The Substitutability of Immigrant and Native Labor: Evidence at the Establishment Level

Raymundo M. Campos-Vazquez*

JOB MARKET PAPER

November 2008

University of California, Berkeley
Department of Economics

Abstract

A key issue for understanding the impact of immigration on native labor market opportunities is the degree of substitutability between immigrants and natives. If immigrants are perfect substitutes for natives, each newly hired immigrant displaces one native. If immigrants and natives are imperfect substitutes, however, the displacement effect can be smaller. In this paper, I use detailed establishment-level data from Germany to study the short- and longer-run displacement effects of increased immigrant hiring by firms after the fall of the Iron Curtain in 1989. I compare employment trends at firms in the same local labor market that either hired or did not hire immigrants using both a matching approach and an instrumental variables strategy that exploits pre-existing immigrant job networks. The empirical results from both approaches show statistically significant but relatively modest displacement effects. Over a 1-2 year horizon, hiring one additional immigrant displaces roughly 0.3 native workers. Over a longer (3-4) year horizon the displacement effects are smaller, and insignificantly different from zero. I show that these results imply an elasticity of substitution between immigrants and natives of between 10 and 15. Perhaps surprisingly, the firm level evidence is consistent with previous estimates based on local and national labor market comparisons.

*E-mail: rcampos@econ.berkeley.edu. Address: University of California, 508-1 Evans Hall, Berkeley, CA 94720-3880. I am extremely grateful to the Institute of Employment and Research (IAB) in Nuremberg, Germany for data access and generous financial support. The Center of Labor Economics and the Institute of Business and Economic Research at UC Berkeley also provided financial support. I thank David Card and Emmanuel Saez for advice and guidance during the completion of this paper. I thank comments by Eva Arceo-Gomez, Monica Deza, Daniel Egel, Patrick Kline, Enrico Moretti, Kevin Stange and Andrea Weber. All remaining errors are my own.

1 Introduction

Despite nearly two decades of research, there is no clear consensus on the degree to which increased immigration harms the labor market opportunities of natives.[1] A key unresolved issue is the degree to which immigrant and native labor are substitutable in production. While early theoretical models (Johnson, 1980) treated immigrants and natives as perfect substitutes, or perfect substitutes conditional on observed characteristics (Borjas, 2003) recent studies have suggested that immigrants and natives may be imperfect substitutes (Ottaviano and Peri, 2006; Manacorda et al., 2006). Even a modest degree of imperfect substitutability can substantially lessen the implied impacts of immigrant inflows on native opportunities, while concentrating more of the impact on immigrants themselves (Ottaviano and Peri, 2006).

In this paper, I present new evidence on the degree of substitutability between immigrants and natives, based on detailed establishment-level data from Germany during the period from 1986 to 1995. After the fall of the Iron Curtain and the outbreak of the Balkan War, the former regions of West Germany received a massive inflow of immigrants (approximately 3 million people). These immigrants settled disproportionately in a few areas, leading to substantial increases in the availability of labor, and ultimately in the employment share of non-native workers.

I use two complementary approaches to measure the establishment-level effects of hiring immigrant labor on native employment. One is a simple matching strategy. To deal with the obvious endogeneity problem that arises because firms that are growing are more likely to hire both immigrants and natives, I condition on a wide variety of observable characteristics (including industry-specific and local labor market trends), as well as previous employment growth rates. The alternative approach is an instrumental variables strategy. Specifically, I use the fact that newly arriving immigrants were more likely to be hired by establishments that had some existing immigrant employees. This "immigrant job network" instrument is similar in spirit to the "city enclave" instrument used in previous research (e.g. Altonji and Card, 1991) but focuses on differences within the same local labor market, and is therefore orthogonal to local labor market demand shocks that potentially confound the city enclave instrument.

Establishment level data enables significant improvement over previous approaches to estimate the effects of immigration.[2] First, establishment level data presents first hand evi-

[1] See the literature reviews in Borjas (1999), Longhi et al. (2005, 2006), and Okkerse (2008).

[2] Identifying the causal effect of immigration on labor market outcomes is complicated by the endogeneity of immigration since immigrants decide where and when to move based in part on labor market conditions. To address this, previous research has estimated the effect of immigration by comparing labor market outcomes

dence to directly test the belief that immigrants "steal jobs" and worsen economic outcomes for natives. Second, this data allows to shed light on the specific mechanisms through which firms adapt to changes in immigrant employment. For instance, firms may layoff natives, decrease their hiring rate, and/or adjust wage schedules at the firm level. And third, firm level data allows to address two main criticisms of previous approaches. Using a large sample of firms allows to control for shocks that affect all firms equally within a city in a given year, such that results are robust to city-level endogenous shocks. Moreover, with firm level data it is possible to compare the employment growth rate of firms which change immigrant employment shares against those that did not, reducing the concerns over the absence of a counterfactual. To my knowledge, my study along with Malchow-Møller et al. (2007) are the first in investigating the effects of immigration on specific firms.

In order to understand the impacts of immigration at the firm level, I develop a model that relates the effect of immigration on employment outcomes to structural parameters in a firm level production function. Based on wage rigidity in Germany, I assume that immigration does not affect natives' wages but may affect native employment.[3] Depending on the structural parameters of the production function, an increase in immigrant employees will lead to different effects for native workers. For example, if immigrants compete substantially with natives (i.e. they are close to perfect substitutes), we expect that an increase in immigrant employment would not affect total employment as each immigrant worker would displace a native worker. On the other hand, if immigrants do not compete with natives then an increase in immigrant employment will just add to total employment with no repercussions for native workers.

Using a balanced sample of establishments for the 1986-1995 period, I implement two identification strategies to estimate the impact of immigration on employment outcomes. First, I construct an instrumental variable to control for the endogeneity of immigrant employment at the firm level. In particular, I interact the share of immigrants employed in a firm in a previous year with the change in total immigration within that firm's city. If

across cities (Card, 1990, 2001; Grossman, 1982; Hunt, 1992) or within skill groups across time (Borjas, 2003; Manacorda et al., 2006; Ottaviano and Peri, 2006). Both approaches have weaknesses: unobserved demand shocks and internal migration may bias the former (Borjas, 2003) while the latter lacks a clear counterfactual (Card, 2005).

[3]Previous empirical findings in Germany are consistent with this hypothesis. Pischke and Velling (1997) use population and employment administrative data across local labor markets and find little effect of immigration on natives' unemployment rates for the period 1985-1989. They claim that given wage rigidity in Germany, the effect of immigration is in quantities. Bonin (2005) analyzes time series of employment and wages by education and experience. He finds that a 10 percent increase in the share of immigrants after 1990 increased the unemployment rate by 1.5 percent, a small negative effect. Glitz (2007) estimates the impact of Ethnic Germans immigration in the second half of the 1990s on labor market outcomes. After 1996, the German government decided to locate Ethnic Germans across Germany. He concludes that 10 more Ethnic migrants displaced up to 4 native workers. Both Bonin and Glitz find no effect of immigration on wages.

immigrants locate in cities with large networks, immigrants may be more likely to find jobs through their networks in firms which previously employed immigrants. The identifying assumption is that unobserved shocks in demand for a firm's output are uncorrelated with that firm's past employment decisions. I include a full set of city-year, industry-year and firm fixed effects to account for macroeconomic shocks and firm-specific employment decisions. I split the sample in different ways in order to counteract both the effect of the inflow of Ethnic and Eastern Germans during the reunification period 1989-1990.[4]

Second, I use a propensity score matching approach in order to compare employment outcomes between firms that changed and did not change immigrant employment. The matching estimator is implemented separately for each year and by region. As this approach relies on different identifying assumptions, it functions as a robustness check for the instrumental variable results. These two approaches confront three main challenges to examining immigration effects during the fall of the Iron Curtain period: the endogeneity of immigrant employment both across cities and at the firm level, and the immigration from Ethnic and Eastern Germans to West Germany during the reunification period.

The increase in immigration after the fall of the Iron Curtain led to a displacement of native jobs. Both the instrumental variable and matching approach show a displacement effect of 2-4 natives jobs for every 10 new immigrants jobs. The result is robust to modifications in the sample and the instrumental variable. Both identification strategies show that most of the displacement effect is concentrated in the short run. However, this effect decreases over time so that three to four years later no significant native displacement is observable. Moreover, firms that increase the number of immigrants decreased the wage of immigrants themselves suggesting a pattern of imperfect substitutability between natives and immigrants. According to the theoretical model I develop, the elasticity of substitution between natives and immigrants is close to 15. Surprisingly, this estimate is close to previous estimates based on local and national labor market comparisons. In sum, West German establishments adjusted to immigration through lower native employment levels and lower immigrant wages in the short run while in the medium term only immigrant wages are affected.

The structure of the paper is as follows: Section 2 describes the immigration history in Germany after the Second World War, particularly during the period around the fall of the Berlin Wall. Section 3 describes the theoretical model that relates the effect of immigration

[4]By the prevailing law at the time, foreign-born citizens with German ethnicity were considered German nationals. After the Fall of the Berlin Wall many Ethnic Germans immigrated, the peak immigration was in 1990 with a gross inflow of 400,000 Ethnic Germans. This complicates the identification strategy because during the reunification period 1989-1990 native employment was growing independently of immigration, which leads to the finding that immigrants did not displace natives.

on employment outcomes. This section will provide the framework in order to understand the magnitude of the effects. Section 4 describes the data and cleaning procedure. Section 5 describes both empirical strategies and the identification assumptions. Section 6 and 7 describe the findings and robustness tests. Section 8 interprets the results at the aggregate level. Section 9 includes the effects of immigration on average wages at the firm level. The final section summarizes the conclusions and future avenues of research.

2 Immigration in Germany

The last fifty years have transformed Germany into an immigration country.[5] Figure (1) plots the gross and net inflows of immigrants to Germany since 1955. Immigrants started to arrive in the country after the government signed Guest Worker programs with countries from Southern Europe and the Mediterranean (Italy, 1955; Spain and Greece, 1960; Turkey, 1961; Morocco, 1963; Portugal, 1964; Yugoslavia, 1968). Before 1973, most foreigners were guest workers, but in 1974, after stopping the recruitment of foreign workers the immigration policy was changed towards a family reunification policy. Thus, from 1973 to the late 1980s the inflows of foreigners were driven mainly by family reunification purposes.

After the fall of the Berlin Wall on November 1989 the number of foreigners surged. These foreigners were coming mainly from Eastern Europe and can be divided in two broad categories: Ethnic Germans, which in the official statistics and in my data are not considered foreigners but natives, and refugees.[6] Figure (2) compares the net inflows of natives and immigrants from abroad during the period from 1983 to 1998.[7] The net inflows of Ethnic Germans are close to zero before 1988, but they increase substantially afterwards. The peak year in immigration of Ethnic Germans is 1990 with a net inflow close to 300,000. The German government restricted further inflow of Ethnic Germans in 1991 by forcing them to obtain a residence permit in their own country and later in 1992 also by setting an annual quota close to 200,000 individuals and limit immigration to those individuals living in the

[5] Excellent references for the study of immigration in Germany in the last 50 years are Chin (2007), Göktürk et al., eds (2006), Herbert (1990) and Siebert (2003). For an economic approach, see Liebig (2007) and Zimmermann et al. (2007).

[6] Ethnic Germans are those individuals with German background. In the 1980s and 1990s, German Law defined nationality in terms of ancestry (origin). After the Second World War with the new borders in Europe, many individuals with German origin did not return to Germany. For the period before 1992, these individuals and their families were granted German nationality if requested. After 1992, stricter rules applied. More details can be found in Glitz (2007) and Zimmermann et al. (2007).

[7] The inflow of Ethnic Germans is usually shown in gross terms and the inflow of immigrants in net terms. For consistency reasons, I include the net inflow of both types of migrants. However, the net inflows of Ethnic Germans are calculated as the the net inflow of German nationals, that is the net change of Germans in the population. Net migration is a more accurate measure than gross migration for both foreigners and nationals. For simplicity, I call the net migration flows of natives as the net migration flows of Ethnic Germans.

former Soviet Union (Liebig, 2007).[8]

Although the surge in Ethnic Germans is substantial, the inflow of immigrants is even higher, especially for the 1991-1993 period as shown in Figure (2).[9] There is a surge in the inflow of immigrants in 1992, most of them as asylum seekers or as refugees. For example, in 1992 Germany received more asylum applications than all the rest of the OECD countries combined (Liebig, 2007). Asylum seekers and refugees were coming mainly from Yugoslavia, Romania, Turkey, Bulgaria and Asian countries. Germany received 122,000 asylum seekers from former Yugoslavia after the start of the Balkan War in 1992. Following the revolution in Romania, Germany received 100,000 Rumanians in 1992, which amounts to 25 percent of all asylum applications. However, the surge in refugees inflows stopped in 1993 after the federal government prohibited asylum to individuals that had been present in a safe third country prior to their trip to Germany.

Germany has strict laws to allow recent immigrants to work. Hence, although immigration inflows in terms of population are large, it could be possible that the shock of immigration in terms of employment is small.[10] Figure (3) analyzes the heterogeneity in immigrant inflows on the share of immigrants in total employment across specific states. During the 1987-1993 period the share of employed immigrants increased by 2 percentage points in West Germany (excluding Berlin), but it was higher in high inflow states like Bavaria where the share of employed immigrants increased by 3 percentage points. This was a substantial increase in the share of immigrants in employment. For example, Hunt (1992) shows that the increase in labor participation among French repatriates from Algeria increased close to 3 percentage points in the two regions that received the largest number

[8] The Appendix includes more statisitics on the inflow of immigrants.

[9] For simplicity, I define immigrants as individuals without German nationality.

[10] German Immigration Law is complex and has been changing since 1990 (Liebig (2007) and Zimmermann et al. (2007)). Individuals within the European Union (EU) are allowed to work freely. Immigrants from outside the European Union with resident permits (i.e. a legal resident for five years) or eligible for residence (i.e. a resident for eight years) do not need to apply for a work permit. Nonetheless, non-EU immigrants, who are nor residents or eligible for residence, need to apply for a work permit in the local Labor Office to be authorized to work. Since the surge in immigration after 1990 was mainly driven by refugees, asylum seekers and family reunification, some of these immigrants needed to apply for a work permit; hence it is important to understand the requirements to get such authorizations. Before 1990, permits were discretionally given. After the Immigration Law in 1990, these permits were granted only under specific conditions, and a work permit could be denied if the local labor office believed the job could be done by a resident (Native or immigrant). Migrants with family reunification status could obtain a work permit immediately or after a one year waiting period. Asylum seekers were not allowed to work before 1990. After 1990, with the new Immigration Law, asylum seekers could work but labor market testing applied. Between 1998 and 2000 asylum seekers were not allowed to work at all; but starting in 2001 they can work again subject to market testing and also to a one year waiting period. As opposed to asylum seekers, recognized refugees, which include Civil War refugees from Yugoslavia and other refugees recognized under the Geneva Convention, are allowed to work immediately. Many asylum seekers from Yugoslavia received work permits during the surge in immigration according to Angrist and Kugler (2003), p. F312.

of repatriates.[11] However, the increase in the share of immigrants is not as large as the one experimented by Miami after the Mariel boatlift in 1980. Card (1990) reports an increase of 7.6 percentage points in the share of Cubans in the labor force between 1979 and 1981. On his part, Peri (2007) reports that immigrant's share in the population of California increased by 5 percentage points between 1990 and 2000.

3 Theoretical Model

The goal of the model is the analysis of the effect of immigration on natives' employment outcomes using a firm-level production function. The model builds on the empirical and theoretical applications by Card (2007), D'Amuri et al. (2008), Grossman (1982) and Johnson (1980). While these papers analyze the effects of immigration *across* local labor markets or at the *aggregate* level, I analyze the effects of immigration *within* local labor markets.

Johnson (1980) is one of the first studies that developed a theoretical framework for the short run analysis of immigration on total employment and displacement. He predicts the effect of immigration on total employment based on labor supply and labor demand parameters. Card (2007) and D'Amuri et al. (2008) formalize Johnson's insight. Using variation across cities or skills between two points in time, they relate the effect of immigrant employment growth as a percent of total previous employment ($\frac{\Delta I}{L_{-1}}$) to total employment growth ($\frac{\Delta L}{L_{-1}}$) in regressions similar to:

$$\frac{\Delta L}{L_{-1}} = \beta \frac{\Delta I}{L_{-1}} + u \tag{1}$$

If immigrants displace native workers from their jobs, then the coefficient β should be close to zero (in the case of complete displacement $\beta = 0$). In contrast, if immigrants just add to the labor force (i.e. there is no displacement) then β should be equal to one. For example, a 1 percent increase in the labor force driven by immigrants can cause a 1 percent increase in the labor force (no displacement) or a 0 percent increase in the labor force (full displacement). Equation (1) can be interpreted easily; for example, a parameter $\beta = 0.5$ implies that a 1 percent increase in immigrants in the labor force causes an increase in the labor force by 0.5 percent, implying that for every two immigrants employed one native was displaced. Card (2007) and D'Amuri et al. (2008) do not relate the estimate β to structural parameters. The model I present below relates structural parameters from a firm-level production function to the displacement effect β.

[11] Given lack of data, Table 1 in Hunt (1992) includes only participation rates of repatriates in 1968 and the proportion of repatriates among the population in 1962.

The theoretical model is simplified by two stylized facts. First, Germany shows more rigid wages than the United States. If this is correct, the impacts of immigration should be concentrated on displacement rather than wages (Pischke and Velling, 1997). I assume natives' wages are rigid and that there is some unemployment in equilibrium.[12] However, I assume immigrant wages are flexible (Grossman, 1982). This could be driven due to differences in union coverage or simply because firms' owners believe they should not follow the wage agreements for immigrants. Figure (4) shows evidence in favor of these assumptions. The figure shows the percent change in average wage across cities with the corresponding percent change in immigration between 1989 and 1995. Natives' wages are fairly constant across immigrant inflows, but immigrants' wages are more disperse suggesting more flexibility in the immigrant wage than in the native wage. Second, I use the heterogeneity in immigrant employment to calculate the effect of immigration on native employment. In particular, *within* local labor markets I compare employment growth in firms with change in immigrant employment against employment growth in firms with no change in immigrant employment. The model will assume that the first set of firms hires both natives and immigrants, whereas the second set of firms employs only natives. If immigrants take native jobs we expect to see a lower growth rate of native employment in firms with both immigrant and native employees than in firms with only native employees.

Given heterogeneity in immigrant employment across establishments, I model the impacts of immigration for two different types of establishments producing a single homogenous good in *each* local labor market. In my model firms hiring decisions about immigrants and natives vary because of differences in their production functions. I further assume that the technology exhibits decreasing returns to scale. A constant returns to scale production function implies a constant marginal cost. In equilibrium, marginal cost equals the price of the good. However, different technologies will imply different marginal costs for certain parameters, implying that the firm with the lowest marginal cost could decrease the price of the good and satisfy *total demand* in the local labor market. Decreasing returns to scale imply that this is not possible at constant prices. Moreover, production functions with decreasing returns to scale exhibit an upward sloping supply curve, an aspect that is beneficial to understanding the general equilibrium effects of an increase in immigration. The disadvantage of technologies with decreasing returns to scale is that establishments enjoy profits, which leads firms to enter the market until profits are zero. In the current model I assume there is no entry; hence the implications of the model need to be interpreted as the short run impacts of

[12] This assumption seems fairly valid given the empirical evidence of no effect of immigration on natives' wages. See for example Bonin (2005), Glitz (2007) and D'Amuri et al. (2008). I test this assumption in the empirical application section and my results are consistent with wage rigidity for natives.

immigration.

Consider first the firm with both immigrant and native employment (firms type H). Assume the firm (p) uses three inputs Capital (K), Natives (N) and Immigrants (I) for production and the production function is CES type:

$$Y_p^H = A_p \left[K^{\phi} + \left(N^{\rho} + \theta I^{\rho} \right)^{\phi/\rho} \right]^{\frac{\psi}{\phi}} \tag{2}$$

where A is a technology shifter, the elasticity of substitution between natives and immigrants is $\sigma_\rho = \frac{1}{1-\rho}$, the elasticity of substitution between capital and labor is $\sigma_\phi = \frac{1}{1-\phi}$, ψ is the degree of homogeneity and θ is the relative efficiency of immigrants. θ is a key parameter in production function (2). I assume differences in θ are the reason why some firms employ immigrants. This parameter is exogenous to the firm.

A firm that does not employ immigrants (firms type NH) has the following production function:

$$Y_p^{NH} = A_p \left[K^{\phi} + N^{\phi} \right]^{\frac{\psi}{\phi}} \tag{3}$$

The owner of the firm understands the complementarities of each production function. As such, in each period the owner uses the technology with the highest profits π:

$$Max(\pi^H, \pi^{NH})$$

In equilibrium, each firm maximizes profits taking as given all prices which are exogenous to the firm. In particular, assume that natives' wages are fixed at $\overline{w}_N$ given wage agreements and that the price of capital is determined internationally at rate $\overline{r}$. Hence, immigrants' wages are determined by supply and demand of immigrant employment. For simplicity, I assume inelastic labor supply of immigrants and consider an exogenous increase in supply to calculate the effects of immigration.

Since the natives' wage is set above competitive level, there is some unemployment. The sum of demand for natives at wage $\overline{w}_N$ is less than the supply of natives $N^S(\overline{w}_N)$, resulting on a fixed level of unemployment U. Depending on the interaction between immigrant wages and native labor demand, an increase in immigrant labor supply could lead to a decrease in native labor demand and increasing unemployment.

