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The Role of Institutions in the Labor Market Impact of Immigration*

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Abstract

We study the role of labor market institutions and policies in affecting the wage impact of immigration using a cross-country meta-analysis approach. We gather information on 1,548 previously reported semi-elasticities from 66 academic studies covering 20 developed countries. We supplement this dataset with country-level institutional structure and coverage data from the OECD. These include employment and wage rigidities, labor mobility, active labor market programs spending, and product market regulation. We relate estimated wage effects and institutional coverage while controlling for local economic conditions, immigrant skill composition, time and region fixed effects and study characteristics. Higher labor market rigidity, as brought about by more widespread institutions, regulations and policies, mitigates the effects on relative wages of high- versus low-skilled natives but exacerbates the impacts on average earnings. Overall, our results suggest that labor market institutions and policies may be effective tools in the economic absorption of foreign workers.

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

Keywords: immigration, wages, 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, their origins and consequences have been intense objects of analysis by economists for more than a century (e.g., Moore, 1911). More recently, labor economists have focused on studying the interplay between these structures and other economic phenomena. Examples include the interaction of institutions and policies with the effect of minimum wage (Neumark and Wascher, 2004), the returns to education (Webber, 2014), income inequality (Farber et al., 2018) and wage determination (Nunziata, 2005) where researchers have documented significant synergies.

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 native born from workplace competition with foreign labor. Specifically, policy-induced labor market imperfections and rigidities may serve as a preventive factor for wage and employment adjustments. In fact, demand for such protection policies may endogenously arise from public pressure towards politicians due to increased migration levels (Rodrik, 1997). Following a supply shock, well-designed policies would provide a smooth transition back into market equilibrium, mitigating any potentially negative impacts caused by foreign workers.

While a large body of literature analyzes the wage and employment consequences of immigration, we know little about their interaction with labor market policies and institutions. The classic theoretical underpinnings of the literature rest on assuming perfectly competitive markets, entirely ignoring any such synergies (e.g., Borjas, 2013). This interplay is of particular relevance because policies and institutions may aid in absorbing foreign workers whilst minimally disrupting the labor market. The substantial returns to improved policies are highlighted by few recent polls suggesting immigration is the top issue among election voters in Europe and the U.S. (European Commission, 2018; Reuters, 2018).

Two major challenges stand in the way of measuring the role of institutions on the wage impact of foreign-born workers. First, institutions are generally set on a national level and do not vary within countries, so a thorough analysis requires a cross-country perspective. Labor markets across the developed world vary greatly in terms of mobility, rigidity, and institutional scope and intensity providing a useful laboratory to study how labor market institutions and policies affect the consequences and absorption of immigrants. Second, gathering comparable micro data across a wide set of countries and time periods is a virtually unattainable task. We circumvent both issues by directly relating estimated cross-country

immigration wage effects and institution scope, enabling us to carefully isolate their relationship. Along the way we show economic theory is compatible with a wide range of scenarios, motivating the empirical assessment of these interactions.

We study how labor market institutions and policies affect the impact of immigration on natives' wages utilizing a cross-country meta-analytic approach. To accomplish this, we gather a novel database covering 1,548 estimates on the wage impacts of foreign workers from 66 published academic articles spanning 20 countries in the developed world and several decades. We supplement this database with Organization for Economic Co-operation and Development (OECD) data on country-specific labor market institutions including employment and wage rigidities, active labor market policies, labor immobility and market entry regulations. We distinguish between average and relative wage effects because (i) they have distinct implications for the wage structure, inequality and workers' welfare more broadly (see [Dustmann, Schönberg, and Stuhler, 2016](#)) and (ii) there is a substantial difference in the two distributions of reported effects as we show in this paper. We then use a linear regression framework to relate the estimated wage effects and institutional coverage while controlling for an array of related characteristics. This type of meta-analysis is a research synthesis that attempts to extract useful conclusions from a body of diverse academic literature (see e.g., [Card and Krueger, 1995](#); [Longhi, Nijkamp, and Poot, 2005](#); [Card, Kluve, and Weber, 2018](#)). Importantly, we include region and decade fixed effects accounting for time-invariant differences across areas and aggregate shocks, country-specific economic conditions (as proxied by GDP growth and unemployment rate) and relative immigrant skill composition. Lastly, to eliminate potential concerns about our selection of estimates and studies we control for study characteristics and use various weighting schemes as robustness checks. Additionally, we can rule out that country-specific time-varying factors such as endogenous responses of institutions to immigration flows are driving our results.

Our main finding is that higher labor market rigidity, as brought about by more widespread institution coverage, has a positive impact on the relative wage effects of immigration on low- versus high-skilled¹ domestic workers and a weaker negative effect on the consequences on average earnings. Labor market policies are effective in dampening the extent to which foreign workers distort earnings differentials between high- and low-skilled natives while exacerbating any effect on average earnings. Intuitively, any policy meant to protect incumbent workers will also diminish potential benefits induced by the

¹We use the terms "low-" and "high-skilled workers" broadly. These could refer to employed individuals across various education/experience groups, occupations or even industries and the exact definitions vary across studies. For instance, the traditional skill-cell approach utilizes a constant elasticity of substitution heterogeneous labor framework and nests experience types within education groups (e.g., [Borjas, 2003](#)). The relative wage effect in this model identify earnings distortions across experience groups within education types. See [Dustmann, Schönberg, and Stuhler, 2016](#) for more details.

newcomers. These may include complementarities in the production process (Ottaviano and Peri, 2012), productivity spillovers (Hunt, 2017) and skill or occupational upgrading (Foged and Peri, 2015), etc. The findings suggest that the significant variation in institutional coverage across countries may, at least partially, drive the substantial diversity of reported elasticities in the literature.

We advance the literature on the labor market effects of immigration by building on existing studies and providing novel insights on the role of institutions in mediating the competition between native and foreign workers. In doing so, this paper presents the first comprehensive analysis of the synergies between a wide range of labor market policies and the wage consequences of immigrants, filling in an important gap in an otherwise large and mature body of literature. Consistent with the previous evidence, we find that labor market rigidity exacerbates the effect of immigrants on the average wage earner. Extending this analysis, we are the first to show that institutions and policies are helpful in mitigating immigration-induced wage distortions among various native skill types. Although several important 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. Because the influence of institutions on relative wage effects are more relevant empirically and because institutions have a stronger effect on relative wages, our novel finding suggests that labor market institutions may be an effective tool in smoothing the economic absorption of foreign workers.

Crucial for our analysis is the reliance on previous studies in the literature. In a series of influential meta-analyses on the labor market effects of immigration, Longhi, Nijkamp, and Poot (2010a; 2008b; 2008a; 2005) show that the estimated elasticities in the U.S. 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 analyze this hypothesis in detail. Perhaps 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. Reduced market flexibility (as measured by higher firing costs, replacement rates and barriers to entry) is associated with increased employment losses brought by foreign workers while wage consequences are not analyzed. Next, Brücker et al. (2014) use a highly structural wage-setting approach to study the immigration earnings effects in three European countries - Denmark, Germany and the U.K. 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 (2017) document an interesting interaction whereby a lower minimum wage exacerbates the immigration-induced earnings and employment consequences in the U.S.

A common theme among these studies is the significant role of institutional structure in altering the

competition forces between incumbent workers and newcomers. Our paper builds on and extends this body of work, distinguishing between average and relative wage effects of immigration, analyzing a wide variety of labor market institution types simultaneously and employing a strictly data-driven approach. Consistent with these earlier studies, we show that reduced labor market flexibility is associated with higher average wage losses due to immigration. In addition and novel to the literature, we go one step further to document that labor market rigidities mitigate relative wage distortions between low- and high-skilled natives. For recent reviews of the broader literature of the labor market consequences of immigration on domestic economies see [NAS \(2017\)](#), [Dustmann, Schönberg, and Stuhler \(2016\)](#), and [Lewis and Peri \(2015\)](#).

