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DISCUSSION PAPER SERIES

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ABSTRACT

Peepoo! Uncovering the Impact of the Eastern European Immigration Shock on Wages Employment and Unemployment in the UK*

No empirical evidence has ever been reported that the large inflow of accession immigrants – following the 2004 expansion of the European Union – led to a fall in wages or employment, or a rise in unemployment in the UK between 2004 and 2006. This immigration shock was unexpectedly larger and faster – as well as more concentrated into areas and occupations – than anticipated, seemingly more akin to an exogenous supply shock than most immigration shocks. Exploiting rich but underused individual level data from the Lifetime Labour Market Database (LLMDB) we estimate the effect of this immigration shock on wages, employment and unemployment of natives and previously existing immigrants in the UK. We confirm once again the finding of little evidence that the inflow of accession immigrants led to a fall in wages, a fall in employment, or a rise in unemployment of natives in the UK between 2004 and 2006. However, we uncover, for the first time, novel evidence of adverse employment and unemployment effects for low paid existing immigrants as a result of the accession immigration inflow. This is more severe for low paid immigrants and young low paid immigrants as well as for long term unemployed immigrants.

JEL Classification: J22

Keywords: immigration, employment, wages, Central and Eastern Europe, UK

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1. Introduction

The expansion of the European Union (EU) triggered a sizeable wave of immigration into the UK. Between May 2004 and May 2006, more than half a million workers entered the UK labour market, as recorded by the Worker Registration Scheme (WRS). Almost immediately, concerns arose about associated negative effects: Jobseeker's Allowance claimant numbers rose by nearly 100,000 and wages were said to be under downward pressure (Blanchflower et al. 2007). Nonetheless, empirical evidence from the period largely failed to support adverse effects on wages, employment or unemployment. More broadly, previous research has shown little supporting evidence of adverse labour market immigration effects in the UK (Dustman et al. 2005 2007; Manacorda et al. 2006; Anderson et al. 2006; Drinkwater et al. 2009; Lemos 2013 2014; Lemos and Portes 2014). [2]

The initial heated debate about the striking lack of evidence of adverse effects gradually turned into a tenuous consensus that this large and fast shock was absorbed without substantial adverse effects on wages or employment. This was particularly relevant at the time, as immigration – even on the face of no adverse effects – was a contentious issue in the wider public debate. And this continues to be relevant now, as immigration remains as contentious a labour market issue as ever.

The main contribution of this paper to the literature is to uncover, for the first time, adverse employment and unemployment effects following the accession immigration inflow. While confirming, once again, the finding of little evidence of a fall in wages, a fall in employment, or a rise in unemployment for natives in the UK between 2004 and 2006, we present novel empirical evidence uncovering adverse employment and unemployment effects for previously existing immigrants in that period. This is more severe for low paid immigrants and young low paid immigrants as well as for long term unemployed immigrants.

This is an important contribution to the literature and policymaking. Firstly, this new evidence helps to reconcile theoretical predictions of adverse effects with previous empirical results showing no such adverse effects – without invalidating previous results. That is, this fresh insight offers a credible explanation of just how the UK labour market adjusted to such a large, fast and concentrated immigration inflow with little overall adverse effects: accession

[2] One branch of the international literature reports limited evidence of adverse effect on employment and wages (Altonji and Card 1991; Card 1990 2001 2005 2007; Carrasco et al. 2008; Chiswick 1980; Friedberg 2001; Grossman 1982; LaLonde and Topel 1991; Pischke and Velling 1997), while another, reports some evidence of adverse effects (Borjas 2003 and 2006; Angrist and Kugler 2003; Orrenius and Zavodny 2007). This debate is rooted in difficulties over identification issues (Borjas 1999 and 2006; Card 2001; Carrington and Lima 1996; Chiswick 1991 1992 1993; Friedberg and Hunt 1995; Hunt 1992) (see Sections 3 and 4).

immigrants heavily competed with low paid low skilled previously existing immigrants. Secondly, this new evidence adds to the very limited evidence on immigration effects in the UK, especially on effects of the 2004 EU expansion, and thus helps to inform policymaking on ongoing immigration issues affecting the economy – all the way to affecting general elections and referendums, in which immigrants do not vote. Although these results are for the UK, adverse effects for previously existing low paid immigrants (instead of for all low paid) have long been speculated. Thus, the evidence here, although first uncovered for the UK, is likely to extend to other countries, and this offers a promising avenue for future research.

This contribution is all the more robust because we use high quality rich but underused individual level data from the Lifetime Labour Market Database (LLMDB). Given that the unavailability of data is among the main reasons for thin evidence on immigration effects, leveraging the LLMDB for immigration research is itself a further contribution to the literature.

A further notable contribution of this paper is that the large, fast and concentrated inflow of accession immigrants is seemingly akin to a natural experiment – largely driven by political decisions – corresponding more closely to an exogenous supply shock than most immigration shocks. The accession shock was one of the largest and fastest immigration inflows into the UK on record (Salt and Miller 2006), roughly equivalent to 2% of total employment. Crucially, it was an immigration inflow unexpectedly larger and faster – as well as unexpectedly more concentrated into areas and occupations – than anticipated. This means that there was limited scope for pre-emptive anticipated labour market adjustments which might otherwise have lessened any adverse impact of the shock. Put differently, both natives' and immigrants' responses – via mobility out of and self-selection into specific areas and occupations – were delayed sufficiently to permit identification of adverse effects. Leveraging the accession shock to outwit such endogeneity issues, usually challenging in the literature, is a further contribution of this paper to the literature.

As we explore in detail below, the identification of any adverse wage or employment effects hinges on how mobile natives are across areas and occupations in response to immigration inflows and on how able immigrants are to self-select into thriving areas and occupations. On the one hand, accession immigrants were heavily concentrated into low paid low skilled occupations – this is where jobs are most accessible to immigrants faced with language and other labour market barriers – and this lessens concerns around immigrants' self-selection bias. On the other hand, low paid low skilled occupations function as relatively closed markets – with limited immediate exit options for natives – and this lessens concerns around mobility bias.

The remainder of this paper explores these issues in detail. Section 2 presents the data used in our analysis. Section 3 outlines the empirical model, followed by a thorough discussion of key identification challenges in Section 4. Section 5 discusses the results within the context of existing literature, and Section 6 concludes.

2. Data

We utilize high quality individual level data from the Lifetime Labour Market Database (LLMDB), a rich but underused administrative dataset. The LLMDB is constructed by linking several government datasets via unique national insurance numbers (NINo), yielding a 1% random sample of the UK population. Since a NINo is required for tax contributions, pension entitlements, and welfare claims, the data offers comprehensive coverage of the working age population. UK born individuals are automatically issued a NINo, while immigrants receive one upon entering the tax or benefits system. Throughout this paper, we use "natives" to refer to UK born individuals and "immigrants" to those born abroad.

One key strength of the LLMDB is its remarkably low attrition rate. Individuals remain in the data unless they have no recorded interaction with the tax or benefits system for more than a year – re-entry into the data occurs as soon as another interaction is recorded. Another strength is its accuracy: since it stems from administrative sources, measurement error is minimal.

A further strength of the LLMDB is that it is a rich dataset, including demographic information (date of birth, date of death, age, sex, address, nationality, place of birth, country of arrival, immigrants' age at entry, immigrants' entry date, etc.), labour market outcomes (employment and unemployment spells, earnings per job, weeks worked, weeks unemployed, number of jobs in the year, type of employment, etc.), and benefit and pension histories. While it lacks information on education – a common limitation in administrative data – we mitigate this by controlling for individual fixed effects and focusing on those aged 18 to 64 who are assumed to have completed their education. Information is not available on the immigrants' entry route (work permit, student visa, family reunification, etc.). However, as we restrict our sample to those already in the labour force, and as A8 nationals entered largely through the Worker Registration Scheme (WRS), we assume these are economic immigrants. We only include those earning between £100 and £1,000,000 in any one tax-year, and we remove the self-employed, for whom earnings are unavailable. We also exclude immigrants who arrived before 1945, due to small sample sizes, and retain only individuals observed in the database at least twice, which is necessary for fixed effects modelling. Finally, we restricted our sample to

April 2004 to March 2006, to leverage what is seemingly a natural experiment – the accession immigration inflow shock (see Section 4). Our final sample includes 321,237 native-born individuals and 72,479 immigrants, generating 1,098,854 observations in total. On average, natives (immigrants) are observed 2.82 (2.65) times.

Thanks to its large sample size, the LLMDB permits disaggregation at fine geographical levels, which we leverage in our identification strategy (see Section 4). The more popular Labour Force Survey (LFS) – a rotating panel survey, available since 1992, with about 140,000 respondents every quarter – limits sub-regional immigration analysis because of sample size constraints (and nationality was not recorded prior to 2011).

Importantly, the LLMDB captures annual earnings within the tax year – that is, total earnings including periods of part-time work or unemployment – whereas the LFS reports weekly earnings for a specific week extrapolated to an annual figure that omits part-time or unemployment spells (which are unobserved). As a result, Table 1 shows that the LFS tends to overstate low earnings compared with the LLMDB.[3] Nevertheless, average earnings trends over time are broadly consistent across both datasets (Lemos 2017; Dustman and Fabbri (2005).

