2

The Impact of Immigration on Productivity

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2.1 Introduction

Immigration to the countries of the Organisation for Economic Co-operation and Development (OECD) has increased dramatically in recent years. This increase has motivated a large debate both in the political arena and among researchers regarding the consequences of immigrant inflows for the receiving economies. On the political arena, many political campaigns have drawn intensively on anti-immigration sentiment to gain votes (e.g. Trump, Brexit, Salvini, and many other presidential campaigns in Europe). Among researchers, a vast labour economics literature has analysed the effects of immigration on wages without reaching any consensus.[1] From a more aggregate perspective, fewer studies

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have examined the economic effects of immigration on receiving countries, focusing on outcomes such as employment, income per capita, total factor productivity (TFP), and inflation (de la Rica et al. 2015).

This chapter provides a cross-country analysis of the impact of immigration on productivity. In particular, it analyses the effect of immigration on GDP per capita, employment rate, hours worked, and unemployment rate using aggregate variation across OECD destination countries. The analysis exploits exogenous variation from country of origin *push* factors (wars, political environment, demographic, and economic factors) leveraged across destination countries by the distance between origin and destination, which determines the choice of the destination country for immigrants that decide to move. Therefore, the variation is not given by the push factors or the distance themselves (which are collinear with fixed effects included in the regression), but by their interaction. For example, the Syrian war pushes more immigrants to Europe than to Australia.

In order to structurally interpret the results, this chapter provides an analytical framework based on a simple production function. This framework allows for some back-of-the-envelope calculations that also shed light on the predicted effects on wages or the marginal productivity of labour. This analysis allows to disentangle the separate effects on the productivity of natives and immigrants, and provides results that are more readily comparable with the large labour economics literature on wage effects of immigration.

First-stage regressions are estimated using bilateral immigrant stocks data collected by Llull (2016). The push-distance interactions provide relevant and arguably exogenous variation that allow for the identification of the results. Second-stage regression results suggest that a one percentage point increase in the share of immigrants in the population reduces the country's GDP per capita by 2%. Furthermore, employment effects are also important: a one point increase in the share of immigrants reduces the employment rate by 0.888 percentage points, reduces average hours worked by those individuals who stay employed by 1.28%, and increases the unemployment rate by 0.55 percentage points.

Back-of-the-envelope calculations based on the production framework introduced in this chapter suggest that the capital supply elasticity is not zero, but also not infinite. This result implies that immigration increases labour market competition because the increase in labour supply is not compensated by a large enough increase in the supply of capital. The downward wage pressure associated with the larger competition may or may not be shared between immigrants and natives. If immigrants and natives are perfect substitutes, the effect is distributed equally across the two groups. However, assuming that natives and immigrants are imperfect substitutes in production (as in Ottaviano and Peri 2012), the effect on native wages is ambiguous because the increase in the number of immigrants increases the demand for the (imperfectly substitutable) native labour. Given the estimated coefficients, the semi-elasticity of native wages to the immigrant share is estimated to be −0.7 if the extensive margin of labour supply is ignored (i.e. if we average in the zeros of natives who stop working) and 0.12 if we compute it only for the individuals who remain at work. Thus, the effect of immigration is large and negative for some natives (those who lose their jobs) and slightly positive for others (those who manage to keep them). Consistent with the literature and with theoretical predictions, the effect of immigration on the wages of immigrants is unambiguously negative.

An earlier version of the research reproduced in this chapter (Llull 2008) was among the first papers to explore the effect of immigration on GDP per worker in a cross-country setting.[2] Angrist and Kugler (2003) use data from European countries to identify the effect of immigration on employment, analysing the role of labour market (lack of) flexibility in channelling these effects. Building on the trade literature, Andersen and Dalgaard (2011) and Ortega and Peri (2014a, b) use cross-country variation to jointly estimate the long-run effects of trade and immigration on income. Using somewhat more structural approaches, di Giovanni et al. (2015) and Docquier et al. (2014) analyse similar questions. Like the previous two studies, di Giovanni et al. (2015) draw from the trade literature and conduct an evaluation of the global effects of international migration using a model of trade with varieties. Docquier et al. (2014) calibrate an aggregate model of the global labour market to fit bilateral stocks data across many countries of origin and destination.

An important concern for the identification of the effect of interest, which has also been raised in the labour economics literature, is the extent to which the inflow of migrants into the different markets is endogenous. In observational studies that do not exploit natural experiments, this has been a prominent complication that has been discussed in many papers. In the labour economics literature, most papers, pioneered by Altonji and Card (1991) and Card (2001), use past settlements of immigrants as instruments for subsequent inflows.[3] This approach has been widely criticized (e.g. see Borjas et al. 1997; Borjas 1999, 2003, 2014). In particular, if the unobservable factors that determine wages and attract immigrants are persistent over time, past settlements are likely to be correlated with current inflows. As noted above, this chapter uses variation in the interaction of push factors and distance to identify the effect. Methodologically, this approach is close to Angrist and Kugler (2003), who leverage the different stages of the Balkans War with distance to Yugoslavia to obtain cross-country over-time variation. It is also related to Llull (2018b), who uses the interaction of similar push factors as those used in this chapter with geographic and cultural distance further interacted with skill-cell dummies to obtain variation across skill-cells for a given country. Ortega and Peri (2014a, b) also use gravity-based instruments in migration regressions, but they do not exploit over-time variation in their predictions. More broadly, this chapter is also related to labour economics papers that exploit natural experiments as push factors (Card 1990; Hunt 1992; Glitz 2012; Dustmann et al. 2017; Monràs 2019).

The labour economics literature on immigration analyses the effect of immigration on wages and other outcomes by comparing different labour markets that face different levels of immigrant penetration. The studies in the literature differ in the way they define labour markets. Traditional studies use cross-city variation to identify the effects (e.g. Grossman 1982; Altonji and Card 1991; Card 2001). The papers following this so-called spatial approach tend to find negligible impacts of immigration on wages and employment. Other papers, pioneered by Borjas et al. (1997) and Borjas (2003), identify the effects at the national level defining labour markets in terms of skills (see also Aydemir and Borjas 2007, 2011; and Llull 2018b). The papers in the skill-cell approach tend to find sizeable effects. The variety of results obtained with the two approaches motivated a long-standing debate.

There are many potential reasons behind the different results in the literature. Dustmann et al. (2016) provide a unified theoretical framework that shows that the skill-cell and the spatial approaches identify different elasticities. Aydemir and Borjas (2011) state that an important part of the differences in the results are attributable to attenuation bias caused by measurement error in the immigrant shares at the local level. Borjas (2006) argues that spatial arbitrage can generate negligible effects in the spatial approach. Lewis (2011) shows that firms adjust their capital adoption decisions depending on immigration, which is another form of spatial arbitrage.

