# Immigration Responses to the Minimum Wage*

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**Abstract**

We study how Germany's introduction of a national minimum wage in 2015 affected the number and selection of immigrants. Exploiting regional variation in exposure to the reform and administrative social security records, we find a significant increase in immigration from Eastern European Union (EU) countries. This is consistent with the reform substantially raising potential earnings for Eastern EU immigrants in the presence of free labor mobility within the EU. The immigration inflow was not offset by a reduction in native employment. The new inflows were low- to medium-skilled but confined to relatively low-complexity occupations, implying positive selection relative to the task requirements of low-wage jobs. Immigration responses were concentrated in localities with elevated employment concentration, supporting the view that minimum wages attract immigrants to monopsonistic employers who have exhausted the local supply of labor at suboptimal wages.

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# 1 Introduction

A growing body of evidence shows that minimum wage policies generate substantial earnings gains for low-wage workers with little or no adverse effect on employment (Cengiz et al., 2019; Dustmann et al., 2022; Rao & Risch, 2026). If so, wage floors may also influence international labor mobility by making high-minimum-wage countries more attractive destinations for immigrants, thereby increasing labor supply in receiving locations while reducing it elsewhere. Understanding this consequence is important: migration responses of this kind could affect native employment and alter workforce composition in both origin and destination economies. Despite this intuitive implication, international migration has received comparatively little attention in the extensive empirical literature on minimum wages, and existing reviews characterize the available evidence on immigration responses to minimum wage policy as limited and inconclusive (Dube & Lindner, 2024; Giulietti, 2015).

One reason why immigration effects have been difficult to establish in prior work is that many countries, including the United States, tightly restrict low-skilled immigration. In such settings, potential migration responses to minimum wage policies may be muted or obscured. We revisit this question in an underexplored context where low-skilled workers can move freely: the integrated labor market of the European Union (EU). Specifically, we examine whether the first-time introduction of a statutory minimum wage in Germany in 2015 increased immigration, whether it altered immigrant selection, and what mechanisms underlie the observed migration response.

This setting offers three key advantages. First, the German minimum wage was implemented with little advance notice and was highly binding, affecting roughly 15% of all workers, and therefore constituting a large and sudden shock to the low-wage labor market. Substantial regional variation in pre-reform wage levels generated plausibly exogenous spatial heterogeneity in exposure, which we exploit using a difference-in-differences design in the spirit of Card (1992). Second, Germany is fully integrated into the EU internal labor market, with free worker mobility across member states. If minimum wages attract immigrants, such effects should be most visible in this low-friction setting, making it a particularly informative laboratory for studying immigration responses to minimum wage policy. Third, credibly identifying

immigration responses places demanding requirements on data: ideally, one needs precise measures of migrant inflows by location and skill level (Giulietti, 2015). We meet this challenge by drawing on comprehensive administrative records.

In particular, our analysis is based on administrative social security data covering the universe of employees in Germany, including all immigrant workers, provided by the Institute for Employment Research (IAB). These data allow us to trace workers' complete employment histories from their first entry into the social security system and to observe detailed worker and job characteristics, including wages, nationality, education, establishment identifiers, and precise locations of work and residence, even when the residence is abroad. This richness enables us to distinguish cleanly between local inflows and outflows of migrants and to measure their skill composition with unusually high precision. The data further allow us to document mechanisms through which the minimum wage affects immigration that have not previously been studied, most notably cross-border commuting and the role of monopsonistic labor markets. Our empirical approach closely follows Dustmann et al. (2022), who analyze the wage, employment, and worker reallocation effects of the reform using the same data but do not examine immigration responses.

While we analyze immigration overall, our primary focus is on inflows from ten Eastern European EU member states, where migration responses to the reform are most likely. Nationals of these countries had unrestricted access to the German labor market at the time of the reform, while prevailing wage levels in their home countries were substantially lower than in Germany.[1] In 2014, more than two-thirds of workers in Eastern EU member states earned wages below the German minimum wage introduced in 2015, generating strong financial incentives to seek employment in Germany. These incentives are also evident in cross-country comparisons: the new German minimum wage of 8.50 per hour was, for example, approximately 2.8 times the cost-of-living-adjusted minimum wage in the neighboring Czech Republic.

Consistent with these stark wage differentials, we find significant effects on immigration from Eastern Europe. Specifically, we estimate that a one-standard-deviation increase in regional exposure to the minimum wage raised the stock of

[1]These countries comprise the Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, the Slovak Republic, and Slovenia (the EU8), as well as Bulgaria and Romania. Institutional details on the accession of these countries to the EU are provided in Section 3.

Eastern European EU workers in a district by 3% within two years of the reform.[2] We find no conclusive evidence of effects on inflows from other, more affluent EU countries, with the exception of Greece—a special case given the severe economic distress it experienced during the 2010s sovereign debt crisis.[3] Immigration from outside the EU was likewise unaffected, likely reflecting high migration costs and binding regulatory barriers.

Decomposing the increase in immigration from Eastern EU countries into inflows and outflows, we find it is driven entirely by higher inflows, with no detectable reduction in outflows. Aggregating across districts, our estimates imply that the reform generated approximately 20,000 additional full-time equivalent (FTE) Eastern EU inflows, corresponding to a 13% increase relative to the pre-reform baseline. We also document that the reform increased cross-border commuting from the Czech Republic to Germany, accounting for 10% of the total increase in inflows. A potential concern is that higher immigrant employment may have come at the expense of native workers. We find no evidence that this was the case.

We further examine immigrant selection and find that the response is driven by increased inflows of low- and medium-skilled workers, while inflows of high-skilled workers remain unaffected. This finding suggests that minimum wages need not attract only low-skilled immigrants, as documented in prior U.S.-based evidence (Giulietti, 2014), but may also draw medium-skilled workers in settings characterized by large cross-country wage differentials and frictionless labor mobility, such as the European Union. Turning to the types of jobs these immigrants enter, we find that the additional inflows are concentrated in occupations with relatively low task complexity, suggesting that the minimum wage increases over-education and positive selection relative to the task requirements of low-wage jobs.

We rationalize our findings using a simple extension of the Roy–Borjas model of immigration (Borjas, 1987). The model predicts higher migrant inflows from the

[2]We focus on short-run effects because the minimum wage was increased incrementally several times starting in 2017, complicating causal identification of medium-run effects. We nevertheless provide suggestive evidence of stable medium-run effects in Section 7.

[3]Greek inflows amount to no more than approximately 1,500 additional FTE workers, which is an order of magnitude smaller than the Eastern EU response. Therefore, we decided to make the Eastern EU immigrants the focus of the paper, but we discuss the Greek case briefly in Section 7.

lower end of the skill distribution following the introduction of a minimum wage, provided the policy does not generate employment losses large enough to offset the positive wage effect on expected earnings. This prediction aligns closely with our findings and is consistent with prior evidence that the employment effects of the German minimum wage were negligible (Bossler & Schank, 2023; Caliendo et al., 2018; Dustmann et al., 2022; Friedrich, 2020).

The absence of negative employment effects of minimum wages in empirical work is often attributed to monopsonistic competition, where firms are supply-constrained and may expand employment when wages rise. Dube and Lindner (2024) argues that under monopsony, workers may be attracted to locations with higher minimum wages. A key contribution of our study is to provide the first direct empirical evidence in support of this mechanism. Specifically, we show that the inflows of Eastern European workers following the reform were strongly concentrated in districts with relatively high employment concentration prior to the reform, a commonly used proxy for local monopsony power. This supports the view that minimum wages limit employers' ability to exercise market power, shifting employment upward along the firm-level labor supply curve toward the competitive equilibrium (Naidu & Posner, 2022). Since we find no effects on native employment, this upward shift appears to be driven by immigrant workers responding to the positive wage effect.

Our study contributes to the limited literature on minimum wages and international immigration. To date, the available evidence is confined to the United States and relies on variation generated by federal or state-level minimum wage increases, with mixed findings: some studies document positive effects on immigration (Boffy-Ramirez, 2013; Giulietti, 2014) while others find negative effects (Cadena, 2014; Orrenius & Zavodny, 2008). We contribute new evidence from a distinct institutional setting characterized by free labor mobility, and exploit the first-time introduction of a statutory minimum wage rather than marginal changes to an existing policy. This allows for a clean comparison of migrant inflows before and after the transition to a new labor market regime, and we are the first to document a statistically and economically significant immigration response to such a reform.

Beyond establishing a positive aggregate effect, we uncover several previously undocumented mechanisms through which minimum wage policy affects immigra-

tion under free labor mobility. First, the increase in immigrant employment is driven entirely by higher inflows rather than reduced outflows. Second, new arrivals are predominantly low- to medium-skilled, yet both groups sort into jobs with relatively low task complexity, implying increased over-education and positive selection relative to the task requirements of these positions. Third, the reform induced cross-border commuting, underscoring the relevance of minimum wage differentials for border regions. Fourth, immigration responses are concentrated in local labor markets with high pre-reform employment concentration, a commonly used proxy for monopsony power. This last finding resonates with recent evidence that minimum wage increases generate more positive employment effects in more concentrated labor markets (Azar et al., 2024).

Our paper also contributes to the literature on whether minimum wage reforms affect internal migration among both native and immigrant workers, where the evidence is likewise mixed (Giupponi et al., 2024; Ma, Li, & Wu, 2024; Minton & Wheaton, 2023; Monras, 2019). We find no increase in internal migration among incumbent immigrants following the reform, consistent with Ahlfeldt, Roth, and Seidel (2018), who study internal migration responses to the German minimum wage without distinguishing between natives and immigrants.

Finally, our study relates to the large empirical literature testing the Roy–Borjas model of immigration (see, for example, Abramitzky, Boustan, & Eriksson, 2012; Chiquiar & Hanson, 2005; McKenzie, Stillman, & Gibson, 2010; Parey et al., 2017). We contribute to this literature by showing that the exogenous compression of the German wage distribution induced by the minimum wage reform (Bossler & Schank, 2023) shifted immigrant selection toward lower skill levels, with inflows moving away from high-skilled workers toward low- and medium-skilled workers, consistent with the model's predictions.

The remainder of the paper is structured as follows: Section 2 describes the data sources used. Section 3 explains the institutional background. Section 4 describes the theoretical background. Sections 5 and 6 present the empirical strategy and main results, respectively. Section 7 contains robustness checks and additional results. Section 8 concludes.

# 2 Data

## 2.1 IAB Employee History Data

The main empirical analysis draws on administrative social security data provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency. Specifically, we use the universe of the Employee History data (Beschäftigtenhistorik, BeH), which cover all workers registered with the German social security system since 1975. For comparability, we closely follow the data construction and processing steps used in the minimum wage study by Dustmann et al. (2022), which relies on the same data.

The data contain a rich set of worker characteristics such as education, nationality, and the locations of work and residence. For each employment spell, we observe exact start and end dates, daily wages, working-time status (full-time, part-time, or marginal employment), and firm identifiers. For the period 2011–2014, the BeH additionally include information on hours worked. We apply the procedure proposed by Vom Berge, Umkehrer, and Wanger (2023) to consistently recover contractual hours from these data. Combined with wage information, this allows us to construct hourly wages, which we use to measure district-level exposure to the minimum wage introduction in the years preceding the reform. Further details on the definition and measurement of the treatment variable are provided in Section 5.2.

Although the BeH currently covers the period 1975 to 2024, our main analysis focuses on 2012–2016. We begin the observation period in 2012 because a change in the coding of working-time status introduced a structural break between 2011 and 2012 in our key outcome variables, which could bias estimates relying on pre-2012 data. We end the main analysis in 2016 to isolate the effects of the initial minimum wage introduction: the statutory minimum wage was first raised in 2017 from 8.50 to 8.84 and was increased several times thereafter, and it is methodologically challenging to separate the effects of these incremental increases from those of the original introduction (Caliendo, Olthaus, & Pestel, 2025). An additional constraint is that the BeH contains information on hours worked only for 2010–2014, limiting our ability to measure district-level exposure to subsequent minimum wage increases in the same way as exposure to the introduction. We nonetheless use data

outside the 2012–2016 window in some instances: to identify first-time immigrant inflows we draw on records going back to 1975, and we provide suggestive evidence on medium-run effects through 2019 in an extension. Further details on the BeH, including sample and variable definitions and data processing, are provided in Appendix B.

### 2.2 Additional Data Sources

In Section 3 and Appendix C, we draw on two survey datasets to document stylized facts on wage inequality in the EU, migration incentives, and the selection of immigrants from Eastern Europe to Germany. These stylized facts guide our theoretical reasoning in Section 4. First, we draw on the EU Statistics on Income and Living Conditions (EU-SILC), an annual labor force survey coordinated by Eurostat, providing representative and harmonized microdata across EU member states. Second, we use the German labor force survey ("Microcensus"). Further details on these surveys and how we process the data are provided in Appendix B. Finally, we use data from the WSI Minimum Wage Database (WSI, 2026) to compare minimum wage levels across Europe.

## 3 Institutional Background and Stylized Facts

### 3.1 Germany's 2015 Minimum Wage Reform

Germany introduced a statutory minimum wage for the first time in its history on January 1, 2015. The reform was passed relatively unexpectedly on July 3, 2014, leaving employers little time to adjust. The wage floor was set at €8.50 per hour, corresponding to 48% of the median hourly wage at the time of introduction (OECD, 2026a). For comparison, the minimum-to-median wage ratio in 2015 was 36% in the United States, 49% in the United Kingdom, and 62% in France. Only a small set of workers was exempt from the German minimum wage, including the long-term unemployed, apprentices, certain interns, and, on a temporary basis, selected industries such as hairdressing, meat processing, and agriculture. Together, these exemptions covered roughly 5% of all workers. The German reform therefore constituted a ma-

jor and highly binding minimum wage increase, affecting approximately 15% of all employees. The policy was introduced during a period of historically low unemployment and strong labor demand, particularly in low-skilled occupations (Dustmann et al., 2025).

The effects of the reform on outcomes other than immigration have been widely studied. Existing evidence indicates that the reform raised wages and reduced inequality without generating significant reductions in employment or hours worked (Bossler, Liang, & Schank, 2026; Bossler & Schank, 2023; Caliendo et al., 2018; Friedrich, 2020). At the same time, it induced a reallocation of workers from low- to high-productivity employers (Dustmann et al., 2022), and firms responded by raising their hiring standards (Butschek, 2022). There is also evidence that employers partially passed increased labor costs on to consumers through higher prices (Link, 2024). Finally, the reform reduced the use of foreign posted workers from lower-wage countries in the manufacturing sector (Muñoz, 2025).[4]

## 3.2 Free Labor Mobility and Wage Differences Across Europe

**Free Labor Mobility in the EU.** Any migration response to a newly introduced minimum wage requires that workers can relocate across national labor markets. Within the European Union, free movement of labor allows workers to migrate from lower- to higher-wage countries, making the EU a natural setting for studying migration responses to minimum wage policies. In the decade preceding Germany's minimum wage introduction, free labor mobility was extended to several Eastern European countries. Eight Central and Eastern European countries (the EU8) joined the EU in 2004 and gained unrestricted access to the German labor market in May 2011. Bulgaria and Romania (the EU2) joined in 2007, with full labor market access granted in January 2014. Finally, Croatia joined the EU in 2013 and gained access to the German labor market in 2015. As Croatia's integration coincided with the introduction of the statutory minimum wage in Germany, we exclude this country

[4]The appendix to Muñoz (2025) considers whether the reduced use of foreign posted workers in food processing was offset by increased domestic employment of foreign nationals, but does not find conclusive evidence in support of such a mechanism.

from our analysis.

