## Topics

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# New Labour? The Effects of Migration from Central and Eastern Europe on Unemployment and Wages in the UK

**Abstract:** The UK was one of only three countries that granted free movement of workers to accession nationals following the enlargement of the European Union in May 2004. The resulting migration inflow, which was substantially larger and faster than anticipated, arguably corresponds more closely to an exogenous supply shock than most migration shocks studied in the literature. We evaluate the impact of this migration inflow – one of the largest in British history – on the UK labour market. We use new monthly micro-level data and an empirical approach that investigates which of several particular labour markets in the UK – with varying degrees of natives' mobility and migrants' self-selection – may have been affected. We found little evidence that the inflow of accession migrants contributed to a fall in wages or a rise in claimant unemployment in the UK between 2004 and 2006.

**Keywords:** migration, employment, wages, Central and Eastern Europe, UK
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## 1 Introduction

In May 2004, ten Central and Eastern European countries joined the European Union (EU). The UK along with Ireland and Sweden were the only EU countries to initially grant full free movement of workers to accession nationals. Around 560,000 accession migrants joined the UK labour market between May 2004 and May 2006, according to the Worker Registration Scheme (WRS). It was one of the largest migration inflows in British history (Salt and Miller 2006), roughly equivalent to 2% of total employment. It was also sufficiently rapid and relatively unexpected to have an impact on the labour market.

The impact of such a shock on wages and unemployment is indeed a crucial labour market issue. This is especially so given the heated public debate on migration – and in particular on migration from current and future accession countries. For example, the accession shock we study here was quickly associated to the concurrent 96,000 rise in the Jobseeker's Allowance (JSA) claimant unemployment. Yet, there is very limited evidence on labour market migration effects on the UK – and even less so on the effects of the recent EU enlargement (Dustmann, Fabbri, and Preston 2005; Dustmann, Frattini, and Preston 2007; Drinkwater, Eade, and Garapich 2009; Manacorda, Manning, and Wadsworth 2006 are among the few).

The main contribution of this article is to help to fill this gap in the literature and to inform policymaking on the face of further EU enlargement. We estimate the effect of the accession migration inflow on the distribution of wages and on claimant unemployment using monthly WRS and JSA microdata, as well as data from the Annual Survey on Hours and Earnings (ASHE). Given that the paucity of suitable data is one of the main reasons for scarce evidence on migration effects for the UK, exploiting the new and large WRS data is a timely contribution.

Another contribution of this article is that the particular nature of the accession migration shock helps us to circumvent, to some extent, identification issues arising from simultaneity bias that usually pose difficulties in the literature. One such identification issue is that natives may respond to the migration inflow by moving away from a particular area or occupation. If natives avoid competing with migrants through increased mobility, potentially adverse effects in that labour market may be offset. Another identification issue is that migrants may respond to specific demand conditions when migrating into a particular area or occupation. If migrants self-select into particularly booming areas or occupations, potentially adverse effects in that labour market may again be offset. Therefore, the extent to which any adverse unemployment and wage effects can be identified depends on how mobile natives are across areas and occupations in response to migration inflows and on how able migrants are to self-select into booming areas and occupations.

These two identification issues would vanish if areas or occupations were closed local labour markets, where natives were confined to compete with migrants; and if migrants distributed randomly across areas or occupations, instead of self-selecting into booming local labour markets. Although areas and occupations in the UK did not constitute completely closed labour markets to which accession migrants were randomly assigned, the accession migration inflow arguably corresponds more closely to an exogenous supply shock than most migration shocks studied in the literature (also see Card 1990, 2007; Hunt 1992; Carrington and Lima 1996; Friedberg 2001). That is because this was a large, rapid and concentrated migration shock that resulted from political decisions. More crucially, this was a shock substantially larger and faster than anticipated (see Dustmann et al. 2003 for forecasts). As a result, both natives' and migrants' responses – through, respectively, mobility out of and self-selection into specific areas and occupations – might have been sufficiently lagged to allow identification of adverse unemployment and wage effects.

For example, accession migrants were overwhelmingly concentrated into low-skilled occupations. Since they are relatively well educated, this suggests occupational downgrading, which is a well-documented phenomenon in the literature. This happens when language or labour market barriers prevent migrants from immediately self-selecting into more favourable occupations (also see Card and DiNardo 2000; Friedberg 2001). Therefore, because the accession inflow was much larger and faster than expected, and because it was heavily concentrated into low-skilled occupations, concerns about migrants' self-selection bias are reduced. Furthermore, low-skilled occupations constitute a relatively closed market, where immediate natives' mobility is limited because it often requires retraining (also see Friedberg 2001; Borjas 2003). Therefore, because the accession inflow was much larger and faster than expected, and because it was heavily concentrated into low-skilled occupations, concerns about natives' mobility bias are also reduced.

We exploit variation in occupation choices, as well as in location choices (at district, county and region levels, in turn), across months to ensure identification in our empirical model. More specifically, we use an empirical approach whereby we investigate which of several particular labour markets in the UK – with varying degrees of natives' mobility and migrants' self-selection – may have been affected.

We found little evidence that the inflow of accession migrants contributed to a fall in wages or a rise in claimant unemployment in the UK between 2004 and 2006. While our results are robust to a number of specification checks and are aligned with other results in the literature, they are not in line with standard theory predictions that migration inflows which are unbalanced across areas or skills exert downwards pressure on wages and employment.[1] One explanation

**1** Our results are in line with evidence in the international (mainly US) literature of little or no effect on employment and wages (Chiswick 1980; Grossman 1982; Card 1990, 2005 and 2007; LaLonde and Topel 1991; Altonji and Card 1991; Pischke and Velling 1997; Friedberg 2001; Dustmann, Fabbri, and Preston 2005; Dustmann, Frattini, and Preston 2007; Manacorda, Manning, and Wadsworth 2006; Carrasco, Jimeno, and Ortega 2008), though in contrast with other evidence of more adverse effects (Borjas 2003 and 2006; Angrist and Kugler 2003; Orrenius and Zavodny 2007). As we discuss in Sections 3–5, the disagreement in the literature is underlined by an ongoing debate on identification issues arising from natives' mobility and migrants' self-selection (see for example Chiswick 1991; Borjas 1999; Card 2001).

here is that as accession migrants overwhelmingly compete with low-paid workers, and as these workers were protected by a concurrently increasing minimum wage, more adverse wage effects may have been mitigated or offset.

We thoroughly discuss the issues above in the remainder of this article. In Section 2 we depict our data. In Section 3 we specify our empirical model and in Sections 4 and 5 we carefully discuss several identification issues. In Section 6 we summarise and discuss the results in light of the existing literature before we conclude in Section 7.

## 2 Data

The migration data we use is from the Home Office administered Worker Registration Scheme (WRS). To work in the UK, accession nationals have to register on the scheme. Registration, in addition to being a legal requirement, offers incentives such as social security benefits (Home Office 2004). As a result, compliance is high, with 560,000 registrations between May 2004 and May 2006. The vast majority of these workers arrived post-accession, though those already in the country could formalise their status with no sanctions.[2] The left panel of Figure 1 shows the monthly WRS inflow between May 2004 and May 2006. The trend is downwards in 2004, dipping in December (7,950), and upwards in 2005, peaking in November (33,784). Numbers fell in early 2006 (to around 23,000 per month).

The WRS data is rich, large, frequent and timely. It records nationality, address, age, gender, number of dependents, application date, entry date, start of work date, hourly wage rate, hours worked, sector, occupation and industry. Table 1 shows that many WRS migrants are young, male, Polish, childless, in London, working full time in low-paid jobs in elementary and machine operative occupations in manufacturing and catering. The WRS is only available for migrants from the ten accession countries, as migrants from

2 For example, while 24.1% of those registering in the WRS in the first two months state “entry date” before January 2004 (these include students, illegal workers, self-employed, etc.), this goes down to 2.1% after 1 year (Home Office 2005). These figures are produced aggregating the data by “application date”, whereas others have also used “entry date” (Gilpin et al. 2006). As the typical accession migrant enters the UK, finds a job, and then applies to the WRS, we instead aggregate the data using “start of work date” to best capture labour market effects and to skew from identification problems associated to using “entry date” or “application date”. (For completeness, however, we re-estimated Equation [1] in Section 3 aggregating the data by “entry date” and obtained qualitatively similar results.)

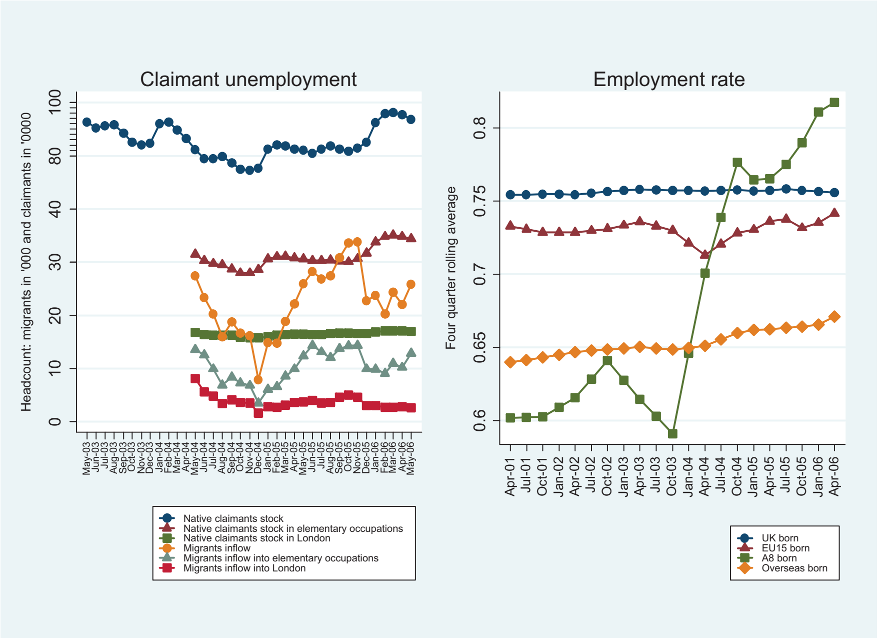


**Figure 1:** Unemployment and employment
Source: Worker Registration Scheme data, Jobseeker's Allowance data and Labour Force Survey data

other countries are not required to register. We restrict our sample to eight of those (A8), Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Slovenia and Slovakia, as Malta and Cyprus already had relative access to the EU labour market.[3]

The unemployment data we use is from the Department for Work and Pensions administered Jobseeker's Allowance (JSA). The JSA is large, frequent

**3** A caveat with the WRS is that it measures inflows only, and thus we cannot calculate the associated netflow and stock. This is because the WRS records jobs, not people: migrants leaving the UK are not counted, whereas migrants re-entering the UK are double counted. Blanchflower, Saleheen, and Shadforth (2007) analyze A8 migration figures across several data sources and conclude that a stock of 500,000 migrants by late 2006 is likely to be an upper bound. Pollard, Latorre, and Sriskandarajah (2008) and Coats (2008) provide similar analysis and conclude that outflow is not zero, in line with evidence on return migration (LaLonde and Topel 1997). We discuss how measurement error in the WRS might affect our estimates in detail in Sections 2.1 and 4.2. Another caveat with the WRS is that the self-employed are not required to register (although they are a minority that already had relative access to EU labour markets).