Table 1 shows that a similar pattern emerges when comparing the LLMDB to the Annual Survey of Hours and Earnings (ASHE) – an employer-based survey available since 1997, which samples about 1% of UK employees using PAYE (Pay As You Earn) records and covers roughly 180,000 jobs, but where limited information on demographic variables hinders immigration analysis. That is, annual earnings are lower in the LLMDB than in the ASHE, although both datasets exhibit comparable trends in average earnings and across selected earnings percentiles over time (Dickens and McKnight 2008).

Table 1 shows this is again the case when comparing the LLMDB and the WRS – a dataset that recorded wages, hours, occupation and location of A8 nationals entering UK employment from May 2004 to April 2011, but where direct immigration analysis comparisons with natives is not possible as only A8 immigrants are included. That is, annual earnings are lower in the LLMDB than in the WRS (Lemos and Portes 2014).

Table 1 and Figure 1 show, that, according to the LLMDB, and in line with the ASHE and LFS, natives' wages are lower, and less dispersed across the distribution than immigrants'.

[3] The LLMDB contains a disproportionately high share of immigrants as it captures both low paid immigrants, who tend to be younger, as well as working foreign students and undocumented immigrants, who are unrecorded in the LFS. Some low earning or low hour workers in very small firms are excluded from the LLMDB, due to nil national insurance contributions, though they are still included if working in medium and large employers. In contrast, the LFS captures earnings for the self-employed, unrecorded in the LLMDB, though it excludes communal establishments, where many immigrants are likely to reside (Lemos and Portes 2014).

Table 1 also shows that immigrants are younger than natives, are less spread across the country, are less likely to be employed and are more likely to be unemployed.

2.1 Descriptive Analysis

Table 1 shows that, consistent with historic clusters, nearly half of all WRS nationals flowed into London, the Southeast and East of England (compare WRS with LLMDB and LFS figures in Table 1). This points to the presence of network pull effects. That is, historic immigration areas are most attractive to new immigrants, regardless of whether other areas are thriving more. The support offered by existing immigrant communities dissuades newcomers from self-selecting into thriving areas (Altonji and Card 1991; Hunt 1992). Given the large numbers of both new A8 immigrants and low paid workers in London, they likely compete for the same jobs.

However, beyond network pull factors, Figure 2 suggests that London itself was booming at the time. Despite sustained inflows of A8 immigrants, both native workers and existing immigrants – including low paid existing immigrants – continued to thrive in 2005, though that trend shifted in 2006. Lemos and Portes (2014) also report that even with the sustained inflow of immigrants in London, the number of claimants there remained stable. However, they report that wages did grow more slowly in London between 2005 and 2006 (2.7%), than in the rest of the country (4.4%). Indeed, Figure 2 shows that elsewhere in the UK, existing immigrants, especially low paid existing immigrants, were not faring as well, and the obvious explanation here is the ongoing influx of accession immigrants.

Accession immigrants are concentrated in low paid low skilled jobs, in contrast with earlier immigrants. Table 1 shows their sectoral concentration in manufacturing (31%) and distribution, hotels and restaurants (27%); and occupational concentration in elementary roles (46%) and machine operative positions (32%). Given the large numbers of both new A8 immigrants and low paid workers in machine operatives and elementary occupations, once again they likely compete for the same jobs.

However, Figure 2 suggests that the low paid job market was itself booming during this period. Despite sustained inflows of A8 immigrants in low paid low skilled jobs, both native workers and existing immigrants – including low paid existing immigrants – continued to thrive in 2005, though that trend also shifted in 2006. Lemos and Portes (2014) also report that even with the sustained inflow of immigrants into machine operatives, many claimants re-entered the labour market into machine operative jobs, which experienced stronger wage growth (3.8%)

than elementary (2.7%) or other occupations (3.5%) between 2005 and 2006. Indeed, Figure 2 shows that existing immigrants – including low paid existing immigrants – continued to thrive, both in London and beyond, before conditions deteriorated in 2006.

Elementary occupations – often a cushion to language and other labour market barriers (Friedberg 2001; Drinkwater et al. 2009) – were the main entry point for accession migrants. That is because this is where jobs are most accessible to immigrants faced with such barriers. The prevalent wage in such occupations was the minimum wage, and Figure 2 shows a persistent decline in total weeks worked when we track existing immigrants who earned the minimum wage in 2004. An obvious explanation here is that these existing low paid immigrants – mostly minimum wage earners – might have been those most adversely affected by the continuing inflow of A8 immigrants.

We leverage this variation in A8 immigrants' location and job choices across tax-years to ensure identification in our empirical model, as we discuss in detail in Sections 3 and 4.

3. Model Specification

We use a common reduced form equation (Borjas 1999; Card 2001; Dustmann et al. 2005):

$$\Delta N_{it} = \beta^n \Delta M_{it} + \lambda^n \Delta X_{it} + f_i^n + \Delta \varepsilon_{it}^n \quad (1)$$

where N_{it} is, in turn, the employment and unemployment rate, M_{it} is the immigration rate, X_{it} are labour demand and supply shifters, f_i^n is area fixed effects and ε_{it}^n is the error term in local authority district $i = 1,\ldots,409$ (ONS 2003) and tax-year $t = 1,\ldots,3$. 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 of individuals who have had at least one week of employment (unemployment) in that district and tax-year, M_{it}^* is the number of A8 immigrants, and P_{it} is the working age population.[4]

We control for area fixed effects by adding local authority district dummies. This removes any permanent differences across districts such that they are equally appealing. In other words, we control for district specific factors, such as more diversity, more amenities, more housing, etc., that may make those districts more appealing to immigrants, natives or both. This helps to isolate the effect of district specific factors from the effect of the A8 shock on the employment

[4] One contribution of our paper is the use of immigrant stock, in contrast to immigration inflows, often used in the literature. If outflows are non-random (e.g. seasonal exits in agriculture), this can introduce omitted variable bias (Gilping at al. 2006; Lemos and Portes 2014). Another contribution is disaggregation at the district level, unlike the usual regional level in the literature, where 33-district London is treated as one regional market, where 17% of WRS immigrants were unevenly distributed (Lemos and Portes 2014).

rate. Equation 1 is estimated in first-difference, so time fixed effects are differenced out. This helps to isolate the effect of other macro shocks, such as seasonal shocks, national and international shocks, etc., from the effect of the A8 shock on the employment rate. The estimate of this baseline specification is a statistically insignificant -0.06 (see column 1 panel 1 Table 2).

We next control for time fixed effect trends by adding time dummies to the first-differenced model. This helps to isolate the effect of macroeconomic trends, such as output growth, inflation, etc., from the effect of the A8 shock on the employment rate. The estimate of this more robust specification is a statistically insignificant -0.07 (see column 2 panel 1 Table 2).

We then control for demand and supply shifters. This helps to isolate the effect of demand and supply shocks from the effect of the A8 shock on the employment rate. Our controls include are age, sex, the proportion of the total population who are immigrants from A8 and from non-A8 countries. This helps to account for lower employment districts, where the shares of women, youngsters, minorities and other immigrants might be higher. The estimate, of what is our preferred specification, is a statistically insignificant -0.07 (see column 3 panel 1 Table 2).

Controlling for pre-A8 existing immigration is crucial in ensuring identification of any employment effects. That is because the presence of existing immigrants in a district might have an impact on the employment rate over and above the impact of the new A8 immigrants. This helps to isolate the net employment effect of the A8 immigration shock, which might otherwise be entangled with the effect of existing immigration. Therefore, this accounts for potential omitted variable bias, especially as A8 immigrants initially concentrated in districts historically associated with immigration (see Section 2.1). Similarly, controlling for new non-A8 immigration is also crucial in ensuring identification of any employment effects. Again, this is because the arrival of other new non-A8 immigrants in a district might have an impact on the employment rate over and above the impact of the new A8 immigrants. This helps to isolate the net effect of the A8 immigration shock, which might otherwise be entangled with the effect of existing immigration on employment.

4. Model Identification

We next control for two additional variables to ensure identification of β^n in Equation 1, which might otherwise – i.e., in the presence of a non-zero correlation between the immigration rate and the error term – be biased. Firstly, this ensues if variables driving both, the immigration and employment rates, were omitted, such as natives' mobility and immigrants' self-selection. Omitted variable bias thus stems from unobservable factors that cannot be directly controlled

for. Secondly, this occurs if variables driving the joint determination of immigration and employment rates were omitted, such as, again, natives' mobility and immigrants' self-selection. Simultaneity bias thus stems from both immigrants and natives making simultaneous decisions regarding employment opportunities. Thirdly, this occurs if measurement error is non-random: this is unlikely in our dataset, and, hence, a lesser concern here.

In Section 3, by including demand and supply shifters and fixed effects, we have already controlled for omitted variables to a certain degree. Controlling for both district and time fixed effects helps to attenuate bias stemming from natives' mobility and immigrants' self-selection. Moreover, accounting for time fixed effect trends further enhances any potential bias attenuation. Finally, including demand and supply shifters captures factors that may motivate immigrants – to move into certain districts – and natives – to move out of these districts – thus further attenuating both self-selection and mobility bias (Borjas 2006).

