The London School of Economics and Political Science

Essays in Corporate Finance

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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 declare that my thesis consists of 61,120 words.

Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with Dr Juanita González-Uribe and I contributed 50% of this work.

Abstract

This thesis explores the different mechanisms behind the heterogeneous success and growth rates of small and medium-sized enterprises.

In the first chapter, I investigate the impact of manager turnover on the performance of young firms and examine how this impact varies with different ownership and control structures. To estimate the causal impact of manager departures, I exploit shocks to managers' outside options induced by an area-based policy in support of local businesses and compare firms with different ex-ante manager departure likelihood and different distance to policy regions. Using a novel manager-shareholder matched dataset of young firms in the UK, I show that manager departure has a significant negative causal impact on the subsequent performance of young firms. Moreover, manager departure leads to a substantial decline in the assets of founder-managed firms and a significant increase in bankruptcy risks of non-founder-managed firms.

The second chapter is co-authored with Dr. Juanita González-Uribe. We use the Great Recession as a laboratory to dissect the implications of financial constraints in small firms. We exploit firm-level eligibility requirements for a credit guarantee scheme launched in the UK in 2009 as an exogenous determinant of financial access during the crisis. Using a difference-in-difference methodology, and novel small-firm data, we show that eligible firms relatively increased their borrowing, employment, sales, profits, and survival, but disinvested as much as non-eligible businesses. The results show that employment can be more sensitive to financial constraints than fixed assets, likely because fixed assets can be pledged as collateral whereas employees cannot.

In the last chapter, I examine how the succession decisions in family firms are affected by the product market competition based on a sample of UK family firms in the manufacturing sector. Using import penetration to measure foreign competition and implementing the import-weighted exchange rate index as an instrumental variable, I find that intense foreign competition causes a significant increase in family manager departures. Specifically, departing family managers are unlikely to be replaced, whereas departing unrelated managers are more often replaced by unrelated managers.

Contents

List of Tables

List of Figures

Chapter 1

Young Firm Manager Turnover and Performance

Over the past two decades, a wide range of studies have focused on CEO successions. However, most evidence comes from examining large and public firms. Little is known about manager turnover in private firms, especially in young firms, where the first successions have taken place.[1] In addition to representing a predominant proportion of the economy, private young firms differ remarkably from large mature firms along several dimensions. These differences generate variations in hypothesized influences of manager turnover on firm prospects and prevent us from extrapolating existing evidence from mature firms to young firms.

Based on a novel and comprehensive dataset of young firms in the United Kingdom, this paper analyses the implications of manager turnover on the performance of young firms to shed light on the trade-off between resolving manager-firm mismatches and the costly replacement frictions. In addition, by comparing founder and non-founder-managed firms, this paper delivers insights into how this trade-off varies when ownership and control are aligned.

For young firms, the benefits and costs associated with manager turnover are different from those of mature firms. On the one hand, shareholders can use the

[1]See, for example, the family firm successions highlighted in Pérez-González (2006), Villalonga and Amit (2006), Bennedsen et al. (2007), and Tsoutsoura (2015). Bennedsen et al. (2010, 2011) examine the CEO's value in private firms. Gao et al. (2017) focus on large private firms in the United States. A few papers investigate founder replacements in venture capitalist (VC)-backed start-ups: Wasserman (2003), Hellmann and Puri (2002), Kaplan et al. (2009), and Ewens and Marx (2018).

threat of replacement to mitigate agency problems (Jensen and Meckling, 1976). CEO turnover as a governance mechanism is more important for mature firms, where ownership and control are typically separate (Berle and Means, 1932).[2] Moreover, the necessary managerial inputs may change as a firm grows. Turnover allows firms to match with managers with appropriate skills. This allocative role of turnover is likely to be more salient for young, fast-growing firms.[3] On the other hand, the friction to replace departing managers appears to be larger in young firms compared with that in mature firms. With fewer inside candidates, small young firms are more likely to turn to outside managers (Helmich, 1977; Dalton and Kesner, 1983). The potentially limited access to professional managers creates more friction in assigning new managers.[4] In addition, whereas general managerial talents are more valuable in large public firms (Custódio et al., 2013, 2017), young firms may require firm-specific managerial skills that are more difficult to replace. The impact of manager turnover on the young firm's performance therefore depends on the benefits of resolving the manager-firm mismatches and the cost of replacement frictions.

The comprehensive information of young firms provides a unique setting to understand how this trade-off varies across different ownership structures, that is, between founder and non-founder-managed firms. First, replacing the founder-manager brings more benefits to founder-managed firms. The controlling owner has no incentive to monitor herself (Bennedsen and Nielsen, 2010), while enjoying the private benefits of control in exchange for profits (Stulz, 1988). In addition, founders are likely to be less capable of managing a fast-growing firm than professional managers, as suggested by the fact that VCs frequently replace founders (Hellmann and Puri, 2002). Second, the cost of turnover friction tends to be higher in founder-managed firms. Founder managers may be reluctant to hire a profession-

[2]This benefit does not necessarily apply to young firms, especially in founder-managed firms, where ownership and control are perfectly aligned at incorporation.

[3]In the competitive and frictionless model, exogenous manager departures of efficiently matched managers can not improve firm performance. For fast-growing young firms, managerial skills of founders are not necessary be optimal for firm growth perspectives. For example, Hellmann and Puri (2002) show that VC firms improve firm value by replacing founder managers with professional managers. Bennett et al. (2016) find that founder-managed firms under perform compared with non-founder-managed firms.

[4]Indeed, the possible high friction of assigning new managers to young firms is supported by my finding of a replacement likelihood of 37.74% conditional on departures using a multi-cohort sample of young firms in the United Kingdom. However, the low replacement rate also may reflect young firm's dim prospects.

al manager or, even, to make themselves replaceable (Rajan, 2012) because they do not want to relinquish the private benefits of control.[5] Moreover, the transition from owner-manager to professional manager generates agency costs from separated ownership and control. Overall, the differential impact of manager turnover on firm performance between founder and non-founder-managed firms depends on the variations in both benefits and costs associated with manager turnover, and therefore is an empirical question.

To empirically investigate the causal impact of manager departure, I construct a novel dataset that covers the population of new firms established from 2005 to 2010 in the United Kingdom. Crucial to the purpose of this paper, I match ownership information with manager appointment and resignation histories from BvD database. The unique ID provided for each manager allows me to follow the mobility of managers over time. I also link manager-shareholder matched dataset with firm financial information, which is usually opaque in young firms.[6] The comprehensive dataset provides an opportunity to examine manager turnover in young firms and to explore the heterogeneity along various dimensions.

I start this paper by providing three stylised facts to fill in the knowledge gap of manager turnover in young firms. First, as firms grow, average manager departure gradually declines and average appointment presents a U-shaped pattern. Manager turnover in founder-managed firms is less frequent and the dynamics over time is more stable compared to non-founder-managed firms. The bankruptcy hazard rate—the fraction of surviving firms going bankrupt every year—decreases with time, and both founder and non-founder-managed firms have similar patterns of this rate (see Figure 1.2). Second, a significant positive association exists between the number of manager departures and the firm's future bankruptcy risks. On average, the number of manager appointments positively correlates with the firm's future asset growth. Third, average manager turnover in other firms that operate

[5]The process of standardization would dissipate the rents that the founders enjoy when controlling the firm. Hurst and Pugsley (2011) find that almost half of the surveyed new firm owners report non-pecuniary benefits as motivation for starting their business. Bennett and Chatterji (2017) report similar evidence.

[6]Young firm balance sheet information is usually not public and hard to obtain. In the UK, small companies are only required to report abbreviated balance sheet accounts, which include detailed information on assets, capital and shareholder funds. Only large companies are required to file detailed financial statements.

in the same industry also predicts an individual firm's prospects. This correlation remains robust when contemporary individual firm's manager turnover, financial information, and industry and local economic shocks are controlled for, suggesting that manager turnover contains aggregate industry information that also affects an individual firm's future performance.

As suggested from the last stylized fact, both manager departure and firm performance could be driven by unobservable shocks, such as weakening product market demands. Therefore, whether manager departure directly affects a firm's performance remains unclear. If the benefit from resolving manager-firm mismatches is larger than the cost associated with replacement frictions [7], then manager departure should lead to better firm performance. When the incumbent manager has (firm-specific) human capital, which is crucial for survival and growth but difficult to replace, then we would expect a negative causal impact of manager departure on a young firm's performance.

To identify the causal impact of manager departures, I examine positive shocks to managers' outside options induced by the 2006 UK Local Enterprise Growth Initiative (LEGI). LEGI is an area-based government program that provides financial support and mentoring advice to local businesses. Einio and Overman (2016) have shown that LEGI had a negative local displacement effect on jobs in untreated areas close to policy regions. In this paper, I focus on firms located outside of policy regions and with different distances to policy regions (*close* vs. *far* firms), both before and after the policy. First, I validate that LEGI indeed generates variation in manager departures. [8] My findings show that *close* firms have significantly more manager departures and have more managers moving to policy regions compared with *far* firms after the policy.[9] To disentangle the causal impact from other policy spillover effects, I divide firms into two groups—with high/low ex-ante departure

[7]Either because the managerial skill did not match the firm's growth perspective or because the incumbent manager was extracting too much private benefit from control (i.e. the value added is less than managers' extraction.).

[8]LEGI does not change the number of new appointments significantly different between firms that are located outside but with different distances to the LEGI policy regions, this corresponds to the baseline summary statistics shown in Table 1.1 that only 37% of firms find a replacement of managers conditional on manager departures.

[9]Managers moving to policy regions are based on sub-samples for which I can identify the subsequent managerial appointments. I match manager IDs using all the managerial information of UK private firms to follow managers mobility.

probabilities. I show that within low ex-ante departure probability group (counter-factual group), manager turnover is not differentially affected by the policy in *close* relative to that in *far* firms. Therefore, any performance differences between *close* and *far* firms in the counter-factual group are from other policy spillovers. The triple difference approach, which compares firms with high departure probabilities to those with low departure probabilities, located *close* and *far* to the policy and both before and after the policy, thus identifies the causal impact of manager departures.

Estimates from the triple difference analysis reveal a negative causal impact of managerial departure on a young firm's performance. Within high departure probability group, the after policy change of bankruptcy risk (assets) gap between *close* and *far* firms is significantly more positive (negative) than that difference within the low departure probability group. The negative causal impact of manager departure on young firm performance is also large in economic terms: Wald estimations show that, on average, one manager departure increases the risk of bankruptcy by six-fold. In surviving firms, total assets decrease by £2.05M annually in the following two years, which is 70% of the average firm size pre-policy . One interpretation of the negative causal impact is that the friction of assigning the new best manager is substantially costlier than the possible mismatch between the incumbent manager and firm growth prospects. One source of friction is the possibility that young firms have fewer inside candidates and a limited access to a large manager pool. The friction also can be attributed to firm-specific human capital, which is difficult to replace even when a large supply of general managerial talents is available.

The final part of this paper examines whether manager departures have a heterogeneous causal impact across firms with distinct ownership and control structures. I conduct my analysis based on sub-samples of founder-managed and non-founder-managed firms. The results show an asymmetric negative causal impact on firm survival and growth. I find a negative causal impact of manager departure on survival in non-founder-managed firms. In contrast, though assets sharply decline in surviving founder-managed firms, the bankruptcy risks do not change significantly. One interpretation of these findings is that founders' private benefits of control prohibit them from closing down firms (Jensen and Meckling, 1976; Morck et al., 1988). The findings are also consistent with founder forming "psychological bonds" with

their ventures so that they are reluctant to shut down inefficient business (O'Reilly and Chatman, 1986; Wasserman, 2003).

The results in this paper stand up to a range of robustness tests. To alleviate concerns from sample truncation and censoring, I run regressions with Cox duration models and find qualitatively similar correlations shown in stylized facts. The identification relies on the assumption that managers in nearby firms are more likely to be attracted to policy areas, because they share the same local product market or local hiring networks (e.g., Topa (2001); Ioannides and Loury (2004)). I also verify the "parallel-trends" assumption by checking the dynamic policy effects. The negative causal impact of manager departures remains robust to alternative classifications of *close* firms using randomly selected distance thresholds. Lastly, I find qualitatively similar results when controlling for firm fixed effects.

This paper contributes to the literature in several ways. First, it adds to a growing body of studies on founder replacements in entrepreneurial firms. Evidence from the existing literature is largely based on small samples of venture-capital-backed firms (Hellmann and Puri, 2002; Wasserman, 2003; Kaplan et al., 2009; Ewens and Marx, 2018).[10] This paper, to my knowledge, provides the first large-scale evidence of manager turnover using the population of multiple cohorts of newly founded firms. My sample includes 336,000 new firms and 762,999 managers. In addition, I extend firm performance measures from successful exit outcomes (IPO or M&A) to bankruptcy status and asset growth rates. A recent study close to this paper is Chen and Thompson (2015), who use a Danish registry dataset with 4,172 new firms. They find that, although firms undergoing founder replacements are more likely to go bankrupt, the surviving ones have a greater potential for higher growth. However, the causal impact is not examined in Chen and Thompson (2015).

Second, this paper contributes to the literature by studying the causal effects of manager turnover on firm performance. A related set of literature exploits CEO

[10]Hellmann and Puri (2002) collect survey data of 170 start-ups in Silicon Valley and shows that VC-backed firms are more likely to replace founder managers with professional managers. Wasserman (2003) documents a "founder paradox" using a survey sample of 200 companies in the United States. He shows that founders are more likely to be immediately replaced after the venture achieves a particular milestone. Kaplan et al. (2009) find a positive correlation between founder replacement and subsequent initial public offering (IPO) events by analysing 50 VC-backed start-ups. Ewens and Marx (2018) examine the role of VCs in founder replacement using over 22,000 VC-funded start-ups in the United States.

deaths or hospitalisations to overcome the challenge of identification (Bennedsen et al., 2010, 2011; Jenter et al., 2016). Nevertheless, CEO deaths and hospitalisations are rare events and are more likely to happen among elderly managers in mature firms.[11] Another concern is that the negative performance found in previous studies may come from lower productivities of grieving employees rather than from exogenous CEO departures. Ewens and Marx (2018) exploit the non-compete laws that change the supply of potential managers as replacements. They show that founder replacements have a positive causal impact on subsequent exit outcomes. This paper looks at a new source of variation in manager departures by focusing on changes in managers' outside options. This source of variation is more common in competitive labour markets and has a wider impact on managers.[12]

This paper also relates to the literature comparing performance and management practices across founder CEO firms and non-founder CEO firms (Adams et al., 2009; Fahlenbrach, 2009; Bennett et al., 2016). The stylized fact of infrequent manager changes and stable dynamics of manager turnover in founder-managed firms is related to the discussion about the "founder's paradox" (Wasserman, 2003)[13] and the standardization of founders' human capital (Rajan, 2012). Furthermore, this paper adds additional heterogeneous causal evidence of manager departures on firm performance between founder and non-founder-managed firms. This result reflects the underlying distinct ownership and control structure, which is emphasised as an important factor to explain the remarkable differences in management styles and philosophies (Mullins and Schoar, 2016).

Lastly, this paper provides a timely and available early predictor of the performance of young firms. Entrepreneurial characteristics, previous experience , financial constraints, and the geographical location at incorporation have been shown to be important to a new venture's performance (Gompers et al., 2010; Lafontaine and Shaw, 2016; Kerr and Nanda, 2009). Two advantages make managerial turnover a

[11]The average age of a departing manager is approximately 60 years old, according to the previous literature. In contrast, the average age of a manager is 41 years old.

[12]Jenter et al. (2016) examine 458 CEO deaths using U.S. public firm samples, and Bennedsen et al. (2010) use 1,053 CEO deaths and 5,738 deaths of CEOs' relatives. Bennedsen et al. (2011) compare 3,167 firms with hospitalised CEOs to a comparable control group comprising 5,631 firms. In my paper, I examine a total of 8,220 manager departures across 5,168 firms from 2004 to 2009.

[13]Based on a sample of 212 U.S. start-ups, Wasserman (2003) shows that, on average, 4 of 5 CEOs are forced to step down. He finds that founders are more likely to be replaced after companies achieve "crucial milestones".

useful early indicator to both policy-makers and investors. First, manager turnover provides more timely information than annually reported financial information. Moreover, manager turnover information is more accessible than are financial accounts, which are usually opaque in small young businesses.[14]

The remainder of this paper is organised as follows. Section 3.1 describes data and sample construction; Section 1.2 provides stylized facts; Section 1.3 investigates the causal impact of manager turnover; Section 3.5 provides robustness tests and further discussions; and Section 3.6 concludes the paper.

1.1 Data

The main datasets used in this study are from Orbis and Financial Analysis Made Easy (FAME)(see Brav (2009) and Michaely and Roberts (2012); González-Uribe and Paravisini (2019)). Geographical data at Lower Layer Super Output Area (LSOA) level is collected from the UK Office for National Statistics (ONS). I briefly introduce the data in this section and more details can be found in Appendix 1.6.

Orbis is a comprehensive database covering manager and ownership information of both private and public firms worldwide. Originally extracting annual reports from the UK registry Companies House[15], Orbis keeps track of nearly 13 million companies in the UK. Within each company, every appointment is recorded with details of the manager's full name, appointment date, resignation date and type of position, as well as the manager's characteristics such as gender, birth date and nationality.

Distinct from previous studies where manager turnover is usually approximately measured, the exact dates of appointments and resignations help reduce measurement errors.[16] As managers can work at different companies, Orbis assign a Unique Contact Identifier to each manager. This is particularly important for me to track

[14]Managerial turnover is required to be reported to the business registry within 14 days of changes, whereas manager information must be reported to the registry for each registered firm. Abbreviated financial accounts filing requirements vary by firm size.

[15]Companies House incorporates and dissolves limited companies in the UK. They register firms and according to the 1967 Companies Act, all companies should file annual reports and financial statements. Although the financial statements information varies with company size, the disclosure is mandatory.

[16]For instance, Ewens and Marx (2018) show that 70% of the appointment dates of replacement executives are missing from the VentureSource dataset.

the mobility of managers when examining the causal impact of manager turnover in section 1.3. In addition, annual recorded shareholder information includes each shareholder's full name, type (individual or firm), and the percentage of shares. Matching shareholder and manager names, I classify managers who also own shares at their incorporation as founder managers and correspondingly, firms with at least one founder manager at incorporation are classified as founder-managed firms.[17]

Besides providing detailed manager and shareholder information, another advantage of this database is its good coverage of UK private firms. All firms registered in the UK are required to file annual reports with manager and shareholder information, regardless of firm age and size. Moreover, Orbis does not delete company information when they become inactive or liquidated. These two features not only avoid the survival ship bias but also refrain from sample selection problems.[18]

The FAME database contains up to ten years of historical financial accounts of firms reported to Companies House. For private UK firms, the standard filing of accounting statements varies with firm size and detailed accounting reports are only required for medium and large firms. Although profit and loss accounts and cash flow statements are poorly covered for small and young firms, this paper focuses on financial information from balance sheet accounts. I combine the archive disk with the FAME web-version data to extend the historical financial accounts information from the year 2003 to 2015. Firm registration information including incorporation date, legal forms, postcode and their industry, as well as company status, is obtained from the snapshot FAME web-version up to the end of 2017. Shareholder information is collected for new firms established between 2005 and 2010 for better coverage. This collection of management details for the population of UK private firms, ownership and financial information allows me to follow manager turnover and performance dynamics among young firms in the UK; and the richness of data offers me an opportunity to explore heterogeneity in different dimensions.

Geographical data is used for analysis in section 1.3. The National Statistics Postcode Lookup in the UK provides coordinates for all UK postcodes, and the full

[17]More details about name-matching procedures are detailed in Appendix 1.6.

[18]Prior research on private young firms' financial structures mainly relies on survey data. For example, the US Fed conducted surveys of small business finances, which included owner characteristics, firm size, income and balance sheet information. The survey ran from 1987 to 2003 and data were collected once every five years. In addition, the Kauffman Firm Survey consist of 4,928 business founded in 2004 and was studied in Robb and Robinson (2014).

clipped boundaries shape files from ONS draw boundaries at the LSOA level.[19] By combining the firms' postcodes, the geographic locations of the postcodes and the LSOA boundary shape files, I use ArcMap to geocode each firm into LSOAs and calculate the distances between LSOAs in policy regions and untreated areas.[20]

1.1.1 Main variable construction

My first measure of manager turnover is the change of manager numbers in year t: $\triangle Num_{it}$. Since both manager departures and appointments may happen without a change in the overall numbers of managers, then I decompose $\triangle Num_{it}$ into two subcomponents: the number of departures and the number of new appointments, denoted by $Num\ departure_{it}$ and $Num\ new_{it}$ respectively. I also look at the number of manager replacements ($Num\ replacement_{it}$), the number of net departures ($Net\ departure_{it}$), and the number of net new appointments ($Net\ new_{it}$).[21]

Specifically, $Num\ replacement_{it}$ is defined as the minimum number of $Num\ new_{it}$ and $Num\ departure_{it}$. The number of net departures only take positive numbers when there are more resignations than appointments: $Net\ departure_{it} = max\{Num\ departure_{it} - Num\ new_{it}, 0\}$. Similarly, net new appointments measure the net inflow of managers: $Net\ new_{it} = max\{Num\ new_{it} - Num\ departure_{it}, 0\}$. By definition, the following equality holds:

$$\begin{aligned} \triangle Num_{it} &= Num_{it} - Num_{i,t-1} \\ &= Num\ new_{it} - Num\ departure_{it} \\ &= Net\ new_{it} + Num\ replacement_{it} - Num\ departure_{it} \\ &= Num\ new_{it} - (Num\ replacement_{it} + Net\ departure_{it}) \end{aligned} \tag{1.1}$$

For firm performance, I focus on two measures: bankruptcy and asset growth.

[19] LSOAs have a minimum population of 1,000 and were built from groups of Output Areas (OA). The 32,482 LSOAs in England are constrained by the boundaries of the Standard Table wards used for the 2001 census outputs and the boundaries do not cross any LA boundaries. In the UK, a postcode is a precise location code system. Every location can be reached using just the house number and its postcode alone.

[20] Details of how to place each firm within the treated and control groups is described in Appendix 1.6. The LSOA shape files were downloaded from https://data.gov.uk/dataset/fa883558-22fb-4a1a-8529-cffdee47d500/lower-layer-super-output-area-lsoa-boundaries.

[21] By definition, for replacements, the maximum gap between appointment date and resignation date in year t is one year. In my analysis sample, the average gap between appointment date and resignation date is 76.5 days.

Dummy variable $D(Bankruptcy = 1)_{i,t+2}$ is equal to 1 if the firm status is liquidated or dissolved in year $t + 2$. The total asset growth rate of firm i from year t to $t + 2$ is calculated using change in total assets divided by the average number of total assets in year t and $t + 2$ ($Asset\ growth_{i,t+2} = 2 \times (Total\ assets_{i,t+2} - Total\ assets_{it})/(Total\ assets_{i,t+2} + Total\ assets_{it})$). Therefore, the growth rates are bounded by -2 and 2 (see Davis and Haltiwanger (1992)).

In order to examine manager turnover in firms with different ownership and control structures, I separate firms into those that are founder-managed and those that are not.[22] The manager turnover measurements defined above are calculated separately within founder and non-founder-managed firms for subsequent analysis, and a full list of variable notations with definitions can be found in Appendix A1.1.

1.1.2 Sample and descriptive statistics

For the population of private limited firms established in the UK, firm-manager-appointment level data is originally obtained from Orbis. For young firms established between 2005 and 2010, I then match the manager data with shareholder information and construct a database with manager turnover measures at the firm-year level.[23] In order to observe firm performance, I then combine the manager turnover data with financial account information from 2005 to 2015 and a snapshot of firm bankruptcy status.

Starting with the intersection of manager, ownership and financial accounts information for the universe of private new firms in the UK incorporated between 2005 and 2010, I add additional restrictions to my sample: (1) I excluded non-trading firms meaning that each firm in my sample has filed at least one year's worth of accounting information with positive gross total assets. (2) Firms belonging to business groups (either parent companies, or controlled subsidiaries), backed by VC investors or in M&A activities are excluded.[24] (3) Firms with only one manager at

[22]Founders are classified as managers who also own shares at the establishment, and correspondingly, founder-managed firms are firms with at least one founder manager at incorporation.

[23]FAME records companies incorporated as early as 1920. Manager information has good coverage after 1980.

[24]Although the Orbis database provides an independent indicator for each firm, this variable is a snapshot of up-to-date ownership information. Therefore, I use the earliest ownership information available. A firm is considered to be part of a business group if at least one of its shareholders is a corporation, or it is a shareholder of another corporation.

incorporation are dropped out of the sample. The concern of including just one-manager firms is that it is unusual to have the only manager departing the firm before filing for bankruptcy.