Following the framework in Dustmann et al. (2016), this chapter identifies the same coefficient as in the spatial correlations approach and yet provides results that are more in line with the findings in the skill-cell approach. This result can be the consequence of the absence of spatial arbitrage and measurement error at the cross-country level, and also of the less elastic capital supply at the national level (small versus large economies).

The remainder of this chapter is organized as follows. Section 2.2 explains the econometric model. Section 2.3 discusses the econometric framework and the differences between the cross-country and cross-metropolitan area spatial approaches. Section 2.4 introduces the data. Section 2.5 presents the estimation results, and Sect. 2.6 presents the results from the back-of-the-envelope simulations before concluding in Sect. 2.7.

2.2 Theoretical Framework

Consider the following production function:

$$Y_{it} = A_{it} K_{it}^{\alpha} \left(\theta N_{it}^{\rho} + \left(1-\theta\right) I_{it}^{\rho} \right)^{\frac{1-\alpha}{\rho}}, \tag{2.1}$$

where Y_{it} denotes GDP in country i and year t, A_{it} is TFP, N_{it} denotes the number of natives, and I_{it} denotes the number of immigrants. Capital is supplied according to $r_t = K^{\lambda}$, where r_t is the interest rate, and $1/\lambda$ is the capital supply elasticity (Dustmann et al. 2016).[4] Let the economy be characterized by a representative firm that produces output using the technology described in Eq. (2.1) and pays competitive wages and interest rates. In such context, equilibrium capital equals:

$$K_{it} = (\alpha A_{it})^{\frac{1}{1-\alpha+\lambda}} L_{it}^{\frac{1-\alpha}{1-\alpha+\lambda}}, \tag{2.2}$$

where $L_{it} \equiv \left(\theta N_{it}^{\rho} + (1-\theta) I_{it}^{\rho}\right)^{\frac{1}{\rho}}$. Substituting Eq. (2.2) into (2.1), dividing by total population ($N_{it} + I_{it}$), and rearranging yields:

$$y_{it} \equiv \frac{Y_{it}}{N_{it} + I_{it}} = A_{it} (\alpha A_{it})^{\frac{\alpha}{1-\alpha+\lambda}} \left(\theta (1-m_{it})^{\rho} + (1-\theta) m_{it}^{\rho}\right)^{\frac{1-\frac{\alpha\lambda}{1-\alpha+\lambda}}{\rho}}. \tag{2.3}$$

where $m_{it} \equiv I_{it}/(I_{it} + N_{it})$ is the share of immigrants. Log-differentiating this expression gives, upon rearrangement:

$$\frac{d\ln y_{it}}{dm_{it}} = \left(1 - \frac{\alpha\lambda}{1-\alpha+\lambda}\right) \frac{(1-\theta) m_{it}^{\rho-1} - \theta (1-m_{it})^{\rho-1}}{\left(\theta (1-m_{it})^{\rho} + (1-\theta) m_{it}^{\rho}\right)}. \tag{2.4}$$

Noting that the denominator is positive, and the term in the first parenthesis ranges between $1 - \alpha$ (when $\lambda \to \infty$) and 1 (when $\lambda = 0$), the sign of the estimated semi-elasticity of interest depends on the sign of the numerator. In particular, a positive effect occurs if and only if:

$$(1-\theta) m_{it}^{\rho-1} > \theta (1-m_{it})^{\rho-1} \quad \Leftrightarrow \quad \theta < \frac{m_{it}^{\rho-1}}{m_{it}^{\rho-1} + (1-m_{it})^{\rho-1}}. \tag{2.5}$$

Simple comparative statics with Eq. (2.5) are informative. Ottaviano and Peri (2012) estimate the elasticity of substitution between natives and immigrants (within the same education-experience group) to average around 20. At the average immigration rate in the data used below, which is around 7%, the estimated effect on productivity should be positive iff

$\theta < 0.533$ and negative otherwise. The intuition is as follows. If natives and immigrants were perfect substitutes ($\rho = 1$), then the threshold would be 0.5: if we are adding less productive individuals, then the effect is negative, and if we are adding more productive individuals, then the effect is positive. The extra margin comes from the imperfect substitutability between natives and immigrants: even if immigrants are slightly less productive than natives, the partial complementarity with natives compensates this negative composition effect. If immigrants are much less productive than natives, the composition effect dominates. All in all, these composition and substitution effects determine whether the amount efficiency units per capita increases or decreases with the increase in the number of immigrants.

The other two parameters, α and λ, play a role in amplifying the positive or negative effects. A large elasticity of capital supply (small λ) implies that capital reacts more to changes in effective labour supply, and, hence, GDP per capita also reacts more. A large value of α makes labour relatively less important in determining GDP than capital, and, therefore, reduces the size of the reaction of GDP per capita to changes in the effective supply of labour.

As discussed in the introduction, a large literature in labour economics has estimated the effect of immigration on native wages (equivalent to native labour productivity in a competitive setting). Equation (2.4) describes the effect of immigration on overall productivity (GDP per capita). Additionally, this framework allows for the derivation of expressions for wages, which are more directly comparable with the estimates in the labour literature. In a competitive economy, workers are paid their marginal product. Therefore, native wages are given by:

$$w_{it}^{(N)} = \theta\left(1 - \frac{\alpha\lambda}{1-\alpha+\lambda}\right) A_{it} \left(\alpha A_{it}\right)^{\frac{\alpha}{1-\alpha+\lambda}} \left(\begin{matrix}\theta\left(1-m_{it}\right)^{\rho} + \\ \left(1-\theta\right) m_{it}^{\rho}\end{matrix}\right)^{\frac{1-\rho}{\rho} - \frac{\alpha\lambda}{\rho(1-\alpha+\lambda)}} \left(1-m_{it}\right)^{\rho-1} \left(N_{it} + I_{it}\right)^{\frac{-\alpha\lambda}{1-\alpha+\lambda}}. \tag{2.6}$$

Noting that $d(N_{it} + I_{it})/dm_{it} = (N_{it} + I_{it})/(1 - m_{it})$ and log-differentiating the above expression gives:

$$\frac{dlnw_{it}^{(N)}}{dm_{it}} = \left(1-\rho-\frac{\alpha\lambda}{1-\alpha-\lambda}\right)\left[\frac{(1-\theta)m_{it}^{\rho-1}-\theta(1-m_{it})^{\rho-1}}{\left(\theta(1-m_{it})^{\rho}+(1-\theta)m_{it}^{\rho}\right)}+\frac{1}{1-m_{it}}\right]. \tag{2.7}$$

The corresponding expression for immigrant wages is given by an analogous expression to Eq. (2.6) in which the first term, θ, is replaced by $1 - \theta$, and the penultimate term, $(1 - m_{it})^{\rho - 1}$, is replaced by $m_{it}^{\rho-1}$. Therefore:

$$\frac{dlnw_{it}^{(I)}}{dm_{it}} = \left(1-\rho-\frac{\alpha\lambda}{1-\alpha-\lambda}\right)\frac{(1-\theta)m_{it}^{\rho-1}-\theta(1-m_{it})^{\rho-1}}{\left(\theta(1-m_{it})^{\rho}+(1-\theta)m_{it}^{\rho}\right)} - \frac{(1-\rho)}{m_{it}}-\frac{\alpha\lambda}{1-\alpha-\lambda}\frac{1}{1-m_{it}}. \tag{2.8}$$