**Immigrant employment in Germany.** Germany experienced a substantial increase in labor immigration during the 2010s. While the share of immigrants in total employment remained stable at around 7% throughout the 2000s, it more than doubled by 2019. A large share of this increase was driven by immigrants from Eastern Europe, as shown in Figure A.1. This group accounted for less than 1% of total employment in Germany in 2010 but steadily increased its share to around 5% by the end of the decade. By contrast, the employment share of immigrants from other EU countries remained fairly stable over this period. The share of immigrant workers from non-EU countries also increased during the 2010s; however, this rise was smaller in relative terms than the increase in immigration from Eastern European countries, underscoring the role of free labor mobility facilitated by the Eastern EU enlargements.

**Wage Differences between Western and Eastern Europe.** The substantial migration flows from Eastern Europe to Germany were plausibly linked to large real wage differences.[5] Table 1 shows that, in 2014, the mean cost-of-living–adjusted hourly wage in Germany (€16.93) was more than twice as high as the corresponding mean hourly wage level across Eastern European countries (€6.57). The introduction of a statutory minimum wage in Germany further amplified these cross-country wage differentials, thereby strengthening incentives for migration from Eastern Europe. Prior to the reform, 14% of workers in Germany earned less than the minimum wage that was subsequently introduced, compared with 78% of workers in Eastern Europe. Moreover, as shown in Table A.1, the German minimum wage was set at 2.8 times the level in the neighboring Czech Republic and 3.7 times the level in Bulgaria. By contrast, the French real minimum wage was higher than the German minimum wage, and only about 10% of French workers earned below the new German wage floor, implying none to minor additional incentives to migrate from France to Germany due to the policy. Taken together, these comparisons suggest

[5]This interpretation aligns with earlier research documenting that large wage gaps fueled mass migration from former East to West Germany in the 1990s after the fall of the Iron Curtain (Hunt, 2006).

that the German minimum wage plausibly acted as a strong pull factor for migrants from Eastern Europe but not for migrants from more affluent Western EU member states.

**Welfare migration.** In addition to wage differences, differences in the generosity of unemployment insurance (UI) and welfare benefits may also shape migration incentives. Net replacement rates for unemployed workers were considerably higher in Germany than in Eastern Europe in 2014. After 12 months of unemployment, (single) workers in Bulgaria received a net replacement rate of only 12%. Similarly, unemployed workers in Poland and the Czech Republic received just 37% and 43% of their previous income, respectively. By contrast, unemployed workers in Germany received 59% (OECD, 2026c). These differences may further strengthen incentives to seek employment in Germany among forward-looking Eastern EU citizens who value inclusion in a more generous social safety net in the long run.[6]

# 4 Theoretical Background

## 4.1 Intuition

This section outlines the expected effects of introducing a minimum wage on the volume and selection of immigrants, and formalizes the intuition below in Appendix C using a simple extension of the Roy–Borjas model (Borjas, 1987).

At a basic level, potential immigrants consider whether expected earnings net of migration costs are higher in the destination than at home. Since expected earnings depend jointly on wages and employment probabilities, whether immigration increases hinges critically on the policy's employment effects. A minimum wage may deter immigration if it reduces employment sufficiently to offset the positive effect of higher wages on expected earnings. If instead the minimum wage has no adverse employment effects—as may arise under monopsonistic competition—expected earnings increase unambiguously, making migration more attractive.

[6]EU immigrants become eligible for UI and welfare benefits in Germany only after having worked for at least one year in a job subject to social security contributions. Consistent with this rule, evidence on immigrant employment and UI receipt suggests that work rather than welfare is the primary motive for immigration to Germany (Brücker, 2025).

Additional immigration will most likely originate from workers at the lower end of the origin country's skill distribution, where the destination minimum wage is most likely to bind. However, if the minimum wage is set sufficiently high relative to wages in the origin country, it may also attract medium-skilled workers for whom employment at the destination country's minimum wage compares favorably to opportunities at home.

## 4.2 Graphical Illustration

Figure 1 provides a graphical illustration of the expected immigration effects of introducing a minimum wage assuming that the employment effects are non-negative as was the case in Germany 2015.

Panel A depicts the pre-reform equilibrium. To maintain tractability while preserving relevance for our empirical setting, we impose a set of simplifying assumptions. First, we assume full employment in the destination country both before and after the reform. By contrast, we allow for unemployment in the origin country up to a skill threshold $\bar{S}_0$. This assumption reflects the fact that unemployment rates were substantially lower in Germany than in Eastern Europe around the reform (OECD, 2026b). Table A.2 shows that even low-skilled workers faced markedly lower unemployment risks in Germany than in Eastern Europe. Second, assume that unemployed workers in the origin country receive unemployment insurance benefits that are lower than the wages attainable in the destination country for skill levels below $\bar{S}_0$. Third, assume that returns to skill are steeper in $D$ than in $O$ for employed workers, consistent with the evidence in Table 1 showing higher wage inequality in Germany than in Eastern Europe prior to the reform. Fourth, assume that migration costs are substantial, for example, due to a high disutility from separation from family and friends.

As a result, wages in $D$ are too low for most employed workers (up to skill level $\bar{S}_2$) to offset migration costs. High-skilled workers nonetheless migrate in response to higher returns to skill in the destination country, as in the standard Roy–Borjas model. At the same time, if unemployment benefits in $O$ fall below wages available in $D$, the lowest-skilled workers also have incentives to migrate. This configuration generates migration at both tails of the skill distribution, consistent with the

empirical baseline shown in Table A.2.

Panel B illustrates the post-reform equilibrium. The minimum wage attracts additional workers from the lower part of the skill distribution, but not from the very bottom, as these workers had already migrated prior to the reform due to unemployment in $O$. Instead, new inflows arise primarily among low- to medium-skilled workers with skill levels between $\bar{S}_0$ and $\bar{S}_1$. Migration remains unattractive for intermediate skill levels between $\bar{S}_1$ and $\bar{S}_2$, and high-skilled migration above $\bar{S}_2$ is also unaffected by the reform.

## 4.3 Predictions

The framework delivers clear qualitative predictions about heterogeneity in immigration responses across origin countries. For workers from lower-wage EU member states such as those in Eastern Europe, the German minimum wage represents a large relative increase in potential earnings and, combined with free labor mobility, can generate a substantial immigration response among low- and potentially medium-skilled workers. By contrast, responses should be weak or absent for workers from high-wage origin countries, where bottom-end wages attainable at home are similar to or exceed the German minimum wage, leaving insufficient earnings gains to offset migration costs. Immigration from outside the EU is likewise unlikely to respond, as administrative and legal barriers such as visa requirements act as high fixed migration costs in the Roy–Borjas framework; even sizeable increases in potential destination earnings are then unlikely to induce additional migration.

These considerations pin down the expected direction of immigration responses but leave their magnitude and skill composition indeterminate. For migration to occur, the minimum wage must raise expected earnings above the total cost of migration, which includes hard-to-measure non-pecuniary components such as separation from family and friends. How many potential migrants lie close to this margin, and how this group is composed, are therefore important empirical questions that our paper addresses.

# 5 Empirical Strategy

## 5.1 Measuring Regional Exposure to the Minimum Wage

Our empirical strategy exploits the fact that Germany introduced a uniform national minimum wage, despite substantial regional heterogeneity in productivity and pre-reform wage levels. As a result, the minimum wage was more binding in some regions than in others. This continuous spatial variation in exposure generates plausibly exogenous differences in treatment intensity that can be used to identify the causal effects of the policy (Card, 1992).

We measure regional exposure to the minimum wage using the so-called gap measure, which has been widely used in the literature on minimum wages (Draca, Machin, & Van Reenen, 2011; Dustmann et al., 2022). Intuitively, it captures the proportional increase in the average wage bill that would occur in a region if all wages below the statutory minimum were mechanically raised to the minimum wage level, holding employment and hours constant. Regions with a larger share of low-wage workers therefore exhibit higher values of the gap and are more strongly exposed to the reform.

Formally, the gap measure is defined for each district $r$ and pre-reform year $t$ as

$$GAP_{rt} = \frac{\sum_{i \in r} h_{it} \max\{0, MW - w_{it}\}}{\sum_{i \in r} h_{it} w_{it}}, \tag{1}$$

where $h_{it}$ denotes weekly hours worked by employee $i$ in year $t$, $w_{it}$ denotes the hourly wage, and $MW$ is the statutory minimum wage of €8.50 per hour. The numerator aggregates the total hourly wage shortfall below the minimum wage within a district, while the denominator scales this shortfall by the district's total pre-reform wage bill. The calculation includes all employees in the sample, not only migrant workers.

To reduce sensitivity to transitory fluctuations, we average the district-level gap measure over the pre-reform years 2012, 2013, and 2014. Weighted by district employment size, the mean gap across districts is 0.014, with a standard deviation of 0.009. These moments are very close to those reported by Dustmann et al. (2022) who use the identical measure and data source.

Figure 2 illustrates the spatial distribution of district-level exposure to the minimum wage reform across Germany. Districts are shaded according to the intensity

of the gap measure, with darker colors indicating stronger exposure. The map reveals pronounced regional heterogeneity. All districts in the former East Germany are among the most strongly affected by the policy.[7] By contrast, the least affected districts are concentrated predominantly in Southern Germany. Districts in Western and Northwestern Germany occupy an intermediate position, with most exhibiting moderate exposure and a smaller subset experiencing relatively strong exposure.

## 5.2 Difference-in-Differences Specification

The pronounced regional heterogeneity in exposure to the minimum wage reform motivates a difference-in-differences design (DiD) for causal identification. We estimate the impact of the reform on a range of outcomes, including wages, immigrant employment, and migration inflows and outflows, using the following fully dynamic event-study regression:

$$\ln(Y_{rt}) = \sum_{z \neq 2014} \beta_z \, Gap_r \cdot \mathbf{I}(t = z) + \alpha_r + \gamma_t + \sum_{z \neq 2014} \delta_z \, Z_r \cdot \mathbf{I}(t = z) + \varepsilon_{rt}, \quad (2)$$

where $Y_{rt}$ denotes the outcome of interest in district $r$ and year $t$ and the coefficients of interest, $\beta_z$, are identified from interactions between the treatment intensity variables ($Gap_r$) and year indicators for the period 2012–2016. We omit the year 2014, which was the last pre-reform year. The specification includes district fixed effects ($\alpha_r$), which absorb time-invariant regional characteristics. The year fixed effects ($\gamma_t$) capture common aggregate shocks.

To account for the possibility that districts with different initial productivity follow systematically different time trends in wages and migration outcomes, we control for interactions between the year indicators and a vector of pre-reform district characteristics, denoted by $Z_r$. Specifically, $Z_r$ comprises population density and the shares of low- and medium-skilled workers in the district, averaged over the pre-reform period 2012–2014.[8]

[7] We exclude all districts in the former East Germany in a robustness check and still find significant effects of the introduction of the minimum wage on Eastern EU immigration.

[8] We omit interactions between the year indicators and the share of high-skilled workers to avoid perfect multicollinearity.

## 5.3 Identifying assumptions

The key identifying assumption underlying our approach is that, in the absence of the minimum wage introduction, districts with different levels of exposure, as measured by the gap variable, would have followed parallel trends in migration outcomes. We assess the plausibility of this assumption empirically by examining the pre-reform coefficients in the event-study specification. As shown in the next section, these coefficients are small in magnitude and statistically indistinguishable from zero, providing support for the parallel trends assumption.

Moreover, the identification strategy assumes limited migration spillovers across districts, such that changes in outcomes in one district are not mechanically driven by treatment intensity in other districts. A potential concern is that immigrant workers may relocate from less exposed to more exposed districts following the reform, generating correlated changes in migration outcomes across space. Several features of our setting suggest, however, that such spillovers are likely to be limited. First, Table A.3 shows that the vast majority of observed inflows into districts consists of newly arriving workers rather than reallocation across German districts, reducing the scope for spillover effects driving our results. Second, prior evidence by Dustmann et al. (2022) indicates that worker reallocation following the introduction of the minimum wage occurred predominantly within rather than between districts.

## 5.4 Definitions and Measurement of Immigration Outcomes

**Immigration headcounts.** To capture immigration responses to the minimum wage, we measure the stock, inflows, and outflows of immigrant workers at the district level in full-time equivalents (FTEs). Following Dustmann et al. (2022), full-time, part-time, and marginal employment are weighted by 1, 0.5, and 0.2, respectively. This weighting scheme approximates the relative distribution of average daily working hours under a seven-day working week.

**Inflows and outflows.** Total inflows to district $r$ in year $t$ consist of immigrant workers employed in district $r$ in year $t$ who were not employed in the same district in year $t-1$. We distinguish three components. First, new inflows comprise immigrant workers entering the German labor market for the first time since 1975. Second,

inflows from other German districts capture workers employed in district $r$ in year $t$ who were employed in a different German district in year $t-1$. Third, residual inflows include workers who were not employed in year $t-1$ but had prior employment in Germany since 1975. This group includes individuals who were previously unemployed, non-employed, in education, or temporarily abroad.

Total outflows from district $r$ in year $t$ include immigrant workers employed in district $r$ in year $t$ who are no longer employed in the same district in year $t+1$. We distinguish between outflows to other German districts and outflows to non-employment. The latter group includes workers whose labor force status is unobserved in year $t+1$, such as individuals who returned to their home country, became unemployed or non-employed, or entered education.

# 6 Results

## 6.1 Effects on Wage

We begin by examining compliance with the introduction of the minimum wage. To this end, we estimate the reform's impact on district-level average hourly wages of all employees. Figure 3 plots the event-study estimates of the coefficients $\beta_z$ from Equation (2). Prior to the reform, average wages evolved similarly in more and less exposed districts, with all pre-reform coefficients being close to zero and statistically insignificant. Following the introduction of the minimum wage in 2015, average hourly wages increased more strongly in districts with higher exposure to the reform. The estimated effect grows further in 2016, approaching a coefficient of approximately one. This suggests that nearly full compliance with the minimum wage had been reached by 2016. Our independent estimates closely align with existing evidence for Germany based on the same data source and identification strategy (Dustmann et al., 2022).

## 6.2 Effects on Employment of Eastern EU Immigrants

We next examine whether the wage increases induced by the minimum wage translated into higher immigration. Figure 4 plots event-study estimates of the reform's

effect on the stock of Eastern EU workers, measured in full-time equivalents (FTEs). As before, we find no evidence of differential pre-reform trends across districts with varying exposure, supporting the identifying assumption. Following the introduction of the minimum wage, the stock of Eastern EU workers grows more strongly in districts with higher exposure, with the effect becoming statistically significant by 2016. The absence of a significant effect in 2015 may reflect incomplete initial compliance or adjustment frictions, such as delayed diffusion of information about the wage increases among potential migrants in Eastern Europe.

When wages are the outcome, the GAP measure yields a straightforward interpretation of the event-study coefficients. For employment outcomes, however, the coefficients are not directly informative about economically meaningful magnitudes. We therefore standardize the GAP measure when analyzing employment outcomes such as the log stock or log inflows of immigrants, so that estimates can be interpreted as the effect of a one-standard-deviation increase in treatment exposure. On this scale, Figure 4 implies that a one-standard-deviation increase in exposure raises the stock of Eastern EU workers in a district by approximately 3% two years after the reform.