We continue in Section [2](#) where we describe and summarize the data we use. Next, in Section [3](#) we briefly discuss how the institutional variables we analyze fit in the theoretical framework in the literature on the labor market effects of immigration. Section [4](#) lays out our empirical strategy and the equation we estimate. Lastly, we present our results in Section [5](#) and conclude in Section [6](#).

2 Data

We combine two data sources – a database on the impacts of foreign workers and information on institutional coverage from the Organization for Economic Co-operation and Development (OECD). We now describe and summarize each one in turn.

2.1 Wage Impacts of Immigration

2.1.1 Data Sources and Sample

Building a database on the labor market effects of immigration requires selecting study inclusion criteria based on a few key priorities. With the goal of studying institutions, we focused on sampling studies analyzing a wide variety of labor market settings across the developed world. This included both geographic and temporal variation. Moreover, to ensure study (and therefore data) quality, we relied on high quality peer-reviewed publications. Lastly, we concentrated exclusively on estimates of the wage effects of immigration as this has been the main focus in the literature.^{[2](#)} Appendix Section [A.1](#) describes

²Analyzing employment effects estimates is more problematic for a few reasons. First, there is large discrepancy in the outcome variable across studies - some use employment and some unemployment; some use (un)employment to population ratios and some do (un)employment rates. A comprehensive analysis will therefore require an implicit assumption on the effect of immigration on labor force participation, which we are not comfortable making. Second, there are far fewer migration-induced employment impact estimates in the literature, introducing severe statistical power difficulties. To this end we point interested readers to [Angrist and Kugler \(2003\)](#) who analyze the interplay between labor market institutions and migration-induced employment losses in the European Economic Area but did not study wage effects.

in greater detail the paper sampling process along with the inclusion criteria and Table A.1 lists the included studies.

Our database consists of 1,548 estimated wage effects of immigration from 66 studies published between 1982 and 2018 spanning 15 European countries, the United States, Canada, Turkey, Israel and Australia. Additionally, when available, we recorded data on the corresponding standard error and t -statistic associated with the null hypothesis of the true effect equals zero, regression specification, sample (e.g., gender, education level), empirical approach (area, skill cell or mixture)³ publication outlet and estimation method (OLS, IV or simulation). To gauge study quality, we included journal impact factors measured by IDEAS/RePEc (2018), a widely used index.

An important feature of our subsequent analysis is the distinction between effects on the relative versus average wages. We make this choice due to (i) the considerable difference between the two and (ii) the significant disparity in reported semi-elasticities along this dimension (more on this below; see Table I and Figure A.1). The latter presents immigration-induced total changes in the average labor market wage. The former one instead identifies relative wage effects, typically across experience or education groups while controlling for the overall impact. Hence, they capture a partial effect on wage differentials and are generally linked to changes in the earnings structure and wage inequality. These relative effects do not necessarily result in changes in total average earnings as a negative wage effect on one group may be offset by a positive one for other groups.

We follow Dustmann, Schönberg, and Stuhler (2016) in classifying effect sizes into estimating average (the spatial approach) or relative wage changes (the national skill-cell and the mixture approaches). Our broad definition of the skill-cell approach includes studies utilizing variation in immigration exposure across occupations (e.g., Friedberg, 2001), industries (e.g., Bratsberg and Raaum, 2012), or the classical education-experience cells (e.g., Borjas, 2003).⁴ We categorize combinations of those with geographic areas as the mixture approach (e.g., Card, 2001). Lastly, studies using only geographic variation in immigration intensity are flagged as estimating effects on average wages (Altonji and Card, 1991). See Dustmann, Schönberg, and Stuhler (2016) for more details on each empirical approach and the parameters they identify.

Differences in regression specifications across studies require a few adjustments to ensure the re-

³This is a common studies categorization in the literature. See Dustmann, Schönberg, and Stuhler (2016) or any review paper for a detailed description of each one.

⁴Our broad definition of skill cells doubles the number of relative effect sizes available (from 589 to 1,092) compared to a narrow skill cell definition excluding industry variation (as well as two papers using firm level variation Malchow-Møller, Munch, and Skaksen, 2012; Malchow-Møller et al., 2013). Qualitatively, our results survive in the smaller sample; the coefficients have the same sign and are roughly of the same magnitude but some lose significance.

ported estimates are measured in comparable units. First, some researchers publish elasticities while others estimate semi-elasticities. The distinction is subtle but important. The former measure the percentage change in natives' wages induced by a percent increase in immigration 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 metric in the literature. Second, the wage effects are sometimes reported for the actual growth in immigration. This is often done in structural models where the simulation is carried out for the actual change in the immigrant share in the analysis period. In that case, we simply rescale the parameter to reflect a one percentage point increase in the proportion of foreign-born. Hence, our estimates are interpreted as the percentage change in wages for one percentage point increase in immigrants as a share of the labor force. 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 effort in assuring comparability across studies, an inherent limitation of the meta-analysis approach is the inevitable differences across samples and regression specifications for which we cannot account. For instance, included control variables or fixed effects across studies are rarely exactly identical. The sample definitions vary as well – e.g., 16-65 versus 21-54 years old, working age population versus labor force, etc. We have no reason to believe these remaining differences may systematically bias our results in a certain direction. Furthermore, it is reassuring that our results are robust to using various weighting schemes, subsamples and control variables. Hence, they seem to reflect fundamental patterns in the data rather than being an artifact of a few study characteristics or estimates.

A key concern for every meta-analysis study is the extent to which the sample of estimates is representative of the entire population. It is well-known that, all else equal, studies with statistically significant estimates are more likely to be published in academic outlets. Unfortunately, this fact introduces an inherent bias towards more significant estimates even in a random sample of published studies. However, under the assumption that authors choose the significant results as their preferred specification, all other reported semi-elasticities in their papers should be less prone to publication bias. We mitigate publication bias concerns to some degree by including all reported elasticities.

To directly test whether such bias may be present in our sample, we follow the standard practice in the literature (e.g., Card and Krueger, 1995; Longhi, Nijkamp, and Poot, 2005; Stanley and Doucouliagos, 2014) and run a regression of effect sizes on the standard error and other characteristics. If publication bias is not an issue, the coefficient on the standard error variable should be statistically in-

distinguishable from zero. The results are presented in Appendix Table [A.2](#), using a different weighting scheme in each column. The weights are chosen to mirror our specifications in the main analysis and are described in Section [4.2](#). In Panel A (B) we present the regression results on our subsample of relative (average) wage effects. We find no conclusive evidence on publication bias in the sample of average effect sizes (Panel B). Some specifications show a positive and some show a negative association between estimated standard errors and effect sizes but this seems to be driven by a few studies with many noisy estimates. The association disappears if we either weight studies equally, or assign higher weight to more precise estimates, better published studies or a combination (our preferred specification). We find some evidence that very noisy relative effects are more likely to be published if the estimate is negative (Panel A). However, this potential issue disappears once we weight by precision, accounting for noise in the study.

Overall, we find no evidence of publication bias in our preferred specification while the results for other specifications are mixed. Large outliers can skew the regression coefficients but our weighting scheme accounting for noise mitigates their influence. Further indication that such bias is not large in our sample comes from the apparent symmetry around zero of the histograms in Figures [A.1](#) described below. Lastly, note that for the purpose of this study, publication bias, even if present, is only problematic if it correlates with the institutional variables we describe below. We have no particular reason to believe this is the case. We now move on to summarizing our sample of effect sizes.