**Table 1:** Descriptive statistics

| Variables | WRS | JSA | ASHE | | LFS | |
|---|---|---|---|---|---|---|
| | May 2004–2006 | May 2004–2006 | May 2004–2006 | | April 2004–June 2006 | |
| | migrants | claimants | workers | | UK born | Overseas born |
| I - Population variables – % of those who are: | | | | | | |
| Aged: | | | | | | |
| Under 16 years old | 0.00 | – | na | na | 0.21 | 0.08 |
| 16 – 24 years old | 0.37 | 0.30 | na | na | 0.12 | 0.11 |
| 25 – 34 years old | 0.45 | 0.24 | na | na | 0.12 | 0.24 |
| 35 – 64 years old | 0.18 | 0.45 | na | na | 0.40 | 0.44 |
| Over 65 years old | 0.00 | 0.00 | na | na | 0.16 | 0.13 |
| Women | 0.43 | 0.74 | na | na | 0.51 | 0.52 |
| Parents (with dependent children) | 0.06 | na | na | na | 0.27 | 0.32 |
| Blacks | – | na | na | na | 0.01 | 0.12 |
| Asians | – | na | na | na | 0.02 | 0.25 |
| Nationality: | | | | | | |
| Polish | 0.61 | na | na | na | – | 0.02 |
| Lithuanian | 0.12 | na | na | na | – | 0.01 |
| Slovakian | 0.10 | na | na | na | – | 0.00 |
| Latvian | 0.07 | na | na | na | – | 0.00 |
| Located in: | | | | | | |
| London | 0.17 | 0.19 | na | na | 0.09 | 0.41 |
| South East | 0.14 | 0.08 | na | na | 0.14 | 0.13 |
| East of England | 0.12 | 0.07 | na | na | 0.09 | 0.08 |
| East Midlands | 0.09 | 0.06 | na | na | 0.07 | 0.05 |
| Yorkshire and the Humber | 0.08 | 0.09 | na | na | 0.09 | 0.06 |
| West Midlands | 0.08 | 0.11 | na | na | 0.09 | 0.07 |
| North West | 0.08 | 0.12 | na | na | 0.12 | 0.07 |
| South West | 0.08 | 0.05 | na | na | 0.09 | 0.05 |

| | | | | | | |
|---|---|---|---|---|---|---|
| Scotland | 0.08 | 0.10 | na | na | 0.09 | 0.04 |
| Northern Ireland | 0.04 | 0.03 | na | na | 0.03 | 0.01 |
| Wales | 0.03 | 0.05 | na | na | 0.05 | 0.02 |
| North East | 0.01 | 0.05 | na | na | 0.05 | 0.02 |
| II - Labour market variables – % of those who are in: | | | | | | |
| Occupations: | | | | | | |
| Elementary occupations | 0.46 | 0.35 | na | na | 0.11 | 0.14 |
| Machine operatives occupations | 0.32 | 0.10 | na | na | 0.08 | 0.07 |
| Skilled trades occupations | 0.06 | 0.11 | na | na | 0.12 | 0.08 |
| Personal services occupations | 0.04 | 0.05 | na | na | 0.08 | 0.08 |
| Unknown occupation | 0.04 | 0.01 | na | na | 0.00 | 0.00 |
| Sales and customer service occupations | 0.03 | 0.13 | na | na | 0.08 | 0.07 |
| Administrative occupations | 0.03 | 0.10 | na | na | 0.13 | 0.09 |
| Professional occupations | 0.01 | 0.04 | na | na | 0.12 | 0.17 |
| Managers and senior officials | 0.01 | 0.04 | na | na | 0.15 | 0.15 |
| Technical occupations | 0.01 | 0.06 | na | na | 0.14 | 0.15 |
| Sectors: | | | na | na | | |
| Manufacturing | 0.31 | na | na | na | 0.13 | 0.11 |
| Distribution, hotels and restaurants | 0.27 | na | na | na | 0.19 | 0.21 |
| Transport and communication | 0.09 | na | na | na | 0.07 | 0.08 |
| Agriculture and Fishing | 0.08 | na | na | na | 0.01 | 0.01 |
| Banking, finance and insurance, etc. | 0.08 | na | na | na | 0.15 | 0.19 |
| Public admin, educ and health | 0.06 | na | na | na | 0.28 | 0.28 |
| Construction | 0.04 | na | na | na | 0.08 | 0.05 |
| Other services | 0.02 | na | na | na | 0.06 | 0.06 |
| Energy and water | 0.00 | na | na | na | 0.01 | 0.01 |
| Part time | 0.08 | na | na | na | 0.26 | 0.22 |
| Employment rate | – | – | na | na | 0.76 | 0.67 |
| Unemployment rate | – | – | na | na | 0.05 | 0.07 |
| Average claim duration | – | 31.32 | na | na | – | – |

(*continued*)

**Table 1:** (*Continued*)

| Variables | WRS | JSA | ASHE | | LFS | |
|---|---|---|---|---|---|---|
| | May 2004–2006 | May 2004–2006 | May 2004–2006 | | April 2004–June 2006 | |
| | migrants | claimants | workers | | UK born | Overseas born |
| Looking for a job in their usual occupation | – | 0.84 | na | na | – | – |
| Average hours worked | 37.83 | – | na | na | 36.87 | 38.37 |
| | | | **2004** | **2006** | | |
| 5th percentile hourly wage distribution | 4.50 | na | 4.77 | 5.16 | 4.50 | 4.61 |
| 10th percentile hourly wage distribution | 4.65 | na | 5.14 | 5.55 | 5.26 | 5.31 |
| 20th percentile hourly wage distribution | 4.85 | na | 5.99 | 6.45 | 6.15 | 6.22 |
| 25th percentile hourly wage distribution | 4.85 | na | 6.43 | 6.95 | 6.58 | 6.69 |
| 30th percentile hourly wage distribution | 4.87 | na | 6.92 | 7.45 | 7.02 | 7.19 |
| 40th percentile hourly wage distribution | 5.00 | na | 7.95 | 8.55 | 7.98 | 8.34 |
| 50th percentile hourly wage distribution | 5.05 | na | 9.18 | 9.89 | 9.06 | 9.59 |
| Average hourly wage distribution | 5.56 | na | 12.04 | 13.09 | 11.02 | 11.88 |
| Standard deviation hourly wage distribution | 2.03 | na | na | na | 7.16 | 8.01 |
| Adult minimum wage | 4.80 | na | 4.50 | 5.05 | 4.80 | 4.80 |
| Number of observations | 5,62,830 | 22,016,120 | 21,915 | 23,725 | 201,294,305 | 21,169,990 |

Source: Worker Registration Scheme data, Jobseeker's Allowance data, Annual Survey of Hours and Earnings and Labour Force Survey.

Notes:

(1) Variables not available or not defined in a particular dataset are indicated by "na". For example, the employment and unemployment rates are not defined for the WRS ASHE or JSA where all individuals are working/unemployed. The proportion of parents from the LFS is for 2006 Q2, where the household weight used is based on 2003 population estimates as re-weighted household datasets are yet unavailable (the other figures are based on 2007 population estimates).

(2) As ASHE is not available at the micro level, we are unable to compute percentiles for the period 2004–2006; we instead report percentiles for 2004 and 2006 directly from the ASHE tables. Similarly, standard deviation is not available.

(3) As detailed in the text (see Section 2), the WRS measures inflows, whereas the JSA and LFS measure stocks. Therefore, the WRS figures are cumulative.

(4) National minimum wage (adult rate) is: £4.50 between 1 October 2003 and 30 September 2004; £4.85 between 1 October 2004 and 30 September 2005; £5.05 between 1 October 2005 and 30 September 2006.

and timely, and like the WRS, permits disaggregation at fine (district and month) levels.[4] This is in contrast with the more widely used Labour Force Survey (LFS), where migration analysis below the region and quarter level is not feasible due to sample size limitations. Furthermore, the JSA measures claimant unemployment, instead of the more broad ILO unemployment, and thus is directly relevant for policymaking.

The JSA data records address, gender, age, usual and sought occupations, claim start and end dates. Table 1 shows that many JSA unemployed are over 35 years old, female, in London and work in elementary occupation low-paid jobs. Between May 2004 and May 2006, JSA claimant unemployment rose by roughly 96,000. The left panel of Figure 1 shows the monthly JSA stock during this period. Claimant unemployment decreased during 2003–2004, dipping in December (803,029), and remained stable during 2005, despite a continuous and growing inflow of A8 migrants. In the first half of 2006 it increased, peaking in March (989,136), while A8 migration decreased. Casual observation suggests perhaps an association between the JSA stock and the WRS inflow in 2006 but not before.

Furthermore, there appears to be little evidence of a negative association between the employment rate of the A8 migrants and that of other workers. This is shown in the right panel of Figure 1, which also illustrates the substantial rise in the A8 employment rate since 2004 using LFS data. The dip in the A8 employment rate in 2003 suggests that migrants deferred their decisions to join the UK labour market to take advantage of the new accession status, announced in December 2002. The employment rate is highest for A8 migrants, perhaps because of their younger age, or perhaps because of their skills, higher productivity and work ethic (more reliability, less sick leave, longer working hours, etc.; see Table 1).

The wage data we use is from the Annual Survey of Hours and Earnings (ASHE) collected by the Office for National Statistics (ONS). The ASHE is derived from employers' data and represents 1% of all employees, containing around 160,000 responses per tax-year (which runs from April to March). Its sample size permits disaggregation at fine (district and tax-year) levels, again in contrast with the LFS, as discussed above. It collects, among other variables, address, gender, age, hourly pay, hours worked, occupation and industry. Table 1 shows various percentiles and the average of the ASHE and WRS hourly wage distributions, whereas Figure 2 plots both distributions for those earning £7 per hour or below. The WRS distribution 50th percentile is roughly lined up with the ASHE

**4** The ONS-defined geographical areas we use are: 409 Local Authority Districts, 49 counties and 12 Government Regions (ONS 2003) (see Table 1).