Any residual omitted variable bias stemming from natives' mobility now depends on the correlation between the immigrants' inflow and natives' netflow, which varies with the context of the inflow (Card and DiNardo 2000; Borjas 2003). If we were able to control for what the employment rate would have been – had natives not moved away from these districts – we would not only correct for natives' mobility bias but also account for labour market conditions prior the accession inflow (Borjas 1999 2006). This would help to isolate the effect of natives relocating from the effect of the A8 shock on the employment rate.

To approximate the counterfactual of how mobile natives would have been in the absence of the immigration inflow, we introduce two alternative variables. The first is lagged working age population growth, which not only captures natives' mobility, but also ensures that the variation in ΔM_{it} is primarily driven by the immigration inflow (Borjas 2003 2006; Dustmann et al. 2005). The second is the natives' netflow rate, defined as $\Delta A_{it} = \frac{\Delta A^*_{it}}{P_{it}}$, where ΔA^*_{it} is the number of natives who have moved away from that district that tax-year. Controlling for the natives' netflow rate is a contribution of our paper, as data on natives' mobility is typically scarce (Lemos and Portes 2014). Both variables yield robust estimates. ΔA_{it} was our preferred variable and the estimate arising from this very stringent and robust specification is a statistically insignificant -0.01 (see row 1 panel 3 Table 2).[5] This is now a smaller, though,

[5] We used alternative dynamics – lagged immigration rate and employment (unemployment) rate, following Gilpin et al. (2006) and Lemos and Portes (2014) – and obtained robust estimates. We also included lagged controls to account for lower employment in districts with historically higher shares of women, youngsters, minorities, or other immigrants, which also helps mitigate serial correlation. We also obtained robust estimates from other robustness checks: alternative definitions of the employment (unemployment) rate (using thresholds higher than

crucially, still statistically insignificant effect. This suggests that any residual non-zero correlation between the error term and the immigration rate is likely weak, implying that any endogeneity bias is not too stern (also see Section 4.3). That is, the remaining variation in the employment rate arguably stems from the A8 immigration shock – and this ensures identification of β^n.

4.1 Endogeneity

One key point in Section 4 is that identification critically depends on accounting for endogeneity stemming from natives' mobility. If natives respond to the immigration inflow by moving away from (or refraining to move into) a district – thus avoiding labour market competition with immigrants through increased mobility – any adverse employment effects in that district may be lessened. Failing to control for the counterfactual – what employment would have been in that district had natives not relocated – compromises identification. Put differently, identification of any adverse employment effects hinges not only on the degree of native mobility across districts in response to immigration inflows but also on the fit of the proxy used as a measure of such a mobility. If our measure of native netflow rate in Section 4 is a close proxy for natives' mobility, then any residual bias in our estimates in Section 3 is likely corrected.

Another key point in Section 4 is that identification also critically depends on accounting for endogeneity stemming from immigrants' self-selection. If immigrants respond to labour demand by self-selecting into a thriving district, any adverse employment effects in that district may again be lessened. Put differently, identification of any adverse employment effects hinges on immigrants ability to self-select into thriving districts. If the nature of the A8 immigration shock is such that this ability to self-select is curtailed, then any bias is mitigated.

The two above identification issues would not arise if districts functioned as closed local labour markets – where both natives and immigrants were bound to competition – and if immigrants were randomly assigned across districts (or were allocated or pulled into districts for reasons other than employment opportunities) – instead of self-selecting into thriving districts. Although areas, sectors, occupations and job types in the UK were not fully closed labour markets where A8 immigrants were randomly assigned to, the large, fast and

one week), aggregation at the county level, a longer sample period (2002-2006), specifications in levels, alternative thresholds for tracking minimum wage workers (see Section 4.6) and alternative weights (working age population, total population, and their April 2004 time-invariant versions). Weighting accounts for district aggregation and the resulting heteroskedasticity. Serial correlation across and within districts is also accounted for.

concentrated inflow of accession immigrants is akin to a natural experiment that approximates an exogenous shock (Chiswick 1991 1992 and 1993; Card 1990 2007; Altonji and Card 1991; Dustman and Glitz 2005). Put differently, we argue that our treatment groups were exposed to the treatment – the shock was such that natives were unable to immediately exit high A8 immigration districts – while our control groups remained relatively unaffected – low A8 immigration districts offer a credible counterfactual.

As with other akin shocks, such as Cuban migration to Miami and Russian migration to Israel in the 1990s (Card 1990; Friedberg 2001; Hunt 1992; Carrington and Lima 1996), the accession shock was primarily driven by push factors in origin countries, rather than pull factors in the destination country. The UK became a preferred destination because of more restrictive policies elsewhere as well as language and existing networks (Bartel and Koch 1991; Dustmann et al. 2003a; Doyle et al. 2006; Pollard et al. 2008). Within the UK, A8 immigrants' choices of districts, sectors, occupations and job types were shaped by these existing networks and labour market barriers – especially language limitations (LaLonde and Topel 1991; Card and DiNardo 2000; Friedberg 2001). Highly educated A8 immigrants were channeled into low paid low skilled jobs in London and surrounding areas (see Section 2.1), unable to self-select into higher paid jobs across the UK due to these barriers. A8 immigrants were only eligible for certain social security benefits two years after registration with the WRS, thus they had a strong incentive to enter the labour market quickly, frequently experiencing occupational downgrading, not an uncommon phenomenon (Card and DiNardo 2000; Friedberg 2001).

Leveraging such an immigration shock to outwit the endogeneity issues discussed above is a contribution of our paper. Beyond the additional controls introduced in Section 4, the very nature of the A8 immigration helps to mitigate the challenging identification issues discussed above. Not only did the accession immigration inflow stem from political decisions, but it was also unexpectedly large and fast (Dustmann et al. 2003). Importantly, it was unexpectedly larger and faster – as well as unexpectedly more concentrated in terms of districts, sectors, occupations and types of jobs – than other such shocks. Consequently, any responses from both, natives – by moving away from – and immigrants – by self-selecting into – particular districts, would have occurred with a sufficient delay to allow identification of employment effects. For instance, nearly a half of WRS immigrants were employed withing 30 days of arrival in the UK (Lemos and Portes 2014).

As the accession inflow was significantly large, fast and heavily concentrated into low paid low skilled jobs and occupations in particular districts, concerns about immigrants' self-

selection bias are lessened. That is, immigrants were effectively pulled – not self-selected – into particular areas and jobs for reasons other than employment opportunities. Furthermore, these low paid low skilled jobs are a relatively closed market that curtail natives' mobility in the short run – given temporal and financial costs of retraining (Friedberg 2001; Borjas 2003). Thus, although endogeneity from natives' mobility and immigrants' self-selection are typically challenging, the very nature of the A8 immigration shock mitigates these concerns here.

4.2 Sample Stratification

In Section 4, we leveraged variation in district choices across tax-years to identify β^n in Equation 1. We treated several districts as distinct local labour markets within the UK characterized by differing degrees of natives' mobility and immigrants' self-selection. That is, we leveraged the fact that some districts function more like closed labour markets than others.

We now stratify our sample to focus on what is a fairly closed – yet severely underexplored – labour market, for which there is no evidence on employment or unemployment immigration effects, for either the UK or other countries (Borjas 2003; Dustmann at al 2005; Manacorda et al. 2012; Ottaviano and Peri 2012; MAC 2012; Foged and Peri 2016).[6] We turn to analyzing the employment effect for low paid low skilled previously existing immigrants, an important and novel contribution of this paper, previously suggested in the literature as a fruitful avenue for research (Lemos and Portes 2014).

We argue that existing immigrants are those most truly constrained – those most exposed and vulnerable to labor market competition from the new A8 immigrants, with the fewest outside options and trapped beneath a confining glass ceiling. While earlier immigrants may inherently exhibit greater mobility than the native population (Clark and Drinkwater 2008; Kritz et al. 2011), the mobility patterns of low paid low skilled existing immigrants differ markedly from those of other workers (Borjas 2003 2006; Cadena 2013). Their reliance on support networks (Altonji and Card 1991; Hunt 1992; Card 2001) and their clustering within ethnic enclaves perpetuates language and skill upgrading barriers. Retraining and relocation is even less feasible for them than for natives (see Section 4.1), and thus, for them, mobility responses would be, if anything, further delayed.

Therefore, low paid low skilled existing immigrants' mobility – to avoid direct competition with A8 immigrants – is even less of a concern than for natives. Put differently, their already

[6] An exception is Foged and Peri (2016) for Canada. However, they examine a specific case of refugee inflow, which differs, in meaningful aspects, from economic immigration inflows, such as the A8 inflow.

restricted mobility – owing to support network dependence and retraining temporal and financial costs – would likely be further curtailed by an unexpectedly fast and large immigration shock such as the A8 shock. Importantly, unless simultaneity bias manifests similarly in the full and stratified samples, focusing on the latter offers a way to assess its extent.

