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IZA DP No. 14158

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ABSTRACT

The Role of Labor Market Institutions in the Impact of Immigration on Wages and Employment*

We study the role of institutions in affecting the labor market impacts of immigration using a cross-country meta-analysis approach. To accomplish this, we gather information on 1,030 previously estimated wage effects and 432 employment effects of immigration from 61 academic studies covering 18 developed countries. The mean and median impact on the relative wage of directly exposed native workers are negative and significantly different from the small positive mean and median impact on the average wage level. This pattern is reversed for employment effects where the magnitudes are smaller. We combine this database with country-level data on labor market institutions from the OECD. The results suggest that institutions may shield native workers from distributional (relative) wage consequences of immigration but exacerbate the impacts on average wages in the economy. We do not detect a significant and robust association for the employment effects of foreign workers.

JEL Classification: D02, J08, J15, J31, J61

Keywords: immigration, wages, employment, labor market institutions, meta-analysis

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1 Introduction

Institutional structures and policies such as collective bargaining, unemployment insurance and active labor market policies play a key role in most interactions in the labor market (Nickell and Layard, 1999; Boeri and Van Ours, 2013). As such, the origins and consequences of various labor market institutions have been intense objects of analysis by economists for more than a century (e.g., Moore, 1911). More recently, economists have focused on studying the interplay between the institutions and other economic phenomena, such as wage determination (Nunziata, 2005) and income inequality (Farber et al., 2018).

A specific aspect of the labor market where institutions may be particularly crucial, and yet are understudied, is the competition between native and foreign workers. Institutions are often meant to protect incumbent natives from competition with foreign-born labor and decrease the volatility of wages and employment. Following a supply shock, well-designed institutions could shield native workers and provide a smooth transition back to market equilibrium. In fact, demand for such protective institutions may endogenously arise from public pressure on policymakers due to increased immigration (Rodrik, 1997). However, institutional rigidities may also limit the ability of the economy to respond efficiently to shocks and, therefore, harm wages and employment (Blanchard and Wolfers, 2000).

A large body of literature analyzes the wage and employment consequences of immigration, but it ignores the role of labor market institutions by assuming perfectly competitive markets. Recent polls, suggesting immigration is the top issue among election voters in Europe and the United States (e.g., European Commission, 2018; Reuters, 2018) highlight the substantial returns to studying the relationship between institutions and the impacts of immigration on native workers.

Two major challenges stand in the way of measuring the role of institutions in the impact on the labor market of foreign born workers. First, institutions are generally set on a national level and rarely vary within countries, while the cross-country variation is substantial, so a thorough analysis requires a cross-country perspective. Second, gathering comparable micro data across a wide set of countries and time periods is a virtually unattainable task. We overcome both issues by collecting previously estimated wage and employment effects of immigration and correlating them with institutions in a cross-country analysis.

Our contribution is twofold. First, we gather a novel database of 1,030 and 432 estimates of the effect of immigration on wages and employment, respectively. This database covers 61 published academic articles spanning 18 developed countries and several decades. Following Dustmann, Schönberg, and Stuhler (2016), we distinguish between estimated relative and total effects. Since immigrants are less skilled than natives in most studies in our sample, we can interpret the relative effect on exposed workers

as the impact on the wage gap between low and high skilled natives. We document significant differences between the previously estimated relative and total wage effects of immigration. The former are mostly negative (median semi-elasticity = -0.21), while the latter are smaller and tend to be positive (0.08). For employment, this pattern is reversed although most effect sizes are small. In other words, immigration asserts a stronger wage inequality effect compared to altering the overall wage level while the impact on employment levels are more negative than the impact on employment differentials but both types of employment effects are small in magnitude.

Our second contribution is to supplement this database with OECD data on country-specific indicators of institutions and rigidities and use a linear regression framework to relate the effect sizes and institutional measures while controlling for study- and country-level characteristics. The correlation between our measures of institutions is strong, and we think of them as all measuring the same underlying quantity – labor market rigidity. We therefore use the terms labor market "institutions" and "rigidity" interchangeably. Analyzing a certain reform in a given country could potentially isolate the impact of one specific institution. Our cross-country approach, on the contrary, picks up the role played by clusters of strong institutions compared with more flexible labor markets. Notable examples are the Southern European models with strict employment protection and high coverage of collective agreements vis-à-vis the Anglo-Saxon pro-competitive labor markets with weaker protections and more flexible wages.

We find that more restrictive labor market institutions are associated with a smaller effect of immigration on the relative wage. In other words, labor market rigidities dampen the wage inequality effect of foreign labor by shielding low-skill workers from wage adjustments. At the same time, more restrictive institutions are negatively associated with the total wage effects on native workers. Hence, rigidities meant to protect incumbent workers also diminish the potential benefits associated with the newcomer,s such as complementarities in the production process (Ottaviano and Peri, 2012), productivity spillovers (Hunt, 2017) and skill or occupational upgrading (Peri and Sparber, 2009; Foged and Peri, 2016). These findings imply that the significant variation in institutional rigidity across countries may explain a substantial proportion of the differences in reported wage elasticities in the literature. We find no robust correlation between employment effects and labor market institutions. This may, at least partly, be attributed to the smaller sample and lack of statistical power.

Our study is related to a series of influential meta-analyses of the labor market effects of immigration, Longhi, Nijkamp, and Poot (2010; 2008b; 2008a; 2005). Longhi and co-authors show that the estimated elasticities in the United States tend to be smaller in absolute value than in Europe. The authors speculate this disparity may stem from differences in labor mobility and/or labor market institutions but do not

explicitly test this hypothesis. We extend their meta-analyses, include estimates from more studies, and document significant differences in the estimated total and relative labor market effects of immigration. Dustmann, Schönberg, and Stuhler (2016) distinguish between these two types of effects and find a similar pattern to the one we document for wages when looking at smaller selection of influential wage impact estimates.

Although recent studies have strayed away from the classic perfect competition framework and begun analyzing the role of individual institutions, a thorough analysis has so far been elusive. The most similar to our study is the paper by Angrist and Kugler (2003) who use the Bosnian wars as quasi-experiments to analyze the interaction of institutions and employment losses induced by labor supply shocks in EU countries. Reduced market flexibility, as measured by higher wage and employment protection, replacement rates and barriers to entry, is associated with increased (total) employment losses while wage consequences are not analyzed. Brücker et al. (2014) use a structural wage-setting approach to study the wage and employment effects of immigration in Denmark, Germany and the United Kingdom.[1] They find a significant role for wage flexibility in determining its magnitude but do not identify the effect of particular institutions or policies. Lastly, Edo and Rapoport (2019) document an interesting interaction whereby a lower minimum wage exacerbates the labor market effect of immigration in the United States. A common theme among these studies is the significant role of institutional structures in altering the competition between incumbent workers and newcomers. Our paper builds on and extends this body of work by distinguishing between the total and relative labor market effects of immigration and analyzing a combination of institutions and how they interact with (previously estimated) wage and employment effects in a strictly data-driven approach.[2]

In section 2, we describe the theoretical concepts behind estimated relative and total effects. explain how they are estimated, and discuss the possible interaction between the impacts of immigration and institutions. Section 3 and section 4 describe, respectively, the labor market institution variables and the constructed database of estimated wage and employment impacts of immigration and provide novel descriptive statistics. Section 5 presents the equation we estimate along with details on the empirical strategy. Section 6 shows our results and Section 7 concludes the paper.

[1] Brücker et al. (2014) replace the labor supply functions of the classical models of the labor market with elasticities between wages and unemployment that differ across countries and skill (education-experience) cells and thus summarize variation in wage rigidities in these dimensions.

[2] A small related literature studies how institutions affect the labor market integration of immigrants (Kogan, 2006; Fleischmann and Dronkers, 2010; Sá, 2011; Bergh, 2017).

2 Theoretical Background and Prior Research

There is, by now, considerable agreement in the literature about the importance of distinguishing between estimated total and relative effects of immigration (e.g., Peri, 2014; Lewis and Peri, 2015; Dustmann, Schönberg, and Stuhler, 2016). In this section, we begin by discussing the theoretical concept behind each type of effect and how it is estimated (section 2.1). Appendix A contains the theoretical derivations along with more technical details. We then move on to summarize the prior research on the interaction between immigration and institutions (section 2.2).

2.1 Total and Relative Labor Market Effects of Immigration

The total effect on native born workers (in a skill cell) captures the change in natives' wages or employment due to immigration when allowing all features of the economy to respond to the arrival of foreign-born workers. The relative effect, on the contrary, mimics the theoretical concept of a partial effect because it isolates the direct competitive impact of an immigration-induced supply change in the same skill cell from all indirect effects through total factor productivity, the capital-labor ratio, the choice of technology and responses of workers and consumers in the economy. This effect is usually illustrated with a downward-sloping labor demand curve and a shift out in an upward-sloping labor supply curve. Hence, the partial effect on wages and employment is negative for workers with similar skills to the immigrants, while complementary workers (in other skill cells) gain from becoming relatively scarce.

Empirical studies utilizing the so-called *national skill-cell approach*, due to Borjas (2003), use variation in the immigrant share across skill (usually education-experience) cells and time in a national labor market and obtain an estimate of the relative, partial effect. Specifically, they capture something close to the theoretical parameter by controlling for (i) skill-cell fixed effects (capturing differences in wage levels across skill cells), (ii) aggregate time fixed effects (absorbing growth in total factor productivity, changes in the relative productivity of capital and aggregate demand), and (iii) worker-group-by-time fixed effects (controlling for impacts of immigration shared within broader categories of workers).[3] Because the empirical strategy effectively compares natives' outcomes in cells that are more versus less exposed to immigration, the estimated parameter is only informative about changes in relative wages or employment. The relative nature of the estimated effect size is important because complementary workers in other cells will magnify the estimated parameter compared to the absolute partial effect.[4]

[3] See Appendix A.5 for a detailed description of the specifications included in our meta-analysis as capturing a relative effect of immigration.

[4] Ottaviano and Peri (2012) provide evidence that immigrants and natives are imperfect substitutes within education-experience cells. This shifts the negative direct effect to be experienced primarily by earlier immigrants rather than natives. Moreover, Peri and Sparber (2009) and Foged and Peri (2016) explain this by different occupational specializations of immi-

Theoretically, the total effect is more complex and its sign is a priori unknown. Empirical papers rarely isolate the total wage effect experienced in a detailed skill cell. Instead, they estimate an average total effect. A negative average total effect reveals that, on average, workers lose, while a positive one implies that the gains from immigration outweigh the losses when averaging across workers.[5] Empirical studies regressing natives' wages on immigrant shares across regions in a difference-in-differences design or a panel-data setting follow the so-called *spatial approach* and estimate an average total effect of immigration.[6] This approach was pioneered by studies such as Card (1990) and Altonji and Card (1991) and later followed by many others including Foged and Peri (2016) and Peri and Yasenov (2019). It captures the total effect of immigration in regions experiencing a surge in foreign-born workers compared to localities with low or no immigration. Acknowledging that workers in these localities are heterogeneous, we will sometimes refer to this as an average total effect or an average effect.

A third category of studies utilizes variation across both skill cells and regional labor markets in a so-called *mixture* or *local skill-cell approach* (e.g., Borjas, 2006; Card, 2009; Glitz, 2012). For our purpose, it is sufficient to note that these specifications also capture a relative effect, since impacts shared within broad categories of workers are again absorbed by fixed effects.

2.2 Immigration and Institutions

Economic theory does not provide definitive answers to how institutions alter the responses of wages and employment to immigration-induced labor supply shocks. Data from the OECD on institutions, also used in this paper and described in Section 3, has spurred a growing empirical literature that explores the interaction between institutions and various economic phenomena. Influential examples are Blanchard and Wolfers (2000) and Nickell, Nunziata, and Ochel (2005) who investigate the interplay between institutions and general macro-economic shocks.

It is possible that the constraints of strong employment protection, high collective agreement coverage and high replacement rates studied in this paper place incumbent native workers in less direct competition with immigrants, and hence, dampen the direct, competitive effect of immigration on wages and employment.

At the same time, institutions may exacerbate the total average effects because the incentives for

grants and natives.

[5] Positive average total effects are possible within the simple textbook model of immigration (see Appendix A). An influx of low-skilled immigrants, for instance, may increase natives' average wages if low-skilled native labor is sufficiently scarce (see Appendix A.7 for the additional assumptions required and a formal derivation of this result). Going beyond the simple model, immigration-induced innovation and knowledge spillovers that positively affect total factor productivity (TFP) as well as efficiency gains from specialization can potentially lift all wages (Ottaviano and Peri, 2006; Peri and Sparber, 2009; Peri, 2012; Lewis, 2013). Furthermore, immigrants are not only workers. They also consume; and hence, boost demand and job creation in the economy.

[6] See Appendix A.6 for a detailed discussion of the estimates we categorize as total effects.

workers and firms to respond efficiently to shocks is smaller (Blanchard and Wolfers, 2000). Angrist and Kugler (2003) extend this idea to immigration. They argue that strong institutions make labor market rigid and increase the negative effects of immigration. Angrist and Kugler apply the spatial approach to data on European countries (their estimates should, therefore, be classified as measuring a total effect) and show that the employment effect is more negative and native employment losses larger in countries with more restrictive institutions. D'Amuri and Peri (2014) provides a potential mechanism for this result. They show that job reallocation in response to immigration was slower in countries with stronger employment protection legislation. Brücker et al. (2014) introduce an interesting heterogeneity whereby wage rigidity varies by workers' skills. The composition of the immigrant inflow and the relative rigidities across skill cells determine the wage and employment effects in this model. For instance, employment effects may be negligible if immigrants enter a segment of the labor market where wages are more flexible. Hence, it is possible that low skilled immigration with highly flexible wages among low skilled workers will cause a drop in their relative wages and tend to reduce average wages in the economy, but employment and wages of other workers is minimally affected in this setting.

Institutional differences across countries and workers are complex and accurate predictions would generally require assumptions about the behavior of workers and firms and the specific rigidities or underlying institutions. However, a fundamental trade-off seems empirically relevant in our context: rigidities that effectively dampen the relative wage effect likely worsen the total wage effect if occupational mobility, gains from a more efficient allocation of workers to tasks (Peri and Sparber, 2009; Foged and Peri, 2016) and endogenous choice of technology (Lewis, 2013) are important mechanism whereby the total effects of immigration improve beyond the direct competitive effect. This is because it is the immigration-induced change in relative wages that stimulates reallocation across jobs, efficient specialization and changes to the mode of production that thereby improve productivity and job creation.

