago ceased to interest themselves in chemistry, Ure had entered the field

131. Lancet, 1844, p. 490. It should be noted that the Lancet's obituary of Prout was uniformly kind, ibid., 1850, 13 April, p. 449.

132. Among the younger generation a number of men had taken up the subject: Gregory, Playfair, R.D. Thomson. Most of these men received a continental education.
of industrial chemistry, Faraday had become a physicist, Fownes's potential was ruined by poor health, and Brande had ceased to make original contributions to chemistry. Only Thomas Graham was doing work of any great significance. It was with good reason that Liebig had exclaimed after his tour of the British Isles in 1837, "England is not the land of Science." (133) English chemistry was at an interregnum. The London Chemical Society founded in 1841 by Phillips, Graham, and a number of younger chemists who had been fired with enthusiasm for the subject by Liebig's work, and the Royal College of Science finally paved the way for a renaissance of English chemistry in the figures of Graham, Brodie, Frankland, Williamson and Odling.

Prout made no reply to Wakley's attack, (134) but it is undoubtedly significant that there are indications of a changing attitude in the final edition of Prout's textbook. There his antipathy towards formulae seems to have been waning, proteine and pepsin are mentioned, and there are several favourable comments on Liebig's work, (135) However, Wakley's general criticisms were still valid and the textbook's lack of chemical formulae and definitive account of continental


134. It is implied that he noted them, and that they stung him, in a conversation between Prout and Thomas Williams of Guy's Hospital and Swansea, reported by Charlotte Hall in her Memoirs of Marshall Hall, 1861, p. 91; note also the "apology" of a reviewer, British & Foreign Medical Review, 16, 480, 1843.

work, led to its rapid replacement by other texts, notably that of Golding Bird. (136)

Why was Liebig so successful compared with Prout? Let us examine Wakley's suggestions, and some others, in more detail. First, there is a purely biographical issue. Prout was a busy physician who was able to devote only his meagre spare time to the investigation of animal chemistry, whereas Liebig was an industrious academic chemist who controlled the activities of a fairly large research team. Moreover, in the 1830s and 1840s Liebig was at the peak of his mental powers, whereas Prout, Liebig's senior by eighteen years, was subjected to a deafness which seems to have profoundly affected his scientific output. It must also be admitted that Prout was by nature of a shy, withdrawn and non-controversial nature, despite his brush with Wilson Philip in 1831; Liebig, on the other hand, was of an extroverted and polemical mould.

Liebig was not one who dwelt on the merits of his predecessors and he made little reference to Prout's writings. Prout seems to have been slightly shocked by this cavalier treatment from his younger contemporary who arrogantly suggested that physiologists and animal chemists before him had never asked the right sort of questions. (137) But Prout had been asking Liebig-type questions about the

136. Urinary Deposits, their Diagnosis, Pathology and Therapeutical Indications, London, 1844; 3rd ed., 1851; 4th ed., 1853; 5th ed., 1857. Bird owed much to Prout with whom he seems to have been on excellent terms: "Dr. Prout's name will descend to posterity as that of one who has not lived in vain, of one who has left us a noble example of scientific zeal curbed by caution, of patient labour guided by a logical mind, and of extensive acquirements rendered more attractive by the modesty of their possession", Preface to 3rd ed., 1851.

progressive chemical changes that took place in digestion and sanguification ever since 1816 when Liebig was still a schoolboy.

If the factor of personalities is put on one side the problem can be rephrased in terms of the books which contained their ideas on metabolism. Why was Liebig's *Animal Chemistry* so much more influential than Prout's *Bridgwater Treatise* or his *On Stomach and Urinary Diseases*? A general answer to this question has already been suggested, namely that Liebig's book was specifically addressed to the theoretical chemist and physiologist, i.e. to the professional research scientist. On the other hand, Prout's *Bridgwater Treatise* was presumably intended for the consumption of a lay audience of Christian apologists; and his pathology textbook was addressed to the practising specialist on urinary and digestive diseases who was (according to Prout) not interested in purely theoretical issues. Thus in terms of presentation and publicity Prout never offered his professional colleagues in chemistry and physiology a seriously- and well-argued discussion of his ideas on matter, nutrition, metabolism and pathology. But Liebig did. One perceptive critic saw this clearly and wrote:

We think that it would not have been amiss to have inserted a few references to Liebig's peculiar opinions, as a guide to the student in the comparison of them with Dr. Prout's views. A good deal of trouble might have been thereby spared to those who ... desire to become fully acquainted with the points at issue between these two distinguished chemists. We

138. *On Stomach*, 4th ed., 1843, Preface, "Some of the views advanced by (Liebig) in his last book, are the same I have long advocated. Others of his views are directly opposed to mine, and seem to me to be neither susceptible of proof, nor even probable". But he did not discuss any of Liebig's opinions in the text.
trust that Dr. Prout may see the desirableness of publishing, in a separate form, and with more amplification, his opinions on these controverted topics, particularly specifying the evidence on which his own views are founded. Professor Liebig's work is almost entirely of an argumentative kind; the data on which his reasonings are founded are for the most part specified; and thus every reader, possessing a competent knowledge of the subject, can form his own opinion —from his knowledge of the probable truth or error of the data, and from his estimate of the logical precision of the reasoning, —as to the value of the conclusions drawn and set forth by the author. In Dr. Prout's treatise, on the other hand, there is more of assertion, and less of even attempts at proof, the data being, for the most part, locked up in the author's own laboratory; and until Dr. Prout shall see fit to give them to the public, he must be content to have his opinions freely questioned, and the accuracy of his conclusions suspected. (139)

Just as Prout's molecular theory never received serious attention (except from Henry) because he never published it in paper form, his ideas on metabolism were equally unfruitful. I have not been able to find a single serious discussion of Prout's ideas on assimilation in either English or continental literature of the period 1830-1850. On the other hand, his work up to 1827 was well assimilated both in Great Britain and on the continent. It seems therefore that as soon as Prout developed his ideas through the medium of his two books, they ceased to be assimilated.

139. British & Foreign Medical Review, 16, 478, 1843; the same critic agreed with Prout's decision to avoid chemical formulae.

140. Occasional complimentary references are given, but no summary or discussion, e.g., Sir R. Kane, Elements of Chemistry, 2nd ed., Dublin, 1849, p. 1041. The best reviews of Prout's books passed a few remarks on his theories, and these have been noted where appropriate. Jonathan Pereira adopted the terms primary and secondary assimilation, but never discussed Prout's theory of digestion, Treatise on Food and Diet, London, 1843, p. 440. The only discussion in this century has been E.V. McCollum, loc.cit., ref. 95, pp. 87-90.
As examples, consider Prout's classification of foodstuffs into saccharinous, albuminous and oleaginous, and his suggestion that milk was the prototype of a balanced diet. Both of these doctrines were adopted by a large number of authorities, including continental ones such as Liebig, Müller and Marchand. These doctrines are to be found in both the Bridgewater Treatise and in On Stomach and Urinary Diseases, so it might be thought that they owed their wide dissemination in the literature to these two publications; but this is not the case. Significantly, these two conceptions are to be found in the earlier Copley medal paper of 1827 which went into French, Italian and German translations, and received the commendation of Berzelius. It is probable that Liebig adopted these ideas from this source. However, Müller in his Physiologie and Marchand in his Lehrbuch der physiologischen Chemie show that there was another indirect source of information, namely Elliotson's English translation of Blumenbach's Physiology which contained extensive

141. See Bibliography of Prout's published papers infra; Jahres Bericht, 8, 244, 1829.

142. J. Liebig, Chemistry in its Application to Agriculture and Physiology, 2nd ed., London, 1842, p. 42 (milk). Liebig did not cite Prout for the milk prototype in his Animal Chemistry, but did cite him for gastric juice, the increase of urea output during exercise, fever and emaciation. The latter could have come only from Elliotson-Blumenbach (see Ref. 143), or from the German translation of Prout's On Urine, of 1823 (see the bibliography of Prout's papers, infra). Liebig also quoted from Prout's sugar analyses of 1827 at various times.
Marchand, as we saw in the last chapter, was an enthusiastic supporter of Prout's multiple weights hypothesis, and in his biochemistry textbook he lost no opportunity of mentioning and praising Prout's "magnificent insight". After citing Prout's classification of foodstuffs, Marchand added that Prout's work on nutrition and digestion was still unpublished (in 1844!), but that John Elliotson had quoted from Prout's unpublished book on the subject. Both Müller and Marchand then proceeded to quote the whole of this; it was the long passage that has been cited at p. 177 of this thesis.

It would appear then that Marchand did not know of either Prout's Chemistry or of his pathology textbook when he compiled his own influential review of animal chemistry. The same ignorance appears to


144. Marchand, Lehrbuch d. phys. Chemie, Berlin, 1844, pp. 392-4. Although without references, there is no doubt that Marchand's source was Elliotson-Blumenbach, Elements of Physiology, 4th ed., London, 1828, p. 310. (He may, of course, have taken the quotation from Müller). Elliotson's version of Blumenbach does not appear to have been translated back into German. The Allgemeines Deutsches Bücher-Lexikon for 1835-41 (1846) says that a German edition of Prout's Bridgewater Treatise appeared during these years. No translator is cited and only a rough title given: Chemie Meteorologie u., s. Natur, die, ihre Wunder, 2 vols.

There seems to be no copy of this book in England; I have to thank Berlin University library for lending me their copy.
be true of Müller and Liebig, although Liebig was never very
careful to cite his reading of his sources of information. The fact
that a kindly German disciple of his protyle theory like Marchand
knew nothing of Prout’s biochemical work after about 1827 has great
bearing on the reason why Prout’s ideas were not disseminated. The
Bridgewater Treatise was translated into German even though there was
not the same interest in natural theology in Germany as in Great Britain;
but no German chemist shows signs of having read it. The German
translation of Prout’s *On Stomach and Urinary Diseases* in 1843 probably
appeared too late for Marchand’s use, even if he would have been
attracted to it by its clinical title. (145) By 1843, in any case,
the revolutionary impact of Liebig’s *Animal Chemistry* had altered the
subject beyond anything that Prout’s two books could have accomplished.

Our original question can now be rephrased for a second time.
What was so special about Liebig’s *Animal Chemistry* compared with
Prout’s treatises? “Few works of comparable scientific import”,
writes a modern commentator on Liebig’s book, “have provided as many
apparent paradoxes as did this remarkable book”. (146) Holmes has
enumerated these paradoxes as follows. Although it laid the founda-
tions for modern biochemistry, few of its details withstood the test
of time; although Liebig apparently feared physiologists would not
understand his chemical approach, it was largely an elaboration of

145. *Ueber das Wesen und die Behandlung der Krankheiten des Morgens
und die Harnorgane*, Leipzig, 1843.

146. F.L. Holmes, *Introduction to Liebig’s Animal Chemistry*, Johnson
reprint, 1964, pp.vii-viii. In what follows I am very much
indebted to this source.
methods familiar to Prout and the early animal chemists; although Liebig claimed his methods were quantitative and exact, he continually resorted to "common-sense" evidence to support his speculations; although Liebig introduced biochemical equations for the first time, virtually none of these were correct in detail; despite the success of the first edition of his Animal Chemistry, Liebig gave up the attempt to publish an expanded new edition. The same author has suggested that Liebig's success was due to the fact that his approach was comprehensive enough to threaten the authority of more orthodox physiological and medical theories. Consequently he was bound to evoke controversies and emotional responses. Only a few years later Liebig's ideas were being judged by the new standards of empirical investigation which his "Animal Chemistry" itself had helped to foster, but by comparison with which the book seemed to retain too much of the older speculative ways of thought. (147)

It cannot be said that Prout gave a comprehensive account of digestion and metabolism, for although paradoxically it was "all embracing" and supported by a theory of matter, it was insufficiently detailed in its actual presentation. For example, where Prout spoke vaguely of the formation of gelatine from albumen by the loss of carbon dioxide in the tissues, Liebig boldly wrote the following equation. (148)

\[ 3(C_{46}H_{36}O_{14}) + 4H_2O = C_{36}H_{28}O_6 + C_{100}H_{16}O_{40} \]

\[ (1/2 \text{ atom of choloidic acid}) \quad (2 \text{ atoms of gelatine,} \]

\[ C_{54}H_{42}N_9O_{20} \]


And whereas Prout had suggested that in destructive secondary assimilation gelatine was broken down into urea, etc., Liebig specifically demonstrated the possible products by means of an equation. (149)

\[
\begin{align*}
\text{gelatine (according to Mulder)} & \quad \text{proteine,} \\
C_{108}^{\text{gelatine}}N_{18}^H_{16}O_{84}^{40} & \quad C_{96}^0N_{12}^H_{72}O_{28}^0 \\
\downarrow & \quad \downarrow \\
C_{48}^0N_{4}^H_{36}O_{14}^0 & \quad \text{allantoine,} \\
(3 \text{ atoms of allantoine),} & \quad (3 \text{ atoms of water)} \\
\downarrow & \quad \downarrow \\
C_{10}^4N_{4}^H_{4}O_{6} & \quad C_{2}^2N_{2}^H_{4}O_{2} \\
(\text{uric acid}) & \quad (\text{urea}) \\
\quad & \quad \quad \text{(water)}
\end{align*}
\]

Neither of these speculative equations proved to be correct, but they seemed to offer the animal chemist a new and better insight into the mysterious workings of metabolism than had ever been offered by Liebig's predecessors such as William Prout. Above all they conveyed a sense of the importance of trying to quantitatively correlate internal chemical changes with net exchanges with the exterior. Thus although Prout had made pioneering experiments on carbon dioxide output (Chapter 4) and had developed clinical methods for urine testing (Chapter 3), it was left to Liebig to imply the value of the measurement of

\[
\text{CO}_2 \text{ output : O}_2 \text{ input :::: N}_2 \text{ output in urine : food imput}
\]

149. *Ibid.*, p.142; "the more probable explanation of the production of these tissues from proteine, is that which makes it [gelatine] dependent on a separation of carbon."
Prout and Liebig shared in common a feeling that chemistry was neglected by physiologists who based their study on comparative anatomy to the impairment of research in their subject. Liebig's strong words on this made in his *Agricultural Chemistry* were an extraordinary echo of Prout's remarks of 1816 and the Gulstonian lectures of 1831. And Prout agreed with Liebig that:

> From the moment that we begin to look earnestly and conscientiously for the true answers to our questions, that we [should] take the trouble, by means of weight and measure, to fix our observations. (152)

But Liebig went on to emphasise that the results of quantitative analysis should be expressed in terms of formulae and equations; and with this Prout could not agree.

In 1834 Prout had been charged with Dalton and others to investigate chemical nomenclature and formulae; a subject which was then a bone of contention among British chemists. The brief majority report that appeared in 1835 supported the continental system of nomenclature and notation; but in a minority comment, Dalton urged his own pictorial system. Although Prout would not have adopted this pictorialism and evidently followed the majority report, there is no doubt that he agreed with Dalton that the Berzelian formulae

were clumsy and unphilosophical. (155) "I have purposely omitted the formulae now so much in fashion among chemists", he wrote in the Preface to the fourth edition of his pathology textbook, "not only because I consider them clumsy and unphilosophical as conventional expedients, but because I am satisfied that very few, if any of them, represent the true constitution of organized substances."

Five years later, in his final words on the subject, he still maintained the basic untrustworthiness of formulae and equations. (156) This positivistic extremism was as much a result of his molecular theory as of his feeling that analytical methods were still too imperfect. Just as Davy had scorned the Daltonian atomic theory for its acceptance of the elementary nature of undecomposed bodies, Prout was wary of an organic chemistry which had not investigated the nature of these elementary bodies. The "multiple relations" between the atomic weights of the elements offered a real clue to their combinations within organized substances, and Prout (who distrusted the atomic weights used by Liebig) was heartened to find Dumas reducing the atomic weight of carbon to a whole number. (157) Chemists had

155. Unfortunately, I have not been allowed to examine a letter of Prout's dated 30 January 1835 in answer to Turner's request for comments on notation. The letter is in the possession of Lt.Col. J.W. Nicol of Ballogie, D.S.O. See National Register of Archives (Scotland), Nicol of Ballogie Papers, No.0060.

156. On Stomach, 5th ed., 1848, Preface. Note that at p.505 of this edition Prout used symbols for sulphur (S), phosphorus (P), and iron (F), and that in the Chemistry he had used symbols for hydrogen, carbon and oxygen, and written empirical formulae for sugars.

recently (1843):

reluctantly admitted the existence of such relations among the four constituent elements of organized bodies. Another generation, I have no doubt, will recognise and admit the important consequences to which these relations lead. (158)

Here Prout was completely deluded. Whatever the reality behind Prout's hypotheses, whether the unitary theory of matter, or the existence of multiple relations between the atomic weights, with one exception, they turned out to have no bearing on the development or understanding of either organic chemistry or biochemistry. The exception to this was, of course, the factor of atomic weight determinations. Accurate analyses, formulae and equations were only possible through accurate atomic weights; a slight alteration in the atomic weights of carbon, oxygen, hydrogen or nitrogen could make considerable differences in the proposed formulae of such complex molecules as albumen. (159) To that extent Prout was right to be sceptical of Liebig's results. "I still believe", he wrote in 1848,

that the true nature of the lithic acid and its compounds is not at present understood; nor moreover can be understood, till the fundamental relations of what are called the atomic weights of hydrogen, carbon, oxygen, azote, are duly taken into account. The same remark applies to most organic compounds. (160)

159. Cf. the example of protein. In 1837 Mälder formulated proteins as \( \text{C}_4\text{H}_3\text{N}_0 \), but later as \( \text{C}_{30}\text{H}_{25}\text{N}_0\text{O}_{10} \) compared with

\[
\text{Liebig's formulation, } \text{C}_4\text{H}_8\text{N}_0\text{O}_{14}. \]

It is strange that Prout did not proceed further along the road taken by Laurent and Gerhardt who accepted formulae as "recipes" or symbols of chemical behaviour, and not as descriptions of real relationships between atoms in a molecule. Even here the adoption of a particular formula in any sequence of reactions depends upon accurate knowledge of atomic weights. But since Prout believed himself in possession of true molecular weights, there does not seem to have been any real hindrance to his use of formulae. I can see no reason to think that had Prout lived on into the 1850s that he would not have proceeded to use synoptical formulae. (161)

If the use of formulae was a fundamental difference between Prout and Liebig, several of the latter's ideas were held in common with the former. As several contemporary reviewers of Prout's work pointed out, Liebig's "metamorphosis of tissues" (162) was the same as Prout's "secondary assimilation"; and Liebig adopted Prout's classification of foodstuffs and his ideas on milk without modification, except that he went on to assign erroneous specific functions to the three aliment. Thus Liebig divided foods into "plastic" or structural nutrients (nitrogenous) which were responsible for the formation of new tissues, the replacement of degraded tissues, and the supply of protein from mammal milk; and "respiratory" or fuel nutrients (non-nitrogenous) which were oxidised in the tissues to produce animal

161. The only other chemist who rejected conventional Berzelian symbols was the positivist anti-atomist, B.C. Brodie; see W.H. Brock and D.N. Knight, Isis, 56,5-25,1965.

These exclusive roles had to be modified in the light of increased experimentation, but partly also because, as Prout had argued, the animal body was capable of transforming one class of nutrients into another. But both Prout and Liebig saw clearly that the physiological properties of living organisms were directly related to the chemical composition of their components.

Liebig's rigid functional division of foodstuffs into plastic and respiratory implied that fats and carbohydrates were of equivalent importance, and that either fats or carbohydrates were not absolutely necessary in a diet. This was in contrast to Prout's teaching that all three aliments were absolutely essential. Time has proved Prout correct. Liebig's rigidity also led him to argue that muscular work was obtained from the degradation of nitrogenous tissues (gelatine) into urea and its derivatives. In this case work should lead directly to an increased output of urea and uric acid, and hence "the amount of tissue metamorphosed in a given time may be measured by the quantity of nitrogen in the urine." Such was the attractive force of this quantitative proposition that it took several years before it was understood that muscular energy came in fact from non-nitrogenous foods (Liebig's respiratory foods), and

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165. F.L. Holmes thinks that only Liebig clearly perceived this; but this overlooks Prout's matter theory. See his Introduction to Liebig's *Animal Chemistry* (ref. 146).
166. Fats are for example an important source of vitamin A.
that proteins were principally responsible (as Liebig had admitted)
ly suggested) for the growth and maintenance of tissues. (168)
Prout had said nothing concerning the origin of muscular work, and
therefore he missed the opportunity seized, albeit incorrectly, by
Liebig for mapping out "a program for investigating these functions
quantitatively under various conditions of nourishment and activity."(169)

Prout also missed the opportunity to say anything concerning the
origins of animal heat. (170) Liebig, on the other hand, was primar-
ily concerned to demonstrate how the overall chemical transformations
in metabolism might be related to the production of heat and work.
This, together with the use of quantitative equations, was the key
to Liebig's success. There was one other factor. Both Prout and
Liebig constructed metabolic theories that have proved to be far too
simple. But there was a greater innate success-factor in Liebig's
over-simplification compared with Prout's because the former construc-
ted a theory (and a methodology) which was testable, or falsifiable
in all sorts of details. Most of these details proved to be incorrect.

168. For the experiments of Smith, Pick & Wisclicenus, and Frank-
land, see E.V. McCollum, op.cit., ref. 164.

169. F.L. Holmes, loc.cit., ref.146, p.lviii. Prout had suggested
that the "nutritive power" of a food was measured by its
percentage carbon content. Foods with more than 80% or less
than 30% carbon were of little value, Medical Gazette, 8,523,
1831, and supra, Chapter 4, p. 177.

170. "It is exceedingly probable, that though the evolution of
carbonic acid gas, may be one of the means possessed by the
animal economy for generating heat; there are yet other means,
the nature of which at present are quite unknown", Prout,
Chemistry, 1st ed., 1834, p.525; unchanged in 3rd ed., 1845,
but this turned out not to matter because, as Holmes has perceptively remarked:

First, [Liebig's] genius and chemical experience enabled him to perceive what must be true of the chemistry of organisms even if technical difficulties had so far prevented physiologists from discovering the phenomena directly. Second, his thorough knowledge of organic compounds, at a time when most physiologists were still relatively unfamiliar with them, enabled him to make an exposition which appeared to them a revelation of phenomena they had not previously understood. Third, a general dissatisfaction with older biological ideas made physiologists receptive to a new approach which promised more than it had yet delivered. Fourth, Liebig's influence as a teacher helped him to attract investigators to explore further his enticing theories. Finally, the rapid spread of experimental physiology provided the means and will to transfer his ideas to the direct investigations of animals. (171)

With the exception of influence as a teacher, Prout shared all these characteristics with Liebig. But since he did not construct "enticing theories", or rather because he did not provide testable chemical and physiological details within his theories, his ideas lacked impact; and they were without influence both in Britain and on the continent because Prout was only an individual without much time to devote to a very complex field of experimental and theoretical investigation.

Henry Bence Jones, a pupil of Liebig's, and an expert in the field of urine chemistry, succinctly summarised the respective merits of Prout and Liebig as follows:

Before anyone else without doubt, Dr. Prout previous to the third edition of his work in 1840, had formed for himself decided ideas on these questions (viz, the relationship between urine and metabolism, etc.); but from the

terms secondary digestion, vitalization, conversion, reduction, and diathesis, others could not obtain clearness, but only some confused comprehension in which no study of Dr. Prout's work could render perfectly distinct. The ideas of Berzelius [on Animal Chemistry] were but little known here, and it was not until Professor Liebig's work on Animal Chemistry appeared, when, with deep chemical knowledge and great clearness of ideas, he set forth the relations of the urine to the changes in the solids and fluids effected by use, and the action of the inspired oxygen, that the meaning of the language of Dr. Prout became evident. (172)

Epilogue

The cosmologist William A. Fowler said recently that Prout had not dared to sign either of his articles on hydrogen as a sub-multiple of the other elements even though he signed his concurrent articles on such non-heretical subjects as the sap of the vine, the ink of the cuttlefish and the excrements of the boa constrictor. (1) "Like many others", Fowler noted, "Prout is remembered principally for his heresy and not for his orthodox medical and scientific studies." This remark is perhaps an indication of the way in which Prout has been almost exclusively portrayed by inductivist historians as a man connected with the theory of the elements, while his other theoretical and experimental work in biochemistry has been generally forgotten, or ignored. In this thesis an attempt has been made to achieve an historical balance by examining both Prout's experimental and theoretical interests, and my conclusion is that there was a unity about Prout's work; namely, that he constantly sought after the laws which he felt sure governed "not only the operations of the animal economy, but the whole material world." By 1834, when he published his Bridgewater Treatise, he believed himself in possession of a corpuscular theory of matter which adequately explained a large number of experimental observations in chemistry, biochemistry, and medicine.

   I owe this reference to Dr. A.J. Meadows of Leicester University.
Progress in animal chemistry in the early nineteenth-century was never held up by vitalism, but rather by the sheer difficulty of organic analysis. Despite his vitalistic beliefs, Prout spent at least twelve years looking for a satisfactory method of organic analysis; he was evidently dissatisfied by the end product even then, if we may judge anything from his analytical silence after 1827. Little of his theory of metabolism stood the test of deeper experimental inquiry, but his brilliant analysis of gastric juice and his convenient classification of foodstuffs were firmly built into the foundations of modern physiology and nutrition. And the building of that foundation was begun with difficulty and with many unsuitable materials some years before Liebig. Prout was a *humoralist* in an age of *solidism* in medicine and physiology. At a time when physiologists were primarily concerned with the dissection and inspection of tissues, Prout dealt with the *chemistry* of fluids and tissues. If he had lived in an earlier age historians would label him an iatrochemist. During his own lifetime he was able to watch the pendulum swing and to observe "the triumph of those principles, which he was the first to enunciate, and which so long remained isolated and unsympathised with". (2) By his chemical insight he was able to suggest in a plausible manner the connections between a large number of urinary disorders, and disorders of digestion and assimilation. As one sympathetic critic noted, this connection was exploited by its author through the whole physiology

2. Dublin J. Medical & Chemical Science, 6, 167, 1848.
... and pathology of assimilation and excretion. The synthesis was wrong, but the approach was right.

Whether the results of future enquiries shall not confirm his particular doctrines, we desire to record here our deliberate conviction that the direction was first given to those enquiries by Dr. Prout, and that the physiologist, the pathologist, and the practitioner, ought therefore to feel themselves under a debt of gratitude to him, which no errors or imperfections in the details of his labours can efface. (3)

Prout's polarity theory which lay at the foundation of his biochemistry involved conclusions that were in many respects identical with those of Avogadro. Although he claimed to have based his anonymous papers of 1815-1816 on Avogadro's law, it was a most confused and imperfect version of it. Between then and 1834, however, he devised arguments to support both the hypothesis that equal volumes of all gases contain equal numbers of molecules, and the related conclusion that all the molecules of elementary gases contain at least two sub-molecules which were stabilised either by their cohesive or chemical union. He then looked upon the gaseous state as a system of self-repulsive molecules which were at least biatomic for theoretical reasons, and because it was observed that a single volume of an elementary gas never gave rise to more than two volumes of a product. Like Avogadro, he deduced from this that the ratio of the weights of two equal volumes of different gases was the same as the ratio of their molecular weights if the same physical conditions were maintained.

However, Prout's corpuscular philosophy led him to compromise with the equivalent school of chemists and to oppose the use of chemical formulae. He never chose to publish his ideas in a way which could attract the attentions of the growing numbers of professional academic chemists and physiologists, and after 1834, he did little outside medicine but revise his books in a rather desultory fashion. Deafness and the responsibilities of medical practice seem to have been chiefly responsible for this. By all accounts Prout was a singularly conscientious physician, and his protestations about his lack of time to produce a definitive scientific study of his theories and experimental results are undoubtedly to be taken seriously and not attributed to mental inertia. By 1830, the experimental sciences were becoming professional and it was increasingly hard, even in Great Britain, for a man to practise science as a hobby. Tiedemann and Gmelin, in their study of digestion, were aided by a team of eight full-time assistants; Liebig's Animal Chemistry was a synthesis of the work of a large number of his research students; Prout worked alone.

The results were personally unfortunate for Prout; for after 1827, when he presented his first and last paper on the analysis of saccharinuous substances to the Royal Society, none of his work became as well known on the continent where under the stimulus of Liebig important new advances in the study of Animal Chemistry were to occur rapidly. This was despite the translation into German of both the Bridgewater Treatise and his urine textbook. In this way, although in these two books Prout published a synthesis of his work in the
fields of matter theory, and experimental chemistry and physiology, this synthesis was either overlooked or rapidly forgotten with the advent of Liebig's quantitative insights. At Prout's death in 1850, he was remembered by medical men for his work on urine, and by chemists for his notorious hypothesis, but his theory of digestion and assimilation which had been replaced by that of Liebig's school, and his polarity theory and support for Avogadro's hypothesis, were already completely forgotten, except by friends like Daubeney. Curiously, the only feature of the Bridgewater Treatise which immediately became adopted by the scientific community was Prout's neologism, convection.

Prout the man is a shadowy figure in the history of science, but Prout's Hypothesis (or Hypotheses, as I have preferred to suggest), has been a continuous source of inspiration to chemists and physicists ever since its formulation in 1815-1816. It became more and more important and significant as the nineteenth-century wore on until Prout's name was only associated with this sometimes apparently erroneous, or sometimes brilliant, insight into the elements and their weights. The Hypotheses were a stimulus to analysis, a stimulus to chemists to interest themselves in atomic weights and therefore in the atomic theory, a stimulus to search for a system of classification, and when that was achieved, a stimulus to speculation about the evolution of elements, a stimulus toward the concept of atomic number and theories of the structure of the atom. More immediately, Prout's anonymous papers gave chemists a value for the specific gravity of hydrogen and a useful link between specific gravities and
atomic weights. With the rejection of the hypotheses by Staš, there was a general adoption of the experimentally convenient \( 0 = 16 \) scale of atomic weights; but far from removing the spectre of the unity of matter, it was reinforced; for apart from the odd weight of hydrogen, the elements were found to be even closer to whole numbers on this scale. By abandoning the \( H = 1 \) scale and Prout's Hypotheses, chemists came right back again to another form of the reductionist speculation. (4) Statisticians pointed out that the closeness of these weights to integral numbers could not be accidental, and various speculative attempts, which usually involved the ether, were made to explain non-integral weights.

By 1916, a century after the formulation of "Prout's Hypothesis", radioactivity, the Rutherford atom, the concepts of isotopes and atomic numbers, all seemed at last to provide its explanation and reveal the essential truth of the hypothesis. Certain problems remained only to be clarified by Aston's concept of the packing fraction. (5) The story has still not ended, of course; for having solved the problem of the nature and unity of the elements, Aston's


5. The present situation is that the simple whole number rule is incorrect. The difference between the isotopic mass (\( M \)) and the mass number given by the whole number rule (\( A \)) is called the mass-defect or excess (\( \Delta \)). (\( \Delta = M - A \)) \( \Delta \) is positive for light and heavy elements, but negative for the intermediate elements. Aston's explanation of \( \Delta \) was that it was due to the "packing" of nuclear particles into a small volume. The packing fraction, \( f = \frac{\Delta}{A} \).
work was the opening of another story, the exploration of the material and energy of the atomic nucleus. The old story of the multiplication of the elements repeats itself today in the bizarre multiplication of atomic particles by physicists. And the same old problems reappear: Can their number be reduced? Do the sub-atomic weights or their energy states, considered as numerals, have any theoretical significance? (6) Do they point towards the unity of matter?

6. In an important Pythagorean paper, "Integral and Rational Numbers in the Nuclear Domain and their Significance", Proc. Nat.Acad.Sciences, 32, 283-8, 1946, Enos E. Witner, proposed a new unit of mass-energy, the prout. (1 prout = \( \frac{m_p}{5160} \)), where \( m_p \) is the mass of a proton. Witner suggests in a Proutian manner that the "masses of all stable nuclei in the ground state are an integral number of prouts".
APPENDIX 1

PORTRAITS OF WILLIAM PROUT

Of Prout's portraits, four known canvases have been traced. A lost portrait in oils painted by John Hayes, a pupil of David, was executed during the 1630s. This portrait has now been traced; it remained in the possession of the Prout family until 1916 when it was inherited by Catherine Nicoll Lewis Nicoll, whose son, John Wilmot Nicoll, has it at his home in Aberdeenshire. (1)

A copy of the Hayes portrait was made by Henry Wyndham Phillips for the Royal College of Physicians in 1855. (2) Until recently this hung on the main stairway of the College's premises in Pall Mall. Prout's youngest son, the Rev. Thomas Jones Prout, was most dissatisfied with the Phillip's portrait, and in a letter to the Royal College (3) he offered them a new reproduction of the Hayes by H. M. Paget. "It will certainly be more satisfactory to

1. See National Register of Archives of Scotland, Nicoll of Ballogie papers, list 0060. I am very greatful to the Nicoll of Ballogie's brother, Lt.Col. F. E. H. Warner, for his great help in the difficult task of locating this portrait. Unfortunately, it has not proved possible to photograph it.


ourselves, as well as to those who may remember our Father and to any others who may care to know what he was like, that the College possesses a picture which does recall him better than the portrait which hangs on their walls at present." This new portrait was presented to the College by Prout and his sister Elizabeth in June 1668. "I venture to ask for it a place on the walls of the College, not as being entirely satisfactory, but as being a decidedly better likeness of my father than that which has hitherto hung there." (4) A final memorandum of Thomas Prout's dated March 1902 stated: "The portrait painted after his death by H. Phillips jun. and constructed partly from the original portrait by Hayes, partly from H. Phillip's own recollections, bears little or no resemblance to Dr. Prout. The most satisfactory copy of Hayes' portrait and therefore a much better likeness of Dr. Prout painted by H. M. Paget in 1888, was offered and accepted by the College in the same year 1888."

The existence of the miniature mentioned by Philip Hartog in his notice of Prout in the Dictionary of National Biography is doubtful. (5) Thomas Prout stated that he had never seen or heard of it before.