In summary, we argue that the UK pre-A8 immigration constitutes a dual labour market: natives and existing immigrants. By estimating Equation 1 using the full sample, our assumption that all A8 immigrants compete with all natives in each district was unrealistic: the vast majority of A8 immigrants did not initially compete with high skilled natives. This unrealistic assumption, common in the literature, stems from data limitations that hinder the study of the low paid at finer aggregation levels. Even when relaxing this assumption, though, Lemos and Portes (2014) still found little evidence of adverse employment effects for the low paid.[7]

We relax this assumption further by assuming that A8 immigrants compete with neither high nor low skilled natives. In effect, the assumption underlying our sample stratification here is that A8 immigrants do not compete with natives at all: they compete with low paid low skilled previously existing immigrants. That is, we treat A8 immigrants and previously existing immigrants as labour substitutes. Accordingly, we restrict our sample to previously existing immigrants only, as our earlier full sample estimates may have diluted any potential adverse employment effects (Altonji and Card 1991). Ascertaining which workers A8 immigrants might displace is vital to identifying any immigration employment effects (Card 2001; Borjas 1999). Stratification for the low paid is promising because this is where more direct competition between new A8 and existing immigrants takes place (Card 2001; Friedberg 2001) – and where simultaneity bias is less of a concern.

4.3 Employment Effects

We start by showing estimates for the three specifications discussed in Section 4 for previously existing immigrants on row 2 panel 1 of Table 2: 0.08, 0.08 and 0.03. The estimate from the most complete and robust specification on panel 3 remains statistically insignificant, confirming that, just as for natives, no employment effect is observed for existing immigrants.

[7] Lemos and Portes (2014) used elementary occupations to stratify for the low paid. While other alternative skill definitions have been used, actual pay might be a more accurate stratification for ascertaining labour substitution (also see Section 4.6). For example, as the extent and quality of education and experience varies across countries, immigrants and natives in the same stratum might not be labour substitutes (Manacorda et al. 2006). Similarly, occupation might not reflect accurate stratification, as immigrants often experience skill downgrading due to barriers such as language.

For completeness and comparability – since most estimates in the literature pertain to all workers (comprising of natives and existing immigrants) – row 3 panel 1 of Table 2 shows estimates for the combined sample.[8] This confirms the main finding in the literature of no adverse employment effect. Although this runs counter to the theoretical and political debate, this result aligns with the current empirical evidence, where estimates range from -0.02 to +0.02 and are typically statistically insignificant (Card 2001; Dustmann et al. 2005; Dustmann et al. 2013b; Manacorda et al. 2012; Longhi et al. 2010 2006; MAC 2012). Thus, the results for the full sample – even when stratified into natives and existing immigrants – do not alter the principal finding in the immigration literature of little evidence of adverse employment effects.

To explore this further, we re-estimate Equation 1, focusing on the low paid (those earning below £4000 in the tax-year),[9] young low paid (those between 18 and 24 years of age) and low paid female – workers more likely to be in direct competition with A8 immigrants.

Panels 2 and 3 of Table 2 show a statistically significant estimate of -0.05 for our most complete and robust specification (column 3) for both low paid and young low paid immigrants. In contrast, panel 4 shows statistically insignificant estimates for females. This suggests that a one percentage point increase in the immigration rate reduces employment among young low paid immigrants by 0.05 percentage points on average. This is double the effect typically reported in the literature and – crucially – statistically significant, unlike many prior estimates.

Therefore, the evidence for the low paid sample – when stratified into natives and existing immigrants – suggests that, although the principal finding of little adverse employment effects for the natives is maintained, novel evidence emerges of a negative employment effect for existing immigrants. That is, while the A8 immigration shock did not affect the employment of low paid natives, it adversely affected the employment of young low paid existing immigrants. The most plausible explanation is that the new A8 immigrant labour is substitute for young low paid existing immigrants, and since mobility is low for the latter, due to barriers and relocation costs, they face competition in a more closed market (see Section 4.2).

The results for low paid and young low paid immigrants are robust, suggesting little evidence of potential endogeneity – if anything, any endogeneity is not stern enough to

[8] Due to the linearity and additivity of our estimation method, and our choice of uniform set of regressors across models, the combined estimate is a weighted average of the native and immigrant estimates – that is, the total employment effect (row 3) is decomposed into the effect for natives and immigrants (sum of rows 1 and 2).

[9] The UK Low Pay Commission defines low pay as earnings below two-thirds of the median, which in 2004, was around £14,000 (LPC 2022). A full time minimum wage worker then earned around £10,000, and a part time worker, £5,000. We use £4,000 to capture those earning below the minimum wage or working fewer hours. We also experiment with the alternative definition of £4,500 to £6,500 (Section 4.7).

significantly bias our estimates. This implies that either existing immigrants did not respond through relocation, or that such relocation was too limited to materially affect the results. We contend that the nature of the A8 immigration shock, along with stratification for low paid existing immigrants, further mitigates concerns of any potential bias.[10]

Our findings align with other research indicating that natives' mobility (which often includes existing immigrants) is relatively limited, even in response to other less exogenous immigration shocks in the UK (Muellbauer and Murphy 1988; Hatton and Tani 2005). As we discussed in Section 4.2, the mobility of existing immigrants is arguably even more limited. Additionally, persistent regional employment disparities in the UK suggest that internal mobility only modestly contributes to local labour market adjustments (Pissarides and McMaster 1990). This mirrors findings for the US of little evidence of natives' mobility in response to immigration shocks (Butcher and Card 1991; Filer 1992; White and Liang 1998; McCormick 1997; Card 2001).[11]

4.4 Unemployment Effects

Next, we re-estimate Equation 1, where ΔN_{it} is now the unemployment rate, as discussed in Section 3. Row 3 panel 5 of Table 2 shows statistically insignificant estimates, indicating no adverse unemployment effects for the full sample. This result aligns with – and adds to – the limited current empirical evidence for the UK, where estimates range from -0.04 to 0.002 and are typically statistically insignificant (Dustmann et al. 2005; Lemos and Portes 2014).[12] It also aligns with results of no employment effects for the full sample in Section 4.1. Rows 1 and 2

[10] Studies employing instrumental variables to correct for endogeneity bias are relatively scarce, particularly for the UK (Hatton and Tani 2005; Saiz 2007; Dustmann et al. 2013a). This is partly due to the difficulty of finding valid and strong instruments, and partly because estimates in the literature are small – and so any associated endogeneity bias is also likely to be small. Although Lemos and Portes (2014) also relied on the exogenous nature of the A8 shock to ensure identification – and confirmed the literature principal finding of small statistically insignificant estimates – they used instrumental variables to further correct for any potential endogeneity bias still remaining. They found little evidence of bias correction and concluded that any such bias was, if anything, small.

[11] Beyond the modest employment, unemployment and wage effects typically found in the literature, other channels for labour market adjustment to immigration shocks include changes in both labour force participation and institutions (unions, minimum wage regulations, employment protection, etc.) (see Section 4.7). One further channel is factor equalisation, whereby internal flows of goods, capital and labour (e.g. native mobility) help equalise markets across regions or skills. However, this mechanism lacks supporting empirical evidence, given persistent regional disparities in most countries. Another further channel is changes in industry and output mix, whereby firms shift towards more labour intensive technologies, sectors or products. While this may be plausible in small open economies such as the UK, again this mechanism lacks supporting empirical evidence. Although neither mechanism seems particularly convincing given how large, fast and concentrated the A8 inflow was, further evidence on both fronts remains a fruitful avenue for research (Lemos and Portes 2014).

[12] The use of a broader definition of the unemployment rate has been suggested in the literature as a fruitful avenue for research, as, due to data limitations, earlier definitions might have instead captured benefit eligibility effects, since only individuals eligible for unemployment benefits were included (Lemos and Portes 2014).

further show statistically insignificant estimates when the sample is again stratified into natives and existing immigrants. Thus, these results do not alter the principal finding in the immigration literature of little evidence of adverse unemployment effects. That is, there is no evidence that the A8 immigration shock hindered the chances of the unemployed exiting unemployment.

To explore this further, we re-estimate Equation 1 focusing on the long term unemployed (those continuously unemployed for 26 weeks or more): workers more vulnerable to heightened labour market competition from A8 immigrants. These individuals already face persistent barriers to re-employment and so have limited prospects of exiting unemployment even before the onset of any additional competition. Further competition from highly educated, young, and childless A8 immigrants would likely exacerbate this disadvantage.

We turn to analyzing the unemployment effect for long term unemployed existing immigrants, an important and novel contribution of this paper. Row 2 panel 6 Table 2 shows statistically significant and robust estimates for long term unemployed existing immigrants, with smaller magnitudes in the more complete specifications: 0.15, 0.12 and 0.11. In contrast, rows 1 and 3 show statistically insignificant estimates for natives and for the combined sample. This suggests that a one percentage point increase in the immigration rate raises unemployment among long term unemployed existing immigrants by 0.11 percentage points on average. This is tenfold the effect typically reported in the literature and – crucially – statistically significant, unlike many prior estimates.

Therefore, the evidence for the long term unemployed – when stratified into natives and existing immigrants – suggests that, although the principal finding in the literature of little adverse unemployment effects for the natives is maintained, novel evidence emerges of a negative unemployment effect for existing immigrants. That is, while the A8 immigration shock did not affect the unemployment of long term unemployed natives, it adversely affected the unemployment of long term unemployed existing immigrants. The likely explanation is that the new A8 immigrant labour intensifies competition, exacerbating even further the disadvantage faced by long term unemployed existing immigrants. Already burdened by persistent barriers to re-employment, this group is particularly vulnerable to further reduced prospects of exiting unemployment.

The results for long term existing immigrants are robust, suggesting little evidence of potential endogeneity. As in Section 4.3, we contend that the nature of the A8 immigration shock, along with stratification for low paid existing immigrants, further mitigates concerns of any potential bias.