The good is sold locally or internationally. I assume the price of the good does not change, which means that increases in production without a decrease in the price are possible. This case implies that firms with only native employment are not subject to general equilibrium effects.

Formally, equilibrium in the local labor market is defined as the triplet $\{w_I, P, U\}$ that

satisfy market clearing conditions as follow:

1. Equilibrium in the Market for the Homogenous Good.

   $Y^H(P, \overline{w}_N, w_I, \overline{r}) + Y^{NH}(P, \overline{w}_N, \overline{r}) = Y^T$ and assuming supply perfectly elastic, or fixed price.

2. Equilibrium for Natives.

   $N^H(P, \overline{w}_N, w_I, \overline{r}) + N^{NH}(P, \overline{w}_N, \overline{r}) = N^S(\overline{w}_N) - U.$

3. Equilibrium for Immigrants

   $I(P, \overline{w}_N, w_I, \overline{r}) = \overline{I}.$

Under the assumption of fixed price, we can eliminate equilibrium condition (1) and work only with the rest of the conditions. Under the production function specified above, an exogenous increase in the supply of immigrants will lead to a change in employment given by:[13]

$$\begin{aligned} \frac{\Delta\%L}{\Delta I/L} &= 1 + \frac{s_N}{s_I}\left(\frac{\left\{(\sigma_\rho - s_K\sigma_\phi) - \frac{s_L}{1-\psi}\right\}s_I}{\left\{(\sigma_\rho - s_K\sigma_\phi) - \frac{s_L}{1-\psi}\right\}s_I - s_L\sigma_\rho}\right) \\ &= -\frac{\left\{\sigma_\phi s_K + \frac{s_L}{1-\psi}\right\}s_L}{\left\{(\sigma_\rho - s_K\sigma_\phi) - \frac{s_L}{1-\psi}\right\}s_I - s_L\sigma_\rho} \end{aligned} \tag{4}$$

where s_X represents the share of X in total cost, for $X = N, I, K, L$. Notice that if natives and immigrants are perfect substitutes, $\sigma_\rho \to \infty$, an increase in immigrant supply leads to complete displacement $\frac{\Delta\%L}{\Delta I/L} = 0$. On the other hand, there is no displacement if $\sigma_\rho = s_K\sigma_\phi + \frac{s_L}{1-\psi}$. The coefficient will be between zero and one as long as $\sigma_\rho > s_K\sigma_\phi + \frac{s_L}{1-\psi}$. Suppose there is no capital $s_K = 0$ and $s_L = 1$, hence the effect is equal to

$$\begin{aligned} \frac{\Delta\%L}{\Delta I/L} &= 1 + \frac{s_N}{s_I}\left(\frac{\left\{\sigma_\rho - \frac{1}{1-\psi}\right\}s_I}{\left\{\sigma_\rho - \frac{1}{1-\psi}\right\}s_I - \sigma_\rho}\right) \\ &= 1 + \frac{s_N}{s_I}\left(\frac{(\rho-\psi)s_I}{(\rho-\psi)s_I - (1-\psi)}\right) \end{aligned} \tag{5}$$

[13] We know the value of the constant-output elasticity of natives given a change in the wages of immigrants $\overline{\eta}^N_{w^I} = \frac{s_I}{s_L}(\sigma_\rho - \sigma_\phi s_K)$, the total elasticity is equal to $\eta^N_{w^I} = \overline{\eta}^N_{w^I} - \left(\frac{1}{1-\psi}\right)s_I$. $\frac{d\log w_I}{d\log I} = \frac{1}{\left\{(\sigma_\rho - s_K\sigma_\phi) - \frac{s_L}{1-\psi}\right\}\frac{s_I}{s_L} - \sigma_\rho}$.

Immigrants fully displace natives when $\rho = 1$ (i.e. natives and immigrants are perfect substitutes).

The effects from formulas (4) and (5) can be summarized in Table (1). The first column represents the elasticity of substitution between capital and labor, the second column represents the degree of homogeneity, and the elasticity of substitution between natives and immigrants is at the top of the table. Hamermesh (1993) shows different estimates of the elasticity of substitution between capital and labor, the median estimate is close to 0.70 (Table 3.1, pp. 78-79). Hence, I include three estimates of the elasticity of substitution between capital and labor: the median estimate by Hamermesh (1993), the Cobb-Douglas benchmark $\sigma_\phi = 1$ and a third estimate implying more substitution between capital and labor $\sigma_\phi = 2$. The elasticity of substitution between immigrants and natives would need to be larger than 20 for us to observe full displacement of natives by immigrants. An elasticity of 10 would imply that around 3 natives are displaced by every 4 immigrants for a range of possible parameters. Table (1) shows that a high level of displacement requires large elasticities of substitution between natives and immigrants.

The intuition of the table is straightforward. The most important parameter is the elasticity of substitution between immigrants and natives. A high elasticity of substitution implies that an increase in immigration leads to a higher displacement effect. A high degree of homogeneity allows the firm to absorb the immigration shock through increases in production and less through changes in native employment. As the degree of homogeneity increases, the displacement effect is lower. The least important factor, according to Table (1) is the elasticity of substitution between capital and labor. Holding constant the elasticity of substitution between natives and immigrants and the degree of homogeneity, the displacement effect barely changes. As the elasticity of substitution between capital and labor increases, the firm substitutes capital instead of natives, and as a consequence, the displacement effect is smaller.

Formula (4) assumes firms with no immigrants are not affected by the shock. A version of the displacement effect that includes firms with immigrants (H) and no immigrants (NH) is given by:

$$\beta = \frac{\Delta\%L^H - \Delta\%L^{NH}}{\Delta I/L} \tag{6}$$

If there are no shocks to firms with only native employment the impact can be defined as $\frac{\Delta\%L^H}{\Delta I/L}$. The control group or counterfactual in equation (6) are firms which did not modify immigrant employment. Equation (6) assumes that firms in each local labor market are subject to similar shocks (for example through changes in technology shifter A) and the only

difference between them is the change in immigrant employment. If firms receive similar shocks, then equation (6) is valid, otherwise it will be biased and the counterfactual will not be valid. I discuss possible biases in (6) later on Section 5.

4 Data

I use a unique confidential dataset kindly prepared and provided by the Institute for Employment Research (IAB) at Nuremberg, Germany. The dataset is a similar version to the Establishment History Panel which includes all establishments with at least one employee registered with the Social Security Administration in Germany since 1991.[14] Instead of using the Establishment History Panel, the IAB prepared a random sample of the universe of establishments since 1975.[15] I describe more thoroughly this randomization process below. Establishment identifiers consider mostly establishments rather than firms. If a firm within a local labor market has different branches, it can apply for a unique establishment identifier. The conditions to grant a unique establishment identifier depend on having exactly the same 3-digit industry classification and the same local labor market (3 digit) with the same owner. In this case, branches in different locations are considered different establishments. The IAB reports that a single establishment identifier is more common in the finance industry, but there are no available figures as to what percent of establishment identifiers represent firms in each local labor market (Dundler et al. (2006), p.13). Employers are required by law to fill a notification of social security for each employee. For each notification, the employer states the establishment identifier and some employee characteristics, like dates of employment, wage, occupation, education, and nationality. The IAB aggregates this information for each employer. The dataset only includes workers liable from Social Security and excludes students, government employees, judges and pensioners. As immigrants require a work permit or residence permit to be able to work for the employer, the possibility of measurement error in the immigrant classification using this data is greatly diminished.

The IAB prepared the sample as follows. First, the establishments with at least 5 full-time workers on average through their employment history were selected from the universe of all establishments registered in the Social Security Administration in West Germany for 1975-2004. Then, I was provided with a 25 percent random sample from these establishments. I further cleaned the dataset by dropping those establishment-year observations with zero or missing average wage, those with only one full time employee and those that moved to East

[14] For a more detailed explanation of the dataset see Dundler et al. (2006). Further information can be gathered at http://fdz.iab.de. Qualified researchers can get access to the dataset after a screening process.

[15] I observe characteristics for this sample as of June 30 of each year.

Germany after unification.

The dataset includes the following information for each plant: establishment size, number of females, number of native workers (part and full-time), number of workers by occupation and education (natives and females),[16] average and standard deviation of gross daily wages (all, natives and females in full-time status and for all workers), the 25th and 75th percentile in the wage distribution for full-time workers, and general characteristics like industry (3 digit code, 293 possible codes) and geographic code at the district level (county). For the purposes of this study, it also includes the number of foreigners by country/region based on the former Guest Worker countries: Turkey, former Yugoslavia, Italy, Greece, Spain, Eastern Europe, Western Europe, United States, Canada and Australia, and Rest of the World.

In order to study the effects of immigration after the fall of the Iron Curtain, I restrict the dataset for the years 1986-1995. The immigrant shock started in 1991, but the Fall of Berlin Wall was in 1989 with a surge in the inflow of Ethnic Germans. Hence, I keep three years of data before 1989 and three years after the immigrant shock of 1991-1992. Figure (3) shows the share of immigrants is constant before 1986 and starts to decline after 1995. I observe around 100,000 plants each year, but for data analysis I focus in a balanced panel of establishments across all 10 years resulting in a number of plants around 73,700 plants. As it is recognized in the employment adjustment literature,[17] some establishments adjust employees in a lumpy way originating outliers in the change of employment measures. In order to decrease this effect, I restrict the sample to establishments which changed employment in absolute value to less than 100 employees. This restriction decreases the number of establishments in 1.2 percent, resulting in a final sample of 72,713 establishments. The Appendix shows a table comparing establishments across data restrictions and shows that this final restriction is more similar to the original data given that large firms are more represented in the balanced panel.[18]

[16] There are five occupational categories trainees/apprentices, unqualified, skilled workers, master craftsmen and foremen, and white collar employees and four education categories: High School Dropouts with No vocational training, High School Graduates or Dropouts with Vocational training, College or Technical University, and Missing Values.

[17] See for example Caballero et al. (1997), Hamermesh (1989) and Varejão and Portugal (2006)

[18] Dropping outliers does not affect the results. The Appendix includes a table with results including all outliers. Keeping outliers increase standard errors, especially for larger firms. In other regressions (not shown), I defined outliers for 50 and 200 change in employment levels, and find consistent results as outlined above.

5 Empirical Specification

The ideal experiment necessary to evaluate the impact of immigration on the employment of natives would require a random assignment of immigrants across identical firms. Then, we would need to compare the employment of natives between the firm with immigrants and those without. Obviously this experiment is not feasible. Nonetheless, the immigration influx after the fall of the Iron Curtain presents an extraordinary opportunity to estimate the impact of immigration on firms' decisions about the employment of natives. There are two main problems with this natural experiment: (1) Immigrants arrived to different parts of the country, hence the immigration influx to specific districts is endogenous, and (2) there is no clear post-treatment period. After the surge in immigration in 1991-1992, a recession hit the German economy. As a result, the number of unemployed between 1991 and 1995 increased by 800,000. The macroeconomic effect hit localities differently and the estimation of the impact of immigration becomes harder to obtain. Previous approaches that estimate the impact of immigration compare natives' labor market outcomes across cities (Card, 2001, 2007), but the identifying assumption relies on similar macroeconomic shocks to different cities. Although that assumption may seem plausible for the U.S. in different periods, it may be incorrect for the case of Germany after the fall of the Iron Curtain. Establishment level data provides the tools to control for unobserved components that affect all firms equally within each city-year and industry-year. Hence, establishment level data can control for possible biases that are not controlled for in the previous local labor market literature.[19] However, establishment level data has the problem of endogenous shocks at the establishment level. I implement two methods to solve for endogeneity at the plant level.

The first method relies on an instrumental variable approach. This identification strategy relies on the intuition that shocks during the immigration shock are uncorrelated with firms' decisions in the past, for example employing immigrants before the immigration shock. This strategy presents some advantages. The instrumental variable is clear and resembles the instrumental variable used by previous researchers.[20] The estimation includes fixed effects by city-year to capture any effect that affects all firms equally. However, given that there is no clear post-treatment period the estimator will be an average of the short run effect across all years. In the robustness test section, I modify the main specification in order to analyze the medium run effect of immigration.

[19] Following the local labor market literature, I implemented a regression using only aggregate data at the city level. I estimate regression $\frac{\Delta L_{ct}}{L_{ct-1}} = \beta \frac{\Delta I_{ct}}{L_{ct-1}} + \eta_c + \eta_t + v_{ct}$ using total employment and immigrant employment by city-year from Social Security records for the period 1989-1995. The coefficient β is close to 1.78. This coefficient is larger than the OLS coefficients that will be estimated in the next subsection.

[20] See for example Altonji and Card (1991) and Malchow-Møller et al. (2007).

In order to corroborate the results using instrumental variables, I also present results using a propensity score matching approach. This identification strategy follows the spirit of the perfect experiment. It compares outcomes between firms which increased immigrant employment and those firms that did not, given these two set of firms are very similar in observable characteristics before the immigrant shock. The propensity score approach permits to compare establishments' employment growth not only contemporaneously but also after the immigration shock, allowing for the analysis of employment growth in the same set of firms in different years. In the next subsections, I explain the details of each empirical strategy.

5.1 Method 1: Instrumental Variable Approach

I estimate the effect of immigrant employment on total employment in the following way:

$$\frac{\Delta L_{pcjt}}{L_{pcjt-1}} = \beta \frac{\Delta I_{pcjt}}{L_{pcjt-1}} + \eta_{ct} + \eta_{jt} + \eta_p + \varepsilon_{pcjt} \tag{7}$$

where the dependent variable is the percent change in total employment at establishment p in city c, industry j and year t, and I refers to immigrant employment. I include a full set of city-year fixed effects and full set of industry-year fixed effects in order to control for any bias coming from city-year and industry-year shocks.[21] These fixed effects control for characteristics that affect all establishments within a city each year. For example, in the case of a boom or recession, the fixed effects capture the mean effect on those cities. Industry fixed effects capture any trend in employment, for example the increase in services employment or any other shock common to all establishments in the same industry. Although the main variables in equation (7) are expressed in growth rates, I also include establishment specific fixed effects to allow for different growth rates across establishments. If plants have different growth rates in employment (for example, because firms are growing constantly due to good management practices), the establishment fixed effect will absorb that trend.

The main identification assumption in regression (7) is that within city-year observations, unobserved components in the change of growth in total employment are not correlated with the increase in employment of immigrants. In particular, the assumption implies that within city-year observations, unobserved shocks affect firms with changes in immigrant employment and firms with no changes in immigrant employment equally. As Germany experienced an exogenous increase in immigrants during 1989-1995, I estimate how plants

[21] There are 327 cities in my sample. I observe three digit industry codes, but I aggregate industries to obtain 23 different industries. Please refer to the Appendix for the definition of industries. In the robustness tests section, I aggregate geographical codes to 150 and check the robustness of the results in order to allow for possible effects of immigration on districts very close to each other.

that modify their number of immigrants affected total employment compared to those plants that did not modify immigrant employment. In other words, the implicit control group in (7) is the growth rate in total employment of firms which did not modify immigrant employment.[22] The estimate β could also be identified using just the variation in the change of immigrant employment across establishments. However, the inclusion of firms which do not hire immigrants allows us to better control for shocks that affect all firms within a city-year.

An obvious concern of regression (7) is that plants increasing (decreasing) native employment may be increasing (decreasing) immigrant employment at the same time. In other words, plants that are growing consistently will hire more immigrants, and β will be positively biased.[23] For example, establishments that are growing increase their labor force independently of immigration, and any increase in immigrant employment will tend to overstate the true effect of immigration on total employment.

Even after including plant specific trends, city-year and industry-year fixed effects, there is a possible correlation between immigrant employment and unobserved labor demand shocks. An instrumental variable at the establishment level is needed to obtain consistent estimates of the effect of immigration on total employment. This instrument needs to be correlated with actual changes in immigrant employment but cannot be correlated with unobserved labor demand shocks. The local labor market immigration literature has often instrumented the share of immigrants in a city with the location of previous immigrants.[24] I follow the spirit of this instrument in order to obtain an instrumental variable at the establishment level. For example, in the local labor market literature the rationale of the instrument is that previous immigrant location decisions are a good predictor of current immigrant location decisions.[25] The same argument applies at the establishment level. New immigrants learn about jobs through networks and, as a consequence, firms with immigrants in previous periods represent a good predictor to where the new immigrants will be employed. In particular, I assume that the share of immigrants at the establishment level in 1986 represents a good predictor for future immigrant employment levels.[26] If this assumption is correct then we expect that an

[22] As an example, consider the case of two establishments, one with total employment growth rate of 25 percent and immigrant growth rate in terms of total labor of 10 percent, and another establishment with total employment growth rate of 20 percent and zero immigrants. Then $\theta = \frac{0.25-0.20}{0.10} = 0.5$. This means that for every 2 immigrants employed one native job was lost or not created. The same interpretation follows when including city-year fixed effects. The control group is firms with no change in immigrant employment within that city-year.

[23] In the example explained earlier, suppose that the total employment growth rate of firms that increase immigrant employment is now 35 percent instead of 25 percent. Then $\theta = 1.5$.

[24] See for example Altonji and Card (1991) and Card (2001).

[25] There is evidence that this is correct for the case of Germany, see for example Pohl (2007).

[26] I select year 1986 in order to maximize the number of observations in my sample. Each year before 1986

increase in immigration in the city where the firm is located should increase the immigrant labor force at the firm level. Using administrative records, I obtain data on total employment at the city level for each year in the analyzed period and calculate the instrumental variable as:

$$Z_{pcjt} = \frac{I_{pcj1986}}{L_{pcj1986}} \cdot (\frac{I_{ct} - I_{ct-1}}{L_{c1986}}) \tag{8}$$

and $(\frac{I_{ct}-I_{ct-1}}{L_{c1986}})$ is the immigrant shock in city c between period t and $t-1$ as a percent of total labor in 1986, where the latter was calculated using administrative data not from my sample. Within a city, establishments with a large share of immigrants in 1986 will absorb more immigrants than establishments with a low share of immigrants. Notice that the share of immigrants in 1986 at the firm level cannot be the instrumental variable given that the variation of this instrument will be absorbed by the establishment fixed effects, so we need variation across establishment-years. As we are using data starting in 1989, the exclusion restriction assumption implies that the number of immigrants employed in 1986 is not serially correlated with unobserved employment shocks after 1989 inclusive. In formal terms, the first stage is

$$\frac{\Delta I_{pcjt}}{L_{pcjt-1}} = \delta Z_{pcjt} + \widetilde{\eta}_{ct} + \widetilde{\eta}_{jt} + \widetilde{\eta}_{p} + \upsilon_{pcjt} \tag{9}$$

and the instrumental variable estimation strategy assumes:

$$Corr(Z_{pcjs}, \varepsilon_{pcjt}) = 0 \quad \forall \ s, t \tag{10}$$

The identification of parameter β is coming from comparing employment growth rates between establishments with and without immigrants in 1986 scaled by the change in immigrant employment for the same type of establishments.

There are different ways in which the instrument could be invalid. If firms employ Ethnic Germans or East Germans because they hired immigrants in the past, the instrument will be positively correlated with unobserved demand shocks leading to an overestimate (large β) of the true effect of immigration on employment. If firms with immigrants in 1986 are substantially different to the rest of the firms such that shocks in the 1989-1995 period affect these firms differently, then the exclusion restriction will be violated. The violation of

decreases the sample size and power in the estimation. However, the instrumental variable is still a good predictor for changes in immigrant employment. On the other hand, a year closer to 1989 or 1991 increases the concerns that the instrumental variable does not satisfy the exclusion restriction. The estimation of fixed effects by first differencing the data implies that I am using information up to 1988. Hence, year 1986 appears to be a sensible choice. In the Appendix, I include a set of results using the share of immigrants in year 1984 instead, based on a balanced panel of establishments from 1984-1995.

the instrument could be more relevant for the period when the Ethnic Germans and East Germans arrived into West Germany (1989-1990).

In order to solve for the possible violations of the instrument, I estimate regression (7) in different ways. First, I estimate regressions for different firm sizes. Employment growth rates differ substantially across firm size, and it is not correct to compare growth rates of large establishment with those of smaller establishments. Second, I restrict the sample to different periods. The baseline period is 1989-1995, but I estimate regressions for period 1991-1995 as well. These restrictions take into account the effect of the arrival of Ethnic Germans and the effect of the recession, respectively. If these two events are not biasing the results, then the results across specifications should be fairly comparable. Third, in order to control for the re-unification period, I also interact the instrumental variable (8) with a binary variable which takes a value of 1 if the period is later than 1990. The instrumental variable is defined as:

$$\widetilde{Z}_{pcjt} = Z_{pcjt} \cdot 1(Year > 1990)$$

The modification of the instrumental variable takes into account any possible correlation between the arrival of Ethnic Germans and the increase in immigration after 1990. This instrumental variable also allows us to control for a previous trend in employment before the immigration shock providing with more robust estimates.

Although I present different tests to check the validity and robustness of the instrument, condition (10) is untestable. In order to corroborate my results even further, I employ a propensity score to match very similar firms that increased immigrant employment against those firms that did not increase immigrant employment. An advantage of this approach consists on the comparison of the effect among the same firms in different years. This comparison allows us to obtain not only the short run effect of immigration but also the medium run effect. Moreover, if both empirical strategies are correctly specified, then they should provide a similar result. The next subsection explains in more detail the estimation of the propensity score.