3 Data on Institutions

This section describes the detailed measures of labor market institutions available from the OECD and how they are used in our analysis (see OECD, 2018). Table 1 lists the countries in our database of wage and employment impacts of immigration (rows) and the distinct institutional variables we use to measure specific rigidities in labor markets (columns). Columns 1 and 2 present measures of employment protections while column 3 is an indicator of the prevalence of employment rigidities in the labor market, namely average job tenure. Columns 4 and 5 display two wage-protecting policies; the coverage of collective agreements and the net replacement rate. Lastly, to gain statistical precision, we combine

all five indicators of labor market institutions into a single index shown in column 6. Namely, we first standardize each variable to have a mean of zero and variance of one and then take the average, which we simply call "Index".[7] All values reflect country averages for the period 1998-2016. Overall, Table 1 shows that there is substantial variation in institutions across European countries, and the United States stands out as having a particularly pro-competitive labor market with the lowest score in our index.[8]

Employment Protection Legislation (EPL) regulates the length and type of employment contracts and affects the cost of firing workers. The two indices are measured on a scale from 0 to 6 and capture, respectively, protection of workers on regular/permanent contracts against individual dismissals (column 1) and the strictness of requirements for collective dismissals (column 2). Higher values correspond to stronger protection of incumbent workers. Southern European countries, such as Spain, Portugal and Italy, have particularly strong employment protection legislation. Workers on regular contracts are by far the most protected against individual dismissals in Portugal (4.52) and the definitions and procedures regarding collective dismissals are particularly strict in Italy (4.10) and Spain (3.73). Canada, the United States, and the United Kingdom, on the contrary, have the lowest level of employment protection in our sample. Countries with stronger EPL have lower job tenure as measured by the average number of years workers have been in their current or main job/employer (column 3).[9] The mean workplace tenure in our sample is roughly 10 years. The longest average stay is found among Southern European countries (e.g., 12.22 in Portugal and 12.03 in Italy), while in the United States it is only 7.51 years.

The collective bargaining coverage rate (column 4) is defined as the share of workers covered by collective agreements among all employees with the right to bargain. It is a direct measure of wage rigiditythat affects a wider spectrum of the labor market than other wage institutions such as the minimum wage.[10] Table 1 shows that European workers are covered by collective agreements at much higher rates (e.g., 97% in Austria and 88% in France) than workers in the United States and Canada (21% and 33%, respectively) while the sample average is 56%.

The net replacement rate (column 5) is the guaranteed compensation rate for a typical two-earner household (as percentage of their salaries) in the initial phase of unemployment. On the one hand, countries with more generous compensation for the unemployed could have higher wage rigidity since

[7] The information from the different institutional measures can also be reduced by means of principal component analysis (PCA) which yields similar results.

[8] We refer the reader to the Online Data Appendix for further information on the institutional data.

[9] The data source for the United States is the Job Tenure Supplement of the Current Population Survey and the sample used to calculate it is all people aged 16-64 in the labor force.

[10] An alternative and related measure of wage rigidity is trade union membership. We prefer collective bargaining coverage because it captures the actual share of the workforce covered by collective agreements. Trade union membership is misleadingly low for some countries with high wage rigidity and, unlike collective bargaining coverage, it has decreased over time due to increased use of extension clauses. For example, 88 percent of workers in France are covered by collective agreements but less than 10 percent are members of a union.

unemployment benefits act as a wage floor. On the other hand, it is important to bear in mind that the compensation level is only one component of the unemployment benefit system: the duration of benefit entitlements, the coverage of the system and the obligations of the unemployed, such as participation in active labor market programs, all affect the value of the benefits for the worker. Compensation levels are higher in Continental Europe and Scandinavia (e.g., 92% in Germany and 93% in Denmark) than in the Anglo-Saxon countries, most notably the United Kingdom (65%) and Australia (65%). The index in column 6, which contains the mean of the standardized institutional variables, confirms that Anglo-Saxon countries have the least strong labor market institutions, while the most rigid labor markets by these institutional measures are found in Southern European countries.

Next, Table 2 shows the correlation matrix of the five measures of institutions which we also refer to as labor market rigidity. The correlation coefficients are always positive and usually in the 0.2–0.8 range. All institutions are positively correlated across countries. For instance, protection of regular workers against individual dismissals is highly correlated with collective agreements coverage (0.68) and they are both strongly associated with average job tenure (0.73 and 0.59, respectively). Hence, one may think of each institution, and certainly the combined index, as measuring the same underlying concept – labor market rigidity. Our goal in the empirical analysis is to correlate the labor market impacts of immigration and these measures of labor market institutions and rigidities.

4 Estimates of the Labor Market Impacts of Immigration

This section describes how we collected the previously estimated effect sizes and determined our inclusion criteria (section 4.1), the weights (section 4.3), the tests for publication bias (section 4.4) and key descriptive statistics based on the database (section 4.5).

4.1 Sample

Building a database of estimates on the labor market effects of immigration requires selecting study-inclusion criteria based on a few key priorities. We focused on sampling studies from a wide variety of labor market settings across the developed world and from recent decades where the institutional data is available. Moreover, to ensure study (and therefore data) quality, we relied on peer-reviewed publications rather than unpublished manuscripts. Appendix Table B.1 provides a list of the studies we use and information on the included estimates.[11] For each paper and empirical specification, we

[11] We refer the interested reader to the Online Data Appendix for a detailed description of our database including selection criteria, the measurement of immigration, the conversion of different effect sizes into a comparable metric, and information on how we collected other study characteristics. Finally, semi-elasticities larger than 3 in absolute value are excluded in the main

determined whether reported effect sizes should be classified as relative effects; total effects; or neither.

Our database consists of 1,030 wage and 432 employment effects of immigration from 61 studies published between 1990 and 2020 spanning 13 European countries, the United States, Canada, Turkey, Israel and Australia. When available, we recorded data on the corresponding standard error, t-statistic (associated with the null hypothesis of the true effect equals zero), underlying sample (entire workforce or by education level), level of variation in the dependent variable, publication outlet, estimation method (OLS, IV, Difference-in-Differences or First-Differences) and empirical strategy (natural experiment, Bartik type instruments, other instruments and OLS).[12] To gauge study quality, we also included journal impact factors measured by IDEAS/RePEc (2020), a widely used index.

4.2 Comparability Across Studies

Differences in the independent variable of interest across studies require that a few adjustments be made to ensure the reported effect sizes are measured in comparable units. First, studies publish elasticities or semi-elasticities. The distinction is subtle but important. The former measure the percentage change in natives' wages or employment induced by a one percent increase in foreign born workers, while the latter do the same for a percentage point increase in foreign born workers. Following Longhi, Nijkamp, and Poot (2005), we convert all elasticities to semi-elasticities since this is the more commonly used unit in the literature. Second, the wage effects are sometimes reported for growth rates rather than levels (of immigration). In that case, we rescale the parameter to reflect a one percentage point increase in the proportion of foreign-born workers. Hence, our estimates are interpreted as the percentage change in wages (or percentage point change in the employment rate) for a one percentage point increase in immigrants as a share of the labor force. The majority of the collected wage effects are from a regression with either hourly, daily or weekly earnings as the dependent variable (332 out of 613 relative and 381 out of 417 total wage effects). The remaining estimates are based on monthly, quarterly or yearly earnings. The employment effect estimates combine the impacts on the employment rate with the negative of the impacts on the unemployment rate.[13] The Online Data Appendix contains a complete description of the operations and technical details involved in creating comparable estimates across studies.

While we attempt to do our best in assuring comparability across studies, an inherent limitation of the meta-analysis approach is the inevitable differences in samples and regression specifications for which we cannot account. For instance, included control variables or fixed effects are rarely exactly identical.[14]

analysis and included in the robustness checks in section 6.3.

[12] Bartik type instruments use past immigrant settlement patterns and national immigrant inflows to instrument local immigrant inflows.

[13] We assume the participation rate is close to one and unaffected by immigration.

[14] However, we lay out some minimal requirements in Appendix A.3 that the included effect sizes need to meet in order to

The sample definitions vary as well – e.g., 16-65 versus 21-54 years old, working age population versus labor force, both sexes versus only men etc. Lastly, immigration may occur due to high demand for labor and the composition of the workforce may react endogenously to immigration creating a bias in the underlying estimates if not properly accounted for. We have no reason to believe these factors are systematically associated with labor market institutions and hence bias our regression results in a certain direction. Section 4.3 presents the steps we take in order to place higher weight on observations that, by objective criteria, are more reliable and precise.

4.3 Weighting

The sheer differences across studies raise the question of how much importance to place on each effect size. For instance, should the semi-elasticities from a paper reporting only five estimates receive equal weights as ones derived from a paper with 40? Similarly, should articles published in the top academic journals, which have arguably undergone a more rigorous review process and are supposedly of higher quality, be weighted similarly to ones from lower ranked outlets? Lastly, one can argue that more precisely estimated semi-elasticities should also be given higher importance as they contain less statistical noise.

To verify the robustness of our results and to account for discrepancies across estimates and studies, we introduce several different weighting factors. First, we assign each effect size an equal weight and we refer to these as our "unweighted" results. Second, we give equal weights to each article, and we call these our "studies" weights. This procedure uses the number of reported semi-elasticities per article as an inverse probability weight and it intuitively down-weighs effect sizes coming from studies reporting many such semi-elasticities.[15] Third, we account for study quality by using journals' impact factors from RePEc and utilizing the inverse score as a weight. We refer to these as "impact" weights. This strategy assigns higher importance to estimates published in higher ranked journals. Fourth, we adjust for the precision of the included estimates by using the inverse standard error as a weighting factor. These are our "precision" weights. Finally, we construct a "combined" weight in which we multiply together the preceding three weighting factors. This measure gives higher importance to effect sizes (i) published in higher ranked outlets, (ii) which are more precisely estimated and (iii) come from papers with fewer published semi-elasticities. We conduct our analyses separately with each of these weighting factors.

be included as either a relative or a total effect.

[15] See the Appendix Table B.1 for a list of included studies and the associated number of estimates.

4.4 Publication Bias

It is well-known that, all else equal, studies with statistically significant estimates are more likely to be published in academic journals. Unfortunately, this introduces an inherent bias towards more significant estimates even in a random sample of published studies.

To analyze whether such a bias may be present in our sample, we follow standard practice in the literature (e.g., Stanley and Doucouliagos, 2014, 2012; Christensen and Miguel, 2018) and conduct two tests. First, Figure 1 presents funnel plots of our estimates of the wage (Panel a) and employment (Panel b) impacts of immigration. This is a scatter plot of estimated precision (inverse standard error) against effect magnitude. For visual clarity, we have removed a few outliers with precision values greater than 80. Estimates generated from smaller, noisier samples form a more dispersed base, while more precisely estimated effects are more narrowly clustered around the "true" effect. The median effect size is marked with a red vertical line. In the absence of publication bias, the scatter plot should be symmetric around this value. For instance, if negative estimates were more likely to be published than positive ones, we would expect an asymmetric excess mass on the left of this vertical line. In our case, this plot does not indicate publication bias.

Second, to formally test the symmetry of the funnel plot, we proceed with a regression analysis. Following the meta-analysis literature in economics (e.g., Stanley and Doucouliagos, 2012; Christensen and Miguel, 2018), we regress the t-statistic on a constant and the standard error. The coefficient of interest is that on the constant, which quantifies the correlation between precision and effect size. In a setting without publication bias, it would not be statistically significantly different from zero. Table 3 presents the results. Each column uses a different weighting factor which we described in section 4.3. Standard errors are clustered by study and shown in parentheses. Only one of the five coefficients in each panel is marginally statistically significant (at the 10% level). The rest are not significant, confirming the visual inspection and concluding symmetry of the (unweighted) funnel plot.

Overall, the results presented in this subsection assert that publication bias is likely not a major concern for the analyses we conduct. For the purpose of the results in section 6, publication bias, even if present, would only be problematic if it is correlated with labor market rigidity across countries. We have no reason to believe this would be the case.

4.5 Summary Statistics

Table 4 presents descriptive statistics for relative (Panel a) and total (Panel b) wage impact estimates along with relative (Panel c) and total (Panel d) employment impact estimates. Each column shows a

different statistic denoted in the header. The top row displays information on our entire sample. First, 1,030 wage estimates are divided into 613 relative and 417 total wage impacts, and 432 employment impact estimates are divided into 222 relative and 210 total employment impacts. A one percentage point increase in the share of foreign workers, on average, lowers the relative wage of unskilled workers by 0.38 percent compared to the relative wage of skilled workers (Table 4 Panel a).[16] The same influx is associated with a 0.14 percent increase in the average labor market wages (Table 4 Panel b). In other words, foreign workers assert stronger pressure on the relative earnings between skill groups than on the overall wage level. This is an interesting finding which has not been thoroughly documented in previous summaries of the literature.[17] It is indeed the prediction of the canonical heterogeneous labor model (see Appendix A) in which immigration-induced changes in relative skill supplies directly alter the earnings differential, while the average total effect may be close to null or even positive when the economy has adjusted to the immigrant inflow.[18]

Panels c and d of Table 4 present analogous summary statistics for employment impacts of immigration. They should be interpreted with caution as some sub-groups are based on few effect sizes. Overall, the employment effects are negative but most of them are small. A one percentage point increase in the share of foreign workers reduces the employment rate of unskilled workers by 0.06 percentage points relative to the employment rate of skilled workers. The same inflow of immigrants reduces overall employment by 0.18 percentage points.

Figure 2 visualizes the distribution of the estimated wage (left panels) and employment (right panels) effects of immigration. Total effects are shown in solid lines and relative effects in dashed lines. Each panel weights the semi-elasticities with a different factor denoted in the panel heading. A few key facts are worth highlighting.

First, the distributions of total and relative wage effects look similar overall and symmetric, but the distribution of relative effects is shifted to the left relative to that of average effects. We can't reject the null hypothesis of first order stochastic dominance of the former relative to the latter (p-value=0.995). For employment effects, this pattern is reversed and weaker (p-value=0.601).[19] The means and the medians of the relative wage effects are negative, while those of the total wage impacts are smaller in

[16]This interpretation is relevant for the majority of the countries in our sample because their immigrant population is less skilled than their native born population. More generally, the parameter captures the effect on the workers who are more versus less similar to immigrants.

[17]Peri (2014), Lewis and Peri (2015) and Dustmann, Schönberg, and Stuhler (2016) discuss influential wage impact estimates and conclude, consistent with our findings, that estimates from the national and local skill-cell approaches (relative partial effects) tend to be more negative than estimates from pure spatial approaches (total effects).

[18]The pooled mean (median) of relative and total wage effect sizes in our sample (not shown in the table) is -0.15 (-0.03) which is very close to the -0.12 (-0.04) reported in Longhi, Nijkamp, and Poot (2005), which is based on a smaller sample of 345 effect sizes from 18 papers.