My final sample consists of 336,000 new firms in the UK that were established between 2005 and 2010. I follow yearly manager turnover and firm performance dynamics until 2015. Figure 1.1 plots the industry and location distributions for both the population of new firms and the analysis sample in each category. For brevity, in this figure I only show industries and locations with more than 1% of the population and analysis samples respectively. Panel A presents the industry distribution using the UK 6-digit Standard Industrial Classification of Economic Activities (6-digit 2007 SIC code). As the figure shows, the top three industries are administrative and support services (17.83%), construction (11.13%), and professional, scientific and technical activities (11.12%). In Panel B, the plot presents the location distribution at the county level. We observe that there is the highest concentration of firms in London (19.67%) followed by Lancashire (5.03%) and West Midlands (5.00%). The industry and location distributions of firms in the analysis sample presents similarly to the universal distribution of all private young firms, but with fewer firms operating in administrative and support service activities (17.83% vs. 22.32%), personal, scientific and technical activities (10.12% vs. 16.10%); more firms concentrated in manufacturing (5.10% vs. 4.51%) and real estate activities (6.78% vs. 4.09%). Geographically, a higher fraction of these firms are located in London (19.67% vs. 17.20%).

Table 1.1 presents the summary statistics over the sample period from 2005 to 2010. Panel A reports the characteristics of the full sample and Panel B compares the founder and non-founder-managed firms. The upper half of Panel A summarises the firms' characteristics and performance. The average total asset is £4.3M and the median firm has £55,074 of total assets. The skewness in firm size is precisely a reflection of the substantial divergence of young firm survival and growth. Fixed assets, on average, account for 35.73% of total assets for firms reporting this information; and total liabilities include current liabilities and long-term debts. Dividing the value of total liabilities by total assets, I calculate an average leverage ratio of 0.60 with a median of 0.67, which is comparable to the leverage documented in other

studies.[25] The average profit among young firms is £66,691, with a median value of £4,369. Bankruptcy hazard rates are calculated as the number of firms going bankrupt every year against the total number of active firms at the beginning of the year. Average bankruptcy hazard rate over the sample period is 8.82%. Conditional on active firms, the average asset growth rate is 1.45%, with a standard deviation of 59.54%. The second half of Panel A summarises manager turnover measures. By sample restriction, each firm has at least two managers to begin with, and the average number is 2.23 over the sample periods, which is also comparable to the average number of two founders shown in previous studies.[26] In a given year, the average number of departures is 0.10 and the mean of appointments is 0.06. Conditional on manager departures, the number of replacements is only 0.38.

In Panel B, I compare 281,690 founder-managed and 54,310 non-founder-managed firms. Looking across the median values, we observe that founder-managed firms and non-founder-managed firms have similar total assets. Founder-managed firms have slightly higher fixed assets to total assets ratio, and they are more levered relative to non-founder-managed firms. Moreover, bankruptcy rates and asset growths are comparable between these two types of firms. However, there appears to be a remarkable difference in manager turnovers. Founder-managed firms have much lower average departures and appointments compared to non-founder-managed firms (0.09 vs. 0.20 departures; 0.05 appointments vs. 0.16 appointments). Conditional on departures, the chance of being replaced is 0.32 in founder-managed firms while the number is 0.75 for non-founder-managed firms.

Overall, the difference between founder and non-founder-managed firms suggests that they are two different types of firms. The low manager turnover frequency in founder-managed young firms is consistent with founder managers reluctance to hire a professional manager or to make themselves to be replaceable (Rajan, 2012), either because of "tight links" with their ventures (Dobrev and Barnett, 2005) or

[25]Robb and Robinson (2014) use Kauffman's firm survey data and the average leverage is 0.64 for a US start-up sample, which includes sole proprietorships and partnerships as well as corporations. González-Uribe and Paravisini (2019) studied a sample of firms established between 2009 and 2011 in the UK without restrictions on management team size and found the equity ratio to be 0.34.

[26]Kaplan et al. (2009) use a sample of 48 venture-backed companies and reports an average of 1.9 founders. Beckman (2006) report 2.2 founders from a sample of 173 companies in Silicon Valley. Wasserman (2003) found an average of 2.2 founders from a sample of 202 start-ups, including both VC-backed and non-VC-backed companies. Details of manager characteristics in my sample, such as gender and nationality at incorporation, are included in Appendix 1.6.

because that the process of standardization would dissipate the private benefits of control. Hurst and Pugsley (2011) find that almost half of the surveyed new firm owners report nonpecuniary benefits as motivation for starting their business. Similar evidence can be found in Bennett and Chatterji (2017), suggesting costlier frictions of assigning a new manager to founder-managed firms.

1.2 Stylized Facts

I start my analysis by presenting three new stylised facts about manager turnover and firm performance in young business. The stylised facts not only fill in the knowledge gap of manager turnover dynamics in young firms but also provide backgrounds for subsequent causal analysis.

1.2.1 Dynamic manager turnover and firm bankruptcy

The first stylised fact is shown in Figure 1.2: as young firms grow, the average manager departure rate declines and average manager appointments present a U-shape pattern (Plots (a) and (b)). In particular, compared to non-founder-managed firms (Plots (c) and (d)), founder-managed firms have less frequent and more stable turnover rates over time (Plot (e) shows that bankruptcy hazard rates gradually decline with time. Moreover, founder and non-founder-managed firms have similar bankruptcy risk dynamics in as shown in Plot (f).

There are two takeaways from the first stylised fact: first, consistent with distinctions found in the summary statistics, founder and non-founder-managed firms also have different manager turnover over time. However, it is puzzling to observe both high appointment numbers and high resignation numbers when the bankruptcy rates are high in the first three years since incorporation. To better understand whether manager turnover correlates with the firm's future prospects. then I separate firms into two groups based on their second-year manager turnover decisions and follow their bankruptcy hazard rates over time. As we can see from Plot (g), firms with manager departures in Year 2 have consistently higher bankruptcy rates than those without departures, until Year 10. Correspondingly, Plot (h) shows higher bankruptcy risks in firms without second-year new appointments. This graphical

evidence suggests that manager departures (appointments) negatively (positively) correlates with the firm's future bankruptcy risk. However, the plots presented above are not controlling for idiosyncratic firm characteristics, so I examine this correlation in regression and find the second stylised fact as follows.

1.2.2 Manager turnover predicts performance within firms

The second stylised fact is that there exists a significant correlation between manager turnover and future performance in the young firm, even after accounting for firm financial information and aggregate shocks at industry and local economy levels.

Specifically, I run the following regression to examine this correlation:

$$y_{i,t+2} = \alpha + \beta x_{it} + \delta z_{i,t-1} + \gamma Num_{i,t-1} + Ind\_Loc\_YearFE + cohortFE + \epsilon_{it} \quad (1.2)$$

where i indexes the firm and t is the firm age. The dependent variable $y_{i,t+2}$ includes two outcome measurements: $D(Bankruptcy = 1)_{i,t+2}$ and $Asset\ growth_{i,t+2}$. x_{it} represents a set of independent variables including $\triangle Num_{it}$, $Num\ departure_{it}$, $Num\ new_{it}$, $Num\ replacement_{it}$, $Net\ departure_{it}$ and $Net\ new_{it}$.[27] $z_{i,t-1}$ is a vector of firm-level control variables including total assets, bank deposits, profits, and leverage. $Num_{i,t-1}$ is firm i's manager number in year $t-1$. Equation 1.2 also controls for the industry-location-year and cohort level fixed effects. The standard errors in regression 1.2 are adjusted for heteroskedasticity and are clustered at the county, cohort, and the 6-digit UK industry code level. The coefficient of interest in Equation 1.2 is β, which captures the average change in firm performance associated with manager turnover changes.

The dependent variable in the first three columns of Table 1.2 is the dummy indicator of year $t+2$ bankruptcy status. Column (1) shows a significant negative relationship between changes in manager numbers and future bankruptcy risks. In

[27] All variable definitions are listed in Appendix A1.1. $\triangle Num_{it}$ measures the change of manager numbers from the end of year $t-1$ to year t. $Num\ departure_{i,t}$ denotes the number of managers departing from firm i in year t (and either being replaced or not). $Num\ new_{it}$ is the number of new appointments in firm i year t. $Num\ replacement_{it}$, $Net\ departure_{it}$ and $Net\ new_{it}$ denote the number of replacements, the net new number of departures and the net new number of appointments, respectively. As defined in Section 3.1, $Net\ departure_{i,t}$ measures the net outflow of managers $Net\ departure_{it} = max\{Num\ departure_{it} - Num\ new_{it}, 0\}$. Similarly, $Net\ new_{it}$ measures the net inflow $Net\ new_{it} = max\{Num\ new_{it} - Num\ departure_{it}, 0\}$.

Column (2), I decompose $\triangle Num_{it}$ into the number of departures and the number of appointments, controlling for the size of the management team in the previous year. The number of departures (appointments) positively (negatively) correlates with future bankruptcy risks, and the coefficients are significant at the 1% level. In order to better characterise manager turnover, I examine the number of net departures, net appointments and replacements in Column (3). As shown in Column (3), both the number of net departures and the number of replacements are positively correlated with future bankruptcy risks, with more significant and sizeable coefficients for net departure number. Moreover, net new appointment number is negatively associated with future bankruptcy risks. Regressions in Columns (4) to (6) mirror those in the first three columns with outcome variables of asset growth rates from t to $t+2$. Looking across the coefficients, most of the signs are flipped (except for $Num\ replacement_{it}$), which is consistent with the findings in the first three columns that manager departures correlate with worse performances, and manager appointments are associated with better prospects.

In addition, I repeat the above analysis based on sub-samples of founder and non-founder-managed firms. Table 1.3 reports the results. The findings show similar correlations for both founder-managed and non-founder-managed firms as shown in Table 1.2.

Overall, the multivariate analysis presents significant within-firm correlations between manager turnover and future performance. However, the correlation can be idiosyncratic or can be confounded with unobservable shocks. For instance, industry level demand shocks may lead to both manager departures and deteriorating firm performance. To understand whether manager turnover also contains aggregate information, I use average manager turnover at the same industry and cohort level to predict individual firm performance. The stylized fact 3 summarises the findings.

1.2.3 Manager turnover predicts performance across firms

The third stylised fact is that average manager turnover in firms operating in the same industry (excluding firm i) also predicts firm i's performance. I run the fol-

lowing regression to show this stylised fact:

$$y_{i,t+2} = \alpha_2 + \beta_2 \bar{X}_{c,j-i,t} + c_2 x_{it} + \delta_2 Bankruptcy\ rate_{c,j,t} + \lambda_2 Average\ asset\ growth_{c,j,t} + \gamma_2 z_{i,t-1} + Num_{i,t-1} + Ind\_loc\_FE + Loc\_year\_FE + Cohort\_FE + \epsilon_{it} \tag{1.3}$$

where j and c denote industry and cohort, and i is the firm index. $\bar{X}_{c,j-i,t}$ are the same cohort, industry level average number of manager departure and appointment, excluding firm i.[28] x_{it} and $z_{i,t-1}$ are manager turnover and financial controls for firm i, the same as in Equation 1.2. I further control for the same industry-cohort bankruptcy rate $Bankruptcy\ rate_{c,j,t}$ and average asset growth rate $Average\ asset\ growth_{c,j,t}$. Furthermore, location-year, industry-location and cohort-level fixed effects are included.

Table 1.4 presents the regression results. Column (1) shows that both the average numbers of departure and appointment in other firms are significant predictors of individual firm performance in the same industry. On average, one more average manager departures in other firms predicts an increase of bankruptcy rate by 0.16, which is economically significant compared to the baseline hazard rate of 0.17.[29] Column (2) adds the firm-level financial controls $z_{i,t-1}$ and the first two rows show that estimations on average manager departures and appointments remain of similar significance and magnitude. The regression results reported in Columns (3) and (4) confirm that average manager turnover in other firms is significantly associated with individual firm asset growth.

The interpretation of this finding is that manager turnover contains some aggregate shock information, which therefore correlates with negative performance in individual firms. The alternative explanation is because of the spillover effects. Though other spillovers may exist, they are likely to suggest an opposite relationship. For example, managers in other firms leave for better outside options instead of experiencing negative shocks in the industry. Then other firms will be more likely to go bankrupt and experience low growth, and our firm will be in better shape as

[28] Note that this variable is calculated using the full sample of firms including one-manager firms at incorporation.

[29] Here, I compare with the benchmark of two-year hazard rates: $0.17 = 1-(1-0.0882)^2$). From Table 1.1

a result of lower competition levels.[30]

As shown in the first stylised fact, founder-managed and non-founder-managed firms present different manager turnover dynamics. Do manager departures and appointments from these two different types of firms also have different predictions about individual firm's performance? In order to answer this question, I calculate the average manager turnover in founder and non-founder-managed firms separately and test their predictions on individual firm performance in Table 1.5. In all specifications, we find that average departures in both founder and non-founder firms strongly correlate with firm's future bankruptcy risk and asset growth. However, results on the average new appointments from founder and non-founder-managed firms are different. Average new appointment in founder-managed firms only significantly negatively correlates with firm future bankruptcy risk. Average new appointment in non-founder-managed firms positively predicts future asset growth.

To sum up these stylised facts, the strong correlation between manager turnover and firm performance implies two non-mutually-exclusive explanations: (1) Manager departure has a negative causal impact on firm prospects; (2) Manager departure contains information that affects firm performance. For instance, managers may have private some information about weakening product demand and jump ship before the worse performance.

When there is a manager-firm mismatch, either because the managerial skill is not optimal-fit to firm's growth perspective or because incumbent manager pursues private benefits of control in exchange for the profits, then turnover allows the firm to benefit from replacing the incumbent manager if the frictions to hire a new better-matched manager is not costlier than the surplus she creates.[31] Therefore, whether manager turnover has a causal impact on a young firm's performance cannot be answered by stylised facts alone. In the next section, I investigate this issue in detail.

[30]The interpretation of manager turnover containing aggregate shocks can also be partially crowded out by the lower competition levels.

[31]The fact that venture capitalists and private equity firms replace founders with professional managers provides supporting evidence of mismatch of managerial skills and firm growth prospects. See, e.g, Hellmann and Puri (2002), Kaplan et al. (2009) and Ewens and Marx (2018).

1.3 Does Manager Turnover Have a Causal Impact?

The challenge in identifying the causal impact of manager turnover is to find a random assignment of manager departures that is not correlated with firms' future performance. In order to overcome this challenge, I exploit the Local Enterprise Growth Initiative (LEGI) in the UK. LEGI is a government program that provides financial support and mentoring advice to local enterprises. The identification strategy is to exploit the positive shock to outside option values of managers who serve firms located outside and near policy regions. I disentangle the causal impact from other policy spillover effects by creating a counter-factual group of firms with ex-ante low departure probabilities. The identification rests on that LEGI does not differentially affect manager turnover between *close* and *far* firms within the counter-factual group. Therefore, a triple-difference approach comparing performances across firms with high and low departure probabilities; between firms located with *close* versus *far* distance to policy areas; and after relative to before the launch of the policy identifies the causal impact of manager departures.

I start this section by introducing the policy background. Then I verify the validity of using policy as an exogenous shock of manager departures. Next, I estimate the overall policy impact on *close* relative to *far* firms. In the third step, I create the counter-factual group of firms. And finally, I estimate the causal impact of manager departures.

1.3.1 Local Enterprise Growth Initiative

Announced in the 2005 Government Budget, the Local Enterprise Growth Initiative (LEGI) was taken into effect in 2006 with the aim to release the productivity and economic potential of the most deprived local areas through enterprise and investment (DCLG, 2010). There are three main national-level objectives: (1) Increase entrepreneurial activities in deprived areas; (2) Support growth and reduce the exit rate of locally owned businesses, and (3) Attract investment using local employers in deprived areas.

By the time of the announcement, there were 91 local authorities eligible to bid

for the LEGI funds. In order to be eligible to bid, a Local Area (LA) has to be either receiving support from them at the time or should be named as eligible for the 2006-2008 Neighbourhood Renewal Fund (NRF). Each of the ten Round 1 and Round 2 winning areas from the bids was announced in February and December of 2006. The LAs in England that received LEGI funds supported are shown in Figure 1.3. Following a change of government, the initiative was abandoned in 2011.

The programme provides a wide range of interventions for the local areas. The LEGI programme offers financial support, premises, networking and communication advice as well as investor development, enterprise education and mentoring services for both start-ups and existing businesses. According to the LEGI final report DCLG (2010), the total expenditure was £418 million, with the highest level of support in 2008 and 2009. 31% of the funds were spent on supporting existing business, with another 30% on entrepreneurship, and the rest was used on advising residents and supporting them in acquiring the skills for finding a job.[32] Government evaluation reports that LEGI had an economically sizeable impact: by the end of 2009, 13,700 new firms had been created, around 45,000 existing business and 240,000 individuals were assisted in LEGI areas DCLG (2010).

The government evaluation DCLG (2010) also compares firms across the treated LEGI area and the propensity-scores-matched control groups at the LA level, the programme has a positive and significant impact on business formation in policy areas but the impact on worklessness performance is not clear. A recent study by Einio and Overman (2016) exploits the eligibility of the programme using the regression discontinuity design and finds a negative local displacement effect on jobs from 2006 to 2009 within a 1km distance of untreated areas. However, they failed to find any significant difference between policy regions and no-policy regions further from the boundary. Based on this fact, I use LEGI as an exogenous source of variation of manager departures in firms located in no-policy regions, but with different distances to the boundaries.

[32]Based on DCLG (2010), of the £268m spent during 2006-2009 period, approximately £84m was spent on projects to assist existing businesses, £78m was devoted to start-ups and £51m was spent on activities to assist residents within the area through employability support.

1.3.2 LEGI policy sample

My analysis focuses on firms located in a no-policy Lower Layer Super Output Areas (LSOAs) within six kilometres of the policy regions.[33] I further restrict my sample to young firms less than ten years old in 2006. I exclude firms belonging to any business groups, those backed by venture capitalists or those involved in M&A activities. Firms located within three kilometres of the policy LSOAs are classified as *close* firms (treated groups) and those located between three and six kilometres from the policy regions are defined as *far* firms (control groups).[34] I also excluded firms with less than three managers in the base year of 2003 because manager turnover in *close* firms and *far* firms were not significantly differently affected by the policy (see Table A1.6).

Table 1.6 displays the summary statistics. Panel A summarises firm characteristics in the pre-sample year 2003. I control for these ex-ante firm characteristics in subsequent regression analyses. The sample includes 5,168 firms, with 1,696 *close* firms and 3,472 *far* firms. The average number of managers is 3.68, with a standard deviation of 1.28. The average firm size is £3.60M and the median total assets are £1.62M. The mean leverage ratio is around 0.48 and the average age is 4.58 years. As we can see from the last row in Panel A, the differences between *close* and *far* firms are not significant in terms of manager number, total assets, leverage and firm age in the pre-sample year of 2003. Panel B shows the number of firms going bankrupt, the average number of manager departures and the average number of managers moving to the policy areas. In the last rows of Columns (6) and (9), the differences of manager departure number and movers to policy regions are compared between *close* and *far* firms. Before the policy, *close* firms have fewer manager departures relative to *far* firms and this difference becomes insignificant after the policy. Moreover, we observe a significantly higher number of managers moving to the policy regions from *close* firms after the policy.[35]

[33]Following Einio and Overman (2016), the distance between the LSOA and the policy region is calculated by the distance of population-weighted centroids between LSOAs to the nearest LSOAs receiving LEGI policy funding.

[34]Estimations are robust with alternative definitions of treatment and control groups and results are shown in section 3.5.

[35]The summary statistics of managers moving to policy regions are based on the sub-sample of managers for whom I can track their subsequent managerial positions based on the universe of UK private firm manager information.

1.3.3 Methodology

LEGI and manager turnover

The empirical methodology rests on the assumption that LEGI policy provides an exogenous shock to outside options of managers in firms located outside and close to the policy regions. The supports that LEGI program provide help firms to survive and grow, therefore generate higher profits to compensate the managers. In the first step, I check whether the policy-induced positive shocks in managers' outside options do indeed change manager turnover decisions in *close* firms relative to *far* firms. I use the difference-in-difference approach with the following specification [36]:

$$\begin{aligned} y_{it} = \alpha_3 + \beta_3 Close_i + c_3 Post_t + \delta_3 Close_i \times Post_t + Z_{i,2003}\gamma_3 + \zeta_3 \bar{X}_{c,j-i,t-1} \\ + LEGI\ Area\ FE + Industry\ FE + \epsilon_{it} \end{aligned} \tag{1.4}$$

where y_{it} is outcome variables of interest for company i in year t, including the (net) number of departures (appointments) and number of managers moving to the policy areas. $Close_i$ is a dummy variable that takes the value of 1 if the firm is located within a three-kilometre distance to the policy LSOAs (treated group). $Post_t$ is a dummy variable takes the value of 1 in the years after the launch of the LEGI program. $Z_{i,2003}$ is a vector containing pre-sample period firm characteristics including firm age, the total number of managers, total assets and leverage in 2003. $\bar{X}_{c,j-i,t-1}$ denote average manager departure number in other firms within same industry and cohort, excluding firm i.[37] From stylized facts, we know that industry average departures may contain unobservable aggregate shocks. So adding $\bar{x}_{c,j-i,t-1}$ further controls for time-varying shocks at industry level. Since there are a total of 20 LAs receiving the LEGI funding, the LEGI-area fixed effects are included to ensure the comparison within same regions around each LA receiving the LEGI support. Industry fixed-effect controls for the industry-specific shocks are at the 6-digit UK SIC code level. The coefficient of interest in Equation 1.4 is δ_3, which measures the average change in manager turnover after the LEGI launch for *close*

[36]The alternative specification is to add firm fixed effects instead of adding firm level control variables. Robust findings are shown in the Section 3.5

[37]In Section 3.5, I show that all results are robust to the alternative specification where firm fixed effects are added.

firms, relative to the *far* firms. I expect a positive δ_3 of (net) number of departures and a positive coefficient for the number of movers to policy regions.

The main assumption underlying this approach is that absent the LEGI programme, average manager turnover in *close* and *far* firms would evolve similarly. I test the "parallel trend" assumption by interacting the treatment with years relative to the policy launch time based on the following specification:

$$y_{it} = \alpha_4 + \beta_4 Close_i + \sum_{m=-2}^{3} c_m \cdot m \text{ years to policy} + \sum_{m=-2}^{3} \delta_m Close_i \times m$$
$$\text{years to policy} + Z_{i,2003}\gamma_4 + \zeta_4 \bar{X}_{c,j-i,t-1} + LEGI \text{ Area } FE + Industry \ FE + \epsilon_{it} \tag{1.5}$$

Figure 1.7 plots the estimations of δ_m on $Close_i \times m$ *years to treatment* in Equation (the omitted variable is the difference three years prior to treatment). Plots (a) and (b) show that before the launch of the LEGI policy, the differences in number of manager departures and net departures between *close* and *far* firms are not significant. In addition, we observe that both estimates of departure number and net departure number increase in *close* firms relative to *far* firms after the LEGI programme launch. Plots (c) and (d) show that there is no significant change in new appointments and net new appointments in. Thus, the dynamic impact of LEGI policy on manager departures validates the satisfaction of the "parallel-trend" assumption.

Another assumption of the identification strategy is that the LA does not manipulate programme eligibility. Two facts suggest the satisfaction of no manipulation assumption. First, there is limited scope for the eligibility manipulation at the LA level. The eligibility to bid for the LEGI funds is determined by the ranking of the LA-level index of deprivation (DETR, 2000), which is a complex index constructed by the government based on pre-policy local economic performance. Only the LAs with rankings of 50 or lower are eligible to bid for LEGI founding. Second, although the LEGI policy is discussed in the year 2005, it is difficult to predict the exact time of the policy implementation and the thresholds. Further more, the potential endogenously selected LEGI regions are not the major concern in this identification framework. Because instead of comparing policy treated regions with the regions

outside of the policy areas, I focus on only firms locate outside of the policy areas.

Construct counter-factual group

However, it is worth noting that a simple difference-in-difference comparison of performance between *close* and *far* firms would not allow me to disentangle the change in performance from the causal impact. As illustrated in Plot (a) of Figure 1.5, average performance difference before and after policy in *close* firms picks up the impacts of exogenous departures, endogenous departures and other policy spillover effects in *close* firms. Comparing performance in *far* firms both before and after the policy generates impacts from the endogenous departures and other policy spillover effects in *far* firms. A second difference between *close* and *far* firms would estimate both causal impact from exogenous departures and the difference of other policy spillover effects between *close* and *far* firms.