5.2 Method 2: Propensity Score

The fundamental problem of causal inference is that we cannot observe outcomes of the same firm when it decides to increase the number of immigrant employees and when it does not. We only observe one state: either changes immigrant employment or not. Define the treatment variable as an establishment's change in immigrant employment. As shown in Figure (3), immigration increased in 1991 and reached its peak in year 1992. Figure (2) also

presents how Ethnic German immigration was substantial before 1991. Therefore, I allow the treatment variable to cover the period when immigration started to increase in Germany and use information before 1991 to predict the treatment in order to take into account only the effect of immigrants and not Ethnic Germans. In formal terms, define the treatment variable as:

$$T_p = \left\{ \begin{array}{l} 1 \quad \text{if } (I_{p1992} - I_{p1991} > 0) \\ 0 \text{ otherwise} \end{array} \right\}$$

and the observed outcome for establishment p in year t as

$$Y_{pt} = T_p Y_{1pt} + (1 - T_p) Y_{0pt}$$

A simple comparison of means is likely to be biased, i.e. $E[Y_{1pt}|T = 1] - E[Y_{0pt}|T = 0]$ does not provide a meaningful estimate of the effect of immigration on firm outcomes. Thus we need to estimate a valid counterfactual for what would have happened to firms had they not increased immigrant employment. The Average Treatment Effect on the Treated for outcome variable Y is defined as

$$ATT_Y = E[Y_{1pt}|T = 1] - E[Y_{0pt}|T = 1].$$

However, ATT cannot be estimated directly because $E[Y_{0pt}|T = 1]$ is not observed without further assumptions. In order to estimate the effect of treatment on outcomes, I assume there is selection on observables defined as:

$$\{Y_1, Y_0\} \perp T|X$$

This assumption means that conditional on observable characteristics X there is no systematic difference between treated and untreated groups before the treatment. This assumption allows the estimation of the ATT conditional on observable characteristics:

$$ATT_Y = E[Y_{pt}|T = 1, X] - E_{X|T=1}\{E[Y_{pt}|T = 0, X]\} \tag{11}$$

where the second term is the conditional expectation over observable characteristics given the treatment. This nonparametric function is over the support of all X variables for the treatment group, and if the number of variables is large, as it is in our case, the "curse of dimensionality" applies. Following Rosenbaum and Rubin (1983), the multidimensional problem can be solved estimating a propensity score $p(X) = \Pr[T = 1|X]$. In particular, if the assumption of selection on observable characteristics holds, then the following condition

also holds:

$$\{Y_1, Y_0\} \perp T | p(X)$$

and we can calculate the nonparametric function over the propensity score:

$$ATT_Y = E[Y_{pt}|T = 1, p(X)] - E_{p(X)|T=1}\{E[Y_{pt}|T = 0, p(X)]\}$$

In order to construct a valid counterfactual, the idea is to find control observations with very similar values in the propensity scores to those of the treated observations, and then we can calculate the ATT.

If the assumption of selection on observable characteristics is correct, the ATT is an unbiased estimator of the effect of immigration on firm outcomes. Is the selection on observables assumption valid? I modify the original sample to try to make the assumptions as valid as possible and present checks whether these assumptions are violated. First, I restrict the sample to small to medium size firms (less than 25 and less than 50 respectively) to make sure the balancing property is correct. I also restrict the sample to only establishments which increased employment during the same period as the treatment variable 1991-1992. In this way, the sample is homogenous to establishments receiving a positive shock which caused them to increase total employment. I further restrict the sample to firms in Bavaria, a southern state with a large increase in the number of immigrants. These restrictions make the sample analyzed homogenous facing similar shocks, with similar local labor markets and same data source as suggested by Heckman et al. (1997) and Michalopoulos et al. (2004). Second, I use five years of data before the treatment to match treated and untreated observations. If firms have different turnover rates or employment growth during these four years of data, this will be taken into account in the estimation of the propensity score. Finally, I use different balance tests to check whether there are significant differences between treatment and matched control samples, if there are substantial differences in unobservable characteristics, it should be reflected in some observable characteristics. The next section shows that the matched control sample is very similar to treatment observations.

Smith and Todd (2005) show that propensity score matching methods can be sensitive both to the matching method and to the way $p(X)$ is estimated. In the robustness tests section, I define the propensity score in two different ways and I use two different matching methods. In order to corroborate my results even further, I estimate similar models to a different state in Germany. Hesse, a state in the center of Germany, also increased its share of immigrants rapidly during the 1989-1992 period.

In order to compare the results from this subsection to the previous one, I focus on the

effect of the treatment variable on the same variables analyzed before: percent change in total employment and in the percent change in immigrant employment as a percent of total labor. I estimate the ATT for these variables as in equation (11) and then calculate the cumulative effect of immigration on total employment as:

$$\widehat{\beta}_t^{pscore} = \frac{\widehat{ATT}_{\frac{\Delta L_t}{L_{1991}}}}{\widehat{ATT}_{\frac{\Delta I_t}{L_{1991}}}} \tag{12}$$

Notice that equation (12) is defined for each year t because the matching is done for different years. If both identification strategies are correctly specified, then there is reason to expect that $\widehat{\beta}^{pscore} \simeq \widehat{\beta}^{IV}$ for years 1992-1993. Moreover, another advantage of using a propensity score approach is to compare the effects of immigration on employment year by year rather than on average. In this way we can further check the effect of the 1992 immigration shock in 1992 in later years to obtain the medium run effect of immigration.[27]

6 Results

6.1 Instrumental Variable

Table (2) includes regression (7) for the final sample. The first column includes city-year, industry-year and establishments fixed effects; the second column excludes establishment fixed effects; the third column excludes industry-year fixed effects, but it includes establishment fixed effects using first differencing; and the fourth column is similar to column 1 but it uses establishment fixed effects by first differencing. Columns 5-8 include a different period of time.

In all the specifications, the OLS coefficient is larger than one. As expected, positive demand shocks correlated with the increase in immigrant employment leads the OLS coefficient to be larger than one. The results show that including establishment fixed effects decrease the estimated coefficient. Industry-year fixed effects do not modify the results after including establishment fixed effects. In the following tables, I include always industry-year fixed effects to be consistent. The results between demeaning and first differencing are very

[27] This approach is similar to the one used by Sianesi (2004) where she analyzed the medium rum effects of a training program in Sweden. The difference here is that my medium run estimates come only from one sample, while in her case she estimates the medium run effects as the mean effect across different specifications. For example, individuals who took the training program after one year of unemployment are mixed with individuals who took the training program after two years of unemployment. Hence, in order to check the effect after three years in the program, she takes the average effect at year three after the program for both samples. My medium run effect, in contrast, uses only one sample: increasing immigrant employment between 1991-1992 or not, and then check the effect for similar firms after the shock.

similar and following Griliches and Hausman (1986) the results suggest that measurement error in the immigrant variable is not a problem.[28]

The OLS results imply that immigrant employment increases total employment. For example, column 1 of Table (2) indicates that, on average, a 1 percent increase in immigrants will lead to a net creation of 0.44 percent jobs for natives. This result implies that establishments do not layoff natives when they increase the number of immigrant workers. In Table (3), I analyze the robustness of these results for different firm sizes. Table (3) includes similar regressions to Table (2) but it includes different firm sizes and it focuses only in regressions with establishment, city-year and industry-year fixed effects. The appendix includes results by First Differencing the data. The first row presents the previous results and the rest of the rows focus on specific firm sizes previous to year 1989.[29] This table shows that the OLS coefficient is still larger than one for different specifications. The OLS coefficient for small firms, less than 25 and 50 employees, is not as large as including all firms. This can be confirmed in the last row. The OLS coefficient for large firms is substantially larger than one.

The OLS results from Tables (2) and (3) suggest that changes in immigrant employment are correlated with unobserved demand shocks leading to a coefficient larger than 1. In order to take this bias into account, I estimate regressions using an instrumental variable for the change in immigrant employment. The instrumental variable is defined as the share of immigrants in 1986 times the immigration shock at the city level scaled by total employment in 1986. Table (4) shows the first stage of the instrumental variable for different firm sizes and periods. The F-statistics for firms with less than 50 employees are around 28 to 70 implying a strong first stage.[30] The coefficients are stable for small firms. For larger firms the standard errors increase substantially, and the coefficient is not as steady as for the rest

[28] Measurement error is defined in the right hand side variable. Consider the true regression (omitting fixed effects and first differencing) $Y = a + bX^* + u$ and $X = X^* + v$ where the classical measurement error v is distributed as $v \sim N(0, \sigma_v^2)$. Then as Griliches and Hausman (1986) have shown the OLS estimator for the within estimator converges to $p\lim \widehat{\beta}^W = \beta\left(1 - \frac{T-1}{T}\frac{\sigma_v^2}{var(\widetilde{X})}\right)$ and the First Difference estimator converges to $p\lim \widehat{\beta}^{FD} = \beta\left(1 - \frac{2\sigma_v^2}{var(\Delta X)}\right)$, hence the "true" estimator is given by $\beta = \frac{\left(\frac{2\widehat{\beta}^W}{var(\Delta X)} - \frac{T-1}{T}\frac{\widehat{\beta}^{FD}}{var(\widetilde{X})}\right)}{\left(\frac{2}{var(\Delta X)} - \frac{T-1}{T}\frac{1}{var(\widetilde{X})}\right)}$ and the measurement error is given by $\sigma_v^2 = \frac{\beta - \widehat{\beta}^{FD}}{2\beta} var(\Delta X)$ where T is the number of periods in the sample, Δ is the difference operator and $\widetilde{X}$ is the demeaned variable. Using both formulas, I find that the measurement error is small, and the "true" estimate is close to the OLS estimates presented in the table. Instrumental variable results by first differencing can be found in the Appendix.

[29] In order to obtain homogenous firm sizes, firm size is defined for years 1986-1989. If the total number of employees in a firm is less than 25 employees for the 1986-1989 period, then that firm is assigned to the less than 25 employees group. The rest of the groups are similarly defined.

[30] Stock and Yogo (2002) present a table including critical values for the test of weak instruments. In their Table 3, including 1 instrument with one endogenous variable, the F-statistic is 16. Using this simple test, I reject the null hypothesis of a weak instrument.

of the firms. The instability is driven by the sample size. The sample contains mostly small firms; hence there is not enough variation to estimate the effect for larger firms precisely. As most of the sample is represented by small firms, I focus on that set of results most of the time.

Table (5) includes the main results in the paper using instrumental variables. The first row includes results for all firms and the next rows include the results for specific firm sizes. The effects are generally close to 0.90 implying a displacement effect of 1 native for every 10 immigrants. However, the effect for large firms is poorly identified with large standard errors and unstable coefficients. When restricting the sample to years later than 1990, the estimate becomes more negative, suggesting a displacement effect of more than 2 natives for every 10 immigrants.

The comparison between columns (1)-(3) in Table (5) suggest that the period immediately after the Wall fell (1989-1990) is biasing the estimate towards 1. The exogenous impact of immigration started after 1990, so it is reasonable to estimate the effect of immigration modifying the instrumental variable to only those years where the impact is exogenous and avoid the increase in native employment during the re-unification period 1989-1990. I modify the original instrumental variable by multiplying it by a dummy variable which takes 1 for years later than 1990 and zero otherwise. However, it is important to notice that allowing for a trend for 1989-1990 will give too much weight to the employment expansion years 1989-1990. The results using this instrumental variable are included in Table (5) column (4). Again, the effects for larger firms are poorly identified given the sample size and the lack of firms with no immigrants. The results imply that the displacement effect is 3 natives for every 10 immigrants in firms with less than 50 employees. The displacement effect of 3 natives by every 10 immigrants is quite robust to several changes in the estimation as shown below.

6.2 Propensity Score

In order to understand not only the contemporaneous effect of immigration but the dynamic effects, I match very similar firms before the immigration shock and analyze how immigration affected employment.[31] The first step is the estimation of the propensity score. There is no consensus as to what variables should be included in the estimation of the propensity score. Heckman et al. (1997) and Rubin and Thomas (1996) mention that omitting important variables can seriously bias the estimation. The consensus of these papers is that

[31] I follow most of the empirical studies, as Caliendo and Kopeinig (2008) suggest, and calculate bootstrap standard errors. However, Imbens (2004) and Abadie and Imbens (2006) suggest to use specific bands. I estimate the propensity score using the program provided by Leuven and Sianesi (2003).

variables that affect both program participation (increase in immigrant employment) and potential outcomes should be included in the propensity score specification. Following these recommendations, I start the estimation of the propensity score using a rich set of covariates for years 1987-1991. In particular, I use as covariates (log) employment, (log) average wages, coefficient of variation, industry (10), region (3), and important characteristics at the firm level: share of immigrants, unqualified, female, part-time, college and low education workers, and finally share of age groups. To allow for a flexible specification of the main variables, I also include interactions of employment and wages, employment and share of immigrants and a square term in the share of immigrant. In order to select the final model, I follow Heckman et al. (1997) procedure and use the Hit-and-Miss method to select the final specification. The Hit-and-Miss method selects the propensity score that maximizes the proportion of observations with predicted values in the propensity score greater than the actual proportion of treated observations. I estimate several models that include different interaction and squared terms, and then I select the model based on the Hit-and-Miss method. I check different specifications of the propensity score in the robustness tests section. In particular, I estimate four propensity scores for the state of Bavaria and Hesse. I estimate one optimal specification defined by the Hit-and-Miss method and one where I include squares of all the variables mentioned above. I also estimate the propensity score by firms with less than 25 and 50 employees.

Propensity score methods rely on the property of balancing the covariates between treatment and control groups (Zhao (2004)). In order to say that the increase in immigration causes outcome Y, it needs to be shown that establishments are not different in other observable characteristics. The literature presents different ways to test for this assumption. I follow most of the tests suggested by Sianesi (2004) and Caliendo and Kopeinig (2008). Sianesi (2004) includes the Pseudo R^2 before and after the matching procedure. Before matching the pseudo R^2 is positive, but after matching it should be very close to zero indicating that selection into treatment based on the covariates is random. Similarly, a likelihood ratio test for the null hypothesis that all coefficients, except the constant, are equal to zero in the matched sample cannot be rejected. Rosenbaum and Rubin (1985) show that after matching, the standardized bias among the covariates should be close to zero.[32] Finally, Dehejia and Wahba (1999, 2002) present another test to balance the covariates. Based on the estimated propensity score, they divide the treatment and control observations in quantiles. Then they test for any significant differences in the covariates within each quantile.

Table (A-12) in the Appendix presents evidence on tests for balance on the covariates

[32]The Standardized Bias is defined as: $SB = \frac{\overline{X}_{T=1} - \overline{X}_{T=0}}{\sqrt{\frac{V(\overline{X}_{T=1}) + V(\overline{X}_{T=0})}{2}}}$. Caliendo and Kopeinig (2008) suggest that a SB less than 5% in the matched sample is considered sufficient.

and shows the maximum value used in the Hit-and-Miss method. I use a radius caliper of 0.01 on the propensity score to check differences in the covariates. All balancing tests are within expected number of rejections. The Standardized Bias is reduced to a median value of close to 1 percent in the matched sample well below the 5 percent suggested by Caliendo and Kopeinig (2008). The Pseudo R^2 in the matched sample is close to zero and the likelihood ratio test cannot be rejected with probability 1. There are no significant differences in the covariates in the matched sample. Using the stratification test, the weighted average number of covariates with significant mean differences is close to one percent, similar to the significance level. These tests consistently show evidence that balance on the covariates is achieved and that selection into treatment conditional on the propensity score is random. However, the tests show that the propensity score does a better job in small firms rather than medium size firms. Also, the propensity score does a better job in balancing the covariates in Bavaria than in Hesse. For example, the standardized bias for the optimal model in Bavaria is 0.92 percent while in Hesse is 2.05 percent.

Table (A-13) in the Appendix shows descriptive statistics of the propensity score by treatment and control group. Although the overlapping among treatment and control groups is not perfect, there is substantial overlapping. For example, the first 95 percent of estimated propensity scores observations in the control group are generally contained in the first 75 percent of estimated propensity scores in the treatment group. Some differences across states and firm size are important. There is less overlapping in larger firms than in small firms, and there is more overlapping in Bavaria than in the state of Hesse. In order to enforce a common support region, I restrict the matching to a caliper of 0.01 (both the radius matching and the nearest neighbor case).

First, I analyze the effect of treatment on the increase in total employment and immigrant employment. Second, I analyze the parameter $\widehat{\beta}_t^{pscore} = \frac{\widehat{ATT}_{\frac{\Delta L_t}{L_{91}}}}{\widehat{ATT}_{\frac{\Delta I_t}{L_{91}}}}$ in formula (12) and calculate the standard error using the delta method. Figure (5) shows the effect of treatment (increasing immigrant employment) on the percent change in total and immigrant employment of small firms (less than 25 employees before 1991 in Bavaria. The base year is 1991. The figure shows that before 1991 both lines are similar, as we would expect. Treated firms increased immigrant employment by 17 percent in 1992 while they only increase total employment by close to 11 percent. Figure (6) includes the calculation of $\widehat{\beta}_t^{pscore}$ and it shows that the short run effect is close to 0.70, a very similar effect to the one obtained using instrumental variables. The medium run effect (comparing 1994-1995 with respect to 1991) of immigration on total employment is close to 1 although the confidence interval widens substantially. The figure shows that the displacement effect at the firm level is concentrated in

the short run. After the immigration shock, firms seem to decrease immigrant employment. and then after two or three years of the immigration shock the displacement effect decreases. For completeness, Figure (7) shows the displacement effect for small firms in Hesse. As the balancing of the covariates for the state Hesse is not as successful as for the state of Bavaria, the displacement effect exhibits substantially larger standard errors. The short run effect using the radius matching is close to 0.5, but in later years the displacement effect decreases and approaches 1. The robustness tests section will show that the displacement effect of 0.5 is the lowest among different matching estimates.

7 Robustness Tests

This section presents some robustness checks on the effect of immigration on natives' labor market outcomes. Previous tables have shown the effects of immigration using different samples, either focusing on specific firm sizes or on different periods. The results across specifications are fairly stable concentrating the displacement effect from 2 to 4 natives for every 10 immigrants with most of the estimates close to 3. In order to summarize the robustness checks, I focus only in firms with less than 50 employees. The Appendix includes tables summarizing the effect for all firms and small firms.

A possible concern in the main regressions is the use of the most detailed geographic code and therefore ruling out possible effects given by intercity migration. I re-estimate the effects using 150 city identifiers instead of the 327 city identifiers included in the sample. The 150 city identifiers were provided by the IAB and they cover large geographical areas. Column (1) of table (6) suggests that the use of the geographic codes is not biasing the results. The estimated coefficients are fairly similar to the ones obtained above in Table (5). Although the IV estimates are less precise, the average effect is concentrated on a displacement effect of 3 natives by every 10 immigrants.

All regressions shown above use total employment at the firm level; that is, they do not distinguish between full-time and part-time employees. If immigrants are hired in full time jobs and as a consequence more natives are hired in part-time jobs; then the estimated displacement effect will appear as if there is no displacement given that firms are increasing jobs for both immigrants and natives. However, the example clearly shows that immigrants are displacing natives from full-time jobs. Hence, it is important to corroborate that assertion and check whether the results hold when restricting the sample only to full-time employees. Column (2) of table (6) presents the results using the full-time distinction. These results are very similar to previous estimates, and they are even more homogenous than before. The results do not indicate a strong source of bias in the main regressions. The main displacement

effect is concentrated in 3-4 native jobs for every 10 immigrant jobs.

Another possible violation of the instrument is the effect of Ethnic Germans on total employment. If Ethnic German employment in years after 1989 is correlated with having immigrants in 1986, then the exclusion restriction would be violated. Previous tables have shown that this effect could be significant given the difference in the results of the two instrumental variables. I construct a new instrumental variable defined only for immigrants from Yugoslavia and Turkey in 1986 instead of all immigrants. In other words, I substitute the share of immigrants in formula (8) by the share of Yugoslavs and Turks in the firm. In this way, the focus is in establishments with only "low skilled" labor. Results in column (3) of table (6) include larger coefficients than previously estimated. Firms with only Yugoslavs and Turks in 1986 seem not to have a negative employment growth compared to other firms.

I also estimate regressions focusing only in manufacturing. If immigrants affect the price of services, then firms with no immigrants will be affected as well, and they will decrease total employment as a consequence of general equilibrium effects. However, the price of manufacturing goods is determined more by international trade than by local labor market conditions, and it is not affected by general equilibrium effects. Column (4) of table (6) shows the results restricting the sample to manufacturing firms. These new estimates are not substantially different to previous calculations. The displacement effect of firms with less than 50 employees is larger than previously estimated, suggesting a displacement effect of 4 natives for every 10 immigrants. The results suggest that general equilibrium effects are not substantially important.