[19]We follow the test procedure outlined in Barrett and Donald (2003); Abadie (2002). See Table B.2 for more details.

magnitude and usually positive. These differences in means and medians are statistically significant (p-values < 0.000). The statistics of centrality for employment are generally small and negative. The differences between relative and total effects are small albeit statistically significant (p-values=0.079 and 0.032 for the means and medians respectively). Second, down-weighing noisy estimates and assigning larger weights to papers in better ranked journals results in eliminating outliers and narrowing the distributions such that all measures of centrality are very close to zero. This holds for both the wage and employment impact estimates.

Rows 2 to 4 of Table 4 group the collected effect sizes by continent. The relative wage effects of immigration are more negative in North America (-0.58) compared to Europe (-0.26) and the rest of the world (-0.03), and the average wage effects are more positive in North America (0.16) compared to Europe (0.13) and the rest of the world (0.11). The relative employment effects are also more negative in North America (-0.23) than in other countries (-0.04), while the opposite is the case for the total employment effect of immigration. Moreover, the relative wage effects are close to zero in Southern Europe (mean = -0.05, median = 0.02, N = 74) which we describe in section 3 as having particularly strong institutions, and the relative wage effects are large in the more flexible Anglo-Saxon economies (mean = -0.51, median = -0.38, N = 348). At the same time, total wage effects are positive but small in Anglo-Saxon economies (mean = 0.17, median = 0.17, N = 223) and negative in Southern Europe (mean = -0.74, median = -0.49, N = 22). Furthermore, relative employment effects are larger in Anglo-Saxon economies (mean = -0.21, median = -0.13, N = 40) than in Southern Europe (mean = -0.06, median = -0.14 , N = 98), while the negative total employment impacts are smaller in magnitude in Anglo-Saxon countries (mean = -0.06, median = -0.06, N = 66) than in Southern Europe (mean = -0.21, median = -0.19, N = 4). This provides suggestive evidence of the idea that labor market rigidity reduces the impacts on wage and employment inequality and on the potential gains associated with immigration.

Researchers have long warned about spurious correlations between wages and immigration levels leading to biased estimates of the labor market impact of immigration. Empirical strategies based on natural experiments or Bartik-style instruments tend to find more negative (total) wage and employment effects than studies utilizing other instrumental variable and selection on observables strategies. This is consistent with the idea that careful identification decreases this bias. Estimates published in the higher ranked journals are typically also the ones based on identification strategies relying on natural experiments. Hence, one may think of the impact weight as giving most weight to better identified estimates.

The next sub-panel summarizes the estimates by effects on high and low skilled natives as defined

in the underlying studies. The relative wage of low skilled workers falls by 0.81 percent following an increase in the immigrant share of one percentage point, while the relative wage effect for high skilled workers is an increase of 0.16 percent. Consistent with this, total wages increase for skilled workers (0.14), while the total wage change for low skilled is close to zero. Finally, we see notable differences in reported effects of immigration by academic journal quality. Namely, papers published in top 50 outlets feature more negative semi-elasticities than the rest.

5 Empirical Strategy

We estimate the following equation:

$$y_{ict} = \alpha + \beta \cdot institution_c + X'_{ct}\gamma + Z'_i\psi + \lambda_r + \varepsilon_{ict},$$

where the subscripts i, c and t refer to effect size, country and time period, respectively. The outcome variable y_{ict} is the estimated immigration wage or employment effect and $institution_{ct}$ is an institutional variable of interest described in section 3. Next, the vector X_{ct} includes GDP growth and unemployment rate and controls for national time-varying economic conditions affecting labor market tightness and institutions. These variables account for situations in which, for example, economic booms absorb foreign workers more smoothly and lead to lower institutional coverage, such as lower rates of collective agreements. The vector Z_i contains study and semi-elasticity characteristics, such as data frequency, estimation method, and the skills of natives. The term λ_r is a vector of continent dummies for Europe and the rest of the world (North America is the reference group), and ε_{ict} is the error term. We estimate this equation with weighted least squares using the various weighting factors described in section 4.3 separately for relative and total wage and employment effects. The standard errors are clustered by study but do not account for the fact that our outcome variable is already estimated (as in Card, Kluve, and Weber, 2018).

The coefficient of interest is β. In an ideal scenario, we would like to estimate the interaction between the labor market effects of immigration and institutions within countries. However, institutional change may arise endogenously from a surge in immigration (Rodrik, 1997) and there is little within country-variation in labor market rigidity in our sample. Hence, the model we estimate identifies β from cross-country variation within continents (while controlling for national economic conditions, X_{ct}). This is perhaps a limitation but we believe it is the closest we can get to an association without wiping out the useful part of the variation in the data. Furthermore, we control for study characteristics, Z_i, and use

various weighting factors to improve comparability of estimates. These steps are important, because, just like any meta-analysis, our findings are limited by the quality and comparability of the underlying studies.

6 The Association Between Institutions and Impacts of Immigration

6.1 Wage Impacts and Institutions

Table 5 displays our main results for the effects on wages. Panel a shows the impacts of labor market rigidity on the relative wage effect of immigration and Panel b on the total wage effect. Each entry is an estimated coefficient $\hat{\beta}$ of an institution variable shown in the rows. Each column corresponds to a different weighting factor denoted in the column header. Standard errors are shown in parenthesis and are clustered by paper. All regressions control for study characteristics, region fixed effects and local economic conditions. To mitigate the influence of a few large outliers, we have removed semi-elasticities larger than 3 in absolute value. We present robustness checks varying these specification choices.

Nearly all coefficients in Panel a are positive and some of them are statistically significant. This suggests that institutional rigidity might be effective in mitigating the relative wage changes induced by foreign workers. The first two sets of results show the regression coefficients of our EPL variables for individual and collective dismissals, respectively. The association between the individual dismissal index and the relative wage effect of immigration is not statistically significant. Four estimates for collective dismissals are significant, while the last is positive but not distinguishable from zero. A one unit increase in this index – a change roughly equivalent to two standard deviations which corresponds to the difference between the United Kingdom and Spain – is associated with a large 0.27-0.50 percentage point increase in the relative wage effects of immigration. The third set of estimates displays the effects of average job tenure in years, which proxies for overall employment rigidity. An increase of this variable by one year – a change a bit smaller than one standard deviation and roughly the difference between the United Kingdom and Norway – is associated with a 0.15-0.32 percentage point higher relative wage effect of foreign-born workers and all coefficients but one are statistically significant.

Next, the fourth row presents the results for wage rigidity as measured by collective bargaining coverage. The coefficients are all positive but noisily estimated and not significant. The fifth row shows the same results for the net replacement rate as a percentage of wages, which do not show a clear pattern. Two of the coefficients are positive and the rest are negative, while none are statistically significant. We are cautious to interpret the fourth and fifth rows as robust associations because of the lack of statistical significance and the large change in magnitude of the coefficients depending on the weighting factor.

The last row displays the results using the combined index of the standardized institution variables. A one unit increase in this index – which is roughly the difference between the United States and Switzerland – is associated with a 0.32-0.73 increase in the relative wage, and all estimates but one are significant.

Panel b tells the opposite story – labor market rigidity is associated with lower total wage effects of immigration. The association presented here is stronger than the relationship shown in Panel a. The first two rows show that a one unit increase in the employment rigidity indices is associated with a 0.12-0.54 percentage point lower wage effect of foreign workers. Regardless of the weighting factor, all signs are negative, pointing to the same conclusion. Similarly, the third row shows higher employment rigidity (longer average job tenure) leads to worse average wage consequences of immigration. An increase in the average tenure by one year is associated with a lowered semi-elasticity of about 0.09-0.41. All five coefficients are statistically significant. Next, our first wage rigidity variable – collective bargaining coverage – is the only one that does not show a consistent pattern in this panel. The coefficients change sign depending on the weighting factor and are not statistically significant. For the second wage rigidity measure, we find evidence that the higher net replacement rates are again associated with lowered total wage effects of immigration. A single unit increase in this variable is related to a 0.53-0.75 decrease in the associated semi-elasticity. Lastly, a one unit increase in the combined index of institutions is associated with a 0.10-0.15 percentage point lower total wage effect.

Overall, Table 5 suggests that higher labor market rigidity is associated with exacerbation of the total wage impacts of immigration and shielding from redistribution consequences. In other words, labor market rigidity may protect incumbent native workers, but it also dampens the potential benefits induced by the immigration. For some of the institution variables, this pattern is stronger and holds across various different weighting factors.

6.2 Employment Impacts and Institutions

Table 6 presents our main results for the impacts on employment. Most coefficients in Panel a are negative, while in Panel b most are positive, but few are statistically significant. The signs suggest that the role of labor market rigidity on employment effects might be reversed compared to wage effects - institutions may be associated with exacerbated employment inequality between skill groups but higher (less negative / more positive) effects on total employment. Currently, we have only weak and limited evidence suggesting this connection. A potential reason, is the small sample sizes – the number of observations is as low as 94, and it is never higher than 170. We do not have enough statistical precision

to make more concrete statements, and we conclude that more research is needed to uncover the role of labor market rigidity in affecting the relative and total employment impact of immigration.

6.3 Robustness Checks

We conducted several robustness checks which we present in Appendix Tables B.3 and B.4 for wages and Appendix Tables B.5 and B.6 for employment. In Tables B.4 and B.6 all columns use the combined weights. First, we ran the same analysis without excluding outlier semi-elasticities larger than 3 in absolute value. The results for wages are shown in Table B.3 and are largely similar to the main results. The broad patterns we observe hold, but the coefficients are generally more noisily estimated (less often significant) as expected with the presence of outliers. Next, in column 1 of Table B.4 (in both panels) we replicate the results from Table 5 described above. In columns 2 and 3 we change the sample by excluding estimates using variation across industries (and skill cells) and based on OLS regressions. Next, in columns 4 through 6 we change the controls by excluding, in turn, study characteristics, proxies for national economic conditions and continent dummies. While there is some idiosyncratic variation in the estimates across columns and panels, they rarely flip signs and the general pattern broadly holds. This is especially true for the results in Panel b where most coefficients are negative and statistically significant.

Tables B.5 and B.6 present the same robustness checks for the impacts on employment. Table B.5 presents slightly stronger evidence of negative coefficients in Panel a and positive coefficients in Panel b, but this association might be purely driven by the addition of a few outliers. The coefficients in Table B.6 are mostly negative, further suggesting using caution when interpreting the impacts on employment. Overall, we do not find robust evidence for a meaningful relationship between labor market rigidity and employment impact estimates.

7 Discussion

We measure labor market rigidity as brought about by more restrictive institutions and investigate their role in intermediating the competition forces between native and foreign workers. We find that these institutions are associated with shielding domestic economies from redistributional wage consequences and have modest adverse total effects on the wage level. We do not find robust patterns for employment effects. Furthermore, we uncover clear patterns in the underlying estimated wage and employment effects of immigration. Our results entail several broad implications.

First, we document significant differences in the estimated relative and total labor market effects

of immigration. For wages, the relative impacts are negative and larger in magnitude compared with the mostly positive but smaller effects on average earnings. For employment, this pattern is reversed – while most magnitudes are small, the consequences on relative employment by skill cell tend to be larger (more negative). Future research should improve our understanding of the causes of these two patterns.

Second, labor market institutions can be adequate and useful tools for policymakers concerned that immigration might worsen wage inequality. Our estimates suggest that a one standard deviation increase in some of the rigidity variables is associated with roughly a 0.12-0.32 and a 0.05-0.50 change in the semi-elasticities for relative and total wage effects, respectively. While these intervals are wide, they suggest that magnitudes of the effects we estimate might be substantial. Unfortunately, our analysis of the impacts on employment does not yield precise conclusions. There are far fewer published employment effects compared with wage effects of immigration, and we call for other researchers to explore this margin of adjustment and its association with institutional constraints more deeply. Third, a thorough understanding of the interaction between labor market rigidity and wage effects is fundamental in generalizing the labor market impact of immigration from one country to another. Relatedly, differences in labor market rigidity across countries are likely important drivers of the large heterogeneity of estimates in the literature on the impact of immigration.

Our study suffers from several limitations. First and foremost, we cannot claim that we have uncovered a causal relationship, and our estimates should be interpreted as associations. Second, the limited statistical power of our analysis does not allow us to make concrete statements about the magnitudes of the effects we observe. Lastly, just like any other meta-analysis, we are not able to fully control for idiosyncratic study-specific characteristics. To the extent that they may be biasing our results, we note that they have to be correlated with country-level labor market rigidity. Moreover, the results from the various robustness checks we present in the appendix broadly follow the patterns we observe in the main specification, suggesting these study differences may not be quantitatively important. With these limitations notwithstanding, we believe the general pattern we find is novel and informative about the labor market impacts of immigration.

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8 Figures and Tables

Table 1: Institutional Strength and Coverage by Country

	(1) *EPL (Regular)*	(2) *EPL (Collective)*	(3) *Average Job Tenure*	(4) *Collective Bargaining*	(5) *Net Replacement Rate*	(6) *Index*
			Europe			
Albania	2.14	3.13	-	-	-	-
Austria	2.61	3.25	10.20	0.97	0.85	0.54
Denmark	2.16	3.25	8.17	0.83	0.93	0.24
France	2.39	3.38	11.03	0.88	0.85	0.60
Germany	2.65	3.63	10.49	0.70	0.92	0.69
Ireland	1.40	3.13	9.81	0.40	0.79	-0.41
Italy	2.76	4.10	12.03	0.80	0.85	1.03
Netherlands	2.92	3.01	9.83	0.78	0.80	0.21
Norway	2.33	2.50	9.13	0.68	0.86	-0.16
Portugal	4.52	2.50	12.22	0.76	0.91	0.92
Spain	2.75	3.73	9.74	0.80	0.88	0.64
Switzerland	1.60	3.63	9.11	0.47	0.90	0.04
United Kingdom	1.17	2.86	8.25	0.44	0.65	-1.09
			North America			
Canada	0.92	2.97	-	0.33	0.85	-
United States	0.26	2.88	7.51	0.21	0.84	-1.13
			Rest of the World			
Australia	1.32	2.88	-	0.70	0.65	-
Israel	2.04	1.88	-	0.52	0.91	-
Turkey	2.36	2.63	-	0.15	0.75	-
Mean	2.13	3.11	9.94	0.56	0.83	0.16
SD	1.03	0.47	1.36	0.27	0.10	0.82

Data source: OECD (2018), job tenure in the United States is from the Job Tenure Supplement of the Current Population Survey.

Notes: Employment protection legislation (EPL) indices range from 0 to 6 (columns 1-2), average job tenure is measured in years (column 3), collective bargaining is the ratio of employees covered by collective agreements divided by all wage earners with the right to collective bargaining (column 4), and the net replacement rate is the net compensation rate in the initial phase of unemployment (column 5). All values are averaged over the 1998-2016 period. Column 6 shows a combined index for the five different variables if they are not missing. The last two rows show the sample means and standard deviations.