4.5 Average Employment Weeks Effects

Finally, we re-estimate Equation 1, where ΔN_{it} is now the share of total employed weeks for natives and existing immigrants relative to the total number of employed weeks across all workers. Panel 7 of Table 2 shows statistically insignificant estimates, indicating no adverse effects on employed weeks for natives, existing immigrants or the combined sample. This result aligns with findings of no employment or unemployment effects for natives or existing immigrants for the full sample in Sections 4.1 and 4.4. It also aligns with the current empirical evidence, where employment effect estimates are typically statistically insignificant (Dustmann et al. 2005; Lemos and Portes 2014), but they are not directly comparable to any current study – these are novel results – as no estimates for the effect of immigration on the share of total employed weeks are available in the literature (Foged and Peri 2016; Dustmann et al. 2017).

To explore this further, we re-estimate Equation 1, focusing on the low paid, as in Section 4.3. In contrast with the full sample results in panel 7, panel 8 of Table 2 shows that the estimates for the low paid are mostly statistically significant. Column 3 shows that while the estimate for natives becomes statistically insignificant, the estimates for existing immigrants and for the combined sample remain statistically significant and robust, respectively -0.01 and -0.02. This suggests that a one percentage point increase in the immigration rate reduces the share of total employed weeks for existing immigrants (combined sample) by 0.01 (0.02) percentage points on average. Given the statistically insignificant estimate for natives, it is likely that the estimate for the combined group is driven by the effect for immigrants.

Therefore, the evidence for the low paid sample – when stratified into natives and existing immigrants – suggests that, although the principal finding in the literature of little adverse employment effects for the natives is maintained, once again, novel evidence emerges of a negative employment effect for existing immigrants, in line with results in Section 4.3. That is, while the A8 immigration shock did not affect the share of employed weeks for low paid natives, it did have an adverse effect for low paid existing immigrants. Once again, the most plausible explanation is that new A8 immigrants are substitute for low paid existing immigrants, even when examining employment adjustment along a different margin. That is, although natives did not, low paid existing immigrants did experience a change in the number of employed weeks as a result of A8 immigration competition. The results for the low paid are robust, suggesting little evidence of potential endogeneity and again mitigating concerns of any potential bias.

4.6 Tracked Minimum Wage Workers

We have above reported novel evidence of adverse employment and unemployment effects, as well as adverse effects on the share of employed weeks, for low paid existing immigrants and long term unemployed existing immigrants, themselves predominantly low paid. We argued that low paid existing immigrants are the most exposed and vulnerable to labour market competition from new A8 immigrants and treated these two groups as direct labour substitutes.

Building on this, we now argue that the new A8 immigrants are even closer labour substitutes to the existing immigrants who earned the minimum wage the year preceding the shock: our descriptive analysis in Section 2.1 shows that A8 immigrants predominantly earned the minimum wage. As argued in Section 4.3, ascertaining which workers the A8 immigrants might have affected is vital to identifying any employment or unemployment effect. Tracking workers who earned the minimum wage in 2003 is a further step in the search for any such affected workers. That is, another way to study the effect of the A8 immigration shock on UK employment and unemployment is to track these most at risk workers. Stratifying the sample to follow the cohort of workers earning the minimum wage in 2003 is a novel approach – distinct from other strategies in the literature, such as stratifying by district, county, region, education, occupation or wage distribution percentile – and a contribution of our paper, previously suggested in the literature as a fruitful avenue for research (Lemos and Portes 2014).

Accordingly, we restrict our sample to the cohort of workers earning the minimum wage the year preceding the shock and follow them for the three subsequent years to estimate the effect of the immigration shock on their employment, unemployment and average hours worked. Column 4 of Table 2 shows the results from re-estimating Equation 1 for this cohort, once again stratified into previously existing immigrants, natives and the combined group. The employment effect estimate for our most complete and robust specification for low paid immigrants is a statistically significant -0.07 (compare with -0.05). This estimate falls to -0.03 for young low paid immigrants (compare with -0.05). In contrast, the estimate for the combined natives and immigrants group is now a significant -0.04. Given the statistically insignificant estimate for natives, it is likely that the estimate for the combined group is driven by the effect for immigrants. Notably, the estimate for female immigrants is now also a statistically significant -0.04. The unemployment effect estimate for long term unemployed existing immigrants is a statistically significant 0.05 (compare with 0.11). As highlighted in Sections 4.3 and 4.4, these adverse employment and unemployment effects are much larger than effects typically reported in the literature and – crucially – statistically significant, unlike many prior

estimates. Finally, the employed week effect estimate for low paid existing immigrants is a statistically significant -0.07 (compare with -0.01).

The results strengthen the robustness of our earlier estimates (compare columns 3 and 4 of Table 2). The evidence for the low paid sample, when stratified into natives and existing immigrants, once again confirms that although the principal finding in the literature – of little adverse employment and unemployment effects for the natives – is maintained, the novel evidence of adverse effects for existing immigrants, including the young and female, as well as the long term unemployed, is robust and compelling.

4.7 Wage Effects

The employment and unemployment effect estimates above highlight two dimensions of the same phenomenon: the most vulnerable – low paid and long-term unemployed existing immigrants, themselves predominantly low paid – were disproportionately adversely affected by the inflow of A8 immigrants to the UK. No statistically significant evidence emerged of adverse impacts on native workers.

One potential explanation is that the labour market adjusted to the A8 immigration shock not solely through changes in employment and unemployment of existing low paid immigrants, but also through changes in wages of natives or existing immigrants. To explore this further, we now consider wage effects as another potential margin of adjustment to the shock.

We re-estimate Equation 1, where ΔN_{it} is, in turn, the 5th and 10th to 90th percentiles as well as the average of the log real total pay distribution. This approach enables us to uncover wage effects, if any, for lower paid workers that might otherwise be obscured by the average wage effect. Column 1 of Table 3 shows that all estimates across the entire pay distribution are statistically insignificant and therefore indicate that the A8 immigration shock had no effect on the pay of either natives or earlier immigrants for the full sample. This result aligns with current empirical evidence, where estimates are typically statistically insignificant, though some statistically significant evidence of negative (positive) effects for the low (higher) paid, typically ranging from -2% to 10%, has also been reported (Dustmann et al. 2005 2007 2013a; MAC 2012; Manacorda et al. 2012; Lemos and Portes 2014; Nickell and Saleheen 2015; Wadsworth 2015; Giuntella et al. 2015; Portes and Forte 2017; Gosh and Dickey 2024).

Given that A8 immigrants are located around the 5th and 10th percentiles of the pay distribution, as we discussed in Section 2.1 (also see Lemos and Portes 2014), this is where we anticipate finding more adverse (or less favourable) wage effects. Indeed, Table 1 shows that

the minimum wage is binding between the 5th and 10th percentiles. Accordingly, as in Section 4.6, we restrict our sample to the most at risk: the cohort of workers earning the minimum wage in 2003. Column 2 of Table 3 shows the results from re-estimating Equation 1 for this tracked cohort, once again stratified into natives and existing immigrants. Notably, estimates for our most complete and robust specification show no evidence of negative wage effects for existing immigrants, even at lower percentiles. There is some evidence of positive wage effects for natives at higher deciles – between the 40th and 80th – and for the average, ranging from 0.08 to 0.15. For instance, a one percentage point increase in the immigration rate raises native wages by 11% on average. Given the statistically insignificant average wages estimate for immigrants, it is likely that the average wages estimate for the combined group is driven by the effect for natives. This is within the range of effects typically reported in the literature and – crucially – statistically significant, unlike many prior estimates.

Once again, the results on wage effects for both the full sample and the tracked cohort of low paid minimum wage workers suggest little evidence of potential endogeneity. As in Section 4.3, we contend that the nature of the A8 immigration shock, along with stratification for natives and existing immigrants, further mitigates concerns of any potential bias.

Thus, these results do not alter the principal finding in the immigration literature of little evidence of adverse wage effects. That is, there is no evidence that the A8 immigration shock depressed wages of natives or existing immigrants. However, this result warrants cautious interpretation, as concurrent increases in the minimum wage during that period might have offset any potential adverse wage effect of the A8 immigration shock among low paid workers. Not only would minimum wage workers be disproportionately cushioned by such protection, but also workers earning just above or below the minimum wage would also benefit from minimum wage spillover effects, a phenomenon well documented in the literature (Lemos 2009). In particular, low paid existing immigrants are more likely than natives to earn below the minimum wage, possibly due to weaker law enforcement mechanisms in their case. Given their overrepresentation at the bottom of the pay distribution, immigrants comprise a relatively larger share of workers who would have been cushioned by the protective effects of a rising minimum wage. More broadly, wage rigidity limits employers' ability to reduce nominal pay, and if inflation rates are modest, such as in that period, the scope of real wage erosion is reduced. Employers under cost pressure are therefore more likely to reduce headcount than wages, to lower the overall wage bill. While one potential response might have been substituting relatively more expensive native workers with cheaper new A8 immigrant workers, our findings

provide no evidence to support this substitution mechanism.

5. Conclusion

The expansion of the EU triggered a sizeable wave of immigration into the UK. Exploiting rich but underused high quality individual level data, we describe and evaluate the impact of this inflow on the UK labour market. Consistent with other results in the literature, our estimates once again confirm the finding of little evidence that the inflow of accession immigrants led to a substantial fall in wages or employment, or a rise in unemployment of natives in the UK between 2004 and 2006. We found no adverse wage effects for natives at any point along the distribution. Neither did we find any adverse employment or unemployment effects for natives.