In 1902, Thomas and Elizabeth Prout commissioned a second copy of the Hayes from Paget for presentation to Edinburgh University where it now hangs on the Secretary's Staircase. (6) Finally, Thomas had photographs made of the Hayes original. A

4. To Pitman, 19 June 1888 (library R.C.P.). Ironically, until recently the College has always displayed the Phillips and stored the Paget. For the striking contrast, see Wolstenholme, op.cit., p. 348, and supra, pp. 30 and 31.

5. Watson reported in his letter to Farre that Phillips did his portrait from "a small miniature in the possession of the family". Munk, who was responsible for establishing cordial relations between Thomas Prout and the R.C.P., asked him about the miniature's provenance. (This was after Munk had mentioned it in his Roll). Prout replied that he knew nothing of it. "If therefore Mr. H. Phillips had such a miniature, it was either his own property inherited from his Father who was an old friend of our Father's, or, if it was our property, Mr. H.P. omitted to restore it." (To Pitman, 5 May 1888).
A similar photograph was presented to the Chemical Society in 1904, though their present copy appears to be a later print.

During 1949, Imperial Chemical Industries used a drawing of Prout by Michael Fyrton for their well-known series of advertisements.


9. e.g. *The Times*, 23 March 1949.
APPENDIX 2

PROUT'S CHILDREN

Prout's wife, Agnes Adam (19 April 1793 - 22 April 1863) was the eldest daughter of Alexander Adam, LL.D. of Edinburgh (1741-1809). There were seven children. (1)

1. Christina Prout (June? 1815 - 17 September 1815)

2. John William Prout (1817 - 2 June 1881)

Read Classics at Wadham College Oxford, B.A. (3rd class) 1839, M.A. (1841); entered Lincoln's Inn 1841, and made a career as a Barrister. He was a Fellow of the Royal Geographical Society and a member of the Athenaeum. He married (i) Miss Preston, in 1842, but his wife died in 1844 without child. (ii) the heiress, Catherine Marie Nicoll (1819-1846), in 1847. Catherine was the daughter of Colonel Nicoll of Hendon, and a granddaughter of Prout's early friend and patient, William Lewis (1757-1823). Their only child, a daughter Catherine Lewis Nicoll (1848-18 December 1914), who was brought up by her aunt, Elizabeth Prout, married in 1873 to William Edward Nicoll (1846-1914) of Ballogie in Aberdeenshire. There were four children: (i) Malcolm William Prout Nicoll (1879-1891); (ii) Margaret Catherine Nicoll (1880-1963) whose marriage to Sir George Redston Warner in 1910 produced Prout's three great great maternal grandsons, all of whom survive; (iii) Dorothy Ada Nicoll (1885-1963) whose genealogical researches and collections of Prout material have been frequently used in this thesis; (iv) Randall James Nicoll

1. For Agnes's death, Gents. Mag., 14, 305, 1863. For much of the data on Prout's children I am indebted to the Nicoll files.
FAMILY TREE OF JOHN WILLIAM PROUT

William Lewis (1757-1823) - Martha Turner (1756-1827)
( Distiller and botanist, friend of Higgins and Prout )

Catherine Clarke
LEWIS (1784-1834) m
Thomas NICOLL (d. 1824)
(landowner)

Thomas NICOLL
William NICOLL
John NICOLL

William Stone
LEWIS (1766-1854) m
Martha LEWIS

John William PROUT (1817-1881)

Catherine Mary NICOLL
(27 Dec. 1818-10 Mar. 1848) m

William Edward NICOL
(1846-1914)
(landowner of Ballogie, Aboyne, Aberdeenshire)

Malcolm
William
Prout NICOL
(1879-1891) m

Margaret Catherine
NICOL

Dorothy Ada
NICOL
(1885-1963)

Randall James
NICOL
(1882-1941)

George Redston
WARNER

Peter Eric Henry
WARNER (1915 - )

Sir Edward Redston
WARNER (1911 - )

John Wilmot
WARNER (1912 - )
assumed surname NICOL in 1941

children

children
(1882-1941). John William Prout lived at Neasdon House in Hendon (now demolished), and his estate there is acknowledged today only by a "Prout Grove". His Horton property was left to his brother Thomas, and his London property to his daughter. (2)

3. Alexander Adam Prout (16 September 1818 - 11 September 1854)

He remains a shadowy figure. The present Warner family thought that he must have died in infancy, and Lady Warner could never recall her great uncle Thomas ever speaking of this brother. The following few facts have been found. According to the Medical Directory for 1855, he was an Assistant Surgeon at the Chelsea Royal Hospital. The hospital records show that he was appointed to this position on 8 September 1846 after returning from India where he had served as Assistant Surgeon to the 13th Foot Regiment (Prince Albert's Regt., later called the Somerset Light Infantry). His commission to the Regiment is dated 8 June 1841, and he evidently served in India between 1843 and 1845. He obtained a Doctor's degree by examination from St. Andrews in 1850. (3)

2. See Alumni Oxoniensis (1715-1886), Oxford, 1888; Lincoln's Inn Admissions Register (1420-1893), London, 1896, vol. 2, p. 175. I have also examined the will proved on 18 July 1881. For William Lewis see infra, Appendix 5.

3. Medical Directory, 1855, p. 661 and Gents.Mag., 42, 430, 1854; Hart's Army List, 1843, p. 164; W. Johnston, Roll of Army Medical Service (1727-1898), Aberdeen, 1917, no. 4641 (gives date of birth). I should like to thank R.N. Smart, Keeper of Manuscripts at St. Andrews University, R.W. Place of the Army Records Centre, and Alan Harris, Assistant Secretary of the Royal Chelsea Hospital, for their invaluable assistance in compiling this brief note.

Educated at Westminster School London, he entered on a military career in 1839 with the 69th Native Infantry, which served in India, first as a Cadet, and then as an Ensign. In 1840 he joined the 56th Bengal Native Infantry, and was made a Lieutenant in 1842, Captain in 1845, and finally a Major in 1855. On 26 September 1855 he married Lucy Tubbs; the couple went out to India when the Mutiny began. He died during the siege of Cawnpore on 15 July 1857 from sunstroke. His name is recorded on the Memorial Column at Westminster School. His wife also died in 1857 during childbirth. Family tradition suggests that a daughter was born though it is not known whether the child survived. "It seems unlikely", writes Lt.Col. Warner, "though not impossible. If she did survive, hidden by an ayah, she would probably have been brought up an Indian, otherwise something concrete would have been known of her. There was an article in The Times a little while ago about a very old woman who appeared to be of European descent, and who was apparently known as Agnes." (4)

5. Rev. Thomas Jones Prout (18 August 1823 – 23 October 1909)

Educated with Walter at Westminster School where he distinguished himself at sports (member of "The Water" rowing team and Cricket XI). Entered Wadham College Oxford in 1842 where he read Classics like his brother John. B.A. (3rd class) 1846, M.A. 1848. He was ordained in 1849 and made a Tutor in Classics at Christ Church in 1851. He was the Vicar of St. Margaret's Church, Binsey, near Oxford, from 1857 to 1891, and a close friend of Charles Dodgson. An enthusiastic mountaineer, he read a paper "On the Ascent of Um Shaumur, the highest peak of the Sinaitic Peninsula in 1862" to the British Association. Like his father he went stone death in old age. He left his effects in 1909 to his spinster sister Elizabeth who allowed each member of Christ Church to choose a book from his library; Osler chose a copy of Lucretius. (5)

6. Elizabeth Prout (4 January 1825 – 11 May 1918)

Did not marry.

7. Agnes Prout (1826 – 10 December 1878)

Married 31 January 1850, just before her father's death, to Nathaniel Stainton (1814–1868), a barrister and friend of her

brother John. There were several children, and the grandchildren survive. Their grandson, Sir John Stainton (1888–1957), was a distinguished lawyer and Parliamentary draftsman. (6)

APPENDIX 3

Russian and American Prouts

1. Dr. John Prout (1790- ?)

A cousin of William Prout and a son of his uncle Robert. John
Prout studied medicine at Guy's and St. Thomas's hospitals in
1815, and was licensed to practice medicine by the Royal College
of Surgeons in the same year. He was elected to membership of the
Medical Chirurgical Society on 21 July 1833, and he obtained the
degree of Doctor of Medicine from the University of Heidelberg in
1835. He became a staff surgeon in the army of Nicholas I of
Russia and died sometime during the Crimean War. His widow, Marie
Augustine Liprandi, moved to Scotland with her five children in
the 1890s. The male Prout line does not survive. One daughter,
Ludmila Elizabeth Prout, married Baron Julian James de Spiganovicz,
and this branch of the family survives. (l)

John William James Ephraim Gabriel Robert PROUT (1758- ?)

m

William PROUT

Eleanor Bick

(8) John PROUT (1790- ?)

m

Marie Augustine Liprandi

Robert PROUT

Mary PROUT

(died young)

Sophie PROUT

(d. 1918)

Catherine PROUT

m

Dr. Timothy Clark

SMITH (American)

Ludmila Elizabeth

PROUT (d. 1918)

m

(Baron) Julian

James de

SPIGANOVICZ

children

children

1. Details from files of Miss Ada Nicol.
2. William Prout of Washington (1746 - 1824)

Nebuchadnezzar Prout - Martha Hale

William Prout (1746-1824)  
Emigrated to America 1780  
Died unmarried  
Sarah Slater (d. 1829)

Jonathan Prout (Rev) (1796-1879)  
Robert Prout (1798-1884)  
Sarah Hale (1802-1881)  
Martha Hale  
May Prout  
Dr. Jonathan Slater (practised in New York)

William Prout of Alabama  
2 Daughters  
4 Children  
Died in infancy  
Descendants with name Prout

In 1830, Prout made contact with his American cousin William, who sent him the following letter.

Washington 3 Oct. 1830

Dear Cousin,

Your friend D. Stephenson has been kind enough to call on me at your instance with a requisites that I should give you some tidings of our family matters which opportunity I here embrace with much pleasure and hope it may be the foundation of future interchanges between us. My Father died as you may have heard six years since (.) My Mother died last Winter leaving five children three sons and two daughters - Jonathan (.) Robert
and myself - Martha Hale and Mary. Jonathan the eldest married and one child (,) Robert a clergyman of the Protestant church married and no children. Myself married five years and four fine children, Martha married to D. McKnight of the U.S. Navy and attached at present to the West India Squadron, May the youngest just of age and unmarried. My Father left us a name very dear and estimable to us and one of his fellow citizens delight to call to mind sterling integrity united to great excellence of heart and simplicity of manners, won for him a reputation that all bearing the name may well be proud of the alliance (,) this epitaph he was an honest man -which comprehends all that can be obtained here below -and we firmly believe he has exchanged to his great gain. My lamented Mother I may bear testimony to her great worth, she was entirely unexceptional in every point -and particular, and her loss is fully present to us at this time. The Poor and needy lost a friend which should be her epitaph (,) She was one of the most respectable and wealthy family, in this part of the Country, all of whom have deceased and at this time we have not a single relative in America. My Father had ammassed a large property here, but owing to heavy speculations and too much confidence in some of his countrymen who deprived him of some considerable sums for endorsements, he left us about $20,000 - a price chiefly in Property - a portion of which is situated in our rapidly rising Western Country -which is now becoming very valuable. I am engaged pretty extensively in mercantile pursuits (,) My Brother Jonathan - at this time unemployed, but expects to embark in the same pursuits as my self in the Spring. I have here my dear relative given you a rapid history of our remains and our prospects for the future, am pleased to say is flattering. It will afford us much pleasure to hear from you as early as convenient with such family information as may be in trusting to your self. I have heard more than once incidentally of yourself as a practitioner in London, quite flattering, to yourself
- and us- all our family join in Love to your self and relations, and remain

With great regard Truly yours

William Prout

Please address me, Washington City DC (2)

2. From files of Miss Ada Nicol. The original letter is in the possession of Nicol of Ballogie; see Nat.Reg.Archives (Scotland) list 0060. The D. Stephenson mentioned has not been identified.
APPENDIX 4

Other non-related nineteenth-century Prouts

None of the following distinguished Prouts were directly related to William Prout.

1. Samuel Prout (1784 - 1852) Artist. (1)

2. John Skinner Prout (1806 - 1876) Artist and nephew of former. (1)

   His uncle Thomas Prout (1785 - 1859) was a London pharmacist and political agitator. His son, William Andrew, was also an agricultural scientist, and his grandson, John Alexander Prout (d. 1958), an artist. (2)

4. Ebeneezer Prout (1835 - 1909) Composer and musicologist. (3)

5. "Father Prout", Francis Sylvester Mahony (1804 - 1866) Nome de plume of Irish priest and humorist. (4)


APPENDIX 5

William Lewis (1757 - 1823)

William Lewis was born in Jamaica and came to England as a child. He became a successful business man with a distillery in London. He studied chemistry with Bryan Higgins who had laboratories in Soho. Besides chemistry, Lewis was also greatly interested in botany, and he became a founder member of the Linnaean Society. He does not seem to have published anything. It was the gout which brought him into contact with Prout and which led eventually, after Lewis's death, to intermarriage between the Lewis and Prout families. There does not seem to be any connection between him and the other, more distinguished eighteenth-century chemist, William Lewis (1708 - 1781).

Among the Prout papers now in the possession of Lt-Col. Warner, there is a packet marked "Prout Lewis Papers". The contents are mostly in Prout's hand; but several loose sheets, watermarked 1804 and 1805 are in a fairer hand. All of them are unimportant copies of literary productions. There is also a considerable amount of Lewis material in the possession of Nicol of Ballogie which I have not seen. On the evidence which has been available to me it is not possible to say whether Prout and Lewis collaborated on any scientific work. (1)

1. Gents. Mag., 93, 185, 1823, with portrait; F.W. Gibbs, "Bryan Higgins and his Circle", Chemistry in Britain, 1, 64, 1965; National Register of Archives Scotland, list 0060.
APPENDIX 6

Transcription of Prout's De Facultate Sentiendi

The first two pages are unpaginated. Deleted material, when decipherable, is placed within square brackets. Some punctuation has been added in brackets.

De Facultate Sentiendi Edinburgi 1810

(i) Proaemium

In the following imperfect sketch my chief object has been to deliver my own opinion only, respecting the curious and interesting subject of sensation, without paying much attention to what has been said by others on it.

I am aware that this subject has occupied the attention of philosophers of all ages and therefore can scarcely hope that any thing that I have here advanced will be new to those who are more conversant with the subject than myself, at the same time however I flatter myself that this will be sufficiently apologized for to my professors when I again assure them that for what I have here advanced I have been little indebted to others and that the greater part of it has been the result of my own reflexion on the subject.

In treating of the nature of matter I have adopted Mr Harris's definition of it, with the names by him given to its different conditions as used by the ancients. With respect to the subject I am aware that it is purely metaphysical and for this I should offer an apology were I not convinced, that my professors will pardon me when they consider how closely metaphysics & physiology are connected [constituted]. emboldened therefore by this hope I have ventured to submit with all its imperfection this rude essay to their perusal on the present occasion, sincerely hoping that it will meet with their approbation.

(ii) De principii vetali &c.

Although as the elegant Hebdend justly observes "principium illud vita vel incognita qua animantia al inanimatis distant", has acquired almost innumerable names among philosophers
yet its nature is still and perhaps will ever remain an inscrutable mystery for we know things only by comparing them with others to which they have a resemblance either in quality or property, but the principle of life has not its like in nature with which we can compare it and hence we are obliged to conclude that it is a principle sui generis or first principle.

While however we are ignorant of its nature yet we are by no means so of its properties and effects and by these we are in general enabled to judge of its presence or absence in the objects around us as well also as to form a definition of it which shall sufficiently in this indirect manner distinguish it from all other principles and such a definition of it is perhaps the following.

The living principle is that principle which when combined with matter has apparently\(^{(1)}\) the power of imparting to it one or more of the three following properties viz vegetation, sensation, and capacity for knowledge, according to the different modes or degrees in which it combines with it.

1. I have said apparently as I am perfectly aware that matter qua matter has not sensation &c., but when it is organized and combined with the living principle that combination is so intimate that the faculties of the former seem to be almost imparted to the latter or as it were divided between them and it is only by the apparent properties of this combination that we can judge of the presence or absence of the former, hence whatever inaccuracy there may be in the definition yet perhaps it may be allowed that it is sufficiently correct for all common purposes. it is true the living principle may sometimes exist in combination with matter without being rendered apparent by any of the usual properties of either of its states of combination, but when these properties are apparent no one doubts of its presence as they are as before showed the only criterions by which its presence can be ascertained. [verso of p.1]
(1) In animals and particularly in man all these three modes or degrees of combination of the living principle with matter exist at the same time in a very eminent degree, and from their properties these modes or portions of the basic principle have acquired the names of the vegetative, the sensitive & the intellectual soul or principles while the combinations of these with matter [I shall] may be termed the vegetative, sensitive & intellectual combinations.

Of these the intellectual principles & of course combination is the chief and is possessed by man in a far more perfect state than by other animals (.) it hence sufficiently distinguishes him from them by giving him the preeminence over them.

The intellectual combination [faculty] is that which is said to have the capacity for knowledge by which is meant that it originally has no knowledge but only the power of acquiring it which it does as we shall find through the medium of a property of the sensitive combination [faculty] termed sensation. The object therefore of this part of the essay is to shew the nature of the knowledge thus acquired as also that of its subject & further

2. Though I am aware that it is in the vegetative, sensitive and intellectual principles themselves that the properties [power] of vegetation, sensitivity & capacity for knowledge reside yet as they appear to reside in their combinations with matter I shall use the terms denoting the combinations instead of those denoting the principles themselves supposing the latter to be understood in the former, (vide last note). Perhaps also the idea [term] of intellectual combination may be objected to yet I cannot help thinking that its existence must be admitted upon reflection for it is very hard to suppose how the intellectual principle can be acted on in the manner it is through the medium of matter in any other way than by combination with it &. [verso of p. ii.]
the particular manner in which it is acquired by the intellectual combination\[principle\], and this is done as preliminary to the proper subject of the essay viz. the investigation of that property of the sensient combination\[principle\] termed sensitive.

This capacity for acquiring knowledge which seems to be peculiar to the intellectual combination\[principle\] appears to be of a compound nature, and to be made up of, or to consist of three other distinct properties or faculties of the same combination\[principle\] termed perception, memory and reason, of each of which we shall offer a definition, having first however attempted one of that property of the sensitive combination\[principle\] termed sensation, through the medium of which perception (the chief of three) has its origin.

Sensation is that property of the sensitive combination\[principle\] which is called into action when matter is brought into contact with any part of the sensitive combination or body of a sensient being and by which the intellectual combination\[principle\] perception or concept of the existence of such matter.

(2) Perception therefore may be defined to be the faculty of the intellectual combination\[principle\] which furnishes the basis or in other words the materials through the medium of sensation with which memory & reason operate in the formation of knowledge. Memory is that faculty of the intellectual combination\[principle\] by which it is enabled to retain inter si perceptionis illas quas consipit at any former time. Reason is that faculty of the same combination\[principle\] by which it is enabled to select from & apply to its use perceptions so retained inter si by the last mentioned faculty of memory.

Having thus attempted to define the nature of these several faculties which are necessary in the formation of knowledge we shall next make a few observations on the subject of the knowledge here alluded to viz matter.
"Matter" according to Mr Harris whose definition (3) of it I shall adopt "is that elementary constituent in composite substances which appertains in common to them all without distinguishing them from one another."

Although according to this definition of it matter be but of one kind yet it exists or at least is supposed to exist in different conditions or forms which may be divided into the primary and secondary the former of which will chiefly occupy our attention in this part of the essay.

The first primary condition or form of matter is that which was distinguished by the ancient [Greek] philosophers under the name of the [Greek] which Harris from Themistius defines to be "the subject or substratum of something actually existing." This condition of matter was supposed to be without "capacity for becoming many things", form, sensible qualities, &c. and hence in its nature it is analogous to the living principle itself in its uncombined state, and may be supposed to signify that condition of it in which combination between them first takes place. The next condition of matter is the of the same philosophers which Harris from the above mentioned author defines to be "that matter which has a capacity of becoming many things before it actually becomes any of them", this condition of matter also is supposed to be without form, sensible qualities, &c. though it is sufficiently distinguished from the last by its having "a capacity of becoming many things", which that is not supposed to have. This condition of matter seems to be analogous in its nature to the first combination of the living principle with matter viz. the intellectual combination [principle], as it would be necessarily that condition of it of which this combination [principle] would have a perception could such perception possibly [be able to] take place in any other way than through the medium of sensation.

3. See "Philosophical Arrangements" Cap. 4 from which cap. also all the other quotations following are taken. [p. 1 verso]
The third condition of matter we shall take notice of is that termed (4) [body] physical, which is the ἐν φύσει embodied with [the sensible qualities] extension and hardness, which for reasons that will hereafter appear I shall term primary sensible qualities, and this is that condition of matter analogous or rather adapted to the sensient combination [principle] or that condition of it which excites in it sensation & through it perception when it is brought in contact with any part of the body of a sensient being.

It is to be observed of the primary conditions of matter above mentioned, that they have no existence with respect to us in nature and that we arrive at the knowledge of them by abstraction and analogy only. matter in its aggregated state next to be mentioned affording to the intellelctive combination by which memory and reason enable it to arrive at such knowledge of them: the object of inserting them here is merely to endeavour to render probable by their means the unity of matter. See for further particulars in "Philosophical Arrangements".

(4) As to the secondary condition of matter with its varieties as they are to be treated of more fully hereafter we shall only briefly enumerate them here.

4. I am aware that the ancients reckoned two intermediate conditions of matter between the ἐν φύσει ὕλη and the body physical viz. the τὸ ἔνεντοριστὸν ὑπὸ ὑποκείμενον "the second matter in body void of quality", consisting of the ἐν φύσει ὕλη "embulked with three extensions", and the body mathematical, or the ἐν ἀριθμῷ combined with extension and figure only. With respect to the qualities I have here termed sensible viz extension & hardness it is to be observed that the extension here meant is . . . . & that it includes also the notion of figure, while what I have termed hardness is a quality or property of organization essential to the existence of body physical. See philosophical arrangements Cap 5. [p. 3 verso; the missing word is indecipherable.]
The only secondary condition of matter is the or immediate matter or as I would rather term it matter in its aggregate state such as wood, stone, water, air &c. Of this there are five varieties depending upon certain differences in the mode of aggregation &c. viz. its aggregated solid state, its liquid state, its aeriform state, its Aetheriform state & lastly its Luciferous state, vide infra.

This secondary condition of matter differs from physical body the last of the primary forms of matter by having in conjunction with the primary sensible qualities extension and hardness a certain secondary sensible quality roughness which in the different varieties of it is variously modified as we shall see hereafter.

We come now to consider the manner in which the intellectual combination [principle] arrives at the knowledge of the existence of matter through the medium of sensation.

It has been before observed that matter in order to excite sensation in the sensitive combination [principle] must be possessed of certain qualities termed sensible, now these qualities are so intimately connected with matter that it is only through their means, or rather through their combinations with it, that we are enabled to know of its existence, hence the b...(?). ly mentioning the manner in which the knowledge of them is acquired must necessarily include also the manner in which the knowledge of matter is acquired.

In the first place therefore with respect to extension it is to be observed that like every thing else of which we have perceptions through the medium of sensation it is a relative term only the positive of which with respect to does not exist in nature, whether this depends upon the manner in which our knowledge of them is acquired or whether it depends upon their nature is not easy to determine it probably however depends upon a combination of these two causes, and chiefly upon the former of them as I flatter myself will appear from the following statement of that manner.
The intellectual combination [rational principle] cannot acquire knowledge by the help of perception alone as we formerly observed, but by the help of this joined with memory and reason. Thus for instance in drawing my finger along the edge of the table a series of perceptions concipitur by the intellectual combination [rational principle] through the medium of sensation by the contact of its different parts which my memory retaining enables me with the assistance of reason to draw comparisons &c. between (,) and thus to know that matter existed at every point between the angles of the tables and hence that these two angles could not be the same angle &c. without the memory & reason however these could not have been known for I should not have then known that the several portions of the table had separate existences, and of course also should have been ignorant that they existed in different ubis. Hence without these all matter would appear to exist in the same ubi. Which ubi appearing to me to be the same invariable attendant of matter would not be recognised and of course extension would not by me be known.

In the same manner also with respect to hardness which is also as before observed a relative term the positive of which (6) does not with respect to us exist in nature and with the knowledge of which also the intellectual [rational] principle would never become acquainted by perception alone without memory & reason, even the perception or knowledge of the existence of matter itself without them would not be conception by the intellectual combination [rational principle] longer than the moment of its action through the medium of sensation in the formation of that perception or knowledge and of course its quality of hardness constantly modifying in every instance such perception would not be at all distinguished (,) by the help however of memory & reason the intellectual combination [rational principle] arrives at the knowledge that hardness is united with matter in different degrees between which by the help of the latter faculty in particular it is enabled to form comparisons & from them to draw
the inferencies that this portion of matter is harder or softer than another &c. and in this manner to arrive at a knowledge of the nature of hardness as a quality of matter only &c.

In nearly the same manner does the intellectual combination acquire a knowledge of time and in short as before observed of everything else since the only manner in which it acquires knowledge is through the medium of sensation, the nature &c. of which we come now to consider.

De facultate sentiendi

Sensation has been defined to be that property of the sensitive combination which is called into action when other matter is brought into contact with any part of the body of a sensitive being and by which the intellectual combination perceptionem concipit of the existence of such other matter.

When matter is brought into contact with any part of a sensitive combination or body of a sensitive being, sensation in it & perception in the intellectual combination immediately follow though in different degrees of intensity &c. according to the acquisiteness &c. of the organization of the part in which the contact takes place; this succession between contact and perception takes place so rapidly that they appear to the vulgar who do not reason upon the subject to be merely cause and effect, the philosopher however is aware of the contrary. reason tells him that these must necessarily be only the two extremes of perhaps

5. The knowledge of time like that of the primary qualities conditions of matter &c. is abstract and is acquired by the operations of the faculties of the intellectual combinations themselves one of these perceptions to wit, affording the other the materials through the medium of sensation from sensible objects. to explain the manner however &c. in which these are accomplished would be here entirely foreign to my subject wherefore I shall pass it by. [p. 5 verso]
a long series of cause & effect of the nature of which he is extrem­ely ignorant but which are as yet absolutely wanting to connect the two extremes: the knowledge of all their immediate links we shall never perhaps be able to arrive at, yet by pa­tient observation & investigation we may at a future period dis­cover the nature of some of the nearest links and thus in a small degree shorten the chain. at present however we cannot but lament how limited our knowledge is and how little the philosopher is able to proceed beyond the vulgar ken with whom as before observ­ed they are considered as mere cause & effect.

That what I have to advance on this subject may be better understood I shall comprise the sum of it in the two following propositions.

First. As Sensation\(^{(6)}\) is a resulting effect or property produc­ed by the combination of only two principles viz. the living principle and matter and as the cause of sensation or matter is but of one kind, it must necessarily follow that sensation is but of one kind also, \([\text{and that of course the only way in which it can be varied is in degree only}]\) and hence also that it must necessarily form the basis or substratum of all those varieties of it which apparently take place when different conditions of matter are brought in contact with different parts of the bod­ies of sentient beings. Now the varieties of matter in its aggreg­ate \([\text{state}]\) condition are five viz. its solid state, its liquid state, its aeriform state, its atheriform state and its luciform state, \([\text{& the varieties or modifications of sensation are also five, viz. touch, taste, smell, hearing and seeing, therefore Secondly. As matter in its aggregate solid form is supposed to be made up of secondary aggregated particles formed by the union of the primary particles of physical body or matter which are}\]

6. It is to be observed that in the following pages perception & knowledge are always understood to follow sensation except when mention is made of the contrary. \([p.6\text{verso}]\)
possessed of extension and hardness, and as such aggregate solid matter becomes thus combined with a secondary sensible quality termed roughness which is known to vary according to the sizes &c. of the secondary aggregated particles composing such [and which is termed roughness] and further as this sensible quality or roughness is distinguished by that modification of sensation termed touch hence it may be inferred that the other modifications of sensation as well as their varieties are analogous to this of touch and that they also distinguish only under their different forms the different sizes &c. of the aggregated particles of the same condition of matter, that is to say that [the different] tastes, smells, sounds and colors as well as their varieties are analogous to, and depend upon the same causes as roughness & its varieties viz. upon the different sizes &c. of the aggregated particles of the same matter.

(9) These propositions being premised we shall next endeavour to elucidate & explain them more particularly.

As sentient beings are compounds of a living principle and inert matter whose connexion is effected and supported by a certain regular organization of the latter and as sensation is a resulting effect or principle of that combination which is called into action by the contact of the same external matter with the bodies of such sentient beings it must I think necessarily follow that such a resulting effect or principle must reside in every part of a sentient being as the same identity however much it may appear to be modified and changed by accidental varieties [in intensity &c] in the matter producing it, accordingly [as before observed] this is found to be the case for every sentient part of the bodies of animals is capable of having sensation excited in it, though in different degrees of intensity, forms, or modes &c. according to the exquisiteness of the organization &c.

That certain parts of the bodies of sentient beings are more exquisitely organized than others and that these parts are more
capable of having sensation excited in them than others are
facts well known, such parts e.g. are the lips, glans penis,
ends of the fingers &c. There are however certain parts of the
body the contact of external aggregated matter with which is
effected through the media of a certain mechanical apparatus
adapted to the mechanical properties of such aggregated matter
in certain subtle forms of existence without which apparatus
sensation could not be excited by [these forms of matter] them,
and what is more also, the sensation produced by [these] means
of such apparatus are variously modified and appear altogether
different from sensation in its usual forms. I allude to the
tongue, the ear &c. and the modifications of sensation termed
tasting, smelling &c.

(10) That I may however be better understood I shall select one
of these the operation of which seems to be better known than
those of the others I mean the eye. the eye is well known to be
an instrument adapted to the [physical] mechanical properties of
That very subtle form of aggregate matter termed light the rays
of which coming from distant objects it concentrates into one
point which falls exactly upon the bare optic nerve where by
contact it produces that modification of sensation termed seeing
whereas without this contrivance the bare optic nerve could not
be exposed to the contacts of light without its delicate organ-
ization being destroyed nor could light probably in its common
rare state without concentration produce vision by contact with
it (. ) such instruments also are the tongue, nose &c. though
these are calculated for the grosser forms of matter, water, air
&c.

Before however we proceed we shall form a table of these
modifications of sensation with the various conditions of matter
calculated to excite them &c. in the apparent subtlety of the
latter of which will be found a beautiful gradation from aggreg-
ated matter in its solid form up to light the most subtle form
As I flatter myself that the general nature of this table will be obvious at first sight I shall therefore at once proceed to the explanation of its parts only more particularly.

First. It has been observed that matter to produce sensation needs only to be possessed of its primary qualities hardness and extension when it is termed physical and that sensation is affected by the contact of such matter with any part of the body. It will however be readily perceived that it has also however been said that matter in the form or condition here understood is imaginary only and that it does not at least to our knowledge exist in such forms. The supposition however of such a condition of matter has been adopted for the sake of analogy and perspicuity and also for the reason that the particles of such a condition of it must necessarily form the basis of the next condition of matter to be noticed viz. its aggregated solid form.

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7. Concerning the nature of the sensation termed heat See Note A Page 23

8. or to use the words of AMMONIUS when making similar observations respecting the non existence of the

Translation. "Not that there ever was in actuality either matter without body or body without quality but we say so as we contemplate the well ordered generation of things dividing those things in imagination which are by nature inseparable.
through it perception in conjunction only with its sensible qualities extension & hardness hence these must necessarily be composite also that is to say they must consist of sensation and perception of matter combined with certain qualities corresponding to those which exist in conjunction with the matter itself & [which must also vary as they vary &c] these qualities of sensation & perception I shall term quantity & intensity supposing the first to correspond with extension & the second with hardness &c.

Secondly. By the term aggregated solid when applied to matter, is meant as before (noted??) that form of it usually denominated its solid state and which is the object of touch. The superficies of all matter in this state is known to produce by contact with the fingers, &c a certain modification of sensation termed roughness. in different portions of matter the degree of this roughness which is known also to depend upon the different sizes &c. of the secondary particles which compose it. if these sec-

(12)ondary particles be all of the same size the roughness may be said to be uniform, if of different sizes irregular. The limits of roughness cannot be defined though (with respect at least to our sensation) such limits actually exist, for bodies with respect to such sensation may be absolutely rough & smooth, that is to say that after certain limits we cannot perceive greater or less roughness in them. This modification of sensation like all others is varied in two ways by the original sensible qualities of physical matter viz. in quantity & intensity, though these like the sensation itself are modified as here the quantity of the modified sensation corresponds with the quantities of surface acting at the same time in its production, while its intensity corresponds with the degree of roughness (whether greater or less) of the superficies of the same matter acting at the same time in producing it.

Thirdly. The next modification of sensation to be considered is taste, of which it is known that it is excited by matter in a
state of fluidity only; fluidity is evidently the next form of aggregated matter after its [aggregated] solid one, and hence we may conclude that the secondary particles themselves as well as their state of cohesion are less than in the last instance, and the same also may be further inferred from their being inappreciable by the last modification of sensation termed touch. A peculiar organ or machine therefore is adapted to this form of matter by means of which it is enabled to act upon a certain part of the body by contact & thus to produce a peculiar modification of sensation. This instrument is the tongue & the modification of sensation is termed taste. (9) Taste like touch &c. varies in quantity & intensity the former corresponding to the quantity of matter acting at the (13)same time, the latter to the different tastes.

Fourthly. The next modification of sensation is produced by aggregate matter in its aeriform state, in which its secondary particles may be supposed to be less than in either of its former conditions, whence they are too small to be appreciated by either of the instruments before mentioned & through them to produce their peculiar modification of sensation. Wherefore a peculiar organ (the nose) is adapted to this form of matter by means of which when brought into contact with that particular part of the body a peculiar modification of sensation is perceived termed smelling. This modification of sensation also like the three last is varied in two ways viz. in quantity & intensity. [the nature of which will be understood from what has been said above] the former corresponding to the quantity of matter acting at the same time the latter to the different smells.