Table 2: Correlation Between Wage and Employment Institutions

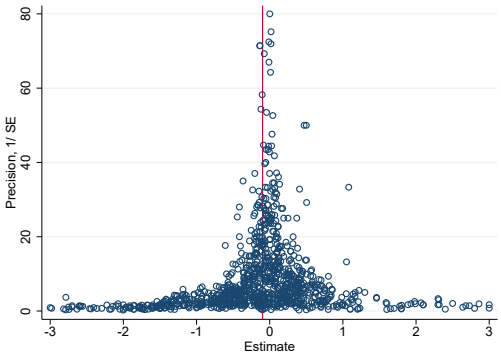
	(1) *EPL (Regular)*	(2) *EPL (Collective)*	(3) *Average Job Tenure*	(4) *Collective Bargaining*	(5) *Net Replacement Rate*
EPL (Regular)	1.00				
EPL (Collective)	0.05	1.00			
Average Job Tenure	0.73^{***}	0.16^{**}	1.00		
Collective Bargaining	0.68^{***}	0.41^{***}	0.59^{***}	1.00	
Net Replacement Rate	0.39^{***}	0.21^{***}	0.29^{***}	0.38^{***}	1.00

Data source: OECD (2018), job tenure in the United States is from the Job Tenure Supplement of the Current Population Survey.

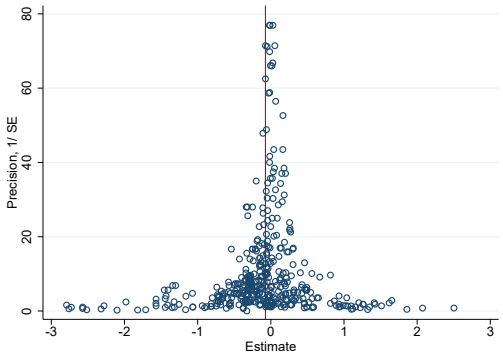
Notes: $^{*}\ p < 0.10$, $^{**}\ p < 0.05$, $^{***}\ p < 0.01$.

Figure 1: Funnel Plots of Estimated Labor Market Impacts of Immigration

(a) Wage



(b) Employment



Notes: Scatter plot of precision (inverse standard error) and magnitude of the estimated wage and employment effects. The red vertical line shows the median value. A few outliers with precision greater than 80 have been removed for clarity.

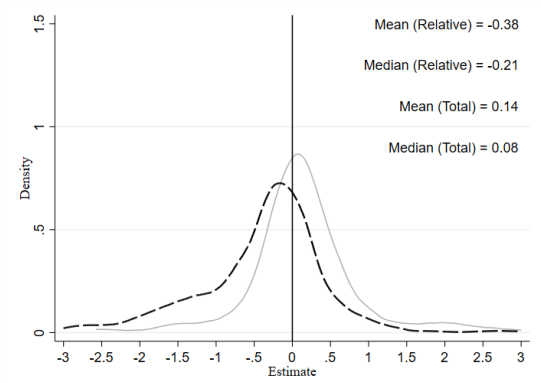
Table 3: Publication Bias Test

	(1) Unweighted	(2) Studies	(3) Impact	(4) Precision	(5) Combined
			Panel a. Wage		
Publication Bias Term	9.276	22.565*	-1.558	-36.409	-3.365
	(8.352)	(12.636)	(0.976)	(37.394)	(3.735)
N	999	999	999	999	999
R^2	0.001	0.003	0.000	0.002	0.001
			Panel b. Employment		
Publication Bias Term	-2.404	2.359	-5.915*	-0.852	2.723
	(3.686)	(5.757)	(3.277)	(2.480)	(3.866)
N	428	428	428	428	428
R^2	0.006	0.004	0.030	0.002	0.006

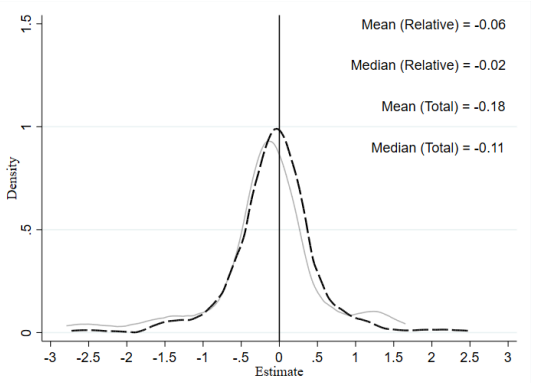
Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each column presents the estimated intercept from a bivariate regression of the t-statistic on precision (inverse standard error) for different weighting factors on a sample of estimated wage and employment impacts of immigration. The first column is an unweighted regression while the next four are weighted: by the inverse number of estimates extracted from the study (Studies), by the inverse journal score (Impact), by the inverse of the standard error of the estimates (Precision), and, finally, by combining all three weights (Combined).

Figure 2: The Distributions of Estimated Labor Market Impacts of Immigration

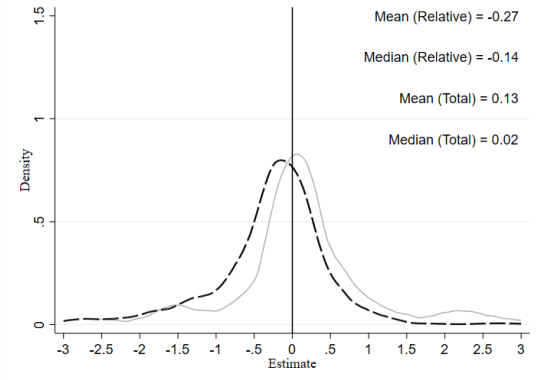
(a) Wage, Unweighted



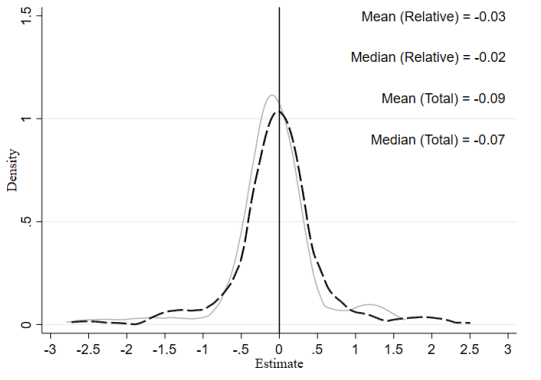
(b) Employment, Unweighted



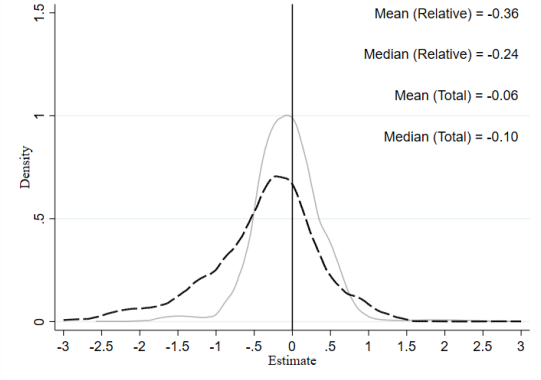
(c) Wage, Studies Weights



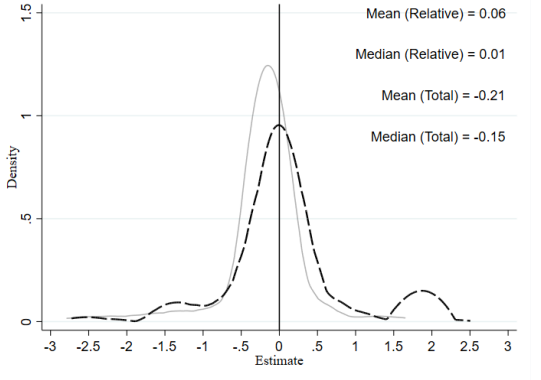
(d) Employment, Studies Weights



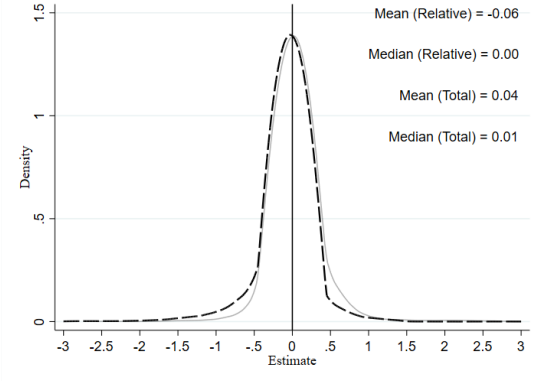
(e) Wage, Impact Weights



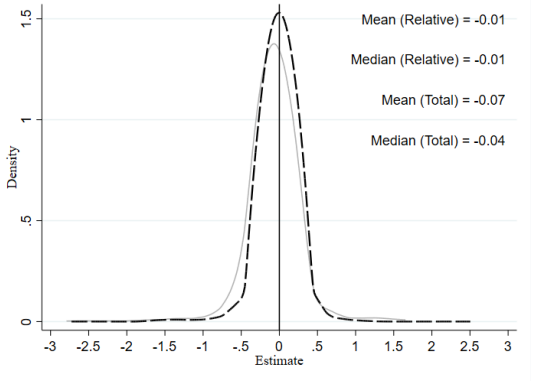
(f) Employment, Impact Weights



(g) Wage, Precision Weights



(h) Employment, Precision Weights



relative, and total

Notes: Kernel densities of relative (dashed) and total (solid) wage and employment effect sizes using the Epanechnikov kernel and Stata's default bandwidth choice. The estimates are unweighted in Panels a and b, while the rest of the panels show weighted distributions: by the inverse number of effect sizes extracted from each study (Panels c and d), by the inverse journal rank score (Panels e and f) or by the inverse standard error (Panels g and h).

Table 4: Summary Statistics of Estimates of the Impacts of Immigration

	Panel a: Wage, Relative				Panel b: Wage, Total				Panel c: Employment, Relative				Panel d: Employment, Total			
	Mean	Median	N	Studies	Mean	Median	N	Studies	Mean	Median	N	Studies	Mean	Median	N	Studies
All	-0.38	-0.21	613	28	0.14	0.08	417	27	-0.06	-0.02	222	17	-0.18	-0.11	210	14
By Region																
Europe	-0.26	-0.16	265	14	0.13	0.11	299	14	-0.06	-0.02	165	12	-0.18	-0.07	137	7
North America	-0.58	-0.41	286	11	0.16	-0.01	117	12	-0.23	-0.16	28	2	-0.07	-0.09	24	4
Rest of the World	-0.03	-0.01	62	3	0.11	0.11	1	1	0.05	-0.01	29	3	-0.22	-0.28	49	3
By Empirical Strategy																
IV (Bartik Type)	-0.17	-0.15	43	7	-0.17	0.20	16	3	0.22	0.20	45	6	-1.07	-1.27	4	1
IV (Natural Experiment)	-0.21	0.06	11	2	-0.01	-0.11	37	5	-0.75	-1.08	4	2	-0.80	-1.07	28	2
IV (Other)	-1.03	-1.30	115	6	0.33	0.24	108	11					-0.18	-0.12	40	7
Natural Experiment	-0.20	-0.04	58	5	-0.35	-0.02	29	8	-0.31	-0.04	35	4	-0.12	-0.12	10	4
OLS	-0.25	-0.16	386	24	0.16	0.10	227	14	-0.07	-0.05	138	13	-0.01	-0.08	128	10
By Native Education Group																
All	-0.40	-0.24	502	26	0.18	0.19	209	19	-0.07	-0.02	204	17	-0.22	-0.12	155	11
High Skill	0.16	-0.10	55	10	0.14	0.06	99	9	-0.13	-0.15	7	3	0.08	0.01	15	3
Low Skill	-0.81	-0.58	56	14	0.06	-0.01	109	12	0.05	0.19	11	5	-0.09	-0.09	40	7
By Journal Rank																
Outside Top 50	-0.19	-0.10	284	15	0.18	0.07	263	14	-0.07	-0.07	119	9	0.02	-0.06	79	7
Top 50	-0.55	-0.41	329	13	0.08	0.12	154	13	-0.06	0.00	103	8	-0.30	-0.15	131	7

Notes: Panels a and b show statistics for relative and total wage effects, respectively. Panels c and d show statistics for relative and total employment effects, respectively. N denotes the number of estimates, and Studies is the number of studies. The total number of studies by subgroups exceeds the total number of studies in our database (61 articles study wage impacts and 29 articles study employment impacts) since some studies report estimates for multiple subgroups and methods.

Table 5: Labor Market Institutions and the Wage Effect of Immigration

	Panel a: Relative					Panel b: Total				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
EPL (Regular)	0.029	0.330	-0.004	-0.034	0.169	-0.252	-0.538**	-0.198**	-0.121**	-0.178**
	(0.261)	(0.334)	(0.304)	(0.092)	(0.271)	(0.167)	(0.217)	(0.094)	(0.056)	(0.066)
N	443	443	443	429	429	411	411	411	394	394
EPL (Collective)	0.393***	0.501**	0.084	0.362***	0.266**	-0.118	-0.136	-0.313	-0.160**	-0.354**
	(0.091)	(0.179)	(0.167)	(0.079)	(0.114)	(0.303)	(0.875)	(0.187)	(0.066)	(0.150)
N	443	443	443	429	429	411	411	411	394	394
Average Job Tenure	0.297***	0.317**	0.152*	0.218***	0.064	-0.245**	-0.408**	-0.175**	-0.092**	-0.115**
	(0.069)	(0.116)	(0.081)	(0.051)	(0.107)	(0.115)	(0.148)	(0.084)	(0.038)	(0.053)
N	363	363	363	349	349	410	410	410	393	393
Collective Bargaining	0.396	1.156	1.448	0.208	1.294	0.131	1.089	-0.277	-0.107	-0.124
	(0.570)	(0.845)	(1.417)	(0.469)	(0.873)	(0.599)	(1.301)	(0.509)	(0.245)	(0.429)
N	443	443	443	429	429	411	411	411	394	394
Net Replacement Rate	0.054	1.643	-2.500	-0.186	-2.260	-0.547	-0.762	-0.536	-0.526*	-0.752**
	(1.334)	(1.962)	(1.808)	(0.775)	(2.129)	(0.792)	(1.456)	(0.492)	(0.291)	(0.319)
N	443	443	443	429	429	411	411	411	394	394
Index	0.665***	0.729***	0.317**	0.421***	0.153	-0.177	-0.367	-0.146*	-0.095**	-0.129**
	(0.108)	(0.135)	(0.151)	(0.131)	(0.229)	(0.139)	(0.285)	(0.081)	(0.037)	(0.052)
N	363	363	363	349	349	410	410	410	393	393
Region FE	X	X	X	X	X	X	X	X	X	X
Country-Level Controls	X	X	X	X	X	X	X	X	X	X
Study Characteristics	X	X	X	X	X	X	X	X	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each entry is an estimated coefficient from a regression of relative (Panel a) or total (Panel b) wage effects of immigration on institutional strength and controls. The institutional variables are described in section 3. To alleviate the influence of large outliers, we exclude all estimates larger than 3 in absolute value. The first column is an unweighted regression while the next four columns in each panel are weighted: by the inverse number of estimates extracted from the study (Studies), by the inverse journal score (Impact), by the inverse of the standard error of the estimates (Precision), and, finally, by combining all three weights (Combined). Region FE are dummies for Europe and the rest of the world (North America is the reference). Country-level controls are GDP growth and unemployment rate. Study characteristics are dummies for high- and low-skilled natives (the entire workforce is the reference), dummy for IV (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference). Standard errors shown in parentheses are clustered by study.

