Essays in Empirical Corporate Finance

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14 November 2018

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Acknowledgements

My most sincere gratitudes to my supervisors Dirk Jenter and Daniel Paravisini who have taught, guided, and challenged me to improve my research over my years at the LSE. I hope, through my work, I can be fair to their brilliant legacy.

I want to give special thanks also to Juanita Gonzalez-Uribe Moqi Groen-Xu, and Ashwini Agrawal for all their mentoring. They helped me since the earliest versions of these papers. They found time on their already very busy schedules to discuss, brainstorm, revise presentations and revise previous versions of this draft. I am truly indebted to them. I also want to thank all faculty at the LSE for their invaluable comments and support. I want to thank the Financial Markets Group (FMG), Abraaj Capital, and the LSE Finance Department for their financial support.

I want to thank my co-authors Bernardo Ricca and Jose Morales. Especially Bernardo, who shared with me a significant amount of time at the FMG office discussing research. My research is heavily influenced by these discussion. One of the chapters of this dissertation could not have been finished without Bernardo and Jose's meticulous and hard work. I want to thank Tom Kirchmaier for his advice on the second chapter of this dissertation.

During my stay at the LSE I had great colleagues that shaped my research. Some of them during my entire career. Thank you Lorenzo Bretscher, Fabrizio Core, Friedrich Geiecke, James Guo, Yueyang Han, Lukas Kremens, Adrien Matray, Olga Obizhaeva, Dimitris Papadimitriou, Marco Pelosi, Michael Punz, Malgorzata (Gosia) Ryduchowska, Una Savic, Seyed Seyedan, Daniel Urban, and Su Wang.

I want to thank all the admin. staff who always worked hard to make us feel like home. Especially I want to thank Elizabeth Bunting, Mary Comben, Catherine Perry, Osmana Raie, and Simon Tuck.

Most importantly I want to thank my parents Jose Antonio and Maria del Valle. They always believed in me, taught me the discipline to succeed, gave me living examples of hard work, and encouraged me whenever I faltered. I want to thank my brother Jose Ignacio for his encouragement. My aunt Aura Leon who taught me how to read. My godfather Luis Vicente and my godmother Olga for their support. My grandparents, who could not see this day, but cared for me always, and would have been incredibly proud. To all my family.

I dedicate this work to my future wife Isabel, who experienced all the ups and downs of the PhD with me. Whenever I had setbacks, she gave me the strength to keep going. Thank you.

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I warrant that this authorisation does not, to the best of my belief, infringe the rights of any third party. Statement of conjoint work I confirm that chapter 2 is jointly co-authored with Bernardo Ricca, Jose Morales and I contributed 1/3 of this work. I declare that my thesis consists of 39165 words.

Abstract

This dissertation summarizes my work on how corporations adjust to both policy driven and random shocks.

In the first chapter I document how corporate investment reacts to immigration. I use an interaction of *ex ante* clusters 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 variation introduced by the immigration policy allows me to control for local economic shocks. 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 indicate that firms might quickly react to an immigration-induced labor supply shock.

In the second chapter, Bernardo Ricca, Jose Morales and I explore the unintended consequence of the Mexican drug war. Using a similar strategy to Dell (2015), we show that close elections in which PAN (the party that implemented the war) cause a significant decrease in export growth. We also provide evidence that worker displacement is an important channel for the effect.

In the third chapter I answer whether managerial experience explain investment to cash sensitivity. Using a unique, innovative hand-constructed database, this study estimates the sensitivity of investment to cash for European football managers with different experience. To avoid endogeneity issues, I exploit random cash awards to clubs. I estimate these random awards using *ex-ante* odds of matches. When odds are close, cash awards can be considered as good as randomly assigned. After a cash windfall, only managers with low experience spend more on new players. The increase in gross investment is not linked to better performance for the team.

1 Effects of Immigration on Investment and Firm Creation

Jesus Gorrin

1.1 Introduction

For many net-receiving countries, immigration has become one of the main sources of new labor over the past decades. 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 topic. 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 arguments against immigration focus on its potential negative short-term wage effects. The logic sounds simple: immigration increases labor supply and, therefore, decreases labor costs. Finding these negative wage effects in the data, however, 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 contributes to the debate by examining 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. Thereby, 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

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

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.

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 an 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 total 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 all firms in the construction sector.

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

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

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 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 pa-

[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.

per 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 cultural 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, over the past two decades, 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

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

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, 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.

1.2 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 implementation of the treaty allowed immigration policy

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

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 from May 2004.

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. 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.

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%

[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.

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.

1.3 Data

1.3.1 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 registration 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 with a standard deviation of 63,042 people. In terms of population, English districts are comparable to counties in the United States.

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

In Table 1.1, 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 to foreigners 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.

1.3.2 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 1.2 and 1.3 provide firm-level summary statistics for the characteristics of the board of directors. Table 1.2 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).

[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.

On the other hand, there seems to be a structural change on the board composition for younger firms. Table 1.3 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 0.08% to almost 1% by 2008.

1.3.3 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 1.4 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.

1.4 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,

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

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. This is an improvement over the standard shift-share instrument that predicts future flows of migrants based on past stock of migrants from the same origin. By exploiting the policy change, I absorb time-invariant characteristics of the locations where original Eastern European migrants settled before the policy.

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 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-

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

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 this 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 European 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

[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.

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 1.6 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 identification using my difference-in-differences strategy. 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.

1.5 Main results

1.5.1 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:

$$\begin{aligned} ShareNewRegisteredWorkersEU8_{dt} = {} & \alpha_d + \alpha_{ct} + \\ & + \beta_1 FractionEastern_d * PostAnnounce_t + \\ & + \beta_2 FractionEastern_d * PostImplement_t + \varepsilon_{dt}. \end{aligned}$$

$ShareNewRegisteredWorkersEU8_{dt}$ 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 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 em-

[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.

pirical strategy. For example, within an area-time, like Inner London in a specific year, the identification captures the effect across different districts.

Table 1.5, 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 1.5, 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. Alternatively, as a robustness check I separate the effect of announcement and policy in two non-overlapping variables. Both the announcement and the implementation are significant, but the effect of the implementation is larger.

1.5.2 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 adjustments should lag labor flows. It is possible that capital takes time to build, but, in

[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%.

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 1.6, 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 1.6 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 controls for district fixed effects and area-time dummies. The sign is positive, but not

[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.

significant. Table 1.6 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.

1.5.3 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]

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

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

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. As a robustness check, and to diminish multicollinearity concerns, I separate the effect of the announcement and the implementation in two non-overlapping variables. The estimation of the effect of the announcement is quantitatively similar and significant. The effect of the implementation remains insignificant.

In Table 1.7, 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 1.7, 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 1.7, 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]

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

[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.

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 1.7, 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 1.7, 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 1.9, 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 1.9, 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).

1.5.4 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 1.6, 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 1.6 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 1.6 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 1.10, 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 1.10, 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 1.7 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.

There is still one important question about firm creation. The creation of new firms might increase the probability of firms leaving. My sample comprises all dead and existing firms over the sample from 2001 until 2006. Firms are forced to provide information to Company's House every year. I assume a firm dies if no information is provided after a particular year or if the firm is officially desincorporated. In the Appendix I show there are no effects after the announcement and implementation of the policy in the destruction of firms. This is true for both the number of firms destroyed and for the probability of a firm dying over a year after it is created.

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

1.5.5 Robustness

One potential shortcoming of my identification strategy is that spillovers across districts might bias the results towards zero. For example, if migrants tend to work in different districts than those in which they register, districts close to Eastern European hubs may also experience increases in investment or potential changes in wages. To avoid this problem I replicate the main results of my paper at a different aggregation level. I use Travel to Work Areas (TTWA) as constructed using the census of 2001. According to the ONS, TTWAs are areas constructed in a way that resemble labour markets, areas in which workers both live and work (Prothero, 2016). If the expected increase in labor force from the policy change induces firms to invest more, the effects should be larger at the higher aggregation level. This is because The cost of a worker moving across different travel to work areas is higher.

The aggregation comes at a cost. The coarser level does not allow me to control for NUTS2 area trends because travel to work areas might be larger than NUTS2 areas. If cities suffer shocks that are particular to them and happen at the same time than the policy announcement, my identification would not provide causal effects. To mitigate this issue, I control for region trends.[22] As expected, the investment results are even stronger and more significant at this level of aggregation. There is significantly more firm creation and fixed asset investment in travel to work areas that had higher cluster of Eastern European workers *ex-ante*. However, the effects on wages are still insignificant.[23].

1.6 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.[24] To 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.

[22] There are 9 regions in England.

[23] See appendix for the regressions under this level of aggregation

[24] 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 migration may be related to more entrepreneurship and investment.

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 1.8 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 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).[25] This

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

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 1.11 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.

1.7 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 results consistent with an increase 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 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. 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 support for the hypothesis that immigration changes 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, or 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 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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1.9 Figures

Figure 1.1: Timeline of immigration decisions by different EU members

This figure summarizes the years in which European countries which are 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.

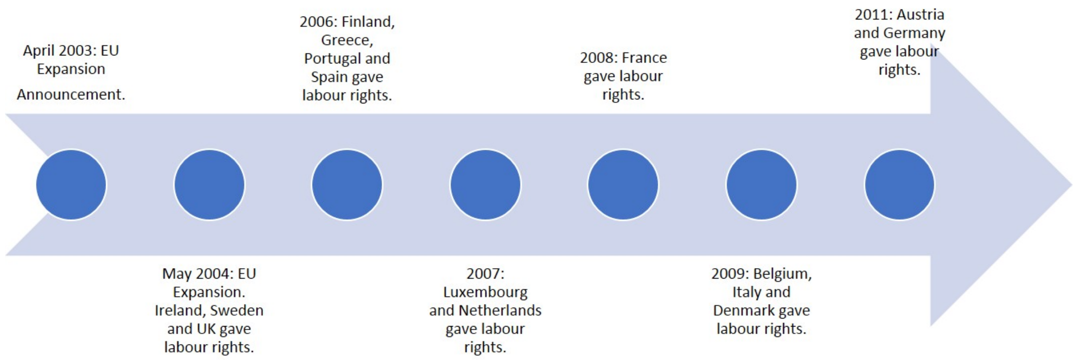


Figure 1.2: New registrations from EU8 and EU15

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.

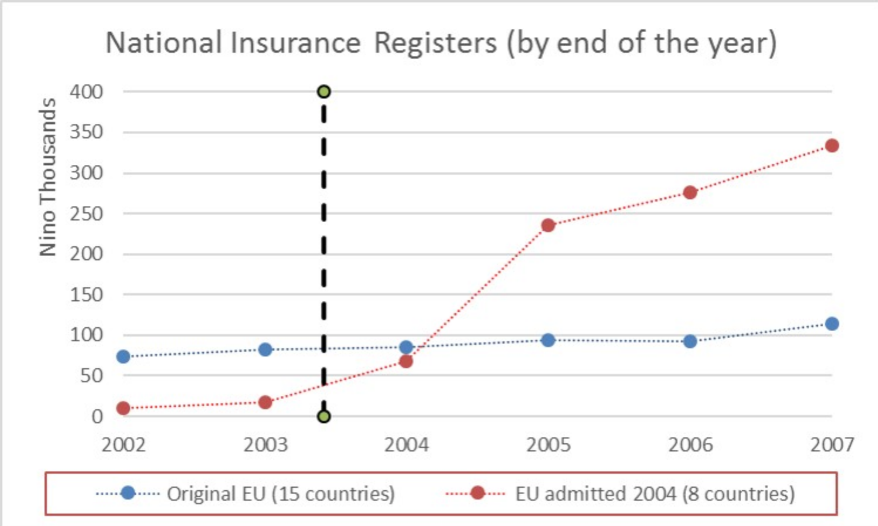


Figure 1.3: New registrations from EU8 and non-EU Eastern Europe

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.

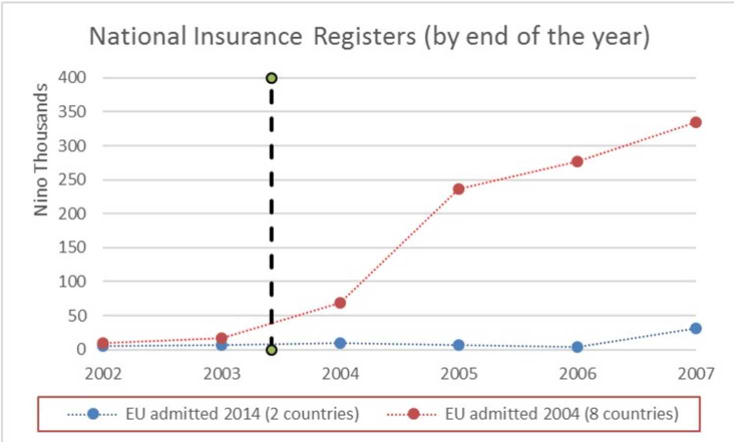


Figure 1.4: Quarterly new registrations of nationals from countries admitted in 2004

I rank the districts according to the shares of pre-existing workers share of workers from Eastern Europe. I then assign each district to a quartile.

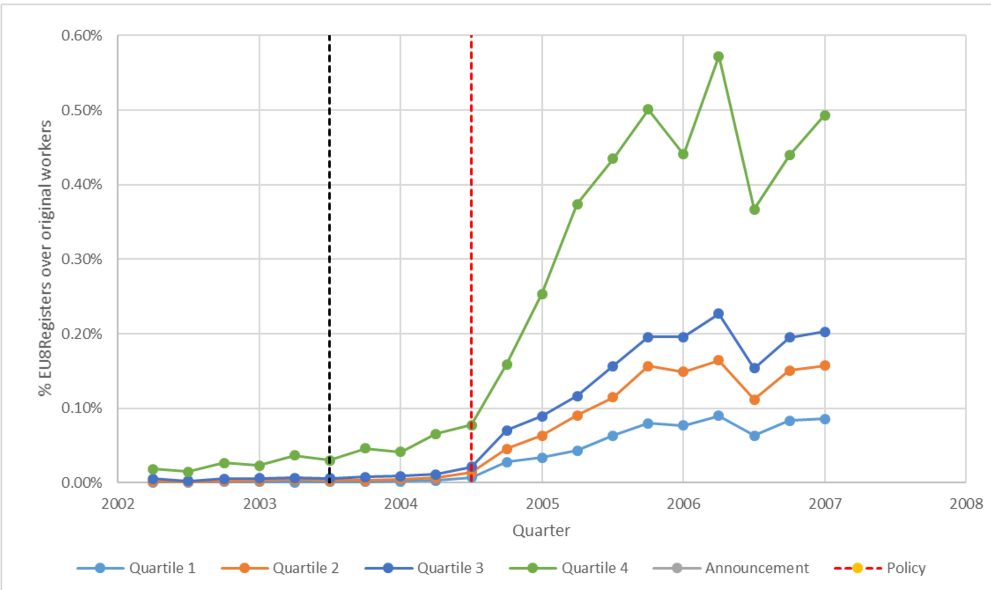


Figure 1.5: New firms incorporated per quarter

I rank the districts according to the shares of pre-existing workers share of workers from Eastern Europe. I then assign each district to a quartile.

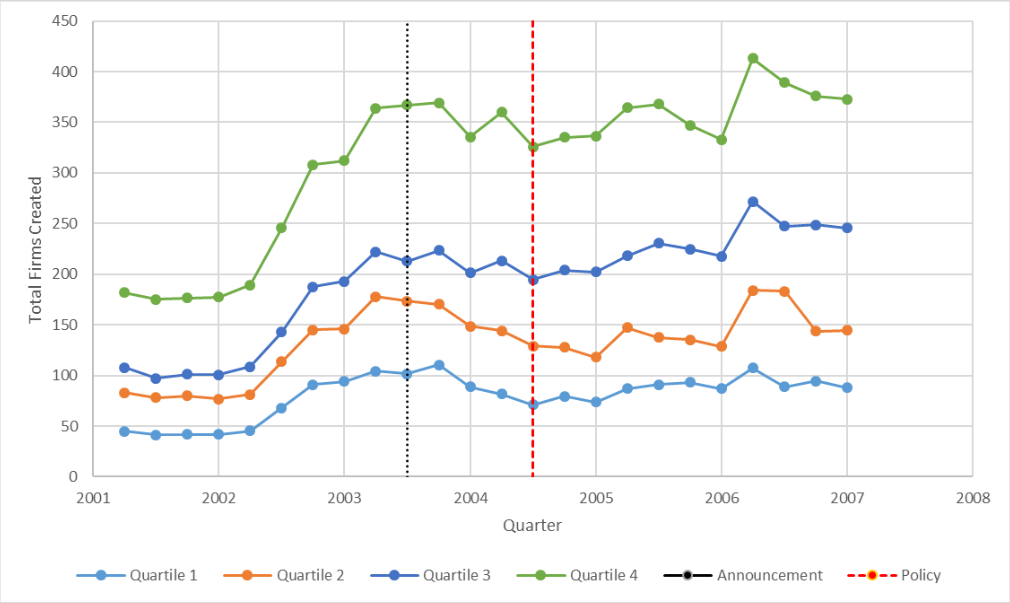
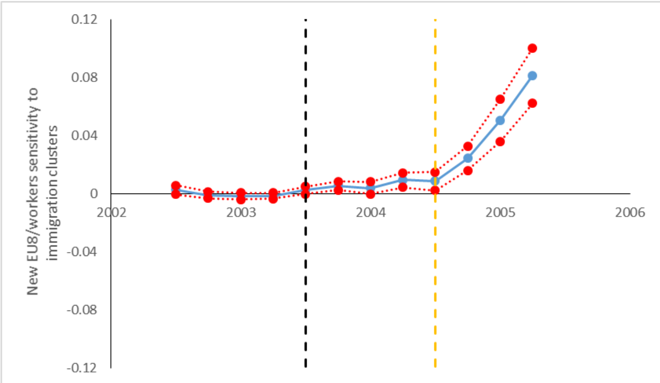


Figure 1.6: Estimation of regression coefficients pre and post Policy

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

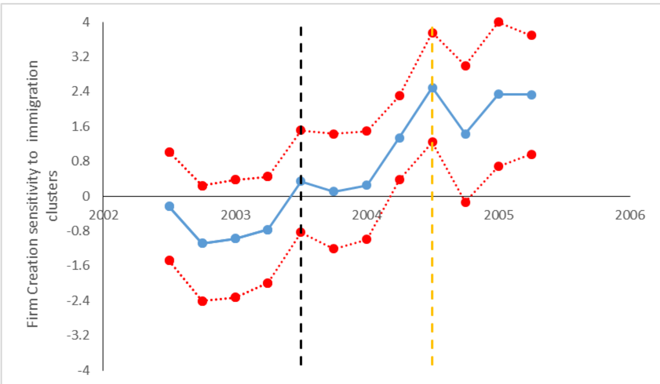


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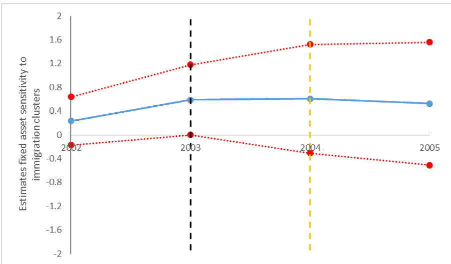
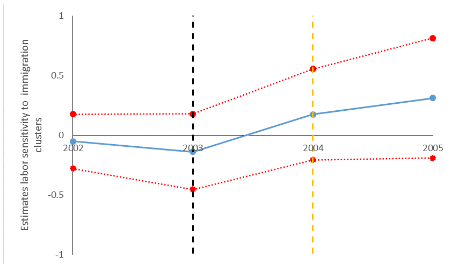
Figure 1.2: Regression estimates of pre-treatment trends within firms

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.