Fifthly. The next modification of sensation is termed Hearing the instrument by which this is effected being the Ear (10) which I suppose to be a machine of a peculiar structure adapted to

9. See note B. pag. 24
10. See note C. Page 26
favor the action of a certain [modification] subtle form of aggregated matter (Ather?) too subtle to be appreciated by any of the above mentioned organs, and thus by its means to produce the modification of sensation in question. Hearing is varied in [two ways] quantity & intensity which are commonly called Loudness, & acuteness and gravity, in common conversation, from which & from what has been said above the nature of the sensation will be sufficiently understood.

Sixthly. The last modification of sensation is seeing the instrument for effecting which is the eye, a mechanical instrument (11) adapted to the [physical] mechanical properties of that most subtle form of matter light, by means of which the latter acts by contact with the body & produces that modification of sensation termed vision. Vision varies in quantity & intensity the latter answering to their brightness, (11) the former to the variety of colors.

From the above specification of the different modifications of sensation and from the attention which has been taken to point out the general analogy which prevails among them I have no doubt but my general meaning will be easily comprehended perhaps there are some points which require a little further elucidation, for which reason therefore we shall give a brief recapitulation of the whole from which I flatter myself they will be better understood.

In order to set forth the [speculation respecting] probability of the unity of matter as distinctly as possible we have begun

11. No doubt the nerves themselves are also adapted in particular ways to the actions of the different varieties of matter specified so as to be effected by no other variety of matter but that particular one to which they are so adapted. but if we may judge from the analogy of the unity &c of the acting matter this adaptation must consist only in different degrees of fineness &c of organization in them. [p. 13 verso]
by endeavouring to trace its identity through all its primary conditions from the To $\nu\nu\kappa\rho\kappa\iota\epsilon\lambda\mu\nu\sigma\nu$ of the ancients down to its physical state, or that condition of it in which it becomes the object of sensation from its being vested with sensible qualities, respecting this notion of the unity of matter I may here observe that it was adopted by most of the ancient philosophers and by many of the moderns among the latter of whom was I believe the immortal Newton himself. the speculations however respecting it have been always, and still are by many accounts visionary, but when we reflect upon the astonishing discoveries that have been made in Chemistry and the progress it is still making, who will say that at some future day they will not be realized at least with matter in its secondary or aggregated condition, and as one discovery lays the foundation for another (15) the knowledge of this may lead to the knowledge of other conditions of it of which at present we can possibly have no conception. setting aside however such speculations as misplaced here let us return to our subjects.

We have said that the first condition under which matter is actually known to us to exist is its secondary aggregated one and that the most simple variety of this is as an aggregated solid, which we have supposed to be made up of secondary particles formed by the primary particles of physical matter variously aggregated, and hence we have drawn the conclusion that all the varieties of this form of matter consist only of variously aggregated particles made up [however] of the same original primary particles of physical matter. We have further observed also that the most simple variety of this secondary condition of matter viz its aggregated solid state presents another apparent sensible quality termed roughness, but that this is of a secondary nature only or in other words that it is only a modification produced by the union of the primary sensible qualities of matter extension & hardness which is assumed from the fact that the modification of sensation produced by it termed touch is liable to be varied in quantity and intensity the same as sensation
itself is by the original primary sensible qualities of physical matter themselves viz. extension & hardness before mentioned; in the same manner also we have endeavoured to shew that the apparent secondary sensible qualities termed tastes smells sounds & colors produced by the other varieties of this condition of matter viz. its liquiform, aeriform and luciform states, are only modifications like roughness produced by the (...) of the original primary sensible qualities extension & hardness, since these modifications of sensation are liable to be varied by these secondary sensible qualities in the same manner as sensation is by the primary sensible qualities namely in quantity & intensity, &c.

(16) In support of the opinion we have advanced that sensation is but one kind the chief reasons we have adduced are first the unity of matter which is the cause or object, and secondly its being a resulting effect or principle produced by the combinations of two principles only (,) viz the living principle & matter. As to its modifications & they have already been so repeatedly enumerated even in this recapitulation that I flatter myself my opinion of their nature is sufficiently understood. I shall therefore only say in brief that I consider touch, taste, smell, hearing & seeing to be modifications only of the same original sensation & that they are caused only by the differences of subtlety & in the aggregated particles of the same secondary or aggregate conditions of matter, which are enabled to act on the sensitive combination by means only of certain apparatus adapted to the mechanical properties of those varieties of aggregated matter.

We shall now make a few observations on the analogy that prevails between touching, tasting, &c. in order to clear up as well as we can some little difficulties respecting them after which we shall close the subject with some few general observations upon the whole.
With respect to the analogies that prevail between the various modifications of sensation, it may in general be observed, that upon reflection they will be found stronger than at first sight they appear to be. Thus between the roughness of bodies, and taste with the varieties of these, however unlike they may at first appear, yet it will be found that strong analogies prevail between them in many respects. Thus both of them as before observed are liable to be varied in intensity and quantity. Both of them have certain limits in these respects (which are however difficult to be ascertained) beyond which at least to our sensations they cease to exist. It is true that we cannot trace the degree of intensity in tastes as we can in roughness and this may probably arise in part from their limited number, this modification of sensation being evidently the most limited and imperfect of all others. Perhaps the varieties of all the modifications of sensation are caused by varieties in the particles of matter producing them whose ratios with respect to each other are contained within [one] 1 and [two] \( \frac{1}{2} \) only, that is to say that these are the extremes of the ratios of the particles of matter producing such varieties. This we know to be remarkably the case in sounds and perhaps also in light, and hence in the present case may we not say that there are three primitive tastes (certainly there are not many more) corresponding to the three principle sounds in the octave (,) the principle

12. The chief difference between sounds and light in this respect is the repetition of the primitive series of the former so as to form a continued one of many octaves &c. whereas the primitive series of color is not repeated. The reason of this is perhaps to be sought for in the different accidental differences in their nature &c. How this can stand with respect to tastes and smells cannot be ascertained. [p. 16 verso]
third & fifth, and the red, yellow, and blue in colors whose proportions at least in sounds are 1, 4/5 and 2/3 and that all other tastes &c are caused by particles whose ratios with respect to each other are intermediate &c, but here I shall be asked which are these primitive tastes, and which has the greatest intensity &c, to this question at present I am not able to reply in any satisfactory manner. I would however say (18) that the acid and the alkaline are two of them & that the acid has the greatest intensity of them but this I request will be taken as conjecture only.

Between tasting and smelling the same analogies hold as in the last instance, and though the number of smells does not appear to be so limited as that of tastes yet I cannot help thinking that there are only two or three primitive smells by the admixture &c of which all the apparent numerous varieties are formed. Of these however I cannot at present even venture a conjecture. as the same analogies &c therefore hold in this as in the last (,) what has been said above with respect to it is also to be understood of this and hence needs not be here repeated.

Modern philosophers are I believe in general of the opinion that air however necessary it may be to the propagation of sound is not at the same time the proper vehicle by which it is conveyed. from the exper. ts and reasonings brought forward in favor of this opinion & which need not be here repeated I have been induced to adopt this opinion as the most probable one and hence have called this supposed medium of sound Ather from the notion of its being a more subtle form of matter than air though I do not pretend to be able exactly to define its nature. The cause of sound is supposed to be vibrations of this medium propagated from the sounding body by which they are produced to (19) the ear and this perhaps upon the whole is one of the most plausible notions that has been advanced, though it must be confessed that it is not without its difficulties, but as these
do not concern us they need not be here repeated (.) we shall therefore suppose that the modification of sensation termed hearing is produced by the striking of successive undulae, or what is nearly the same (,) by the successive striking of the same undula upon the auditory nerves. Now these undulae and of course the sensations produced by them differ in many respects according to the size, shape, degree of regularity &c of the bodies producing them. thus if the surface of the sounding body be irregular the sensations [of the sound] produced by it will also be irregular or rough, & hence may be deduced the striking analogy that prevails in this instance between the roughness of bodies & the roughness of sounds, a fact that has long been well known. this roughness of the sensation is occasioned by the aggregate particles of the sounding substance being not all of the same size but from their being a mixture of small and greater ones, but if they are all small or all great, or all of any other intermediate size the sensation of the sound produced by them will perhaps nevertheless be smooth and uniform but different in gravity only. when one undula is performed in the same time as any two others the octave is produced by such two undulae. now the times of these undulae are as the sizes &c of their causes and hence there are seven points between the two extremes of the octave including the first which are more marked than the rest on account of the simplicity of their ratios &c. and which

(20)thus constitute the seven primitive sounds gradually rising above each other in intensity of which the principle third and fifth are the chief. with respect to the quantity of sound or its loudness, this depends upon the number of vibrations acting at the same time and also, upon the force with which they strike. (13)
The analogy between sound and light is still more striking, the degree of intensity between these two modes of sensation being exactly the same, while the analogy between brightness & loudness are also equally well marked, thus in the former case there are seven primitive sounds & seven primitive colors and what is still more remarkable as the illustrious Newton has shown in his Optics the breadths of two of the colors in the prismatic spectrum viz the orange & indigo are only equal to about half the others thus corresponding to the two intervals called semitones in the octave, (14) In the prismatic spectrum also there are three colors which seem to be more original than the rest, these being formed as it were by their mixture viz the

13 (cont.) will passed over by them (.) hence the greater the space the greater will be the momentum with which they fall upon the ear &c. [p.19 verso]

14. Vide Opt. Newton Lib. 1. Prop. 3 &c. It is however to be observed as Newton & others have pointed out that the semispaces in the prismatic spectrum do not correspond in place with the semitones in the octave in either of the modes now in common use namely the major & minor. The use of this mode however whose intervals as given by Newton are $1, \frac{8}{9}, \frac{5}{6}, \frac{3}{4}, \frac{2}{3}, \frac{2}{5}, \frac{9}{16}$ and $\frac{1}{2}$ is not impossible, & it is highly probable that if it were duly understood by composers of music that it might be used with advantage. Indeed if my memory does not deceive me I think I have read of strains having been occasionally introduced into their composition in this mode by [the older] some celebrated masters with very fine effect. [p.19 verso]
red, yellow & blue, and in like manner the principle, third and fifth are more strongly marked than the others in the octave which latter therefore may be supposed to correspond with the former &c. Perhaps also in support of the opinion that colors depend upon the state of the superficies of bodies may be addressed the fact that some blind people can distinguish colors by touch.

The analogy between tasting and smelling & hearing and seeing is not so striking (.), however the general analogy above (21)mentioned subsists between them, & I have not the least doubt that if the nature of the former were better understood that a more particular one also would be found between them, thus we know that all the varieties of colors & sounds can be reduced to seven primitive ones and in like manner also it is possible as before observed that all the varieties of tastes & smells can be reduced to perhaps a still less number probably only to two or three at furthest which may perhaps correspond to the three more marked colors & sounds before mentioned &c.

Much more might be said on this curious and interesting subject at present however I shall content myself with what has been advanced and merely close the subject with some general observations upon the whole.

It will perhaps be objected to the above hypothesis that it does not make us a whit the wiser respecting the real nature of sensation and that we may just as well suppose tasting smelling &c to be distinct sensations as only modifications of the same. whatever foundations there may be for this objection, yet at the same time I must confess that I cannot help flattering myself that it is not entirely without its use for while it concentrates our attention upon one object, and teaches us to believe that in all the instances above mentioned we see only modifications of that object and not new ones, the comparison &c of these may [lead] induce us [at some future day] to form a more just notion of its properties, and thus at some future day lead us to the
discovery of its real nature independently of this however
the beautiful simplicity which the idea presents of the oper-
ations of the author of nature, and the coincidence which in
this respect it points out as existing between this and the rest
of his works cannot fail to recommend it to the attention, and
I may here observe that it was chiefly this that led me to
prosecute the subject on the present occasion.

I may also observe that it was no small gratification to
me to find in Mr Harris's "Philosophical Arrangements" so often
referred to the following striking passage corresponding exactly
with the same idea. "Tis more than probable" says he "that from
the variation in these universal and as I may say primary forms
(extension figure and aggregation, or as I term them extension
& hardness (15)) arise most of those secondary forms usually
called qualities sensible & such are roughness and smoothness,
hardness and softness, the tribes of colors, savors and odors
not to mention those powers of character more subtle the powers
electric, magnetic, medicinal &c." (16)

If I am asked how or why the various states of the same
aggregated matter produce roughness to the touch, taste to the
tongue &c. between which the analogies are so very imperfect &
remote as scarcely to be discernible, I must at once reply that
I know not. all I know is that the same variety of matter in
innumerable instances is capable of producing by admixture or
combination with another variety of matter in different propor-
tions & only modifications equally striking in their apparent
forms & qualities (,) as for example oxygen and nitrogen, and
from knowing this circumstance & from the analogies and reasons-
ings above mentioned I have been induced to adopt the opinion as
it were implicitly without being able to comprehend its nature.

15. See note Page 3
16. Cap. 5 p. 88-9
fully or to explain the reason of it.

(23) To enter further upon the field of speculation which this hypothesis lays open would be here quite out of place. I shall now therefore put a period to it, sincerely hoping that upon the present occasion it will meet with the approbation of my Professors.

Notae quaedam

I here subjoin some notes as explanatory of certain circumstances mentioned in the essay which could not have well been introduced into it in their proper places without causing confusion.

(Note A Pag. 10) De calore &c. With respect to the sensation termed heat it may be observed that it differs so much in its nature &c from those modifications of it we have mentioned, that in the strict sense of the term it perhaps can scarcely be called a sensation. A certain degree of free caloric we know to be necessary to support that action continually going on in the bodies of sentient beings and which action is necessary to the keeping up of that existence of that combination between the living principle and matter which constitutes their beings. We know also that one of the chief objects effected by this action (particularly that of the vascular system) is to separate that very free caloric by which itself is chiefly supported, thus forming as it were a perpetual motion in the true sense of the term, but how does this free caloric operate in supporting this action of the vascular system by which itself is principally separated? No doubt in two ways viz. as free caloric in preserving the fluidity of the blood and other fluids of the body, and as affording a stock to be always ready to enter into combination, as latent caloric with the nervous system through the means of which alone it can act upon the vascular

17. [A note has been deleted here successfully by gumming over with paper.]
one &c. If this combination of caloric from any affection of the nervous system takes place too rapidly with it, that is to say more rapidly than it is extricated by the vascular system the unnatural abstraction of free caloric by producing (text damaged) will cause a corresponding sensation termed cold or chilliness in the neighbourhood of such nerves, while this same superabundant quantity of combined caloric when it arrives at its proper place &c of action upon the vascular system will prove an extraordinary stimulus to it, and thus increase its action and of course the extrication of more free caloric than natural from which excess of stimulus a sort of sensation termed heat will take place. It is to be observed however that neither of the above are generally, at least at first apprizable by the thermometer. In the same manner also when free caloric is abstracted or added to the bodies of sensible beings ab externo, do the same phenomena take place with this difference only that besides the above mentioned species of sensations from excess or defect of stimulus taking place, such abstraction or addition as in common matter is readily indicated by the thermometer &c. Thus in the present instance it is probable that when caloric is abstracted from any part of the body ab externo that a very rapid combination of caloric takes place in other parts of it with the nervous system (thus producing the sensation of cold over the whole body by sympathy) by which when it comes to act on the vascular one the latter is excited into extraordinary action in order to supply the defect —or vice versa—when caloric is added to the body ab externo the combination with the nervous system in other parts of the body is diminished &c (thus producing the sensation of heat by sympathy over the whole body) from its not being required (?..?) free caloric & that these two acts keep up that equilibrium often(?.?) (by the assistance no doubt however of other means) which is found constantly to exist in the body in a state of health &c. The sensation called (22 verso) heat therefore is produced by the presence of more free
caloric in the body than is necessary to support and carry on
the functions of life, while the difference between this spec-
ies of sensation and true sensation is chiefly in the nature
of their causes, the former existing in every part of the body
itself and the latter only acting by contact ab externo &c.

(24)(Note B.-Pag.12 &c.) De gustu &c. Nothing is more frequent in
common language than to confound taste with flavor, which appear
to me to be modifications of sensation altogether different from
each other. I shall therefore make a few observations on them
as also smelling with which the latter is intimately connected
also, with the view at the same time also of explaining what I
have before advanced more particularly.

Taste is that modification of sensation which is caused
by the contact of certain substances soluble in water or saliva
with the tongue, the nostrils being at the same time closed and
the tongue not being in contact with any other part of the mouth.
Upon making exp'ts. this way it will be found that many substanc-
es usually said to have such and such tastes have really little
or no taste at all or at least are very different from what is
generally believed. Thus for example it will be found that even
the nutmeg and other aromatics have little or no taste as all
that can be perceived upon their application to the tongue is a
little pungency only which may be supposed to be occasioned by
their stimulant properties in general.

After numerous trials made with various substances I have
been inclined to believe that the number of distinct tastes is
very limited (.). at present however I cannot pretend to mention
their number &c. but it appears to me that the class of saliva
tasted is the chief, which includes the acid and the alkaline.
there appears also to be a bitter taste, and perhaps one or two
more, the nature of which from their imperfect states I cannot
define.

(25) Substances which do not by their stimulant &c properties
excite sensation by contact with other parts of the body
deprived of the cutis or in other words do not act chemically upon these parts do not in general appear to be capable of exciting taste, and hence it is that saline matters form so large a proportion of tastes, as they are known to act more readily this way than most other substances; and here I cannot help observing how strongly this seems to point out the close connexion between taste and common sensation, and to shew that they are only as it were one degree removed from each other; while taste therefore seems to be so little different from sensation, it is at the same time also as before observed the most limited and imperfect of all the modifications of sensation, what is usually denomimated taste being really flavor (,) a modification of sensation which seems to be formed by the combination of taste and smell or in other words to be of a nature intermediate between them. hence persons who have lost the power of smelling have been said also to lose that of tasting. that is of flavor mistaken for it, while it is probable that in many instances the power of real tasting remained though from its imperfect nature it almost escaped observation. 

As to the nature of the action of the tongue as a machine adapted to the mechanical &c properties of aggregate matter in its fluidiform condition and thus rendering it capable of producing taste I can advance nothing with certainty. probably however it is chemical or galvanic. Smell is that modification of compound sensation which is excited by various sorts of matter either in an aeriform state or in a state of extremely fine mechanical division when these are drawn in with the air through the nose, like the tongue the nose is also lubricated with a fluid of its own secretion which besides moistening it serves also no doubt some other important use. Smelling seems to be entirely independent of tasting though tasting or rather flavoring be not of smelling—indeed smell may be influenced by flavor as the following expt. seems to shew. when strong distilled vinegar is held in the mouth so that its flavor be strongly perceived it will be found that ammonia
can be held to the nostrils without its stimulant effects being felt in any very conceivable degree. does this depend upon the two neutralizing each other? The reverse however of this expt. does not succeed as ammonia has little or no flavor more than any other salt.

[There is also another curious fact respecting smell which must however be]

(26) As to the nature of the action of the nose as a machine it is probably like the tongue either chemical or galvanic. Flavor This is a modification of sensation which as before observed appears to be produced by the union of the two last though it has generally been confounded with taste or at least the two terms have been often indiscriminately used for each other. Nothing more is requisite to the production of this modn. of sensation than that substances be applied to the different parts of the mouth by means of the tongue, such substances being at least soluble or partly so in the saliva. it is to be observed however that those substances in general have the sharpest flavor that are volatizable or partly soluble in air as well as in water such particularly are the volatile oils &c. Ammonia however is an exception to this which though it has a very strong smell yet has very little flavor than similar saline matters. The principle seat of flavor appears to be the palate, fauces, back part of the nose & perhaps the pharynx & upper part of the cesophagus. The existence evidently depends more upon the nose than upon the tongue (though both are essentially necessary to its production) as appears by stopping the nose with the fingers when it immediately ceases -and what is still more remarkable inspiration does not seem to be necessary to its formation, as flavor is usually strong during expiration as inspiration either through the mouth or nostrils -though neither of them be going on, whereas smelling is chiefly found during inspiration &c. From all this therefore I think it may be finally inferred that flavor is very distinct either from taste or smell though it is
by no means an easy task to point out in any satisfactory manner in what this difference consists. (18)

(Note C. Pag. 10) De aure &c. The ear is a machine of such a complicated and delicate structure as almost to defy the skill of the most expert anatomist to demonstrate its parts without at the same time destroying their texture. The physiology also of this organ is very obscure & hither to no satisfactory theory of its action ............ has been advanced. were I to give my ....... I would say that the ear is a machine ........... -ties of sounds in the same manner as the ................ harmonies may be classed among the ...........

18. There are two unreferenced, rough side-notes on the verso of p. 25:  
"Sweet & sour taste"
"Flavors are more(?..?) than tastes or odors"

19. The manuscript ends here; damaged portions of the manuscript are indicated by the dotted lines.
APPENDIX 7

Transcriptions from Prout's Lecture Notes of 1814

1. The first lecture: 13 pp. in neat hand which appears to be the final version of the lecture. The versos contain different matter.

(1) Introduction to a Course of Lectures on Vegetable & Animal Chemistry

The department of the comprehensive science of chemistry which it is the professed object of these lectures to treat, is what is usually termed Vegetable & Animal chemistry. These terms are employed to distinguish it from chemistry properly so called whose objects & agents are inanimate, i.e. not necessarily connected with veg.ble or animal life, while on the contrary the department under consideration professes to treat of the elements & operative qualities constituting animals & vegetables.

These two grand departments of chemistry viz. of inorganic & of organized matter gradually run into each other so that it is impossible to draw the line of demarcation between them; as we proceed however towards the latter the subject gradually becomes more intricate and difficult, & this is the more unfortunate because it becomes more interesting in the

(3) same or even a greater degree. Many circumstances conspire to produce these difficulties but the chief is the mysterious nature of the agents operating & which we only hitherto know by their effects. these agents or elements which thus distinguish organized from inorganized matter have puzzled the philosophers of all ages & have acquired a variety of names - many of them absurd & of no meaning at all, others expressive of a provisional character either real or fancied. for my own part I shall be content with those which are best known & which are not likely to convey erroneous notions. I shall call them the living veg.ble & animal principles, and therefore define them to be those principles or qualities which exist in veg.bles & animals & which have the power of appropriating inorganized matter to their use & of assimilating
it to their own nature—that is rendering it organized.

(5) From what has been said my audience must be aware that a
previous acquaintance of common chemistry is absolutely requisite
before this department be entered upon. taking this
firstly, therefore for granted at least for the present, I shall proceed at once to develop
the plan which it is intended to be pursued in the treatment of
our subject.

This diagram which is intended to convey a general outline is
to be thus understood. The objects of nature, either as actually
known to us, & known by their effects only I divide into two great
classes which are denominated Elements & Agents. between these two
I do not pretend to draw a distinct line, but suppose them to run
into each other or in other words that every element is more or
less an agent & v.v. those however which from their characters
being stronger marked appear better than others to deserve these
appellations are placed at the extremities of the scale.

(9) while the others are placed between them according to their suppos-
ed nature. here I endeavour to express my meaning by the two
triangles drawn [the scale] the apex of the one opposed to the
base of the other, the application of which is so obvious as to
need no comment.

The first part of the table comprises the elements & agents,
one or more of which enters into the composition of everything we
see around us; these are doubtless much more numerous than the
simplicity of nature's operations requires, but in the present state
of our knowledge we must admit them, or launch into the ocean of
hypothesis, which the present strict modes of philosophizing very
properly forbid.

In the second part are enumerated the chief of what are termed
the proximate principles of veg bles & at the other extremity of
the scale is added a new agent—the living principle of veg bles—
by the powers of which the

(11) operations of the other agents are variously modified, & the
features of organization stamped upon the results.

In the third part are the proximate principles of animals &
at the other extremity is a new agent—the living principle of animals in which their peculiar organization & properties are supposed to depend.

I have before said that it is impossible to say where common chemistry ends & vegetable & animal begins. the same is true of vegetable & animal, yet at certain points they become distinctly marked, so that no one will mistake a stone for a tree or a tree for a man;

I wish also to be understood to mean that the vegetable living principle is connected with, or a modification of galvanic agency & that the animal living principle is in like manner connected with the vegetable & consequently with galvanism—or in other words that they gradually run into one another like their chemistry, from which I think this is a fair indication. proofs of this would be irrelevant here & hence I reserve them till I treat of these principles.

On the other hand I still think, as it happens with the Chemistry that at certain points they are distinct, that is that there is a distinct vegetable & a distinct animal living principle—further also, that as many of the processes carried on in vegetable & animals are purely chemical, & that as many of the processes in animals are vegetable, that it is possible that in animals both vegetable & animal living principle may exist independently of each other, & this is agreeable to the notions of the ancients who talked of the vegetable & animal souls.

Thus then it happens that the more powerful [animal]being is the more complicated as requiring the joint labor of all the agents & principles below itself.

Experience teaches us that knowledge is progressive. this is remarkably the case with Chemistry which by combination or separation can form agents still more powerful than it before possessed, witness Sir H. Davy's brilliant discoveries; and who will say when this will end. may we not hope to hunt down nature as it were who has got the start of us, & pursue her into her very laboratory?

Let us indulge for once in speculation & apply this principle of progressive improvement to the operations of nature herself as
we learn them from inspecting her works. What was the original material of this globe we inherit, whether water as some suppose, or whether anything else I shall not stay to enquire—all seem to agree that minerals are the oldest of its component parts—that when these were first formed plants & animals were not. the production of minerals was a step gained—minerals afforded the pabula

(17) or principles of plants the next order of created beings, the same office that minerals performed to plants, plants performed to animals, the last & most complicated of nature's works. now it is evident from the present grades of things that this must have been the order, for plants could not have existed without minerals nor animals without plants, perhaps a future race of beings still more refined may be brought into existence at some future time.

It must be remembered that the operations of nature are upon a large scale & when measured by the evanescent existence of mortals appear infinitely slow—myriads of ages therefore might have elapsed in the completion doing of what I have mentioned, & myriads yet to come may be too short for the completion of her works.

It has been the opinion of some philosophers & many circumstances conspire to support it that organized beings are the subordinate agents employed by Nature in the execution of her

(18) great plans—may some have gone so far as to say that this plan consists in the gradual change of the water of which they suppose this globe to have originally existed into solid matter, that the waters of the globe diminish & solid matter increases seems to be an opinion sufficiently supported by observations & facts, & there can be little doubt also but that veg. & animals who have the power of converting one species of matter into another, contribute in a great degree or in other words are very extreme (?) agents in this business.

(half page cut away, and two following sheets missing)

(25) The first of these suppositions may be thus considered. we will suppose an active & a passive principle of such qualities as to have the power of neutralizing each other, we will suppose
that this has happened—that is, that a portion of the one & a portion of the other have thus entered into combination & that all action has ceased between them. The result will be a tertium quid having all the properties of an element with the original active principle & all the properties of an agent with the original passive one. We will suppose that this tertium quid has combined with another portion of the original agent so as to neutralize each other the result will be a second tertium quid with two parts of agent & one of element. The like may be supposed to happen with the first tertium quid & the original element & in this case we shall have a third new principle or element containing two parts of the element & one of the agent. And in this manner upon principles strictly chemical may elements & agents of different capabilities be multiplied without end, and that originally from two principles only.

This explanation as before observed is exactly consonant to our improvement in knowledge & especially chemical knowledge. In this manner the gradual development of nature's plans may be explained as before attempted. First minerals, then plants, then animals each affording the means by which that above him is supported, and the last and most complicated depending upon & thus including within itself all the preceding. In this manner too, to compare small things with great, may we account for the numerous immediate principles or elements of plants & animals—may we account for the eternal action going on in them, by supposing that the different agents & elements have different capabilities of acting & being acted upon, that the mechanical operation going on in plants & animals are the means by which these different agents & elements are brought into each other's sphere of action & that in this manner

(29) new combinations are continually forming &c. When the mechanical operations are fixed and limited as in plants & animals there will be a regular routine of actions & changes which will stamp a particular character on the individual plant or animal to which they are proper, & in this manner may the numerous varieties of
2. The versos of the above sheets are in bad handwriting; their chief importance is that they show that Prout had read Humphry Davy's *Elements of Chemical Philosophy*.

3. Draft of the first lecture, labelled "Synopsis"

Two quarto sheets folded into 8 pp. Watermarked 1810

**Synopsis**

**Lecture 1st**

**Introduction**

Brief account of the objects of nature—their development—mutual differences & relations—these the proper objects of chemistry. Of Chemistry in general its origins & progress. Of the origin & progress of veg. & animal Chemistry, its great importance & difficulty.

**Plan of the course**

Professed object—the chemistry of vegetables & animals—explanation & the impossibility shewn of treating of these subjects alone, without considering the objects of nature & chemistry in general.

The objects of nature may be divided into Elements & agents or the passive & active principles (corresponding to the pos. & neg.). Ancient & modern conjectures on the nature of the ultimate passive & active principles—general coincidence these in the notion of their being but of one kind—that their is but one passive & one active principle in nature from which all others are deduced & modified. Probability of this hypothesis & the obvious & necessary conclusions drawing from its admission viz. First that ordinary or compound elements & agents can differ from one another in degree only, i.e. according as the active or passive principles predominate in their composition & secondly, that the more active principles can act over all the less active ones.
According to this view every object in nature as we see it must partake both of the nature of an element & agent, i.e. it is passive with regard to the more active ones & v.v.

Usual division of the objects of nature into Mineral vegetable & animals adopted - Tabular arrangements of the proper objects of each of these [kingdom] divisions according to the above notion.

(3) General expalanation of these tables

In each of these tables two principles or elements and an agent are chosen & placed at the extremities of the scale & are supposed to be proper & characteristic of the division. Thus in the first table Metal, of which hydrogen is considered the most characteristic & pure is placed as the primary element -& galvanism with its modifications as the ultimate agent -between which two lie all the other elements & agents of this division.

In the second table Carbon is introduced as the characteristic element of vegetables & the vegetable living principle as its characteristic agent. this table also includes all the mineral principles above mentioned.

In the third table -nitrogen is placed as the characteristic element of animals & the animal living principle as the corresponding agent. this table also includes all the elements and agents of the two preceding ones.

General observations on this arrangement. arguments drawn into its favor from a contemplation of the existing economy of nature -from geological facts &c. Speculations on the nature & development of the characteristic elements & agents of each division.

(5) Particular mode of treating the subject will be in general, that pointed out by the tabular arrangements -the objects, & laws of action of Mineral or common Chemistry will be first briefly considered & no further than absolutely necessary -it being assumed that the audience in general is acquainted with this necessary preliminary knowledge.

Division of Metal -Hydrogen. Of Oxygen & chlorine -of Caloric
First -of Light. of the galvanic -electric & magnetic fluids
or agencies, of obvious compounds - oxides - water - potass - soda oxide Iron - muriatic acid - muriate soda (Water one of the oldest, before vegetables)

General account of the other compounds, of their mode of accretion.

Crystallization.

Division Of vegetables in general [of their mechanics or anatomy]
Second Of their external characters & phenomena compared with those of minerals. Of their internal construction or anatomy. [Conclusion from these that there also exists in them another & more powerful agent than exists in minerals] development of another chemical principle common to them all, viz. Carbon.

Proofs also of an agent acting in them superior to any of those of common chemistry.

of carbon, of the operation & modes of uniting of the elements & minor agents of common chemistry, of Hydrogen. Water. metallic oxides - muriate soda, of oxygen chlorine of caloric & Light, of the galvanic - electric & magnetic fluids, of the agent proper to vegetables or the vegetable living principle.

Of their mode of accretion or growth compared with that of minerals.

Of the results of the operations of the above elements & agents & of the products of the proximate principles of plants.

(7) Of the Proximate principles of plants in general, of their compound nature, their easy distributability arising from their being in a forced & artificial state with respect to the elements & agencies of common chemistry which prevail by their quantity, & perhaps even modify the actions of veg. living principle itself. hence when deprived of those living principles by the series of actions which evolve it being broken they are decomposed.

General arrangement will depend as far as possible in their being more or less agents or elements i.e. positive or negative. Hydrogen the element oxygen agent.

Hydrogen carbon oxygen
water
ether inflammables

<table>
<thead>
<tr>
<th>Ether</th>
<th>Water</th>
<th>Carbon</th>
<th>Oxygen</th>
<th>Acids</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>inflam</td>
<td></td>
<td></td>
<td>acids</td>
</tr>
</tbody>
</table>

Proofs also of an agent acting in them superior to any of those of common chemistry.
Vegetable substances containing the characteristic element of animals, viz **nitrogen** will be afterwards considered.

Division General observations on animals—of their external third characters & phenomena compared with those of veg. & minerals. of their internal constitution or anatomy & development of another chemical element common to them all viz. nitrogen. Proofs also of an agent acting in them superior to any of those of veg. or minerals.

of **nitrogen** of the operations & modes of acting of the minor agents of common & vegetable chemistry. Of Hydrogen. Water. Metallic oxides. Muriatic soda oxygen chlorine. calcium light galvanism electric & magnetic fluids. (Ammonium? Is it an animal product or was it made before animals?) Veg. living principle. Of the agent proper to animals the animal living principle.

Comparison of their modes of accretion or growth compared with minerals & veg.

(Ms. ends here; versos contain unrelated notes)

4. Another draft of the First lecture

Foolscap scrap of 4 pp. Watermarked 1810

**Primary Elements & Agents**

Hydrogen carbon Nitrogen oxygen galvanised current Electricity

Magnetism


3 Earths gum
1 Metals sugar &c
2 Sulph &c
Acids Acids
5. Loose leaf offering commentary on a table similar to above

From an inspection then of this table it will be seen that the most objects or elements which form the object of common chemistry are Hydrogen Metals Phosphorus Sulphur Oxygen & Chlorine, principle of attraction either identical with which (?) or m... of it are supposed to be the galvanic electric & magnetic agencies & light & heat. I have omitted Carbon and nitrogen because it is known that these belong more exclusively the one to the vegetable the other to the animal kingdoms & hardly indeed are found out of it. And as we shall endeavour to make it appear hereafter are generated or actually produced by the objects in these kingdoms. Now it is an important fact that all the objects strictly or truly belonging to the mineral kingdom are either one or other of these elements themselves or compounds of two or more of them, that is to say they are compounds of one more positive or active than another, the most active one being very frequently oxygen.

With respect to the laws which regulate the combination or rather which they appear to follow in them, it has been one of the greatest ...... of modern times to shew, that they are extremely simple & are founded upon principles primarily mathematical. This happy generalization which was first distinctly f.... at by Mr Dalton & subsequently enlarged upon & improved by some other prominent chemists in this country among whom Dr Wollaston and Dr
Thomson are the chief ....