To assess the aggregate importance of this average district-level response, we next aggregate the district-level effects to the national level using a simple accounting exercise. From Equation (2), the change in the stock of immigrants in district $r$ between 2014 and 2016 can be written as

$$\ln Y_{r,2016} - \ln Y_{r,2014} = \beta_{2016}\,\mathrm{GAP}_r.$$

Exponentiating both sides yields the corresponding level ratio,

$$\frac{Y_{r,2016}}{Y_{r,2014}} = \exp\big(\beta_{2016}\,\mathrm{GAP}_r\big),$$

Rearranging and aggregating across districts yields the implied national-level change in the stock of Eastern EU workers $\Delta Y$:

$$\Delta Y = \sum_{r=1}^{R} \Big( \exp\big(\beta_{2016}\,\mathrm{GAP}_r\big) - 1 \Big)\, Y_{r,2014}. \tag{3}$$

Estimating Equation (3) suggests that the minimum wage introduction increased the stock of Eastern EU workers in Germany by approximately 20,070 FTEs. This corresponds to an increase of about 4% relative to the 2014 baseline of 509,141 Eastern EU workers (see Table 2).

## 6.3 Effects on Employment of Germans

An increase in immigration following the introduction of the minimum wage could, in principle, displace (low-productivity) native German workers. If such displacement occurred, the welfare implications of the policy would differ markedly from a setting with no adverse employment effects on natives. Figure 4 therefore also presents event-study estimates of the impact of the minimum wage on the stock of native German workers. We find precisely estimated null effects throughout the post-reform period. This evidence indicates that the increase in immigration induced by the minimum wage did not come at the expense of employment opportunities for native German workers. This finding is also reassuring because it suggests that the rise in employment of Eastern EU immigrants was not driven by unobserved positive shocks in districts highly exposed to the minimum wage that would have raised employment more generally.

## 6.4 Inflows vs. Outflows

An increase in the stock of Eastern EU workers can arise through higher inflows, lower outflows, or a combination of both. Distinguishing between these mechanisms is important for assessing whether the minimum wage primarily attracts new immigrants or instead reduces exits among incumbent immigrant workers. We therefore separately examine the effects of the minimum wage on inflows and outflows of Eastern EU workers, as defined in Section 5.4.

Figure 5a presents event-study estimates for total immigrant inflows into a district. As in the previous analyses, we find no evidence of differential pre-reform trends across exposure levels. Following the reform, however, inflows increased more strongly in districts with higher exposure, with the effect turning positive and statistically significant in 2016. The figure also assesses the effects on first-time inflows of immigrants who were not previously observed as employed in the German labor market. The estimated response for first-time inflows is even larger than that for total inflows, indicating that the post-reform increase in immigrant employment is driven primarily by new arrivals from abroad.

Consistent with this interpretation, Figure 5b shows no statistically significant effect of the minimum wage on immigrant outflows. This finding suggests that the

reform did not operate through improved retention of incumbent immigrants, but rather through increased attraction of new immigrant workers.[9]

To assess the economic magnitude of the estimated effect on new inflows, we again aggregate the district-level effects to the national level using Equation (3). The results reported in Table 2 suggest that the minimum wage attracted 20,313 new immigrants from Eastern EU countries, which is sufficient to fully account for the estimated increase in the total stock of immigrants. The table further shows that, in relative terms, the effect on new inflows is substantially larger than the effect on the total stock. Relative to a baseline of 153,554 new inflows of Eastern EU immigrant workers in 2014, the minimum wage increased the volume of this flow by 13%.

In light of these findings, we focus on inflows of new immigrants in the remainder of the analysis. We think that this outcome also most closely corresponds to common notions of immigration.

## 6.5 The Role of Cross-Border Commuting

Germany borders the Czech Republic and Poland to the east, two countries with substantially lower average wages. Workers living near the border can commute to Germany, implying particularly low migration costs, and prior studies show that this group is highly responsive to policy changes affecting the costs and returns to labor migration (Dustmann, Schönberg, & Stuhler, 2017; Illing, 2025). The introduction of the German minimum wage could therefore induce a pronounced labor supply response among potential cross-border commuters.

To examine this mechanism, we restrict the estimation sample to German districts bordering Poland or the Czech Republic and define a cross-border commuter as a Polish or Czech national employed in a bordering German district whose registered place of residence is abroad. Poland borders nine German districts, while the Czech Republic borders sixteen German districts. We estimate the effects of the minimum wage separately for new cross-border commuters from the Czech Republic

[9] In Section 7, we further assess the robustness of this result by ruling out effects on internal migration of incumbent immigrants across German districts, as well as transitions from non-employment among immigrants previously observed working in Germany.

and Poland.[10]

As shown in Figure 2, German districts bordering the Czech Republic display substantial variation in exposure to the reform, with gap measures ranging from 0.010 in high-wage Southern border districts to 0.049 in Eastern border districts. This heterogeneity provides a favorable setting for identifying the effect of the minimum wage on cross-border commuting from the Czech Republic. Districts in Eastern Germany, in particular, would be expected to experience a stronger inflow of new Czech cross-border commuters following the introduction of the minimum wage. This is precisely what we find. Figure 6a shows a sharp increase in inflows of new Czech cross-border commuters in more exposed districts after the reform, with no evidence of a pre-trend prior to 2015.

In contrast, we find no statistically significant increase in inflows of cross-border commuters from Poland following the minimum wage introduction (Figure 6b). We caution, however, against over-interpreting this null result. All German districts bordering Poland are highly exposed to the reform, with gap measures ranging from 0.030 to 0.056. This limited variation in treatment intensity, combined with the small number of districts, substantially restricts our ability to identify differential effects for Polish cross-border commuters.

The coefficient estimates from the cross-border commuting regressions are large, reflecting relative changes within small subpopulations. To assess the economic magnitude of these effects, we again aggregate the district-level estimates to the national level using Equation (3). The results reported in Table 2 suggest that the minimum wage attracted 2,085 additional cross-border commuters from the Czech Republic. Relative to a baseline of 3,483 new Czech cross-border commuters in 2014, this corresponds to a sizable increase of nearly 60%. Nevertheless, because the Czech Republic is a relatively small country, these additional commuters account for only 10% of the total immigrant inflow induced by the minimum wage. Table 2 also reports an estimated inflow of approximately 561 additional cross-border commuters from Poland; however, this estimate is statistically insignificant.

[10]Given the small number of district–year observations in the border-region panels, which leaves limited degrees of freedom for estimating Equation (2), we omit the interactions between year fixed effects and district-level population density and skill composition in this exercise.

## 6.6 Heterogeneity by Labor Market Concentration

In our theoretical framework, a positive migration response to the minimum wage is plausible only if the policy does not generate employment losses large enough to offset the effect of higher wages on immigrants' expected earnings. Most studies evaluating the employment effects of the German minimum wage find statistically insignificant effects on the number of regular jobs subject to social security contributions, which is the type of employment most relevant for potential immigrants considering a move to Germany (for a systematic overview, see Dütsch, Ohlert, and Baumann (2025)). The absence of negative employment effects is commonly rationalized by the presence of monopsonistic competition in labor markets (Manning, 2003). Consistent with this interpretation, recent evidence shows that minimum-wage employment effects tend to be more positive in local labor markets with a higher degree of concentration, which is often used as a proxy for local monopsony power (Azar et al., 2024).

We follow this approach by examining heterogeneity in the effects of the minimum wage on immigrant inflows from Eastern EU countries across districts that differed in labor market concentration prior to the reform. We measure local labor market concentration at the district × 3-digit industry level using the Herfindahl–Hirschman Index (HHI) and aggregate this measure to the district level using employment weights.[11] We then split the 400 districts in our sample at the employment-weighted median of the HHI into relatively low- and high-concentration local labor markets. The median HHI is 1,778, which lies very close to the lower threshold of 1,800 used in the most recent US Horizontal Merger Guidelines to classify markets as highly concentrated (U.S. Department of Justice & Federal Trade Commission, 2023).

To estimate heterogeneous effects, we re-estimate a simplified difference-in-differences version of Equation (2) in which all regressors are fully interacted with an indicator for above-median pre-reform labor market concentration.[12] Column (1) of Table 4 reports estimates for districts with below-median concentration, while column (2) reports estimates for districts with above-median concentration. In low-concentration

[11] Additional details on the construction of the HHI are provided in Appendix D.

[12] Specifically, for ease of exposition, we collapse the pre- and post-reform years into a single pre- and post-reform indicator, omitting the reference year 2014 as before. This amounts to moving from a fully dynamic event-study specification to a standard DiD framework.

districts, the estimated effect of the minimum wage on immigrant inflows is positive but small and statistically insignificant. In contrast, in high-concentration districts, the estimated effect is an order of magnitude larger and statistically significant. In neither group of districts is there evidence of differential pre-trends with respect to subsequent exposure to the minimum wage.

Taken together, these results suggest that the minimum wage draws additional immigrants from Eastern Europe primarily into monopsonistic local labor markets. The policy likely limits employers' ability to exercise market power, thereby shifting employment upward along the firm-level labor supply curve and closer to the competitive equilibrium (Naidu & Posner, 2022). As we do not find effects on native employment, this upward shift appears to be driven by immigrant workers who increase their labor supply due to the positive wage effect. To our knowledge, this study provides the first direct empirical evidence for this mechanism, which has previously been discussed only at a theoretical level (Dube & Lindner, 2024).

## 6.7 Selection of Immigrants

The Roy–Borjas framework outlined in Section 4 predicts that the introduction of a binding minimum wage in a destination country increases immigration primarily by raising the returns to low-skilled work. As a result, the policy should attract additional immigrants from the lower end of the skill distribution in the origin country. If the minimum wage is set sufficiently high, it may also make immigration attractive to medium-skilled workers. We test these predictions by examining changes in the skill composition of new inflows from Eastern EU countries. Specifically, we distinguish between three levels of educational attainment: workers without vocational training or a university degree (low-skilled), workers with at least some vocational training (medium-skilled), and workers with a university degree (high-skilled).

Table 3 shows a clear post-reform increase in inflows of low- and medium-skilled workers into more exposed districts (columns 1 and 2). In contrast, we find no statistically significant effect on inflows of high-skilled workers from Eastern EU countries following the introduction of the minimum wage (column 3). None of the pre-reform coefficients is statistically significant, supporting a causal interpretation of the results.

While this pattern is broadly consistent with the theoretical framework, it is somewhat surprising that the estimated effects for medium-skilled immigration are stronger than those for low-skilled immigration. Several plausible mechanisms may account for this finding. First, the German minimum wage had a particularly strong "bite" relative to wage levels in Eastern EU countries, implying that many medium-skilled workers in these countries could substantially increase their earnings by taking up minimum-wage employment in Germany. Second, prior evidence indicates that employers in Germany raised their hiring standards in response to the minimum wage introduction (Butschek, 2022), plausibly increasing demand for more productive and hence medium-skilled workers from abroad even when jobs are paid at the minimum wage. Third, skill transferability from Eastern Europe to Germany is likely imperfect due to language barriers or the non-recognition of formal qualifications (Brücker et al., 2021), which may be especially relevant in the context of increased hiring standards. Finally, a growing body of evidence, including from the German reform, documents spillover effects of minimum wages to higher parts of the wage distribution (Autor, Manning, & Smith, 2016; Bossler & Schank, 2023). Such spillovers could, in principle, attract medium-skilled immigrants by increasing returns to medium-skilled work in Germany relative to their home countries.

To assess the relative importance of these mechanisms, we examine the types of occupations in which minimum-wage-induced immigrants are employed. Using the fifth digit of the occupation codes in the BeH data, which classifies jobs by task complexity, we assign immigrants to one of three task-complexity categories based on their first job in Germany.[13] We find that the increase in immigration is confined to occupations with relatively low task complexity (column 4), with no evidence of increased inflows into medium- or high-complexity occupations (columns 5 and 6). This pattern suggests that the minimum wage attracts both low- and medium-educated workers from Eastern Europe into low-complexity jobs, consistent with the first three mechanisms discussed above. By contrast, we find no evidence that spillover effects of the minimum wage to more complex occupations are sufficiently strong to directly induce additional immigration into medium- or high-complexity

[13]See the note to Table 3 for details on the measurement of task complexity using the BeH occupation codes.

jobs.

# 7 Additional Results and Robustness Checks

## 7.1 Alternative Specifications

This subsection examines the robustness of our main findings to alternative empirical specifications. For ease of comparison, we summarize the event-study estimates from Equation (2) by reporting average pre- and post-policy coefficients rather than year-specific effects. Table 5 presents the results for two outcomes: new inflows from Eastern EU countries (Panel A), our main outcome of interest, and hourly wages of full-time employees (Panel B), which serve as our "first stage." Column (1) reproduces the baseline estimates. Consistent with our main results, it shows no evidence of differential pre-trends and sizable post-reform effects on both wages and immigrant inflows.

To show that our results do not depend on the choice of specific control variables, column (2) omits the interactions between year fixed effects and district-level population density and skill composition. This change leaves the inflow estimates largely unaffected, with no significant pre-trend and a slightly larger post-policy effect. For wages, however, a statistically significant pre-trend emerges, suggesting stronger wage growth in more exposed districts prior to the reform. We therefore view the baseline specification as more conservative and prefer it as our main specification.

Column (3) addresses the concern that unobserved time-varying differences between Eastern and Western Germany may drive the results, as districts in former East Germany are more strongly exposed to the reform (see Figure 2). Excluding these districts yields a smaller wage effect, consistent with the weaker bite of the minimum wage in former West Germany, while the estimated effect on immigrant inflows becomes larger but less precise. Given the increase in standard errors, we refrain from overinterpreting this heterogeneity. Overall, the results indicate that our findings are not driven exclusively by districts in former East Germany or by long-running migration trends related to German division.

Column (4) addresses a related concern that districts closer to Germany's eastern border may follow different unobserved migration trends. Allowing year effects to

vary with distance to the Polish or Czech border leaves the estimates essentially unchanged, indicating that border proximity does not confound our results.

Column (5) implements an alternative treatment definition based on a median split of the gap distribution. comparing districts above the median (more affected) with those below the median (less affected). The smaller coefficient magnitudes reflect this change in definition rather than a substantive difference. Consistent with the baseline results, districts most affected by the minimum wage experienced inflows from Eastern EU countries that were approximately 8% higher than those in the least affected districts, with no evidence of differential pre-trends. Wage effects remain positive. For wages we only find a weak pre-trend that is economically negligible. This very modest pre-trend in wages nevertheless complicates the interpretation, however, and further motivates our preference for the baseline specification.

Finally, to facilitate comparison with the country-specific estimates in the following section, where we employ a Poisson model because the outcome variables contain zeros, we re-estimate our baseline specification using a Poisson model. Reassuringly, the Poisson estimates shown in column (6) are very close to our baseline results.

**Immigration from Other Source Countries.** Next, we examine whether the minimum wage reform affected new inflows from other migrant groups: workers from Northern and Western EU countries, Southern EU countries, non-EU countries, migrants overall, and Syrians—the latter given that Germany experienced a large refugee inflow from Syria around the time of the reform.[14] For ease of exposition, we present coefficients averaged across the pre- and post-reform periods rather than year-specific estimates for each of the six outcomes. Figure 7 shows that, besides the previously documented positive effect on inflows from Eastern Europe, there is a positive effect of similar magnitude on inflows from Southern Europe. Results for Western Europe, non-EU countries, and Syria are inconclusive, with estimated policy effects that are generally statistically insignificant or display pronounced pre-trends. Further analysis reveals that the positive effect on Southern

[14]Because inflows from Syria are zero in many district–year cells prior to 2015, we estimate Poisson models for Figure 7 rather than a log-linear specification, as recommended by Chen and Roth (2024). The set of regressors is identical to that in the baseline specification in Equation (2).