2.1.2 Summary Statistics

Table [1](#) presents some summary statistics, splitting the sample into relative (Panel A) and average (Panel B) wage impact estimates. Each column shows a different statistic denoted in the header. The first row shows information on our entire sample of 1,548 estimates, divided into 1,092 estimated relative wage impacts and 456 estimated average wage impacts.

The estimated impacts on average wages are small but positive and the effects on relative wages are slightly larger in magnitude and always negative. This pattern holds for both the mean and the median and across alternative weighting schemes (see Appendix Table [A.3](#)). A one percentage point increase in the share of foreign workers, on average, lowers the wage of unskilled workers by 0.28 percent relative to the wage of skilled workers, *ceteris paribus* (Panel A). The same influx is associated with a 0.15 percent increase in the average labor market wages (Panel B). In other words, foreign workers exert much stronger pressure on the relative earnings between skill groups than on the average native. This is an interesting finding which has not been thoroughly documented in previous summaries of the literature.

It is indeed the prediction of the canonical heterogeneous labor model in which immigration-induced distortions in relative skill supplies change the relative earnings differential while the overall effect is close to null. This result further motivates our distinct treatment of the two types of semi-elasticities in the regression analysis. The pooled mean (median) in our sample (not shown here) of previously estimated semi-elasticities is -0.15 (-0.03) which is very close to the -0.12 (-0.04) reported in Longhi, Nijkamp, and Poot (2005) based on a smaller sample of 345 effect sizes from 18 papers. In Appendix Figure A.1 we present densities for relative (black dashed border line) and average (light gray solid fill) wage elasticities, respectively. The entire distribution of relative effects is shifted to the left compared to the distribution of average effects.

We continue by discussing the distribution of effect sizes across study characteristics after which we move on to comment on the magnitude. The next four rows break down the sample by region. More than half of our estimates (921 from 38 distinct papers) are from European countries and a third are from North America (559 from 23 articles). In the following rows we split the semi-elasticities by estimation method. 766 of them are estimated via ordinary least squares (OLS) and 408 via an instrumental variable (IV) strategy. The latter are designed to correct for the inherent endogeneity stemming from the non-random location choices of foreign-born workers. 382 semi-elasticities are derived from simulating structural labor demand models.⁵ Next, we summarize the effects on natives by skill group. We have 885 estimated semi-elasticities for the entire population and 315 (356) for high-(low-)skilled native workers only.⁶ In the next two rows we divide our sample into estimates of short- versus long-run immigration effects as proxied by the frequency of the underlying data (annual or more frequent versus less frequent, such as decennial). Lastly, we show the distribution of estimates by publication status – Top 50 outlets as measured by RePEc’s database versus the rest of the sample. 639 of our estimates come from these elite peer-reviewed journals and 917 are from other high-quality outlets.

We find several robust patterns worth noting. Relative wage effects follow similar patterns to the estimates reported in Longhi, Nijkamp, and Poot (2010a, 2005) while average wage effects seem to move in the opposite direction in a number of dimensions. First, Longhi, Nijkamp, and Poot find that the estimated semi-elasticities are smaller in the U.S. than in Europe. We find the same for relative wage effects but the opposite pattern for average wage effects such that the largest effect on wage levels is found in North America. Given the disparities in labor market institutions between the two regions, this

⁵Even though some underlying parameters, such as the elasticity of substitution between skill types, are still estimated via OLS or IV we prefer to keep these as a separate category.

⁶The definitions of these skill groups may certainly differ across studies. Moreover, if reported, we categorize any estimates on the impact on medium-skilled natives as effects on high-skilled.

is a first direct hint that labor market policies and features may play a role in affecting these estimates.

Second, IV estimates of relative wage impacts (-0.80) are more negative than OLS ones (-0.15), possibly reflecting corrected endogeneity stemming from immigrants' location choices in the direction we expect. Namely, that they locate in booming labor markets with higher wages. Third, consistent with Longhi, Nijkamp, and Poot (2005) we find that impacts on low-skilled natives (-0.61) are more adverse than on their high skilled counterparts (0.50). A possible explanation is stronger competition for jobs in the bottom of the wage distribution and spillover effects induced by innovation and patents, relevant for high skill natives. However, average wages increase more for the low skilled.

In addition to Longhi and coauthors' findings we document three new patterns in the full sample of wage effect sizes. The average wage effect is larger in the long run (0.30) than in the short run (0.08) and the distributional effects are slightly smaller in magnitude in the long run (-0.27) than in the short run (-0.28) as measured by yearly or more frequent data. This pattern is expected as adjustment mechanisms such as capital accumulation and technology adoption will not be present in the short run but will likely play a role in longer time frames. Next, the median relative wage impact estimates are generally smaller in magnitude than the means, indicating the presence of negative outliers skewing the overall distribution. Lastly, estimates in top publications are generally larger in magnitude than those published in journals outside top 50.

2.2 Labor Market Institutions Coverage

2.2.1 Data Source

We collected country-level information on institutional structure and strength from OECD (2018). This data covers all members of the OECD which are a destination to the vast majority of economic immigrants across the globe (Peri, 2016). We selected several variables with extensive coverage reflecting a wide range of institutions related to the labor market. We then merged these data to our sample of estimates by the first year of study sample. If the institution variable was missing for this year, we allowed a gap of up to five years.⁷ With this procedure we merged 91 percent of our semi-elasticity estimates to at least one of the five institutional variables shown in Table 2 leaving us with a sample of between 628 and 1,323 effect sizes depending on the OECD variable considered. More details and information on the OECD variables, the merge rate with our sample of estimates, and the entire database are available

⁷For instance, if a paper used data from the 1980 and 1990 U.S. Censuses we assigned the U.S. institution variable value as measured in 1980. However, if it was missing we assigned the respective values from 1975-1985. If no data was available in this time period then the final entry was coded as missing. A few research articles provide estimates for different sub-periods within their sample in which case we used the first year in each of the subsamples.

in the Online Data Appendix.

2.2.2 Variable Description and Summary Statistics

We include data on five distinct institutional variables which we list and describe in detail below. Table 2 presents the coverage and summary statistics for each one. Each row denotes a separate country and each Panel designates a distinct institutional variable. Note that the sample size here refers to the number of merged country-year observations, not semi-elasticities. The latter is larger since each country-year observation can be matched to more than one semi-elasticity.

Employment Rigidity (StrictEmpInd): We use a strictness of employment protection legislation index which evaluates the procedures and costs involved in dismissing individual workers on regular contracts. Higher values correspond to a larger degree of strictness and therefore higher employment security/rigidity among incumbent workers. Panel A of Table 2 shows summary statistics on this variable. The data covers most of the countries in our sample with rich variation across and little to no variation within each one (as denoted in the S.d. column). Unsurprisingly, the U.S. and the U.K. have the most flexibility (0.26, and 1.16 respectively), while employees are most protected in Portugal (5.00), the Netherlands (2.84), Sweden and Greece (2.80). We are able to link 47 unique country-year level observations to our database of wage impacts.⁸

Wage Rigidity (CollBarg): We measure wage rigidity by the collective bargaining coverage rate - the ratio of the number of employees covered by collective agreements, divided by all wage earners with right to bargaining. As such, it is a direct measure of wage rigidity in the labor market commonly used in the literature (e.g., Avouyi-Dovi, Fougère, and Gautier, 2013). Panel B presents some summary statistics on this variable. European countries are covered at much higher rates (e.g., 98% in Austria and 91% in Sweden) than the U.S. and Canada (24% and 34% respectively) and the average is 62%. We acknowledge the heterogeneity of collective bargaining agreements across countries or even within countries across industries which, to some degree, may be a limitation of our analysis. Nevertheless, we believe our results will speak about an overall effect of wage rigidities which is still informative and relevant and better captured by this variable. The match rate is slightly improved relative to the previous Panel as we link wage impact data to 53 unique country-year observations of the wage rigidity indicator.⁹

⁸Alternative measures of employment protection are available in the OECD data. In a robustness checks we use a variable measuring employment protection for individual workers on temporary contracts (StrictEmpTemp). Two other variables measuring individual and collective dismissals are not used due to much lower coverage and weaker similarity with our main measure of employment rigidity.

