Effects of Immigration on Investment

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Abstract

This paper documents how corporate investment reacts to immigration. I use an interaction of an *ex ante* cluster of immigrants and a change in immigration policy in the United Kingdom to provide evidence that the amount of investment increases in anticipation of immigration flows. The time variation in the immigration policy allows me to control for local economic shocks, reducing endogeneity concerns. Part of the increase in investment occurs through a transitory increase in fixed assets. The major change occurs in the extensive margin, through an increase in firm creation. The increase is larger for the knowledge and the service sectors, suggesting that human capital is an important driver of the effect. The results suggest that firms quickly react to an immigration-induced labor supply shock.

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1 Introduction

For many net-receiving countries, immigration has become one of the main sources of new labor. According to the International Labor Organisation (ILO) (2015), international immigration to industrialized countries increased at a yearly rate of 30% from 2010 to 2013. Concerns about the economic effects of immigration on the native population make immigration a contentious political issue. According to reports by the House of Commons, in 2007, British voters reported immigration as their biggest policy concern (Lang, 2008). Polls also suggest that the Brexit vote in the United Kingdom is connected to voter's attitudes toward immigration. An Ipsos poll documents that one week before the 2016 referendum on Britains membership in the European Union, more than half of voters supporting Leave considered immigration a key issue .[1]

The main economic argument against immigration focuses on its potential negative short-term wage effects. The logic is simple: immigration increases labor supply and, therefore, decreases labor costs. Finding these negative wage effects in the data is difficult. According to Peri (2014), in 27 empirical studies, estimates of elasticities of wages to increases in the share of immigrant workers range from -0.8 to +0.8, with most studies reporting a zero effect.

My paper examines how corporate investment adjusts to labor supply increases caused by immigration. If investment adjusts contemporaneously to labor, average wages might not decrease. To empirically measure the relationship between immigration and investment, I explore a unique natural experiment that increased immigration to the United Kingdom: a change in policy that gave full working rights to nationals from countries admitted to the EU in 2004. I use a difference-in-differences strategy. I combine the policy change with cross-sectional variation from *ex ante* clusters of immigrants to provide reduced-form estimates of the effects of immigration on investment and firm creation.

The results show different responses of investment to immigration in the intensive and the extensive margins. First, for the intensive margin, firms located in districts with higher *ex ante* immigration exposure show a significant increase in fixed asset investments after the EU expansion announcement. A one-standard-deviation increase in *ex ante* immigration exposure is associated with a 1.9% within-firm increase in long-term fixed assets. The increase in fixed asset investment is not significant when combining the effect of the policy announcement and the implementation. Fixed assets do not increase more after the implementation of the policy. Furthermore, total within-firm assets do not significantly change either after announcement or after implementation of the policy. These results suggest a simple, yet powerful, explanation for why labor costs do not drop on average even if immigration increase labor supply: long-term adjustments to capital investment may occur in anticipation of the labor supply increase.

[1] See https://www.ipsos.com/ipsos-mori/en-uk/immigration-now-top-issue-voters-eu-referendum.

Second, for the extensive margin, the results show a significant increase in the incorporation of new firms after the open policy announcement and a further significant increase after the implementation. A one-standard-deviation increase in *ex ante* immigration exposure leads to a 1.78% increase in the number of firms incorporated. The data show an additional increase of 3% in the number of incorporated firms after the policy implementation. The increase is significant when combining the effect of the policy announcement and implementation. Using the interaction between the policy announcement and the *ex ante* immigration clusters as instrument, a 0.5% immigration-induced labor supply increase—the average UK labor force growth—translates into a 17.5% increase in the number of incorporated firms.

Next, I examine whether there are heterogeneous effects across the different sectors. Whether firm adjustments occur through the expansion of existing firms or through the incorporation of new firms depends on the sector. For construction, there is a persistent increase in both fixed assets and total assets. The IV estimation reports that a 0.5% immigration-induced labor supply shock translates into a 5% within-firm increase in fixed capital investment.

For firm creation, the effects are larger in sectors that rely on human capital or that provide services. Following Jeffers (2017), I define knowledge-intensive sectors based on the type of occupations employed in the industry. I define knowledge firms as those with a main classification in computer programming, information technologies, architecture, business consulting, engineering technical consulting, research, design, health, or education.[2] New firm incorporation significantly increases both in the knowledge and in the service sector in districts with higher immigration. These increases are associated with a fundamental shift in the economic environment. The average firm in these sectors becomes smaller. Existing firms in the service sector significantly decrease their total assets. For the knowledge sector, there is also a decrease in existing firms assets, but it is not significant.

Regarding changes in wages, this paper shows that wages do not significantly change at the district level. The same results hold for the average remuneration within firms and when separating firms by sectors of the economy. Moreover, the signs of the estimates are not consistent. In construction, where adjustments occur through increases in fixed capital, the sign of the estimated wage elasticity is positive. In the knowledge and the service sectors, where adjustments occur through an increase in the number of firms, the signs are negative. However, in all of these sectors, the wage effects are insignificant for the average worker in pre-existing firms.

The results in this paper offer a potential explanation for why prior studies have failed to find large effects for immigration-induced labor supply increases on wages. In a model

[2]The exact industries are reported in the Appendix.

with constant returns to scale, a labor supply increase generates negative short-term wage effects if firms do not invest enough. The lack of investment causes the marginal value of labor to decrease in the short-term. As my results suggest, if investment adjusts in anticipation of labor flow increases, the transfer from workers to capital need not occur. Investment decisions can also depend on immigration itself. Immigrants could set up new firms or bring human capital necessary for the expansion of certain industries. This paper also provides evidence of this mechanism.

Immigration has potential benefits: it can change the talent pool and offer incentives to create new firms . A varied workforce can also improve the development of certain sectors and reduce incentives for outsourcing (Ottaviano, Peri, and Wright (2013). If capital flows to areas in which it is scarce in relation to incoming labor, the economy enjoys the benefits of immigration without paying the short-term economic costs in terms of lower wages.[3] Moreover, not all immigration is equal. If immigration generates positive changes in the skill composition of workers, then complementarities with capital can smooth out the wage effects (Lewis, 2013; Friedberg and Hunt, 1995).

Studying the relationship between immigration and investment is challenging because the potential endogeneity concerns are many. Immigrants may settle in places where growth is already expected.

This paper addresses these concerns using the following strategy. First, I rely on a pre-determined cross-sectional measure related only to the immigrant group treated by the policy. This strategy relies on the observation that immigrants relocate to places where their peers are, rather than to places where the economy grows regardless of immigration. Nonetheless, the existing immigration clusters could already predict future growth patterns. Area-time dummies restrict the effects to the local level. For endogeneity to arise, the immigrant group needs to predict economic growth at a local level that is smaller than a city. Because of the policy change, the empirical strategy can control for unobservable time-invariant differences at the district level when studying firm creation. Third , I rely on micro data at the firm-level to determine the intensive margin effects. I use firm-level fixed effects to control for the firms time-invariant characteristics. The paper presents evidence that parallel trend assumptions are likely to hold for the variables of interest in the period before the policy. Assuming the trends would have remained parallel in the absence of the policy change, the reduced-form estimates have a causal interpretation.

The paper also explores mechanisms that explain the main results in firm-level investment and in firm creation. Categorizing firms by their board composition in 2001, I examine whether firm-level investment and employment decisions are related to the cul-

[3]Some groups may still be harmed if new immigrants compete with workers from certain levels of skill, as discussed by Borjas (1999) and Borjas (2003). Also, Card (2009) discusses the effects of immigration on inequality.

tural proximity between firm directors and the immigrants in a specific location.[4] There is no evidence that firms with Eastern European majority boards increase their fixed assets or employ more workers than their counterparts in the same district.

On the other hand, both UK and Eastern European nationals create more firms after the immigration shock. This suggests that new British entrepreneurs also benefit from increased immigration. Furthermore, the rate of firms created by Eastern European directors as a proportion of the total increases significantly. These results suggest that firm creation is driven by immigrants and not by previously existing social or cultural ties.

Another potential mechanism behind the increase in investment is the change in the skill mix that immigration brings. If immigration is predominantly low-skill, immigration might substitute capital because immigrants take jobs in danger of automation (Lewis, 2011). If immigration is predominantly high-skill then it complements capital (Friedberg and Hunt, 1995). Manacorda, Manning, and Wadsworth (2012) provide evidence that high-skill workers tend to immigrate to the United Kingdom. I complement that evidence in three ways.

First, the data show a significant increase in firm creation in the knowledge sector, evidence that is in line with the findings of Ashraf and Ray (2017) for the United States. Local-level immigration exposure is associated with a significant increase in the number of firms incorporated in the knowledge sector, which, by definition, relies on specialized labor. Second, I show that, after the immigration policy shock, the educational attainment of Eastern European immigrants, compared to that of natives, significantly improves. Third, the data show that the remuneration to the highest-paid director within firm significantly drops in the service sector. There are also negative effects for directors in the knowledge sector, but they are not significant. For average workers, the effect is never significant and the magnitude is smaller. Hence, the negative effects on compensation concentrate in the higher part of the income distribution within the firm. The negative wage effects for the best paid support the hypothesis that, in this setting, immigration increases competition in the top part of the skill distribution.

This paper contributes to three strands of literature. First, I study the interaction between labor markets and firm-level decisions. Like in Dustmann and Glitz (2011) and Ashraf and Ray (2017), I document results opposite to the economic literature that shows substitution among immigrants and capital investment (Lewis, 2011). My results show that, in the short run, immigrant labor can complement capital investment in industries like construction. Furthermore, immigration can also generate adjustments in the creation of new firms in sectors that rely on human capital. Two key elements are necessary for this result to occur: first, the change in the skill composition of immigrants and, second,

[4]This is the channel explored by Burchardi and Hassan (2013).

UK policies. More specifically, Eastern European immigration to the United Kingdom, in terms of educational attainment, tends to be of higher skill after the immigration policy change, and the open border policy in the United Kingdom did not cap legal immigration from Eastern Europe, but allowed a delay between the announcement and the implementation.

I contribute, empirically, to the extensive finance and macroeconomic literature on capital adjustments. Capital investments take time. There are costs of maintaining capital to react to new investment opportunities (Mitchell, Pedersen, and Pulvino, 2007; Duffie, 2010). Moreover, fixed capital investments require both adjustment costs and that assets are not easily traded in secondary markets (Cooper and Haltiwanger, 2006). However, in the setting used in this paper, I show that fixed capital investments react in anticipation of labor flows in construction. In other sectors, such as the knowledge sector, which relies on human capital, or the service sector, which relies on labor-intensive tasks, adjustments arise through new entrepreneurial activity. My paper suggests immigration can also reduce barriers to entry when human capital is scarce. Entrepreneurship increases, although the average firm is smaller.