To disentangle the causal impact of manager departures from other policy spillovers, a counter-factual group of firms whose manager turnovers are not differentially affected by the policy across *close* and *far* regions would be ideal. As shown in Plot (b) in Figure 1.5, a performance comparison within counter-factual groups and between *close* and *far* regions, before and after the policy only picks up the performance differences from other spillovers. Therefore, under the assumption that the differential impacts of other policy spillovers in *close* relative to *far* regions are the same between the counter-factual group and the treated group, the triple-difference approach identifies the causal impact of exogenous manager departures.

A candidate of counter-factual groups whose manager turnovers are not differentially affected by the policy are firms with no manager departures; however, the realised manager departure decisions are endogenous. I use exogenous policy shock and ex-ante firm characteristics to predict manager departure probabilities. Firms with low predicted departure probabilities are classified as the counter-factual group. Manager departure probability is estimated using Logit regression in Equation 1.6 below:

$$Pr(Num\ departure_{it} > 0) = \alpha_5 + \beta_5 Close_i + c_5 Post_t + \delta_5 Close_i \times Post_t + Z_{i,2003}\gamma_5 + \zeta_5 \bar{X}_{c,j-i,t} + LEGI\ Area\ FE + Industry\ FE + \epsilon_{it} \tag{1.6}$$

where $Pr(Num\ departure_{it} > 0)$ is the probability of having a positive number of manager departures in firm i and year t. The high probability of departure dummy variable $Pr\ high_{it}$ is equal to 1 if the fitted value from Equation 1.6 is above the 75% threshold of the predicted value distribution (see Figure 1.6). In Section 1.3.6, I show that indeed firms with low ex-ante departure probabilities have similar manager turnover in *close* and *far* firms after the policy relative to before.

Triple-difference

The third difference across firms with high and low probability of departures allows me to disentangle the impact of exogenous departures and differential impacts from other spillovers. The triple-difference compares the change in firm performance in response to the policy across firms with high departure probabilities and firms with low departure probabilities, located in *close* to relative *far* from policy areas, before and after the policy change.

Specifically, I follow Equation 1.7 to estimate the causal impact:

$$\begin{aligned} y_{i,t+2} = \alpha_6 + \beta_6 Close_t + c_6 Post_t + \delta_6 Pr\ high_{it} + \gamma_6 Close_i \times Post_t + \eta_6 Close_i \\ \times Pr\ high_{it} + \theta_6 Post_t \times Pr\ high_{it} + \kappa_6 Close_i \times Post_t \times Pr\ high_{it} \\ + LEGI\ Area\ FE + Industry\ FE + Z_{i,2003}\gamma_6 + \zeta_6 \bar{X}_{c,j-i,t} + \epsilon_{it} \end{aligned} \tag{1.7}$$

where, $y_{i,t+2}$ is the outcome variable, including bankruptcy status, assets and profit levels in year $t+2$.[38] $Pr\ high_{it}$ is an indicator for high departure likelihoods, defined as a dummy variable with a value of 1 if the predicated probability $Pr(Num\ departure_{it} > 0)$ is above the 75% percentile of predicted departure distribution from Equation 1.6. All other variables are defined the same as in Equation 1.6. κ_6 is the coefficient of interest. It measures the after the policy changes of firm performance gap across *close* and *far* firms relative to that change in performance gap in the counter-factual group. I expect a negative κ_6 of assets and profits and a positive κ_6 of bankruptcy rate if manager departures indeed have a negative impact on young firm's performance.

[38] In this equation, firm performance is measured in year $t+2$ instead of year t. Because manager departures happen in year t, I analyse the causal impact in time two years later.

One concern with the research design that uses LEGI as an exogenous shock to the managers' outside options and therefore turnover is the reverse causality: managers with inside information about the impact of LEGI policy on their firm's future performance endogenously choose to leave the firm. While the reverse causality concern cannot be addressed directly, two pieces of evidence help to alleviate the concern. I find non-negative spillover effects on the performance of *Close* firms (See in Section 1.3.5), which are more likely to be affected by the policy, relative to the *Far* firms. In addition, firms with high ex-ante departing probabilities are those large and low levered firms. If any negative effects from the LEGI policy are expected from the policy, these "treated" firms, i.e. with high ex-ante departing probabilities are less likely to be think of as vulnerable relative to the smaller and younger firms. Similarly, this is inconsistent with the case where endogenous match between under-performed firms and managers with low managerial skills, as large and low leverage firms are more attractive to more qualified managers for higher compensation and lower risks. Moreover, I found that 63% of manager departures are left with no replacement for newly established firms (See Table 1.1), suggesting that the main effects are driven by the departures of incumbent mangers.

1.3.4 LEGI policy and manager turnover

Table 1.7 presents the estimated impact of the LEGI policy on manager turnover. The results displayed in this table are based on regression from Equation 1.4. Column (1) shows that the average number of manager departures increases significantly by 0.052 after the policy in *close* firms, relative to the firms locating far away. The coefficient is also economically sizeable, representing about 9.27% of the unconditional mean (9.27%=0.052/(0.2795+0.2875))[39]. Column (2) confirms that the policy indeed affected the manager turnover. The outcome variable in Column (2) is the net outflow of managers. *Close* firms on average lose 0.041 more managers without replacements relative to *far* firms after the policy. Columns (3) and (4) correspond to appointment numbers. As we can see, there is no significant difference between the *close* and *far* firms both in terms of the total new appointment number and net new appointment number. In the last column of this table, I conduct analy-

[39]The benchmark numbers are from Table 1.3.2

sis conditional on firms with manager departures and restrict to a sub-sample of firms for which I can match subsequent manager appointments.[40] On average, 0.08 more managers left *close* firms and moved to firms in the policy region relative to those from *far* firms and joined policy-supported firms (25.30% of pre-policy mean: 25.30%=0.081/(0.1606+0.1587)).

In summary, the regression results in Table 1.7 verify that LEGI policy leads to more manager departures in *close* firms relative to firms located far away. More importantly, the fact that more managers move to policy regions from *close* firms to policy areas lends support to the premise that after the policy more manager departures are induced by exogenous reasons-better outside options.

1.3.5 Overall impact of LEGI policy on firm performance

The unconditional impact of LEGI policy on firm performance is estimated by following Equation 1.4. Table 1.8 presents the results. Columns (1) to (5) display estimates with outcome variables of assets, profits, and bankruptcy status measured in year $t+2$ receptively. Positive coefficients of $Close_i \times Post_t$ from Columns (1) to (3) suggest that, on average, *close* firms increases their assets more than *far* firms in response to policy. Columns (4) and (5) show a non-significant difference in profits between *close* and *far* firms. Moreover, a positive but insignificant coefficient in Column (5) suggests that the policy did not significantly change bankruptcy risks in *close* firms relative to that in *far* firms. To address the concerns of skewed firm size distribution, I also run the regressions using logarithm transformations. The results are shown in the lower panel of Table 1.8, and we don't observe significant worse performance in *close* relative to *far* regions.

Recall from the discussion in Section 1.3.3, the simple difference-in-difference estimates the combined impact from exogenous manager departures and other policy spillovers. In order to disentangle the causal impact, I next construct the counterfactual group of firms.

[40]Each departing manager's ID is matched to the assembled managerial data covering the population of UK private companies.

1.3.6 Create the counter-factual group

The counter-factual group of firms are constructed based on the predicted manager departure probabilities. Specifically, I use a set of ex-ante firm characteristics and exogenous policy shock to estimate manager departure probabilities. By estimating Equation 1.6, I calculate the fitted value as the predicted probability of experiencing manager departures for each firm-year.

Table A1.5 in the Appendix reports the regression estimates under different specifications. In the first two columns of Table A1.5, all the independent variables are the same as in the previous specifications. In the last two columns, the average departure number in other firms that operates in the same industry and in the same cohort are included. For Columns (1) and (3), year fixed effects are not controlled for. The results are similar across Columns (1) to (4). Larger-sized firms with bigger management teams and lower leverage level in the base year are predicted to have higher manager departure probabilities. For the subsequent analysis, I use the specification in Column (3) Table A1.5. Under this specification, I control for the lagged average departure at the same industry and cohort level. Industry, location, as well as the year fixed effects are included. Figure 1.6 presents the distribution of the predicted values of $Pr(Num\ departure_{it} > 0)$ from Equation 1.6. The dashed lines in Figure 1.6 pick up the 25%, 50% and 75% thresholds, with bankruptcy probabilities of 0.1553, 0.2135 and 0.3591 respectively. From this figure, we observe that both the histogram distribution and kernel density distribution show a right-skewed distribution, consistent with the fact that departure is not a frequent event. Then, I define the dummy variable $Pr\ high_{it}$ as an indicator of high likelihoods of manager departure and takes the value of 1 if $\widehat{Pr}(Num\ departure_{it} > 0)$ exceeds the 75% threshold of 0.3591.[41]

Now I turn to verify that the construction of the counter-factual group is valid. To satisfy the validity, the policy should not affect manager departures in *close* firms and *far* firms significantly differently within the firms that are classified as with low predicted departure probabilities. Table 1.9 presents the difference-in-difference estimates for the number of manager departures, and the number of managers moving

[41] In Section 3.4.4, I show that the results are more pronounced for the 25% highest departing probability of the sample against the bottom 50% departing probability of the sample.

to policy regions based on sub-samples with high and low departure probabilities separately. The estimated coefficients on $Close_i \times Post_t$ are positive and significant in Columns (1) and (3), suggesting that there is significant increase of manager departure numbers in *close* relative to *far* firms within firms with high departure probabilities. In contrast, insignificant coefficients in Columns (2) and (4) corresponds to the definition of the counter-factual group: LEGI policy do not change manager departures differently in *close* firms relative to *far* firms. Therefore, a within counter-factual group comparison of performance between firms located *close* and *far* to the policy regions both before and after the policy only picks up the differential effects from other policy spillovers.

1.3.7 Triple-difference results

Table 1.10 reports the triple-difference analysis results from estimating Equation 1.7. The same as in the previous analysis, all regressions in Table 1.10 include firm-level control variables, as well as same industry, cohort average departure (excluding firm i). For brevity, estimations on firm characteristics are not reported. The coefficient of interest is on $Close_i \times Post_t \times Pr\ high_{it}$. Column (1) shows that for bankruptcy risks, the gap between *close* and *far* firms with high probabilities of manager departures is 0.0347 larger relative to the bankruptcy risk difference in firms with low departure probabilities in response to the policy. Columns (2) to (5) correspond to the change in assets and profits. Total assets and fixed assets both decline sharply, as shown in Columns (2) and (3) (Total assets decrease by £0.423M and fixed assets decrease by £0.529M). There is no significant change in current assets and profits in surviving firms. To further ensure that the results are not purely driven by the skewed distribution of firm size, I run regressions using logarithm transformations of asset levels. From the middle panel of Table 1.10, we find robust negative impact on both total assets and fixed assets.

Taken together, the estimated results in Table 1.10 show that relative to the differences gap between *close* and *far* firms with low probability of manager departures, there is a higher probability of bankruptcy, and sharper decline in assets for *close* firms relative to *far* firms within high departure probability group. This evidence suggests a significant negative causal impact of manager departure on a

young firm's survival and asset growth.

Having established the causal impact of manager turnover on young firm's performance, I now quantify the causal impact of one manager departure by calculating the Wald estimations. To obtain the treatment-on-the-treated estimates, I scale the triple-difference estimates by the realised difference in manager departures. The Wald estimations are presented in the bottom panel of Table 1.10. From Columns (1) to (5), the first two rows of lower panel present results from triple-difference regressions with 95% confidence intervals. In Column (6), I report the coefficient on the number of manager departures following the same triple-difference approach in Equation 1.7. Then rescaling the coefficients from Column (1) to Column (5) by 0.0846 from Column (6), I report the Wald estimations in the next row. One manager departure leads to an average increase in bankruptcy probability of 0.4102 (0.4102=0.0347/0.0846), more than six times of the unconditional sample average before the policy (6.46%=((121+213)/5168, from summary statistics in Table 1.6). The average causal impact on bankruptcy risk has a range between 0.43% and 6.50% at 95% confidence levels. On average, one more manager departure result in a £5.01M decrease in total assets, with a range from £1.53 M to £8.48 M. The causal impact is economically sizeable compared to the pre-sample average of £3.60M and this corresponds to a yearly drop of almost 70% of the total asset (0.697=5.01/2*3.59, 3.59 is the unconditional average firm size). Moreover, for fixed assets, there is, on average, a decrease of £6.26M, with a 95% confidence interval between £0.95 M to £11.57M.

1.3.8 Causal impact in founder and non-founder-managed firms

From the first stylised fact, we know that there is a remarkable difference in manager turnover dynamics between founder and non-founder-managed firms, a reflection of underlying distinct ownership structure. In this section, I am taking the advantage of the young firm sample, which provides a unique setting, to invest how this trade-off varies across different ownership structures, i.e. between founder and non-founder-managed firms.

While founder-managed firms are more likely to have mismatched-managers, they

may also face higher frictions to assign new managers than non-founder-managed firms. Though absent the agency problem when ownership and control are perfectly aligned, the controlling owner has no incentive to monitor her "alter ego" (Bennedsen and Nielsen, 2010) from enjoying private benefits of control in exchange for profits (Stulz, 1988). In addition, founders are less likely to be the optimal-matched managers throughout the young firm's early life cycle because the capabilities of managing an expanding firm could differ from those required to found a new venture.[42] However, founder managers may be reluctant to hire a professional manager or even to make themselves to be replaceable (Rajan, 2012) because of "tight links" with their ventures (Dobrev and Barnett, 2005) or private benefits of control. The process of standardization would dissipate the rents that the founders enjoy when controlling the firm.[43] This suggests costlier frictions of assigning a weakly better new manager to founder-managed firms. Overall, the differential impacts of manager turnover on firm performance in founder and non-founder-managed firms depend on the relative variations of this trade-off.

In order to understand the variation in this trade-off with more or less aligned ownership and control, I repeat the triple-difference analysis using sub-samples of founder and non-founder-managed firms respectively. Similar to the analysis in Section 1.2, firms are classified as founder-managed firms if managers who join the firm at incorporation also own shares in the pre-sample period (the year 2003). Among the 5,168 firms in total, there are 4,235 founder-managed firms and 933 non-founder-managed firms. In Table 1.11, Panel A and Panel B present the results for the founder-managed and non-founder-managed sub-samples, respectively. From Panel A, we observe that in founder-managed firms, there is no significant change in bankruptcy likelihoods as a result of manager departure, even though assets decline sharply, as shown in Columns (2) to (5). In contrast, Panel B shows that in non-founder-managed firms, manager departures are more likely to cause subsequent failures. However, surviving non-founder-managed firms do not experience any significant change in assets investment or profitability.

[42]The fact that venture capitalists replace founders with professional managers provides supporting evidence. See, for example, Hellmann and Puri (2002), Kaplan et al. (2009) and Ewens and Marx (2018).

[43]In line with this argument, Hurst and Pugsley (2011) find that almost half of the surveyed new firm owners report nonpecuniary benefits as motivation for starting their business. Similar evidence can be found in Bennett and Chatterji (2017).

These findings are consistent with founders' private benefits of control, prohibiting them from closing down firms. If founders are enjoying the private benefits of control, they are less likely to shut down their firms even though their firms are undergoing a poor performance. One typical type of private benefits of control is non-pecuniary (Hamilton, 2000; Moskowitz and Vissing-Jorgensen, 2002). For instance, founders tend to stick with their ventures. Based on a sample of 212 US start-ups, Wasserman (2003) finds that, on average, four out of five CEOs are forced to step down. In Wasserman (2003), he also proposes that founders face the "paradox" of higher risks of being replaced after achieving milestones. These findings can also be attributed to variations in motivation and growth of entrepreneurial firms (Hurst and Pugsley, 2011). Bennett et al. (2016) also show evidence suggesting that potential heterogeneous growth and motivation also affect the managerial practices. While other channels may also be consistent, the interpretations should be taken with care.

1.3.9 Heterogeneity

In Section 1.3.6, high departure probability firms are classified using the 75% threshold of predicted departure probabilities. The causal impact, therefore, comes from comparing firm performance between the highest 25% manager departure likelihoods relative to the other 75%. Which part of the bottom 75% departure probabilities is contributing to the significant performance difference? I then compare the highest departure probability firms with the second highest, third highest and the bottom quartile of predicted departure likelihood firms in each panel. The first column in Table 1.12 shows that the bankruptcy risk is positive and significant when we compare the first quartile of departure probability firms with the third or bottom quartile firms. Moreover, the magnitude of the coefficient in Column (1) almost doubles in Panel C compared to that in Panel B, suggesting a larger difference between the top and bottom quartiles. Though the coefficients in Columns (2) to (8) in Panel C are not statistically different from 0, the estimates are more negative for total assets and fixed assets both in levels and logarithm transformations when comparing the top quartile with the bottom quartile. Taken together, the results in this table suggest that the significant decline in assets and increase in bankruptcy

risks are likely to be driven by the differences between firms with the top quartile of departure probabilities, and the bottom 50% of departure likelihoods, supporting the previous findings in Table 1.10.

My sample is restricted to young firms less than ten years old when the policy was taken into effect. But are younger firms also more vulnerable to exogenous manager departures? I explore the differential causal impact on firms less than five years old and older firms up to ten years old. Table 1.13 shows the estimates of $Close \times Post \times Pr\ high$ based on younger firm and older firm sample in Panel A and B respectively. From Panel A, we find that the exogenous manager departure leads to a significant increase in bankruptcy risk and a a decrease in assets in younger firms. However, the impact on relative older firms is insignificant from 0, as shown in Panel B. Overall, the negative causal impact appears to be more pronounced in younger firms than in older firms.

1.4 Robustness Checks

I provide robustness checks in this section to alleviate the sample truncation and censoring concerns and run two additional robustness checks of identification assumptions in the triple-difference analysis.

The analysis sample consists of multiple cohorts of new firms, and firm performance is truncated in 2015. OLS regressions bring the advantage of saturated models with fixed effects, but may suffer from truncation and censoring bias. In order to address this concern, I estimate the predictions in Cox duration models for bankruptcy. Specifically, I use COX model stratified at cohort and industry-location group. The regressions results presented in Appendix A1.3 and Appendix A1.4 show consistency with the OLS estimations.

There are two concerns regarding the triple-difference analysis: (1) the violation of the "parallel-trend" assumption that the treatment and control groups would evolve similarly in the absence of the policy; and (2) the results are not robust for the other definitions of *close* and *far* firms. To test the "parallel-trend" assumption, I estimate the dynamic response to the policy following similar types of regression in Equation 1.3.3 and plot coefficients with 95% confidence intervals in Figure 1.8.

The plots show that there are no significant differential trends in bankruptcy risks, assets and profits before the policy. And all differences come from the period after the policy launch, supporting the "parallel trend" assumption.

In addition, I run placebo tests by randomly selecting 80 alternative distance thresholds between 2km to 4km to classify the *close* and *far* firms. Following the same methodology from Equation 1.2 to 1.7, I estimate the change of manager turnover and causal impact on firm performance. Estimated coefficients with 95% confidence intervals are plotted in Figure 1.9 and Figure 1.10. Estimations on departure numbers and the net number of departures are both positive and significantly different from zero around a small window of the actual threshold of 3km used in Section 1.3. As the placebo thresholds move away from three kilometres towards two or four kilometres, not only do the confidence intervals widen but the estimations also change signs. Similar patterns can be observed in Figure 1.10, where I plot the estimations of the causal impact on firm performance. In summary, the placebo tests with different distance thresholds show that my results are unlikely to be spuriously driven by any particular threshold.

Moreover, because of the young firm data limitation, observable control variables for young firms are not well populated. In all the reported estimations, I only control for the firm size, age, leverage and bank deposits. The concern is that policy may affect different types of firms in a different way. To address this concern, I repeat all estimations adding firm fixed effects, industry, and location fixed effects. Regression results within firms are presented in Table A1.7, and they reconfirm that all results are robust.

Lastly, the results remains robust when I change the absolute changes of manager departures/appointments to the dummy variables. Results are reported in Appendix Table A1.8 and TableA1.9.

1.5 Conclusion

Despite the fact that young firms are crucial job creators and managers in young firms represent a big proportion of all managers in the economy, we have little knowledge about managers in young firms. In addition, the remarkable differences

between mature and young firms stop us from extrapolating existing empirical evidence from CEO successions in large and established corporations to small and young firms. This paper fills this knowledge gap by examining manager turnover and its impact on young firm's performance with heterogeneity across founder and non-founder-managed firms.

I compile a novel and comprehensive dataset of managerial information covering the population of private firms in the UK, which allow me to explore the heterogeneity of manager turnover in various dimensions. By combining manager data with ownership stakes and the financial accounts of new firms, I construct a multi-cohort young firm sample. This paper documents three stylised facts of manager turnover in young firms. The average number of manager departure declines as firms grow, whereas average appointment number presents a U-shape pattern. In addition, I find significant correlation between manager turnover measures and a young firm's future performance. Moreover, the industry average manager turnover predicts individual firm performance, suggesting that manager turnover contains aggregate information. To understand whether manager turnover has a causal impact on young firm's performance, I exploit shocks to the manager's outside options induced by the Local Enterprise Growth Initiative (LEGI) in the UK. I create a counter-factual group of firms based on ex-ante firm characteristics and apply a triple-difference approach to disentangle the causal impact from other policy spillover effects. My findings show that one manager departure leads to a six-fold increase of the probability of bankruptcy and for surviving firms, total assets shrink by £5.01M on average in the following two years.

In addition, this paper investigates the heterogeneity of manager turnover in firms with distinct ownership structures by comparing firms run by founder managers and non-founder managers. I find that there is a remarkable difference in manager turnover dynamics: non-founder-managed firms have a higher average manager departure and appointment, while founder-managed firms have less frequent managerial changes. There is also a heterogeneous causal impact of manager departures on founder and non-founder-managed firms. Exogenous manager departures in founder-managed firms only result in a sharp decline of assets without a significant change in bankruptcy risks. In contrast, non-founder-managed firms are more

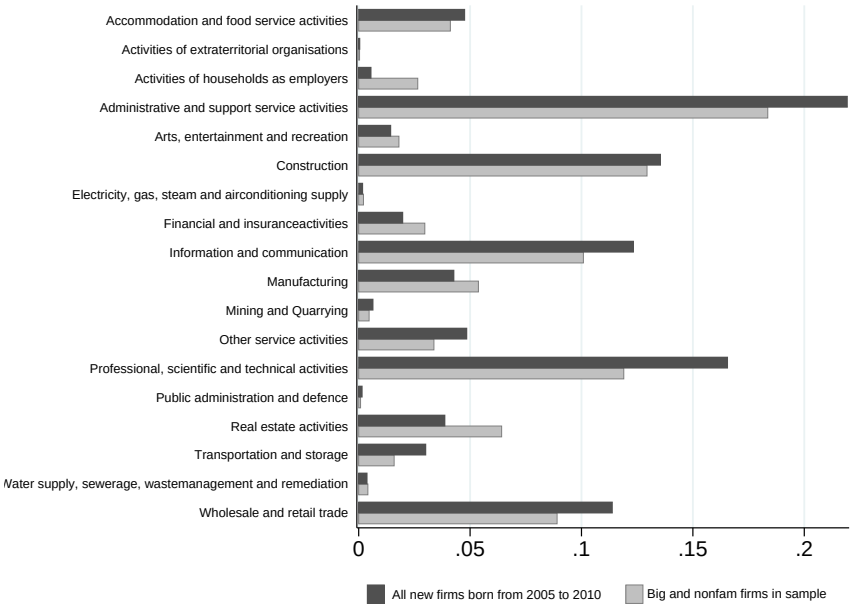
likely to go bankrupt as a result of manager departures.

This paper documents novel stylised facts about manager turnover in young firms based on a large sample of entrepreneurial firms in the UK. Average manager turnover at the industry level provides a timely and available early indicator to a young firm's future performance, which is important for both policy-makers and investors. The economically large negative causal impact of manager turnover indicates that the costs of assigning a new appropriate manager are substantially larger than the benefits of resolving the potential mismatches between incumbent managers and profit maximization in young firms. The frictions of replacement may come from the possible difficulty to replace firm-specific human capital, or a young firm's limited access to managerial labour markets. Lastly, heterogeneity across founder and non-founder-managed firms reflects underlying distinctions of ownership and control structures.

Figure 1.1: Industry and Location Distribution in UK Young Firms

This figure plots the industry and location distributions of young firms in the analysis sample (the grey bar) and across the UK (the blue bar). For industry distribution, I used the 2007 6-digit standard industrial classification of economic activities in the UK. The location distribution is presented at the county level. There are a total of 609 industries and 97 industries in the analysis sample, the plots only include industries and locations with more than 1% of the sample firms.