⁹An alternative and related measure of wage rigidity is trade union density. We prefer collective bargaining coverage be-

Labor Immobility (AverageTenure): Our worker (im)mobility measure is the average job tenure as indicated by the number of years workers have been in their current or main job or with their current employer.¹⁰ Hence higher values correspond to lower labor mobility. Panel C shows the coverage rate, mean and standard deviation. The average workplace tenure is slightly above 9 years. European labor markets are more rigid and with the longest average stay in Southern European countries (Greece 12.68 years, Italy 11.85 years) while in the U.S. it is only 7.42 years. Lastly, we have information on labor immobility for 30 distinct country-year pairs that we combined with our database on wage effects of immigration.

Active Labor Market Policies (ALMPExp): This measure is defined as the public expenditure on ALMPs as percent of GDP. These may include public employment services (e.g., job finding assistance), training programs (e.g., courses and apprenticeships), short-term employment subsidies, etc. Panel D shows that, on average, countries spend 0.77% of GDP on ALMPs. This number varies widely across countries – it is as low as 0.20% in the U.S. and Portugal and as high as 1.51% in Scandinavian countries such as Denmark and Sweden. We link 43 unique country-year observations of this variable to wage effect estimates. Once again, we observe little variation of ALMPs expenditure within countries.

Business Regulation (StateControl): This index measures the degree to which business enterprises are covered by formal regulations that inhibit or promote competition in the product market. It differs from the aforementioned institutions and rigidities measures as it affects the labor market indirectly through the regulations in the product market. Table 2 shows the index takes on lowest value in the U.K. (1.42) and highest in Greece (4.24) displaying wide disparities across states. This is our institutional variable with poorest coverage – we merge only 13 unique country-year observations to our database.¹¹

Overall, Table 2 asserts the notion that policies and institutions are generally determined at the national level which motivated our cross-country perspective. Unsurprisingly, we see that labor markets in North America and the U.K. are characterized by lower rigidities and weaker incumbent protection as compared to European countries. In addition, we also use information on country-level GDP growth, unemployment rates and the relative skill distribution of immigrants and natives from the OECD and study characteristics such as estimation method and data frequency which we include as control variables.

cause it better captures the actual share of the labor force covered by collective agreements. Trade union density (membership) is misleadingly low for some countries with high wage rigidity - e.g., 98 percent of workers in France are covered by collective agreements but less than 10 percent are members of a union.

¹⁰The data source for the U.S. is the Job Tenure Supplement of the Current Population Survey (CPS) and the sample used to calculate it is all people age 16-64 in the labor force.

¹¹The alternative measures at our disposal have the exact same coverage and measure different barriers to entrepreneurship (BTE) and barriers to trade and investment (BTIT). We include them in our robustness checks.

3 Theoretical Considerations

Before proceeding to the empirical section, it is useful to briefly discuss the ways these labor market institutions play out in the canonical model of labor supply and labor demand used to study the impacts of immigration on native workers. The main theoretical apparatus in the literature is a neoclassical model, featuring a perfectly competitive economy with no institutional frictions or remedies. On the demand side, capital and heterogeneous labor, aggregated in a constant elasticity of substitution framework, are combined to produce a single good with constant returns to scale. In equilibrium all markets clear. The model mechanically predicts relative wage changes mirroring relative supply distortions induced by foreign workers. For instance, an influx of low-skilled immigrants makes low-skilled labor relatively more abundant, decreasing its productivity and therefore price relative to high-skilled natives.

Average wages depend only on the elasticity of capital supply.¹² Fixed capital drives down labor's total productivity as immigrants come in and average wages decline while they remain unchanged if capital is fully flexible. Elastic capital supply is probably the most accurate assumption even for very short time horizons because immigration is generally a slow process where firms have time to adjust in response. This argument is especially relevant in open economies where investments can come in from abroad whenever there is an excess return to be earned. Table 1 shows that average wage effects are slightly positive even in the short run (annual or more frequent data) and higher in the long run (less frequent data). Positive total effects could reflect a more efficient allocation of workers to tasks, skill upgrading, productivity spillovers and demand-side effects.¹³

Contrary to the neoclassical model, real world labor markets do not always clear. On the one hand, this could be due to distortionary institutions such as minimum wages and employment protection.¹⁴ On the other hand, well-designed policies could reduce market imperfections such as search frictions and imperfect information. For instance, ALMPs such as job centers and training programs for unemployed are indeed meant to improve matching between workers and firms and help unemployed workers upgrade their skills and redirect their career towards firms and industries with better employment prospects.

Our institutional variables (*Employment Rigidity*, *Wage Rigidity*, *Labor Immobility*, *ALMP spending*, and *Business Regulation*) are defined such that higher values indicate stronger institutions and lower

¹²Capital is assumed to be skill neutral and therefore does not affect relative wages. See Lewis (2013) for a discussion of models with capital-skill complementarity.

¹³These adjustments are beyond the canonical model where each worker type is preassigned to one task, has a predetermined skill level, has no effect on the productivity of other workers beyond simple complementarities and where immigrants are workers (not consumers).

¹⁴Angrist and Kugler (2003) set up a model in which firing costs protect incumbent workers in the short run but decrease equilibrium employment.

occupational mobility likely deteriorating the total effect of immigration on native wages through the distortionary effects of rigidities and public spending. This is especially the case if immigrants are not covered by the institutional protections, making them more attractive to firms relative to incumbent workers¹⁵

Several problems arise in attempting to generalize the impact of labor market institutions on the relative wage effects of foreign workers. First, especially because institutional coverage and strength likely vary across skill groups, we need a model where workers respond differently to wage changes. Heterogeneous labor supply elasticities imply that relative wage and relative employment effects can be of opposite signs. Moreover, the relative wage of low-skilled natives could even increase in response to low-skilled immigration if the former is sufficiently elastic, a phenomenon termed “perverse effects” by [Dustmann, Schönberg, and Stuhler \(2016\)](#). Hence, the mere effect of institutions on (heterogeneous) workers’ labor supply makes it impossible to sign the interplay between institutions and relative wages in the economy. Second, institutions likely influence the elasticity of labor demand in heterogeneous ways across workers also due to varying coverage and strength. Third, many policies and regulations have different implications for the short and the long run (as in [Angrist and Kugler, 2003](#)). Institutions in the form of various rigidities and high costs of job mobility likely prolong transitions and convergence to new equilibrium levels.¹⁶ Empirically it is usually not possible to separate short and long run outcomes because immigration is generally a slow and gradual process.

To sum up, the main workhorse model used by most researchers assumes perfectly competitive markets and ignores the role of labor market institutions. They, however, could affect labor supply, labor demand as well as the size of the distortion in heterogeneous ways across skill groups and time horizons, making it virtually impossible to sign the effect on relative wages. Nevertheless, to the extent that stronger institutions are associated with larger frictions we expect that they worsen the effect on average wages. We thus conclude that an empirical approach is required to fully understand the interactions between labor market policies and the wage consequences of foreign-born workers.

4 Empirical Strategy

4.1 Regression Equation

We estimate the following equation:

¹⁵See e.g. [Angrist and Kugler \(2003\)](#) where costs of firing (*Employment Protection*) is born solely by incumbent workers thereby harming their long run outcomes.