In order to check the robustness of the instrument and test whether the exclusion restriction is satisfied, I construct a new instrumental variable. I use as an instrument the lagged share of immigrants employed in the industry who reside outside the city of the firm interacted with the immigration shock at the city level in the current year.[33] I call this new instrumental variable W. The first part in W is exogenous given the absence of network effects outside the city. The second part is exogenous to the firm given the inclusion of city-year fixed effects. The use of this instrumental variable is useful for two reasons: first, it provides an estimate different to previous results, and second, it is possible to test whether the exclusion restriction is satisfied, as shown by the overidentification test. Results using this instrumental variable are shown in Column (5) of table (6). The regression using only the share of immigrants in the industry outside the city shows less displacement than the

[33] I use the disaggregated level of industry to calculate the share of immigrants in the industry outside the city. There are 95 different industries according to this definition. The new instrumental variable is defined as $W = \left(\frac{I^{-c}_{jt-1}}{L^{-c}_{jt-1}}\right) \cdot \left(\frac{I_{ct}-I_{ct-1}}{L_{ct-1}}\right)$ where the first term is the share of immigrants in the same industry as the firm but accounting only the firms outside the city of the relevant firm, and the second term is the supply effect.

original instrumental variable. However, when I restrict the instrumental variable only for years later than 1990 the results are strikingly similar. To check whether the exclusion restriction is satisfied, I include the original instrumental variable of share of immigrants in 1986 with the new instrumental variable W. The regressions including the two instrumental variables are found in column (6) of table (6). The p-value of the overidentification test is in parenthesis besides the F-statistic from the first stage. The overidentification test cannot be rejected providing more credibility to the instrument used above. Moreover, the displacement effect is very similar to what previous tables have shown, but it has a smaller standard error. The main message of columns (5)-(6) is that the displacement effect is robust to changes in the instrumental variable. The displacement effect is concentrated around 3-3.5 natives for every 10 immigrants for firms with less than 50 employees (around 3 for firms with less than 25 employees).

In order to compare the IV estimates to the propensity score estimates, I re-estimate the main regressions but restricting the sample to firms which increased total employment between 1991 and 1992. The results are shown in column (7) in table (6). The column shows that results are slightly more negative than in column (1), but they still suggest a displacement effect between 3-4 native jobs for every 10 new immigrant employees. However, the displacement effect is closer to the estimates given by the propensity score approach corroborating the causal effect of immigration on displacement.

If the instrumental variable used above affects directly employment growth, then the instrumental variable estimates will be biased. In the terminology of Conley et al. (2008), the instrument used is "plausibly exogenous". Based on the suggestions in Conley et al. (2008), I consider how my estimates are sensible to "plausibly exogenous" instruments. I run instrumental variable regressions as in equation (7), but now I include a "guess" estimate for the direct effect between the instrument and the outcome variable. Excluding fixed effects and subscripts, I run regressions like $\frac{\Delta L}{L} - \varphi Z = \beta \frac{\Delta I}{L} + u$ using instrumental variable Z for endogenous variable $\frac{\Delta I}{L}$. I explore how $\widehat{\beta}$ is affected for different values of φ. I define values of φ to be equal to 0.10 and -0.10. The parameter φ refers to the direct effect of the instrument on employment growth at the firm level. Table (7) indicates that results are not strongly affected by the "plausibly exogenous" restriction giving more credibility and robustness to the main estimates in the paper. For example, the estimate $\varphi = 0.10$ for firms with less than 25 employees shows a larger displacement effect from 0.71 to 0.69.

I also estimate the IV specification with different lags in order to compare the results to the propensity score specification. Instead of estimating the effect of immigration from the previous year to the current year, I estimate the effect of immigration from two, three and four years to the current year. In other words, the dependent (independent) variable

represent the total (immigrant)employment growth rate between year $t-2$, $t-3$ and $t-4$ to year t, instead of measuring the growth rate between year $t-1$ and t. I only estimate the regression for period 1991-1995 because including year 1989-1995 includes the instrumental variable (share of immigrants in 1986). Table (8) shows results for lags 2-4 and includes the benchmark estimate for lag 1. The estimates indicate that the effect of immigration is restricted to the first two years of the immigration shock. When we compare the employment growth rates between longer periods of time, the negative effect vanishes suggesting that immigration affects employment in the short run.[34]

In the case of the propensity score, I estimate the displacement effect for full-time workers and all workers using the radius matching approach and a nearest neighbor approach. I estimate the propensity score using the Hit-and-Miss method, but I also include a specification with squares in all explanatory variables. I estimate different propensity scores for firms with less than 25 and 50 employees. The result is that I have 16 different displacement effects for each year (2 definitions of workers, 2 firm sizes, 2 matching algorithms and 2 propensity score specifications). Figure (8) includes the final results for Bavaria (Appendix Tables (A-14)-(A-16) include more results for Hesse and Bavaria). The graph includes a confidence interval for each of the 16 estimates. The results are precisely estimated for the first two years of the impact of immigration, but the displacement effect decreases in later years.

8 Implications

Using the predicted values of formula (4) from Table (1) and the results shown previously, the elasticity of substitution between immigrants and natives is between 4-15. This finding seems to be fairly consistent to results for the U.S. and Europe. For example, Manacorda et al. (2006) find an elasticity of substitution between natives and immigrant within skills in England close to 5 and 7; in Germany, D'Amuri et al. (2008) find an elasticity of substitution between 15 and 20; for the U.S., Ottaviano and Peri (2006) find an elasticity of substitution between 5-10, and Card (2008) finds an elasticity of substitution close to 25. The surprising result is that low values of this elasticity seem to provide a substantial negative effect on employment levels. However, the work by Angrist and Kugler (2003) implies, consistent with the results shown above, that the effects of immigration are exacerbated in countries with more rigid labor markets.

A "back-of-the-envelope" calculation provides insightful evidence on the effect of immi-

[34]The Appendix includes Table (A-9) for establishments with positive employment growth between 1991-1992. The results are similar to Table (8).

gration at the national level. If we take at face value the estimates obtained above, we conclude that every immigrant employed displaces 0.3 natives. During the period 1991-1993, immigrant employment increased by close to 185,000 but total native unemployment increased by close to 550,000. This means that the increase in immigrant employment during the 1991-1993 period explains close to 10 percent of the increase in native unemployment. Moreover, the aggregate estimate is likely to be smaller if we consider that the displacement effect comes from small firms, and employment in small firms (less than 25 employees) only represents approximately 20% of total employment. Hunt (1992) finds that immigration from repatriates from Algeria to France explains up to 29 percent of the increase in unemployment. The calculation above suggests that immigration in Germany during 1991-1993 explains only a small fraction of the rise in unemployment and that there are other factors relatively more important.

9 Explaining Results in terms of wages

This section analyzes how firms adjust to the immigration shock in terms of wages. The results shown above imply that for every 10 immigrants, 6-8 new jobs are created and only 2-4 natives are displaced. The effect is large compared to the mean effect found in the meta-analysis of Longhi et al. (2006). However, the effect of immigration is similar to the result found by Glitz (2007). Glitz is interested in the effect of Ethnic Germans on natives' wages and employment for the period after 1996. He finds a displacement effect close to 4 natives for every 10 Ethnic Germans. Although it may seem that the effect is large, the estimated effect only focuses in the short-run. Previous research has focused more on long-term responses to immigration, and as shown above, the effect of immigration on firms' employment after two or three years of the shock is close to zero. Most of the immigration impact in terms of employment is operating through short-run channels. Also, as Angrist and Kugler (2003) mention, wage rigidities exacerbate the displacement effect in the short-run.

Throughout the empirical application, I have considered that natives' wages are rigid and immigrants' wages are flexible. In this section, I explore the effect of immigration on wages. Specifically, I explore to what extent immigration affects average wages. As mentioned in previous analyses of immigration, immigrants themselves are affected by the immigrant shock. In this case, immigrant wages should decrease substantially. This hypothesis is related to the downgrading of immigrants after arrival. I therefore analyze the effect on wages using both the instrumental variable approach and the propensity score approach. However, immigrant wages cannot be analyzed directly because there are firms with zero immigrants leading to a missing wage for immigrants. Given this difficulty, I analyze average

total wages and native wages instead.

The effect of immigration on average wages can be written as[35]

$$\frac{\Delta \%\overline{w}}{\Delta I/L} = \left(\frac{w^I - w^N}{\overline{w}}\right)\left(1 - s^I\beta\right) + \frac{w^I}{\overline{w}}\eta_{w^I,I} \tag{13}$$

where s^X is the share of $X = N, I$ in total labor and w^X is the average wage of factor X, β is the displacement effect and $\eta_{w^I,I}$ is the elasticity of immigrant's wages with respect to immigrant employment. The first term in formula (13) represents the mechanical effect of an increase in immigrant employment on average wages. As immigrants are cheaper than natives on average, an increase in immigrant employment mechanically decreases the average wage. The second term refers to the behavioral component. It refers to the effect of an increase in immigrant employment on immigrant wages. Table (9) predicts formula (13) for different parameters in the production function. A high elasticity of substitution between natives and immigrants leads to lower effects of immigration on average wages because the firm will adjust through employment levels, not through wages. On the other hand, low levels of the elasticity of substitution imply that natives and immigrants are highly imperfect substitutes, which causes immigrant wages to be more affected than native employment.

Table (10) estimates the effect of immigration on average wages and native wages using as instrumental variable the share of immigrants in the firm in 1986 interacted by the supply shock. The dependent variable is the percent change in average wage and native wage, and the main independent variable is the percent change in immigrant employment with respect to previous total employment. All regressions include establishment, city-year and industry-year fixed effects.[36] The first three columns refer to the effect of immigration on average wages and the next 3 columns refer to the effect of immigration on native wages. The estimate $\gamma = \frac{\Delta \%\overline{w}}{\Delta I/L}$ is an estimate of formula (13). The effect of an increase of 1 percent in immigrant employment leads to a decrease in average wages by 0.17 percent. On the other hand, immigrant employment does not seem to affect natives' wages. When the dependent variable is natives' wage, the coefficient is imprecise and unstable.

Figures (9) and (10) show the estimation of the raw effect of treatment on total and native wages for the states of Bavaria and Hesse. These figures show that native wages do not decrease too much. In fact, in Bavaria they increase marginally immediately after the shock and then decrease. In Hesse native wages decrease up to 1 percent, but this result

[35] Average wages are defined as $\overline{w} = s^N w^N + s^I w^I$ where s^X is the share of X in total labor. Differentiating that expression with respect to an increase in immigration and assuming native's wages are rigid we obtain $\frac{\Delta \%\overline{w}}{\Delta I/L} = \frac{\Delta\left(\frac{N}{L}\right)}{\Delta I/L}\frac{w^N}{\overline{w}} + \frac{\Delta\left(\frac{I}{L}\right)}{\Delta I/L}\frac{w^I}{\overline{w}} + \frac{\Delta\left(w^I\right)}{\Delta I/L}\frac{\frac{I}{L}}{\overline{w}}$, after some algebra and simplification we get the expression in the text.

[36] The regression is written as $\Delta \%\overline{w}_{pcjt} = \gamma\frac{\Delta I_{pcjt}}{L_{pcjt-1}} + \eta_p + \eta_{ct} + \eta_{jt} + \xi_{pcjt}$.

is never significantly different from zero. Figures (11)-(12) show an estimate of $\gamma = \frac{\Delta \% \overline{w}}{\Delta I/L}$ for the state of Bavaria and Hesse for small firms using the radius matching approach. The estimate is around -0.10 in Bavaria and -0.15 in Hesse, these estimates are slightly lower than the IV estimates, but the standard errors are large. Moreover, the effect on wages appears to be permanent. This suggests that immigrant wages are affected permanently, at least after 3 or 4 years of the immigration shock.

The estimates of the effect of immigration on wages help to have a closer estimate of the elasticity of substitution between natives and immigrants. If the displacement effect is close to 0.70 and the effect on average wages is close to -0.17 to -0.15 then the elasticity of substitution is between 10-15 as indicated in Tables (1) and (9). The elasticity of substitution estimate is very similar to what previous studies have found. An added value of this estimate is the issue of aggregation. Previous estimates of the effect of immigration rely on aggregate data at the local market level or at the national level. Based on the results by Fisher et al. (1977), we know that the aggregate estimate of the elasticity of substitution may not reflect the true elasticity of substitution at the firm level. The results shown in this paper confirm the aggregate elasticity of substitution obtained in previous analyses.

10 Conclusions

This paper analyzes the substantial increase in immigration after the fall of the Iron Curtain in Germany in order to identify the effect of immigration on natives' labor market outcomes. The problem with the identification of the effect of immigration is the construction of a valid counterfactual, i.e. what would have happened had the immigrants not arrived in the first place. This paper is at the center of that discussion and seeks a simple way to construct a counterfactual for the effect of immigration using establishment level data. Within local labor markets, I analyze employment patterns for establishments which modify immigrant employment against those that did not modify immigrant employment; this is a substantial improvement over previous analyses of immigration.

Using an instrumental variable and a propensity score matching approach, I conclude that an increase in 10 immigrant workers causes a displacement of 3 native workers on average. The estimate is more negative than previously found in the immigration literature, however the estimate presented here is a short-run effect rather than the long-run effect that is commonly estimated. Moreover, the result is consistent with views that rigid labor markets exacerbate the effect of immigration on the employment margin (Angrist and Kugler, 2003).

A surprising result is the fact that the displacement effect is mostly concentrated in the year of the immigration shock. Firms rapidly adapt their employment levels, and three

to four years after the immigration shock the displacement effect decreases. One possible explanation of this finding is that natives and immigrants are imperfect substitutes in the short run, but later on firms are able to adapt to immigrant shocks through changes in capital or other non-labor inputs. Nevertheless, more research is needed to relate the displacement effect at the firm and at the aggregate level given that firm-level data does not account for the unemployed and how hard they are looking for a job after the immigration shock.

Firms adapted to the immigration shock in two ways. First, firms which increased immigrant employment did not increase native employment as much as the rest of the firms. In other words, the effect of immigration is related to differences in hiring rates, not to layoffs. Second, firms which increased immigrant employment also reduce the average wage of immigrants. Average wage in the firm decreased 2 percent on average with no corresponding effect to the average wage of natives. The patterns of employment and wages suggest an elasticity of substitution between natives and immigrants between 10 and 15. Perhaps surprisingly, the firm level evidence is therefore consistent with estimates based on local and national labor market comparisons.

There are two important avenues of future research on this topic that are not analyzed in this paper. First, if immigrants boost the creation of new firms, the displacement effect in the aggregate could be lower than the one found in this paper. Second, a very interesting topic is a formal dynamic analysis on how specific firms adapt to the immigration shock in the medium and long run. With the new advances in matched employer-employee data across the world, this new research agenda seems more likely to be completed in the near future.

References

Altonji, Joseph G. and David Card, "The Effects of Immigration on the Labor Market Outcomes of Less-skilled Natives," in John M. Abowd and Richard B. Freeman, eds., *Immigration, Trade and the Labor Market*, National Bureau of Economic Research Conference Report, University of Chicago Press, 1991, pp. 201–234.

Angrist, Joshua D. and Adriana D. Kugler, "Protective or Counter-Productive? Labour Market Institutions and the Effect of Immigration on EU Natives," *The Economic Journal*, June 2003, *113* (488), F302–F331.

Bonin, Holger, "Wage and Employment Effects of Immigration to Germany: Evidence from a Skill Group Approach," IZA Discussion Papers 1875, Institute for the Study of Labor (IZA) December 2005.

Borjas, George J., "The Economic Analysis of Immigration," in O. Ashenfelter and D. Card, eds., *Handbook of Labor Economics*, Vol. 3 of *Handbook of Labor Economics*, Elsevier, May 1999, chapter 23, pp. 1697–1760.

_ , "The Labor Demand Curve is Downward Sloping: Reexamining the Impact of Immigration on the Labor Market," *The Quarterly Journal of Economics*, November 2003, *118* (4), 1335–1374.

Caballero, Ricardo J., Eduardo M.R.A. Engel, and John Haltiwanger, "Aggregate Employment Dynamics: Building from Microeconomic Evidence," *The American Economic Review*, March 1997, *87* (1), 115–137.

Caliendo, Marco and Sabine Kopeinig, "Some Pracical Guidance for the Implementation of Propensity Score Matching," *Journal of Economic Surveys*, February 2008, *22* (1).

Card, David, "The Impact of the Mariel Boatlift on the Miami Labor Market," *Industrial and Labor Relations Review*, January 1990, *43* (2), 245–257.

_ , "Immigrant Inflows, Native Outflows, and the Local Labor Market Impacts of Higher Immigration," *Journal of Labor Economics*, January 2001, *19* (1), 22–64.

_ , "Is the New Immigration Really so Bad?," *The Economic Journal*, January 2005, *115* (507), 300–327.

_ , "How Immigration Affects U.S. Cities," CReAM Discussion Paper Series 0711, Centre for Research and Analysis of Migration (CReAM), Department of Economics, University College London June 2007.

_ , "Local Labor Market Adaptation to increased immigration," Working Paper, University of California, Berkeley April 2008.

Chin, Rita, *The Guest Worker Question in Postwar Germany*, Cambridge University Press, 2007.

Conley, Timothy G., Christian Hansen, and Peter E. Rossi, "Plausibly Exogenous," Working Paper Series, University of Chicago 2008.

D'Amuri, Francesco, Gianmarco I.P. Ottaviano, and Giovanni Peri, "The Labor Market Impact of Immigration in Western Germany in the 1990's," Working Paper 13851, National Bureau of Economic Research March 2008.

Dehejia, Rajeev H. and Sadek Wahba, "Causal Effects in Nonexperimental Studies: Reevaluating the Evaluation of Training Programs," *Journal of the American Statistical Association*, December 1999, *94* (448), 1053–1062.

_ **and** _ , "Propensity Score Matching-Methods for Nonexperimental Causal Studies," *The Review of Economics and Statistics*, February 2002, *84* (1), 151–161.

Dundler, Agnes, Melanie Stamm, and Silke Adler, "The Establishment History Panel," FDZ Datenreport 3/2006, Institut für Arbeitsmarkt-und Berufsforschung 2006.

Fisher, Franklin M., Robert M. Solow, and James M Kearl, "Aggregate Production Functions: Some CES Experiments," *Review of Economic Studies*, June 1977, *44* (2), 305–20.

Glitz, Albrecht, "The Labor Market Impact of Immigration: A Quasi-Experimental Evidence," Working Paper, Universitat Pompeu Fabra October 2007.

Griliches, Zvi and Jerry A. Hausman, "Errors in Variables in Panel Data," *Journal of Econometrics*, February 1986, *31* (1), 93–118.

Grossman, Jean Baldwin, "The Substitutability of Immigrants and Natives in Production," *The Review of Economics and Statistics*, November 1982, *64* (4), 596–603.

Göktürk, Deniz, David Gramling, and Anton Kaes, eds, *Germany in Transit. Nation and Migration 1955-2005* University of California Press 2006.

Hamermesh, Daniel S., "Labor Demand and the Structure of Adjusment Costs," *The American Economic Review*, September 1989, *79* (4), 674–689.

_ , *Labor Demand*, Princeton University Press, 1993.

Heckman, James J., Hidehiko Ichimura, and Petra E. Todd, "Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme," *The Review of Economic Studies*, October 1997, *64* (4), 605–654.

Herbert, Ulrich, *A History of Foreign Labor in Germany, 1880-1980*, The University of Michigan Press, 1990.

Hunt, Jennifer, "The Impact of the 1962 Repatriates from Algeria on the French Labor Market," *Industrial and Labor Relations Review*, April 1992, *45* (3), 556–572.

Johnson, George E., "The Labor Market Effects of Immigration," *Industrial and Labor Relations Review*, April 1980, *33* (3), 331–341.

Leuven, Edwin and Barbara Sianesi, "PSMATCH2: Stata module to perform full Mahalanobis and propensity score matching, common support graphing, and covariate imbalance testing," Statistical Software Components, Boston College, Department of Economics April 2003.

Liebig, Thomas, "The Labour Market Integration of Immigrants in Germany," OECD Social Employment and Migration Working Papers 47, OECD Publishing 2007.

Longhi, Simonetta, Peter Nijkamp, and Jacques Poot, "The Impact of Immigration on the Employment of Natives in Regional Labor Markets: A Meta-Analysis," IZA Discussion Papers 2044, Institute for the Study of Labor (IZA) March 2006.

Malchow-Møller, Nikolaj, Jakob Roland Munch, and Jan Rose Skaksen, "Do Immigrants Affect Firm-Specific Wages?," IZA Discussion Papers 3264, Institute for the Study of Labor (IZA) December 2007.

Manacorda, Marco, Alan Manning, and Jonathan Wadsworth, "The Impact of Immigration on the Structure of Male Wages: Theory and Evidence from Britain," IZA Discussion Papers 2353, Institute for the Study of Labor (IZA) October 2006.

Michalopoulos, Charles, Howard S. Bloom, and Carolyn J. Hill, "Can Propensity-Score Methods Match the Findings from a Random Assignment Evaluation of Mandatory Welfare-to-Work Programs?," *The Review of Economics and Statistics*, February 2004, *86* (1), 156–179.

Okkerse, Liesbet, "How to Measure Labour Market Effects of Immigration: A Review," *Journal of Economic Surveys*, 2008, *22* (1), 1–30.

Ottaviano, Gianmarco I.P. and Giovanni Peri, "Rethinking the Effects of Immigration on Wages," NBER Working Papers 12497, National Bureau of Economic Research August 2006.

Peri, Giovanni, "Immigrants' Complementarities and Native Wages: Evidence from California," Working Paper 12956, National Bureau of Economic Research March 2007.

Pischke, Jörn-Steffen and Johannes Velling, "Employment Effects of Immigration to Germany: An Analysis Based on Local Labor Markets," *The Review of Economics and Statistics*, November 1997, *79* (4), 594–604.