Table 6: Labor Market Institutions and the Employment Effect of Immigration

	Panel a: Relative					Panel b: Total				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
EPL (Regular)	-0.160	-0.119	-0.714**	-0.143	-0.185	0.237	0.125	-0.002	0.131	-0.067
	(0.242)	(0.254)	(0.281)	(0.188)	(0.234)	(0.144)	(0.130)	(0.204)	(0.174)	(0.162)
N	170	170	170	169	169	128	128	128	125	125
EPL (Collective)	0.048	0.209	-0.954	-0.049	-0.069	0.520	0.267	-0.030	0.305	-0.369
	(0.530)	(0.397)	(0.734)	(0.139)	(0.107)	(0.301)	(0.341)	(0.428)	(0.545)	(0.295)
N	170	170	170	169	169	128	128	128	125	125
Average Job Tenure	-0.117	-0.081	-0.256**	-0.066	-0.215	0.186**	0.092	0.139	0.091	-0.072
	(0.092)	(0.110)	(0.112)	(0.059)	(0.138)	(0.078)	(0.075)	(0.125)	(0.129)	(0.136)
N	147	147	147	146	146	97	97	97	94	94
Collective Bargaining	-0.430	-0.486	0.400	-0.253	-0.170	0.825	0.373	0.165	0.402	0.369
	(0.601)	(0.737)	(1.613)	(0.302)	(0.659)	(0.612)	(0.226)	(0.797)	(0.376)	(0.229)
N	170	170	170	169	169	128	128	128	125	125
Net Replacement Rate	-0.881	0.047	-4.888**	-0.267	-0.316	1.416	0.747	-0.052	0.824	-0.757
	(1.375)	(1.168)	(2.004)	(0.448)	(0.418)	(0.831)	(0.865)	(1.189)	(1.264)	(0.952)
N	170	170	170	169	169	128	128	128	125	125
Index	-0.116	0.015	-0.392**	-0.066	-0.125	0.239**	0.120	0.170	0.113	-0.068
	(0.174)	(0.172)	(0.151)	(0.087)	(0.120)	(0.105)	(0.108)	(0.160)	(0.155)	(0.150)
N	147	147	147	146	146	97	97	97	94	94
Region FE	X	X	X	X	X	X	X	X	X	X
Country-Level Controls	X	X	X	X	X	X	X	X	X	X
Study Characteristics	X	X	X	X	X	X	X	X	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each entry is an estimated coefficient from a regression of relative (Panel a) or total (Panel b) employment effects of immigration on institutional strength and controls. The institutional variables are described in section 3. To alleviate the influence of large outliers, we exclude all estimates larger than 3 in absolute value. The first column is an unweighted regression while the next four columns in each panel are weighted: by the inverse number of estimates extracted from the study (Studies), by the inverse journal score (Impact), by the inverse of the standard error of the estimates (Precision), and, finally, by combining all three weights (Combined). Region FE are dummies for Europe and the rest of the world (North America is the reference). Country-level controls are GDP growth and unemployment rate. Study characteristics are dummies for high- and low-skilled natives (the entire workforce is the reference), dummy for IV (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference). Standard errors shown in parentheses are clustered by study.

A Appendix: Classification of Effect Sizes

The empirical literature on the labor market impacts of immigration builds on classic factor demand theory. This appendix derives the theoretical counterparts of the two main empirical approaches used in the primary studies that we draw from in our meta-analysis and explains the underlying assumptions.

A.1 Factor Demand Theory

The effect sizes that we use fit into a general framework where capital has an equal degree of substitutability with all workers.[20] Hence, the following production function for region r (a national or a local economy) is general enough to encompass all the effect sizes:

$$Y_r = G(A_r, K_r, L_r), \tag{1}$$

$$L_r = F(A_{r1}, L_{r1}, A_{r2}, L_{r2}, ..., A_{rs}, L_{rn}). \tag{2}$$

Y_r is the output, K_r is the physical capital, L_r is a labor aggregate and A_r captures total factor productivity (TFP) as well as the relative productivity of capital.[21] The aggregate labor factor combines n distinct labor inputs, $L_{r1}, L_{r2}, ..., L_{rn}$, and their relative productivity parameters, $A_{r1}, A_{r2}, ..., A_{rn}$. Depending on the study, workers may be differentiated by education or occupation and sometimes experience, and we refer to each combination as a skill cell s. Immigrant (IMM) and native (NAT) workers can be correctly assigned to these cells and are perfect substitutes within them: $L_{rs} = L_{rs}^{NAT} + L_{rs}^{IMM}$.[22] Both functions $G(\cdot)$ and $F(\cdot)$ are homogeneous of degree one (i.e., exhibit constant returns to scale), strictly increasing and strictly concave (implying some degree of substitutability between inputs).

Profit maximization and perfect competition imply that workers are paid their marginal product. Ignoring the subscript r, we have the following labor demand function:

$$w = \frac{\partial Y}{\partial L} = G_L, \tag{3}$$

$$w_s = \frac{\partial Y}{\partial L}\frac{\partial L}{\partial L_s} = G_L F_s. \tag{4}$$

[20] See Lewis (2011, 2013) and Lafortune, Lewis, and Tassada (2019) for analyses of relative complementarity between capital and skilled (college-educated) workers. These papers are not included in our meta-analysis.

[21] The simplest textbook model features identical workers. In such a world, by construction, there are no distributional effects of immigration and capital flexibility pins down the impact on workers. Following an influx of foreign-born workers, wages decrease in the short run due to declining marginal product of labor. In the long run, capital adjusts and there is no wage or employment effect. This is too simplistic. Hence, we proceed here with n distinct types of workers.

[22] Downgrading of immigrants' skill (Dustmann, Schönberg, and Stuhler, 2016) and imperfect substitutability (Manacorda, Manning, and Wadsworth, 2012; Ottaviano and Peri, 2012) lead to an overstatement of the negative relative, partial effect estimated in national and local skill-cell approaches. The spatial approach is immune to these biases because it relies on the overall immigrant share.

Wages in each skill cell, w_s, are then determined by the marginal productivity of the labor aggregate, G_L, and the marginal contribution of skill group, s, to that labor aggregate, F_s, given the labor supplies.[23] The first term changes if TFP changes ($dA \neq 0$) or the capital-labor ratio changes ($d\frac{K}{L} \neq 0$). The second term changes if the relative skill supply is altered ($\frac{dL}{L} \neq \frac{dL_s}{L_s}$).

A.2 Partial and Total Effects

The "total effect" of immigration on natives in skill cell s includes all changes occurring on the right-hand-side of equation (4) in response to immigration. It captures the change in natives' wages or employment due to immigration when allowing all features of the economy to respond to the arrival of foreign-born workers. The "partial effect," on the contrary, isolates the direct impact of an immigration-induced supply change in the same skill cell, holding fixed all cross-cell effects (in F_s) as well as impacts on TFP and the productivity of capital (in G_L). The next section adds more structure in order to be able to derive the (log) linear estimating equations used in the literature.

A.3 Nested CES

A popular characterization of the workforce distinguishes workers by education and experience and combines them in a nested constant elasticity of substitution (CES) structure.[24] Furthermore, labor and capital are combined in a Cobb-Douglas (or a CES) production function. Hence, equations (5) and (6) take the following specific form:

$$Y = AK^{\alpha}L^{1-\alpha}$$

$$L = \left[\sum_{g=1,2,..n} A_g L_g^{\frac{\beta-1}{\beta}}\right]^{\frac{\beta}{\beta-1}} \tag{5}$$

$$L_g = \left[\sum_{a=1,2,..m} A_{ga} L_{ga}^{\frac{\gamma-1}{\gamma}}\right]^{\frac{\gamma}{\gamma-1}} \tag{6}$$

where g and a typically denote education levels and age groups, respectively. The parameter β is the elasticity of substitution across education groups and γ is the elasticity of substitution across experience groups. The education (experience) groups are perfect substitutes when β (γ) goes to infinity. Skill cells are now ga combinations, reflecting the two skill dimensions.[25]

[23] Notice that w in equation (3) is not the average wage in the economy, as sometimes stated, but rather an artificial theoretical concept because L is an efficiency-weighted aggregate of n distinct labor inputs (not body counts). See section A.7.

[24] Due to influential papers by Borjas (2003), Manacorda, Manning, and Wadsworth (2012) and Ottaviano and Peri (2012).

[25] Lewis and Peri (2015) and Dustmann, Schönberg, and Stuhler (2016) derive common estimating equations using similar CES production functions. We borrow from both to keep the discussion as simple as possible and still general enough to encompass the effect sizes we use. Unlike these papers, we present the estimated partial (relative) and total effects in more general fixed effect models.

The CES structure is popular because it provides a simple expression of the log marginal productivity of each skill cell as a function of its supply (L_{ga}), the aggregate supplies and the elasticities of substitution across skill categories. To see this, re-state the first order condition of the profit maximization problem (equation 4):

$$\begin{aligned} w_{ga} &= \frac{\partial Y}{\partial L_{ga}} = \frac{\partial Y}{\partial L}\frac{\partial L}{\partial L_g}\frac{\partial L_g}{\partial L_{ga}} \\ &= (1-\alpha)AL^{-\alpha}K^{\alpha}A_g L_g^{\frac{-1}{\beta}} L^{\frac{1}{\beta}} A_{ga} L_{ga}^{\frac{-1}{\gamma}} L_g^{\frac{1}{\gamma}}, \end{aligned}$$

and take logs and total derivatives to get the labor demand expressed in percentage changes

$$\frac{dw_{ga}}{w_{ga}} = \alpha\left(\frac{dK}{K} - \frac{dL}{L}\right) + \frac{1}{\beta}\left(\frac{dL}{L} - \frac{dL_g}{L_g}\right) + \frac{1}{\gamma}\left(\frac{dL_g}{L_g} - \frac{dL_{ga}}{L_{ga}}\right). \tag{7}$$

Immigration is the only source of variation in L, L_g and L_{ga} if native labor supply is perfectly inelastic and we simply have to replace the change in each of them with the immigration-induced labor supply shock – and assume something about capital adjustments – to calculate the impact of immigration on wages (and employment is unaffected).

Employment effects occur in these models because native workers react to wage changes (elastic supply). The stronger the labor supply responses, the smaller the (total and partial) wage effects and the larger the corresponding employment effects. Infinite elastic supply mutes the wage effects and creates complete crowding out, i.e., one immigrant worker displaces one native worker. Without modeling the supply side, we can allow for labor and capital supply responses by re-parameterizing equation (7):[26]

$$\frac{d\text{outcome}_{ga}}{\text{outcome}_{ga}} = \underbrace{a\left(\frac{dL}{L}\right)}_{\leq 0} + \underbrace{b}_{\geq 0}\left(\frac{dL}{L} - \frac{dL_g}{L_g}\right) + \underbrace{c}_{\geq 0}\left(\frac{dL_g}{L_g} - \frac{dL_{ga}}{L_{ga}}\right) \tag{8}$$

where a, b and c are functions of demand side (elasticities of substitution) and supply side (elasticities of supply) parameters, the outcome is either employment or wages and we substitute in the immigration shock (measured in efficiency units similar to the labor aggregates in equations (5-6).

[26] We assume that the labor supply elasticity is the same for all workers. Heterogeneous labor supply elasticities mean that some groups react more strongly than others to changes in wages, meaning that relative supplies are affected through this channel too (not only through the immigrant inflow and the substitution elasticities) and this can possibly reverse the standard predictions for some groups. See the online appendices of Dustmann, Schönberg, and Stuhler (2016) for Dustmann, Schönberg, and Stuhler (2017) for treatment of this possibility.

A.4 Three Important Insights

1. The first term in equation (8) disappears if capital is perfectly elastic. This is a typical long-run assumption but it might be a reasonable assumption even in the short run when immigration is a slow and gradual process, such that investors have time to react to increasing rental rates and adjust the capital stock.

   The first term has an equal negative effect on all workers if immigration reduces capital per worker (this is the first assumption in section A.1).

2. An immigrant inflow with the same skill composition as the incumbent workers has no effect on the economy, beyond the potential short-run effect in point 1.

3. Immigration decreases wages and employment of natives with skills most similar to immigrants and may increase wages and employment for other workers; especially those most dissimilar (complementary) to immigrant workers in production. Specifically:

   - The second term in equation (8) tends to reduce wages and employment for education group g and may improve outcomes for everyone else through complementarity. Hence, low skilled immigration reduces wages and employment of low skilled workers and may improve the outcomes of other education groups.
   - The last term in equation (8) implies that an immigrant inflow concentrated in skill-cell ga further lowers wages and employment for this group (e.g., young workers) compared to other workers in the same education group g.