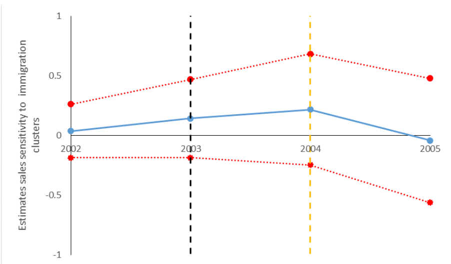
(a) Logarithm Fixed assets



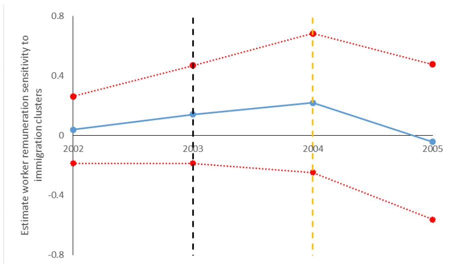
(b) Logarithm Employees



(c) Logarithm Sales



(d) Logarithm Average Remuneration



1.10 Tables

Table 1.1: 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 (1510.1)	1556.3 (2,471.7)
New registrations EU8	34.3 (98.5)	572.1 (829.8)
New registrations EU15	177.6 (330.8)	236.9 (480.3)
New registrations new to EU 2007	15.8 (48.7)	32.53 (144.8)
New registrations per ex ante workers (%)	0.93% (1.31%)	1.82% (2.13%)
EU8 new registrations per ex ante workers (%)	0.04% (0.09%)	0.73% (0.81%)
EU15 new registrations per ex ante workers (%)	0.21% (0.32%)	0.27% (0.47%)
New to EU 2007 per ex ante workers (%)	0.02% (0.04%)	0.04% (0.14%)
Activity Rate (%)	79.85% (5.47%)	78.19% (4.85%)
Workers	72,807 (46,892)	75,659 (49,757.1)
	Mean (St Dev)	Mean (St Dev)

Table 1.2: 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 1.3: 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 1.4: 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 (St Dev)	Mean (St Dev)	Mean (St Dev)	Mean (St Dev)	Mean (St Dev)	Mean (St Dev)	

Table 1.5: 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

	$ShareNewRegisteredWorkersEU8_{dt}$
$FractionEastern_d * PostAnnounce_t$	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

	$ShareNewRegisteredWorkersEU8_{dt}$
$HighFractionEast8_d * PostAnnounce_t$	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 1.6: 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 1.7: 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 1.8: 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 1.9: 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 1.10: 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 1.11: 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 1.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 1.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 1.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 1.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 1.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

Table 1.A.6: Firm Destruction

FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. All regression control for district and area*time fixed effects. The hazard rate is computed as the proportion of created firms that are destroyed the following year.

	Destruction/L	HazardRate(1 year)
FrEast*Ann	0.02	0.001
	(0.03)	(0.001)
FrEast*Imp	-0.08	0.001
	(0.06)	(0.001)
N	2233	2560
Adj R2	-0.02	0.03

Table 1.A.7: Reduced Form Regressions at the Travel to Work Area Level

FraEast refers to the proportion of workers from EU8 plus Romania and Bulgaria by 2001. At the extensive margin (firm creation) the regression controls for travel to work area fixed effects and for region*time fixed effects. At the intensive margin (fixed capital investment and wages) the regression controls for company fixed effects and region*time dummies.

Panel A: Regressions Annnoucement and Implementation

	Ln(Firms)	Ln(FixedAssets)	Ln(AverageWage)
FrEast*Ann	1.98	0.56***	0.11
	(1.23)	(0.21)	(0.16)
FrEast*Imp	1.7	0.01	0.18
	(1.33)	(0.22)	(0.15)
N	3360	351898	205129
Adj R2	0.93	0.93	0.97

Panel B: Reduced Form Annnoucement

	Ln(Firms)	Ln(FixedAssets)	Ln(AverageWage)
FrEast*Ann	5.55**	0.45**	0.1
	(2.4)	(0.2)	(0.001)
N	2130	263638	2560
Adj R2	0.94	0.94	0.03
Frequency	Quarterly	Yearly	Yearly
Aggregation	Travel To Work Areas (TTWA)		

2 The impact of the Mexican Drug War on trade

Jesus Gorrin, Jose Morales, and Bernardo Ricca

2.1 Introduction

From 2007 to 2011, the homicide rate in Mexico almost tripled, reaching 22.6 murders per 100 thousand people in 2011. This severe growth in violence has been causally linked to the Mexican Drug War (Dell, 2015).[26] In tandem, and despite a set of liberalizing economic reforms that started in the mid-nineties, Mexican economic performance has been rather disappointing.[27] The compounded average growth rate of GDP per capita in Mexico between 2006 and 2011 was approximately 0.6%, below its Latin American peers. Can violence sparked by the Mexican Drug War explain Mexico's missing opportunities? If so, what is the main channel through which violence generates negative economic effects? The answers to these questions provide valuable lessons for the role of anti-drug and anti-crime policies in developing economies, the role of violence in hampering economic opportunities in developing economies (especially in Latin America), and the limits of economic reform in areas suffering from chronic violence.

In this paper, we study whether this sharp increase in violence and economic underperformance are causally connected. Crime and violence can distort economic decisions and alter outcomes through different mechanisms. Firms incur in insurance and protection costs against perceived threats. Violence can affect workers' productivity through increased levels absenteeism and stress. Fighting crime draws scarce public resources away from alternative, productive uses by national and local governments. Violence affects the location decisions of firms and workers. Both anecdotal and academic evidence suggests large negative economic impacts from violence, especially in regions with high levels of crime, such as Latin America.[28] Yet there is little evidence about the operation of specific mechanisms through which crime and violence affect economic decisions.

A clear challenge to the existing literature is the endogeneity problem: crime is correlated with a wide range of local non-observable economic variables that affect firms' prospects. This limits the internal validity of cross-country or cross-state regressions. Another issue is measurement error due to underreporting, which can cause significant biases, since underreporting is correlated with regional characteristics.[29]

For several reasons, the Mexican Drug War is an interesting setting to assess the eco-

[26] In Dell (2015) part of that increase in violence comes from a change in drug-trafficking routes, which decreases the efficacy of the policy and generates external effects on areas that were less exposed to drug-trafficking related violence before.

[27] For a discussion on the constraints to productivity and growth in Mexico, see Levy (2018).

[28] See Soares and Naritomi (2010) for an overview.

[29] Soares (2004) shows that crime reporting, measured as the fraction of the total number of crimes that is actually reported, is correlated with institutional stability, police presence and perceived corruption.

nomic consequences of crime. Firstly, the increase in violence after the Drug War was large. Secondly, data from surveys indicate that firms were severely affected. For instance, according to the World Bank Enterprise Survey, the percentage of establishments paying for security increased from 41.5% in 2006 to 59% in 2010, and the percentage of establishments that experienced losses as a result of theft, robbery or vandalism doubled in the same period (from 15% to 30%). Thirdly, the war was mainly led by one political party - the the National Action Party (*Partido Acción Nacional, PAN*). The deployment of law enforcement tends to be correlated with trends in violence. But, as proposed by Dell (2015), the fact that the PAN led the war allows us to employ an empirical strategy that uses close municipal elections as a source of exogenous variation in the intensity of the fight against drugs.

Dell (2015) shows that homicides increase sharply after close elections of PAN mayors. Since in close elections a PAN win is as good as randomly assigned and has a clear effect on homicides, it is a candidate for an instrumental variable for the effect of homicides on the economy. The remaining condition to be satisfied is the exclusion restriction: the instrument should affect the outcome of interest only through its effect on homicides. This is unlikely to be the case. Firstly, new incumbents from the PAN might implement policies that affect the business environment. Secondly, the increase in violence was probably not restricted to homicides - the incidence of other crimes that hinder the business environment, such as robbery, kidnappings and extortion, could also have increased. Thirdly, since the Mexican president throughout the period studied is also from the PAN party, municipalities governed by PAN mayors may also receive more support from the federal government.[30] Finally, there may be spillovers to the control group. Therefore, a reduced-from form regression of an economic outcome on close PAN victories cannot be interpreted directly as the effect of the Drug War. It should be interpreted as the effect of a PAN election. An instrumental variable strategy that uses close PAN wins as an instrument for homicides would provide biased results.

Nevertheless, because of the features of Mexico's institutional setting, we still can learn about the unintended consequence of the Mexican Drug War on the economy. In the absence of the Drug War, PAN municipalities were likely to receive an economic benefit. The PAN party is deemed a more market-friendly party. The federal administration is likely to benefit PAN municipalities, since they belong to the same party. Spillovers to the control group attenuate the effects. All these biases underestimate the hypothesized negative effects of crime and violence on the economy. We exploit the combination of factors that should benefit municipalities after an election with tougher anti-drug policies because of the mayor's affiliation. We argue, therefore, that our estimates provide a lower bound to the economic impact of the Drug War.

[30] Azulai (2017) shows, in the context of Brazil, that partisan connections distorts the allocation of public goods.

We focus our analysis on trade, using both municipal and firm level data. Exports are an important part of the Mexican economy and they are a good measure of economic activity at the local level.[31] Moreover, exports are less likely to be driven by local demand, which could be an additional challenge to the validity of our estimates. We directly control for demand shocks by comparing exports of the same product to the same country of destination. We find evidence that the Mexican Drug War had a negative effect on trade. Export growth in municipalities governed by PAN decreased by 40%. We run a placebo test, using the previous local elections, and find that the effect on export growth is not statistically significant. We explore the heterogeneity of the effect across product characteristics and find that exports of more complex products are severely affected (65% decrease). The effect is not significant for less complex products.

To ease potential concerns that the negative effects are driven by the party in power, we exploit potential spillovers across municipalities. We show that municipalities that are randomly exposed to a neighbor electing a PAN mayor experience higher levels of violence. Moreover, we also show a significant decrease in export growth for these municipalities.

Many mechanisms can explain the evidence that violence affects exports growth. One is that violence harms the business environment. For example, it causes losses and extra expenses to firms and drives away skilled workers. Another mechanism is the following: cartels smuggle part of their products disguised in legal products. Usually they set up exporting firms of simple products, such as fish, vegetables and canned food.[32] Our evidence supports the first channel: exports only decrease for more complex products, and the effect is stronger for countries that are not part of the main smuggling routes. We also show displacement of workers from municipalities exposed to the Drug War.

Our results are consistent with the general findings in the literature on the negative economic effects of crime, violence and political conflicts. Pshisva and Suarez (2010) use firm-level data in Colombia to analyze the impact of kidnappings on corporate investment. They show that firms investment is negatively correlated with kidnappings that target firm owners and managers. Abadie and Gardeazabal (2003) explore the unilateral truce declared by ETA in 1998. They find that stocks of firms with a significant part of their business in the Basque Country showed a positive relative performance. Besley and Mueller (2012) find a negative relation between killings and house prices in Northern Ireland. Similarly, Frischtak and Mandel (2012) provide evidence that the pacification of favelas caused an increase in house prices in Rio de Janeiro, Brazil.

We also relate to the literature that covers the effects of the Mexican Drug War, being the closest Dell (2015), and the mechanism through which the effects might operate:

[31] The ratio exports/gdp was 30.37% in 2005 (World Bank national accounts data).

[32] Business Insider UK: Frozen sharks, fake carrots, and catapults: The bizarre ways smugglers like 'El Chapo' Guzmán get drugs across borders. Access: http://uk.businessinsider.com/el-chapo-guzman-strange-drug-smuggling-methods.

worker displacement. Consistent with the worker displacement channel, Contreras (2014) points out that immigration to US cities at the border increased during the war, despite the fact that immigration to the US as a whole decreased. Robles, Calderón and Magaloni (2015) document negative effects on labor participation and the proportion of unemployed in areas affected by increased violence during the Drug War. We also show evidence of emigration from affected areas, using a different source of exogeneous variation than other papers in the literature.

Our results suggest that policies that actively engage in violence against drug trafficking can have important negative unintended consequences for the economy. They seem to hamper exports of complex products at the local level through a displacement of workers.

2.2 Mexican political landscape and the Drug War

Throughout most of the twentieth century, Mexico experienced a *de facto* dictatorship with a single party domination. For 71 years, the Institutional Revolutionary Party (*Partido Revolucionario Institucional, PRI*) ruled the country. In the nineties, politicians from different parties started winning local elections, and in 2000 Mexico elected the first non-PRI president since 1929. Some analysts suggest that during the PRI rule there was a tacit agreement between the government and the drug traffickers (O'Neil (2009)). The agreement allowed cartels to operate as long as they complied with some rules. For example cartels could not cause major disruptions to civilian life. Importantly, violence was contained. When other parties started winning elections, the relationship was shaken, as cartels had to negotiate with the new incumbents from other parties. The election of Vicente Fox (PAN) as president in 2000 triggered some institutional changes. The competition between the PAN affiliated president and politicians from other parties in the parliament, states and municipalities forced a transfer of power from the presidency to other branches of government. Executive changes were limited though, since PAN was outnumbered in congress. It is only on July 2nd, 2006, when Felipe Calderón (PAN) was elected president, that changes started to intensify. Calderón governed from December 1st 2006, to 30th November 2012. As soon as he took office, he declared the war on drugs, sending the army to several provinces. The policy had tragic consequences. The arrest or assassination of a kingpin can cause a bloody dispute for power. Members from the same organization or from rival cartels can exploit the weakening of the leadership to try to gain control of the organization. Once in charge, new leaders have to assert their authority, in many cases through the use of violence. Cartels also retaliated against the state, killing politicians, police officers, and journalists.

During Calder on's administration, the number of homicides increased by 160%, from 10,452 in 2006 to 27,213 in 2011 (Figure 2.1). Total homicides between 2006 and 2011 - as well as the absolute increase from the total between 2001 and 2005 - are concentrated in the northern regions of the country , closer to the US border (Figure 2.2). These are the regions where the main cartels operate the smuggling of drugs to the US. In reaction to the crackdown, there is evidence that cartels begun to diversify their activities into other crimes, such as extortion, human trafficking, oil theft, kidnapping and robbery.

The main strategy targeted cartel leaders. We gathered the information of all confirmed deaths and arrests of high ranked members of 9 different Mexican cartels. During the Calderón presidency, we confirm 13 killings and 54 arrests performed by governmental authorities over 49 Mexican municipalities. These operations were mainly organized at the federal level, but coordination with municipal police was important.

Municipal presidents, the Mexican equivalent to mayors, are elected by popular vote. All municipalities and states in Mexico control a police force. The municipality has the power to remove or appoint the municipal police chief. According to article 115 of Mexican Constitution, the municipal police has the responsibility to provide security and prevent crime. The important role of mayor in the implementation of the Drug War can also be seen in practice. According to figures from associations that gather majors, from 2006 until 2014 organized crime killed 63 current or former majors.[33] Furthermore, municipal presidents have denounced extorsion from cartels.[34] Hence, municipal elections are an important source of variation in the way the Drug War policy was implemented at the local level.

Finally, at the time of the war on drugs Mexico already had competitive election. Among major parties, PAN is more economically liberal and business oriented than their national opponents. As evidence of this, PAN was elected on a economic platform based on globalization and an increase in foreign investment (Krauze (2006)). Its main rival in the 2006 elections, the Party of the Democratic Revolution (*Partido de la Revolución Democrática, PRD*), is suspicious of free markets and globalization. The other rival, the PRI, is more diverse; however it has an important historical baggage. When in power PRI was in charge of the nationalization of many industries in the 80's.

2.3 Data and descriptive statistics

We collect data on local elections results from the Electoral Tribunals of each state. Local elections are held every three years, and usually elections at different states happen in different times. We focus on municipalities with elections in 2007 and 2008 because the terms of mayors elected in those years started and finished during Calderón's administration. Monthly data on homicides are from the National Institute of Geography and Statistics (*Instituto Nacional de Estadística y Geografía, INEGI*), and are available since 1990. Data on other types of crimes tend to be noisier due to underreporting. The issue

[33] Webpage:http://www.24-horas.mx/impunes-63-asesinatos-de-alcaldes-en-mexico/

[34] Webpage:http://archivo.eluniversal.com.mx/nacion/165947.html

of underreporting is severe in developing countries, where both the police and victims do not report all crimes. The most reliable source of crime data at the municipality level is The National Public Security System (*Sistema Nacional de Seguridad Pública, SNSP*). The system started to publish the data in 2011. Data on municipality characteristics are from the National System of Municipal Information (*Sistema Nacional de Información Municipal, SNIM*). Data on exports are from the Atlas of Economic Complexity.[35]

Table 2.1 reports summary statistics of municipalities that held elections in 2007 and 2008. In terms of population, municipalities are small. They have, on average, 35 thousand inhabitants compared to 100 thousand for the average county in the US. Furthermore, by 2006 Mexico was already a violent country compared to the US. The American rate of 6 homicides per 100,000 pales with respect to 11.7 in Mexico. However, compared to some Latin American countries, such as Brazil (26), Colombia (37), Venezuela (49), and El Salvador (58), Mexico's homicide rate was relatively small in 2006 (Berthet and Lopez (2011)). Although PAN was already an important party, only 0.27 of municipalities had an incumbent PAN mayor. Municipalities that elected PAN mayors (treatment group) are richer, less violent and have a higher share of the population aged between 16 and 29, in comparison to municipalities that did not elect PAN mayors (control group). However, once the sample is limited to municipalities where PAN won or lost by a small margin, the baseline characteristics are not statistically different in treatment and control. This result provides evidence the close PAN victories are as good as randomly assigned. Moreover, the loss of power caused by the restriction of the sample does not drive the results. For all significantly different variables in the unrestricted sample, we see smaller differences when we restrict to the 5% spread.

We also report the results for the neighbor treatment instrument. There are differences in literacy rates and in age. Municipalities neighboring a close PAN win tend to be older and have a smaller literacy rates. Nonetheless, there are no differences in years of schooling, or in economic performance. Moreover, municipalities in this sample are similar to the average Mexican municipality, which is important for interpreting the results as the average effect.

Panel A of figure 2.3 shows the geographical distribution of all municipalities in which elections took place in 2007 and 2008, while Panel B shows the geographical distribution of close elections in the same years. In the unconditional sample we can see that, even when PAN wins are not clustered, the losses are. We also see that PAN loss the majority of the municipal elections. However, when we restrict to the 5% spread we see that the distribution of losses and wins are regionally dispersed. This is important for our identification for two reasons. First, this undermines the possibility that regional shocks,

[35]Webpage: http://complejidad.datos.gob.mx. The Atlas was developed at Harvard's Center for International Development. The original data comes from the Tax Administration Service (*Servicio de Administración Tributaria, SAT)*, Mexican's customs authority.

and not the treatment, drive our results. Second, it diminishes concerns of spillovers in control municipalities when restricting to the close elections sample.