Pohl, Carsten, "On the location choice of newly arrived immigrants in Germany," Technical Report, Institute for Economic Research (ifo) October 2007.

Rosenbaum, Paul R. and Donald B. Rubin, "The Central Role of the Propensity Score in Observational Studies for Causal Effects," *Biometrika*, 1983, *70* (1), 41–55.

_ **and** _ , "Constructing a control group using multivariate matched sampling methods that incorporate the propensity score," *The American Statistician*, 1985, *39* (1), 33–38.

Rubin, Donald B. and Neal Thomas, "Matching Using Estimated Propensity Scores: Relating Theory to Practice," *Biometrics*, 1996, *52* (1), 249–264.

Sianesi, Barbara, "An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s," *The Review of Economics and Statistics*, February 2004, *86* (1), 133–155.

Siebert, Horst, "Germany -an Immigration Country," Kiel Working Paper 1189, Kiel Institute for World Economics November 2003.

Smith, Jeffrey and Petra E. Todd, "Does Matching Overcome Lalonde's Critique of Nonexperimental Estimators?," *Journal of Econometrics*, 2005, *125* (2), 305–353.

Stock, James H. and Motohiro Yogo, "Testing for Weak Instruments in Linear IV Regression," NBER Working Papers 0284, National Bureau of Economic Research November 2002.

Varejão, José and Pedro Portugal, "Employment Dynamics and the Structure of Labor Adjustment Costs," IZA Discussion Papers 1922, Institute for the Study of Labor (IZA) January 2006.

Zhao, Zheng, "Using Matching to Estimate Treatment Effects: Data Requirements, Matching Metrics, and Monte Carlo Evidence," *The Review of Economics and Statistics*, February 2004, *86* (1), 91–107.

Zimmermann, Klaus F., Holger Bonin, René Fahr, and Holger Hinte, *Immigration Policy and the Labor Market. The German Experience and Lessons for Europe*, Springer, 2007.

Table 1: Predicted Displacement Estimate assuming Different Elasticities

		σ_ρ						
σ_φ	ψ	2	4	6	8	10	15	20
No K	0.7	1.588	0.844	0.575	0.436	0.351	0.236	0.178
No K	0.8	2.248	1.227	0.844	0.643	0.519	0.351	0.265
No K	0.9	3.851	2.248	1.588	1.227	1.000	0.684	0.519
0.7	0.7	1.211	0.634	0.430	0.325	0.261	0.175	0.132
1	0.7	1.255	0.658	0.446	0.338	0.271	0.182	0.137
2	0.7	1.400	0.739	0.502	0.380	0.306	0.205	0.155
0.7	0.8	1.691	0.902	0.615	0.467	0.376	0.253	0.191
1	0.8	1.731	0.926	0.632	0.479	0.386	0.260	0.196
2	0.8	1.865	1.002	0.685	0.521	0.420	0.283	0.213
0.7	0.9	2.927	1.643	1.142	0.875	0.709	0.481	0.364
1	0.9	2.960	1.663	1.157	0.887	0.719	0.488	0.369
2	0.9	3.067	1.731	1.206	0.926	0.751	0.510	0.386

Note: The first column represents the elasticity of substitution between capital and labor, the second column represents the degree of homogeneity. Coefficients derived from Formula (4) defined in text. The first three rows assume No Capital. Share of Capital is 0.33 and Share of Labor is 0.67, the Share of Immigrants is 0.05. In the case of No Capital the Share of Immigrants is defined as .089 from the ratio 0.05/0.67.

Table 2: OLS Results

	1989-1995					**1989-1993**		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]
β	1.4264	1.4703	1.3736	1.3729	1.3911	1.4375	1.3476	1.3469
	[0.0701]	[0.0669]	[0.0586]	[0.0587]	[0.0831]	[0.0767]	[0.0741]	[0.0742]
Fixed Effects								
City-Year	Y	Y	Y	Y	Y	Y	Y	Y
Industry-Year	Y	Y	N	Y	Y	Y	N	Y
Establishment	Y	N	Y	Y	Y	N	Y	Y
Differencing	N	N	Y	Y	N	N	Y	Y

Note: Each entry represents a different regression. Robust standard errors with clustering at the city level are in brackets. There are 327 cities and 23 industries.

Table 3: OLS Results by Firm Size

	1989-1995	**1989-1993**	**1991-1995**
	[1]	[2]	[3]
A. All Firms			
β	1.4264	1.3911	1.4428
	[0.0701]	[0.0831]	[0.0862]
N	508991	363565	363565
B. Firms <25 Employees			
β	1.1936	1.1908	1.2155
	[0.0444]	[0.0599]	[0.0566]
N	337253	240895	240895
C. Firms <50 Employees			
β	1.3034	1.3044	1.3378
	[0.0613]	[0.0804]	[0.0795]
N	419853	299895	299895
D. Firms >100 Employees			
β	0.3923	1.8514	2.0759
	[0.1107]	[0.1056]	[0.1420]
N	34237	24455	24455

Note: Each entry represents a different regression. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. There are 327 cities and 23 industries. All regressions include city-year, industry-year and establishment fixed effects.

Table 4: First Stage Results

	1989-1995	**1989-1993**	**1991-1995**	**1989-1995**
IV	*Z*	*Z*	*Z*	*Z (Yr>1990)*
	[1]	[2]	[3]	[4]
A. All Firms				
δ	4.5402	4.8101	4.5876	4.1971
	[0.4652]	[0.7778]	[0.5141]	[0.4977]
F	95.3	38.2	79.6	71.1
N	508991	363565	363565	508991
B. Firms <25 Employees				
δ	4.2753	4.4228	4.5532	4.3564
	[0.5837]	[0.9404]	[0.6992]	[0.6795]
F	53.6	22.1	42.4	41.1
N	337253	240895	240895	337253
C. Firms <50 Employees				
δ	4.4296	4.4194	4.5847	4.2871
	[0.5254]	[0.8280]	[0.6142]	[0.5888]
F	71.1	28.5	55.7	53.0
N	419853	299895	299895	419853
D. Firms >100 Employees				
δ	5.8008	8.452	5.4611	4.7692
	[0.7343]	[1.1225]	[0.8238]	[0.8034]
F	62.4	56.7	43.9	35.2
N	34237	24455	24455	34237

Note: Each column represents a different regression. F-statistic from the first stage regression below standard errors. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include city-year, industry-year and establishment fixed effects. There are 327 cities and 23 industries. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990.

Table 5: IV Results

	1989-1995	**1989-1993**	**1991-1995**	**1989-1995**
IV	*Z*	*Z*	*Z*	*Z (Yr>1990)*
	[1]	[2]	[3]	[4]
A. All Firms				
β	0.8035	1.0737	0.6817	0.6445
	[0.1018]	[0.1453]	[0.1180]	[0.1223]
N	508991	363565	363565	508991
B. Firms <25 Employees				
β	0.9051	0.9905	0.7873	0.7146
	[0.1378]	[0.2039]	[0.1451]	[0.1426]
N	337253	240895	240895	337253
C. Firms <50 Employees				
β	0.8925	1.0494	0.7665	0.7108
	[0.1159]	[0.1824]	[0.1274]	[0.1330]
N	419853	299895	299895	419853
D. Firms >100 Employees				
β	0.3923	0.7564	0.2393	0.2178
	[0.1670]	[0.1359]	[0.2305]	[0.2474]
N	34237	24455	24455	34237

Note: Each column and row represent a different regression. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include city-year, industry-year and establishment fixed effects. There are 327 cities and 23 industries. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990.

Table 6: IV Robustness Tests: Firms L<50

	Aggregation to 150 cities	Full-time	Share of Turks and Yugoslavs in 1986	Manufacturing	Industry IV (W)	Z + Industry IV (W)*	Positive employment 1991-1992
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
1989-1995: Z							
β	0.9004	0.7239	1.0477	0.7558	0.9629	0.9148	0.7447
	[0.1280]	[0.1216]	[0.1412]	[0.3057]	[0.1851]	[0.1111]	[0.0986]
F	11.0	83.4	27.6	24.0	54.7	58.5 (0.68)	104.7
N	419853	419853	419853	114114	419853	419853	189126
1991-1995: Z							
β	0.7236	0.6417	0.9685	0.5641	0.8809	0.8005	0.6568
	[0.1970]	[0.1403]	[0.1631]	[0.3236]	[0.2258]	[0.1282]	[0.0984]
F	10.8	76.0	23.1	19.4	38.3	46.8	131.3
N	299895	299895	299895	81510	299895	299895	135090
1989-1995: Z (Yr>1990)							
β	0.6937	0.6350	0.9458	0.6076	0.6500	0.6905	0.6116
	[0.1661]	[0.1381]	[0.1617]	[0.3105]	[0.2013]	[0.1269]	[0.0989]
F	10.8	74.1	25.4	20.1	53.5	54.1	143.3
N	419853	419853	419853	13650	419853	419853	189126

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Column (1) includes only 150 aggregate city codes, Column (4) includes only 13 industries within manufacturing. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990. All regressions use these instrumental variables except for columns (3), (5) and (6). Column (1) aggregates 327 city codes into 150 city codes. Column (2) restricts the sample to full-time workers. Column (3) modifies the IV by using the share of immigrants from Turkey and Yugoslavia in the firm in 1986 instead of using the share of immigrants in the firm in 1986. Column (4) restricts the sample to manufacturing firms. Column (5) uses as IV the share of immigrants outside the city of the relevant firm interacted by the supply shock of immigrants in the city of the relevant firm (W). W was calculated using the most detailed coding of industry. Column (6) includes instrumental variables (W,Z) in the IV regression. * means that the column includes the p-value of the overidentification test besides the F-statistic from the first stage (in parenthesis). Column (7) restricts the sample to those establishments with positive employment growth between 1991-1992.

Table 7: "Plausibly Exogenous" Instrument Robustness Check

	1989-1995			1991-1995		
IV	*Z (Yr>90)*			*Z*		
φ	Original	0.1	-0.1	Original	0.1	-0.1
A. All Firms						
β	0.6445	0.6207	0.6683	0.6817	0.6599	0.7035
	[0.1223]	[0.1279]	[0.1253]	[0.1180]	[0.1150]	[0.1170]
F		71.1			79.6	
N		508991			363565	
B. Firms <25 Employees						
β	0.7146	0.6917	0.7376	0.7873	0.7653	0.8093
	[0.1426]	[0.1485]	[0.1458]	[0.1451]	[0.1435]	[0.1466]
F		41.1			42.4	
N		337253			240895	
C. Firms <50 Employees						
β	0.7108	0.6874	0.7341	0.7665	0.7447	0.7883
	[0.1330]	[0.1344]	[0.1316]	[0.1274]	[0.1241]	[0.1265]
F		53.0			55.7	
N		419853			299895	
D. Firms >100 Employees						
β	0.2178	0.1969	0.2388	0.2393	0.2209	0.2576
	[0.2474]	[0.2511]	[0.2465]	[0.2305]	[0.2321]	[0.2287]
F		35.2			43.9	
N		34237			24455	

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 13 industries within manufacturing. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990. Sample restricted to establishments which increased total employment between 1991 and 1992.

Table 8: Displacement Effect for different lags

	1991-1995			
IV	*Z*			
Lag	1 year	2 year	3 year	4 year
A. All Firms				
β	0.6817	0.6992	0.8007	0.8575
	[0.1180]	[0.1349]	[0.1445]	[0.1974]
F	79.6	38.4	25.5	11.8
N	363565	363565	363565	363565
B. Firms <25 Employees				
β	0.7873	0.7558	0.8660	1.0365
	[0.1451]	[0.2369]	[0.2535]	[0.3423]
F	42.4	33.1	22.8	10.4
N	240895	240895	240895	240895
C. Firms <50 Employees				
β	0.7665	0.7503	0.8700	0.9902
	[0.1274]	[0.1814]	[0.1795]	[0.2404]
F	55.7	35.6	25.7	12.1
N	299895	299895	299895	299895
D. Firms >100 Employees				
β	0.2393	0.3624	0.5408	0.6690
	[0.2305]	[0.2090]	[0.1813]	[0.2152]
F	43.9	36.7	53.6	49.8
N	24455	24455	24455	24455

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Each column uses a different lag for employment growth and immigrant employment growth. Column 1 uses the results from Table (5), Columns 2-4 use as dependent (independent) variable the employment (immigrant) growth between year t and t-2, t-3 and t-4 respectively. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1, t-2, t-3, t-4 (depending on column 1-4) scaled by total employment in the city in 1986.

Table 9: Estimated Effect of a 1 percent increase in immigrant employment on wages

		σ_ρ						
σ_φ	ψ	2	4	6	8	10	15	20
No K	0.7	-0.5151	-0.3161	-0.2441	-0.2069	-0.1842	-0.1535	-0.1379
No K	0.8	-0.4863	-0.3065	-0.2390	-0.2036	-0.1819	-0.1522	-0.1370
No K	0.9	-0.4164	-0.2808	-0.2248	-0.1943	-0.1751	-0.1483	-0.1344
0.7	0.7	-0.5315	-0.3214	-0.2469	-0.2087	-0.1855	-0.1542	-0.1384
1	0.7	-0.5296	-0.3208	-0.2466	-0.2085	-0.1854	-0.1541	-0.1384
2	0.7	-0.5233	-0.3188	-0.2455	-0.2078	-0.1849	-0.1539	-0.1382
0.7	0.8	-0.5106	-0.3146	-0.2433	-0.2064	-0.1839	-0.1533	-0.1378
1	0.8	-0.5088	-0.3141	-0.2430	-0.2062	-0.1837	-0.1532	-0.1378
2	0.8	-0.5030	-0.3121	-0.2420	-0.2056	-0.1833	-0.1530	-0.1376
0.7	0.9	-0.4567	-0.2960	-0.2333	-0.1999	-0.1792	-0.1506	-0.1360
1	0.9	-0.4553	-0.2955	-0.2330	-0.1997	-0.1790	-0.1506	-0.1359
2	0.9	-0.4506	-0.2938	-0.2321	-0.1991	-0.1786	-0.1503	-0.1358

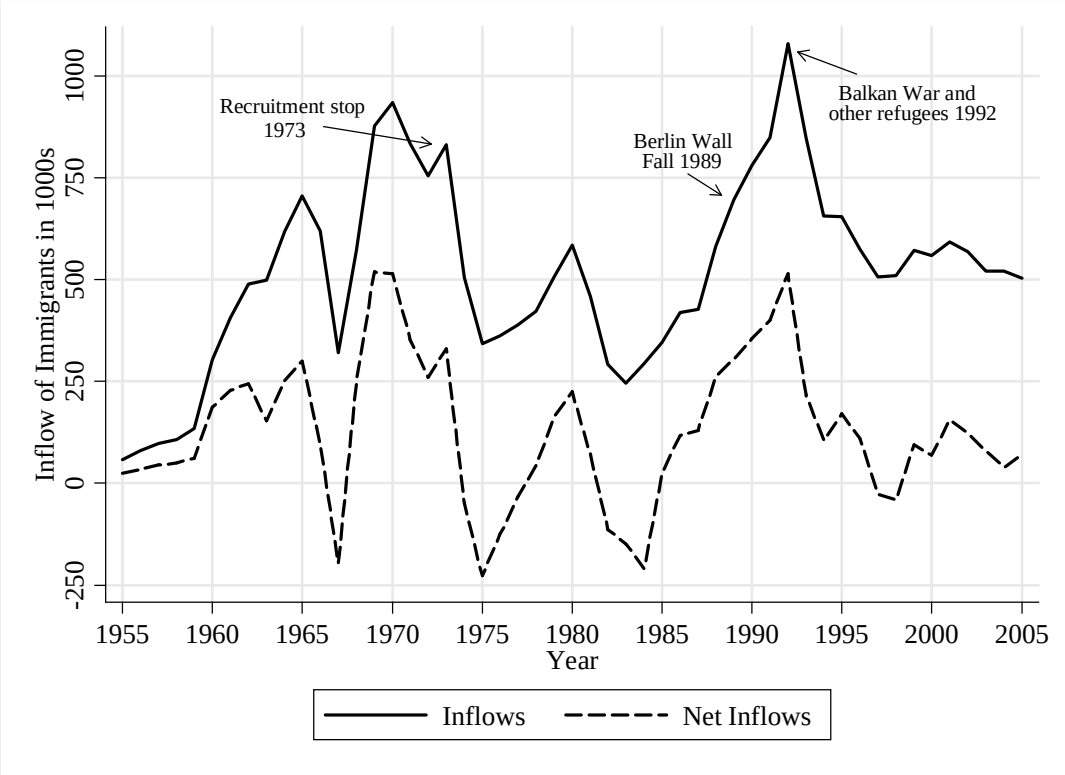
Note: The first column represents the elasticity of substitution between capital and labor, the second column represents the degree of homogeneity. Coefficients derived from Formula (13) defined in text. The first three rows assume No Capital. Share of Capital is 0.33 and Share of Labor is 0.67, the Share of Immigrants is 0.05. In the case of No Capital the Share of Immigrants is defined as .089 from the ratio 0.05/0.67. The wage gap is -0.09 and the ratio of immigrant wage over total average wage is equal to 0.91

Table 10: Effect on Wages

	1989-1995	**1991-1995**	**1989-1995**	**1989-1995**	**1991-1995**	**1989-1995**
IV	*Z*	*Z*	*Z (Yr>1990)*	*Z*	*Z*	*Z (Yr>1990)*
	[1]	[2]	[3]	[4]	[5]	[6]
		Avg Wage			Native Avg Wage	
A. All Firms						
γ	-0.0912	-0.1468	-0.1922	0.1364	0.1973	-0.0193
	[0.0391]	[0.0469]	[0.0490]	[0.0746]	[0.0948]	[0.1047]
F	95.3	85.4	76.2	105.9	99.6	85.0
N	508991	363565	508991	508222	362948	508222
B. Firms <25 Employees						
γ	-0.0974	-0.1404	-0.1722	0.1542	0.2400	-0.0285
	[0.0551]	[0.0623]	[0.0606]	[0.1027]	[0.1164]	[0.1270]
F	53.7	45.4	44.0	51.8	49.4	46.0
N	337253	240895	337253	336510	240297	336510
C. Firms <50 Employees						
γ	-0.0926	-0.1462	-0.1881	0.1524	0.2090	-0.0350
	[0.0472]	[0.0551]	[0.0558]	[0.0959]	[0.1171]	[0.1253]
F	71.1	59.8	56.9	75.3	67.1	61.6
N	419853	299895	419853	419089	299283	419089
D. Firms >100 Employees						
γ	-0.0156	-0.0659	-0.1260	0.1132	0.1343	0.0925
	[0.05556]	[0.0683]	[0.0714]	[0.0759]	[0.0881]	[0.0924]
F	62.4	47.5	37.5	62.4	47.5	37.5
N	34174	24410	34174	34174	24410	34174

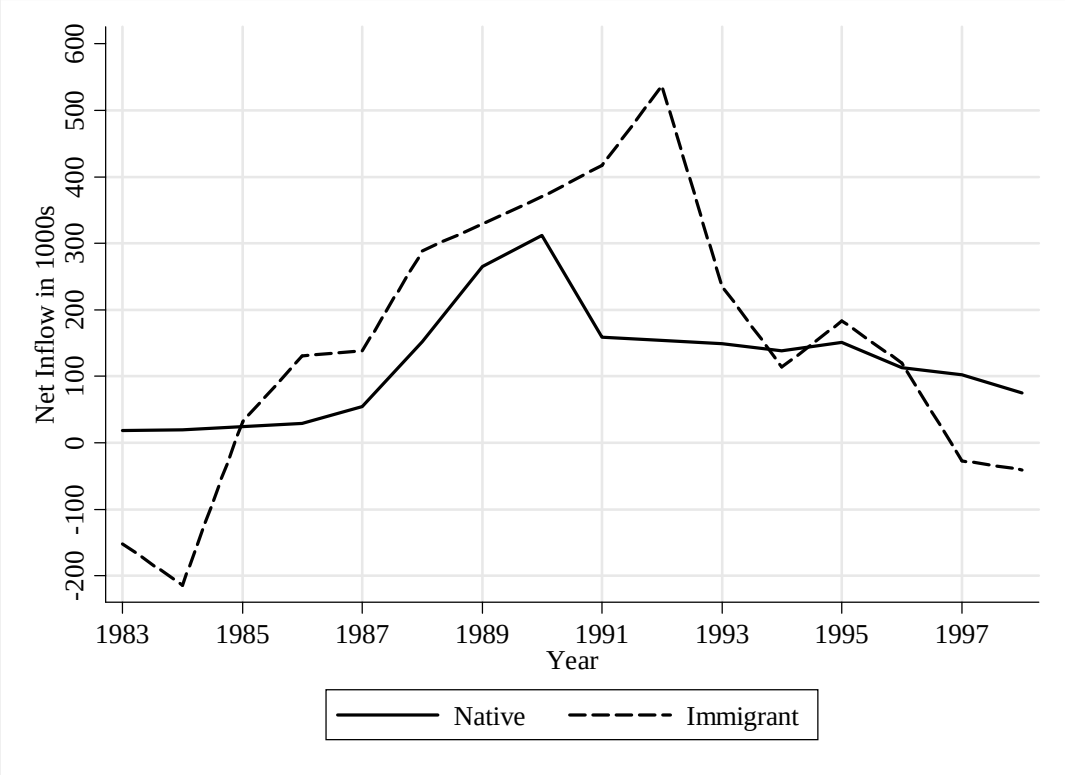
Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990.