I also contribute to the extensive literature on the effects of immigration on labor markets (see Card, 1990; Borjas, 2001; Ottaviano and Peri, 2012; Peri, 2012; Ottaviano, Peri, and Wright, 2013). I provide additional evidence that average wages do not decrease when immigration increases. Finally, I document another positive link between immigration and entrepreneurship.[5] My interpretation of the results provided in this paper suggests a more nuanced view of the costs and the benefits of immigration.

2 The immigration policy change

My analysis focuses on a major change in immigration policy in the United Kingdom triggered by the expansion of the European Union in 2004, a time during which the United Kingdom was a member.

After a long period of discussions, in April 2003, the EU announced the Treaty of Accession, with the objective of incorporating new members. The treaty implementation date was May 2004. The treaty keeps some immigration policy discretion for a limited period of time. Old EU members could delay working rights for nationals from new admitted countries for a maximum of 7 years. Only 3 older members—the United Kingdom, Sweden, and Ireland—allow nationals from incoming country members to work freely.

For the case of the United Kingdom, foreign nationals from the newly admitted countries had the right to work conditional on registration to National Insurance. This registration did not provide welfare benefits. Furthermore, registration was not automatic.

[5] See Hunt (2011), Decker et al. (2014), and Fairlie and Lofstrom (2013).

However, it was in the best interest of immigrants to register since it was a legal requirement.

The paper focuses on immigration from 8 newly admitted Central and Eastern European countries: Estonia, Latvia, Lithuania, Poland, Czech Republic, Slovakia, Slovenia and Hungary.[6] Figure 1 offers a summary of the immigration decisions across older EU members.

Following the policy change, the United Kingdom experienced a large inflow of people from Central and Eastern Europe. The amount of immigration was underestimated by the British government at the time of the policy implementation, partly because the government was expecting more EU countries to also grant full labor rights. A report by the Home Office (Casanova et al., 2003) estimated an influx of 13,000 long-term immigrants per year. According to figures from the Organisation of National Statistics (ONS), the number was closer to 50,000 per year. After the 2004 expansion and the subsequent open border policy, British attitudes toward immigration changed significantly. According to an immigration report by the House of Commons (Lang, 2008), polls documented that in the 1990s only 5% of the British population considered immigration the most important issue in Britain. By 2007, the number increased to 40%. For the next EU expansion, the British government changed its policy. When the opportunity resurfaced in 2007 with new members, the British government decided not to open labor markets. In other words, for the subsequent expansion the United Kingdom adopted a restrictive policy similar to the ones adopted by other European countries in 2004. This policy is consistent with the idea that the British government decided to control immigration after the open border policy of 2004.

The result for the United Kingdom was a large inflow of people from Central and Eastern Europe. The amount of immigration was underestimated by policymakers at the time of the policy implementation, partly because the United Kingdom was expecting more EU countries to also grant full labor rights. A report for the Home Office by Casanova *et al* (2003) estimated an influx of 13,000 long-term migrants per year. According to figures from the Organisation of National Statistics (ONS), the number was closer to 50,000 per year. After the 2004 enlargement and the subsequent open border policy, the British population attitudes towards immigration changed significantly. According to an immigration report by the House of Commons (Lang, 2008), polls documented that in the 90's only 5% of the British population considered immigration the most important issue in Britain. By 2007 the number increased to 40% of the population. For the next EU expansion, the British government changed its policy. When the opportunity resurfaced in 2007, with new members, the British government decided not to open labor markets.

[6]Malta and Cyprus were also admitted, but their effect was small and, for historical reasons, they already had some rights in the United Kingdom. Moreover, their population inside the United Kingdom was not large enough to be reported at the local level in the Census.

In other words, a similar policy to those adopted by the rest of European countries in 2004. This is consistent with the idea that the British government decided to put more controls on immigration after the open border policy of 2004.

According to ONS data, National Insurance registrations increased after 2004, pointing to an important immigration-induced labor supply shock. As I show in Figures 2 and 3, the increase is driven by incoming nationals from Central and Eastern European countries. After the implementation of the open border policy, nationals from these countries of origin (commonly referred to as the EU8 group) became the most representative group in terms of registrations. They represented 3.1% of the registrations by 2002 and 38.4% by 2005. However, it does not seem that the increase came at the expense of a reduction in the number of new workers from other groups. Figure 2 shows that registrations remain constant for nationals from European countries with pre-existing labor rights (EU15) after the policy . Figure 3 reveals that nationals from other European countries not yet admitted to the EU, but that would be admitted in 2007, registered at the same rate .[7] Therefore, the policy expanded the number of workers and should not be interpreted as a mere recomposition of the immigrants that were admitted as workers in the United Kingdom.

3 Data

3.1 Employment and National Insurance data

To measure immigration at the district level, I use both employment data from the Department of Work and Pensions and census data from the Organisation of National Statistics (ONS). After the EU expansion of 2004, nationals from the newly admitted countries needed to register a National Insurance number to obtain the right to work in the United Kingdom. Figure 1 represents the number of national insurance numbers registered by year. I divide the registrations into two groups: nationals from new countries and nationals from countries that were already part of the EU. The figure shows that, after the policy change, registrations from the new group surpassed those from the original EU members.

National Insurance number (NINO) registrations are not a measure of long-term immigration, and they do not account for immigrates who return to their native country. Registrations only account for the district in which immigrants register their intention to work in the United Kingdom. For registration, any immigrant needs a UK address. This address determines the district of registration.

Despite its problems, not accounting immigrates that return and accounting for reg-

[7] The same patterns emerge if I use registrations of workers from the rest of Europe.

istration near the first address of the registrant, the number of NINO registrations is the best possible measure in this paper for several reasons . First, long-term immigration is normally measured at the local level in the census, but my analysis requires a higher frequency. To determine the effects of new immigration on investment, I need at least yearly data. Therefore, I use NINO registrations as a proxy.

I aggregate labor data at the district level. Because of the availability of data, my analysis is restricted to England. I use the 326 English districts to construct the summary statistics. The average population of a district, as of 2002, is approximately 92,000 people. In terms of population, English districts are comparable to counties in the United States.

In Table 1, I provide the summary statistics for the National Insurance number (NINO) registrations and employment data both after and before the 2004 EU expansion. The total number of NINOs by any country of origin doubled after the EU expansion, going from 842.9 to 1,556.3 registrations. Most of the increase is related to the inflow of nationals from countries admitted in 2004 (the EU8 group). Before the change, an average of 34.3 EU8 workers registered in a specific district, but after the policy change, registrations increased to 572.1 per district. This number made the EU8 group the largest source of registrations, surpassing the previous dominant group: the old EU members who had free labor mobility since the 1990s. Between 2004 and 2007, one-third of all NINO registrations in England were issued to nationals of countries admitted to the EU in 2004.

In the census, the data are reported at the local level, which is, in some cases, smaller than the district level. When a local authority does not form a unique district, I aggregate the data at the district level. Mapping between local authorities and districts is not one-to-one, because sometimes a local authority belongs to multiple districts. If this is the case, I assign each local authority to a single district based on how much of the territory belongs to the local authority.

I use 2001 census data to construct the pre-existing immigration cluster measures. The measure is constructed using the percentage of workers from Central and Eastern European origins. The 2001 census does not provide the EU8 subdivision. I use a proxy that accounts for the number of people from EU8 plus Romania and Bulgaria. An average of 2.3% (SD = 2.1%) of workers have this origin as of 2001.

3.2 Firm directors' data

The data are retrieved from Bureau van Dijk's ORBIS and FAME firm databases. The data on directors (board members) cover the entire universe of firms in the United Kingdom.

Tables 2 and 3 provide firm-level summary statistics for the characteristics of the board of directors. Table 2 provides information about the board characteristics for all firms in the United Kingdom that were incorporated by 2000. Firms established before

the policy have boards with a similar nationality composition over time.[8] Around 91% of directors are British. This proportion slowly increases over time. Likewise, the proportion of EU directors remains relatively flat over time. Around 4.5% of directors are nationals from old EU members. Only 0.08% are nationals from countries that were admitted by the EU in 2004 (EU8).

On the other hand, there seems to be a structural change on the board composition for younger firms. Table 3 provides information for the composition of newly created firms by the firms year of incorporation. Firms created after 2000 are more diverse in terms of the nationalities of the directors. The proportion of directors from countries admitted before 2004 (EU15) increased from 4.6% in 2000 to a maximum of 9.6% in 2006. Similarly, the percentage of board members from EU countries admitted in 2004 (EU8) increased from 0.1% to almost 1% by 2008.

3.3 Firm financial data

I collect the financial data from BvD's FAME database.[9] To study firm-level employment, I restrict the sample to firms that report at least one employee between 2001 and 2005. Table 4 reports the summary statistics .

The average total remuneration by firms to workers remains constant over time . The average number of employees increases from 243 to 310 over the sample. Moreover, the average salary per employee decreases over the sample. On the other hand, both total director remuneration and the remuneration for the highest-paid director increases over this period. The pay gap between workers and directors widens. The increase in directors' compensation is consistent with the stylized facts in the executive compensation literature (Edmans, Gabaix, and Jenter, 2017). More importantly, it is also consistent with patterns among public firms in the FTSE 100 (CIPD Executive Pay Report, 2017).

Financial reporting is not required for all firms, and, even when required, not all firms file the same variables. Normally, firms limit themselves to providing information about their assets.

For some of the analysis, I aggregate the data at the district level. In most instances, ORBIS directly reports the firms district. However, for special cases, like London, the data report the whole city and not specific districts. In these cases, I identify the firms postal code and then aggregate postal codes at the district level. Once I assign each firm location to a district, I match this information with immigration and census data.

[8] This does not mean that director turnover is zero. These results could be driven by two reasons: (1) the persistence of directors or (2) the replacement of directors with other directors who have a similar origin.

[9] This database was a joint effort by Juanita Gonzalez-Uribe, Daniel Paravisini, Su Wang, the Abraaj Group at FMG, the LSE library team and Bureau Van Dijk.

4 Empirical setting

There are many identification challenges to disentangle in determining the causal effects of immigration on investment and firm creation. First, the decision to settle in a specific location is potentially driven by other factors that increase labor demand. Furthermore, demand factors are persistent. Hence, immigrants could be settling in districts that would have higher investment regardless of immigration. If this is the case, a standard ordinary least square (OLS) regression of immigration on investment would lead to biased results.

An event study on the effects of the open border policy does not completely address these problems. The EU8 admission to the European Union in May 2004 is an endogenous decision, and the admission itself was planned. Furthermore, the expansion required the agreement of all EU members. EU negotiations considered the economic conditions at the time. Moreover, the adoption of an open border policy in the United Kingdom after the European Union expansion is also endogenous. This decision reveals information about the state of the economy, even in the counterfactual case of no change in immigration policy. If the British economy was expected to grow significantly and demand more labor, regardless of the EU8 admission, the difference before and after the policy overestimates the effect of immigration.