2.4 Effect on violence

Usually governments allocate their enforcement arms to regions where violence is increasing. Therefore a regressions of violence on some measurement of law enforcement provides biased results. To address this challenge, We identify the effect of violence in two ways. We follow Dell (2015) and identify the direct effect on violence of electing a PAN mayor in a close election. We then use a new identification strategy. We exploit spillovers of these elections on neighbor municipalities to show that being close to a neighbor electing a PAN mayor is enough to cause an increase in violence.

There are two ways in which our modification helps with the identification. First, the treatment using neighbors provides more power. Differently from the relatively small number of municipalities that experience a close election themselves, there are many municipalities that have neighbors experiencing a close election. Moreover, having a neighbor that elected a PAN major in a close election is as good as randomly assigned. Second, our identification diminishes concerns that the effects are driven by particular policies that mayors adopt in a treated municipality, rather than the spillovers in violence.

As a baseline, we provide results using the same treatment of Dell (2015). Dell (2015) uses close elections as a source of exogenous variation in the intensity of the war on drugs. We use the 2007 and 2008 elections in Mexico. The administration of mayors elected in those years started at the beginning of the war, and finished around its peak, in 2011. One party, PAN, pushed for stronger actions on the Mexican drug cartels. As we show in Table 2.1, and consistent with the evidence found by Dell (2015), for close elections municipalities are similar among observables. This supports the assumption that close PAN wins are as good as randomly assigned. Also, PAN wins and losses in close elections are regionally dispersed, which diminishes concerns of spillovers between the treatment and control group.

Following Dell (2015), the direct effect specification has the form

$$y_m = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \gamma X_m + \epsilon_m \tag{1}$$

where m denotes municipalities, $PANwin_m$ is dummy that takes value 1 when PAN wins, X_m is a vector of municipality controls, and $f(Margin_m, PANwin_m)$ is a polynomial on the vote margin and dummy of PAN victory. We restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%.

Panel A of Table 2.2 shows the results of estimation of equation 2 when the outcome variable is the annual average of homicides over the new incumbent's term. Under the standard OLS, the signs are positive, but not significant. In this setting a weighted regression is more appropriate. It is likely that in smaller municipalities crime is under-reported. The weighted regression addresses the problem of endogeneous sampling. Ideally, we would weight for the inverse of the probability of being sampled (Solon et al., 2013). We weight for population. When the regressions are weighted by population size as of 2005, a PAN victory causes a increase between 25 and 41 homicides per 100,000 population. As suggested by Solon et al. (2013), we always report robust standard errors when weighting.

Panel B of Table 2.2 shows that a PAN victory is not associated with any pre-trend increase in homicides: municipalities where PAN won by a close margin do not experience higher homicides rates before the election. Panel C analyses the impact on the absolute change in homicides: before and after the elections. A PAN win is associated with an increase of 37 in the homicide rate. In Panel D, we use the 2004 and 2005 elections to run a placebo test. Most of the administration of mayors elected in those elections occurred before the war. Close PAN wins are not associated with higher homicides over the new incumbent's term. Therefore, a PAN victory in itself did not cause higher violence at the municipality level. It seems that the main driver of violence was the combination of PAN victory with the implementation of the war on drugs.

Table 2.A2 in the Appendix reports the same regressions when we restrict the sample to municipalities where PAN won or lost by a margin smaller than 3%. The results are consistent. Coefficients increase slightly and remain significant at 5%. Results are also similar when we increase the degree of the RD polynomial (Table 2.A3 in the Appendix).

A natural question is whether the incidence of other types of crime also increased. It could be the case that homicides were concentrated in the war between rival cartels and the war between state and cartels. In this scenario, other crimes, such as robbery, kidnapping, and extortion, could remain unchanged. There are some limitations in documenting the effects on other crimes. Data is noisier due to underreporting. Furthermore, the most reliable source started publishing crime statistics per municipality only in 2011. Therefore, differently from homicides where we could test the impact over the whole term, we can only test the impact on the level observed in 2011, and we cannot run a placebo test with previous elections. Table 2.A1 in the appendix reports results for six different types of crime. In general, crime increases, but the effects are not always statistically significant. Effects on extortion and robbery are statistically significant.

We now focus on our alternative identification. We construct a treatment that exploits the interaction between having a neighbor experiencing a close election and that neighbor electing a PAN major. We provide two versions of this treatment: having at least on treated neighbor or population of the treated neighbors. The second version uses the fact that larger municipalities are more likely to generate spillover effects. The main results of this paper are similar when using any of the two versions of the treatment.

The neighbor specification has the form (notice the change in subscripts):

$$y_m = \alpha + \beta PANwin_n + \delta f(Margin_n, PANwin_n) + \gamma X_m + \epsilon_m \tag{2}$$

where m denotes municipalities and n denotes a neighbor municipality, $PANwin_n$ is dummy that takes value 1 when at least one neighbor elects PAN in a close election and zero if they elect other parties in close elections, X_m is a vector of municipality controls, and $f(Margin_n, PANwin_n)$ is a polynomial on the vote margin and dummy of PAN victory in neighbor elections. We restrict the sample to municipalities that had at least one neighbor where PAN won or lost by a margin smaller than 5%.

One way to provide intuition for our treatment is by showing the equivalent RDD graph. The discontinuity is the outcome in the average close election in the neighbor. Municipalities can have multiple neighbors with close elections. In this paper we restrict the sample to municipalities that do not have neighbors with mixed treatment. i.e., we restrict to municipalities that either had all their neighbors with close elections electing PAN, or all of them electing another party.[36]

Graph 2.4 shows the cumulative homicides for three years after the relevant municipal elections. On the "x" axis we show the average difference in the elections of the neighbors. Because we lose information when we take the average of the neighbors, it is hard to find significance in this graph. Nonetheless, we find some evidence of spillovers from neighboring municipalities. A municipality with neighbors that elect a PAN mayor in a close election experience more homicides.

Table 2.3 shows the effect of a close PAN win in a neighbor on a municipality. We show significant spillover effects in terms of homicides. From the OLS specification we find a significant increase in homicides both when treatment is a dummy or when it is the standardized population of PAN close wins in neighbor municipalities. The dummy instrument shows that having at least one neighbor that elected a PAN mayor in a close election translates into 11.17 more homicides (per 100 inhabitants) over a period of three years. Moreover, having a one standard deviation increase in the population of neighbors electing a PAN mayor translates into a significant 3.7 (per 100 inhabitants) more killings in our municipality. The results are robust when we use the standard OLS regression. Moreover, for the instrument that accounts for heterogeneous treatment based on the population of the neighbors, the results are significant both in the OLS and in the weighted regression. When we include polynomial controls for the neighbor margin the effects are not significant. However, for the weighted regression coefficients do not

[36]Results do not depend on this assumption, but this allows us to control for a polynomial using election margins on the average neighbor. The results are robust if we estimate the regression on a PAN win neighbor treatment and no sample restrictions

change.

2.5 Economic consequences

In this section we combine the identification based on close municipal elections with disaggregated municipal and firm level data. Our focus on exports combined with disaggregated data allows us to concentrate on supply effects. This is different from the rest of the literature studying the effects of violence in the economy. Violence could potentially affect the economy by diminishing the likelihood of individuals to consume or to demand certain type of goods. Our effect is driven by a drop in the production of goods that are not affected by local demand shocks.

The municipality-product-destination data allows us to control for demand specialization. Regressions on firm or economic outcomes have the same form as regressions on homicides (equation 2). When the data is disaggregated, we will also include a set of dummies to control for foreign demand shocks or for firm shocks.

Even though the dummy close PAN win is as good as randomly assigned, to draw conclusions about the actual effects of the Drug War we need to show that the underperformance was not triggered by the election of PAN itself, but was triggered by propensity to engage in the war on drugs. To approach this question, we provide placebo estimates of the same specification for the 2004-2005 elections. We show that in previous PAN wins, there were no negative economic effects.

Moreover, we also use the identification based on neighbor electing PAN majors in close elections and show exports decrease. We also show that the negative economic effects exists even when the major is not from PAN, but a neighbor is. Making it less likely for the effects to be caused by other policies that PAN majors implement at the relevant municipality.

2.5.1 International trade

For several reasons, the main economic variable of interest in this paper is exports. First, exports are a good measure of economic activity at the local level and they are important determinants of local level growth. Second, the effect of the close election can drive both supply and demand. For example, if violence increases in a particular municipality it could drive workers out of the municipality. If we study local production instead of exports, then a negative shock could be driven by both a decrease in demand of those products by local workers and from firms experiencing a decrease in labor supply. If we concentrate on exports, then we can keep demand fixed (or at least exogenous to the local shock). Third, export data is disaggregated at the municipality-product-destination levels. This allows us to control for foreign demand shocks.

In this section, we test whether the Drug War affected exports. For each municipality m, we observe the annual amount (in Pesos) of product p exported to country c. There is one caveat about the data. When a firm has a single plant or all their plants are in the same municipality, the exports reflect directly the municipality. When firms have multiple plants in different municipalities within the same state, then an approximation is made based on the workforce of each plant. Regressions take the form:

$$y_{mcb} = \alpha + \beta PANwin_m + \delta f(Margin_m, PANwin_m) + \alpha_{cb} + \epsilon_{mcb} \tag{3}$$

where y_{mcb} is the growth in exports of product p to country c in municipality m. More specifically, y_{mcb} is the log of the amount exported in the third year of the new administration, divided by the amount exported in the third year of the previous administration, when elections took place. α_{cb} is a set of country of destination-product dummies, which allows us to control for foreign demand shocks, similar to the strategy implemented by Paravisini et al. (2014).

Table 2.1 provides descriptive statistics and tests if municipalities where PAN won differ from municipalities where PAN lost. Municipalities won by PAN tend to be more open. The mean of total exports is higher. These differences are not statistically significant. Differences remain not significant after reducing the sample to close elections. Moreover, if we use the instrument that considers neighbor exposure, treated and control municipalities are similar in their levels of trade *ex-ante*.

In table 2.4 we report the regressions of export growth on close PAN wins using the same weighting by population.[37] With country of destination dummies, we show that a close PAN win caused a decrease of 42% in export growth. When we control for destination-product dummies, export growth decrease by 40%. These controls also alleviate concerns that differential changes in the terms of trade of certain products drive the result. Therefore, after the implementation of the Drug War, municipalities performed worse in terms of trade even when the more open party was elected.

To test whether the negative effect on trade is due to the PAN election itself and not their implementation of the Drug War, we run a placebo regression on the previous municipal elections. Data is available from 2004, so we take export growth until 2006, the first year of the Drug War.[38] Table 2.6 reports the results from the elections after and before the Drug War. Before the Drug War, the close PAN wins had no effects on exports growth. After the Drug War, the effect on export growth is significantly negative. This favors the hypothesis that the Drug War, and not the PAN election in itself, had negative effects on trade.

[37] Miss-reporting, or lack of information, for firms in smaller municipalities is still a concern in this setting. Therefore, we decide to weight by population. Results are robust in the standard OLS regression

[38] The Drug War started in December 2006

Still the effects could be driven by particular policies implemented by PAN mayors in the treated municipality. To alleviate this concern we use a different identification. We define treatment as having a neighbor municipality that elected a PAN mayor in a close election. This is not a RDD in itself. However, we can still perform some of the standard RDD tests. For example, we can show exports growth on a running variable that accounts for the neighbors average voting shares in close elections. The main result of the paper can be observed graphically in figure 2.5. As the difference among vote shares approach zero we can see a discontinuous and significant negative effect on export growth.

As we can see in Table 2.7, a PAN mayor elected in a neighbor municipality has a significant negative effect on export growth when controlling only for destination fixed effects. When controlling for destination-product fixed effects, we find an insignificant 5% decrease in export growth if the municipality has at least one neighbor that elected a PAN mayor in a close election.

Finally, we breakdown the results according to the degree of complexity in different products. We use the Product Complexity Index (PCI) from the Atlas of Economic Complexity developed by Hausmann et al (2011) to separate products. This measure uses trade data to determine the complexity of a product according to two characteristics: ubiquity and the average diversity of its exporters. In theory, a more complex product is produced by countries that export many products, but it is also produced by few countries (Hausmann et al, 2011). Complexity is relevant in our setting because it predicts future GDP growth. More complex economies tend to grow more (Hausmann et al, 2011). If the Drug War affected more complex products, then the long term effects would be more pernicious. Second, since complex products are exported by few countries, they are more likely to be traced. In consequence, they are not the most desirable legal products to hide illegal trade. An effect on complex products is less likely to be related to illegal trade, but to external effects of the Drug War on the economy.

In table 2.8 we report a monotonic pattern in export growth. We divide products in four quartiles depending on how they rank in terms of the economic complexity index. For low complexity the effects on export growth are indistinguishable from zero, or positive if we control for product-destination dummies. The higher the complexity the more negative and significant the effects over export's growth. This suggests that in the treated municipalities the negative impacts are concentrated in more complex industries.

Overall the results suggest that the election of PAN had significant negative effect on trade at the municipality level. By running placebo regression on previous elections, we established that this effect is not related to the election of PAN itself, but on the election of PAN at the time of the Drug War. This suggests that the main driver of the negative performance was the implementation of the Drug War. Furthermore, we find indicative evidence that the effects are related to unintended consequences of the policy and not to a drop in export of illegal goods.

2.5.2 Change in firms' exports

Whether we are looking directly at the sample of municipalities that experienced close PAN elections or at their neighboring municipalities, the nature of this electoral discontinuity allows us to study the economic effects of increased violence at a microeconomic level. Leveraging from a panel of formal plants in Mexico[39], we now evaluate whether being exposed to a marginal PAN victory in a firm's municipality or neighboring municipality leads to a change in its export performance. This helps us assess whether local exposition to the war on drugs negatively affect economic activities of exporting firms.

We focus on firms that, at baseline, exported from a single plant[40], and evaluate the change in their exports. Specifically, for both samples of municipalities with close PAN elections and municipalities neighboring close PAN elections, we estimate the following equation:

$$log\left(\left[\frac{X_{fm}^{t'}}{X_{fm}^{t}}\right]^{(t'-t)}\right) = \beta_0 + \beta_1 PANwin_m + \delta f(Margin_m, PANwin_m) + \psi_i + \epsilon_{fm} \quad (4)$$

Where X_{fm}^{t} stands for the exports of firm f located in municipality m in baseline year t. The dependent variable captures the logarithm of the average yearly growth factor in total exports at the firm level between years t and t'. β_1 captures the percent difference in the average yearly growth factor of the exports of firms marginally exposed to a PAN mayor in their municipality or in a neighboring municipality. We control for industry fixed-effects and cluster standard errors at the municipality level.

Table 2.9 shows the results to these specifications. Panel A captures the effect of locating in a municipality under a close PAN victory on a firm's exports between 2007 and 2010. Panel B shows this effect between 2004 and 2006, providing a pre-trend estimate of the regressions in Panel A. Panel C provides a placebo specification using electoral results for local governments inaugurated in 2004 on a firm's exports between 2004 and 2006. Panel D through F show analogous estimates but for firms but for the sample of municipalities neighboring close PAN elections. Each column provides estimates

[39] This anonymous panel of formal plants in Mexico between 2004 and 2014 is built with administrative data provided by Mexican Social Security and Tax Authorities. It constituted part of the microdata used in the Mexican Atlas of Economic Complexity. We worked with this data locally at Harvard's Center for International Development, who partnered with the Mexican government in developing this data visualization tool. Information about the Mexican Atlas of Economic Complexity is available at http://complejidad.datos.gob.mx.

[40] We work with single-plant firms so as to ensure the adequate location of origin in exports.

for different samples of firms, according to either their size or the economic complexity of their industries. Column 1 shows estimates for all plants. Columns 2 and 3 show estimates for plants below and above the median firm size in the sample. Columns 4 and 5 show estimates for plants in industries representing the bottom and top quartiles of the complexity distribution[41].

Estimates in table 2.9 show important negative effects. Panel A shows that for the sample of all single plant firms, the growth factor of firms marginally exposed to a PAN mayor is about 12% lower. This effect seems to concentrate on firms below the median size in the sample, for which the effect is of about 20%. We observe no effect for the sample of firms above the median firm size. The effect seems to be greater for high complexity firms: While being marginally exposed to PAN mayors in the lowest complexity quartile associates with export growth ratios 16% lower, this effect is over 40% lower in the high complexity quartile. While Panel B shows pre-trend coefficients that are statistically significant for all firms and firms below the median size, these have the opposite sign. Placebo estimates in Panel C only show statistical significance at the 90% of confidence for the sample of low complexity firms. Panel D shows similar results for the sample of municipalities neighboring close PAN elections. Export growth ratios are about 17% lower for firms marginally neighboring a close PAN mayor in the full sample of firms, in the sample of firms below the median size and in the sample of firms in low complexity sectors, but we do not find an effect for firms in high complexity sectors. Now again, Panel E shows only one statistically significant pre-trend coefficient for small firms exposed to neighboring PAN victories, with the opposite sign. All coefficients in the Placebo estimates in Panel F are statistically insignificant.

2.5.3 Displacement of Firms' Operations

One important channel through which the Drug War can affect firms is by displacing their operations away from locations experiencing increased levels of violence. We can evaluate whether this was the case by assessing how the share of a firm's wagebill changes as a consequence of a PAN victory in its municipality or in a neighboring municipality. In particular, we evaluate whether the share of a firm's wagebill in municipalities with a close PAN election had lower growth if these were treated municipalities where PAN obtained a victory, or municipalities neighboring such treatment.

To better test for this hypothesis, we work with a sample of firms that:

- Operated inside and outside of our sample of municipalities with close PAN elections at baseline.

[41] Plants are segmented by economic complexity levels according to scores for each of the 256 industry classifications available the Mexican Atlas of Economic Complexity.

- Operated either in treatment or in control municipalities (that is, we exclude firms that operated in both treatment and control municipalities).

For this sample of firms, we run the following regression:

$$log\left(\left[\frac{W_{fm}^{t'}/W_{f}^{t'}}{W_{fm}^{t}/W_{f}^{t}}\right]^{(t'-t)}\right) = \beta_0 + \beta_1 PANwin_m + \delta f(Margin_m, PANwin_m) + \psi_i + \epsilon_{fm} \quad (5)$$

Where W_{fm}^{t} is the wagebill of a firm f in close-election municipality m at time t. The dependent variable would express the logarithm of the yearly average growth factor in the share of employment of a firm in a given municipality in our sample of close elections. β_1, our coefficient of interest, measures the effect of being on (or neighboring) a close PAN victory on the growth of the share of firms' operations in close election municipalities. We control for industry fixed effects and size of the municipality. We cluster standard errors at the municipality level.

Table 2.10 shows the results for these regressions, and is structured as table 2.9. Panels A and C show results between 2007 and 2010 for the sample of municipalities with close PAN elections or neighboring close PAN elections respectively, while Panels B and D provide the respective placebo specifications for the same sample of municipalities but evaluating changes in firms' wagebill between 2004 and 2007. Similarly, we provide estimates for different size/complexity segments of the sample of firms in the different columns of each panel.