A.5 Estimating (Relative) Partial Effects

The nested CES model of section A.3 suggests an estimate of the partial own-wage elasticity $\frac{dw_{ga}/w_{ga}}{dL_{ga}/L_{ga}} = -\frac{1}{\gamma} < 0$ can be obtained by simply regressing the change in log wages in skill-cell ga on the change in the cell-specific log labor force while holding the aggregate and the education-specific labor supplies constant by absorbing their changes with fixed effects ($d\log w_{gat} = d\pi_t + (d\pi_t \times s_g) - \frac{1}{\gamma} d\log L_{gat}$). The more careful skill-cell specifications control for shocks that are common to all workers, such as TFP growth (π_t), shocks that are common to everyone in the same education group ($s_g \times \pi_t$), shocks that are shared within experience groups ($x_a \times \pi_t$) and the education-experience specific productivity ($s_g \times x_a$) in fixed effect models of the following form:

$$\log w_{gat} = \theta^{skill} p_{gat} + s_g + x_a + \pi_t + (s_g \times x_a) + (s_g \times \pi_t) + (x_a \times \pi_t) + \varphi_{gat}. \tag{9}$$

These models estimate the partial effect on exposed workers relative to less or not exposed workers. The natural logarithm of L_{gat} is replaced by $p_{gat} = \frac{L_{gat}^{Im}}{L_{gat}}$ in the empirical models and θ^{skill} in equation (9) becomes a semi-elasticity capturing the percentage change in the wage for a one percentage-point change in the immigrant share.[27]

Specification (9) follows Borjas (2003) and is by far the most common in the estimation of relative, partial wage effects representing 12 out of 28 studies and 195 out of 613 wage effect estimates classified as providing relative wage effects (82 of 417 employment estimates from 5 out of 27 studies follow this specification). An additional 50 wage estimates from 4 papers and 2 employment estimates from one paper provide a similar estimate using occupation-experience cells (e.g., Basten and Seigenthaler, 2019). In total, 18 and 12 papers provide a relative wage semi-elasticity by using, respectively, education and occupation, and sometimes dropping the differentiation by age (e.g, Friedberg, 2001) and/or distinguishing regional labor markets, r, in the variation in the immigrant share and changing the fixed effects accordingly (for employment estimates, 9 and 8 papers use education and occupations, respectively).[28] Estimates based on variation in some broadly defined skill-cells and regions in a "mixture approach" are important contributions to the pool of estimated relative effects. In fact, the second and third most common variation after the classical education-experience-time are occupation-region-time (80 wage estimates from 5 papers and 34 employment estimates from 5 studies, e.g., Addison and Worswick, 2002) and education-region-time (65 wage estimates from 5 papers and 29 employment estimates from 3 papers, e.g., Borjas, Freeman, and Katz, 1996).

A.6 Estimating Total Effects

The total effect on skill-cell ga can be obtained by relating wages of workers in the cell to the immigrant share across regions and time, controlling for level-differences across regions (z_{gar}) as well as aggregate time shocks common to all regions (π_{gat}):[29]

$$y_{gart} = \mu_{ga}^{spatial} p_{rt} + z_{gar} + \pi_{gat} + \varphi_{gart}.$$

[27]The Online Data Appendix explains how we convert estimates from equations with alternative measures of immigration into a comparable semi-elasticity.

[28]A few studies further differentiate workers by gender or industry, see Appendix Table B.1. These are not published in top journals and hence contribute little when we weight by impact (inverse journal score). Note, Barrett, Bergin, and Kelly (2011) and Glitz (2012) provide estimates based on education as well as occupation and are, therefore, counted twice here. We have 28 papers providing some type of relative effect.

[29]Note, equation (10) is estimated separately for each skill cell ga and hence produce an estimate of the total effect for each of skill cell (see Dustmann, Schönberg, and Stuhler, 2016, for a detailed discussion of of this parameter).

Many studies that rely on spatial variation only report estimates for all workers (or by high and low skilled). We call these average total effects and the estimations take the following general form:

$$y_{gart} = \mu^{spatial} p_{rt} + s_g + x_a + z_r + \pi_t + \varphi_{gart}. \tag{10}$$

The important terms are z_r and π_t because this is the level of variation in the immigrant share defining the spatial approach. These regression usually include some control for education and age (s_g and x_a). Notice, that TFP, production technology and factor supplies in equations (5) and (6), and hence, wages differ across regions, highlighting the importance of region fixed effects in the spatial approach. The aggregate time effects account for the macro-economic conditions.

We flag 417 wage estimates from 27 studies and 210 employment estimates from 14 studies as estimating a total effect based on spatial variation. A few, mainly old studies, report estimates based on cross-sectional variation only (e.g., Altonji and Card, 1991) and they are included as well. More recent examples of the spatial approach include Foged and Peri (2016); Peri and Yasenov (2019).

A.7 The Average Total Effects

Most of the studies providing estimates based on the spatial approach do not consider detailed skill-cells but simply estimate an average total effect. This effect behaves differently from the wage level in equation (3), which either decreases or is unaffected depending on the elasticity of capital supply. For simplicity, consider the case of two broad skill categories, L_1 and L_2. Using the same notation as in section 2, average wages for natives can be expressed as:

$$\begin{aligned} \bar{w}^N &= \frac{1}{L_1^N + L_2^N} \left(L_1^N w_1 + L_2^N w_2 \right) \\ &= \frac{G_L}{L_1^N + L_2^N} \left(L_1^N F_1 + L_2^N F_2 \right), \end{aligned}$$

where F_1 and F_2 denote the respective partial derivatives. Assume that $\frac{K}{L}$ is a constant.[30] An immigration-induced supply change in one skill group ($dL_1^{Im} > 0$) produces the following impact on the average

[30]This can be justified if immigration is a gradual process, allowing firms to invest in capital and expand production as immigrants come in and create an upward pressure on the return to capital. If the immigration episode is abrupt and unexpected, one may think of the derivations as describing the economy in the long run.

wages of native workers:

$$\begin{aligned}\frac{\partial \bar{w}^N}{\partial L_1} &= \frac{G_L}{L_L^N + L_2^N}\left(L_1^N F_{11} + L_2^N F_{21}\right) \\ &= \frac{G_L F_{21}}{L_1^N + L_2^N}\left(L_1^N \frac{F_{11}}{F_{21}} + L_2^N\right). \end{aligned} \tag{11}$$

The expression in equation (11) is positive if:[31]

$$\begin{aligned} L_1^N \frac{F_{11}}{F_{21}} + L_2^N &> 0 \\ L_2^N &> L_1^N \frac{-F_{11}}{F_{21}} \\ L_2^N &> L_1^N \frac{-F_{11} L\sigma}{F_1 F_2}. \end{aligned} \tag{12}$$

Therefore, average wages of native workers could increase in response to, e.g., low-skilled immigration, if (i) the economy has relatively few low skilled natives (L_1^N), (ii) the complementarity with high-skilled workers is strong (small σ) and (iii) the decline in the marginal productivity of low-skilled workers is modest (small $|-F_{LL}|$). Hence, immigration does not need to increase innovation and TFP growth in order to produce positive average effects in the long run. Instead, this may arise from simple complementarities with the native-born workforce.

[31] Notice, that $F_{11} < 0$ and $F_{21} > 0$ and the elasticity of substitution takes the following form $\sigma = \frac{F_1 F_2}{F F_{12}}$ because F is homogeneous of degree one. We also know that F_1 is homogeneous of degree zero when F is homogeneous of degree one and hence $F_{11} L_1 + F_{21} L_1 = 0$, also known as the Euler identity.

B Appendix B: Additional Material

Table B.1: List of Studies, Outcomes Estimated, Variation Used and Empirical Strategy

Authors	Year	Journal	Countries	Data Period	N Wage	N Employment	Variation	Empirical Strategy
Addison and Worswick	2002	The Economic Record	Australia	1982-1996	26	-	ort	IV (Other); OLS
Altonji and Card	1991	Immigration, trade, and the labor market	U.S.	1970-1980	20	8	r; rt	IV (Other); OLS
Angrist and Kugler	2003	Economic Journal	European Economic Area	1983-1999	-	63	rt	OLS; IV (Natural Experiment)
Aydemir and Borjas	2011	Journal of Labor Economics	Canada, U.S.	1960-2001	28	-	gat; gart	OLS
Aydemir and Kirdar	2017	European Economic Review	Turkey	1985-1990	-	28	rt	OLS; IV (Other)
Barrett et al.	2011	The Economic and Social Review	Ireland	1999-2007	30	-	gat; oat	IV (Other); OLS
Basten and Siegenthaler	2019	Scandinavian Journal of Economics	Switzerland	2002-2011	4	2	oat	OLS; IV (Bartik Type)
Bauer et al.	2013	Review of International Economics	Germany	2000-2005	19	13	rt	IV (Other); OLS
Borjas	2014	Immigration Economics	U.S.	1960-2010	3	-	gat	OLS
Borjas	2017	Industrial and Labor Relations Review	U.S.	1977-1992	6	-	rt	Natural Experiment
Borjas	2003	The Quarterly Journal of Economics	U.S.	1960-2000	28	-	gart; gat	OLS
Borjas et al.	1996	American Economic Review	U.S.	1980-1990	20	-	r; gr; grt	OLS
Borjas et al.	1997	Brookings Papers on Economic Activity	U.S.	1960-1990	28	14	grt	OLS
Bratsberg and Raaum	2012	Economic Journal	Norway	1998-2005	33	-	ot	IV (Other); OLS
Bratsberg et al.	2014	Scandinavian Journal of Economics	Norway	1993-2006	31	-	gat	OLS
Brunelloa et al.	2020	Journal of Labor Economics	Italy	2006-2016	4	-	rt	IV (Bartik Type)
Card	1990	Industrial and Labor Relations Review	U.S.	1982-1979	3	3	rt	Natural Experiment
Card	2001	Journal of Labor Economics	U.S.	1990	14	14	or	OLS; IV (Bartik Type)
Card and Peri	2016	Journal of Economic Literature	U.S.	1960-2010	4	-	gat	OLS
Carrasco et al.	2008	Journal of Population Economics	Spain	1991-2001	10	13	gak; gakrt; gakr; gakt	OLS
Carrington and De Lima	1996	Industrial and Labor Relations Review	Portugal	1975-1973	7	-	rt	IV (Natural Experiment); Natural Experiment
Cattaneo et al.	2015	Journal of Human Resources	Western Europe	1994-2001	14	14	ort	OLS; IV (Bartik Type)
Clark and Drinkwater	2009	Nordic Journal of Political Economy	U.K.	2000-2007	6	6	gat; oat	Natural Experiment
Cohen-Goldner and Paserman	2011	European Economic Review	Israel	1989-1999	20	20	iot; ort; gat; ot	Natural Experiment
D'Amuri and Peri	2014	Journal of the European Economic Association	Western Europe	1996-2010	-	22	gart	IV (Bartik Type); OLS
Dustmann and Glitz	2015	Journal of Labor Economics	Germany	1985-1995	11	-	grt	IV (Bartik Type); OLS
Dustmann et al.	2005	Economic Journal	U.K.	1983-2000	5	12	rt	OLS; IV (Other)
Dustmann et al.	2013	Review of Economic Studies	U.K.	1997-2005	66	-	rt	OLS; IV (Other); IV (Bartik Type)
Dustmann et al.	2017	The Quarterly Journal of Economics	Germany	1990-1993	24	-	rt	Natural Experiment; IV (Natural Experiment)
Edo	2015	The B.E. Journal of Economic Analysis & Policy	France	1990-2002	29	55	gat	OLS
Enchautegui	1995	Contemporary Economic Policy	U.S.	1980-1990	8	-	rt	OLS; IV (Other)
Foged and Peri	2016	American Economic Journal: Applied Economics	Denmark	1995-2008	12	-	rt	OLS; IV (Natural Experiment)
Friedberg	2001	The Quarterly Journal of Economics	Israel	1989-1994	16	3	ot; o	Natural Experiment; IV (Natural Experiment); OLS
Gavosto et al.	1999	LABOUR	Italy	1990-1995	15	-	irt	Natural Experiment
Glitz	2012	Journal of Labor Economics	Germany	1996-2001	13	10	grt; ort	Natural Experiment; IV (Natural Experiment)
González and Ortega	2011	Labour Economics	Spain	2001-2006	6	12	grt	IV (Bartik Type); OLS
Hausmann and Nedelkoska	2018	European Economic Review	Albania	2012-2014	5	-	rt	Natural Experiment; IV (Natural Experiment)
Hothckiss et al.	2015	Southern Economic Journal	U.S.	1995-2005	10	-	irt	OLS
Hunt	1992	Industrial and Labor Relations Review	France	1962-1962	5	4	r; rt	Natural Experiment; IV (Natural Experiment)

Jean and Jimenez	2011	European Journal of Political Economy	OECD countries	1984-2003	-	24	gart; rt	OLS
LaLonde and Topel	1991	Immigration, trade, and the labor market	U.S.	1980	16	-	rt	OLS
Lemos and Portes	2014	The B.E. Journal of Economic Analysis & Policy	U.K.	2004-2006	8	36	ot; rt; ort	IV (Other); OLS
Llull	2017	Journal of Human Resources	U.S., U.S. and Canada	1960-2000	104	-	gat; gart	IV (Other); OLS
Mitaritonna et al.	2017	European Economic Review	France	1996-2005	6	-	rt	OLS; IV (Bartik Type)
Monras	2020	Journal of Political Economy	U.S.	1994-1995	4	-	rt	IV (Natural Experiment)
Moreno-Galbis and Tritah	2016	European Economic Review	Western Europe	1998-2004	-	6	ort	IV (Bartik Type); OLS
Olney	2012	Canadian Journal of Economics	U.S.	2000-2006	20	-	irt	OLS; IV (Bartik Type)
Orrenius and Zavodny	2007	Labour Economics	U.S.	1994-2000	31	-	ort	OLS; IV (Other)
Ortega and Verdugo	2014	Labour Economics	France	1968-1999	14	18	gat; gart	IV (Bartik Type); OLS
Pedace	2006	American Journal of Economics and Sociology	U.S.	1980-1990	21	-	r	IV (Other)
Pedace	1998	Eastern Economic Journal	U.S.	1980-1990	12	-	r	IV (Other)
Peri and Yasenov	2018	Journal of Human Resources	U.S.	1973-1991	5	1	rt	Natural Experiment
Pischke and Velling	1997	The Review of Economics and Statistics	Germany	1985-1989	-	12	rt	OLS; IV (Bartik Type)
Reed and Danziger	2007	American Economic Review	U.S.	1989-1999	12	12	rt	OLS; IV (Other)
Schmidt and Jensen	2013	The Annals of Regional Science	Denmark	1997-2006	2	-	rt	OLS
Smith	2012	Journal of Labor Economics	U.S.	1980-2007	6	-	rt	IV (Other)
Steinhardt	2011	The B.E. Journal of Economic Analysis & Policy	Germany	1975-2001	49	1	gat; oat	IV (Other); OLS
Tumen	2016	American Economic Review	Turkey	2010-2013	1	3	rt	Natural Experiment
Winter-Ebmer and Zweimüller	1999	Journal of Population Economics	Austria	1989-1991	-	3	r	OLS; IV (Other)
Winter-Ebmer and Zweimüller	1996	Oxford Economic Papers	Austria	1991	4	-	r	IV (Other); OLS
Zorlu and Hartog	2005	Journal of Population Economics	U.K., Netherlands, Norway	1989-1998	132	-	r	OLS; IV (Other)

Notes: Year refers to the year of publication. Data period refers to the overall period considered in the study; estimates within a study are sometimes based on sub-periods. N denotes the number of wage and employment effect sizes obtained from each study. The second-last column shows the variation used to identify the impact of immigration where g stands for education groups, a age groups, t is time, r is region, k gender, i industry and o occupations. Estimates based on region (r) or, more commonly, region-time variation (rt) are classified as following the spatial approach and estimating a total effect. While the remaining either resemble the skill-cell or mixture approach and identify a relative partial effect. The last column shows the different types of empirical strategies used in a study.