6. Other lecture material includes notes on albumen, fibrin or coagulable lymph, the colouring matter of the blood, urea, curd, respiration.
**APPENDIX 8**

**Prout's Polarity Theory**

(From a bound quarto exercise book, undated, in an envelope titled "Prout Lewis papers" in the possession of Lt.Col. Warner. The text only occupies the first dozen pages, the rest of the book being unused. There is no pagination. Propositions 1-3 are found in the Bridgewater Treatise of 1834; and the whole text is paraphrased in proposition form in the 3rd edition (1845) of that treatise. The reference to Mosotti dates the manuscript from 1837).

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**Assumptions**

It is assumed,

First. That every portion of matter attracts, and is attracted by every other portion of matter according to laws which have claimed universal assent.

Secondly. That all matter, as it is known to us, exists in the condition of molecules, which molecules we consider to be virtually spheres or spheroids.

Thirdly. That all the spherical or spheroidal molecules, when unimpeled, have a tendency to revolve on their axes, with velocities, which in molecules having the same weight, are under similar circumstances fixed and definite; but which velocities, (according to a law which need not be here specified) /[/ to be hereafter investigated]/, increases in molecules of different weights, as the weights of the molecules diminish.

Fourthly. That the axes of contiguous molecules of bodies in the solid state are arranged parallelly & symmetrically as in \( \text{E} \); that in the liquid state the axes of contiguous molecules are arranged at right-angles to each other as in \( \text{E} \).
and that in the gaseous state the axes of contiguous molecules are arranged parallelly & inversely, as in \\

(2) Of a single globe in motion; and of the relations of two globes in motion to each other.

In considering the revolution of a globe, our especial notice is claimed by the line, or axis on which the globe revolves, and by the two extremities of this axis -- its poles, as they are termed. The axis and the poles of a globe are particularly distinguished by being absolutely at rest while every part of the globe is in motion. Hence in considering the relations of two globes in motion to each other; we shall in the first place consider the relations of two equal globes with reference to their axes; and afterwards with reference to their equators; and secondly the relations of two unequal globes with reference to their axes & afterwards with reference to their equators.

1. Of the relations of two equal globes in motion with reference to their axes; and with reference to their equators.

Two globes of equal size and revolving with equal velocities, in free space if attracted by each other would naturally arrange themselves so that the axes of both should be in the same straight lines in which position the two globes would revolve together. For the axes and their poles being at rest; the attractive forces would operate as far as their points are concerned precisely in the same manner and degree as if the two globes were wholly at rest.

We have next to notice the equatorial relations of two equal revolving globes. If we suppose two globes of equal size and that then two globes being close together, and having their axes parallel, revolve with the same velocity, and in the same direction, the surfaces of their globes will move in opposite directions. Any effect therefore that may be produced by the motion of the surface of one of two globes revolving in the same direction is
exactly counteracted by the motion of the surface of the other
globe. Hence two contiguous globes of equal size revolving with
the same velocity & in the same direction & being contiguous at
their equators have a tendency to stop the revolution of each
other; and to produce a state of rest, which may be considered
favourable for their exerting that mutual attractive force which
according to our first assumption every particle of matter exerts
on all other matter. In consequence two such globes so revolving,
will arrest each others revolution & will cohere.

On the other hand if two globes of equal size revolve contigu-
ously and with the same velocity but in opposite directions, in-
stead of annihilating the velocity of each others revolution, they
will / continue to revolve in consort so that their cohesion will
be impossible. Besides if the substance of such contiguous globes
revolving in opposite directions be fluid, on account of the great-
er energy of the centrifugal force at their equators, the equato-
cial diameters of both globes will be elongated. The two globes
will thus become oblate spheroids and will recede still further
from each other.

2. Of the relations of two globes differing in size and velocity
with reference to their axes, and with reference to their
equators.

As in the case of globes revolving with the same velocity two
globes revolving with different velocities may be supposed to
come into contact by gravity at their poles. But the effects of
the contiguity of globes revolving with different velocities will
depend on the actual & on the relative magnitude of the globes. If

* Of course the poles are the only place where gravity can act.

But as gravity can act at the poles; in the case of a infinite
large number of molecules ...... the sum of attraction will be
greater at the ......... than the sums of repulsions by an almost
infinitely small quantity, that is to say by the amount of an
infinite number of polar points of attraction. This fulfils the
........ of Mosotti's hypothesis.
two globes having appreciable ...... & differing but little from each other in weight and velocity revolve contiguously to each other, their axes being in the same line, they may be suppos-
ed to cohere & then perform a revolution resulting from their combined velocities. If the two globes be infinitely small; accord-
ing to the established principles of attraction, their mutual attraction except when infinitely small will likewise be infinitely small while their velocity will be infinitely great. Hence though two globes of extreme tenuity and revolving with extreme velocity may be supposed to assume in .......... at their poles and remain attracted axially; their mutual attraction being likewise extreme-
ly small they can scarcely be supposed to cohere or ...... ...... . So also if one of the globes be infinitely less than the other, the two globes may be supposed to remain in contact at their poles

without actual cohesion, though their respective motions & tendency to cohesion will influence each other; the larger globe may stand still while the smaller globe may continue to revolve, but with diminished velocity; or both may continue to revolve, with different but diminished velocity. But no two revolving globes whatever may be the velocity of their revolution can be supposed to remain in contact equatorially, that is to say, at right angles to their axes of revolution, unless they revolve with exactly equal velocity, for these revolving motions can counteract each other only when their velocities are equal.

Application of the preceding observations to the Molecular Const-
ituation and Phenomena of Matter.

All the matters with which we are acquainted are divided by chem-
ists into two classes; the imponderable and the ponderable; that is into those which under no circumstances have any sensible weight, or exert any attractive force; and those which do possess weight, in other words attract and are attracted. The imponderable matters are light & heat (including electricity and magnetism). All other

substances belong to the class of ponderable matters. Now accord-
ing to our hypothesis the molecules of light and heat must be inconceivably less, almost infinitely less than those of any ponderable body; and equally the rapidity of the motions of these imponderable matters must be inconceivably, almost infinitely greater. Hence from this great rapidity of motion & from their infinitely small attraction imponderable bodies are little disposed to cohere together into sensible masses like ponderable bodies, at least per se, or without some conjunction with ponderable bodies. On the other hand the molecules of ponderable bodies being much larger & having less intensity of motion readily combine with each other axially; which molecules of the same size & having similar motions, when their axes are parallel & symmetrical totally arrest each others motion & cohere equatorially into masses; thus giving occasion to the solid form of bodies, in which the molecules of which they are composed are in a state of absolute rest. If the motions of the molecules contiguous to each other are too intensive to allow of their stopping each other but at the same time not so intensive as to cause them to so arrange themselves as to confuse with each others motion, such molecules may be supposed to have a natural tendency to assume that immediate position in which their axes are at right angles (or approximate to right angles) to each other and in which their motions will not interfere either to arrest or to corroborate each other. molecules / so situated may be supposed to exist nearly in contact, but at the same time to have free motion among each other -in short to constitute the liquid form of bodies. But if the motions of the molecules contiguous to each other are too intense to allow of this immediate state of neutrality, and cause them to assume that relative position in which their axes are parallel but reversed & in which their motions instead of stopping, conspire with each other, such molecules will constitute the fluid or gaseous form of bodies & will be mutually self-repulsive.

We have said that the molecules of imponderable bodies are almost infinitely less than those of ponderable matters; they may be supposed though to pervade and operate within and among the
molecules of ponderable bodies & thus not only to influence but altogether to subvert their natural motions. Thus imponderable molecules assume as it were the character of agents, and may in a certain point of view be so considered; for the intensity of the motions of ponderable bodies as they exist around us, depend apparently altogether on the presence or absence of the molecules of imponderable matter.

(8) The preceding assumptions and observations naturally lead to this next & general conclusion; that no molecule naturally exists alone & independent, but that every individual molecule has relation to some other individual molecule having similar properties & motions. Hence that all aggregates of molecules must consist of aggregates of

\[ \text{Fluid} \quad \text{Solid} \]

such binary arrangements and not of single molecules. In solids (crystalline) all the axes of the molecules are parallel & symmetrical & thus any two similars may form a binary arrangement. In liquids the axes of every contiguous two molecules are at right angles; while in fluids the axes of every contiguous two molecules are reversed, but parallel. The consequences of this arrangement then are that in their natural state the motions of every molecule is [sic] exactly neutralized by the transverse, only the opposite motions of its antagonist molecule; so that its motion as such is no longer apparent, but the resultant effects of the binary arrangement only are apparent. The motions of these binary arrangements of imponderable molecules when symmetrically opposed constitutes what we term [heat] caloric; while similar motions of the binary arrangements of ponderable molecules themselves constitute sensible heat.

But what is the consequence of a number of molecules moving together in the same direction without their motions being
neutralized by their antagonists?

When a number of molecules move together in the same direction the first necessary consequence from the above binary arrangement, is that an equal number of / molecules must move together in the opposite direction. For the parallel & symmetrical molecules of solids being fixed & immovable they can never be supposed to fulfill the conditions of the supposition; which consequently can only be fulfilled by molecules free to move as they are in the liquid & more particularly in the fluid state in which alternate molecules are opposed to each other; and in which form consequently as above stated no number of molecules can be supposed to move together in one direction without a similar number of molecules being detached are made to move together in the opposite direction.

When a number of molecules come together in the same direction they constitute what is called sensible polarity; & give occasion to the phenomena of electricity and magnetism as follows.

\[ \text{Fig. 1} \]

\[ \text{Fig. 2} \]

Let us suppose a number of molecules to be arranged symmetrically (as in the .... Fig.1) as they are supposed to exist in fluids. In this form as before observed they exactly counterbalance each others motions & nothing of polarity is perceptible; but let us imagine them by some agency to be brought into the relative position Fig.2 it is obvious that the terminal molecules $Ee$ and $E'e'$ of each extreme of the series will move together alone, and in opposite directions. Thus giving occasion to opposite & equal (10)polarities. Sensible electricity according to this view of the
subject may depend on the motions of any molecules, whether imponderable or ponderable the intensity of the polarity depending upon the size & velocity of the molecules (that of the imponderable by the most intense & ....... electricity &c) while the quantity of polarity may be supposed to depend upon the greater or less number of molecules in motion at the same time. Thus the sensible polarities resulting from the motions of oxygen or water are of much less intensity than that resulting from the motions of molecules of heat & light which appear to principally constitute as we have said, common electricity & magnetism.

Galvanic electricity. The peculiar properties of Galvanic electricity seem to depend on the intensely excited action (by ordinary electricity of the molecules of oxygen & hydrogen, &c.

In describing the relative motions of two globes to each other we formerly observed that the line of their axes was the only direction which their naturally attractive forces could operate on each other without being interfered with by their motions; & that such natural attractive force could only be exerted between other points except when they were at rest; which state of rest could only be produced when the motions of contiguous molecules were equal & their axes symmetrical & parallel. Now the force naturally exerted by molecules towards each other in the line of their axes we consider to be the cause of (to be, in short) chemical (11)attraction, & to be / what is properly called electrical attraction; while the self same force occasionally exerted during the rest of the molecules between other points than their axes of motion to be what is denominated the cohesive force, which can thus be only exerted under peculiar circumstances; while the phenomena resulting from the equatorial motion of imponderable molecules in particular produce what we term sensible magnetic phenomena; and the motion considered relatively to the axis (the pole being in the centre) constitute the phenomena of sensible electricity.

The attractive & repulsive phenomena of common electricity and magnetism as usually observed & understood may be supposed to result from the motions of the molecules as follows.
Electricity. We have said that the sensible exhibition of polarity depends upon what may be considered as a forced or unnatural separation of the molecules of fluids from their antagonistic & balancing molecules \( E' \) and by which they are made to move together alone. Now the immediate tendencies & effects of such a state may be considered of a threefold character. First the natural tendency to restore the balance of the motion; that is to combine with the antagonistic molecule \( E' \) which must be moving together alone in the opposite direction in the neighbourhood. This tendency though different may be supposed to act in conjunction with the electrical or chemical attraction properly so called before mentioned. Secondly. When a number of imponderable molecules move together in the same direction, (or when a number of ponderable molecules move together in the same direction with velocity far beyond those natural & proper to them), they cannot of course cohere, & from the interference of their motions the molecules naturally repel each other & separate by a sort of centrifugal force. Hence bodies similarly electrified, i.e. having similar motions, repel each other and produce what is called electrical repulsion. Thirdly. From the tendency of the isolated molecular motions above mentioned to seek antagonist or rather to separate & to induce them, in contiguous molecules, the contiguous molecules \( E' \) pass more or less abruptly into an opposite or reverse state of motion, & thus give origin to opposite, or what are denominated, returning collateral currents & these currents again to others &c.

collateral original collateral

It being always remembered &c that collections or groups of similar molecules act as one.
in other words to all the phenomena of what is termed, electricity & by induction.

The phenomenon of magnetism & the distinction of electricity from it may be thus explained.

Magnetism is the visible effect of the equatorial motion of molecules moving on their axes when the contiguous molecules are separated & exhibit the peculiar disposition above mentioned continually producing the sensible phenomena of electricity.

(13) A magnetic needle when brought into the vicinity of an electrical current, arranges its poles transversely to the direction of the electric current the north pole being to the right or left according as the motion of electricity is in this or that direction. (fig. 1) And it is to be particularly observed that the magnetic needle to exhibit the phenomenon of polarity must be always a chord only (fig. 2) to the circular track (?); if it extends to the diameter (fig. 3) instead of lying in the course of the electrical current it will revolve with it.

The inference from this is that the common magnetic needle must be a chord of some extensive circular current the electrical current producing which must be at right angles to the circular current & passing through its centre. Moreover in order that such a needle shall be influenced by such a disposition of the currents, there must be induced in it (the needle) currents as in the margin.
APPENDIX 9

A Note on Prout's Dissertatio de Sonis et Actione Harmoniae Auris humanae, Edinburgh, 1810.

This Latin essay of 39 pp. is in a good hand, and held together by binder's thread. At p. 22, Prout sums up his thesis as follows:

1. All sounds are formed in the ear alone; they are therefore able to exist in living creatures.

2. Tremors making sounds are produced by the tympanum and conducted through the bones of the skull to the acoustic nerves which are situated in the semicircular canals.

3. The tympanum produces tremors by sympathy.

4. Various parts of the tympanum have various grades of tension, so that all the sounds in the octave are included.

5. Simple or distinct sensation of sounds is conceived particularly in the semicircular canals; but the action of external tremors which exist at the same time in the fenestrum rotundum modulate and even control it.

6. The function of the muscles of the tympanum is to stretch its parts more or less, according to the stimulus.

7. The function of the muscles of the stapes is to enlarge or diminish the cavity of the labyrinth, and so give greater or lesser tension to those parts enclosed by it. Prout wonders whether this aids clarity of sound.

I am very grateful to Dr. R.K. French of Leicester University for help with this note.
APPENDIX 10

Published Papers

The following offprints will be found in the sleeve of this thesis.

1. "Prout's Chemical Bridgewater Treatise" (1963)


4. With Dr. David M. Knight: "The Atomic Debates: (Memorable and interesting evenings in the life of the Chemical Society)" (1965)
BIBLIOGRAPHY

The Bibliography is divided into primary and secondary source materials. A complete bibliography of Prout's published papers and books is followed by a list of his manuscripts and letters. Other primary sources have been sub-divided into those which deal with matter theory, animal chemistry, or which are of a miscellaneous character. No attempt is made to cover the literature after 1850. Secondary materials have been placed into four groups: those that deal directly with Prout's life and work, works of a general character, works that relate to matter theory, and finally, materials that relate to animal chemistry. Books are not separated from articles; and unless otherwise stated, London is the place of publication.

1. A List of Prout's Published Papers and Books

(This list is based on the Royal Society's Catalogue of Scientific Papers).


4. "Observations on the Quantity of Carbonic Acid Gas Emittted from the lungs during Respiration, at different times, and


Schweiggers *Journal für Physik und Chemie*, 15,47-76,1815.


9. "On the colouring matter, or ink, ejected by the cuttle fish", *Annals of Philosophy*, 5,417-420,1815.


14. "Observations on the Nature of the Proximate Principles of the Urine with a few remarks upon the means of preventing those Diseases connected with a morbid state of that fluid", Medico-Chirurgical Transactions, 8,526-549,1817.
Annals of Philosophy, 11,352-356,1818 (first half only).
Annales de Chimie, (2)10,369-388,1819 (" ");


16. "Description of an acid principle prepared from lithic or uric acid", Philosophical Transactions, 1818, 420-428.
Annales de Chimie, (2)11,48-57,1819.

17. "Description of an Urinary Calculus composed of the lithate or urate of ammonia", Medico-Chirurgical Transactions, 10,389-395,1819.
Meckel's Archiv für Anatomie und Physiologie, 6,366-372,1820 (not seen).

Journal de Physique, 88,298-311,373-382,1819.
Meckel's Archiv für Anatomie u.Physiologie, 6,78-115,1820 (not seen).


*Annales générales des sciences physiques* (Brussels), 3, 301-302, 1820 (not seen).


*Untersuchung über das Wesen und die Behandlung des Harnriesses, Hornsteins und anderen Krankheiten, die mit einer gestörten Thatigkeit der Hornwerzeuge zusammenhängen*, Weimar, 1823.


31. "Note on Black Urine", in a paper by Alexander Marcet, "Account of a singular variety of urine which turned black soon after being discharged; with some particulars respecting its chemical properties" *Medico-Chirurgical Transactions*, 12, 37-43, 1822 (Procut, pp. 43-45).

*Annals of Philosophy*, 20(24), 71-72, 1823.
Edinburgh Philosophical Journal, 8, 63-76, 1823.
Annals of Philosophy, 21(-5), 100-111, 1823 (with correction).


Annales de Chimie, (2) 27, 36-41, 1824.
Journal de Physiologie, 4, 294-299, 1824.
Tromsdorff's Journal der Pharmacie, 10, 113-120, 1825.
Annals of Philosophy, 24(-8), 117-119, 1824.
Quarterly Journal of Arts and Sciences, 18, 142-144, 1825.


36. An Inquiry into the Nature and Treatment of Diabetes, Calculus and other Affections of the Urinary Organs; with remarks on the importance of attending to the state of the urine in organic diseases of the kidney and bladder; and some practical rules for determining the nature of the disease from the sensible and chemical properties of that secretion, Second "revised and much enlarged" edition, May 1825, 328 pp., with index.
Also at Philadelphia, U.S.A., 1826, 308 pp., with notes and additions by Dr. S. Golhoun.
37. A Synoptical View of Urinary Calculi, undated (May 1825?). A set of engravings by Lunn, with text, from item 36.


39. "Remarks on certain observations made by M. Leuret and Lassaigne, and Professors Tiedemann and Gmelin, in their works on digestion recently published; particularly with respect to the presence of free muriatic acid in the stomachs of animals", Annals of Philosophy, 26:12, 405-410, 1826.

Giornale di Farmacia, 7, 247-260, 1828 (not seen).
Poggendorff Annalen der Physik und Chemie, 12, 263-273, 1827.
Tromsorff's J. der Pharmacie, 18, 238-269, 1829.
Quarterly Journal Science & Arts, (2) 2, 480-482, 1827.


44. Gulstonian Lecture I: "Observations on the Application of Chemistry to Physiology, Pathology and Practice", Medical Gazette, 8,257-265,1831.

45. Gulstonian Lecture II, Medical Gazette, 8,321-327,1831.

46. Gulstonian Lecture II and III, Medical Gazette, 8,385-391,1831.

47. Dr.Prout's Reply to Dr.Philip, Medical Gazette, 8,705-707,1831.


49. Dr.Prout's Rejoinder to Dr.Philip's Reply, Medical Gazette, 8,769-770,1831.

50. Dr.Prout in Reply to Dr.Philip, Medical Gazette, 8,802-804,1831.

51. "Dr.Prout on the Application of Chemistry to Physiology, Pathology and Practice, in answer to certain strictures of Dr.W. Philip", Medical Gazette, 9,38-46,1831-2.


54. Chemistry, Meteorology, and the Function of Digestion Considered with Reference to Natural Theology, the Eighth Bridgewater Treatise, Wm. Pickering, 3 February 1834.


56. Reply to Dr. (W.C.) Henry, Philosophical Magazine, (3)5,132-133,1834.


Ueber das Wesen und die Behandlung der Krankheiten des Morgens und die Harnorgane, Leipzig, 1843.

Also at Philadelphia, U.S.A., 1843.


2. Manuscript material

Letter, Prout to Gilbert, 8 October 1830 (in Royal Institution of Cornwall).
Letter Daubeney to Prout, 27 October 1831 (in Royal Institution London).

Two letters in Royal College of Physicians to unknown correspondents on subject of Stevens's work on blood, c. 1830.

T. J. Prout to Sir Henry Pitman, 2 April 1888 and 5 May 1888, 16 June 1888 and 19 June 1888; also Memorandum March 1902 (R.C. Physicians).

Letter Prout to J. Herschel, 4 March 1827 (Royal Society, HS 14.192 and HS 14.191).

Letter to Chairman of the Physiological Committee, 15 April 1833, (Roy. Soc., MC 2.79 and MC 2.80).


The Wills of John William Prout (proved 1881); Thomas Jones Prout (proved 1909); Catherine Lewis Nicol (proved 1915); Elizabeth Prout (proved 1918).

List of Plate and Pictures lent to Miss (E.) Prout of 44 Gloucester Terrace, H. yde Park, London W. By Mr and Mrs Nicol of 37 Queens Gate London SW.

Papers now in the possession of Lt. Col. F. E. H. Warner:
Horton Rental Book 1665-1741 and Horton Book to Measurement

A Catalogue of Books in Horton Library, 1758.

W. Prout, De Facultate Sentendi, Edinburgh, 1810, 26 pp.

Envelope labelled "Medical (sic) Papers of Dr William Prout" (contains Lecture Notes on Animal Chemistry of 1814, and manuscripts copy of Prout's "Observations on the Analysis of Organic Substances" [item 16 of published papers].)
Drafts of Prout's polarity theory, quarto sheets held together by brown paper band with red seal.

Waste book, 8½ by 13 inches, consisting of abstracts of Prout's reading in animal and vegetable chemistry, circa 1812-1814.


Offprints of published items, 14, 32, 34, and 45-47.

Offprint of A. Fyfe junior, Dissertatio Chemico-Physiologica Inauguralis De Copia Acidii Carbonici & Pulmonibus Inter Respirandum Evoluti, Edinburgi, 1814, with Author's dedication to Prout.

Anon, Prout obituary from Edinburgh Medical J., 76,126-183,1851, with pp. 126-8 missing.

Copy of Prout's Chemistry, 1834, in publisher's cloth, with signatures of Prout, and Elizabeth Prout.

Cardboard file of Dorothy Ada Nicol containing notes on the families of Adams, Lewis, and Prout; also the watercolour of Horton Church by Prout, c. 1803.

Small red notebook of Miss Nicol on genealogy of Allways, Bennetts, Prouts, etc.

Further manuscripts in the possession of Lt. Col. J.W. Nicol of Ballogie, D.S.O., have not been examined.

3. Primary sources on Matter Theory

A. M. Ampere, Lettre à Berthollet, translated, Phil. Mag., 45, 41, 188, 344, 1815.


J. Harris, *Philosophical Arrangements*, 1775.


ibid., 4, 180, 260, 1814.

J. Pelouze, Sur les equivalents chimiques considérés comme les multiples simples de l'hydrogène, Comptes rendus, 15, 959-962, 1842; ibid., 20, 1047, 1845.

F. Penny, Determination of several equivalent numbers, Phil. Trans., 1839, 13-33.


Review of Thomson's First Principles, ibid., 26(-10), 138-147, 1825.

Table of equivalents from Thomson's First Principes, ibid., 26(-10), 293-298, 1825.

Attack of Berzelius on Dr Thomson, Phil. Mag., (2)4, 450-453, 1828.

Researches on chemical equivalents, Phil. Trans., 1839, 35-38.


J. Fridesoux, On the atomic weight of oxalic acid, Phil. Mag., (2)6, 166, 1829.

On the composition of barium chloride, ibid., (2)7, 276, 1830.


Further remarks on the specific gravity of hydrogen gas and on Prout's modification of the atomic theory, ibid., 27(-11), 187-194, 1826.

J. S. Stas, Oeuvres Complètes, 3 vols., Brussels, 1894.


T. Thomson, ON specific gravities, Ann. Phil., 1, 177, 1813.
T. Thomson, On the Daltonian theory of Definite Proportions,
Ann. Phil., 2, 32-52, 109, 167, 298, 1813; 3, 134, 375, 1814;
4, 11, 83, 1814.
Annual Report for 1815, ibid., 7, 17, 57, 1816.
Observations on the Relation between specific gravities
and atomic weights, ibid., 7, 343-346, 1816.
Analysis of air, ibid., 8, 231-232, 1816.
Annual Report for 1816, ibid., 9, 1, 1817.
Additional observations on Atomic weights, ibid., 12,
338-350, 1818.
Annual Report for 1818, ibid., 13, ix-xl, 1819.
On the specific gravity of hydrogen, ibid., 14, 65-66, 1819.
On the specific gravities of gases, ibid., 16, 160-177, 1820.
On the true atomic weight of barytes, potash, etc., ibid.,
16, 329, 1820.
On Ferrocyhazate of iron, ibid., 16, 216-223, 1820
Experiments on atomic weights, ibid., 17(2), 241-252, 1821;
18(2), 120-146, 1821.
Answer to Brande's Review of Thomson's System, ibid.,
19(3), 241-275, 1822.
The influence of humidity in modifying the specific
An Attempt to Establish the First Principles of Chemistry
By Experiment, 2 vols., 1825.
Reply to Rainy on the specific gravity of hydrogen, Ann.
Phil., 26(10), 352-360, 1825.
On the method of analyzing zine sulphate, ibid., 26(1),
363-371, 1825.
Answer to Ure's review of First Principles, ibid., 27(11),
1-14, 1826.
Reply to Berzelius, Phil. Mag., (2)5, 217, 1829.
T. Thomson, On atomic weights, Records of General Science, 3, 179, 251, 1836.
E. Turner, On atomic weights, Phil. Mag., (3) 1, 109, 1832.
Experimental researches on atomic weights, Phil. Trans., 1833, 523-544.
A. Ure, On chemical equivalents, Phil. Mag., 57, 95-116, 1821.
[Ure with W. T. Brande?], Review of 5th ed. of Thomson's System,
Dr. Thomson and his 'Answer', ibid., 13, 333-353, 1822.
Review of Thomson's First Principles, ibid., 20, 113-141, 1826.
A. S. Wilson, The Unity of Matter, 1855.

4. Primary Sources on Vegetable and Animal Chemistry

W. Allen and W. H. Pepys, On the changes produced in atmospheric air
by Respiration, Phil. Trans., 1808, 249-261.
W. Beaumont, Experiments and Observations on Gastric Juice and the
Physiology of Digestion, (1833), Dover Reprint, 1959.
J. J. Berzelius, A View of the Progress and Present State of Animal
Chemistry, 1813.
On the composition of animal bodies, Ann. Phil., 2,
19, 195, 377, 415, 1813.
Experiments on the definite proportions of organic
nature, ibid., 4, 323, 401, 1814; 5, 93, 174, 260, 1815.
Prout on Blood, Jahresbericht, 1, 122-123, 1822.
Prout on Urine, ibid., 1, 127-129, 1822.
Prout on Purpuric acid, ibid., 3, 204, 1823.
Sir Gilbert Blane, Elements of Medical Logick, 1819.


E. W. B. (Rayley), On the true source of the amniotic acid of Vauquelin, *Phil.Mag.*, (3)1, 319, 1832.

H. Bright, Prout's observations of urea in blood, *Bright's Medical Reports*, 1, 24, 76, 84, 1827.

C. Hawkins (ed.), *Works of Sir Benjamin Collins Brodie*, 3 vols., 1865 (several references to Prout).


C. T. Coathupe, *Products of Respiration at different times of the day*, *Phil.Mag.*, (3)14, 401-414, 1839.


S. E. Hoskins, *Researches on Decomposition of Phosphate Vesical Calculus*, *Phil.Trans.*, 1843, 7-16.


Contributions to the Chemistry of Urine, *Phil.Trans.*, 1845, 335-349; 1849, 235-270; 1850, 651.

On Animal Chemistry, and its Application to Stomach and Renal Diseases, 1850.


Chemistry in its Application to Agriculture and Physiology, trans. L. Playfair, 2nd ed. 1842.


J. Pereira, *Treatise on Food and Diet*, 1843.


E. Smith, *Inquiries into the Chemical Phenomena of Respiration*, *Phil. Trans.*, 1859, 681-714.


*Digestion of Vegetable Albumen, Fat and Starch*, *Phil. Mag.*, (3)26, 322-324, 418-424, 1845.

F. Tiedemann and L. O. Melin, Reply to the remarks of Dr Prout
inserted in the Annals of Philosophy, Phil. Mag., (2)4,
3–5, 1828.
Recherches Experimentales, Physiologiques et Chimiques
A. Ure, On ultimate analysis, Phil. Trans., 1822, 457.
L. Vauquelin, Sur l’acide produit par l’action de l’acide nitrique
sur l’acide urique, J. de physique, 88, 456–459, 1819;
Quart. J. Science, 8, 157–159, 1820.
Les ecorçemens des serpens (sic), Ann. chim., 21, 440–
442, 1822 (dated 18 Janvier 1823).
R. Venables, On Siliceous Gravel, Quart. J. Science, 6, 234, 1829.
F. Wöhler, Artificial Preparation of Urea, Annalen d. Physik und
Chemie, 12, 253–256, 1828;
J. Yelloly, Observations on Dr Prout’s Estimate of Mortality from
Lithotomy, Ann. Phil., 17(1), 363, 1821.
Remarks on the tendency to Calculus Diseases, Phil. Trans.,
1829, 55–81; 1830, 415.

5. Miscellaneous Primary Sources
F. Baily, Description of a new Barometer, Phil. Trans., 1837, 431.
J. J. Berzelius, Essai sur la théorie des proportions chimiques,
Paris, 1819.
Traité de Chimie, 8 vols., Paris, 1829–33; 2nd
E. G. Söderbaum (ed), Jacob Berzelius Bref, 6 vols.,
W. Buckland, On Coprolites or Fossil Faeces, Trans. Geological Soc.,
3, part 1, 223–236, (1829) 1834.
G. Cusor, Natural Theology: Arguments of Paley, Brougham and the
Bridgewater Treatises Examined, 1836. (copy London Univ.)
J. F. Daniell, Introduction to the Study of Chemical Philosophy,
1839; 2nd ed., 1843.


J. D. Enys (ed.), *Correspondence Regarding the Appointment of the Writers of the Bridgewater Treatises Between Davies Gilbert and Others*, priv. printed, J. Gill & Son, Penryn, Cornwall, 1877, 32 pp. (Enys library).


G. Fownes, *Chemistry as Exemplifying the Wisdom and Beneficence of God*, 1844.


D. Gilbert, *Award of Copley Medal to Prout*, *Phil. Mag.* (2) 3, 61-62, 1823.

Statement on Bridgewater Treatises, *ibid.*, (2) 9, 200, 1831.


H. Hennell, Reaction between sulphuric acid and alcohol, *Phil. Trans.*, 1826, 240 (mentions Prout's analytical apparatus).


E. B. Bax (trans.), *Kant's Prolegomena and Metaphysical Foundations of Natural Science*, 1903.

G. H. Lewis, *Comte's Philosophy of the Sciences*, 1853.

F. Lunn, *Chemistry*, *Encyclopaedia Metropolitana*, vol. 4, 1845.


6. Secondary Sources of Information on Prout


Anon., Review of Copley paper of 1827, Quarterly Reviews, 52, 406, 1834.


Lancet, 1840-1, 277-278.


Edinburgh Medical & Surgical J., 67, 301-302, 1847.


Edinburgh Medical & Surgical J., 70, 419-453, 1848.
Lancet, 1848, 1, 582.
London Medical Gazette, (2)7, 41, 1848.


W. Munk and W. MacMichael, Gold headed cane, 1884, Prout, p. 203.


Portrait of Prout (Paget), with note by F.E. Clarke, J. Chemical Education, 16, 401, 1939; rep. ibid., 17, 7, 1940.


H.M. Smith, Torchbearers of Chemistry, 1949, Phillips portrait p. 206, with wrong date for discovery of HCl.


F. H. Ward, Men of the Reign, 1885, (from Munk).


7. General Secondary Sources


A. C. Burr, On thermal conductivity (convection), Isis, 21, 177, 1934.


C. Hall, Memoirs of Marshall Hall, 1861.

R. A. Houston, Light and Colour, 1923.


Die Entwicklung der Chemie in den neuen Zeit, Munich, 1873.

W. R. Le Fanu, British Periodicals of Medicine, a list, Bull. History of Medicine, 5, 1937; 6, 1938.


8. Secondary Sources on Matter Theory

A Short History of Atomism, 1931.
J. W. Mallet, Stas Memorial lecture, Memorial Lectures of the Chemical Society (1893-1901), 1901.
A. Meldrum, Avogadro and Dalton, Edinburgh, 1906.

R. Siegfried, Chemical Basis of Prout's Hypothesis, ibid., 33, 263-266, 1956.

9. Secondary Sources on Animal Chemistry and Biochemistry

D.G. Bates, Background to John Young's thesis on Digestion, Bull. History of Medicine, 36,341,1962.


E. Ebstein, Ueber die Entdeckung der freien Salzsaure in Magensaft, Mitteilungen zur Geschichte der Medizin und Naturwissenschaften, 13, 161, 1913 (inaccurate).


A.W. Hofmann, The Life Work of Liebig, 1876.


T. Holmes, Sir Benjamin Collins Brodie, 1898.


A.M. Kasich, Frout and the Discovery of Hydrochloric Acid in the Gastric Juice, Bull. History of Medicine, 20, 340-358, 1946.


N.S. Papaspyros, History of Diabetes Mellitus, 1952.
W.A. Shenstone, Justus von Liebig, 1901.