European inflows is driven exclusively by immigration from Greece (Figure A.2). Greece experienced severe economic distress during the 2010s, as the sovereign debt crisis brought the country close to default and youth unemployment reached record levels. It is therefore plausible that Greek workers unable to find employment at home were attracted by the German minimum wage and a labor market operating near full employment. Our accounting exercise using Equation (3) suggests that approximately 1,500 new FTE workers from Greece were attracted by the reform in addition to the 20,000 workers from Eastern Europe.

**Country-Specific Immigration from Eastern Europe.** This subsection examines whether the increase in new inflows from Eastern EU countries documented above is driven by specific origin countries. We estimate country-specific effects of the minimum wage reform on new inflows from each Eastern EU country.[15] Figure A.3 reports the results. Again, for ease of exposition, we present coefficient estimates averaged across the pre- and post-reform years, respectively. We find positive policy effects for 8 of the 10 Eastern EU countries in our sample, suggesting that the minimum wage affected immigration from Eastern Europe broadly. In several cases, however, these estimates are not statistically significant, such as for Bulgaria and Estonia. In two cases, Hungary and Slovenia, we detect statistically significant pre-trends. The clearest positive immigration responses are observed for Germany's Eastern neighbors, the Czech Republic and Poland, where we find no evidence of pre-trends and statistically significant policy effects. Overall, the results suggest that the immigration response to the German minimum wage was driven to a considerable extent, though not exclusively, by inflows from the Czech Republic and Poland.

**Internal Reallocation.** In principle, the increase in total immigrant inflows into districts more affected by the minimum wage could also reflect internal reallocation of incumbent immigrants from other German districts rather than new immigration exclusively. If so, the minimum wage would not increase the total number of immigrants in Germany, but would merely reshuffle existing immigrant workers across

[15] We estimate a Poisson model again in this exercise because new inflows from several smaller countries are zero in many district–year cells.

locations. A natural test of this surmise is to examine inflows of immigrants arriving directly from other German districts as an outcome of Equation (2). Figure A.4a reports the corresponding event-study estimates. The results show a clear positive pre-trend and no discrete increase at the time of the reform, ruling out internal reallocation of immigrants across German districts as a driver of our main findings.

For completeness, Figure A.4b examines the residual component of total inflows after excluding new inflows from abroad (the main outcome of interest) and inflows from other German districts. This residual captures re-entries of immigrants who had previously been employed in the focal district but were not employed immediately prior to the reform, for example because they temporarily exited employment to claim unemployment benefits or returned to their home countries. We again find no evidence that these inflows from non-employment were affected by the introduction of the minimum wage.

**Heterogeneity by Gender.** As an additional robustness exercise, we examine heterogeneity in the inflow response by gender. Figure A.5 presents the results separately for men and women. The estimates indicate that the relative post-reform increase in new inflows is substantially larger for men (Panel a) than for women (Panel b). In contrast to men, female inflows show little immediate response at the time of the minimum wage introduction. The effect becomes significantly positive roughly two years after the reform, albeit at a markedly smaller magnitude than for men. This pattern is consistent with the idea that labor market frictions are larger for women than for men. If commuting and relocation costs are higher for women due, for instance, to family responsibilities or stronger local ties, then female migration decisions may adjust more slowly and less strongly to changes in expected earnings. This explanation is widely supported by the evidence in Le Barbanchon, Rathelot, and Roulet, 2021, Caldwell and Danieli, 2024, and Agrawal, Janeba, and Jahn, 2024 who find that the marginal costs of commuting for women are indeed higher.

**Medium-Run Effects.** A natural question is whether the effects documented above were merely temporary. Addressing this question is challenging for several reasons. First, the national minimum wage was increased incrementally over time,

from €8.50 to €8.84 in 2017 and to €9.19 in 2019. Prior research shows that the districts most affected by the initial introduction of the minimum wage are not necessarily those most affected by subsequent increases (Caliendo, Olthaus, & Pestel, 2025). Second, we observe administrative data on hours worked only up to 2014, which limits our ability to measure exposure to later minimum wage increases as precisely as exposure to the initial reform. Third, the onset of the Covid-19 pandemic in early 2020 led to a sharp and prolonged interruption of immigration into Germany, rendering an extension of the analysis into the 2020s largely uninformative.

For these reasons, we focus primarily on the short-run immigration effects of the minimum wage introduction through 2016, which we can identify most cleanly. Nevertheless, we provide suggestive evidence on potential medium-run effects by extending our event-study analysis through 2019, while emphasizing that these results should be interpreted with some caution.

Figure A.6a shows that average hourly wages continued to grow more strongly after 2016 in districts that were more exposed to the minimum wage introduction. In particular, the acceleration in wage growth between 2016–2017 and 2018–2019 suggests that the GAP measure continues to capture variation related to minimum wage increases, albeit probably imperfectly. We also find that both the stock and inflows of workers from Eastern EU countries continued to rise in more exposed districts through 2017. Thereafter, the immigration response slows and appears to level off. This pattern suggests that districts more affected by the minimum wage introduction experienced a persistent upward shift in the employment of immigrants from Eastern Europe.

# 8 Conclusion

This paper shows that the introduction of a national minimum wage in Germany in 2015 increased the number of immigrant workers from Eastern European EU countries. The increase was driven entirely by new inflows rather than reduced outflows, and the additional immigrants were low- to medium-skilled. These findings are consistent with the predictions of a simple Roy–Borjas framework in which the introduction of a binding minimum wage draws immigrants from the lower end of the

skill distribution if there are no large negative employment effects. The immigration response was particularly pronounced for workers from neighboring Poland and the Czech Republic, underscoring the importance of mobility costs, which play a central role in the Roy–Borjas model. Among Czech workers, we additionally observe an increase in cross-border commuting following the reform.

Immigration effects were concentrated in districts characterized by elevated labor market concentration prior to the reform, suggesting that minimum wages induce immigration into monopsonistic labor markets. Consistent with this interpretation, the increase in immigrant employment did not come at the expense of employment losses among native workers.

Taken together, our findings advance the understanding of how institutional wage setting shapes cross-border labor mobility in economically integrated regions such as the EU. From a policy perspective, the results indicate that minimum wage policies can increase the attractiveness of destination labor markets for immigrants without displacing native workers. More broadly, the observed rise in migrant inflows suggests that wage floors influence not only wages and the reallocation of incumbent workers (Dustmann et al., 2022), but also labor supply and migration patterns, particularly in local labor markets characterized by monopsonistic structures.

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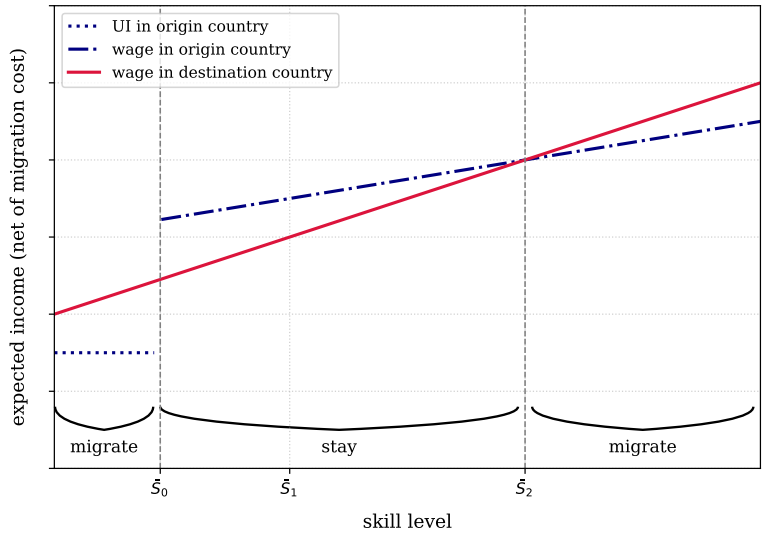
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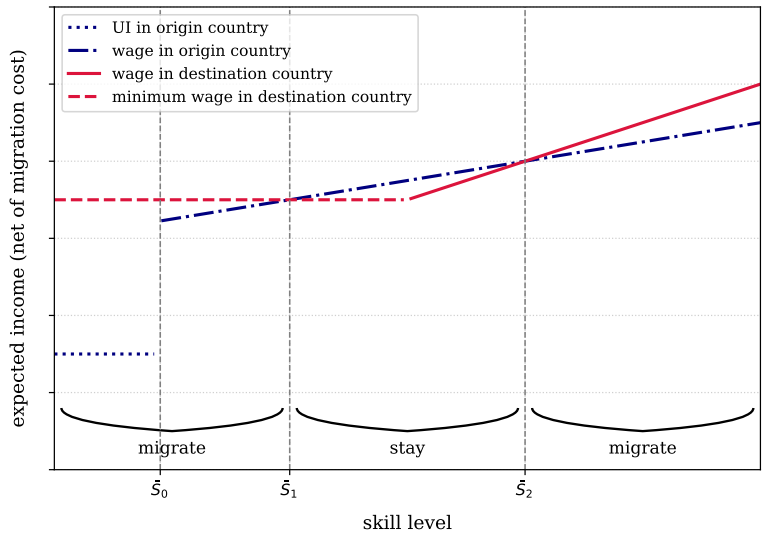
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# Figures

Figure 1: Minimum Wage Effects on Immigration When Employment Effects are Non-Negative in the Roy-Borjas Model



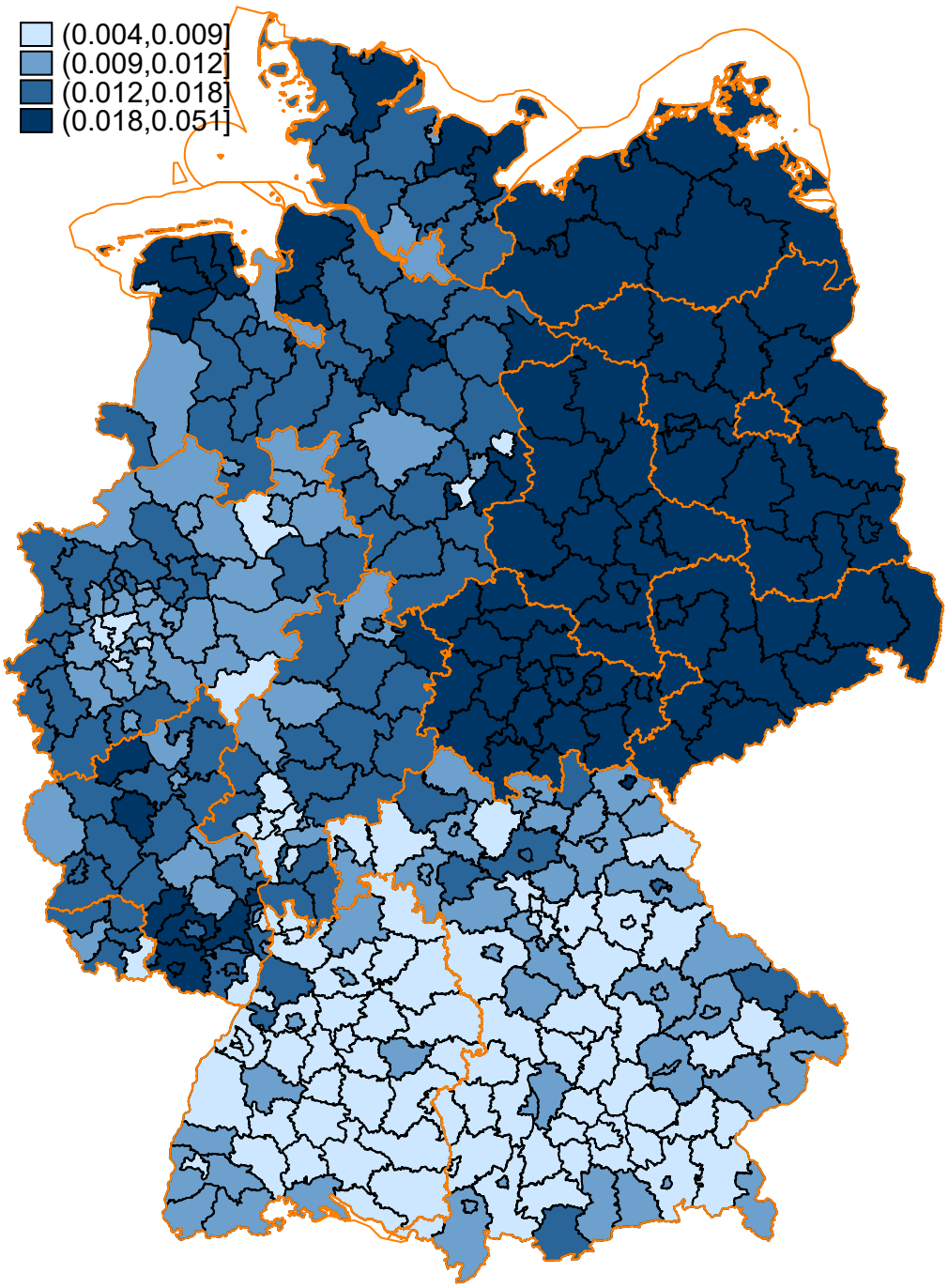

(a) Immigration with Low-Skilled Unemployment in O




(b) Immigration with Low-Skilled Unemployment in O and a MW in D

*Notes:* This figure illustrates how the introduction of a binding minimum wage in the destination country affects migration decisions in a simple extension of the Roy–Borjas model. Panel (a) depicts a setting with unemployment among low-skilled workers in the origin country. In this case, low-skilled individuals may migrate due to limited employment opportunities at home, while high-skilled individuals migrate in response to steeper returns to skill in the destination country. Panel (b) introduces a binding minimum wage in the destination country. If the minimum wage does not reduce employment, it raises expected income for low- to medium-skilled workers and induces additional migration along this margin.