Figure 1: Gross and Net Inflows of Immigration in Germany: 1955-2005



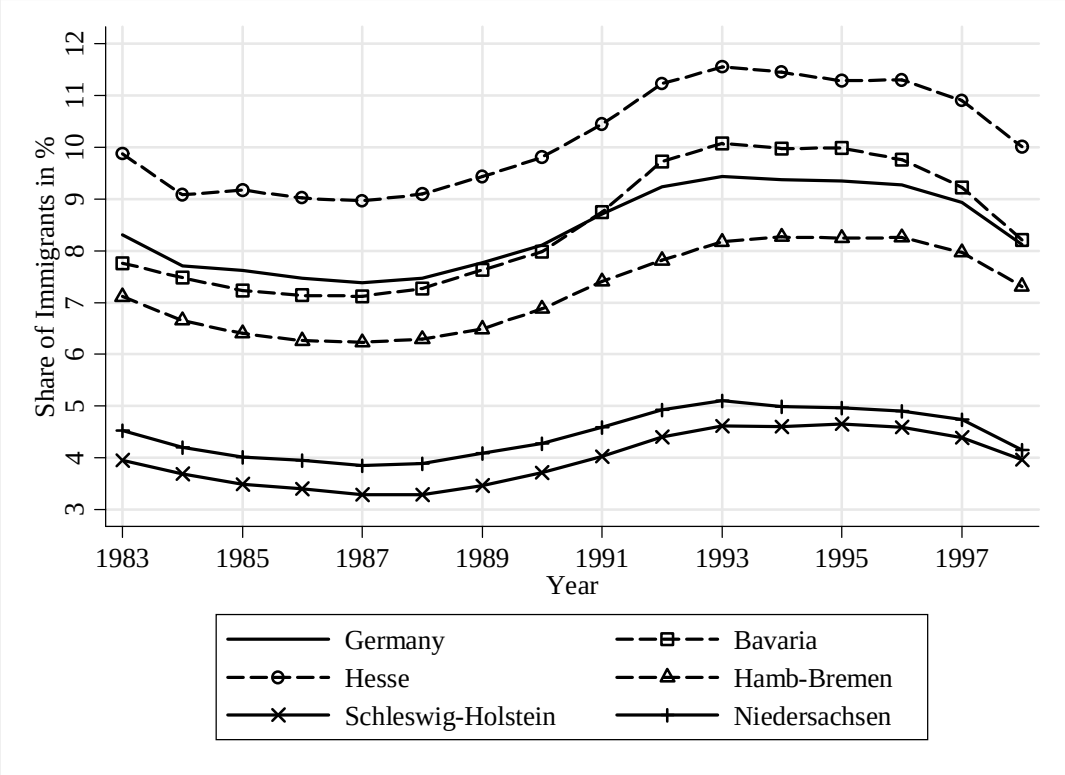
Note: Data from Statistical Office and Statistical Yearbook different years. Flows only for the Former Federal Republic of Germany excluding Berlin.

Figure 2: Net Inflows of Natives and Immigrants. 1983-1998.



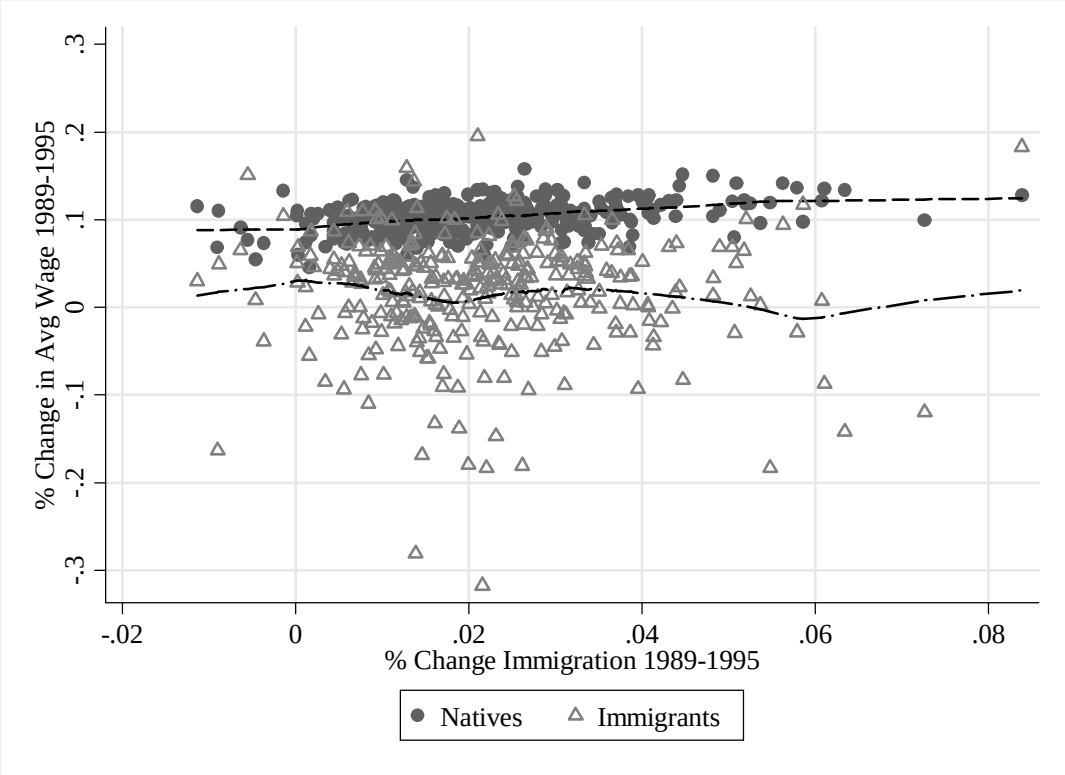
Note: Data from Statistical Office and Statistical Yearbook different years. Flows only for the Former Federal Republic of Germany excluding Berlin.

Figure 3: Share of Immigrants in Total Employment. Different States. 1983-1998.



Note: Total Employment and Immigrant Employment Data from Social Security Records obtained from Institute of Employment and Research (IAB). Germany includes only West Germany across all periods. Berlin is not included in the calculations.

Figure 4: % Change in Total and Native average wage wrt % Change in Immigrant Employment: 1989-1995

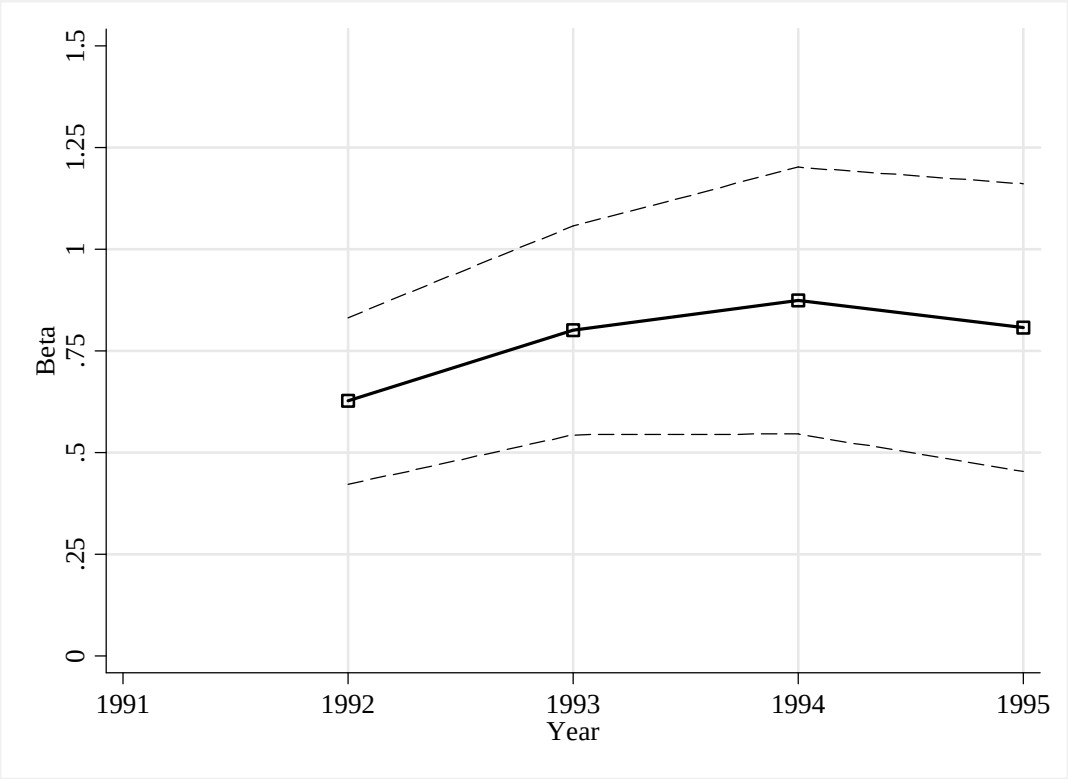


Note: Total Employment and Immigrant Employment across cities provided by the Institute of Employment and Research (IAB). Change in average wage calculated using final balanced sample as described in the text. Berlin is excluded. There are 326 points in the graph corresponding to each city in West Germany.

Figure 5: Percent Change in Total Labor and Immigrant Labor. Small firms (L<25) in Bavaria.

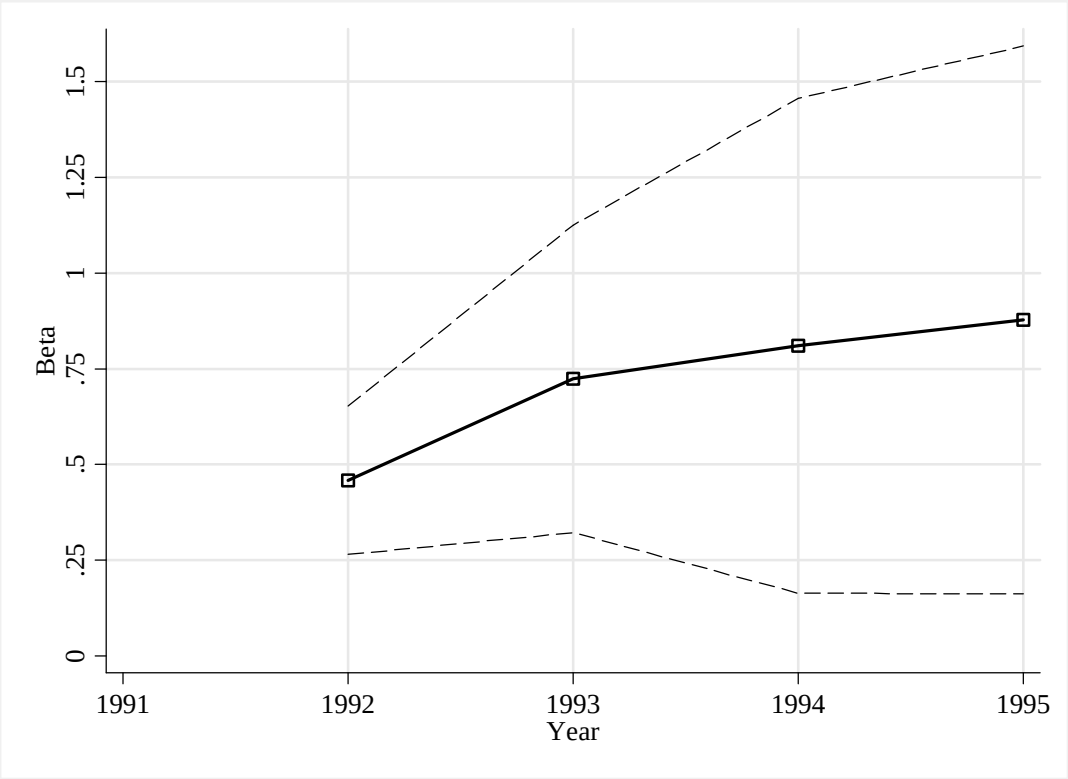
Note: Base year is 1991. Change in total and immigrant employment with respect to their respective values in 1991. Radius caliper 0.01 matching in the propensity score. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 6: Displacement Effect for small firms (L<25). Bavaria.



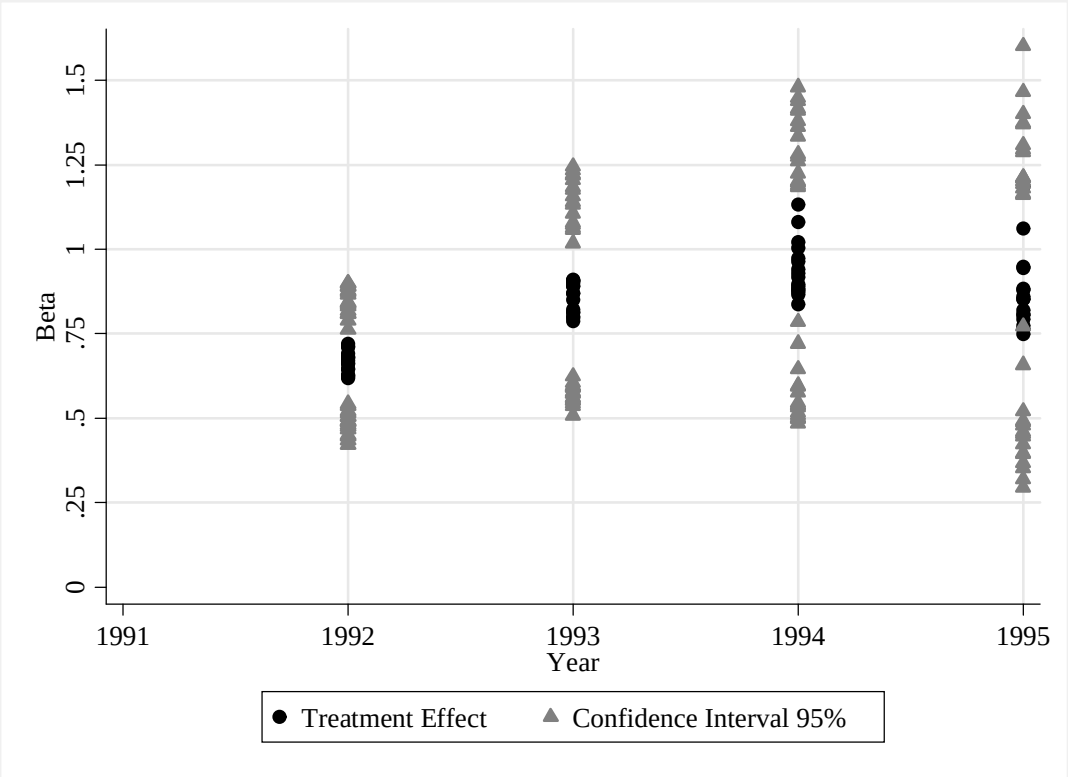
Note: Displacement effect defined as the effect of treatment on total employment growth over the effect of treatment on immigrant employment growth. Base year is 1991. Increase in total and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 7: Displacement effect for small firms (L<25). Hesse.



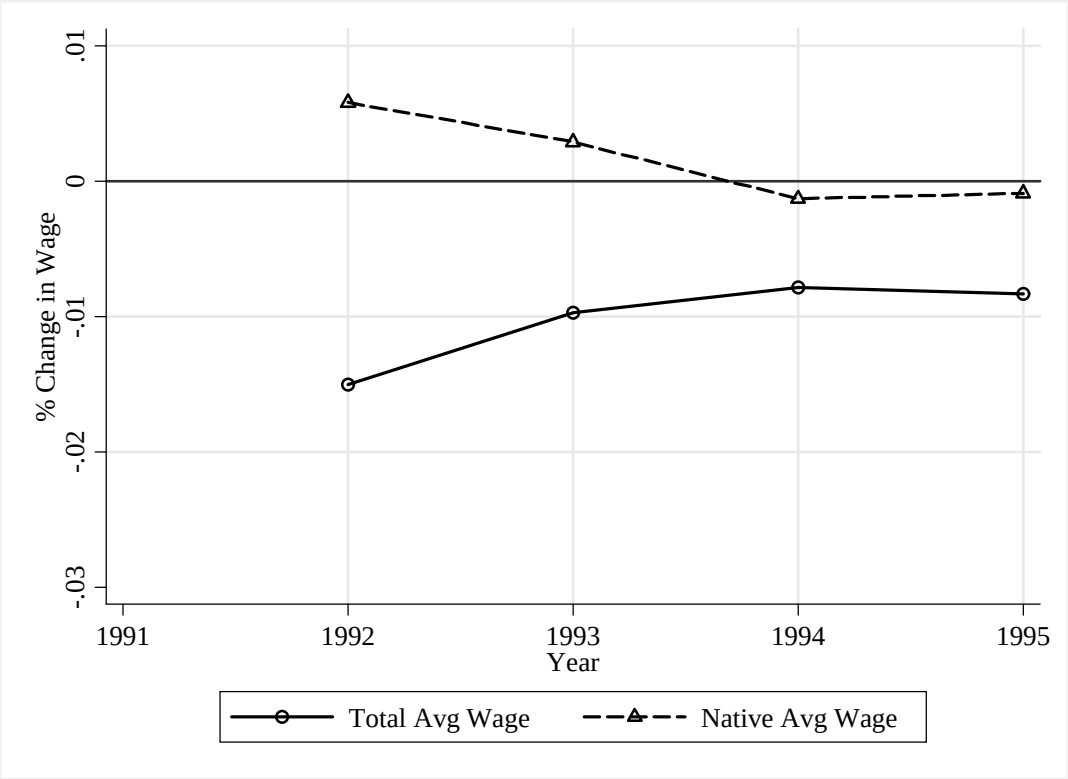
Note: Displacement effect defined as the effect of treatment on total employment growth over the effect of treatment on immigrant employment growth. Base year is 1991. Increase in total and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Hesse. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 8: Summary of Displacement Effect. Bavaria.



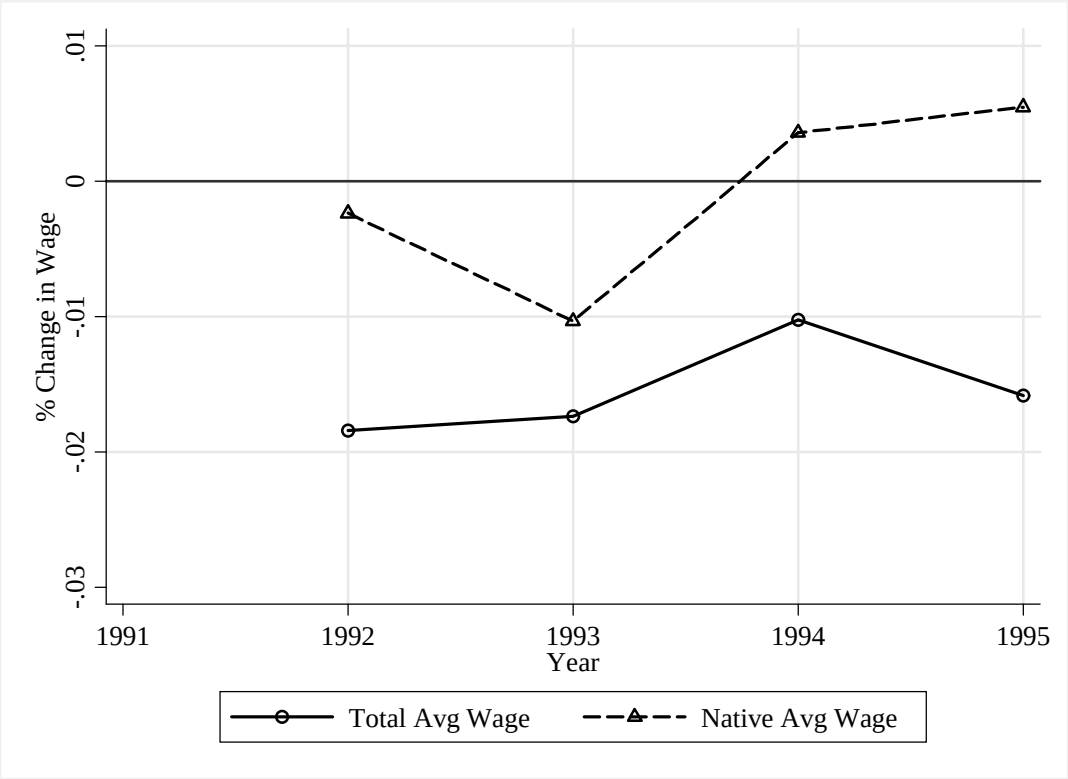
Note: Graph plots 16 different displacement effects. Displacement effect defined as the effect of treatment on total employment growth over the effect of treatment on immigrant employment growth. Base year is 1991. Increase in total and immigrant employment with respect to their value in 1991. Effects included: Small firms (L<25) and Medium-size firms (L<50); All and full-time employees; radius matching and nearest neighbor; optimal and square specification of the propensity score. Bootstrap standard errors, 100 replications. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 9: Wage Effect: Total Wages and Native Wages. Bavaria.