A.H. Smith, Centenary of a unique discovery (HCl in gastric juice),
Scientific Monthly, 17, 238-244, 1923.

SUMMARY

Prout is principally remembered for his speculation concerning the chemical elements, while his other theoretical and experimental work in biochemistry has been generally forgotten, or ignored. In this thesis an attempt has been made to achieve an historical balance by examining both Prout's experimental and theoretical interests. The study is divided into two parts. In the first section, after a brief biography, Prout's contributions to experimental science are considered. A discussion of his development of apparatuses and techniques for organic analysis is followed by surveys of his work in the fields of urine chemistry, the physiology of respiration, and the chemistry of digestion. A full summary of an unpublished manuscript on sensation is also given. The first part ends with a short treatment of Prout's barometric interests. The second part is concerned with Prout as a theorist, and the transition is made through a study of his Bridgewater Treatise and his support for natural theology and vitalism. A detailed analysis of Prout's Hypothesis from 1800 to 1850 is followed by a final chapter which explores Prout's molecular theory and attempts to link together his theoretical ideas with his experimental work in the field of animal chemistry. Reasons for Liebig's success, and Prout's failure, to establish the science of biochemistry are also considered. Transcriptions of some unpublished manuscript material are given in appendices.
The argument for the existence of God from the "wonder of His works," forms the province of natural theology. This was a subject of great popularity during the eighteenth and early nineteenth centuries (1), and one which seemed quite valid until Darwin's theory of evolution demolished it in its original form. Until then, no clergyman felt himself fully equipped unless he had a fair knowledge of natural history or natural science. In the seventeenth century, both Boyle and Newton had stressed that religion and science were not in conflict, and that the "Mechanical World Picture," far from leading to atheism, actually supported the existence of a Designer. "And thus much concerning God, to discourse of whom from the appearances of things, does certainly belong to Natural Philosophy," Newton had written (2). When Boyle left a sum of £50 at his death for the delivery of eight annual sermons—the Boyle Lectures—for the refutation of "notorious infidels," Richard Bentley, the first lecturer, drew examples from Newton's mechanics to prove the existence of a Deity.

The number of books written on natural theology is enormous, but they are all well epitomized by the most famous treatise on the subject, William Paley's immensely successful "Natural Theology" of 1803. It has been said of Paley's arguments that

... he paid far too much attention to finding examples of apparent design in Nature, and not enough to the analogical inference that design in Nature (necessarily) implies the existence of a designer (3).

Bridgewater Treatises

This criticism also applies to the Bridgewater Treatises, "that strange and deadly series (4)" which appeared during the third decade of the last century. These eight treatises were sponsored by the will, dated 1825, of the eighth and final Earl of Bridgewater, Francis Henry Egerton, who died in 1829. Son of a bishop, Egerton was a clergyman who "assiduously neglected his parish (4)" for literary and historical scholarship. Never a scientist, he was nevertheless made a Fellow of the Royal Society in the days when the Royal Society was largely a gentlemen's dining club rather than a scientific institution. The Earl was an extremely rich man, for besides his legacy to the Royal Society, he also left £12,000 to the British Museum for the provision of his Egerton manuscripts. He was also one of England's great eccentrics.

The terms of the will must have provided a problem to the Royal Society's president, Davies Gilbert, who himself did not live to see the publication of the treatises. Eight thousand pounds had been invested in Bank Annuities stock and held at the disposal of Gilbert to be paid to the person or persons nominated by him. The person or persons appointed, were required to

... write, print and publish, 1000 copies of a work on the Power, Wisdom and Goodness of God, as manifested in the Creation; illustrating such work by all reasonable arguments, as for instance the variety and formation of God's creatures in the animal, vegetable and mineral kingdoms; the effect of digestion, and thereby of conversion; the construction of the hand of man, and an infinite variety of other arguments (5).

Gilbert thought the task was too much for one man, and perhaps that £8000 was too great a windfall; hence, alone, or by consultation, he decided that the legacy should be broken down into eight lots of £1000—a decision that was harshly criticized by the "Edinburgh Review" and "Athenaeum." At this stage, Gilbert allocated the task of choosing the eight authors to the Royal Society Secretary, Peter Roget. Roget, who is widely known as the author of "Roget's Thesaurus," with the aid of the Archbishop of Canterbury and the Bishop of London, had by July, 1830 obtained the agreement of eight authors. Through 1833-6 the treatises appeared, and went through further editions well into Victoria's reign.

William Prout, Physician and Chemist

The eighth Bridgewater Treatise was "Chemistry, Meteorology and the Function of Digestion (5)" by the chemist and physician, William Prout, who is best known as the foremost nineteenth century exponent of the speculation that all the chemical elements possess atomic weights that are integral multiples of the atomic weight of hydrogen (6). Prout was born in 1785 at Horton in Gloucestershire. A country lad, he received a negligible education until, at the age of 17, he suddenly became aware of his own ignorance. Ambitiously, he inserted an advertisement in a local paper asking advice on his prospects for further education! A reply came from the Rev. Thomas Jones who ran a classical seminary near Bristol. Prout, who was later to name his youngest son Thomas Jones Prout, spent two happy years with Jones who interested Prout in the chemistry of Lavoisier and Davy and urged him to become a doctor. Prout entered Edinburgh University in 1808, and was graduated as an MD in 1811 with an uninspired dissertation on fevers. During his medical course he boarded with Alexander Adam, the Rector of Edinburgh High School, whose daughter, Agnes, he married in 1814. There were six children and the boys reflected Prout's new social status by becoming respectively soldier, lawyer, surgeon, and Oxford don.
After receiving the best medical education it was possible to obtain in Britain, Prout came to London, where he set up a medical practice and soon established a reputation for work on what was then termed "Animal Chemistry"—in fact biochemistry. Over the years, with improved apparatus and techniques for organic analysis, he analyzed a tremendous variety of substances, prepared pure urea, and wrote a useful textbook of urine pathology which established him as a specialist on urinaiy and digestive disorders (7). In 1823, he analyzed the gastric juice and solved the old problem as to the nature of its vital ingredient, hydrochloric acid. From there he went on to consider the biochemistry of digestion and assimilation, in which his views foreshadowed those of Liebig.

Nevertheless, despite a list of impressive achievements, we find that, apart from the hypothesis, neglect of Prout's chemical and pathological work had already set in long before his death in 1850. There are two reasons for this. First, his active scientific career was essentially finished by 1834. Thereafter he devoted himself entirely to his medical practice and the revision of his books, while severe deafness prompted him to avoid scientific company. Second, in direct consequence, Prout was unable to keep abreast with the rapid scientific developments in chemistry and physiology, so that although much of his early research pres­ cured that of Liebig and his school, in the main, Prout found himself overshadowed by the great achievements of the 1830's and 1840's. One aspect of this was his refusal to use chemical formulas and equations. The Royal Society, of which he was a Copley medallist, ignored his death, as did the infant Chemical Society; only the physicians remembered him and paid him handsome tribute in their chief medical journals. For the general public he was just an author of a Bridgewater Treatise.

There were three editions of Prout's treatise during his lifetime, and a posthumous fourth edition which appeared in 1855. John Tyndall, the agnostic physicist and a Copley medallist, ignored his death, as did the infant Chemical Society; only the physicians remembered him and paid him handsome tribute in their chief medical journals. For the general public he was just an author of a Bridgewater Treatise.

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Prout's Chemistry

The "Chemistry" is of considerable interest. Apart from a few historians of science, who know today that the word convection was first coined by Prout in this book, or that Prout clearly stated and accepted long before the majority of chemists what we today recognize as Avogadro's law (10)? His attitude toward the atomic theory of Dalton is best summarized in a letter he wrote to his friend Charles Daubeny in 1831.

...There is no one can have a greater respect for Mr. Dalton, and all that he has done, than myself, and I am a firm believer in his principles as far as they go, because I believe them to be founded in truth (11).

However, Prout did not believe they contained "all the truth, and that consequently in their present state they are inadequate to explain the operations of nature." What were these inadequacies?

Dalton's classical atomic theory had assumed (12) that matter was composed of atoms that were qualitatively and quantitatively identical for the same element, or different for unlike elements. Since he had no chemical means for determining the molecular formulas of compounds, he simply assumed that compounds were binary, unless there were reasons to suppose otherwise. Thus, using Berzelian nomenclature, of the possible formulas of water, HO; H2O, HO2, H2O2, etc., Dalton chose the simplest, HO. He had then shown how this assumption could be fruitfully used to assign an atomic weight to every element, if the results of quantitative analysis were known. Thus, if water was HO, then the atomic weight of oxygen was 8 (H = 1). Dalton's atomic weights are of course equivalent weights.

The rule of simplicity was quite unsatisfactory, and the history of nineteenth century atomism may be read as a long search for reliable molecular formulas. This search was only completed by Cannizzaro in 1858, yet some minds, including that of Prout's, had seen the answer to the difficulty very much earlier, although their suggestions were unnoticed or unapproved.

In the Bridgewater Treatise, Prout discussed Gay Lussac's law of combining volumes of gases, and Gay Lussac's and Dalton's law of gaseous expansion. He
went on to remark that these were "very striking facts which apparently lead to the additional conclusion that— all perfectly gaseous bodies under the same pressure and temperature contain the same number of self-repulsive molecules." His argument was:

...If different gaseous bodies contained unequal numbers of self-repulsive molecules, the molecules of those gases which contain the least number, must exert the greatest self-repulsive power; in other words, the expansive energies of the molecules of a gas must increase as their number diminishes; and not only so, but the expansive energy must increase, neither more nor less, but exactly as the number of molecules diminishes. Such a conclusion would be obviously false; for if we imagine an extreme case, and suppose the number of self-repulsive molecules in a given volume of gas to be reduced to a few, or to one for instance, this single molecule must be supposed to exert a self-repulsive power, equal to the self-repulsive power exerted by myriads of molecules under ordinary circumstances.

It is not known when Prout arrived at Avogadro's law, but it must have been prior to 1815. We have his word that he arrived at the law independently of Avogadro, or the versions of Ampere and Dumas.

Dalton himself had thought of the hypothesis of equal volumes, but only to immediately reject it, since he identified what Prout was to call the molecule with the atom. To accept it was to destroy his axiom concerning the indestructibility of the atom. Thus, 1 vol. H (n atoms per vol.) + 1 vol. Cl (n atoms per vol.) → 2 vols. HCl (n/2 atoms per vol.)

In order to save the hypothesis, the atoms of the reactants would have to be "split" into two parts—a step Dalton was not prepared to make, so he abandoned the hypothesis. The alternative was to say that the "Dalton atom" was not the atom, but a particle, or molecule, or physical atom, composed from at least two indivisible chemical atoms.

However, Dalton and the majority of chemists refused to take this step, and it was left to Avogadro, Ampere, Prout, and later Dumas and Gaudin to point out the way for a more reliable set of criteria for the establishment of atomic weights.

Prout argued that the production of water proceeded according to, at least,

\[ 2H_2 + O_2 \rightarrow 2H_2O \]

and consequently the atomic weight of oxygen was 16 and not 8, or a volume of oxygen weighed 16 times an equal volume of hydrogen. He concluded,

...the weights of the self-repulsive molecules of all bodies are as the specific gravities (i.e., relative densities) of these bodies in the gaseous state; or bear certain simple ratios to these specific gravities.

Thus,

\[ \text{Relative density of gas } x \ (H = 1) = \frac{\text{Mol. wt. of } z}{\text{Mol. wt. of } H} \]

whence we derive the familiar equation, "molecular weight is twice relative density." Prout had stated this equation in the form,

\[ \text{specific gravity of gas } x = \left( \frac{\text{sp gr of oxygen}}{2} \right) \times \text{at. wt. of } x \]  

\[ (\text{air } = 1) \]

\[ (0 = 10) \]

in his famous anonymous paper of 1815, "On the Relation of the Specific Gravities of Bodies in their Gaseous State and the Weights of their Atoms (19)."

In this paper he incidentally noticed that the calculated relative densities were integral numbers. This led him to speculate that all the elements were condensed from hydrogen, or something simpler than hydrogen; and the abandonment of another Daltonian axiom—that there were as many atomic species as chemical elements. It has often been noticed that Prout's hypothesis was an explicit statement of hints and speculations dropped by Thomson and Davy, and that he was directly influenced by these chemists (14). However, we can go further and now suggest that the Bridgewater Treatise shows that Prout's acceptance of the "Avogadro law" prepared his mind to split the Daltonian atom whenever necessary. Hence, he found no difficulty in moving to the position where all the elements were polymers of the hydrogen atom or some simpler species.

In fact, having derived the law of Avogadro and having perceived its entailments, Prout saw no good reason not to permit the existence of molecular aggregates like (H_2O)_2, (H_2O)_3, (H_2O)_n, etc. and series of combining weights, 3, 6, 9, 12, etc. Unlike Dalton and Avogadro, Prout never thought it necessary to build a simplicity rule into his atomic theory. Of the possible molecular equations satisfying Gay Lussac's and Avogadro's law,

\[ 2H_4 + O_4 \rightarrow 2H_2O \]

\[ 2H_2 + O_2 \rightarrow 2H_2O \]

\[ 2H_4 + O_2 \rightarrow 2H_2O \]

\[ \text{etc.} \]

Avogadro had chosen the simplest consistent one. Indeed it was a source of criticism leveled against Avogadro: Why should molecules cease building at O_2, H_2, etc.? Why not H_2, H_4, O_4, etc.? Prout thought it likely that such molecules did exist.

...The molecule of water, on entering into combination, is often found to be divided into two, or three (perhaps more) parts. Now as we cannot admit the division of the ultimate molecule, or atom; we must of course conclude, that the molecules of oxygen and of hydrogen, are much more compounded...and must each of them contain at least, three components, or submolecules. Hence the molecule of water will consist of at least nine component submolecules (viz. three of oxygen, and six of hydrogen) which we may suppose to be associated, in the first place, the hydrogen with the oxygen chemically; and afterwards the three submolecules of water with one another cohesively, so as to constitute one spherical molecule.

As a book of apologetics, probably Prout's Bridgewater Treatise had little scientific influence. His unitary hypothesis was already widely known, and in any case it was not figured prominently in the treatise. His views on atoms and molecules, which roughly coincided with the more publicized suggestions of Dumas in France, were discussed favorably by Daubeney in the second edition of his "Introduction to the Atomic Theory (11)." However, this did not appear until 1850, by which time Avogadro's law had already received its reappraisal from Dumas's pupils Laurent and Gerhardt, and was to be supported by Williamson after his return from France in the same year.

If we can find no use made of Prout's ideas, this need not in any way diminish the interest of his chemical Bridgewater Treatise to us, for it enables us to determine the attitude of a chemist who was prepared to think out, and when necessary reject, the basic assumptions of Dalton's atomic theory, rather than
follow the majority of chemists who tended to take a phenomenalist or nonrealist attitude toward the atom. As Daubeny wrote: Prout showed

...a thorough mastery of the details of his subject, but also much ingenuity in unraveling the mysteries which beset us when we attempt to speculate on the intimate constitution of matter.

**Literature Cited**


(5) *Prout, W.*, "Chemistry, Meteorology and the Function of Digestion," W. Pickering, London, 2 eds. 1834, 3rd ed. 1845, 4th ed. posth. 1855. See terms of will at front of all editions. All quotations are from the first, or third editions, copies of which are to be found in the Edgar Fahs Smith Memorial Collection, University of Pennsylvania.

(6) "Prout’s Hypothesis," Alembic Club Reprints No. 20, Oliver & Boyd, Edinburgh, 1932.


(8) Tyndall’s unpublished private journal, Nov. 19, 1854, in library of Royal Institution, London.

(9) *Prout, W.*, *Phil. Trans.*, pp. 335–88, 1827. Prout received the Copley medal for this paper.


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REFERENCES

2. —— Experiments and observations on the different modes on which death is produced by certain vegetable poisons, *Phil. Trans.*, 1811, 101, 194.
3. —— Further experiments and observations on the action of poisons on the animal system, *Phil. Trans.*, 1812, 102, 205.
4. —— Physiological Researches, collected and reprinted from the *Philosophical Transactions*, London, Longmans, 1851.

WHO WERE THE EDITORS OF *THE ANNALS OF MEDICINE AND SURGERY*?

The short-lived journal, *The Annals of Medicine and Surgery; or Records of the Occurring Improvements and Discoveries in Medicine and Surgery and the Immediately Connected Arts and Sciences* appeared quarterly—31 March, 30 June, 30 September, and 31 December—in two volumes, 1816-17. It contained sections for (1) original papers—the only notable one was by Prout; (2) reviews—the greater part of each issue; (3) *Intelligence*. The latter section always included an interesting tabulated 'Comparative View of the State of the Atmosphere, prevalent Diseases, and Mortality of the Metropolis', indicative of the current interest in meteorological theories of disease. The Royal College of Physicians' copy of Volume I has inscribed on a fly-leaf: 'To Dr Baillie From the Editors', and from internal evidence it would seem that there were two editors. It would be interesting to know the identity of these gentlemen.

There is a suggestion by one of William Prout's obituarists that Prout and John Elliottson were responsible for the journal's appearance.

It has been said that this journal was conducted by Dr. Elliottson and Dr. Prout; but the correctness of this statement we have no means of ascertaining. The first volume of the *Annals of Medicine and Surgery* is inscribed in a Latin dedication to Matthew Baillie, M.D., London; the second volume, in the same language, to James Hamilton, M.D., Edinburgh, at that time Physician to the Royal Infirmary.

However, none of Prout's writings makes any allusion to such a responsibility, and I understand that Dr. Harley Williams cannot recall any such indication from Elliottson's work. Does any historian have proof for this claim? It is possible that the editorship might be discovered from internal evidence, and I therefore offer a few remarks which, though conflicting, may shed some light on the problem for other historians of medicine.

1. Prout and Elliottson were very good friends. They had been contemporaries at Edinburgh, and afterwards had walked the wards together at the Borough hospitals of St. Thomas's and Guy's. There are many references to the effect that Elliottson had
submitted pathological samples to Prout for analysis. Prout suggested an iodine treatment for goitre which was successfully applied by Elliotson at St. Thomas's.

2. An anonymous article signed 'A.B.', entitled 'Case of an earthy Mass discharged from an encysted Tumour in the Nape of the Neck', which appeared in the June issue of the *Annals* in 1816, was in fact by Elliotson. This was revealed by Prout in 1819 when he published an analysis of the men so discharged. However, such was the burden of anonymity, Elliotson had referred to Prout in the original note simply as 'A chemist'. Although this anonymity might be held as indicative of Elliotson's connection with the journal, there is, on the other hand, a signed communication by Elliotson addressed to the editors in the second volume. This could mean that Elliotson was not an editor, or that he was only co-editor of Volume I, but not Volume II; or just a deception.

3. Prout's important 'Inquiry into the Origin and Properties of the Blood' was published incompletely in three parts in the first volume, and later republished in a slightly abridged form (and again incompletely) in Thomson's *Annals of Philosophy*. Prout gave as his reason for republication that 'the Work in which it (originally) appeared had a very limited circulation'. But he omitted to refer to the journal by name! In the fourth issue there appeared the note, 'The Editors are sorry to hear from Dr. Prout that particular circumstances have prevented him from concluding his paper on Blood.' This may indicate Prout was not an editor.

4. The title-page of both volumes stated that the journal was 'sold by most of the principal bookellers in the United Kingdom, and on the Continent'. The original publishers were E. Cox and Son, St. Thomas's Street, Borough (Southwark), perhaps significantly close to the United hospitals of St. Thomas's and Guy's. The final two numbers (7 and 8) were published by Thomas and George Underwood of Fleet Street.

5. Several reviews bear the mark of Prout's authorship. It is reasonable to suppose that if Prout were an editor he would himself have reviewed any books which related to his own research interests. We should therefore pay particular attention to the analyses of Scudamore's *Nature and Cure of Gout* and Marcet's *Chemical History and Treatment of Calculous Diseases*. For example, consider these two analogous passages from the review of Marcet's 'Experiments on the Chemical Nature of Chyle' and Prout's essay on blood:

Marcet review. The reviewer commented on Marcet's claim that some chyme he had examined contained albumen.

We cannot account for the presence of albumen in this case, but we will venture to ask—ought a substance to be called albumen, till it be found to possess all the properties of the albumen of the blood of the animals from which it is taken. . . . The chyle, even when it enters the blood, contains, besides the albuminous principles, a considerable proportion of a substance which has been compared to caseous matter or cream, and moreover occasionally of a perfectly developed oil or fat, especially when the animal has been fed on flesh . . . Vauquelin compared it to the fatty matter which he found in the brain, and we made the same remark upon examining the chyle some time ago.

Prout's article

I have ventured to call by the name of *incipient* albumen, a peculiar principle uniformly found in the chyle of the mammalia, and which appears to decrease in quantity as the two albuminous principles increase. Concerning the nature of this principle, various opinions have been entertained. One of the oldest and most common has been, that it is similar to the caseous principle of milk. . . . What makes the resemblance still more striking is, that in the chyles an oily or
butyaceous fluid is very often present, which rising to the top of its serum, in conjunction with the caseous-like principle of which we have been speaking, forms an appearance exactly resembling the cream of milk; and these principles are often so abundant, especially in the chyle of animals fed on flesh, that, as Dr. Marcet has observed, they may be readily detected, even in the blood itself. Vauquelin remarked the near resemblance of this fatty matter to that which he had extracted from the brain, and I made the same remark before I had seen Vauquelin's paper. . . .

It may prove possible to identify other reviews as Elliotson's, for books on animal magnetism and the doctrines of Gall and Spurzheim were analysed.

W. H. BROCK

REFERENCES

1. Copies at Royal College of Physicians, Royal Society of Medicine, Manchester University Medical Library, Hereafter referred to as A1 and A2.

2. The fact that second editions of nos. 1, 2, and 3 were announced for the summer of 1818 suggests that these first issues were sold completely; cf. A2, p. 484.

3. Yet A1, p. 240, refers to a 'Committee of Editors'.


5. A recent biographer of Elliotson. See his Doctors Differ, 1946.

6. E.g. Med. Chir. Trans., 1818, 9, 474 (a blood sample); ibid., 1819, 10, 390 (ammonium urate calculus).

7. W. Prout, Chemistry, Meteorology and the Function of Digestion, 1834, p. 100n. On Prout, see my forthcoming article, 'Life and Work of William Prout'.


10. 'On the efficacy of vaccination against distemper in dogs', A2, pp. 1–2. Also, at A2, p. 112, 'Editors return thanks to Dr. Elliotson' for his communication.


13. Ibid., p. 13. Yet at A2, p. 158, the editorial reviewer says 'as our Work is very extensively circulated. . . .'. In another review, it was remarked that Magendie had evidently not seen Prout's articles in Vol. I.


Book Reviews


The long editorial experience of Gordon Wolstenholme, Director of the Ciba Foundation, London, has been most felicitously directed towards the production of
THE ATOMIC DEBATES:
"Memorable and interesting evenings in the life of the Chemical Society"

BY
W. H. BROCK AND D. M. KNIGHT
The Atomic Debates:
"Memorable and interesting evenings in the life of the Chemical Society"

By W. H. Brock * and D. M. Knight **

EARLY SKEPTICAL ATTITUDES TO THE ATOMIC THEORY

It may appear surprising that in 1869 the President of the London Chemical Society found it necessary to give a lecture in support of the atomic theory in chemistry, for more than sixty years had passed since John Dalton's atomic theory had been published, and ten since James Clerk Maxwell had read to the British Association his paper on the kinetic theory of gases. In the same year Dmitri Mendeleev had published his periodic table of the elements. Our surprise that the lecture was required is increased when we find that Alexander Williamson's distinguished audience remained unconvinced by his address. It will be the object of this paper to trace the skepticism towards the atomic theory that prevailed in Britain from Dalton's day to the middle of Queen Victoria's reign, and even beyond; and thus perhaps to explain how conviction had, in 1869, still to be secured.

When Dalton published his theory, he received notable support at first from William H. Wollaston's accurate researches on superacid and subacid salts. In this paper of 1808, Wollaston appeared as strongly atomistic: "all the facts that I had observed," he wrote, "are but particular instances of the more general observations of Mr. Dalton, that in all cases the simple elements of bodies are disposed to unite atom to atom." However, he was a more subtle and cautious thinker than Dalton, and he saw that a distinction should be made between the observed laws of combination and the models used in explanation. He tried to account for the ratios he found by the use of a model of spherical atoms; and he wrote:

** Durham University, England. Presented in part before the British Society for History of Science, 28 June 1963. The author would like to thank the Department of Scientific and Industrial Research for a research studentship, during which this paper was written.

The authors would like to thank R. Harré (Oxford) for suggesting their collaboration and for reading the manuscript.

** Ibid., p. 39.
I am further inclined to think, that when our views are sufficiently extended, to enable us to reason with precision concerning the proportions of elementary atoms, we shall find the arithmetical relation alone will not be sufficient to explain their mutual action, and that we shall be obliged to acquire a geometrical conception of their relative arrangement in all the three dimensions of solid extension.

His meticulous approach would not allow him to confuse these speculations with solid experimental evidence; but in 1813 he published a paper elaborating his atomic geometry. In the next year came a complete change, and we find him abandoning the attempt to achieve anything more than an arithmetical relationship. In the paper describing his synoptic scale of chemical equivalents, he wrote: "When we estimate the relative weights of equivalents, Mr. Dalton conceives that we are estimating the aggregate weights of a given number of atoms, and consequently the proportion which the ultimate single atoms bear to each other." It appears that Wollaston could no longer agree, and Dalton's most powerful supporter relinquished his theory on the grounds that there are no means of telling which compounds are truly binary or ternary, so that there can never be any certainty of true atomic weights. Therefore equivalents must suffice.

Dalton had sought to escape from this difficulty by a number of simplicity axioms: if only one compound between two elements was known, it must be assumed to be binary (of the form $AB$); if two were known, one must be binary and the other ternary ($AB_2$ or $A_2B$); and so on. The axioms, whose arbitrary nature had been commented on adversely, notably by Bostock, had led to the formula $HO$ for water.

We should note that Wollaston's skepticism over atoms was confined to chemistry. For in chemistry the atomic theory appeared to postulate unnecessary entities whose weights were arbitrary and whose arrangements were inaccessible. Until the rise of structural organic chemistry, following Friedrich August Kekulé, Jacobus Henricus van't Hoff, and Joseph Achille le Bel, hypotheses of atoms arranged in space were sterile, as Wollaston's earlier ideas had been. Later, Wollaston devised a physical experiment to establish that the earth's atmosphere was finite and therefore particulate. He was prepared to carry his argument forward into chemistry, for since the law of definite proportions had been found true for all kinds and phases of matter, "we may without hesitation conclude that all those equivalent quantities, which we have learned to appreciate by proportionate numbers, do really express the relative weights of elementary atoms, the ultimate objects of chemical research." Many chemists felt unable to share Wollas-
ton's confidence, perhaps because he had no proof that the particles or molecules of his argument were the Daltonian atoms. After all, it was far safer, and equally useful, to stick to the synoptic scale of chemical equivalents!

Wollaston's contemporary, Humphry Davy, never accepted Dalton's theory, though he was a convinced corpuscularian at the outset of his career. His grounds were less clear perhaps than those of Wollaston. Throughout his life Davy was concerned at the number of the chemical elements, and in a lecture of 1809, he argued that since many compound bodies behaved rather like elements, and since air and the various oxides of nitrogen were all made up of two constituents only, it was not unlikely that most chemical elements were really compounds. "Whoever," he declared,

compares the complication of the systems which have been hitherto adopted, and the multitude, as it were, of insignificant elements with the usual simplicity and grandeur of nature, will surely not adopt the opinion, that the highest methods of our science are already attained; or that events so harmonious as those of the external world, should depend upon such complex and various combinations of numerous and different materials."

Definite proportions would, he believed, "be found to depend upon the identity of the matter really acting upon each other"; the true explanation would be found in a reduction of the number of elements. This "unity of matter" idea is one of the leading motifs in nineteenth-century matter theory. Later in life Davy adopted the Boscovich force-center atom, and in a dialogue unfinished at his death he attempted to incorporate these atoms with the spherical molecules of which he conceived the chemical elements to be built up. Following Wollaston, he thought these spheres went to form crystals.

Though Davy wrote that "the only use of a hypothesis is that it should lead to experiments; that it should be a guide to facts," he did not take up a simple positivistic position towards the atomic theory. John Davy's claim that his brother's views were just "a modification of those of Mr. Dalton—the same, in regard to fact, stripped of all speculation," will not do, though it might be applied to Wollaston, and was made by all who opposed Dalton's theory. While Wollaston rejected Dalton's theory because it required occult and unnecessary entities, Davy's objection was rather that the theory was, in its acceptance of the chemical elements as given and indivisible, insufficiently radical. For if one accepted the hypothesis that


10 Humphry Davy, Syllabus of a Course of Lectures at the Royal Institution (London, 1822).


13 Ibid., Vol. 8, p. 346.

the chemical elements were in fact compound—a hypothesis certainly easier to verify, in principle, than Dalton's axioms—then Dalton's ideas might seem too much bound up with the present limits of chemical analysis. So Wollaston and Davy were, for very different reasons, agreed in their advocacy of equivalent weights as enough for chemists. While most chemists who supported the unity of matter theory shared Davy's suspicions of the atomic theory, it was quite possible for a reductionist to subscribe wholeheartedly to atomism. Thomas Thomson was Dal ton's earliest disciple, and yet the principal exponent of William Prout's hypothesis. Later Thomas Graham, "as strict an atomist as could be found," wrote an extremely influential paper suggesting that there might be but one kind of ponderable matter.

Most chemists accepted, at least when the atomic theory first became public, that matter was corpuscular. But in the next generation some rejected even this belief on experimental and philosophical grounds. We do not usually think of Faraday and Whewell as chemists, yet both became President of the Chemical Section of the British Association. Faraday rejected corpuscular atoms because he could not reconcile them with his experiments on electrical induction, and instead embraced the Boscovich atom. He wrote: "the words definite proportions, equivalents, primes, &c. . . . fully express all the facts of what is usually called the atomic theory in chemistry." Whewell followed the lead given by Davy in his address when the Royal Medal was awarded to Dalton; the laws of chemical combination are vital, "but the view of matter as constituted of atoms . . . is neither so important nor so certain." Hypothetical configurations of atoms, unless confirmed from crystal structures, were valueless. The term "atom" might, he thought, be kept, but "it need not be understood as claiming our assent to the hypothesis of indivisible molecules." In a later work he was even more severe; Wollaston's astronomical proof, he showed, proved nothing at all; no facts of chemistry, he affirmed, could afford any satisfactory evidence for the ultimate indivisible atom. The utility of an atomic theory did not imply its physical reality.

The majority of British chemists indeed used the term "atom" in this way, and since this seems to have been the case with most of the textbooks written in the half-century or so after Dalton's publication, we shall call

16 R. A. Smith, Life and Works of Thomas Graham (Glasgow, 1884), p. 111.
18 L. Pearce Williams, Contemporary Physics, 1960, 2: 93–105.
20 Davy, Works, Vol. 8, p. 97. Davy was careful to separate Dalton's speculations and Wollaston's experimentally based equivalents which made "the status of chemistry depend upon simple questions in subtraction and multiplication."
22 Ibid., p. 150.
24 Ibid., p. 405.
25 Ibid., p. 418.
this the “textbook tradition.” Thus William Thomas Brande wrote that “the atomic doctrine or theory of definite proportionals, has been much blended with hypothetical views, but it will be most satisfactorily and usefully considered as an independent collection of facts.” More interesting was the attack on this school made by the Irish chemist Michael Donovan in 1839. He argued that the atomic theory and the law of equivalents involved the same assumptions, and therefore were equally hypothetical.

Dalton remarked, and Berzelius agreed with him, that without an atomic theory, definite proportions would be “mysterious,” but this is not very different from what Newton’s Cartesian critics had said about gravity. Against the help which some chemists derived from the atomic model, we must set the alarm of “some timid persons” who suspected that the atheistic system of the Greek atomists was being reintroduced. Although most chemists of this epoch used the atomic theory in a rather attenuated form, Prout took up an extreme position. Like Davy, he believed that the elements were complex, but he regarded the atomic theory as no more than a useful fiction. In his Gulstonian lectures of 1831, he declared that “the light in which I have always been accustomed to consider it, has been very analogous to that in which I believe most botanists now consider the Linnaean system; namely, as a conventional artifice, exceedingly convenient for many purposes, but which does not represent nature.”

Brodie’s Ideal Chemistry

In the mid-century the unity of matter school was much encouraged by the work of the spectroscopists who had recently begun to investigate the spectra of the heavenly bodies. This work followed the suggestion that the dark lines in the solar and stellar spectra must be due to the presence of bodies in the atmospheres of the sun and stars that would give bright lines at the same frequencies in their arc spectra. In 1864, William Allen Miller and William Huggins published papers on the spectra of planets, fixed stars, and later, nebulae. In a nebula the most exciting observation was made; a bright nitrogen line was visible, while other lines of this element, which should have been equally obvious, could not be found. Huggins asked whether this one line indicated a form of matter more elementary than

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29 *Quart. Rev.*, 1848, 83: 55.


33 *Phil. Trans.*, 1864, 154: 413-435; 487-444.
nitrogen. These spectroscopists had then come to a position like that of Davy—perhaps because both he and they were handling new analytical techniques—and their view rapidly became accepted among other workers in the field.⁴⁴ The suggestion was made that perhaps the high temperatures of the stars and nebulae could cause the earthly "elements" to dissociate into simpler substances.

Prout's arguments were taken up strongly by the chemist who set off the discussions on atomism that came to a head in 1869. Sir Benjamin Collins Brodie was the son of a famous president of the Royal Society, who, after training with Liebig at Giessen, became Professor of Chemistry at Oxford. Brodie began as a supporter of the atomists,⁴⁵ but by the 1860's he became their dangerous adversary when he proposed a new set of symbols for chemistry which took into account the laws of chemical combination without lending themselves, as Wollaston's equivalents had, to an atomic interpretation.

In 1866, after a public encouragement from William Odling,⁴⁶ Brodie published in the *Philosophical Transactions* the first part of his "Calculus of Chemical Operations." He wrote that Dalton's atomic theory no longer explained the facts of chemistry, and that in his calculus such hypothetical entities as atoms would not appear. His major debts were to C. F. Gerhardt for his positivism over what symbols represent—not structures but recipes—and to George Boole for his logical algebra.