To provide more convincing evidence, I use a difference-in-differences strategy. The source of cross-sectional variation is the proportion of Eastern European workers as of 2001 in a specific district. To add time variation, I interact this measure with the UK immigration policy change. To address potential endogeneity problems, I control for district fixed effects and wider area economic trends when studying district-level outcomes.[10] I control for firm fixed effects, rather that district fixed effects, and economic area trends when studying firm-level outcomes.

To identify the causal effects of immigration on investment, the ideal research design consists of an experiment that randomly allocates different levels of immigration across districts in the United Kingdom and then measures the effects of immigration on investment. This paper relies on an interaction between a natural experiment (the announced immigration policy change) and an *ex ante* measure of immigration clusters. This identification is similar to the shift share instrument Altonji and Card (1991) and Card (2001) originally used.

My empirical strategy resembles the ideal experiment in two ways. First, the pre-existing clusters of immigrants affect the intensity with which each district is treated. Immigrants are more likely to settle in locations where there is a larger community of immigrants with the same origin. For this reason, I use EU8 worker clusters, and not

[10] I control for NUTS2-time dummies to capture local economic-wide shocks. There are 34 such areas in England.

total immigration. Using stocks of immigrants from a specific origin diminishes concerns that aggregate demand shocks drive immigration. One important identifying assumption is that the immigration pull factors are related to closeness to peers rather than economic characteristics of particular locations. However, this strategy cannot control for district-level differences. If the settlement of immigrants in the past is related to unobservable and persistent district-level characteristics, the strategy may still overestimate the benefits of immigration.

To address this issue, I complement the strategy by exploiting the time variation introduced by the policy change. The time variation allows me to control for district-level fixed effects. I also control for area-wide trends. Therefore, I can address the problem pointed out by Borjas (1999) of serially correlated shocks causing the immigration clusters in the first place.

Spillover effects and open economy adjustments are some weaknesses of using an identification that relies on spatial differences across locations. For example, it is possible that an increase in immigration in one location displaces native workers to another location with fewer immigrants. I cannot rule out is possibility. Accordingly, my results should be interpreted as local effects, and care should be taken when assessing the effects at higher levels of aggregation.

To construct the cross-sectional measure of *ex ante* immigration clusters, I use the proportion of Eastern European workers in an English district as of 2001.[11] The average proportion of Eastern European workers is 2.3%, with a standard deviation of 2.1%.[12] To exploit the time variation from the policy and control for unobservable district characteristics, my measure of immigration exposure is the interaction between the *ex -ante* immigration cluster, an indicator for the policy announcement, and another indicator for the policy implementation.

The main identification assumption is that all different unobservable factors that may drive the outcome variables are time invariant, conditional on controls, and can be controlled for with a fixed effects specification. To provide evidence in favor of this assumption, Figures 6 and 7 present the graphical results of a regression of the relevant outcome variable on the relevant fixed effects and the interactions of the indicators and the cross-sectional exposure measure. The specification controls for area-time dummies and the relevant fixed effects; firm fixed effects for fixed assets, employees, sales, and average remuneration; and district fixed effects for firm creation and new Eastern Eu-

[11] My analysis is restricted to England because of data availability.

[12] The ONS did not separate the EU8 group in the 2001 Census. Instead, they provide the number of workers from a group called EUplus, which accounts for what is now known as EU8 plus Bulgaria and Romania. Alternatively, the ONS provides data on Polish workers, a predominant group. These data are less accurate because the ONS only reports aggregate data if at least 15 workers are identified. As a result, the Polish group has more missing districts. However, even when using *ex ante* Polish workers as the source of cross-sectional variation, results in investment, employment, and firm creation are significant and exhibit the same signs. Nonetheless, average within-firm average remuneration decreases.

ropean registrations. The figures also report the 95% confidence intervals. Because the intensive margin data are yearly, there are only two observations pre-treatment. Hence, I can estimate only one coefficient in the pre-treatment period. For firm creation and new EU8 registrations, I rely on quarterly data. Therefore, Figure 7 provides coefficient estimates up to four periods before the policy announcement.

All the coefficients before the policy announcement are statistically indistinguishable from zero. Therefore, I cannot reject the hypothesis of no differential trends in the pre-treatment period. As long as this assumption also holds for the post-treatment period, which is not testable, the reduced-form regressions provide an estimation of the causal effect of *ex ante* immigration clusters on future immigration, the intensive margin investment, and firm creation.

The interpretation of the reduced-form effects relies purely on the identifying assumptions discussed before. However, Figure 7 shows a positive and significant relationship between the interaction of policy and *ex ante* immigration clusters on new EU8 registration. Table 6 shows a positive and significant relationship between an interaction that combines the policy announcement and implementation into one indicator function and the immigration exposure measure. This paper uses this fact to proceed to an instrumental variable (IV) estimation of new EU8 registrations on corporate-level capital investments and on the creation of new firms. Contrary to the reduced-form estimates, the IV estimation has a direct economic interpretation.

For IV to provide a causal estimation of the local average treatment effect in a heterogeneous effect model, four assumptions must be satisfied (Imbens and Angrist, 1994). First, a first stage between the instrument and the independent variable must exist. Evidence points in favor of this assumption. Second, conditional on controls, treatment must be as good as randomly assigned. This assumption is the same as that required for the difference-in-differences strategy I use. Third the instrument affects the outcome variable only through the variable of interest, an assumption known as the exclusion restriction. Fourth, the instrument affects the variable of interest in one direction only, an assumption known as monotonicity.

The IV estimation comes at a cost. In general, it is more difficult to satisfy the identifying assumptions for IV than for difference-in-differences. Furthermore, because the policy is not immediately implemented, there might be anticipation between the announcement and the implementation. There are employment restrictions for immigrants in this window, but not for firm creation or for investment. Therefore, I need to combine the effects of the announcement and the policy in a single interaction term with the *ex ante* immigration measure. This makes the estimation less precise.

However, IV provides a direct estimation of the effect of the increase of new registered workers on the outcome variables. If the identifying assumptions hold, IV can be

interpreted as the causal effect of gross increases in new EU8 registration on firm-level investments and firm creation.

5 Main results

5.1 Predicting the allocation of new EU8 registrations

Before I document the effects of immigration on investment, I test whether the measure of immigration exposure—the *ex ante* immigration clusters interacted with the policy—positively predicts immigration after the policy shock.

To generate the measure of immigration exposure, I collect the data from the 2001 Census. The Census does not separate the EU8, but accounts for a group that includes the EU8 plus other two countries: Romania and Bulgaria. I use this group to construct my proxy for the *ex ante* proportion of workers. [13]

I test whether the interaction between immigration clusters and the policy predicts future patterns using the following specification:

$$\begin{aligned} ShareRegisteredWorkersEU8_{dt} = \alpha_d + \alpha_{ct} + \\ + \beta_1 FractionEastern_d * PostAnnounce_t + \\ + \beta_2 FractionEastern_d * PostImplement_t + \varepsilon_{dt}. \end{aligned}$$

$ShareRegisteredWorkersEU8_{dt}$ measures the proportion of NINO registrations issued in a quarter divided by the number of workers in 2001. I normalize by workers in 2001 to avoid the mechanical increase in the denominator caused by the immigration policy change. Changes in the share of registered workers can be interpreted as a shift in the labor supply. α_d is district-level fixed effects that account for time-invariant unobservables. α_{ct} is an area-time dummy to account for local-level shocks. An area covers a group of contiguous districts. Area refers to the NUTS2 statistical aggregation from the Office of National Statistics (ONS). This aggregation covers neighboring districts all over England. There are 34 such areas, covering around 10 districts each. $FractionEastern_d$ is the *ex ante* proportion of workers who are Eastern European nationals.[14] $PostAnnounce_t$ is a dummy variable that takes the value of 1 after the expansion is announced in the second quarter of 2003. $PostImplement_t$ is a dummy variable that takes the value of 1 after

[13] The ONS also reports the number of Polish workers, the most prevalent nationality among the EU8 group, per district. I can also use the data that account for Polish nationals separately. I prefer to use the Eastern European group, which better predicts future immigration patterns. Moreover, the ONS reports the number of immigrants only when that number surpasses 15 workers in a local authority. The Polish group is a subset of EUplus and, hence, has more missing data.

[14] That is, EU8 plus Romania and Bulgaria.

the implementation of the expansion in May 2004. The time series goes from the first quarter of 2002 until the fourth quarter of 2006.

The main specification controls for area-quarter fixed effects. Therefore, the variation between districts inside an area-time determines the source of identification in this empirical strategy. For example, within an area-time, like Inner London in a specific year, the identification captures the effect across different districts.

Table 5, Panel A, shows that the measure of exposure (i.e., *ex ante* proportion of Eastern workers) positively and significantly predicts an increase of new registrations, both after the policy announcement and after the policy implementation. The effect is larger after policy implementation. Accounting for both the announcement and implementation of the policy, a one-standard-deviation change in the *ex ante* ratio of Eastern European leads to an additional quarterly flow of 0.15% new workers, as a proportion of the initial workforce in 2001.

To provide better economic interpretation, I separate districts by a dummy, $HighFraction_d$, which takes the value of 1 if the district has an above-median proportion of Eastern European workers and 0 if it has a value below. Table 5, Panel B, provides the results. Combining the effect of the announcement and the policy, every quarter, highly exposed districts receive an increase in the flow of workers equivalent to 0.15% of the initial workforce in 2001, that is, the same as the standardized result using the continuous measure.

As a comparison, over the 20th Century, the average yearly UK employment growth was 0.5% (Lindsay, 2003). Taking 2001 as the base year and assuming the rate of growth to be constant year by year, the increase in labor supply by 2004 is approximately 0.51% over a year, or 0.13% over a quarter. Therefore, a one-standard-deviation shift in the *ex ante* immigration cluster causes an effect larger than the average labor force growth. This is an economically meaningful shock. [15]

The results are robust and even more significant if I use a yearly frequency and control for area-year dummies. This result is also important because the financial data are only available at a yearly frequency. Hence, the effect within firms is only analyzed at a yearly frequency.

5.2 District-level investment

In the standard model with homogeneous labor, an increase in labor supply makes capital relatively more scarce and, therefore, more valuable. In labor economics, researchers typically assume that, in the short-term, capital is fixed and labor is not (Borjas, 2014). However, if capital markets are efficient, there is less reason to believe that the capital

[15] To provide this back-of-the-envelope calculation, I take year 2001, my base year, as a 100. I measure the total change in the index from 2003 to 2004. The change is equivalent to 0.51. As a percentage of the base year, this is 0.51%.

adjustments should lag labor flows. It is possible that capital takes time to build, but, in this setting, firms could increase capital in anticipation of the open policy. On the other hand, until the policy was implemented, firms had restrictions on hiring foreign workers.

In this paper, intensive margin investment refers to long-term physical capital investment. Since, under the accounting conventions, only changes in fixed assets can be interpreted as long-term capital investments, I use this measure. The effects are positive, but not significant, if I measure the effects over total assets and restrict the sample to firms that have positive fixed assets.