While the results in table 2.10 do not show statistically significant estimates for the full set of firms, segmenting by complexity levels allows us to observe negative and statistically significant effects on the sample of firms in high complexity sectors of the economy. Panel A shows that with a 90% level of confidence, the average yearly growth factor in the share of a firm's wagebill in a municipality with a close election is 6.5% lower for municipalities with PAN victory. The corresponding pre-treatment and placebo coefficients shows positive and non-statistically significant results. The analysis of the sample of municipalities neighboring PAN elections in Panel D shows an effect of 19% lower growth ratio in the wagebill share of high-complexity firms in municipalities neighboring PAN mayors, but the respective pre-treatment result also shows a similar negative and statistically significant coefficient. These results lend some additional support to the findings described above, outlining how the economic effects of the Mexican Drug War concentrated in relatively advanced sectors of the Mexican economy.

2.6 Mechanisms

In this section we test whether the main effects are driven by: (1) spillovers from illegal markets to legal markets, (2) labor displacement from the affected regions.

A reduction in exports in the legal markets can be capturing a drop in illegal markets. One of the methods employed by cartels to smuggle drugs is hiding them in legal exports. Given the substantial size of Mexican drug exports, it could be the case that the reduction in exports is driven by illegal products, and not a debilitated business environment.[42]

Testing this hypothesis is challenging, because of the lack of information about the illegal market, thus the evidence that we provide is only suggestive. We will use the variation in exports across different destinations to test whether the effects are concentrated in destinations that are likely to be trafficking hubs.

What are the patterns across trading partners? In tables 2.11 and 2.A4 (in the appendix) we separate the effect across important trading partners of Mexico. We divide countries in four groups: Europe, China, United States and a group formed by three countries: Colombia, Peru and Bolivia. These regions are not only important trading partners in legal products, but, with the exception of China, play important roles in the drug business. The US and Europe are the main consumers, while Colombia, Peru, and Bolivia are the main exporters of coca and cocaine.[43]

First, we look at countries that are main export destinations for drug trafficking from Mexico. According to the World Drug Report (2010) Mexico is a main producer of opioids. It also plays an important role in the distribution of cocaine. In 2004, Mexico exported 90% of the cocaine consumed in the US (O'Neil (2009)). Therefore, if we are only capturing an effect related to drug trafficking then only destinations like US or Europe should be affected. Indeed, we find a significant drop for the US. But no effect for Europe. On the other hand, we find a significant and larger decrease in export growth to China, which is unlikely to be related to drug trafficking itself.

We then revisit the evidence on complexity. We argue that trades on high complexity products are easier to trace and, therefore, less likely to be useful as covers for illegal trade. We find significantly larger negative effects on municipality level export growth for high complexity products. This effect persists when we use firm level data. Furthermore, firms that produce more complex products face larger and significant decreases for wage bill growth. The larger results on high complexity products are less likely to support the hypothesis that decreases in illegal exports, instead of worse economic conditions, explain

[42] Estimates of total Mexican drug exports to the US vary substantially, from US$ 6.6 billion to as much as US$ 39 billion (Kilmer et al. (2010)). In 2008, Mexico legal exports to the US amounted to US$ 230 billion.

[43] In 2006, 84% of the Mexican legal exports went to the US; 4.5% to Europe, 0.7% to China, and 1,3% to Colombia, Peru and Bolivia. Out of total imports, 49.04% comes from the US, 12% from Europe, and 9.7% from China, and 0.43% from Colombia, Peru and Bolivia.

our findings.

We also test whether our effects are driven by worker scarcity at the local level. There is plenty of research and evidence on how violence makes it more likely for people to emigrate from a location. [44]

To test this hypothesis we collect census data about worker migration at the municipality level inside Mexico. At 2009, workers are asked whether they were living at a different municipality 5 years before and they specify their municipality of origin. We identify workers that left a particular municipality, but now leave in a new one as migrants.

Using this data, we find some evidence of workers displacement. Although the significance is not robust to weighting, we can see from table 2.12 that the coefficient is always positive and economically significant.[45] Moreover, when we use the instrument that accounts for the size of the treated neighbor, the coefficient is always positive and significant. In the baseline OLS regression, a one standard deviation shock to the population of neighboring municipalities that elected a PAN major causes a 28% increase in the number of workers that leave the municipality of interest. The direct effect of a PAN major in the municipality translates to a non statistically significant effect equivalent to a 23% increase in migration from that municipality of origin.

Notice that our data only accounts for workers that remain inside Mexico. According to Encuesta Nacional de Dinámica Demográfica 1.64 million Mexicans left Mexico for the US in the period from 2005-2009. Our results are likely to underestimate the impact of the Drug War because a large share of Mexican workers migrate abroad. Moreover, emigration might not be the only in which violence affects the workforce. It could, for example, increase absenteeism at the firm level or make it harder for workers to take after hour jobs. These are interesting questions, but we cannot explore these channels with the current data.

Banking results also agree with our channel. At face value, it would be hard to obtain results in credit expansion after a close election result. There is evidence that government owned banks might increase lending in municipalities controlled by the Central government (Carvalho, 2014) or politicians might increase credit in areas where elections would be more competitive (Cole, 2009). Therefore, our effect is likely a lower bound of the real effect of violence on the banking sector. In our estimations we find no significant effect on credit, but we do find significant decreases in bank savings at local branches, the number of bank accounts opened, and a significant decrease in new branches in mu-

[44] For general evidence in how violence affects migration decisions see Adhikari (2013). For evidence specific to Latin America see Clemens (2017), Arceo-Gómez (2013), Cantor (2014), Chamarbagwala & Morán (2011), Engel & Ibañez (2007), Ibañez & Vélez (2008), and Martínez (2014).

[45] The regressions are log-level regressions.i.e., a variation of 1 unit in the independent variable translates into 100*β% change in the dependent variable.

nicipalities where PAN won a close election.[46] We interpret the disruption in everyday bank operations as a consequence of worker displacement.

2.7 Conclusion

The Mexican Drug War has drawn the attention of the population, the media and the academia because of the scale of its consequences. We confirm the results in Dell (2015), who provides evidence that homicides increase disproportionately in municialities where the rollout of the war effort was supported by PAN mayors. We provide evidence that other crimes increased as well, albeit our estimates are only suggestive. These overall increases in crimes suggest other potential unintended consequences of the Drug War.

We take a step further and try to assess how the Drug War affected the real economy. We argue that a direct, reduced-form approach would yield lower-bound estimates of the negative economic effects of increased violence, and we provide placebo estimates on previous elections to test the direct economic effects of narrow PAN victories outside the context of the Mexican Drug War.

We document a negative change in trade patterns, with export growth decreasing significantly after a close PAN win. The declines do not depend on whether the destination is a main international drug trade route through Mexico. If anything, the effects are stronger for countries that are not part of the main drug trade routes, like China. Additionally, we find that the effects are stronger for more complex products. We interpret the results as evidence of external effects from the Drug War, as these effects are not observed outside the context of the Drug War.

Observing firm-level microdata, we find that firms locating in a municipality that was exposed to a PAN mayor or that neighbored a PAN mayor faced lower export growth rates, and that these effects may have been greater for smaller firms and for high complexity sectors of the economy. We also find evidence of workforce displacement of high-complexity firms away from municipalities with a PAN mayor or neighboring a PAN mayor.

The main results suggest that the Drug War did not only cost many lives, but also negatively changed Mexico's economy.

2.8 References

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2.9 Figures

Figure 2.1: Annual homicides

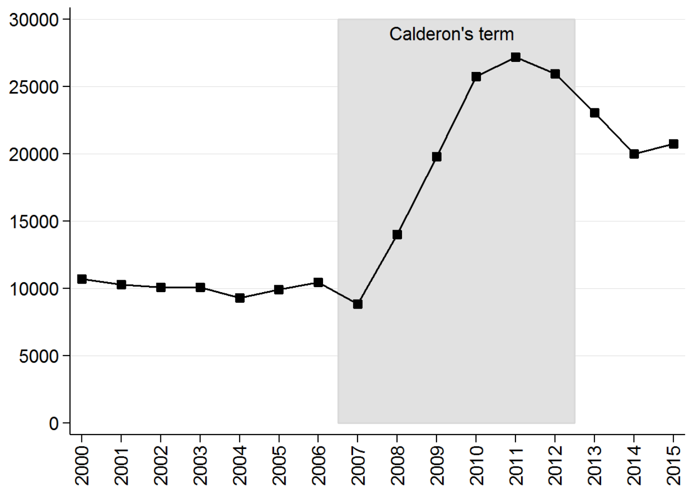
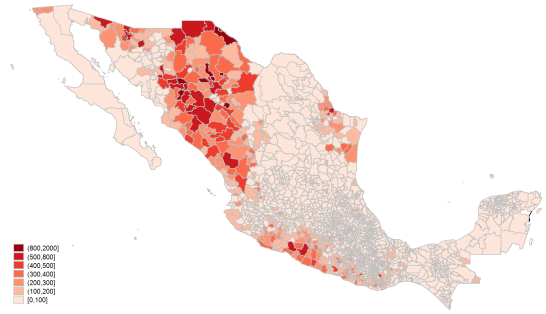
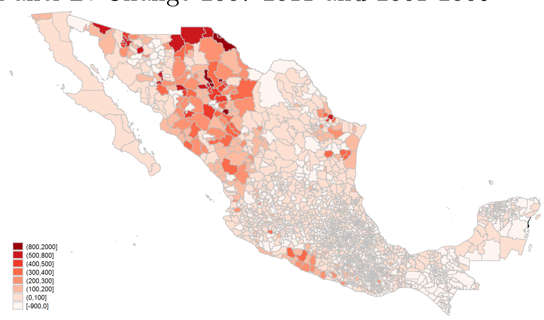


Figure 2.2: Spatial distribution of homicides

Panel A. Total homicides 2007-2011



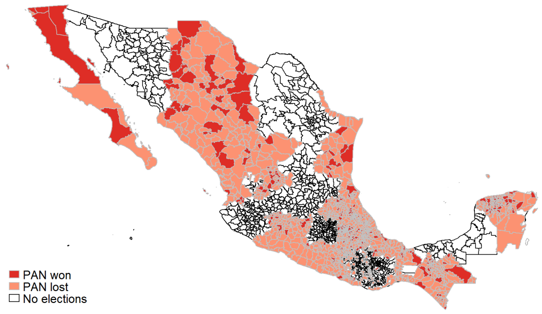
Panel B. Change 2007-2011 and 2001-2006



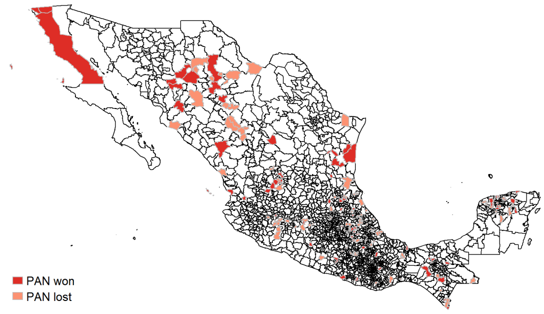
Notes: Panel A depicts the geographical distribution of total homicides between 2007 and 20011 per 100,000 inhabitants. Panel B depicts total homicides between 2007 and 2011 minus total homicides between 2001 and 2006, per 100,000 inhabitants. It is not possible to compute growth rates or logs because many municipalities have zero homicides.

Figure 2.3: Spatial distribution of of electoral outcomes

Panel A. All municipalities



Panel B. Spread 5%



Notes: Panel A depicts the geographical distribution of PAN victories and losses in the 2007 and 2008 local elections. Panel B depicts PAN victories and losses by a margin smaller than 5%.

Figure 2.4: Cumulative Homicides on the Average Differences of Close Elections in Neighbor Municipalities

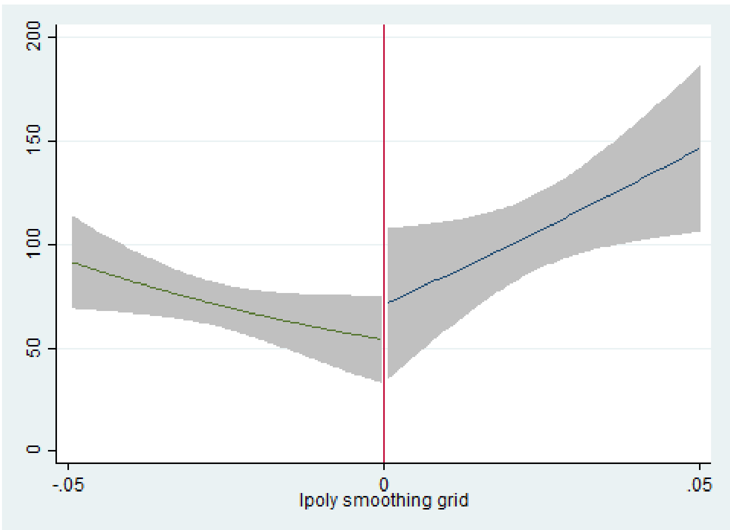
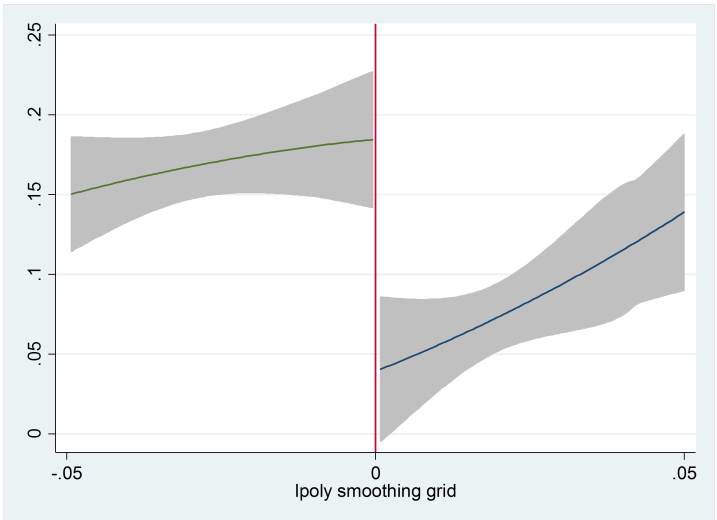


Figure 2.5: Exports on Average Differences in Close Elections of Neighbor Municipalities



2.10 Tables

Table 2.1: Baseline characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
	Total sample				Spread 5%			Neighbor election (5% Spread)		
	All	PAN won	PAN lost	P-value	PAN won	PAN lost	P-value	PAN won	PAN lost	P-value
Panel A: baseline characteristics										
Population 2005	35019	38396	34270	0.54	59232	42934	0.44	37968	33170	0.48
	(97487)	(126163)	(89949)		(190580)	(103344)		(111913)	(66014)	
Population ages 15-29	25.6	26.2	25.5	0	26.2	25.9	0.33	26	25.6	0.05
(% of total)	(2.5)	(2.2)	(2.5)		(2.3)	(2.6)		(2.3)	(2.5)	
Population density, 2005	151.9	162.9	149.4	0.61	209.6	188.14	0.75	129.5	148.14	0.48
	(381.5)	(385.1)	(380.8)		(465.8)	(466.3)		(287.2)	(377.1)	
PAN incumbent	0.27	0.28	0.26	0.49	0.31	0.32	0.84	0.24	0.32	0.02
	(0.44)	(0.45)	(0.44)		(0.47)	(0.47)		(0.43)	(0.47)	
GDP per capita	5740	5996	5683	0.09	6085	6228	0.74	5699	5814	0.55
(USD, 2005)	(2678)	(2942)	(2613)		(3360)	(2759)		(2392)	(2723)	
Literacy rate ages	95.2	95.6	95.1	0.13	95.5	96.1	0.29	95.1	95.8	0.03
(ages 15-24, 2005)	(4.9)	(4.1)	(5.1)		(4.3)	(3.2)		(4.7)	(3.6)	
Mean years of	5.9	6.1	5.9	0.16	6.1	6.1	0.97	6	6	0.64
schooling, 2005	(1.4)	(1.4)	(1.4)		(1.4)	(1.4)		(1.4)	(1.3)	
Mean Homicides, 2006	11.77	9.31	12.31	0.04	12.03	12.66	0.86	12.18	11.36	0.64
per 100 Population	(20.75)	(19.09)	(21.07)		(20.77)	(21.62)		(23.16)	(23.14)	
Observations	1416	257	1159		87	111		300	386	
Panel B: Baseline trade characteristics										
Total exports	52.5	81	46.1	0.14	178.6	71.5	0.35	37.2	47.7	0.45
	(340)	(681.6)	(195.7)		(1160.4)	(259.2)		(140.8)	(202.3)	
Exports: number	19	19.5	18.9	0.71	22.6	22.6	1	18.9	18.7	0.85
of countries	(19.9)	(22.5)	(19.3)		(27.2)	(23.6)		(19.3)	(19.6)	
Exports: number of	2.2	2.5	2.1	0.07	3.2	2.6	0.4	2.2	2.2	0.88
products per country	(2.8)	(4.1)	(2.4)		(6.1)	(3.7)		(3.3)	(2.2)	
Total imports	29.7	59.9	23	0.04	147.6	50.5	0.31	21.8	26.2	0.67
	(266.2)	(570.2)	(120.3)		(971)	(229.6)		(118.8)	(143.2)	
Imports: number	7.7	8.4	7.5	0.45	11.4	10.7	0.82	7	7.6	0.61
of countries	(16.7)	(20.1)	(15.8)		(27.4)	(19.7)		(17.3)	(15.1)	
Imports: number of	2.8	3.3	2.7	0.11	4.7	3.6	0.35	2.7	2.8	0.87
products per country	(5.5)	(6.8)	(5.2)		(9.6)	(7)		(5.7)	(4.9)	
Observations	1416	257	1159		87	111		320	413	

Notes: Columns 1-3 report means for all municipalities in which elections occurred in 2007 and 2008. Columns 5-6 restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%. Columns 8-9 restrict for municipalities that had at least one neighbor facing a close election and in all of those close neighboring elections either PAN won or PAN lost (i.e we exclude municipalities exposed to a treatment and a control at the same time). Columns 4, 7, and 10 report p-values of t-tests on the difference in means. Standard errors are reported in parentheses.

Table 2.2: Effect on homicides, 5% spread

	(1)	(2)	(3)	(4)	(5)	(6)
		OLS			WLS (Pop. 2005)	
Panel A: Average homicide 3 years after election (07 and 08 elections)						
PAN win	0.02	0.79	0.79	25.90**	41.22**	41.22*
	(5.34)	(9.63)	(7.30)	(12.65)	(18.98)	(19.79)
Linear pol.	No	Yes	Yes	No	Yes	Yes
Cluster: state level	No	No	Yes	No	No	Yes
Observations	198	198	198	198	198	198
R-squared	0.000	0.001	0.001	0.172	0.253	0.253
Panel B: Average homicide 3 years before election (07 and 08 elections)						
PAN win	1.59	1.36	1.36	3.29	3.76	3.76
	(2.48)	(4.00)	(5.87)	(2.71)	(4.32)	(4.80)
Observations	198	198	198	198	198	198
R-squared	0.002	0.002	0.002	0.030	0.034	0.034
Panel C: Average homicide 3 years after minus 3 years before election (07 and 08 elections)						
PAN win	-1.57	-0.56	-0.56	22.61**	37.47**	37.47**
	(4.44)	(8.27)	(3.84)	(10.80)	(16.62)	(16.81)
Observations	198	198	198	198	198	198
R-squared	0.001	0.002	0.002	0.179	0.301	0.301
Panel D: Placebo, average homicides 3 years after election(04 and 05 elections)						
PAN win	2.73	5.97	5.97	-5.08**	-0.81	-0.81
	(2.09)	(5.71)	(3.52)	(2.22)	(3.09)	(2.35)
Observations	247	247	247	247	247	247
R-squared	0.006	0.016	0.016	0.095	0.122	0.122

Notes: Columns 1-3 report standard OLS regressions. Columns 4-6 report weighted regressions. Weights are determined by population size in 2005. The dependent variable in panels A and D is average annual homicides per 100,000 population in the three years following local elections; in panel B the dependent variable is average annual homicides per 100,000 population in the three years preceding local elections; and in panel C the dependent variable is the difference between the dependent variables of panels A and B. In panels A, B and C, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. In panels D, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections. Robust standard errors are reported in parentheses.