Equations (2.7) and (2.8) provide a metric that allows for the comparison of the results with those in the labour economics literature, by means of a simple back-of-the-envelope calculation. Theoretically, if $\lambda = 0$ (perfectly elastic capital supply) and $\rho = 1$ (natives and immigrants are perfect substitutes), the effect of immigration on wages of both natives and immigrants will be zero. This is so because all changes in labour supply are compensated by adjustments in physical capital. Imperfect substitutability between immigrants and natives makes natives to gain relative to immigrants. In particular, if $\lambda = 0$ and $\rho < 1$, natives wages increase and immigrant wages decrease by a similar amount, so that the average effect is zero. If, additionally, $\lambda > 0$, then immigrant wages unambiguously decrease, whereas the effect on native workers becomes ambiguous: the overall negative wage effects generated by the partial adjustment of capital may or may not be offset by the imperfect substitutability effects.

This implication highlights an important part of the empirical contribution below. Dustmann et al. (2016) provide a unified framework to understand the differences in estimates obtained in spatial and skill-cell approaches. Results from the spatial approach, the type of variation exploited in this chapter, crucially depend on the capital supply elasticity. The debate between Borjas (2003) and Ottaviano and Peri (2012) highlights that the importance of the capital supply elasticity and imperfect substitutability between immigrants and natives is also fundamental to understand the effects. Borjas (2013) shows that the overall effects on aggregate wages in the Borjas/Ottaviano-Peri structural frameworks is completely determined by the assumed capital supply elasticity (which Borjas assumes to be zero, and Ottaviano and Peri assume to be infinite). Lewis (2011) provides evidence of capital adjustments to immigration, suggesting that there is some, potentially imperfect, adjustment of capital. The analysis on GDP per capita from this chapter, based on Eq. (2.4) allows for indirect (back-of-the-envelope) inference on the capital supply elasticity.

Results presented below also provide evidence of the impacts of immigration on labour supply, both at the intensive and extensive margins. Such labour supply effects generate an effective overall increase in labour supply that is smaller than the increase in the number of individuals in the population. Assuming that the effects on labour supply are homogeneous across workers, this variation enters the wage equations through the change in the last term in Eq. (2.6). Let Δ_E denote the increase in effective labour (e.g. employment rate). In this case, the effective labour supply increase is $d(N_{it} + I_{it})/dm_{it} = (1 + \Delta_E)(N_{it} + I_{it})/(1 - m_{it})$. Therefore, Eqs. (2.7) and (2.8) rewrite as:

$$\frac{dlnw_{it}^{(N)}}{dm_{it}} = \left(1-\rho-\frac{\alpha\lambda}{1-\alpha-\lambda}\right)\frac{(1-\theta)m_{it}^{\rho-1}-\theta(1-m_{it})^{\rho-1}}{\left(\theta(1-m_{it})^{\rho}+(1-\theta)m_{it}^{\rho}\right)} + \frac{(1-\rho)}{1-m_{it}} - \frac{\alpha\lambda}{1-\alpha-\lambda}\frac{1+\Delta_E}{1-m_{it}} \tag{2.9}$$

and:

$$\frac{dlnw_{it}^{(I)}}{dm_{it}} = \left(1-\rho-\frac{\alpha\lambda}{1-\alpha-\lambda}\right)\frac{(1-\theta)m_{it}^{\rho-1}-\theta(1-m_{it})^{\rho-1}}{\left(\theta(1-m_{it})^{\rho}+(1-\theta)m_{it}^{\rho}\right)} - \frac{(1-\rho)}{m_{it}} - \frac{\alpha\lambda}{1-\alpha-\lambda}\frac{1+\Delta_E}{1-m_{it}}. \tag{2.10}$$

In words, a positive change in employment intensity implies a larger labour supply shock, which puts extra negative pressure on wages if $\lambda > 0$, and a negative change reduces downward wage pressures (for those individuals who work). If $\Delta_E = -1$, there is a perfect displacement effect, and the last term, which captures the labour supply effect, cancels.

2.3 Cross-Country Spatial Regressions

Analysing the economic effects of immigration requires a counterfactual comparison of a given labour market in the presence and in the absence of immigration. Because we are unable to observe the reality in such parallel worlds, comparing outcomes across different but similar markets is the only chance to identify these effects. A typical paper in the labour economics literature uses the following regression (Aydemir and Borjas 2011):

$$\omega_k = \phi m_k + \sum_h \varphi_h z_{kh} + \upsilon_k, \tag{2.11}$$

where ω_k is the outcome of interest in market k, and z_{kh} are control variables that may include period fixed effects, region fixed effects, skill-cell fixed effects, and/or any other variable that generates differences in wage levels across labour markets. Identification requires defining labour markets that are penetrated differently by immigrants and make before-after and across-groups comparisons to identify the effect.

This chapter estimates a similar regression for GDP per capita (and also some employment variables) defining labour markets as OECD countries. Dustmann et al. (2016) show in a unified framework that Eq.

(2.11) estimates a different native wage elasticity depending on the definition of labour market. In their expression, the spatial approach identifies how the overall inflow of immigrants affects native wages and employment of a given group. The focus on GDP per capita identifies yet a different parameter, described in Eq. (2.4).

The labour economics literature has estimated many versions of Eq. (2.11) using spatial variation. It is useful to discuss here the main empirical challenges and results they have encountered in order to define the empirical strategy to follow in this chapter. Seminal papers by Grossman (1982) and Borjas (1987) estimate elasticities from different production functions using Census data variation across Standard Metropolitan Statistical Areas (SMSAs) for 1970 and 1980, respectively. The common conclusion of their studies is that the elasticity of native wages with respect to immigration is very small (around −0.02). A similar conclusion is achieved by the majority of studies defining labour markets as metropolitan areas. One of the most influential papers in the literature, Card (1990), found very negligible effects of the large labour supply increase generated by the Cuban refugees that arrived during the Mariel Boatlift (in 1980) on the relative wages of Miami compared to other four control cities.[5] Other studies reached similar conclusions with different setups (LaLonde and Topel 1991; Altonji and Card 1991; Card 2001).

For years, economists have been trying to reconcile these results with the most simple demand and supply theoretical models that would imply that an increase in (homogeneous) labour supply should be associated with a decrease in equilibrium wages. Three types of empirical issues have been discussed as potential drivers of this result: endogeneity, spatial arbitrage, and measurement error.