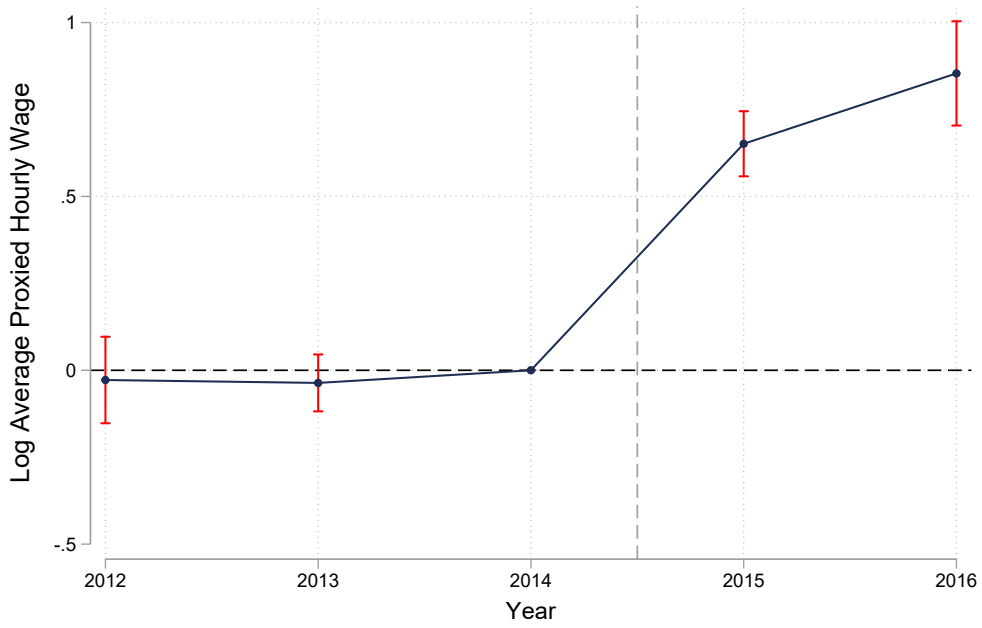
Figure 2: Minimum Wage Exposure at District Level by Quartiles of Gap Intensity




*Notes:* This figure maps Germany's 400 districts and displays their exposure to the statutory minimum wage, measured by the gap intensity defined in Equation (1). Districts are grouped into quartiles of the gap distribution and shaded accordingly, with darker colors indicating greater exposure to the minimum wage. The gap measure is averaged over the pre-reform years 2012–2014. Orange lines delineate the borders of the 16 German federal states.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

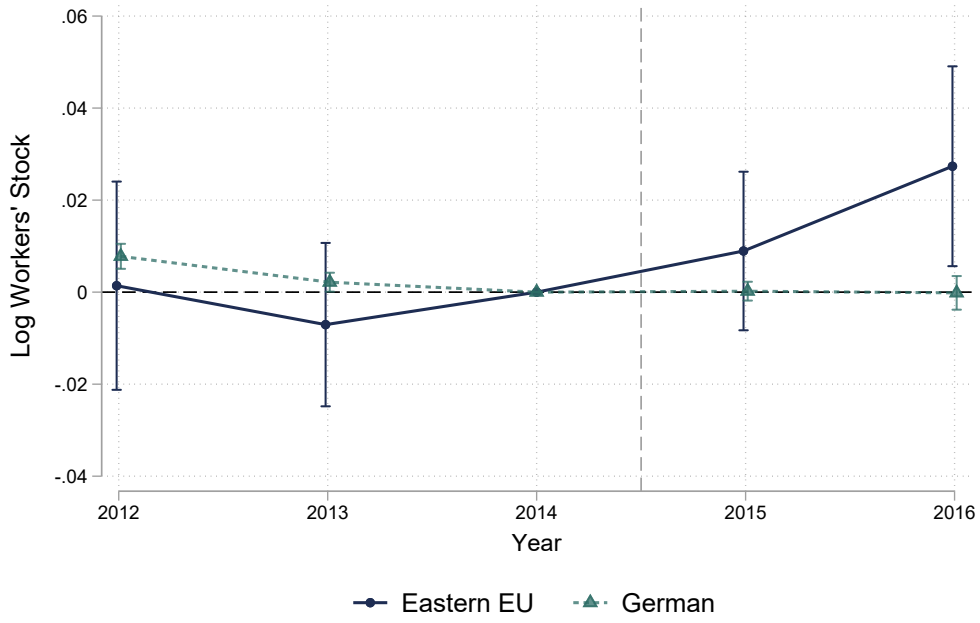
Figure 3: Effect of the Minimum Wage on Average Hourly Wages




*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) average hourly wages in a district. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Since our administrative data on hours end in 2014, we proxy hourly wages as daily wages divided by the average number of working hours in each working time category (full-time, part-time, and marginal employment). This follows the procedure in (Dustmann et al., 2022). More details are given in Appendix B.1. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

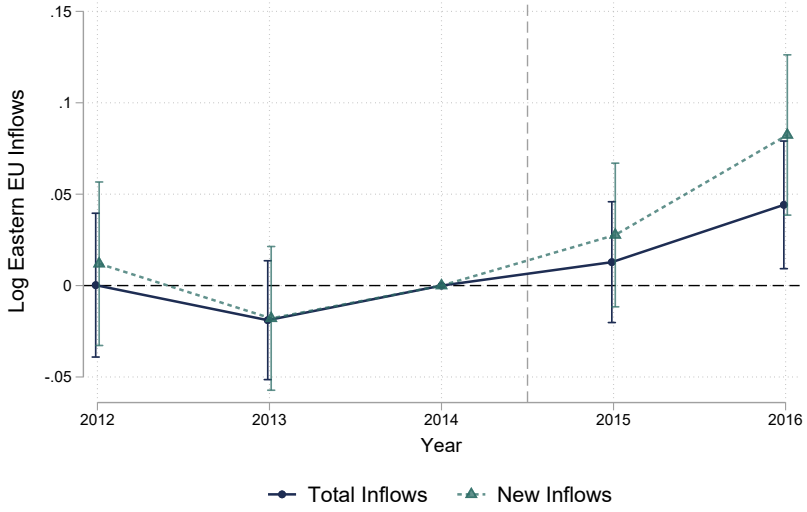
Figure 4: Effect of the Minimum Wage on the Employment of Eastern EU Workers and Natives in Germany



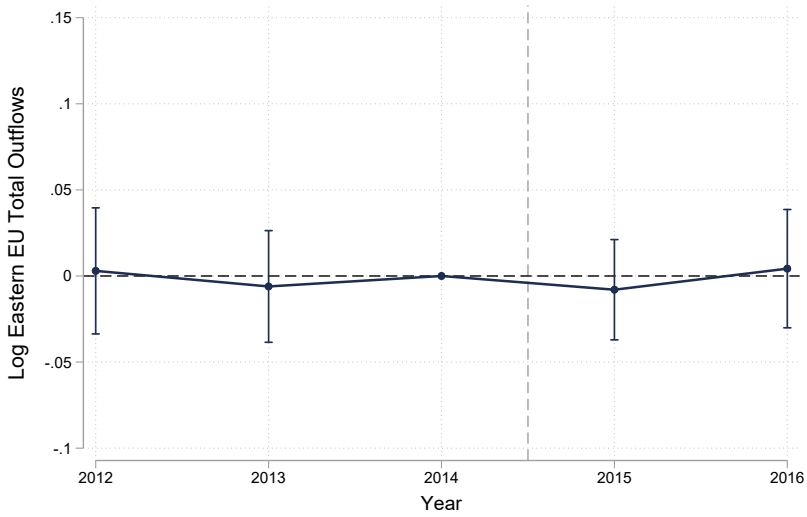

*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on the (log) stock of Eastern EU workers and the (log) stock of German workers. The outcome is defined as the total number of Eastern EU or German workers employed in a district. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals based on standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure 5: Effect of the Minimum Wage on Eastern EU Inflows and Outflows




(a) Inflows

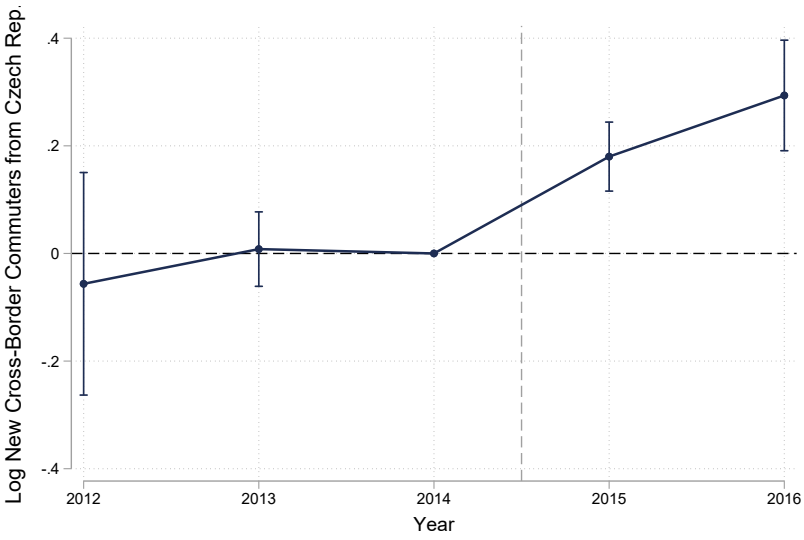



(b) Outflows

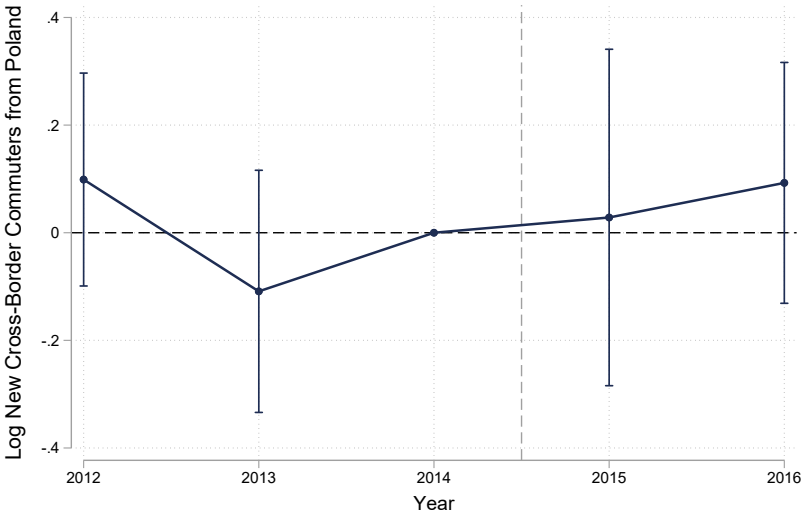
*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) inflows and (log) outflows of Eastern EU workers. Panel (a) analyzes total inflows (solid line), defined as Eastern EU workers employed in district $r$ in year $t$ who were not employed in that district in year $t-1$, and new inflows (dotted line), defined as Eastern EU workers employed in district $r$ in year $t$ who appear for the first time in the German employment records in year $t$. The new inflows are our preferred measure of immigration. Panel (b) analyzes total outflows, defined as the number of Eastern EU workers employed in district $r$ in year $t$ who are no longer employed in that district in year $t+1$. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure 6: Effect of the Minimum Wage on New Cross-Border Commuters from the Czech Republic and Poland




(a) Czech Cross-Border Commuters

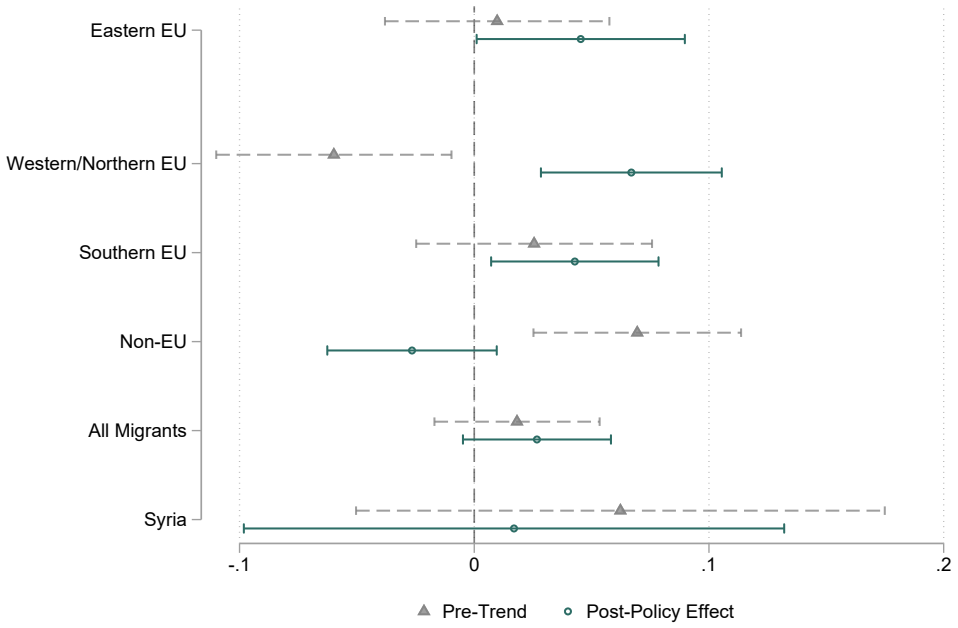



(b) Polish Cross-Border Commuters

*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on the (log) number of new cross-border commuters from the Czech Republic (Panel a) and Poland (Panel b), relative to the pre-policy year 2014. The sample is restricted to German districts bordering the Czech Republic or Poland. A cross-border commuter is defined as a Czech or Polish national who is employed in a German border district and whose residence is registered abroad. New cross-border commuters in year $t$ are individuals who appear in the German employment records for the first time in year $t$. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure 7: Effect of the Minimum Wage on New Inflows from Other Countries




*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) new inflows from different migrant groups, relative to the pre-policy year 2014. New inflows are defined as workers who appear in the German employment records for the first time in year $t$. Because the outcome variable contains zeros in some group–district–year cells, estimation is carried out using a Poisson regression model. For ease of exposition, the fully dynamic event-study specification is collapsed by aggregating all pre-reform years into a single pre-period and all post-reform years into a single post-period, while omitting the transition year 2014. The reported "Pre-trend" coefficients correspond to averages of the estimated pre-reform effects for the years 2012–2013, while the reported "Policy effect" coefficients correspond to averages of the estimated post-reform effects for the years 2015–2016. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level. Migrant groups are defined as follows. Eastern EU countries include Poland, the Czech Republic, Hungary, Slovenia, Slovakia, Lithuania, Latvia, Estonia, Bulgaria, and Romania. Western and Northern EU countries include Austria, Belgium, the Netherlands, Luxembourg, France, Denmark, Sweden, Finland, Ireland, and the United Kingdom. Southern EU countries include Italy, Spain, Portugal, Greece, Malta, and Cyprus. Non-EU countries include all remaining countries not covered by the above groups.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

# Tables

Table 1: Wage Distributions in Germany, Eastern Europe, and France in 2014

| | Mean Hourly Wage in € | SD Log Wage | Share Earning Below 8.50€/h |
|---|---|---|---|
| | (1) | (2) | (3) |
| Germany | 16.83 | 0.54 | 0.14 |
| Eastern Europe | 6.56 | 0.46 | 0.78 |
| France | 14.51 | 0.46 | 0.10 |

*Notes:* This table reports summary statistics of individual hourly wage distributions in selected European countries in 2014, one year prior to the introduction of the statutory minimum wage in Germany. "Eastern Europe" pools Bulgaria (BG), the Czech Republic (CZ), Estonia (EE), Hungary (HU), Latvia (LV), Lithuania (LT), Poland (PL), Romania (RO), the Slovak Republic (SK), and Slovenia (SI). Hourly wages are expressed in 2015 euros at German prices and adjusted for cross-country differences in purchasing power using price level indices provided by Eurostat (2026). Column (3) reports the share of workers earning below €8.50 per hour, the wage floor introduced in Germany in 2015.

*Source:* EU-SILC, `Scientific Use File, 2022 v2`; authors' calculations.