Table 2.3: Homicides Regressions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Standard OLS				WLS (Pop. 2005)			
Panel A: Average homicide 3 years after election (2007 and 2008 elections)								
PAN win Neighbor	4.46**	0.18			11.17*	11.15		
	(2.07)	(4.21)			(6.14)	(12)		
Standardized Pop Treated Neighbor			2**	1.85*			3.7***	3.5***
			(1.02)	(1.06)			(1.22)	(1.27)
Linear polynomial	No	Yes	No	Yes	No	Yes	No	Yes
Observations	686	686	686	686	686	686	686	686
R-squared	0.01	0.01	0.01	0.02	0.05	0.06	0.21	0.22
Panel B: Average homicide 3 years before election (2007 and 2008 elections)								
PAN win Neighbor	2.95**	-5.66*			1.8	-0.91		
	(1.28)	(2.89)			(1.3)	(2.71)		
Standardized Pop Treated Neighbor			0.003	-0.25			0.6***	0.5***
			(0.31)	(0.38)			(0.17)	(0.18)
Observations	686	686	686	686	686	686	686	686
R-squared	0.01	0.03	0.00	0.03	0.01	0.01	0.02	0.03
Panel C: Average homicide 3 years after election minus 3 years before election(2007 and 2008 elections)								
PAN win Neighbor	1.51	5.84*			9.36*	12.06		
	(1.68)	(3.33)			(5.23)	(10.11)		
Standardized Pop Treated Neighbor			2**	2**			3.1***	2.9***
			(0.79)	(0.79)			(1.07)	(1.10)
Observations					686	686	686	686
R-squared	0.00	0.00	0.01	0.01	0.06	0.08	0.24	0.25
Panel D: Placebo, average homicides 3 years after election(2004 and 2005 elections)								
PAN win Neighbor	-1.53	-3.79			-10.13*	-0.93		
	(2.7)	(5.29)			(5.37)	(5.71)		
Standardized Pop Treated Neighbor			-2.51***	-2.10***			-2.4***	-1.4***
			(0.46)	(0.81)			(0.83)	(0.39)
Observations	662	662	662	662	662	662	662	662
R-squared	0.01	0.00	0.01	0.01	0.04	0.07	0.02	0.07

Notes: Columns 1-3 report standard OLS regressions. Columns 4-6 report weighted regressions. Weights are determined by population size in 2005. The dependent variable in panels A and D is average annual homicides per 100,000 population in the three years following local elections; in panel B the dependent variable is average annual homicides per 100,000 population in the three years preceding local elections; and in panel C the dependent variable is the difference between the dependent variables of panels A and B. In panels D, we study the effects after the 2004 and 2005 elections (before the Drug War policy) as a placebo. The explanatory variable PAN win Neighbor is a dummy that takes value 1 if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose PAN. It takes a value of zero if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose a party different from PAN. (i.e, we exclude municipalities with no neighbors facing close elections or with multiple neighbors facing close elections with mixed results between them). Standardized Population gives us the $\frac{Pop-Mean(Pop)}{sd(Pop)}$ of the municipalities that elected a PAN mayor in a close election. Where the mean and standard deviations are obtained from the means of all Mexican municipalities. Robust standard errors are reported in parentheses.

Table 2.4: Total exports & imports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				WLS (Pop. 2005)			
Panel A: Exports								
PAN win	-0.18***	-0.36***	-0.41***	-0.25***	-0.23***	-0.53***	-0.55***	-0.40***
	(0.06)	(0.07)	(0.07)	(0.08)	(0.08)	(0.08)	(0.07)	(0.08)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Country of destination FE	No	No	Yes	No	No	No	Yes	No
Product-country of destination FE	No	No	No	Yes	No	No	No	Yes
Observations	17,735	17,735	17,721	15,185	17,735	17,735	17,721	15,185
R-squared	0.00	0.00	0.04	0.53	0.00	0.00	0.03	0.59
Panel B: Imports								
PAN win	-0.11	-0.21*	-0.26**	-0.18	-0.15**	-0.12	-0.17**	-0.14**
	(0.07)	(0.13)	(0.12)	(0.11)	(0.07)	(0.08)	(0.07)	(0.07)
Observations	23,181	23,181	23,164	19,892	23,181	23,181	23,164	19,892
R-squared	0.00	0.00	0.02	0.30	0.00	0.00	0.03	0.40

Notes: Columns 1-4 report standard OLS regressions; columns 5-8 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In panel A (B), the dependent variable is the natural logarithmic of total exports (imports) in the final year of the new incumbent's term, divided by total exports (imports) in the year when elections took place. In panel B, country of destination dummies refer to country of origin dummies. The sample is comprised of triples municipality-country of destination (origin)-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the dependent variable for the triple is positive over the new incumbent's term.

Table 2.5: Placebo and pre-trends: total exports

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				WLS (Pop. 2005)			
Panel A: Exports, placebo 2004-2005 Elections								
PAN win	-0.10***	-0.14**	-0.11	-0.18**	-0.11***	-0.12	-0.13	-0.21
	(0.04)	(0.06)	(0.07)	(0.08)	(0.03)	(0.09)	(0.10)	(0.14)
Linear RD Polynomial	No	No	Yes	Yes	No	Yes	Yes	Yes
Country of destination FE	No	No	No	Yes	No	No	Yes	No
Product-country of destination FE	No	No	No	No	No	No	No	Yes
Observations	17,508	17,508	17,495	14,682	17,058	17,508	17,495	14,682
R-squared	0.00	0.00	0.03	0.02	0.00	0.00	0.03	0.60
Panel B: Exports, pre-trends 2007-2008 elections								
PAN win	0.06	0.13	0.14*	0.35**	0.10*	0.09	0.11	0.47***
	(0.06)	(0.08)	(0.08)	(0.15)	(0.06)	(0.09)	(0.07)	(0.11)
Observations	13,572	13,572	13,572	10,959	13,572	13,572	13,552	19,959
R-squared	0.00	0.00	0.01	0.46	0.00	0.00	0.01	0.59

Notes: Columns 1-4 report standard OLS regressions; columns 5-8 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In panel A the dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place for the election that happened before the Drug War was implemented. In panel B the dependent variable is the natural logarithmic of total exports one year before the election took place, divided by the initial exports three years before. The sample is comprised of triples municipality-country of destination (origin)-product where (i) PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections and (ii) the dependent variable for the triple is positive over the term.

Table 2.6: Placebo: total exports & imports

	(1)	(2)	(3)	(4)	(5)	(7)	(7)	(8)
	2007 and 2008 elections				Placebo: 2004 and 2005 elections			
Panel A: Exports								
PAN win	-0.17***	-0.41***	-0.42***	-0.40***	-0.12***	-0.11	-0.12	-0.19
	(0.05)	(0.04)	(0.04)	(0.11)	(0.03)	(0.08)	(0.10)	(0.13)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Country of destination FE	No	No	Yes	No	No	No	Yes	No
Product-country of destination Fe	No	No	No	Yes	No	No	No	Yes
Observations	18,147	18,147	18,133	15,556	16,007	16,007	15,995	13,480
R-squared	0.00	0.00	0.02	0.57	0.00	0.00	0.04	0.61
Panel A: Imports								
PAN win	-0.11**	-0.10	-0.14*	-0.12	0.01	0.19***	0.19***	0.16***
	(0.05)	(0.11)	(0.08)	(0.09)	(0.05)	(0.04)	(0.03)	(0.03)
Observations	23,517	23,517	23,499	20,202	23,244	23,244	23,228	19,864
R-squared	0.00	0.00	0.02	0.39	0.00	0.00	0.02	0.41

Notes: All columns report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In panel A (B), the dependent variable is the natural logarithmic of total exports (imports) in the second year of the new incumbent's term, divided by total exports (imports) in the year when elections took place. The sample is comprised of triples municipality-country of destination (origin)-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the dependent variable for the triple is positive over the first two years of the new incumbent's term.

Table 2.7: Trade Effects of a close PAN election in a neighbor municipality

	(1)	(2)	(3)	(4)	(6)	(7)
		OLS			WLS (Pop. 2005)	
Panel A: Log export growth (2007 and 2008 elections)						
PAN win	-0.17***	-0.11**	0	-0.31***	-0.37***	-0.05
Neighbor	(0.06)	(0.04)	(0.04)	(0.09)	(0.09)	(0.11)
Linear polynomial	Yes	Yes	Yes	Yes	Yes	Yes
Country	No	Yes	No	No	Yes	No
Product-country	No	No	Yes	No	No	Yes
Observations	36630	36619	33567	36309	36298	33251
R-squared	0.0015	0.0336	0.5525	0.0018	0.0233	0.62
Panel B: Placebo, log export growth (2004 and 2005 elections)						
PAN win	-0.05	-0.05	-0.04*	-0.02	-0.05*	-0.04
Neighbor	(0.05)	(0.03)	(0.06)	(0.03)	(0.03)	(0.06)
Observations	24853	22052	22052	24853	24840	22052
R-squared	0.0002	0.5413	0.5413	0.0002	0.0233	0.5413
Panel C: Log export growth (2007 and 2008 elections)						
Standardised Pop	-0.06***	-0.07***	-0.01	-0.05***	-0.06***	0.01
Treated Neighbor	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Observations	36630	36619	33567	36309	36298	33251
R-squared	0.0029	0.0355	0.55	0.0019	0.0234	0.5649
Panel B: Placebo, log export growth (2004 and 2005 elections)						
Standardised Pop	-0.008	-0.01**	-0.003	0	-0.008***	-0.002
Treated Neighbor	(0.007)	0.008	(0.009)	(0.002)	(0.003)	(0.008)
Observations	24853	26439	22052	24853	24840	22052
R-squared	0.0001	0.0295	0.5077	0.0001	0.0233	0.5413

Notes: Standard errors are clustered at the municipality level. Weights are determined by population size in 2005. The PAN win Neighbor Dummy takes value 1 if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose PAN. It takes a value of zero if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose a party different from PAN. (i.e, we exclude municipalities with no neighbors facing close elections or with multiple neighbors facing close elections with mixed results between them). Standardized Population gives us the $\frac{Pop-Mean(Pop)}{sd(Pop)}$ of the municipalities that elected a PAN mayor in a close election. Where the mean and standard deviations are obtained from the means of all Mexican municipalities.

Table 2.8: Exports per quartile of product complexity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	1st quartile (low)		2nd quartile		3rd quartile		4th quartile (high)	
Panel A: Exports (WLS)								
PAN win	-0.07	0.11	-0.17	-0.32	-0.68***	-0.32***	-0.88***	-0.65***
	(0.25)	(0.34)	(0.14)	(0.23)	(0.06)	(0.05)	(0.29)	(0.11)
Linear RD Polynomial	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Country of destination FE	Yes	No	Yes	No	Yes	No	Yes	No
Product-country of destination FE	No	Yes	No	Yes	No	Yes	No	Yes
Observations	3,899	3,535	3,790	3,220	4,695	4,011	5,306	4,418
R-squared	0.10	0.58	0.06	0.57	0.06	0.60	0.05	0.59
Panel B: Exports (OLS)								
PAN win	-0.07	-0.07	-0.23**	-0.22*	-0.59***	-0.28**	-0.60***	-0.48***
	(0.14)	(0.13)	(0.10)	(0.13)	(0.12)	(0.14)	(0.18)	(0.12)
Observations	4,558	4,113	4,511	3,853	5,698	4,820	6,622	5,480
R-squared	0.09	0.58	0.08	0.52	0.05	0.51	0.05	0.49
Panel C: Exports (WLS)								
PAN win	0.02	-0.01	-0.07	0.02	-0.83***	-0.25	-0.31**	-0.1
Neighbor	(0.12)	(0.11)	(0.11)	(0.12)	(0.14)	(0.18)	(0.13)	(0.18)
Observations	12,485	12,056	8,202	7,463	7,198	6,361	8,375	7,326
R-squared	0.0708	0.61	0.07	0.66	0.05	0.60	0.03	0.62
Panel D: Exports (OLS)								
PAN win	-0.02	0.001	-0.02	0.03	-0.55***	-0.04	-0.22*	-0.06
Neighbor	(0.05)	(0.04)	(0.08)	(0.07)	(0.15)	(0.15)	(0.12)	(0.13)
Observations	12,739	12,310	8,256	7,519	7,202	6,365	8,384	7,328
R-squared	0.14	0.64	0.1	0.59	0.04	0.49	0.04	0.49

Notes: All columns report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. The dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place. Country of destination dummies refer to country of origin dummies. In Panel A and B The sample is comprised of triples municipality-country of destination (origin)-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the dependent variable for the triple is positive over the new incumbent's term. In Panel C and D the sample is comprised of triples municipality-country of destination (origin)-product for municipalities that had (i) at least one neighbor facing a close elections and (ii) if multiple neighbors face a close elections, the results are not mixed across parties. The explanatory variable PAN win Neighbor is a dummy that takes value 1 if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose PAN. It takes a value of zero if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose a party different from PAN. (i.e, we exclude municipalities with no neighbors facing close elections or with multiple neighbors facing close elections with mixed results between them). Products are divided in 1241 categories. We divide the 1241 products in four groups according to their complexity as defined by the Atlas of Economic Complexity.

Table 2.9: Firm-level export growth regressions

	(1) All firms	(2) Small firms	(3) Large firms	(4) Low complexity firms	(5) High complexity firms
Panel A: Firm-level regressions - own municipality					
PAN win	-0.131***	-0.217***	-0.0231	-0.183**	-0.552***
	(0.0473)	(0.0635)	(0.126)	(0.0902)	(0.180)
Observations	1,543	771	771	525	194
R-squared	0.14	0.17	0.21	0.13	0.24
Panel B: Firm-level pre-treatment - own municipality					
PAN win	0.299**	0.318*	0.268	0.287	0.422
	(0.132)	(0.168)	(0.182)	(0.176)	(0.401)
Observations	1,831	914	915	604	356
R-squared	0.12	0.18	0.17	0.09	0.07
Panel C: Firm-Level placebo - own municipality					
PAN win	-0.0193	-0.0987	0.0677	-0.256*	0.0815
	(0.0683)	(0.119)	(0.120)	(0.143)	(0.212)
Observations	1,360	681	679	452	238
R-squared	0.14	0.19	0.20	0.14	0.12
Panel D: Firm-level regression - neighboring municipalities					
PAN win	-0.187**	-0.181*	-0.18	-0.194**	0.784
	(0.0760)	(0.0965)	(0.187)	(0.0806)	(0.954)
Observations	1,846	922	924	749	209
R-squared	0.14	0.18	0.21	0.12	0.21
Panel E: Firm-level pre-treatment - neighboring municipalities					
PAN win	0.09	0.24**	-0.22	0.18	-0.09
	-0.0871	(0.109)	(0.209)	(0.130)	(0.282)
Observations	2,189	1,094	1,095	867	409
R-squared	0.09	0.12	0.16	0.06	0.07
Panel F: Firm-level placebo - neighboring municipalities					
PAN win	-0.21	-0.10	-0.25	-0.30	-0.35
	(0.156)	(0.262)	(0.261)	(0.234)	(0.264)
Observations	1,076	538	538	441	160
R-squared	0.15	0.22	0.24	0.14	0.17

Notes: All regressions include 1st order polynomial terms around the electoral discontinuity, as well as population controls and industry-level fixed effects. Standard errors are clustered at the municipality level. Panel A captures the effect of locating in a municipality under a close PAN victory on export growth between 2007 and 2010. Panel B shows this effect between 2004 and 2006, providing a pre-treatment estimate of the regressions in Panel A. Panel C uses electoral results from 2004 to predict the effect between 2004 and 2006, providing Placebo estimates. Similarly, Panel D shows the effect of locating in a municipality neighboring a close PAN victory on the growth of exports of a firm between 2007 and 2010, while panels E and F follow the same approaches of panels B and C, functioning as pre-treatment and placebo estimates of the results in Panel D. The different columns provide estimates for different samples of firms, according to either their size or the economic complexity of their industries. Column 1 shows estimates for all plants. Columns 2 and 3 show estimates for plants below and above the median plant in the sample. Columns 4 and 5 show estimates for plants in industries representing the bottom and top quartiles of the complexity distribution. Firms are segmented by economic complexity levels according to scores for each of the 256 industry classifications available the Mexican Atlas of Economic Complexity.

Table 2.10: Firm-level wage bill displacement regressions

	(1) All firms	(2) Small firms	(3) Large firms	(4) Low complexity firms	(5) High complexity firms
Panel A: Firm-level regressions - own municipality					
PAN win	0.00364	-0.00684	0.00915	0.00692	-0.0647*
	(0.0113)	(0.0107)	(0.0165)	(0.0157)	(0.0363)
Observations	4,915	2,308	2,603	2,899	161
R-squared	0.046	0.089	0.062	0.033	0.257
Panel B: Firm-level pre-treatment - own municipality					
PAN win	-0.00618	0.00495	-0.0240	0.00891	0.0548
	(0.0136)	(0.0212)	(0.0169)	(0.0133)	(0.0678)
Observations	5,003	2,478	2,525	2,792	160
R-squared	0.029	0.070	0.046	0.023	0.322
Panel C: Firm-Level placebo - own municipality					
PAN win	-0.0237	-0.0432	-0.00262	0.00804	0.0321
	(0.0175)	(0.0295)	(0.0227)	(0.0277)	(0.0445)
Observations	4,579	2,050	2,529	2,336	175
R-squared	0.043	0.082	0.053	0.037	0.241
Panel D: Firm-level regression - neighboring municipalities					
PAN win	0.00241	-0.00616	0.000284	0.000860	-0.176*
	(0.0118)	(0.0151)	(0.0193)	(0.0160)	(0.0952)
Observations	4,633	2,012	2,621	2,830	121
R-squared	0.054	0.105	0.081	0.023	0.622
Panel E: Firm-level pre-treatment - neighboring municipalities					
PAN win	0.0200	0.0423	-0.00818	0.0225	-0.162*
	(0.0222)	(0.0273)	(0.0320)	(0.0276)	(0.0819)
Observations	4,667	2,160	2,505	2,675	111
R-squared	0.039	0.071	0.063	0.023	0.419
Panel F: Firm-level placebo - neighboring municipalities					
PAN win	-0.0351	-0.0679	-0.0101	-0.0235	-0.596
	(0.0257)	(0.0448)	(0.0358)	(0.0342)	(0.506)
Observations	5,004	2,255	2,749	2,910	122
R-squared	0.033	0.060	0.060	0.022	0.384

Notes: All regressions include 1st order polynomial terms around the electoral discontinuity, as well as population controls and industry-level fixed effects. Standard errors are clustered at the municipality level. Panel A captures the effect of locating in a municipality under a close PAN victory on the share of a firm's wage bill between 2007 and 2010. Panel B shows this effect between 2004 and 2006, providing a pre-treatment estimate of the regressions in Panel A. Panel C uses electoral results from 2004 to predict the effect between 2004 and 2006, providing Placebo estimates. Similarly, Panel D shows the effect of locating in a municipality neighboring a close PAN victory on the share of a firm's wage bill between 2007 and 2010, while panels E and F follow the same approaches of panels B and C, functioning as pre-treatment and placebo estimates of the results in Panel D. The different columns provide estimates for different samples of firms, according to either their size or the economic complexity of their industries. Column 1 shows estimates for all plants. Columns 2 and 3 show estimates for plants below and above the median plant in the sample. Columns 4 and 5 show estimates for plants in industries representing the bottom and top quartiles of the complexity distribution. Firms are segmented by economic complexity levels according to scores for each of the 256 industry classifications available the Mexican Atlas of Economic Complexity.