The endogeneity concern arises because immigrants are more likely to settle in areas where labour market opportunities are more promising, and this can build a positive correlation between wage shocks and immigration that can bias the results. The estimation using panel data and controlling for permanent unobserved heterogeneity, as initiated by Altonji and Card (1991), is partially a solution, but is not enough, and an instrumental variables analysis is needed. Altonji and Card (1991) and Card (2001) propose a shift-share instrument that allocates aggregate inflows of immigrants in the United States into metropolitan areas based on the historical settlements of previous immigrants from the same country of origin. This approach has been as widely used in the literature as it has been criticized (e.g. see Borjas 1999, 2014). In particular, if economic shocks in a given region are persistent, the endogenous factors that attract immigrants today could be correlated with the factors that attracted immigrants in the past, which would break the exogeneity assumption.[6]

This chapter follows a different approach. In particular, it exploits the variation in costs of immigration across destination countries, summarized by distance, and the origin country-specific factors that drive individuals to move across countries, namely *push* factors such as wars, or political and economic conditions. These instruments are based on the so-called gravity equations, which are very often used in the international trade literature (e.g. Frankel and Romer (1999) use them to analyse the effect of trade on economic growth). In the economics literature, fewer papers have such gravity-based exogenous variation in a cross-country setting (Angrist and Kugler 2003; Llull 2008, 2011, 2018b; Ortega and Peri 2014a, b).[7]

Compared to the more standard gravity instrument (Frankel and Romer 1999; Ortega and Peri 2014a, b), there is an important difficulty that has to be circumvented. The estimation of a panel data model with fixed effects requires time variability of the instrument. In particular, fixed determinants (such as the distance between two countries) are collinear with country dummies and hence do not identify the desired effect. Likewise, push factors do not generate cross-destination variation and are collinear with time dummies. However, we can exploit the joint variation of these two different sources to find instruments that vary across destinations and over time. For example, a war in Syria pushes more people to Europe than to Australia. Put differently, a change in an origin country's living conditions does not equally affect all destination countries. Therefore, the variation from the interaction of a push factor and distance provides relevant exogenous variation that allows to identify the coefficients of interest.[8]

The estimation procedure is implemented in two stages. The first stage consists of a bilateral regression of push factors, distance, and their interaction (along with destination country and time fixed effects). In particular, the share of immigrants from country q in country i at time t, defined as $m_{iqt} \equiv I_{iqt}/N_{it} + I_{it}$, is given by:

$$
\begin{aligned}
m_{iqt} = {} & \beta_1 War_{qt} \times lnDistance_{iq} + \beta_2 PolityIV_{qt} \times lnDistance_{iq} + \beta_3 PolityIV_{qt}^2 \\
& \times lnDistance_{iq} + \beta_4 Pop_{qt} \times lnDistance_{iq} + \beta_5 PPP_{qt} \times lnDistance_{iq} \\
& + \beta_6 War_{qt} + \beta_7 PolityIV_{qt} + \beta_8 PolityIV_{qt}^2 + \beta_8 Pop_{qt} + \beta_9 PPP_{qt} \\
& + \beta_{10} \ln Distance_{iq} + \beta_{11} CommLang_{iq} + \beta_{12} Colony_{iq} \\
& + \beta_{13} Border_{iq} + \zeta_i + \upsilon_t + \epsilon_{iqt}, \qquad (2.12)
\end{aligned}
$$

where the different regressors are defined in Sect. 2.4. The second-stage estimation is a version of Eq. (2.11) in which the outcome is log GDP per capita, and where m_{it} is replaced by its predicted value from the first stage, namely $\hat{m}_{it} \equiv \sum \hat{m}_{iqt}$. In particular:

$$lny_{it} = \gamma \hat{m}_{it} + \eta_i + \delta_t + \varepsilon_{it}. \qquad (2.13)$$

An analogous version of this regression is estimated for different employment outcomes.

The second empirical issue that has been discussed in the literature is spatial arbitrage. In particular, if natives respond to the entry of immigrants into a local labour market by moving their labour to other areas, native wages are equalized across areas. Borjas (2006) finds that the measured impact of immigration on wages in local labour markets is attenuated by 40–60% for states and metropolitan areas respectively as a consequence of the native migration response. On the contrary, Card (2001) finds that intercity mobility rates of natives and early immigrants are insensitive to immigrant inflows. In the German context, Dustmann et al. (2017) find some evidence of geographical displacement, even though, in their context, "movement from and to non-employment is far more relevant than movement across areas" (p. 475). Borjas (2003, 2006), Cortés (2008), and Aydemir and Borjas (2011) estimate the skill-cell standard regressions for different geographical definitions of a labour market showing that the more locally is defined a labour market, the smaller are the effects that are estimated. Arbitrage is also a concern at the skill-cell level. In particular, results in Llull (2018a) indicate that native adjustments in skills are also important. Exploiting geographic variation, Lewis

(2011) shows that another important margin of adjustment is technology adoption, especially in the presence of capital-skill complementarities.

These concerns are mitigated in this chapter. One of the main advantages of the cross-country analysis is that countries are much more closed labour markets than cities: if there is a concern of native reaction to immigration by moving to other metropolitan areas as Borjas (2006) suggests, then such concern should vanish in a cross-country setting. Furthermore, the use of spatial variation alone reduces the concerns of arbitrage across skill groups. And finally, the analysis of effects on GDP per capita allows for indirect inference (through back-of-the-envelope calculations) on the intensity of adjustment of physical capital.

The third empirical issue is measurement error. Aydemir and Borjas (2011) show that the different spatial results at different levels of aggregation can be explained partially by attenuation bias due to measurement error in the computation of immigrant shares. Using restricted data from the Canadian census, these authors estimate larger negative effects of immigration relative to the elasticities obtained with public use samples. They also show that this conclusion can be extrapolated to the United States. However, as in Llull (2018b), this chapter's use of instrumental variables that are uncorrelated with this measurement error eliminates this concern. Furthermore, the accuracy of immigrant shares at the national level is much larger than at finer geographic definitions of the labour market.

The advantages of the cross-country analysis in tackling these three empirical issues come at some costs. First, it is difficult to conduct a direct analysis of wages due to the lack of cross-country wage data for a long period of time. Therefore, the analysis of the effects on productivity obtained from GDP per capita regressions cannot be complemented with a similar regression analysis on wages. Hence, conclusions for wages can only be extracted from back-of-the-envelope calculations based on Eqs. (2.7) through (2.10). Additionally, only the total stock of immigrants is observed, but not its disaggregation by educational levels or other categories, which prevents the estimation of the so-called mixed approaches, which combine spatial and skill-cell variation. It also prevents the use of production functions that are more comparable to those estimated by Borjas (2003) or Ottaviano and Peri (2012) for the United States or Manacorda et al. (2012) for the United Kingdom. Therefore, the conclusions below are somewhat harder to compare to those in the literature.

2.4 Data

Observing international migration is not easy. In general, origin countries do not collect statistics on the amount of people who leave the country, so the main source of data is at the destination. The fact that different countries count immigrants in different ways requires additional effort from the researcher to work on the comparability of the different statistics.