Table B.2: Difference in Distribution Statistics between Total and Relative Effects (p-values)

	(1) Wages	(2) Employment
Tests for Equality of:		
Means	0.000	0.079
Medians	0.000	0.032
Variances	0.007	0.029
Tests for Stochastic Dominance:		
First Order	0.995	0.601
Second Order	0.993	0.958

Notes: Each row presents the p-value from a test of the differences of a given distribution statistic between total and relative effects of immigration on wages (column 1) and employment (column 2). For means we use a two-sample t-test with unequal variances, for medians we use a Wilcoxon rank-sum test and for variances we use a Levene's test. The tests of first and second order stochastic dominance are based on the procedure outlined in Abadie (2002); Barrett and Donald (2003) where the null hypotheses are stochastic dominance. Note that for wages we hypothesized that the distribution of average effects dominates that of relative effects, while for employment we hypothesized the opposite.

Table B.3: Institutions and the Wage Effect of Immigration, Including |Effect Sizes| > 3

	Panel a: Relative					Panel b: Total				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
EPL (Regular)	0.044	0.196	-0.149	-0.013	0.140	-0.299	-0.530**	-0.197**	-0.118**	-0.178**
	(0.294)	(0.334)	(0.466)	(0.105)	(0.267)	(0.204)	(0.232)	(0.093)	(0.056)	(0.066)
N	497	497	497	483	483	425	425	425	408	408
EPL (Collective)	0.313	0.385	-0.431	0.395***	0.268**	-0.239	-0.030	-0.279	-0.157**	-0.357**
	(0.217)	(0.245)	(0.457)	(0.100)	(0.117)	(0.333)	(0.696)	(0.192)	(0.067)	(0.150)
N	497	497	497	483	483	425	425	425	408	408
Average Job Tenure	0.129	0.214	-0.008	0.219***	0.058	-0.199	-0.149	-0.164*	-0.089**	-0.114**
	(0.224)	(0.192)	(0.219)	(0.062)	(0.118)	(0.163)	(0.300)	(0.080)	(0.038)	(0.052)
N	415	415	415	401	401	424	424	424	407	407
Collective Bargaining	-1.556	-0.102	0.707	-0.029	1.176	-0.317	-0.042	-0.267	-0.103	-0.137
	(1.117)	(1.107)	(1.872)	(0.554)	(0.884)	(0.922)	(1.986)	(0.498)	(0.244)	(0.425)
N	497	497	497	483	483	425	425	425	408	408
Net Replacement Rate	1.026	1.896	-3.905	0.135	-2.303	-1.419	-4.161**	-0.521	-0.547*	-0.759**
	(1.746)	(1.481)	(4.203)	(0.838)	(2.209)	(1.208)	(1.825)	(0.507)	(0.270)	(0.318)
N	497	497	497	483	483	425	425	425	408	408
Index	0.417	0.574***	0.170	0.450***	0.136	-0.249	-0.437	-0.138*	-0.095**	-0.129**
	(0.316)	(0.194)	(0.346)	(0.148)	(0.249)	(0.159)	(0.256)	(0.078)	(0.035)	(0.052)
N	415	415	415	401	401	424	424	424	407	407
Region FE	X	X	X	X	X	X	X	X	X	X
Country-Level Controls	X	X	X	X	X	X	X	X	X	X
Study Characteristics	X	X	X	X	X	X	X	X	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each entry is an estimated coefficient from a regression of relative (Panel a) or total (Panel b) wage effects on institutional strength and controls. The institutional variables are described in section 3. The first column is an unweighted regression while the next four columns in each panel are weighted regressions: by the inverse number of estimates extracted from the study (Studies), by the inverse journal score (Impact), by the inverse of the standard error of the estimates (Precision), and, finally, by combining all three weights (Combined). Region FE are dummies for Europe and the rest of the world (North America is the reference). Country-level controls are GDP growth and unemployment rate. Study characteristics are dummies for high- and low-skilled natives (estimates based on the entire workforce is the reference), dummy for IV (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference). Standard errors shown in parentheses are clustered by study.

Table B.4: Institutions and the Wage Effect of Immigration, Additional Robustness Checks

	Panel a: Relative						Panel b: Total					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
EPL (Regular)	0.169	0.210	-0.389***	0.163	-0.109	0.016	-0.178**	-0.178**	-0.187***	-0.226***	-0.189***	-0.007
	(0.271)	(0.274)	(0.125)	(0.262)	(0.104)	(0.049)	(0.066)	(0.066)	(0.050)	(0.073)	(0.059)	(0.031)
EPL (Collective)	0.266**	0.175	0.431***	0.240**	0.195*	-0.095	-0.354**	-0.354**	-0.405***	-0.478***	-0.247	-0.069
	(0.114)	(0.105)	(0.115)	(0.115)	(0.107)	(0.107)	(0.150)	(0.150)	(0.125)	(0.157)	(0.160)	(0.111)
Average Job Tenure	0.064	-0.145	-0.013	0.125	0.096	0.005	-0.115**	-0.115**	-0.154**	-0.144**	-0.138**	-0.010
	(0.107)	(0.133)	(0.103)	(0.091)	(0.119)	(0.046)	(0.053)	(0.053)	(0.061)	(0.054)	(0.057)	(0.026)
Collective Bargaining	1.294	1.290	-1.158***	1.279	0.106	0.108	-0.124	-0.124	-0.282	-0.278	-0.153	0.085
	(0.873)	(0.857)	(0.273)	(0.832)	(0.538)	(0.254)	(0.429)	(0.429)	(0.412)	(0.471)	(0.702)	(0.175)
Net Replacement Rate	-2.260	-1.399	-0.444	-2.426	-1.863	0.138	-0.752**	-0.752**	-0.897***	-1.079***	-1.038***	-0.607*
	(2.129)	(1.924)	(1.923)	(2.274)	(2.019)	(0.780)	(0.319)	(0.319)	(0.249)	(0.343)	(0.342)	(0.308)
Index	0.153	-0.048	0.060	0.243	0.072	0.006	-0.129**	-0.129**	-0.159***	-0.174***	-0.167***	-0.011
	(0.229)	(0.268)	(0.250)	(0.210)	(0.245)	(0.079)	(0.052)	(0.052)	(0.042)	(0.057)	(0.056)	(0.043)
Region FE	X	X	X	X	X	No	X	X	X	X	X	No
Country-Level Controls	X	X	X	X	No	X	X	X	X	X	No	X
Study Characteristics	X	X	X	No	X	X	X	X	X	No	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are weighted linear regressions with the effect size as the dependent variable. Weights correspond to the combined weights used in Table 5. The first column in each panel replicates results from Table 5. Column 2 excludes effect sizes based on variation across industries. Column 3 excludes effect sizes estimated with OLS if IV estimates are reported within studies. Column 4 shows estimates without study characteristics as controls. Column 5 shows estimates from regressions without country-level controls (unemployment rate and GDP growth). Column 6 shows estimates without region FE. Country-level controls and study characteristics are described in Table 5. Standard errors in parentheses are clustered by studies. See the Online Data Appendix for a complete description of all the variables.

Table B.5: Institutions and the Employment Effect of Immigration, Including |Effect Sizes| > 3

	Panel a: Relative					Panel b: Total				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
EPL (Regular)	-0.376	-0.253	-1.343**	-0.250	-0.246	0.977***	0.611	0.556	0.368	-0.030
	(0.326)	(0.346)	(0.465)	(0.240)	(0.297)	(0.313)	(0.372)	(0.433)	(0.353)	(0.186)
N	176	176	176	175	175	134	134	134	131	131
EPL (Collective)	-0.255	0.238	-1.909	-0.116	-0.084	2.139***	1.551*	1.132	1.020	-0.275
	(0.703)	(0.569)	(1.195)	(0.199)	(0.131)	(0.519)	(0.799)	(0.863)	(0.931)	(0.398)
N	176	176	176	175	175	134	134	134	131	131
Average Job Tenure	-0.165	-0.125	-0.351*	-0.094	-0.233	0.649**	0.402*	0.576*	0.270	-0.031
	(0.130)	(0.142)	(0.167)	(0.078)	(0.157)	(0.207)	(0.217)	(0.271)	(0.255)	(0.170)
N	153	153	153	152	152	103	103	103	100	100
Collective Bargaining	-0.595	-0.908	0.643	-0.413	-0.322	3.369	1.131	2.339	0.890	0.436
	(0.897)	(1.012)	(2.933)	(0.422)	(0.832)	(2.026)	(0.911)	(2.111)	(0.908)	(0.251)
N	176	176	176	175	175	134	134	134	131	131
Net Replacement Rate	-1.960	-0.152	-8.610**	-0.487	-0.380	5.826***	4.021*	3.189	2.522	-0.505
	(1.897)	(1.617)	(3.521)	(0.640)	(0.510)	(1.578)	(2.209)	(2.444)	(2.328)	(1.179)
N	176	176	176	175	175	134	134	134	131	131
Index	-0.211	0.009	-0.588**	-0.109	-0.131	0.840**	0.520	0.720*	0.328	-0.029
	(0.245)	(0.216)	(0.228)	(0.117)	(0.128)	(0.271)	(0.300)	(0.350)	(0.312)	(0.179)
N	153	153	153	152	152	103	103	103	100	100
Region FE	X	X	X	X	X	X	X	X	X	X
Country-Level Controls	X	X	X	X	X	X	X	X	X	X
Study Characteristics	X	X	X	X	X	X	X	X	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Each entry is an estimated coefficient from a regression of relative (Panel a) or total (Panel b) employment effects on institutional strength and controls. The institutional variables are described in section 3. The first column is an unweighted regression while the next four columns in each panel are weighted regressions: by the inverse number of estimates extracted from the study (Studies), by the inverse journal score (Impact), by the inverse of the standard error of the estimates (Precision), and, finally, by combining all three weights (Combined). Region FE are dummies for Europe and the rest of the world (North America is the reference). Country-level controls are GDP growth and unemployment rate. Study characteristics are dummies for high- and low-skilled natives (estimates based on the entire workforce is the reference), dummy for IV (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference). Standard errors shown in parentheses are clustered by study.

Table B.6: Institutions and the Employment Effect of Immigration, Additional Robustness Checks

	Panel a: Relative						Panel b: Total					
	(1)	(2)	(3)	(4)	(5)	(6)	(1)	(2)	(3)	(4)	(5)	(6)
EPL (Regular)	-0.185	-0.190	-0.430*	-0.174	-0.276	-0.017	-0.067	-0.067	-0.191*	-0.177	-0.020	-0.026
	(0.234)	(0.234)	(0.233)	(0.220)	(0.312)	(0.216)	(0.162)	(0.162)	(0.098)	(0.119)	(0.103)	(0.057)
EPL (Collective)	-0.069	-0.069	1.098*	-0.072	-0.129	-0.255	-0.369	-0.369	-0.608**	-0.523**	-0.229	-0.240
	(0.107)	(0.108)	(0.602)	(0.106)	(0.193)	(0.225)	(0.295)	(0.295)	(0.244)	(0.181)	(0.249)	(0.228)
Average Job Tenure	-0.215	-0.215	-0.186	-0.073	-0.276	-0.071	-0.072	-0.072	-0.117***	-0.133	-0.020	-0.033
	(0.138)	(0.138)	(0.248)	(0.106)	(0.199)	(0.104)	(0.136)	(0.136)	(0.016)	(0.074)	(0.057)	(0.070)
Collective Bargaining	-0.170	-0.185	-0.831	-0.176	-0.343	0.015	0.369	0.369	-0.550	-0.259	0.183	0.063
	(0.659)	(0.664)	(0.462)	(0.582)	(0.932)	(0.824)	(0.229)	(0.229)	(0.390)	(0.321)	(0.181)	(0.150)
Net Replacement Rate	-0.316	-0.317	2.202	-0.325	-0.438	-0.160	-0.757	-0.757	-1.355*	-1.286*	-0.362	-0.725
	(0.418)	(0.419)	(1.822)	(0.410)	(0.650)	(0.364)	(0.952)	(0.952)	(0.627)	(0.618)	(0.689)	(0.922)
Index	-0.125	-0.125	-0.252	-0.078	-0.187	-0.083	-0.068	-0.068	-0.118***	-0.149	-0.024	-0.048
	(0.120)	(0.120)	(0.653)	(0.111)	(0.201)	(0.126)	(0.150)	(0.150)	(0.017)	(0.100)	(0.085)	(0.114)
Region FE	X	X	X	X	X	No	X	X	X	X	X	No
Country-Level Controls	X	X	X	X	No	X	X	X	X	X	No	X
Study Characteristics	X	X	X	No	X	X	X	X	X	No	X	X

Notes: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Models are weighted linear regressions with the effect size as the dependent variable. Weights correspond to the combined weights used in Table 5. The first column in each panel replicates results from Table 5. Column 2 excludes effect sizes based on variation across industries. Column 3 excludes effect sizes estimated with OLS if IV estimates are reported within studies. Column 4 shows estimates without study characteristics as controls. Column 5 shows estimates from regressions without country-level controls (unemployment rate and GDP growth). Column 6 shows estimates without region FE. Country-level controls and study characteristics are described in Table 5. Standard errors in parentheses are clustered by studies. See the Online Data Appendix for a complete description of all the variables.

Online Data Appendix for the Article:

"The Role of Labor Market Institutions in the Impact of Immigration on Wages and Employment"

by Mette Foged, Linea Hasager and Vasil I. Yasenov

February 25, 2021

Contents

1 Sample and Inclusion Criteria

We collected studies for the database in two steps. First, through extensive searches on Google Scholar using the phrases "labor market impact immigration", "wage impact immigration" and "employment effect immigration". Second, by reviewing citations in earlier meta-analyses, literature reviews and all the empirical studies we found in the search process. The pool of studies that we identified was then restricted according to the following inclusion criteria:

1. Immigration is measured as a share of the total population, relative to the native population or as a percentage change in the population. Hence, all wage effects are either elasticities or semi-elasticities. We do not include studies where immigration is defined in terms of diversity.

2. The study is published in a peer-reviewed journal or as a book chapter.[1]

3. All studies based on immigration to European countries are included while older North-American studies are undersampled. There is a large and mature literature on U.S. data. We wanted to sample a wide range of countries from the developed world. The resulting database includes more European and more recent research than any previous review and meta-analysis.

4. Studies based on historical immigration episodes are not included either. We focus on recent decades where data on institutions are available.

The third criteria could potentially introduce publication bias in our analysis if studies that fail to document a significant effect do not get published. However, since there is a general consensus in the literature that the impacts of immigration are small, we do not believe this to be a problem for our analysis. Indeed, our results confirm that the effect sizes are centered around zero and many are not significantly different from zero at conventional significance levels. More importantly, Longhi and coauthors investigated the issue of publication bias in a series of meta-analyses (Longhi, Nijkamp, and Poot, 2008b,a, 2005) and concluded that it is not an issue (Longhi, Nijkamp, and Poot, 2010a,b).