Table 2: Magnitudes of the Minimum Wage Induced Immigration Responses

| | Level in 2014 | Estimated MW-Induced Change, 2014–2016 | |
|---|---|---|---|
| | | Absolute | Relative |
| | (1) | (2) | (3) |
| Eastern EU Workers | 509,141 | 20,070 [ 4,058; 36,749] | 0.039 [ 0.008; 0.072] |
| Eastern EU New Inflows | 153,554 | 20,313 [ 9,081; 32,632] | 0.132 [ 0.059; 0.213] |
| New Commuters from Czech Rep. | 3,483 | 2,085 [ 1,108; 3,522] | 0.599 [ 0.318; 1.011] |
| New Commuters from Poland | 10,090 | 561 [ -476; 3,549] | 0.056 [ -0.047; 0.352] |

*Notes:* This table reports estimates from an accounting exercise based on Equation (3), quantifying the aggregate change in immigration outcomes between 2014 and 2016 attributable to the introduction of the statutory minimum wage. The first column reports the baseline level of each outcome in 2014. Columns (2) and (3) report the estimated minimum-wage-induced change over the 2014–2016 period in absolute terms and relative to the 2014 baseline, respectively. Bracketed values denote 95% confidence intervals constructed using robust standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Table 3: Minimum Wage Effects on New Inflows of Eastern EU workers by Educational Attainment and Task Complexity

| | Educational Attainment | | | Task Complexity | | |
|---|---|---|---|---|---|---|
| | Low | Medium | High | Low | Medium | High |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Pre-trend | 0.009 | -0.017 | 0.013 | -0.002 | -0.061 | 0.011 |
| | (0.025) | (0.022) | (0.030) | (0.021) | (0.048) | (0.040) |
| Policy effect | 0.050** | 0.078*** | 0.030 | 0.059*** | 0.020 | -0.028 |
| | (0.023) | (0.023) | (0.026) | (0.020) | (0.047) | (0.036) |

*Notes:* This table reports estimates of the effect of the introduction of the statutory minimum wage on new inflows of Eastern EU workers by educational attainment (columns (1)–(3)) and task complexity (columns (4)–(6)). The estimates are obtained from a difference-in-differences specification derived from Equation (2), in which the fully dynamic event-study structure is collapsed by aggregating all pre-reform years into a single pre-period and all post-reform years into a single post-period; the transition year 2014 is omitted. The row labeled "Pre-trend" reports the coefficient on the gap variable measuring treatment exposure interacted with the pre-reform indicator and provides a test for differential trends prior to the reform. The row labeled "Policy effect" reports the corresponding post-reform coefficient and captures the short-run causal effect of the minimum wage introduction. New immigrant inflows are counted separately by education and task-complexity group at the district level, and the logarithm of these counts serves as the dependent variable. Educational attainment is classified into low, medium, and high based on workers' highest level of schooling as reported by employers in the BeH data. Low education comprises workers without vocational training or tertiary education; medium education comprises workers with completed vocational training but no tertiary degree; and high education comprises workers with tertiary education. Task complexity is measured using the fifth digit of the occupation code in the 2010 German Classification of Occupations ("Klassifikation der Berufe 2010"). Unskilled and semi-skilled occupations are grouped as low complexity, complex occupations as medium complexity, and highly complex occupations as high complexity. Standard errors clustered at the district level are reported in parentheses. Significance levels are denoted by *** 1%, ** 5%, and * 10%.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Table 4: Minimum Wage Effects on Eastern EU Immigration by Pre-Reform Labor Market Concentration in the District

| | Pre-Reform HHI | |
|---|---|---|
| | At or Below Median | Above Median |
| | (1) | (2) |
| Pre-trend | 0.016 | -0.038 |
| | (0.032) | (0.034) |
| Policy Effect | 0.018 | 0.060** |
| | (0.033) | (0.031) |
| No. of Districts | 113 | 287 |

*Notes:* This table reports estimates of the effect of the minimum wage introduction on new inflows of Eastern EU workers, separately for districts with relatively low pre-reform labor market concentration (column 1) and relatively high pre-reform labor market concentration (column 2). Estimates are obtained from a difference-in-differences specification based on Equation (2), in which the event-study structure is collapsed by aggregating all pre-reform years into a single pre-period and all post-reform years into a single post-period; the transition year 2014 is omitted as the reference period. To examine heterogeneity, all regressors are interacted with an indicator for above-median pre-reform labor market concentration. The row labeled "Pre-trend" reports the coefficient on the gap measure interacted with the pre-reform indicator, providing a test for differential pre-reform trends. The row labeled "Policy Effect" reports the corresponding post-reform coefficient and captures the short-run causal effect of the minimum wage introduction. Labor market concentration is measured using the Herfindahl–Hirschman Index (HHI), as described in Appendix D. Districts are split at the employment-weighted median HHI into low- and high-concentration labor markets. The HHI is correlated with several regional characteristics including district size, average firm size, and industry composition, as documented in Table D.2. We control for these potential confounders of the split by HHI through including interaction terms of their pre-reform levels with year fixed effects in the specification. Standard errors clustered at the district level are reported in parentheses. Significance levels: *** 1%, ** 5%, * 10%.

*Source:* IAB Employee History (BeH), version `100800_202412`, and IAB Establishment History Panel (BHP), version `7522 v1`; authors' calculations.

Table 5: Estimates from Alternative Specifications

| | Baseline Model | Without Controls | West Germany | Border Distance | Median Split | Poisson Model |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| **Panel A: (Log) New Inflows of Eastern EU Workers** | | | | | | |
| Pre-trend | -0.003 | -0.004 | 0.063* | -0.003 | 0.016 | 0.010 |
| | (0.020) | (0.016) | (0.035) | (0.020) | (0.029) | (0.024) |
| Post-policy coeff. | 0.055*** | 0.065*** | 0.090** | 0.055*** | 0.084** | 0.045** |
| | (0.019) | (0.015) | (0.042) | (0.020) | (0.034) | (0.023) |
| Observations | 2000 | 2000 | 1620 | 2000 | 2000 | 2000 |
| **Panel B: (Log) Hourly Wages of All Workers** | | | | | | |
| Pre-trend | -0.001 | -0.002*** | -0.000 | -0.001 | -0.002*** | |
| | (0.000) | (0.000) | (0.001) | (0.000) | (0.001) | |
| Policy Effect | 0.007*** | 0.009*** | 0.003** | 0.007*** | 0.007*** | |
| | (0.001) | (0.000) | (0.001) | (0.001) | (0.001) | |
| Observations | 2000 | 2000 | 1620 | 2000 | 2000 | |

*Notes:* This table reports robustness checks for the effect of the introduction of the statutory minimum wage on (log) new inflows of Eastern EU workers (Panel A) and on (log) hourly wages of all workers (Panel B). The estimates are obtained from difference-in-differences specifications derived from Equation (2), in which the fully dynamic event-study structure is collapsed by aggregating all pre-reform years into a single pre-period and all post-reform years into a single post-period; the transition year 2014 is omitted. The row labeled "Pre-trend" reports the coefficient on the gap variable measuring treatment exposure interacted with the pre-reform indicator and provides a test for differential trends prior to the reform. The row labeled "Policy effect" reports the corresponding post-reform coefficient and captures the short-run causal effect of the minimum wage introduction. In columns (1)–(4), treatment intensity is measured by the continuous gap variable, so the reported coefficients reflect the effect of an increase in exposure from zero to one. Column (1) reports the baseline specification, which controls for year effects interacted with population density and with the employment shares of low- and medium-skilled workers in the district. Column (2) omits these interactions and instead assumes common year effects across all districts. Column (3) reports estimates from the baseline specification restricting the sample to districts in former West Germany. Column (4) augments the baseline specification by additionally interacting the year effects with the district's distance to the nearest Czech or Polish border. In column (5), treatment exposure is defined as a binary indicator equal to one for districts above the median of the gap measure and zero otherwise, so the reported coefficients capture average differences between highly and weakly exposed districts. Column (6) re-estimates the baseline specification using a Poisson model instead of a log-linear regression. Standard errors clustered at the district level are reported in parentheses. Significance levels are denoted by *** 1%, ** 5%, and * 10%.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

# A Additional Tables and Figures

## A.1 Additional Tables

Table A.1: Statutory Minimum Wages in Germany, France and Eastern EU countries as of January 2015

| | Nominal | Real (German prices) | Ratio |
|---|---|---|---|
| | (1) | (2) | (3) |
| Germany | 8.50 | 8.50 | 1.00 |
| France | 9.61 | 8.95 | 0.95 |
| Bulgaria | 1.06 | 2.30 | 3.70 |
| Czech Republic | 2.00 | 3.01 | 2.82 |
| Estonia | 2.34 | 2.93 | 2.90 |
| Hungary | 1.96 | 3.42 | 2.49 |
| Latvia | 2.17 | 4.41 | 1.93 |
| Lithuania | 1.82 | 2.86 | 2.97 |
| Poland | 2.42 | 4.29 | 1.98 |
| Romania | 1.30 | 2.43 | 3.50 |
| Slovak Republic | 2.18 | 3.11 | 2.73 |
| Slovenia | 4.57 | 5.46 | 1.56 |

*Notes:* The table reports statutory minimum wages per hour, effective January 2015. Column (1) shows nominal hourly minimum wages in euros. Column (2) reports real minimum wages adjusted for cost-of-living differences and expressed in euro-equivalent units at German price levels as of 2015. These values are obtained by converting nominal wages into Purchasing Power Standards (PPS) provided by the World Bank and rescaling them so that the German minimum wage equals its nominal value. Column (3) reports the ratio of the German minimum wage to the corresponding minimum wage in each country, based on the real values in column (2); values greater than one indicate that the German minimum wage is higher than the respective country's wage after adjusting for price level differences.
*Source:* Minimum Wage Database of the Institute of Economic and Social Research (WSI, 2026); columns (2) and (3) are based on the authors' own calculations.

Table A.2: Characteristics of Eastern EU Workers by Place of Work and German Workers in 2014

| | Eastern Europeans | | Germans |
|---|---|---|---|
| | At home | In Germany | |
| | (1) | (2) | (3) |
| Educational attainment (ISCED): | | | |
| Low | 0.107 | 0.194 | 0.108 |
| Medium | 0.648 | 0.557 | 0.633 |
| High | 0.245 | 0.249 | 0.259 |
| Unemployment rate: | | | |
| Low skilled | 0.166 | 0.079 | 0.094 |
| Medium skilled | 0.082 | 0.064 | 0.042 |
| High skilled | 0.034 | 0.055 | 0.019 |
| Number of observations | 53,851 | 4,691 | 218,795 |
| Data source | EU-SILC | German Microcensus | |

*Notes:* This table reports descriptive characteristics of Eastern EU workers residing in their home countries (column 1), Eastern EU workers employed in Germany (column 2), and native German workers (column 3) in 2014. Educational attainment is classified using broad ISCED categories. Unemployment rates are reported by ISCED category. All entries are shares.

*Sources:* EU-SILC, `Scientific Use File, 2022 v2`; German Microcensus , `Scientific Use File 2014`; authors' calculations. Details on these additional data sources and our processing steps in preparing them for the analysis are given in Appendix B.

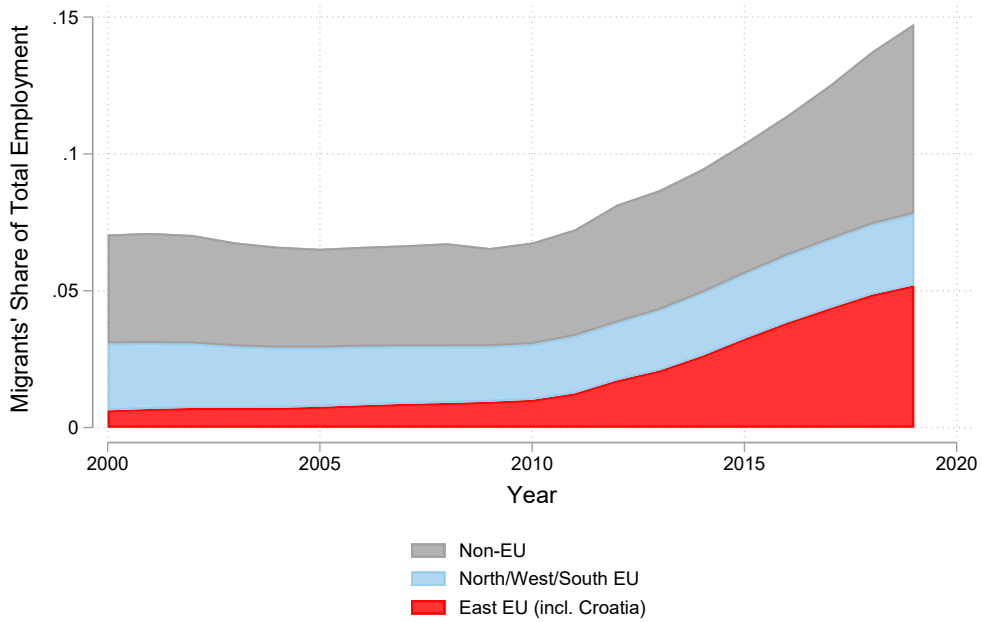
Table A.3: Descriptive Statistics on Immigration of Eastern EU workers as of 2014

| | (1) Mean | (2) SD | (3) Min | (4) Max |
|---|---|---|---|---|
| **Panel A: All Districts** | | | | |
| Stock | 1273 | 1804 | 85 | 22387 |
| Total Inflows | 559 | 724 | 37 | 8263 |
| New Inflows | 384 | 487 | 21 | 5366 |
| Inflows from GER distr. | 102 | 132 | 4 | 1582 |
| Inflows from Non-Empl. | 74 | 118 | 2 | 1315 |
| Total Outflows | 387 | 525 | 26 | 6355 |
| Outflows to GER distr. | 136 | 187 | 11 | 2394 |
| Outflows to Non-Empl. | 251 | 349 | 14 | 3961 |
| **Panel B: Districts below median gap measure** | | | | |
| Stock | 1691 | 2097 | 153 | 22387 |
| Total Inflows | 720 | 823 | 61 | 8263 |
| New Inflows | 483 | 544 | 39 | 5366 |
| Inflows from GER distr. | 141 | 159 | 13 | 1582 |
| Inflows from Non-Empl. | 96 | 131 | 6 | 1314 |
| Total Outflows | 491 | 604 | 39 | 6355 |
| Outflows to GER distr. | 184 | 227 | 11 | 2394 |
| Outflows to Non-Empl. | 307 | 387 | 25 | 3961 |
| **Panel C: Districts above median gap measure** | | | | |
| Stock | 854 | 1332 | 85 | 16548 |
| Total Inflows | 398 | 568 | 37 | 6738 |
| New Inflows | 284 | 399 | 21 | 4553 |
| Inflows from GER distr. | 62 | 81 | 4 | 889 |
| Inflows from Non-Empl. | 51 | 98 | 2 | 1296 |
| Total Outflows | 284 | 408 | 26 | 4721 |
| Outflows to GER distr. | 89 | 117 | 11 | 1237 |
| Outflows to Non-Empl. | 195 | 297 | 14 | 3484 |

*Notes:* This table shows descriptive statistics for the outcome variables as of 2014 for: all 400 districts (Panel A); the 200 less treated districts (Panel B); and the 200 more treated districts (Panel C).
*Source:* IAB's Employee History (BeH) data, own calculations.