(a) Industry Distribution



(b) Location Distribution

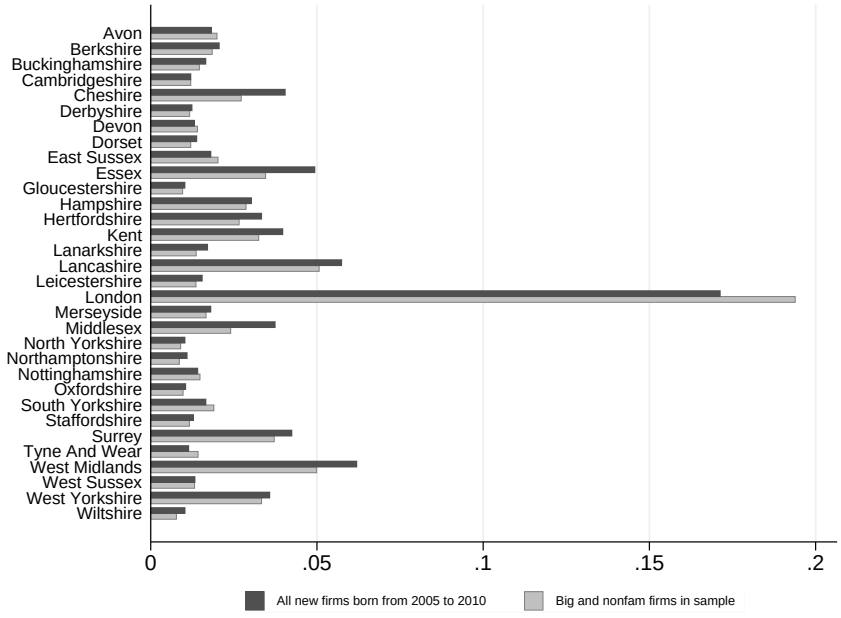
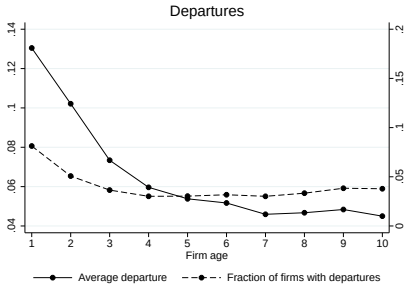
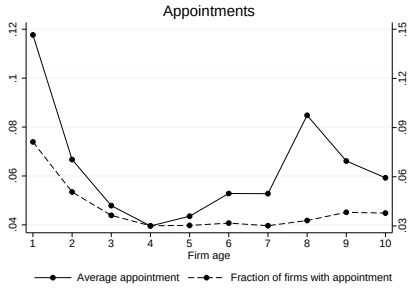


Figure 1.2: Dynamic Manager Turnover and Bankruptcy in Young Firms

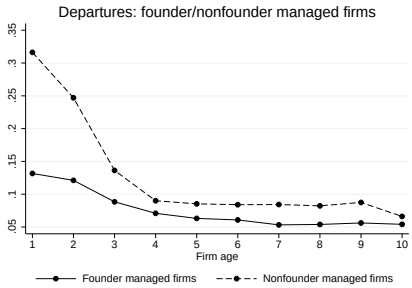
This figure presents the dynamics of turnover and bankruptcy in young UK firms based on the analysis sample of UK young firms incorporated between 2005 and 2010. Plots (a) and (b) show the average number of departures/appointments with fractions of firms losing/hiring managers respectively. Plots (c) and (d) compare the average manager departures/appointments between founder-managed and non-founder-managed firms. The bankruptcy hazard rate is shown in Plot (e) and plotted separately for founder-managed and non-founder-managed firms in Plot (f). Plots (g) and (h) present the bankruptcy risks based on the sub-sample of firms classified by their second-year manager turnovers.



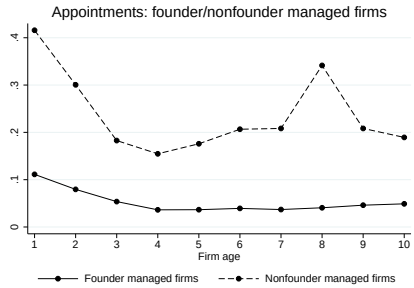
(a)



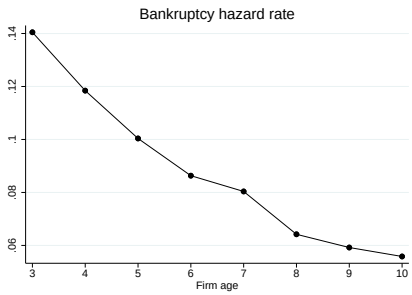
(b)



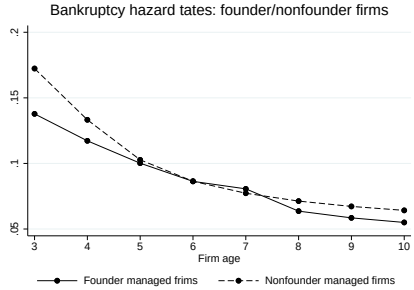
(c)



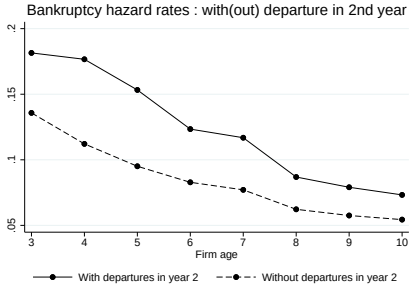
(d)



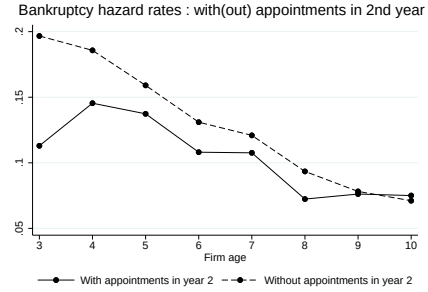
(e)



(f)



(g)



(h)

Figure 1.3: Map of LEGI Supported Local Authorities in England

Figure 1.4: 1km-wide LSOA Rings (Croydon and Barking & Dagenham)

This figure depicts the one-kilometre wide control and treatment LSOA rings outside the Croydon and Barking & Dagenham areas. The LSOA labels indicate the ring. For example, the one-kilometre treatment ring is labelled "T1" and the four-kilometre control ring is labelled "C4". LSOAs with more than a six-kilometre but less than a ten-kilometre distance to the LEGI LSOAs are only labelled with numbers. Similar to Einio and Overman (2016), distances are calculated using LSOA population-weighted centroids. For example, the population-weighted centroid of a one-kilometre treatment ring LSOA is located in a non-LEGI area and within 0-1000 metres from the nearest LEGI LSOA population- weighted centroid. The GIS data were taken from the ONS and processed via Arcmap.

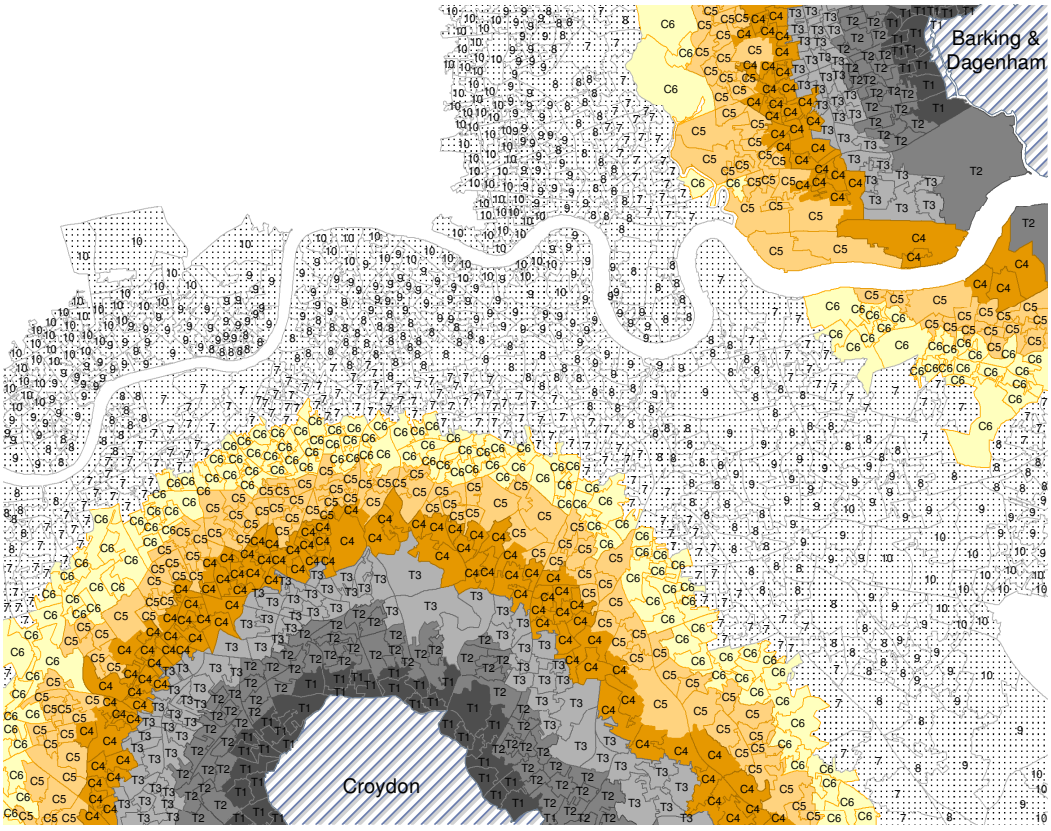
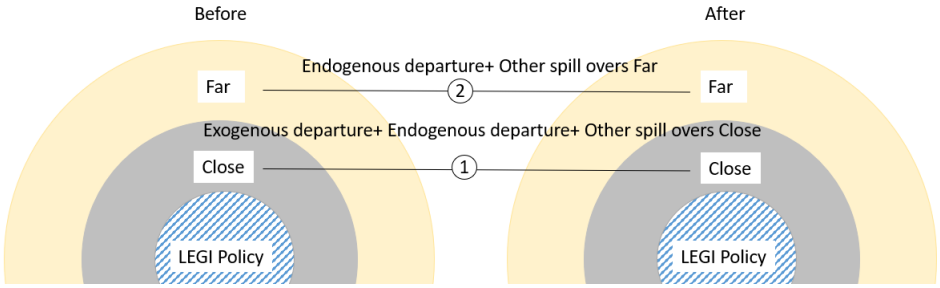


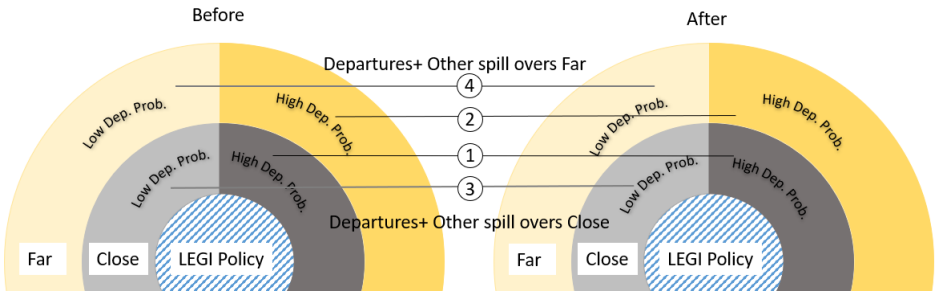
Figure 1.5: Triple-Difference Methodology Illustration

This figure illustrates the triple-difference methodology. Plot (a) shows that a simple difference-in-difference analysis is misleading. The difference between close firms after the policy relative to before, includes policy impact from exogenous departures, endogenous departures and other spillover effects on close firms. The difference between far firms after the policy relative to before includes policy impacts from endogenous departures, endogenous departures and other spillover effects on close firms. The difference between (1) and (2) is a combined impact from both exogenous manager departures and differences from other spillover effects. Plot (b) displays the triple-difference framework. By splitting firms in both close and far regions into two groups: firms with high/low departure probabilities, and assume that the counter-factual group of firms does not have different manager turnovers , then $(3)-(4)$ only pick up the relative differential impact from other spillovers. Under the assumption that these differential impacts from other spillovers are the same in low and high probability departure firms, a triple difference of $[(1)-(2)]-[(3)-(4)]$ identifies the exogenous departure's causal impact.



(a)

(1)-(2): Exogenous departure+Other spill overs Close-Other spill overs Far



(b)

(1) − (2): Exogenous departure+Other spill overs Close-Other spill overs Far
(3) − (4): Other spillovers Close-Other spill overs Far

Figure 1.6: Distribution of Predicted Probability of Departure

This figure plots the density distribution of the predicted probability of departure based on the Logit regression in Equation 1.6 $Pr(Num\ departure_{it} > 0) = \alpha_5 + \beta_5 Close_i + c_5 Post_t + \delta_5 Close_i \times Post_t + Z_{i,2003}\gamma_5 + \zeta_5 \bar{X}_{c,j-i,t} + LEGI\ Area\ FE + Industry\ FE + \epsilon_{it}$. The X axis is the predicated probability of manager departures, and the y axis is the density. The yellow bars show the histogram distributions, with the solid line plotting the kernel density. The dashed lines pick up the 25%, 50%, and 75% thresholds of the distribution respectively.

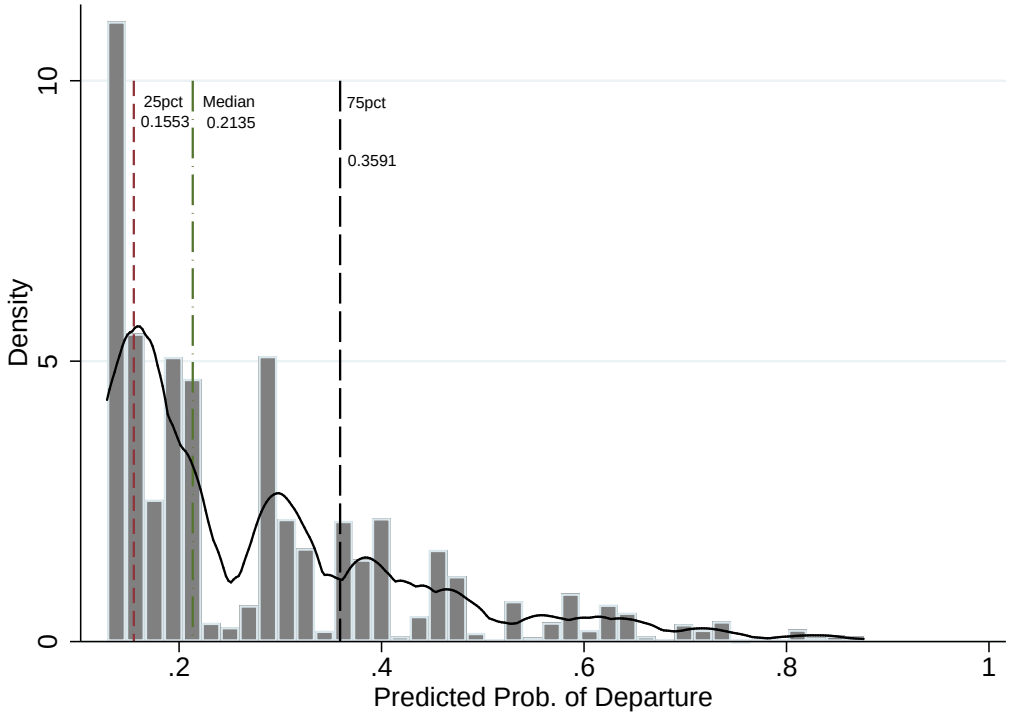
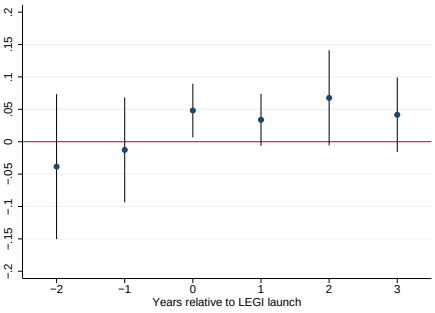


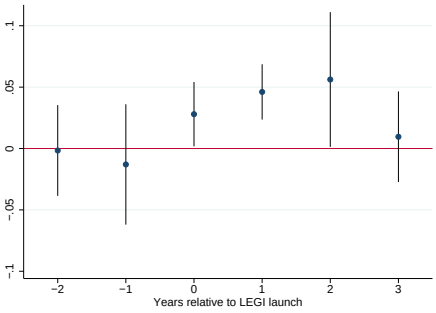
Figure 1.7: Difference in Manager Turnover by Years to LEGI Launch

This figure presents the estimations of δ_m from the equation $y_{it} = \alpha_4 + \beta_4 Close_i + \sum_{m=-2}^{3} c_m m \ years\ to\ policy + \sum_{m=-2}^{3} \delta_m Close_i \times m \ years\ to\ policy + Z_{i,2003}\gamma_4 + \zeta_4 X_{c,j-i,t-1} + LEGI\ Area\ FE + Industry\ FE + \epsilon_{it}$, where m denotes the relative years to the LEGI policy launch time. Dependent variables y_{it} include $Num\ departure_{it}$, $Net\ departure_{it}$, $Num\ new_{it}$, and $Net\ new_{it}$. δ_m estimate the difference in manager turnover between the close and far firms to the policy area, after the enforcement of the LEGI policy relative to before .

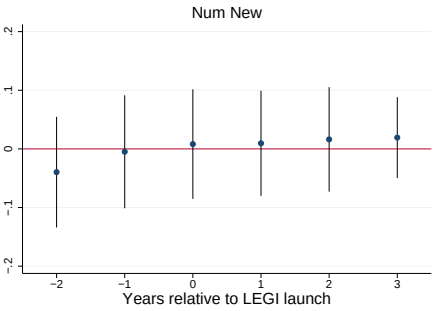
(a) *Num departure*



(b) *Net departures*



(c) *Num appointments*



(d) *Net appointments*

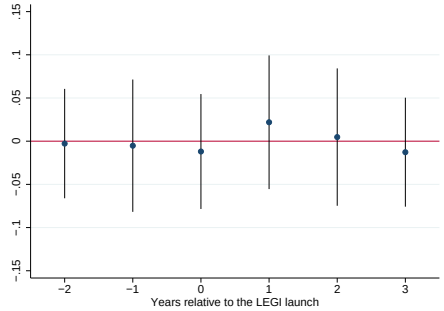
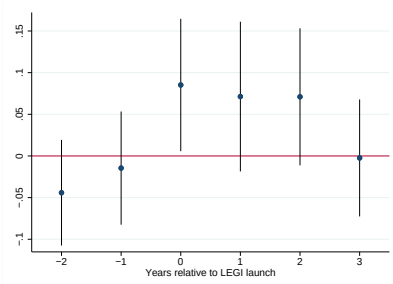


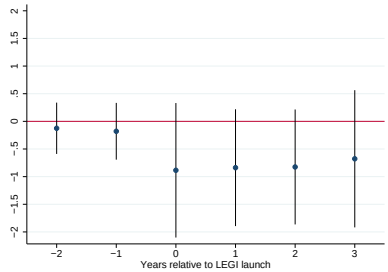
Figure 1.8: Difference in Firm Performance by Years to LEGI Launch

This figure presents the estimations of δ_m from the equation $y_{it} = \alpha_4 + \beta_4 Close_i + \sum_{m=-2}^{3} c_m m \ years \ to \ policy + \sum_{m=-2}^{3} \delta_m Close_i \times m \ years \ to \ policy + Z_{i,2003}\gamma_4 + \zeta_4 X_{c,j-i,t-1} + LEGI \ Area \ FE + Industry \ FE + \epsilon_{it}$, where m denotes the relative years to the LEGI policy launch time. Dependent variables y_{it} are total assets, fixed assets, current assets and profit-and-loss account. δ_m estimates the difference in firm performance between the close and far firms to the policy area both before and after the enforcement of the LEGI policy.

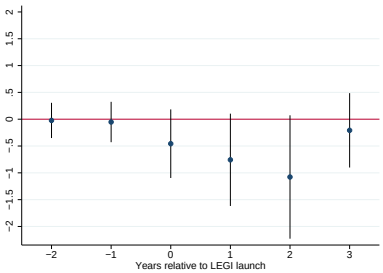
(a) *Bankruptcy*



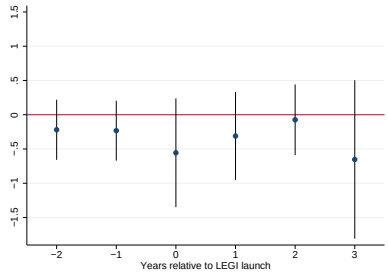
(b) *Total Assets*



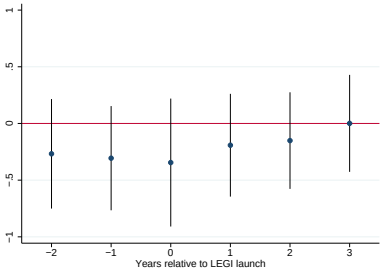
(c) *Fixed Assets*



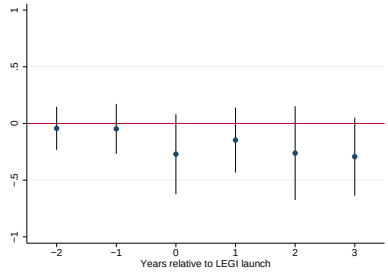
(d) *Current Assets*



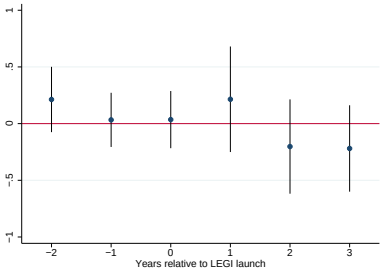
(e) *Profit and Loss Account*



(f) *Log(Total Assets)*



(g) *Log(Fixed Assets)*



(h) *Log(Current Assets)*

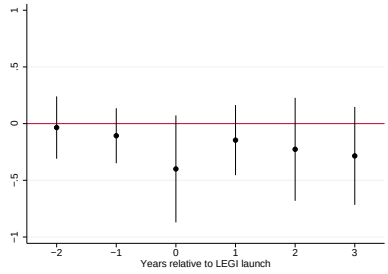
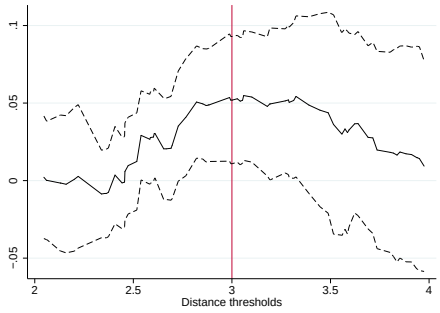


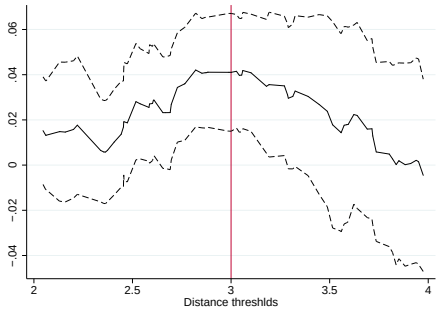
Figure 1.9: Placebo Tests: Different Distance Thresholds

This figure plots the coefficients with 95% confidence intervals from the placebo tests of the LEGI impact on manager turnover from Equation 1.4: $y_{it} = \alpha_3 + \beta_3 Close_i + c_3 Post_t + \delta_3 Close_i \times Post_t + Z_{i,2003}\gamma_3 + \zeta_3 X_{c,j-i,t-1} + LEGI\ Area\ FE + Industry\ FE + \epsilon_{it}$. 80 distance thresholds are randomly selected between two and four kilometres to define the close firms. The solid lines represent the coefficient estimations, and the dashed lines are the upper and lower 95% confidence intervals respectively.