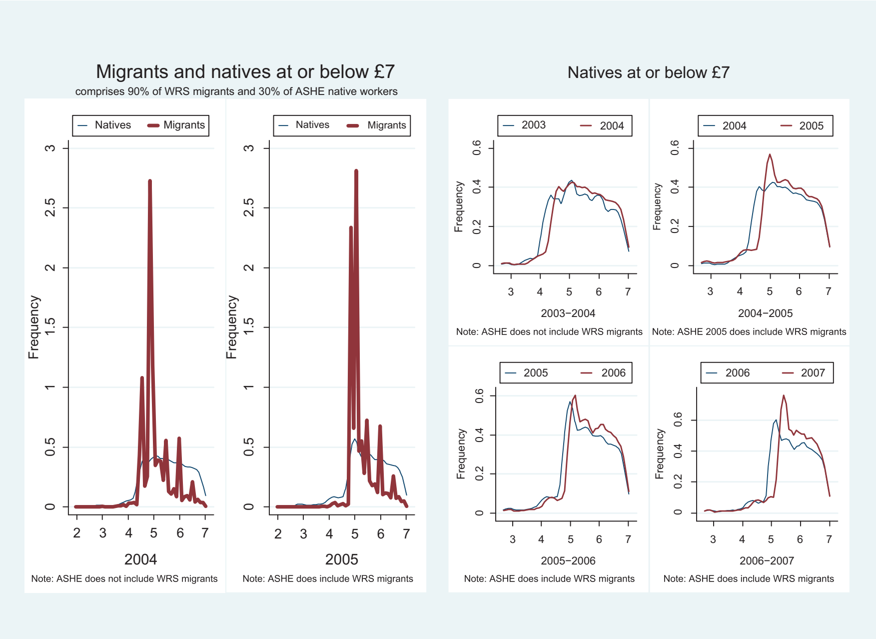


**Figure 2:** Hourly wage distributions
Source: Worker Registration Scheme data and Annual Survey of Hours and Earnings data

distribution 5th percentile (see Table 1). This indicates that the typical WRS migrant earns around the minimum wage, which is also the wage for the lowest paid UK workers. The left panel of Figure 2 shows a sizeable spike at the minimum wage in the WRS distribution, which dwarfs the spike in the ASHE distribution. It also shows how remarkably compressed the WRS distribution is over 90% (75%) of migrants earn between £2.00 (£4.00) and £7.00 an hour. While the average wage is £5.56 for a WRS migrant, it is £12.57 for a UK worker, although these figures should be treated with caution, as ASHE includes WRS migrants after 2004 (also see Footnote 20).

Table 1 and Figure 2 suggest little evidence of a negative association between the WRS inflow and wages. Table 1 shows little evidence of depressed wages at the average or other points of the distribution. It instead shows wage growth throughout, more generous at the very bottom of the distribution in 2005–where it is probably driven by minimum wage increases.

Finally, we use data from the LFS to define control variables that describe the natives' population. ("Natives" here and throughout the article include UK-born and overseas-born nationals who are UK residents.) The LFS is a rotating panel survey that interviews around 60,000 households with about 140,000 respondents every quarter and represents 0.5% of the population. It collects information on personal characteristics and labour market variables. Table 1 summarises some of these variables between April 2004 and June 2006.

A8 nationals represent just under 4% of the stock of migrants in the LFS. The average number of migrants in the WRS inflow was 763, whereas the average number of non-A8 working age migrants in the LFS netflow was 263 across districts and years during the period (see Footnote 3). The associated correlation between the WRS inflow and the LFS netflow is 0.05, whereas the correlation between the two, normalised by the working age population, is –0.02. This suggests that the WRS migration shock was unique and not just a larger-scale shock of overall migration patterns to the UK. Similar in nature to the 1990s inflow of Cubans to Miami and Russians to Israel (Card 1990; Friedberg 2001; Hunt 1992; Carrington and deLima 1996), the WRS migration shock is characterized by accession nationals choosing to migrate following political decisions.

## 2.1 Descriptive analysis

In line with pre-accession A8 clusters, almost half of all WRS nationals migrated into London, the South East and East of England (see Table 1). This suggests the presence of network pull effects, which is a well-documented phenomenon in the literature. This happens when areas traditionally associated with migration are most attractive to new migrants, even if there are other more booming areas. Put differently, the support offered by existing migrants somewhat discourages migrants to immediately self-select into more booming areas (Altonji and Card 1991; Hunt 1992; Card 2001). Given the disproportionate numbers of WRS migrants and JSA claimants in London, it is likely that both groups compete for the same jobs. Figure 1 shows a continuing inflow of migrants but a relatively stable number of claimants in London. However, wages grew slower in London between 2005 and 2006 (2.7%) than in the rest of the country (4.4%).

WRS migrants are concentrated predominantly in low-skilled jobs, in contrast with earlier migrants (see Table 1). The most popular sectors are manufacturing (31%) and distribution hotels and restaurants (27%), where WRS migrants represent less than 2% of total employment. The most popular occupations are machine operatives (32%) and elementary (46%). Given the disproportionate numbers of WRS migrants and JSA claimants in machine operatives and elementary occupations, it is likely that both groups compete for the same jobs. Figure 3 shows that despite the continuing inflow of migrants into machine operatives, more claimants

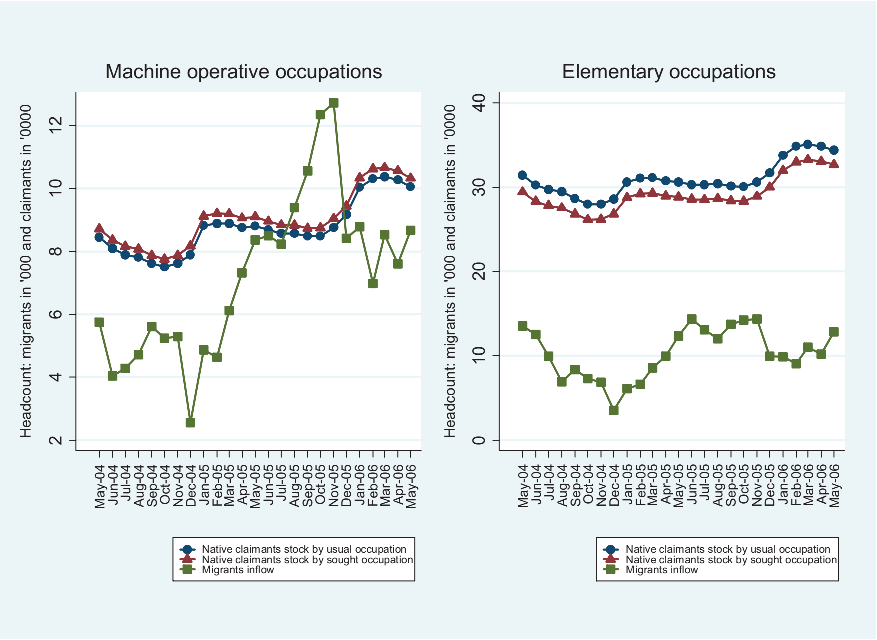


**Figure 3:** Migrants and claimants by occupations
Source: Worker Registration Scheme data and Jobseeker's Allowance data

switched to this from other (usual) occupations. Also, wages grew faster in machine operatives between 2005 and 2006 (3.8%) than in elementary (2.7%) or other occupations (3.5%). This suggests that demand side factors may have driven both migrants and claimants into machine operative jobs.

Although there is no indication of demand side factors attracting migrants into elementary occupations, this is probably where they were most able to find jobs because of language or other labour market barriers (Friedberg 2001; Drinkwater, Eade, and Garapich 2009). This is also the usual occupation for most claimants (35%), and Figure 3 shows that some of them switched from looking for jobs in (usual) elementary to other (sought) occupations. The switch could either be because migrants pushed natives out or because of other factors, including occupational progression, sectoral or occupational shocks, macro shocks, etc., which we account for in our empirical model in Section 3. An example of such shocks, as discussed above, is the claimant unemployment increase across all occupations in early 2006, which hints at macro-effects in addition to any WRS migration effects.

We exploit the variation in these location and occupation choices across months to ensure identification in our empirical model, as we discuss in detail in Sections 3 and 4. The correlation between our claimant unemployment (netflow) rate variable $\Delta N_{it}$ against our migration (inflow) rate variable $\Delta M_{it}$ across $i$ districts and $t$ months is –0.01. In line with our analysis at the national level in Section 2, this suggests little evidence of a negative association between the two variables at the district level. This is also the case at the occupational level. The correlation here is 0.06, suggesting that claimant unemployment did not grow faster in districts and occupations that received relatively more migrants. The correlation between the average (10th percentile) of the distribution of log hourly pay $W_{iy}$ in first-difference across $i$ districts and $y$ tax-years against the April-March yearly migration rate $\Delta M_{iy}$ is 0.02 (–0.02). Again, in line with our analysis at the national level in Section 2, this suggests little evidence of a negative association between the two variables at the district level. In other words, this suggests that wages did not grow slower in districts that received relatively more migrants.[5]

The above correlations offer little support to standard theory predictions that migration inflows exert downwards pressure on wages and employment. However, such raw correlations need to be proved robust when the effect of other variables (demand and supply shocks, area and occupation specific shocks, etc.) on wages and claimant unemployment is accounted for. We control for such variables in our regression models in Sections 3 and 4, where we further discuss associated identification issues.

# 3 Model specification

We estimate the effect of the WRS migration inflow on the UK claimant unemployment netflow using a reduced form equation grounded on standard theory (see for example, Borjas 1999; Card 2001; Dustmann, Fabbri, and Preston 2005):

$$\Delta N_{it} = \beta^n \Delta M_{it} + \lambda^n \Delta X_{it} + f_t^n + \Delta\varepsilon_{it}^n \quad [1]$$

5 We define $\Delta N_{it} = \frac{\Delta N_{it}^*}{P_{it}}$ and $\Delta M_{it} = \frac{\Delta M_{it}^*}{P_{it}}$, where $N_{it}^*$ is the number (stock) of JSA claimants, $M_{it}^*$ is the number (stock) of WRS migrants, and $P_{it}$ is the working age population. As discussed in Section 2, whereas we observe the stock of claimants and can calculate the netflow of claimants as $\Delta N_{it}^* = N_{it}^* - N_{it-1}^*$; we do not observe the stock of migrants. We therefore re-define the netflow of migrants as $\Delta M_{it}^* = I_{it} - O_{it}$, where $I_{it}$ is inflow and $O_{it}$ is outflow of migrants. As we do not observe outflow, we again re-define $\Delta M_{it}^* = I_{it}$, as is common in the literature (see for example, Card 2001; Dustmann and Glitz 2005) and interpret it as a variable in differences. Similarly, we define the native (netflow) rate $\Delta A_{it} = \frac{\Delta A_{it}^*}{P_{it}}$ and $\Delta A_{it}^* = I_{it}^A - O_{it}^A$, where $I_{it}^A$ is inflow and $O_{it}^A$ is outflow of natives. We also run robustness checks where our migration and unemployment variables in eq. [1] were not normalised (i.e. re-defining $\Delta N_{it} = \Delta N_{it}^*$ and $\Delta M_{it} = \Delta M_{it}^*$) and found qualitatively similar results.

where $\Delta N_{it}$ and $\Delta M_{it}$ are our unemployment and migration variables, defined in Section 2.1, $X_{it}$ are labour demand and supply shifters, $f_t^n$ is time-fixed effects, and $\varepsilon_{it}^n$ is the error term in district $i = 1, \ldots, 409$ and month $t = 1, \ldots, 24$ (i.e. $409 \times 24$ cells). The interpretation of our coefficient of interest is that a one percentage point increase in the migration rate changes the claimant unemployment rate by $\beta^n$ percentage points.