¹⁶Note, no adjustment costs prevent the economy from jumping from one equilibrium to the other in the neoclassical model.

$$y_{ict} = \alpha + \beta \cdot institution_{ct} + \gamma X_{ct} + \psi Z_i + \delta_t + \lambda_r + \varepsilon_{ict}.$$

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 effect, and $institution_{ct}$ is an institutional variable of interest¹⁷. X_{ct} includes GDP growth and unemployment to control for local economic conditions affecting labor market tightness and institutional structure. These variables account, for instance, for situations in which economic booms absorb foreign workers more smoothly and spur lower institutional coverage such as ALMP spending. Finally, the vector X_{ct} also contains a variable measuring the relative skill distribution of immigrants and natives defined as low skilled share of all immigrants over low skilled share of all natives. It controls for the possibility that countries with, say, higher labor market rigidity may have more restrictive immigration policies. This may, in turn, lead to relatively fewer low-skilled workers and hence smaller wage impacts when natives are relatively skilled.

The vector Z_i contains study/semi-elasticity characteristics such as data frequency, estimation method, and the skills of natives. The term δ_t is decade fixed effects accounting for worldwide differences across time periods and λ_r is a vector of region dummies for Nordic countries, the rest of Europe and the rest of the world, excluding North America which is the reference group. Lastly, ε_{ict} is an idiosyncratic mean-zero error term.

The coefficient of interest is β . We estimate this equation with weighted OLS (see the next Subsection) separately for average and relative wage effects. The standard errors are clustered by country but do not account for the fact that our outcome variable is already estimated (as in [Card, Kluve, and Weber, 2018](#)). In an ideal scenario, we would like to estimate the interaction between the wage returns to immigration and institutional coverage within countries. Nevertheless, given that institutions are set at the country level, this relationship will mainly be identified from cross-country variation within regions (while controlling for local economic conditions, X_{ct}). This is perhaps a limitation but we believe it is the closest we can get to causality without wiping out the useful part of the variation in the data.

4.2 Weighting Schemes

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 to ones derived from a paper with 30? Similarly, should articles published in the top academic

¹⁷Including all institution variables simultaneously results in much reduced sample size (see Section [2.2](#)) but the results are broadly in line with this specification.

journals, which have undergone 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 studies we introduce several different weighting schemes. First, we assign each wage effect size an equal weight and we refer to this as unweighted results. Second, instead, we give equal weights to each article. This procedure uses the number of reported semi-elasticities per article as an inverse probability weight and, intuitively, it downweights effect sizes coming from studies reporting many such semi-elasticities (see Appendix Table [A.1](#)). Appendix Figure [A.1a](#) and Appendix Figure [A.1b](#) show that the distributions of effect sizes are very similar under both weighting schemes.

Third, we account for study quality by using journals' impact factors data from RePEc and utilizing the inverse of the score as a weight. We refer to this as estimates weighted by impact. The procedure assigns higher importance to estimates published in higher ranked journals. Fourth, we adjust for the precision of the included estimates by using the inverse of the standard error as weighting factor. Weighting by impact factor (proxy for journal quality) leads to small modifications of the distributions while taking the statistical uncertainty into account leads to a large reduction of the mass in the tails and higher concentration of effect sizes around zero (see Appendix Figure [A.1c](#) and [A.1d](#)). 92 percent of relative and 90 percent of average wage effects are now concentrated in the interval $(-0.5, 0.5)$. Hence, weighting estimates by the inverse standard error penalizes noisily-measured entries and rewards more precise ones, minimizing the influence of effect sizes that are too large in magnitude to be informative on the plausible wage effect of immigration.

Finally, we construct a combined weight in which we multiply the 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. As such, it combines the virtues of all specifications and it is our preferred specification. We separately incorporate all these weighting schemes in estimating the regression equation.

5 Results

Table [3](#) displays our main results. Panel A shows the effects on relative wages and Panel B presents the results for average wages. Each entry is an estimated coefficient $\hat{\beta}$ of an institution variable denoted in the left column. Standard errors are shown in parenthesis and are clustered by country. Each column

corresponds to a different weighting scheme denoted in the column header. The rows below each regression coefficient denote the sample size (N) and the included controls. All regressions control for study characteristics, decade and region fixed effects, local economic conditions and the relative skill composition of immigrants.

Nearly all coefficients in Panel A are positive and many of them are statistically significant at the one percent level. Institution- and policy-induced market rigidities are effective in mitigating the relative wage changes induced by foreign workers. The first set of results shows the regression coefficient of our Employment Rigidity variable. A one unit increase in this index - a change equivalent to more than half the difference between the U.S. and Europe - is associated with a roughly one percentage point increase in the relative wage effects of immigration. All estimates are statistically significant. Next, the second row presents the results for wage rigidity as measured by collective bargaining coverage. A one percentage point increase in collective bargaining coverage leads to a weak increase in the relative wage effects of immigration by 0.01-0.02 percentage points. Note that such a change in coverage is minor since the sample mean of collective bargaining coverage is 62 percent and even within Europe we see differences of about 50 percentage points. Most of the coefficients are not statistically significant while all signs are positive and lead to the same conclusion. The third row displays the effects of labor immobility as measured by average job tenure in years. An increase in average tenure by one year – around a tenth of the sample mean – is associated with 0.22-0.36 percentage points higher relative wage effects of foreign-born workers and all coefficients are statistically significant. The second to last row shows the same results for ALMP expenditure as share of GDP. This is the only variable that does not show a clear pattern. Finally, the last row presents the coefficients from a regression of immigration earnings effects on product market regulation. A one unit increase in this index equivalent to roughly 40 percent of the sample mean is associated with approximately 0.68-0.94 percentage points higher relative wage impacts, and our preferred estimate that combines all weights implies a statistically significant relationship.

Panel B tells the opposite story – our preferred estimates (last column) point towards lower average wage effects in more rigid labor markets, but institutions are more weakly related to the immigration-induced impacts on average earnings. The negative relationship is consistent with [Angrist and Kugler \(2003\)](#) who find that stronger institutions worsen the employment effects of immigration using the area approach. The first row in Panel B shows a unit increase in employment rigidity variable is associated with 0.02 - 0.78 percentage points lower wage effects of foreign workers. Regardless of the weighting factor, all signs in this row are negative, pointing to the same conclusion. Similarly, the third shows less

labor mobility 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 0.09-0.51. More prevalent active labor market policies seem to also be negatively associated with estimated wage impacts of immigration but the coefficients are not statistically significant. Lastly, we find mixed evidence that business regulation affects the wage consequences of foreign-born workers.¹⁸

Overall, Table 3 shows that labor market rigidities exacerbate the average wage impacts of immigration while shielding from redistribution consequences caused by wage distortion between high- and low-skilled natives. This pattern holds across various different weighting factors. Furthermore, in Panel A of Appendix Table A.4 we show the interaction between relative wage impacts and institutions is also robust to excluding outliers (Column 2), excluding OLS estimates from studies that provide IV estimates (Column 3), focusing only on the “preferred” estimate (Column 4) and excluding study characteristics (Column 5). Hence, the relationship we describe is not correlated with study characteristics and persists across different subsamples of estimates. Panel B shows similar robustness checks for average effects. As in the main analysis, we find some evidence of a negative relationship between average wage impacts and institutional coverage but average wage impacts are less strongly related to institutions.

We also checked the sensitivity of our results to utilizing only the cross-sectional dimension for identifying the interaction between institutions and wage impacts of immigration (Column 6). These results eliminate important threats to identification such as the concern that immigrants could simultaneously affect institutions and macroeconomic conditions. Second and more importantly, institutions may respond directly to a surge in immigration introducing a reverse causality problem. To eliminate these concerns, we simply use the average value of each institutional variable in the country and show that our results survive this reduction in identifying variation. This is unsurprising since, as we show in Table 2, the variation in institutional scope is driven by cross-country and not within-country differences. The Online Appendix describes and presents a series of further robustness checks not discussed here. All these robustness tests are certainly reassuring as they inform us that no particular subset of studies or estimates drive our results.