This lack of evidence of adverse effects is striking, given the accession immigration shock unprecedented scale and pace. This shock stemmed from political decisions and was unexpectedly larger and faster – as well as unexpectedly more concentrated into areas and occupations – than anticipated. Akin to a natural experiment that approximates an unanticipated exogenous shock, there was limited scope for pre-emptive labour market adjustments that might otherwise have lessened any adverse impact of the shock. So, the main focus of the literature has been to examine just how the labour market adjusted to such a shock.

Although our results once again confirm the principal finding in the literature of little adverse effect for natives, we uncover, for the first time, novel evidence of adverse employment and unemployment effects for low paid existing immigrants as a result of the accession immigration inflow. This is more severe for low paid immigrants and young low paid immigrants as well as for long term unemployed immigrants.

Our estimates are in line with our descriptive analysis and are robust to a number of alternative specifications and stratifications of the labour market for different sub-samples of workers. Our estimates proved robust across a comprehensive set of specification checks addressing the two primary identification challenges highlighted in the literature: natives' mobility and immigrants' self-selection. We discussed extensively that neither source of endogeneity appeared strong enough to sternly bias our results. First, we controlled for omitted variables, to some a certain degree, by including fixed effects and demand and supply shifters. Our estimates were significant and robust for the low paid. Second, we further controlled explicitly for natives' netflow rate, to account for the counterfactual of how mobile would natives have been in the absence of the immigration inflow. Our estimates remained significant

and robust for the low paid – if anything, any bias correction was minimal. Third, we leveraged the very nature of the shock to argue that immigrants' self-selection was, if anything, minimal, as immigrants were effectively pulled – not self-selected – into particular areas and occupations for reasons other than employment opportunities. That is, the nature of the shock was such that the ability to self-select into thriving districts was severely curtailed, and hence, any bias was mitigated, and our estimates remained robust. Fourth, we controlled for stratified labour markets in various dimensions to permit alternative substitutability assumptions between new immigrant workers and current workers. In particular, we stratified our sample to low paid jobs – where labour competition with the new A8 immigrants realistically occurs – which represent a relatively closed market that curtail natives' mobility in the short run; and into which immigrants were channelled as a result of barriers that curtailed immigrants' self-selection elsewhere. Our estimates remained significant and robust for the low paid. Fifth, we further stratified our sample to focus on what is a fairly closed market, low paid low skilled previously existing immigrants – workers who are most exposed and vulnerable to labour market competition from the new A8 immigrants. Our estimates remained significant and robust for the low paid existing immigrants. Sixth, as the new immigrants heavily concentrated on minimum wage jobs, we restricted our analysis to a tracked cohort of workers who earned the minimum wage the year preceding the shock, who are likely to be even more direct labour substitutes, and hence, most at risk. Our estimates remained significant and were even more robust for the low paid. Robustness in the estimates across alternative stratification is reassuring because, unless simultaneity bias manifests similarly across different samples, this robustness offer further indication that any remaining bias is minimal.

In summary, our estimates are statistically significant and robust across a broad set of specifications, sub-samples and estimation strategies – and are not sensitive to the underlying assumptions of each model. Notably, we find no evidence that endogeneity bias – through immigrants' self-selection or natives' mobility – drives the results.

This novel evidence is an important contribution to the literature and policymaking. Firstly, this new evidence helps to reconcile theory predictions of adverse effects with previous empirical results showing no such adverse effects – without invalidating previous results. That is, this is a fresh insight on the role played by previously existing immigrants on the adjustment of the UK labour market to such a large, fast and concentrated immigration inflow that was followed by so little overall adverse effects for natives. Secondly, this new evidence adds to the very limited evidence on immigration effects in the UK, especially on effects of the 2004

EU enlargement, and thus helps to inform policymaking on ongoing immigration issues affecting the labour market. Although the finding of adverse effects affecting previously existing low paid immigrants is, here, firstly uncovered for the UK, this evidence is likely to extend to other countries, and this offers a promising avenue for future research.

To conclude, our novel findings represent an important contribution to the literature by applying a thorough and comprehensive empirical estimation approach to a rich but underused dataset to study an immigration inflow that is akin to an exogeneous shock, helping to outwit identification issues central to the debate in the literature.

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Table 1 - DESCRIPTIVE STATISTICS

VARIABLES	LLMDB		WRS	JSA	ASHE		LFS	
	April 2004 - March 2006		May 2004 - May 2006	May 2004 - May 2006	May 2004 - May 2006		April 2004 - June 2006	
	natives	immigrants	migrants	claimants	workers		UK born	Overseas born
I - POPULATION VARIABLES - % of those who are:								
Aged:								
under 16 years old	0.00	0.00	0.00	-	-	-	0.21	0.08
16 to 24 years old	0.13	0.08	0.37	0.30	-	-	0.12	0.11
25 to 34 years old	0.21	0.34	0.45	0.24	-	-	0.12	0.24
35 to 64 years old	0.65	0.58	0.18	0.45	-	-	0.40	0.44
over 64 years old	0.00	0.00	0.00	0.00	-	-	0.16	0.13
Women	50.29	49.52	0.43	0.74	-	-	0.51	0.52
Parents (with dependent children)	-	-	0.06	-	-	-	0.27	0.32
Nationality:								
Polish	-	0.05	0.61	-	-	-	-	0.02
Lithuanian	-	0.01	0.12	-	-	-	-	0.01
Slovakian	-	0.00	0.10	-	-	-	-	0.00
Lativian	-	0.00	0.07	-	-	-	-	0.00
Located in:								
London	0.12	0.45	0.17	0.19	-	-	0.09	0.41
South East	0.13	0.12	0.14	0.08	-	-	0.14	0.13
East of England	0.09	0.07	0.12	0.07	-	-	0.09	0.08
East Midlands	0.07	0.04	0.09	0.06	-	-	0.07	0.05
Yorkshire and the Humber	0.09	0.04	0.08	0.09	-	-	0.09	0.06
West Midlands	0.09	0.06	0.08	0.11	-	-	0.09	0.07
North West	0.12	0.06	0.08	0.12	-	-	0.12	0.07
South West	0.08	0.05	0.08	0.05	-	-	0.09	0.05
Scotland	0.09	0.05	0.08	0.10	-	-	0.09	0.04
Northern Ireland	0.03	0.02	0.04	0.03	-	-	0.03	0.01
Wales	0.05	0.02	0.03	0.05	-	-	0.05	0.02
North East	0.05	0.02	0.01	0.05	-	-	0.05	0.02
II - LABOUR MARKET VARIABLES - % of those who are in:								
Occupations:								
elementary occupations	-	-	0.46	0.35	-	-	0.11	0.14
machine operatives occupations	-	-	0.32	0.10	-	-	0.08	0.07
skilled trades occupations	-	-	0.06	0.11	-	-	0.12	0.08
personal services occupations	-	-	0.04	0.05	-	-	0.08	0.08
unknown occupation	-	-	0.04	0.01	-	-	0.00	0.00
sales and customer service occupations	-	-	0.03	0.13	-	-	0.08	0.07
administrative occupations	-	-	0.03	0.10	-	-	0.13	0.09
professional occupations	-	-	0.01	0.04	-	-	0.12	0.17
managers and senior officials	-	-	0.01	0.04	-	-	0.15	0.15
technical occupations	-	-	0.01	0.06	-	-	0.14	0.15
Sectors:					-	-		
manufacturing	-	-	0.31	-	-	-	0.13	0.11
distribution, hotels & restaurants	-	-	0.27	-	-	-	0.19	0.21
transport & communication	-	-	0.09	-	-	-	0.07	0.08
agriculture and Fishing	-	-	0.08	-	-	-	0.01	0.01
banking, finance & insurance etc	-	-	0.08	-	-	-	0.15	0.19
public admin, educ & health	-	-	0.06	-	-	-	0.28	0.28
construction	-	-	0.04	-	-	-	0.08	0.05
other services	-	-	0.02	-	-	-	0.06	0.06
energy and water	-	-	0.00	-	-	-	0.01	0.01

(continued)

Table 1 - DESCRIPTIVE STATISTICS (continued)