In this section, I present evidence that fixed capital investment increases for the average firm in anticipation of the change in immigration policy. More importantly, capital flows to locations where it becomes more valuable: districts that are expected to have a bigger influx of immigrants after the open border policy. Nonetheless, the change is only a one-off event. If I combine the effect of the announcement and the policy, the increase in investment is not statistically significant.

Because of data constraints, I report regressions of fixed assets at a yearly frequency. The regression uses all firms in the sample, both newly incorporated and previously existing firms, and measures how the average fixed assets of a firm located in a particular district change when exposed to immigration changes. To calculate the district-level averages, I first take the logarithm of fixed assets for each firm and then take the average within each district-year.[16] The results are described using the following equation:

$$\begin{aligned} ln(y_{it}) = \alpha_i + \alpha_{ct} + \\ + \beta_1 FractionEastern_d * PostAnnounce_t + \\ \beta_2 FractionEastern_d * PostImplement_t + \varepsilon_{dt}. \end{aligned}$$

Table 6, Panel A, shows that fixed assets significantly increase after the announcement, but they decrease, though not significantly, after the implementation of the policy. After the EU expansion announcement, a one-standard-deviation increase in the exposure measure increases fixed assets at the district level by 1.8%.[17] If I subsume the announcement and implementation of the policy in a single dummy variable and interact it with the *ex ante* immigration cluster, the effect is positive and equivalent to an increase of 1.9% on fixed assets. However, this result is not statistically significant.

Panel D of Table 6 presents the results of the effect of an increase in the share of new EU8 registration on fixed asset investment for all firms in a district. The regression

[16] The advantage of this approach, as explained in Borjas (2014), is the interpretation of the average. The average of the log is the geometric mean. On the other hand, the log of the average does not have a similar interpretation. Fortunately, in this setting, the two options yield qualitatively similar results.

[17] The standard deviation of the immigration clusters is 0.021. The regression is log-level, so % Δy=100*($e^{\beta} - 1$) for every unit x increases.

controls for district fixed effects and area-time dummies. The sign is positive, but not significant. Table 6 also shows the elasticity of the average wage within a district to an increase in the share of Eastern European immigrants. Even though, the signs are negative, they are statistically insignificant.

The district-level regressions combine the two margins in which investment can react to an increase in labor supply. On the one hand, investment can increase in the intensive margin, as existing firms increase capital expenditures to incorporate incoming workers. In the extensive margins, the labor supply increase may make it easier for new firms to enter the market. I disentangle these effects next.

5.3 Firm-level results

In this section I use firm-level data to provide evidence that the increase in investment in long-term capital is significant for firms that were created before 2001 only at the moment of the announcement. The effects are not persistent on average, but they are persistent for a particular sector: construction. When I study the effects over total assets, stark differences emerge. The construction sector also experiences a significant, persistent increase in total assets. Nonetheless, for the service sector, the data show a significant decrease in total asset investment. This does not mean that investment in the knowledge and in the service sectors decrease as a whole. The margin of adjustment is different in these sectors. Later, I will show that the total number of firms created in these sectors significantly increases.

These results are relevant for two reasons. First, I document results consistent with complementarities between Eastern European migration and long-term fixed capital investment for the construction sector. This result is not obvious. The complementarities depend on the skill composition of the incoming workforce. In particular, immigration could replace capital in automatized industries (Lewis, 2011). Evidence of an increase in capital accumulation supports complementarities between immigrant workers and capital investment. Second, for immigration to decrease average wages in the short-term, capital should lag labor (Borjas, 2014). I show that the flow of capital, at least in the United Kingdom during 2004, anticipated the labor flows from immigration. This is a potential explanation for why the search for negative wage effects from immigration has been elusive in the labor literature.[18]

[18]See Kerr and Kerr (2011) for a survey of the economic impacts of immigration on employment and on wages.

$$ln(y_{it}) = \alpha_i + \alpha_{ct} + \\ + \beta_1 FractionEastern_d * PostAnnounce_t + \\ \beta_2 FractionEastern_d * PostImplement_t + \varepsilon_{dt}$$

In this regression I control for $alpha_i$, that is, firm-level fixed effects. I also control for area-time dummies. The regression reports, within a geographical area-time, how much firms located in a high *ex ante* Eastern European immigration district increase their fixed assets compared to firms located in a low Eastern European district.

In Table 7, Panel A, I document a significant increase in fixed assets within firms after the announcement of the EU expansion. To ease interpretations, I provide standardized results for the reduced-form regression. A one-standard-deviation increase in the size of the *ex ante* immigration cluster translates into an increase of approximately 1% in fixed assets. The increase in the number of employees within firms after the policy implementation is quantitatively similar. A one-standard-deviation increase in the *ex ante* immigration cluster translates into an increase of 0.76% in the number of employees.

These results are in line with the particularities of the policy. Before the policy implementation, firms could not hire EU8 nationals without issuing a work permit. The United Kingdom lifted the restriction in 2004. Firms could invest more in expectation of a labor supply increase from the open policy implementation, but could not yet hire new immigrants. If capital takes time to build, the result that fixed capital investment precedes the labor supply shock is natural.

Second, I explore the effects of immigration exposure sales per employee. This is a proxy for productivity. As Peri (2002) shows, immigration can also affect firm-level productivity. In Table 7, I show that the effects are positive and statistically significant only after the announcement, that is, before foreign workers can be hired by the firm. This effect disappears when I combine the effects of the announcement and the policy implementation. Therefore, the data do not support the claim that immigration increased productivity within existing firms.

One important cost immigration may have on the native workers is a potential decrease in their remuneration. Firms could also face different factor prices when immigration increases. A positive labor supply shock could reduce average labor costs. I estimate the average employee remuneration within the firm. I find no evidence of a significant reduction in average remuneration. Table 7, Panel A, shows the within-firm effects for the average worker in the firm and for the highest-paid director. Both results are not significant. [19]

[19] Dustmann and Glitz (2011) use a different methodology but find similar results. They find within-firm factor price adjustments are not significant, but changes in factor intensities are.

I adopt an IV approach to measure the effect of immigration on capital investment, employment, and sales per employee. For IV to be interpreted as the local average treatment effect, the instrument needs to satisfy three assumptions in addition to the difference-in-differences strategy, which only requires random assignment conditional on controls.

First, a first stage must exist. This assumption is directly testable, and in Table 7, I find evidence that the ex ante immigration measure significantly predicts future migration patterns .[20] Second, the exclusion restriction, which in this case requires that my measure of *ex ante* immigration exposure affects the outcome variable only through changes in the share of new Eastern European workers, must exist . Third, ex ante immigration exposure affects future immigration patterns monotonically.

If these assumptions hold, the IV estimation provides a direct estimate of the effects of immigration on firm-level fixed asset investment, employment, and sales per employee. The reduced-form results from the difference-in-differences estimation do not have this interpretation. In Table 7, I report the effects of an increase in the share of EU8-registered workers on the change in fixed assets, employment, and sales per employee. The data show, on average, no permanent effects within the firm through productivity adjustments, factor price adjustments, or investment. There is a significant and permanent increase in firm-level employment, but only after the policy implementation.

At the same time, the data show differential effects when separating firms by economic sectors. Table 9, Panel B, combines the effect of the announcement and the policy into one indicator variable. It treats the interaction between *ex ante* immigration exposure and the announcement as the explanatory variable. This result can be interpreted as a permanent shift to the outcome variable of interest after the announcement of the EU expansion. There is a permanent and significant increase in fixed asset investment only for construction. Table 9, Panel C, shows the estimate for an IV regression in which the proportion of new EU8 registrations per worker is instrumented by the interaction between ex ante immigration clusters and the expansion announcement. A 1% increase in the proportion of new EU8 registers in a district translates into an increase of 1.26% in fixed asset investments at the firm level for construction firms located in that district. For total assets, the increase is equivalent to 19.1%, which is not statistically significant.

For the service and the knowledge sectors, there is no persistent increase in fixed asset investment. Moreover, for the service sector, the total assets significantly decrease. In the next section, I document another margin by which the changes are persistent. Immigration increases the rate at which firms are created in the economy.

[20]The F-stat of a regression on the excluded instruments is well above the minimum requirement (i.e., F-stat = 10) suggested by Stock and Yogo (2005).

5.4 Firm creation

In this subsection, I explore the effects of immigration on investment in new firms across two dimensions. First, I show the effects of immigration exposure on the number of firms created at the district level. I analyze these effects across different sectors of the economy. Second, I explore the effects on the size of the new firms.

Because I observe the exact date at which each firm is incorporated, I estimate regressions at a quarterly frequency. Annual regressions provide consistent results. The following equation summarizes the main specification:

$$\begin{aligned} ln(Firms_{dt}) = \alpha_d + \alpha_{ct} + \\ + \beta_1 FractionEastern_d * PostAnnounce_t + \\ + \beta_2 FractionEastern_d * PostImplement_t + \varepsilon_{dt}. \end{aligned}$$

$Firms_{dt}$ is the total number of firms created in a district. There are no firm fixed effects in this specification because firm creation is measured at the district level. The time series goes from the first quarter of 2002 until the fourth quarter of 2006.

In Table 6, I show firm creation significantly increases in districts with higher *ex ante* exposure to immigration. After the announcement, a one-standard-deviation increase in *ex ante* Eastern European workers correlates with an increase of 1.78% in firm creation. Furthermore, the policy implementation increases firm creation by an additional 3, which is an economically and statistically significant effect.

I use IV to show the effect of an increase in immigration flow in firm creation. Table 6 provides the estimates. The IV estimation shows a significant increase in firm creation. The average quarterly flow of EU8 workers in the sample is around 0.20% of the labor force. The IV estimation shows that an additional 0.20% quarterly flow of EU8 workers as a proportion initial workforce translates into a 6.7% increase in firm creation at the district level.

Next, I examine whether the new firms created after the immigration policy change are different in size compared with the firms created before the policy change. Normally, young firms do not report their assets for the year of incorporation. To minimize this source of attrition, I collect data on fixed assets for each company either, in the year of incorporation or one year after. Still attrition is important. I summarize each district by the average of the natural logarithm of the fixed assets of created firms. Table 6 shows the results. The estimates are inconclusive mainly because of the large standard errors, but the sign suggests that these new firms are smaller than the ones created before 2003. I combine the effects of the announcement and the policy implementation and find a one-standard-deviation increase in immigration *ex ante* exposure translates into a 0.65%

decrease in the fixed assets of the average entering the market.

After dividing the effects among the sectors, the data show another source of heterogeneity. Table 10, Panel B, shows a significant increase in the number of firms in the knowledge sector, a sector characterized by human -capital-intensive tasks.[21] Panel C presents the IV estimates. New EU8 registrations, which are equivalent to 1% in the labor force, are associated with a significant increase of 7.59% in the number of knowledge firms. The data show a similar result for the service sector, although the skills needed for these tasks are lower than those needed for the knowledge sector. Table 10, Panel C, documents that a 1% increase in new EU8 workers registrations translates into a significant increase of 9.96% in firms created in the service sector.