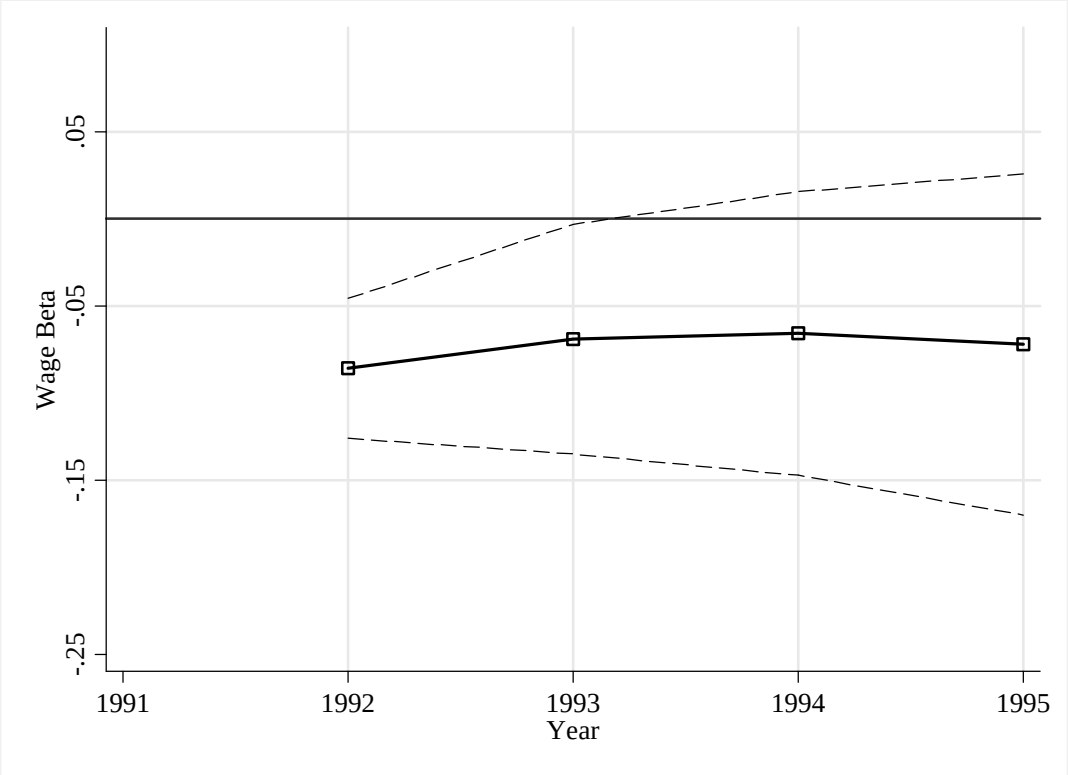
Note: Base year is 1991. Change in total average wage and native average wage with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 10: Wage Effect: Total Wages and Native Wages. Hesse.



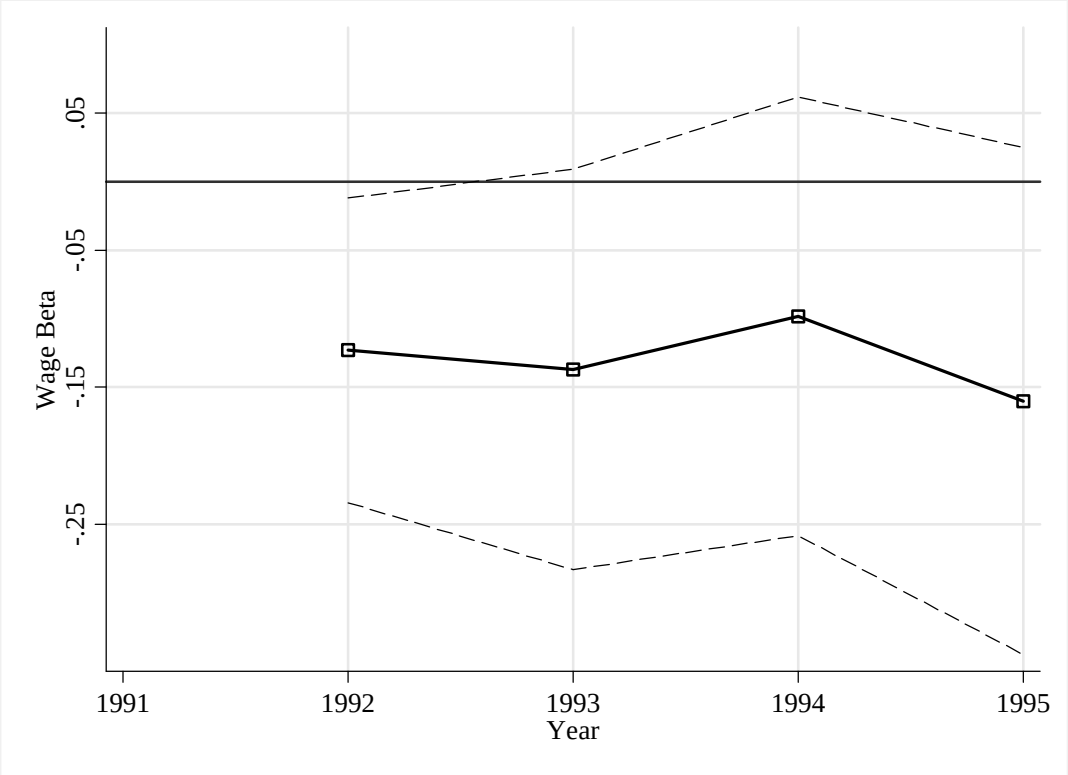
Note: Base year is 1991. Change in total average wage and native average wage with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 11: Wage Effect in terms of percent of Change of Immigrant labor. Small firms (L<25) in Bavaria.



Note: Wage effect defined as the effect of treatment on average wage growth over the effect of treatment on immigrant employment growth. Base year is 1991. Change in wage and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Figure 12: Wage Effect in terms of percent of Change of Immigrant labor. Small firms (L<25) in Hesse.



Note: Wage effect defined as the effect of treatment on average wage growth over the effect of treatment on immigrant employment growth. Base year is 1991. Change in wage and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score. Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method. Firms with less than 25 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

A Appendix

A.1 Data

A.1.1 Data Cleaning

Table (A-1) compares establishments across data restrictions for years 1989 and 1992. The final sample is similar to the original sample. A balanced panel is biased towards large establishments while restricting for outliers or large changes in employment mainly affects large establishments leading to a balancing of the sample towards the original sample.

Table A-1: Descriptive Statistics for different samples

	[1] Original		[2] Balanced Panel		[3] Final Sample	
Variable	**1989**	**1992**	**1989**	**1992**	**1989**	**1992**
Labor	45.73	47.24	55.05	58.49	39.15	42.58
	(360.29)	(353.16)	(421.81)	(421.10)	(95.67)	(101.13)
% Hiring Immig	42.44	51.99	43.65	53.43	42.99	52.87
Share Immig (%)	7.94	9.51	7.76	9.16	6.99	8.71
Share Immig Non-EU (%)	5.05	6.16	4.89	5.95	4.44	5.73
Share Unqualified (%)	22.03	21.87	21.87	21.56	20.00	20.25
Share College (%)	6.35	6.93	6.61	7.12	5.32	5.82
Share Age 30-44 (%)	34.06	37.64	34.11	37.56	33.64	37.04
Share Age > 44 (%)	32.94	31.56	33.24	32.22	32.5	31.75
% < 25 Employees	72.35	70.66	68.09	65.22	68.97	66.05
% <50 Employees	85.54	84.7	82.77	81.02	83.82	82.04
% + 100 Employees	7.21	7.63	8.91	9.79	7.79	8.69
% No Natives	0.23	0.46	0.09	0.16	0.09	0.16
Avg Wage (2004 Euro)	86.02	90.6	86.97	91.84	82.45	87.56
Avg Wage Natives	86.72	91.6	87.65	92.82	82.98	88.43
Avg Wage Immig	77.90	81.08	78.89	82.12	75.40	78.44
N	102664	106569	73690	73690	72713	72713

A.1.2 Definition of Variables

- Immigrant: Any employee who is defined as non-national.
- Immigrant from EU countries: Western and Central Europe, United States, Canada and Australia.
- Immigrant from non-EU countries: Former Yugoslavia, Turkey, Greece, Russia, Eastern Europe and rest of countries.
- Cities: There are 327 district (ao_kreis). This variable is the most disaggregated variable for geographic codes. I also use a variable which includes only 150 codes.
- Industry: I aggregate information for 23 industries using the three digit classification of German industries according to year 1973 (WS1973). The classification is as follows:
  - Agriculture and Utilities: WS1973 codes 0-49.
  - Mining: 0-59.
  - Manufacturing: Chemical Products and Plastics: 90-139.
  - Manufacturing: Glass and Stones: 140-169.
  - Manufacturing: Iron and Steel: 170-229.

- Manufacturing: Metal Products: 230-249.
- Manufacturing: Metal Equipment: 250-279.
- Manufacturing: Motor vehicle: 280-329.
- Manufacturing: Electrical equipment: 330-349.
- Manufacturing: Mechanical products: 350-359.
- Manufacturing: Precision Instruments: 360-399.
- Manufacturing: Wood and Furniture: 400-429.
- Manufacturing: Paper and publishing: 430-449.
- Manufacturing: Textile and Leather: 450-539.
- Manufacturing: Food and Tobacco: 540-589.
- Construction: 590-619.
- Wholesale and Retail Trade: 620-629.
- Transportation and Communication: 630-689.
- Finance and Banking: 690-699.
- Hotels and Restaurants: 700-719.
- Cleaning: 720-739.
- Education: 740-759.
- Other Services: 760-999.

Table (A-2) shows the distribution of plants across industries. In order to save space, I aggregated a few industries. A very small number of firms switched industries in the period. The distribution of employment across the same industries can be seen in Table (A-3).

Table A-2: Industry Composition

	[1] Original		[2] Balanced Panel		[3] Final Sample	
Industries	**1989**	**1992**	**1989**	**1992**	**1989**	**1992**
Agriculture, Fishing, Mining, Utilities	2.0	2.0	2.2	2.1	2.1	2.1
Manufacturing	28.2	27.6	28.8	28.9	28.6	28.7
Construction	13.4	13.1	13.4	13.4	13.6	13.6
Whole and Retail Trade	19.8	19.5	18.7	18.6	18.8	18.7
Transportation	5.9	6.0	5.3	5.3	5.3	5.3
Finance and Banking	3.1	3.0	3.6	3.6	3.6	3.6
Hotels and Restaurants	5.1	5.2	4.6	4.6	4.6	4.6
Cleaning	1.7	1.7	1.6	1.6	1.6	1.6
Education	2.6	2.6	3.1	3.0	3.1	3.1
Other Services	18.2	19.4	18.8	18.8	18.7	18.8
N	102664	106569	73690	73690	72713	72713

Table A-3: Share of Total Employment across industries

	Total Employment		**Immig Employment**	
Industries	**1989**	**1995**	**1989**	**1995**
Agriculture, Fishing, Mining, Utilities	0.024	0.022	0.017	0.017
Manufacturing	0.344	0.326	0.482	0.426
Construction	0.080	0.083	0.118	0.122
Whole and Retail Trade	0.140	0.139	0.083	0.103
Transportation	0.048	0.046	0.039	0.041
Finance and Banking	0.053	0.055	0.010	0.012
Hotels and Restaurants	0.036	0.040	0.060	0.068
Cleaning	0.011	0.013	0.032	0.042
Education	0.036	0.036	0.025	0.023
Other Services	0.228	0.240	0.134	0.146

- Education: There are five educational categories. I construct a variable with three educational categories. Low education: I include low education and missing values here. Medium Education: Workers with vocational training or completed High School. High Education: University degree.

- Occupation: Four occupational categories: Trainees and part time, unqualified, mid-qualified and high-qualified workers.

- Wages. All wages are in real terms defined in 2004 Euros. Wages refer to average daily gross wage.

A.2 Immigration Statistics

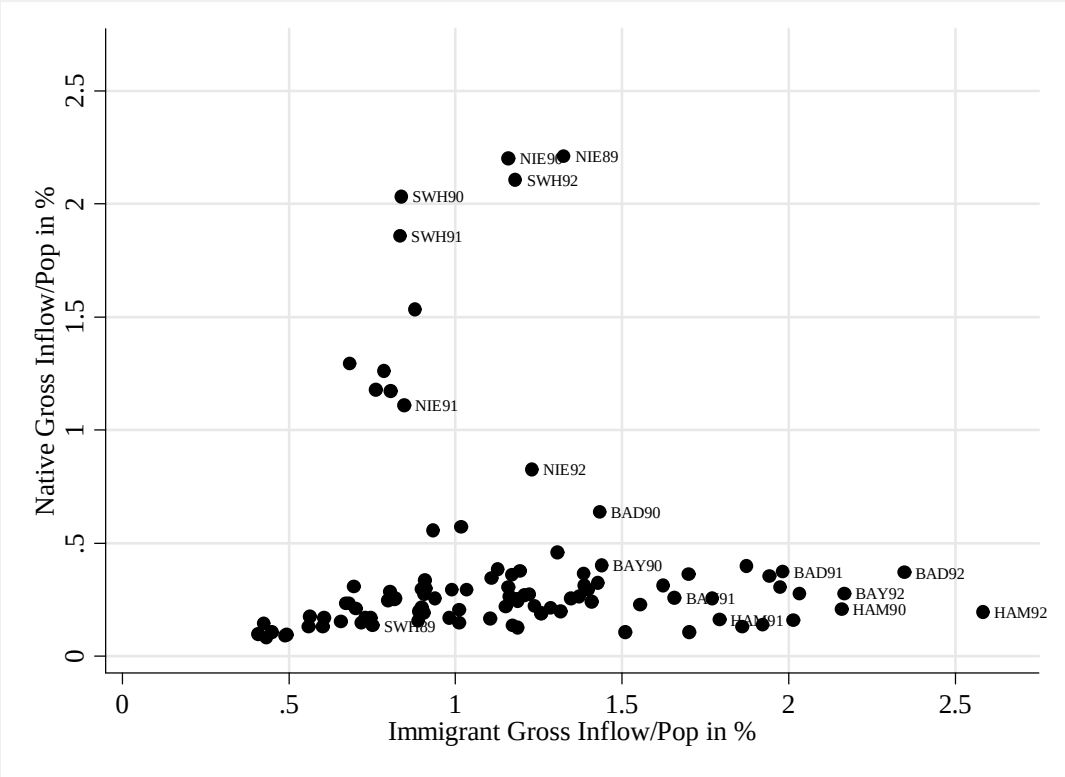
This section includes additional figures and statistics to further corroborate that the immigration shock was important in terms of population and that it was heterogenous across states.

- Figure (A-1) includes gross inflows of Ethnic Germans and Immigrants by state-year. It shows that immigrants settled in southern states while Ethnic Germans settled in the northern states (except Hamburg and Bremen).

- Table (A-4) shows the share of immigrants in the population and in total employment across state-years.

- Table (A-5) shows the influx of immigrants by state and year in terms of total and immigrant population in previous period.

Figure A-1: Gross Inflows as % of Population in previous period across states: 1985-1995.



Note: Inflow and Population Data from Statistical Office and Statistical Yearbooks different years. Each dot represents a different combination of state-year. BAD=Baden-Württemberg, BAY=Bayern or Bavaria, HAM=Hamburg, NIE=Niedersachsen or Upper Saxony, SWH=Schleswig-Holstein. Berlin is not included in the states.

Table A-4: Share of Immigrants by State in terms of population and employment

State		1986	1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Germany	Pop	7.4	6.9	7.3	7.7	8.2	8.8	9.5	9.9	10.1	10.3	10.4
	Emp	7.5	7.4	7.5	7.8	8.1	8.7	9.2	9.4	9.4	9.4	9.3
Baden	Pop	9.3	9.3	9.7	10.1	10.1	10.9	11.7	12.3	12.3	12.4	12.4
	Emp	10.5	10.3	10.4	10.7	11.1	12.1	13.2	13.3	13.1	13.1	13.0
Bavaria	Pop	6.3	5.8	6.2	6.6	7.2	7.9	8.4	8.9	9.0	9.1	9.2
	Emp	7.1	7.1	7.3	7.6	8.0	8.8	9.7	10.1	10.0	10.0	9.8
Bremen	Pop	7.5	7.0	7.6	8.5	9.2	10.1	11.1	11.3	11.5	11.9	12.1
	Emp	4.8	4.7	4.9	5.1	5.4	5.7	6.1	6.2	6.3	6.4	6.4
Hamburg	Pop	11.2	9.4	9.7	10.2	11.8	12.8	13.9	14.7	15.4	16.1	16.9
	Emp	6.8	6.8	6.9	7.1	7.5	8.1	8.5	8.9	9.0	9.0	9.0
Hesse	Pop	9.5	8.7	9.1	9.8	10.5	11.6	12.6	13.2	13.3	13.6	13.8
	Emp	9.0	9.0	9.1	9.4	9.8	10.5	11.2	11.6	11.5	11.3	11.3
Niedersachsen	Pop	4.0	3.7	4.0	4.3	4.5	4.9	5.6	5.8	5.9	6.0	6.1
	Emp	4.0	3.9	3.9	4.1	4.3	4.6	4.9	5.1	5.0	5.0	4.9
North-Rhine Westphalia	Pop	8.2	7.7	8.1	8.5	9.0	9.6	10.3	10.6	10.7	11.0	11.1
	Emp	7.4	7.3	7.4	7.7	8.0	8.6	9.2	9.4	9.4	9.4	9.4
Rhineland Pfalz	Pop	4.7	4.3	4.5	4.9	5.3	6.0	6.7	7.0	7.1	7.3	7.5
	Emp	4.8	4.7	4.8	5.0	5.4	5.9	6.7	6.9	6.9	7.1	7.0
Saarland	Pop	4.5	4.2	4.5	4.8	5.3	5.7	6.3	6.6	7.0	7.2	7.4
	Emp	5.2	5.0	5.1	5.5	5.9	7.0	7.7	8.0	8.4	8.8	8.9
Schleswig Holstein	Pop	3.3	3.0	3.2	3.5	3.9	4.2	4.7	4.9	4.9	5.0	5.1
	Emp	3.4	3.3	3.3	3.5	3.7	4.0	4.4	4.6	4.6	4.7	4.6

Note: Population from Statistical Office and Statistical Yearbook different years. Employment data constructed based on Social Security Records obtained from IAB. Germany includes only West Germany across all periods. Berlin is not included in the calculations. Pop refers to Population and Emp to employment.

Table A-5: Influx of Immigrants by state in terms of total and immigrant population in previous period

State		1987	1988	1989	1990	1991	1992	1993	1994	1995	1996
Germany	Pop	0.8	1.1	1.2	1.3	1.4	1.7	1.3	1.0	1.0	0.9
	Immig	10.5	15.3	17.1	17.3	17.0	19.5	14.1	10.4	10.3	8.8
Baden	Pop	0.9	1.2	1.3	1.4	2.0	2.4	1.9	1.4	1.4	1.2
	Immig	10.1	12.8	13.5	14.2	19.6	21.5	16.0	11.6	11.3	9.8
Bavaria	Pop	0.7	1.0	1.2	1.4	1.7	2.2	1.6	1.2	1.2	1.0
	Immig	11.9	17.5	19.3	21.9	23.1	27.4	19.3	13.4	12.8	11.0
Bremen	Pop	1.2	1.9	2.0	1.9	1.5	1.7	1.3	1.0	1.2	0.9
	Immig	15.9	26.7	26.7	22.7	16.3	16.9	11.3	8.7	10.2	7.8
Hamburg	Pop	1.4	1.8	1.9	2.2	1.8	2.6	2.0	1.4	1.3	1.2
	Immig	12.2	18.8	20.1	21.2	15.2	20.2	14.2	9.6	8.4	7.7
Hesse	Pop	0.9	1.1	1.2	1.4	1.6	2.0	1.7	1.2	1.2	1.1
	Immig	9.4	12.7	12.7	14.3	14.8	17.6	13.5	9.2	9.3	7.8
Niedersachsen	Pop	0.4	0.7	1.3	1.2	0.9	1.2	0.9	0.8	0.8	0.7
	Immig	10.6	18.6	33.5	27.1	18.8	25.0	16.1	13.8	13.4	11.1
North-Rhine Westphalia	Pop	0.7	0.9	1.0	1.1	1.1	1.3	1.0	0.8	0.8	0.7
	Immig	8.2	12.1	12.6	13.3	12.2	13.7	9.7	7.5	7.6	6.7
Rhineland Pfalz	Pop	0.6	0.8	1.0	1.2	1.4	1.4	1.2	0.9	0.9	0.8
	Immig	12.1	19.1	22.8	24.4	25.2	23.2	17.5	13.0	12.7	11.0
Saarland	Pop	0.5	0.6	0.7	0.9	0.8	1.2	0.9	0.7	0.7	0.6
	Immig	10.0	14.5	16.4	18.8	15.1	20.5	14.3	10.3	10.0	8.9
Schleswig Holstein	Pop	0.4	0.6	0.8	0.8	0.8	1.2	0.9	0.7	0.8	0.6
	Immig	12.2	19.9	23.5	23.9	21.7	28.2	18.7	14.0	15.6	11.5

Note: Population from Statistical Office and Statistical Yearbook different years. Employment data constructed based on Social Security Records obtained from IAB. Germany includes only West Germany across all periods. Berlin is not included in the calculations. Pop refers to Population and Emp to employment.

A.3 Additional Results

This section includes additional results to corroborate the displacement effect estimate.