The clearest exposition of Brodie's "Ideal Chemistry" was given by its author before the Chemical Society in 1867.⁴⁷ He then stated that he believed theory to be essential to the existence of chemistry, for the science had only begun with the phlogiston theory; but he noted that Davy had made his great discoveries without a theory, and instead "rested content simply with the facts of numerical analysis and the laws of combination deduced from them."⁴⁸ Dalton's theory was in fact more audacious than the phlogiston theory because it postulated that the observed continuity of matter was an illusion. In the sixty years since the theory had come out, chemistry had seen plenty of change, but no progress. Brodie had recently seen advertised a set of balls and wires for building up models of molecules, and this seemed to him the last straw. That the atomic theory should have resulted in such a "thoroughly materialistic bit of joiner's work" proved that chemistry had gone off the rails of philosophy, for such a bathos could only have come

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⁴⁵ H. E. Roscoe, *Journal of the Chemical Society (Transactions)*, 1881, 39: 182–183, for a rather poor notice of Brodie; the *Dictionary of National Biography* (London, 1886) account is not only poor, but also misleading.
⁴⁶ *Phil. Trans.*, 1860, 140: 804.
⁴⁷ *British Association Reports*, 1864, 33: 23.
⁴⁹ Williamson was in the Chair. Cf. Benjamin C. Brodie, *Ideal Chemistry* (London, 1880). This is a corrected reprint of the lecture from *Chemical News, London*, 1867, 15: 295–305. The discussion which followed the reading to the Royal Society in 1866 was not reported. William Crookes gave considerable publicity to Part I of the "Calculus," *Chem. News*, 1867, 15: 269, but significantly, perhaps, he ignored Part II.
from a whole series of errors and misconceptions. In place of this, Brodie proposed his calculus, whose object was "to discover what is the nature and the number of operations by which chemical substances are made or constructed," and to bring chemists under rules in which their symbols, unlike the existing ones, would have agreed and distinct meanings, and be combined under definite laws.

In the calculus, substances are considered in the condition of perfect gases at normal temperature and pressure; the "unit" of ponderable matter is that portion of it which occupies 1000 cc. "Operations" are performed on this unit, and to illustrate this Brodie produced a hollow glass cube and performed the operation of tapping it. In chemistry the operations are symbolized by Greek letters. These symbols indicate not only the weight but also the kind of matter in a substance, as vectors indicate size and direction. The operations are in fact "packing," and the combinations of matter with space and matter with matter are analogous. If we are asked what combination consists in, all that we can do is to give a recipe; no metaphysical or atomic hypothesis throws the slightest light upon the question.

\[ a \text{ is the symbol of the operation performed upon the unit of space, of which the result is a unit of hydrogen. The weight of this unit is assumed as the unit of comparison, and has assigned to it the weight 1. This relation is expressed in the equation } w(a) = 1. \]

The only hypothesis, he claims, is that the symbol of the unit of hydrogen is represented by a single letter.

Brodie noted that, according to his system, there were three distinct categories of elements. First were those like hydrogen, \( a \), produced in one operation. Next came those made in two or more similar operations, like oxygen, \( \xi \). The third and largest class was made in two or more different operations; an example was chlorine, whose symbol was \( 2\xi \). This last class was rather exciting, for the symbol of chlorine was very similar to that of hydrogen.

\[ 2a\chi = 2\xi + a \]

The only hypothesis, he claims, is that the symbol of the unit of hydrogen is represented by a single letter.
peroxide, , . Did the substance symbolized by exist or not? The symbol of a simple weight was just a prime factor, and not necessarily the symbol of a simple thing; but still might symbolize a real entity. Though the symbols were not necessarily of real physical things, they were not unreal either, for they had not just come out of Brodie's head. So he called them "ideal," like geometrical entities, and his system became "Ideal Chemistry." There was no need to speculate on the reality of the subelementary units. But Brodie did so, and foreshadowed Crookes' theory of the evolution of the chemical elements in the process. When the earth was being formed, Brodie suggested, substances like might have existed in the form of perfect gases. As the temperature fell these would have combined, and some forms of matter might have become stable to the exclusion of others; some indeed so stable that their decomposition could not again occur. But we might hope to find such bodies as in the stars and nebulae. Brodie noted William Miller and William Huggins' speculations and, more encouragingly, that few of the elements detected in the sun were of his third type; perhaps the sun was too hot for them to be formed. About ten years later he became very excited at Victor Meyer's possible decomposition of chlorine, on heating, to oxygen.

A discussion followed the lecture in which the atomists were thrown into disarray by Brodie's startling arguments. Williamson confessed his strong obligation to Brodie, and remarked that while he could not fully appreciate all the expressions used, he was sure that the calculus' introduction would "inaugurate an exceedingly important era in chemical language and notation." The two most interesting points were made by the specially invited physicists. Maxwell thought that the truth or falsehood of the atomic theory could be settled from dynamics; he referred chemists to his own derivation of Avogadro's law from the kinetic theory of gases. In the next few years, however, chemists took no notice, and the issue remained little affected by physical arguments. Stokes made the point that can always be made against such positivist constructions, that while the calculus took in all existing knowledge and made no assumptions, it would be liable to change with any increase in knowledge. Indeed, it was the new facts of isomerism, so naturally explained by an atomic theory, which led to the total neglect of Brodie's calculus. In 1879, Brodie asked Alfred Naquet, who translated his papers into French, for time to work on this problem, but he died shortly afterwards with no solution published.

49 Odling remarked to Sir Harold Hartley that until Cannizzaro's essay appeared in 1858, chemists were influenced only by chemical arguments and not by physical evidence. But this still seems to have been the case in 1867. Compare Hartley's foreword to L. L. Whyte (ed.), *Roger Joseph Boscovich ...* (London: Allen & Unwin, 1961).
50 *Phil. Mag.*, 1879, 7 (series 5): 480. For
and, though the problem was not insoluble, nobody else tried to complete the calculus.

To chemists the main interest of the calculus seems to have been the hope that it gave that the elements might be decomposed. At the discussion, E. W. Brayley and George Carey Foster were excited by this prospect; and when the lecture was republished in 1880, H. E. Armstrong, in a review, singled out for praise this anticipation of Lockyer's conclusions. Crookes' headline over his report of the lecture in Chemical News was "The Chemistry of the Future," with no question mark. Even outside chemistry, Brodie's notion of the atomic theory as an inefficient kind of abacus was bound to arouse attention, and in 1868 an anonymous article on "The Atomic Theory of Lucretius" appeared in the North British Review. The author claimed that the question of atoms was now as open as ever. Speculation on the issue was sometimes treated as impious, but the wave theory of light had led to new discoveries, and so might an atomic theory. In William Thomson's vortex atom theory the continuous fluid school of physicists had, rather oddly, arrived at atomism of a kind; but in chemistry the support given to the theory by Dalton, who had made it "heretical" to doubt atoms, had been undermined by Brodie whose ideas "seem independent if not subversive to the simple atomic faith."

**The Chemical Society Presidential Lecture of 1869**

It has been suggested by E. F. Caldin that a significant source of atomic skepticism was the positivism of the French scholar Auguste Comte, since this included the thesis that knowledge only consists in the description of the coexistence and succession of phenomena. But oddly, Comte's only English disciple was the defender of atoms, A. W. Williamson. Williamson's attitude was not that of Comte, nor was it, as we shall see, anything like the positivism of other English chemists. It might be said that while Williamson accepted Comte's sociology, and his positivism at a methodological level, he did not find this incompatible with atomism. In fact his attitude towards atomism is best described as pragmatic, and his intellectual model as Lavoisier, not Comte. So that, whereas for Comte and the later phenomenologists, atoms were only convenient pictures, for Williamson they


For a calculus that covers stereochemistry of elementary particles is, and ever must be, unknown to us, and therefore no proper object in The Concept and Role of the Model in Mathematics and Natural and Social Sciences (Dordrecht, Holland: D. Reidel, 1961).


54 Anon., North British Review, 1868, 48: 211.


56 "... and the real mode of agglomeration of elementary particles is, and ever must be, unknown to us, and therefore no proper object
had a real existence and properties that were capable of serious study. Atomism was a verifiable hypothesis, not a logical artifice, because empirical support and reasoning could be marshaled for it. But what kind of empirical support? After all, the atom was too minute to see. And yet seeing the atom was for some extreme positivists the only possible verification of the atomic theory. It was certainly not this kind of verification that Williamson had in mind, but rather (to use an expression of Whewell's) that he could marshal a solid consilience of inductions for it.

Williamson's belief in the atomic theory was expressed very forcibly in his famous Presidential Address that was delivered to the London Chemical Society on 3 June 1869. The title of the lecture was "On the Atomic Theory," and in its day, it was "generally regarded as the best exposition and defence of the doctrine yet made, and which may be consulted with profit by those desiring to obtain a clear statement of the principal research adduced for its confirmation." But when it is read today, its loose organization makes it rather dull reading unless something is known of the tide of skepticism it was designed to abate, and of the extraordinary discussion to which it gave rise in the autumn.

Williamson's contention was that it was time to accept the atomic theory unreservedly: atoms existed, the atomic theory gave a clear and consistent explanation of the facts, and it had valuable predictive powers. Reading from a handful of unarranged notes, Williamson professed himself shocked by the skepticism of his fellow chemists as disclosed by their textbook statements. After ineffectively quoting from several (without mentioning author or title), he declared:

It certainly does seem strange that men accustomed to consult nature by experiment so constantly as chemists do, should make use of a system of ideas of which such things can be said. I think I am not overstating the fact, when I say that, on the one hand, all chemists use the atomic theory, and that on the other hand, a considerable number of them view it with mistrust, some with positive dislike.

This was a poor state of affairs thought Williamson; a theory utilized, but privately discredited, or treated with reservations. If the atomic theory was so bad, then it was time it was replaced by something better. If not, covered by experiment, and enables us to compare them with one another and to classify them. 2. It leads to the anticipation of new facts, by suggesting new compounds which may be made; at the same time it teaches us that no compounds can exist with their constituents in any other than atomic proportions and that experiments which imply the existence of any such compounds are faulty.

then it deserved praise as "among the best and most precious trophies which the human mind has earned." For such reasons, Williamson felt that the case in favor of the atomic theory should now be put. His long review of the evidence for the atomic-molecular theory, which was by no means so cogently and brilliantly argued as Brodie's lecture two years before, was made from three main standpoints: the doctrines of equivalent weights, of molecules, and of valency. From each level, he was able to find support for atomism and hence claim an overall strong case for the atomic theory. As an example of Williamson's brief, we shall consider his discussion of equivalent weights.

After reminding his audience how equivalent weights were defined relative to the unit weight of hydrogen, and how the same element could have more than one equivalent, Williamson turned to the law of multiple proportions, which Dalton had explained with atoms. What was the evidence for this law? And with what proof, if any, did the law provide the atomic theory? Like Donovan thirty years previously, Williamson pointed out that there were involved in the very foundation of the law, hypothetical assumptions only justified by the law's agreement with a great number of experiments. The law seemed to be a valid generalization for simple inorganic substances, but did that justify its application to some of the complex substances of organic chemistry? Williamson believed it did, but he wanted to point out the assumption involved. For organic substances the proportional numbers were often quite high, so that a single quantitative analysis by itself could not decide the chemist in favor of one empirical formula rather than another. From single experiments the only conclusions would have been that some elements in simple compounds were capable of uniting with one another in simple multiples of certain weights. But Williamson knew of no chemist who limited the law of multiple proportions in that way (i.e. to equivalents).

It is applied to all elements and to all compounds of them, in spite of the fact that the usual results of observation require straining to agree with any multiple formula, and the common majority of substances have not yet been reduced by the process to any definite formula whatever.

Therefore, Williamson strongly criticized those chemists who, following tradition, claimed that the law of multiple proportions was a direct representation of experimental facts, whereas the atomic theory was a hypothesis.
independent of the law. He was able to carry Donovan's point over into the contemporary practice of the organic chemist.

The actual process by which we establish the composition of such complex bodies is by assuming that the composition of each of them must correspond to entire multiples of the atomic weights of their elements, and by treating as errors of observation any divergence between the proportions discovered by analysis and such atomic proportions.\(^6\)

These presuppositions unveiled, with what evidence did the law of multiple proportions provide the atomic theory? Atomism, he thought, gave a consistent and satisfactory explanation of variable equivalents, but the law certainly did not necessarily imply the atomic theory. It would have been possible, and logically consistent, to express say carbon dioxide and monoxide by the formulas CO and C\(_2\)O instead of CO\(_2\) and CO. This would allow the present carbon "atom" to be divisible, and

As far as the proportion of the elements is concerned, we have no better right to suppose the carbon is indivisible, and that the acid contains twice as much oxygen as the oxide, than we have to suppose that the carbon is divisible, and that half of the carbon is taken out of the carbonic oxide in forming carbonic acid.\(^7\)

Of course, this was saying no more than that elements possessed more than one equivalent whose values were obtained by dividing the atomic weights by the valency, e.g. C, C/2, Fe/2, Fe/3. However, it did enable Williamson to again press the point that whenever the law of multiple proportions was mentioned, chemists did not refer to fractional expressions like Fe/2, C/4, Cu/2, etc., but to the normal atomic symbols.

They describe in fact atoms as occurring thus combined with one another in the proportions of entire multiples of their weight; in fact the so-called law of multiple proportions has no existence apart from the atomic theory; those who adopt it seem not to be aware that they are using the notion of atoms, or else they are shy of mentioning it.\(^8\)

Nevertheless, Williamson was prepared to admit that the fact of varying equivalents by itself was as much evidence against the atomic theory as for it. Yet, he claimed, no chemist had ever used such variations in this way. Instead, when one of these skeptics

has ascertained by analysis the percentage composition of a compound, and wants to find its formula, he divides the percentage weight of each element by its atomic weight. He seeks for the smallest integral numbers which represent the proportion of atoms, and he attributes to impurity of his sample or to errors of analysis any deviation from the atomic formula thus obtained. He looks to the reaction of the body for aid in constructing his


\(^8\) Ibid., p. 339.
atomic formula, and controls his analyses by considerations derived from well established reactions, but whenever he is led by any of these considerations to a formula which contains a fraction of any atomic weight, he takes a multiple of the formula sufficiently high to be entirely free from such fractions. In no case does he reason on a basis independent of the atomic theory.**

Having pointed out the hypothetical assumptions involved in the “factual” law of multiple proportions, and having admitted that it by no means proved atomism, Williamson reminded his audience that a system should not be judged solely on the evidence of one of its parts.* This led him on to discuss at length additional support for atomism from the doctrines of molecules and valency.

Williamson considered his case proved and the opponent’s case a mere negation, and therefore valueless. Surely here was a golden opportunity to have tackled the challenge laid down by Brodie in the “Chemical Calculus”? But mysteriously, Williamson ignored it.

What was the atom? How should it be visualized? Did it have structure? In answer to such obvious questions, Williamson took the pragmatic line of Lavoisier and Kekulé. The atom, like the element for Lavoisier, was no absolute term.

In using the atomic language and atomic ideas, it seems to me of great importance that we should limit our words as much as possible to statements of facts, and put aside into the realm of imagination all that is not in evidence. Thus the question whether our elementary atoms are in their nature indivisible, or whether they are built up of smaller particles, is one upon which I, as a chemist, have no hold whatever, and I may say that in chemistry the question is not raised by any evidence whatsoever.**

Quite clearly the word atom, when it was applied to the elements, simply denoted the fact that they had not been decomposed under any known conditions.*** Whether the atom was divisible, possessed structure or not,

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** Ibid., p. 340.
*** See note 69.

* Williamson, “On the Atomic Theory,” p. 605. Our italics. There is more than a hint of Comte in this statement; but compare Lavoisier: discussions on the nature and number of the elements are metaphysical.

I shall, therefore, only add upon this subject, that if by the term elements, we mean to express those simple and indivisible atoms of which matter is composed, it is extremely probable that we know nothing about them; but if we apply the term elements or principles of bodies, to express our ideas of the last point which analysis is capable of reaching, we must admit, as elements, all those substances into which we are able to reduce bodies by decomposition. The elements might really be complex, but since “they act with regard to us as simple substances, we ought never to suppose them compounded until experiment and observation has proved them to be so.” Robert Kerr’s translation, Elements of Chemistry, . . . (Edinburgh: W. Creech, 1790), p. xxiv. In 1867 Williamson’s friend Kekulé, in an attack on Brodie, had said, “The question whether atoms exist or not has but little significance from a chemical point of view: its discussion belongs rather to metaphysics. In chemistry we have only to decide whether the assumption of atoms is an hypothesis adapted to the explanation of chemical phenomena.” Laboratory, 1867, i: 304, reprinted by Richard Anschütz, Auguste Kekulé (Berlin: Verlag Chemie, 1929), Vol. 2, pp. 364–369. Finally, Justus Liebig, Letters on Chemistry (4th ed. London, 1859), p. 105, had said that chemists used the term atom in a sense precisely analogous to the word element.

was not for the time being a chemical question, but a metaphysical one. All Williamson claimed was that there did exist least particles of a chemical nature which combined together to produce the chemical phenomena that he had so carefully reviewed. Chemistry demanded a corpuscular language of interpretation, and atomism was the most consistent explanation of the facts. But whether the atom was a solid ball, or an etherial vortex, Williamson "knew nothing about it."  

**The Autumn Debate of 1869**

The Presidential lecture had been planned to abate the tide of atomic skepticism, so Williamson must have been surprised when he found the opposition unconverted. There was no time for any discussion after his long lecture, or at the next meeting of the Chemical Society when Jean Baptiste Dumas delivered the inaugural Faraday lecture. But at the final meeting that summer, an organic chemist, E. J. Mills, presented some criticisms of Williamson's address. "No discussion followed" Mills' paper, "but the President (Williamson) said he should feel honoured if, after seeing in print what he himself had put together on the subject, they thought it worth while to go into the question some evening." This suggestion was acted upon at the earliest opportunity, namely at the first meeting of the Society after the summer vacation, on 4 November, when a "Discussion of Dr. Williamson's Lecture on the Atomic Theory" was held. Oddly, it does not seem that this meeting attracted anything like the same attention as Brodie's earlier lecture. The Chemical News did not publicize the meeting, ran no editorials, and only published an abstract of the discussion instead of the previous "veritable tour de force of the shorthand writer." Nevertheless, the discussion was afterwards described, like Brodie's lecture, as "a memorable and interesting evening in the life of the Chemical Society."  

The skeptical attitudes which we have analyzed for the first half of the nineteenth century all found spokesmen at the discussion. The published abstracts only report speeches by Williamson, Brodie, Edward Frankland, William Odling, William Allen Miller, John Tyndall, George Carey over in the abstract.
There were others present who spoke or interrupted, but since no minutes were kept, their names and contributions must remain unknown.

The occasion must have had an aura of theater about it, for with dramatic irony, the roles of Brodie and Williamson in 1867 were reversed with Brodie in the Chair. The biased Chairman immediately reestablished his own dislike of the atomic theory; and like a good disciple of Gerhardt and positivism, he urged that chemistry would be well rid of structural conceits since these deluded “chemists into the belief that they understood things about which they knew nothing.” It appears that without mentioning his calculus, Brodie argued that chemistry could only be based upon such experimental phenomena as the laws of gaseous combination, or atomic heats. Hypotheses about the physical divisibility of matter had to be separated from such fact, and the separation of facts from fiction was something that Williamson had failed to do.

Williamson naturally claimed that he had never confused the two. Brodie and he were not talking the same language, for their presuppositions were poles apart: a positivist versus a pragmatist. Once more Williamson stated that his sole aim had been to give the chemical evidence for atoms and molecules. A variety of independent chemical observations corroborated the theory which formed a “perfectly homogeneous whole.” Beyond the chemical evidence he was content to be as pragmatic as Kekulé, for “whether the particles of matter have a spherical form or not, whether they are in their nature indivisible, whether they are in reality the ultimate atoms of matter . . . he knew not, nor did such questions exist for him as a chemist.”

However, this pragmatic defense was insufficient for some of Williamson’s colleagues assembled at Burlington House. Frankland, still a skeptic, opposed any realist approach. Unlike Williamson, he taunted, he was “no blind believer in atoms.” Yet, since there did not seem to be, for the moment, any better explanation of chemical phenomena, it was right to use it as a “kind of ladder to assist the chemist.” We saw that this conventionality had been attacked by Williamson in his lecture, and it now rather

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88 Divers, op. cit., p. xli, reveals that Charles Wright was also present.
90 Ibid., p. 434. Perhaps Williamson could have expressed himself more clearly, as was certainly done by a correspondent to Chem. News after the debate had been reported. John Sprague suggested a compromise between the atoms and molecules of the chemist and the continuity revealed by mathematics (Chem. News, 1869, 20: 272):
Both sides are correct. The atoms are the ultimate forms of matter as we know it, in the form of the chemical elements. But no one can affirm, or would think of doing so, that these elements are positively the ultimate forms of matter itself. On the other hand there is a genuine feeling, justified by the remarkable relations between the atomic weights and the properties of the elements, that they are probably compounds of yet simpler forms of matter, though their decomposition may not be possible to man. The chemist was entitled to believe that matter is infinitely divisible and nonatomic in ultimate essence, yet appears atomic and indivisible in the forms with which he dealt empirically.
surprisingly called down the wrath of Brodie. It was nonsense to employ a theory in which you did not believe, or were willing to deny while exploiting its successes to the full. Brodie could agree with Williamson here. It was all or nothing: either one accepted atomism because one believed atoms existed, or one rejected atoms completely. There was no room for utilitarian compromise or for the Comtean claim that the atomic hypothesis was a useful logical artifice. Here, Frankland and Brodie illustrate two antagonistic aspects of the positivist's attitude towards theoretical entities: the extreme phenomenalists who only deal with observables, and the moderate conventionalists who are prepared to employ them as useful fictions.

Odling, who represents our "textbook tradition," also continued to be skeptical of atoms. He agreed with Frankland that the continuity or discontinuity of matter was a metaphysical question, but he did not think Williamson's lecture had shown that at all. Odling implied that Williamson claimed that chemical facts pointed to the discontinuity of matter, whereas of course, what Williamson had said, or meant to say, was that a discontinuity was shown. Odling could not agree that all chemists' actions were colored by the atomic theory, and he sought support from Davy's Dalton address of 1826. The laws of combination, he said quoting Davy, were like Kepler's laws of planetary motion, simply generalizations from observation; the atomic theory was superinduced upon them. Tyndall neatly turned the tables on Odling by pointing out that Newton had "superinduced" the acceptable gravitational theory upon Kepler's laws. Many years later, for reasons at present uninvestigated, Odling became a convinced atomist. At a banquet held by the Chemical Society in 1898, he publicly admitted that he had been mistaken to question Williamson's stand in 1869. Modestly, he said that he had always been content to follow in Williamson's footsteps, but that unfortunately he had more than once lagged some way behind.

It was now the turn of those who, for one reason or another, tended to side with Williamson in the controversy. Ironically, this support came chiefly from the physicists, although in his lecture, Williamson had paid scant attention to their arguments for atomism from kinetic theory. Miller, admitted that his attitude towards atomism owed much to Faraday, whose arguments he quoted. At the same time Frankland found chemical difficulties of his own because he pictured the atoms statically. He seems to have ignored the dynamic atomism of Williamson and the molecular physicists.

After the discussion, W. H. Walenn, an industrial chemist who may have been present, wrote that all Williamson's points were debatable. "When the measure of comparison is weight, it serves no purpose to suppose that matter consists of a collection of individual atoms; for these are not practically weighable, more especially as there is no proof to the eye, the touch, or other sense of the existence of atoms," Phil. Mag., 1870, 39 (series 4) : 122-126. Another member of this group was Berthelot, cf. note 57.

For example, Comte, Wollaston, and Prout.

See his own Manual of Chemistry (London, 1861), Part 1, pp. 2-3. Oddly, he had recently published a periodic table in which Cannizzaro atomic weights were crucial; cf. Quarterly Journal of Science, 1864, 1: 642.

Dalton had said this, too (Roscoe and Harden, op. cit., p. 159).


His only reference: "[Molecules] are also discovered by an examination of the mechani-
the spectroscopist, argued the useful fiction case from the atomist’s side. The human mind demanded that we explain any extensive series of phenomena. If we did not have the atomic theory then the laws of chemical combination would have to be explained some other way, and so far, as Williamson had said, no one had devised any alternative. Those who criticized had a duty to try and devise something equally good. Miller then repeated the parallel drawn by the anonymous writer in the *North British Review*; the wave theory of light was quite generally accepted by physicists, yet it was in a very similar position in physics to the atomic theory in chemistry. However, the physicists did not create a fuss about it!

The physicist John Tyndall thought Miller’s wave theory analogy extremely apt. It would be easy for an Odling-type physicist to say that the wave theory had been superinduced upon the simple phenomena of light, and hence that it was a mere fiction. But physicists were evidently better behaved than chemists, and the wave theory of light would stand so long as it explained all the facts. Tyndall returned to elaborate this point at the British Association the following year.

Many chemists of the present day refuse to speak of atoms and molecules as real things. Their caution leads them to stop short of the clear, sharp, mechanically intelligible atomic theory . . . and to make the doctrine of multiple proportions their intellectual bourne. I respect their caution, though I think it here misplaced. The chemists who recoil from these notions of atoms and molecules accept without hesitation, the Undulatory Theory of Light. Like you and me they one and all believe in an ether and its light producing waves.

Perhaps the most interesting contribution came from George Carey Foster, a chemistry pupil of Williamson’s who had turned physicist and become Professor of Physics at University College, London. No one present, he thought, wanted to deny that the atomic theory was useful; what they had to argue was whether the theory was true, and whether Williamson had proved this. The utilitarianism of Miller was all very well, but rather beside the point since one had only to look at the history of science to notice repeatedly erroneous theories which had been most useful in their day; e.g. the phlogiston theory of combustion, and (added Tyndall) the corpuscular theory of light. Foster then momentarily placed the whole discussion on a higher plane by pointing out that preconceptions concerning chemical composition forced the chemist to explain chemical phenomena corpuscularly. We think of a “compound” as composed of its elements. Consider mercuric sulphide: automatically we presuppose that the mercury
and sulphur occupy separate portions of space — that there is no interpenetration of these elements. Hence by mental division, we can separate mercury and sulphur from out of the smallest conceivable portion of cinnabar. However, if we could remove such corpuscular preconceptions, the process of chemical change that occurred when mercury "combined" with sulphur might very well be conceived in some other manner. Possibly Foster had in mind Brodie's recent efforts, or even the explanations of chemical change deriving from Aristotle and the alchemists and evident in Lamarck's chemistry. There seems to be no evidence that Lamarck had any influence at all in chemistry, let alone in England during the 1860's, but Foster certainly echoed his feeling that chemistry should be concerned not with matter or form, but with flux and process. Foster maintained that we might legitimately look upon chemical change as a transmutation, "the actual mutability of matter."  

We know that between the bodies which disappear and the body which appears, there are certain relations, not only qualitative but quantitative, the total mass of the disappearing substance being equal to that of the appearing substances; but we may perhaps return, sometimes, at any rate, with great benefit, to the notion that one portion of matter is actually transmuted into another; that it ceases to exist as such, but something else comes in place of it. From such ideas the existence of atoms would not follow of necessity, but with our present mode of stating and reasoning about chemical changes, an atomic hypothesis or basis appears to be inevitable.

The final reported speech by Edmund Mills was also of a philosophical nature, but the abstractor has so compressed his speech as to make it incomprehensible unless reference is made to the series of erudite papers which he published beginning in 1869. All the sciences, he claimed, converged to a common limit or "a most general idea." He longed for the law from which all phenomena might be deduced; but the most general idea against which all scientific ideas should be measured was, at the present time, motion.

In his application of his "universal criterion" to atomism, Mills had an approach that was far from empirical. He referred to Davy, Wollaston, and

100 Foster owed the example to the German thermochemist, Alexander Nauman, Grundriss der Thermochemie (Brunswick, 1869), pp. 5-6, which he referred to by its subtitle, Lehre von den Beziehungen zwischen Warne und Chemischen Erscheinungen (Relations of Heat to Chemistry). Compare Wilhelm Ostwald's phenomenalistic interpretation of compounds, Fundamental Principles of Chemistry, trans. Harry W. Morse (London/New York: Longmans, Green & Co., 1909), pp. 256-257.


Faraday as those whose arguments against atomism had never been answered, and he repeated Davy's empirical arguments for the complexity of the chemical elements. Isomerism was not explained by atomists, for space and position could only be relative, and to talk of position in empty space was to talk nonsense. Assertions about indivisibles, which had neither observation nor analogy to support them, could give no explanation, and neither could inadequately defined phrases. Instead the chemist had to show any phenomenon to be an instance of known and general phenomena. The law of definite proportions was tinged with an element of continuity: there were mechanical mixtures, definite compounds, and indefinite compounds like albumen. The law did not necessitate atoms, for definite ratios between curves going off to infinity were perfectly possible, and continuous substances could similarly have definite combining ratios. “Surrounded on all sides with continuity, motion, and change,” he wrote, “our most popular ideas relate to limits, repose, and stability.” The atomic theory was one of these.

The atomic and phlogiston theories might aptly be compared (as Brodie had done); atoms were invisible, so had been phlogiston; and atomic weights had changed with no less facility than the properties of phlogiston. Mills concluded that “the atomic theory has no experimental basis, is untrue to nature generally, and consists in the main of a materialistic fallacy, derived from appetite more than from judgement.”

Such arguments may make interesting reading, but we cannot imagine that they had much effect on the working organic chemists who had happily applied the ideas of valency and atomic structure to their otherwise intratable subject. In fact, we have it on record that Mills, although a competent and sometimes brilliant practical chemist, was dismissed as a metaphysician when it came to theory.

The Aftermath

There was one spokesman who was present at the 1869 meeting but was not reported. This was the organic chemist Colin Alder Wright, a pupil of Henry Enfield Roscoe and Carl Schorlemmer, and a pioneer of alkaloid chemistry. As it turned out, Wright brought the controversy over the atomic theory before the Chemical Society for a third and perhaps final time, in a protracted dispute with Williamson's assistant, Robert William Atkinson. Wright held to extreme views in the textbook tradition over what was fact and hypothesis in chemistry. How this applied to the new structural chemistry, he made the subject of a long paper read to the Chemical Society in February 1872. Wright aimed to show two things.
First, that the main facts and generalizations upon which chemistry was founded could be expressed in words, or represented by symbols, without in any way involving the use of a hypothesis of material atoms; and second, that although the atomic hypothesis supplied a clear *raison d'être* of such facts and generalizations, it did not account for all of them. Thus like Brodie, Wright claimed that atomism was both unnecessary and insufficient; but unlike Brodie, he believed that “the ordinary symbols do not necessarily involve the atomic hypothesis at all—that, by suitably choosing definitions, the symbol may be employed and yet the mind of the chemist be free from the atomic doctrine.”

The *Chemical News* was able to report, for the third time, that “a long and very interesting discussion ensued.” Frankland and Mills were present among others, but only Heinrich Debus appears to have tried to defend atomism. Wright concluded in the manner of Prout and Brodie that the proper use of atomism was as “an algebraic expression of facts rather than as an hypothesis, and although he could not go so far as Dr. Mills in considering it a chemical evil, he had no doubt that a textbook could be written without the employment of the atomic theory.”

Wright was not left unchallenged by the Williamson set; Atkinson dissected Wright’s arguments before the Chemical Society in May. However, the details of this later controversy need not detain us, for, not surprisingly, these polemics made no startling conversions of either party.

One thing that these long discussions had accomplished was to bring out sharply, for all to see, the undercurrent of atomic skepticism which had beset chemistry since the beginning of the century. It should be clear that until the work of the stereochemists in the 1870’s, an adequate and consistent nonatomic chemistry would have been a distinct possibility. In an editorial, the new weekly scientific journal *Nature* remarked of the 1869 debate:

> The general theoretical tone of the discussion last Thursday must have surprised most who were present. Our own position is necessarily an impartial one; but it will probably be agreed that between the parties there is a gulf, deeper and wider than at first appears, and perhaps unprovided with a bridge.

*Nature* was unduly pessimistic, even though the gulf did widen for a time, especially among physical chemists; for a bridge was ultimately found in the early years of the present century, but only at the expense of giving structure to the atom through the efforts of the unity of matter school.

Nevertheless, even in 1907, Edward Divers, a pupil of Williamson, and

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115 *Nature*, 1869-1870, 1: 44-45. The editor was the reductionist Lockyer.
his obituarist, could write of the 1869 affair, "but nothing came of it all, and chemists remain not much less divided on the subject now than they were then." Yet he admitted that Williamson's structural formulas, in contrast with Brodie's symbols, had turned out to be a "veritable calculus of chemical operations."  

The final word on the subject shall be Williamson's. In his farewell address to the Chemical and Physical Society of University College, he said:

A scientific theory, I suppose, ought to be the most condensed statement of general facts: and I take it that the atomic theory is that. To me it is nothing more. I do not use atomic reasoning, or refer to considerations relating to atomic operations, otherwise than as indicating limits which exist to our efforts to break up matter. Whether we like them or not they exist. There are many thinkers who prefer overstepping that boundary and discussing what there may be beyond it — whether, for instance, these limits by which we are stopped at present, and which I think it is well to recognise as existing in our present state of knowledge may not be overstepped. As a chemist I have nothing to do with that.

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118 Divers, *op. cit.*, p. xli. In 1902, in a British Association address, Divers had tried to formulate an atomic theory without hypothesis. His views were strongly attacked by Andrew N. Meldrum in his published thesis, *Avogadro and Dalton* (Edinburgh: W. F. Clay, 1904).