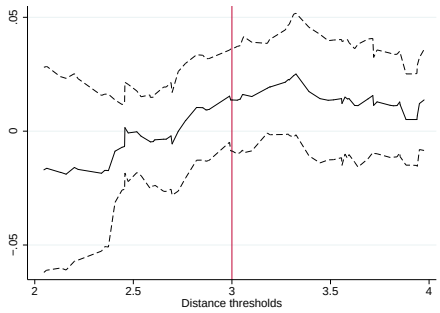
(a) *Num departure*



(b) *Net departure*



(c) *Num appointments*



(d) *Net appointments*

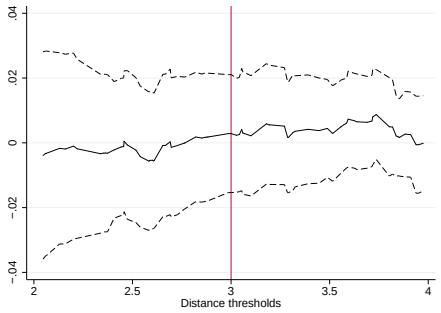
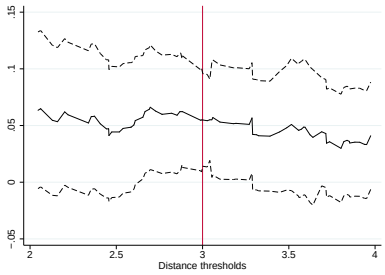


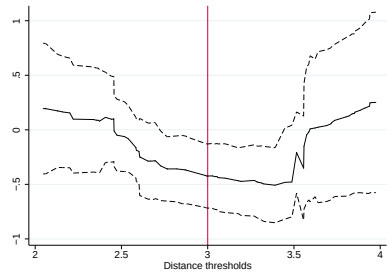
Figure 1.10: Placebo Tests on Firm Performance: Different Distance Thresholds

This figure plots the coefficients with 95% confidence intervals from placebo tests of the LEGI impact on firm performance from Equation 1.7: $y_{i,t+2} = \alpha_6 + \beta_6 Close_t + c_6 Post_t + \delta_6 Pr\ high_{it} + \gamma_6 Close_i \times Post_t + \eta_6 Close_i \times Post_t + \theta_6 Post_t \times Pr\ high_{it} + \kappa_6 Close_i \times Post_t \times Pr\ high_{it} + LEGI\ Area\ FE + Industry\ FE + Z_{i,2003}\gamma_6 + \zeta_6 \bar{X}_{c,j-i,t} + \epsilon_{it}$. 80 distance thresholds are randomly selected between two and four kilometres to define the close firms. The solid lines represent the coefficient estimations and the dashed lines are the upper and lower 95% confidence intervals respectively.

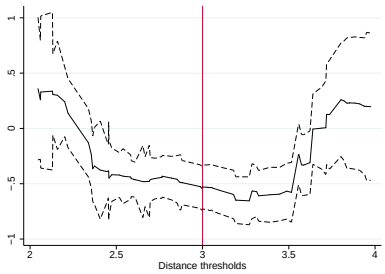
(a) *Bankruptcy*



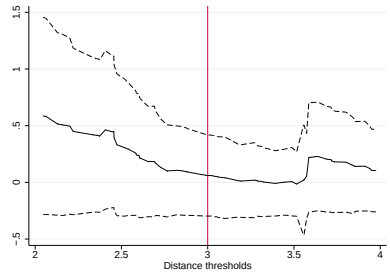
(b) *Total Assets*



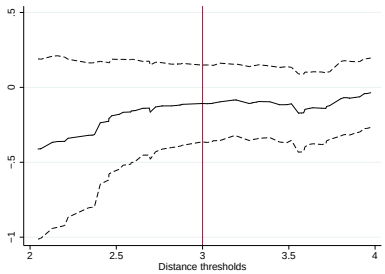
(c) *Fixed Assets*



(d) *Current Assets*



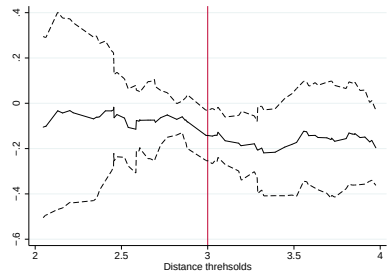
(e) *Profit and Loss*

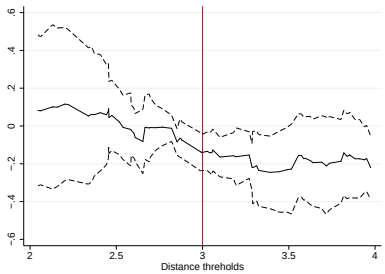


(f) *Log(Total Assets)*

(g) *Log(Fixed Assets)*

(h) *Log(Current Assets)*





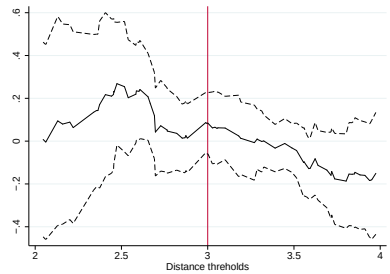


Table 1.1: Summary Statistics

This table summarises firms in the analysis sample with the full sample in Panel A and the sub-sample of founder/non-founder-managed firms in Panel B respectively. Founders are classified as managers with shares at the establishment of the firm. Firms with at least one founder are defined as founder-managed firms. All financial variables are reported in £ unit. The analysis sample includes 336,000 private limited new firms incorporated from 2005 to 2010 with at least two managers at incorporation. The sample excludes firms with one manager at incorporation, firms belonging to business groups, in M&A activities or backed by VC firms; non-trading businesses are also excluded.

Panel A: Full Sample						
	Mean	SD	25%	50%	75%	Obs.
Total assets	4,307,727	7,178,035	15,472	55,047	193,138	1,723,280
Fixed assets/Total Assets	0.3573	0.3350	0.0565	0.2378	0.6293	1,353,090
Current Assets/Total Assets	0.7288	0.3229	0.5192	0.8891	0.9955	1,701,267
Bank deposits/Current Assets	0.3667	0.3405	0.0571	0.2558	0.6416	1,505,067
Total Liabilities	394,630	1,693,170	14556	43751	147795	1,621,010
Leverage	0.5969	0.3378	0.3269	0.6736	0.9101	1,723,280
Shareholder funds	3,939,335	7,396,873	152	4,638	39,083	1,722,021
Profits	66,691	1,866,079	-505	4,369	36,118	1,578,741
Bankruptcy hazard ratio	0.0882					
Asset growth rate	0.0145	0.5954	-0.2378	0.0101	0.3541	1,129,257
Num. of managers	2.2336	0.6171	2	2	2	2,404,364
Num. of departures	0.0993	0.3757	0	0	0	2,226,366
Num. of appointments	0.0646	0.3389	0	0	0	2,226,366
Num. of replacements|departure	0.3774	0.6289	0	0	1	177,560

Panel B: Founder and non-founder-managed firms						
	Founder Managed			Nonfounder Managed		
	Median	SD	Obs.	Median	SD	Obs.
Total assets	56,082	5,148,220	1,394,090	54,983	3,403,497	149,190
Fixed assets/Total Assets	0.3300	0.3326	1,057,509	0.2322	0.3581	115,581
Current Assets/Total Assets	0.8845	0.3205	1,374,337	0.8894	0.3504	146,930
Bank deposits/Current Assets	0.2102	0.3402	1,195,199	0.2596	0.3439	129,868
Total Liabilities	41,650	1,374,989	1,302,695	43,883	1,796,860	138,315
Leverage	0.6879	0.3244	1,039,654	0.6077	0.3614	149,190
Shareholder funds	4,620	5,045,864	1,392,959	4,639	3,851,943	149,062
Profits	2,124	4,773,555	1,263,604	4,587	36,459,571	120,093
Bankruptcy hazard ratio	0.0874			0.0969		
Asset growth rate	0.0161	0.5958	844,219	-0.0051	0.5900	87,038
	Mean	SD	Obs.	Mean	SD	Obs.
Number of managers	2.2355	0.6209	2,014,509	2.204	0.5697	387,825
Number of departures	0.0909	0.3539	1,854,204	0.1992	0.5661	192,162
Number of appointments	0.0564	0.31	51,84,204	0.1625	0.5728	192,162
D(replacements>0|departure>0)	0.3194	0.5734	153,854	0.7542	0.8153	23,706

Table 1.2: Correlation Between Manager Turnover and Firm Performance

This table shows the analysis to access the correlation between manager turnover and firm performance in the two subsequent years based on Equation 1.2. The outcome variables are $D(Bankruptcy = 1)_{i,t+2}$, an indicator of whether firm status is liquidated or dissolved; and $Asset\ growth_{i,t+2}$, the asset growth rate from year t to $t+2$. $\triangle Num_{it}$ is the change in manager numbers from year $t-1$ to year t. $Num\ departure_{it}$ and $Num\ new_{it}$ denotes the number of manager departures and the number of appointments. $Net\ departure_{it}$, $Num\ replacement_{it}$ and $Net\ new_{it}$ are the number of net departures, the number of replaced managers and the number of net new appointments respectively. In all regressions, firm-level controls include total assets, bank deposits, profits (from the balance sheet) and the leverage ratio. Location-Industry-year and cohort-fixed effects are included. Standard errors shown in parentheses are clustered at the industry, location and cohort levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
	$D(Bankruptcy = 1)_{i,t+2}$			$Asset\ growth_{i,t+2}$		
$\triangle Num_{it}$	-0.0459***			0.0488***		
	(0.0029)			(0.0079)		
$Num\ departure_{it}$		0.0619***			-0.0361***	
		(0.0040)			(0.0074)	
$Num\ new_{it}$		-0.0387***			0.0719***	
		(0.0030)			(0.0106)	
$Net\ departure_{it}$			0.0667***			-0.0301**
			(0.0040)			(0.0080)
$Num\ replacement_{it}$			0.0133**			0.0244
			(0.0035)			(0.0910)
$Net\ new_{it}$			-0.0311***			0.0801***
			(0.0036)			(0.0109)
$Num_{i,t-1}$		-0.0206***	-0.0207***		0.0026	0.0024
		(0.0019)	(0.0019)		(0.0013)	(0.0013)
$Total\ assets_{i,t-1}$	-0.0014	-0.0024	-0.0004	-0.2766	-0.2990	-0.2980
	(0.0580)	(0.0376)	(0.0369)	(0.1609)	(0.1811)	(0.1815)
$Bank\ deposits_{i,t-1}$	-1.8001**	-1.0727**	-1.0612**	0.5260	0.1335	0.1496
	(0.5336)	(0.3277)	(0.3174)	(0.8771)	(1.0202)	(0.9909)
$Profits_{i,t-1}$	-0.0349	-0.0335	-0.0312	0.8556*	0.8824*	0.8909*
	(0.0592)	(0.0501)	(0.0505)	(0.4079)	(0.4362)	(0.4346)
$Leverage_{i,t-1}$	0.0306***	0.0297***	0.0297***	0.0461***	0.0461***	0.0460***
	(0.0021)	(0.0021)	(0.0021)	(0.0032)	(0.0031)	(0.0031)
Observations	1,217,158	1,217,158	1,217,158	758,474	758,474	758,474
R-squared	0.122	0.125	0.125	0.106	0.106	0.106
$Loc \times Ind \times Year$ FE	Yes	Yes	Yes	Yes	Yes	Yes
$Cohort$ FE	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.3: Correlation Between Manager Turnover and Firm Performance

This table shows the analysis showing the correlation between manager turnover and firm performance in the subsequent two years based on 1.2, and based on founder and non-founder-managed firm samples in Panel A and B respectively. Dependent and independent variables are the same as described in Table . In all regressions, the firm-level controls include total assets, bank deposits, profitloss account(from the balance sheet) and the leverage ratio. For brevity, estimations on firm-level control variables are not included in this table. Location-Industry-year and cohort-fixed effects are included, however. Standard errors shown in parentheses are clustered at the industry, location and cohort levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Founder-managed firms						
	$D(Bankruptcy = 1)_{i,t+2}$			$Asset\ growth_{i,t+2}$		
$\triangle Num_{it}$	-0.0465***			0.0449***		
	(0.0028)			(0.0044)		
$Num\ departure_{it}$		0.0615***			-0.0284***	
		(0.0034)			(0.0062)	
$Num\ new_{it}$		-0.0393***			0.0743***	
		(0.0031)			(0.0078)	
$Net\ departure_{it}$			0.0660***			-0.0254***
			(0.0032)			(0.0049)
$Num\ replacement_{it}$			0.0113***			0.0388*
			(0.0024)			(0.0240)
$Net\ new_{it}$			-0.0320***			0.0786***
			(0.0035)			(0.0080)
$Num_{i,t-1}$		-0.0197***	-0.0198***		0.0029	0.0029
		(0.0020)	(0.0020)		(0.0029)	(0.0029)
Observations	1,125,639	1,125,639	1,125,639	700,424	700,424	700,424
R-squared	0.124	0.127	0.127	0.108	0.108	0.108
Panel B: Non-founder-managed firms						
$\triangle Num_{it}$	-0.0444***			0.0753**		
	(0.0085)			(0.0238)		
$Num\ departure_{it}$		0.0671***			-0.0810**	
		(0.0133)			(0.0245)	
$Num\ new_{it}$		-0.0384***			0.0804**	
		(0.0083)			(0.0236)	
$Net\ departure_{it}$			0.0781***			-0.0831*
			(0.0157)			(0.0336)
$Num\ replacement_{it}$			0.0196*			0.0008
			(0.0081)			(0.0151)
$Net\ new_{it}$			-0.0227**			0.0779**
			(0.0062)			(0.0281)
$Num_{i,t-1}$		-0.0270**	-0.0275**		0.0088	0.0089
		(0.0076)	(0.0077)		(0.0087)	(0.0068)
Observations	63,126	63,126	63,126	37,904	37,904	37,904
R-squared	0.258	0.264	0.265	0.260	0.261	0.261
$Loc \times Ind \times Year$ FE	Yes	Yes	Yes	Yes	Yes	Yes
$Cohort$ FE	Yes	Yes	Yes	Yes	Yes	Yes
$Firm\ level\ controls$	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.4: Industry Average Manager Turnover and Individual Firm Performance

This table shows the analysis of the industry average manager turnover and individual firm performance. The outcome variables are $D(Bankruptcy = 1)_{i,t+2}$ in the first two columns and $Asset\ growth_{i,t+2}$ in the last two columns. The average manager departures and appointments in firms (excluding firm i) operating in the same industry and within the same cohort are the main dependent variables of interest. In all regressions, manager turnover in firm i is controlled. Location-Industry, location-year and cohort-level fixed effects are also included. Firm-level accounting information is not controlled for in columns (1) and (3) regressions. Standard errors shown in parentheses are clustered at the industry, location and cohort levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	$D(Bankruptcy = 1)_{i,t+2}$		$Asset\ growth_{i,t+2}$	
$Average\ departure_{c.j-i,t}$	0.1575***	0.1573***	-0.1000**	-0.0971**
	(0.0286)	(0.0289)	(0.0291)	(0.0299)
$Average\ new_{c,j-i,t}$	-0.0560*	-0.0558*	0.0584**	0.0569**
	(0.0232)	(0.0230)	(0.0197)	(0.0189)
$Num\ departure_{it}$	0.0436***	0.0413***	-0.0311***	-0.0341***
	(0.0034)	(0.0034)	(0.0024)	(0.0028)
$Num\ new_{it}$	-0.0241***	-0.0227***	0.0461***	0.0481***
	(0.0021)	(0.0022)	(0.0052)	(0.0052)
$Num_{i,t-1}$	-0.0138***	-0.0127***	0.0034**	0.0051***
	(0.0014)	(0.0015)	(0.0011)	(0.0012)
$Bankruptcy\ rate_{c,j,t}$	0.1643*	0.1608*	0.0170	0.0147
	(0.0654)	(0.0649)	(0.0195)	(0.0203)
$Average\ asset\ growth_{c,j,t}$	-0.0828***	-0.0773**	-0.0373	-0.0297
	(0.0205)	(0.0206)	(0.0297)	(0.0312)
Observations	1,349,446	1,349,446	955,032	955,032
R-squared	0.031	0.039	0.020	0.031
Firm level control	No	Yes	No	Yes
$Location \times Year$ FE	Yes	Yes	Yes	Yes
$Location \times Industry$ FE	Yes	Yes	Yes	Yes
$Cohort$ FE	Yes	Yes	Yes	Yes

Table 1.5: Average Manager Turnover in (Non)Founder-Managed Firms and Firm Performances

This table shows the analysis of the industry average manager turnover in founder/non-founder-managed firms and individual firm performance. The outcome variables are $D(Bankruptcy = 1)_{i,t+2}$ and $Asset\ growth_{i,t+2}$ in Columns (1)-(2) and (3)-(4) respectively. The average manager departures and appointments in firms (excluding firm i) operating in same industry and within the same cohort are calculated based on founder and non-founder-managed firms separately. $Average\ departure^{F}_{c,j-i,t}$ and $Average\ departure^{NF}_{c,j-i,t}$ denote the average number of departures from founder and non-founder-managed firms. In all regressions, firm i's manager turnover is controlled for; location-industry, location-year and cohort-level fixed effects are also included. Firm level accounting information is not controlled for in Columns (1) and (3). Standard errors shown in parentheses are clustered at the industry, location and cohort level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	$D(Bankruptcy = 1)_{i,t+2}$		$Asset\ growth_{i,t+2}$	
$Average\ departure^{F}_{c.j-i,t}$	0.1346***	0.1347***	-0.0810**	-0.0756*
	(0.0328)	(0.0329)	(0.0208)	(0.0315)
$Average\ departure^{NF}_{c.j-i,t}$	0.0159**	0.0160**	-0.0144**	-0.0142**
	(0.0053)	(0.0057)	(0.0047)	(0.0048)
$Average\ new^{F}_{c,j-i,t}$	-0.0483*	-0.0478*	-0.0123	-0.0108
	(0.0236)	(0.0236)	(0.0392)	(0.0372)
$Average\ new^{NF}_{c,j-i,t}$	-0.0061	-0.0061	0.0211**	0.0204*
	(0.0057)	(0.0060)	(0.0061)	(0.0094)
$Num\ departure_{it}$	0.0441***	0.0419***	-0.0342***	-0.0395***
	(0.0034)	(0.0032)	(0.0058)	(0.0066)
$Num\ new_{it}$	-0.0244***	-0.0229***	0.0729***	0.0765***
	(0.0022)	(0.0024)	(0.0080)	(0.0092)
$Num_{i,t-1}$	-0.0138***	-0.0127***	0.0000	0.0030
	(0.0014)	(0.0015)	(0.0019)	(0.0018)
$Bankruptcy\ rate_{c,j,t}$	0.1897**	0.1860**	0.0818	0.0795
	(0.0680)	(0.0673)	(0.0458)	(0.0452)
$Average\ asset\ growth_{c,j,t}$	-0.2334***	-0.2208***	-0.2958*	-0.2668
	(0.0326)	(0.0323)	(0.1331)	(0.1344)
Observations	1,285,463	1,285,463	912,362	912,362
R-squared	0.030	0.038	0.018	0.029
Firm level control	No	Yes	No	Yes
$Location \times Year$ FE	Yes	Yes	Yes	Yes
$Location \times Industry$ FE	Yes	Yes	Yes	Yes
$Cohort$ FE	Yes	Yes	Yes	Yes

Table 1.6: Summary Statistics of LEGI Policy Sample

This table shows the summary statistics based on the samples used for LEGI analysis. Panel A presents the summary statistics of the control variables used in the analysis of manager turnover and firm performance. Panel B shows the number of firms going bankrupt, manager's mobility and manager turnover in different years relative to the policy's launch time. The standard errors are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Summary Statistics in 2003 before the sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Number of Managers			Total Assets (£M)			Leverage			Age		
	All	Close	Far	All	Close	Far	All	Close	Far	All	Close	Far
Mean	3.679	3.689	3.674	3.596	3.446	3.893	0.475	0.482	0.472	4.578	4.568	4.583
sd	1.279	1.308	1.265	6.273	6.166	6.325	0.387	0.387	0.387	2.409	2.416	2.407
Median	3	3	3	1.615	1.558	1.765	0.542	0.569	0.524	4	4	4
Number of observations	5,168	1,696	3,472	5,168	1,696	3,472	5,168	1,696	3,472	5,168	1,696	3,472
Mean difference close-far		0.016			-0.447			0.011			0.015	
		(0.038)			(0.723)			(0.011)			(0.071)	

Panel B: Bankruptcy and manager turnover over time

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Bankruptcy Num.			Avg. movers to policy			Avg. departure		
Relative Year	All	Close	Far	All	Close	Far	All	Close	Far
-2	121	51	70	0.1620	0.1606	0.1626	0.3298	0.2795	0.3545
-1	213	79	134	0.1740	0.1587	0.1830	0.3092	0.2875	0.3197
0	232	70	162	0.1579	0.2084	0.1271	0.2955	0.2889	0.2987
1	239	81	158	0.1827	0.2029	0.1710	0.2782	0.2676	0.2834
2	226	69	157	0.1538	0.1774	0.1419	0.2397	0.2735	0.2234
3	248	80	168	0.1981	0.2602	0.1705	0.2080	0.1661	0.2251
4	204	76	128	*Mean difference before LEGI*		0.0095	*Mean difference before LEGI*		-0.0395***
5	153	52	101	*Mean difference after LEGI*		0.0453***	*Mean difference after LEGI*		0.0011

Table 1.7: LEGI Effect on Manager Turnover: Close vs Far to Policy Firms

This table shows the estimations of the LEGI impact on manager turnover. The analysis is based on a sample of firms locate within a six-kilometre distance to the policy area, incorporated after 1995 and managed by at least two managers in 2003. $Close_i$ is a dummy variable equal to one if the firm is located within 3 km distance to the policy-supported area. $Post_t$ is an indicator equal to 1 if the year is after the LEGI launch. The dependent variables are concurrent counts of (net) departures and appointments shown in Columns (1) to (4). In Column (5), the dependent variable is the number of managers moving to the policy region, which is measured base on a sub-sample of managers for whom I can find subsequent managerial position appointments using universal management data from Orbis. All regressions are controlled for location and local authority fixed effects. Standard errors reported in parentheses are clustered at the industry and location level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Num. of departures	Net departures	Num. of new	Net new	Num. moving to policy
$Close_i$	-0.0274*	-0.0267**	0.0003	0.0010	0.0293
	(0.0143)	(0.0104)	(0.0128)	(0.0096)	(0.0205)
$Post_t$	-0.0820***	-0.0628***	-0.0297**	-0.0106**	0.0032
	(0.0161)	(0.0095)	(0.0110)	(0.0049)	(0.0180)
$Close_i \times Post_t$	0.0519**	0.0411***	0.0137	0.0028	0.0808**
	(0.0199)	(0.0126)	(0.0109)	(0.0088)	(0.0319)
Age	-0.0020	-0.0021	0.0005	0.0004	-0.0046
	(0.0026)	(0.0013)	(0.0023)	(0.0011)	(0.0032)
$Num\ of\ managers_{2003}$	0.0890***	0.0545***	0.0441***	0.0096***	0.0100*
	(0.0081)	(0.0064)	(0.0055)	(0.0030)	(0.0052)
$Total\ assets_{2003}$	0.0269***	0.0093**	0.0351***	0.0175***	-0.0000
	(0.0045)	(0.0037)	(0.0062)	(0.0032)	(0.0000)
$Leverage_{2003}$	-0.0132	-0.0093	-0.0034	0.0005	-0.0000
	(0.0130)	(0.0077)	(0.0155)	(0.0094)	(0.0000)
$Average\ departure_{c,j-i,t-1}$	0.0030	0.0233	-0.0287	-0.0084	-0.0203
	(0.0426)	(0.0470)	(0.0315)	(0.0268)	(0.0419)
Observations	19,972	19,972	19,972	19,972	3,035
R-squared	0.079	0.046	0.088	0.048	0.139
Location FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Table 1.8: LEGI Effect on Firm Performance: Close vs Far to Policy

This table reports the unconditional estimations of LEGI impact on firm performances. The analysis is based on the same set of samples as in Table 1.7. $Close_i$ is a dummy variable equal to 1 if the firm is located within a three-kilometre distance to the policy-supported area. $Post_t$ is an indicator equal to 1 if the year is after the LEGI launch, and 0 if before. All dependent variables are firm performance measurements in year $t + 2$. All the regressions are controlled for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1) D(Bankruptcy=1)	(2) Total Assets	(3) Fixed Assets	(4) Current Assets	(5) Profits
$Close_i$	-0.0064	-0.0965	-0.1562	0.0754	0.1024*
	(0.0115)	(0.2209)	(0.1363)	(0.1150)	(0.0512)
$Post_t$	0.0701***	0.1701	0.0742	0.1207**	0.0321
	(0.0088)	(0.1054)	(0.0816)	(0.0442)	(0.0627)
$Close_i \times Post_t$	0.0026	0.3587*	0.1487*	0.2384	0.2340
	(0.0109)	(0.1959)	(0.0752)	(0.1678)	(0.1375)
Age	-0.0088***	-0.0905	-0.0695	-0.0360	0.0087
	(0.0018)	(0.0653)	(0.0599)	(0.0249)	(0.0107)
$Num\ of\ managers_{2003}$	-0.0042	0.0334	-0.0498	0.0633	0.0190
	(0.0027)	(0.0818)	(0.0940)	(0.0405)	(0.0187)
$Total\ assets_{2003}$	-0.0188***	2.0512***	0.9518***	1.2160***	0.4208***
	(0.0029)	(0.5071)	(0.2659)	(0.2705)	(0.1145)
$Leverage_{2003}$	-0.0190	-0.8614	-0.8247	-0.3518	-0.0997
	(0.0143)	(0.7044)	(0.7235)	(0.2299)	(0.1369)
$Average\ departure_{c,j-i,t-1}$	0.0245*	0.1828	0.2505**	0.0199	0.0599
	(0.0121)	(0.1249)	(0.1047)	(0.0872)	(0.0453)
Logarithm transformation					
$Close_i \times Post_t$		-0.0970	0.0579	-0.0963	
		(0.0667)	(0.0524)	(0.0674)	
Observations	19,972	14,863	12,177	14,508	13,889
R-squared	0.149	0.256	0.184	0.330	0.182
Industry FE	Yes	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes	Yes