In recent years, several authors collected data from different sources to construct cross-country bilateral datasets (e.g. Docquier and Marfouk 2006; Özden et al. 2011; Llull 2016). Llull (2016) collected census-based data from National Statistical Offices of the 24 richest OECD countries.[9] The data contain stocks of immigrants by country of origin at 10-year frequency from 1960 to 2000. The purpose of that paper is to look at the determinants of bilateral migration. Therefore, the credibility of the estimates relies on the quality of migration data. Additionally, the paper estimates bilateral regressions with large amounts of observations. This chapter estimates cross-destination country regressions with few observations. Additionally, as the immigrant share is instrumented, measurement error is not as important as long as it is uncorrelated with the instrument, which is very plausible in this case. Under this premise, the database is extended to 5-year frequency. To this end, data from all destination countries that carry censuses every 5 years are included, and information from other sources like labour Force Surveys or, in recent years, small annual versions of censuses like the ACS in the United States are also added. For a small subset (21 country-time observations), the available census estimates are interpolated.[10]

Data are based on destination countries' censuses.[11] From each census, data on the stock of immigrants by country of birth or country of nationality are collected. The dataset contains information on stocks of immigrants from 188 countries of origin (sometimes in grouped categories) into each of the 24 listed OECD countries.[12]

This dataset has important advantages relative to other datasets in the literature. First, it covers 100% of stocks of immigrants in all of these destination countries, without imputations. Moreover, unlike some of the existing datasets (e.g. International Migration Database from the OECD), it contains data on stocks. For economic and statistical reasons, it is more attractive to work with stocks rather than flows: from an economics point of view, the marginal effects derived in Eqs. (2.4) and (2.7) through (2.10) are expressed in terms of immigrant shares. Econometrically, it has long been recognized that migration flow data are less reliable than stock data, because of the impossibility of evaluating emigration and return migration movements (Docquier and Marfouk 2006). Additionally, although censuses do not record all illegal immigrants, they do a much better job in counting them than issues of residence and work permits (especially when census data are physically collected directly at the dwelling). Finally, the dataset covers a wide time period (from 1960 to 2005).

There are a few comparability issues that are worth mentioning. They are unlikely to affect the analysis below because this source of measurement error is unlikely to be correlated with the instrument. First, the definition of immigrant is different across countries. Some countries define immigrants on the basis of the place of birth, while others base it on nationality. Although this may affect the comparison of stocks across destination countries, observations are likely to be comparable within countries, which provide the relevant variation since regressions include country fixed effects. Second, census dates vary across destination countries: roughly a half of them are carried in 0- and 5-ended years (1960, 1965, 1970, etc.) and the other half in 1- and 6-ended ones (1961, 1966, 1971, etc.). Dates are generally consistent, however, so the difference between two censuses is always of 5 or 10 years. The analysis below shows robustness to the date of measurement of the dependent variable.

Figure 2.1 shows the evolution of immigrant rates (i.e. stock of immigrants over population) across destination countries over the sample period. The same scale on the left axis is used in order to make the plots comparable. The level and slope of these curves is very different. The observed patterns are as follows: stable low-immigration countries (Korea and Japan), stable high-immigration countries (Australia, Canada and New Zealand), old immigration countries with a strong increasing trend (United States, Luxembourg, Switzerland, and the United Kingdom), old immigration countries with a slight decrease (Belgium and France), and new immigration countries (Spain, Italy, Austria, Greece, Portugal, and the Nordic countries).

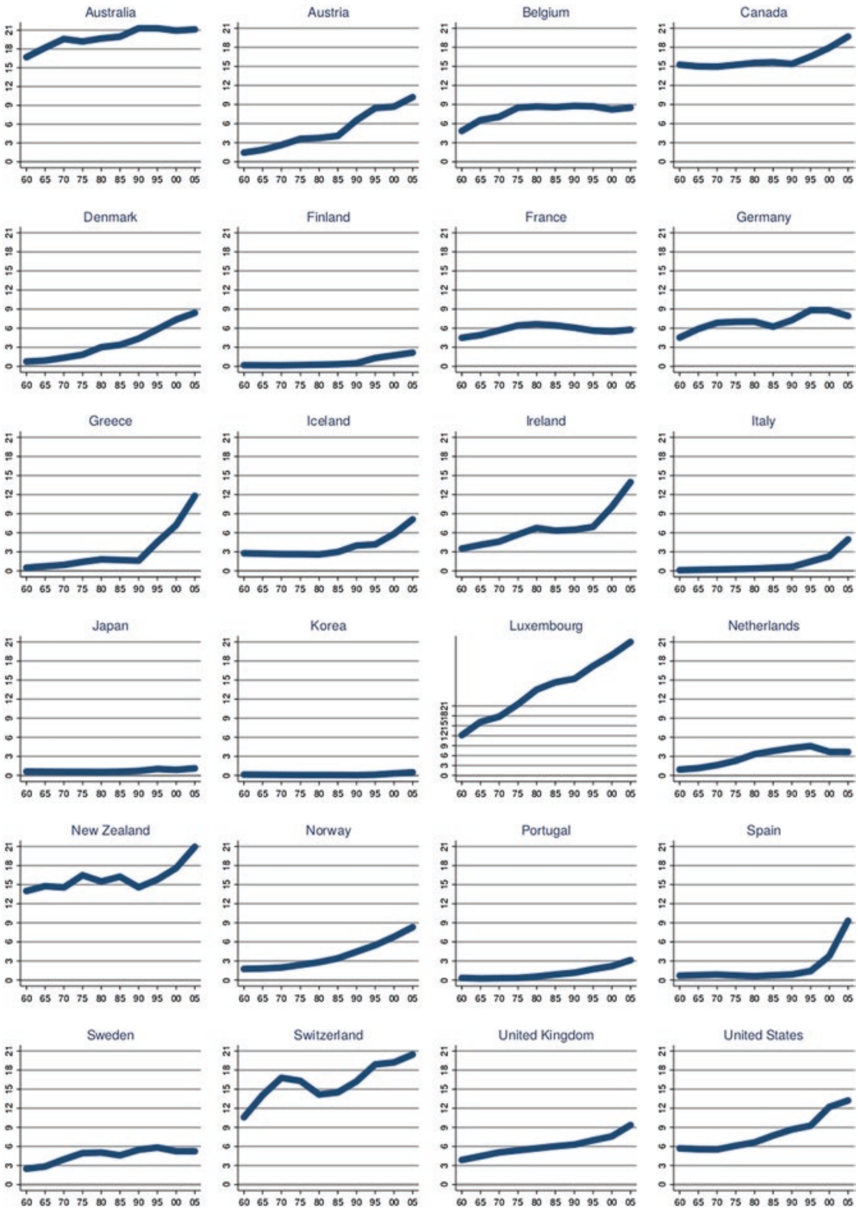

Fig. 2.1 Immigrant share (%) for a sample of OECD countries (1960–2005). Note: Black solid lines represent immigrant shares (in %), that is total stocks of immigrants in a given destination country over its total population. See main text for a data description. Immigrant share is plotted on the left axis, which is of common scale for all destination countries, ranging from 0% to 21% —it is compressed for Luxembourg due to its exceptionally large fraction of immigrants (40.3% in year 2005)

The different outcomes used in the second-stage regressions are obtained from different sources. GDP per capita comes from Penn World Tables 7.0 (PWT) and is measured at constant international dollars. Employment rates (total employment over population) and hours worked per worker come from the Total Economy Database (Conference Board). Unemployment rates come from OECD (Economic Outlook). For all dependent variables, different dates are used for different specifications (see Sect. 2.4).