Table 2.11: Exports per region

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				WLS (Pop. 2005)			
Panel A: Europe								
PAN win	-0.14**	-0.22*	-0.23**	-0.11	-0.16*	-0.14	-0.12	0.01
	(0.06)	(0.12)	(0.12)	(0.11)	(0.09)	(0.16)	(0.16)	(0.22)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Country of destination FE	No	No	Yes	No	No	No	Yes	No
Product-country of destination FE	No	No	No	Yes	No	No	No	Yes
Observations	2,924	2,924	2,922	2,453	2,924	2,924	2,922	2,453
R-squared	0.00	0.00	0.02	0.67	0.00	0.00	0.03	0.69
Panel B: Bolivia, Colombia and Peru								
PAN win	-0.43**	-0.88***	-0.87***	-0.51**	-0.61**	-1.15***	-1.14***	-0.77*
	(0.19)	(0.22)	(0.22)	(0.22)	(0.25)	(0.41)	(0.41)	(0.39)
Observations	1,013	1,013	1,013	857	1,013	1,013	1,013	857
R-squared	0.02	0.03	0.03	0.62	0.03	0.04	0.04	0.69
Panel C: United States								
PAN win	-0.14**	-0.39***		-0.22	-0.16*	-0.55***		-0.47***
	(0.07)	(0.12)		(0.15)	(0.08)	(0.14)		(0.15)
Observations	4,363	4,363		4,185	4,363	4,363		4,185
R-squared	0.00	0.00		0.30	0.00	0.00		0.36
Panel D: China								
PAN win	0.04	-0.53		-1.08**	-0.04	-1.25***		-1.61***
	(0.19)	(0.42)		(0.41)	(0.22)	(0.38)		(0.44)
Observations	330	330		284	330	330		284
R-squared	0.00	0.01		0.51	0.00	0.01		0.54

Notes: Columns 1-4 report standard OLS regressions; columns 5-8 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In all panels, the dependent variable is the natural logarithmic of total exports in the final year of the new incumbent's term, divided by total exports in the year when elections took place. The sample is comprised of triples municipality-country of destination-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the dependent variable for the triple is positive over the new incumbent's term. When the region is comprised of a single country, product-country of destination dummies are actually product dummies, and the regressions with country of destination dummies are redundant.

Table 2.12: Aggregate labor displacement

	(1)	(2)	(3)	(4)	(5)	(6)
	OLS			WLS (Pop. 2005)		
PANwin	0.23			1.62*		
	(0.60)			(0.92)		
PAN win		0.05			0.8**	
neighbor		(0.11)			(0.38)	
Standardized Pop			0.19***			0.28***
Treated Neighbor			(0.07)			(0.04)
R-squared	0.00	0.00	0.01	0.16	0.04	0.18
Observations	194	873	873	194	786	786

Notes: In all panels, the dependent variable is the natural logarithmic of migrating workers. In the direct close election regressions we apply linear polynomial controls. The explanatory variable PAN win Neighbor is a dummy that takes value 1 if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose PAN. It takes a value of zero if (i) a municipality has at least one neighbor facing a close election (ii) all of these neighbors with close elections choose a party different from PAN. (i.e, we exclude municipalities with no neighbors facing close elections or with multiple neighbors facing close elections with mixed results between them). Standardized Population gives us the $\frac{Pop-Mean(Pop)}{sd(Pop)}$ of the municipalities that elected a PAN mayor in a close election, where the mean and standard deviations are obtained from the means of all Mexican municipalities.

Appendix

Table 2.A1: Effect on other crimes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS		WLS		OLS		WLS	
	Panel A: Robbery (business establishments)				*Panel B: Assaults*			
PAN win	7.9	29.8**	46.5	68.5	13.5	42.6	142.9**	192.8
	(7.491)	(14.839)	(35.284)	(45.864)	(17.919)	(37.687)	(66.821)	(119.079)
Linear polynomial	No	Yes	No	Yes	No	Yes	No	Yes
Observations	139	139	139	139	139	139	139	139
R-squared	0.008	0.043	0.106	0.143	0.004	0.050	0.175	0.235
	Panel C: Extortion				*Panel D: Kidnapping*			
PAN win	-0.5	-0.5	1.7	4.7*	-0.4	0.1	0.3	1.4
	(0.685)	(1.456)	(2.189)	(2.646)	(0.303)	(0.558)	(0.643)	(1.026)
R-squared	0.004	0.041	0.026	0.169	0.011	0.018	0.006	0.098
	Panel E: Robbery (banks branches, cash-in-transit vehicles)				*Panel F: Robbery (all cases, excluding business and banks)*			
PAN win	0.4	0.6	1.3	2.8*	39.8	254.4**	455.0	917.1***
	(0.301)	(0.423)	(0.865)	(1.616)	(65.096)	(126.760)	(299.769)	(345.038)
R-squared	0.015	0.019	0.118	0.323	0.003	0.049	0.123	0.217

Notes: Columns 1-2 and 5-6 report standard OLS regressions. Columns 3-4 and 7-8 report weighted regressions. Weights are determined by population size in 2005. In all panels the dependent variables are averages of a certain crime type per 100,000 population in 2011. In panel A the dependent variable is robberies that targeted business establishments (including cargo theft); in Panel B, assaults; in panel C, extortions; in Panel D, kidnapping; in Panel E, robberies that targeted bank branches and cash-in-transit vehicles; and in Panel F, robberies (excluding business and banks). For all regressions, the sample is comprised of municipalities where crime data is available and where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections. Robust standard errors are reported in parentheses.

Table 2.A2: Effect on homicides, 3% spread

	(1)	(2)	(3)	(4)	(5)	(6)
	Standard OLS			Weighted OLS (Population 2005)		
Panel A: Average homicide 3 years after election						
PAN win	-0.66	-0.20	-0.20	28.97**	47.91**	47.91**
	(6.83)	(9.06)	(6.26)	(13.87)	(18.87)	(19.36)
Linear polynomial	No	Yes	Yes	No	Yes	Yes
Cluster: state level	No	No	Yes	No	No	Yes
Observations	123	123	123	123	123	123
R-squared	0.000	0.005	0.005	0.185	0.306	0.306
Panel B: Average homicide 3 years before election						
PAN win	1.29	1.48	1.48	4.23	2.40	2.40
	(3.38)	(4.41)	(3.29)	(3.15)	(4.57)	(4.85)
R-squared	0.001	0.004	0.004	0.049	0.057	0.057
Panel C: Average homicide 3 years after election minus 3 years before election						
PAN win	-1.95	-1.68	-1.68	24.74**	45.51***	45.51**
	(5.74)	(7.68)	(4.84)	(11.75)	(17.29)	(18.01)
R-squared	0.001	0.004	0.004	0.182	0.340	0.340

Notes: Columns 1-3 report standard OLS regressions. Columns 4-6 report weighted regressions. Weights are determined by population size in 2005. The dependent variable in panel A is average annual homicides per 100,000 population in the three years following local elections; in panel B the dependent variable is average annual homicides per 100,000 population in the three years preceding local elections; and in Panel C the dependent variable is the difference between the panel the dependent variables of panels A and B. For all regressions, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 3% in the 2007 and 2008 elections. Robust standard errors are reported in parentheses.

Table 2.A3: Effect on homicides, RD polynomials

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				WLS (Pop. 2005)			
Panel A: Average homicide 3 years after election, 5% spread								
PAN win	0.79	-0.25	20.92	27.27	41.22*	52.98***	53.04**	68.11**
	(7.30)	(6.41)	(32.52)	(35.61)	(19.79)	(17.57)	(21.86)	(23.88)
Degree of polynomial	1st	2nd	3rd	4th	1st	2nd	3rd	4th
Observations	198	198	198	198	198	198	198	198
R-squared	0.00	0.01	0.04	0.05	0.25	0.30	0.30	0.33
Panel A: Average homicide 3 years after election, total sample								
PAN win	3.30	3.62	4.31	0.21	14.86	24.61**	31.65*	47.36**
	(2.13)	(3.33)	(6.02)	(5.83)	(9.94)	(11.46)	(15.61)	(22.20)
Observations	1,416	1,416	1,416	1,416	1,416	1,416	1,416	1,416
R-squared	0.00	0.00	0.01	0.01	0.02	0.03	0.03	0.05

Notes: Columns 1-4 report standard OLS regressions. Columns 5-8 report weighted regressions. Weights are determined by population size in 2005. The dependent variable is average annual homicides per 100,000 population in the three years following local elections. In Panel A, the sample is comprised of municipalities where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections. In Panel B the sample is comprised of all municipalities in which elections occurred in 2007 and 2008. All standard errors are clustered at the state level.

Table 2.A4: Imports per region

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	OLS				WLS (Pop. 2005)			
Panel A: Europe								
PAN win	0.05	-0.12	-0.15	-0.17	0.10	-0.00	-0.07	-0.11
	(0.11)	(0.15)	(0.14)	(0.12)	(0.10)	(0.15)	(0.12)	(0.08)
Linear RD Polynomial	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Country of origin FE	No	No	Yes	No	No	No	Yes	No
Product-country of origin FE	No	No	No	Yes	No	No	No	Yes
Observations	5,922	5,922	5,921	4,762	5,922	5,922	5,921	4,762
R-squared	0.00	0.00	0.02	0.32	0.00	0.00	0.02	0.44
Panel B: Bolivia, Colombia and Peru								
PAN win	-0.21	0.04	-0.12	0.87	-0.39*	0.32	0.22	1.16**
	(0.26)	(0.33)	(0.31)	(0.63)	(0.20)	(0.37)	(0.34)	(0.55)
Observations	106	106	106	68	106	106	106	68
R-squared	0.00	0.01	0.05	0.59	0.01	0.03	0.06	0.74
Panel C: United States								
PAN win	-0.07	-0.16		-0.13	-0.08**	-0.12		-0.13
	(0.05)	(0.11)		(0.12)	(0.03)	(0.08)		(0.09)
Observations	6,264	6,264		6,106	6,264	6,264		6,106
R-squared	0.00	0.00		0.20	0.00	0.00		0.29
Panel D: China								
PAN win	-0.19	-0.43**		-0.56***	-0.24	-0.44**		-0.60***
	(0.15)	(0.20)		(0.17)	(0.16)	(0.16)		(0.18)
Observations	2,411	2,411		2,252	2,411	2,411		2,252
R-squared	0.00	0.01		0.27	0.00	0.01		0.34

Notes: Columns 1-4 report standard OLS regressions; columns 5-8 report weighted regressions. Weights are determined by population size in 2005. Standard errors are clustered at the municipality level. In all panels, the dependent variable is the natural logarithmic of total imports in the final year of the new incumbent's term, divided by total imports in the year when elections took place. The sample is comprised of triples municipality-country of destination (origin)-product where (i) PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections and (ii) the dependent variable for the triple is positive over the new incumbent's term. When the region is comprised of a single country, product-country of destination dummies are actually product dummies, and the regressions with country of destination dummies are redundant.

Table 2.A5: Bank Baseline Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total sample				Spread 5%		
	All	PAN won	PAN lost	P-value means diff.	PAN won	PAN lost	P-value means diff.
Saving Accounts	5403	4684	5563	0.55	4112	5022	0.52
	(21289)	(11519)	(22898)		(9184)	(10194)	
Branches	2.5	2.5	2.4	0.86	2.5	2.7	0.77
	(6.4)	(5.37)	(6.59)		(4.44)	(4.68)	
Number of employees	18.6	18.5	18.6	0.97	19.1	21.5	0.68
working in branches	(57.75)	(40.68)	(60.9)		(37.69)	(43.45)	
Demand deposits	136	129	138	0.79	144	150	0.89
(million Pesos)	(485)	(288)	(519.24)		(290)	(276)	
Time deposits	110	103	112	0.73	99	137	0.26
(million Pesos)	(372)	(240)	(395)		(192)	(268)	
Observations	1416	257	1159		87	111	

Notes: Columns 1-3 report means for all municipalities in which elections occurred in 2007 and 2008. Columns 5-6 restrict the sample to municipalities where PAN won or lost by a margin smaller than 5%. Columns 4 and 7 report p-values of t-tests on the difference in means. Standard errors are reported in parentheses.

Table 2.A6: Bank Operations

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2007 and 2008 elections						Placebo: 2004 and 2005 elections					
	OLS			WLS (Pop. 2005)			OLS			WLS (Pop. 2005)		
Panel A: Saving accounts												
PAN win	-735	-2,901	-3,442**	-1,048**	-2,450***	-2,585***	-230	-2,082	-2,064	-424	473	144
	(1,067)	(1,925)	(1,705)	(407.195)	(774)	(756)	(1,365)	(2,932)	(3,141)	(615.012)	(1,192)	(1,279)
Linear RD Polynomial	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank dummies	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	250	250	250	250	250	250	197	197	197	197	197	197
R-squared	0.00	0.01	0.13	0.014	0.03	0.40	0.00	0.01	0.17	0.001	0.00	0.38
Panel B: Branches												
PAN win	-0.1	-0.4	-0.4	-0.0	-0.1**	-0.1**	-0.1	-0.2	-0.2	-0.1***	-0.1	-0.1
	(0.2)	(0.3)	(0.3)	(0.0)	(0.1)	(0.1)	(0.1)	(0.2)	(0.2)	(0.0)	(0.1)	(0.1)
Observations	346	346	346	346	346	346	227	227	227	227	227	227
R-squared	0.00	0.00	0.08	0.00	0.00	0.26	0.00	0.00	0.32	0.01	0.01	0.46
Panel C: Employees												
PAN win	-1.3	-3.8	-3.8	0.3	1.6	1.0	-1.9*	-2.4	-2.6	-0.2	-2.9**	-3.1**
	(1.6)	(3.2)	(3.2)	(1.3)	(2.5)	(2.6)	(1.0)	(1.8)	(2.0)	(0.8)	(1.3)	(1.5)
Observations	356	356	356	356	356	356	228	228	228	228	228	228
R-squared	0.00	0.00	0.06	0.00	0.00	0.18	0.01	0.01	0.36	0.00	0.01	0.49
Panel D: Demand deposits (million Pesos)												
PAN win	-4.7	-57.8	-50.9	-0.3	-3.8	-2.2	-18.5	-10.0	-11.9	-28.0	54.5***	44.3**
	(19.6)	(41.1)	(39.4)	(8.4)	(11.7)	(14.1)	(13.6)	(29.1)	(29.2)	(22.5)	(18.4)	(17.0)
Observations	317	317	317	317	317	317	226	226	226	226	226	226
R-squared	0.00	0.01	0.04	0.00	0.00	0.07	0.01	0.01	0.30	0.01	0.05	0.41
Panel E: Time deposits (million Pesos)												
PAN win	15.7	9.4	7.4	5.2	20.4	20.4	8.6	0.2	-0.9	5.6	-2.9	-2.8
	(14.9)	(22.9)	(23.6)	(15.8)	(15.9)	(16.9)	(5.8)	(7.7)	(7.1)	(3.8)	(8.8)	(9.3)
Observations	319	319	319	319	319	319	215	215	215	215	215	215
R-squared	0.00	0.02	0.07	0.00	0.01	0.26	0.00	0.00	0.36	0.00	0.00	0.53

Notes: Columns 1-3 report standard OLS regressions; columns 4-6 report weighted regressions. Weights are determined by population size in 2005. The sample is comprised of municipality-bank level data where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections (after the Drug War). Columns 7-9 report standard OLS regressions; columns 10-12 report weighted regressions. The sample is comprised of municipality-bank level data where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections (before the Drug War). Standard errors are clustered at the municipality level.

Table 2.A7: Bank Credit

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	2007 and 2008 elections						Placebo: 2004 and 2005 elections					
	OLS			WLS (Pop. 2005)			OLS			WLS (Pop. 2005)		
Panel A: Change in total credit (million Pesos)												
PAN win	-10.5	54.9*	45.3	1.9	7.5	11.6	-133.4	25.6	33.8	-12.4	-0.3	-3.9
	(28.1)	(30.6)	(28.5)	(6.4)	(10.0)	(11.7)	(135.6)	(38.4)	(50.8)	(9.9)	(10.7)	(11.8)
Linear RD Pol.	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Bank dummies	No	No	Yes	No	No	Yes	No	No	Yes	No	No	Yes
Observations	733	733	733	733	733	733	592	592	592	592	592	592
R-squared	0.00	0.00	0.07	0.00	0.01	0.03	0.00	0.00	0.03	0.00	0.00	0.01
Panel B: Change in unsecured credit (million Pesos)												
PAN win	11.2	23.4	25.9	1.6	-1.2	2.7	-499.0	120.8	190.4	-17.1	8.8	7.1
	(7.3)	(14.7)	(16.4)	(5.0)	(8.5)	(10.2)	(501.5)	(169.2)	(247.6)	(14.1)	(10.8)	(11.0)
Observations	559	559	559	559	559	559	357	357	357	357	357	357
R-squared	0.00	0.01	0.02	0.00	0.01	0.05	0.00	0.01	0.03	0.00	0.00	0.00
Panel C: Change share of credit balance in default												
PAN win	-0.01	0.01	0.01	-0.03	0.00	-0.01	-0.04	-0.08*	-0.05	-0.07*	-0.06	0.00
	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)	(0.04)	(0.03)	(0.04)	(0.04)	(0.03)	(0.07)	(0.04)
Observations	733	733	733	733	733	733	592	592	592	592	592	592
R-squared	0.00	0.01	0.07	0.01	0.01	0.11	0.00	0.01	0.18	0.02	0.02	0.40

Notes: Columns 1-3 report standard OLS regressions; columns 4-6 report weighted regressions. Weights are determined by population size in 2005. In all panels, the dependent variable is the change in total credit. The sample is comprised of municipality-bank level data where PAN won or lost by a margin smaller than 5% in the 2007 and 2008 elections (after the Drug War). Columns 7-9 report standard OLS regressions; columns 10-12 report weighted regressions. The sample is comprised of municipality-bank level data where PAN won or lost by a margin smaller than 5% in the 2004 and 2005 elections (before the Drug War). Standard errors are clustered at the municipality level.