VARIABLES	LLMDB		WRS	JSA	ASHE		LFS	
	April 2004 - April 2006		May 2004 - May 2006	May 2004 - May 2006	May 2004 - May 2006		April 2004 - June 2006	
	natives	immigrants	migrants	claimants	workers		UK born	Overseas born
Part time	-	-	0.08	-	-	-	0.26	0.22
% in work:								
0 weeks in the year	0.28	0.56	-	-	-	-	-	-
1 to 25 weeks in the year	0.17	0.16	-	-	-	-	-	-
26 to 50 weeks in the year	0.12	0.10	-	-	-	-	-	-
51 to 52 weeks in the year	0.43	0.17	-	-	-	-	-	-
Average number of employed weeks in the year	39.91	33.10	-	-	-	-	-	-
Average number of unemployed weeks in the year	25.03	27.08	-	-	-	-	-	-
Average number of jobs in the year	1.45	1.61	-	-	-	-	-	-
Average hours worked	-	-	37.83	-	-	-	36.87	38.37
Employment rate	-	-	-	-	-	-	0.76	0.67
Unemployment rate	-	-	-	-	-	-	0.05	0.07
Week-Employment rate	0.72	0.44	-	-	-	-	-	-
Week-Unemployment rate	0.06	0.03	-	-	-	-	-	-
Average claim duration	-	-	-	31.32	-	-	-	-
Looking for a job in their usual occupation	-	-	-	0.84	-	-	-	-
5th percentile hourly wage distribution	3.82	4.03	4.50	-	4.77	5.16	4.50	4.61
10th percentile hourly wage distribution	4.99	5.60	4.65	-	5.14	5.55	5.26	5.31
20th percentile hourly wage distribution	6.81	7.63	4.85	-	5.99	6.45	6.15	6.22
30th percentile hourly wage distribution	8.26	9.35	4.87	-	6.92	7.45	7.02	7.19
40th percentile hourly wage distribution	9.72	11.23	5.00	-	7.95	8.55	7.98	8.34
50th percentile hourly wage distribution	11.33	13.42	5.05	-	9.18	9.89	9.06	9.59
Average hourly wage distribution	18.93	29.76	5.56	-	12.04	13.09	11.02	11.88
Standard deviation hourly wage distribution	67.36	132.24	2.03	-	-	-	7.16	8.01
Adult minimum wage	4.50	5.05	4.80	-	4.50	5.05	4.50	5.05
number of individuals	321237	72479	-	-	-	-	-	-
number of observations	906620	192234	562830	22016120	21915	23725	201294	21169

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

(1) Variables not available or not defined in a particular dataset are indicated by "-". For example, the employment and unemployment rates are not definied 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) 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. The weighed average adult minimum wage in the period is £4.81.

(5) Week-Employment (Unemployment) "rate" is the proportion of observations where the individual did (did not) work at least one week.

(6) Total pay is reported in the LLMDB as annual pay, which is the variable used in the regressions. For comparison purposes here, real hourly pay was calculated for those in full time employment, working 52 weeks and assuming 37 hours week (this can be thought of as an upper estimate, since it excludes part time workers and workers who did not work a full year).

Table 2 - Employment Effects

Model	(1) Base Model		(2) Unconditional Model		(3) Conditional Model		(4) At MW Tracked Cohort	
	coefficient	s. errors	coefficient	s. errors	coefficient	s. errors	coefficient	s. errors
Employment Rate								
(1) Full Sample								
Natives	**-0.06**	0.03	**-0.07**	0.03	**-0.01**	0.02	**0.00**	0.01
Immigrants	**0.08**	0.03	**0.08**	0.03	**0.03**	0.02	**0.00**	0.01
Natives and Immigrants	**0.02**	0.01	**0.02**	0.01	**0.02**	0.01	**0.00**	0.01
(2) Low Paid								
Natives	**-0.09**	0.05	**-0.05**	0.06	**-0.02**	0.06	**0.01**	0.04
Immigrants	**-0.05**	0.02	**-0.03**	0.02	**-0.05**	0.02	**-0.07**	0.02
Natives and Immigrants	**-0.14**	0.06	**-0.08**	0.06	**-0.07**	0.06	**-0.06**	0.04
(3) Young Low Paid								
Natives	**-0.07**	0.03	**-0.04**	0.03	**0.00**	0.04	**-0.01**	0.02
Immigrants	**-0.04**	0.02	**-0.04**	0.01	**-0.05**	0.01	**-0.03**	0.01
Natives and Immigrants	**-0.11**	0.03	**-0.08**	0.03	**-0.05**	0.04	**-0.04**	0.02
(4) Female Low Paid								
Natives	**0.00**	0.04	**0.00**	0.04	**0.01**	0.04	**0.01**	0.03
Immigrants	**-0.03**	0.02	**-0.02**	0.02	**-0.02**	0.02	**-0.04**	0.01
Natives and Immigrants	**-0.03**	0.04	**-0.03**	0.04	**-0.02**	0.04	**-0.03**	0.03
Unemployment Rate								
(5) Full Sample								
Natives	**0.14**	0.09	**-0.10**	0.09	**-0.03**	0.09	**0.01**	0.04
Immigrants	**0.04**	0.05	**0.02**	0.04	**0.00**	0.04	**-0.02**	0.02
Natives and Immigrants	**0.19**	0.10	**-0.08**	0.09	**-0.04**	0.10	**-0.01**	0.04
(6) Long Term Unemployed								
Natives	**0.12**	0.09	**-0.17**	0.09	**-0.15**	0.09	**-0.06**	0.03
Immigrants	**0.15**	0.04	**0.12**	0.04	**0.11**	0.04	**0.05**	0.02
Natives and Immigrants	**0.27**	0.09	**-0.05**	0.08	**-0.04**	0.09	**-0.01**	0.04
Average Employed Weeks								
(7) Full Sample								
Natives	**-0.32**	0.10	**-0.05**	0.08	**-0.07**	0.09	**-0.01**	0.03
Immigrants	**-0.02**	0.03	**0.02**	0.03	**-0.01**	0.03	**0.00**	0.01
Natives and Immigrants	**-3.01**	4.78	**2.82**	4.55	**1.69**	4.77	**-0.01**	0.03
(8) Low Paid								
Natives	**-0.02**	0.01	**-0.01**	0.01	**-0.01**	0.01	**0.00**	0.00
Immigrants	**0.00**	0.00	**0.00**	0.00	**-0.01**	0.00	**-0.01**	0.00
Natives and Immigrants	**-0.02**	0.01	**-0.02**	0.01	**-0.02**	0.01	**-0.01**	0.00
Local Authority District Fixed Effects	**Yes**		**Yes**		**Yes**		**Yes**	
Time Fixed Effects	**Yes**		**Yes**		**Yes**		**Yes**	
Time Trend Fixed Effects	**No**		**Yes**		**Yes**		**Yes**	
Controls	**No**		**No**		**Yes**		**Yes**	
Sample Size	**1098854**		**1098854**		**1098854**		**70593**	

(a) These are GLS estimates weighted by the sample size used to calculate the dependent variable.

(b) The dependent variable is in turn the employmen rate, the unemployment rate and the share of share of total employed weeks relative to the total number of employed weeks (see Sections 3 and 4).

(c) Time fixed effects are modelled with month dummies, area fixed effects are differenced out, trend fixed effects are modelled with tax-year dummies. See Section 3 for discussion on demand and supply controls.

(e) The interpretation of the coefficient is that a one percentage point increase in the immigration rate changes the depend variable by B percentage points.

(f) Estimates in column 4 are to be compared with estimates in column 3.

(g) Because of the employment effect decomposition we employ, the combined estimate in the last row of each panel is the exact sum of the estimate for the native and existing immigrants.

Table 3 - Wage Effects

Model	(1) Full Sample		(2) At MW Tracked Col	
	coefficient	s. errors	**coefficient**	s. errors
5th percentile				
Natives	**1.22**	1.43	**0.04**	0.28
Immigrants	**-4.78**	6.06	**0.00**	0.37
Natives and Immigrants	**2.37**	1.38	**0.05**	0.21
10th percentile				
Natives	**-0.50**	1.07	**0.19**	0.15
Immigrants	**-2.02**	3.83	**-0.25**	0.31
Natives and Immigrants	**1.02**	0.99	**0.15**	0.12
20th percentile				
Natives	**-0.92**	0.67	**0.07**	0.06
Immigrants	**-0.30**	2.34	**-0.09**	0.20
Natives and Immigrants	**-0.24**	0.59	**0.07**	0.04
30th percentile				
Natives	**-0.55**	0.49	**0.03**	0.06
Immigrants	**-1.75**	1.77	**0.10**	0.18
Natives and Immigrants	**0.01**	0.42	**0.07**	0.03
40th percentile				
Natives	**-0.42**	0.36	**0.08**	0.04
Immigrants	**0.12**	1.34	**-0.20**	0.15
Natives and Immigrants	**0.19**	0.28	**0.04**	0.03
50th percentile				
Natives	**-0.19**	0.26	**0.11**	0.05
Immigrants	**-0.16**	1.11	**-0.05**	0.12
Natives and Immigrants	**0.16**	0.21	**0.08**	0.04
60th percentile				
Natives	**-0.15**	0.21	**0.11**	0.07
Immigrants	**0.48**	1.00	**0.01**	0.11
Natives and Immigrants	**0.16**	0.17	**0.10**	0.05
70th percentile				
Natives	**-0.25**	0.17	**0.15**	0.04
Immigrants	**-0.35**	0.98	**-0.09**	0.11
Natives and Immigrants	**-0.14**	0.17	**0.13**	0.04
80th percentile				
Natives	**-0.04**	0.17	**0.12**	0.03
Immigrants	**-0.97**	0.95	**-0.05**	0.10
Natives and Immigrants	**0.08**	0.15	**0.07**	0.04
90th percentile				
Natives	**0.16**	0.19	**0.03**	0.05
Immigrants	**0.61**	1.33	**-0.10**	0.11
Natives and Immigrants	**0.14**	0.23	**0.05**	0.04
Average				
Natives	**-0.15**	0.34	**0.11**	0.04
Immigrants	**-0.66**	1.31	**-0.06**	0.11
Natives and Immigrants	**0.29**	0.28	**0.09**	0.03
Local Authority District Fixed Effects	**Yes**		**Yes**	
Time Fixed Effects	**Yes**		**Yes**	
Time Fixed Effects Trend	**Yes**		**Yes**	
Controls	**Yes**		**Yes**	
Sample Size	**1098854**		**70593**	

(a) Notes as in Table 2, except that the dependent variable is now various percentiles and the average of the wage distribution, and her

the combined estimate in the last row of each panel is no longer the sum of the estimates for natives and existing immigrants.