This increase in firm creation is associated with evidence of competition with pre-existing firms in these sectors. Table 7 shows that pre-existing firms decrease their total assets in the service and in the knowledge sectors. The decrease is statistically significant for the service sector. A 1% increase in the share of immigration-driven labor supply decreases the average service firm by 12.8%. For the knowledge sector, the decrease, although not statistically significant, is 7.73%.

The data show no significant effects for the remuneration of the average worker within the firm in any of the main economic sectors studied. It does show a significant decrease in remuneration for the highest-paid director in the service sector after the policy implementation. If I combine the effects of the policy announcement and policy implementation , the highest-paid directors experience a decrease in their pay in the service and in the knowledge sectors. The results are not statistically significant, but they are economically meaningful. In the knowledge sector, an 1% increase in Eastern European worker registrations as a proportion of existing workers decreased the highest-paid director's remuneration by 12% . For the service sector, the decrease is equivalent to 11%. This is consistent with the increase in competition from the newly incorporated firms.

6 Cultural proximity and social ties or changes in worker's skill-mix

In this section I explore the potential mechanisms behind the effects on existing firm investment and firm creation. Are the changes in investment and number of employees at the firm level related to social ties between firm directors and the immigrants? If cultural or social factors play an important role in the decision to invest, it should be the case that firms with EU8 directors benefit more from the immigration policy change.[22] To

[21] I provide a list of the industries included in this sector in the appendix.

[22] Munshi (2003) shows that networks play an important role in worker earnings. More recently, Burchardi and Hassan (2013) and Burchardi, Chaney, and Hassan (2016) showed that social ties and

test this hypothesis, I collect data on the nationalities of directors for all firms registered in the United Kingdom. I define EU8 majority firms as those in which at least half of the directors in the board are from Eastern European origin as of 2001. The advantage of using data from 2001 is that the board composition is less likely to be affected by the immigration policy. The results are similar if I use contemporaneous board composition.

First, I test whether existing firms with a majority of EU8 directors invest more. I estimate the following equation:

$$\begin{aligned} ln(y_{it}) = \alpha_i + \alpha_{ct} + \\ + \beta_1 FractionEastern_d * PostAnnounce_t + \\ + \beta_2 FractionEastern_d * PostImplement_t + \\ + \beta_3 EU8Firm_i * PostAnnounce_t + \\ + \beta_4 EU8Firm_i * PostImplement_t + \\ + \beta_5 FractionEastern_d * EU8Firm_i * PostAnnounce_t + \\ + \beta_6 FractionEastern_d * EU8Firm_i * PostImplement_t + \varepsilon_{dt}. \end{aligned}$$

The coefficients of interest in this setting are β_5 and β_6. They represent the triple interaction of a firm with a majority of EU8 directors *ex ante*, a firm located in a district with high immigration exposure*ex ante*, and the policy change.

Table 8 shows the within-firm regressions. I only report the relevant coefficients. Although the results are not significant, investment for EU8-directed firms in fixed assets decreases. Employment results are positive, but they are also not statistically significant. On aggregate, this channel does not explain either fixed asset investment or employment decisions.

On the other hand, I can test whether EU8 directors are more likely to create firms after the policy change. I test whether the proportion of firms created by EU8 majority firms increases as a proportion of the total. First, both EU8 majority firm creation and UK majority firm creation increase. However, EU8 firms increase also proportionally to total firms in a district after the announcement. I do not have data on the time of arrival of the directors, but the differential effects between the new and the existing EU8 firms suggest these directors are coming to the United Kingdom.

As discussed by Lewis (2011, 2013), the increase in investment depends on the skill composition of the labor supply shock. Furthermore, from Manacorda, Manning and Wadsworth (2012) there is evidence that immigration to the United Kingdom is predominantly high-skill. High-skill labor is more likely to complement capital. Moreover, an increase in the inflow of high-skill labor can also explain the significant increase in the

migration may be related to more entrepreneurship and investment.

incorporation of knowledge firms.

In this section, I use district aggregate data to provide evidence of two patterns in the data. First, the log odds of high-skill over low-skill labor immigrants in relation to the same ratio for British workers is negatively correlated with *ex ante* immigration in the cross section. The log odds ratio measure selection and sorting since Roy (1951).[23] This implies immigrants positively sort into districts with higher ex ante immigration. Second, the change in the log odds of immigration by high- to low-skill workers before and after the policy is positively correlated to the immigration exposure measure. This implies that the policy changed the skill distribution of immigrants toward high-skill labor.

To measure the proportion of Eastern European workers within a district, I rely on census data. These data are provided for 2001 and for 2011. Skill in this setting is only measured by educational attainment. High-skill workers are those with at least a higher national diploma in the United Kingdom. Low-skill workers are those with no qualifications. Table 11 shows the *ex ante* negative selection of Eastern European immigrants compared to British workers. The log adds positively change when compared with the 2011 census data. These results suggest an improvement in the selection of new immigrants to districts that were *ex ante* more exposed.

7 Conclusion

This paper suggests a causal link between immigration, firm creation, and fixed capital investment. To identify the relationship between immigration and investment, I rely on a modified version of the shift-share measures used in the labor literature. I combine the *ex ante* clusters of immigrants from the same nationalities with a natural experiment: the modification in immigration policy by the United Kingdom triggered by the expansion of the European Union. This time variation allows me to control for local economic shocks and, therefore, reduces the concerns of endogeneity.

My results suggest firms responses to immigration occur in anticipation of future labor flows after the policy implementation. Once the EU announced its expansion, firm creation in districts with a high *ex ante* proportion of workers increased significantly. For pre-existing firms, the adjustments are different. I document a permanent increase in fixed capital and total asset investment only for the construction sector. I find no evidence that the average firm-level remuneration changes after the change in immigration policy in any sector.

I document evidence of competition in the sectors in which adjustment occurs through the incorporation of new firms. For the service and the knowledge sectors, the increase in the number of firms came at the expense of existing firms. Firms are smaller in terms

[23]For an application, see Grogger and Hanson (2011).

of total assets. I find no evidence that this adjustment affects the average worker. I do find evidence that it decreases the compensation of the highest-paid directors at firms in industries where the number of firms increases.

I also explore the channels through which the adjustment happens. I find evidence that EU8 nationals create more firms as a proportion of all firms created in districts more exposed to the change in immigration policy. On the other hand, existing firms with EU8 majority boards do not increase investment in fixed assets. This implies that the increase in EU8 firm creation is more likely caused by new immigrants rather than firms employing existing immigrants. Furthermore, investment is not determined by previously existing ties.

On the other hand, I find evidence in favor of changes in the labor skill composition. I find correlations that suggest that, after the open border policy, the skill selection of immigrants significantly improved. Furthermore, the increase in firm creation concentrates in sectors that rely on human capital, the knowledge sector, of that rely on labor intensive tasks, the service sector. Finally, the only wage effects I find are concentrated on the remuneration of the highest-paid directors in the service and in the knowledge sectors.

My results are economically relevant for shaping the UKs immigration policy. Corporate investment increases in anticipation of immigration labor supply even in the short-term. Moreover, immigration also increases the number of firms created in sectors that rely on human capital. Evidence in the United Kingdom points to adjustments through factor investments and the creation of new firms, rather than through factor.

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Figures

Figure 1: Timeline of immigration decisions by different EU members

This figure summarizes the years in which European countries already members of the EU open their labor markets to nationals from the newly admitted countries. Opening refers to allowing nationals from those countries to work without a Visa or sponsorship application process.

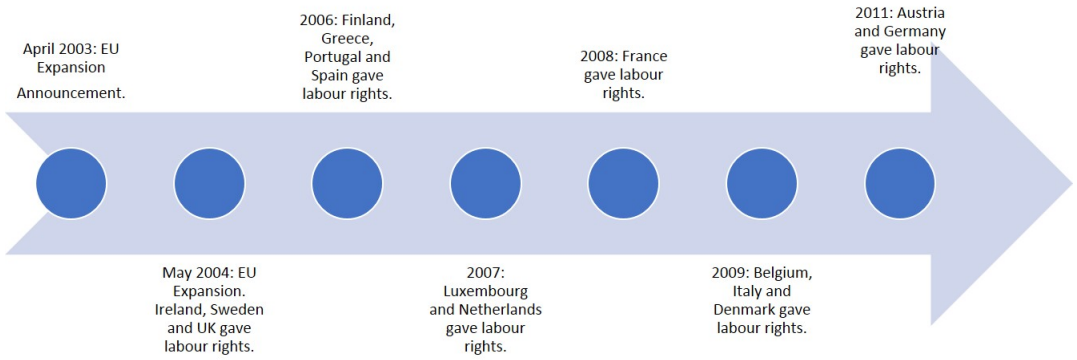


Figure 2: New registrations from EU8 and EU15

NINO is an abbreviation for National Insurance Number. EU8 refers to countries admitted to the EU in 2004. EU15 are countries that already belonged to the EU by 2004.

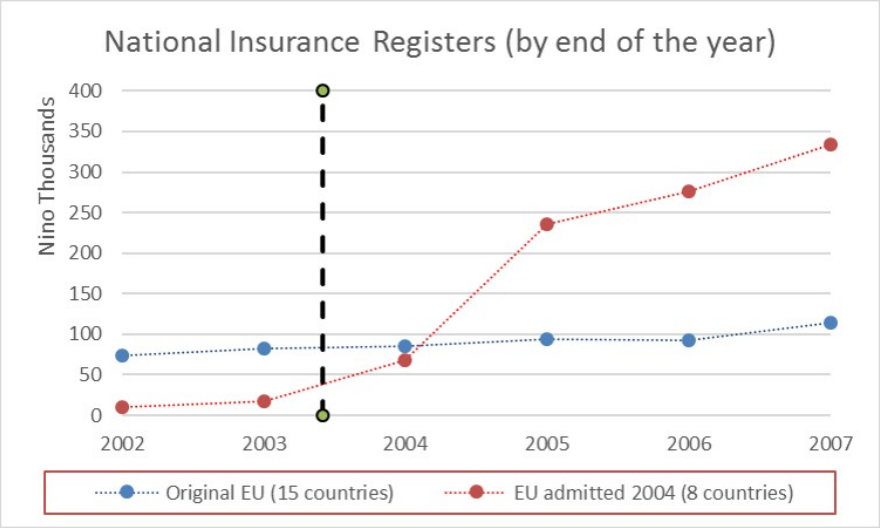


Figure 3: New registrations from EU8 and non-EU Eastern Europe

NINO is an abbreviation for National Insurance Number. EU8 refers to countries admitted to the EU in 2004. Non admitted EU are Bulgaria and Romania. These are European countries that were not part of the EU by 2004 and were also not incorporated in the expansion. They were incorporated in the next expansion, but obtained labor rights within the UK in 2014.