6 Discussion

We measure a wide spectrum of labor market rigidities as brought about by more pervasive institutions, regulations and policies and investigate their role in intermediating the competition forces between na-

¹⁸This is also the institutional variable with poorest coverage.

tive and foreign workers. We consistently find that labor market institutions are effective in mitigating relative wage distortions caused by immigrants but mildly exacerbate effects on average wages. In other words, these policies are effective in shielding domestic economies from redistributive consequences and have modest adverse impacts for the average worker. Hence, we document an important role of institutions in explaining differences in labor market impacts of immigration across countries.

A discussion on the magnitude of the estimated coefficients is in order. Our preferred specification shows that a one standard deviation increase in labor market rigidity is associated with a 0.16-0.88 decrease in the estimated impact of immigrants on natives' relative wages. That is, such an intensification in labor market policies can dampen distortions among unskilled and skilled natives' wages due to immigration flows by 0.16 to 0.88 percentage points. The wide intervals mask significant heterogeneity among the different labor market institutions - ALMP and wage rigidity have the smallest impact (0.16 and 0.35) and they are not significant while the other three coefficients lie between 0.50 and 0.88. Next, a similar calculation shows a single standard deviation increase in the scope of labor market institutions leads to a decreased average wage effect of immigration by 0.04-0.71 percentage points. Labor immobility results in the largest change (0.71 percentage points) while the other four variables lie between 0.04 and 0.28. Given the sample statistics of reported estimates, these are certainly large effect sizes.

Our main result entails three main implications. Institutions and policies can be adequate and useful tools in successfully absorbing foreign worker flows into the labor market. This implication is especially relevant to policymakers concerned with wage differentials between the low- and high-skilled native workforce in the age of globalization. Second, a thorough understanding of the interaction between institutions and wage effects is fundamental in generalizing the labor market impact of immigration from one country to another. Lastly, differences in labor market rigidities across states are important drivers of the large heterogeneity of estimates in the literature.

7 Tables

Table 1: Summary Statistics of Previously Estimated Immigration Wage Effects

	Panel A: Relative Wages				Panel B: Average Wages			
	Mean	Median	N	Studies	Mean	Median	N	Studies
Total	-0.28	-0.10	1,092	42	0.15	0.11	456	26
<i>By Region</i>								
Nordic Countries	-0.34	-0.14	143	6	0.06	0.34	74	3
North America	-0.24	-0.15	411	12	0.30	0.08	148	12
Rest of Europe	-0.32	-0.04	474	23	0.09	0.10	222	10
Rest of World	-0.06	-0.01	64	3	-0.03	-0.00	12	2
<i>By Estimation Method</i>								
IV	-0.80	-0.49	245	19	0.22	0.24	163	14
OLS	-0.15	-0.12	501	29	0.14	0.07	257	20
Simulation	-0.09	0.01	346	14	-0.13	-0.06	36	4
<i>By Native Education Group</i>								
All	-0.47	-0.18	630	36	0.12	0.21	247	19
High Skill	0.50	-0.01	249	27	0.15	0.05	107	10
Low Skill	-0.61	-0.01	213	33	0.22	0.06	102	13
<i>By Data Frequency</i>								
Long Run	-0.27	-0.12	381	13	0.30	0.12	145	11
Short Run	-0.28	-0.09	711	30	0.08	0.11	311	15
<i>By Journal Rank</i>								
Outside Top 50	-0.15	-0.03	580	23	0.11	0.08	329	16
Top 50	-0.42	-0.21	512	19	0.24	0.23	127	10

Notes: Panels A and B show statistics for relative and average wages 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 (66) since some studies report estimates for multiple subgroups and methods.

Table 2: Summary Statistics of Institution Variables by Country

Country	Panel A: <i>Employment Rigidity</i>			Panel B: <i>Wage Rigidity</i>			Panel C: <i>Labor Immobility</i>			Panel D: <i>ALMP</i>			Panel E: <i>Business Regulation</i>		
	Mean	S.d.	N	Mean	S.d.	N	Mean	S.d.	N	Mean	S.d.	N	Mean	S.d.	N
Total	2.02	0.94	47	0.62	0.26	53	9.29	1.39	30	0.77	0.48	43	2.66	0.83	13
<i>Nordic Countries</i>															
Denmark	2.16	0.03	5	0.83	0.01	4	8.66	0.23	4	1.51	0.31	4	2.32	-	1
Finland	2.79	-	1	0.85	-	1	10.73	-	1	0.83	-	1	-	-	-
Norway	2.33	0.00	4	0.71	0.01	3	9.20	0.02	2	0.91	0.18	4	2.81	-	1
Sweden	2.80	-	1	0.91	-	1	10.56	-	1	1.51	-	1	-	-	-
<i>Rest of Europe</i>															
Austria	2.75	0.00	2	0.98	-	1	10.54	-	1	0.31	0.07	2	-	-	-
France	2.34	0.00	2	0.82	0.14	4	10.71	0.34	2	0.96	0.35	2	3.41	-	1
Germany	2.62	0.05	8	0.80	0.06	8	10.04	0.06	3	1.06	0.32	8	2.57	-	1
Greece	2.80	-	1	0.82	-	1	12.68	-	1	0.28	-	1	4.24	-	1
Ireland	1.44	-	1	0.44	-	1	9.19	-	1	0.97	-	1	3.00	-	1
Italy	2.76	-	1	0.80	-	1	11.85	-	1	-	-	-	-	-	-
Netherlands	2.84	0.00	2	0.80	-	1	9.63	0.30	2	1.34	0.13	2	2.97	-	1
Portugal	5.00	-	1	0.69	0.01	3	-	-	-	0.20	-	1	-	-	-
Spain	2.36	0.00	2	0.80	0.02	2	9.24	0.08	2	0.71	0.01	2	2.49	-	1
Switzerland	1.60	-	1	0.46	-	1	9.03	-	1	0.21	-	1	-	-	-
United Kingdom	1.16	0.09	5	0.49	0.19	6	8.00	0.11	4	0.37	0.10	4	1.42	0.37	2
<i>North America</i>															
Canada	-	-	-	0.34	-	1	-	-	-	-	-	-	-	-	-
United States	0.26	0.00	5	0.24	0.07	10	7.42	1.20	4	0.20	0.04	5	1.96	-	1
<i>Rest of the World</i>															
Australia	1.17	0.00	4	0.74	0.13	3	-	-	-	0.47	0.24	4	2.28	-	1
Turkey	2.31	-	1	0.07	-	1	-	-	-	-	-	-	3.66	-	1

Notes: The mean, standard deviation and number of observations are calculated based on the OECD panel restricted to the years we merge to our sample of estimates. N denotes the number of country-year observations we merge to our database of effect sizes.