Push factors are averaged over the period $t-5$ to $t-1$ and come from different sources. Four variables are considered: wars, Polity IV index, population, and purchasing power parity (PPP). The war variable is based on data from the Polity IV project (Center for International Development and Conflict Management 2006). This variable measures the fraction of months over the previous decade that the country was in any type of war. The autocracy-democracy index Polity IV comes from the same source. It ranges from −10 (autocracy) to 10 (democracy). Values of 0 indicate anocracy, some sort of instability and lack of control either by an autocratic or a democratic power. Population and PPP are obtained from PWT.

Distance variables include physical distance (great circle distance between the two capitals) and dummies for having a common language, a past colonial relationship, and a common border. Interactions with *push* factors are included only with distance. The distance variable is based on data from Rose (2004), extended to cover all the sample. The common language dummy was constructed using data by Alesina et al. (2003). A pair of countries is considered to share a particular language if that language is spoken by at least 10% of the population in each country of the pair. Those data are complemented with The World Factbook from the Central Intelligence Agency (2007). The colonial relationship dummy and the common border variable are also constructed using information from the CIA.

Finally, some of the regressions below control for trade (instrumented in the same way as migration). Bilateral trade data are obtained from Rose (2004) for 1960–1995 and from UN Comtrade for 2000 and 2005.

2.5 Estimation Results

This section presents the estimation results for the model and regressions presented in Sect. 2.3. Table 2.1 shows the estimation results from the first-stage regression (2.12). This regression is estimated with bilateral data on migrant stocks, described in Sect. 2.4. The data include 12,287 origin-destination-time observations, some of them representing grouped categories, which are weighted accordingly.

Overall, Table 2.1 shows a strong relevance of the instruments. The signs of the coefficients are also interpretable. The negative coefficient of the war interaction indicates that the effect of a war on migration (which is positive) is reduced when countries are far away. For example, the Syrian war pushes people to all countries, but more so to Europe than to Australia.

The Polity IV variable ranges from −10 (autocracy) to 10 (democracy), with intermediate values (around 0) representing societies where the central authority is weak or non-existent (anocracies). The findings in Llull (2016) suggest that anocracies favour migration, as risk-averse people have a dis-utility of living in such an unstable environment (individuals would also like to flee from autocracies, but migration is often more restricted in those contexts). To capture this non-linearity, Eq. (2.12) includes a quadratic on the index. Results confirm the findings in Llull (2016), and also show, through the interaction terms, that this quadratic relation is less strong for further away countries. Put differently, the fall of Gaddafi's autocratic regime in Libya and the subsequent situation of instability (anocracy) push more migrants to Italy than to the United States.

The last two variables used as push factors (population and PPP) are proxies for demographic and life quality measures. An increase in population increases the competition in the labour market, increasing, as a result, the likelihood of moving. This effect, however, is again mitigated by distance. PPP captures two different factors. From a long-run perspective, it is a measure of economic development of the origin country. Lower development levels are associated with larger gains from migration and, hence, larger migrant flows. Short-run (negative) shocks are a form of economic instability (e.g. hyperinflation, currency attacks, bad

Table 2.1 First-stage regression

Interactions:		
War x Log Distance	−0.806	(0.277)
Polity IV x Log Distance	−0.003	(0.007)
Polity IV^2 x Log Distance	0.005	(0.002)
Population x Log Distance	−0.111	(0.078)
PPP x Log Distance	0.309	(0.124)
Non-interacted terms:		
War	7.201	(2.387)
Polity IV	0.059	(0.059)
Polity IV^2	−0.036	(0.019)
Population	1.154	(0.652)
PPP	−2.718	(1.094)
Log distance	−0.978	(0.148)
Common language	2.970	(0.398)
Colony	0.351	(0.128)
Border	5.343	(0.704)
Observations	12.287	
Adjusted R^2	0.14	
F-statistic	18.46	
F-statistic (interactions only)	7.9	

Note: The regression includes destination country fixed effects and time dummies and it is estimated at the bilateral level (destination-origin-year). Demographic and political variables refer to origin countries at a point in time. Geographic variables refer to a country pair and are constant over time. F-statistic tests the joint significance of all coefficients. An F test for the joint significance of interactions is also reported. Robust standard errors in parentheses

economic policies, etc.) and are also positively associated with migration. Once again, however, this effect is larger for countries that are nearby, and it gradually decreases with distance.

Table 2.1 also shows results to tests of joint relevance of the instruments. The F-statistic for the joint significance of all excluded coefficients (all the coefficients except time and country dummies) is relatively large (18.6), clearly rejecting the null hypothesis of insignificance of all coefficients. The F-statistic for the joint significance of the interaction terms (the only subset of excluded instruments that remains non-collinear with the time and country dummies after aggregation) is slightly smaller (7.9), but still well above the Stock and Yogo's (2005) threshold for rejection of weak instruments if a maximum of 5% bias (towards Ordinary Least Squares, OLS) is allowed in the second stage (at the 5% significance level).

Table 2.2 Effect of immigration on GDP per capita

	Without trade		With trade	
1. Baseline (census date)	−2.061	(1.125)	−2.078	(1.125)
2. All in the same year	−1.958	(1.121)	−2.038	(1.263)
3. Four-year average	−1.947	(1.084)	−2.066	(1.228)
4. Least squares	−0.023	(0.720)	−0.012	(0.756)

Note: All regressions include country fixed effects and time dummies. All specifications estimated by 2SLS (see first-stage regression in Table 3.1). Right column includes trade as a control variable (instrumented with a bilateral first stage using the same instruments). Dependent variable: Log of GDP per capita at constant international dollars. All coefficients correspond to immigrant rate. Immigrant rate measured at Census dates (either at 1- and 6-ended years or at 0- and 5-ended ones). Specification 1 (baseline) measures the dependent variable at Census date as well. Specification 2 measures the dependent variable at 1- and 6-ended years. Specification 3 includes a 4-year average. And Specification 4 is the OLS estimate of the baseline specification. Number of observations: 240. Robust standard errors in parentheses

Table 2.2 presents the second-stage results for Eq. (2.13). All specifications are instrumented using the constructed instrument based on the aggregation of the first-stage regression in Table 2.1, except for the last row that presents OLS estimates. The baseline specification introduces the dependent variable at the corresponding census date, that is at the exact year immigrant share is observed. Results suggest an important effect of immigration on income per capita. In particular, 1 percentage point increase in the immigrant share reduces wages by 2%. Although precision is low, due to the small number of observations (24 × 10 = 240 obs.), this estimate is significantly different from zero. The structural interpretation of the results is discussed in the next section.