3 A Managerial Explanation of Investment Sensitivity to Cash: Evidence from European Football Tournaments

Jesus Gorrin

3.1 Introduction

An important implication of the Modigliani-Miller (1958) theorem is that, under perfect capital markets, financing decisions and therefore cash availability should not matter for firm's investment decisions. To show whether it does matter and why is complicated. Since Kaplan and Zingales (1997), it has been accepted that positive investment sensitivities to cash can be explained by more than financial constraints. Endogeneity is an important concern when measuring investment sensitivity to cash. Cash is not random and is potentially related to unobservables, such as investment opportunities.[47]

The first contribution of this paper is to diminish endogeneity concerns when measuring investment sensitivity to cash. To achieve this, I use a quasi-experimental setting in which cash is as good as randomly assigned. Specifically, this setting is football teams of the European Champions League that obtain awards in close matches.

Second, and more important, this paper provides a new explanation—manager's experience—for this positive sensitivity of investment to cash. In general, the literature focuses on growth opportunities and/or financial constraints to explain this positive sensitivity. However, managers have discretion to choose a firm's investment. Theoretically their role is crucial, but, empirically, a firm's choice of manager is endogenous to other unobservables, leading to bias. The setting use here provides a unique opportunity to causally provide a link between investment to cash sensitivities and managerial characteristics. Thirdly, this paper explores two different theories than can explain investment sensitivity to cash flows: financial constraints (caused by informational asymmetries) or agency costs of free cash flow.

Information asymmetries can make internal funding cheaper than outside funding (Myers and Majluf, 1984). If prices for outside funding are too high, then positive net present value (NPV) projects only can be financed by internal funds. If internal funds are not enough to finance good projects or are costly, the lack of investment leads to a loss in potential economic value. Convincing evidence from the literature indicates that cash constraints lead to underinvestment. For example, Paravisini (2008) uses a natural experiment to provide evidence on credit-constrained banks, and De Mel, McKenzie, and Woodruff (2008) use random assignment to provide evidence of cash constraints in

[47] Kaplan and Zingales (1997), Almeida, Campello, and Weisbach (2005)

micro-enterprises. On the other hand, for some firms, internal funds may be misused. Misuse is related to the agency costs of free cash flow proposed by Jensen (1986). Other evidence also suggests that cash may be more valuable for well-governed firms (Dittmar and Mahrt-Smith, 2007) and that firms may use excess cash to invest in less-desirable projects (Lang, Stulz, and Walking, 1991). For some type of firms, cash may lead to a problem of over-investment.

In both theories, one expects to observe a positive sensitivity of investment to cash flow. Nonetheless, the result of increasing investment by using cash should be the opposite in each theory. Because cash does not suffer from information asymmetries, one expects to observe an improvement in performance for financially constrained firms after a random cash windfall. If these firms were suffering from under investment then we should observe investment in projects that improve the firm. We would observe the opposite if firms were subject to agency costs of free cash flow. This paper answers two questions. Can managerial characteristics explain investment sensitivity to cash? And if so, is this sensitivity consistent with over or under investment?

Explaining investment sensitivities to cash with *ex-ante* manager experience can be consistent with both financing constraints and free cash flow problems. Inexperienced managers could be a source of informational asymmetry, because there is more uncertainty about their quality. Also, lack of experience can be related to the propensity of managers to exhibit value destroying behavior. An important result of this paper is that investment is only sensitive to cash if managers are inexperienced. Furthermore, new investments made by a manager do not improve the team's performance ex-post. This result is more likely to support the free cash flow hypothesis.

This paper proposes one channel for positive investment sensitivity to cash flows: constraints may be caused by managerial qualities. The next question is whether those firms with inexperienced managers are better off after they receive extra cash. My evidence points to a theory were investment constraints may be optimal for a firm with inexperienced managers because they prevent them from using cash for private benefits rather than to improve performance. This is consistent with the free cash flow hypothesis.

In the spirit of Blanchard, Lopez-de-Silanes, and Shleifer (1994), I use a quasi-experimental setting that allows me to identify truly exogenous cash flows. This, in conjunction with information about managerial quality, helps me identify how investment responds to cash windfalls. To find this causal link, this study uses European football as its experimental setting. I gather data on European football player trades, managers, and cash awards from international football competitions. Because total cash in European international tournaments is related to team quality and managerial characteristics, I use cash generated in close games an instrument. I provide evidence that cash from close games is as good as randomly assigned and is uncorrelated with managers' experience. For inexperienced managers, a £1 increase in cash translates into £1.1 spent on new players during the winter market. When we account for the value of players sold, the effect is £0.68. The effect on team performance in the local league after managers spend the cash is close to zero. This investment also does not affect the probability of participating in more competitive programs in the next season. It also survives, and becomes stronger when we use the sub-sample of teams that did not advance stage in the tournament.

The European football industry provides an excellent experimental setting for testing theories with different empirical predictions on investment sensitivity to cash flow, and how managerial characteristics are linked to these predictions. First, players (or, more specifically, player contracts) are frequently traded across teams. Furthermore, players are the main input football clubs use to increase performance in the short term. Also, for most cases, managers' experience and full working history is perfectly observable. Moreover, we can consistently measure managers experience because jobs are within the same industry. Second, many clubs participate in European competitions that give cash awards based on performance. Many of the matches in these competitions are played between very similar teams. Thus, some wins are as good as random, and they provide an observable cash award. On the other hand, teams also play in the local league, which serves as a measure of performance that is independent of the results in the European competition. Third and finally, clubs can decide how to invest in players during very specific windows. Therefore, we can observe a natural lag between cash and investment, which reduces concerns on reverse causality.

To search for close matches, I construct a database with all the gambling odds for each match in the Champions League from January 2003 until May 2014. Also, I gather data on all the awards that have been given in the Champions League from 2003 until 2014 to each participating team. Close games are identified by narrow differences in the *ex-ante* odds. The lack of predictability of the odds in the neighborhood of a zero difference between teams provides convincing evidence that results in close games are as good as randomly assigned. My measure of random cash awards is the total amount of cash obtained in close matches. I then use IV to estimate the sensitivity of investment in the acquisition of new player contracts on total cash awards. Only teams with inexperienced managers have a significant positive sensitivity of investment to cash flow in the baseline regression.

This paper provides a clean setting for causally identifying the effect of cash on investment. However, more than a methodological contribution, this paper sheds new light on the relation between investment sensitivities and managerial characteristics. It shows that a lack of managerial experience may exacerbate investment sensitivity to cash flow. Interestingly, inexperienced managerial disbursements of cash do not translate into a significant improvement in performance. So restricting the investment opportunities of inexperienced managers may be a good idea for a firm, at least, until managers prove their skill.

3.2 Background

European football is a sport followed and played by millions of people worldwide. According to FIFA Big Count initiative, in 2006, the sport comprised around 65 million registered male and female players. Furthermore, 327,008 clubs are registered with national federations affiliated to FIFA.[48] Over the years, football has become more than just a sport; it is an important industry. According to figures from transfermarkt.co.uk, for 2014, the top-100 clubs had an estimated aggregate market value of £13.03 billion sterling.[49] They are managed by professionals who intervene in both the tactical decisions of the game and the decisions to trade players.

The particularities of the tournaments and rules of the sport provide a good setting for testing theories in finance and economics. For example, penalty kicks in football provide a good experiment for testing game-theory predictions. Because penalty taking can be observed as a zero-sum game with limited decision-making involved and only one outcome, it was used to provide evidence in favor of agents following a min-max strategy (Palacios-Huerta, 2003). Moreover, European football was used to estimate unintended consequences of policy changes and to verify the role of non-monetary incentives. First, by exploiting a change in rules, Garicano and Palacios-Huerta (2006) provide evidence of how teams use destructive play to take advantage of new rules. This brings negative consequences to the authorities in charge of the game by making the game less attractive to the audience. On the other hand, evidence suggests referees systematically favor home teams and succumb to crowd pressure (Garicano, Palacios-Huerta, and Prendergast, 2005).

Some authors in finance use European football results to measure the effect of changes in sentiment on financial markets. For instance, Edmans, Garcia, and Norli (2007) find abnormal negative returns in the domestic stock market just after a national team has been eliminated from the World Cup. Their results are robust and draw on evidence about negative psychological factors affecting investors. Also, there is evidence on how initial public offerings (IPOs) of football teams affect their performance (Baur and McKeating, 2011) and on the effect of team performance on their stock prices (Renneboog and Vandrabant, 2000). However, to the best of my knowledge, no other paper has tried to link the random nature of the prizes given in tournaments with managerial decisions.

I use the particularities of football tournaments and the industry in general to test

[48] FIFA Big Count Initiative. 2007. FIFA Communications Division. Information Services.

[49] Football Market Values. Retrieved from [transfermarkt.co.uk]

for theories about sensitivity of investment to cash flow. Examining the football industry can give us reliable estimates of the sensitivity of investment to cash for multiple reasons. First, clubs have a standardized and well-regulated market, where players are traded. Players are allowed to play for one club at a time. They can only change clubs at the end of their contract or if the contract is bought or borrowed by another team and accepted by the player. Trades only occur during specific windows. So even if teams secure cash after playing at different dates, all of them spend their money during almost the same windows in Europe. Even when there are differences between windows (most of them only differ in hours, if at all), trade with countries in which windows are closed is not possible. This diminishes concerns that investment in player contracts is driven by timing, rather than actual cash awards.

Second, there is a monetary market for odds in football. The institutional bookmakers in these markets act like market aggregators. They make profits by overestimating the odds of each event and, therefore, paying less than what the market-implied probability would require for breaking even. So the implied odds should aggregate market information. These implied probabilities can be used to predict the expected cash flows a team should receive from awards. Deviations from these expectations can be interpreted as random. This is the main source of exogeneiy in this paper.

Third, there is public information about the awards teams received in tournaments, the performance of the teams, the players traded, and the managers in charge of each team. Different from other industries, it is easier to measure managerial experience because managerial changes are publicly observable for all relevant professional teams. Therefore, I can follow the career of any particular manager over time. Furthermore, it is easier to measure experience, since manager careers are always in the same industry. This gives a good setting in which to test some of the most important relationships between management qualities and the standard predictions of either financial constraints or the free cash flow hypothesis.

3.3 Data

I compile three data sets for clubs that participated in the UEFA Champions League from 2003 until 2014.

3.3.1 Betting odds data for each game of the UEFA Champions League

I gather historical betting odds data from oddsportal.com for each game of the UEFA Champions League from January 2003 until May 2014. This web page provides data on the average betting odds offered by online betting companies, including, when available, the most popular ones, such as bwin, ladbrokes, and bet-at-home. The median number of bookmakers is 5, but, depending on the popularity of the game, it can range from 1 to 15 bookmakers. Bookmakers assume initial odds. Then each bookmaker updates the odds from observing demands for each result. To close the books, bookmakers adjust the odds such that they add to more than one. This way a gambler that takes all positions loses money, and the bookmaker makes money.[50] Therefore, because odds aggregate public information with real money at stake, one can reasonably assume they reveal market expectations about match results.

I focus on three potential outcomes: a win, a tie, or a loss.[51] Based on the odds, I estimate the implied probability for each event by dividing the odd of each event by the sum of the odds of the three possible events. Table 3.1 summarizes the statistics of odds for a home team win, tie, or loss. As we can see, home teams have better odds of winning before the match. Tying is the less likely event. Moreover, the probabilities significantly differ game by game.

3.3.2 Player transfer data by teams

Player transfer data were collected for all the 247 teams that participated at least in the Champions League initial stage at any point from January 2003 until May 2014 from transfermarkt.co.uk. This source has been used in other settings (Bryson, Frick, and Simmons, 2013). This web page collects the numbers of player transfers and their prices by each team per season. Nonetheless, because some transfer prices are never revealed, attrition may be a concern. However, for important transfers, Transfermarkt provides an estimate based on the news. The sample of teams treated by the instrument is larger than the average. So attrition bias is not likely to be a major problem.

Player contracts give teams the exclusive right to use a player in competition, although this right can be traded with other teams. Player transfers happen during two windows: a winter window, which occurs in the middle of the season, and a summer window, which occurs after the season ends. Even after a trade, a player can only play for one team in a particular season in the UEFA Champions League. However, they can play for several teams in the domestic league during the same season. So players bought during the winter transfer can only improve performance in the domestic league. Unfortunately, Transfermarkt data do not separate the trades by the specific windows. Therefore, I collect trade-by-trade information to determine in which window a player was traded. To minimize the data-gathering process, for winter transfers, I only collect data for teams that face close matches.[52] Because some teams go bankrupt or do not have available data

[50] On very rare occasions, the initial odds are set up in a way that the adjustments would still leave arbitrage opportunities. In my sample, this only happens in 14 of 2,126 matches.

[51] Gamblers could bet in other more complex events. However, the awards given to a team per match would depend on whether the team wins, ties, or loses.

[52] A close match in this setting is defined as one in which for each team the probability of losing minus

for earlier years, the panel is unbalanced. I am able to identify most transfer information from 2003 until 2014 for all the teams that advanced beyond the qualification stage of the Champions League at least once. Table 3.2 summarizes the relevant trade statistics by team.

According to the data, most teams do not buy or sell player contracts at a fee. There are two reasons for this. First, teams hire players from their academies or when they are already out of contract for no extra fee. Second, attrition may be strong, especially for small teams. If the main reason is attrition, it could lead to a bias. However, for small teams, the awards are relatively larger in size and therefore should affect managers investment decisions more. If small teams are also more likely to have less-experienced managers would bias my results downward. i.e., attrition would undermine the possibility of finding investment to cash sensitivity for less experienced managers.

On another matter, teams that face a close match tend to trade more actively. In a model with heterogeneous treatment effects, which are reasonable in this setting, I would find only the local average treatment effect (LATE). Therefore, it is important to keep in mind that, in this setting, the set of compliers is likely to cover a sample of teams that is bigger and more active than the average.

3.3.3 Money awards from UEFA Champions League

The UEFA Champions League awards fixed amounts in each stage, performance-based bonuses in the group stage, and a variable amount that depends on the value of the TV licenses of each team. Moreover, each match provides a performance-contingent payment.

Only cash generated until the group stage is available for the winter transfer market. For the purpose of this paper, the winter transfer market is more important for three reasons. First, players acquired in this market can be used in the domestic league immediately. Second, the window for planning is smaller in the middle of the season, so managerial discretion is a more pressing issue. Third, winter transfers diminish the likelihood of a managerial change before player transfers occur. Hence, I focus on awards obtained during the group stage.

UEFA's financial statements provide data on the awards for each team. Until the 2005/2006 season, awards were paid in Swiss Francs. Since the 2006/2007 season, they have been paid in Euros. Because the team trade data are denominated in pounds sterling, data on awards are converted to pounds, using the average exchange rate over the year (see the appendix for the data on sterling-denominated awards).

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the probability of winning is less than 5% in absolute value.

3.3.4 Manager data

I also collected manager's data from transfermarkt.co.uk. Managers experience is measured as the difference between the managers first professional appointment and the beginning of the football season.[53] Table 3.3 shows the average experience of a manager is 13 years. Managers are on average 50 years old and typically manage the same team for 2 years. The median tenure is below the mean and, under further inspection, shows positive skewness. Therefore, managers that spend more time than the median are likely to remain with the same club for a long time.

3.4 Empirical Setting

As a symptom of financial constraints, I am interested in the effect of cash on investment. If a firm is unconstrained, cash should not affect investment decisions. This study also seeks to test whether credit constraints change with managerial experience. The model is as follows:

$$Investment_{it} = \alpha + \rho Cash_{it} + \boldsymbol{x}_{it}\boldsymbol{\beta} + \varepsilon_{it}.$$

ρ is the coefficient of interest. Total cash refers to awards obtained before the relevant transfer window. Two measures of investment in new players are used. First, gross investment is measured as the total amount spent on new player contracts during the winter transfer window; second, net investment is measured as the difference between the total expenditure on new player contracts and the income from contracts sold during the winter window.$\boldsymbol{x}_{it}$ represents potentially unobservable characteristics that are correlated to cash.

A simple regression of investment on cash is likely to suffer from endogeneity. First, unobservables that affect investment may be also correlated with cash. Some examples are the quality of clubs' youth academies, manager quality, and the popularity of the team. Unobservables cause omitted variable bias in a standard ordinary least squares (OLS) regression. Second, both cash and investment are determined in the same economic model, so there may be a problem of simultaneity. Common shocks to cash and investment may lead to spurious correlation. Also, if a team anticipates certain cash flows, then it can plan its future investments. This leads to a reverse causality problem.

The ideal experiment would consist of randomly allocating cash to teams with different types of managers. This study is a close approximate of the ideal experiment. I find a source of exogenous variation that affects investment only through cash flows, and I use it as my instrument. This instrument is cash generated in close matches at the group stage

[53] I only consider appointments of full managers at the professional level. I do not consider appointments in smaller roles like coaches or scouts.

of the European Champions League. This section demonstrates that the instrumental variable (IV) estimation captures the LATE of cash on investment.

Ex-ante odds are used to determine close games, which are defined as those in which the difference between the probability of winning and the probability of losing is less than 0.05 in absolute value. Maximum uncertainty about the outcome exists for these matches. Thus, any cash flow generated under these circumstances is as good as randomly assigned.

In theory, odds have predictive power. As the home team's probability of winning increases in relation to the other team, we should observe a higher proportion of actual wins. We observe this pattern, except around a difference of zero. Figure 1 illustrates how the actual predictability of odds become flatter around the point at which the difference in probability of the home team winning or losing is zero.

The second evidence of randomness in close matches is even more powerful. The team with the highest probability of winning is defined as the favorite. If probabilities are indicative of the likelihood of a team winning, then, on average, the proportion of games the favorite wins should be significantly higher than its ties or losses. I show this is true for the unrestricted sample of games. However, when I restrict the sample so that both teams have an almost equal probability of winning, differences between the proportions disappear. In other words, the three outcomes are equally likely.

To test whether results are random, I first look at the favorites before an specific match. I then look at whether the favorite won, tied, or lost. In Table 3.4, I show the proportion of each result that is realized. In the unconditional sample, the proportion of times the favorite wins is higher than the number of ties and so on. However, the sample of close games generates realizations that are indistinguishable from one-third. That means results in close matches are evenly split.

In Table 3.5, I test how significant the differences between proportions are, using a one-tailed test under the hypothesis that the better outcome is more likely for the favorite team. For the full sample, the difference is significant at the 1% level for the favorite winning versus tying, for the favorite winning versus losing, and for the favorite tying versus losing. However, if the sample is restricted to close games, there are no significant differences between the proportions of any two results (see Table 3.5).

I also apply a chi-squared test under the null hypothesis that match outcomes from a discrete uniform distribution, where each event is equally likely. If all events are equally likely, then there is maximum uncertainty. As expected, for the unrestricted sample, we cannot accept the null hypothesis that the distribution is a discrete uniform distribution with equal probabilities of each event. On the contrary, for the sample of close matches, we cannot reject the null hypothesis that the outcome behaves as a draw from a discrete uniform distribution.[54] As a consequence, any result from a close game can be interpreted

[54]The results are summarised in the Appendix Table 3.12

as random because any event is equally likely.

