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Wages After the Wave

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on native pay*

October 2026

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Foreword

One problem with the post-2021 migration debate is that the benefits of the so-called Boriswave were exaggerated by government, while the costs were downplayed or ignored. The economic case was made in terms of totals: more people meant more output, more workers meant more tax receipts, and younger migrants could offset some of the fiscal pressure from an ageing population. But this said little about the wages of people already here.

This gap has always concerned me. Britain has suffered from weak productivity growth and disappointing real wage growth since the 2008 financial crisis, while immigration policy has repeatedly been justified by reference to aggregate GDP and labour shortages.

This paper gets closer to that question than most of the British literature. The authors obtained monthly data on payrolled employment and median pay for UK and non-UK nationals across 147 English local authorities from July 2014 to December 2025. To their knowledge, this is the first publicly available dataset to track native and migrant employment and pay separately, locally and at high frequency.

The raw employment figures are striking. Between 2015 and 2025, payrolled employment rose by 2.8 million, with 2.3 million of the increase accounted for by non-UK nationals. It would have been easy to stop there and say that migrants had taken jobs from British workers. Thankfully, the authors do not. They show that the UK-national working-age population was falling, while the share of UK nationals in work was rising, making the employment story considerably more complicated than the headline suggests.

Pay is where the results become interesting. Outside London, each additional migrant job per 100 residents reduced median pay among UK nationals by about 1.1 per cent. Applied to the rise in migrant employment since 2015, the typical British worker outside the capital earned roughly £1,060 less in 2025 than they otherwise would have done.

This is concentrated in the lower half of the earnings distribution and comes mainly through lower hourly pay rather than fewer hours. By the 80th percentile, the effect has disappeared. These workers have had the least room to absorb weak wage growth, yet for years they have been told that migration is good for the economy because GDP is higher. That may be true in aggregate, but it is cold comfort to those whose pay packets are lower than they otherwise would have been.

I also appreciate that the paper does not stop at convenient numbers. It goes further in trying to understand what is driving the results. Holding the local industrial mix constant barely changes the estimated loss, from £1,060 to £1,010, suggesting that the effect is not simply the consequence of more low-paid industries in migrant-heavy areas. The authors also test whether the result looks more like ordinary labour-market competition or employer bargaining power. The evidence leans towards competition, but they are careful not to overstate what the data can tell us.

The paper is equally clear about its limits. It does not claim to know whether migration has made Britain richer or poorer overall. It does not measure GDP, prices or the public finances, and the larger wage-bill estimates are not a clean measure of total income lost. London behaves differently, and some of the adjustment there may have come through housing rather than wages.

That does not weaken the paper; it makes the result harder to dismiss. We now have better evidence that lower-paid British workers outside London have borne a measurable wage cost from migration, and we have it over the period that matters most, including the post-2021 surge.

For too long, the debate has been dominated by averages. This paper gives us a better view of what has been happening underneath them.

Damian Pudner

Director, Great British Think Tank

October 2026

Executive Summary

Polling over the past decade has consistently placed immigration and asylum among voters' foremost concerns.¹ Since the Brexit referendum and the COVID-19 pandemic, public concern has intensified. Public opinion has shifted toward an increasingly negative view of migration,² with voters now becoming increasingly likely to question the supposed benefits of these foreign human capital flows. Despite these concerns, net-migration has remained high. The post-2021 "Boriswave" saw the largest influx of migration in modern British history, with net-flows only returning to their pre-COVID levels in 2025.

That net flows have only now fallen back to pre-pandemic levels, themselves historically high, suggests a political class that has been not merely slow to act, but structurally reluctant to do so. Crucially, this reluctance does not come from nowhere – policymakers have been operating within an intellectual consensus that has historically held a supportive attitude toward migration. The prevailing orthodoxy emphasises migration's role in sustaining economic growth, easing the fiscal pressures of ageing populations, and importing critical skills to underpin essential public services. Migration, in effect, has become a panacea for the developed world's problems. A default prescription that is seldom interrogated precisely because the weight of established scholarship appears to leave so little room for dissent.

This paper offers new evidence on the distributional consequences of migration in Britain. Earlier UK studies found small wage losses at the bottom of the pay distribution; this paper measures the cost to British workers directly, using administrative pay data split by nationality and local authority that cover the post-2021 inflow. Using newly acquired PAYE data from HMRC, released under Freedom of Information and not previously published, we observe the number of payrolled jobs and their median pay, for UK and non-UK nationals separately, in each of 147 English local authorities every month from July 2014 to December 2025. The figures are striking. Between 2015 and 2025, the number of payrolled jobs across the 147 authorities rose by 2.8 million, with jobs held by non-UK nationals accounting for 2.3 million of the rise – 80% of a decade's worth of total job growth.

A common defence is that this rise in migrant employment was necessary as natives could not or would not fill them, thereby requiring migrants to fill the gap. Our findings suggest this defence is partly right about who filled the new jobs, but that it misses a cost to native pay. Over the past decade, our estimates imply that migration has reduced the native wage bill outside London by a cumulative £78 billion, including an £18.2 billion reduction in 2025 alone.

¹ YouGov (2026a), <https://yougov.com/en-gb/trackers/the-most-important-issues-facing-the-country>

² YouGov (2026b), <https://yougov.com/en-gb/articles/55329-britons-have-a-more-negative-view-of-immigration-in-2026-than-in-2016>

Core Findings:

1. Migration lowers native pay outside London: Each additional migrant job per 100 residents reduces UK-national median pay by 1.06 per cent. Across England, the estimate is smaller and not distinguishable from zero, because migrant inflows into London behave differently, partly due to their different countries of origin.
2. The cost to the native wage bill is large and growing with the inflow: Outside London, the 2025 median-equivalent native wage bill (payrolled jobs multiplied by median pay) was about £18.2 billion lower than it would otherwise have been translating to an annual pay penalty of about £1,060 for the typical native worker, with cumulative losses approaching £78 billion over the last decade.
3. The cost is borne by the lower-paid: The burden of this wage suppression is largely borne by the bottom half of earners, mainly through lower hourly pay rather than fewer hours, while the effect fades above the median and disappears by the 80th percentile. At the very bottom, the National Living Wage appears to hold hourly pay up.
4. New destinations appear to bear a larger cost per migrant, but not a larger total cost: Outside London, the pay effect per migrant appears largest in the third of authorities with the fewest migrants in 2015, but this estimate is imprecise and the gradient is not statistically significant. Because established areas received larger inflows, the total loss is similar everywhere, and the smaller effect per migrant in those areas partly reflects British workers leaving payroll employment. It does not imply that more migration would benefit natives or offset the negative pay effect.
5. Migrants mostly did not take native jobs: The UK-national population of working age fell between 2015 and 2024, while the share of it in work rose. Outside London about two native payrolled jobs were lost per five migrant jobs added. Inactivity did not rise where migrants arrived, and the number of residents who want a job was unchanged. Receiving authorities did, however, lose residents on net to other authorities.
6. Who arrives matters: Inflows from lower-income origin countries drive the pay fall. Inflows from higher-income origins, concentrated in London, raise native median pay but cut native payrolled employment and raise house prices, a pattern at least as consistent with residential sorting as with complementarity. On average, poor English proficiency does not seem to amplify the negative pay effect of migration.

Policy Recommendations:

1. **Cut Visa Routes to Low-Wage Work:** The data demonstrate that the downward pressure on native wages falls on hourly pay in the lower half of the distribution, and that the new jobs created alongside migration are concentrated in low-paying industries. No evidence supports a “curvature” whereby the negative impacts of migration on native pay dissipate as migrants become more concentrated. Immigration policy must therefore treat the overall volume of migration as the primary mechanism for mitigating wage suppression, with restrictions applied to visa-routes that support low-paying industries. The government should strictly limit pathways like the Temporary Shortage List to roles genuinely crucial to national industrial strategy. This incentivises low-wage sectors, such as hospitality, basic manufacturing, and food processing, to adapt through technological investment and domestic upskilling rather than relying on continuous, wage-suppressing inflows.
2. **Pivot from Language Mandates to Salary Thresholds for Economic Routes:** Current integration policies heavily emphasise English language proficiency across all visa categories, yet our data shows no evidence that, on average, poor English proficiency amplifies the negative effects of migration on native pay or employment. Instead, localised wage reductions depend more on the economic profile of the migrant-origin-country and the kind of local work migrants enter. On average, these data give no support to language requirements as a tool for protecting British wages; where proficiency does appear to matter, the direction depends on the local mix of jobs. The proficiency measures are also area-level while language tests apply to individuals, and the January 2026 change post-dates the data. Policymakers should not treat language testing as a tool for protecting domestic wages and instead calibrate eligibility for work visas around robust salary thresholds. Salary floors act directly on how much lower-paid migration a route admits, although this paper does not test their effect. English proficiency mandates are best justified on student, family, and settlement routes, where the primary policy objectives are academic viability and long-term social cohesion.
3. **Institutionalise Granular Data Transparency via HMRC:** Effective local governance requires real-time economic visibility. Currently, detailed figures on payrolled employment and median income split by nationality at the local-authority level are only accessible via Freedom of Information requests. The government should mandate that HMRC publish this data monthly as official statistics. Providing this transparency empowers the people and allows the country, policymakers, and politicians to make informed decisions about migration policy. HMRC should also give accredited researchers secure access to payroll records linked to employers and visa routes, so that the mechanism behind the pay effect can be identified.

1. Introduction

Migration has for too long been viewed as a panacea.

In the past five years, Britain's net migration exceeded the total population growth of the UK in the 1970s and '80s. Faced with inflows on this scale, the electorate has raised increasing concerns regarding the supposed benefits of mass migration to the British economy. Unfortunately, the establishment class of Britain's political parties has done little to address these concerns in any serious manner, choosing instead to obfuscate, dismiss, or wholly ignore any economic and social costs associated with mass migration. It is therefore unsurprising that incumbent parties have sought to frame opposition to migration as prejudice rather than as a legitimate grievance.³ Political discourse on the subject rarely proceeds on substantive terms; instead, it tends to collapse into a binary in which scepticism toward migration is conflated with prejudiced views – often attributed to a poor education – rather than a signal worth heeding that exposes deep questions regarding the efficacy of how Britain has managed its labour markets, economy, and most importantly, its people.

Considerable intellectual effort has been devoted to delegitimising the migration-sceptic vote, with a now-familiar body of commentary drawing causal or associative links between support for populist or anti-immigration parties and educational attainment, as though the concerns of those communities could be safely dismissed once their demographics had been documented.

When the reflexive response to dissent is to profile the dissenter rather than engage with the argument, it exposes an intellectual fragility, a consensus that sustains itself only through the delegitimation of those who question it, and it is telling that the political class has only begun to concede the potential material costs of migration now that the electorate has grown too large to safely ignore.⁴

Through the econometric interrogation of newly acquired PAYE data from HMRC, released under Freedom of Information and not previously published, this paper aims to reform the national migration debate by offering the intellectual arguments necessary to argue that worries surrounding migration are not just a spectre haunting the minds of a specific 'type' of voter, who, by manner of education, upbringing, class, and geography, may be "susceptible" to being captured by populist rhetoric.

Even before any formal econometric analysis – which is detailed in the Technical Appendix – the results of the descriptive data are striking. Between 2015 and 2025, the number of payrolled jobs across the 147 authorities rose by 2.8 million, with jobs held by non-UK

³ See, for example, BBC News (2010) 'Gordon Brown "bigoted woman" comment caught on tape', 28 April, reporting the then Prime Minister describing as "bigoted" a Rochdale voter who had raised immigration with him. Available at: <http://news.bbc.co.uk/2/hi/8649012.stm>; and BBC News (2025) 'Keir Starmer calls Reform migrant policy "racist" and "immoral"', 28 September. Available at: <https://www.bbc.co.uk/news/articles/cj6xdw1dq4zo>.

⁴ Prime Minister's Office (2025) 'PM remarks at Immigration White Paper press conference: 12 May 2025', GOV.UK, 12 May. Available at: <https://www.gov.uk/government/speeches/pm-remarks-at-immigration-white-paper-press-conference-12-may-2025>; Home Office (2025) Restoring Control over the Immigration System: White Paper, CP 1326. Available at: <https://www.gov.uk/government/publications/restoring-control-over-the-immigration-system-white-paper>. Launching the white paper, the Prime Minister said that parts of the economy had come to rely on importing cheap labour rather than training workers already here, and that recent migration had shifted towards work that puts downward pressure on wages.

nationals accounting for 2.3 million of the rise – 80% of the decade's worth of total job growth.

Stark as this is, the data does not by itself show that migrants took jobs from natives. There exists a plethora of sensible explanations as to why this may be the case, the most obvious being demographic. Between 2015 and 2024, the UK-national working age population in England fell by around 215,000, as large older cohorts reached retirement and smaller cohorts replaced them. The non-UK working-age population grew by almost a million, at a pace set by immigration policy rather than simple birth-rate and demographic restrictions. When working-age natives are becoming fewer and working-age migrants more numerous, migrants will fill most new jobs even if nobody is crowded out.

If anything, the headline figure understates how well natives did. Had natives and migrants stayed in work at their 2015 rates, migrants would have filled every new job and more, and around 160,000 fewer natives would have been in work. Instead, the share of natives in work rose, leaving some 380,000 more natives employed than demography alone would predict. None of this, however, settles the question of cause. Did migrants arrive where natives were already out of work, competing with them for scarce vacancies, or where employers could not find anyone to hire? The raw figures cannot answer these questions, so this paper goes further.

To establish causality, this research uses the fact that new migrants tend to settle where earlier migrants from the same origin countries settled before them. These long-standing networks are useful predictors of where new arrivals go – irrespective of the state of the local labour market today – and so allow the effect of migration to be separated from the pull of local labour demand. Our results show that outside London, each additional migrant job per 100 residents lowers the median pay of UK nationals by about 1 per cent, with the brunt of this effect being concentrated in the bottom half of the pay distribution. When summed across authorities, the median-equivalent pay of British workers outside London was about £18 billion lower in 2025 than it would otherwise have been, with a cumulative reduction of £78 billion since the start of our panel period. Displacement from work is modest by comparison, at roughly two native jobs lost for every five migrant jobs added outside London. Across England the effect on pay cannot be distinguished from zero, because the inflows into London are of a different kind.

It is important to be clear about what this paper does not say. It does not say that migration has imposed a cost on the British economy as a whole. The cost it measures is the pay of UK nationals, and nothing more. Lower pay for native workers is not the same as lower output or productivity. Some of what natives lose in pay may be gained by employers and consumers through higher profits or improved consumer surplus, and whether the economy is larger or smaller as a result is a question for different research and different data. Our figure measures how the rewards for work have been distributed, not whether the total has grown.

Where the economic basis of scepticism is tested, it is typically by asking whether less-skilled respondents object more to less-skilled migrants, or whether people's own finances have worsened.⁵ Neither test asks whether migration has actually lowered the

⁵ Hainmueller, J. and Hiscox, M.J. (2010) 'Attitudes toward highly skilled and low-skilled immigration: evidence from a survey experiment', *American Political Science Review*, 104(1), pp. 61–84; Mutz, D.C. (2018) 'Status threat, not economic hardship, explains the 2016 presidential vote', *Proceedings of the National Academy of Sciences*, 115(19), pp. E4330–E4339.

pay of people like them, and a reanalysis of one prominent study found economic interests at least as important as the status concerns it had emphasised.⁶

The differences in support for lower migration by education and other voter characteristics are therefore read as differences in values, with the more educated taken to be more tolerant and better informed about immigration's benefits.⁷ The implication, rarely stated but hard to miss, is that scepticism about migration is a consequence of less education rather than of experience. In wider debate, what presents itself as a neutral academic finding thus supports a moral and normative view of the world in which scepticism about migration is something to be educated away, rather than a view that may rest on real costs. The presence of these real costs is precisely what this paper finds, and it is what we hope will break the panacea view of migration across Britain's policymaking space.

⁶ Morgan, S.L. (2018) 'Status threat, material interests, and the 2016 presidential vote', *Socius*, 4.

⁷ Hainmueller, J. and Hiscox, M.J. (2007) 'Educated preferences: explaining attitudes toward immigration in Europe', *International Organization*, 61(2), pp. 399–442.

A Workforce from Abroad

Four in five of the payrolled jobs added since 2015 are held by non-UK nationals, and from 2023 jobs held by UK nationals have fallen.

Investigating the effect of migration on native employment and pay is a remarkably difficult affair. Before any question of reverse causality arises, there is the more basic problem of data. The Census, the most detailed local source, asks no question about income and records employment by country of birth only once a decade. The Annual Population Survey reports employment by nationality every year, but its local samples are small, its estimates correspondingly noisy, and it too says nothing about pay. HMRC publishes payrolled employment by nationality from tax records, but only for regions and industries, not local authorities.

Much of the academic literature has therefore had to follow particular groups of migrants over their working lives, but the British datasets that allow this, such as the ONS Longitudinal Study and Understanding Society, are samples too small to describe any one local labour market, let alone the British labour market in its entirety. As such, earlier studies of local labour markets have to rely on survey estimates or proxies, estimating migrant employment shares from small samples or from assumptions that carry limitations of their own.⁸

That is precisely why the data released under our Freedom of Information request to HM Revenue and Customs (FOI2026/85595), which provides the monthly counts of payrolled employments and monthly median incomes of UK-national and non-UK national employees, by local authority from July 2014 to December 2025 is so important. It is, to our knowledge, the first publicly available dataset tracking the employment and pay of migrants and natives separately at high frequency, in every local authority.

What the tax records show

The released data paints a picture of how far Britain's labour market has come to rest on foreign workers. Since 2015, payrolled employment among non-UK nationals has grown twenty-five times faster than among UK nationals. Jobs held by non-UK nationals rose by almost two-thirds between 2015 and 2025. Jobs held by UK nationals rose by less than 3 per cent.⁹

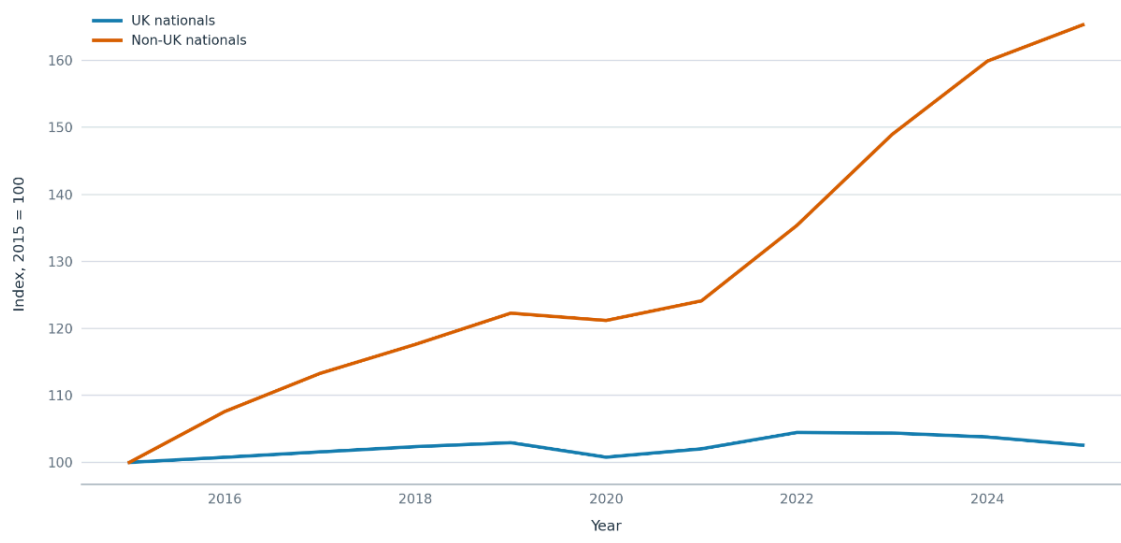
⁸ See, for example, Dustmann, C., Frattini, T. and Preston, I.P. (2013) 'The effect of immigration along the distribution of wages', *Review of Economic Studies*, 80(1), pp. 145–173; Nickell, S. and Saleheen, J. (2015) 'The impact of immigration on occupational wages: evidence from Britain', Bank of England Staff Working Paper No. 574.

⁹ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595: payrolled employments and median pay of UK-national and non-UK-national employees, by local authority, July 2014 to December 2025. Nationality is as recorded at National Insurance registration.

MIGRATION AND NATIVE PAY

Migrants account for almost all the growth in payrolled employment

Payrolled employments in 147 English local authorities, indexed to 2015 = 100.



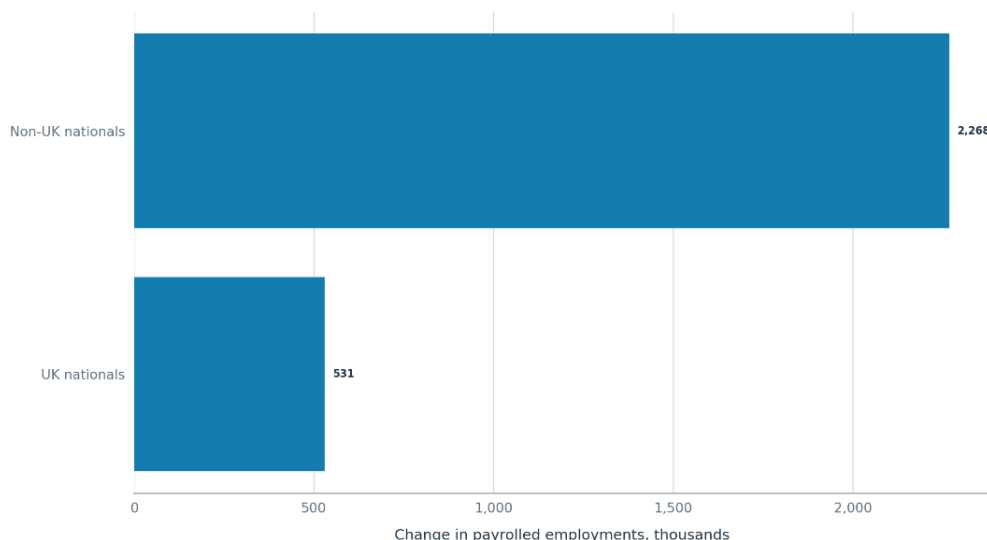
Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

In absolute terms, this represents an increase in the number of payrolled jobs of 2.8 million. Jobs held by non-UK nationals accounted for 2.3 million of this increase, representing four in every five. Jobs held by UK nationals on the other hand accounted for only 531,000.