As we estimate Equation [1] in first-difference, area-fixed effects were differenced out. This way we remove any permanent differences across districts and make them equally attractive. In other words, we control for specific factors in a district (such as more schools, more housing, higher wages, etc.) that may make it more attractive to migrants or natives or both. This enables us to separate the effect of district specific factors from the effect of the WRS shock on claimant unemployment. We model time-fixed effects using 24 month dummies. This enables us to separate the effect of other macro-shocks (such as seasonal shocks, national and international shocks, etc.) from the effect of the WRS shock on claimant unemployment.

We also control for demand and supply shifters. This enables us to separate the effect of demand and supply shocks from the effect of the WRS shock on claimant unemployment. Controls in $X_{it}$ include the proportion of the total population who are women, young (those between 18 and 24 years of age), ethnic minorities and migrants from outside the A8 countries. This enables us to control for higher unemployment in a particular district due to the presence of relatively more women, youngsters, minorities or other migrants – groups who often experience high unemployment.

Controlling for the effect of migrants from outside the A8 countries is particularly important to ensure that we separate out unemployment effects due to existing migrants from any unemployment effect due to the new migrants. This way we gauge the net effect of WRS migration, which might otherwise pick up the effect of wider migration on unemployment (see Section 2). In other words, we control for an important source of potential omitted variable bias. This is particularly so because some of the districts where WRS migrants concentrate are also districts traditionally linked to wider migration. Incidentally, existing and new migrants are different, so their effect on unemployment is likely to differ. For example, WRS migrants are young, white and mostly childless (see Section 2).

Further controls include the lagged proportion of WRS migrants who are women, young and parents (along with average number of children). We also control for the lagged average hours worked by WRS migrants to account for potentially higher claimant unemployment in districts where migrants work longer hours (which may increase labour substitutability). We include the lagged proportion of WRS migrants in elementary and machine operative occupations to control for occupation-district specific shocks affecting claimant unemployment. We also include the lagged proportion of unemployed who are women and young. Finally, we include lagged claim duration, which accounts for higher unemployment in districts with historically long spells of unemployment; it also alleviates problems arising from serial correlation in the residuals.[6]

We perform a Generalized Least Square (GLS) correction to account for the relative importance of each district and for heteroskedasticity arising from aggregation.[7] Also, we correct the standard errors for serial correlation across and within districts.

# 4 Identification

The estimate of $\beta^n$ in eq. [1] would be biased in the presence of a non-zero correlation between the error term and the migration rate. Firstly, this correlation would be non-zero if variables driving both the migration and unemployment rates were omitted. Two such omitted variables are of particular concern: natives' mobility and migrants' self-selection. Secondly, this correlation would be non-zero if the unemployment and migration rates were jointly determined: that is, if both migrants and natives made simultaneous decisions to join the labour market based on observed job opportunities. Two main sources of such simultaneity bias arise, again, from natives' mobility and migrants' self-selection. Thirdly, this correlation would be non-zero in the presence of non-random measurement error. We now carefully discuss each of these sources of endogeneity bias in turn.

6 As in Gilpin et al. (2006), we experimented with two types of dynamics (lagged migration rate and lagged claimant unemployment rate), which, however, did not qualitatively alter our results. Although dynamics allow for lagged adjustments due to slow responses in employment, migration effects are generally expected to be lower in the longer than in the shorter run (Altonji and Card 1991; Dustmann, Fabbri, and Preston 2005).

7 The appropriate weight here is the sample size used to calculate the dependent variable (working age population), but our estimates were also robust to using total population as weight instead – which reduces concerns of a potential correlation between the weight and the dependent variable affecting the results. (Also, as discussed in Section 2.1, we run robustness checks where our unemployment and migration variables were not normalised and found qualitatively similar results.) Our estimates were also robust to using, in turn, April 2004 working age population and April 2004 total population as time-invariant weights (see Card 2001; Borjas 2006).

We begin by arguing that we control for omitted variables to some extent through fixed effects and demand and supply shifters. Controlling for both area- and time-fixed effects helps to correct for natives' mobility (omitted variable) bias and migrants' self-selection (omitted variable) bias. Also, controlling for demand and supply shocks helps to account for factors that may motivate income-maximising natives to move to other districts and thus helps to correct for natives' mobility (omitted variable) bias (Borjas 2006).

We further account for natives' mobility (omitted variable) bias by explicitly including two further controls. Ideally, we would want to use a variable that measures what would have been the observed natives' net migration had migrants not arrived – which would also introduce the initial labour market pre-accession conditions into the regression analysis (Borjas 1999 and 2006). This would allow us to separate the effect of the WRS shock on claimant unemployment from the effect of natives moving away from (or refraining to move into) a district. Put differently, it would allow us to some extent to build a counterfactual of how mobile natives would have been in the absence of the migration inflow. This would help to correct for natives' mobility (omitted variable) bias.[8]

As such a counterfactual is not observable, we add two observable proxies to $\Delta X_{it}$ in turn. The first proxy we use is lagged working age population growth (Borjas, Freeman, and Katz 1997; Borjas 2006) – which incidentally ensures that the variation in $\Delta M_{it}$ that identifies $\beta^n$ comes from the numerator (migration inflow) and not from the denominator (working age population) (Borjas 2003). To avoid repeating the dependent variable as a regressor, we use lagged working age population growth by education group (Dustmann, Fabbri, and Preston 2005; Borjas 2006).[9] The second proxy we use is the native netflow rate $\Delta A_{it}$, defined in Section 2.1.

By adding these two extra controls, we obtain fairly stringent specifications. We assume that any further correlation between the error term and the migration rate is potentially weak and thus any associated endogeneity bias is not too severe. (We relax this assumption in Sections 4.1 and 4.2 and find robust results). Thus, we argue that most of the remaining variation in the claimant unemployment rate is likely due to changes in the WRS migration inflow – and this ensures the identification of $\beta^n$.

**8** The severity of any such omitted variable bias depends on the extent of the correlation between the migrant inflow and natives' netflow (see Section 4.2). This is, ultimately, an empirical matter and will vary according to the particular phenomenon studied (Card and DiNardo 2000; Borjas 2003).

**9** Our three groups comprise: those with a degree and above, those with GCSE and below, and those in between. Robustness checks showed qualitatively similar results when the last was omitted.

**Table 2:** Unemployment effects of migration

| Models | Coefficient | Standard errors |
|---|---|---|
| **A – District level** | | |
| (1) Baseline specification | **0.037** | 0.087 |
| (2) Adding working age population growth | **0.020** | 0.075 |
| (3) Adding native netflow rate | **0.003** | 0.078 |
| **B – County level** | | |
| (1) Baseline specification | **0.071** | 0.078 |
| (2) Adding working age population growth | **0.062** | 0.085 |
| (3) Adding native netflow rate | **0.057** | 0.086 |
| **C – Region level** | | |
| (1) Baseline specification | **0.134** | 0.081 |
| (2) Adding working age population growth | **0.119** | 0.108 |
| (3) Adding native netflow rate | **0.115** | 0.106 |

Notes:
(a) These are GLS estimates weighted by the sample size used to calculate the dependent variable.
(b) The dependent variable is the UK claimant unemployment rate and the independent variable of interest is the WRS migration rate (see Sections 3 and 4).
(c) Time fixed effects are modeled with month dummies; area fixed effects are differenced out. See Section 3 for discussion on demand and supply controls.
(d) There are $409 \times 24$ observations in the models at the district level, $49 \times 24$ in the models at the county level, and $12 \times 24$ in the models at the regional level.
(e) The interpretation of the coefficient is that a one percentage point increase in the WRS migration rate changes the UK claimant unemployment rate by B percentage points.

Row 1 of panel A of Table 2 shows an insignificant 0.037 $\beta^n$ estimate. This is our baseline estimate from eq. [1] (the controls are, in the main, significant and of the expected sign here as well as in the remaining models in the article). The estimate remains positive and insignificant, 0.020 and 0.003, when we control for lagged working age population growth and for native netflow rate in rows 2 and 3. These estimates are still small – if anything, smaller – offering little evidence that natives' mobility offsets potentially more adverse effects, in line with our earlier descriptive analysis in Section 2.1.

Thus, our results indicate little evidence of adverse claimant unemployment effects at the district level.[10] Furthermore, our results suggest that any endogeneity arising from natives' mobility is not strong enough to severely bias our

**10** We re-estimated Equation [1] at the district and year level – to check whether variation is greater when the data is aggregated at the yearly instead of monthly level – and the results remained qualitatively the same.

estimates. We probe our results further using five different alternative identification strategies in Sections 4.1–4.3.

## 4.1 Aggregation level

In addition to assessing the extent of any natives' mobility (omitted variable) bias by explicitly controlling for lagged working age population growth and native netflow rate when estimating Equation [1] at the district level, we now assess the extent of any such a bias by re-estimating Equation [1] at the county and region levels in turn. If natives' mobility is not exacerbated by the migration inflow, then the estimates at the district, county and region level should not differ much, as we now explain.

The main concern here, as in much of the literature, is that natives may respond to the migration inflow by moving away from a particular district. If natives avoid competing with migrants through increased mobility, potential adverse effects in that district may be offset. This undermines identification because we do not observe what would have been the wages and employment level in that district had natives not moved away. Thus, the extent to which such effects can be identified depends on how mobile natives are across districts in response to migration inflows. This problem would vanish if districts were closed local labour markets, where natives were confined to compete with migrants. If districts are not closed local markets – and if lagged working age population growth and native netflow rate are poor proxies for natives' net migration – then our estimates in Section 4 may be biased.