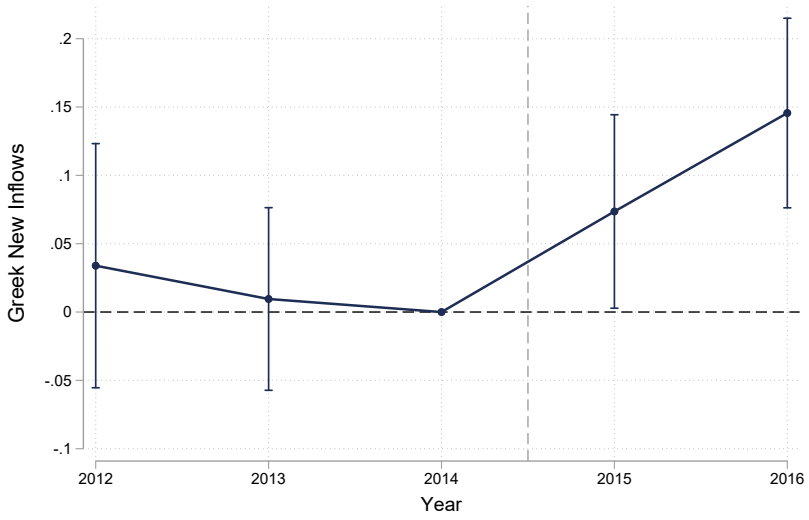
## A.2 Additional Figures

Figure A.1: Development of Immigrants' Share of Total Employment in Germany

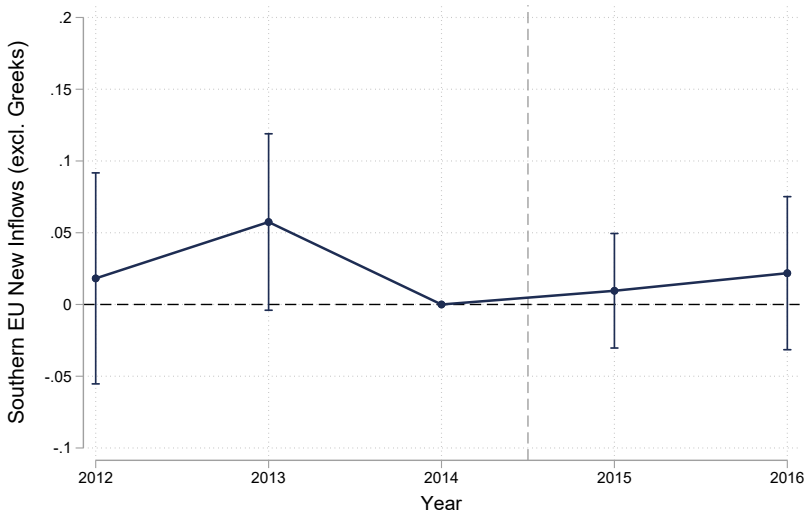



*Notes:* This figure shows the evolution of immigrants' share of total full-time-equivalent (FTE) employment from 2000 to 2019. Immigrants are divided into three groups: Eastern EU immigrants, other EU immigrants from Western, Northern, and Southern Europe, and non-EU immigrants. Eastern EU countries include Bulgaria (EU accession 2007; free access to the German labor market in 2014), Croatia (2013; 2015), the Czech Republic (2004; 2011), Estonia (2004; 2011), Hungary (2004; 2011), Latvia (2004; 2011), Lithuania (2004; 2011), Poland (2004; 2011), Romania (2007; 2014), Slovakia (2004; 2011), and Slovenia (2004; 2011). Croatia is included here for completeness but is excluded from the remainder of the analysis, as it gained access to the German labor market in the same year the statutory minimum wage was introduced. The underlying data exhibit a structural break in the working-time category variable used to construct FTE employment in 2010–2011; this break is corrected using the method proposed by Fitzenberger and Seidlitz (2020).
*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure A.2: Effect of the Minimum Wage on Immigration from Greece and other Countries in Southern Europe




(a) Greece

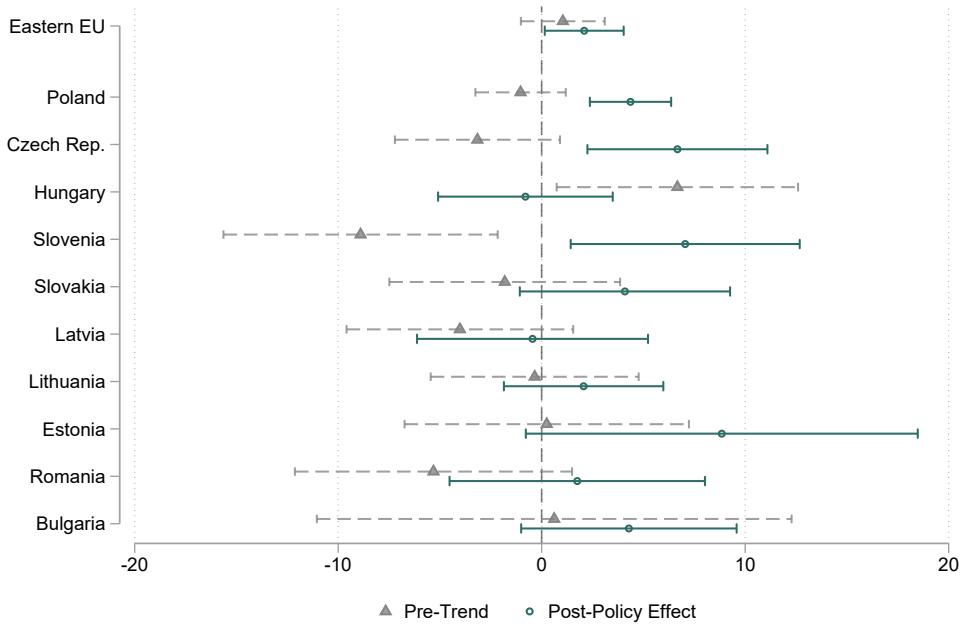



(b) Southern Europe Excluding Greece

*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on the (log) number of immigrants (new inflows) from Greece (Panel a) and Southern Europe excluding Greece (Panel b), relative to the pre-policy year 2014. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

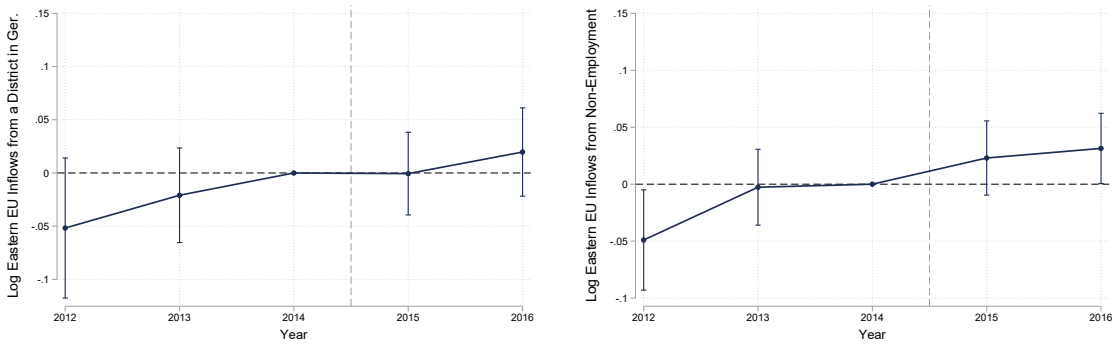
Figure A.3: Effect of the Minimum Wage on Eastern EU New Inflows by Country




*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) new inflows from individual Eastern EU countries, relative to the pre-policy year 2014. New inflows are defined as workers who appear in the German employment records for the first time in year $t$. Because the outcome variable contains zeros in some country–district–year cells, estimation is carried out using a Poisson regression model instead of a log-linear regression model. For ease of exposition, the fully dynamic event-study specification is collapsed by aggregating all pre-reform years into a single pre-period and all post-reform years into a single post-period, while omitting the transition year 2014. The reported "Pre-trend" coefficients correspond to averages of the estimated pre-reform effects for the years 2012–2013, while the reported "Policy effect" coefficients correspond to averages of the estimated post-reform effects for the years 2015–2016. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure A.4: Effect of the Minimum Wage on Eastern EU Inflows from Within Germany and from Non-Employment



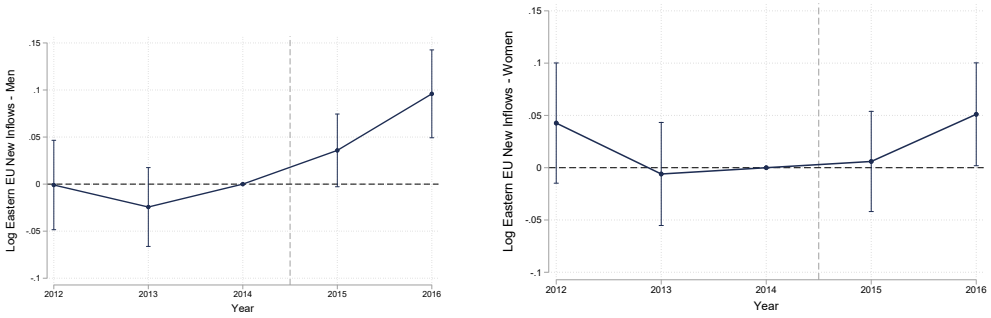

(a) From another German District

(b) From Non-Employment

*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) inflows of Eastern EU workers. Panel (a) shows inflows from another German district, defined as Eastern EU workers employed in district $r$ in year $t$ who were employed in a different German district in year $t-1$. Panel (b) shows inflows from non-employment, defined as Eastern EU workers employed in district $r$ in year $t$ who were not employed in Germany in year $t-1$ but have been employed in Germany at some point in the past (i.e., excluding first-time entrants). Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

Figure A.5: Eastern EU New Inflows by Gender

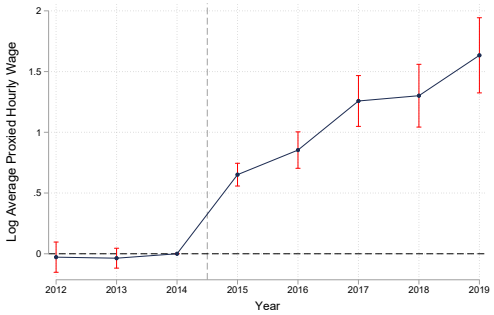



(a) Men (b) Women

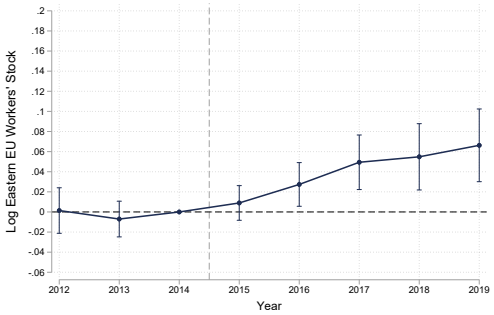
*Notes:* This figure plots estimates of the effect of the introduction of the statutory minimum wage on (log) new inflows of Eastern EU workers. Panels (a) and (b) report results separately for men and women respectively. New inflows are defined as Eastern EU workers employed in district $r$ in year $t$ who appear for the first time in the German employment records in year $t$. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

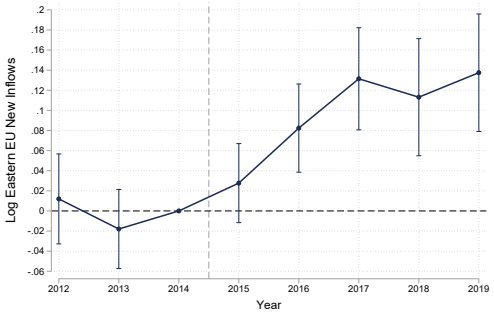
Figure A.6: Medium-Run Effects on Main Outcomes until 2019




(a) Average Hourly Wages




(b) EU Eastern Stock




(c) New Inflows of EU Eastern Immigrants

*Notes:* This figure plots medium-run estimates of the effect of the introduction of the statutory minimum wage on the main outcomes using data until 2019. Panel (a) shows effects on (log) average proxied hourly wages. Panel (b) shows effects on the (log) stock of Eastern EU workers in Germany. Panel (c) shows effects on (log) new inflows of Eastern EU workers into Germany. Plotted coefficients correspond to $\beta_z$ from Equation (2) and trace the evolution of the outcome in districts differentially exposed to the minimum wage, relative to the last pre-reform year 2014. Vertical bars denote 95% confidence intervals constructed from standard errors clustered at the district level.

*Source:* IAB Employee History (BeH), version `100800_202412`; authors' calculations.

# B Data Appendix

## B.1 IAB Employee History (BeH) Data

**Data coverage and reliability.** Our main data source is the Employee History data (Beschäftigtenhistorik, BeH, version `100800_202412`), provided by the Institute for Employment Research (IAB) of the German Federal Employment Agency. The BeH are used to calculate social security contributions and are therefore highly reliable. Employers face substantial financial penalties for misreporting earnings, implying that wage information is measured with a high degree of accuracy. Misreporting of other information, such as age or nationality, may likewise trigger sanctions if it results in improper eligibility for social transfers or facilitates the concealment of illegal employment or migration. As a result, the statistical information recorded in the BeH can be regarded as highly reliable more generally. Covered workers account for approximately 80% of total employment in Germany. The remaining share consists primarily of self-employed individuals, civil servants, and military personnel, who are not included in the analysis.

**Sample construction.** To construct our analysis sample, we closely follow the procedures in Dustmann et al. (2022). We restrict attention to employees aged 18 to 59 and exclude apprentices and interns. Apprentices were not covered by the statutory minimum wage, while interns were only partially covered. We retain all individuals employed on June 30 of each year. In cases where an individual holds multiple jobs, we select the employment spell with the highest daily wage. If daily wages are identical, we prioritize full-time employment over part-time employment, and part-time employment over marginal employment. We drop workers with top-coded wages, as this population is highly unlikely to respond to the introduction of the minimum wage.

**Marginal employment.** We include marginally employed workers holding so-called mini-jobs in the analysis. These jobs are disproportionately paid at or near the minimum wage and therefore particularly relevant for studying the effects of the reform. During our observation period, marginal employment was limited to monthly earnings of up to €450 and exempt from social security contributions.

**Working hours and hourly wages.** For the period 2011–2014, the BeH data include administrative information on hours worked. Because some employers may report actual hours worked instead of contractual hours, we apply the correction procedure proposed by Vom Berge, Umkehrer, and Wanger (2023). This procedure recovers the contractual hours of each worker consistently. Combined with information on daily wages, this allows us to calculate hourly wages by dividing average daily wages by average daily working hours. These hourly wages are used to construct the treatment variable measuring district-level exposure to the minimum wage introduction, as described in Section 5.2. To analyze wage outcomes over the full observation period, including years in which hours data are unavailable, we proxy hourly wages beyond 2014 by dividing daily wages by average daily working hours assigned by working-time status. Following Dustmann et al. (2022), we assume a seven-day working week and use average daily hours of 5.28 for full-time employment, 3.30 for part-time employment, and 1.18 for marginal employment.

**Identifying immigrants.** We identify immigrants based on workers' nationality, which employers are required to report in the social security records alongside earnings and other employment information. While these data should be highly reliable in principle, they seem to not always be updated consistently by employers. For example, individual employment histories sometimes display implausible changes in nationality, such as switching from Polish to German and back to Polish. Therefore, we impute nationality by assigning each worker their first reported nationality throughout their entire employment history. This approach relies on the assumption that the nationality recorded when a worker first enters the social security system is, on average, more reliable than subsequent employer reports. Accordingly, we define an immigrant worker as an individual whose nationality in their first recorded job is not German.