MIGRATION AND NATIVE PAY

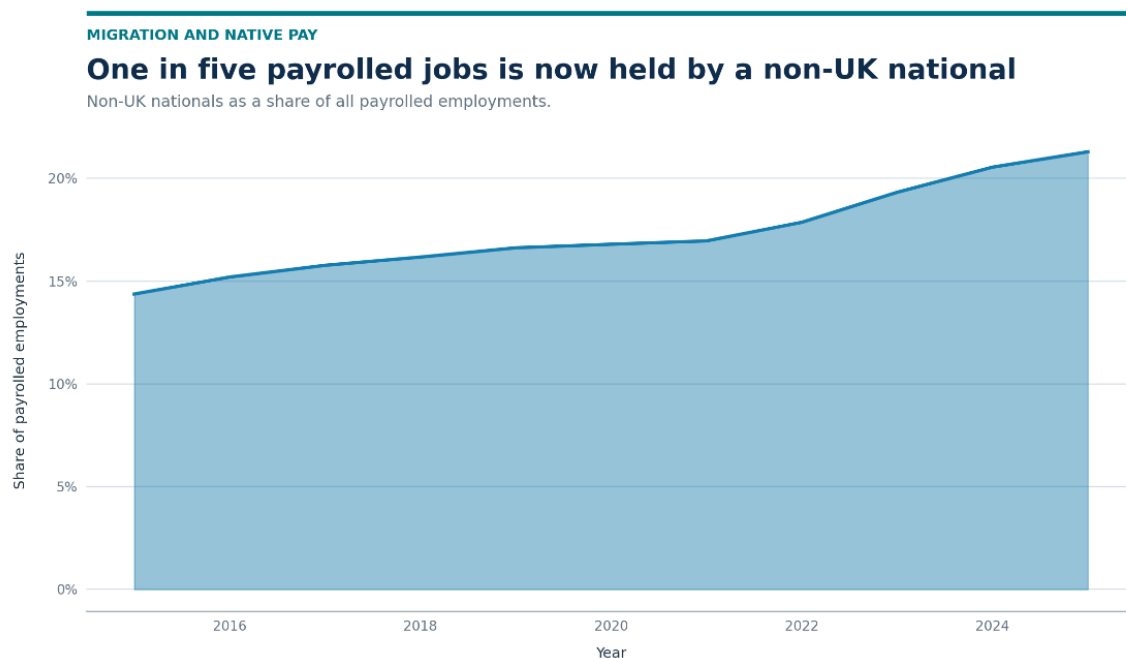
Four in five of the extra payrolled jobs since 2015 went to non-UK nationals

Change in payrolled employments, 2015 to 2025, thousands.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

One in five payrolled jobs is now held by a non-UK national. Most of that change is recent, with the increase of this share accelerating since 2021, climbing by more than four percentage points in the four years that followed. From here, two things happened at once. Jobs held by non-UK nationals grew faster than at any earlier point in the decade, and from 2023 jobs held by UK nationals began to fall.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

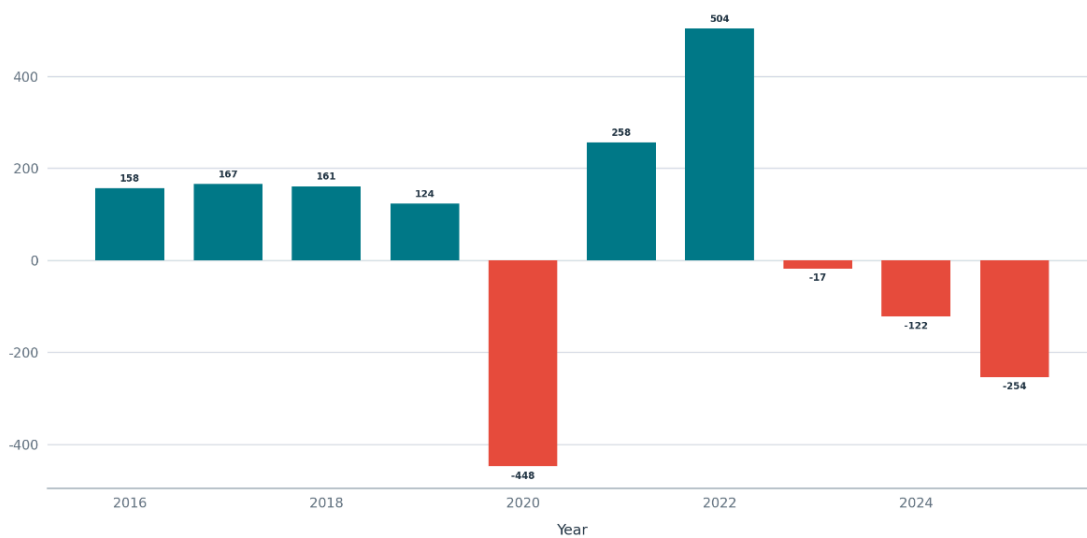
The figure below shows the second of those changes year after year. Between 2016 and 2019, jobs held by UK nationals grew by between 120,000 and 170,000 a year. They fell by 448,000 in the pandemic year of 2020, then more than recovered over 2021 and 2022. Since then, they have fallen for three consecutive years, with each fall being larger than the last. Over the same three years, jobs held by non-UK nationals rose by more than a million.¹⁰

¹⁰ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595: payrolled employments and median pay of UK-national and non-UK-national employees, by local authority, July 2014 to December 2025. Nationality is as recorded at National Insurance registration.

MIGRATION AND NATIVE PAY

UK-national payrolled employment has fallen for three years running

Year-on-year change in UK-national payrolled employments, thousands.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

This is not a count of jobs taken from British workers. The figures record net changes in who holds payrolled jobs, not who was hired. That being said, the data does show that all the growth in payrolled employment over the last three years has come from workers who were foreign nationals when they registered for National Insurance.

Fewer natives of working age

While stark, the increase in migrant employment does not have to be inherently negative. One explanation is demographic. Between 2015 and 2024, the number of UK working-age nationals in England fell by around 215,000 as large post-war cohorts reached retirement whereas the number of non-UK working-age nationals rose by almost a million.¹¹

This matters because the supply of native workers is set largely by birth rates, whereas the supply of migrant workers can change within a year. Logically, it follows that when the native working-age population is shrinking and the migrant working-age population is growing, migrants will account for most new employment even if no British worker loses a job. Had British and foreign nationals stayed in work at the rates they had in 2015, the number of UK nationals in work would have fallen by around 160,000, while the number of non-UK nationals in work would have risen by several hundred thousand.

That is not what the data observes. British workers did not withdraw from work. The share of working-age UK nationals in employment rose from 74.1 to 75.4 per cent, and 218,000 more UK nationals were in work in 2024 than in 2015. Rising participation therefore placed approximately 380,000 more British workers into employment than demography alone would have supplied.¹²

¹¹ Office for National Statistics, Annual Population Survey: population aged 16 to 64 and employment, by nationality, England. Nationality is recorded at interview. Cambridge Circus Research calculations.

¹² Office for National Statistics, Annual Population Survey: population aged 16 to 64 and employment, by nationality, England. Nationality is recorded at interview. Cambridge Circus Research calculations.

In other words, natives performed better than expected, but nonetheless this is not the picture of a native workforce being largely pushed out of employment.

Two cautions apply. These are survey estimates of people rather than tax records of jobs, so they do not match the payroll figures above exactly, and they run only to 2024. The payroll data show a larger rise in jobs held by UK nationals than the survey shows in UK nationals in work, because they count jobs rather than people, so that second jobs count twice, because some self-employed workers moved onto payroll, and because the survey carries sampling error. The survey also records nationality at the time of interview, so migrants who have since become British are counted as UK nationals. If anything, this data may understate the fall in the native working-age population, and the decline among those born British is likely to be larger.

Irrespective of these facts, a shrinking native workforce still explains why migrants filled most of the new jobs. However, it does not tell us whether the arrival of migrants affected natives through channels such as employment or pay. Answering that requires us to separate the effect that migration has on native outcomes from everything else happening in local labour markets, which is precisely what our methodology aims to decompose.

Are Migrants Taking Our Jobs?

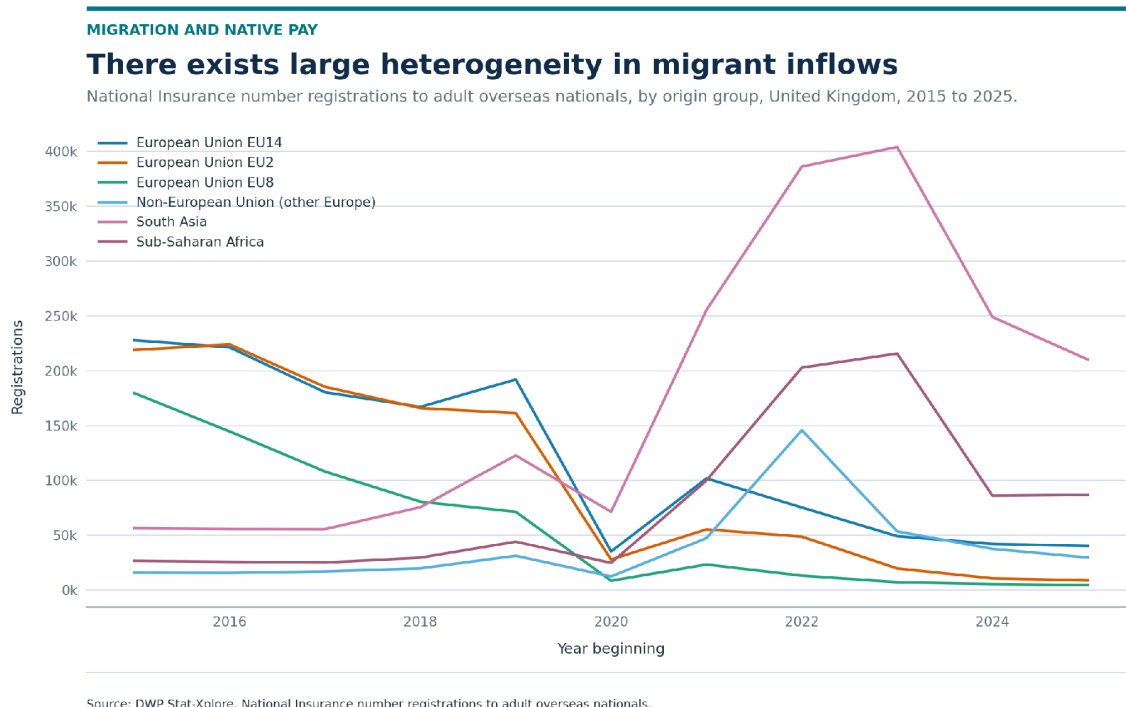
Not all of them. When migrant employment rises, fewer British residents are in work, but by far less than one for one.

On their own, these trends are not enough to establish whether migration has led to negative outcomes for natives. Migrants do not settle randomly. They move to where jobs are being created and pay is rising, and so do British workers. A simple comparison of places or analysis of national trends would therefore credit migration with the effects of local or national growth or blame it for the effects of local and national decline.

The difference matters. Authorities where migrant employment rose saw native employment fall relative to others in their region, which at face value looks like displacement. But most of that relationship disappears once the pull of local demand is removed, which is why this report does not rely on it.

To separate cause from coincidence, our analysis uses a feature of how migration works. New migrants tend to settle where earlier migrants from the same countries settled before them. A town that attracted workers from Poland and the other 2004 accession states in the 2000s received more of them when that migration rose nationally, and fewer when it fell after the referendum.

Those national rises and falls were driven by policy and events, such as EU enlargement, the end of free movement and the expansion of work and study visas, not by conditions in any single local labour market.¹³



¹³ Department for Work and Pensions, National Insurance number registrations to adult overseas nationals, Stat-Xplore. Registrations by financial year, United Kingdom.

Combining where each nationality settled between 2002 and 2010 with how many arrived nationally each year gives a prediction of each authority's migrant inflow that does not depend on how its own economy is performing. Each authority is then compared with itself over time, and with other authorities in its region in the same year. Throughout this report, results significant at the 5 per cent level are treated as findings, and results significant only at the 10 per cent level as suggestive. The full model, its tests and its limitations are set out in the Technical Appendix.¹⁴

Two native jobs lost for every five migrant jobs

Once the pull of local demand is removed, the effect of migration on native employment is modest. Across England as a whole, the best estimate is that each additional migrant job displaced about a quarter of a native payrolled job. The uncertainty around that figure is wide enough to include no effect at all but narrow enough to rule out losses of more than about one native job for every two migrant jobs.¹⁵

MIGRATION AND NATIVE PAY

Migrants replace some British workers, but most fill extra jobs

For every 10 extra jobs held by non-UK nationals in an authority: the fall in jobs held by UK-national residents, and the extra jobs on top.



Source: HMRC FOI2026/85595; ONS population estimates; authors' calculations.

Central estimates. 95% intervals per 10 jobs: excluding London 0.2 to 7.8; England below 0 to 5.2.

Outside London the estimate is larger and statistically significant. This report takes England outside London as its headline for three reasons: London boroughs are poor units for measuring a labour market, because so many residents work in other boroughs; London's housing market behaves differently; and London's migrants come from a different mix of countries, as the next chapter shows. The Technical Appendix also shows that the inflows which identify the estimate agree on its direction outside London but not in the full sample. In this specification, each additional migrant job displaced about two-fifths of a native job. The uncertainty, however, is wide, running from almost nothing to around four-fifths of a job, but even its upper end falls short of one native job for every migrant job. The employment effect outside London also appears only once changes in local industrial

¹⁴ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595. See the Technical Appendix for the native employment models, the displacement ratio and the instrument.

¹⁵ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595. See the Technical Appendix for the native employment models, the displacement ratio and the instrument.

demand are considered which serves as an important control. It is also the least robust of the main estimates: if clustering by region rather than by authority widened its uncertainty in the same proportion as it does for pay, it would no longer be statistically significant, whereas the pay effect and the net loss of residents would be.

Our estimates indicate that when migrant employment rises, the number of British residents in work does fall, but this is not one-for-one. Specifically, when estimated for England excluding London, we find that for every ten migrant jobs added, about four native jobs are lost, so that total employment rises by about six. In plain terms, migrants appear to replace some British workers rather than all of them. That does not make migration costless for British workers. As the next chapter shows, the larger cost has fallen on what their jobs pay rather than on how many of them they hold.

Areas receiving migrants lose residents on net

A fall in native employment in an authority can come about in three ways. British workers may lose their jobs and become unemployed. They may stop looking for work and leave the labour force altogether. Or they may move away, taking their work, or their search for it, somewhere else. Because the tax data record people where they live, we can observe whether there are fewer UK-national jobs held by the residents of a specific authority. Combined with data on moves between authorities, we can therefore tell displacement in the ordinary sense apart from a population that is simply relocating.

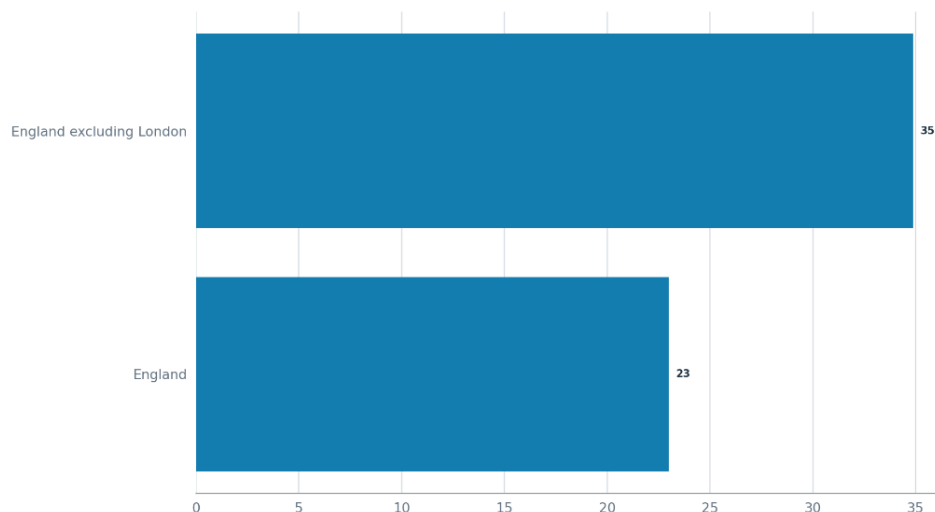
The evidence points most clearly to the third. Authorities that receive more migrants lose residents, on net, to the rest of the country. Each year, for every 100 additional migrant jobs, the balance of arrivals from elsewhere in Britain over departures falls by around 35 people outside London, and by around 23 across England as a whole. The data cannot say which side of that balance moves: taken separately, neither arrivals nor departures change significantly, and the estimates point, if anything, to fewer people arriving rather than more people leaving. Alongside the pay effect examined in the next chapter, this net loss of residents is one of the most firmly established results in this report.¹⁶

¹⁶ Office for National Statistics, internal migration estimates by local authority. Cambridge Circus Research calculations.

MIGRATION AND NATIVE PAY

Receiving authorities lose residents to the rest of Britain

Net outflow of residents to the rest of Britain each year, for every 100 extra migrant jobs in an authority.



Source: HMRC FOI2026/85595; ONS internal migration estimates; authors' calculations.

Includes migrants moving on. 95% intervals: England 13 to 33; excluding London 20 to 50.

By contrast, our results show no sign of residents dropping out of the labour market. The number of residents who are out of work but want a job does not rise as migration increases, and neither does economic inactivity.¹⁷ This is not proof that no one has been discouraged from looking for work. However, it suggests that the local fall in native jobs reflects the net loss of residents to other authorities rather than being discouraged from re-entering the labour market. The first route, unemployment, is not examined as an outcome here, because the claimant count enters the model as a control.

Two limits apply. The migration flows count everyone who moves, not only British nationals, so some of those leaving may be migrants moving on to other parts of the country. And the data cannot show whether those who leave keep their jobs, find new ones, or move for housing or family reasons. What they do show is that a local loss of native jobs is not the same as British workers losing work. Some of the jobs recorded as lost in one authority may simply be held by the same people living somewhere else.

¹⁷ Office for National Statistics, Annual Population Survey, economic inactivity by local authority. Cambridge Circus Research calculations.

The Cost of Migration is in Pay

Outside London, the rise in migrant employment since 2015 has left the typical British worker about £1,060 a year worse off, with the loss concentrated in the lower half of the pay distribution.

Between 2015 and 2025, migrant employment in the average authority outside London rose from about 4.5 to 7.9 jobs for every 100 residents. Using the same method as for employment, each additional migrant job per 100 residents reduced the median annual pay of British workers by about 1.1 per cent.¹⁸ This is the most important finding of this research.

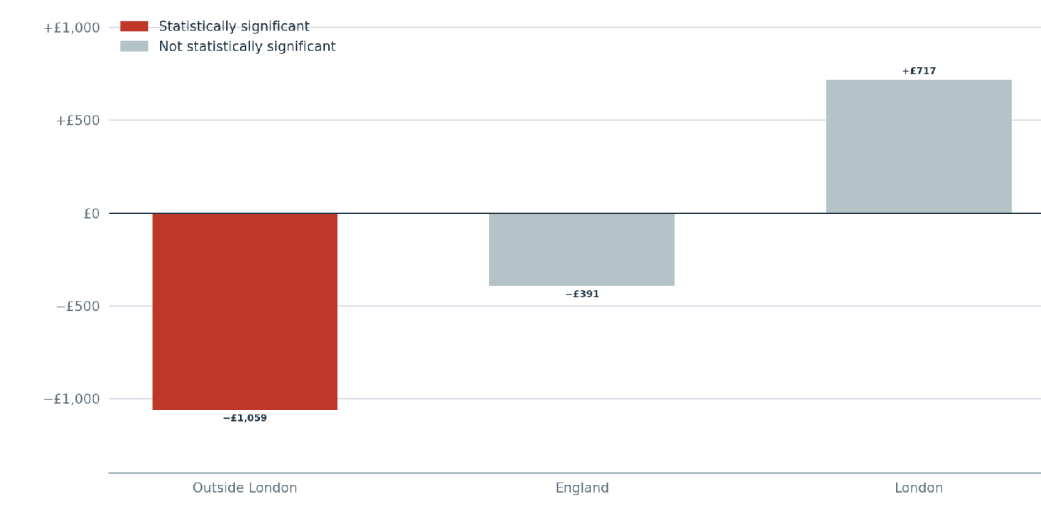
Applied to the rise since 2015, that amounts to a reduction of about 3.6 per cent. On median pay of around £29,100 in 2025, the typical British worker outside London earned about £1,060 less than they would have done had migrant employment stayed at its 2015 level.

The uncertainty around that figure runs from about £710 to £1,410, so the loss is clearly distinguishable from zero.

MIGRATION AND NATIVE PAY

The typical British worker outside London earns about £1,060 a year less

Estimated effect of the rise in migrant employment since 2015 on the median annual pay of UK-national workers in 2025.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

95% ranges: outside London -£1,410 to -£710; England -£970 to +£190; London -£850 to +£2,280.

This does not mean that pay fell. Pay rose in cash terms almost everywhere over the decade. The estimate shows that it rose by less in places where migrant employment rose more, compared with other authorities in the same region in the same year. Because the tax records identify nationality, the figure covers British workers only, so it is not pulled down by the pay of migrants themselves.

¹⁸ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595. Median annual pay of UK-national payrolled employees, 115 upper-tier authorities in England outside London. See the Technical Appendix for the pay models and the per-worker calculation.

Across England the estimate is smaller, at about £390 a year, and cannot be distinguished from zero. Largely, this is because London behaves differently to the rest of the country. In London the estimate is positive but so uncertain that it could equally be a loss, and pooling the capital with the rest of the country pulls the national figure towards zero. The reasons for that, and why this report takes England outside London as its headline, are set out at the end of this chapter.

The lower-paid bear the cost

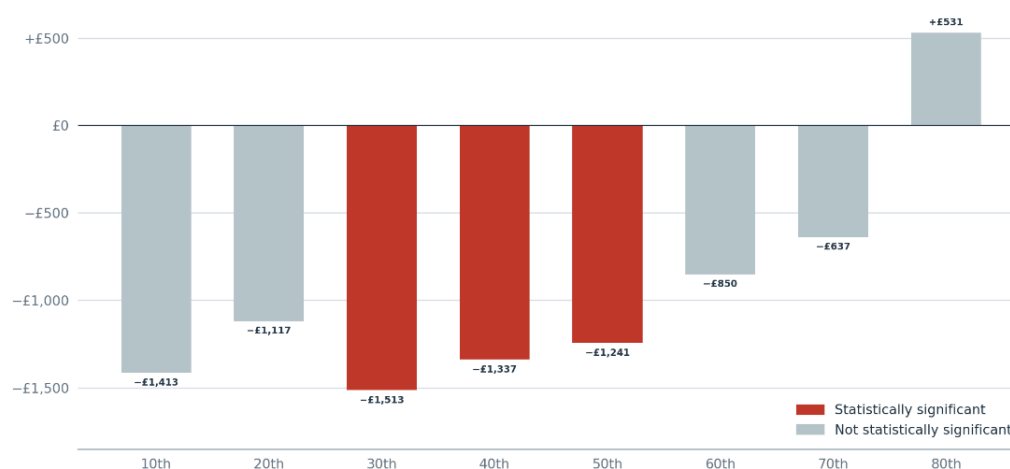
The median describes the typical worker, but not how the cost is shared. For that, the analysis turns to the Annual Survey of Hours and Earnings, which records pay at each point of the local pay distribution. It is a survey of all employees, so unlike the tax records it cannot separate British from migrant workers.¹⁹

Workers at the 30th, 40th and 50th percentiles, paid less than most but not among the very lowest paid, earned between £1,240 and £1,510 a year less as a result of the rise in migrant employment since 2015. All three estimates are statistically significant. Estimates for the lowest paid tenth and fifth of workers are of a similar size, at £1,410 and £1,120, but the survey is too thin at the bottom of the distribution to be sure of them. Above the median the effect shrinks, and by the 80th percentile it has disappeared.

MIGRATION AND NATIVE PAY

The loss falls on the lower-paid

Estimated effect of the rise in migrant employment since 2015 on annual pay at each percentile, from the lower-paid (10th) to the higher-paid (80th), outside London.



Source: ONS Annual Survey of Hours and Earnings; HMRC FOI2026/85595; authors' calculations.

Covers all employees, not only UK nationals, so figures differ from the payroll median in the previous chart.

A cost of similar size in pounds weighs more heavily on a smaller pay packet. At the 30th percentile it amounts to around 6.5 per cent of annual pay, against about 4 per cent at the median. The cost of migration has therefore fallen hardest on workers in the lower half of the pay distribution, who have the least room to absorb a loss.

¹⁹ Office for National Statistics, Annual Survey of Hours and Earnings, Tables 8.7a (annual pay), 8.5a (hourly pay) and 8.9a (paid hours worked), all by place of residence. The share of part-time jobs is taken from the job counts in Table 8.9a, which ONS advises are not reliable estimates. Cambridge Circus Research calculations.

One limitation that must be noted is that because the survey includes migrants, part of the fall at lower percentiles may reflect new arrivals taking lower-paid jobs rather than existing workers being paid less. The payroll figure for British workers above is not affected in this way.