- Table (A-6) presents OLS and IV estimates using first-differences.
- Table (A-7) includes IV estimates for the full sample including outliers.
- Table (A-8) includes IV estimates for a balanced sample of firms for period 1984-1995. Estimates are close to 0.70 as pointed in the text.
- Table (A-9) includes the dynamic effects on displacement restricting the sample to establishments with positive employment growth between 1991-1992.
- Tables (A-10)-(A-11) include robustness checks for all firms and small firms.
- Tables (A-12)-(A-13) provide evidence on the balance of covariates in the propensity score specification.
- Tables (A-14)-(A-16) include the results of the propensity score approach using the radius and nearest neighbor matching.

Table A-6: OLS and IV Estimates using First-Differences

	1989-1995				1989-1993				1991-1995		
	[1]	[2]	[3]	[4]	[5]	[6]	[7]	[8]	[9]	[10]	[11]
	OLS	*1st St*	*Z*	*Z (Yr>90)*	*OLS*	*1st St*	*Z*	*Z (Yr>90)*	*OLS*	*1st St*	*Z*
A. All Firms											
β	1.3729	4.7960	0.7576	0.6212	1.3469	5.1930	0.8482	0.6827	1.3959	4.5149	0.7983
	[0.0587]	[0.6212]	[0.1318]	[0.2015]	[0.0742]	[0.7894]	[0.1439]	[0.2364]	[0.0635]	[0.5884]	[0.1499]
F			59.60	36.36			43.30	24.70			58.83
N			436278				290852			290852	
B. Firms <25 Employees											
β	1.1779	4.886	0.8315	0.6938	1.1719	5.226	0.8703	0.697	1.2166	4.3601	0.9719
	[0.0416]	[0.8306]	[0.1593]	[0.2303]	[0.0481]	[1.0343]	[0.1832]	[0.2773]	[0.0548]	[0.7905]	[0.1853]
F			34.60	9.08			25.53	16.24			30.42
N			289074				192716			192716	
C. Firms <50 Employees											
β	1.2854	4.7955	0.7863	0.6544	1.2835	5.1219	0.8403	0.6748	1.3136	4.4406	0.8653
	[0.0585]	[0.7171]	[0.1487]	[0.2206]	[0.0789]	[0.8938]	[0.1678]	[0.2637]	[0.0600]	[0.6849]	[0.1640]
F			44.72	29.16			32.84	19.89			42.04
N			359874				239916			239916	
D. Firms >100 Employees											
β	1.9846	5.9554	0.4025	0.453	1.8177	7.35	0.4677	0.5162	2.0654	5.6637	0.3837
	[0.0968]	[0.9368]	[0.2084]	[0.2140]	[0.1180]	[1.0249]	[0.1657]	[0.1856]	[0.1241]	[1.0330]	[0.2507]
F			40.41	45.16			51.43	50.55			30.06
N			29292				19528			19528	

Note: Each column and row represent a different regression. F-statistic from the first stage regression below IV standard errors. Estimates from first-differencing data. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include city-year, industry-year and establishment fixed effects. There are 327 cities and 23 industries. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990.

Table A-7: IV Estimates for full sample including outliers

	1989-1995	**1989-1993**	**1991-1995**	**1989-1995**
IV	*Z*	*Z*	*Z*	*Z (Yr>1990)*
	[1]	[2]	[3]	[4]
A. All Firms				
β	0.9024	1.2307	0.6434	0.5791
	[0.2176]	[0.2086]	[0.1205]	[0.1708]
F	53.3	47.3	95.8	83.4
N	515830	368450	368450	515830
B. Firms <25 Employees				
β	0.8723	0.9020	0.7719	0.7014
	[0.1474]	[0.2153]	[0.1546]	[0.1484]
F	52.9	23.4	45.6	44.4
N	337351	240965	240965	337351
C. Firms <50 Employees				
β	0.8568	0.9700	0.7475	0.6963
	[0.1219]	[0.1827]	[0.1297]	[0.1314]
F	69.7	30.3	59.6	57.2
N	420042	300030	300030	420042
D. Firms >100 Employees				
β	-0.1703	0.9082	-0.2344	-0.3587
	[1.0705]	[0.2542]	[0.9257]	[0.9869]
F	4.0	83.2	6.2	5.5
N	40194	28710	28710	40194

Note: Each column and row represent a different regression. F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include city-year, industry-year and establishment fixed effects. There are 327 cities and 23 industries. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990. Sample includes outliers.

Table A-8: IV Estimates using sample from 1984-1995

	1989-1995	**1991-1995**	**1989-1995**
IV	*Z*	*Z*	*Z (Yr>1990)*
	[1]	[2]	[3]
A. All Firms			
β	0.7295	0.6367	0.5454
	[0.1192]	[0.1518]	[0.1681]
F	69.9	56.3	49.1
N	471996	337140	471996
B. Firms <25 Employees			
β	0.7939	0.6915	0.5605
	[0.1766]	[0.2031]	[0.2171]
F	34.0	28.0	26.5
N	300902	214930	300902
C. Firms <50 Employees			
β	0.7931	0.6937	0.5914
	[0.1433]	[0.1734]	[0.1902]
F	46.2	36.7	34.7
N	382788	273420	382788
D. Firms >100 Employees			
β	0.6780	0.5341	0.5387
	[0.1246]	[0.1625]	[0.1624]
F	63.4	40.3	37.1
N	30618	21870	30618

Note: Each column and row represent a different regression. F-statistic from the first stage regression below IV standard errors. Balanced panel of firms from 1984-1995. Robust standard errors with clustering at the city level are in brackets. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include city-year, industry-year and establishment fixed effects. There are 327 cities and 23 industries. Z is constructed as the share of immigrants in 1984 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1984. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990.

Table A-9: Displacement Effect for different lags. Sample of establishments with positive employment growth between 1991 and 1992.

	1991-1995			
IV	*Z*			
Lag	1 year	2 year	3 year	4 year
A. All Firms				
β	0.6140	0.6493	0.7594	0.9627
	[0.0876]	[0.1356]	[0.1679]	[0.3317]
F	153.0	38.8	20.3	6.6
N	168605	168605	168605	168605
B. Firms <25 Employees				
β	0.7066	0.7259	0.8300	1.1643
	[0.1160]	[0.1978]	[0.2871]	[0.5838]
F	96.8	33.2	16.8	4.7
N	105780	105780	105780	105780
C. Firms <50 Employees				
β	0.6568	0.6701	0.8056	1.1024
	[0.0984]	[0.1650]	[0.2055]	[0.4186]
F	131.3	36.5	19.6	6.1
N	135090	135090	135090	135090
D. Firms >100 Employees				
β	0.4668	0.4192	0.4138	0.2876
	[0.1581]	[0.1871]	[0.2412]	[0.4373]
F	53.7	30.5	24.3	11.8
N	12850	12850	12850	12850

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. In other words, if a firm is always less than 25 employees for the period 1986-1989 then that firm is defined as a firm with less than 25 employees. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Each column uses a different lag for employment growth and immigrant employment growth. Column 1 uses the results from Table (5), Columns 2-4 use as dependent (independent) variable the employment (immigrant) growth between year t and t-2, t-3 and t-4 respectively. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1, t-2, t-3, t-4 (depending on column 1-4) scaled by total employment in the city in 1986. Sample restricted to establishments with positive employment growth between 1991-1992.

Table A-10: IV Robustness Tests. All Firms.

	Aggregation to 150 cities	Full-time	Share of Turks and Yugoslavs in 1986	Manufacturing	Industry IV (W)	Z + Industry IV (W)*	Positive employment 1991-1992
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
1989-1995: Z							
β	0.8170	0.6841	0.8709	0.7695	0.9069	0.8296	0.6980
	[0.1087]	[0.1068]	[0.1362]	[0.2091]	[0.1702]	[0.0978]	[0.0893]
F	10.4	107.1	41.5	42.3	41.3	63.1 (0.51)	116.6
N	508991	508991	508991	145383	508991	508991	236047
1991-1995: Z							
β	0.6478	0.6020	0.8166	0.6585	0.7907	0.7083	0.6140
	[0.1780]	[0.1277]	[0.1669]	[0.2351]	[0.2089]	[0.1175]	[0.0876]
F	11.0	104.7	36.2	33.5	33.2	59.0 (0.54)	153.0
N	363565	363565	363565	103845	363565	363565	168605
1989-1995: Z (Yr>1990)							
β	0.6290	0.6051	0.8530	0.7759	0.5182	0.6097	0.5698
	[0.1527]	[0.1286]	[0.1628]	[0.2267]	[0.1899]	[0.1185]	[0.0903]
F	10.4	96.4	38.1	33.6	39.9	58.6 (0.45)	168.7
N	508991	508991	508991	145383	508991	508991	236047

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Column (1) includes only 150 aggregate city codes, Column (4) includes only 13 industries within manufacturing. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990. All regressions use these instrumental variables except for columns (3), (5) and (6). Column (1) aggregates 327 city codes into 150 city codes. Column (2) restricts the sample to full-time workers. Column (3) modifies the IV by using the share of immigrants from Turkey and Yugoslavia in the firm in 1986 instead of using the share of immigrants in the firm in 1986. Column (4) restricts the sample to manufacturing firms. Column (5) uses as IV the share of immigrants outside the city of the relevant firm interacted by the supply shock of immigrants in the city of the relevant firm (W). W was calculated using the most detailed coding of industry. Column (6) includes instrumental variables (W,Z) in the IV regression. * means that the column includes the p-value of the overidentification test besides the F-statistic from the first stage (in parenthesis). Column (7) restricts the sample to those establishments with positive employment growth between 1991-1992.

Table A-11: IV Robustness Tests. Small Firms (L<25).

	Aggregation to 150 cities	Full-time	Share of Turks and Yugoslavs in 1986	Manufacturing	Industry IV (W)	Z + Industry IV (W)*	Positive employment 1991-1992
	[1]	[2]	[3]	[4]	[5]	[6]	[7]
1989-1995: Z							
β	0.9055	0.7311	1.0871	0.9617	1.1076	0.9720	0.7791
	[0.1493]	[0.1433]	[0.1721]	[0.3668]	[0.2155]	[0.1308]	[0.1231]
F	9.2	60.7	21.9	21.3	40.3	43.6 (0.32)	81.0
N	337253	337253	337253	89586	337253	337253	148092
1991-1995: Z							
β	0.7164	0.6619	1.0298	0.8413	1.0440	0.8639	0.7066
	[0.2202]	[0.1658]	[0.1977]	[0.3829]	[0.2637]	[0.1495]	[0.1160]
F	10.0	54.2	20.3	17.1	32.5	39.8 (0.25)	96.8
N	240895	240895	240895	63990	240895	240895	105780
1989-1995: Z (Yr>1990)							
β	0.6802	0.6449	0.9810	0.8193	0.7460	0.7247	0.6435
	[0.1867]	[0.1596]	[0.1875]	[0.3575]	[0.2296]	[0.1416]	[0.1152]
F	9.4	53.3	23.8	17.7	40.5	43.4 (0.87)	105.7
N	337253	337253	337253	89586	337253	337253	148092

Note: F-statistic from the first stage regression below IV standard errors. Robust standard errors with clustering at the city level. Firm size selected according to the employment history of 1986-1989. All regressions include establishment, city-year and industry-year fixed effects. There are 327 districts and 23 industries. Column (1) includes only 150 aggregate city codes, Column (4) includes only 13 industries within manufacturing. Z is constructed as the share of immigrants in 1986 at the establishment level interacted by the increase in immigrant employment at the city level between year t and t-1 scaled by total employment in the city in 1986. Z (Yr>1990) is constructed as Z but interacted with an indicator variable valid for years later than 1990. All regressions use these instrumental variables except for columns (3), (5) and (6). Column (1) aggregates 327 city codes into 150 city codes. Column (2) restricts the sample to full-time workers. Column (3) modifies the IV by using the share of immigrants from Turkey and Yugoslavia in the firm in 1986 instead of using the share of immigrants in the firm in 1986. Column (4) restricts the sample to manufacturing firms. Column (5) uses as IV the share of immigrants outside the city of the relevant firm interacted by the supply shock of immigrants in the city of the relevant firm (W). W was calculated using the most detailed coding of industry. Column (6) includes instrumental variables (W,Z) in the IV regression. * means that the column includes the p-value of the overidentification test besides the F-statistic from the first stage (in parenthesis). Column (7) restricts the sample to those establishments with positive employment growth between 1991-1992.

Table A-12: Balance of Propensity Score across specifications

Model	Hit and Miss Mean[a]	Std Bias: Before Matching[b]	Std Bias: After Matching[c]	Pseudo R2 Before[d]	Pseudo R2 After[e]	LR Test Match[f]	Stratif % Unbalanced 25 Blocks[g]	Matching: % Reject[h]	% (#) Treated Outside Radius as % T
Bavaria									
L<25									
Optimal	41.8	16.71	1.94	14.2	0.7	1	0.75	0	2.4 (34)
Squares	41.0	19.33	1.61	16.4	1.4	1	0.61	0	1.4 (20)
L<50									
Optimal	42.6	18.59	2.16	15.0	1.0	1	1.96	0	0.9 (17)
Squares	41.6	21.57	1.66	16.6	1.4	1	1.42	0	0.7 (13)
Hesse									
L<25									
Optimal	40.1	21.63	1.45	18.8	1.1	1	0.73	0	5.7 (31)
Squares	38.7	22.07	1.63	25.1	3.4	1	1.15	0	10.5 (57)
L<50									
Optimal	41.4	27.17	2.49	18.5	1.3	1	0.55	0	4.5 (34)
Squares	40.4	28.36	2.50	23.0	3.5	1	0.93	0	5.2 (39)

Note: (a) Hit and Miss method calculates the proportion of firms with pscores greater than proportion of treated firms. (b) Calculates the Standardized bias before the matching. (c) Calculates the Standardized bias after matching, only for observations in the matched sample. (d) Using the full sample, regress the treatment variable on the covariates used to calculate the pscore. Calculate Pseudo R2. (e) Using the matched sample, regress the treatment variable on the covariates used to calculate the pscore. Calculate Pseudo R2, this coefficient should be close to zero. (f) Using the matched sample, regress the treatment variable on the covariates used to calculate the pscore. Calculate the likelihood ratio test for all coefficients (except the constant) in the regression to be equal to zero. The test cannot be rejected. Number shown is p-value. (g) Stratification Method: Divide the sample in 25 blocks and check within each block for significant differences in the mean of the covariates used in the estimation of pscore. A significance level of 0.01 is used. The number in the table is the percent of rejection for all possible rejections (variables within each block). (h) Using the matched sample check whether there are significant differences in the mean of the covariates used in the estimation of pscore. A significance level of 0.01 is used.

Table A-13: Propensity Score Overlapping

	L<25				L<50			
	Optimal		Squares		Optimal		Squares	
State \ Treatment	0	1	0	1	0	1	0	1
Bavaria								
min	0.018	0.037	0.000	0.016	0.018	0.053	0.004	0.025
p5	0.070	0.137	0.057	0.133	0.076	0.146	0.063	0.144
p25	0.142	0.270	0.128	0.269	0.153	0.295	0.141	0.294
p50	0.228	0.406	0.218	0.425	0.245	0.443	0.238	0.458
p75	0.342	0.572	0.343	0.603	0.369	0.613	0.366	0.632
p95	0.558	0.831	0.574	0.856	0.603	0.859	0.604	0.881
max	0.945	0.998	0.945	0.999	0.971	0.998	0.965	0.997
N	3087	1416	3087	1416	3319	1813	3319	1813
Hesse								
min	0.001	0.007	0.000	0.010	0.002	0.013	0.000	0.009
p5	0.034	0.107	0.013	0.086	0.042	0.124	0.023	0.095
p25	0.095	0.242	0.068	0.263	0.109	0.283	0.089	0.293
p50	0.168	0.389	0.146	0.457	0.195	0.437	0.183	0.477
p75	0.296	0.595	0.279	0.690	0.331	0.631	0.319	0.690
p95	0.563	0.863	0.549	0.951	0.597	0.872	0.607	0.934
max	0.892	0.987	0.922	0.999	0.899	0.992	0.963	0.999
N	1418	539	1418	539	1671	750	1671	750

Note: Table includes descriptive statistics of the estimated propensity score for the states of Bavaria and Hesse as well as by the optimal specification and the squares specification as desribed in text. pX refers to the Xth precentile in the propensity score distribution.

Table A-14: Displacement Effect using propensity score. Bavaria. Different Specifications.

			1992		1995	
Specification	Sample	Worker	NN	Radius	NN	Radius
Optimal	<25	All	0.6792	0.6270	0.9473	0.8077
			[0.0839]	[0.1047]	[0.2176]	[0.1806]
Optimal	<25	Full-time	0.7204	0.6619	0.9443	0.7939
			[0.0907]	[0.1098]	[0.2330]	[0.1893]
Square	<25	All	0.6775	0.6220	0.7487	0.7734
			[0.0962]	[0.0952]	[0.2315]	[0.2072]
Square	<25	Full-time	0.7109	0.6908	0.7648	0.8049
			[0.0966]	[0.1015]	[0.2275]	[0.2082]
Optimal	<50	All	0.6455	0.6186	0.8515	0.8516
			[0.0839]	[0.0730]	[0.2331]	[0.1838]
Optimal	<50	Full-time	0.7132	0.6607	0.8609	0.8206
			[0.0903]	0.0803	0.2595	0.1909
Square	<50	All	0.6449	0.6283	1.1865	0.8834
			[0.0999]	[0.0824]	[0.2117]	[0.2063]
Square	<50	Full-time	0.6820	0.6707	1.0613	0.8783
			[0.0980]	[0.0847]	[0.2061]	[0.2133]

Note: Displacement effect defined as the effect of treatment on total employment growth over the effect of treatment on immigrant employment growth. Base year is 1991. Increase in total and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score and Nearest Neighbor Matching (NN). Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method (Optimal) and taking squares of all relevant variables (Squares). Firms with less than 25 and 50 employees in the period 1987-1991 in Bavaria. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Table A-15: Displacement Effect using propensity score. Hesse. Different Specifications.

			1992		1995	
Specification	Sample	Worker	NN	Radius	NN	Radius
Optimal	<25	All	0.5130	0.4583	1.0335	0.8774
			[0.0982]	[0.0988]	[0.4276]	[0.3649]
Optimal	<25	Full-time	0.7526	0.5464	1.3501	1.0032
			[0.1412]	[0.1213]	[0.4297]	[0.3617]
Square	<25	All	0.5445	0.4685	1.1643	0.9531
			[0.1219]	[0.0944]	[0.4185]	[0.3394]
Square	<25	Full-time	0.6011	0.5309	1.1654	0.9794
			[0.1706]	[0.1309]	[0.4489]	[0.3525]
Optimal	<50	All	0.4891	0.5754	0.6487	0.6745
			[0.1243]	[0.0962]	[0.3650]	[0.3085]
Optimal	<50	Full-time	0.5412	0.6236	0.8081	0.8445
			[0.1553]	[0.1293]	[0.3279]	[0.3168]
Square	<50	All	0.7167	0.6307	1.0796	0.8053
			[0.1249]	[0.1061]	[0.4309]	[0.3637]
Square	<50	Full-time	0.7470	0.6553	1.3005	0.9013
			[0.1692]	[0.1389]	[0.4589]	[0.3822]

Note: Displacement effect defined as the effect of treatment on total employment growth over the effect of treatment on immigrant employment growth. Base year is 1991. Increase in total and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score and Nearest Neighbor Matching (NN). Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method (Optimal) and taking squares of all relevant variables (Squares). Firms with less than 25 and 50 employees in the period 1987-1991 in Hesse. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.

Table A-16: Wage Effect using propensity score. Different Specifications.

			1992		1995	
Specification	Sample	State	NN	Radius	NN	Radius
Optimal	<25	Bavaria	-0.1107	-0.0859	-0.1112	-0.0721
			[0.0265]	[0.0205]	[0.0569]	[0.0499]
Optimal	<25	Hesse	-0.1244	-0.1232	-0.2754	-0.1601
			[0.0701]	[0.0567]	[0.1269]	[0.0944]
Square	<25	Bavaria	-0.0673	-0.0910	-0.0546	-0.0769
			[0.0236]	[0.0192]	[0.0518]	[0.0444]
Square	<25	Hesse	-0.0561	-0.1185	-0.1144	-0.1044
			[0.0968]	[0.0626]	[0.1131]	[0.0827]
Optimal	<50	Bavaria	-0.0958	-0.0956	-0.0685	-0.0790
			[0.0213]	[0.0202]	[0.0539]	[0.0410]
Optimal	<50	Hesse	-0.0519	-0.1482	-0.2483	-0.2862
			[0.0520]	[0.0696]	[0.1115]	[0.0959]
Square	<50	Bavaria	-0.1068	-0.0928	-0.1190	-0.0749
			[0.0255]	[0.0215]	[0.0499]	[0.0463]
Square	<50	Hesse	-0.0959	-0.1122	-0.2254	-0.1627
			[0.0677]	[0.0436]	[0.1231]	[0.1034]

Note: Wage effect defined as the effect of treatment on average wage growth over the effect of treatment on immigrant employment growth. Base year is 1991. Change in wage and immigrant employment with respect to their value in 1991. Radius caliper 0.01 matching in the propensity score and Nearest Neighbor Matching (NN). Bootstrap standard errors, 100 replications. Functional form of the propensity score was selected based on the Hit-and-Miss method (Optimal) and taking squares of all relevant variables (Squares). Firms with less than 25 and 50 employees in the period 1987-1991 in Bavaria and Hesse. Only firms with positive employment growth during 1991-1992 are included. Treatment is defined as increase in immigrant employment during 1991-1992.