One way to assess the extent of any such bias is to aggregate the data at different levels. Ideally, the level of data aggregation should conform to the actual radius of job search for natives competing with migrants. However, as the boundaries of the actual radius of job search for natives are an empirical matter, we experiment with three levels of aggregation.

We began, in Sections 3 and 4, by assuming that there are 409 closed labour market districts in the UK (i.e. $409 \times 24$ cells). While districts are unlikely to exactly coincide with local labour markets, they may represent a realistic practical radius of job search for most low-skilled natives competing with WRS migrants, who concentrate in low-paid jobs (see Table 1). Low-skilled natives are more area-bound because the cost–benefit of cross-regional mobility is often prohibitive. This effectively means that they compete in a relatively more closed market. We use work address for WRS and ASHE workers – to eliminate concerns that they may live in one district and work in another – and home address for JSA claimants, who we assume, search for jobs primarily in their neighbourhood. Nonetheless, it is possible that claimants live in one district and search for jobs in another, as districts are close and commuting costs are relatively low in big cities.

Thus, we next relax the assumption that districts are independent and closed labour markets by aggregating the data across 49 counties (i.e. $49 \times 24$ cells). While counties are unlikely to coincide with local labour markets throughout the country, they may represent a realistic practical radius of job search to relatively area-bound low-skilled natives in big cities who are likely to choose districts nearby (within the same county) to commute or move to. Thus, counties can be regarded as more closed labour markets than districts. Likewise, regions can be regarded as more closed labour markets than counties. Therefore, we end by aggregating the data across regions (i.e. $12 \times 24$ cells), which, incidentally, ensures that our results are comparable to those in the literature.[11]

In sum, we use three levels of aggregation, in turn: districts, counties and regions. By changing the level of aggregation, we are changing the boundaries of the radius of job search – i.e. the degree of natives' mobility – and allowing the search to take place on ever wider labour markets. Our final level of aggregation is the national level, as we discuss in Section 4.3, which scraps all boundaries and allows natives full mobility within the country.

The idea is that the greater the degree of natives' mobility, the larger the associated estimate bias across different aggregation levels (Borjas 2006). If natives are district-bound then estimates at the district, county or region level should not differ much. If however natives are mobile across districts, but not across counties, potentially adverse effects are offset at the district level but uncovered at the county level. Similarly, effects offset at the county (region) level might be uncovered at the regional (national) level.

In line with the estimates at the district level in panel A of Table 2, the estimates at the county and region level in panels B and C are also positive and insignificant. The estimates are remarkably robust within each panel, suggesting little evidence that natives' mobility offsets potentially more adverse effects either at the county or at the region level.

The region estimates are twice as large as the county estimates, which are twice as large as the district estimates. This may be interpreted as evidence of

11 Most studies for the UK use data from the LFS, where migration analysis below the region and quarter level is not feasible (see Section 2). The implicit assumption in these studies is that there are 12 regional closed labour markets in the UK, where the whole of London is treated as one data point – even though London has 33 districts, where 41% (17%) of all (WRS) migrants are unevenly distributed. We overcome this weakness in the literature by aggregating the data at finer (district and county) levels.

natives' mobility offsetting more adverse effects at the district and county levels. However, this evidence is weak. Firstly, although the estimates are numerically larger the wider the aggregation level, they are statistically indifferent from zero. Secondly, although larger estimates might be expected at wider aggregation levels as a result of theoretical predictions regarding natives' mobility (Borjas 2003 and 2006); they might also be expected as a result of modelling choices (Peri and Sparber 2008). One example is that region dummies do not control for as many area-specific shocks as district dummies do, which may result in a larger $\beta^n$ estimate at the region level. Moreover, serial correlation is more of a concern in more aggregate data, which again could result in a larger $\beta^n$ estimate at the regional level (despite appropriate GLS corrections at each level). Another example is that implicit area weights differ across aggregation levels. For instance, at the district level, different parts of London receive different weights, and each district has a small weight; in contrast, at the county and region levels, London is treated as one single labour market. This could result on a larger $\beta^n$ estimate at the region level, weighed towards London.

In sum, our results indicate little evidence of adverse claimant unemployment effects at the district, county or region level. Furthermore, our results suggest that any endogeneity arising from natives' mobility is not strong enough to severely bias our estimates. This suggests that either native did not respond to the migration inflow by moving away or that the extent to which they did so is not enough to severely bias our results. We argue that the nature of the particular phenomenon we study here reduces concerns that any such bias is severe.

Our results are in line with other evidence that suggests that natives' mobility is limited even in response to other (more endogenous) UK migration shocks (Muellbauer and Murphy 1988; Hatton and Tani 2005). Furthermore, the evidence for the UK of relative persistence of employment and unemployment differentials across regions also suggests that mobility only facilitates labour market adjustments to a limited extent (Pissarides and McMaster 1990). This is also in line with some studies for the United States, where little evidence was found that natives respond to migrants through mobility (Butcher and Card 1991; White and Liang 1998; Card 2001), although it is in contrast with other evidence where a stronger or larger association was found (Filer 1992; Frey 1995; Borjas 2006).

#### 4.1.1 Demographic groups

The implicit assumption so far is that all WRS migrants compete with all natives in each area (district, county and region), which may not be realistic. This is because the vast majority of WRS migrants do not compete with highly skilled natives. We relax this assumption by assuming that WRS migrants are only substitutes for low-skilled natives within each area. We also experiment with other vulnerable groups, such as female and young natives.

Put differently, we now restrict our sample to specific demographic groups as our earlier estimates for the entire pool of unemployed workers may be diluting more adverse effects for low-wage workers (Altonji and Card 1991). Also, the mobility behaviour of low-wage workers may be different (Borjas 2006). We thus re-estimate Equation [1] for three groups in turn: low skilled (those in elementary occupations), young (those between 18 and 24 years of age) and women. These are workers likely to be competing directly with WRS migrants (see Section 2.1). For example, employers may substitute away from mothers with small children or unskilled young workers and towards male migrants.

Row 1 of Table 3 shows an insignificant −0.021 estimate for low-skilled workers at the district level (compare with the insignificant 0.037 estimate in

**Table 3:** Unemployment effects of migration (demographic groups)

| **Models** | **Coefficient** | **Standard errors** |
|---|---|---|
| **A – District Level** | | |
| (1) Low Skilled | **−0.021** | 0.028 |
| (2) Young | **−0.030** | 0.033 |
| (3) Female | **0.015** | 0.017 |
| (4) London South East and East of England | **0.051** | 0.057 |
| (5) Agriculture | **0.073** | 0.014 |
| **B – County Level** | | |
| (1) Low Skilled | **0.043** | 0.029 |
| (2) Young | **0.006** | 0.028 |
| (3) Female | **0.020** | 0.013 |
| (4) London South East and East of England | **−0.055** | 0.014 |
| (5) Agriculture | **0.043** | 0.037 |
| **C – Region Level** | | |
| (1) Low Skilled | **0.043** | 0.041 |
| (2) Young | **0.106** | 0.038 |
| (3) Female | **0.015** | 0.024 |
| (4) London South East and East of England | **−0.166** | 0.278 |
| (5) Agriculture | **−0.014** | 0.070 |

Notes:
(a) Notes as in Table 2.
(b) All estimates here to be compared with estimates in row (1) of each respective panel of Table 2.

row 1 of panel A of Table 2). This suggests, if anything, a less adverse effect for the low skilled at the district level. The estimate is a more adverse but insignificant 0.043 when allowing low-skilled workers to search for jobs at the county or region level. This suggests that the low skilled are area bound and offers little evidence that migrants are substitutes for low-skilled natives (see Section 4.1).

Row 2 shows that for young workers, the estimates are more adverse the wider the aggregation level: an insignificant −0.030 (0.006) at the district (county) level, and a significant 0.106 at the region level. Thus, an increase of one percentage point in the WRS migration rate increases UK youth claimant unemployment by 0.106 percentage points when young workers' local labour market is within a region. This suggests that migrant labour may be a substitute for youth labour. It also suggests that young natives may be more mobile than other natives and that more adverse effects at the region level might have been concealed at the district and county levels (see Section 4.1).

In contrast, row 3 shows that for female workers the estimates do not change much across aggregation level. This suggests that women are area-bound, perhaps because they are tied movers/stayers (Borjas 2006). The insignificant 0.015 and 0.020 estimates offer little evidence that migrants are substitutes for native women.

We further check the robustness of our estimates by restricting our sample to areas with relatively high proportions of WRS migrants (see Table 1). The motivation here is that our estimates for all areas may be diluting more adverse effects for affected areas. We thus re-estimate Equation [1] in turn for: London, the Southeast and East of England areas, and agricultural areas (comprising 5% or more of the working age population in agricultural jobs).

Row 4 shows, interestingly, that for London, the Southeast and East of England, the estimates are less adverse the wider the aggregation level (see Section 4.1). The estimate is an insignificant 0.051 (−0.166) at the district (region) level, though it is a significant −0.055 at the county level. This suggests that an increase of one percentage point in the WRS migration rate decreases claimant unemployment by 0.055 percentage points in the London, Southeast and East of England areas when natives' local labour market is within a county.

Similarly, row 5 shows that for agricultural areas, the estimates are less adverse the wider the aggregation level. The estimate is a significant 0.073 at the district level and an insignificant 0.043 (−0.014) at the county (region) level. This suggests that an increase of one percentage point in the WRS migration rate increases claimant unemployment in UK agricultural areas by 0.073 percentage points when natives' local labour market is within a district. This suggests that competition among native agricultural workers and migrants takes place in small neighbourhoods.

Thus, our conclusion from before is broadly maintained. We found only sparse evidence of adverse effects on claimant unemployment. While low-skilled and female claimant unemployment was not adversely affected, we found a small adverse effect for young natives at the region level. Similarly, while claimant unemployment was not adversely affected in London, the Southeast and East of England, we found a small adverse effect in agricultural areas at the district level.

Our main conclusion thus far is that there is little evidence that an increase in the WRS migration rate adversely affected the claimant unemployment rate in the UK between 2004 and 2006. Our results are in line with the international literature, where adverse employment effects are small. They are also in line with the very limited evidence for the UK: Dustmann, Fabbri, and Preston (2005) reported insignificant employment and unemployment effects using LFS data for the 1980s and 1990s. They also reported insignificant effects for high- and low-skilled workers, though small and significant adverse effects for the middle group.[12]

## 4.2 Instrumental variables

Our uninstrumented estimates in Sections 4–4.1.1 suggest little evidence of adverse claimant unemployment effects and little evidence of severe natives' mobility (omitted variable) bias. Despite controlling for natives' mobility using two alternative proxies, we found little evidence of an associated bias. Also, we found little evidence of a bias when we allowed increased natives' mobility through aggregating the data at successively wider levels.