Mostly lower hourly pay, not fewer hours

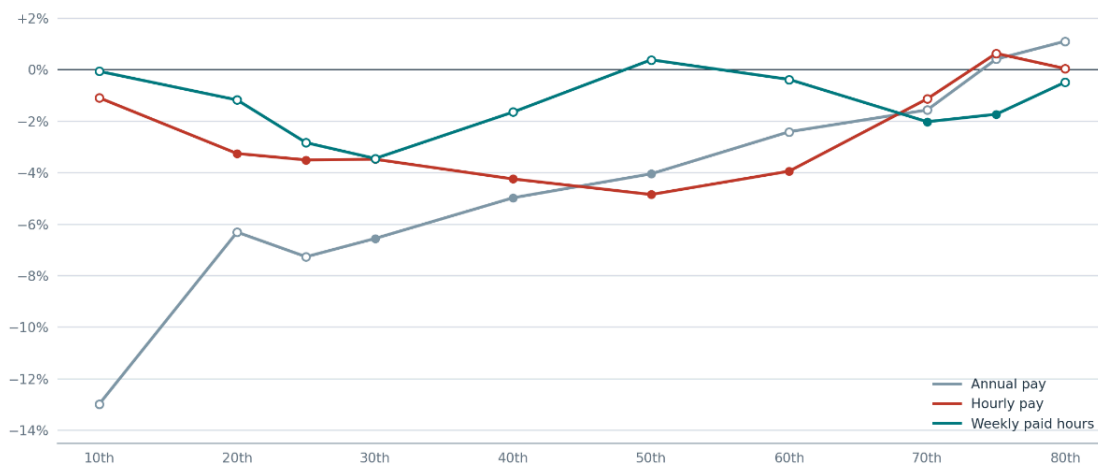
A smaller annual pay packet can come from two places: fewer hours of work or less pay for each hour worked. The distinction matters. Fewer hours would mean British workers are being squeezed out of shifts whereas lower hourly pay would mean the work itself is being paid less. The same survey records hourly pay and weekly paid hours for the residents of each authority, so the two can be compared directly to determine where the pay loss of migration originates from.²⁰

Outside London, hourly pay falls across the lower and middle of the distribution. Between the 20th and 60th percentiles, hourly pay is between 3 and 5 per cent lower because of the rise in migrant employment since 2015, and every one of those estimates is statistically significant. At the median, the fall in hourly pay, 4.8 per cent, is at least as large as the fall in annual pay. Weekly paid hours fall by less. Around the 30th and 40th percentiles there is suggestive evidence of shorter hours, significant only at the 10 per cent level, and the only falls significant at the 5 per cent level are small ones, of under an hour a week, among those working longer hours. The share of jobs that are part-time has not risen.

MIGRATION AND NATIVE PAY

Outside London, the loss is in hourly pay, not hours

Estimated effect of the rise in migrant employment since 2015 at each percentile of annual pay, hourly pay and weekly paid hours, outside London.



Source: ONS Annual Survey of Hours and Earnings; HMRC FOI2026/85595; authors' calculations.

Filled markers are statistically significant at the 5% level. All measures by place of residence; covers all employees.

These survey estimates line up closely with the tax records. Over the rise in migrant employment since 2015, median annual pay among all employees outside London is

²⁰ Office for National Statistics, Annual Survey of Hours and Earnings, Tables 8.7a (annual pay), 8.5a (hourly pay) and 8.9a (paid hours worked), all by place of residence. The share of part-time jobs is taken from the job counts in Table 8.9a, which ONS advises are not reliable estimates. Cambridge Circus Research calculations.

about 4.0 per cent lower on the survey, and median hourly pay 4.8 per cent lower, against 3.6 per cent for British workers in the payroll data.

The survey figures are slightly larger, as would be expected if part of the fall reflects lower-paid migrants joining the workforce, but the differences are well within the uncertainty of the estimates. Nonetheless, two different sources, measuring pay in different ways, pointing to an effect of the same size helps verify our estimates.

The bottom of the distribution behaves differently, and the absence of a result there is informative in its own right. At the 10th percentile, neither hourly pay nor hours moves: hourly pay is about 1 per cent lower, hours are essentially unchanged, and neither estimate can be distinguished from zero. A floor under hourly pay is what the National Living Wage is designed to provide, and it appears to be holding. If employers cannot pay less per hour, the adjustment might be expected to come through hours instead, but surprisingly the data do not show that either. Annual pay at the 10th percentile does appear to fall, but that estimate is imprecise and may reflect time out of work during the year, which weekly hours do not capture, as much as any change in pay.

Outside London, then, most of the cost of migration to British workers comes through lower hourly pay; around the 30th percentile hours may fall as well, though that estimate is less precise. The effect on hourly pay is sharpest just above the wage floor.

These results also show the limits of what the published statistics can show. The survey covers all employees, British and migrant alike, so it cannot say whether the fall in hourly pay describes British workers being paid less or new arrivals starting on lower rates. And at the bottom of the distribution, where hours vary most from week to week and the survey is thinnest, the estimates for hours are too imprecise to rule out a meaningful effect.

HM Revenue and Customs already receives a band for each employee's normal weekly hours through the payroll system, and already links payroll records to nationality for the figures used in this report.

The Office for National Statistics could publish local survey estimates separately for British and non-British employees. Either would allow hours and hourly pay to be measured for British workers directly, and would show whether the pattern here holds for them.²¹

£18 billion in 2025, £78 billion over the decade

Applying the estimate to each authority's own rise in migrant employment and to the pay of its British workforce gives the amount by which British workers' total pay was lower than it would otherwise have been. Outside London, the median-equivalent wage bill of British workers, their number of payrolled jobs multiplied by median pay, was around £540 billion in 2025. The estimate implies that this was about 3.4 per cent, or £18.2 billion, less than it would have been had migrant employment stayed at its 2015 level. The uncertainty of this estimate runs from £12.1 billion to £24.3 billion, making it statistically distinguishable from

²¹ Office for National Statistics, Annual Survey of Hours and Earnings, Tables 8.7a (annual pay), 8.5a (hourly pay) and 8.9a (paid hours worked), all by place of residence. The share of part-time jobs is taken from the job counts in Table 8.9a, which ONS advises are not reliable estimates. Cambridge Circus Research calculations.

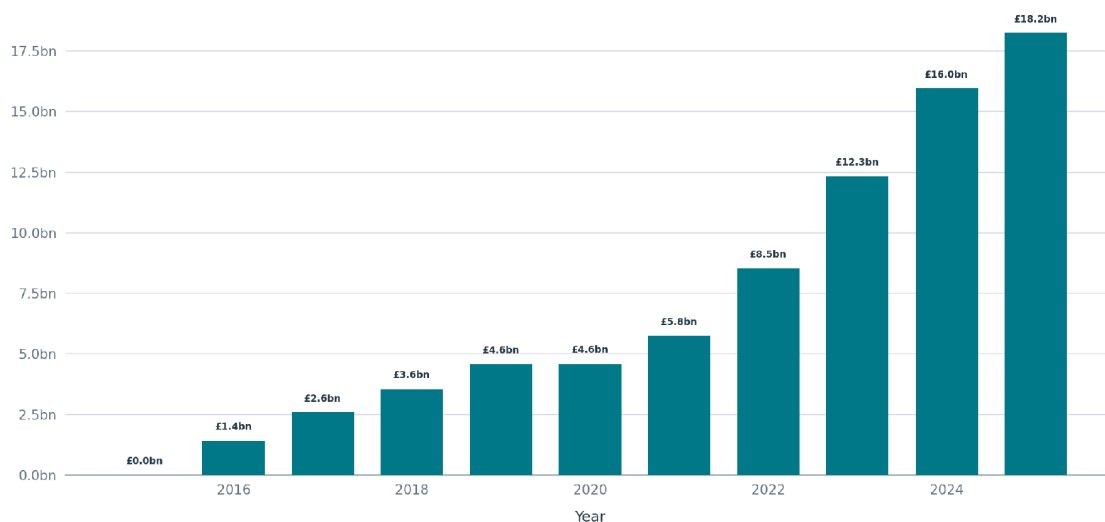
zero. Because the figure applies the effect on median pay to the whole bill, it is best read as the loss in typical pay, summed across British workers' jobs.²²

The shortfall has grown with the inflow. It was £1.4 billion in 2016 and still under £5 billion in 2020, when the pandemic briefly halted migration. It then rose quickly, to £8.5 billion in 2022, £16.0 billion in 2024 and £18.2 billion in 2025. Around three-fifths of the rise in migrant employment outside London since 2015 arrived after 2021, and the cost has followed.

MIGRATION AND NATIVE PAY

The annual shortfall has grown with the inflow

Estimated reduction in the UK-national wage bill, England excluding London, in each year from 2015 to 2025, relative to 2015 migrant employment.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

A partial-equilibrium figure on a median-pay base; see the technical appendix for what the design does and does not identify.

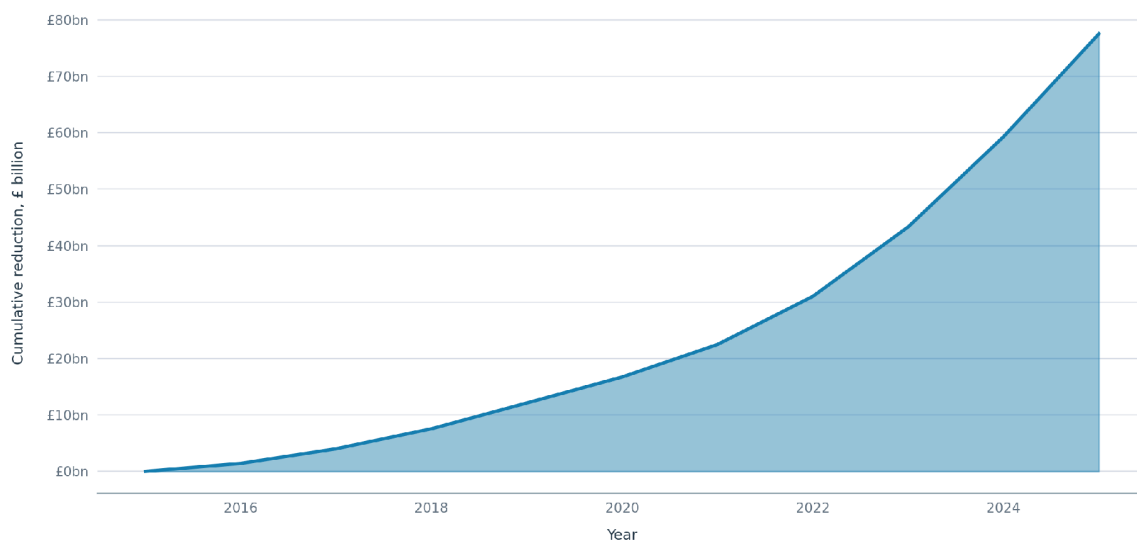
Each year's shortfall is pay that British workers did not receive in that year. Adding the years together gives the total forgone over the decade, around £78 billion between 2016 and 2025, with a range of £52 billion to £103 billion.

²² Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595. Cash terms. The Technical Appendix sets out the wage-bill calculation, its uncertainty and what the design does and does not identify.

MIGRATION AND NATIVE PAY

Cumulative effect, 2015 to 2025

Running total of the annual figures in the previous chart, England excluding London.



Source: HMRC FOI2026/85595 (PAYE Real Time Information and Migrant Worker Scan); authors' calculations.

A partial-equilibrium figure on a median-pay base; see the technical appendix for what the design does and does not identify.

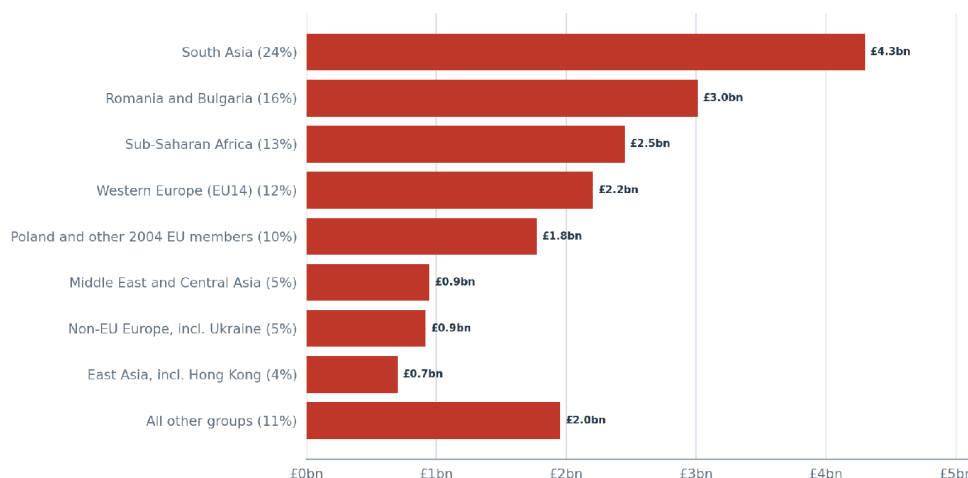
This result can also be decomposed among the inflows that produced it. Splitting each authority's rise in migrant employment among origin groups in proportion to their National Insurance registrations since 2015, arrivals from South Asia account for £4.3 billion of the 2025 figure, from Romania and Bulgaria £3.0 billion, from Sub-Saharan Africa £2.5 billion and from western Europe £2.2 billion. Together these four groups account for about two-thirds of the total. This is an accounting split, not a separate estimate, meaning it applies the same effect to every migrant job, so it shows where the total comes from rather than whether some inflows reduce pay more than others. It should not be read as the harm done by each group: the western European inflows, for example, are among the higher-income inflows found below to raise the measured median in London.²³

²³ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595. Cash terms. The Technical Appendix sets out the wage-bill calculation, its uncertainty and what the design does and does not identify.

MIGRATION AND NATIVE PAY

Where the 2025 inflow came from

The 2025 reduction in British workers' pay outside London, divided among origin groups by their National Insurance registrations since 2015. An accounting split that applies the same estimate to every group, not a measure of each group's effect.



Source: HMRC FOI2026/85595; DWP Stat-Xplore; authors' calculations.

Why London is different

London is the exception to almost every finding in this chapter. In the capital, the estimated effect on the pay of the typical British worker is positive, at about £720 a year, although the uncertainty is so wide that it could equally be a loss. The question is why the capital should differ, whether its result should be read as a sign that migration benefits British workers there, and whether we can justify excluding London when reporting our headline estimates.

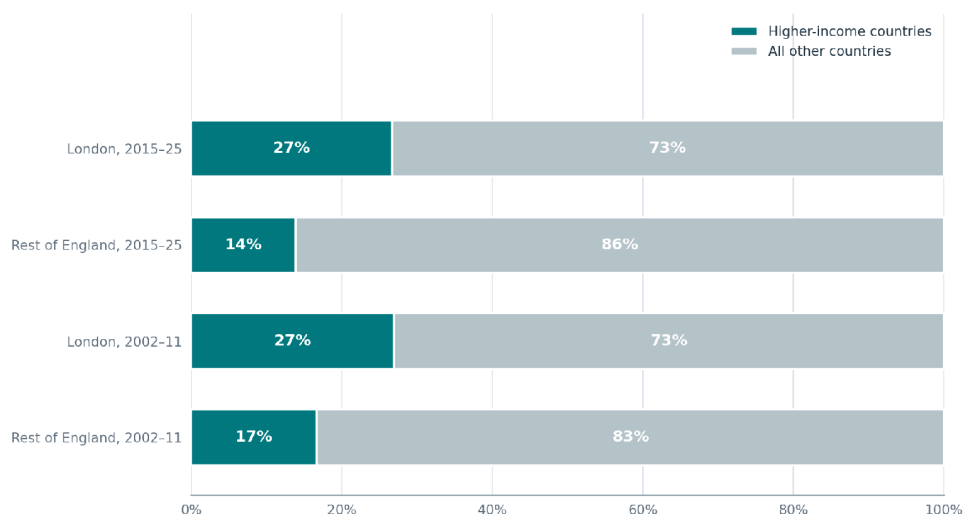
Part of the answer lies in who moves to London. Migrants tend to settle where earlier migrants from the same countries settled before them, and London's established communities include many from higher-income countries: Western Europe, the United States, Canada, Australia and New Zealand. Between 2015 and 2025, around one in four National Insurance registrations in London were by nationals of these countries, against about one in seven in the rest of England. London received more than half of all registrations from these countries, but only about a third of registrations from everywhere else. A similar pattern held in the 2000s, when the communities that later arrivals followed were established.²⁴

²⁴ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595; Department for Work and Pensions, Stat-Xplore; HM Land Registry, UK House Price Index; Office for National Statistics, Annual Population Survey. Higher-income origins are Ireland, Germany, France, the Netherlands, Italy, Spain, Portugal, Australia, Canada, New Zealand and the United States: the higher-income nationalities with enough registrations to be counted separately. Registrations are by financial year and local authority of residence. See the Technical Appendix, Section A.4.8.

MIGRATION AND NATIVE PAY

London draws twice the share of arrivals from higher-income countries

Share of National Insurance number registrations by adult overseas nationals from higher-income countries.



Source: DWP Stat-Xplore, NINO registrations to adult overseas nationals; authors' calculations.

Higher-income: Australia, Canada, France, Germany, Ireland, Italy, Netherlands, NZ, Portugal, Spain, US.

That difference matters for pay. Where inflows from higher-income countries rise, the median pay of British workers rises too. Where inflows from lower-income countries rise, as they have across most of England, British pay falls.

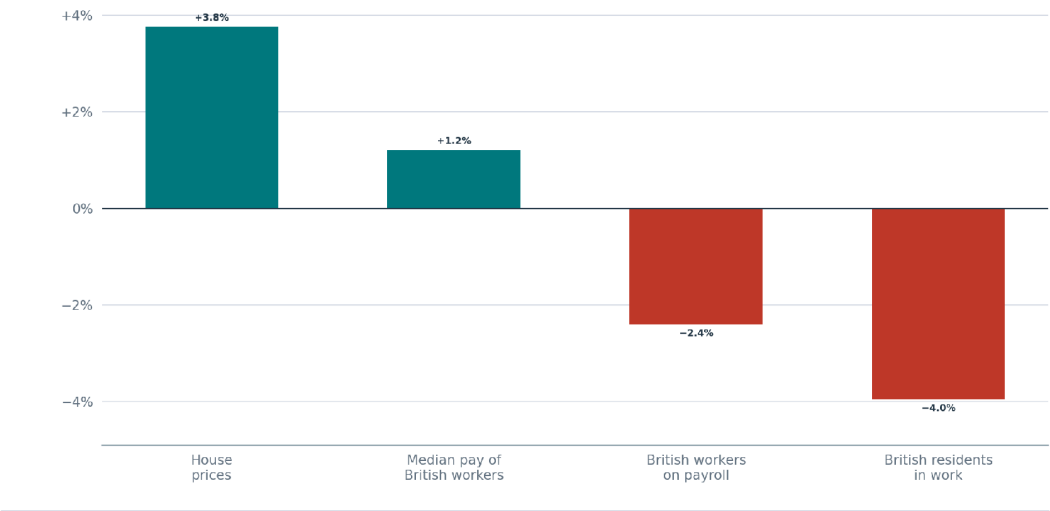
But the rise in the London median does not appear to come from anyone being paid more. When inflows from higher-income countries rise in a borough, the number of British workers on payroll there falls by 2.4 per cent for each extra migrant job per 100 residents, and the number of British residents in work of any kind falls by 4.0 per cent. These workers are not moving into self-employment, and they are not leaving the labour market. At the same time, house prices rise by 3.8 per cent, the largest and most precisely measured response of all.

The explanation most consistent with these results is that affluent arrivals push up housing costs, and lower-paid British residents leave the boroughs concerned. When the lower-paid leave, the median pay of those who remain rises, even if nobody's pay has changed. The data do not track British nationals moving between authorities, and the flow data do not show the outflow this account predicts: in London, neither net internal migration nor moves out respond significantly to these inflows. That does not rule it out, because British residents may instead have left the country, and because the survey records nationality at interview while the tax records use nationality at National Insurance registration. We therefore advance it as a hypothesis. It does, however, mean that the positive London figure should not be read as evidence that migration has raised British pay in the capital. It is at least as likely to reflect who can still afford to live there.

MIGRATION AND NATIVE PAY

In London, house prices rise and fewer British residents are in work

Estimated effect in London boroughs of inflows from higher-income countries, for each extra migrant job per 100 residents.



Source: HMRC FOI2026/85595; UK House Price Index; ONS Annual Population Survey; authors' calculations.

All four estimates are statistically significant.

For that reason this report takes England outside London as its headline. Outside the capital, the estimate is both larger and far more certain, and the pay of British workers is measured in places where house prices are not reshaping the population to the same degree. In London, the cost of migration may simply be paid in a different currency.

Is Language the Problem?

Poor English has become more common where migrants have settled, but the cost to British workers is no larger where it is most concentrated. Where English does seem to matter, it depends on whether local jobs need it.

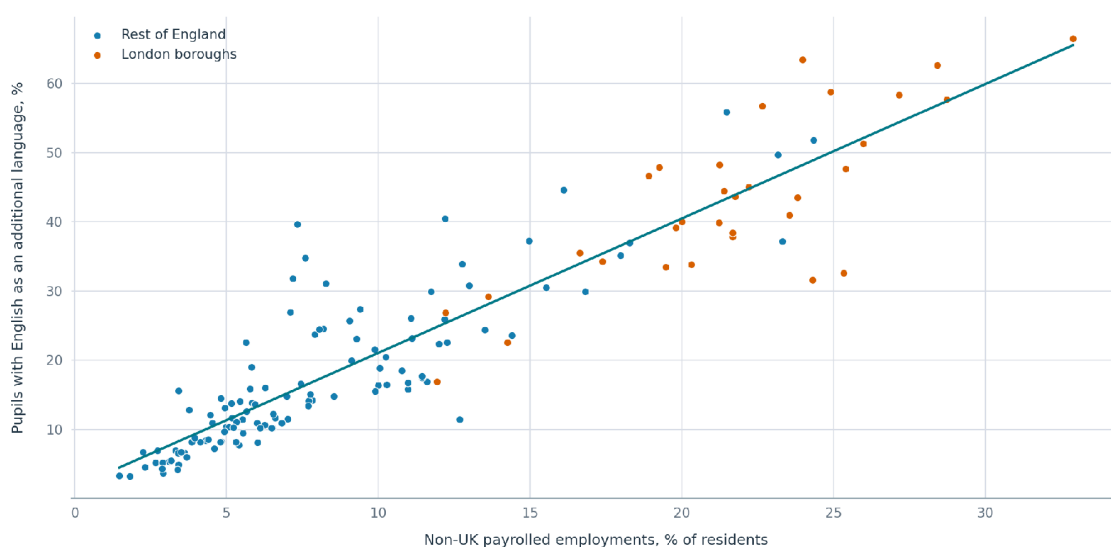
A common explanation for why migration might hold down wages is language. Migrants who speak little English, the argument states, are confined to the same low-skilled jobs as the lowest-paid British workers, compete directly with them, and cannot move up into better-paid work. If that were the main channel, the cost of migration to British workers would be largest where poor English is most common. It is also the logic behind stricter language requirements with respect to immigration policy with the Government raising the English requirement for skilled worker visas from intermediate to upper-intermediate level in January 2026.

There is no doubt that poor English has become more common where migrants have settled. Outside London, the share of pupils with English as an additional language rose from 11.6 per cent in 2015 to 17.1 per cent in 2025, as migrant employment rose from 4.5 to 7.9 jobs for every 100 residents. Authorities where more migrants work have far more pupils who have English as a second language, and the relationship is rather close.²⁵

MIGRATION AND NATIVE PAY

Where migrants work, more pupils speak English as a second language

Local authorities in 2025. Each point is one authority.



Source: HMRC FOI2026/85595; Department for Education school census; authors' calculations.

Correlation 0.86 outside London, 0.92 across all 147 authorities. FAL pupils include UK-born children.

The cost is no larger where English is poorest

The question is whether poor English changes the effect of migration on British workers.

²⁵ Department for Education, Schools, pupils and their characteristics, pupils with English as an additional language; HM Revenue and Customs FOI2026/85595. Authority averages for the 115 upper-tier authorities outside London. Cambridge Circus Research calculations.