A potential criticism of my instrument is that more total cash awards in close games can mask better performance at the team level. Therefore, total cash awards would be correlated with unobservable drivers of quality. This would make my instrument not random, and, therefore invalid. I cannot show explicitly this is not the case, but I can provide indicative evidence in favor of my assumption that cash awards from close games are indeed random. If we believe that close wins are correlated with quality, we should observe persistently better performance for winners and worse performance for losers. However, no team wins more than one close match in a season. Moreover, out of my sample of 183 teams facing close matches, only 6 experience more than one close loss in a particular season. The results are robust to the exclusion of these teams.

Additionally, I forecast future random awards based on past team observable characteristics. We can see in the Appendix (Table 3.13) that the main variables of interest related to team past performance, managerial experience, and player contract trades do not significantly explain cash awards in close games.

If we assume the treatment is randomly assigned, then there are still requirements for the exclusion restriction to be satisfied. The treatment affects investment only through a change in cash. This assumption is stronger. Some potential violations include if winning one particular close match changes the values of player contracts, alters the public perceptions of a team, alters the teams business model, or changes the manager. In any of these cases, the instrument would not be valid.[55]

The exclusion restriction is the most problematic assumption in my setting. However, it is likely that it is still satisfied for a few reasons. First, the cash awards are not large enough to change the whole structure of the team. Second, a good result in one game in the group stage does not guarantee that a team will advance in the tournament. It is also unlikely to trigger a managerial change. In the data, a close match win does not change the probability of a manager staying or leaving a team. Random close wins are also unlikely to change the values of player contracts because a small sample of particular matches are not enough to accurately assess player qualities. Players potentially play more than 50 games per season. Teams in this sample are exposed to four close matches, at most, in the group stage.

Even if a team advances rounds just after winning a close match, players acquired in the winter window can be used in the domestic league, but not in the Champions League. Furthermore, the tournaments in which a team plays are pre-determined at the beginning of the season, so it is unlikely that cash obtained in close matches brings new investment opportunities.

Another variation of the previous critique of my instrument is the following: there

[55]The reduced-form result would still have a causal interpretation though.

could be complementarities between the winning of close matches and more matches available in the season. This would mean that managers may be forced to acquire new players to keep performance at the same level. Therefore, no significant change in performance would not imply that managers are spending resources on unnecessary player contracts. For example, a team that has to play more games as a consequence of winning close matches might need to acquire players just to maintain its performance. Although this is a valid concern, I provide evidence that this is not the main driver of the results of this paper. To test this I exploit an extra source of variation, by using a sample of "unlucky winners". These are teams that obtain more money in close matches in the groups, but do not advance the stage. The results are even stronger in this sample.

To satisfy the LATE theorem, monotonicity is needed in the treatment. In this setting monotonicity is trivially satisfied because, ceteris paribus, winning a close match increases the total cash the team makes in the tournament.

Finally, a first stage is needed for a valid instrumental variable estimation. A strong first stage exists, and the signs are as expected. Because the first stage is also an interesting result in and of itself, it is further discussed in the next section.

3.5 Results

As discussed by Bound, Jaeger, and Baker (1995), a weak instrument leads to bias in the IV estimation. Also, to avoid bias from multiple instruments, my model is exactly identified. The requirements in the first stage are stronger than those in the simple t-statistic tests. I first show that cash obtained from random awards is a strong positive predictor of the total cash obtained by a team in the group stage. As evident in Table 3.6, £1 million from close games translates into £2.8 million in total awards. This number is big, because the position within a group also affects the total cash paid by the tournament organizers. Thus, winning a close match also generates a higher total group payment at the end. To test the strength of the instrument, I verify the F-statistic of the regression on the excluded instrument, that is, the random awards. It is well above the $F = 10$ threshold that Stock, Wright, and Yogo (2002) suggest.

I divide the sample into two groups by managerial experience. If the manager has less experience than the median manager in the whole sample, that manager is defined as ”inexperienced”. Otherwise, a manager is defined as experienced. Importantly, the correlation between actual manager experience and random awards is 0.018. Therefore, the instrument is not correlated with the way the sample is broken down. As seen in Table 3.7, the first-stage results of close games on total awards are qualitatively and quantitatively similar in both sub-samples.

Like in most papers that rely on estimations around a sufficiently small window, the number of observations is limited. Therefore, it is difficult to estimate precisely the effect of cash on investment. Table 3.7 shows that, although the sign is positive and the number is statistically meaningful, the effect of cash on new player acquisitions is not significant in the unrestricted sample. However, when I separate the teams by the manager's experience, I find that inexperienced managers significantly increase their expenditures in the winter market. The experienced ones do not. For inexperienced managers, one extra pound of cash translates into an increase of £1.1 used to acquire new players.

For net player contract investments, the results are qualitatively similar. Table 3.8 shows that for every pound inexperienced managers obtain in cash, they increase their net expenditure in player acquisitions by £0.61. Although this result is not statistically significant, it is economically meaningful. For experienced managers, the net investment is close to zero.

My next question is whether these new player acquisitions translate into better performance for the team. My measure for performance is the average points earned in the local league, which is independent of the performance in the Champion's League. Furthermore, new players can be used in the domestic league regardless of whether they played for other teams in the Champion's League. A regression of cash on average points in the league is difficult to interpret. Therefore, all variables are standardized, so as to interpret the coefficients in terms of standard deviations. No evidence of an improvement in performance is found. As reported in Table 3.9, when a regression of random awards on points per game is run, is the sign is indistinguishable from zero, even though it is positive for inexperienced managers. Thus, it seems that new players are not improving the overall performance of the team.

There is a potential criticism of my identification. If close match wins significantly change team investment opportunities differently for each type of manager, the result may be driven by complementarities between previous close wins and the necessity to acquire new players to keep performance at the same level.

I restrict to a sample of unlucky winners. These are teams that won random cash awards, but that failed to clear the stage and pass to the next round. This regression has less power because the database decreases substantially. Nonetheless, the main results persist. As we can see in Table 3.10 when we restrict the sample to teams that did not proceed stage, we observe a significant investment sensitivity to cash flow overall. The magnitude is larger for teams with inexperienced managers; although not significant. This lack of significance is influenced by an increase in standard errors and an important decrease in the sample size. In terms of the point estimates, the results are robust in this subsample. Therefore, it is unlikely that results are driven by the fact that certain managers get better opportunities after winning a close match.

3.6 Conclusions

In this paper I have identified a causal relationship between investment and cash. I relate this investment sensitivity to managerial quality, showing that inexperienced managers significantly increase the amount of money spent on player contracts after football teams receive unanticipated cash flows. Therefore, it seems that investment sensitivity to cash can be linked to managerial quality. For inexperienced managers, a £1 increase in cash translates into a £1.1 increase in gross investment in new players and a £0.61 increase in net investment in new players.

Another important contribution of this paper concerns how I identify the relationship between cash and investment. I use a quasi-experimental setting in European football by exploiting two important features of the sport. First, players are traded publicly in the market within well-defined transfer windows. Second, when teams participate in international competitions, they are awarded money based on the results of a single match. Betting market information is used to calculate the implicit odds of each result and show that close matches are unpredictable. The money obtained in these close matches is used as an instrument for the total cash award. I then separate teams by their managers experience, which appears to be uncorrelated with the results from the close matches. This setting allows me to test whether managerial quality is related to financial constraints. Indeed, it is.

Surprisingly, an increase in player acquisition does not improve a teams overall performance. My interpretation is that managers use cash flows to increase their own benefits in a way that is not necessarily optimal for the team.

These results suggest that hiring inexperienced managers may exacerbate financial constraints. Nonetheless, these constraints may arise because of potential agency conflicts. Manager inexperience can be related to information asymmetries or to agency cost of free cash flow. My results show that performance does not improve after the investment is done. Suggesting that agency cost of free cash flow are a more likely explanation.

3.7 References

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3.8 Tables

Table 3.1: Statistical Summary of Odds Data

	Win at Home	Tie at Home	Lose at Home
Mean	0.4770	0.2434	0.2796
SD	0.2076	0.0554	0.1793
Median	0.4724	0.2617	0.2440

Note: This table summarizes the implied odds of all games played in the Champions League from season 2002/2003 until 2013/2014.

Table 3.2: Statistical Summary of Player Transfers Data

	All Teams		*Teams Exposed to Close Matches*			
	Selling	Buying	Selling	Buying	Winter Sales	Winter Buys
Mean	£5.06	£6.41	£24.13	£16.82	£2.65	£4.41
SD	£11.56	£16.71	£16.72	£18.72	£0.48	£9.96
Median	£0.308	£0.1625	£15.93	£10.3	£5.16	£0.83

Notes: This table summarizes the value of players sold and bought by football teams. Winter refers to the player trading window that occurs in the middle of the season. Teams exposed to close matches are those with at least one match in the Champions League during the group stage in which the difference between their *ex-ante* probability of winning versus the probability of losing is less than 5% in absolute value.

Table 3.3: Statistical Summary of Manager Characteristics

	Experience (yr)	Age (yr)	Tenure (yr)
Mean	13	49.6	2
SD	8.6	7.9	3.4
Median	11.7	49	1.13

Notes: This table summarizes manager characteristics. Experience is defined as the years from the managers first appointment by a professional team until the beginning of the relevant season. Tenure is defined as the years that a manager has been managing a team at the beginning of the relevant season.

Table 3.4: Proportion of times each result is observed given the odds

	Favorite Wins	Favorite Ties	Favorite Loses
Full Sample	0.5974	0.2253	0.1773
Close Games (Less 5% Diff. in Odds)	0.3387	0.3065	0.3548

Notes: This table summarizes the proportion of times the favorite (the team with the highest probability of winning) gets each of the three possible realizations: a win, a tie, or a loss.

Table 3.5: p-Values of One-Sided t-Test of Proportion Differences

	Wins versus Tie	Tie versus Lose	Win versus Lose
Full Sample	2.02×10^{-87}***	1.69×10^{-87}***	1.68×10^{-87}***
Close Games (Less than a 5% Diff. in Odds)	0.2920	0.2910	0.1677

Note: This table summarizes the results from a one-tailed hypothesis test assuming better results are more likely for the favorite team.

Table 3.6: First-Stage results

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$Cash_{it}$	$Cash_{it}$	$Cash_{it}$
$RandomAwards_{it}$	2.76***	2.95***	2.57***
	(0.51)	(0.60)	(0.72)
Observations	143	71	72
R^2	0.17	0.19	0.14
F	29.57	23.92	13.08
# Teams	64	44	43

Notes: $Cash_{it}$ is the total cash generated by a team in the UEFA Champions League until the winter break. After the winter break, the relevant player transfer market opens. $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage.

Table 3.7: Money Spent on New Football Player Acquisitions

Panel A: Reduced-Form Regressions

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$WinterBuys_{it}$	$WinterBuys_{it}$	$WinterBuys_{it}$
$Cash_{it}$	0.50	3.25*	-1.89
	(4.24)	(1.81)	(4.1)
Observations	143	71	72
R^2	0.0003	0.0279	0.0028
F	0.05	3.22	0.21
# Teams	64	44	43

Panel B: Final Regressions

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$WinterBuys_{it}$	$WinterBuys_{it}$	$WinterBuys_{it}$
$RandomAwards_{it}$	0.18	1.1*	-0.74
	(0.77)	(0.61)	(1.64)
Observations	143	71	72
R^2	0.0164	0.19	0.14
# Teams	64	44	43

Notes: $Cash_{it}$ is the total cash generated by a team in the UEFA Champions League until the winter break. After the winter break, the relevant player transfer market opens. $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage. $WinterBuys_{it}$ is the amount spend on acquiring new players in the winter transfer window.

Table 3.8: Net Money Spent on Football Players

Panel A: Reduced-Form Regressions

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$WinterNet_{it}$	$WinterNet_{it}$	$WinterNet_{it}$
$Cash_{it}$	0.70	1.78	-0.17
	(2.5)	(2.69)	(4.14)
Observations	143	71	72
R^2	0.0005	0.0058	0.0000
F	0.08	0.08	0.00
# Teams	64	44	43

Panel B: Final Regressions

All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*	
	$WinterNet_{it}$	$WinterNet_{it}$	$WinterNet_{it}$
$RandomAwards_{it}$	0.25	0.60	-0.07
	(0.90)	(0.91)	(1.58)
Observations	143	71	72
R^2	0.0081	.	
# Teams	64	44	43

Notes: $Cash_{it}$ is the total cash generated by a team in the UEFA Champions League until the winter break. After the winter break, the relevant player transfer market opens. $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage. $WinterNet_{it}$ is the amount spend on acquiring new players minus the amount obtained from selling players in the winter transfer window.

Table 3.9: Points per Match on Random Awards (Standardized Regression)

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$Performance_{it}$	$Performance_{it}$	$Performance_{it}$
$RandomAwards_{it}$	-0.004	0.03	-0.04
	(0.068)	(0.1)	(0.09)
Observations	147	72	75
R^2	0.0000	0.0015	0.0022
F	0.00	0.10	0.16
# Teams	64	44	44

Notes: $Performance_{it}$ is measured as the points per match obtained by a team in a domestic competition. $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage. To improve the intuition, I standardize all variables by subtracting the mean and dividing by the standard deviation. This allows me to interpret the results in relation to the standard deviation.

Table 3.10: Sensitivity of Investment to Cash "Unlucky Winners"

Panel A: IV Regressions Winter Buys

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
	$WinterBuy_{it}$	$WinterBuy_{it}$	$WinterBuy_{it}$
$Cash_{it}$	2.88**	4.4	2.61***
	(1.35)	(6.17)	(0.89)
Observations	68	28	39
Wald	4.53	0.51	8.68
# Teams	43	23	28

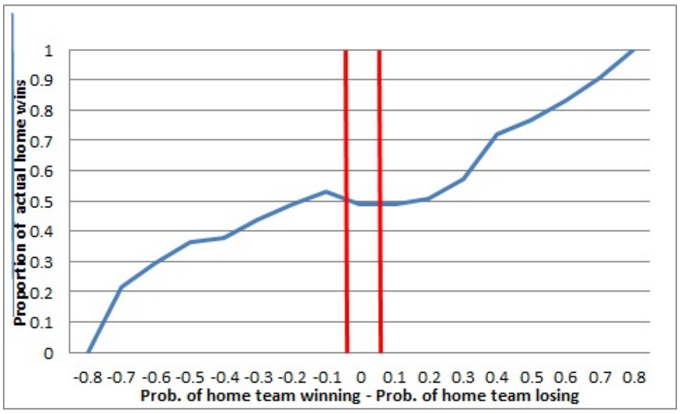
Panel B: IV Regressions Winter Net Investments

All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*	
	$WinterNet_{it}$	$WinterNet_{it}$	$WinterNet_{it}$
$RandomAwards_{it}$	3.13*	4.23	3.04**
	(1.73)	(6.89)	(1.28)
Observations	68	28	39
Wald	3.31	0.38	5.64
# Teams	43	23	28
Instrument		Random Awards	

Notes: $Cash_{it}$ is the total cash generated by a team in the UEFA Champions League until the winter break. After the winter break, the relevant player transfer market opens. $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage. $WinterNet_{it}$ is the amount spent on acquiring new players minus the amount obtained from selling players in the winter transfer window. $WinterBuys_{it}$ is the amount spent on acquiring new players during the winter transfer window (just after the award is received).

3.9 Figures

Figure 3.10: Home Wins as a Proportion of Games in which a Team Won



Notes: In this graph only games in which there was a winner (no ties) are considered. The y-axis shows the proportion of wins by the home team divided by games with no ties. The x-axis shows the difference between the ex-ante odds of the home team winning minus the ex-ante odds of the home team losing. Around zero, predictability becomes low.

Appendix

Explanation of International European football tournaments

The format of European football tournaments from 2003 until 2014 has consisted of two games against each rival: one at home and one away. To enter the competition, each team must finish in the top places of the domestic league or cup. Not all leagues get a direct spot for their best teams. Depending on the performance of previous teams, each domestic league is directly assigned spots (i.e., qualification is not required), and/or indirect spots. Teams that enter the indirect spots need to undertake qualification rounds against teams from other leagues in the same situation. Teams knockout each other in one to one series until all available spots (32 teams) are filled. To qualify, a team must get a better aggregate goal average (i.e., goals scored minus goals received) in both games; in the case of a tie in the goal average, then the team that scores more goals away advances, and, if there is still a tie, one team must win in extra time or in penalty kicks.

After qualification, the teams enter a group stage in which the best two teams advance to the next round. The best two teams are defined as those with the most points in a group. A win is worth 3 points, a tie is worth 1 point, and a loss is worth 0 points. In the case of a tie in points, the following criteria are applied to determine which team advances:

(a) The team with the best goal average (difference between goals scored and goals received) among teams that gathered the same number of points in their group matches. If this criterion does not generate a tie break move to the next criterion.

(b) Among teams with the same number of points and goal average, the team with the most goals scored advances. If this criterion does not generate a tie break move to the next criterion.

(c) Among teams wuth the same number of points, goal average and goals scored, the team with the most goals scored away (in rival stadiums) advances. If this criterion does not generate a tie break move to the next criterion.

(d) Repeat the previous steps for statistics against all the teams (instead of teams with the same number of points) in the group.

The following rounds have the same rules as the qualification stage, and they continue until only one team prevails. In the round of 16, teams are randomly matched conditional on having always a best of a group with a second best. From there, the best team after the two matches advances. The last two surviving teams play a single match for the final.

UEFA Champions League average awards over the sample

Table 3.11: Statistical Summary of Awards Data

	Awards (Millions GBP)
Fixed Amounts Groups	5.4
Wins in Groups	0.57
Ties in Groups	0.28
Round 16	2.18
Quarter-finalist	2.46
Semi-finalist	3.08
Runner-up	4.18
Champion	6.96

Note: This table summarizes the awards in the Champions League from season 2002/2003 until 2013/2014.

Table 3.12: p-Value of Chi-Squared Test

Full Sample	$3.81\times10^{-147***}$
Close Games (Less than a 5% Diff. in Odds)	0.7127

Note: This table tests the hypothesis of whether the proportion of observed results is the same as a discrete uniform distribution in which each event is equally likely.

Table 3.13: Forecasting Random Awards using Observables

	All Teams	*Teams with Inexperienced Managers*	*Teams with Experienced Managers*
		$RandomAwards_{it}$	
$ManagerExperience_{it}$	0.001	0.01	0.002
	(0.003)	(0.01)	(0.008)
$Points_{it-1}$	-0.11	-0.15	-0.07
	(0.09)	(0.15)	(0.14)
$Buys_{it-1}$	0.001	0.001	0.002
	(0.001)	(0.002)	(0.002)
$Sells_{it-1}$	0.002	0.0005	0.002
	(0.002)	(0.002)	(0.003)
Observations	141	70	71
R^2	0.04	0.05	0.05
F	1.98	0.85	1.98
# Teams	63	43	43

Notes: $RandomAwards_{it}$ is the cash generated by a team during a close match in the group stage. $Manager_Experienceit$ is the number of years since a managers first professional appointment and the beginning of the current season. $Points_{it-1}$ is the points per game in the domestic league in the previous season (a measure of performance). $Buysit-1$ is the money spent in the acquisition of player contracts in the previous season. $Sellsit-1$ is the money obtained from the sale of player contracts in the previous season.