Figure 1 - Earnings Distribution for Natives, New and Existing Immigrants

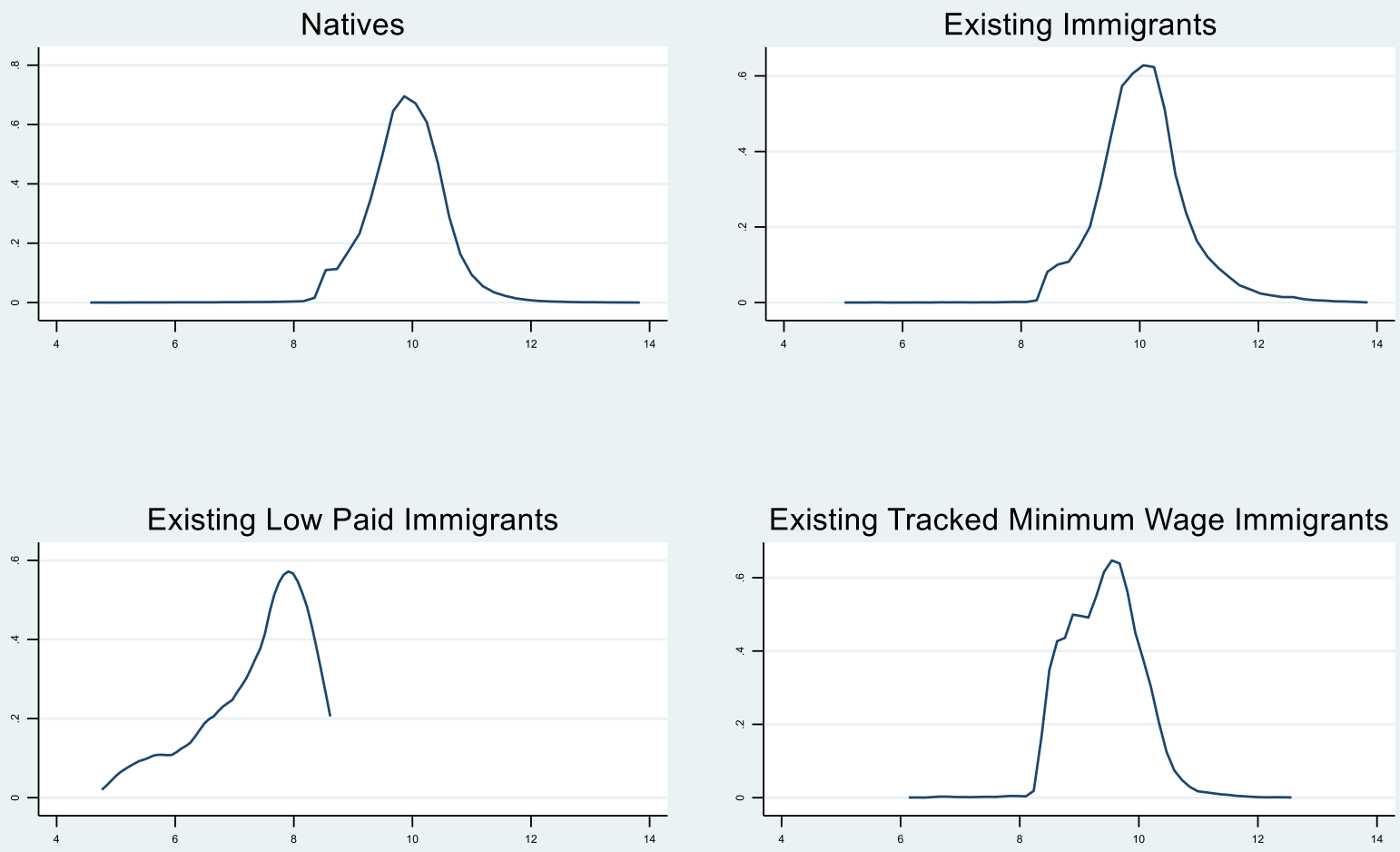
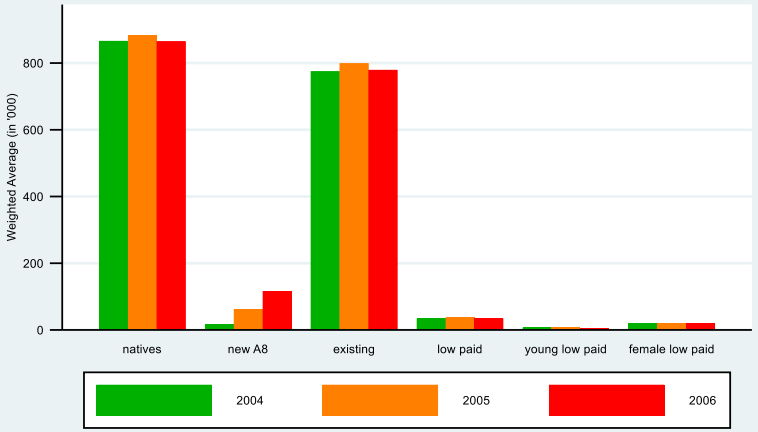


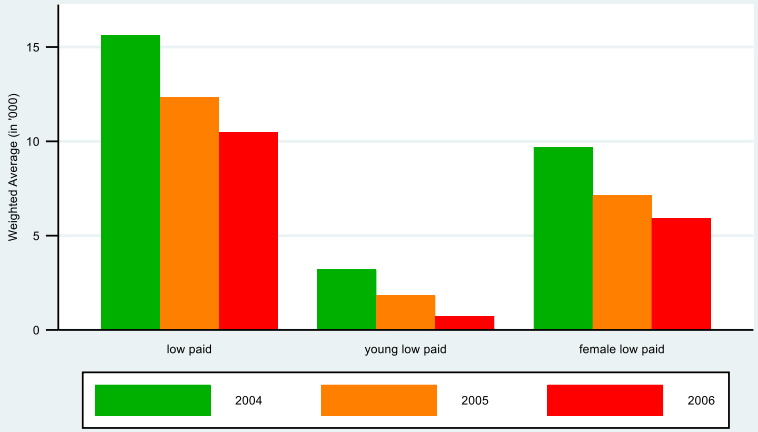
Figure 2 - Total Number of Employed Weeks for Natives, New and Existing Immigrants

Whole Country



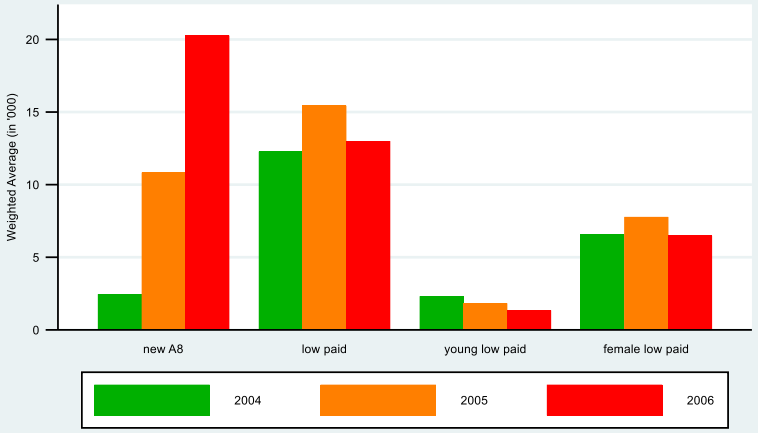
Figures for natives are divided by 10. The last three groups are existing immigrants: low paid, young low paid, female low paid.

Tracked Minimum Wage Existing Immigrants



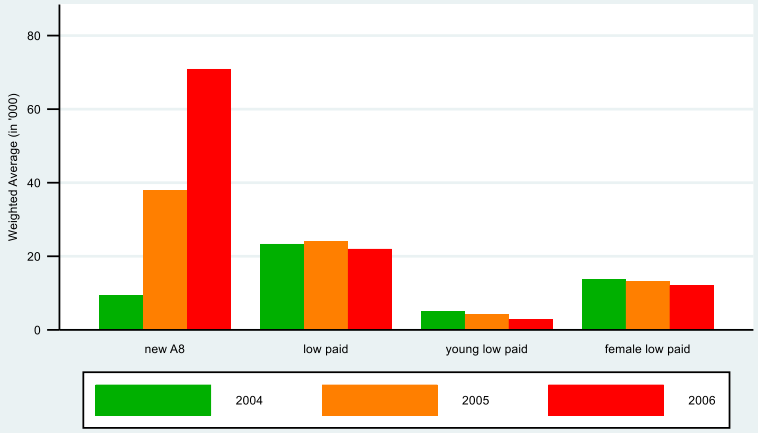
The last three groups are existing immigrants: low paid, young low paid, female low paid.

London



The last three groups are existing immigrants: low paid, young low paid, female low paid.

Rest of the Country



The last three groups are existing immigrants: low paid, young low paid, female low paid.