Table 1.9: Policy Impact on Manager Turnover: Different Sub-sample Groups

This table shows the estimations of LEGI impact on manager turnover following regression 1.4 $Close_i$ is a dummy variable equal to 1 if the firm is located within a three-kilometre distance to the policy-supported area. $Post_t$ is an indicator equal to 1 if the year is after the LEGI launch and 0 if before. Columns (1) and (3) are based on sub-samples of firms with high departure probabilities and Columns (2) and (4) are based on sub-samples of firms with low departure probabilities. Classification of high and low probabilities are based on predicted values from Equation 1.6. All dependent variables are firm performance measurements in year $t+2$. All the regressions are controlled for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location level. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)
	Number of departures		*Managers moving to policy*	
Sample	*High dep. prob.*	*Low dep. prob.*	*High dep. prob.*	*Low dep. prob.*
$Close_i$	-0.1008	0.0493**	0.0057	0.0541*
	(0.0551)	(0.0228)	(0.0307)	(0.0308)
$Post_t$	0.0253	-0.0287	-0.0452*	0.0408
	(0.0800)	(0.0184)	(0.0261)	(0.0269)
$Close_i \times Post_t$	0.0887**	-0.0169	0.1745***	-0.0091
	(0.0304)	(0.0321)	(0.0454)	(0.0462)
Age	-0.0073	-0.0012	-0.0047	-0.0021
	(0.0073)	(0.0021)	(0.0052)	(0.0044)
Num_{2003}	0.0757**	0.0303***	-0.0019	0.0107
	(0.0214)	(0.0065)	(0.0114)	(0.0067)
$Total\ assets_{2003}$	0.0113	0.0044	-0.0007	-0.0000
	(0.0193)	(0.0050)	(0.0031)	(0.0000)
$Leverage_{2003}$	0.0181	-0.0027	0.0172	-0.0000
	(0.0533)	(0.0163)	(0.0186)	(0.0000)
$Average\ departure_{c,j-i,t-1}$	-0.1144***	0.0311	-0.0203	-0.0542
	(0.0137)	(0.0687)	(0.0419)	(0.0595)
Observations	3,062	9,147	1,406	1,481
R-squared	0.099	0.016	0.179	0.159
Location FE	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes

Table 1.10: Difference-in-Difference-in-Difference Regression Results

This table reports the estimations from Equation 1.7. The analysis is based on the same sample set as in Table 1.7. $Close_i$ is a dummy variable equal to 1 if the firm is locate within a three-kilometre distance to the policy-supported area. $Post_t$ is an indicator equal to 1 if the year is after the LEGI launch and 0 if before. *Pr high* is a dummy variable that takes the value of 1 if the predicted probability from Equation 1.6 is above the 75% threshold value of 0.3591. All dependent variables are measured in year $t + 2$. The Wald estimations are calculated using point estimates and 95% confidence interval ranges divided by the point estimates in Column (6). All the regressions are controlled for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: DDD Estimates					
	(1)	(2)	(3)	(4)	(5)
	D(Bankruptcy=1)	Total assets	Fixed assets	Current assets	Profits
$Close_i$	-0.0069	0.0369	-0.0056	0.0438	0.0146
	(0.0084)	(0.1188)	(0.0437)	(0.0619)	(0.0260)
$Post_t$	0.0495***	0.2615***	0.0881*	0.1681***	0.0724***
	(0.0068)	(0.0543)	(0.0468)	(0.0414)	(0.0219)
$Close_i \times Post_t$	0.0015	0.0247	0.0210	0.0010	0.0506
	(0.0110)	(0.0733)	(0.0636)	(0.0377)	(0.0300)
$Pr\ high_{i,t}$	0.0185**	0.0971	0.0571	-0.0476	-0.0768
	(0.0086)	(0.1110)	(0.0655)	(0.0796)	(0.0736)
$Close_i \times Pr\ high_{it}$	-0.0234*	-0.1300	-0.0978	-0.0297	0.0132
	(0.0124)	(0.1386)	(0.1249)	(0.1005)	(0.0549)
$Post_t \times Pr\ high_{it}$	-0.0197*	0.7143**	0.7080**	0.2438	0.032
	(0.0114)	(0.3217)	(0.2782)	(0.1428)	(0.0544)
$Close_i \times Post_t \times Pr\ high_{it}$	0.0347**	-0.4233***	-0.5294**	0.0607	-0.1078
	(0.0155)	(0.1424)	(0.2177)	(0.1733)	(0.1244)
Log transformation					
$Close_i \times Post_t \times Pr\ high_{it}$		-0.1423***	-0.1394**	-0.0837	
		(0.0459)	(0.0515)	(0.0694)	
Observations	19,067	14,609	11,986	14,267	13,672
R-squared	0.165	0.219	0.161	0.312	0.258
Control variables	Age,$Total\ assets_{2003}$, $Leverage_{2003}$, Num_{2003}				
FE	Industry Location				
Cluster	Industry Location				

Panel B: Wald estimations: Causal impact						
	(1)	(2)	(3)	(4)	(5)	(6)
	Bankruptcy	Total assets	Fixed assets	Current assets	Profits	Num departure
$Close \times Post \times Pr\ High$	0.0347**	-0.4233***	-0.5294**	0.0607	-0.1078	0.0846**
95% confidence interval	[0.0043, 0.0650]	[-0.7173, -0.1294]	[-0.9787, -0.0800]	[-0.2970, 0.4185]	[-0.3646, 0.1490]	
Wald estimation	0.4102**	-5.0059***	-6.2607**	0.7178	-1.2748	
95% confidence interval corresponding bounds	[0.0504, 0.7688]	[-8.4824, -1.5306]	[-11.5737, -0.9466]	[-3.5124, 4.9486]	[-4.3116, 1.7621]	

Table 1.11: Difference-in-Difference-in-Difference Regression Results: Founder-Managed vs Non-Founder-Managed Firms

This table shows the estimations of Equation 1.7 using founder-managed and non-founder-managed firms in Panels A and B respectively. Founder-managed firms are classified as firms run by at least one manager who also owns stakes in pre-sample year 2003. For brevity, this table only reports the estimations of γ for each regression. *Log_ta*, *Log_fa* and *Log_ca* denote the logarithms of total assets, fixed assets and current assets. All regressions are controlled for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Founder-managed firms								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D(Bankruptcy=1)	Total Assets	Fixed Assets	Current Assets	Profits	Log_ta	Log_fa	Log_ca
$Close \times Post \times Pr\ high$	0.0304	-0.4835*	-0.6294*	0.0939	-0.1306	-0.1192	-0.0676	-0.1498
	(0.0400)	(0.2754)	(0.3593)	(0.2513)	(0.1525)	(0.1138)	(0.3631)	(0.1243)
Observations	15,299	11,476	9,791	11,237	10,820	11,476	9,791	11,237
R-squared	0.139	0.345	0.327	0.389	0.191	0.463	0.550	0.490
Panel B: Non-founder-managed firms								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D(Bankruptcy=1)	Total Assets	Fixed Assets	Current Assets	Profits	Log_ta	Log_fa	Log_ca
$Close \times Post \times Pr\ high$	0.1290*	0.1612	-0.0726	0.1881	0.1016	0.0084	-0.4724*	0.2159
	(0.0694)	(0.7475)	(0.6753)	(0.3333)	(0.2820)	(0.1790)	(0.2492)	(0.2611)
Observations	3,768	3,133	2,195	3,030	2,852	3,133	2,195	3,030
R-squared	0.126	0.551	0.424	0.542	0.525	0.583	0.513	0.563
Firm level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location, Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.12: Difference-in-Difference-in-Difference Regression Results: by Different Predicted Probability

This table shows estimations of Equation 1.7 using different control groups. For all the regressions, I include the observations with the highest 25% predicted departure probability in the treated group. Panel A shows the results comparing the top and second quartiles (between the top 25% and the median). Panel B shows the comparison between the top and third quartiles of the predicted manager departure probability. In Panel C, the regressions are based on observations of the bottom 25% and top 25% quartiles. For brevity, this table only reports the estimations of γ for each regression. *Log_ta*, Log_fa and *Log_ca* denote logarithms of total assets, fixed assets and current assets. All the regressions include firm-level controls and account for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Top quartile vs second quartile of predicted departure probability								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D(Bankruptcy=1)	Total Assets	Fixed Assets	Current Assets	Profits	Log_ta	Log_fa	Log_ca
Close × *Post* × *Pr high*	0.0180	0.5247	-0.1832	0.0132	0.4068	0.4383*	0.0730	-0.2229
	(0.0672)	(0.8143)	(1.1103)	(0.4640)	(0.2474)	(0.2252)	(0.4111)	(0.3199)
Observations	6,367	4,973	3,786	4,818	4,429	4,973	3,786	4,818
R-squared	0.143	0.431	0.438	0.487	0.354	0.523	0.517	0.528

Panel B: Top quartile vs third quartile of predicted departure probability								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	D(Bankruptcy=1)	Total Assets	Fixed Assets	Current Assets	Profits	Log_ta	Log_fa	Log_ca
Close × *Post* × *Pr high*	0.0406***	-0.7972	-0.2911	-0.7319	-0.9739	-0.0555	-0.2897	-0.1111
	(0.0048)	(0.8762)	(0.9113)	(1.2191)	(0.8215)	(0.2520)	(0.3471)	(0.1995)
Observations	6,359	4,867	3,748	4,842	4,465	4,867	3,748	4,842
R-squared	0.239	0.392	0.401	0.437	0.267	0.500	0.495	0.511

Panel C: Top quartile vs bottom quartile of predicted departure probability								
	(1)	(2)	(4)	(3)	(5)	(6)	(7)	(8)
	D(Bankruptcy=1)	Total Assets	Fixed Assets	Current Assets	Profits	Log_ta	Log_fa	Log_ca
Close × *Post* × *Pr high*	0.0971**	-0.5171	-0.5554	-0.3373	0.0541	-0.01519	-0.1315	0.0468
	(0.0369)	(0.5425)	(1.1107)	(0.4092)	(0.3296)	(0.1898)	(0.2998)	(0.3530)
Observations	6,292	4,643	3,607	4,461	4,245	4,643	3,607	4,461
R-squared	0.156	0.375	0.455	0.444	0.197	0.557	0.530	0.574
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location, Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Table 1.13: Difference-in-Difference-in-Difference Regression Results: by Age

This table shows estimations of the Equation 1.7 splitting sample based on firm age. Panel A shows estimations using young firm samples with firms incorporated after 2000, and Panel B shows results based on older firms incorporated between 1995 and 2000. For brevity, this table only reports the estimations of γ for each regression. *Log_ta*, *Log_fa* and *Log_ca* denote the logarithms of total assets, fixed assets and current assets. All regressions are controlled for industry and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Young firms incorporated after 2000

	(1) D(Bankruptcy=1)	(2) Total Assets	(3) Fixed Assets	(4) Current Assets	(5) Profits	(6) Log_ta	(7) Log_fa	(8) Log_ca
Close × *Post* × *Pr high*	0.0795*	-0.7960*	-0.6625	-0.4896**	-0.0745	-0.3004**	-0.2309**	-0.2692*
	(0.0431)	(0.4315)	(0.4409)	(0.2230)	(0.2182)	(0.1391)	(0.0918)	(0.1558)
Observations	9,467	6,690	5,664	6,569	6,340	6,690	5,664	6,569
R-squared	0.139	0.345	0.327	0.389	0.191	0.463	0.550	0.490

Panel B: Old firms incorporated between 1995 and 2000

	(1) D(Bankruptcy=1)	(2) Total Assets	(3) Fixed Assets	(4) Current Assets	(5) Profits	(6) Log_ta	(7) Log_fa	(8) Log_ca
Close × *Post* × *Pr high*	0.0407	0.1401	-0.0631	0.4642	0.3759	-0.1011	0.2833	-0.0969
	(0.0296)	(0.3993)	(0.2366)	(0.3884)	(0.2448)	(0.2058)	(0.1796)	(0.2358)
Observations	9,600	7,919	6,322	7,698	7,332	7,919	6,321	7,698
R-squared	0.126	0.551	0.424	0.542	0.525	0.583	0.513	0.563
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Location, Industry FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

1.6 Appendix

Data

The main data sources used in this study are Orbis and Financial Analysis Made Easy (FAME) databases provided by Bureau van Dijk (BvD). I augment the manager and shareholder information with geographical data at Lower Layer Super Output Area (LSOA) levels from the UK Office for National Statistics (ONS) in the analysis in Section 1.3.

Orbis is a comprehensive database covering manager and ownership information for both private and public firms worldwide. UK manager information is originally extracted from Companies House. Companies House is the business registry sponsored by the UK Department for Business, Energy & Industrial Strategy. Companies House provides services to establish and dissolve limited companies, register and store company information and make it available to the public.[44]

According to the Companies Act (1985), all companies in UK should file annual reports and annual financial statements to Companies House. Directors of private companies are responsible for ensuring that accounts are delivered within nine months from the end of the accounting reference period. Late filings are subject to penalty fees and failure to file is considered a criminal offence.[45] The directors are also required to update information to Companies House within two weeks whenever they make changes to directors[46] or their personal details and they must inform Companies House within a month of new shares being issued. My original extract of managerial information from Orbis encompasses 7,916,404 firms. The earliest date of incorporation is 1856, the earliest managerial information begins from 1942 and becomes more readily available from the 1990s onwards.

The Orbis dataset keeps managers' detailed information from annual report filings. Within each company, every appointment is recorded, along with details of the manager's full name, appointment date, resignation date and type of position, as well as characteristics such as gender, birth date and nationality. As managers can work at different companies, Orbis also provides a Unique Contact Identifier for each individual, following a complex algorithm with both company and personal information. This added value of the database is especially important for examining the causal impact of manager turnover in Section 1.3. Using the manager's ID, I can now not only link managers to their previous positions before new appointments, but also follow which new firms they join after they resign from their previous firm.

In addition, Orbis covers information from annual reports including each shareholder's name,

[44]For more information about Companies House, see `https://www.gov.uk/government/organisations/\companies-house.`

[45]See https://www.gov.uk/government/publications/late-filing-penalties/late-filing-penalties.

[46]In annual reports, details of directors and secretaries are both included, although this paper only focuses on directors. According to the Companies Act, a director is "legally responsible for running the company and making sure company accounts and reports are properly prepared. In Orbis, the type of position for directors is classified as senior managers. Considering that the analysis sample in this paper mainly covers newly established private small firms, I will refer to directors as managers from this point.

type (individual or firm), percentage of shares and the date of ownership information on a yearly basis. Historical shareholder information is available for the past ten years on a web-version database. For the scope of this study, I obtained shareholder information of every newly established firm since 2005.

Besides the firm-level detailed information, another advantage of this database is its good overall coverage of private firms in the UK. Specifically, Orbis does not delete firms when they become inactive or liquidated. In addition, UK firms are required to file annual reports containing the same quality of information about managers and shareholders, regardless of firm age and size, to Companies House. These two features avoid not only the survival ship bias but also the sample-selection problems associated with other private firm datasets[47].

The FAME database contains up to ten years of historical financial information for each firm that files annual accounts to Companies House. The accounting standards vary with firm sizes according to the Companies Act, and detailed accounts are only required for medium and large-sized firms. Although profit-and-loss accounts and cash flow statements are poorly covered in small and young firms, this paper focuses on assets and profit-and-loss from balance sheet accounts. In order to avoid the truncation of ten years' worth of data provided by FAME, I combined the achieve disks with the web-version data to extend the historical financial information from 2003 to 2016.

FAME also provides firm registration information such as incorporation date, legal forms, the postcode, and its industry as well as the company's status. Firm status information with exact information dates are important to define bankruptcy status as one main measure of firm performance. The company statuses are classified into four main categories: active inactive in liquidation, and dissolved. The information date associated with the company status is the date when the filing is accepted at Companies House. Also, a company will be struck off the register after two months of notice sent to Companies House. After this, a second notice will be published in the Gazette this will mean that the company will no longer legally exist (it will have been dissolved). In the FAME data, the dissolution date is recorded as the second-notice date. The firm registry information is kept up to date in the web-version and I use the snapshot of 2017 information for the main analysis in this paper.

A postcode is assigned with geographic shape data for analysis in Section 1.3. I classify each LSOA into LEGI policy LSOAs and no-policy LSOAs. Policy LSOAs are considered to be within a policy region if it is located within the boundaries of a LA receiving LEGI funding. The exact geocoding comes from the fact that the LSOA boundaries do not cross the LA boundaries. In this paper, I focus on firms in no-policy LSOAs with differential distances to LEGI policy regions. I calculate the distance between LSOA centroids using the population-weighted centroid at LSOA level data from ONS. An LSOA is considered to be close to the policy region if it is located within

[47]In the US, the Fed conducted surveys of small business finances, which included owner characteristics, firm size, income and balance sheet information. The survey was ran from 1987 to 2003 and took place once every five years. Another type survey data with similar information used in this paper is the Kauffman Firm Survey, which details 4,928 business and was founded in 2004. The Kauffman Firm Survey data was used in Robb and Robinson (2014).

a three-kilometre distance to the nearest policy LSOA; and those located within a three to six-kilometre distance to the nearest policy LSOA are in the control group. Thus, following the above steps of assigning firms to LSOAs and then separating LSOAs into treated and control areas, I am able to classify my firms into close (treated) and far (control) groups.

Matching shareholder and manager names

To identify managers as founder managers, I need to combine the manager data with ownership information. Although manager details and ownership information are both obtained from the Orbis dataset, the same individual can be contained within two parts of the dataset with slightly different names. This may be because of data input errors or because of the different standardisation algorithms.

Considering the size of the dataset and the relatively low diversity of name origins, I simplified the rules stipulated in Bena and Xu (2017) into the following steps:

1. I kept all the individual shareholder data and transformed it to firm-person panel

2. I standardised the names in shareholder data and ownership datasets. I eliminated the prefix and titles in front of names, eliminated all the spaces between the names, and transformed all the letters to lower case.

3. Within each firm, I matched shareholder names and manager names, including direct match, or using initials with their last names as well as swapping the first and last names.

This simplified algorithm worked well in my dataset both in terms of accuracy and the speed of calculation. Remember that almost 90% of managers have British nationality so the names are already standardised. One drawback to this method is that I did not specifically correct any spelling errors, which are relatively rare in my sample.

Table A1.1: Variable Definitions

Variable	Definition
Num_{it}	Number of managers in firm i at the end of year t
$\triangle Num_{it}$	Change in number of managers from t to $t+1$
$Num\ departure_{it}$	Number of managers departing firm i in year t
$Num\ new_{it}$	Number of managers appointed to firm i in year t
$Net\ replacement_{it}$	Number of managers being replaced in firm i in year t
$Net\ departure_{it}$	Number of managers departing the firm without replacement in firm i in year t
$Net\ new_{it}$	Number of net new managers joining firm i in year t
$Num\ of\ founder\ departure_{it}$	Number of founder manager departures in firm i year t; founder managers are those who joined the firm and own shares at incorporation date (within three months of incorporation date).
$Num\ of\ nonfounder\ departure_{it}$	Number of nonfounder manager departures in firm i year t
$Num\ of\ founder\ managers_{it}$	Number of founder managers in firm i at the end of year t
$Num\ of\ nonfounder\ managers_{it}$	Number of nonfounder managers at the end of year t
$Average\ departure_{c,j-i,t}$	Average number of departing managers within the same cohort c, and industry j excluding firm i in year t
$Average\ no\ replacement_{c,j-i,t}$	Average number of net departing managers within the same cohort c, and industry j excluding firm i in year t
$Average\ replacement_{c,j-i,t}$	Average number of replaced managers within the same cohort c, and industry j excluding firm i in year t
$Average\ new\ manager_{c,j-i,t}$	Average number of new managers within the same cohort c, and industry j excluding firm i in year t
$Average\ net\ new\ manager_{c,j-i,t}$	Average number of net new managers within the same cohort c, and industry j excluding firm i in year t
$Bankruptcy\ rate_{jt}$	Total number of firms going bankrupt in year t divided by the total number of firms in industry j at the beginning of year t
$Total\ assets_{it}$	Total assets in firm i year t
$Bank\ deposits_{it}$	Bank deposits in firm i year t
$Profit\ loss\ account_{it}$	Profits-and-loss account (from balance sheet) in firm i year t
$Leverage_{it}$	$Leverage_{it} = \frac{(Current\ liabilities_{it} + long\ term\ liabilities_{it})}{Total\ assets_{it}}$
$Fixed\ assets_{it}$	Fixed assets in firm i year t
$Current\ assets_{it}$	Current assets in firm i year t
$D(Bankruptcy = 1)_{i,t+2}$	The Dummy variable takes the value of 1 if the firm i status is liquidated or dissolved in year t+2
$Asset\ growth_{i,t+2}$	Total assets growth rate in firm i in year $t$and $t+2$. $\frac{2\times(Total\ assets_{i,t+2} - Total\ assets_{i,t})}{(Total\ assets_{i,t+2} + Total\ assets_{i,t})}$
$Close_i$	The Dummy variable takes the value of 1 if the firm is outside but within three kilometres of the nearest LEGI policy area
$Post_t$	The Dummy variable takes the value of 1 in years 2006/2007 2009 for firms close to the first/second round of LEGI program.
$Pr\ high_{i,t}$	The Dummy variable takes the value of 1 if the predicted departure probability in firm i year t is above the 75% threshold
Age	The age of the firm is defined as the firm's age in 2003.
Log_ta, Log_fa, Log_ca	Logarithm of total assets, fixed assets and current assets

Table A1.2: Manager Turnover and Firm Survival Dynamics

This table summarises firm entry and exit dynamics over time. Analysis sample includes 336,000 private limited new firms incorporated from 2005 to 2010 with at least two managers at incorporation. The sample excludes firms with one manager at incorporation, firms belonging to business groups, firms in M&A activities or those backed by VC firms; non-trading businesses are also excluded.

Panel A: Manager characteristics at incorporation

Year		2,005	2,006	2007	2008	2009	2010	2011	2012	2013	2014	2015
Num. of new firms		62,640	63,310	58,604	48,750	49,406	53,182					
	All		278	4,006	11,155	28,240	22,740	21,344	21,665	18,084	17,688	14,016
	2005		278	3,780	7,198	10,097	4,571	2,987	2,241	2,092	2,027	1,651
	2006			226	3,705	11,293	5,641	3,617	2,839	2,579	2,480	1,958
Num. of firms	2007				252	6,472	6,519	4,140	3,217	2,887	2,861	2,179
going bankrupt	2008					378	5,697	4,479	3,132	2,825	2,873	2,264
	2009						312	5,642	4,510	3,365	3,537	2,829
	2010							479	5,726	4,336	3,910	3,135

Panel B: Manager characteristics at incorporation

	Mean	sd	p25	p50	p75	No. firm or manager
Number of managers	2.2288	0.6048	2	2	2	336,000
Number of founder managers	1.7089	0.8359	1	2	2	336,000
Number of female managers	0.6510	0.6574	0	1	1	336,000
Number of UK managers	2.0349	0.8341	2	2	2	336,000
Number of experienced managers	0.0476	0.2918	0	0	0	336,000
Manager age	41.777	11.265	33	40	49	762,999

Table A1.3: Manager Turnover and Firm Bankruptcy: Estimations from Duration Regression

This table shows the duration regression estimations following Regression Equation 1.2 based on a proportional hazards model. All regressions are stratified at the $Location \times Industry \times Year$ and $Cohort$ groups.

	(1)	(2)	(3)
	$D(Bankruptcy = 1)_{i,t+2}$		
$\triangle Num_{it}$	-0.3708***		
	(0.0127)		
$Num\ departure_{it}$		0.5059***	
		(0.0138)	
$Num\ new_{it}$		-0.3792***	
		(0.0208)	
$Net\ departure_{i,t}$			0.1556***
			(0.0220)
$Num\ replacements_{i,t}$			0.4950***
			(0.0148)
$Net\ new_{i,t}$			-0.4295***
			(0.0353)
$Num_{i,t-1}$		-0.2318***	-0.2309***
		(0.0095)	(0.0095)
Observations	1,237,315	1,237,315	1,237,315
Strata level		Cohort, Location-Industry	
Cluster		Location-Industry, Cohort	
Model-pvalue	0.0000	0.0000	0.0000

Table A1.4: Industry Average Manager Turnover and Firm Bankruptcy: Estimations from Duration Regressions

This table shows the duration regression estimations following Equation 1.3 based on a proportional hazards model. All regressions are stratified at the $Location \times Industry$, $Location \times Year$ and $Cohort$ groups.