However, while those could be consistent estimates of an underlying true zero effect, they could also be biased estimates of an underlying positive effect. This is because we have so far focused on tackling one of the sources of endogeneity discussed in Section 4: natives' mobility. We now use instrumental variable estimation techniques to correct for other omitted variable bias, simultaneity bias and measurement error bias. Incidentally, instrumental variables will also correct for any natives' mobility bias that we might not as yet have uncovered and will thus attest to the suitability of our earlier identification strategies.

12 Although WRS migrants overwhelmingly concentrate in low-skilled elementary occupations, for completeness we also run robustness checks for middle and high-skilled occupations and found no evidence of adverse effects.

We begin by arguing that for the phenomenon we study a non-zero correlation between the error term and the migration variable is potentially weak and thus any associated endogeneity bias is not too severe. One concern here, as in most of the literature, is that migrants may respond to specific demand conditions when migrating into a particular area (also see Section 4.1). Indeed, in many migration studies (where smaller inflows occur over a longer period of time), migrants' decisions are primarily determined by labour market conditions. If migrants self-select into particularly booming areas, potential adverse effects in that labour market may be offset. If, however, migrants' decisions are a function of non-labour market factors (such as language, multiculturalism, cluster and network pull effects), the extent of the migrants' self-selection bias is lessened (Bartel and Koch 1991). And, if migrants distributed randomly across areas, this identification issue would entirely vanish. Although WRS migrants' location choices were not random, they were initially mainly driven by clusters and networks.[13] For instance, in the first month, 65% of all WRS migrants located in London, the South East and East of England, where pre-accession A8 clusters existed (see Section 2.1).[14] Although this number goes down to 52% during the first 6 months, it remains at 48% during the first 12 months, before going down to 37% during the next 12 months.[15]

Another source of bias in our estimates is measurement error, such as, for example, the presence of non-random outflow across areas. As the WRS does not record outflows, we assume it to be zero, and define our migration variable as the migration (inflow) rate, as is common in the literature (see Sections 2 and 2.1). If outflows are not zero and are large, implying severe measurement error, then our estimates will be biased towards zero. If in addition, outflows are strongly correlated with the migration (inflow) rate, then the direction and severity of the bias will depend on the sign and extent of such a correlation.

**13** While it is possible that some (more able or better informed) WRS migrants studied their UK employment probabilities and applied for jobs prior to migrating, the vast majority of them arrived in the UK without a job and without much knowledge of the labour market or the language (see Section 2). For example, the average number of days between entry and date of start of work is 116.3; 42.4% of WRS migrants were employed within 30 days, a further 11.2% within 60 days, and the other half took longer than 2 months to find work.

**14** It is possible that the pre-accession regional distribution is itself endogenous. London and the South East have more dynamic economies than other regions, though they also have higher unemployment (see Table 1). These are areas traditionally associated with migration from all countries (40% of all migrants reside in London).

**15** We performed robustness checks restricting our sample to the first 6 and 12 months and found no evidence of more adverse effects (also see Gilpin et al. 2006). We also performed robustness checks restricting our sample to London, the South East and East of England and found no evidence of more adverse effects (see Section 4.1.1).

For example, outflows could systematically vary across areas or over time. We performed robustness checks restricting our sample to agricultural areas, where outflows have a more systematic seasonal component, and found no evidence of more adverse effects, as discussed in Section 4.1.1 (see Table 3). We also performed robustness checks restricting our sample to low-skilled workers in elementary occupations, where outflows would be non-random if these occupations are used as entry-level jobs from which more able individuals exit relatively quickly. Again, we found no evidence of more adverse effects for the low skilled, as discussed in Section 4.1.1 (see Table 3). Our results are in line with those in Gilpin et al. (2006), who provide a detailed discussion on measurement error in the WRS in the context of a model comparable to Equation [1] and conclude that any associated bias is not too severe.

In sum, we argue that none of the above sources of endogeneity appears strong enough to have severely biased our estimates in Sections 4–4.1.1.[16] Nonetheless, we further check their robustness by re-estimating Equation [1] using the Generalized Method of Moments (GMM).

We begin with a novel instrument: the number of days elapsed between "entry date" and "start of work date" for WRS migrants and its lags. Therefore, the greater the number of migrants entering the country, the greater the number of days elapsed. However, the number of days elapsed is not simultaneously determined with claimant unemployment. The results in row 1 of panel A of Table 4 confirm that the instruments are relevant and not endogenous. The estimate of interest remains insignificant and positive.

We also define another novel instrument: the proportion of pre-accession occupational switchers in the working age population. We define switchers as those natives that have been unemployed at least since April 2004 and report a sought occupation different from their usual occupation in area $i$ and time $t$. Therefore, the greater the number of migrants entering an area-occupation, the greater the number of switchers. However, the actual number of switchers is not simultaneously determined with claimant unemployment, since they are already claimant unemployed, whatever their usual/sought occupation. As we define this for each occupation, we have nine instruments. The implicit assumption is

16 It is worth noting that, during the sample period, the number of WRS migrants eligible and in receipt of JSA is negligible. This is because A8 nationals only had access to certain social security benefits, such as the JSA, 2 years after registration in the WRS (Home Office 2004) (see Section 2). For similar reasons, the number of other recently arrived non-eligible non-A8 migrants is also negligible; earlier eligible migrants are treated as UK residents (see Sections 2 and 6). Furthermore, our variable of interest is JSA claimant unemployment, as opposed to broader (ILO) unemployment or employment, and this reduces further concerns of simultaneity bias.

**Table 4:** Unemployment effects of migrantion (instrumented)

| Models | Coefficient | Standard errors | Hansen–Sargan test | | Hausman test | | F test | | |
|---|---|---|---|---|---|---|---|---|---|
| | | | Statistic | *df* | Statistic | *df* | Statistic | *df* | |
| **A – District level** | | | | | | | | | |
| (1) Number of days elapsed between "entry date" and "start of work date" | **0.078** | 0.148 | 7.56 | 5 | 0.46 | 1 | 6.51 | 6 | 8530 |
| (2) Proportion of pre-accession occupational switchers | **0.154** | 0.209 | 11.27 | 8 | 0.26 | 1 | 2.87 | 9 | 8527 |
| **B – County level** | | | | | | | | | |
| (1) Number of days elapsed between "entry date" and "start of work date" | **0.129** | 0.164 | 7.58 | 5 | 1.61 | 1 | 5.23 | 6 | 1201 |
| (2) Proportion of pre-accession occupational switchers | **0.159** | 0.168 | 4.62 | 8 | 2.17 | 1 | 5.90 | 9 | 1198 |
| **C – Region level** | | | | | | | | | |
| (1) Number of days elapsed between "entry date" and "start of work date" | **0.176** | 0.251 | 9.40 | 5 | 0.84 | 1 | 9.29 | 6 | 214 |
| (2) Proportion of pre-accession occupational switchers | **0.110** | 0.168 | 8.12 | 8 | 0.32 | 1 | 11.11 | 9 | 211 |

Notes:
(a) Notes as in Table 2, except that these are now GMM estimates.
(b) All estimates here to be compared with estimates in row (1) of each respective panel of Table 2.

that these pre-accession (long-term) unemployed natives would switch to a different sought occupation in response to the WRS shock in their usual occupation. This is a strong assumption, as unemployed natives might switch occupations for reasons other than the WRS shock. However, we already control for other demand and supply shocks and for area- and time-fixed effects in our model. Although defining our instrument over the sample of long-term unemployed in each period might have yielded a more relevant instrument, defining it over the sample of pre-accession long-term unemployed yields a more exogenous instrument. Row 2 confirms that the instruments are relevant and not endogenous. The estimate of interest remains positive and insignificant.[17]

Panels B and C show that the estimates at the county and region levels are also positive and insignificant. This is reassuring of our earlier main conclusion of little evidence of adverse claimant unemployment effects. Dustmann, Fabbri, and Preston (2005) also reported close insignificant instrumented and uninstrumented unemployment effect estimates, suggesting that any associated endogeneity bias was not too severe.

## 4.3 National and occupational level

Our estimates exploiting variation in the migration and unemployment rates across areas (district, counties and regions) in Sections 4–4.2 derive from four different identification strategies and reassuringly confirm the same main conclusion of little evidence of adverse claimant unemployment effects. We now

17 Finding valid instruments really is very hard, especially because, ideally, they need to vary at the month and district level. This largely constrained us to instruments derived from our datasets. Obvious instruments were lags of our migration rate. We also experimented with lags of the entry-migration rate, where we use "entry date" to define the instrument and "start of work date" to define the variable of interest (see Section 2). We then defined predicted migration rates using the A8 distribution in the 1991 and 2001 Census (Card 2001; Dustmann, Frattini, and Preston 2008). We also experimented with instruments derived from Civil Aviation Authority (CAA) data, such as a flight indicator and its interaction with the distance between a particular A8 country and a particular UK district; the minimum, maximum and average air fare prices; the number of air fares (one way and return); and the number of passengers travelling (arriving and departing) between A8 countries and UK districts. Although the associated results were qualitatively similar, these instruments were less relevant and the estimates less precise. We further used other instruments suggested in the literature, such as historic migration rates, house prices, vacancy rates and temperature (Hatton and Tani 2005; Saiz 2007; Hunt 1992). However, the poor quality of the data and/or the lack of variation at the district and month level cast doubt on the results.

probe the robustness of these results further by exploiting variation in the migration and unemployment rates across occupations.

Stratification across occupations is fruitful because migrants and natives compete more directly within occupations than across areas (see for example Card 2001; Friedberg 2001). In addition, natives' mobility bias and migrants' self-selection bias are less of a concern across occupations, as we discuss below. Furthermore, unless simultaneity bias and measurement error bias manifest in exactly the same way across areas and occupations, aggregation across occupations will attest to the suitability of our earlier identification strategies.[18]

We begin by arguing that the nature of the particular phenomenon we study here makes stratification across occupations particularly appealing. Not only was the WRS inflow much larger and faster than expected but it was also heavily concentrated in low-skilled elementary occupations. While elementary occupations received 46% of the WRS migrants, other occupations received between 1% and 6% (excluding machine operatives; see Table 1).

Firstly, the nature of the WRS shock reduces concerns that natives' mobility bias is severe. Occupations are more closed labour markets than areas because occupation mobility often requires retraining (Friedberg 2001; Borjas 2003). In the longer run, migration might drive natives' skill upgrading and thus migrants and natives occupational choices might become endogenously determined (Peri and Sparber 2008). However as this takes time, occupational labour markets are relatively closed in the short run. Thus, most low-skilled natives in elementary occupations competing with WRS migrants would not have immediate access to jobs in other occupations (some limited mobility here derives from occupational progression, which we control for in our regression models, as we discussed in Sections 2.1 and 3). This is especially so because the WRS inflow was faster than expected and thus natives' response through retraining and mobility away from elementary occupations would probably be lagged enough to ensure identification of adverse unemployment effects.