[1] Altonji and Card (1991), Borjas (2014) and LaLonde and Topel (1991) are book chapters.

2 Conversion of Estimates to Comparable Effect Sizes

2.1 Wage Effects

Most articles on the labor market impacts of immigration evaluate the effect on some measure of (log) wages. Immigration exposure is either measured as the *share* of immigrants in the labor force or as the *ratio* of immigrants to native workers in the labor force. We ignore this distinction. The difference is negligible as long as immigrants represent a small fraction of the population.

More importantly, some use the immigrant share (or ratio) directly and some use the *log* immigrant share. The former considers the effect of an increase in the immigrant share of one percentage point while the later considers a one percent increase in the share of immigrants.

In order to make the different wage effect estimates comparable we convert all elasticities to semi-elasticities, η_g, using the average proportion of immigrants in the labor market, m_g, in the following relationship:[2]

$$\eta_g = \frac{\partial \log w_g}{\partial m_g} = \frac{\partial \log w_g}{\partial \log m_g} \cdot \frac{1}{m_g} \tag{1}$$

Here w_g denotes the average wage in a group of workers g, and similarly m_g measures the share of immigrants in that particular group.[3] If the immigrant share m_g is not reported by group, we apply the average share of immigrants m for conversion instead. We use η_g as our dependent variable in the meta-analytic assessment of the impact of immigration on natives' wages.

Furthermore, we rescale effect sizes if they are reported for an actual percentage point change instead of a *one* percentage point change in the immigrant share. We also convert wage effect sizes using the mean wage in the local currency if the outcome is wages instead of the log of wages. We refer to Section 3.1 "Information about estimate and conversion" for a more detailed description of these conversions. Note that even after conducting these operations some differences can remain since the measure of labor income can vary between studies – some

[2]This is identical to the approach in Longhi, Nijkamp, and Poot (2005). We take the average immigrant share as reported in the relevant study to convert the estimated elasticities. For three studies we had to use external data sources (Dustmann and Glitz (2015), Aydemir and Kırdar (2017), and Barrett, Bergin, and Duffy (2006)). For the first two studies we used OECD data, and for the last article we used data from a later study by Barrett, Bergin, and Kelly (2011).

[3]Groups could be high and low skilled. If the estimate concerns an overall wage effect, then w_g is simply the average wage and m_g is the overall immigrant share.

authors use hourly wages while others study annual earnings etc. which we are not able to convert into identical measures. Whenever stated in a paper, we note at which level wages are reported. If the level of wages is not clearly stated it is marked *wxunclear*.

2.2 Employment Effects

We convert the estimated impact on employment and unemployment rates to a common metric, assuming that labor force participation effects are negligible:

$$\theta_g = \frac{\partial E_g}{\partial m_g} = \frac{\partial(1-U_g)}{\partial \log m_g} \cdot \frac{1}{m_g} = -\frac{\partial U_g}{\partial \log m_g} \cdot \frac{1}{m_g}, \tag{2}$$

where U_g denotes the unemployment rate, and E_g is the employment rate in each region or skill group. The interpretation of equation (2) is that a one percentage point increase in the immigrant share affects the native employment rate by θ_g percentage points.

A few studies consider log employment as their outcome instead of the employment rate, which means that their results can be interpreted as a percent change in employment, rather than a percentage point change in native employment in response to an influx of immigrants. We convert estimates where the outcome is log employment to θ_g using the average employment rate, E_g, in the labor market:[4]

$$\theta_g = \frac{\partial E_g}{\partial m_g} = \frac{\partial \log E_g}{\partial m_g} \cdot E_g \tag{3}$$

3 Description of Variables in Our Database

3.1 Variables Constructed from Our Sample of Studies

We provide a brief description of all variables in our database below. The variable *estimate* contains the estimated effect sizes from the primary studies and it is our main variable of interest. For studies that do not report semi-elasticities the effect size has been converted such that *estimate* denotes a semi-elasticity. The conversion is described in *factor*.

- **variable name:** Variabel label. (Values and value labels when necessary.)

[4]The factors used for this conversion are taken from each study. Not all studies report the average employment rate in main paper. In these cases we review the supplementary material of articles to find the figure. If the study does not report the average employment rate, we use OECD data instead. This is the case for three articles (Pedace, 1998; Docquier, Ozden, and Peri, 2014; D'Amuri and Peri, 2014).

- **paper_id:** Unique paper id.

Bibliographic information

- **authors:** Last name of authors.
- **year:** Publication year.
- **journal:** Name of journal/book.

Information about data

- **country:** Country/countries considered in the study.
- **data_period:** Data period in the study. For structural papers *data_period* refers to the period used to estimate underlying parameters. If the labor market impacts are simulated for specific time periods this can be seen in *other_comments*.
- **data_frequency:** Frequency of data (annual or more frequent vs. less frequent data).

Information about estimate and conversion

- **table:** Table number in article where the particular estimate is found.
- **wage:** Takes value 1 if outcome is wages (both wages and logwages).
- **logwage:** Takes value 1 if outcome is log(wage). If the wage estimate is not in logs, then the estimate is converted to a semi-elasticity using the mean wage (contained in *factor*).
- **outcome:** Outcome variable of consideration for estimate. For wages, outcome can be: w (hourly), wxday (daily), wxweek (weekly), wxmonth(monthly), wxquarter (quarterly), wxyear (yearly), wxunclear (unclear). For employment estimates, outcome can be: e01 (employment rate), u01 (unemployment rate), p (labor force participation), e (other measures, typically fraction of time period worked).
- **logoutcome:** Takes value 1 if outcome is log.
- **estimate:** Impact estimate from study converted to comparable metric.
- **se:** Standard error of estimate if reported.
- **t:** T-value of estimate if reported.
- **share:** Takes value 1 if independent variable is the share of immigrants.

- **log_share:** Takes value 1 if independent variable is log(share of immigrants). If *log_share*=1, the estimate is divided by *mean_foreign_share* to convert to semi-elasticity.
- **ratio:** Takes value 1 if independent variable is the ratio of immigrants to natives.
- **mean_foreign_share:** Contains the mean foreign share reported in the study or approximated using external data sources, in this case see *other_comments*. If *log_share*=1, the estimate is divided by *mean_foreign_share* to convert to semi-elasticity.
- **empl:** Takes value 1 if the outcome is employment (employment, log(employment) or unemployment). If the outcome is unemployment, the estimate is multiplied by -1. If the outcome is log(employment) then the estimate is converted using the mean employment rate (contained in *factor*).
- **factor:** Is always multiplied by estimate and standard error to scale estimates and their standard errors to comparable metrics. Thus multiplying estimate by $\frac{1}{factor}$ yields the original estimate reported in the study (if *log_share*=1, then estimates and standard errors must also be multiplied by *mean_foreign_share* to reach the original number reported in the study). If the labor market impact is not reported for a 1 percentage point increase in the share of immigrants, *factor* scales the estimate to a 1 percentage point increase in the share of immigrants (this can be the case for structural papers where impacts are typically simulated for an increase in the share of immigrants greater than 1 percentage point). If the wage impact is not in percentage terms (*logwage* $\neq 1$), then *factor* contains the mean wage in the labor market. Finally, *factor* also contains the mean employment rate if the outcome is log(employment).
- **type_of_immigrant:** Indicates which type of immigrant is considered in the study if applicable.
- **refugees:** Takes value 1 if the immigrants are mainly refugees.

Information about empirical strategy

- **variation_org:** Level of variation in the variable of interest (rt: area, gat: skill, grt: mixture, jt: firm, ot: occupation, i: industry, r: region/area. t: time. g: education/skill group. a: age/experience. j: firm. o: occupation. i: industry. k: gender).
- **variation:** Level of variation in the variable of interest aggregated into area, skill or mixture.

- **naturalexperiment:** Takes value 1 if the study is a natural experiment.
- **method:** Estimation method (OLS, IV, FD, DID, FDIV or DIDIV, where DID include generalized FE versions).
- **IV:** Describes instrument (bartik_type, lagged_inflow[5], other). bartik_type is noted when estimated based on variation of classical shift-share. If other, description of IV is in other_comments.
- **preferred:** Takes value 1 if the authors *explicitly* state that the estimate/specification is their preferred.
- **preferred_spec:** Preferred specification as stated by authors directly or indirectly. There can be multiple if an article provides estimates for both wages and employment or for several countries. Estimates where all skill groups are pooled together are prioritized. If not available then low skilled are chosen. Estimates for men are preferred if not full population. For wages the smallest unit are the preferred measure (hourly $\succ$ daily $\succ$ weekly $\succ$ monthly $\succ$ annual earnings). Employment estimates are preferred to unemployment estimates. Short run estimates are preferred to long run.
- **other_comments:** Other comments.
- **variation_mix:** The type of variation (education experience, education age, education, industry, occupation) used in combination with geographical variation if effect size is estimated using the mixture-approach (*variation=Mixture*).

Information about analysis subjects (natives)

- **skill:** Skill type of natives considered in the study (high, low or all). See *skill_comments* for elaboration.
- **skill_comments:** Comments on natives' skills.
- **gender:** Gender of natives (1=males, 2=females, 3=both).
- **eu:** Takes value 1 if the country considered in the study is in Europe.
- **scandinavia:** Takes value 1 if the country considered in the study is in Scandinavia.
- **restofeu:** Takes value 1 if the country considered in the study is in Europe, but not in Scandinavia.

[5]Potentially also including the lagged variable squared in line with Altonji and Card (1991).

- **northamerica:** Takes value 1 if the country considered in the study is in North America.
- **region:** Indicates region of country considered in the study (Europe, North America or rest of the world).
- **firstyear:** First year of *data_period*.

3.2 RePEc Variables

A set of variables is extracted from RePEc on February 2, 2020 in order to rank studies according to the impact factor of the journal in which the study was published. The impact factors and journal rankings are merged to our database of effect sizes by journal name such that each effect size can be weighted by its impact factor. Studies that are not published in a peer-reviewed journal are given a rank corresponding to the lowest ranking study in our sample.

- **journal_ranking:** Ranking of journal determined by the score described below. If the study is not published in a peer-reviewed journal the study is given the same ranking as the lowest ranked peer-reviewed study in our sample.
- **Score:** This ranking aggregates the individual ranking methods by taking the harmonic mean of the invidual rankings (plus one), leaving aside the best and the worst ranking. Citation counts are adjusted to exclude citations from the same series. Only series or journals with 50 or more items are ranked.
- **SimpleIF:** The simple impact factor is the ratio of the number of citations by the number of items in the series. Citation counts are adjusted to exclude citations from the same series. Only series or journals with 50 or more items are ranked.
- **RecursiveIF:** The recursive impact factor weighs each citation by the impact factor of the citing items, this impact factor being itself computed recursively in the same fashion. The recursive impact factors are normalized so that the average citations has a weight of 1. Citation counts are adjusted to exclude citations from the same series. Only series or journals with 50 or more items are ranked.
- **DiscountedIF:** Discounted impact factor wherein each citation is divided by its age in years (one for the current year). Thus, in 2007, a citation from an article in 2004 counts for 0.25. Citation counts are adjusted to exclude citations from the same series. Only series or journals with 50 or more items are ranked.

- **RecursiveDiscountedIF:** Recursive discounted impact factor wherein each citation is divided by its age in years (one for the current year). Thus, in 2007, a citation from an article in 2004 counts for 0.25. Each citation is also weighted by this same impact factor recursively. Citation counts are adjusted to exclude citations from the same series. Only series or journals with 50 or more items are ranked.

3.3 Institutional Variables

The variables measuring institutional features are extracted from OECD's database (OECD, 2018).[6] We merge OECD data to our sample of estimates by calculating averages of the institutions over the full time period. We present a short description of each institutional variable below.

3.3.1 Employment Protection Legislation

- **EPL (*StrictEmplnd*):** Index of strictness of employment protection legislation (EPL) for individual dismissals. Measured for workers on regular contracts. This OECD indicator of employment protection legislation measures the procedures and costs involved in dismissing individual workers. Higher values correspond to a larger degree of strictness and therefore higher employment security/rigidity among incumbent workers. The data-series is extracted from the OECD database and covers the period 1985-2013 (version 1).
- **EPL collective (*StrictEmpColl*):** Index of strictness of employment protection legislation (EPL) for collective dismissals. This OECD indicator of employment protection legislation measures the additional procedures and costs involved in dismissing groups of workers (compared to the cost of individual dismissal). The data-series is extracted from the OECD database and covers the period 1998-2013.

3.3.2 Wage Rigidities and the Structure of Income

- **Collective bargaining coverage (*CollBarg*):** Share of employees covered by collective bargaining agreements. Collective bargaining coverage rate corresponds to the ratio of employees covered by collective agreements, divided by all wage earners with right to

[6] Average Tenure for the U.S. is taken from CPS (2018).

bargaining. The data-series is extracted from the OECD database and covers the period 1960-2016.

- **Net replacement rate (*NetReplacementRate*):** Net compensation rates (share) in the initial phase of unemployment for two-earner married couples with two children and earnings corresponding to 67 pct. of average wages (using pre-unemployment wage for the unemployed spouse) where the family does not qualify for cash housing assistance or social assistance "top-ups". The data-series is extracted from the OECD database and covers the period 2001-2015.

- **Trade union density (*TradeUnionDens*):** Share of employees that are members of trade unions. The OECD database contains information from administrative data on the number of employees that are members of a trade union, the number of employees and union density defined as the ratio of union members divided by the total number of employees. The data-series is extracted from the OECD database and covers the period 1960-2016.

3.3.3 Other Indicators of Institutions

- **Average tenure (*AverageTenure*):** Average tenure in current job measured in years. Job tenure is measured by the length of time workers have been in their current or main job or with their current employer and are expressed in numbers of years. This information is valuable for estimating the degree of fluidity in the labor market and in identifying the areas of economic activity where the turnover of labor is rapid or otherwise. Higher values correspond to lower labor mobility. The data-series is extracted from the OECD database and covers the period 1992-2016. Note that the data on average tenure for the U.S. is from Current Population Survey (CPS) (2018) and covers the following years: 1983, 1987, 1996, 1998, 2000, 2002, 2004, 2006, 2008, 2010, and 2012, and the sample used to calculate it is all people age 16-64 in the labor force.

3.4 Macroeconomic Conditions

- **GDP growth (*GDPgrowth*):** Annual GDP growth rates in percentages. GDP is calculated using the expenditure approach. The data-series is extracted from the OECD database and covers the period 1950-2017.

- **Unemployment rate (*Unemployment*):** The unemployment rate in percentages for the

total labor force (ages 15-64). The data-series is extracted from the OECD database and covers the period 1960-2016.

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