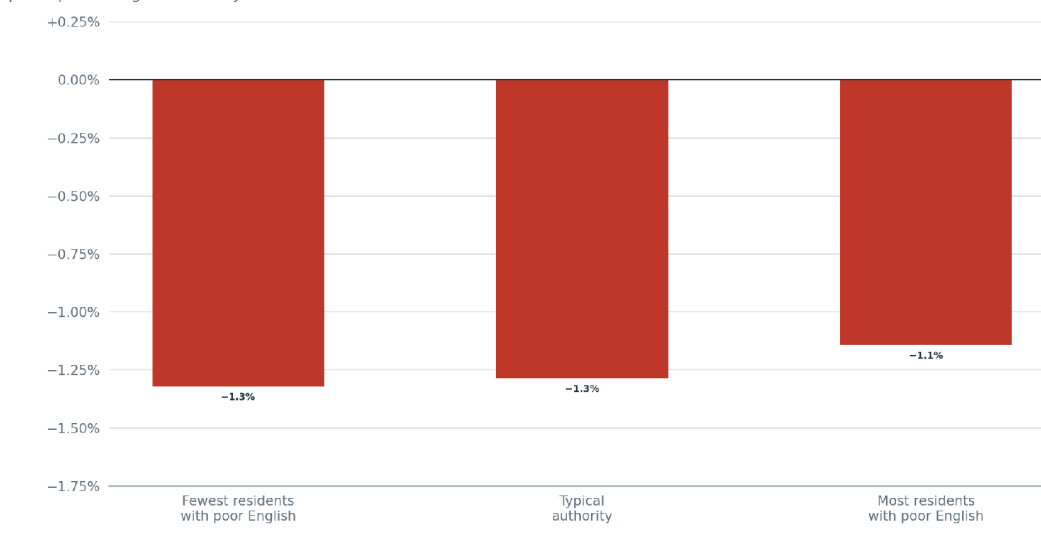
To test it, the analysis allows the effect of migration on British pay and employment to differ with how common poor English is in each authority, using three separate measures: the share of residents who told the 2011 Census they cannot speak English well, the share of pupils with English as an additional language in 2015, and an annual nowcast combining the two with enrolments on English classes. If language were the channel, the effect of migration would grow as English proficiency falls.²⁶

It does not. Outside London, each extra migrant job per 100 residents reduces the median pay of British workers by 1.3 per cent in the authorities with the fewest residents with poor English, by 1.3 per cent in the typical authority, and by 1.1 per cent in those with the most. The effect on employment follows the same pattern. Across all three measures, and for both pay and employment, not one estimate shows a larger effect where English is poorer, and none of the differences is statistically significant.

MIGRATION AND NATIVE PAY

The pay loss is no larger where English is poorest

Change in British workers' median pay for each extra migrant job per 100 residents, outside London, by how common poor spoken English is locally.



Source: HMRC FOI2026/85595; ONS Census 2011; authors' calculations.

Authorities ranked by the 2011 Census share of residents who cannot speak English well: 10th percentile, median, 90th percentile.

Nor is the result a product of London, where poor English and high migration are both concentrated. Estimated separately, the effect outside London is no larger where proficiency is poor, and within London the effect on employment is, if anything, smaller in the boroughs where poor English is most common.²⁷

²⁶ Office for National Statistics, Census 2011, proficiency in English; Department for Education, school census; English for Speakers of Other Languages enrolments. The interaction models and the estimates by sample are set out in the Technical Appendix, Sections A.2.7, A.3.5 and A.4.4. The estimates by level of English come from a model that lets the effect vary with the 2011 Census measure, so they differ slightly from the headline estimate of 1.06 per cent.

²⁷ Office for National Statistics, Census 2011, proficiency in English; Department for Education, school census; English for Speakers of Other Languages enrolments. The interaction models and the estimates by sample are set out in the Technical Appendix, Sections A.2.7, A.3.5 and A.4.4. The estimates by level of English come from a model that lets the effect vary with the 2011 Census measure, so they differ slightly from the headline estimate of 1.06 per cent.

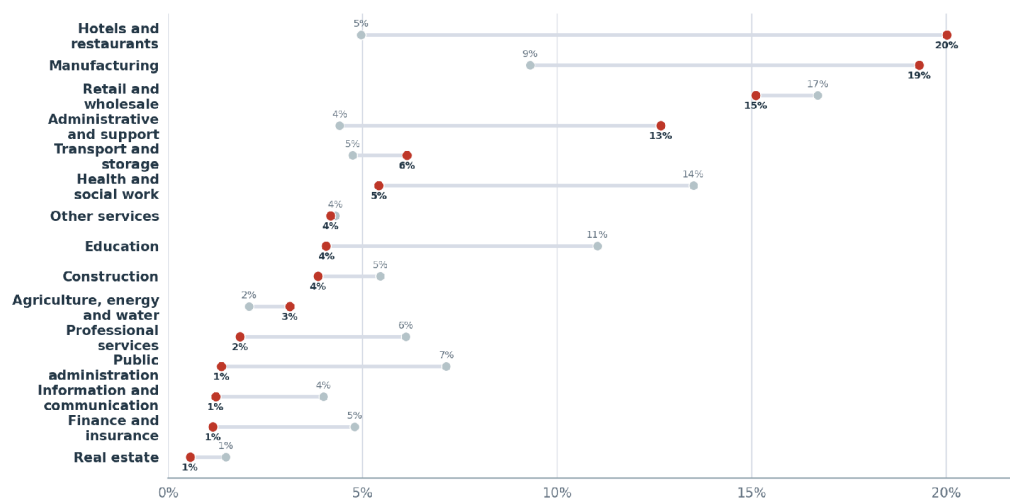
Does English matter only where the jobs need it?

There is an obvious objection to this test. Many of the jobs migrants take may not require much English to begin with. If migrants work mainly on production lines, in kitchens or in warehouses, how well they speak English would not change whom they compete with – and they could get by quite easily without ever learning the language properly – meaning that the test above would find nothing even if language mattered wherever the work required it.

MIGRATION AND NATIVE PAY

Workers with poor English are concentrated in a few industries

Share of employees in each industry, England, 2011: native English speakers (grey) and people who cannot speak English well or at all (red).



Source: ONS Census 2011 (DC2604EW); authors' calculations.

Each group's employees sum to 100% across industries.

The first half of that argument is right. In the 2011 Census, just over half of employees who could not speak English well worked in three industries: hotels and restaurants, manufacturing, and administrative and support services, which includes cleaning, security and agency work. Less than a fifth of native English speakers did. Among workers whose first language was not English, a quarter of those in manufacturing and in administrative and support services could not speak English well, against around one in twenty in finance, information and communication, and professional services.²⁸

That makes the second half testable. Using those national patterns, each authority's mix of jobs in 2015 shows how much of its work is done by people without good English.

²⁸ Office for National Statistics, Census 2011, tables DC2604EW (proficiency in English by industry, employees, England) and DC2602EW (by occupation); Business Register and Employment Survey 2015; Annual Population Survey 2015; HM Revenue and Customs FOI2026/85595. The share of work done without good English is each authority's 2015 mix of employees by industry, weighted by the national share of non-native English speakers in each industry who cannot speak English well. Authorities outside London are split at the median. The difference between the two groups is significant at the 10 per cent level with the Census measure of English and at the 5 per cent level with the school measure; excluding public administration, education and health, it is significant at the 1 per cent level. See the Technical Appendix, Section A.5.8.

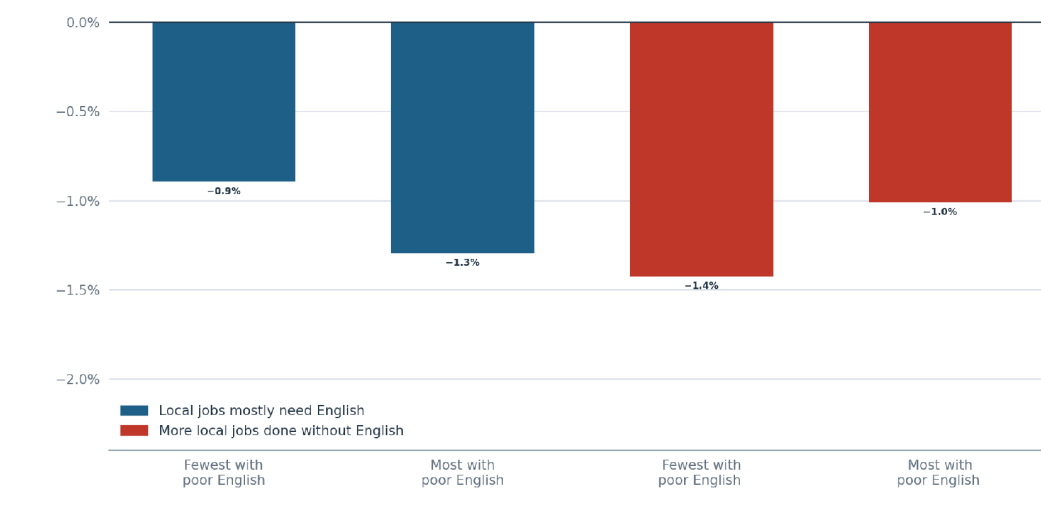
Splitting the authorities outside London at the middle of that measure, the effect of poor English can be estimated separately where local jobs mostly need English and where more of them do not.

The answer is that English does matter, but in opposite directions. Where local jobs mostly need English, the pay loss is larger where poor English is more common: around 1.3 per cent for each extra migrant job per 100 residents in authorities with the most residents with poor English, against 0.9 per cent in those with the fewest. Where more local work is done without English, the pattern reverses: 1.4 per cent where poor English is least common, 1.0 per cent where it is most common. Averaged across all authorities, the two cancel out, which is why the overall test finds no difference.

MIGRATION AND NATIVE PAY

Poor English matters in opposite ways depending on the local jobs

Change in British workers' median pay for each extra migrant job per 100 residents, outside London, by how common poor spoken English is locally and how much local work needs English.



Source: HMRC FOI2026/85595; ONS; authors' calculations.

Fewest and most: 10th and 90th percentiles of residents who cannot speak English well.

One reading is that where most work needs English, migrants who lack it are confined to the few jobs that do not, and compete harder for them, while where plenty of work can be done without English, those who cannot speak it well work in roles apart from British workers, and it is migrants with good English who compete with them most directly. The data cannot confirm that account. With the Census measure of English the difference between the two groups of authorities is significant only at the 10 per cent level, and so is suggestive on its own, but it is significant at the 5 per cent level with the school measure and more strongly when public-sector jobs, whose pay is set nationally, are left out. But each estimate within the groups is less precise, and the same pattern measured by occupation rather than industry points the same way without being statistically significant. It should be read as a finding worth testing with better data, not a settled result.²⁹

²⁹ Office for National Statistics, Census 2011, tables DC2604EW (proficiency in English by industry, employees, England) and DC2602EW (by occupation); Business Register and Employment Survey 2015; Annual Population Survey 2015; HM Revenue and Customs FOI2026/85595. The share of work done without good English is each authority's 2015 mix of employees by industry, weighted by the national share of non-native English speakers in each industry who cannot speak English well. Authorities outside London are split at the median. The difference between the two groups is significant at the 10 per cent level with the Census measure of English and at the 5 per cent level with the school measure; excluding public

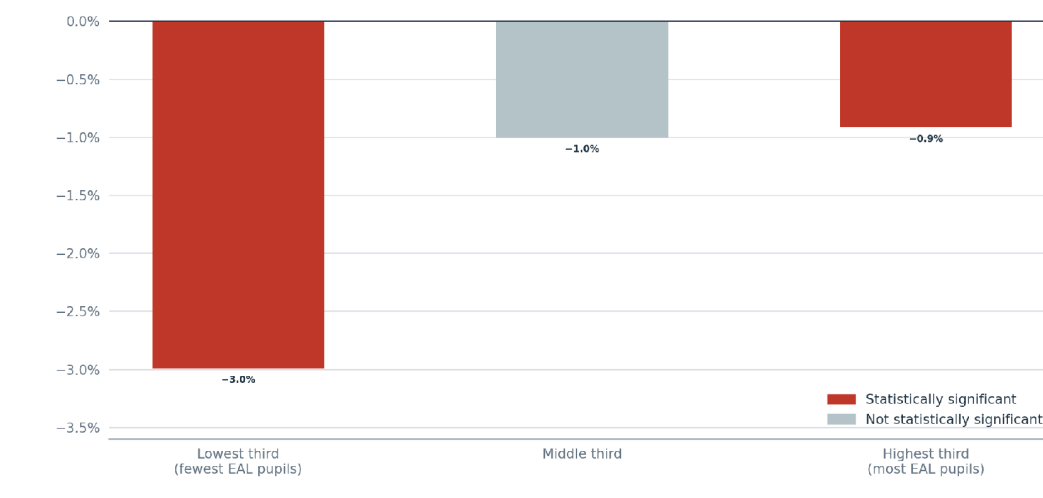
New destinations appear to bear more of the cost per migrant

What appears to make a difference is how established migration already was. Splitting the authorities outside London into thirds by their share of pupils with English as an additional language in 2015, a measure of how many migrant families had already settled there, the effect on British pay is largest in the third with the fewest. There, each extra migrant job per 100 residents reduced British median pay by 3.0 per cent, against 0.9 per cent in the third with the most.³⁰

MIGRATION AND NATIVE PAY

The pay loss is largest where few migrants had settled before

Change in British workers' median pay for each extra migrant job per 100 residents, outside London, authorities split into thirds by the 2015 share of pupils with English as an additional language (EAL).



Source: HMRC FOI2026/85595; Department for Education school census; authors' calculations.

95% ranges: lowest third -4.9% to -1.0%; middle third -3.0% to +1.0%; highest third -1.5% to -0.3%

This is the opposite of what the language argument predicts. The places with the fewest pupils learning in a second language, where poor English is least common, are the places where migration has cost British workers most per migrant. The pattern is consistent with authorities receiving migrants in numbers for the first time, whose labour markets have not yet adjusted to them, absorbing the inflow with a larger fall in pay than places with long-established migrant communities. The estimate for the lowest third is imprecise, running from about 1 to 5 per cent, and rests on a weak prediction of how much migrant employment rose in those authorities.

administration, education and health, it is significant at the 1 per cent level. See the Technical Appendix, Section A.5.8.

³⁰ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595 and the Department for Education school census; see the Technical Appendix, Section A.4.7. Total effects multiply each third's estimate by its average 2015 to 2025 rise in migrant employment. The within-authority estimates add the square of migrant employment to the model, instrumented with the square of the shift-share prediction. Changes in British payroll employment and self-employment (Annual Population Survey, all nationalities) and in occupations (Annual Population Survey, SOC major groups) are estimated by third and with a continuous interaction with 2015 migrant employment. The selection bound assumes every British employment lost was below the median and uses the gap between the 25th and 75th percentiles of annual pay (Annual Survey of Hours and Earnings, 2015).

A test that lets the effect vary smoothly with the 2015 share, rather than in thirds, finds no statistically significant gradient, so the pattern should be read with caution rather than as a precise measurement.

A smaller cost per migrant is not a smaller total cost

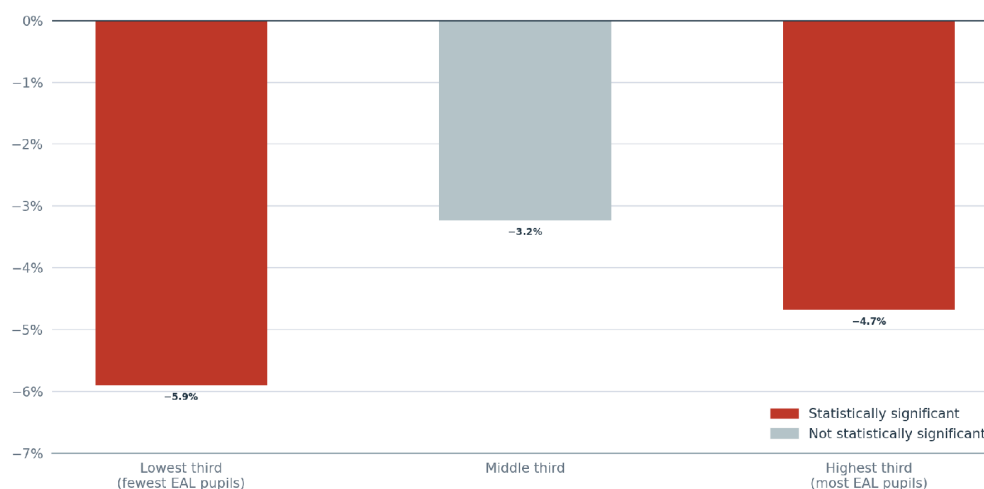
It might be tempting to read this pattern the other way round: if each extra migrant does less damage where migrants are already concentrated, perhaps migration becomes harmless, or even beneficial, once enough migrants have arrived. The evidence does not support this claim.

First, the smaller effect per migrant is offset by larger inflows. The authorities with the most settlement saw migrant employment rise by 5.1 jobs per 100 residents between 2015 and 2025, against 2.0 in those with the least. Multiplying each effect by the rise, the total loss to British median pay is 5.9 per cent in the third with the least settlement and 4.7 per cent in the third with the most. The cost per migrant differs, but the totals are not significantly different.

MIGRATION AND NATIVE PAY

But the total loss is similar everywhere

Estimated effect of the 2015 to 2025 rise in migrant employment on British workers' median pay, outside London, authorities split into thirds by the 2015 share of pupils with English as an additional language (EAL).



Source: HMRC FOI2026/85595; Department for Education school census; authors' calculations.

Average rise in migrant jobs per 100 residents: 2.0, 3.2 and 5.1.

Second, within the same place the damage done by each additional migrant does not shrink as migrants become more concentrated. Outside London, allowing the effect to change as migrant employment rises within each authority, each extra migrant job per 100 residents lowers British pay by about 0.8 per cent where there are 2 migrant jobs per 100 residents, 1.0 per cent at 11 and 1.2 per cent at 20. The differences are not statistically significant, but there is no sign of the effect fading. Reaching a high concentration of migrant employment means passing through every stage of the loss along the way.

Third, part of the smaller measured effect in established areas reflects who is left in payroll work rather than what they are paid. Where migrants were already most concentrated, each extra migrant job reduces British payroll employment by more, and self-employment

risers. If the British workers who leave payroll employment are mostly among the lower paid, the median of those who remain rises even though no one's pay has improved.

On a generous assumption that could account for up to about three-quarters of a percentage point of the effect, about a third of the gap between the authorities with the most settlement and those where migration is newest. Nor is there evidence that British workers in these areas move up into better-paid occupations.³¹

A lower cost per migrant in places with long-established migrant communities is therefore not evidence that admitting more migrants would make migration good for British workers. It reflects larger inflows, fewer British workers left in payroll employment, and a change in who is measured.

These results measure English proficiency across whole authorities, not the English of individual migrants, and the share of pupils with English as an additional language includes children born in Britain. They show that, on average, where poor English is common, migration does no more damage to British pay and employment than elsewhere. They do not show that English is unimportant: it matters for integration, for access to public services and for migrants' own earnings, none of which this report examines. Authorities where recent arrivals speak less English do have lower British employment, but that is an association between places, not an effect this design can identify.³²

For the question this report asks, however, the main answer is clear. On average, the cost of migration to British workers does not run through language. Where English does seem to matter, it depends on the local jobs and works in opposite directions, so a single national English requirement is a blunt tool for protecting British pay. Tighter English requirements may serve other purposes, but there is no evidence in these data that, applied across the country, they would protect the pay of natives.

³¹ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595 and the Department for Education school census; see the Technical Appendix, Section A.4.7. Total effects multiply each third's estimate by its average 2015 to 2025 rise in migrant employment. The within-authority estimates add the square of migrant employment to the model, instrumented with the square of the shift-share prediction. Changes in British payroll employment and self-employment (Annual Population Survey, all nationalities) and in occupations (Annual Population Survey, SOC major groups) are estimated by third and with a continuous interaction with 2015 migrant employment. The selection bound assumes every British employment lost was below the median and uses the gap between the 25th and 75th percentiles of annual pay (Annual Survey of Hours and Earnings, 2015).

³² Office for National Statistics, Census 2011, proficiency in English; Department for Education, school census; English for Speakers of Other Languages enrolments. The interaction models and the estimates by sample are set out in the Technical Appendix, Sections A.2.7, A.3.5 and A.4.4. The estimates by level of English come from a model that lets the effect vary with the 2011 Census measure, so they differ slightly from the headline estimate of 1.06 per cent.

What is Driving the Cost?

We do not know. The evidence shows that rising migrant employment has lowered British pay, but not how.

The estimates in this report compare local authorities with their neighbours as migrant employment rises in some faster than in others. Because the rise is predicted from where earlier migrants settled, rather than from how each local economy was doing, the comparison can separate the effect of migration from the pull of local growth. That is what allows the report to say that migration has caused British pay to fall, not merely that the two have moved together.

What this kind of analysis cannot do is look inside the local labour market. We cannot follow individual workers from job to job, or see which employers hire migrants and what they pay. The estimate tells us that the cost exists and how large it is, but it does not tell us the precise process that produces it.

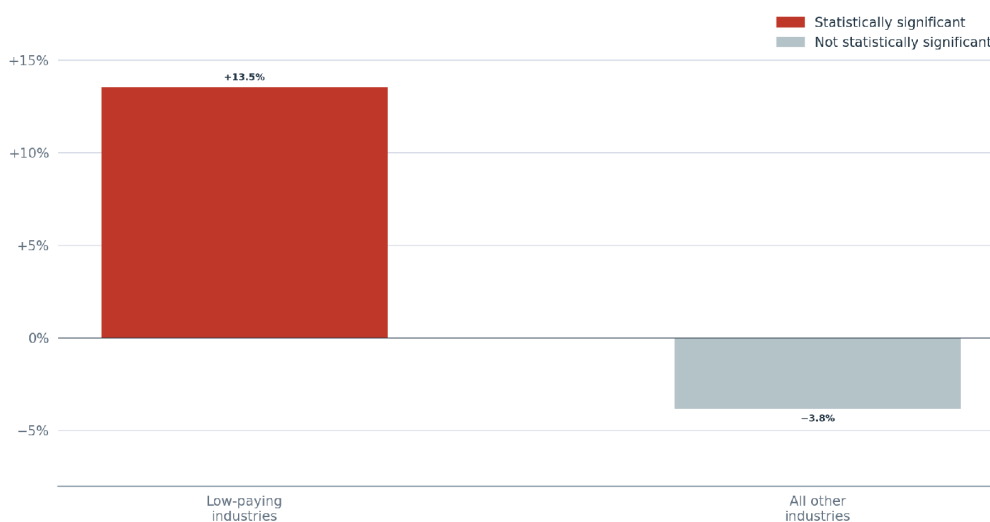
That matters for policy. If migrants lower pay because they compete directly for the same work, the answer lies in the numbers and kinds of workers admitted. If they lower pay because employers can pay less when workers find it hard to move jobs, the answer lies as much in the terms on which migrants are employed.

What the evidence rules out

MIGRATION AND NATIVE PAY

New jobs are concentrated in low-paying industries

Estimated change in the number of employees since 2015 due to the rise in migrant employment, outside London.



Source: ONS Business Register and Employment Survey; HMRC FOI2026/85595; authors' calculations.

Low paying industries as defined by the Low Pay Commission. Jobs counted where they are located, all nationalities.

A fall in the typical British worker's pay does not have to mean that anyone is paid less. The median can fall because the jobs in an area change, because different people are

counted, or because the same people take on extra, lower-paid work. Each of these was tested directly.³³

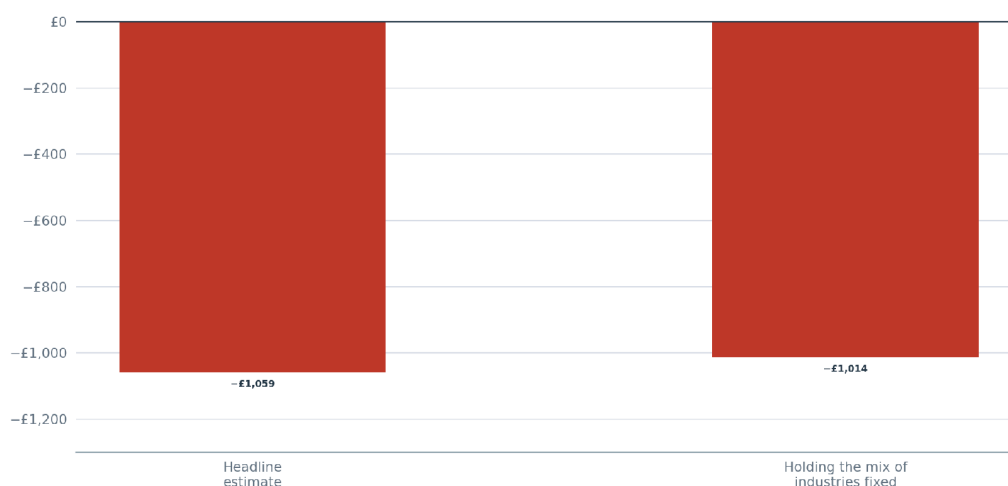
The most plausible of them is that migration changes the jobs themselves. Areas that receive more migrants do gain jobs in low-paying industries such as hospitality, retail, food processing and care. Outside London, the rise in migrant employment since 2015 has added around 14 per cent to employment in those industries, with no significant change elsewhere.