**18** Several skill definitions have been used in the literature: occupation, education, education-experience, and so on (see for example Card 2001; Borjas 2003). Occupation is measured more accurately than education and experience. Firstly, the extent and quality of education varies across countries. Therefore, migrants and natives in the same education cell may have different skills and compete for different jobs. Secondly, occupation measures the effective reward that migrants obtain, after usual skill downgrading due to language or other labour market barriers. Thirdly, there is evidence that natives and migrants are imperfect substitutes within education groups in the UK (Manacorda, Manning, and Wadsworth 2006). As discussed in Section 4.1, identifying accurately who competes with whom is crucial, as poor skill group allocation results in poor identification.

Secondly, the nature of the WRS shock also reduces concerns that migrants' self-selection bias is severe. It is well documented in the literature that migrants often experience occupation downgrading on arrival. This happens when language or labour market barriers prevent migrants from immediately self-selecting into more favourable occupations (see for example Card and DiNardo 2000; Friedberg 2001). This means that migrants' longer run (more endogenous) occupational choice might differ from their (more exogenous) entry-level occupation. Since WRS migrants are relatively well educated, their overwhelming concentration into low-skilled elementary occupations suggests they were unable to self-select into more favourable or booming occupations. Put differently, the WRS inflow into elementary occupations is a supply shock relatively unaccompanied by a demand shock. This is in contrast with machine operatives, which might have been hit simultaneously by demand (e.g. booming construction industry) and supply shocks (e.g. WRS migration inflow). So, while in machine operatives self-selection bias might be more severe, in elementary occupations it is less of a concern. (We perform robustness checks excluding machine operatives from our regressions below.)

In sum, we argue that elementary occupations are relatively closed markets, where immediate natives' mobility is limited. We also argue that elementary occupations are a relatively constrained occupational choice, as migrants are unable to self-select into other more booming occupations. Finally, we argue that, other occupations, which received relatively low proportions of migrants (excluding machine operatives), constitute a clear counterfactual.

To exploit this, we now aggregate the data across occupations. Incidentally, this enables us to relax the assumption we made in Sections 4, 4.1 and 4.2 that all WRS migrants compete with all natives; our implicit assumption now is that low-skilled (high-skilled) WRS migrants compete with low-skilled (high-skilled) natives in a national market. That is, we assume that migrants and natives are only substitutes within occupations. Furthermore, given that the majority of WRS migrants concentrate in elementary occupations, and given that low-skilled natives are relatively region-bound, our final assumption is that migrants and natives are only substitutes within occupations within regions. The main difference is that at the national-occupation level (i.e. $9 \times 24$ cells), migrants and natives compete across the country; whereas at the regional-occupation level (i.e. $12 \times 9 \times 24$ cells), they compete only within the region. Given that crossing the country for a low-paid job may be financially prohibitive for a native, it may be more realistic to stratify the labour market at the regional-occupation level than at the national-occupation level for the particular phenomenon we study here (see Section 4.1). However, as before, if low-skilled natives are relatively region bound, then estimates at both levels should not differ much.

We therefore re-estimate Equation [1] replacing $i$ with $j = 1, \ldots, 9$ to mean occupations (see Table 1)[19] and re-defining $X_{jt}$, due to data limitations, to include the lagged proportion of WRS migrants who are women, young and parents (along with average number of children); their lagged average hours worked; the lagged proportion of unemployed who are women and young; the lagged average claim duration; and a proxy for netflow rate.

The netflow rate proxy we use here is a variable that we construct to measure what would have been natives' unemployment netflow had migrants not arrived. This is not the ideal counterfactual – this would have been natives' working age population netflow had migrants not arrived – which we do not observe, as discussed in Section 4. It is instead based on the unemployed and their usual (versus sought) occupation. More specifically, we measure the inflow of natives into a (sought) occupation as the number of natives who report a move from a different (usual) occupation; conversely, we measure outflow as the number of natives reporting a move in the opposite direction. We then calculate the netflow rate as before (see Section 2.1) and call this the counterfactual native unemployment netflow rate.

Row 1 of panel A of Table 5 shows that our baseline estimate is an insignificant 0.017. Controlling for native netflow rate yields a significant 0.008 estimate in row 2, which indicates a very small adverse effect. This suggests that any natives' mobility (omitted variable) bias, here corrected for by the explicit

**Table 5:** Unemployment effects of migration (by occupation)

| Models | Coefficient | Standard errors |
|---|---|---|
| **A – Occupation level** | | |
| (1) Baseline specification | **0.017** | 0.025 |
| (2) Adding counterfactual native unemployment netflow rate | **0.008** | 0.002 |
| *(2a) Excluding machine operative occupations* | **0.012** | 0.003 |
| **B – Occupation-Region level** | | |
| (1) Baseline specification | **0.056** | 0.066 |
| (2) Adding counterfactual native unemployment netflow rate | **−0.001** | 0.009 |
| *(2a) Excluding machine operative occupations* | **−0.040** | 0.023 |

(a) Notes as in Table 2, except that the number of observations is now $9 \times 24$ at the national-occupation level and $9 \times 12 \times 24$ at the regional-occupation level. As before, time-fixed effects are modeled with month dummies. Area and/or occupation fixed effects (and their interaction) are differenced out.

19 Our results using sought occupation, which better captures labour market effects, were also robust to using usual occupation instead.

inclusion of the native netflow rate proxy, is not too severe, since the estimate remains small – in fact, smaller. Finally, restricting the sample in row 2a to exclude machine operative occupations, where self-selection bias may be a concern, yields a significant 0.012 estimate. Thus, an increase of one percentage point in the WRS migration rate increases claimant unemployment by 0.012 percentage points when machine operatives are excluded.

Thus, our main conclusion from before of little evidence of adverse claimant unemployment effects is again maintained. This is in contrast with results in Borjas (2006), where more adverse effects were found at wider aggregation levels. Although our results were also successively larger at the district, county and region levels, they are smaller at the nation level (compare Tables 2 and 5). As we argued in Section 4.1, natives' mobility may not fully explain larger effects in wider areas. Furthermore, our results in Sections 4.1 and 4.2 suggest that natives' mobility responses to the WRS migration shock were fairly modest.

Nonetheless, we check whether these small estimates at the national-occupation level are driven by omitted area-fixed effects by aggregating the data at the regional-occupation level. As we argue above, the later may be more relevant, as region-bound low-skilled natives are more likely to compete with WRS migrants. We re-estimate Equation [1], where $i$ and $j$ are defined as before, and $X_{ijt}$ includes the same variables as $X_{jt}$.

Panel B shows a larger but insignificant 0.056 baseline estimate in row 1. Controlling for native netflow rate in row 2 yields a smaller and insignificant −0.001, alongside a smaller and insignificant −0.040 when we restrict the sample to machine operatives in row 2a. Thus, our main conclusion from before of little evidence of adverse claimant unemployment effects is yet again maintained.

In sum, the estimates at the national-occupation and at the regional-occupational levels do not differ much. This confirms that low-skilled natives are relatively region bound; it also confirms that there is little evidence that natives' mobility offsets more adverse effects. Our results are again in contrast with those in Borjas (2003), who reports smaller estimates when labour markets stratified by education-experience are limited by geographical boundaries. However, our results are in line with those in Card (2001), who reports small employment effects even when labour markets stratified by occupation are limited by geographical boundaries and does not find evidence that natives' mobility offsets more adverse effects.

# 5 Wage effects

One explanation for little evidence of adverse claimant unemployment effects in Sections 4–4.3 is that adjustment to the WRS inflow occurred not through unemployment but through wages. We thus estimate the effect of the WRS migration inflow on UK wages using a reduced form equation grounded on standard theory (see for example, Borjas 1999; Card 2001; Dustmann, Fabbri, and Preston 2005):

$$\Delta W_{iy} = \beta^w \Delta M_{iy} + \lambda^w \Delta X_{iy} + f_y^w + \Delta\varepsilon_{iy}^w \qquad [2]$$

where $\Delta W_{iy}$ and $\Delta M_{iy}$ are our wage and migration variables, defined in Section 2.1, in district $i = 1, \ldots, 409$ and tax-year $y = 1, \ldots, 3$; $f_y^w$ is time-fixed effects; $\varepsilon_{iy}^w$ is the error term; and $X_{iy}$ are labour demand and supply shifters that include the proportion of the total population who are women, young, ethnic minorities and migrants from outside the A8 countries; the lagged proportion of WRS migrants who are women, young and parents (along with average number of children). As before, we estimate Equation [2] in first-difference using GLS and thus area-fixed effects were differenced out; time-fixed effects are now modelled using year dummies. The interpretation of our coefficient of interest is that a one percentage point increase in the migration rate changes wages by $\beta^w$ %.[20]

We then re-estimate Equation [2] re-defining $W_{iy}$ to mean, in turn, the 5th, 10th, 20th, 25th, 30th, 40th and 50th percentiles of the log hourly pay distribution. This is to uncover potential wage effects for lower paid workers that could be masked by the average wage effect. Table 6 shows positive and insignificant estimates. They are 0.110, 0.323 and 0.438 respectively for the 10th, 25th and 50th percentiles. This suggests that wage effects are smaller at the bottom of the distribution, where migrants and natives are likely to be substitutes (e.g. those in elementary occupations are located in the 5th and 10th percentiles), and larger higher up, where migrants and natives are likely to be complements (e.g. those in machine operative occupations are located around

**20** Unlike the JSA unemployment data, which contained a negligible number of WRS migrants, the ASHE wage data contains both natives and WRS migrants, as discussed in Section 2. Thus, we estimate the effect of the WRS migration shock on the wages of both natives and WRS migrants. In other words, it is possible that simultaneity bias might be more of a concern in our wage models. Nevertheless, this is a common feature of employment and wage data in most migration studies in the literature and what is actually uncommon is to have a relatively simultaneity-bias-free measure of unemployment, as we do (see Footnote 16). The drawback here is that our wage estimates were not subject to instrumental variable robustness checks. (They were robust, however, to natives' mobility (omitted) variable bias, when we controlled for lagged working age population growth and natives' netflow rate, as in Section 4.) This is due to data limitations – since our wage data is not as rich as the claimant unemployment data – but also because there was little evidence of severe endogeneity bias (deriving from omitted variable bias or measurement error bias) in Sections 4.1–4.3. As a result of this drawback, a cautious view is that our wage estimates derive from a descriptive (not causal) model.