	(1)	(2)	(3)	(4)
	$D(Bankrutpcy = 1)_{i,t+2}$			
$Average\ departure_{c.j-i,t}$	0.1984***	0.1911***		
	(0.0373)	(0.0337)		
$Average\ new_{c,j-i,t}$	0.0492***	0.0569***		
	(0.0163)	(0.0167)		
$Average\ departure^{F}_{c.j-i,t}$			0.1390***	0.1324***
			(0.0316)	(0.0358)
$Average\ departure^{NF}_{c.j-i,t}$			0.0141**	0.0143**
			(0.0053)	(0.0049)
$Average\ new^{F}_{c,j-i,t}$			-0.9684*	-0.3619
			(0.4846)	(0.3062)
$Average\ new^{NF}_{c,j-i,t}$			-1.4635	-1.1391
			(2.4258)	(1.8399)
$Num\ departure_{it}$	0.3814***	0.3836***	0.3787***	0.3813***
	(0.0133)	(0.0130)	(0.0134)	(0.0131)
$Num\ new_{it}$	-0.3135***	-0.3349***	-0.3213***	-0.3409***
	(0.0203)	(0.0201)	(0.0213)	(0.0210)
Industry level controls	$Bankruptcy\ rate_{c,j,t}$, $Average\ asset\ growth_{c,j,t}$			
Firm character controls	No	Yes	No	Yes
Observations	1,236,621	1,236,621	1,225,553	1,225,553
Strata level	Cohort, Location-Industry, Location-Year			
Cluster	Location, Industry, Cohort			
Model-pvalue	0.0000	0.0000	0.0000	0.0000

Table A1.5: Estimating the Departure Probability—Logit Regression

This table shows the Logit regression results from Estimating Equation 1.6. Columns (1) and (2) show the number of managers, total assets and leverage ratios in 2003 as control variables; and in Columns (3) and (4) lagged average departure numbers at the same industry and cohort levels are also included. For the analysis in Section 1.3, I use the specification in Column (3) to estimate the probability of a departure.

	(1)	(2)	(3)	(4)
	$D(Num\ departure > 0)$			
$Close_i$	-0.1076*	-0.1100*	-0.1035	-0.1059
	-0.0639	-0.0639	-0.0644	-0.0644
$Post_t$	-0.4026***	0.0616	-0.3845***	0.0746
	-0.0481	-0.1109	-0.0487	-0.1123
$Close_i \times Post_t$	0.2249***	0.2310***	0.2120**	0.2182***
	-0.0823	-0.0825	-0.0832	-0.0834
Age	-0.0049*	-0.0045*	-0.0052*	-0.0047*
	-0.0028	-0.0024	-0.0029	-0.0024
$Num2003$	0.1988***	0.1997***	0.1963***	0.1971***
	-0.0152	-0.0152	-0.0152	-0.0152
$Total\ assets_{2003}$	0.1115***	0.1126***	0.1141***	0.1152***
	-0.0158	-0.0159	-0.0159	-0.016
$Leverage_{2003}$	-0.1221*	-0.1212*	-0.1267*	-0.1257*
	-0.0653	-0.0654	-0.0657	-0.0658
$Average\ departure_{c,j-i,t-1}$			0.0189	-0.0227
			-0.1518	-0.1532
Constant	-1.8447***	-1.8570***	-1.9767***	-1.9754***
	-0.4928	-0.4949	-0.5926	-0.5941
Observations	19,444	19,444	19,031	19,031
Location Industry FE	Yes	Yes	Yes	Yes
Year FE		Yes		Yes
Pseudo R-square	0.0807	0.0828	0.0784	0.0807

Table A1.6: LEGI Policy and Manager Departures: Firms with Less Than Three Managers

This table shows the estimations of the LEGI impact on manager turnover based on an alternative sample of firms with less than three managers in the pre-sampling year (2003). Other sample restrictions are similar to before: firms located within a six-kilometre distance of the policy area, those incorporated after 1995. $Close_i$ is a dummy variable equal to 1 if the firm is located within a three-kilometre distance to the policy-supported areas. $Post_t$ is an indicator equal to 1 if the year is after the LEGI launch. The dependent variables are concurrent counts of (net) departures and appointments in Columns (1) to (4). In Column(5), the dependent variable is the number of managers that move to a policy region, which is measured based on a sub-sample of managers for whom I can find subsequent managerial position appointments using the universal management data from Orbis. All regressions are controlled for location and LA-fixed effects. Standard errors reported in parentheses are clustered at the industry and location levels. *, **, and *** denote statistical significance at the 10%, 5%, and 1% levels, respectively.

	(1)	(2)	(3)	(4)	(5)
	Num. of departures	Net departures	Num. of new	Net new	Num. moving to policy areas
$Close_i$	-0.0011	0.0004	0.0019	0.0034*	0.0422
	(0.0029)	(0.0020)	(0.0033)	(0.0018)	(0.0285)
$Post_t$	-0.0032*	0.0010	-0.0103***	-0.0061***	0.0129*
	(0.0018)	(0.0012)	(0.0020)	(0.0012)	(0.0069)
$Close_i \times Post_t$	0.0052	0.0013	0.0015	-0.0024	0.0617
	(0.0069)	(0.0029)	(0.0037)	(0.0020)	(0.0757)
Age	-0.0005	-0.0004**	-0.0001	0.0000	-0.0020
	(0.0004)	(0.0002)	(0.0006)	(0.0003)	(0.0028)
Num_{2003}	0.0369***	0.0319***	-0.0063**	-0.0113***	0.0115
	(0.0023)	(0.0018)	(0.0029)	(0.0023)	(0.0157)
$Total\ assets_{2003}$	0.0255***	0.0099***	0.0416***	0.0260***	-0.0000***
	(0.0062)	(0.0029)	(0.0091)	(0.0058)	(0.0000)
$Leverage_{2003}$	-0.0031	0.0018	-0.0057	-0.0008	-0.0000***
	(0.0040)	(0.0025)	(0.0040)	(0.0023)	(0.0000)
$Average\ departure_{c,j-i,t-1}$	0.0090	0.0110	-0.0015	0.0005	-0.0917
	(0.0094)	(0.0086)	(0.0096)	(0.0072)	(0.0888)
Observations	139,030	139,030	139,030	139,030	1,304
R-squared	0.026	0.019	0.025	0.016	0.156
Location FE	Yes	Yes	Yes	Yes	Yes
Industry FE	Yes	Yes	Yes	Yes	Yes

Table A1.7: LEGI Policy, Manager Departures, Firm Performance—Robustness Check

This table shows the estimations of the LEGI impact on manager turnover, firm performance in close and far firms in Panel A and B respectively. The DDD estimations is displayed in Panel C. All regressions are estimated with firm fixed effects, industry fixed effects and location fixed effects. Variable definitions are the same as previous tables.

Panel A: LEGI policy and manager turnover: close vs far

	(1)	(2)	(3)	(4)	(5)
	Num. of departures	Net departures	Num. of new	Net new	Num. moving to policy
$Close_i \times Post_t$	0.0546**	0.0404*	0.0219	0.0077	0.0823**
	(0.0208)	(0.0159)	(0.0161)	(0.0100)	(0.0438)
Observations	19,978	19,978	19,978	19,978	3,045
R-squared	0.237	0.167	0.294	0.212	0.209
FE	Firm, Industry, Location				

Panel B: LEGI policy and unconditional firm performance: close vs far

	(1)	(2)	(3)	(4)
	Total Assets	Fixed Assets	Current Assets	Profits
$Close_i \times Post_t$	0.3614**	0.1905*	0.2369	0.2300*
	(0.1710)	(0.0967)	(0.1452)	(0.1276)
Log transformation				
$Close_i \times Post_t$	0.0473	-0.0254	-0.0947	
	(0.0438)	(0.0487)	(0.0625)	
Observations	14,889	12,919	14,683	13,394
R-squared	0.724	0.715	0.841	0.628
Industry FE	Yes	Yes	Yes	Yes
Location FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Panel C: DDD regression results

	(1)	(2)	(3)	(4)
	Total Assets	Fixed Assets	Current Assets	Profits
$Close \times Post \times Pr\ high$	-0.3136***	-0.3616**	-0.1223	0.0881
	(0.0880)	(0.1586)	(0.2143)	(0.1252)
Log transformation				
$Close \times Post \times Pr\ high$	-0.0259**	-0.0363**	-0.0238	
	(0.0118)	(0.0143)	(0.1020)	
Observations	14,631	11,993	14,293	13,690
R-squared	0.779	0.749	0.785	0.723
FE	Firm, Industry, Location			

Table A1.8: LEGI Policy and Manager Departures—Robustness Check

This table is a robust test of the correlation between manager turnover and firm performance. This table corresponds to the Table 1.2. Instead of using absolute numbers of manager departure and manager appointments, I use dummy variables $Dum\ Departure_{it}$ and $Dum\ New_{it}$. They take the value of one if there is a positive number of departure or new appointment.

	(1)	(2)
	$D(Bankruptcy = 1)_{i,t+2}$	$Asset\ Growth_{i,t+2}$
$Dum\ Departure_{it}$	0.0777***	-0.0429**
	(0.0036)	(0.0116)
$Dum\ New_{it}$	-0.0489***	0.1129***
	(0.0040)	(0.0109)
$Num_{i,t-1}$	-0.0194***	0.0019
	(0.0019)	(0.0014)
$Total\ Assets_{i,t-1}$	0.0185	-0.3008
	(0.0359)	(0.1762)
$Bank\ Deposits_{i,t-1}$	-1.0787**	0.2307
	(0.3477)	(1.0772)
$Profits_{i,t-1}$	-0.0268	0.8808*
	(0.0522)	(0.4352)
$Leverage_{i,t-1}$	0.0297***	0.0460***
	(0.0021)	(0.0031)
Observations	1,217,158	758,474
R-squared	0.125	0.106
$Loc \times Ind \times Year\ FE$	Yes	Yes
$Cohort\ FE$	Yes	Yes

Table A1.9: LEGI Policy and Manager Departures—Robustness Check

This table is a robust test of the estimations of the LEGI impact on manager turnover. This table corresponds to the Table 1.7, with dummy variables as outcome variables.

	(1)	(2)	(3)	(4)	(5)
	D(Num. of departures>0)	D(Net departures>0)	D(Num. of new>0)	D(Net new>0)	D(Num. move to policy>0)
$Close_i$	-0.0154*	-0.0182***	-0.0025	0.0007	0.0211
	(0.0085)	(0.0065)	(0.0071)	(0.0052)	(0.0176)
$Post_t$	-0.0532***	-0.0456***	-0.0192**	-0.0060*	-0.0019
	(0.0091)	(0.0055)	(0.0075)	(0.0029)	(0.0157)
$Close_i \times Post_t$	0.0300**	0.0285**	0.0099*	0.0016	0.0749***
	(0.0118)	(0.0103)	(0.0055)	(0.0046)	(0.0273)
Age	-0.0009	-0.0013	0.0013	0.0008	-0.0044
	(0.0018)	(0.0013)	(0.0016)	(0.0009)	(0.0028)
$Num\ of\ managers_{2003}$	0.0343***	0.0219***	0.0210***	0.0056**	0.0102**
	(0.0052)	(0.0041)	(0.0028)	(0.0022)	(0.0046)
$Total\ Assets_{2003}$	0.0175***	0.0082***	0.0227***	0.0125***	-0.0000
	(0.0033)	(0.0025)	(0.0035)	(0.0019)	(0.0000)
$Leverage_{2003}$	-0.0179**	-0.0127*	-0.0069	-0.0014	-0.0000
	(0.0081)	(0.0063)	(0.0099)	(0.0070)	(0.0000)
$Average\ departure_{c,j-i,t-1}$	0.0039	0.0089	-0.0022	-0.0002	-0.0149
	(0.0184)	(0.0214)	(0.0145)	(0.0149)	(0.0399)
Observations	19,972	19,972	19,972	19,972	3,035
R-squared	0.067	0.033	0.094	0.046	0.132
Location FE	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES

Chapter 2

Dissecting the Effect of Financial Constraints on Small Firms

Managers of small firms often complain that they are unable to grow because they have difficulties raising finance and lack the collateral that lenders require as security for loans. For example, in a survey of small firms in UK, only 18% of those that sought finance said they obtained all they needed, and 20% cited insufficient security as the reason why their financial provider rejected their application for more funds (BIS, 2012). Consistent with managers' contentions, an extensive theoretical and empirical literature suggests that small firms are constrained in their access to finance (see: Whited (1992); Rajan and Zingales (1998); Beck et al. (2005); Liberti and Sturgess (2014)). How do small firms adapt to financing constraints? And what are the costs of these adjustments? In this paper, we answer these relatively unexplored questions, using as laboratory the Great Recession. We exploit differential access to a Credit Guarantee Scheme as exogenous firm-level determinant of financial access, and take advantage of novel administrative small-firm data in the UK. We show compelling evidence that financial constraints during the crisis prolonged the real effects of negative demand shocks, particularly by affecting small firms' ability to finance employment rather than fixed assets.[1] Our results provide *prima facie* evidence of small firm's direct need to finance labor (cf., Benmelech et al. (2015); Bakke and Whited (2012); Pica and Pagano (2014)); and highlight how financial

[1]This evidence is supported by the result found in Campello et al. (2010) that firms planned to cut employment during the crisis.

constraints can be more binding for employment than fixed assets, likely because fixed assets can be pledged as collateral whereas employees cannot (e.g., Almeida et al. (2011)).

Our empirical strategy exploits the revenue-based eligibility requirements of a Credit Guarantee Scheme (CGS) launched in the UK during 2009. CGSs were prevalent policy tools used during the Great Recession that provided credit guarantees for small firms by promising to pay lenders a fraction of firms' outstanding balances in case of default (OECD, 2018; World Bank, 2015).[2] We use a rich and novel dataset for small UK firms reporting revenues in 2008. Our sample includes businesses that were within a small window around the CGS eligibility threshold. We analyze the data using a difference-in-difference methodology that compares trends between eligible and non-eligible firms during 2005-2011 and provides an Intent-to-Treat (ITT) estimate of the effect of the CGS on corporate outcomes (cf., Angrist and Pischke (2008)). Our main identification assumptions are that the CGS increased access to external finance for eligible firms, and that, absent this scheme, investment opportunities and cost of capital would have evolved similarly for eligible and non-eligible businesses.

Our results show that after the scheme's launch, eligible firms increased their external debt and did not substitute for (or attract) other sources of finance, such as debt and equity from owners or trade credit. Eligible firms fired relatively fewer employees and started rehiring faster, although they disinvested as much as non-eligible businesses in 2009 and showed no relative investment recovery during 2010-2011. Eligible firms also performed better on several dimensions: they had relatively higher revenues, costs of goods sold, profits, and survival rates. We validate our approach by performing placebo tests using firms that did not qualify for the CGS because they were active in non-qualifying industries. We also show evidence against several methodological concerns, such as potential biases from sorting of firms, spurious trends, window selection, and serial correlation of outcomes.

Our interpretation of these results is that employment rather than capital investments was the main margin of adjustment to the financial constraints that small

[2]More than half of all countries in the world have a CGS for small firms, and the number is growing. The need for these studies has been widely recognized, including as part of the G20/OECD High Level Principles on SME Financing (see: G20/OECD (2015)) and in public guarantee arrangements (World Bank, 2015).

firms faced during the crisis. The insensitivity of capital is not a mechanical consequence of scheme requirements, because the CGS imposes no restriction on uses of funds, dedicated loan lines for fixed assets exist in the scheme, and the maximum loan size (£1M) exceeds both the average unconditional investment in the sample (£0.3M) and the average disinvestment during 2009 (£0.9M). In addition, while the irreversible nature of capital investments may induce companies to avoid such investments during the slump, this explanation is less convincing during the recovery years in the later part of our sample. Instead, a more plausible explanation is that the "collateral pledgeability value" of plants, property, and equipment makes investment in fixed assets less sensitive to financial constraints than investment in employment (cf., Benmelech and Bergman (2009); Almeida et al. (2011)). We remain agnostic about the underlying cause of financial constraints during the crisis. However, additional results suggest that these constraints were induced partly by the well-documented tightening of banks' lending requirements (cf., Ivashina and Scharfstein (2010), rather than exclusively by potential decreases in the value of firms' pre-existing redeployable collateral during the crisis. In particular, we find that the results are invariant to the tangibility of firms' assets pre-crisis, which is used as a proxy of firms' redeployable collateral values (cf., Benmelech and Bergman (2009)).

Other alternative interpretations are less consistent with the findings. The main alternative explanation is that the CGS did not relax firms' financial constraints, but rather set incentives for firms or banks to make inefficient investment and employment decisions (i.e., negative net present value projects). For example, several theories show that collateral pledges by firms to lenders helps discipline managers and set monitoring incentives for banks (e.g., Chan and Thakor (1987); Rajan and Winton (1995); Park (2000); Liberti and Sturgess (2014)). In addition, CGS critics contend that public guarantees can deteriorate incentives of banks and borrowers (cf., Lelarge et al. (2010); D'Acunto et al. (2017)). A related alternative interpretation is that layoffs during the crisis were corrections for the negative present value projects pursued before it, and that access to guaranteed loans allowed eligible firms to avoid these efficient but privately costly corrections. For example, several papers show that the boom period preceding the crisis led to market distortions (Adelino

et al., 2015; Gopinath et al., 2017; Borio et al., 2016; Charles et al., 2016). Evidence also exists for lay-offs being personally costly to managers and/or generating reputational concerns for firms (Agarwal and Kolev, 2017; Folger and Skarlicki, 1998). The difficulties of empirically measuring investment opportunities are well known, and like previous papers in the literature, we are unable to fully ascertain whether the projects that eligible firms pursue were value maximizing. However, we exploit the richness of the UK data to present suggestive evidence against these alternative narratives. Chiefly, the increased relative performance and survival of eligible firms are the main pieces of evidence supporting our preferred interpretation, as negative net present value projects would instead likely lead to decreases in profitability and survival.

Under the exclusion restriction that the CGS affected firm outcomes by increasing external debt and not through any other channel, we estimate the sensitivity of employment to external finance using an instrumental variables (IV) approach. We argue that this exclusion restriction is plausible in our empirical setting for several reasons. For example, the small size of the scheme (relative to the UK financial system) implies that its launch is unlikely to have changed the aggregate cost of capital in the market (and thus non-borrowers' cost of finance). The pressing macroeconomic conditions at the time also make it unlikely that firms cut their firing in anticipation of borrowing through the EFG in the future. However, we note that the exclusion restriction is fundamentally untestable, and our sensitivity estimates should be interpreted with this caveat in mind.

The IV estimates imply that a £100,000 increase in external finance leads to 1.3 additional employees. This sensitivity suggests that the types of workers retained during the crisis by EFG borrowers were high-skill employees (the average annual salary in UK is £28,677). This type of employees can be very costly to retain by constrained firms in recessions (particularly in the short term), but can presumably generate large returns for firms, for example by increasing firm productivity during the recovery (otherwise firms would not retain and rehire these employees). Consistent with this notion, we estimate large returns to external finance, measured as additional gross profits per unit of external debt. The IV estimates show that returns average 53% per year in the "complier" businesses that respond to the re-

lief. These estimates point to sizeable efficiency costs of financial constraints in small firms, as the annual marginal profitability is more than six times the average scheme interest rates (7.8%) and more than nine times the average UK loan rates (5.8%) for small businesses. They are however comparable to the return to capital estimates for constrained businesses in developing economies (de Mel et al., 2008; Banerjee and Duflo, 2014; Udry and Anagol, 2006).

Our results are of interest along several dimensions. First, we contribute to the extensive literature on the real effects of financial constraints (e.g., Kaplan and Zingales (1997); Blanchard et al. (1994); Lamont (1997); Rauh (2006)), by showing that these constraints can lead small firms to make inefficient employment decisions. Moreover, we provide novel empirical evidence that financial constraints can be more binding for employment than collaterizable assets (e.g., Almeida et al. (2011)). The results are especially consistent with theoretical and empirical work suggesting that firms face collateral constraints (Stiglitz and Weiss, 1987; Besanko and Thakor, 1987; Whited, 1992; Kiyotaki and Moore, 1997; Hennessy et al., 2005; Liberti and Sturgess, 2014). Relative to the parallel literature on the collateral channel that shows how shocks to real estate assets have investment and employment effects, we focus on a set of firms that are not included in these empirical exercises by design—i.e., the majority of small businesses that do not own real estate and are not owned by real estate owners.[3]

Second, we contribute to the finance and labor literature by providing compelling evidence of firms' intrinsic need to finance labor, as evidenced by the robust employment reactions and the simultaneous imperceptible investment adjustments to the CGS (cf., Bakke and Whited (2012)). Previous studies have looked for this evidence by measuring employment sensitivities to financial shocks (e.g., Chodorow-Reich (2013); Burcu et al. (2015); Benmelech et al. (2019)).[4] However, the link between these sensitivities and firms' intrinsic need to finance labor is not straightforward, as these sensitivities may be driven instead by the mechanical employment adjust-

[3]Some of the papers in the collateral channel literature include: Chaney et al. (2012); Schmalz et al. (2017); Jensen et al. (2014); Corradin and Popov (2015); Adelino et al. (2015); Kerr et al. (2015); and Bahaj et al. (2018).

[4]Several papers provide evidence that financing frictions affect labor during non-recessionary periods: Agrawal and Matsa (2013); Cantor (1990); Sharpe (1994); Matsa (2010); Bakke and Whited (2012); Efraim Benmelech (2012); and Michaels et al. (2018). Other papers in the literature include Almeida et al. (2011) and Pica and Pagano (2014).

ments that would follow capital investment reactions to external finance because of the labor-capital complementarities in the production process (e.g., Benmelech et al. (2015)). Theoretically, the availability of external finance can directly affect employment decisions for a number of reasons; for example, if a mismatch between payments to labor and the ultimate generation of cash flow exists (see, for example, Greenwald and Stiglitz (1993)), or if labor market frictions make it costly for firms to adjust their labor force (e.g., Oi (1962); Sharpe (1994); Michaels et al. (2018)). These theories are not mutually exclusive, and we find suggestive support for both in our data. The results hold for both firms with large and small mismatches between the payment of inputs and the ultimate generation of cash flow as measured by accounts receivables over revenues pre-crisis. The point estimates are also similar (although not statistically significant given statistical powers issues) in the subsample of manufacturing firms where labor typically needs to be financed throughout the production process (in contrast to, say, service industries; cf., Benmelech et al. (2015)).

We also contribute to the literature on the employment effects of the Great Recession.[5] An important challenge in this literature has been to distinguish the effects of banking sector disruptions on small firm employment, given data limitations and other concomitant factors during the crisis such as demand effects (see, US: Mian and Sufi (2010); UK: Bunn and Rostom (2015)).[6] Under plausible identification assumptions, our results estimate the causal impacts of financial constraints on the employment decisions of representative small firms during the crisis, holding constant firms' investment opportunities and cost of capital. Previous studies have looked instead for larger sensitivities of employment among firms that were more likely to be financially constrained according to observable firm characteristics, such as firm leverage in public US firms (Giroud and Mueller, 2016) and "lenders' health" for US firms with syndicated loans (Chodorow-Reich, 2013). However, the employment sensitivities in these studies are hard to generalize—e.g., public firms, and particularly firms with syndicated loans, could substitute bank debt with bonds

[5]The literature has focused on the role of household balance sheets (Sufi et al., 2013; Mian and Sufi, 2014), lenders' balance sheets (Chodorow-Reich, 2013), and borrowers' balance sheets (Giroud and Mueller, 2016; Gilchrist et al., 2017).

[6]See Mian and Sufi (2018) and the papers cited there. The focus on small firms in this literature is motivated by the fact that, in contrast to other recessions, the Great Recession was characterized by declines in employment that were disproportionately concentrated among small firms.

during the crisis, whereas the majority of small firms have no access to public debt (Adrian et al., 2013; Ivashina and Scharfstein, 2010; Chodorow-Reich, 2013). The relationship between such sensitivities and financial constraints is also not straightforward, as variables measuring firms' (or banks') financial health may also correlate with firms' investment opportunities (cf., Kaplan and Zingales (1997); Alti (2003); Moyen (2004)).