But the change in jobs explains little of the fall in pay. Holding each area's mix of industries fixed, the estimated loss for the typical British worker outside London falls only from about £1,060 to £1,010 a year. British workers are, for the most part, being paid less for similar work, rather than being moved into lower-paid industries.

MIGRATION AND NATIVE PAY

The change in jobs explains little of the pay loss

Estimated effect of the rise in migrant employment since 2015 on the median annual pay of British workers in 2025, outside London.



Source: HMRC FOI2026/85595; ONS Business Register and Employment Survey; authors' calculations.

95% ranges: headline -£1,410 to -£710; holding the industry mix fixed -£1,360 to -£670.

Two further explanations fail as well. If migration drew lower-qualified or younger people into work, the employed workforce would become less qualified and younger; instead, where migration rises it becomes more qualified and more prime-aged, which would push a median up, not down.

And if British workers were taking second jobs, the number of payroll jobs per British worker would rise; it does not. Together with the earlier findings that the loss comes mainly through lower hourly pay rather than fewer hours and is, on average, no larger where English is poor, the evidence narrows the question considerably.

³³ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595; Office for National Statistics, Business Register and Employment Survey (employees by two-digit SIC 2007 division, county and unitary geography as of April 2023) and Annual Population Survey (employment by qualification and age). Low-paying industries follow the Low Pay Commission's definition. The industry, workforce-composition and second-jobs tests are set out in the Technical Appendix, Section A.5.6.

What the evidence points towards

What remains is that the loss falls on hourly pay in the lower half of the distribution, but not at the very bottom, where the National Living Wage sets a floor. Migration adds jobs as well as displacing some British workers. The effect of each migrant appears largest in places where few migrants had settled before, although the total loss is similar everywhere, and where migrants are concentrated more British workers leave payroll employment. And areas receiving migrants lose residents, on net, to the rest of the country.

Economists have several explanations that fit this pattern. The two that matter most for policy are competition and employer power. If migrants compete with British workers for the same work, more workers for similar jobs lowers the wage those jobs pay. If migrants find it harder to change jobs, particularly those whose visa ties them to a sponsoring employer, employers can pay less, and the lower rate spreads to the British workers alongside them.

Competition or employer power?

The two explanations make different predictions, and some of those can be checked with published data. Each check below uses characteristics of local labour markets measured in 2015, before most of the rise in migrant employment that is present in our sample, and counted from employees, businesses or workers rather than residents, so that migration itself cannot move them.³⁴

The first check is what happens to migrants' own pay. Under competition it should fall too. It does, and by more. Outside London, the rise in migrant employment since 2015 has lowered the median pay of migrant workers by around 5.8 per cent, against 3.6 per cent for British workers. That fits workers who are close substitutes for one another. Part of the fall may simply reflect newer arrivals earning less than those already here, so this is supportive rather than decisive.

Employer power predicts a larger loss where a few large employers dominate, and a smaller one where unions are strong. The loss is no larger where more people work in workplaces with 250 or more employees, whether across all industries or in low-paying ones, and no smaller where private-sector union membership is higher. Nor did the effect of each additional migrant worker grow after 2021, when free movement ended and most new arrivals came on visas tied to a sponsor.

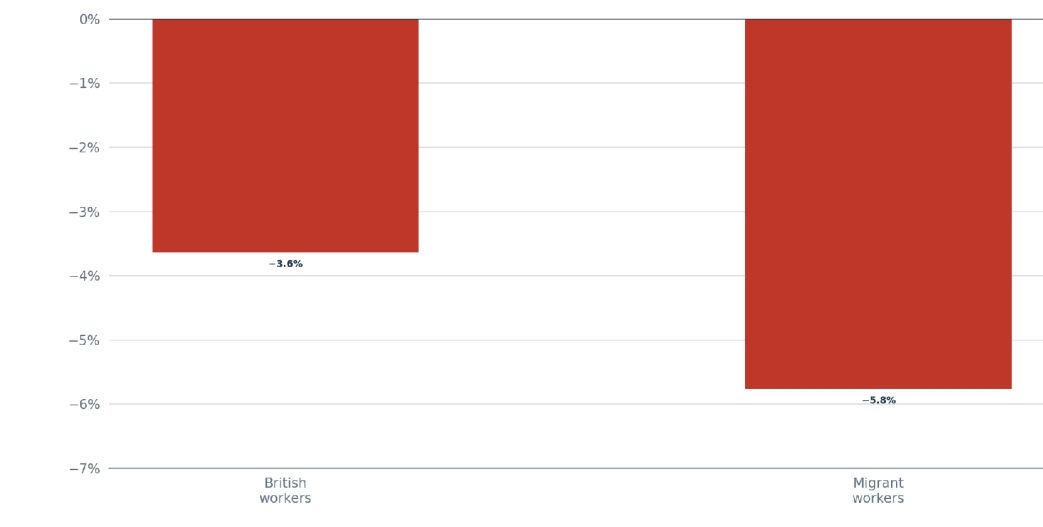
Competition predicts a larger loss where local jobs are concentrated in the industries and occupations that migrants enter. The estimates point that way for industries but not for occupations, and neither difference is statistically significant.

³⁴ Cambridge Circus Research calculations using HM Revenue and Customs FOI2026/85595 (median pay by nationality) and payrolled employments by nationality and industry; Office for National Statistics, UK Business Counts (local units by employment size band), Business Register and Employment Survey, Annual Survey of Hours and Earnings (Table 4.7a) and Annual Population Survey (employment by occupation); Census 2011 (occupation by year of arrival); and Department for Business and Trade, Trade Union Membership Statistics (density by industry). Employment in large workplaces is estimated from the midpoints of size bands. Exposure to migrant-intensive industries and occupations, and private-sector union membership, apply national 2015 rates by industry or occupation to each area's 2015 job mix. The public sector is public administration, education, and health and social work. The EU and non-EU inflows could not be compared directly because the EU-origin prediction is too weak outside London. The tests are set out in the Technical Appendix, Section A.5.7.

MIGRATION AND NATIVE PAY

Migrant workers' pay falls too

Estimated effect of the rise in migrant employment since 2015 on median annual pay, outside London.



Source: HMRC FOI2026/85595; authors' calculations.

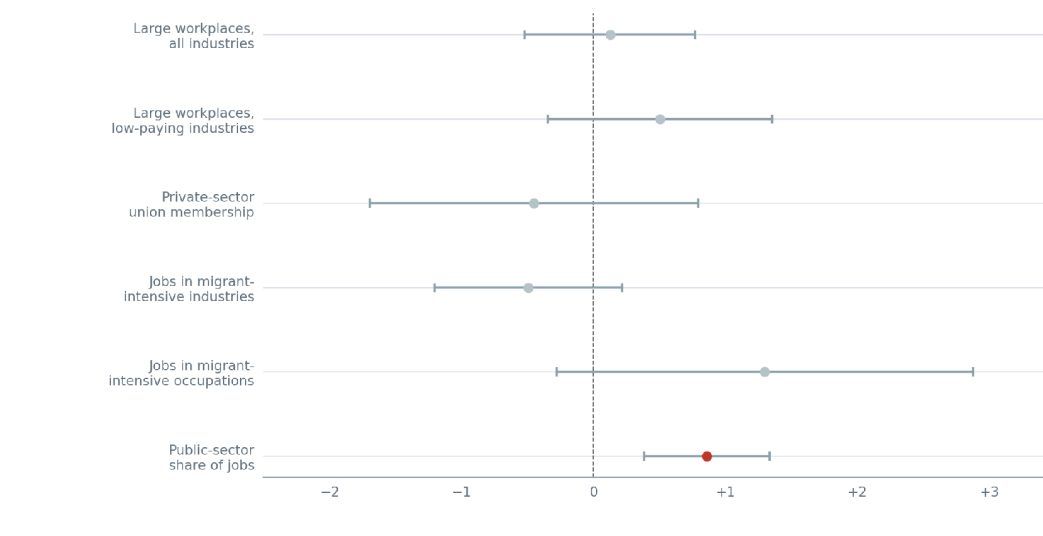
The migrant median also falls if newer arrivals earn less.

The loss is also smaller where more jobs are in the public sector, by close to a percentage point for an area one standard deviation above average. Public-sector pay is set nationally, so it cannot adjust to local conditions. That is what either explanation would predict, and it is a useful check that the estimate reflects local pay setting rather than something else in the data.

MIGRATION AND NATIVE PAY

Local labour-market structure makes little difference

Difference in the estimated pay loss since 2015 for an area one standard deviation above average on each characteristic, outside London (percentage points; positive means a smaller loss).



Source: HMRC FOI2026/85595; ONS; Census 2011; Department for Business and Trade; authors' calculations.

Average loss: -3.6%. Red: statistically significant. Lines show 95% ranges.

These checks are blunt. Workplace size is a rough stand-in for how few employers workers can choose between, and industry and occupation are measured nationally and applied to local job mixes. A check that finds nothing is weak evidence against an explanation, not proof that it is wrong. Taken together, the evidence leans slightly towards competition but does not settle the over-arching question of the precise causal mechanism.

What it would take to find out

Telling these explanations apart requires following workers and employers rather than places and building models in which the mechanism is written down and its predictions tested. Each explanation makes predictions that the others do not, and each can be tested with data that exists but is not published.³⁵

Competition for the same work predicts that British pay falls most in the occupations and skill levels that migrants enter, and that migrants' own pay falls too. The second holds in the area-level data; the first cannot be seen in them.

Testing it properly means estimating how closely British and migrant workers substitute for one another within occupations and skill groups, which requires worker-level records of pay, occupation and qualifications, such as the Annual Survey of Hours and Earnings linked to the Census.

Employer power predicts larger effects where a few employers dominate a local labour market, and where migrants are tied to a sponsoring employer, as on the Skilled Worker and Health and Care routes, rather than free to change jobs, as EU workers were before 2021. Testing means measuring how far each employer's pay falls short of what its workers could earn elsewhere, which requires payroll records that link workers, nationalities and employers, held by HM Revenue and Customs. Only then can the concentration of hiring be measured in the local labour market each worker faces, rather than approximated by workplace size.

Weaker bargaining and job creation predicts that vacancies and hiring rise with migration and that the pay of new recruits falls before the pay of existing staff, but testing this requires vacancy data by area and the pay of new hires by nationality.

Moving and housing predicts that British workers who leave high-migration areas keep their earnings, so that part of the local loss is spread across the country rather than lost. Testing here would require following individuals as they move, through longitudinal records, and a model of how workers, wages and housing costs adjust across areas.

³⁵ For the underlying approaches see, among others: on substitution between groups of workers, Card, D. (2001) 'Immigrant inflows, native outflows, and the local labor market impacts of higher immigration', *Journal of Labor Economics*, 19(1), pp. 22–64, and Ottaviano, G.I.P. and Peri, G. (2012) 'Rethinking the effect of immigration on wages', *Journal of the European Economic Association*, 10(1), pp. 152–197; on migration and employer power, Amior, M. and Manning, A. (2020) 'Monopsony and the wage effects of migration', CEP Discussion Paper 1690, Centre for Economic Performance, London School of Economics, since published in the *Economic Journal*, 136(674); on search and bargaining, Chassamboulli, A. and Palivos, T. (2014) 'A search-equilibrium approach to the effects of immigration on labor market outcomes', *International Economic Review*, 55(1), pp. 111–129; and on internal migration and local adjustment, Borjas, G.J. (2006) 'Native internal migration and the labor market impact of immigration', *Journal of Human Resources*, 41(2), pp. 221–258, and Monras, J. (2020) 'Immigration and wage dynamics: evidence from the Mexican peso crisis', *Journal of Political Economy*, 128(8), pp. 3017–3089.

None of this is out of reach. HMRC, the Office for National Statistics and the Department for Work and Pensions already hold the records, and researchers can apply to use them in secure settings. The findings in this report are a reason to do so.

Migration has lowered what British workers are paid. Understanding why is the next task, and it matters as much for policy as the size of the cost itself.

Conclusion

Migration has cost British workers more in pay than in jobs. The cost has fallen on the lower-paid, mainly through lower hourly pay rather than fewer hours. Policy should therefore narrow the routes into low-wage work, stop relying on language rules to protect wages, and publish the data that made these findings possible.

Until now the debate about migration and pay has been conducted largely without local evidence. The tax records obtained for this report change that, and they show that four in five new payrolled jobs since 2015 are held by non-UK nationals. Outside London, rising migrant employment has displaced some British workers, about four for every ten migrant jobs, but most of the adjustment has come elsewhere. Areas receiving migrants have lost residents on net, and pay has fallen behind.

The pay effect is our central finding. Outside London, the typical British worker earned about £1,060 less in 2025 than they would have done had migrant employment stayed at its 2015 level. The loss is concentrated in the lower half of the pay distribution, coming mainly through lower hourly pay rather than fewer hours. The obtained tax records and the earnings survey, measuring pay in different ways, point to an effect of the same size. Added together, the median-equivalent wage bill of British workers outside London was around £18 billion lower in 2025, and around £78 billion lower over the decade.

The cost is not where the public debate often places it. On average, it is no larger where poor English is most common. The cost per migrant may be largest where few migrants had settled before, but the total loss is similar everywhere, and the smaller cost per migrant in established areas partly reflects British workers leaving payroll employment. London is different again, its arrivals come disproportionately from higher-income countries, and there the cost appears to have been paid in housing rather than wages.

These findings have limits. They compare authorities with their neighbours, so they do not measure national effects on output, prices or the public finances, nor what migrants themselves gain. They estimate what British workers have forgone, not whether migration has been good or bad for the country. But they establish that the cost to British workers is real, that it can be measured, and that it falls on those with the least room to absorb it. That is enough to inform policy.

1. Restrict visa routes into low-wage work: The cost to British pay rises with the number of migrants, it falls hardest on hourly pay in the lower half of the distribution, and the new jobs that come with migration are concentrated in low-paying industries. The estimates therefore imply that reducing lower-paid migration would reduce the cost. The Government should keep sponsored routes into lower-paid occupations narrow and temporary. It should follow the MAC's recommendation to limit the Temporary Shortage List to a small number of occupations with firm end dates, and resist pressure to widen or extend it. Employers who have relied on these routes should be expected to raise productivity and pay, or train British workers, rather than depend on a continuing supply of new arrivals.
2. Target pay, not language, on work routes: This report tested whether migration does more damage to British pay and employment where English proficiency is poor. On average, it does not, and where English does seem to matter, it depends on the local jobs and works in opposite directions. What matters more

is the kind of work migrants do, not how well they speak English.

On average, these data give no support to raising the English requirement on work routes, as the Government did in January 2026, as a tool for protecting British wages, although they are area-level and pre-date the change. Salary thresholds are better suited, because they act directly on how much lower-paid migration a route admits, although this report does not test them directly. The Government should make robust salary thresholds the main control on economic routes, and review whether the higher English requirement on work routes adds cost to employers without protecting British workers. English requirements remain justified where the purpose is integration: on settlement, family and study routes.

3. Publish local payroll data by nationality: The findings in this report rest on data that HMRC holds but does not publish at local level: payrolled employment and median pay by nationality for each local authority. HMRC now publishes employment by nationality for regions, which is welcome but cannot show what is happening in individual towns and cities. That matters most for the towns and cities where migrant employment is rising fastest. HMRC should publish these series by local authority as official statistics, regularly and ideally monthly, alongside the weekly hours band it already receives through payroll. Voters, councils and Parliament could then see the effects of migration where they live, without waiting for a Freedom of Information request. Researchers should also be given secure access to payroll records linked to employers and visa routes, so that the mechanism behind the pay effect can be identified.

We are now at the point where we must accept that migration affects what British workers are paid, and the question has become whether policy will take that cost into account.

Technical Appendix A: Methodology

A.1 Introduction

This appendix outlines the data, estimation strategy, results, and robustness checks supporting our paper’s findings on the effect of local migrant employment on the pay and employment of UK-nationals. Our principal findings, established in sections A.4 and A.5, are as follows. Outside London, a one-percentage point increase in migrant employment rates reduces the median pay of UK nationals by 1.06%. Across England this pay estimate is smaller and not statistically distinguishable from zero – not because migration has zero cost on UK-nationals workers – but rather because London is an exception to the rule with its settlement networks having an opposite effect to those across the rest of Britain. Additionally, our paper finds no evidence of large displacement of UK-national workers from payrolled employment, with a modest negative effect outside London appearing only once industrial demand shocks are fixed. Our research also finds that a lack of English proficiency does not seem to be a significant variable in amplifying the effect of migration on the employment or salaries of UK-nationals. As well as this, we report that local authorities that receive migrants lose residents to other authorities on net, and that the rise in migrant employment since 2015 implies a median-equivalent UK-national wage bill outside London about £18.2 billion lower in 2025 than it would otherwise have been. Outside London the pay loss is concentrated in the lower half of the distribution and comes mainly through lower hourly pay rather than fewer hours.

A.2 Data and Variables

A.2.1 Employment and Pay by Nationality

The core data are drawn from a Freedom of Information (FOI) release by HM Revenue and Customs (FOI2026/85595), which provides monthly counts of payrolled employments and monthly median income of UK-national and non-UK national employees, by local authority of residence, from July 2014 to December 2025. The source systems are PAYE Real Time Information and the Migrant Worker Scan (MWS), which attaches the nationality recorded at NI number registration to each payrolled employment.

Because these counts are of employments and not employees, a person holding two payrolled jobs appears twice. As well as this, because the geography from this FOI request is residential, the data measure the labour-market outcomes of an authority’s residents wherever they work, which is the correct basis for a study of residents’ outcomes but should not be compared against or confused with a workplace-based series.

A.2.2 Origin-Group Inflows

The instrument described in Section A.3.2 is sourced from DWP’s StatXplore series of NI number registrations to adult overseas nationals, by nationality group, across local authority, and year. A registration counts an adult entering the tax or benefit system at their address of registration; fiscal years to March are mapped to the calendar year in which they begin. A finer version of the same series, by individual nationality (235 nationalities), is used in Section A.5.4. Cells with fewer than five registrations are suppressed in that version, so only the 29 nationalities observed in at least half of authority-years over the base window are used as separate series, with the remainder pooled.

A.2.3 Other Series

The remaining data used in this report are as follows: Resident population, all ages, is taken from the ONS mid-year estimates on NOMIS. The claimant count and the share of residents holding NVQ4 or RQF4 qualifications and above are from NOMIS and the Annual Population Survey. English-language proficiency is measured from the 2011 and 2021 Censuses (“cannot speak English well”), the Department for Education school-census share of pupils with English as an additional language, and ESOL enrolments; Section A.2.7 describes how these are combined. Percentiles of annual pay (Table 8.7a), hourly pay (Table 8.5a) and weekly paid hours (Table 8.9a) for all employees, all by place of residence, are from the Annual Survey of Hours and Earnings; ASHE Table 7.5a job counts are used for the job-growth placebo of Section A.5.3. Employees by SIC 2007 section for every district, 2015 to 2024, are from the Business Register and Employment Survey open-access release and are used to construct the industrial-demand control of Section A.2.6. Internal migration flows are the ONS estimates of moves between local authorities; inactivity counts, split by whether the inactive person wants a job, and counts of people in employment by nationality and of the self-employed are from the Annual Population Survey via the NOMIS API; and house prices are UK House Price Index annual means by district.

A.2.4 Sample Construction

The FOI is supplied on 2020-vintage geography. Districts are therefore summed into their two-tier counties so that the panel matches the geography on which covariates are available. Buckinghamshire maps to its county unit; Bournemouth, Dorset and Bournemouth–Christchurch–Poole are excluded because the 2019 reorganisation reconciles with neither geography; the Isles of Scilly are excluded; and the City of London is removed by a validity screen, having both a negative proficiency nowcast and a legacy migrant share, on our calculation, of 264.8%, neither of which is a possible value. The resulting panel comprises 26 shire counties (including Buckinghamshire), 53 unitary authorities, 36 metropolitan districts and 32 London boroughs, 147 authorities in all. County median pay is the employment-weighted mean of district medians, which is exact only for single-district units.

The estimation panel comprises 147 upper-tier authorities observed annually from 2015 to 2024, giving 1,470 observations in a balanced panel. The year 2025 is retained for descriptive purposes only, since the covariates end in 2024. Excluding the 32 London boroughs leaves 115 authorities and 1,150 observations; this sub-sample is used throughout, for reasons that Sections A.4.2 and A.5.2 make clear.

A.2.5 Dependent Variables and the Migrant Employment Rate

The principal dependent variable is the log of UK-national median annual pay. We measure employment in three ways: the log of UK-national payrolled employments; the level in thousands, which is the basis of the displacement ratio; and UK-national employment per hundred residents on a population base fixed at its 2015 value. The last of these is preferred to a rate on current population due to the instrument moving the population itself, so a current-population rate falls whether any native loses a job. The distributional analysis of Section A.4.6 uses the log of ASHE annual pay, hourly pay and weekly paid hours at each percentile from the tenth to the eightieth, for all employees.

The explanatory variable of interest, the migrant employment rate, is defined as:

$$M_{it} = 100 \times \frac{L_{it}^{nonUK}}{Pop_{it}}$$

Where L_{it}^{nonUK} denotes non-UK national payrolled employments in local authority i and year t , and Pop_{it} represents the resident population. The denominator is population rather than total employment, so that the regressor does not mechanically contain the dependent variable. The alternative, non-UK employments as a share of all employments, is tied by construction to any employment outcomes, and is therefore only used descriptively.

A.2.6 Control Variables

All reported models utilise four time-varying controls. The claimant count, as a percentage of the working-age population, to capture local labour-market slack. The share of residents holding NVQ4 qualifications or above captures the quality of local human capital. The nowcast share of residents who cannot speak English well, described in Section A.2.7, captures the linguistic composition of the local population. The fourth, an industry mix-shift-share, requires further explanation to demonstrate its value.

The concern an industry mix-shift share addresses is that migrant settlement is not at all random. Patterns of migrant settlement may very well be places whose industries happened to grow over 2015 and 2024, in which case the model instrument would attribute industrial growth to migration. The control therefore measures each authority's exposure to national industry trends:

$$B_{it} = \sum_{k=1}^{21} s_{ik,2015} \ln\left(\frac{E_{k,t}^{(-i)}}{E_{k,2015}^{(-i)}}\right)$$

Where $s_{ik,2015}$ denotes local authority i 's share of employees in SIC section k in 2015. $E_{k,t}^{(-i)}$ denotes national employees in section k in year t , leaving out local authority i . In other words, it is the growth in local employment that would be predicted by national sectoral trends alone. Two caveats are associated with it. The base year is the first panel year rather than a pre-panel year, because the BRES release used does not extend earlier; the shares are fixed and interact only with national trends, so the variable is close to a pre-determined exposure, but it is not clean pre-period. Furthermore, the national sectoral shifts are not purged of migration. For example, care and hospitality grew nationally from 2022 to 2024 partly because visa routes filled them, so the control absorbs any effect of migration that operates through national sectoral growth, and the coefficient on migrant employment is the effect of local migrant employment over and above that channel. Section A.5.4 reports every headline estimate with and without it.