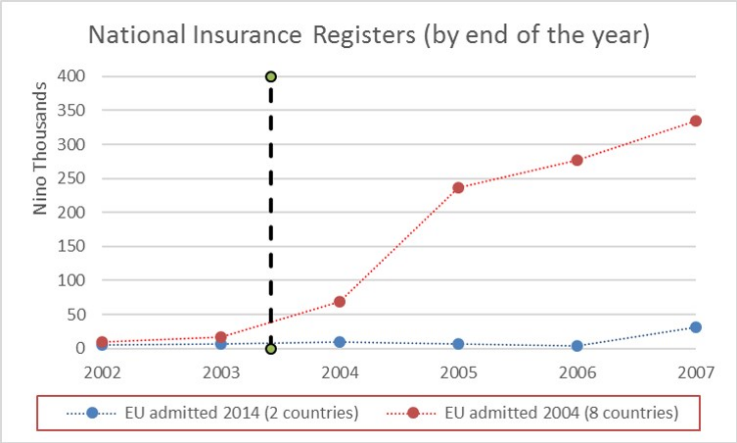


Figure 4: Quarterly new registrations of nationals from countries admitted in 2004

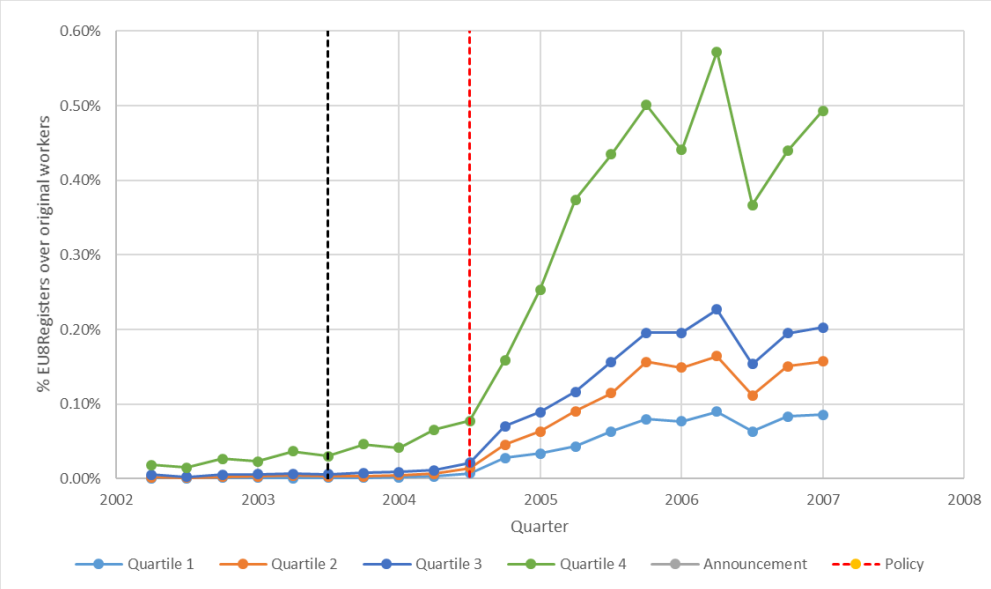


Figure 5: New firms incorporated per quarter

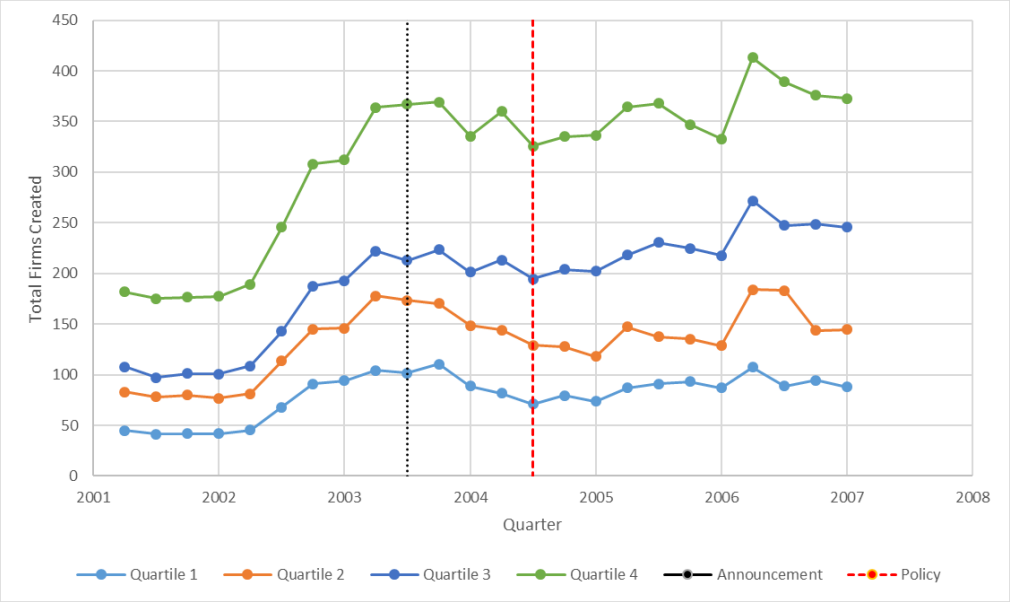
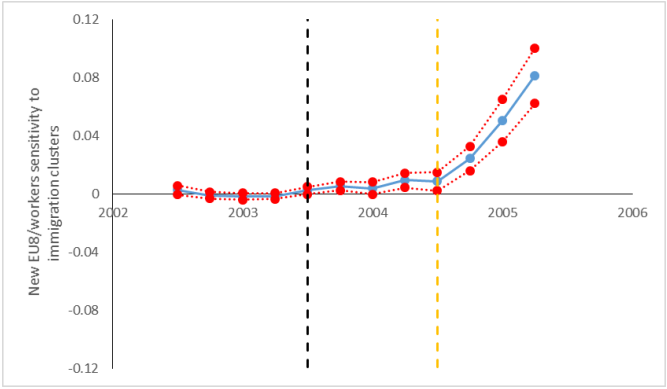


Figure 6: Estimation of regression coefficients Pre and Post Policy

Coefficient estimates of each variable of interest on an interaction between *ex-ante* immigration and a dummy variable. 95% confidence intervals reported in red. The vertical lines represent the open policy announcement and implementation.

(a) Share of new EU8 registrations



(b) Logarithm of total new firms created in a district

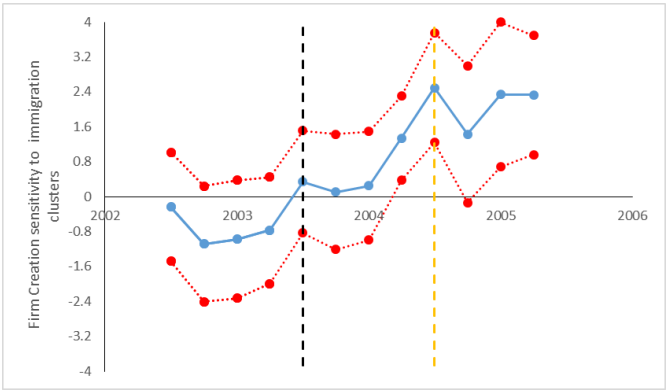
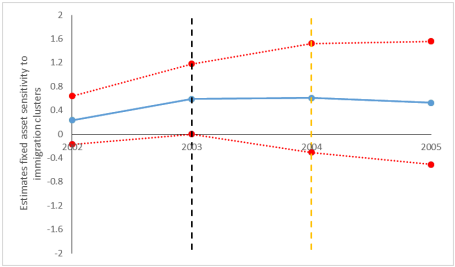


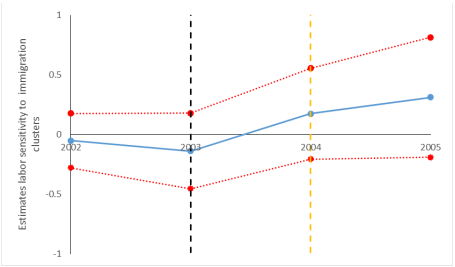
Figure 7: Regression estimates of pre-treatment trends within firms

Coefficient estimates of each variable of interest on an interaction between *ex-ante* immigration and a dummy variable. 95% confidence intervals reported in red. The vertical lines represent the open policy announcement and implementation. For firm-level data I have only two periods before the announcement. Therefore, there is only one coefficient estimate before the announcement.

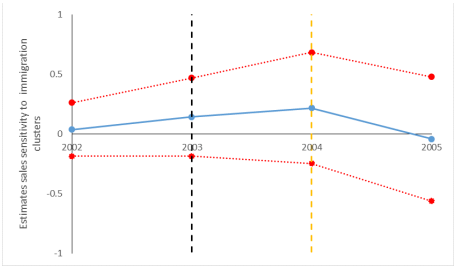
(a) Logarithm Fixed assets



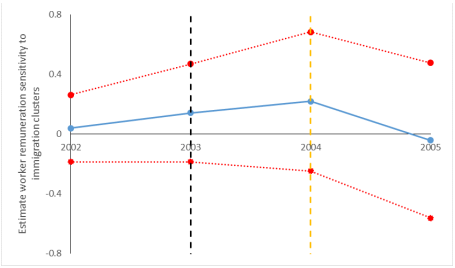
(b) Logarithm Employees



(c) Logarithm Sales



(d) Logarithm Average Remuneration



Tables

Table 1: District-level summary statistics for Immigration and Labor Data

All data are from the Department of Work and Pensions (DWP). New registrations refer to new national insurance numbers issued to incoming workers of all nationalities. EU8 refers to nationals from countries admitted to the EU in 2004. EU15 refers to nationals from countries that belonged to the EU before the 2004 expansion. The new countries admitted to EU in 2007 refer to Bulgaria and Romania.

	Pre EU8 admission (2002-2003)	Post EU8 admission (2004-2007)
New registrations	842.9	1556.3
	(1510.1)	(2,471.7)
New registrations EU8	34.3	572.1
	(98.5)	(829.8)
New registrations EU15	177.6	236.9
	(330.8)	(480.3)
New registrations new to EU 2007	15.8	32.53
	(48.7)	(144.8)
New registrations per ex-ante workers (%)	0.93%	1.82%
	(1.31%)	(2.13%)
EU8 new registrations per ex-ante workers (%)	0.04%	0.73%
	(0.09%)	(0.81%)
EU15 new registrations per ex-ante workers (%)	0.21%	0.27%
	(0.32%)	(0.47%)
New to EU 2007 per ex-ante workers (%)	0.02%	0.04%
	(0.04%)	(0.14%)
Activity Rate (%)	79.85%	78.19%
	(5.47%)	(4.85%)
Workers	72,807	75,659
	(46,892)	(49,757.1)
	Mean (St Dev)	Mean (St Dev)

Table 2: Firm-level summary statistics on board composition. Firms incorporated before 2000

All data are from BvD's Orbis and Fame databases. UK directors are directors with British nationality. EU15 includes directors with a nationality from any of the countries that were members of the European Union before 2004, excluding the UK. EU8 includes nationals from the countries admitted to the European Union in the 2004 expansion.