Two additional specifications are estimated to check the robustness of the results to the different measurement issues described in Sect. 2.4. The first of these two specifications, presented in the second row, adjusts the measurement of the dependent variable to the exact census date, as opposed to the 0- or 5-year-ended date that the census is assumed to represent. The second one, in the third row, replaces it by a 4-year average. In both specifications, results are virtually unchanged by these changes, which suggests that the timing of the data is unlikely to be a source of concern.

Another concern is that the instruments may be correlated with the error term because of their correlation with international trade. Indeed, Frankel and Romer (1999) or Ortega and Peri (2014a, b) use geographic instruments (in levels, not their interaction with distance) to instrument for trade. To account for this concern, the specifications in the second column of Table 2.2 reproduce the same regressions controlling for trade, which is also instrumented by the same variables. Point estimates are again virtually unchanged (even though precision falls in some cases). Therefore, results are robust to controlling for trade.

The last row of Table 2.2 presents the OLS regression coefficients. Point estimates are virtually zero, indicating an important positive bias. This bias is motivated for the non-random allocation of immigrants across destination countries. For example, Southern European countries were doing poorly compared to OECD countries from 1960s to 1980s and had virtually no immigrants, but their rapid convergence to the income levels of their European partners is associated with a drastic increase in the stock of immigrants in most of them (see Fig. 2.1).

Table 2.3 provides an analysis of the employment and labour supply effects. In particular, results are presented for three different outcomes using the same instrument and specifications. Each panel presents the results for a different outcome: employment rate, log hours worked, and unemployment rate.

Results in Table 2.3 suggest that 1 percentage point increase in the immigrant share reduces employment by about 0.8–0.9 percentage points, reduces average hours worked (conditional on working) by 1.2%, and increases unemployment by 0.5–0.6 percentage points. The latter is very consistent with the findings of Angrist and Kugler (2003) using similar sources of variation. The results on hours worked are in line with Borjas (2003), who finds a significant reduction in hours worked as a consequence of immigration. In all three cases, OLS estimates are considerably biased towards less severe effects of immigration. As in the case of wages, this bias indicates that immigrants migrate to the countries that offer better work conditions. The large magnitude of such biases motivates the use of instrumental variables in the estimation.

Table 2.3 Effects of immigration on employment, hours worked, and unemployment

	Without trade		With trade	
A. Employment rate (employed/population)				
1. Baseline (census date)	–0.888	(0.268)	–1.002	(0.256)
2. All in the same year	–0.849	(0.265)	–0.966	(0.255)
3. Four-year average	–0.754	(0.251)	–0.878	(0.240)
4. Least squares	0.601	(0.151)	0.579	(0.157)
B. Log hours worked				
1. Baseline (census date)	–1.281	(0.366)	–1.321	(0.403)
2. All in the same year	–1.267	(0.360)	–1.315	(0.397)
3. Four-year average	–0.450	(0.341)	–0.291	(0.347)
4. Least squares	–0.581	(0.166)	–0.593	(0.176)
C. Unemployment rate (unemployed/labour force)				
1. Baseline (census date)	0.550	(0.150)	0.566	(0.167)
2. All in the same year	0.512	(0.146)	0.541	(0.165)
3. Four-year average	0.513	(0.139)	0.578	(0.152)
4. Least squares	–0.117	(0.094)	–0.127	(0.097)

Note: All regressions include country fixed effects and time dummies. All specifications estimated by 2SLS (see first-stage regression in Table 3.1). Right column includes trade as a control variable (instrumented with a bilateral first stage using the same instruments). Dependent variables: employment rate, log hours worked, and unemployment rates. All coefficients correspond to immigrant rate. Immigrant rate measured at Census dates (either at 1- and 6-ended years or at 0- and 5-ended ones). Specification 1 (baseline) measures the dependent variable at Census date as well. Specification 2 measures the dependent variable at 1- and 6-ended years. Specification 3 includes a 4-year average. And Specification 4 is the OLS estimate of the baseline specification. Number of observations: 240. Robust standard errors in parentheses.

2.6 Structural Interpretation and Wage Effects: Some Back-of-the-Envelope Calculations

This section provides a set of back-of-the-envelope calculations that allow for a structural interpretation of the results and for inference on wage effects on natives and on immigrants. Table 2.4 summarizes the main results of this exercise. The top panel describes the main assumptions and data inputs used in the calculation. The central panel provides the parameters implied by these assumptions and the results in Tables 2.2 and 2.3. Finally, the bottom panel shows the implications for wage effects of immigration.

Table 2.4 Back-of-the-envelope calculations: Wage effects

Inputs	
Native-immigrant wage gap (Adserá and Chiswick 2007)	−0.401
Average immigration rate (data)	0.070
Elasticity of substitution (Ottaviano and Peri 2012)	20.000
Implied parameters	
Inverse elasticity of substitution (ρ)	0.950
Relative native efficiency (θ)	0.629
Capital share (α)	0.300
Inverse elasticity of capital supply (λ)	0.520
Wage effects	
Natives	−0.702
Immigrants	−1.466
Wage effects (netting out employment effects)	
Natives	0.126
Immigrants	−0.638

Note: Author's calculations using the expressions in the text and the inputs listed in the first panel

Borrowing from the findings of Ottaviano and Peri (2012), we fix the elasticity of substitution between natives and immigrants to 20, which implies that $\rho = 0.95$. Using this parameter, an estimate of θ can be obtained from the comparison of native and immigrant wages. Dividing Eq. (2.6) by the analogous expression for immigrant wages yields:

$$\frac{w_{it}^{(N)}}{w_{it}^{(I)}} = \frac{\theta}{1-\theta}\left(\frac{1-m_{it}}{m_{it}}\right)^{\rho-1} \tag{2.14}$$

Using data for EU-15 countries, Adserà and Chiswick (2007) estimate a native-immigrant (log) wage gap of 0.401 (Table 5.1). Substituting this estimate in the left-hand side of Eq. (2.14), and for an average immigrant rate of 7% (from the data), the implied value for θ is 0.629. Following many papers in the literature (e.g. Borjas 2003), we fix the capital share to $\alpha = 0.3$. Given all these parameters, we recover λ as the only unknown in an equation that equates the right-hand side of Eq. (2.4) to the baseline estimate in Table 2.2. The resulting value is 0.52. This value is different from the two extremes that have been considered in the literature, and suggests, as in Lewis (2011), that it is very important to account for capital adjustments in understanding labour market impacts of immigration.