Job density is deliberately not used as a control variable as it represents local labour demand realised within each local authority, which is one of the channels through which migration would act and therefore conditioning on it would absorb part of the effect. Adding it anyway leaves our headline findings unchanged, at -1.05 against -1.06 . Its 2015 value is used only as a pre-period characteristic in the heterogeneity analysis of Section A.4.7.

A.2.7 English-Language Proficiency Measures

Three measures of proficiency are used. The primary measure for the interaction models is the 2011 Census share of residents who cannot speak English well, held fixed. The second is the 2015 share of pupils with English as an additional language, also held fixed.

The third is a time-varying nowcast: a two-period fixed-effects fit of Census proficiency on ESOL enrolments and the EAL pupil share, anchored to the Census in 2011 and 2021.

Within a local authority, the nowcast is essentially the EAL series (correlation 0.915), and the fit against the Census observations, is in-sample (R^2 0.99). The nowcast serves as a control in every model.

A.3 Methodology

A.3.1 Estimating Equation

Our estimation strategy is a Two-Way Fixed Effects panel model estimated using two-stage least squares (2SLS). The equation is:

$$y_{it} = \beta M_{it} + X_{it}\gamma + \alpha_i + \delta_{r(i)t} + \varepsilon_{it}$$

Where:

- y_{it} denotes the outcome for UK-national workers in resident authority i in year t
- M_{it} is the migrant employment rate constructed in Section A.2.5
- X_{it} is the vector of four controls from Section A.2.6
- α_i denotes the local authority fixed effect
- $\delta_{r(i)t}$ denotes the region-by-year fixed effect over the nine ONS regions
- ε_{it} represents the error term.

The local authority effects absorb time-invariant characteristics of each LA for instance: its industrial history, housing stock, and distance from London. The region-by-year effects absorb all factors that affected a whole region in a given year such as a recession, pandemic, regional pay trends, and any national policy whose incidence was regional. What remains is to identify β 's variation within an authority over time, relative to other authorities in its region in the same year. The coefficient of interest is therefore the change in the outcome when an authority's migrant employment rises by one percentage point faster than its regional neighbours'.

A.3.2 The Endogeneity Problem and Solutions

Because migrants settle where labour demand is strong, an Ordinary Least Squares (OLS) estimator would not recover a causal effect. Since M_{it} counts migrants in work, the demand-pull channel is a direct local demand shock raising migrant employment and native outcomes together, therefore biasing the OLS coefficient toward zero or upward on pay. Reverse channels, migrants entering areas that natives are leaving, bias it the other way.

Our instrument follows the shift-share design of Card (2001) and Dustmann, Frattini and Preston (2013). It predicts each authority's migrant inflow from two components including historic settlement networks, and the national inflow of each origin group in each year.

$$Z_{it} = \frac{100}{Pop_{it}} \sum_{g=1}^{15} s_{ig} F_{g,t}^{(-i)}$$

$$s_{ig} = \frac{\sum_{\tau=2002}^{2010} R_{ig\tau}}{\sum_{j=2002}^{2010} \sum_{j \neq i} R_{jg\tau}}$$

$$F_{g,t}^{(-i)} = \sum_{j \neq i} R_{jgt}$$

Where R denotes NI registrations by nationals of origin group g in local authority i in year t . s represents the local authority i 's share of all group g registrations over the base window 2002/3 to 2010/11, which closes before the panel opens. $F^{(-i)}$ represents the national inflow of group g in year t , excluding local authority i 's own registrations so that no own contribution remains. The fifteen groups are EU14, EU8, EU2, other EU/EEA, non-EU Europe, the Middle East and Central Asia, East Asia, South Asia, South-East Asia, Sub-Saharan Africa, North Africa, North America, Central and South America, Oceania, and other or unknown.

The identifying variation is the interaction of pre-determined settlement maps with origin-specific national shocks whose timing was set by policy and events abroad: the EU8 accession wave (294,000 national registrations in 2007/8, 7,000 in 2023/4); the 2014 opening of the labour market to EU2 nationals (65,000 in 2013/14 rising to 193,000 in 2014/15); the collapse of EU inflows after the referendum; the Health and Care visa and student surge of 2022 to 2023 (South Asia from 59,000 in 2014/15 to 404,000 in 2023/4, Sub-Saharan Africa from 30,000 to 216,000); the Ukrainian scheme (non-EU Europe from 31,000 in 2019/20 to 146,000 in 2022/3); and the Hong Kong BNO route (East Asia from 24,000 in 2019/20 to 66,000 in 2022/3). An authority with a large Polish network in 2005 saw its predicted inflow fall after 2016; one with a large South Asian network saw it jump in 2022. Neither movement was caused by conditions in that authority. All 147 authorities match to the NINO geography and hold between 78% and 89% of national base-period registrations in every group. The instrument correlates 0.92 with actual local registrations and 0.85 with the PAYE migrant employment rate.

A.3.3 Two-Stage Least Squares (2SLS) Estimation

The first stage regresses the migrant employment rate on the instrument, controls, and fixed effects:

$$M_{it} = \pi Z_{it} + X_{it} \theta + \alpha_i + \delta_{rt} + v_{it}$$

The second stage estimates the structural equation with the fitted value $\hat{M}_{it}$ in place of M_{it} .

For the interaction models of Section A.3.5 the endogenous vector is $(M_{it}, M_{it} \times \tilde{E}_i)$, estimated jointly.

Standard errors are cluster-robust by local authority. For the two-stage estimator the covariance is:

$$\hat{V}(\hat{\beta}) = (\hat{X}'\hat{X})^{-1} \left[\sum_{g=1}^G \hat{X}'_g \hat{u}_g \hat{u}'_g \hat{X}_g \right] (\hat{X}'\hat{X})^{-1} \cdot \frac{G}{G-1} \cdot \frac{n-1}{n-k}, \quad \hat{u} = y - \hat{X}\hat{\beta}$$

Where g indexes the $G = 147$ local authority clusters and $\hat{X} = P_Z X$. The residual is formed from the actual regressors and not the fitted values. Estimation therefore uses an Instrumental Variable (IV) routine. Throughout, estimates significant at the 5% level are treated as findings, and estimates significant only at the 10% level as suggestive.

A.3.4 Instrumental Variables (IV)

Instrumental variables estimates are causal if two conditions are held. The first, relevance, is that the instrument moves the endogenous regressor: predicted inflows must actually track migrant employment. The second, exclusion, is that conditional on the authority and region-year effects and the controls, base-period settlement networks affect native outcomes in 2015 to 2024 only through the inflows they route.

The relevance criterion is fully testable and is established in Section A.4.1. Exclusion is not testable in full, but it has implications that are. An instrument that moves the outcome while not moving the regressor at all cannot be reaching the outcome through migration (see Section A.5.1). If the settlement networks are exogenous, the panel-period predicted inflow, which cannot have caused anything before 2015, should not predict outcome growth before the panel began (Section A.5.3).

If the networks proxy for industrial growth in the same places, holding industrial demand fixed should move the estimate (Section A.5.4). And if the separate origin groups are all valid instruments for a single effect, they should agree with one another (Section A.5.2). An estimate that passes all four probes has survived every falsification test the data allow; passing them is necessary for a causal reading, not sufficient.

A.3.5 The Proficiency Interaction Model

The hypothesis that the effects of migration are amplified where poor English proficiency is concentrated is tested by the following equation:

$$y_{it} = \beta_1 M_{it} + \beta_3 (M_{it} \times \tilde{E}_i) + X_{it}\gamma + \alpha_i + \delta_{rt} + \varepsilon_{it}$$

Where $\tilde{E}_i = E_i - \bar{E}$ denotes a pre-determined, time-invariant proficiency measure centered at its cross-authority mean. Given that the level of E_i is absorbed by the local authority effect; only the interaction is estimated and is not included in the specification for brevity, with $Z_{it} \times \tilde{E}_i$ as its instrument. The friction hypothesis predicts a negative effect of the interaction term on pay and employment.

A.3.6 Heterogeneity, Distribution, and Aggregation

Three further sets of models apply the same design to different variables. The distributional analysis detailed within Section A.4.6 estimates the structural equation separately for the log of ASHE annual pay, hourly pay and weekly paid hours at each

percentile, and reports the PAYE native median alongside a benchmark that is free of the composition effect that arises because ASHE percentiles include migrant workers. The heterogeneity analysis of Section A.4.7 estimates the equation separately within subsets of authorities defined by pre-period characteristics, and estimates a single interacted model in which several characteristics compete:

$$y_{it} = \beta_1 M_{it} + \sum_k \beta_k (M_{it} \times \tilde{D}_{k,i}) + X_{it} \gamma + \alpha_i + \delta_{rt} + \varepsilon_{it}$$

The aggregate impact accounting shown in Section A.4.9 takes 100,000 draws of the pay coefficient from its clustered sampling distribution, applies each draw to every authority's observed 2015 to 2025 rise in migrant employment and its 2025 native wage bill, and sums:

$$\Delta W^{(s)} = \sum_i \beta^{(s)} (M_{i,2025} - M_{i,2015}) L_{i,2025}^{UK} w_{i,2025}^{UK}$$

Where $L_{i,2025}^{UK} w_{i,2025}^{UK}$ denotes the local authority's 2025 native wage bill. The share of draws below zero is reported as the probability of a cost, and the 2.5th and 97.5th percentiles as the interval. The expression is linear in the coefficient, and it is the one used for every reported figure. Converting the log effect into levels instead, by multiplying each authority's bill by $1 - \exp(-\beta \Delta M)$, would give a figure about 2% larger (£18.6bn rather than £18.2bn outside London in 2025). Because the bill is the number of UK-national payrolled employments multiplied by median pay, the result is a median-equivalent wage bill rather than total pay.

A.4 Results

A.4.1 First Stage

Table A.1 presents the first-stage relationship between the instrument and migrant employment rate, together with the instrument-strength statistic and the Anderson-Rubin test on the native employment rate, measured on a population base fixed at 2015. The single-series shift-share of the earlier literature, which interacts one 2001 settlement share with one national inflow series, is shown for comparison.

Table A.1: First Stage Estimates and Instrument Strength

Instrument set	Endogenous regressor	$\hat{\pi}$	SW F	AR F on native employment rate (fixed 2015 base)	N
Single-series 2001 shift-share (A.5.1)	M_{it}	-0.127 (0.191)	0.44	5.48**	1,470
Origin shift-share Z	M_{it}	+0.593*** (0.09)	43.9	2.48	1,470

Origin shift-share Z , excl. London	M_{it}	+0.806*** (0.118)	47	7.17***	1,150
$Z, Z \times \tilde{E}$ ($\tilde{E}$ = Census 2011)	$M_{it}, M_{it} \times \tilde{E}_i$		48.8, 28.6		1,450
15 group-specific Z_g	M_{it}	—	18.6	9.10***	1,470

Standard errors in parentheses beneath each estimate, clustered by local authority.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.

The origin-group instrument predicts migrant employment strongly. In the full sample a one-point rise in the predicted inflow raises the migrant employment rate by 0.59 points, significant at the 1% level, and the Sanderson–Windmeijer F-statistic of 43.9 is far above the threshold of 10 below which weak-instrument bias becomes a concern. Outside London the coefficient is larger, at 0.81, and the F-statistic is 47.0. The interaction instrument used in the proficiency models has adequate strength on both endogenous regressors (Sanderson–Windmeijer F of 48.8 and 28.6), and the fifteen group-specific instruments taken together have an F of 18.6.

The comparison with the single-series instrument is instructive. Its first-stage coefficient is negative and insignificant, and its F-statistic of 0.44 means that it explains essentially none of the variation in migrant employment. Yet its Anderson–Rubin statistic on the native employment rate, on a population base fixed at 2015, is 5.5, significant at 5%: the instrument moves the outcome while not moving the regressor it is meant to move. As Section A.5.1 discusses, a reduced form without a first stage is the signature of an instrument that reaches the outcome by some route other than migration, and it is the reason the single-series form is not used.

The origin-group instrument, by contrast, has a strong first stage in both samples.

A.4.2 Native Pay

Table A.2 presents the central result of the appendix: the effect of the migrant employment rate on the log of UK-national median annual pay, for the full sample and for England excluding London, under OLS and under the instrument. The last two columns report the share of Monte Carlo draws below zero and the 95% interval of the draws, from the simulation described in Section A.3.6.

Table A.2: Native Pay Model – log UK-national median annual pay

Sample	Estimator	$\hat{\beta}$	SW F	AR F	P ($\beta < 0$)	95% CI
Full, 147 LAs	OLS TWFE	−0.0016 (0.0012)	—	—		
Full	IV	−0.0035 (0.0027)	43.9	1.35	0.91	[−0.0087, +0.0016]

Excl. London, 115 LAs OLS TWFE -0.0026** — —
(0.0013)

Excl. London IV -0.0106*** 47 45.7*** 1 [-0.0141, -0.0071]
(0.0018)

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

Outside London the instrumented estimate is -0.0106, significant at the 1% level: a one-percentage-point rise in the migrant employment rate lowers UK-national median pay by 1.06%. The Anderson–Rubin statistic of 45.7 confirms that this is not a weak-instrument artefact, and every one of the 100,000 draws from the estimate’s sampling distribution lies below zero.

Applied to the mean rise in migrant employment outside London between 2015 and 2025, 3.4 points, the estimate implies UK-national median pay about 3.6% lower in 2025 than it would otherwise have been, or about £1,060 a year for a worker at the median (95% interval £710 to £1,410).

The OLS association outside London is -0.26%, a quarter of the instrumented effect. That the IV estimate exceeds OLS in magnitude is what the demand-pull thesis predicts whereby migrants go where pay is rising, so the raw association understates the causal effect. Our IV model is therefore the preferred approach.

Across England the picture is different. The instrumented estimate is -0.35%, not significant at conventional levels, and the Anderson–Rubin test does not reject zero. The interval runs from -0.87% to +0.16%. The full-sample estimate is not an attenuated version of the ex-London one; it is an average over inflows that have opposite effects. Section A.5.2 shows that the origin groups whose settlement networks are concentrated in London return positive just-identified estimates while the remainder return negative ones, and Section A.4.8 identifies the composition of those inflows as the reason. The paper’s headline is therefore the ex-London estimate, and the full-sample figure is reported as not established rather than as small.

A.4.3 Native Employment

Table A.3 presents the employment models for the full sample. Four measures are reported: the log of UK-national payrolled employments; the level in thousands; the employment rate on a population base fixed at 2015; and, for the reason explained below, the rate on current population.

Table A.3: Native Employment Models, full sample

Outcome	OLS TWFE	IV $\hat{\beta}$	AR F
log UK-national employments	-0.0099*** (0.0018)	-0.0058 (0.0048)	1.03
UK-national employments (000)	-1.399***	-0.927*	2.05

	(0.24)	(0.529)	
UK employment rate, fixed 2015 denominator	-0.348***	-0.272*	2.48
	(0.062)	(0.139)	
UK employment rate, current denominator	-0.579***	-0.621***	12.0***
	(0.058)	(0.128)	
log resident population (×100)	+0.83***	1.33***	18.0***
	(0.12)	(0.31)	

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

Under OLS every measure returns a negative and highly significant association: authorities where migrant employment rose saw UK-national employment fall relative to their region. Under the instrument this association largely disappears. The log estimate is -0.58% per point with a standard error of 0.48, and the Anderson–Rubin statistic of 1.03 does not reject zero.

The level estimate, -0.93 thousand employments per point, is significant only at the 10% level and again fails the Anderson–Rubin test, as does the fixed-denominator rate. The IV estimates being smaller than OLS on employment says that most of the raw association is not caused by migration.

The current-population rate is the exception, with an instrumented estimate of -0.62 points and an Anderson–Rubin statistic of 12.0. This result is arithmetic. Writing the rate as $r_{it} = L_{it}^{UK} / Pop_{it}$, we can see that the instrument moves the denominator directly, because the migrants it predicts are residents. The instrumented effect of one point of migrant employment on log population in the last row of Table A.3 is +1.33%, significant at the 1% level with an Anderson–Rubin statistic of 18.0. This means that, with native unemployment held constant, a native employment rate of 37.7% would fall mechanically by $0.377 \times 1.33 = 0.50$ points, against an estimated fall of 0.62 shown in the current denominator model. Fixing the denominator at its 2015 value therefore removes most of this effect (-0.27, standard error 0.14). The current population rate is therefore not a measure of displacement and is not reported as one.

Outside London the employment results are somewhat stronger. The log estimate is -0.75% per point and is significant at 5% with an Anderson–Rubin statistic of 4.10, and the fixed-denominator rate is -0.36 points (standard error 0.13) with a statistic of 7.17. Section A.5.4 shows that this effect appears only once industrial demand is held fixed: without the industry control the ex-London log estimate is -0.58 and insignificant. The interpretation offered there is that migrant inflows and industrial growth had offset each other in the earlier specification. Expressed as a displacement ratio, the number of UK-national employments lost for each additional non-UK employment, the level estimates, -0.93 thousand employments per point (standard error 0.53) across England and -1.62 thousand (0.78) outside London, divided by the thousand migrant employments that one point of the rate represents in the average authority (3.76 and 4.03), imply 0.25 (standard error 0.14) across England and 0.40 (0.19) outside London, where the 95% interval runs from 0.02 to 0.78: some displacement, but well short of one for one. Outside London the

level estimate is significant at 5%, but its Anderson–Rubin statistic, 3.13, is significant only at 10%, so the ratio is best read as suggestive.

A.4.4 English-Language Proficiency

Table A.4 presents the interaction models detailed in Section A.3.5, instrumented jointly, for each of the three proficiency measures and for both log employment and log pay. The coefficient β_1 is the effect of migrant employment at an authority of average proficiency whereas β_3 is the change in that effect per percentage point of the proficiency measure (the interaction term of interest).

Table A.4: English-Proficiency Interaction IV Models

E_i	Outcome	$\hat{\beta}_1$	$\hat{\beta}_3$	SW F on M, on M × E	N
Census 2011, fixed (primary)	log employment	−1.45*** (0.46)	+0.328* (0.183)	48.8, 28.6	1,450
Census 2011, fixed (primary)	log pay	−0.56** (0.27)	0.08 (0.087)	48.8, 28.6	1,450
EAL 2015, fixed	log employment	−1.78*** (0.45)	+0.042* (0.024)	41.2, 30.7	1,470
EAL 2015, fixed	log pay	−0.67*** (0.23)	0.011 (0.008)	41.2, 30.7	1,470
Nowcast, time-varying	log employment	−1.22*** (0.44)	0.186 (0.144)	35.5, 17.4	1,470
Nowcast, time-varying	log pay	−0.48 (0.34)	0.039 (0.083)	35.5, 17.4	1,470

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

The proficiency measures are in percentage points of their own scales, so the interaction coefficients are not comparable across rows; the fixed-measure rows keep the time-varying nowcast as a control, and the Census sample loses two authorities without a 2011 observation.

The friction hypothesis predicts negative interactions. Every one of the six is positive. None is significant at the 5% level; the two employment interactions on the fixed measures reach the 10% level, both in the direction of a smaller adverse effect where proficiency is worse. The instruments on the interaction terms are strong, with Sanderson–Windmeijer statistics between 17.4 and 30.7, so this is not a null produced by weak instruments. The hypothesis with which the project began is therefore rejected: the effect of migration on native pay and employment is not larger where poor English is concentrated. If anything, Section A.4.7 shows the reverse.

A natural objection is that the positive sign is London: 28 of the 49 authorities in the top third of the EAL distribution are London boroughs, so an interaction with proficiency may be an interaction with the capital. Re-estimating the model by sample separates the two. On pay, the interaction is null everywhere once London is treated on its own: +0.075 (0.061) on the Census measure and +0.005 (0.008) on EAL outside London, and –0.080 (0.108) and –0.013 (0.007) within London; in a full-sample model that adds a London interaction, the proficiency term falls to +0.001 (0.070) while the London term takes +1.34 (0.59), significant at 5%. The positive pay interaction was London. On employment the picture is different: outside London the interaction is null (+0.037 (0.104) and +0.008 (0.014)), but within London it is positive at the 10% level, which we treat as suggestive (+0.42 (0.21) on the Census measure, +0.072 (0.036) on EAL; $p = 0.06$ and 0.05 against a t distribution with 31 degrees of freedom, since the London sample has only 32 clusters), and it survives the addition of a London interaction. The weaker employment effect in lower-proficiency areas is a suggestive pattern across London boroughs, not an artefact of London versus the rest.

Either way the conclusion stands: outside London the effect of migration is no larger where proficiency is poor, and inside London it is, if anything, smaller.

Table A.4b reports the implied effect outside London at low, typical and high proficiency, from the same interaction models estimated on the authorities outside London; these are the estimates shown in the main text. The effect on pay is significant at every level of proficiency and slightly smaller where proficiency is poorer. The effect on employment is significant at 5% where proficiency is poorest and is suggestive elsewhere.

Table A.4b: Effect of Migrant Employment by Level of English Proficiency, England excluding London (IV, % per pp)

Measure and outcome	10th percentile	Median	90th percentile
Census 2011, log pay	–1.32*** (0.28)	–1.29*** (0.26)	–1.14*** (0.19)
Census 2011, log employment	–0.89* (0.51)	–0.87* (0.48)	–0.80** (0.37)
EAL 2015, log pay	–1.24*** (0.34)	–1.22*** (0.31)	–1.13*** (0.21)
EAL 2015, log employment	–1.04* (0.54)	–1.00** (0.48)	–0.86** (0.35)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Effect of a one-point rise in the migrant employment rate evaluated at the 10th, 50th and 90th percentiles of each proficiency measure across authorities outside London (Census 2011: 0.28%, 0.73% and 2.63%, 113 authorities; EAL 2015: 2.3%, 7.4% and 25.8%, 115 authorities).*

The level of proficiency, entered as a control, is negatively associated with native employment on every measure (ESOL –0.129, standard error 0.052; the first principal component of ESOL and EAL –0.037, standard error 0.017) and not with pay. Where recent inflows are less proficient, native employment is lower.

A.4.5 Internal Migration, Inactivity and House Prices

Table A.5 applies our model design to three outcomes that bear on how a local labour market absorbs migrant inflows: whether existing residents move away, whether they withdraw from the labour force, and whether the inflow is capitalised into housing.

The flows count all residents, natives and migrants alike, and the OLS association for net internal migration is -0.197 (standard error 0.032) in the full sample and -0.218 (0.038) outside London. One result in this table is identified as firmly as the pay effect. A one-point rise in the migrant employment rate lowers net internal migration by 0.23 points of population in the full sample and 0.35 points outside London, both significant at 1% with Anderson–Rubin statistics above 9. Authorities that receive migrants lose residents, on net, to other authorities. The gross in- and out-flows are each too imprecisely estimated to say whether fewer residents arrive or more leave.

Measured on a population base aged 16 to 64, economic inactivity outside London falls by 0.81 points per point of migrant employment (standard error 0.50 , $p = 0.10$), just short of significance at the 10% level. Any fall is in residents who do not want a job (-0.92 , standard error 0.51 , significant at 10% and therefore suggestive), the pattern produced when arrivals who are mostly in work join the working-age population in the denominator; the involuntary component, residents who are inactive but want work, is unmoved in both samples.

Suffolk and Somerset are excluded from the inactivity estimates because their survey geography does not match the panel. There is thus no sign of natives being pushed out of the labour force. House prices do not respond to the pooled instrument in either sample; the house-price response found in Section A.4.8 is specific to inflows from higher-income origins into London.

Table A.5: Internal Migration, Inactivity and House Prices (IV)

Outcome, per pp of migrant employment rate	Full sample, 147 LAs	Excl. London, 115 LAs
Net internal migration, in minus out (pp of population)	-0.230^{***} (0.053)	-0.349^{***} (0.076)
AR F	9.50^{***}	10.97^{***}
Internal in-migration rate (% of population)	-0.28 (0.27)	-0.51 (0.54)
Internal out-migration rate (% of population)	-0.05 (0.25)	-0.16 (0.50)
Economic inactivity rate (% of population aged 16–64)	-0.19 (0.30)	-0.81 (0.50)
Inactive and wants a job (% of population aged 16–64)	0.12 (0.16)	0.15 (0.14)
Inactive and does not want a job (% of population aged 16–64)	-0.19 (0.32)	-0.92^* (0.51)
log mean house price ($\times 100$)	0.8 (0.52)	0.11 (0.79)

Standard errors in parentheses beneath each estimate, clustered by local authority.
 * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.