Table 3: Institutions and the Wage Effect of Immigrants on Natives

	Panel A: Relative Wages					Panel B: Average Wages				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
<i>Employment Rigidity</i>	0.935*** (0.226)	1.088*** (0.271)	0.783*** (0.224)	0.638** (0.263)	0.935*** (0.293)	-0.278 (0.207)	-0.778*** (0.146)	-0.053 (0.142)	-0.015 (0.078)	-0.204 (0.152)
N	780	780	780	539	539	422	422	422	402	402
<i>Wage Rigidity</i>	0.533 (1.136)	2.101* (1.103)	0.691 (0.971)	0.188 (0.857)	1.337 (0.791)	-0.373 (0.892)	1.233 (2.944)	0.776 (0.685)	0.284 (0.177)	1.094 (1.098)
N	868	868	868	627	627	455	455	455	434	434
<i>Labor Immobility</i>	0.255** (0.093)	0.266*** (0.056)	0.217*** (0.069)	0.250*** (0.073)	0.360*** (0.097)	-0.085 (0.323)	-0.234 (0.817)	-0.409** (0.172)	-0.279 (0.204)	-0.507** (0.170)
N	668	668	668	461	461	351	351	351	334	334
<i>ALMP</i>	-0.339 (0.537)	0.048 (0.447)	-0.069 (0.326)	0.306 (0.367)	0.341 (0.337)	-0.404 (0.371)	-2.105 (1.211)	-0.039 (0.193)	0.010 (0.117)	-0.301 (0.392)
N	749	749	749	508	508	410	410	410	390	390
<i>Business Regulation</i>	0.805*** (0.152)	0.923** (0.327)	0.822*** (0.163)	0.682 (0.382)	0.944*** (0.260)	1.175** (0.376)	0.963 (0.914)	0.003 (0.191)	0.067 (0.045)	-0.054 (0.142)
N	395	395	395	292	292	233	233	233	222	222
Region FE	X	X	X	X	X	X	X	X	X	X
Decade 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: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors in parentheses are clustered by countries. Models are linear regressions with the effect size as the dependent variable. Effect sizes are relative wage effects in panel A and average wage effects in panel B. All institutional variables are described in section 2.2.2. 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 combining all three weights (Combined). Country-level controls are GDP growth, unemployment rate, and the ratio of the share of low skilled immigrants to low skilled natives. Study characteristics are dummies for high- and low-skilled natives (estimates based on the entire workforce is the reference), dummies for IV and simulation (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference).

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A Appendix

A.1 Sampling, Inclusion Criteria and Final List of Effect Studies

We collected studies for the database in two steps. First, through extensive searches on Google Scholar using the phrases “labor market impact immigration” and “wage impact 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. The study estimates the impact of immigration on natives’ wages or earnings¹⁹
2. Immigration is measured as a share of the total population or relative to the native population. This implies that we do not include studies where immigration is defined in terms of diversity.
3. The study is published in a peer-reviewed journal or as a book chapter²⁰
4. 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.

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).

Table A.1 lists all the studies used in our analysis.²¹

¹⁹The database contains 672 estimated employment and unemployment effects not used in this article.

²⁰Altonji and Card (1991) and LaLonde and Topel (1991) are book chapters.

²¹See the Online Data Appendix for a complete list of studies in our database, including articles that only consider employment or unemployment outcomes.

Table A.1: List of Studies Included in Our Meta-Analysis

Authors	Year	Journal	Countries	Data Period	N
Addison and Worswick	2002	The Economic Record	Australia	1982-1996	27
Altonji and Card	1991	Immigration, trade, and the labor market	U.S.	1970-1980	20
Aydemir and Borjas	2011	Journal of Labor Economics	Canada, U.S.	1960-2001	28
Barrett et al.	2006	The Economic and Social Review	Ireland	1966-2002	18
Barrett et al.	2011	The Economic and Social Review	Ireland	1999-2007	30
Bauer et al.	2013	Review of International Economics	Germany	2000-2005	19
Borjas	1987	Industrial and Labor Relations Review	U.S.	1980	32
Borjas	2003	The Quarterly Journal of Economics	U.S.	1960-2000	33
Borjas et al.	1996	American Economic Review	U.S.	1980-1990	20
Borjas et al.	1997	Brookings Papers on Economic Activity	U.S.	1960-1990	34
Bratsberg and Raaum	2012	Economic Journal	Norway	1998-2005	32
Bratsberg et al.	2014	Scandinavian Journal of Economics	Norway	1993-2006	31
Brücker and Jahn	2011	Scandinavian Journal of Economics	Germany	1980-2004	25
Brücker et al.	2014	European Economic Review	Denmark, Germany, U.K.	1993-2009	36
Card	1990	Industrial and Labor Relations Review	U.S.	1982-1979	3
Card	2001	Journal of Labor Economics	U.S.	1990	14
Carrasco et al.	2008	Journal of Population Economics	Spain	1991-2001	12
Carrington and De Lima	1996	Industrial and Labor Relations Review	Portugal	1962-1981	16
Cattaneo et al.	2015	Journal of Human Resources	Western Europe	1994-2001	14
Ceritoglu et al.	2017	IZA Journal of Labor Policy	Turkey	2010-2013	11
Chassamboulli and Palivos	2014	International Economic Review	U.S.	2000-2009	24
Chassamboulli and Palivos	2013	Journal of Macroeconomics	Greece	2000-2007	57
Clark and Drinkwater	2009	Nordic Journal of Political Economy	U.K.	2000-2007	6
Cohen-Goldner and Paserman	2011	European Economic Review	Israel	1989-1999	20
D'Amuri et al.	2010	European Economic Review	Germany	1992-2001	8
De New and Zimmermann	1994	Journal of Population Economics	Germany	1984-1989	8
Docquier et al.	2013	Economic Journal	Sweden, Finland, Germany, Denmark	1990-2000	36
Dustmann and Glitz	2015	Journal of Labor Economics	Germany	1985-1995	13
Dustmann et al.	2005	Economic Journal	U.K.	1983-2000	5
Dustmann et al.	2013	Review of Economic Studies	U.K.	1997-2005	66
Dustmann et al.	2017	The Quarterly Journal of Economics	Germany	1990-1993	24
Edo	2015	The B.E. Journal of Economic Analysis & Policy	France	1990-2002	56
Edo and Toubal	2015	Review of International Economics	France	1990-2010	48
Enchautegui	1995	Contemporary Economic Policy	U.S.	1980-1990	12

Foged and Peri	2016	American Economic Journal: Applied Economics	Denmark	1995-2008	12
Friedberg	2001	The Quarterly Journal of Economics	Israel	1989-1994	17
Gavosto et al.	1999	LABOUR	Italy	1990-1995	15
Gerfin and Kaiser	2010	Swiss Journal of Economics and Statistics (SJES)	Switzerland	1991-2008	16
Glitz	2012	Journal of Labor Economics	Germany	1996-2001	16
González and Ortega	2011	Labour Economics	Spain	2001-2006	12
Greenwood et al.	1997	Labour Economics	U.S.	1980	16
Grossman	1982	The Review of Economics and Statistics	U.S.	1970	3
Hofer and Huber	2003	Empirica	Austria	1991-1994	8
Hothckiss et al.	2015	Southern Economic Journal	U.S.	1995-2005	10
Hunt	1992	Industrial and Labor Relations Review	France	1962-1962	5
LaLonde and Topel	1991	Immigration, trade, and the labor market	U.S.	1980	16
Lemos and Portes	2014	The B.E. Journal of Economic Analysis & Policy	U.K.	2004-2006	8
Llull	2017	Journal of Human Resources	Canada, U.S.	1960-2000	104
Llull	2017	Review of Economic Studies	U.S.	1967-2007	20
Malchow-Møller et al.	2012	Scandinavian Journal of Economics	Denmark	1993-2004	25
Malchow-Møller et al.	2013	American Journal of Agricultural Economics	Denmark	1980-2008	16
Manacorda	2012	Journal of the European Economic Association	U.K.	1975-2005	2
Mitaritonna et al.	2017	European Economic Review	France	1996-2005	6
Olney	2012	Canadian Journal of Economics	U.S.	2000-2006	20
Orrenius and Zavodny	2007	Labour Economics	U.S.	1994-2000	63
Ortega and Verdugo	2014	Labour Economics	France	1968-1999	14
Ottaviano and Peri	2012	Journal of the European Economic Association	U.S.	1990-2006	45
Pedace	2006	American Journal of Economics and Sociology	U.S.	1980-1990	24
Pedace	1998	Eastern Economic Journal	U.S.	1980-1990	12
Peri and Sparber	2009	American Economic Journal: Applied Economics	U.S.	1990-2000	1
Peri and Yassenov	2018	Journal of Human Resources	U.S.	1973-1991	5
Schmidt and Jensen	2013	The Annals of Regional Science	Denmark	1997-2006	6
Steinhardt	2011	The B.E. Journal of Economic Analysis & Policy	Germany	1975-2001	49
Tumen	2016	American Economic Review	Turkey	2010-2013	1
Winter-Ebmer and Zweimüller	1996	Oxford Economic Papers	Austria	1988-1991	19
Zorlu and Hartog	2005	Journal of Population Economics	Netherlands, Norway, U.K.	1989-1998	132

Notes: N denotes the number of wage effect sizes in each study. Year refers to the year of publication. The Online Data Appendix contains a full list of studies in our database. Eight estimates extracted from Carrington and De Lima (1996) cannot be classified into relative or average effects since they only exploit variation across time.