The Life and Work of William Prout

W. H. Brock

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THE LIFE AND WORK OF
WILLIAM PROUT

by

W. H. BROCK

He was an example of a man gifted by nature with high intellectual endowments improving those endowments by constant study, investigation, and reflection. An amount of professional labour, such as would have wearied many men, was daily performed by him; and from this he turned for relaxation to arduous chemical and mechanical researches. His mind was of that rare quality which is ever open to the reception of truth, and which steadily pursues that object, undismayed by difficulties, and indifferent alike to ridicule and neglect. . . .*

Nearly thirty years after the death of the English physician and chemist, William Prout, Munk recorded in his Roll of the Royal College of Physicians:® 'I am not aware that any full and searching estimate of Dr. Prout's merits as a philosopher and chemist has yet appeared.' Munk's remark remains true today, for no definitive life of Prout has ever been written, and indeed, Munk's own sympathetic account is still the most readily available biographical source. Such neglect is surprising, for Prout's name is a familiar one in textbooks of chemistry and physics associated with the unitary hypothesis that the chemical elements possess atomic weights which are integral multiples of the atomic weight of hydrogen.® He is otherwise featured in a minor way in histories of chemistry and medicine as the discoverer of hydrochloric acid in gastric juice, and as an early organic analyst.® The purpose of the present essay is to attempt to provide a satisfactory account of Prout's intellectual life based upon contemporary sources, his own writings, and some information kindly given by his descendants.

*Ibid., p. 110.
®Ibid., p. 110.
* Prout's lecture notes were passed to me for examination. Fortunately—or unfortunately—they do not contain any significant alterations to the present paper, although they considerably expand our knowledge of Prout's ideas on the nature of matter.
®A full text will be published elsewhere.
The neglect of Prout's work had already set in long before he died in 1850. At least two reasons may be proposed for this. First, Prout's chemical career was essentially finished by 1834 when he published his Bridgewater Treatise; thereafter he devoted himself almost entirely to his medical practice and the revision of his books. Second, as these revisions show only too clearly, Prout was unable to keep abreast of contemporary developments in chemistry and physiology, so that although much of his research had foreshadowed that of Liebig and his school, he found himself and his work eclipsed by their achievements in the 1830s and 1840s. The Royal Society and the Chemical Society ignored his death and it was left to the physicians to pay him tribute in the chief medical journals of the day.

William Prout was born on the 15 January 1785 at Horton, near Chipping Sodbury, in Gloucestershire, where his family had lived on their own property for some generations. His parents were of farming stock—the father, John Prout, died in 1820 'in consequence of having run a thorn into his hand which occasioned a locked jaw'. Like many other nineteenth-century physicians of humble origin, Prout's earlier education was almost negligible. Although he learned to read and write at a local Dame school, and later at Badminton village school, this elementary education had ceased by the age of thirteen. From then until he was seventeen, little is known of him except for the report that during his youth he suffered from an intense earache—no doubt a forerunner of the deafness which later forced him to withdraw from scientific society. At the age of seventeen Prout became critically aware of his own educational deficiencies, and with an awakened interest in mechanical things, mathematics and music, he determined to engage upon some systematic learning. With this aim he left home for some eighteen months between 1802 and 1804 to join a private Academy at Sherston in Wiltshire run by the Rev. John Turner, Vicar of Horton and Luckington in Wiltshire. Here he acquired the rudiments of Latin and Greek—an essential training for a university course, whether or not he yet had that ambition. However, Prout returned home, either dissatisfied with his own progress, or with the standards of Turner's Academy, since some time in 1805-6 he took the extraordinary step of advertising in a local newspaper for advice on the prospects for further learning for an ill-educated twenty-year-old.

A reply came from another clergyman, the Rev. Thomas Jones (1758-1812) who ran a classical seminary at Redland, Bristol. Jones had been educated at Cambridge and Dublin, and had been Vicar of two Devonshire parishes before he opened his 'classical seminary for young gentlemen' at the turn of the century. Prout spent two happy and formative years with Jones. In return for his tuition, he taught the younger pupils of the Academy, and stimulated by a pupil's curiosity in chemistry (these were the exciting days of early electro-
The Life and Work of William Prout

chemistry), Prout himself began to form what was to become a lifelong passion for the subject. It was Jones who urged Prout to become a doctor and recommended him to enter the University of Edinburgh. (Oxford and Cambridge were naturally out of the question as Prout's social status was so low.) Thus in 1808, at the mature age of twenty-three, Prout went to Edinburgh armed with a letter of introduction to Jones's old teaching friend, Dr. Alexander Adam, Rector of the Edinburgh High School. He remained in Edinburgh for the three-year period except for visits to the country villages of Duddingston and Morningside during the summer vacations. These were sufficiently close to the university (we are told) to ensure full use of the library; however, we may suspect that Adam's elder daughter, Agnes, was an additional Scottish attraction. Nothing is known of Prout's days as a medical student, for like the vast majority of undergraduates he left no mark upon his university, or the medical societies of Edinburgh.\(^{12}\) His teachers would have been Monro tertius, Duncan and Hope, and among medical students contemporary with him were Marshall Hall, John Davy, Henry Holland, and the man who evidently became one of Prout's closest friends, John Eliotson.\(^{13}\) Prout graduated M.D. on 24 June 1811, with a thesis on intermittent fevers which contained no original features: it is a straightforward academic review of fevers proceeding by way of definitions, symptoms, causes, pathology, prognosis and treatment.

After graduation, Prout left Edinburgh and took rooms off Leicester Square in London where, until he gained the licentiate of the Royal College of Physicians on 22 December 1812, he walked the wards of the United Hospitals of St. Thomas's and Guy's.\(^{14}\) In this way he came into contact with the great surgeon, Astley Cooper, and the animal chemist, Alexander Marcet. Armed with his licence, Prout set up a practice at 4 Arundel Street, just off the Strand, and it was here in 1813 that he began his active career as a chemist and physiologist. Thomas Thomson on his return from Sweden in 1812 had begun to publish an important monthly periodical, the *Annals of Philosophy*,\(^{15}\) and it is from early issues of this journal that we learn that in 1814 Prout delivered a course of lectures at his home, 'the attendance on which though small was select, and so highly was he already esteemed, that his audience included Astley Cooper'.\(^{16}\) Two published advertisements read:

Dr. Prout intends in the course of the winter, to deliver a series of lectures on Animal Chemistry. The object of these will be to give a connected view of all the principal facts belonging to this department of chemistry, and to apply them, as far as the present state of our knowledge will permit, to the explanation of the phenomena of organic actions.\(^{17}\)

\(^{12}\) I owe confirmation of this to Dr. Douglas Guthrie.

\(^{13}\) Harley Williams, J. H., *Doctors Differ*, London, 1946, Part 2. The friendship is not noted in this short biography of Eliotson.

\(^{14}\) *De febris intermittentibus*, Edinburgh, 1811, 27 pp. There is no dedication or indication of membership of student societies. Two copies Edinb. Univ. library.

\(^{15}\) Munk, *op. cit.*, p. 109. The oral Latin examination for the licentiate consisted of questions in physiology, pathology, therapeutics and the interpretation of a passage from either Celsus or Sydenham.

\(^{16}\) *Ann. Philos.*, October 1813, 2, 312.
Dr. Prout will commence a course of lectures on Animal Chemistry on Friday, February 18 [1814], at half-past eight in the evening. These lectures will be given at his residence, 4 Arundel Street, Strand, and will be continued weekly at the same hour.  

Lecture courses in private houses were not uncommon at this time, although accounts of them were rarely published. From hints in Prout's publications, we may conjecture that he lectured on respiration and urine chemistry.

The year 1814 was an extremely busy and eventful one for Prout since, besides his lecture course which would have brought him forward prominently in the circle of London scientists, he was elected a Fellow of the Medico-Chirurgical Society, he was married, he visited Paris, he continued analysing animal materials, and he performed his offices as a professional physician. His election to the flourishing Medical Society took place on 10 May, and his association with it continued for many years. He read several papers to its members, served as a member of Council from 1817 to 1819, and as a Vice-President in 1823 and from 1833 to 1835.  

Prout was married on 22 September 1814 at St. John's Church, Westminster, to Agnes Adam (1793–1863). Europe was momentarily at peace, so the Prouts were able to pay a honeymoon visit to Paris where, like Humphry Davy before them, they were able to have a private view of the paintings and treasures which Napoleon had collected during his campaigns with the aid of Baron Dominique Denon. On their return to England, the Prouts settled at Southampton Street, Bloomsbury, where a daughter was born to them in 1815. The child only survived a few months, but there were six further children. A son, John William, who became a lawyer, was born in 1817; a second son, Alexander Adam, of whom little is known, came in 1818; Walter, born in 1820, lost his life as a major during the Indian Mutiny; a fourth son, Thomas Jones, was born in 1823 and became a clergyman and classics don at Christ Church, Oxford. There were also two daughters, Elizabeth born in 1825, and Agnes in 1826. The house where the family resided from 1821 to 1850 (40 Sackville Street, Piccadilly) is no longer standing.

Early analytical work

Apart from the dissertation of 1811, Prout published nothing until 1813. However, a review note on progress in physiology for 1820 written by a 'friend' of Thomson for his *Annals* (it is possible that the writer was Prout) discussed

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19 There was an 'Animal Chemistry Club' or 'Society for the Promotion of Animal Chemistry' whose membership included Brodie, Home, Davy, Babington, Brande and Clift, but this had 'degenerated into a mere dining club' by 1813.
20 At *Gent. Mag.*, October 1814, p. 392. For Adam, see *D.N.B.* Agnes was his eldest daughter by a second marriage.
21 I have to thank Lt.-Colonel P. E. H. Warner, M.B.E., M.C., Prout's great-great-grandson, for these details.
23 A passage in the review on the nervous system also hints at Prout's authorship. 'It would be well for physiology if its cultivators would leave for a while this abstruse and difficult subject and turn their attention to something more within their power; [like the] chemical and mechanical constitution of organic bodies; for till this is known, it is evident we can hope for very little progress in physiology.' *Ibid.*, p. 113. A similar plea was made by Prout in his Gulstonian lectures. However, Prout's paper on blood (1819) was not mentioned in the review. Would he have ignored this and yet have resurrected an idea from 1811?
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Blainville's theory that teeth were analogous to hair and nails, and mentioned that:

A similar opinion was advanced in 1811 by Dr. Prout, who at that time drew up the sketch of a paper, the object of which was to prove that the teeth are to be considered as appendages to the integuments, and to be classed with horns, nails, &c. The opinion was principally founded upon extensive anatomical inquiries, showing the analogy between the formation of the teeth, horns, feathers, &c., and partly also upon physiological and pathological reasonings. The paper was never published, owing to reasons which need not be mentioned, but the opinion was stated to many of the author's friends at the time, and he intends at some future opportunity to lay the subject before the public in extended form.

Needless to say, no paper on this subject was ever published by Prout, and his first paper written in June 1813 was of a histochemical character. It would seem, therefore, that his earliest interests were anatomical. The problem which Prout had examined was how to stain the blood vessels of anatomical specimens by the old art of anatomical injection.® ® After several trials he had found that a saturated solution of potassium ferrocyanide and dilute ferric sulphate would successfully plant prussian blue in the morbid tissues. The injections were made through a syringe pipe after the solutions had been heated to 100°F., but the order in which the solutions were injected was immaterial. In an investigation of the vascular nature of the ox's eye, Prout found that 'the vessels of all parts of this organ appear to communicate freely with one another; the part least connected with the rest is the retina, and this is supplied by its own proper artery'. He successfully stained the lens capsule and decided that the hyaloid membrane 'in the adult state at least ... derives all its vessels from the great arterial communication situated a little behind the ciliary ligament, and not from the retina, as usually stated'.

In the same year (1813) Prout published his first paper in Thomson's Annals, 'Observations on the Quantity of Carbonic Acid Gas Emitted from the Lungs during Respiration, at different Times, and under different Circumstances'.® ® His intention was to determine both whether the quantity of carbon dioxide exhaled in the breath was constant throughout the day, and constant for the individual. The analyses were made on himself with the aid of a breathing apparatus similar to the modern spirometer.® ® A strict regimen was necessary 'which consisted in keeping myself as nearly as possible in the same state in every respect'. He stuck to this 'arduous' discipline for nearly three weeks during August 1813, 'making experiments every hour, and sometimes oftener, during the day, and occasionally during the night also'.® ® Prout thought he perceived a pattern in his results, and he stated as a law:

The quantity of oxygen consumed, and consequently of carbonic acid formed during respiration, is not uniformly the same during the 24 hours, but it is always greater at one and the same part of the day than at any other, that is to say, its maximum occurs between 10 a.m. and 2.0 p.m., or generally between 11.0 a.m. and 1.0 p.m.; and its minimum commences about 8.30 p.m., and continues nearly uniform till about 3.30 a.m.® 5

® He made it clear that he was measuring potential carbon dioxide formation.
® Op. cit., p. 348. He appears to have worked at night on twelve occasions in three weeks.
® Ibid., p. 339.
Prout was of course misled by his regimen and ignorance of cellular chemistry; however, all of his results are explicable in terms of the influence of $p$HCO$_3$ on the oxygen dissociation of haemoglobin, and nervous stimuli. In a second generalization Prout foreshadowed the findings of Haldane and Priestley:

Whenever the quantity of oxygen gas consumed, and consequently of carbonic acid gas formed, has been by any cause increased, or raised above the standard of the period, it is subsequently as much decreased or depressed below that standard, and vice versa.®

His experimental work included investigating the effects of exercise, eating and drinking (alcohol and tea), and the 'depressing passions' (i.e. emotional states conducive to yawning, sighing, or deep inspirations). Prout speculated that a diurnal cycle—the presence or absence of the sun—might bring about these observed variations, and he returned to this hypothesis the following year in a further paper® with an elaborate graphical plate which showed how the increase in carbon dioxide exhaled 'always uniformly occurred soon after the commencement of twilight, and before sunrise'® throughout the year. The increase was greatest when the nights were longest, and there was a uniform lessening as the nights decreased in length, 'a circumstance which, however, appears to have been chiefly owing to a diminution having taken place in the usual minimum quantity towards the morning, either probably from the fatigue of watching or from drowsiness'.® This remarkable erroneous observation seems to have been ignored by all of Prout's contemporaries who were little interested in such physiological experiments.

Much more attention was paid to Prout's anonymous papers of 1815 and 1816 which dealt with the calculation of the specific gravities (i.e. relative densities) of the elements from the published data of other chemists.® Among many results, he was able to give an excellent value for hydrogen which, owing to its lightness, had been extremely difficult to determine experimentally with any accuracy. Although when read today it is possible to see that the more important theme of Prout's papers was Avogadro's hypothesis that equal volumes of gases under identical physical conditions contain the same numbers of molecules, the papers were written in such a confused 'hasty and imperfect manner' that this molecular hypothesis became overshadowed for the contemporary reader by the protyle hypothesis. Thus it was not clear from these papers alone what Prout understood by volume, atom, and molecule; but it was clear from his results that there was a remarkable connection between the atomic weights of the elements on the hydrogen scale. The suggestion that the chemical elements were condensed from hydrogen atoms became known as 'Prout's hypothesis' and it attracted the attention of analysts and theorists throughout the nineteenth century.® Although these papers were written anonymously, Prout quickly identified himself as the author when he found his ideas had been accepted by so eminent a chemist as Thomas Thomson.® Prout was content to

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®® Prout submitted his papers with the greatest diffidence under the cloak of anonymity, yet subsequently went out of his way to acknowledge his authorship on many occasions. Thomson's revelation is well known, Ann. Philos., November 1814, 4, 331–7.  
®® Ibid., p. 333.  
leave the promotion of his speculation to the status of a law to Thomson while he himself returned to organic analysis where, during the decade 1815–25, he attained a considerable reputation both in Great Britain and on the Continent.

Prout continually searched for a perfect technique of organic analysis. A good example of his qualitative method is his analysis of a snake's excreta in 1815. The faeces of a boa constrictor, then currently on exhibition in the Strand, was dissolved in hydrochloric acid and the insoluble portion shown to be uric acid by the murexide test. The soluble portion was found to consist of lime (by an ammonium oxalate test) and ammonium chloride. Prout was obviously surprised that the excrement was almost pure uric acid (90.16%), for he wondered whether the serpent had become diseased through captivity, though he recalled that Wollaston had previously demonstrated that the bird-droppings from South America called guano also comprised uric acid. Confirmation of Prout's analysis soon came independently from his friend John Davy in Ceylon, and from Davy's cousin, Edmund Davy in 1819.

In the gravimetric analysis of organic substances, Prout demonstrated how Wollaston's equivalent slide-rule, or Synoptic Scale of Equivalents, could be used to work out—what we would now call—the empirical formula of a substance assuming, as Berzelius had maintained, that organic substances obeyed the law of definite proportions. At this time, c. 1815, Prout's method of analysis was still the technique introduced by Gay-Lussac and Thenard of oxidation by potassium chlorate 'in an apparatus somewhat similar to [that used by] Berzelius'. Prout stressed the necessity for drying materials to be analysed and suggested the use of sulphuric acid for this purpose in a vacuum apparatus of his own design. When Gay-Lussac introduced the use of copper oxide in 1816, Prout rapidly developed a spirit-lamp apparatus which was 'susceptible of far greater precision, and is much less troublesome to use than any that has hitherto been recommended for the analysis of organized substances'. As a comment on this, Daubeny later wrote:

The greater part of Dr. Prout's analyses were made with an apparatus of his own which, however ingenious it might be, was far more difficult to use, and required for its success many more precautions than that at present in the hands of chemists, and hence the precision to which he attained is the greater subject for commendation. Add to which, that these delicate investigations were carried on by him, unassisted, amid constant interruptions, at intervals snatched from the daily demands made upon his time by professional engagements.

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24 'I soon found my progress obstructed by insuperable difficulties. The first and chief of these was the want of accurate data; and the infinity of objects comprehended by chemistry prevented the hope of acquiring by individual exertion, however unremitting, a sufficiency for the establishment of general laws. Professional duties still further limited my exertions, and at length obliged me to relinquish chemistry in general, and confine my attention solely to the chemistry of organic substances.' Phil. Trans., 1827, p. 354.


27 Ann. Philos., 1816, 6, 259–73. Wollaston's slide-rule, which was sold by instrument-makers, had been described to the Royal Society in 1813; Phil. Trans., 1814, pp. 1–22. At this time, of course, empirical and molecular formulæ were thoroughly confused.


Eventually, in his constant quest for accuracy, Prout returned to Lavoisier’s method of analysis by direct oxygen combustion. But long before this development he was able to claim:

I have for several years been engaged in the analysis of organized products, and have at length extended my researches to almost every distinct and well-defined substance. The results, when compared with one another, are most interesting, and seem to throw no small light not only on the nature of chemical compounds in general, but upon many important points connected with animal and vegetable physiology and pathology.

This statement brings us to his work on urine and digestion.

Prout’s interest in the chemistry and pathology of the urine dated from the lecture period of 1814, and his researches on the analysis and chemotherapy of diseased urine were fully expounded in a paper which he read before the Medico-Chirurgical Society on 24 June 1817. He had been the first person to obtain a really pure sample of urea from urine in 1814, and his method, which involved the use of animal charcoal, became the standard technique in chemical textbooks. After he had carefully described the chief chemical and physical properties of urea, Prout reported on its analysis. The atomic weight values he used were those of the anonymous paper of 1815, and the proportional number (i.e. empirical formula weight) of 37.5 which he calculated was employed by Wöhler in his famous paper on the synthesis of urea ten years later. Prout also gave analyses of urea nitrate, sugar, sugar of milk, and diabetic sugar, but he found so little difference between the sugars that he concluded ‘I am inclined to think the primary and simple saccharine principle is composed of one atom of each element, and that the varieties in its external characters are to be attributed to the influence of the presence of minute portions of foreign matters, analogous, for example, to what occurs in the mineral called aragonite.’ Ten years later he elaborated this speculation into the cumbersomely titled concept of ‘merorganization’. The chemical portion of the paper was concluded with three generalizations:

1. The atomic theory or theory of definite proportions, holds good in all these instances.
2. The above compounds appear to be formed by the union of more simple compounds, as urea of carburetted hydrogen and nitrous oxide, lithic acid of cyanogen and water, &c.; circumstances which render almost certain that their artificial formation falls within the limits of common chemistry.
3. The remarkable relation found to subsist between urea and sugar, seems to explain in a very satisfactory manner the phenomena of diabetes, which may in fact be considered to consist in a depraved secretion of urea. Thus the weight of the atom of sugar (18.75) the absolute quantity of hydrogen in a given weight of both is equal, while the absolute quantities of carbon and oxygen in a given weight of sugar are precisely twice those in urea.

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W. H. Brock

2. Med. chir. Trans., 1817, 8, 526-49. There is a bound offprint in Edinb. Univ. library which bears the dedication, ‘Dr. Duncan, Sen. with the authors best respects.’ Andrew Duncan (1744-1838), Prof. of Physiology, was one of Prout’s teachers at Edinburgh.
6. Ibid., pp. 540-1.
7. My italics. This generalization is an early example of the radical theory. Prout evidently made several attempts to synthesize urea as he ruefully informed readers of his later clinical text, On Stomach and Renal Diseases, 5th ed., 1848, p. 530. All references will be to this ed., cited as On Stomach.
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Such analyses afforded Prout 'glimpses of laws that will hereafter be found to influence the whole system of Nature's operations'. The Pythagoreanism, or fixation with numbers, first noticed in the anonymous paper of 1815, which reappears in the last paragraph of this quotation, was returned to in a further paper on the chemical constituents of urine in 1818. Since the previous paper Prout had found many imperfections in his analytical methods, and after a tabulation of corrected results he drew the reader's attention to 'the extraordinary relations that exist among the ... numbers'. The molecular weights of sugar, urea, uric acid, cystine (the presence of sulphur was overlooked) and oxalic acid, seemed to form a simple arithmetical series. From here it could have been but a small step to the metamorphosis theme of the Bridgewater Treatise where molecules underwent reduction or completion to the designs of organic agents.

<table>
<thead>
<tr>
<th>Substance</th>
<th>H</th>
<th>C</th>
<th>O</th>
<th>N</th>
<th>Mol. wt.</th>
</tr>
</thead>
<tbody>
<tr>
<td>Sugar</td>
<td>6.66</td>
<td>40.00</td>
<td>53.33</td>
<td>—</td>
<td>187.5</td>
</tr>
<tr>
<td>Urea</td>
<td>6.66</td>
<td>20.00</td>
<td>26.66</td>
<td>46.66</td>
<td>37.50</td>
</tr>
<tr>
<td>Uric acid</td>
<td>2.22</td>
<td>40.00</td>
<td>26.66</td>
<td>31.11</td>
<td>56.25</td>
</tr>
<tr>
<td>Cystine</td>
<td>5.00</td>
<td>30.00</td>
<td>53.33</td>
<td>11.66</td>
<td>75.00</td>
</tr>
<tr>
<td>Oxalic acid</td>
<td>4.44</td>
<td>20.00</td>
<td>75.55</td>
<td>—</td>
<td>112.5</td>
</tr>
</tbody>
</table>

A second part of the 1817 paper was addressed to the medical practitioner. Although empirical in theme and content, Prout firmly stated his belief that 'reason will become triumphant eventually, so that chemotherapy will be placed upon a deductive basis'. Unlike many physicians, Prout had no wonderful remedy to offer for the stone; the only real solution was the knife, for 'when a calculus is once formed, a further enlargement is probably a common chemical process, and will proceed whether the urine be healthy or not, for all the urine naturally contains the ingredients most commonly met with in calculi'. He was very sceptical of so-called chemical remedies both because of their potentially dangerous side-effects, and because 'the object of the chemical practitioner is ... to prevent the effects of diseases rather than to remove them'. He mentioned that he had practised uromancy for several years before he had met Charles Scudamore, 'who I found entertained similar views, and had prosecuted the subject much further than I had done'.

Prout's work with urine for which he devised a special hydrometer led him to the discovery of a substance Wollaston and he named purpuric acid. This research formed the subject of Prout's first communication to the Royal Society to whom it was read by Wollaston, and it led directly to his election to a
Fellowship in March 1819. The proposal was made in the hand of Alexander Marcet and was countersigned by Wollaston, Warburton, Koenig, Roget, Leigh Thomas, Blane and Baillie. The purple colour produced by the action of dilute nitric acid on uric acid had been first described by Scheele, the discoverer of lithic or uric acid. Prout now explained the origin of the colour as due to an ammonium salt of an acid—to be called purpuric 'from its remarkable property of forming compounds with most bases of a red or purple colour'. After accurately describing its preparation, he explained how the acid could be freed from the ammonia of the dark red crystalline salt by the addition of a mineral acid. However, as Prout later realized, it was not actually the free acid that was prepared in this way, but the substance Liebig called murexid. The remainder of his paper was devoted to a description of a variety of inorganic purpurates. He wrongly believed that ammonium purpurate was responsible for the pink sediment 'in urine of those labouring under febrile affections', and he speculated that purpuric acid and its compounds were the basis of many animal and vegetable colours, and hence that it might prove of industrial and artistic use.

Another new acid was announced in 1822 when Alexander Marcet read a paper to the Medico-Chirurgical Society on 'a singular variety of urine which turned black soon after being discharged'. Marcet had obtained the sample from Babington way back in 1814; the patient had been a male child of seventeen months who was evidently suffering from alkaptonuria. Marcet had retained the sample and only recently sent it to Prout for analysis. The latter reported that the black colour was due to an unknown principle combined with ammonia which he appropriately dubbed melanic acid. As Partington has pointed out, this was homogentisic acid. The fact that Marcet, who was more than capable of making such an analysis himself, sent the sample to Prout, illustrates the position which Prout had attained as an expert in urine analysis. As for the reasons for his researches, Prout stated in the same year:

The views which I published some years ago respecting the atomic theory, seem to be now generally known in this country. These views at the time led me to others which I was exceedingly anxious to verify; and as I was interested, for other reasons, in the composition of organic substances, it struck me that by submitting these substances to analysis, I might not only obtain a knowledge of their composition, but by investigating the laws which might regulate the union of the elements, hydrogen, carbon, oxygen, and azote, be able to obtain an insight into the laws which regulate the union of other elementary principles. With these views, therefore, I set to work, and after a very great labour, and no trifling expense in apparatus, &c., succeeded, as I supposed, in analyzing more or less perfectly almost every well-defined and crystallized organic substance that I could procure. A few of my earlier results...
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were published, perhaps prematurely, but the great mass, as is well known to several of my friends, still remain by me, nor have I for various reasons, the least inclination to publish them at present.®

Unfortunately, it would seem that little of this analytical mass was subsequently published. We may suspect that Prout was a perfectionist and that he was unprepared to publish authoritative analyses until he was certain that his technique was accurate and not open to improvement.

The whole of Prout’s work on urine was elaborated into a book which established his reputation as a chemist and practical physician”.® In a preface to the Inquiry into the Nature and Treatment of Gravel, Calculus and Other Diseases of the Urinary Organs (1821),® Prout mentioned ‘his original intention to prefix an historical introduction respecting the urine, with a detailed account of the chemical experiments on which many of his peculiar views are founded; but upon reflection he was induced to relinquish both these objects for the present, and to confine his attention chiefly to practical points’. Indeed, the practical tone of the book remained paramount in all five editions—a misfortune to us, and a source of justifiable irritation to his reviewers.

Work on Digestion

There was a short-lived medical journal called The Annals of Medicine and Surgery; or Records of the Occurring Improvements and Discoveries in Medicine and Surgery and the immediately connected Arts and Sciences, which appeared quarterly during 1816-17. The editors are not definitely known, although there is a suggestion by one of Prout’s obituarists that they were Prout and his friend John Elliotson.® Unfortunately, none of Prout’s published writings makes any allusion to the editorship, and it has not proved possible to demonstrate the association of Prout and Elliotson with the journal from internal evidence. However, it is not impossible, for Prout and Elliotson were good friends; Elliotson frequently gave pathological samples to Prout for analysis,® and Prout’s suggested iodine treatment of goitre was successfully performed by Elliotson at St. Thomas’s Hospital.® But the evidence is conflicting and the matter is by no means settled.

It was in this journal, however, that Prout published the first three parts of

® Munk, op. cit., p. 109.
® London, 1821, 277 pp. A hand-painted endpiece was designed to illustrate the various colours assumed by sedimented urine. A 2nd ed. which appeared in 1825, retitled Inquiry . . . Nature of Diabetes, Calculus, and other Diseases, etc., 328 pp, with index, contained a handsome pull-out sheet of calculi illustrations. These engravings by Lunn were also published separately as A Synoptical View of Urinary Calculi, 12 x 9 in. (copy R.C.P.). There were French and German trans. of 1st ed., and an American printing of 2nd ed. in 1826.
® Edinb. med. surg. J., 1851, 76, 144±. ‘It has been said that this journal was conducted by Dr. Elliotson and Dr. Prout; but the correctness of this statement we have no means of ascertaining. . . .’ See my query concerning the editorship, Med. Hist., 1964, 8, 291-3.
® e.g. Med. chir. Trans., 1818, 9, 474 (blood sample); ibid., 1819, 10, 330 (calculus); ibid., 1833, 18, 82.
® Prout, W., Chemistry, Meteorology and Function of Digestion, London, 1834, p. 100n. All refs. to this ed. cited as Chemistry. He had used potassium iodate as a remedy for goitre in 1816 ‘after having previously ascertained by experiments on himself that it was not poisonous in small doses. . . . The above employment of the compounds of iodine in medicine was at that time made no secret; and so early in 1819, the remedy was adopted in St. Thomas’s Hospital by Dr. Elliotson, at the author’s suggestion.’
an ‘Inquiry into the Origin and Properties of the Blood’. Later he evidently felt
that the *Annals* had been too limited in circulation, for he had the paper
republished in a slightly modified—but again incomplete—form in Thomson’s
*Annals of Philosophy* in 1819. The missing part which entailed a long chemical
investigation of the chicken’s egg was finally presented independently to the
Royal Society in 1822. As the three papers on sanguification show clearly,
Prout was involved in the problems of digestion as far back as 1816 when he
announced that

the blood begins to be formed, or developed from the food, in all its parts from the first moment
of its entrance into the duodenum, or even, perhaps, from the first moment of digestion, and
that it gradually becomes more and more perfect as it passes through the different stages to
which it is subjected, till its formation be completed in the sanguiferous tubes, when it represents
an aqueous solution of the principal textures and other parts of the animal body to which it
belongs.

At this time, he divided the process of blood formation into four stages: (1) diges­
tion (confined to the stomach), (2) chymification (confined to the duodenum),
(3) chylification (confined to the lacteals), (4) sanguification proper (confined
to the blood vessels). Thus we can see how Prout was led to a complete study
of digestion as part of a larger programme of physiological research.

In the review of digestion he described an examination of the contents of a
rabbit’s stomach some two hours after feeding; the food mass was acidic and
analysis showed ‘traces of alkaline muriate (chloride), with slight traces of an
alkaline phosphate and sulphate; also of various earthy salts, as the sulphate,
phosphate and carbonate of lime’. A similar acidity—a well-authenticated
observation in contemporary literature—was found in the stomachs of a pigeon,
trench and mackerel. As the heterogeneous nature of the fluids in an active
animal stomach had caused much confusion, he proposed to divide the contents
into saliva, the mucous coat and exhalents of the stomach, and the gastric juice
proper whose identity was ‘unknown, it never having been obtained in a
separate state’. It is evidently some volatile acid, from its effects on litmus
paper . . . I considered it in the pigeon as carbonic. There appears, however,
to be occasionally another acid which is of much more permanent nature, and
it is probably the phosphoric acid. . . .’ Thus, at this period, 1816–19, Prout
was far from identifying the gastric acid with hydrochloric acid.

Prout had analysed the chymes of several animals, including dogs and rabbits,
in order ‘to ascertain if the chyme exhibited any traces of the albuminous
contents of the blood’, since the stomach contents had not. From comparative
analyses of the ‘chymous’ contents of the duodenum, caecum, colon and rectum,
he detected the presence of an ‘incipient albumen’ and concluded that such

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71 *Ann. Philos.*, 1819, 13, 12–25, 265–70. There were French and German trans. of the 1819 version. The
p. 277), was later published as ‘Experiments on the changes which take place in the fixed principles
of the egg during incubation’, *Phil. Trans.*, 1822, pp. 377–400.
changes as occurred in the alimentary tract were of a purely chemical nature and therefore probably reproducible under laboratory conditions. He suggested that similar comparative analyses should be made of the chyle from various parts of the lacteal system. It did not seem possible to separate the fourth stage of actual sanguification from another physiological process: respiration. However, animal chemists remained quite ignorant of the actual respiratory process and purpose. Prout believed that one function of respiration was to convert chyle into blood by the removal of unwanted carbon, but was ‘the carbonic acid given off as carbonic acid by the blood? and an equal volume of oxygen gas absorbed; or was the carbon only given off, which, by combining with the oxygen of the atmosphere, forms the carbonic acid?’ It was clear that both animal heat and the assimilation of food were in some way connected with respiration, but Prout thought that ‘from the vital character of the processes, we shall probably ever remain ignorant of their precise nature’.

This paper of 1816, and its reprint in 1819, raised far more questions than it answered, and today it would be classed as a review. Prout had originally planned a book on digestion, and even composed much of it, for imminent publication was twice announced in the forthcoming books list of Thomson’s Annals. It was to be called Observations on the Functions of the Digestive Organs, especially those of the Stomach and Liver, with practical Remarks on the Treatment of some of the Diseases to which these Organs are liable. A description stated that the work would comprise the results of some of the more important chemical changes which take place during the digestion and assimilation of the food. The practical remarks will principally relate to the proper adjustment and use of remedies, and to the pernicious effects liable to be produced in delicate habits by the constant operations of various slowly acting causes, especially impure or hard waters: illustrated by analyses of the principal waters in common use in the metropolis and its vicinity.

Prout never published any water analyses. Despite two advertisements in February and November 1823, the book was never published, though more than likely some of its intended contents found their way into the 1840 edition of Prout’s enlarged treatise on urinary diseases. The explanation given later by Prout was that his discovery of hydrochloric acid in the gastric juice in the autumn of 1823 so thoroughly disorganized his assembled material and ideas on the nature of assimilation that he had to abandon the book. The unknown writer of the Edinburgh Medical and Surgical Journal’s obituary notice of Prout gave three different reasons: (1) A new edition of Prout’s popular text on urine had been called for after the stock of the 1821 edition had become exhausted; (2) Prout’s medical practice had considerably increased because of that book’s publication, and patients suffering from the stone flocked to his

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20 Phil. Mag., 1828 (2), 4, 121.
Sackville Street consulting rooms; (3) Such private time as he had was occupied by the analysis of wines for his Edinburgh friend, Dr. Alexander Henderson.\textsuperscript{79} The latter's well-known classic, *History of Wines, Ancient and Modern* appeared in 1824, and Prout's analyses of some eighteen wines were presented in an appendix.