A.4.6 Distribution of the Pay Effect

Table A.6 presents the effect at each percentile of three ASHE distributions, annual pay, hourly pay and weekly paid hours, all measured by place of residence, for England excluding London, with the PAYE native median as a benchmark. ASHE percentiles include migrant workers, so a low percentile can fall by composition, as migrants enter at the bottom of the distribution, without any incumbent's wage changing; the PAYE native median is free of that effect and is reported alongside. Figure A.1 plots the estimates.

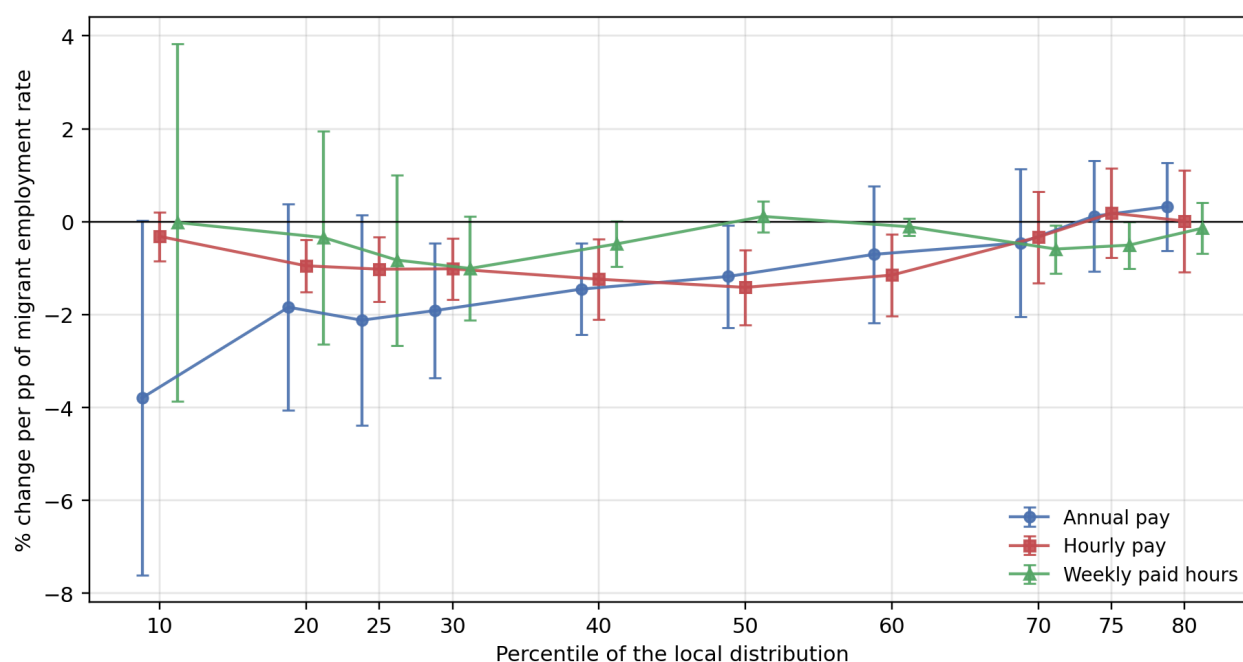


Figure A.1. Instrumented effect of a one-point rise in the migrant employment rate on ASHE percentiles of annual pay, hourly pay and weekly paid hours, England excluding London, all employees by place of residence, with 95% intervals.

The effect is concentrated in the bottom half of the distribution and is absent above the 75th percentile. In annual pay the estimates from the 30th to the 50th percentiles are between -1.2% and -1.9% per point and significant at 5% or better; at the 10th and 25th percentiles they are larger but significant only at 10%, and therefore suggestive; above the 60th they are small and insignificant. Hourly pay, measured on the same place-of-residence basis, falls significantly from the 20th to the 60th percentile, by between 0.95% and 1.41% per point, and at the median by at least as much as annual pay.

Weekly paid hours fall by less. Below the 70th percentile the only movements are at the 30th and 40th percentiles (-1.01 and -0.48 , significant at 10% and therefore suggestive), and the falls at the 70th and 75th percentiles, significant at 5%, are small; the mean is unchanged (-0.40 , standard error 0.36) and the part-time share of employee jobs does not rise (-0.06 points, standard error 0.35). Outside London the main margin of adjustment is therefore the hourly rate, although at the 30th percentile, where the hours estimate is about half the annual pay estimate, working time may contribute as well. At the 10th

percentile neither hourly pay nor hours moves, consistent with a binding National Living Wage; the marginal fall in annual pay there may reflect weeks worked over the year, which the hours table does not measure. The ASHE all-worker median falls by 1.2% and the PAYE native median by 1.1%, so composition does not drive the result at the middle of the distribution. It may contribute at the 10th percentile, where no native benchmark exists, and part of the fall in hourly pay may reflect new arrivals starting on lower rates.

Table A.6: Effect on Annual Pay, Hourly Pay and Weekly Paid Hours by Percentile, England excluding London (% per pp)

Percentile	Annual pay	Hourly pay	Weekly paid hours
P10	-3.77* (1.96)	-0.32 (0.27)	-0.02 (1.96)
P20	-1.84 (1.13)	-0.95*** (0.29)	-0.34 (1.17)
P25	-2.12* (1.15)	-1.02*** (0.35)	-0.83 (0.94)
P30	-1.91*** (0.74)	-1.01*** (0.34)	-1.01* (0.57)
P40	-1.45*** (0.5)	-1.24*** (0.44)	-0.48* (0.25)
P50	-1.18** (0.56)	-1.41*** (0.41)	0.11 (0.17)
P60	-0.70 (0.75)	-1.15** (0.45)	-0.11 (0.10)
P70	-0.46 (0.81)	-0.33 (0.50)	-0.59** (0.26)
P75	0.12 (0.61)	0.19 (0.49)	-0.50** (0.26)
P80	0.32 (0.48)	0.01 (0.56)	-0.14 (0.28)
Mean	0.61 (0.73)	-0.22 (0.39)	-0.40 (0.36)
Part-time share of employee jobs (pp)	—	—	-0.06

(0.35)

PAYE UK-national median (benchmark)	-1.06***	—	—
	(0.18)		

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels. P90 omitted due to too few observations as a consequence of ASHE suppression. Annual pay, hourly pay and weekly paid hours are from ASHE Tables 8.7a, 8.5a and 8.9a, all by place of residence; the part-time share is calculated from the job counts in Table 8.9a, which ONS advises are not reliable estimates.*

A direct test of the distributional pattern regresses the log difference between the 10th and 75th percentiles on migrant employment. Outside London the gap widens significantly in annual pay (-3.54, standard error 1.71) but not in hourly pay (-0.42, standard error 0.50) or in weekly paid hours (+0.51, standard error 2.03). The bottom-heavy shape of the annual pay effect is therefore statistically established outside London; in hourly pay the loss is spread across the lower and middle percentiles rather than concentrated at the bottom.

A.4.7 Geographic Heterogeneity

Table A.7 estimates the pay effect separately within terciles of two pre-period characteristics: the 2015 share of pupils with English as an additional language and the 2015 non-UK share of payrolled employment. The last column counts the London boroughs in the top tercile. Table A.8 estimates the effect separately by type of authority.

Table A.7: Migrant Pay Effects by Pre-Period Characteristic (% per pp)

Split variable (2015)	Low tercile	Mid	High	London LAs in high
EAL pupil share	-3.66*** (0.92)	-1.45*** (0.53)	0.34 (0.4)	28 of 49
PAYE non-UK employment share	-0.62 (0.46)	-1.30*** (0.34)	0.22 (0.39)	29

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

Table A.8: Migrant Pay and Employment Effects by Authority Type (x100)

Authority type	log pay (×100)	log employment (×100)	SW F	AR F
Shire counties (26)	-1.94 (1.19)	-1.54 (1.11)	2.7	1.31
Unitaries (53)	-1.02*** (0.28)	-1.09*** (0.42)	22.5	18.2***

Metropolitan districts (36)	−0.86*** (0.28)	1.29 (0.89)	20.8	8.56***
London boroughs (32)	0.42 (0.47)	0.00 (0.92)	16.3	0.7

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

Two splits show a strong gradient. The pay effect is −3.7% per point in the third of authorities with the fewest EAL pupils in 2015, −1.5% in the middle third, and absent in the top third, of which 28 of 49 authorities are London boroughs. The split by the 2015 non-UK share of payrolled employment does not show the same shape: the effect is largest in the middle tercile (−1.30%) and is not significant in the lowest (−0.62%). Splits by 2015 population and by 2015 job density, reported in the working-paper appendix, show no gradient. By authority type, the effect is present and precisely estimated in the unitaries (−1.0%) and metropolitan districts (−0.9%), with Anderson–Rubin statistics above 8; it is imprecisely estimated in the shire counties, where the instrument is weak (F of 2.7); and it is absent in London.

Because the EAL and prior-presence splits are so heavily London-weighted at the top, a horse race was run in which four interactions compete within one instrumented model, each instrumented by its own product with Z. On log native pay the London indicator dominates: the effect of migrant employment in an authority outside London with average EAL share, population and job density is −1.02% (0.21), or −0.71% (0.19) averaged across all authorities once the London indicator is also centred; the interaction with London is +1.43 (0.60), significant at 5%, while the interactions with EAL (−0.004, standard error 0.006), log population (−0.16, 0.32) and job density (−0.14, 0.55) are all null. The language and prior-presence splits were proxying for London, and the London indicator absorbs them.

A gradient does survive inside the non-London sample, however. Splitting the 115 non-London authorities by 2015 EAL gives −2.99% (0.99) in the lowest tercile, −1.00 (1.01) in the middle and −0.91% (0.32) in the highest. The effect is largest where prior migrant presence is smallest. This is a “new destinations” pattern whereby authorities receiving migrants for the first time absorb them with a larger pay effect than authorities with established migrant communities. It is the reverse of the friction hypothesis, and it is the pattern the proficiency interactions of Section A.4.4 were picking up with their positive signs.

Table A.7b sets the ex-London gradient beside the quantities needed to interpret it, and Table A.7c tests it directly. Two qualifications apply. The instrument is weak within the lowest and middle thirds (F of 4.0 and 6.9), and a single model in which the effect varies smoothly with the 2015 EAL share finds no significant gradient. Nor does a smaller effect per point imply a smaller total: the most-settled third saw the largest rise in migrant employment, so the total effects of the 2015 to 2025 rise, −5.9%, −3.2% and −4.7%, are not significantly different.

Table A.7b: Effects by Third of the 2015 EAL Pupil Share, England excluding London (IV)

Outcome	Lowest third	Middle third	Highest third
Log UK-national median pay (×100), per pp	-2.99*** (0.99)	-1.00 (1.01)	-0.91*** (0.32)
First-stage SW F	4.0	6.9	18.1
Mean rise in migrant employment rate, 2015–25 (pp)	1.97	3.22	5.12
Total effect of the 2015–25 rise on median pay (%)	-5.90*** (1.95)	-3.23 (3.26)	-4.68*** (1.65)
Log UK-national payrolled employments (×100), per pp	+0.05 (0.97)	-1.57 (1.44)	-1.02* (0.54)
Self-employed, % of population, per pp	+0.49 (0.39)	+0.38 (0.80)	+0.37 (0.36)
Employed in SOC 1–3, % (APS), per pp	+5.77* (3.08)	+0.38 (7.46)	-1.64 (1.30)
Maximum rise in the median from selection (×100), per pp	0.00	+1.14	+0.73

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Authorities outside London split into thirds by the 2015 share of pupils with English as an additional language. Total effects multiply each estimate by the third's mean rise. The selection bound assumes every UK-national employment lost was below the median and uses the 2015 gap between the 25th and 75th percentiles of ASHE annual pay. APS outcomes cover all nationalities.*

Table A.7c: Gradient and Within-Authority Curvature of the Pay Effect, England excluding London (IV)

Model and outcome	Estimate	SW F
M × 2015 EAL share (per SD): log median pay (×100)	+0.05 (0.08)	43.1
M × 2015 migrant employment rate (per SD): log median pay (×100)	-0.00 (0.08)	56.3
... log UK-national payrolled employments (×100)	-0.27** (0.11)	56.3
... UK-national employment rate, fixed 2015 base (pp)	-0.10*** (0.04)	56.3
... self-employed, % of population	+0.18*** (0.06)	57.1
... employed in SOC 1–3, %	-0.38* (0.20)	56.1
Quadratic in M: marginal effect on log pay at M = 2.0 (10th percentile)	-0.79** (0.32)	80.4, 83.1
... at M = 10.8 (90th percentile)	-0.98*** (0.19)	

... at M = 20.0 (99th percentile) -1.18*** (0.27)

Quadratic in M: cumulative effect from M = 2.0 to 10.8 (%) -7.77*** (2.10)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Interaction rows give the change in the per-point effect for an authority one standard deviation above the mean on the 2015 characteristic, instrumented with Z interacted with the same characteristic; SW F refers to the interaction. Quadratic rows come from a model with M and M² instrumented with Z and Z²; the coefficient on M² is -0.011 (0.013).*

A.4.8 Origin Composition and the London Mechanism

The disagreement among origin groups reported in Section A.5.2 suggests that inflows differ in the kind of worker they bring. To test this without selecting on outcomes, the thirty origin series of the finer instrument (Section A.5.4) were split before estimation by origin-country income: the eleven higher-income origins observed individually (seven EU14 members, namely Ireland, Germany, France, the Netherlands, Italy, Spain and Portugal, plus Australia, Canada, New Zealand and the United States), and the eighteen other named nationalities together with the pooled residual. Each half is summed into its own instrument and used alone. Table A.9 reports the results.

The London sample comprises 32 boroughs and 320 observations. The higher-income instrument has no first stage outside London, since those networks are concentrated in the capital, and is not interpreted there.

The two halves of the inflow do different things, and the difference is not sampling noise: when both instruments are used together in the full sample, the cluster-robust over-identification test rejects, with a Hansen J statistic of 7.6 ($p = 0.006$). Where higher-income-origin networks route migrants, which is overwhelmingly London, UK-national median pay rises, by 1.8% per point in the full sample and 1.2% in London, with strong first stages and Anderson–Rubin statistics above 14. Where lower-income-origin networks route them, native pay falls, as in the headline.

This difference lies behind the London result, although, as the qualifications below show, a rising native median in London is at least as likely to reflect residential sorting as complementarity between migrant and British workers.

Table A.9: Native Pay and Employment by Origin Composition of the Predicted Inflow (IV)

Sample, instrument	First stage	SW F	log pay (×100)	AR F	log employment (×100)	AR F
Full, higher-income origins	+0.885** *	12.3	+1.76**	16.2** *	-3.01***	6.31**
	(0.107)		(0.73)		(1.00)	
Full, lower-income origins	+0.507** *	23.5	-0.89***	5.34**	0.14	0.04
	(0.105)		(0.33)		(0.65)	

Lon only, higher-income origins	+1.085** *	22.8	+1.22***	14.5** *	-2.40***	8.40** *
	(0.227)		(0.38)		(0.7)	
Lon only, lower-income origins	+0.296**	5.2	-0.17	0.05	1.62	1.15
	(0.13)		(0.68)		(1.88)	
Excl. Lon, higher-income origins	-0.807	0.4	—	—	—	—
	(1.28)					
Excl. Lon, lower-income origins	+0.880** *	86.5	-1.02***	36.4** *	-0.63*	2.42
	(0.095)		(0.17)		(0.36)	

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

Two qualifications govern what can be claimed from the positive estimate. First, under the same instrument UK-national payrolled employment falls, by 3.0% per point in the full sample and 2.4% in London, significantly. A rising median with falling employment is consistent with genuine complementarity, but it is equally consistent with composition: lower-paid natives leaving payroll, which raises the median of those who remain without anyone's pay rising. Second, outside London the higher-income-origin networks predict native pay growth directly (a reduced form of +2.4, standard error 1.3) while predicting no rise in migrant employment. Those networks sit in affluent commuter-belt authorities whose pay was rising for other reasons, so for this instrument the exclusion restriction fails outside the capital and the London-only estimate is the one to rely on.

Table A.10 applies the higher-income instrument, within London, to the outcomes that separate the two readings. If the employment fall were an exit from payroll into self-employment, the survey count of British people in work of any status would hold up and self-employment would rise. If it were residential sorting driven by housing costs, house prices would rise and the survey count would fall with the payroll count.

Table A.10: The Higher-Income-Origin Instrument in London

Outcome, per pp of migrant employment rate	IV	AR F
log mean house price ($\times 100$)	+3.77*** (0.59)	24.5***
UK-national median pay, PAYE (log $\times 100$)	+1.22*** (0.38)	14.5***
UK-national payrolled employments, PAYE (log $\times 100$)	-2.40*** (0.7)	8.4***
UK nationals in employment, any status, APS (log $\times 100$)	-3.96**	11.7***

	(1.59)	
APS UK employed minus PAYE UK employments (% of population)	-0.72	2
	(0.56)	
Self-employed, APS (% of population)	-0.29	1.2
	(0.24)	
Internal out-migration rate (% of population)	-0.04	0.05
	(0.17)	
Net internal migration (pp)	-0.16	2
	(0.12)	
Economic inactivity rate (% of population aged 16–64)	-0.11	0.10
	(0.31)	

Standard errors in parentheses beneath each estimate, clustered by local authority.

** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

The lower-income-origin instrument has a weak first stage in London (F of 5.2) and moves none of these outcomes.

The exit-from-payroll reading fails. The survey count of British people in work of any status falls by 4.0% per point, at least as much as the payroll count; the gap between the two does not open; and self-employment does not rise. The complementarity reading is not supported on its own terms either: fewer British residents are in work, and inactivity does not rise, so those no longer counted have neither stopped working in place nor been reclassified.

What remains most consistent with the evidence is sorting. House prices rise by 3.8% per point of migrant employment, the largest and most precisely estimated response in the table, with an Anderson–Rubin statistic of 24.5. The internal flow series do not show the outflow this reading predicts (net internal migration -0.16 , standard error 0.12; out-migration -0.04 , standard error 0.17). Because those series exclude moves to and from abroad, the gap cannot be explained by international arrivals offsetting British departures; the reading therefore requires either British residents leaving the country, or a difference in how nationality is recorded, at interview in the survey but at National Insurance registration in the tax data.

The reading most consistent with the full set of results is that affluent inflows are capitalised into London housing and lower-paid British residents leave the boroughs concerned, which raises the measured median of those who remain. A native-specific flow series, which the data do not contain, would settle the question. Until then the positive London pay estimate should be described as a composition effect of residential sorting at least as readily as a wage effect, and it should not be described as evidence that such inflows are unambiguously beneficial to British workers.

A.4.9 Implied Effect on the Native Wage Bill

Table A.11 reports the aggregate accounting of Section A.3.6: the 2025 effect of the observed 2015 to 2025 rise in migrant employment on the UK-national wage bill, computed authority by authority and summed, with 100,000 draws of the pay coefficient. Figure A.2 shows the distributions of the draws for each headline quantity.

Table A.11: Implied Effect on the 2025 UK-National Wage Bill

Sample	Mean ΔM 2015–25	Native wage bill 2025	Median ΔW	95% CI	$P(\Delta W < 0)$
Full, 147 LAs	+3.70 pp	£637bn	–1.2% = –£7.6bn/yr	[–£18.9bn, +£3.5bn]	0.91
Excl. London, 115 LAs	+3.42 pp	£540bn	–3.4% = –£18.2bn/yr	[–£24.3bn, –£12.1bn]	1

Draws: 100,000 from the clustered sampling distribution of the pay coefficient (standard errors clustered by local authority). Median ΔW is the median draw, the 95% CI runs from the 2.5th to the 97.5th percentile of the draws, and $P(\Delta W < 0)$ is the share of draws below zero.

The 2025 wage-bill figures in Table A.11 apply the same draws to the 2015 to 2025 rise. Orange: median. Red dashed: 95% interval. The displacement-ratio panel reports the probability of a positive ratio, since a positive ratio is displacement.

Outside London, the rise in migrant employment since 2015 is associated with a 2024 UK-national wage bill about £16bn a year lower than it would otherwise have been, and a 2025 wage bill about £18bn lower, in each case with an interval that excludes zero; the increase from one year to the next reflects the further rise in migrant employment in 2025 and nominal growth in the wage bill, not a new estimate. For England as a whole the central estimates are £7bn and £8bn with intervals that include zero, because the London result is pooled in. The sum is a partial-equilibrium, within-region quantity: it is gross pay, not welfare or output, and it is not a national general-equilibrium total.

These are level effects in the year concerned, not flows that have accrued at that size since 2015. Summing the annual gaps year by year, each pricing that year's rise in migrant employment since 2015 against that year's wage bill, gives a cumulative effect outside London of £59bn over 2016 to 2024 (95% interval £39bn to £79bn) and £78bn over 2016 to 2025 (£52bn to £103bn); for England as a whole the totals are £25bn and £33bn, with intervals that include zero. The cumulative figures are about two-fifths of a naive multiplication of the latest annual gap by the number of years, because the rise in migrant employment ramped up over the decade and about three-fifths of it arrived after 2021. The calculation applies the same coefficient to every year, which the design does not test, and it inherits every qualification attached to the annual figures.

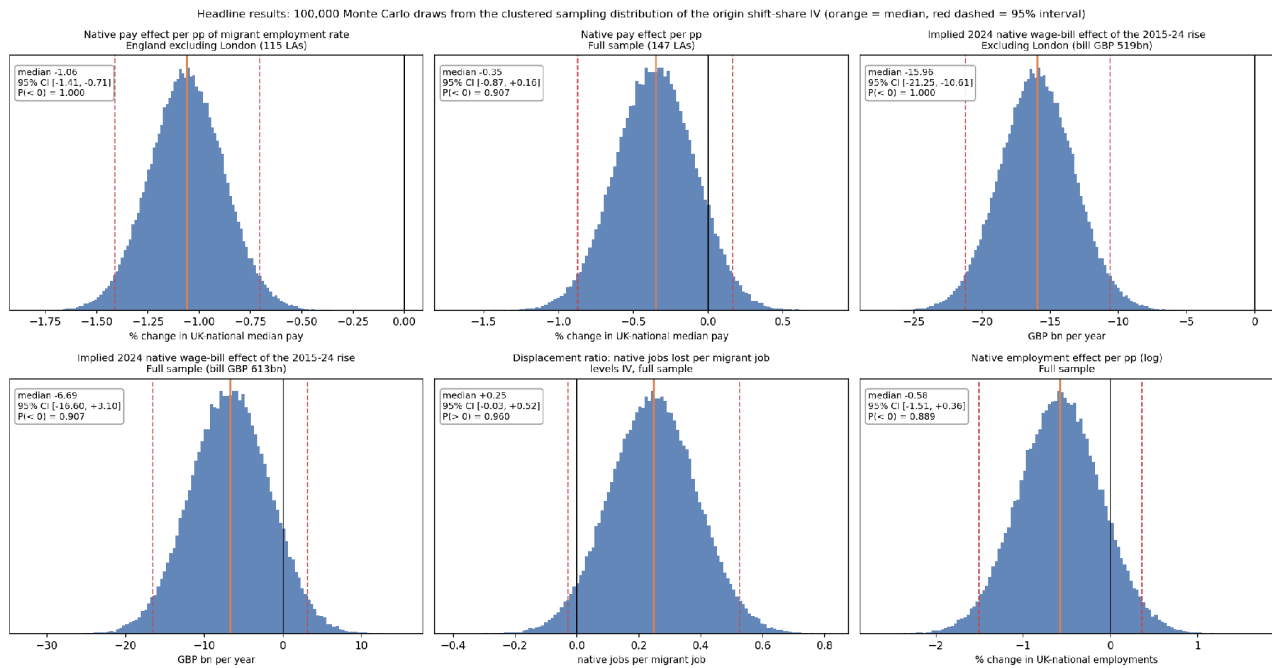


Figure A.2. 100,000 draws from the clustered sampling distribution of each headline estimate: the pay coefficient (excluding London; full sample), the implied wage-bill effect in 2024 for each sample, the full-sample displacement ratio and the full-sample log employment effect.