A.2 Test for Publication Bias

Table A.2: Publication Bias Test

	Panel A: Relative Wages					Panel B: Average Wages				
	Unweighted	Studies	Impact	Precision	Combined	Unweighted	Studies	Impact	Precision	Combined
Standard Error	-0.065*** (0.002)	-0.062*** (0.003)	-0.062*** (0.006)	-0.042 (0.057)	0.040 (0.150)	-0.741** (0.295)	-0.728 (0.433)	-0.306 (0.350)	0.073 (0.433)	0.525 (0.515)
N	680	680	680	664	664	438	438	438	434	434
Region FE	X	X	X	X	X	X	X	X	X	X
Decade 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: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors in parentheses are clustered by studies. Models are linear regressions with the effect size as the dependent variable. Effect sizes are relative wage effects in panel A and average wage effects in panel B. 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 combining all three weights (Combined). Country-level controls are GDP growth, unemployment rate and the ratio of the share of low skilled immigrants to low skilled natives. Study characteristics are dummies for high- and low-skilled natives (estimates based on the entire workforce is the reference), dummies for IV and simulation (OLS is the reference), and a dummy for annual or more frequent data (less frequent is the reference).

A.3 Distribution of Wage Effect Sizes

Table A.3: Main Statistics of Our Sample of Wage Estimates

	Unweighted	Studies	Impact	Precision	Combined
<i>Panel A: Relative Wages</i>					
Mean	-0.28	-0.28	-0.25	-0.05	-0.01
Median	-0.10	-0.06	-0.17	-0.00	-0.00
S.d.	2.69	2.03	1.75	0.43	0.14
N	1,092	1,092	1,092	831	831
<i>Panel B: Average Wages</i>					
Mean	0.15	0.12	0.04	0.07	0.03
Median	0.11	0.07	-0.07	0.01	0.00
S.d.	1.30	1.58	0.53	0.47	0.26
N	456	456	456	435	435

Notes: Panel A and B contain summary statistics for relative and average wage effect sizes respectively. First column is based on the unweighted effect sizes. In the next four columns show summary statistics under different weighting schemes: by the inverse number of effect sizes extracted from each study (Studies), by the inverse journal score (Impact), by the inverse standard error (Precision) and lastly combining the three weights (Combined).

Figure A.1: Distribution of Wage Effect Sizes

Notes: The graphs show the distributions of relative and average wage effect sizes respectively. Estimates are top and bottom coded at $+/-2$. The effect sizes are unweighted in panel a), while panels b)-d) show the distribution under different weighting schemes: by the inverse number of effect sizes extracted from each study, by the inverse journal score or by the inverse standard error.

A.4 Robustness Checks

Table A.4: Institutions and the Wage Effect of Immigrants on Natives, Robustness Checks

	Panel A: Relative Wages						Panel B: Average Wages					
	Main (1)	Excl. SE > 1 (2)	No OLS (3)	Preferred Estimate (4)	No Study Characteristics (5)	Avg. Institution (6)	Main (1)	Excl. SE > 1 (2)	No OLS (3)	Preferred Estimate (4)	No Study Characteristics (5)	Avg. Institution (6)
<i>Employment Rigidity</i>	0.935*** (0.293)	0.914*** (0.278)	1.320*** (0.265)	1.360* (0.660)	0.877*** (0.188)	0.011 (0.343)	-0.204 (0.152)	-0.170 (0.116)	-0.196 (0.141)	0.009 (1.204)	-0.172 (0.167)	-0.231 (0.160)
<i>Employment Rigidity (Temporary)</i>	-0.086 (0.113)	-0.045 (0.110)	-0.001 (0.129)	0.250 (0.183)	-0.063 (0.113)	-0.012 (0.186)	-0.396 (0.299)	-0.366 (0.280)	-0.488* (0.257)	-0.302 (0.234)	-0.382 (0.298)	-0.473 (0.363)
<i>Wage Rigidity</i>	1.337 (0.791)	1.668** (0.581)	1.029 (0.719)	1.192 (2.279)	1.110 (0.868)	2.160*** (0.546)	1.094 (1.098)	1.144 (1.131)	2.029 (1.428)	-3.167 (3.403)	1.244 (1.124)	1.512 (1.955)
<i>Labor Immobility</i>	0.360*** (0.097)	0.398** (0.127)	0.387*** (0.069)	0.597*** (0.123)	0.092 (0.103)	0.082 (0.204)	-0.507** (0.170)	-0.502** (0.165)	-0.575*** (0.096)	-0.199 (0.316)	-0.567** (0.172)	-0.293 (0.170)
<i>ALMP</i>	0.341 (0.337)	0.281 (0.393)	0.553 (0.367)	0.601 (0.399)	0.360 (0.323)	0.297 (0.303)	-0.301 (0.392)	-0.286 (0.386)	-0.181 (0.454)	-0.411 (0.531)	-0.230 (0.384)	-0.627 (0.736)
<i>Barriers Entrepreneurship</i>	1.800*** (0.420)	2.406*** (0.558)	2.754*** (0.738)	2.937 (6.249)	1.926** (0.582)	-0.538 (0.604)	-2.115 (1.274)	-1.036** (0.356)	-6.377 (4.516)	-6.693*** (0.000)	-2.170 (1.409)	-3.129 (2.291)
<i>Barriers Trade and Investment</i>	-0.802 (0.713)	-1.893 (1.430)	1.028 (1.576)	0.684 (8.412)	-0.413 (1.127)	-0.642 (0.501)	-1.736 (1.077)	-0.811** (0.248)	-5.418 (3.778)	-20.366*** (0.000)	-1.775 (1.176)	-0.242 (1.284)
<i>Business Regulation</i>	0.944*** (0.260)	1.210** (0.385)	2.408*** (0.288)	3.352 (2.767)	0.934*** (0.180)	0.700** (0.307)	-0.054 (0.142)	-0.167** (0.048)	-0.187 (0.334)	-3.635*** (0.000)	-0.075 (0.101)	-0.359 (0.348)
Region FE	X	X	X	X	X	X	X	X	X	X	X	X
Decade FE	X	X	X	X	X	X	X	X	X	X	X	X
Country-Level Controls	X	X	X	X	X	X	X	X	X	X	X	X
Study Characteristics	X	X	X	X	No	X	X	X	X	X	No	X

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Standard errors in parentheses are clustered by countries. Models are weighted linear regressions with the effect size as the dependent variable. Weights correspond to the combined weights used in Table 3. The first column in each panel replicates results from Table 3. Column 2 excludes effect sizes with standard errors greater than 1. Column 3 uses one preferred estimate from each study. Column 4 excludes OLS estimates if IV estimates are reported within studies. Columns 5 show estimates without macroeconomic controls. Column 6 displays estimates where the institutional variable is replaced by its mean value within countries. Country-level controls and study characteristics are described in Table 3. See the Online Data Appendix for a complete description of all the variables.