**District-year panel.** We aggregate the individual-level data to a district-by-year panel. The final dataset covers 400 districts over the period 2012–2019. Until July 2021, Germany consisted of 401 districts. In that month, the district-free city of Eisenach merged with the district of Wartburg (Wartburgkreis). Following this administrative change, the BeH data were retroactively harmonized. As a result,

the version of the BeH used in this study, which we obtained in 2023, contains only 400 districts, even though our observation period predates the merger.

## B.2 EU-SILC data

The EU Statistics on Income and Living Conditions (EU-SILC) is an annual household survey coordinated by Eurostat and implemented by national statistical offices across EU member states. The reference population comprises all individuals living in private households. We use the 2014 cross-sectional personal files for Germany, France, and ten Eastern European EU member states (Bulgaria, Czech Republic, Estonia, Hungary, Latvia, Lithuania, Poland, Romania, Slovak Republic, Slovenia).

**Sample restrictions.** We construct a harmonized analysis sample designed to compare wage distributions across countries in a consistent way. First, we restrict to employees aged 18–59 with positive annual gross employee cash income ($\texttt{PY010G} \leq 0$) and require non-missing information on hours (PL060) and months worked (PL073). Finally, we focus on individuals employed full-time in the income reference period by requiring positive months of full-time employment and (near-)zero months of part-time employment and self-employment. The focus on full-time workers is necessary to compute hourly wages consistently from the data.

**Computation of hourly wages.** We compute hourly wages (in euros) by dividing annual gross employee cash income by total annual hours implied by usual weekly hours in the main job and months worked full time:

$$w_i \;=\; \frac{\texttt{PY010G}_i}{\texttt{PL060}_i \times 4.35 \times \texttt{PL073}_i},$$

where 4.35 converts months into weeks. We drop observations yielding undefined or infinite wages and trim the hourly wage distribution at the 1st and 99th percentiles within each country to reduce the influence of outliers. For cross-country comparisons of real wage levels, we express wages in German prices using 2014 price level indices provided by Eurostat (2026).

**Education coding.** We assign education using the EU-SILC highest-education variable (PE040). We map the information into three broad ISCED-based groups.

Low education corresponds to at most lower secondary schooling (typically up to about 9 years of schooling). Medium education captures upper secondary and post-secondary non-tertiary education (typically around 12–13 years of schooling, such as general high school completion or vocational tracks). High education corresponds to tertiary education (for example, a bachelor's degree or higher, typically 16 or more years of schooling).

## B.3 German Microcensus

The German Microcensus is an annual household survey conducted by the Federal Statistical Office (Destatis) that covers roughly 1 % of Germany's resident population. It contains detailed information on demographics, nationality, education, and labor-market outcomes and includes design weights that allow the construction of population-representative statistics.

**Sample and variable definitons.** We use the 2014 Scientific Use File (SUF) and restrict the sample to individuals aged 18–59. We drop observations from non-private households. Our main outcome of interest in these data, educational attainment, is based on the Microcensus ISCED measure. We construct three broad education groups that parallel the EU-SILC coding. We use these groups to compute education shares and to define education-specific unemployment rates. The latter are given by the weighted share of unemployed within each education group among individuals who are not inactive.

**Identifying Germans and Eastern European EU workers.** We identify native Germans and Eastern EU workers using citizenship information from the survey, which is provided in aggregated form for confidentiality reasons. Because the Microcensus Scientific Use File does not allow separate identification of all ten Eastern European EU member states used elsewhere in the paper, this analysis is restricted to those that can be consistently distinguished: Bulgaria, Czech Republic, Hungary, Poland, Romania, and Slovakia. The remaining Eastern European EU member states (Estonia, Latvia, Lithuania, and Slovenia) are excluded from analyses using these data, as they are grouped with other Northern or Southern European coun-

tries.

# C Theoretical Appendix

## C.1 Baseline Scenario

Individuals residing in the origin country ($O$) decide whether to migrate to the destination country ($D$) by comparing expected earnings net of migration costs. Expected earnings depend on wages and the probability of employment.

Let the distribution of expected log wages in country $k \in \{O, D\}$ be given by a base wage $\mu_k$ and a return $b_k$ to individual skill $S$, which is normally distributed as $S \sim \mathcal{N}(0, 1)$:

$$\ln w_{ik} = \mu_k + b_k S_i.$$

Employment is uncertain. Individuals face an employment probability $P_k(S) \in [0, 1]$ in each country $k$, which depends positively on skill, $P_k'(S) > 0$. We define the migration cost $C$ as being proportional to the expected earnings in the origin country, $C = \pi[P_O(S)w_{iO}]$, where $\pi$ represents the time-equivalent cost of migration.

An individual chooses to migrate if expected earnings in the destination exceed expected earnings in the origin plus migration costs:

$$P_D(S)w_{iD} > P_O(S)w_{iO} + C$$

Substituting the cost function and factoring the right-hand side, we obtain:

$$P_D(S)w_{iD} > P_O(S)w_{iO}(1 + \pi).$$

Taking logs and using the standard approximation $\ln(1+\pi) \approx \pi$ for small $\pi$, the migration condition can be re-written as:

$$(\mu_D - \mu_O) + S(b_D - b_O) + (\ln P_D(S) - \ln P_O(S)) > \pi. \tag{4}$$

Equation (4) implies that migration occurs when the combined gains from higher wages and higher employment probabilities in the destination exceed migration costs.

In the context of migration from Eastern Europe to Germany, Table 1 shows that the destination country offers both higher mean wages ($\mu_D > \mu_O$) and greater

wage dispersion, implying steeper returns to skill ($b_D > b_O$). In the simplest possible Roy–Borjas framework, these features would predict positive selection on skill. In our framework, however, selection is additionally shaped by cross-country differences in employment probabilities, captured by the term $\ln P_D(S) - \ln P_O(S)$.

Differences in employment probabilities can affect selection in two distinct ways. If the destination offers substantially better (worse) employment prospects for high-skilled workers, positive selection is reinforced (attenuated). Conversely, if low-skilled workers face particularly high (low) unemployment risk in the origin country relative to the destination, a push (pull) factor emerges at the lower end of the skill distribution.

Table A.2 shows that prior to the introduction of the German minimum wage in 2015, low-skilled workers in Eastern European countries faced substantially higher unemployment rates than comparable workers in Germany, whereas high-skilled workers in Eastern Europe experienced lower unemployment rates at home than in Germany. Consistent with these cross-country differences, Eastern European immigrants to Germany before the reform were disproportionately drawn from the lower end of the skill distribution. At the same time, high-skilled immigrants were represented in proportions similar to those of high-skilled stayers, despite higher returns to skill in Germany, suggesting that their relatively low unemployment risk at origin limited positive skill selection. Medium-skilled workers were underrepresented among Eastern European immigrants, resulting in a polarized pattern of migrant selection.

## C.2 Effects of Introducing a Minimum Wage

Now suppose a statutory minimum wage $\bar{w}$ is introduced in the destination country in a setting such as the one just described. The policy can affect expected earnings of low-skilled workers through two channels: wages and employment probabilities.

**Impact on Wages.** A floor is imposed on potential wages in the destination, resulting in the following wage schedule:

$$\tilde{w}_D(S) = \begin{cases} \bar{w} & \text{if } \mu_D + b_D S < \ln \bar{w}, \\ \exp(\mu_D + b_D S) & \text{otherwise.} \end{cases}$$

The minimum wage binds for skill levels below $S^* = (\ln \bar{w} - \mu_D)/b_D$. For workers from origin countries for whom, at skill level $S^*$, $\ln \bar{w} \geq \mu_O + b_O S$, the reform raises potential earnings and may generate additional incentives to migrate. Whether these incentives translate into higher realized immigration, however, hinges crucially on the employment effects of the policy.

**Impact on Employment.** The effect of the minimum wage on employment probabilities depends on the structure of the labor market for workers with $S < S^*$. In a competitive labor market, denoted by the superscript $c$, a minimum wage set above the market-clearing level reduces labor demand. Employment probabilities for low-skilled workers fall from $P_D(S)$ to $P_D^c(S)$, and expected earnings become $P_D^c(S) \cdot \bar{w}$. If labor demand is sufficiently elastic, the decline in employment probabilities may offset the wage increase, dampening migration incentives. In contrast, under monopsonistic competition, denoted by the superscript $m$, employment probabilities for low-skilled workers may remain unchanged or even increase as wages rise toward $\bar{w}$, that is, $P_D^m(S) \geq P_D(S)$. In this case, expected earnings increase unambiguously, as workers benefit from both higher wages and stable (or improved) employment prospects.

## C.3 Testable Predictions

**Competitive Labor Market.** If the minimum wage in $D$ is set above the market-clearing wage, employment probabilities for low-skilled workers decline, reducing immigration if this effect outweighs potential wage gains. Fewer low-skilled workers will immigrate, but high-skilled immigration is unaffected. This would strengthen positive selection among immigrants. If the minimum wage is set at or below the market-clearing wage, migration outcomes are unchanged relative to the baseline model.

**Monopsonistic Labor Market.** If the minimum wage in $D$ lies between the prevailing wage and the competitive wage, it both guarantees $\bar{w}$ for workers with $S < S^*$ and stabilizes employment probabilities. Expected earnings for low-skilled workers therefore rise. As a result, immigration increases, with migrants most likely

drawn from the lower part of the skill distribution. If $\ln \bar{w}$ is set sufficiently high relative to $\mu_O + b_O S$, immigration nonetheless may also extend to higher skill levels. For example, some medium-skilled workers in $O$ may find employment at the minimum wage in $D$ more attractive than medium-skilled employment opportunities at home. Whether this occurs, and to what extent, is ultimately an empirical question. In any case, the average skill level of the immigrant cohort arriving after the reform should c.t. decrease relative to the baseline as the composition shifts toward low- and potentially medium-skilled workers.

# D Measurement of Employment Concentration

In Section 6.6, we study heterogeneity in the effect of the minimum wage on immigration from Eastern Europe by the degree of employment concentration prevailing in local labor markets prior to the reform. To this end, we construct a district-level measure of labor market concentration based on the Herfindahl–Hirschman Index (HHI), averaged over the pre-reform period 2012–2014, for each of the 400 districts in our sample.

We implement this procedure using the Establishment History Panel (BHP) of the IAB (version `7522 v1`), which aggregates the worker-level BeH data used throughout the paper to the establishment level and provides additional information, including harmonized industry classifications. We proceed in four steps. First, we define local labor markets at the district × 3-digit industry level and compute total employment in each market-year cell. Second, within each district–industry–year cell, we calculate the HHI according to the standard formula

$$\text{HHI}_{m,t} = \sum_{j=1}^{J_m} \left( 100 \times \frac{E_{jmt}}{\sum_{k=1}^{J_m} E_{kmt}} \right)^2,$$

where $E_{jmt}$ denotes employment at establishment $j$ in labor market $m$ at time $t$, and $J_m$ denotes the number of establishments in that market. Market shares are expressed in percentage points, so that $\text{HHI}_{m,t} \in [0, 10{,}000]$, with higher values indicating greater labor market concentration.

Third, we aggregate the HHI from the district × 3-digit industry × year level to

the district-year level by taking the employment-weighted mean across industries.[16] Finally, for each district, we compute the average HHI over the three-year pre-reform period from 2012 to 2014.

The resulting employment-weighted distribution of district-level HHIs is summarized in Table D.1. The median HHI is 1,778, which lies very close to the lower threshold of 1,800 used in the most recent US Horizontal Merger Guidelines to classify markets as highly concentrated (U.S. Department of Justice & Federal Trade Commission, 2023). Accordingly, in the heterogeneity analysis, we split districts at the employment-weighted median into 113 relatively low- and 287 high-concentration local labor markets. Almost all districts classified as above-median in this heterogeneity analysis (281 out of 286) can therefore be regarded as exhibiting a high degree of employment concentration according to the official antitrust benchmark.

Table D.1: Summary Statistics of the District-Level Herfindahl–Hirschman Index

| | Mean | SD | p10 | p25 | p50 | p75 | p90 |
|---|---|---|---|---|---|---|---|
| HHI | 1,926 | 836 | 1,126 | 1,347 | 1,778 | 2,313 | 2,965 |

*Notes:* This table reports summary statistics of the district-level Herfindahl–Hirschman Index (HHI), measured as the employment-weighted average of industry-level concentration indices within each district. The HHI is averaged over the pre-reform period 2012–2014. Higher values indicate greater employment concentration. The sample consists of 400 districts. *Source:* IAB Establishment History Panel (BHP), version `7522 v1`; authors' calculations.

[16] This aggregation mirrors the approach in Azar, Marinescu, and Steinbaum (2022), who measure concentration at the commuting zone–occupation–year–quarter level and aggregate to the commuting zone level for descriptive analysis (see, for example, their Figure 2).

Table D.2: Regional Characteristics by Pre-Reform Employment Concentration

| | Pre-Reform HHI | | Difference | | |
|---|---|---|---|---|---|
| | At or Below Median | Above Median | Coef. | S.E. | $R^2$ |
| | (1) | (2) | (3) | (4) | (5) |
| *Panel A: Economic characteristics* | | | | | |
| Log firm size | 1.226 | 1.246 | 0.020** | (0.009) | 0.011 |
| Log wage | 4.488 | 4.496 | 0.008 | (0.014) | 0.001 |
| *Panel B: Sectoral composition* | | | | | |
| Primary | 0.016 | 0.012 | -0.004** | (0.002) | 0.014 |
| Manufacturing | 0.279 | 0.307 | 0.028*** | (0.010) | 0.018 |
| Services | 0.705 | 0.680 | -0.025** | (0.010) | 0.013 |
| *Panel C: Worker composition* | | | | | |
| Female share | 0.494 | 0.495 | 0.000 | (0.003) | 0.000 |
| Average age | 42.490 | 42.448 | -0.042 | (0.099) | 0.000 |
| Low-education share | 0.120 | 0.115 | -0.005 | (0.004) | 0.005 |
| Medium-education share | 0.714 | 0.737 | 0.023*** | (0.007) | 0.030 |
| High-education share | 0.134 | 0.122 | -0.012** | (0.006) | 0.012 |
| Immigrant share | 0.078 | 0.067 | -0.012** | (0.005) | 0.016 |
| *Panel D: Regional characteristics* | | | | | |
| Log district size | 11.675 | 10.863 | -0.812*** | (0.071) | 0.281 |
| Urban agglomeration indicator | 0.531 | 0.408 | -0.123** | (0.055) | 0.012 |
| East Germany indicator | 0.204 | 0.185 | -0.019 | (0.044) | 0.000 |
| No. of Districts | 113 | 287 | | | |

*Notes:* This table compares average regional characteristics across districts with low versus high employment concentration, measured by the Herfindahl–Hirschman Index (HHI). Districts are split at the median of the pre-reform HHI distribution, computed over the period 2012–2014. Columns (1) and (2) report mean values for districts at or below versus above the median, respectively. Column (3) reports differences in means estimated from bivariate regressions of each characteristic on an indicator for above-median HHI. Column (4) reports robust standard errors. Column (5) reports the corresponding partial $R^2$ from these regressions. Sectoral shares refer to employment shares in the primary, manufacturing, and service sectors. The urban agglomeration indicator equals one for districts classified as urban by the German Federal Office for Building and Regional Planning (BBSR). Education shares correspond to the distribution of workers across low, medium, and high education groups. Significance levels: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The sample consists of 400 districts. *Source:* IAB Establishment History Panel (BHP), version `7522 v1`; authors' calculations.