Table A.12: Attribution of the 2025 Wage-Bill Reduction to Origin Groups

Origin group	Excl. London, £bn	Share	Full sample, £bn	Share
South Asia	-4.3	24%	-1.8	24%
EU2 (Romania, Bulgaria)	-3.0	16%	-1.2	16%
Sub-Saharan Africa	-2.5	13%	-1.0	13%
EU14	-2.2	12%	-1.0	13%
EU8	-1.8	10%	-0.7	9%
Non-EU Europe (incl. Ukraine)	-0.9	5%	-0.4	5%
Middle East & Central Asia	-0.9	5%	-0.4	5%
East Asia (incl. Hong Kong)	-0.7	4%	-0.3	4%
All other groups	-2.0	11%	-0.8	11%
Total	-18.3		-7.6	

Accounting attribution: the pooled coefficient for each sample is applied to every authority's rise in migrant employment, divided among origin groups by that authority's National Insurance registrations over 2015 to 2025. No group-specific effects are estimated, so no significance levels are reported.

Table A.12 attributes the 2025 figure to origin groups. Each authority's rise in migrant employment is divided among groups in proportion to that authority's National Insurance registrations by group over 2015 to 2025, and the effect summed. This is an accounting

attribution that applies the pooled coefficient to every group; it answers which inflows account for the figure, not which had the largest effect per worker.

Components are rounded, so the total (–£18.3bn) differs from the headline £18.2bn in the last digit. Two-thirds of the ex-London figure is attributable to four inflows, South Asia, EU2, Sub-Saharan Africa and EU14, in proportion to their share of registrations.

Outside London these are also the groups whose own instruments return negative just-identified estimates (Section A.5.2), so the attribution and the identification point the same way; in the full sample they do not, which is a further reason the ex-London figure is the one to report.

A.5 Robustness and Diagnostic Tests

A.5.1 Instrument Validity: The Single-Series Instrument

The exclusion restriction implies that an instrument may reach the outcome only through the regressor. The single-series instrument of Section A.4.1 fails this on simple grounds: it does not move the regressor, with a first-stage F on the migrant employment rate of 0.44, yet it moves the outcome, with an Anderson–Rubin F on the native employment rate (fixed 2015 base) of 5.5, significant at 5%, and a reduced-form F of 20.8 on the level of native employment in the specification without the industry control. An instrument that moves the outcome without a first stage cannot be reaching it through migration.

This is a failure of exclusion, not merely of relevance, and it is why the single-series form is discarded. A comparison of the size of first-stage and reduced-form statistics is not itself a test of exclusion, since a valid instrument with a large effect on a precisely measured outcome can have a reduced-form statistic above its first-stage statistic; the origin-group instrument, with a first-stage F of 43.9, is assessed instead by the tests of Sections A.5.2 to A.5.4. The difference between the two instruments lies in the fifteen distinct national series: with one series, the identifying variation is a single cross-section scaled by a common trend, and the 2001 settlement map proxies for the persistent urban growth of the places migrants first settled.

A.5.2 Over-Identification and Heterogeneity across Origin Groups

With the fifteen group-specific instruments in place of the pooled one, a cluster-robust Hansen J test rejects the hypothesis that they estimate a common coefficient, with statistics of 25.4 on the native employment rate measured on a population base fixed at 2015 ($p = 0.031$), 27.4 on log employment ($p = 0.017$) and 30.4 on pay ($p = 0.007$), each with 14 degrees of freedom. The test cannot say whether some instruments are invalid or whether the true effect differs across inflows; either way, the pooled coefficient is a weighted average whose weights must be shown. A leave-one-group-out analysis, reported in the working-paper appendix, shows that the pooled instrument leans on the South Asian and EU14 inflows and that, in the full sample, the pay estimate depends on EU2. The decomposition below makes the same point more precisely.

The decomposition follows Goldsmith-Pinkham, Sorkin and Swift (2020). The pooled just-identified estimate is exactly a weighted average of the just-identified estimates from each group's own instrument, where tildes denote residuals from the controls and fixed effects and the weights sum to one. The weights identify whose settlement maps drive the estimate, and the dispersion of the group estimates shows whether they agree. Table A.13 reports them for log native pay.

$$\hat{\beta} = \sum_g \alpha_g \hat{\beta}_g, \quad \alpha_g = \frac{\tilde{Z}'_g \tilde{M}}{\tilde{Z}' \tilde{M}}, \quad \hat{\beta}_g = \frac{\tilde{Z}'_g \tilde{y}}{\tilde{Z}'_g \tilde{M}}$$

The top two groups carry 62% of the absolute weight in both samples, but the two samples differ in kind. In the full sample the pooled estimate of -0.35% is a disagreement: EU2's instrument says -1.5% , while South Asia's and EU14's say $+0.4\%$ and $+2.3\%$, and non-EU Europe's $+2.0\%$, the last two significant.

Outside London the four groups whose weights sum to 1.04, or 80% of the absolute weight, are South Asia, Sub-Saharan Africa, the Middle East and EU2, and all four return negative estimates, three of them individually significant.

The ex-London headline is therefore not a favourable sub-sample chosen after the fact; it is the sample in which the origin shocks agree on the sign of the effect, and the full-sample attenuation is the positive London-concentrated estimates pulling against them. The heterogeneity is itself a finding as EU8 accession inflows, Health and Care visa inflows, and BNO inflows do not have the same local effects, and Section A.4.8 shows that origin-country income is what they differ by.

Table A.13: Rotemberg Weights and Group-Specific Estimates, log native pay

Group	α_g , full sample	$\hat{\beta}_g$, full ($\times 100$)	α_g , excl. London	$\hat{\beta}_g$, excl. London ($\times 100$)
South Asia	0.42	0.37 (0.85)	0.57	-1.14^{***} (0.24)
EU2	0.36	-1.51^* (0.82)	0.09	-2.14^{**} (0.84)
EU14	0.19	$+2.26^{***}$ (0.85)	-0.07	-1.58^* (0.84)
Non-EU Europe	-0.07	$+1.96^{***}$ (0.72)	0.01	-2.01 (1.30)
Sub-Saharan Africa	0.06	1.1 (1.52)	0.24	-0.82^{***} (0.16)
EU8	0.05	-0.89 (1.22)	-0.08	-0.15 (0.81)
Middle East & C. Asia	0.04	-2.17^* (1.24)	0.14	-0.14 (0.15)
Pooled $\hat{\beta}(\times 100)$		-0.35 (0.27)		-1.06^{***} (0.18)

Standard errors in parentheses beneath each estimate, clustered by local authority.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.

A.5.3 Pre-Trends and Placebos

If the settlement networks are exogenous, they should not predict outcomes before the panel began. Two tests are applied. The first concerns the 2001 shares of the single-series instrument, regressing ASHE job growth over 2004 to 2010 on them across 342 authorities: the coefficient is +0.52 (standard error 1.28) without regional effects and –1.08 (1.59) with them, neither distinguishable from zero.

The second is the direct test of the origin-group instrument. The panel-period predicted inflow, which cannot have caused anything before 2015, is regressed against pre-panel outcome growth together with the same regression on the two highest-weight base shares in the full sample and on all fifteen shares jointly. Table A.14 reports the results for six pre-period outcomes.

$$\Delta y_{i,2011 \rightarrow 2014} = \lambda_0 + \lambda_1 \Delta Z_i + \delta_r + e_{it}, \quad \Delta Z_i = \bar{Z}_{i,2021-2024} - \bar{Z}_{i,2015-18}$$

Table A.14: Pre-Trend Placebos (HC1 standard errors, regional effects)

Pre-period outcome	on ΔZ	on $s_{South\ Asia}$	on s_{EU2}	All 15 shares: F	N
Annual P10 growth 2011–14	0.84 (2.49)	0.55 (3.17)	3.38 (3.90)	5.42***	99
Annual P25 growth 2011–14	+1.51** (0.69)	+2.27** (1.00)	–1.80 (1.14)	7.53***	132
Annual P50 growth 2011–14	–0.09 (0.5)	–0.17 (0.73)	0.74 (1.09)	2.13**	146
Hourly P50 growth 2011–14	0.33 (0.83)	0.28 (1.30)	–1.25 (1.92)	3.78***	147
Payrolled jobs growth 2011–14	0.56 (0.81)	0.63 (0.99)	1.20 (1.13)	5.10***	147
ASHE jobs growth 2004–10	–2.47 (1.68)	–2.90 (2.37)	2.52 (2.63)	2.61***	141

HC1 heteroskedasticity-robust standard errors in parentheses beneath each estimate; regional effects included. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$, applied to coefficients and to the joint F-statistic alike.

The instrument passes on the variation that identifies it. The change in predicted inflow is unrelated to pre-period growth on five of the six outcomes, and the two shares carrying 62% of the Rotemberg weight, South Asia and EU2, are unrelated to pre-period growth on

five of the six outcomes at the 5% level; the exception is again annual pay at the 25th percentile, where the South Asian share is positive (+2.27, standard error 1.00). Both rejections are on annual pay at the 25th percentile, and both are positive: authorities that would later receive larger predicted inflows had slightly faster lower-quartile pay growth in 2011 to 2014, so any continuation of that trend would bias the estimated effect toward zero rather than away from it. Outside London, however, the second-highest weight belongs to Sub-Saharan Africa (0.24) rather than EU2 (0.09), and its share is not tested separately here. No pre-trend test is possible on the PAYE native median itself, since the FOI series begins in July 2014.

The fifteen shares taken jointly do predict pre-period growth on every outcome. This is the standard caution of Goldsmith-Pinkham, Sorkin and Swift that settlement maps are not random, and it is the main remaining vulnerability of the design; the reassurance is that the particular linear combination of the shares that the estimator uses is not what carries the association.

A.5.4 The Industry-Mix Control and Thirty-Origin Instrument

Table A.15 reports every headline estimate without and with the industry-mix control of Section A.2.6. The concern the control addresses is real: outside London the migrant instrument predicts the industry shock (a reduced form of +0.174, standard error 0.056, $\times 100$), so settlement networks do sit in places with favourable industry trends.

Table A.15: Headline Estimates Without and With the Industry-Mix Control (IV)

Sample, outcome	Without (previous specification)	With (primary)	SW F	AR F (primary)
Excl. London, log pay ($\times 100$)	-1.06*** (0.18)	-1.06*** (0.18)	47	45.7***
Excl. London, log employment ($\times 100$)	-0.58 (0.37)	-0.75** (0.35)	47	4.10**
Excl. London, employment rate, fixed denominator (pp)	-0.29** (0.13)	-0.36*** (0.13)	47	7.17***
Full, log pay ($\times 100$)	-0.47* (0.28)	-0.35 (0.27)	43.9	1.35
Full, log employment ($\times 100$)	-0.41 (0.47)	-0.58 (0.48)	43.9	1.03
Full, employment rate, fixed denominator (pp)	-0.22 (0.14)	-0.27* (0.14)	43.9	2.48

Standard errors in parentheses beneath each estimate, clustered by local authority.
** $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Asterisks on test statistics (SW F, AR F, Sargan J) denote the same significance levels.*

The pay effect outside London is unchanged to two decimal places, which is the strongest available evidence that it is not industrial demand in disguise. Native employment outside London moves from a null to a modest negative effect once industry demand is held fixed, consistent with migrant inflows and industry growth having offset each other in the earlier specification. In the full sample the pay estimate weakens and loses significance, which is the London pattern again.

A finer instrument built from thirty origin series (29 individually observed nationalities and a pooled residual, constructed exactly as in Section A.3.2) confirms the headline. Outside London it nearly doubles first-stage strength (F of 81) and returns a pay effect of -0.97% (0.17), within half a standard error of the fifteen-group estimate.

Rotemberg weights by nationality outside London show that India carries 0.33 of the weight, the pooled residual 0.25, Pakistan 0.17 and Nigeria 0.16, with just-identified estimates of -1.08 , -0.99 , -1.05 and -0.57 , so the four largest origin shocks agree on sign and order of magnitude.

In the full sample the disagreement reappears at nationality level (Romania -2.13 , Italy $+1.62$, India -0.04). The effective number of independent origin shocks, one over the Herfindahl index of the first-stage covariance shares, rises only from 4.6 to 5.9, because the additional nationalities are small; the finer instrument adds precision rather than independent variation, and the design's identification rests on the exogeneity of the settlement shares rather than on a many-shocks argument.

A.5.5 Cross-Sectional Dependence

Authorities are not independent labour markets. The Pesaran CD statistic on the residuals of the fixed-effects model is 13.3, significant at 1%: independence is rejected. The mean absolute pairwise correlation is 0.68 but the mean signed correlation only $+0.04$, so positive and negative cross-authority correlations largely cancel after two-way fixed effects. Driscoll–Kraay standard errors, robust to arbitrary cross-sectional dependence, are 0.43 times the authority-clustered ones, and clustering by region (nine clusters) gives 1.4 times. Authority clustering is retained for reporting because nine clusters are too few for conventional cluster-robust inference, but the comparison matters: if every standard error rose by that factor, the pay effect outside London (t of about 4.2) and net internal migration (about 3.3) would remain significant, while log employment and the displacement ratio outside London (about 1.5) would not. A wild cluster bootstrap at region level is left for future work. Same-region and different-region pairs have near-identical mean absolute correlations (0.71 and 0.68), so the dependence is a common factor rather than neighbour-to-neighbour spillover. A within-region spatial lag of migrant employment cannot be added as after region-by-year effects it is identified only off variation in the number of authorities per region (variance inflation factor 2,808).

$$CD = \sqrt{\frac{2T}{N(N-1)}} \sum_{i < j} \hat{\rho}_{ij} \sim N(0, 1)$$

A.5.6 The Industry Mix of Local Jobs, Workforce Composition and Second Jobs

A fall in the native median could reflect a change in the jobs themselves rather than lower pay for the same work: if migration draws low-paying industries into an authority, the median falls as the mix of jobs shifts. Table A.16 applies the design to the industry composition of employees from the Business Register and Employment Survey, at the level of two-digit SIC 2007 divisions. Low-paying industries are defined before estimation from the Low Pay Commission's list: agriculture (01), food processing (10), textiles and clothing (13–15), retail (47), hospitality (55–56), employment agencies (78), travel (79), security (80), cleaning and maintenance (81), social care and childcare (87–88), sport and leisure (93), and hair, beauty and personal services (96).

Table A.16: Industry Mix of Employees (IV, per pp of migrant employment rate)

Outcome	England	Outside London
Low-paying industries' share of employees (pp)	+0.42 (0.47)	+1.25** (0.57)
Employees in low-paying industries (log ×100)	+0.89 (1.44)	+3.96** (1.98)
Employees in other industries (log ×100)	−0.83 (1.07)	−1.11 (0.84)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Employees from the Business Register and Employment Survey, counted by workplace and for all nationalities, on county and unitary geography as of April 2023; Cumbria and Northamptonshire are the sums of their successor authorities.*

Outside London, a one-point rise in the migrant employment rate raises the share of employees in low-paying industries by 1.25 points, significant at 5%, with an Anderson–Rubin statistic of 3.61, significant at 10%. The shift comes from growth in low-paying industries, whose employees rise by 3.96%, rather than from a significant fall elsewhere (−1.11%). In the full sample none of the three outcomes moves significantly. The change in the jobs does not, however, account for the pay effect: holding the low-paying share fixed, the ex-London estimate on UK-national median pay moves from −1.06% (0.18) to −1.02% (0.18). Because the share is itself an outcome of migration, that comparison is descriptive, but it indicates that the pay effect is lower pay for similar work rather than a move into lower-paying industries. Because the Business Register counts jobs where they are located and for all nationalities, the test cannot show whether British workers themselves moved industry.

Table A.16b: Workforce Composition and Second Jobs (IV, per pp of migrant employment rate)

Outcome	England	Outside London
Employed with level 4+ qualifications, % of employed, 2015–24	+0.92*** (0.32)	+1.29*** (0.39)
Employed with NVQ4+, % of employed, 2015–21 only	+1.20** (0.57)	+0.24 (1.03)
Employed with level 4+ (log ×100)	+2.89*** (0.74)	+3.20*** (0.96)
Employed below level 4 (log ×100)	+0.31 (1.16)	−2.28* (1.19)

Employed aged 16–24, % of employed	+0.08 (0.30)	+0.50 (0.38)
Employed aged 50–64, % of employed	–1.29*** (0.48)	–1.07** (0.50)
Employed aged 25–49 (log ×100)	+2.90** (1.13)	+2.78* (1.66)
Employed aged 50–64 (log ×100)	–3.76* (2.19)	–1.44 (1.42)
UK-national payrolled employments per APS UK worker (log ×100)	–0.09 (0.95)	–0.61 (0.90)
Non-UK payrolled employments per APS non-UK worker (log ×100)	+2.52 (2.33)	+2.23 (4.05)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Qualifications and age from the Annual Population Survey, all nationalities. APS samples are smaller than the panel where survey cells are suppressed: 94 authorities outside London for the employment ratios and 98 for the age shares.*

Two further alternatives are tested in Table A.16b. If migration drew lower-qualified or younger people into work, or pushed the more qualified or older out, the median would fall by composition. The employed workforce instead becomes more qualified and more prime-aged where migration rises, which would raise a median rather than lower it, although the qualification result rests on the RQF years and is not significant outside London on 2015 to 2021 alone. If migration pushed British workers into second jobs, UK-national payrolled employments per UK worker would rise; they do not. Neither alternative accounts for the pay effect.

A.5.7 Competition and Employer Power

Two mechanisms remain consistent with the pay effect: competition, in which migrant and British workers substitute for one another, and employer power, in which migrants' weaker outside options, particularly under employer-sponsored visas, allow employers to set lower pay. They make different predictions. Competition implies that non-UK pay also falls, and that the effect is larger where local jobs overlap more with the industries and occupations migrants enter. Employer power implies a larger effect where employment is concentrated in large employers, a smaller one where unions are stronger, and a larger effect per migrant after 2021, when free movement ended and new arrivals were mostly on sponsored visas. Table A.17 applies the design to non-UK pay and the UK/non-UK pay ratio from the HMRC FOI, adds an interaction of migrant employment with an indicator for 2021 onwards, and interacts migrant employment with characteristics of each authority's labour market in 2015. Each characteristic is standardised across the 147 authorities; the interaction and the corresponding instrument interaction follow the proficiency model. All are fixed in 2015 and built from counts of employees, businesses or workers rather than residents, so that later migration cannot move their numerator or denominator: employment in local units with 250 or more employees (ONS UK Business Counts, size-band midpoints), for all industries and for low-paying industries; union density among private-sector employees (Department for Business and Trade, by industry, applied to BRES employee shares outside public administration, education and health); the non-UK share of payrolled employments by industry (HMRC) applied to BRES shares; the share of recent arrivals by occupation (Census 2011) applied to Annual Population Survey occupation shares; and the share of employees in public administration, education and health, where pay is set nationally.

Outside London, non-UK median pay falls by 1.68% per point, more than UK-national pay, and the ratio of UK to non-UK pay rises, though not significantly. This fits competition between close substitutes but does not exclude employer power, and the non-UK median also falls if later arrivals earn less than earlier ones. The effect per migrant did not increase after 2021. None of the characteristics that distinguish the two mechanisms changes the effect significantly outside London: it is no larger where employment is concentrated in large local units or where private-sector union density is lower, and the industry and occupational overlap interactions are not significant, the latter with the opposite sign to that predicted. The one significant interaction is with the public-sector share: the effect is smaller where more pay is set nationally, as any mechanism operating through local pay setting implies. A version with union density across all sectors gives a significant positive interaction, but that measure is correlated at 0.86 with the public-sector share and neither is significant when both are included. The characteristics are coarse proxies, so the nulls are weak evidence against either mechanism. The evidence leans towards competition but does not distinguish the two, which requires linked employer–employee data.

Table A.17: Competition and Employer Power (IV, per pp of migrant employment rate)

Outcome or interaction	England	Outside London
Non-UK median pay (log ×100)	−0.98*** (0.35)	−1.68*** (0.45)
UK / non-UK median pay (log ×100)	+0.63 (0.39)	+0.62 (0.49)
UK-national pay: additional effect from 2021	−0.08 (0.08)	+0.06 (0.10)
× Employment in 250+ local units, all industries	−0.09 (0.09)	+0.04 (0.10)
× Employment in 250+ local units, low-paying industries	−0.06 (0.11)	+0.15 (0.13)
× Private-sector union density	−0.21* (0.12)	−0.13 (0.19)
× Industry overlap with non-UK employment	−0.08 (0.10)	−0.14 (0.11)
× Occupational overlap with recent arrivals	+0.04 (0.18)	+0.38 (0.24)
× Public-sector share of employees	+0.09 (0.07)	+0.25*** (0.07)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Interaction rows (×) are the change in the effect on log UK-national median pay (×100) per standard deviation of the 2015 characteristic, instrumented with the instrument interacted with the same characteristic. Sanderson–Windmeijer F statistics are at least 11.8 for every endogenous regressor. Anderson–Rubin F on non-UK pay: 7.10*** and 7.17***.*

A.5.8 English Proficiency and the Language Requirements of Local Jobs

The proficiency interaction of Section A.4.4 is null on average. One explanation is that migrants work mainly in jobs that do not require English, so that proficiency does not change whom they compete with. The premise holds in the 2011 Census (DC2604EW): 52% of employees who cannot speak English well work in accommodation and food services, manufacturing, and administrative and support services, against 19% of native English speakers. The explanation implies that the interaction should differ with the language requirement of local work.

The revealed requirement of an industry is the share of its non-native-speaking employees who cannot speak English well, which ranges from 4% in finance to 28% in agriculture, energy and water; conditioning on non-native speakers separates the requirement from migrant intensity. Each authority's 2015 BRES employee shares across the fifteen Census industry groups, weighted by these national rates, measure how much local work is done without English. Authorities are split at the median of that measure, and the proficiency model is estimated in each half (instruments Z and $Z \times E$), in a pooled model with interactions of both endogenous regressors with the half indicator, and with a continuous standardised measure. The measure is repeated excluding public administration, education and health, and with APS 2015 occupation shares weighted by the equivalent Census 2011 rates by occupation (DC2602EW).

Table A.18: Proficiency Interaction by Language Requirement of Local Jobs (IV, log UK-national median pay $\times 100$)

Outside London	Census 2011	EAL 2015
M $\times$ English, local jobs need English more	-0.170* (0.090)	-0.024** (0.010)
M $\times$ English, local jobs need English less	+0.177* (0.106)	+0.014* (0.008)
Difference, pooled (less minus more)	+0.312* (0.180)	+0.043** (0.021)
M $\times$ English $\times$ job requirement, per SD	+0.184* (0.096)	+0.022** (0.009)
Difference, private-sector job mix	+0.465*** (0.153)	+0.057*** (0.022)
Difference, occupational job mix	+0.341 (0.297)	+0.049 (0.055)

*Standard errors in parentheses, clustered by local authority. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. English is the centred 2011 Census share who cannot speak English well or the centred 2015 EAL pupil share, in percentage points. Sanderson–Windmeijer F on the reported term, Census then EAL: 23.2, 14.7; 22.8, 57.9; 29.1, 26.7; 28.0, 22.4; 39.8, 18.4; 9.5, 2.5. 113 authorities (Census) and 115 (EAL).*

Where local work mostly requires English, the pay effect is larger where proficiency is poorer; where more work is done without English, it is smaller. The two cancel in the pooled interaction of Table A.4. The difference is significant at 10% with the Census measure and 5% with the EAL measure, and at 1% with both once the public sector, whose pay is set nationally, is excluded. The occupational measure has the same sign but is not significant, and its first stage is weak with the EAL measure. The pattern is consistent with migrants with poor English crowding into the few jobs that do not require it where such jobs are scarce, and working apart from British workers where they are common, but the data cannot confirm that mechanism, and the within-half estimates are significant at 10% only, so they are suggestive.