Year	%Directors from UK	%Directors from EU countries admitted pre-2004 (EU15)	%Directors from EU countries admitted in 2004 (EU8)	Number of firms
2000	90.1% (29.9%)	4.4% (20.6%)	0.08% (2.7%)	771,625
2001	91% (28.6%)	4.5% (20.7%)	0.08% (2.7%)	702,960
2002	91.5% (27.9%)	4.4% (20.6%)	0.07% (2.7%	634,613
2003	91.7% (27.6%)	4.4% (20.5%)	0.08% (2.7%)	584,909
2004	91.8% (27.4%)	4.4% (20.5%)	0.07% (2.7%)	549,130
2005	91.9% (27.3%)	4.5% (20.6%)	0.08% (2.7%)	520,854
2006	91.9% (27.3%)	4.5% (20.7%)	0.08% (2.7%)	500,311
2007	92% (27.1%)	4.6% (20.9%)	0.08% (2.8%)	484,098
2008	92% (27.2%)	4.7% (21.1%)	0.08% (2.9%)	468,542
	Mean (St Dev)	Mean (St Dev)	Mean (St Dev)	

Table 3: Firm-level summary statistics on board composition for firms by year of incorporation

All data are from BvD's Orbis and Fame databases. The UK directors are those British nationality. EU15 includes directors with a nationality from any of the countries that were members of the European Union before 2004, excluding the UK. EU8 includes nationals from the countries admitted to the European Union in the 2004 expansion.

Incorporation	%Directors from UK	%Directors from EU countries admitted pre-2004 (EU15)	%Directors from EU countries admitted in 2004 (EU8)	Number of firms
2000	83.5% (37.1%)	4.6% (20.9%)	0.1% (3.2%)	123,487
2001	80.9% (39.3%)	4.6% (21%)	0.1% (3.2%)	124,395
2002	82% (38.4%)	4.7% (21.1%)	0.14% (3.7%)	199,048
2003	80.7% (39.5%)	5% (21.7%)	0.26% (5.1%)	283,884
2004	76.1% (42.6%)	8% (27.1%)	0.36% (6%)	250,750
2005	72.8% (44.5%)	9.5% (29.3%)	0.55% (7.4%)	272,563
2006	72.3% (44.7%)	9.6% (29.5%)	0.74% 8.6%	306,941
2007	73.9% (43.9%)	7.3% (26%)	0.93% (9.6%)	363,816
2008	76.2% (42.6%)	7.7% (26.7%)	0.96% (9.8%)	290,796
	Mean (St Dev)	Mean (St Dev)	Mean (St Dev)	

Table 4: Firm-level summary statistics on fixed assets for firms that had at least one employee over the sample

data are from ORBIS and Fame Databases. All numbers are in thousands except employees and number of firms. Number of firms refers to firms that have data at least on fixed assets. All nominal values are in pounds sterling.

Year	Fixed assets	Total employee remuneration	Total directors remuneration	Number of employees	Average employee remuneration	Remuneration highest paid director	Number of firms
2001	497.4	487.7	289	243	17.4	243.8	86,788
	(6,392.7)	(1,639.7)	(688.3)	(2,955)	(38.8)	(494)	
2002	489.6	488.8	294.3	243	17.7	249.7	86,597
	(6,753.4)	(1,357.9)	(674.5)	(2,968)	(37)	(463.2)	
2003	476.6	494.6	305.5	250	17.8	258.3	86,478
	(6,902.1)	(2,836.8)	(786.4)	(3,091)	(37.2)	(659)	
2004	460.2	458.8	333.7	264	18	260.1	85,104
	(3,803.7)	(1,081.9)	(891.9)	(3,381)	(39.8)	(775.3)	
2005	474.5	484.3	371.9	310	19.3	268.6	55,889
	(3,916.1)	(1,131.9)	(1,062.2)	(3,999)	(43)	(744.3)	
	Mean	Mean	Mean	Mean	Mean	Mean	
	(St Dev)	(St Dev)	(St Dev)	(St Dev)	(St Dev)	(St Dev)	

Table 5: Allocation of EU8 new registrations at a quarterly frequency

FractionEastern refers to the fraction of workers from the EU8 plus Bulgaria and Romania as of the 2001 census. PostAnnounce is an indicator variable with value one after the announcement of the EU expansion in the second quarter of 2003. PostImplement is an indicator variable with value one after the implementation of the open border policy in the second quarter of 2004. All standard errors are clustered at the district level. Area refers to NUTS2 statistical areas that cover all England.

Panel A: Continuous Exposure Measure

	$ShareRegisteredWorkersEU8_{dt}$
$FractionEastern_d * PostAnnounce_t$	0.005***
	(0.001)
$FractionEastern_d * PostImplement_t$	0.07***
	(0.007)
$AdjR^2$	0.8275
District FE	Yes
Area*Quarter FE	Yes
N	7,704

Panel B: Dummy Exposure Measure

	$ShareRegisteredWorkersEU8_{dt}$
$HighFractionEast8_d * PostAnnounce_t$	0.0000422***
	(6.81e-06)
$HighFractionEast8_d * PostImplement_t$	0.00146***
	(0.00015)
$AdjR^2$	0.7094
District FE	Yes
Area*Quarter FE	Yes
N	7,704

Table 6: District-level regressions

FraEast refers to the fraction of workers from the EU8 plus Bulgaria and Romania as of the 2001 census. Ann is an indicator variable with value one after the announcement of the EU expansion in the second quarter of 2003. Imp is an indicator variable with value one after the implementation of the open border policy in the second quarter of 2004. All standard errors are clustered at the district level. All regressions use district fixed effects and area-time dummies. Fixed assets refer to the average firm fixed assets that existed in the district. Mean wage is obtained directly from the census data. The district level results are similar if I use the average employee remuneration from the FAME firm-level data.

Panel A: District-Level Regressions Announcement and Implementation

	Quarterly	***Yearly***		
	ln(Firms)	*ln(FixedAssets)*	*ln(FixedAssetsNew)*	*ln(MeanWages)*
*FraEast*Ann*	0.84**	0.88*	0.64	-0.25
	(0.40)	(0.49)	(11.25)	(0.20)
*FraEast*Imp*	1.41***	-0.52	-1.18	-0.01
	(0.53)	(0.64)	(0.85)	(0.14)
N	7661	1595	1595	1585
Adj R2	0.95	0.93	0.52	0.96

Panel B: First stage policy and announcement combined

	NewEU8/L	*NewEU8/L*		
*FraEast*Ann*	0.05***	0.16***	0.16***	0.20***
	(0.006)	(0.02)	(0.02)	(0.02)
N	4644	1147	1147	1585
F	75.95	109.76	109.76	143.02

Panel C: Reduced form policy and announcement combined

	ln(Firms)	*ln(FixedAssets)*	*ln(FixedAssetsNew)*	*ln(MeanWages)*
*FraEast*Ann*	1.67**	0.82	-0.31	-0.26
	(0.71)	(0.62)	(1.11)	(0.22)
N	4644	1147	1147	1585
Adj R2	0.96	0.91	0.57	0.96

Panel D: IV

	ln(Firms)	*ln(FixedAssets)*	*ln(FixedAssetsNew)*	*ln(Wages)*
NewEU8/L	32.3**	5.07	-1.93	-1.31
	(13.07)	(3.94)	(6.93)	(1.09)
N	4644	1147	1147	1585
Centered R2	0.97	0.94	0.74	0.97

Table 7: Firm-level regressions, firms incorporated before 2001

FraEast fraction of workers from the EU8 plus Bulgaria and Romania as of the 2001 census. Ann is an indicator variable with value one after the announcement of the EU expansion. Imp is an indicator variable with value one after the implementation of the open border policy. WorkRem is the average employee remuneration in the firm. DirRem is the remuneration of the highest paid director. NewEU8/L is the fraction of new EU8 registrations over 2001. Sales/L is total revenue per worker. K/L is fixed assets per employee. Standard errors are clustered at the district level. All regressions use firm fixed effects and area-time dummies.

Panel A: Firm-level regressions announcement and implementation

	Factor Remuneration		***Productivity***	***Factor Adjustments***			
	ln(WorkRem)	*ln(DirRem)*	*ln(Sales/L)*	*ln(TotalAssets)*	*ln(FixedAssets)*	*ln(Employees)*	*ln(K/L)*
*FraEast*Ann*	0.12	0.29	0.39**	-0.24	0.47**	-0.11	0.20
	(0.14)	(0.30)	(0.17)	(0.29)	(0.23)	(0.12)	(0.22)
*FraEast*Imp*	-0.01	-0.34	-0.10	0.03	0.00	0.36**	-0.68**
	(0.12)	(0.25)	(0.23)	(0.32)	(0.29)	(0.11)	(0.34)
N	269557	72444	216779	415518	351898	299847	269557
Adj R2	0.98	0.85	0.97	0.85	0.93	0.96	0.95

Panel B: Reduced form announcement and implementation combined

	ln(WorkRem)	*ln(DirRem)*	*ln(Sales/L)*	*ln(TotalAssets)*	*ln(FixedAssets)*	*ln(Employees)*	*ln(K/L)*
*FraEast*Ann*	0.09	-0.14	0.34	-0.49	0.32	0.02	-0.19
	(0.18)	(0.27)	0.23	(0.35)	(0.32)	(0.13)	(0.27)
N	195362	55153	156546	314628	263638	217446	192206
Adj R2	0.99	0.87	0.98	0.88	0.94	0.96	0.96

Panel C: IV announcement and implementation combined

	ln(WorkRem)	*ln(DirRem)*	*ln(Sales/L)*	*ln(TotalAssets)*	*ln(FixedAssets)*	*ln(Employees)*	*ln(K/L)*
NewEU8/L	0.79	-1.17	3.04	-3.94	2.61	0.17	-1.75
	(1.56)	(2.25)	(1.92)	2.69	(2.68)	(1.21)	(2.45)
N	195362	55153	156546	314628	263638	299847	192206
Centered R2	0.99	0.92	0.98	0.91	0.96	0.98	0.97

Table 8: Cultural proximity or new entrepreneurs

All standard errors are clustered at the district level. FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. Ann is an indicator variable that takes value 1 after 2003, the year the EU expansion was announced. Imp is an indicator variable that takes value 1 after the EU expansion was implemented. EU8 Firms refer to firms with a majority of members with a EU8 nationality.