Given these parameter values, the bottom panel provides simulated values for Eqs. (2.7) through (2.10). The first two values measure the wage semi-elasticity to immigration not taking into account labour supply adjustments. Therefore, this result implicitly averages in the "zeroes" for the individuals that stop working because of the extra immigration. The estimated semi-elasticities are obtained to be roughly −0.7 and −1.5. These values imply that a 1% increase in immigration reduces native wages by 0.7% and immigrant wages by 1.5%. These results are in line with the results obtained with the skill-cell approach (e.g. Borjas 2003; Aydemir and Borjas 2007; Llull 2018b) and, with the spatial approach, only with those computed for the groups of less skilled natives (e.g. Altonji and Card 1991; Dustmann et al. 2013).

The calculations obtained for Eqs. (2.9) and (2.10) account for the effect on employment, and, therefore, exclude the individuals that no longer work in the presence of immigration. The estimated effect on the employment rate in Table 2.3 is −0.888 (which is interpreted as Δ_E). Given this value, the implied wage correction is 0.828, which implies that the predicted effects for natives become slightly positive (0.126), and the ones for immigrants stay negative (−0.638). These results imply that the wages of the natives who remain employed after a 1% increase in the share of immigrants in the population increase by 0.1%, whereas those of the immigrants that stay at work decrease by 0.6%.

These results provide evidence of downward wage pressure (even on natives) after immigration. This is so because the capital supply elasticity is estimated to be less than infinity. This result is in contrast with most of the results in the literature using the spatial approach, which tends to find a negligible effect. This discrepancy can be the result of several factors. First, the spatial arbitrage (e.g. Borjas 2006) is unlikely to operate at the cross-country level. Second, metropolitan areas are likely to be small open economies, whereas countries are more likely to influence the capital markets. And third, the attenuation bias generated by measurement error (Aydemir and Borjas 2011) is unlikely to apply here both because of the higher accuracy at the national cross-country level and also because the instrumental variables used in this chapter are likely uncorrelated with the measurement error in the computation of immigrant shares.

2.7 Conclusions

This chapter provides a cross-country analysis of the impact of immigration on productivity. In particular, it analyses the effect of immigration on GDP per capita, the employment rate, hours worked, and the unemployment rate using aggregate variation across OECD destination countries. The analysis exploits exogenous variation from the interactions of push factors at origin and distance between origin and destination countries. The push-distance interactions provide relevant and arguably exogenous variation that allows for the identification of the results. Second-stage regression results suggest that 1 percentage point increase in the share of immigrants in the population reduces the country's GDP per capita by 2%. Furthermore, employment effects are also important: a one point increase in the share of immigrants reduces the employment rate by 0.888 percentage points and average hours worked by those individuals who stay employed by 1.28%, and increases the unemployment rate by 0.55 percentage points.

In order to structurally interpret the results, this chapter provides an analytical framework based on a simple production function. This framework allows for back-of-the-envelope calculations that also shed light on the predicted effects on wages or the marginal productivity of labour. These calculations suggest that the capital supply elasticity is not zero, but also not infinite. This result implies that immigration increases labour market competition because the increase in labour supply is not compensated by a large enough increase in the supply of capital. Given the estimated coefficients, the semi-elasticity of native wages to the immigrant share is estimated to be −0.7 if the extensive margin of labour supply is ignored (i.e. if we average in the zeros of natives who stop working) and 0.12 if we compute it only for the individuals who remain at work. Thus, the effect of immigration is large and negative for some natives (those who lose their jobs), and slightly positive for others (those who manage to keep them). Consistent with the literature and with theoretical predictions, the effect of immigration on the wages of immigrants is unambiguously negative.

These results are only a first step towards stronger policy implications. In particular, it would be interesting to understand whether the large effects on employment are driven by institutions, as suggested by Angrist and Kugler (2003). Furthermore, more freedom of capital mobility, and the recent access to the capital markets of large countries, such as China, may increase the capital supply elasticity, which could reduce the negative effects on GDP per capita. Finally, it would be useful to study the role of policies that redistribute the gains of those who benefit from immigration (e.g. the capitalists or the immigrants themselves) to those whose labour market prospects are negatively affected.

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Notes

1. See Borjas (2003), Ottaviano and Peri (2012), Dustmann et al. (2013), and Llull (2018a, b) among many others.
2. See Llull (2011) for another version that has been cited in the literature.
3. A notable exception is provided by some specifications of Peri and Sparber (2009), which instead use distance to leverage inflows of Mexican workers across different US states.

4. Borjas (2013) also discusses, in a theoretical framework, the importance of the capital supply elasticity in predicting theoretically the effects of immigration on wages.
5. Borjas (2017) and Borjas and Monràs (2017) revisited this and other natural experiments and disputed some of the results.
6. Borjas (2003) introduced the skill-cell approach, which defines labour markets in terms of skills, rather than spatially. That paper argues that, even though endogeneity is still a potential concern, it is less so than in the spatial approach. Llull (2018b), using exogenous sources of variation that are in a similar spirit to those explained below, shows that endogeneity is also a concern in the skill-cell approach.
7. Beyond its use as instruments, gravity-based migration models have been popularized in the migration literature. Beine et al. (2015) provide a comprehensive review of this literature.
8. Llull (2016) estimates a model in which the importance of income gains in determining migration are heterogeneous between country pairs. Angrist and Kugler (2003) interact dummies for the different phases of Balkans War with distance as an instrument to analyse the consequences of immigration on employment in Europe. Ortega and Peri (2014a, b) interact push factors with immigration laws as an instrument in analysing the effect of immigration on different production factors.
9. These countries include Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany, Greece, Iceland, Ireland, Italy, Japan, Korea (Rep.), Luxembourg, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, United Kingdom, and United States.
10. In some cases, the data are grouped for several origin countries. The extreme case is when only the total stock of immigrants is observed. Llull (2016) presents a wide discussion on the importance of this issue. The implication for the present study is that some observations from the first-stage regression enter as a group and are accordingly weighted (using the number of countries in the group as the weight). The instruments are grouped consequently. The asymptotic properties of the second-stage estimator are unaffected by this issue.
11. Nordic countries replaced their censuses in 1970s and 1980s for continuous population registers.
12. These countries include all Member States of United Nations except Andorra, Liechtenstein, Monaco, Myanmar, Marshall Islands, Nauru, San Marino, Timor-Leste, and Tuvalu (none of them are available in

Penn World Tables). Additionally, they include the dependent territories of Taiwan, Macao, Hong Kong, Bermuda, and Puerto Rico. Netherlands Antilles and Serbia and Montenegro are considered as sole countries, even though Montenegro gained its independence from Serbia in 2006 and the Netherlands Antilles, dependent territory from the Netherlands, dissolved in 2010 into Curaçao, Sint Marteen, and three special municipalities.

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