These three reasons, coupled with the discovery of hydrochloric acid in gastric juice, would undoubtedly have led to the postponement or abandonment of the book on digestion.

Prout's great discovery of hydrochloric acid in the gastric juice of animals was announced to the Royal Society on 11 December 1823.\textsuperscript{80} Now recognized as a 'classic of scientific reasoning', the published account in the *Philosophical Transactions* of 1824 was extremely terse so that whereas most physiologists agreed with Prout's identification, there were chemists prepared to argue the validity of his analytical deductions. Challenges came from Leuret and Lassaigne in France, who claimed to have found free lactic acid in the stomach, and from Tiedemann and Gmelin in Germany, who had found free acetic, butyric and hydrochloric acids. Both parties were engaged on the problems of digestion at the same time as Prout,\textsuperscript{81} and the resulting controversy—which historians of medicine have overlooked—allowed Prout to describe his discovery in more detail:

I detected the free muriatic acid in a fluid ejected from the human stomach so long ago as 1820, but then thought that its presence was accidental, or that by some means or other, I had deceived myself; and when I actually commenced the experiments in question, I was actually prejudiced in favour of a destructible acid, viz., the lactic acid of Berzelius (though the distinct nature of this acid always, I confess, appeared to me somewhat problematical).\textsuperscript{82} In consequence of this prejudice therefore, the inquiry was conducted in a much more rigorous and elaborate manner than it probably otherwise would have been; and after a series of the most complete evidence that perhaps was ever brought to bear on a chemical point, I was obliged to conclude, is in opposition to my preconceived notion, that the acid was the muriatic acid and no other. On reflecting, however, on this most unexpected fact, I soon saw its importance, and that in short, it was one of those leading facts that opens up an entire new field of inquiry. So satisfied indeed was I of this, that a work on the digestive functions, in which I had long been engaged, and which I had actually begun to print, was suppressed; and since that time I have been engaged in an entire new field of research which I fear will yet occupy me for several years to come.\textsuperscript{83}

He then proceeded to justify the conciseness of the announcement of his discovery to the world. Prout had been completely satisfied that

\begin{itemize}
  \item the acid present was the muriatic and no other, at least in any appreciable quantity. Now it was in the knowledge thus previously acquired, and not at random, that the method proposed
\end{itemize}

\\textsuperscript{79} Munk, *op. cit.*, vol. iii, p. 69.
\textsuperscript{80} *Phil. Trans.*, 1824, pp. 45–9; reviewed by Kasich, *op. cit.*, ref. 3. The date of the discovery, 1823, has been misprinted 1803 in many secondary sources; others suggest 1834 and 1840—the latter source attributes the discovery to 'Sir William Prout'.
\textsuperscript{82} i.e. presence of lactic acid should have been obvious if it really were present. Notice that Prout does not mention that in 1816 he had favoured phosphoric acid.
\textsuperscript{83} *Phil. Mag.*, 1828 (2), 4, 120–1.
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was founded... If it be objected that these preliminary experiments ought to have been given, I can only say that I did not at the time think this necessary, nor do I now. The muriatic acid was not a new substance, nor one difficult to be identified; besides, such a preliminary inquiry seemed to be sufficiently indicated by the method proposed; for who would ever think of proposing a formal method of analysis, involving the quantities of substances, without determining beforehand what those substances were? Further, my paper was intended to be little more than a simple announcement of an important fact which, before it could be established, I well knew must be corroborated by other experiences than mine; and lastly, something must be ascribed to a sort of innate antipathy to long-winded dissertation which is too apt to cause me to err on the side of brevity.

The discovery of free hydrochloric acid in the stomach was confirmed by Tiedemann and Gmelin in February 1824, and by J. G. Children, Secretary of the Royal Society, also in 1824 from the observation of a dyspepsic and sceptical friend. None the less, there were many sceptics; the most bitter opponent was Richard Thomson, Lecturer in Practical Chemistry at Glasgow, and nephew of Thomas Thomson. Like Prout, Thomson seems to have been unaware of Beaumont's findings, and he attacked Prout before the British Association in 1839 and in several articles. Prout made a dignified reply in his clinical textbook, remarking that from the way Thomson had operated, he was not surprised that the presence of hydrochloric acid had escaped him. However, it should be noted how little Prout made of his discovery in later writings and the extent to which he played down the presence of the acid in the stomach. The acid, he suggested, was formed by galvanism (electrolysis) from blood chlorides.

The original work on digestion ended in 1827 with the Copley Medal-winning paper, 'On the Ultimate Analysis of Simple Alimentary Substances, with some Preliminary Remarks on Organic Analysis'. This, the last of Prout's purely chemical papers, was read to the Royal Society on 14 June 1827. It was planned to be the first of three papers in which he discussed in turn the three food aliments which he was the first to classify as the saccharinous (carbohydrates), the oliginous or oily (fats), and the albuminous (proteins). However, only this first paper on the saccharine foods and oxygen combustion analysis was ever published. The latter technique had been perfected after the standard copper oxide method had proved too inaccurate; it involved a return to Lavoisier's direct oxygen combustion procedure and it employed a very elaborate piece of apparatus. As far as is known, Prout's apparatus was not adopted by any other chemist, and within a few years Liebig introduced the simple rapid procedures which are still essentially used today.

Prout was a vitalist who believed that organic materials contained 'independent existing vital principles or agents superior to, and capable of controlling...
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and directing, the forces operating in inorganic matters. The four alimentary principles, water, saccharines, fats and albumines, were able to combine together, or in emergencies even transform one into the other, under the influence of organic agencies. Like Liebig later, Prout urged that a satisfactory diet should include all four foodstuffs and be modelled upon the great alimentary prototype—milk. God had designed a system of 'universal voracity' whereby lower organisms converted the essential organic elements (C, N, O, H, etc.) into the four proximate principles. This was a 'cuisine obligée for the wants of the higher' organisms, since by preying on lower animals they found materials already assimilated to their own structures and were thus saved the trouble of creating them.

Prout divided metabolic processes into primary and secondary assimilation, and the third edition of his urine textbook was concerned with the pathology of these actions. Primary assimilation included the processes of digestion and sanguification; secondary assimilation—Liebig's 'metamorphosis of tissues'—included both the processes of tissue formation from the blood (formative process) and the destruction and removal of unwanted parts from the animal system (destructive process). As a result of his molecular speculations, Prout laid great stress on the role of water in assimilation. Materials which contained small proportions of the elements of water were referred to as strong or high; others, usually substances of a more unstable character which contained large proportions of the elements of water, were called weak or low. The conversion of strong into weak substances by the absorption of water was described as reduction, and Prout believed that this was the principal chemical feature of digestion. Once the chyle entered the lacteals, however, the opposite chemical process of completion occurred whereby the aliments were raised from the weak to the strong state and poured into the blood-stream. Here, any combined water was released during the respiratory process. The whole scheme was very ingenious, but naturally Prout was unable to give many details. In any case he did not believe that the processes of reduction and completion were simply chemical; vitalizing agents were supposed to be present in both the stomach and the lacteals. Organization could not occur without the presence and admixture of 'foreign parts'—that is, elements other than the traditional organic elements of carbon, hydrogen, oxygen and nitrogen, or the addition or subtraction of water. Prout had first made this point in 1817, and in 1827 he coined the word merorganized or merorganization to describe the isomerism and vitalization of organic materials.

By these incidental matters ... the ordinary chemical properties of the essential elements of the organized living structure are variously modified; in particular, that the essential elements are hindered from assuming a regular crystallized form. Moreover, these incidental matters

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On Stomach, p. 452.

Like the archet of van Helmont, these agents seem analogous to enzymes; however, Prout intended that no chemical interpretation should be placed on them. The possibility of a transmutation of matter into calcium was suggested by Prout, Phil. Trans., 1822, p. 377; and into nitrogen, Chemistry, p. 500.

Chemistry, p. 472.


The word was coined by a Mr. Lunn (the engraver?). The abstract, Proc. roy. Soc., 1815/30, 2, 324-6, uses the word protorganized.
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entering into the composition of a living body apparently furnish the organic agent new powers utterly beyond our comprehension.

Daubeny thought merorganization an attractive way to explain some kinds of isomerism, but no other chemist or physiologist adopted the word; and Prout dropped it—but not the concept—after 1831.

Meteorology

Prout designed and constructed (probably with the help of John Newman, the instrument-maker) an expensive barometer some time prior to 1830. This instrument was highly praised by the meteorologist James Forbes who thought it 'one of the finest philosophical instruments I have ever had the pleasure of seeing'. Prout's son, the Rev. Thomas Jones Prout, presented it to the Oxford Museum in 1860, but its present whereabouts is unfortunately not known. In 1835 the Royal Society decided to commission a new standard barometer from John Newman who had built the Daniell standard barometer in 1821, and this instrument appears to have been modelled upon the barometer of Prout who was made responsible for its effective construction. It was completed in 1836, but Newman’s bill evidently shocked Council, for they referred it to Prout for comment. Prout replied that the bill was not unreasonable; from published accounts the total cost was close on £70. Like Prout’s own barometer, the Royal Society’s instrument has also disappeared.

Prout appeared in the role of meteorologist at the British Association meeting at Oxford in 1832 when he read a paper summarizing his observations on the specific gravity of air, and the law of expansion of air. The paper is interesting not only for its precision, but also for one extraordinary observation which had led Prout to make an equally extraordinary speculation. Atmospheric air in both dry and moist states had been weighed daily at noon free from carbon dioxide from December 1831 until March 1832. His mean value from eighty-six experiments was that 100 cub. inches of air, barometer 30 inches, temperature 32°F., London latitude, weighed 32.7958 grains (± 0.0507). However, a strange event had occurred on 9 February 1832:

... on which day the weight of the air was 32-8218: and it is remarkable that after this period, during the whole time that the experiments were continued, the air almost uniformly possessed a weight above the usual standard; so that... the mean of the 42 observations after this crisis (32-8018), exceeds the mean of the 44 preceding it (32-7900) by no less than 0.0118 grains. The apparatus employed, and the care taken were the same throughout, and there can be no doubt that the difference, whatever it depended on, really existed, and did not arise from error of experiment.

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102 In a letter to Lubbock, 10 March 1835, Prout agreed to oversee the progress of the instrument, but he made it clear that the Royal Society should trust Newman to handle the matter competently by himself (*Rep. Sr.*, MC2 164). Prout determined the sp. gr. of the mercury to be used. The instrument was fully described by Baily, F., *Phil. Trans.,* 1837, pp. 431-41.
With his typical flare for generalization, Prout wondered whether this anomaly was in some way connected with the terrible cholera outbreak.

It may be worth while to observe that almost precisely at the period above mentioned, the wind veered round to the north and east, where it continued for a considerable time, and that under these circumstances the epidemic cholera first made its appearance in London. It would seem therefore, as if some heavy foreign body had been diffused through the lower regions of the atmosphere about this period, and which was, some how or other, connected with the disease in question.\(^{10}\)

Even Berzelius was impressed by this argument, for he observed that if a lesser analyst than Prout had suggested it, it would not have deserved consideration.\(^{10}\)

Prout remained particularly fond of this speculation since he repeated it in his Bridgewater Treatise and in the later editions of his clinical textbook.\(^{10}\)

Daubeny thought it a fine example of Prout's 'power of generalization' and recalled with what scientific caution he had proposed it.

Although he was understood to have continued the meteorological researches alluded to during the whole period of the cholera in 1832, he delayed their publication until they could be still further corroborated. Unfortunately, when the cholera broke out a second time in 1848, his health was too enfeebled to allow of his undertaking, in addition to a large medical practice, a similar course of laborious investigations, so as to satisfy his own scrupulous mind as to their truth.\(^{10}\)

In another train of argument Prout reasoned that there should be some variation of the specific gravity of air due to wind direction. Air which had travelled over the whole extent of London from the East to his house in the West End (i.e. an east wind) would probably have had a considerable quantity of oxygen removed and replenished by carbon dioxide. Since Prout removed carbon dioxide before making his measurements, it followed that air from the east would 'be necessarily found lighter'. Such differences were small, he admitted, yet his measurements did seem to confirm that air from the east after travelling across populated London did contain a fraction of a per cent less oxygen than air from other quarters. The argument, although correct, could hardly have been significantly demonstrated from such a small number of experiments, especially when the differences involved were no greater than the likely experimental errors.

At the same meeting of the British Association, Prout was elected to the Chemistry Committee,\(^{10}\) and a paper on atomic weights was read by Edward Turner. At Cambridge the following year, there was a further paper from Turner\(^{11}\) who helped over certain analytical difficulties by Prout was very critical of Thomson's analyses, and his new results did not confirm Prout's

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10 During 1832 every London physician was puzzled for an explanation of the cholera. Prout's observation was mentioned in the review, Henry, W. C., Laws of Contagion, Brit. Ass. Reports, 1834, p. 92. For a reinvestigation and confirmation during the 1854 cholera season, Thomson, R. D., ibid., 1855, pp. 71-3.
11 Jahres-Bericht, 1834, 13, 52; quoted Benfey, op. cit., ref. 5.
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hypothesis. Probably as a result of this, the Chemistry Committee awarded £50 to Dalton and Prout for the investigation of atomic weight values and specifically to test the integral weight hypothesis.\(^{111}\) Nothing is known of this intriguing collaboration. Although Prout's name disappeared from the Committee in 1834, he was charged with Dalton and others in the same year to investigate chemical nomenclature and formulae—then a bone of contention among English chemists.\(^{112}\) The brief majority report which appeared in 1835 supported the continental nomenclature;\(^{113}\) in minority, Dalton urged his own pictorial system. Although Prout would not have adopted this pictorialism, there is no doubt that he agreed with Dalton that the Berzelian formulae were clumsy and unphilosophical.

Prout interested himself in all the sciences which could be aided by chemistry. We have seen him in the roles of chemist, physiologist, physician and meteorologist; in 1829 he briefly filled the role of geochemist. His two geological analyses of bezoar stones and coprolites were undertaken at the request of William Buckland who presented the results to the Geological Society.\(^{114}\) Both these analyses were connected with Prout's physiological work, especially that of coprolites which he showed consisted of large amounts of calcium phosphate, and confirmed Buckland's opinion that it was a fossil faeces.

The Gulstonian Lectures of 1831

Prout, who had been elected a Fellow of the Royal College of Physicians on 25 June 1829, was appointed to the 1831 Gulstonian lectureship. His three lectures on 'The Application of Chemistry to Physiology, Pathology and Practice'\(^{115}\) were a continuation of the theme of the 1827 Copley paper, and anticipated the third Book of the Bridgewater Treatise. Prout suggested that the physiologist of his day paid too much attention to mechanical or even metaphysical explanations in biology, whereas for him biology called for the application of chemistry. There had been a certain lack of success in animal chemistry, both because of the intrinsic difficulty of the subject and through the incompetence and incomprehension of the pure (i.e. inorganic) chemist when he had begun to work in the unfamiliar field of biology. Prout's plea for progress was: physiologists become chemists.\(^{116}\)

In conjunction with the phenomena presented by living organized bodies with which he ought to be thoroughly acquainted, he must carefully study their common chemical properties, their ultimate composition, the laws of their formation and change, and a multitude of other matters which the mere chemist is apt to overlook, or knows not how to appreciate even if he observes them.

The elements of organized substances were the very same as those of inorganic materials, and it was totally unnecessary to suppose that an organic principle

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\(^{111}\) Brit. Ass. Reports, 1833, p. xxxvi. Later Prout and Thomas Clark, Prof. of Chemistry at Aberdeen, were awarded £40 for sp. gr. measurements, ibid., 1839, p. xxix.

\(^{112}\) Brit. Ass. Reports, 1835, p. xxi.

\(^{113}\) Brit. Ass. Reports, 1835, p. 207; Daubeney, ibid., 1836, p. xxxiv.


\(^{116}\) Ibid., p. 258. He had anticipated this in 1816, Ann. Med. Surg., p. 269; cf. also ref. 25.
held these elements together when the ordinary observed chemical affinities of one element for another was quite a sufficient explanation. At this level at least, vitalism was superfluous. Yet it was a fact that organic materials differed fundamentally from inorganic substances, and here Prout reintroduced the idea of merorganization whereby minute quantities of ‘impurities’ performed ‘an office which may be termed interstitial—that is to say, that they operate by being interposed, as it were, between the essential elementary atoms of organized substances, and thus prevent them from assuming the crystallized form, in which stage they would be totally unfit for the purposes of the economy of living organized beings’. Organic agents were then introduced to attend the design of merorganization.

Chemistry was both a science and an art (i.e. technology), but as a theoretical science it was little understood, and Dalton’s atomic theory by stopping where it had, had retarded chemistry rather than advanced it, ‘for to suit the imaginary standards of this bed of Procrustes, real results, I fear, have been too often extended or compressed beyond all legitimate bounds and thus truth sacrificed to error’. This statement came ironically from Prout whose own unitary hypothesis had led a devoted Thomson close to doing just that. What Prout meant—and he should have been explicit—was that the classic atomic theory of Dalton had stopped short of the molecular concept and therefore, in his opinion, it had hindered the progress of pure chemistry. In any case his commitment to a reduction in the number of elements had also led him to a positivist position that the atomic theory was only a ‘conventional artifice’; atomic or equivalent weights were really only single terms in an arithmetical series peculiar to each substance. ‘Thus 9, the number assumed to represent the combining weight of water, is to be considered as one term of the series 3 : 6 : 9 : 12 : 15, &c., in all which proportions (and perhaps in still lower submultiples of them) this fluid enters into combination, perhaps quite as often as in the proportion 9, especially in the organic kingdom.’ This crude foreshadowing of valence was fully exploited in the Bridgewater Treatise where the digestion and assimilation of foodstuffs were explained by a sort of Pythagorean number mysticism.

The three Gulstonian lectures were printed in pamphlet form for private circulation and favourably reviewed in the Medical Gazette, except for a note of rebuke sounded over Prout’s professed dislike of Daltonian atomism. However, the lectures were not received so sweetly by the physiologist Wilson Philip, who strongly objected to Prout’s suggestion that there had been almost no progress in physiology in twenty years. Characteristically, in a reply, Philipcatalogued his own achievements at great length and detail. An answer from Prout was followed by a rejoinder from Philip, and so began a dispute concerning the place of chemistry in physiology in which knocks were given and taken on both sides. Dr. McMenemey has observed that ‘this acrimonious correspondence reveals surprising shortcomings in the mind of Wilson Philip, a man who in his earlier days was obviously a thinker. He was, it seems, entirely

\[\text{\cite{117 Med. Gaz., 1831, 8, 260.}\]
\[\text{\cite{118 Ibid., p. 262.}\}
\[\text{\cite{119 Ibid., p. 263.}\]
\[\text{\cite{120 Chemistry, pp. 460 et seq.}\]
\[\text{\cite{121 Med. Gaz. 1831, 8, 468.}\]
\[\text{\cite{123 The row may be followed intermittently through Med. Gaz., 1831, 8, and 1831/2, 9.}\]
oblivious to the possibilities of clinical and especially chemical pathology in the practice of medicine.\textsuperscript{122} Ironically, it appears that by the 1840s, the vitalist Philip had come round to Prout's viewpoint completely, even to the extent of claiming it as his originally (e.g. that the nervous system was essentially chemical). Prout, on the other hand, had become more deeply committed to vitalism, as another Gulstonian antagonist, the Manchester surgeon, John Roberton, had predicted. Roberton objected to the implied vitalism of the term organic agent, and argued that it calculated 'by its mysticalness to retard or discourage the study of this science'.\textsuperscript{123} However, Prout refused to be drawn into any argument over vitalism.

The Bridgewater Treatise\textsuperscript{125}

The eighth and final Earl of Bridgewater, who died in 1829, bequeathed £8,000 to the Royal Society as a payment to the person or persons chosen by its President who would write, print and publish, 1000 copies of a work on the Power, Wisdom and Goodness of God as manifested in the Creation; illustrating such work by all reasonable arguments, as for instance the variety and formation of God's creatures in the animal, vegetable and mineral kingdoms; the effect of digestion, and thereby of conversion; the construction of the hand of man, and an infinite variety of other arguments.\textsuperscript{126} Prout was one of the eight authors chosen by the President of the Royal Society, Davies Gilbert, and his numerous advisers. The Chemistry, Meteorology and the Function of Digestion with Reference to Natural Theology appeared in 1834.\textsuperscript{127} As we have seen, Prout was well qualified to write with originality on the diverse topics included in the title; however, in 1854 when the agnostic physicist John Tyndall was asked to edit this Treatise for the popular Bohn Library series, his sarcastic private comment was:

I should have thought more highly of Dr. Prout had I not read his book. Certainly if no better Deity than this can be purchased for the eight thousand pounds of the Earl of Bridgewater, it is a dear bargain. It is very evident that Dr. Prout would never have written such a book through the spontaneous promptings of his own spirit; it was written for money, and lacks even common scientific depth, not to speak of religious inspiration.\textsuperscript{128}

Tyndall was unfair in his estimate of the book's scientific standard, for many of its pages had simply dated. Possibly there was something in the financial sneer, though this was a common criticism of all the Treatises' authors, for we have Munk's report that 'in pursuing his scientific investigations, and especially those on the atmosphere, expense was not regarded by Dr. Prout, and much of his apparatus was of the most elaborate and costly character.\textsuperscript{129} In addition, Prout

\textsuperscript{122} Med. Gaz., 1831, 8, 745. Later, after Prout's Bridgewater had appeared, Roberton attacked his vitalism in Critical Remarks on certain recently published opinions concerning Life and Mind, 90 pp., 1836 (not seen).


\textsuperscript{124} Prout, W., Chemistry, publisher's announcement, p. vii.


\textsuperscript{126} Tyndall's private journal, 19 November 1854, in the Royal Institution. Tyndall was perhaps disconcerted by Prout's vitalism.

\textsuperscript{127} Munk, op. cit., p. 111 (from Med. Times, p. 17),
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had his family to support with children at Westminster School and later at Oxford. No doubt then the Bridgewater legacy was very welcome.

The theological content of Prout's treatise followed the pattern of its companions; examples of apparent utility and design in both the inorganic and organic kingdoms were, by cumulative effort, made an argument for the unity of design and purpose and beneficence of a Superior Chemist. Munk, when he wrote in 1878 after the argument for the existence of God from Design had received hard knocks from the evolutionists, still described the book as 'a work of high merit and of much originality'. Indeed, in my view the work was the most daringly speculative of all the Bridgewater Treatises and consequently, perhaps, the least successful in sales. Daubeny considered that Prout showed 'much ingenuity in unravelling the mysteries which beset us when we attempt to speculate on the intimate constitution of matter. While soaring into this elevated region, he caught a glimpse of those views respecting the distinction between physical and chemical atoms, from the development of which Dumas has since derived so much celebrity'. This distinction—really that between molecule and atom—was the subject of a controversy between Prout and the Daltonian, William Charles Henry. In a letter to the Philosophical Magazine, Henry objected to Prout's version of Dalton's atomic theory, in particular the doctrine that equal volumes of gases under the same conditions contained the same number of atoms—an opinion which Dalton had outrightly rejected. Henry also repudiated, as had Dalton, the logical corollary that the 'atom' (i.e. molecule) was divisible, or as Dumas and Gaudin had put it, that there was a distinction between the chemical and physical atom. Prout replied characteristically with a short letter in which he refused to debate the subject, but he implied that he had held such ideas from 1815. It is very unfortunate that Prout did not seek to justify his molecular viewpoint at greater length in such an important journal as the Philosophical Magazine, for although the Bridgewater went through four editions, it must be assumed that its attraction for readers was as a book of apologetics rather than as a chemical textbook. Thus its readership would have been lay rather than scientific; the only chemist who seems to have been at all influenced by Prout's molecular views was his friend Charles Daubeny. Consequently, although like its companion volumes, Prout's treatise is no longer read, and scientists and historians associate Prout's name with the unitary hypothesis, at the time of his death it was recalled that 'he was deservedly known to the public generally by his various contributions to the advancement of medical science, particularly by his Bridgewater Treatise'.

1835–50, Clinical work, death and character

Nothing has come to light of Prout's clinical career and practice during the remainder of his life. A third edition, or rather a completely new version of his 'magnum opus', On the Nature and Treatment of Stomach and Urinary Diseases

141 Phil. Mag., 1834 (3), 5, 132-3.
142 Gent. Mag., 1850, p. 442; Athenæum, 1850, p. 420, col. 3.
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appeared in September 1840,14 but the book's interest is diminished for the historian of science by Prout's practical bias and lack of theoretical challenge. Indeed, as edition followed edition, even contemporary reviewers criticized Prout for not examining and explaining some of the theoretical issues involved in physiology.15 Today the most interesting section of the book is the introductory first part with its succinct account of the physiology of the digestive and urinary systems—much of it a specialized version of material previously presented in the third Book of his Bridgewater Treatise. Yet, in the two subsequent editions Prout unwisely placed this section at the end of the volume as a third Book, and so further emphasized the work's practical bias. It was, however, impossible to understand the first two Books which employed Prout's special vocabulary and nomenclature, without first referring to the third Book!

Although as one obituarist said the ideas in this work 'excited by their novelty, considerable attention, but do not seem to have made the impression their importance deserved',16 Prout himself was really to blame for the neglect and ineffectualness of his studies. He was inherently a conservative man and he consequently suffered the chagrin of living to see Liebig and other continental chemists and physiologists build a science on many of the principles which he had stated or foreshadowed. Thomas Wakley, in a Lancet editorial,17 made a personal attack on Prout's inertia and conservatism in 1844 which went directly to the heart of the matter. This was the period when ardent efforts were made to woo Liebig into an English Chair of Chemistry in order to stimulate the teaching and study of the subject in this country. A patriot had asked Wakley why England needed a Liebig when they already had a distinguished chemist in Prout. That was just the trouble, argued Wakley: 'Many individuals hold Dr. Prout to be the first of British organic chemists; but several of the doctrines he espouses are opposed to those which are now taught in the continental schools that possess the highest repute.' One had only to read the 1843 edition of Prout's Urinary Diseases to see this. For instance: Prout had ignored the discovery of pepsin and relegated the views of Schwann and Müller to a brief footnote; he had old-fashionedly stated that exact compositions of albuminous substances could not be given, and he still regarded them as compounds of just four elements 'merorganized' by minute portions of certain 'incidental minerals'; he had completely ignored Mulder's proteine; he more or less ignored Liebig's argument for the progressive changes of organic compounds in the living state, and questioned the accuracy of the analyses upon which such views were based; finally, Prout never referred to the action of oxygen on tissues.

15 e.g. Brit. for. med. Rev., 1841, 11, 336: 'Dr. Prout assumes to himself merit for "avoiding all controversial points"; he means the discussion of these; so far from avoiding such points . . . he settles them invariably ex cathedra, without the least apparent misgivings as to the rectitude of his decisions. However grateful it may be to some readers to meet with a conviction so strong that it almost displays itself in dogmatism, we for our part, feel persuaded that it diminishes the real usefulness of the volume.'
16 Med. Times, 1850, 1, 17.
17 On the labours of Prout, Lancet, 1844, 1, 486-90.
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whereas Liebig and Wöhler based all their studies upon the concept of tissue oxidation. This was a formidable list of justified complaints, and Wakley was in little doubt of the reasons for Prout's own failure. (1) Prout had declined to use chemical formulae which he scorned as unphilosophical expedients because they did not represent true compositions. Although we can see how Prout was placed in this position by his molecular theories, we must agree with Wakley that this showed a disastrous inability on Prout's part to keep pace with the development of science. (2) Prout deserved his reputation, but as an historic figure, for his work on gastric juice and the alimentary principles. No doubt continental chemists had begun where Prout left off. But why had it been left to them? Because 'Dr. Prout's name and authority exercises an influence that is detrimental' to the teaching and progress of chemistry in Great Britain. Science declined when 'the authority of those who, having earned a reputation for themselves, cast unfounded doubts upon the labours of others, neglect and repudiate, without sufficient cause, the methods followed by their competitors, and deny them that honour to which they are justly entitled by their discoveries. We regret to find Dr. Prout in this category.'

Prout seems to have made no reply to these criticisms but a few changes were made in the fifth and final edition of his textbook which appeared in 1848. However, its lack of chemical formulae and distillation of continental work led to its rapid replacement by other texts, notably that of Golding Bird.

Deafness afflicted Prout long before his death, and this caused him to withdraw from scientific society—to its loss, and perhaps even more so to his. Thus there is no mention of him at the Royal College of Physicians after 1834, or at the Royal Society after 1836, or at the British Association after 1839. He fell ill in 1848, and became worse in the summer of the following year. An autumn excursion into the country did not improve his health, and, emaciated, he returned to London to continue with his practice. His health grew worse in the spring of 1850, and when the President of the Royal Society, the surgeon, Sir Benjamin Brodie, called to see him on the 9 April, Prout told him that he knew that he was dying. The end came the same day; the cause apparently gangrene of the lung following a burst abscess. He had requested that no post-mortem should be made. Prout is buried somewhere in Kensal Green cemetery, and there is a simple memorial tablet in Horton village church which records:

Sacred to the Memory of William Prout, M.D., F.R.S.
Born in this parish 15 January 1785
Died in London 9 April 1850
Scintillulam contulit

Prout was an early riser who did some of his own work before he breakfasted at 7 a.m.; the remainder of the day was devoted to his patients. 'Besides his extensive town practice, scarcely a day passed that boxes and parcels did not arrive from the country, and the analyses of their contents, together with the

1843 Lancet, 1844, 1, 490.
1844 Bird, Golding, Urinary Deposits, their Diagnosis, Pathology and Therapeutical Indications, 1844, 3rd ed., 1851, 4th ed. 1853, 5th ed. 1857. Bird owed much to Prout with whom he was on excellent terms.
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necessary correspondence, consumed no small portion of each day. There are also references to an extensive foreign correspondence, but no trace of this has come to light. Deafness must have been especially tragic for him since he had a great love of music. At some time in his life he built an organ which he played with great skill; several anthems were also composed by him—presumably strictly for family consumption. He possessed a similar love for painting, and his consulting rooms were hung with several canvases.

A notable estimate of his character has been quoted at the head of this essay. Prout undoubtedly impressed those with whom he came into contact. Thus Thomas Thomson thought most highly of him, and W. C. Henry, who disagreed so profoundly with Prout's interpretation of the atomic theory, admitted that like Wollaston and Davy, Prout possessed 'a taste for extreme exactitude' and an 'unrivalled manual expertness' never achieved by Dalton. The highest praise came not unexpectedly from Prout's great friend, Daubeny: Prout was 'a great original thinker as well as an accurate and scrupulous experimentalist'.

Iconography

The following general description of Prout is recorded:

He was of middle height, and of slim figure. His head was nobly developed, and the intellectual qualities strongly marked; the hair soft and snowy white. His features were delicately chiselled, eyes brilliant, complexion very pale, but the expression of his countenance combined benevolence with great intelligence. There was a blandness in his manner which inspired confidence, and set the most nervous patient at ease. He always dressed with scrupulous neatness, usually in black, with gaiters, or silk stockings.

Of Prout's portraits, three of four known canvases have been traced. A lost portrait in oils painted by John Hayes, a pupil of David, was executed during the 1830s. This portrait undoubtedly remained in the possession of the family until at least as late as 1909, but it has not proved possible to discover its present location. A copy of the Hayes was made by Henry Wyndham Phillips for the Royal College of Physicians in 1855, and until recently this hung on the main stairway of the College's premises in Pall Mall. Prout's youngest son, the Rev. Thomas Jones Prout, Scholar of Christ Church, Oxford, was most dissatisfied with the Phillips portrait, and in a letter to the Royal College he offered them a new reproduction of the Hayes by H. M. Paget. 'It will certainly be more satisfactory to ourselves, as well as to those who may remember our Father and to any others who may care to know what he was like, that the College possess a picture which does recall him better than the portrait which hangs on their walls at present.' This new portrait was presented to the College by Prout and his sister, Elizabeth, in June 1888. 'I venture to ask for it a place on the

* Since writing this article I have discovered the location of this portrait. But permission to reveal the owner's name has not yet been obtained.


A note by Munk intended for a later ed. of his Roll records that on 20 February 1856, Phillips was given £30 for the portrait and frame. But a letter in R.C.P. from Sir Thomas Watson to Dr. Farré, 24 December 1873, reports that Phillips had been surprised to be paid in guineas, not pounds.

140 To Sir Henry Pitman, 5 May 1888 (R.C.P.).
walls of the College, not as being entirely satisfactory, but as being a decidedly better likeness of my father than that which has hitherto hung there.¹⁴³

The existence of the miniature mentioned by Philip Hartog in his *Dictionary of National Biography* notice of Prout is doubtful.¹⁴⁴ Thomas Prout stated that he had never seen or heard of it before.

In 1902 Thomas and Elizabeth Prout commissioned a second copy of the Hayes from Paget for presentation to Edinburgh University,¹⁴⁵ where it now hangs on the Secretary's Staircase. Finally, Thomas had photographs made of the Hayes original. A photograph was given to William Osier some time in 1905, and this is now at McGill's Osler Library.¹⁴⁶ A similar photograph was presented to the Chemical Society in 1904,¹⁴⁷ though their present copy appears to be a later print.

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¹⁴³ To Pitman, 19 June 1888 (R.C.P.). Ironically the College has always displayed the Phillips and stored the Paget. For the striking contrast, see Wolstenholme, *op. cit.*, p. 348.

¹⁴⁴ Watson reported in his letter to Farre that Phillips did his portrait from 'a small miniature in the possession of the family'. Munk, who was responsible for establishing cordial relations between Thomas Prout and the R.C.P., asked him about the miniature's provenance. (This was after Munk had mentioned it in the Roll.) Prout replied that he knew nothing of it. 'If therefore Mr. H. Phillips had such a miniature, it was either his own property inherited from his Father who was an old friend of our Father's, or, if it was our property, Mr. H.P. omitted to restore it.' (To Pitman, 5 May 1888.)