Panel A: Differential effects firms with EU8 boards

	ln(FixAssets)	*ln(Employees)*	*ln(K/L)*
*FrEast*EU8Firm*Announcement*	0.06	0.19	(0.41)
	(1.49)	(0.87)	(1.46)
*FrEast*EU8Firm*Implementation*	-0.87	0.79	-0.95
	(1.51)	(0.59)	(1.54)
Interactions	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes
Area*Year FE	Yes	Yes	Yes
Adj R2	0.94	0.96	0.96
N	351898	299847	265694

Panel B: New firms board nationalities

	ln(UKFirms)	*ln(EU8FIrms)*	*%EU8Firms*
*FrEast*Announcement*	1.36***	1.14	0.043***
	(0.48)	(1.72)	(0.01)
*FrEast*Implementation*	0.46	2.25	0.00
	(0.40)	(2.33)	(0.01)
District FE	Yes	Yes	Yes
Area*Year FE	Yes	Yes	Yes
Adj R2	0.96	0.62	0.13
N	7661	1196	7657

Table 9: Intensive margin firm-level results by economic sector

All standard errors are clustered at the district level. All regressions include a firm fixed effects and a year*area dummy. FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. Ann is an indicator variable that takes value 1 after 2003, the year the EU expansion was announced. Imp is an indicator variable that takes value 1 after the EU expansion was implemented. NewEu8/L refers to new registrations from EU8 divided by the total number of workers in 2001. The sectors are knowledge, construction and services. For more information about the construction of these sectors refer to the appendix.

Panel A: Firm Level Regressions Announcement and Implementation

	ln(FixedAssets)			*ln(Employees)*			*ln(TotalAssets)*			*ln(WorkRem.)*			*ln(Dir.Rem.)*		
*FrEast*Ann*	0.47	0.99	2.68***	0.19	-0.44	-0.34	-0.49	2.40*	-0.97	0.14	0.26	-0.52	-0.17	0.93	-1.45
	(0.68)	(0.69)	(0.75)	(0.27)	(0.34)	(0.36)	(0.76)	1.25	(0.90)	(0.52)	(0.41)	(0.51)	(1.14)	(0.80)	(1.23)
*FrEast*Imp*	0.03	0.85	0.38	0.11	0.39	0.79	0.52	-0.14	-1.14	-0.60	0.21	0.55	-1.68	0.26	-2.37**
	(0.70)	(0.62)	(1.02)	(0.27)	(0.45)	(0.50)	(0.74)	(0.77)	(1.13)	0.42	(0.54)	(0.54)	(0.89)	(0.99)	(1.18)
N	42855	27583	24388	36869	22164	20488	51542	31870	30619	31977	19415	17855	7948	6562	3786
Adj R2	0.91	0.93	0.9	0.97	0.96	0.96	0.84	0.86	0.79	0.97	0.98	0.98	0.9	0.87	0.89
Sector	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv

Panel B: Reduced form policy and announcement combined

	ln(FixedAssets)			*ln(Employees)*			*ln(TotalAssets)*			*ln(WorkRem.)*			*ln(Dir.Rem.)*		
*FrEast*Ann*	0.52	1.26**	1.14	0.11	-0.40	0.18	-0.95	2.46*	-1.55*	-0.24	0.14	-0.76	-1.50	0.55	-1.48
	(0.79)	(0.61)	(1.26)	(0.33)	(0.37)	(0.44)	(0.78)	1.45	(0.91)	(0.48)	(0.39)	(0.62)	(1.18)	(0.99)	(1.63)
N	32390	20853	18450	26981	16104	14889	39325	24207	23358	23461	14067	12979	6330	5117	2960
Adj R2	0.92	0.93	0.92	0.97	0.97	0.96	0.86	0.88	0.82	0.98	0.99	0.98	0.91	0.89	0.89
Sector	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv

Panel C: IV

	ln(FixedAssets)			*ln(Employees)*			*ln(TotalAssets)*			*ln(WorkRem.)*			*ln(Dir.Rem.)*		
NewEU8/L	4.52	10.11*	10.25	1.09	-3.69	1.74	-8.04	19.13	-13.72*	-2.20	1.29	-7.28	-12.82	5.10	-11.79
	(6.91)	(5.28)	(11.90)	(3.10)	(3.40)	(4.20)	6.66	11.83	(8.09)	(4.49)	(3.71)	(6.32)	(10.13)	(9.08)	(12.77)
N	32390	20853	18450	26981	16104	14889	39325	24207	23358	23461	14067	12979	6330	5117	2960
Centered R2	0.95	0.95	0.94	0.98	0.98	0.98	0.90	0.91	0.87	0.98	0.99	0.99	0.95	0.93	0.93
Sector	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv	Know	Constr	Serv

Table 10: District-level firm creation regressions by economic sector

All standard errors are clustered at the district level. FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. Ann is an indicator variable that takes value 1 after 2003, the year the EU expansion was announced. Imp is an indicator variable that takes value 1 after the EU expansion was implemented. NewEu8/L refers to new registrations from EU8 divided by the total number of workers in 2001. The sectors are knowledge, construction and services. For more information about the construction of these sectors refer to the appendix. EU8 firms refer to firms with a majority of EU8 national in the boards at the moment of incorporation.

Panel A: District-Level Regressions Announcement and Implementation

	ln(Firms)			*ln(EU8Firms)*		
*FraEast*Ann*	0.20	-1.15	0.48	-0.88	2.38	-1.28
	(0.89)	(0.89)	(0.81)	(1.46)	(1.65)	(1.40)
*FraEast*Imp*	0.33	2.24**	1.98	1.72	-1.27	-0.05
	(0.71)	(0.91)	1.08	(1.70)	(1.78)	(1.52)
N	1914	1909	1912	1839	1793	1819
Adj R2	0.94	0.93	0.92	0.82	0.75	0.82
Sector	Know	Constr	Serv	Know	Constr	Serv

Panel B: Reduced form policy and announcement combined

	ln(Firms)			*ln(EU8Firms)*		
*FraEast*Ann*	1.62**	0.82	2.04**	0.58	1.89	-1.14
	(0.81)	(0.93)	(0.99)	(1.39)	(1.69)	(1.24)
N	1594	1592	1593	1535	1506	1520
Adj R2	0.94	0.94	0.92	0.82	0.77	0.82
Sector	Know	Constr	Serv	Know	Constr	Serv

Panel C: IV

	ln(Firms)			*ln(EU8Firms)*		
NewEU8/L	7.32**	3.72	9.22**	2.79	8.58	-5.27
	(3.60)	(4.06)	(4.52)	(6.66)	(7.51)	(5.69)
N	1594	1592	1593	1535	1506	1520
Centered R2	0.96	0.96	0.94	0.88	0.84	0.87
Sector	Know	Constr	Serv	Know	Constr	Serv

Table 11: Selection of migrants

FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. The first two regressions are cross-sectional. The last regression measures the change between 2011 and 2001 and can be interpreted as accounting for a district fixed effect. All regressions control for the NUTS2 Areas.

	ln(OddsEU8)-ln(OddsUK)	*ln(OddsEU8)-ln(OddsUK)*	*ln(OddsEU8)-ln(OddsUK)*
FrEast	-7.58***	-1.79	5.79***
	(1.28)	(1.68)	(1.85)
Area FE	Yes	Yes	Yes
Census Year	2001	2011	Change
Adj R2	0.94	0.96	0.96
N	323	323	323

Appendix

Table A.1: Most frequent industries by firms incorporated in 2001

NACE	Industry Name	Incorporated 2001	% Over Total 2001	Incorporated 2006	% Over Total 2006
8299	Other business support activities	22,302	16.9	51,634	21.72
7022	Business and other management consulting activities	7,380	5.59	12,003	5.05
6209	Other Information technology and computer service activities	6,847	5.19	8,751	3.68
6920	Accounting bookkeeping and auditing activities; tax consultancy	3,704	2.81	2,945	1.24
6820	Renting and operating of own or leased real state	3,626	2.75	4,345	1.83
4110	Development of building projects	3,540	2.68	6,826	2.87
4120	Construction of buildings	3,345	2.53	6,025	2.53
9609	Other personal service activities	3,193	2.42	6,342	2.67
6202	Computer consultancy activities	2,695	2.04	6,913	2.91
5829	Other software publishing	2,512	1.9	494	0.21

Table A.2: EU8 firm creation in top 10 industries

NACE	Industry Name	Incorporated by EU8 board 2001	% Over EU8 2001	Incorporated by EU8 board 2001	% Over EU8 2006	% Increase
8299	Other business support activities	21	21.88	250	16.93	10.90
7022	Business and other management consulting activities	2	2.08	30	2.03	14.00
6209	Other Information technology and computer service activities	3	3.13	25	1.69	7.33
6920	Accounting bookkeeping and auditing activities; tax consultancy	2	2.08	13	0.88	5.50
6820	Renting and operating of own or leased real state	1	1.04	3	0.2	2.00
4110	Development of building projects	2	2.08	22	1.49	10.00
4120	Construction of buildings	3	3.13	94	6.36	30.33
9609	Other personal service activities	4	4.17	80	5.42	19.00
6202	Computer consultancy activities	1	1.04	36	2.44	35.00
5829	Other software publishing	0	0	1	0.07	NA

Table A.3: Industries classified as knowledge sector

NACE Code	Industry
5821	Publishing of Computer Games
5829	Other Software Publishing
6110	Wired telecommunications activities
6120	Wireless telecommunications activities
6130	Satellite telecommunications activities
6190	Other telecommunications activities
6201	Computer programming activities
6202	Computer consultancy activities
6203	Computer facilities management activities
6209	Other information technology and computer service activities
6311	Data processing, hosting and related activities
6312	Web portals
7022	Business and other management consulting activities
7111	Architectural activities
7112	Engineering activities and related technical consultant
7120	Technical testing and analysis
7211	Research and experimental development on biotechnology
7219	Other research and experimental development on natural sciences and engineering
7220	Research and experimental development on social sciences and humanities
7410	Specialised design activities
7420	Photographic activities
7490	Other professional, scientific and technical activities n.e.c.
7500	Veterinary activities
8510	Pre-primaryeducation
8520	Primary education
8531	General secondary educatio
8532	Technical and vocational secondary education
8541	Post-secondary non-tertiary education
8542	Tertiary education
8560	Educational support activities
8610	Hospital activities
8621	General medical practice activities
8622	Specialist medical practice activities
8623	Dental practice activities

Table A.4: Industries classified as construction sector

NACE Code	Industry
4110	Development of building projects
4120	Construction of residential and non-residential buildings
4211	Construction of roads and motorways
4212	Construction of railways and underground railways
4213	Construction of bridges and tunnels
4221	Construction of utility projects for fluids
4222	Construction of utility projects for electricity and telecommunications
4291	Construction of water projects
4299	Construction of other civil engineering projects n.e.c.
4311	Demolition
4312	Site preparation
4313	Test drilling and boring
4321	Electrical installation
4322	Plumbing, heat and air conditioning installation
4329	Other construction installation
4331	Plastering
4332	Joinery installation
4333	Floor and wall covering
4334	Painting and glazing
4339	Other building completion and finishing
4391	Roofing activities
4399	Other specialised construction activities n.e.c.

Table A.5: Industries classified as service sector

NACE Code	Industry
5610	Restaurants and mobile food service activities
5621	Event catering activities
5629	Other food service activities
5630	Beverage service activities
8299	Other business support activities
9700	Activities of households as domestic personnel