**Table 6:** Wage effects of migration

| Models | Coefficient | Standard errors |
|---|---|---|
| (1) Average wage | **0.246** | 0.276 |
| (2) 5th percentile | **0.212** | 0.190 |
| (3) 10th percentile | **0.110** | 0.220 |
| (4) 20th percentile | **0.162** | 0.305 |
| (5) 25th percentile | **0.323** | 0.313 |
| (6) 30th percentile | **0.365** | 0.239 |
| (7) 40th percentile | **0.453** | 0.250 |
| (8) 50th percentile | **0.438** | 0.307 |

(a) Notes as in Table 2, except that the dependent variable is now the average and various percentiles of the wage distribution across years and districts, and that the number of observations is now 409 × 3.

the 40th percentile).[21] Nonetheless, as the estimates are insignificant throughout, our main conclusion is that there is little evidence that an increase in the WRS migration rate adversely affected wages in the UK between 2004 and 2006.

Our estimates are in line with evidence in the international literature, where adverse wage effects are small. They are also in line with the limited evidence available for the UK. Using LFS data for the 1980s and 1990s, Dustmann, Fabbri, and Preston (2005) found no evidence of adverse wage effects and hinted that this may be in part because migrants' skill distribution resembles that of natives. However, Manacorda, Manning, and Wadsworth (2006) argue that the associated relative labour supply change ought to have induced wage effects. Using LFS and BHPS data between the 1970s and 2000s, they also found no adverse wage effects and argue that this is because natives and migrants are imperfect substitutes. They then detected some adverse wage effects for earlier migrants. This is in line with findings in Dustmann, Frattini, and Preston (2007) of negative wage effects at the bottom of the distribution – where earlier migrants are more concentrated – and positive effects higher up the distribution, when using LFS data for the 1990s and 2000s.

Given that WRS migrants overwhelmingly concentrate around the 5th and 10th percentiles of the wage distribution, we also expected to find more adverse

**21** Claimant unemployment effects were not substantially more adverse when we excluded machine operatives (see Table 5), as might have been expected if demand factors attracted both migrants and (claimant) natives (see Section 2.1). An explanation here is that most such demand factors were controlled for in the model. Another explanation is that natives other than claimants were attracted to machine operative's demand shock.

(or less favourable) effects there. Our estimates were indeed smaller at the bottom than higher up the distribution, but they were insignificant throughout. Although Dustmann, Frattini, and Preston (2007) found significant instrumented estimates, their associated uninstrumented estimates were also insignificant throughout the distribution. For example, our insignificant 0.246 estimate of the average effect is close to their insignificant 0.266 uninstrumented estimate, though smaller than their associated significant 0.396 instrumented estimate.[22] In contrast with Dustmann, Frattini, and Preston (2007), our instrumented estimates are often less precise than our uninstrumented estimates (see Tables 2 and 4). Also, endogeneity bias is less of a concern here because the supply shock we study is more exogenous (see Section 4.2).

# 6 Discussion

Our main conclusion is that there is little evidence that the WRS migration inflow adversely affected wages or claimant unemployment in the UK between 2004 and 2006. This conclusion is in line with the limited evidence available for the UK and with other evidence in the international literature. Our wage effect estimates are positive, small and insignificant, smaller at the bottom of the distribution. Our unemployment effect estimates are small and, in the main, insignificant. These estimates are in line with our earlier descriptive analysis and are robust to a number of specification checks and estimation methods as well as to several different stratifications of the labour market and different sub-samples of workers. In particular, we have thoroughly checked the robustness of our estimates to two main identification issues that underline the debate in the literature: natives' mobility and migrants' self-selection.

The crucial point was to establish whether the location and occupation choice of migrants was strongly driven by local labour market conditions and whether natives strongly responded to migration inflows by moving to other

**22** Our insignificant 0.323 and 0.438 estimates for the 25th and 50th percentiles are larger than their 0.136 and 0.234 insignificant estimates (their associated instrumented estimates are an insignificant 0.211 and a significant 0.660). Our insignificant 0.110 estimate for the 10th percentile is again close to their insignificant uninstrumented –0.094 estimate (their associated significant instrumented estimate is –0.516), although in the opposite direction. One explanation here is that the minimum wage was in force and increasing throughout the period we study, possibly mitigating or offsetting more adverse wage effects for lower paid workers, as we discuss below. One fruitful avenue of research is to extend the sample period in Dustmann, Frattini, and Preston (2007) accordingly.

areas or occupations, which would invalidate the cross-areas and cross-occupations analysis here. We established that neither source of endogeneity was strong enough to severely bias our estimates. We stratified labour markets in various dimensions (district, county, region, nation-occupation, region-occupation, agriculture, low skilled, young and female) to test alternative assumptions on the substitutability between migrants and natives. Firstly, we allowed migrants and natives to compete across ever wider areas and found small positive and insignificant estimates. Secondly, our estimates were, if anything, smaller and insignificant when we allowed migrants and natives to compete on a national labour market across occupations. Thirdly, our estimates were still small positive and insignificant when we explicitly controlled for natives' mobility using two different proxies. Fourthly, our instrumented estimates remained small and insignificant when we corrected for potential correlation between the migration variable and omitted (migrants' self-selection and natives' mobility) variables.

In sum, our estimates are reassuringly small and insignificant across a number of specifications, sub-samples and estimation methods and are not sensitive to the counterfactual underlying each model. In particular, our results do not appear to be driven by endogeneity bias – we found no evidence that migrants' self-selection or natives' mobility offset more adverse effects.

While our results are robust and are in line with other results in the literature, they are puzzling. This is because standard theory only predicts no adverse wage and employment effects when migrants' skill composition resembles that of natives – i.e. when the migration inflow is balanced across area or skill. If the inflow increases relative labour supply in a particular area or skill, then downward pressure on wages and employment is expected. In particular, the wage structure should be affected: competing (complement) workers should have lower (higher) wages. Given that the WRS inflow was large, rapid and not balanced across areas or occupations, and given the little evidence of adjustment in wages or claimant unemployment, the obvious question is how the UK labour market adjusted.

One answer is that, as WRS migrants overwhelmingly compete with low-skilled workers, and as these workers were protected by a concurrently increasing minimum wage, more adverse wage effects may have been mitigated or offset. This might in turn suggest a more adverse unemployment effect for minimum wage workers. While we could not directly estimate such an effect, as we do not observe (past) wages for the unemployed and therefore cannot identify who previously earned the minimum wage, we argue, tentatively, that the small positive and (sometimes) significant effects we report for agriculture and young workers, whom disproportionately earn the minimum wage, suggest a more adverse effect for minimum wage workers. However, we found no effect for the low-skilled in elementary occupations, where minimum wage workers are also overrepresented. Since WRS migrants heavily concentrated in elementary occupations, this was an obvious place to find adverse effects on claimant unemployment. Another group likely to earn the minimum wage is earlier migrants. Thus an avenue for future research is to estimate the effect of the WRS inflow on wages and unemployment for this group (unfeasible here because neither the ASHE nor the JSA records nationality).

A further avenue for future research is to use a broader definition of unemployment, including workers who are not eligible for JSA (e.g. workers with insufficient past contributions) and workers who choose not to claim JSA (e.g. workers unwilling to enter the JSA contract for job searching, discouraged workers). This, in turn, suggests another potentially important explanation for our findings of little evidence of adverse effects on claimant unemployment and wages. We might not have captured a purely labour market effect. We might, instead, have captured a social security benefit effect. That is, another interpretation of our model is that it estimates the effect of the WRS migration shock on the demand for JSA (refer "unemployment benefit"). In this sense, our results edge on the literature on the impact of migration on the demand for social security benefits (Borjas 1994; Kvist 2004; Wadensjo 2007). An explanation that can be applied to our results, deriving from this literature, is that instead of increasing the demand for unemployment benefit, the WRS migration shock discouraged workers from looking for a job. These workers then effectively withdrew from the labour market. In other words, our results concern claimant unemployment only and more adverse effects might be found if broader measures of unemployment, which captured a more direct labour market effect, were used (naturally, this strategy would bring its own identification issues, and suitable data, at fine aggregation levels, might be hard to find).

Other answers common in the literature on how labour markets adjust to migration shocks, given small wages and employment effects, include factor equalisation as well as industry structure and output mix adjustments. The first explanation is that internal flows of goods, capital and labour (i.e. natives' mobility) equalise labour market opportunities across areas or skills following a migration inflow. Yet, the large body of evidence on persistency in regional wage and employment differentials in the US and UK following other shocks make it implausible that markets adjust instantaneously to migration shocks – particularly one as unexpectedly large and fast as the WRS. Furthermore, this explanation implies that there are unobserved factors correlated with the migration variable that would severely bias wage and employment coefficients away from adverse effects. Yet, despite some evidence of instrumental variable bias correction, adverse effects remain modest in the literature – in particular, we found little evidence of endogeneity severely biasing our estimates.

The second explanation is that firms adjust their production function and production mix to take advantage of the shift in the relative supply of labour. This might be an appealing explanation in a small open economy such as the United Kingdom. Yet, the available (mostly United States) evidence suggests that industry structure changes offer little explanation on how large migration inflows are absorbed – again, it seems implausible that UK firms would adjust instantaneously to a migration shock as unexpectedly large and fast as the WRS.

Although neither explanation offers an immediate solution to the puzzle, a fruitful avenue for future research is more UK-based evidence on both fronts. This would help to understand how native workers respond to competition from migrants and how firms alter their production function and production mix in response to migration-led labour supply shifts.

# 7 Conclusion

Following the enlargement of the EU in May 2004, there was a large, rapid and concentrated inflow of accession migrants into the UK. We described and evaluated the impact of this inflow on the UK labour market. We found little evidence that the inflow of accession migrants contributed to a fall in wages or a rise in claimant unemployment in the UK between 2004 and 2006.

This new evidence is an important contribution to the very limited UK migration literature – in particular, it helps to fill a gap in the literature on the effects of the recent EU enlargement. This new evidence is also an important contribution to the international literature because it applies a thorough and comprehensive empirical estimation approach to new and rich monthly micro datasets to study a migration inflow that was larger and faster than anticipated. Such a shock arguably corresponds more closely to an exogenous supply shock than most migration shocks studied in the literature – and this helps, to some extent, to circumvent identification issues that underline the debate in the literature.

Most crucially, this new evidence is an important contribution to informing policymaking on the face of further EU enlargements. Given the heated public debated on migration – and in particular on migration from current and future accession countries – this is a timely contribution. For example, the relatively benign evidence for the UK might have helped to influence policymakers' decisions in ten other EU countries to either lift or alleviate restrictions on accession migrants in 2006, 3 years before their final deadline.

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