Finally, our work also relates to the literature exploiting policy interventions to explore the prevalence of financial constraints (e.g., Banerjee and Duflo (2014); Bach (2013); Paravisini (2008); Zia (2008)). We contribute by focusing on a recessionary period, and on an indirect policy, which is important because indirect policies are much more popular policy tools than direct lending programs (c.f., OECD (2018)). Our analysis contributes to the specific policy literature exploring CGSs in other settings, which has produced mixed results on these programs' effect on employment and efficiency (e.g., France: Lelarge et al. (2010); Italy: De Blasio et al. (2015); US: Brown and Earle (2017); D'Acunto et al. (2017); Chile: William and Toro (2018)).

The rest of this paper proceeds as follows. In Section 2.1, we describe the data in detail. In Section 2.2, we characterize the institutional setting of the UK CGS. We explain the empirical strategy in Section 2.3. Section 2.4 presents the results. In Section 2.5, we summarize a battery of robustness checks. Section 2.6 concludes the paper.

2.1 Data

In this section, we summarize our data and variable construction. The data source used in this study is the Financial Analysis Made Easy (FAME) database. FAME is provided by Bureau Van Dijk (BVD) and contains financial information for incorporated companies in the United Kingdom that was originally extracted from Companies House (CH), the admistrative business register in the UK (see Brav (2009) and Michaely and Roberts (2012); González-Uribe and Paravisini (2019)).

Our original extract from FAME encompasses a 7-year period from 2005 to 2011. We exclude firm-year observations with missing or negative values of total assets and restrict the sample to firms in eligible sectors that have more than 50 employees and

total assets above £3.26M in 2008 (one year prior to the EFG launch). Smaller firms with fewer employees and smaller asset sizes can file abbreviated accounts that exclude information on revenues, our sorting variable in the empirical strategy, as we explain in detail below.

There is no survivorship bias in our sample, as FAME reports historical information for up to 10 years, even if a firm stops reporting financial data. To mitigate the potential impact of outliers, we winsorize variables at the most extreme 2% in both tails of the distribution.

The filings have detailed and audited information for a number of financial variables.[7] We list the main regression variables we use here and present a detailed discussion of their construction.

We measure year-to-year changes in four broad types of capital sources: Δexternal debt, Δinside debt, Δtrade credit and Δissued equity. External debt refers to bank loans, overdrafts, and other long- or short-term loans, and includes guaranteed loans. Inside debt includes short- and long-term group and director loans, where group loans correspond to loans from parent companies, loans from subsidiaries, or loans from non-director owners. Trade credit corresponds to loans from suppliers. Finally, issued equity corresponds to the sum of the called-up share capital and share premium account (see González-Uribe and Paravisini (2019)). We also report firms' Δtotal equity, which corresponds to the FAME account shareholders' funds, and equals the sum of issued capital, share premium account, and retained earnings over time.

The filings do not distinguish between government guaranteed loans and other sources of external loans.[8] Hence, we use changes in external debt to trace the usage of guaranteed debt. While this measure may introduce some bias, the direction of this bias is not clear. Changes in external debt will underestimate the real usage of CGS debt if the guaranteed loans are used to replace other external debt sources. However, the CGS usage can also be overestimated if guaranteed loans help companies attract alternative external debt sources (e.g., William and Toro (2018)).[9] The

[7]By law, financial filings are audited for firms with revenues above £1M. The financial information in the effective analysis sample is audited by design, as all analysis firms meet this auditing revenue threshold (see Section 3.3).

[8]In addition, reporting on account components is sparserfor example, we have several missing observations for long-term debt (see Table 2.1). While they cannot be fully ruled.

[9]While they cannot be fully ruled out, there is little support for side effects in survey evidence

filings do not include information on loans' interest or default rates either. Hence, similar to other papers in the literature (e.g., Lelarge et al. (2010)), we use the survival probability of companies—which we can track in the dataas the main measure of default.

We measure the year-to-year in employees: Δnumber of employees. Data on employee wages is not available in FAME, and data on managerial compensation is not well populated for small firms. We measure capital investment with the year-to-year changes in fixed assets: Δfixed assets. However, we also measure investment more broadly with year-to-year changes in total assets, as in practice firms need not buy fixed assets when they may make capital investments, and many small firms are in low-tangibility industries (e.g., services; see Section 2.4.1). By keeping track of changes in total assets, we thus keep count of investments in working capital, such as cash for operations. We also keep track of changes in cash and accounts receivables (cf., Bakke and Whited (2012)). We note that FAME does not have information on research and development expenses.

We measure year-to-year changes in revenue, cost of sales, and profits: Δrevenue, Δcost of sales and Δprofits, respectively. We focus on gross measures of profits in order to measure profitability impacts stemming from production rather than from other non-operational sources such as lower financial costs.

Finally, we present our main results using logarithmic transformations of our outcome variables (e.g., $\Delta ln(external\ debt + 1)$) in order to mitigate the potential impact of outliers (which we show below can significantly affect results: see Section 2.4.2). However, we also present results using the untransformed variables in order to best interpret the magnitudes of results.

2.2 Institutional Context: UK SMEs and the Enterprise Finance Guarantee

The Enterprise Finance Guarantee (EFG) is the largest UK government program aimed at alleviating potential financial constraints faced by small firms.[10] The EFG

(IFF, 2016; London Economics, 2017; BIS, 2013)

[10]EFG is managed by the British Business Financial Services, a wholly owned subsidiary of British Business Bank that remains on the balance sheet of the Department for Business, Innova-

is a CGS introduced in 2009 as a replacement of the Small Firms Loan Guarantee created in 1981. Its launch was part of a wider trend worldwide in the expansion of CGSs as countercyclical policy tools during the aftermath of the financial crisis (see Gozzi and Schmukler (2016)). Between 2009 and 2014, over £2B worth of loans were guaranteed by the EFG, peaking at £536M in 2009 after the onset of the financial crisis (BBB, 2017a). The EFG covers a small part of the financial system; EFG loans approved between January and March of 2018 represented 0.4% of the loan volume to small- and medium-sized enterprises during the same period.[11]

The EFG provides lenders with a government-backed guarantee for 75% of the value of each individual loan given out through the scheme (i.e., the EFG repays 75% of the outstanding balance in case of default).[12] In contrast to the SBA program in the US, EFG lenders have full decision-making control. They perform all the credit screening and monitoring functions and decide upon all commercial matters, including type of facility (e.g., new loans, conversion of overdrafts into loans), interest rates, and other fees.[13] In case of default, lenders follow standard commercial recovery functions before they make a claim against the government guarantee, including calling upon any personal guarantees.[14]

The EFG has three additional unique features designed to curtail potential risk-taking incentives for banks and borrowers. First, individual loan guarantees are subject to a cap on the total exposure across a lender's annual portfolio of EFG-backed lending, which means that banks are exposed to all of the remaining bad debts after this limit.[15] Second, borrowers are required to pay a 2% annual premium over the costs and fees charged by the lender.[16] The premium is collected quarterly

tion, and Skills.

[11]The value of new EFG loans issued in the first trimester of 2018 was £57.3 M. The value of new loans to small (medium) companies in the UK during the same period was £4,875M (£11,419M). Source: BBB (2017b).

[12]Currently, there are over 40 participating lenders. For more details on the application process and the list of lenders see: BBB (2014).

[13]Term limits are also imposed: between 3 months and 10 years for term lending and between 3 months and 3 years for overdrafts.

[14]The extent of any security or guarantee taken is a commercial matter for the lender, but any security taken applies to the debt as a whole and may not be attributed solely or preferentially to cover the 25% of the EFG loan not covered by the government guarantee.

[15]The cap was originally set at 9.75% but was revised in 2012 to 15% per lender.

[16]The percentage and the way fees are applied vary among CGSs worldwide. There are schemes where a registration fee for processing the application is required. In Europe, as well as in developing countries, the fee is typically about 1 percent of the loan amount. Others schemes usually impose an annual or per-loan fee that ranges from 1 to 2 percent. The premium ranges between

in advance throughout the life of the loan, and is assessed based on the loan's outstanding capital balance. The premium is unlikely to screen out high-quality borrowers, as it is low relative to the cost of unsecured loans. For example, the premium in a £200,000 loan increases the average cost to 8.5% (from a gross cost of 6.5% including fees). However, this cost is one order of magnitude smaller than that of an unsecured loan (the outside option for the scheme's target of eligible companies with no collateral) for the same amount, which fluctuates between 22.8% (subject to revenue conditions) and 49% (subject to no restrictions) outside of the scheme.[17] Third, and in contrast to other CGSs (e.g., France) banks are allowed to take additional personal guarantees. The only exception from normal commercial practice is that lenders are prohibited from taking a charge (collateral pledge) over a principal private residence of a borrower or guarantor as security for an EFG facility.

Eligible borrowers in 2009 consisted of small firms operating in the UK that had revenues of no more than £25M and operated in a business sector that was eligible for the EFG.[18] Eligible businesses must use the funds for an eligible purpose (most purposes are eligible and the most significant exclusion is the financing of specific export orders).[19] Almost all sectors are eligible, and where exclusions apply they arise from EU State Aid rules. Sectors where partial or full restrictions exist include agriculture (including horticulture); banking, finance, and associated services; membership organizations (including professional, religious, and political) and trade unions; coal; education; fisheries, and aquaculture; insurance and associated services; public administration; national defense and compulsory social security; and transport.

The EFG is by no means unique to the UK. CGSs are the most widespread instrument to support SME access to finance (OECD, 2018), and cover sizeable volumes of credit (e.g., 5.7% in Japan, 0.4% in France, 0.1% in the US; see OECD (2018)). While most schemes have not been rigorously evaluated, the common folklore among policy-makers is that these schemes are the most effective tool for increasing small firms' access to finance (Beck et al., 2008; OECD, 2018).

50 and 150 basis points in France, ranges between 0% and 3.75% in the US, and is between 1 to 2 percent depending on the borrower's default history in Chile.

[17] See, for example, `https://www.money.co.uk/business-loans.htm`

[18] In 2012, eligibility was further extended to businesses with revenues of no more than £41M.

[19] Alternative forms of assistance for exports are provided by UK Export Finance.

Eligible businesses (as measured by the revenue threshold, location, and business sector of the firm) at the launch of the program in 2009 roughly corresponded to 60% of UK firms during the 2004-2012 period. By June 2017, more than 28,000 loans had been drawn down under the EFG, to a value of over £3B (see Figure A2.1 in the Appendix). For the average EFG-backed loan in 2009, the size was £100K, the interest rate was 5.8%, the fee as a percentage of loan value was 2%, and the loan term was 76 months (See Figure A2.1 in the Appendix). The total cost of defaults for 2011-2012 EFG borrowers was £6.5M by 2014, which corresponds to less than 1% of the total value of the loans (£965M).[20]

2.3 Empirical Strategy

We exploit exogenous firm-level requirements for accessing the EFG to dissect the implications of financial constraints in small firms. We validate the approach by using firms that are ineligible for the EFG due to their industry to run placebo tests. In this section, we describe the analysis sample and the empirical methodology. The results are presented in Section 3.4.

2.3.1 Sample

We classify firms into two groups, eligible and non-eligible, according to their revenues reported in 2008 (below or above £25M, respectively). For the classification, we use the value of revenues the year before the launch of the EFG to minimize concerns of firms manipulating their revenues to become eligible (we examine below the extent to which such manipulation occurs). We restrict the analysis sample to firms that report revenues in 2008 ranging between £12M and £38M to ensure that we are comparing eligible and non-eligible firms of similar size (a £13M bandwidth around the revenue-based eligibility threshold; we verify the results are robust to alternate sample definitions in Section 2.5.2). We also exclude firms in sectors that are not eligible for the EFG (see Section 2.2 for more details), as well as those with less than 50 employees in 2008 and total assets below £3.26M in 2008, so as to

[20]See https://assets.publishing.service.gov.uk/government/uploads/system/uploads/attachment_data/file/85761/13-600-economic-evaluation-of-the-efg-scheme.pdf

make sure that firms report detailed financial statements the year pre-launch (see Section 2.1). There are 5,044 eligible firms (38,341 firm-year observations) and 2,679 non-eligible firms (20,172 firm-year observations) in our final sample (7,723 firms in total and 58,513 firm-year observations).

Table 2.1 presents summary statistics for the main firm-year sample used in the analysis. The table reports firm level characteristics. The average firm in the sample has an average of £22.33M in revenue for 2008. The firm's book value of total assets, total equity, and total non-equity liabilities amount to £19.31M, £6.31M, and 13.02M, respectively. The average firm has £6.57M annual profits, and 200 employees. The main source of capital is non-equity liabilities. The mean leverage ratio—calculated as non-equity liabilities over total assets—is 68%, which compares to the mean historical ratio for public firms of 60% reported by Graham, Leary, and Roberts (2015). External debt is an important source of capital, corresponding to 21% of non-equity liabilities and 17% of total assets.

Panel A in Figure 2.1 shows that the industry distribution (at the SIC 2007 2-digit level) is comparable to that of the universe of reporting UK firms in 2008 (those with more than 50 employees in 2008 and total assets above £3.26 M in 2008; see Section 2.2). Relative to the universe of reporting UK firms, the sample is slightly more concentrated in manufacturing (28.9% vs. 19.3%) and information and communication (9.2% vs. 8.5%), and slightly less concentrated in wholesale and retail trade (15.7% vs. 18.0%), construction (9.4% vs. 10.3%), and administrative activities (9.7% vs. 12.6%).[21]

2.3.2 Methodology

We compare external debt issuances and trends in investment, revenue, and profitability across eligible and non-eligible firms by estimating the following type of

[21]Relative to prior work on CGS, our sample is more concentrated in the manufacturing sector and is composed of larger firms. For example, the sample of US companies used by Brown and Earle (2017) in their study of SBA is concentrated in the services sector (circa 40%) and composed of companies that have fewer than 20 employees on average. By design, the sample of Lelarge et al. (2010) is also composed of smaller firms (i.e., 1.82 employees), as they focus on start-ups. Finally, the sample of William and Toro (2018) also includes smaller firms in terms of employees (fewer than 25 employees on average), reflecting the eligibility restrictions of the CGS in Chile they study, as well as the average size differences between UK and Chilean firms.

difference-in-difference equation:

$$\Delta k_{it} = \alpha_i + \gamma_t \times Industry\ FE + \beta Eligible_i \times Post_t + \epsilon_{it} \tag{2.1}$$

where $Eligible_i$ is an indicator variable of eligible firms and $Post_t$ is a dummy equal to one in the years 2009-2011. All regressions use year-to-year changes in firm outcomes (see Section 2.1) as dependent variables to account for the trends in these variables. The remaining potential serial correlation is accounted for estimating errors clustered at the firm level (Petersen, 2009). We also present results using year-to-year changes in logs, which has the advantage of dampening the effect of any remaining outliers after winsorizing. We control for varying macroeconomic conditions and industry shocks with year dummies for each industry using the 5-digit 2007 SIC classification. Industry controls are important given the heterogeneity in external debt issuance across industries (see Panel B, Figure 1; cf., González-Uribe and Paravisini (2019)). Finally, firm fixed effects account for differential firm-specific trends in all variables.

The coefficient of interest in 2.1 is β, which measures the average change in the dependent variable (Δk_{it}) after the EFG launch for eligible firms, relative to firms that did not qualify for the credit guarantee scheme in 2008 because their revenues were £25M or above. A positive β would imply that the average of the dependent variable increased for eligible firms after the EFG launch.

This difference-in-difference methodology provides an Intent-to-Treat (ITT) estimate of the effect of the CGS on corporate outcomes (cf., Angrist and Pischke (2008)), which has a casual interpretation as long as two assumptions are satisfied. First, that firms did not manipulate their EFG eligibility status during the program's launch in 2008. Second, that, absent the credit guarantee scheme, the average outcomes of eligible and non-eligible firms would have evolved in parallel.

Three facts suggest that the first assumption is likely satisfied.

First, firms have limited scope for eligibility manipulation. In our analysis, eligibility is measured against the value of revenues one year before the program launch, which mitigates manipulation concerns. In addition, while there was an active discussion about the launch of the program prior to 2008, there was uncertainty about its final approval, and the exact level of the qualifying threshold was not known by

the public beforehand.

Second, the EFG eligibility threshold was difficult to predict. Small firms have not been uniquely defined across government programs or over time in the UK. For the purpose of Research and Development Tax Relief, the tax authority in the UK (HMRC) defines a small firm as a business with no more than 500 employees and an annual turnover not exceeding £100M. For the purposes of collecting statistics, the Department for Business, Energy & Industrial Strategy (BEIS) defines small firms as companies with fewer than 250 employees. For accounting purposes, CH defined a small firm in 2008 as a company with revenues of no more than £29.5M, total assets of no more than £12.9M, and no more than 250 employees. For the purpose of government procurement contracts, the UK government uses the European Commission's definition of a small firm (EU recommendation 2003/361), which defines it as an entity engaged in economic activity that employs fewer than 250 people, and has either turnover revenue below €50M or total assets below €43M.

Third, the distribution of revenues in 2008 appears continuous at the eligibility threshold of £25M as shown in Figure 2.2. The McCrary test gives a discontinuity estimate (log difference in density height at the eligibility threshold) of -0.05 with a standard error of 0.09, which is insignificantly different from zero. In Section 2.5.1 on robustness, we present further evidence in support of the first and second assumptions.

Other potential methodological concerns include bandwidth choice and spurious trends. In Section 3.5, we discuss these issues in more detail and show suggestive evidence from several robustness checks against their empirical relevance.

2.4 Results

Tables 2.2, 2.3, and 2.4 summarize results from estimating different versions of equation 2.1. Panels C and D in Table 2, and Panel B in Tables 2.3 and 2.4 present results after collapsing the data to two observations per firm (one before and one after the EFG launch) in order to mitigate further any inconsistency in standard errors from potential serial correlation in outcomes (cf., Bertrand et al. (2004)). Table 2.5 presents results from estimating a more flexible version of equation 2.1,

where we include a full set of interactions between year dummies and the variable $Eligible_i$.

2.4.1 External debt

Table 2.2 shows that the EFG launch had economically significant effects on external debt issuance. After the launch and relative to non-eligible firms, eligible firms increased their average external debt issuance by £502,560 (Panel A, Column 1). This estimate corresponds to a 280% increase over the unconditional issuance (179,669; see Panel A, Column 1), and to a 32% increase over non-eligible firms (Panel B, Column 1). The external debt response was quick and persisted for one additional year after the launch (see Table 2.5, Panel B, Column 1). Results point to increases in both long- (Column 2) and short-term (Column 3) debt. However, the dataset has some missing information on long-term debt (see Section 2.2 and Table 2.1), and short-term debt results are less robust (see Table 2.2, Panel D, Column 3).

The increase in external debt issuance that we estimate is roughly four times the average loan size reported in official EFG statistics for the universe of EFG borrowers (see BBB (2016)). However, it is not immediately clear how to interpret the differences in magnitudes between the estimate and the average loan size for a number of reasons. First, our sample firms are larger than most EFG users by design.[22] Because smaller EFG users tend to borrow smaller amounts, our estimates will thus tend to be larger than the average loan size of the scheme (cf., London Economics, 2017). Second, EFG loans can "crowd-in" other sources of external debt. For example, EFG loans can help companies build credit records and new banking relationships, thus potentially amplifying the direct financing effect of the program (cf., William and Toro (2018)). Third, the difference-in-difference methodology provides an ITT estimate, which will differ from the Treatment-on-the-Treated (TOT)—i.e., the amount borrowed through the scheme by "EFG-takers"—because eligible firms can decline to participate.

Because UK firms only report consolidated external debt statements to CH (see Section 2.1), we cannot assess the practical relevance of these explanations em-

[22]For identification purposes we restrict attention to eligible firms close in size to the £25M eligibility threshold; see Section 2.1.

pirically. By the same token, we cannot measure take-up rates and adjust ITT estimates to obtain TOT estimates (cf.,Angrist and Pischke (2008)). Under our identification assumptions, however, the economically significant increase in external debt reported in Table 2.2 allows us to rule out the possibility that EFG loans are used exclusively to replace or crowd out non-EFG external loans (in that case the estimate on external debt issuance would be non-positive). We explore potential substitution/complementarity between EFG loans and other capital sources (different from external debt) in the remaining columns of Table 2.2. This exercise is made possible by the detailed UK data: firms distinguish between different funding sources, although they do not detail the components of each source. Across both panels, we find no evidence suggestive of replacement or amplification effects. There are no robust, significant changes in internal debt (Column 4), trade credit (Column 5), or issued equity (Columns 6).

2.4.2 Employment and investment

Table 2.3 shows that the EFG elicited significant relative increases in employment. Column 2 in Panel A shows that the number of employees increased by an average of 5.35, an increase that is roughly 1.7% larger than the increase for non-eligible firms (Column 4, Panel A, Table 2.3). This relative increase is economically significant, as it corresponds to a 78% increase in employment over the unconditional mean (3; see Column 2, Panel A, Table 2.3). The employment response to the EFG was quick. Figure 2.5 shows that eligible firms fired relatively fewer employees during 2009 and started rehiring during 2011, whereas non-eligible businesses had no significant increases in employment that year. The significant employment effects are consistent with results found in policy evaluations of the EFG using data from EFG borrowers and matched samples of control firms (e.g., London Economics, 2017).[23]

In contrast to the robust employment adjustments, Table 2.3 shows no compelling evidence of significant adjustment in capital investments to the EFG. While a positive effect on fixed-assets investment is shown in Column 1, Column 3 shows

[23]A policy report in 2013 found no differences in self-reported employment for 2009 borrowers and matched companies. However, it is hard to make tight inferences from this evidence: 2009 borrowers and matched companies can have different investment opportunities, and well-known issues exist with survey-based evidence (e.g., reporting biases). For more details on issues with this policy report see: London Economics (2017).

that the increase is likely driven by outliers, as in the specification with logs the estimate is very close to zero (0.001; see Column 3 in Panel A) and is not statistically significant. Total and current assets show similar imperceptible adjustments. We find only weak evidence of any relative adjustments in these variables for eligible firms following the EFG launch (see Table A2.2 in the Appendix). Further, Figure 2.5 shows no evidence of different patterns in investment across eligible and non-eligible businesses after the EFG launch. Both types of firms saw similar decreases in fixed assets during 2009 and no significant evidence of subsequent recovery during the final years of the sample (i.e., no positive changes in investment). In unreported analysis, we also looked at evidence of changes in two specific working capital accounts, cash and accounts receivables, given prior work on how managers manage accounts receivables to respond to cash shortfalls (cf., Bakke and Whited (2012)). However, we find no evidence of significant changes in either type of account. Overall, the results point to robust and large adjustments on employment following the EFG launch, and no robust pattern for investments in fixed assets (or total assets more generally).

2.4.3 Interpretation of results

Our preferred interpretation of these results is that the average small firm was financially constrained during the crisis, and that its main margin of adjustment was employment rather than fixed assets. One plausible explanation for why employment decisions appear more sensitive than investment to financial constraints regards the differences in the degree of "collateral pledgeability value" between plants, property, and equipment, on the one hand, and employment, on the other (cf., Benmelech and Bergman (2009)). Simply put, fixed assets have intrinsic collateral value whereas employees do not. Note that the relative insensitivity of investment is not "mechanical" because the EFG imposes no restrictions on the uses of funds, has products dedicated to fixed-asset investments, and has a maximum loan amount (£1M) that both comfortably exceeds the unconditional average investment in fixed assets for sample firms (£0.3M) and offsets the average disinvestment in 2009 (£0.9M). While it cannot fully be ruled out, the alternative explanation that irreversible capital investments were avoided by risk-averse managers during the crisis, is less persuasive

in the later years of the sample that span the start of the recovery.

The combined robust effects on employment and the weak evidence on investment, add to previous work that finds similar asymmetries in firms' reactions to financial shocks (cf., Bakke and Whited (2012)). These results also constitute novel direct evidence of small firms' dependence on external finance to fund labor. Prior work looking for this evidence estimates employment sensitivities to financial shocks (e.g., Chodorow-Reich (2013); Burcu et al. (2015); Benmelech et al. (2019)). However, linking these sensitivities to firms' intrinsic need to finance labor is not straightforward as the former may instead reflect mechanical labor changes following capital adjustments, given the complementarity of labor and capital in production (cf., e.g.,Benmelech et al. (2015)).

Two broad types of theories on the interaction between finance and labor exist: those that assume a mismatch between input payments and the ultimate generation of cash flow (e.g., Greenwald and Stiglitz (1993)) and those that instead assume labor market frictions that make it costly to fire, hire, or replace workers (e.g., Oi (1962); Sharpe (1994); Michaels et al. (2018). These theories are not mutually exclusive, and we find suggestive support for both in our data. In support of the theories on cash flow mismatches, we show in Panel E of Table 2.6 that the point estimates are similar (although not statistically significant given reduced statistical power) when we restrict the sample to manufacturing firms where labor typically needs to be financed throughout the production process in contrast to, say, service industries (cf, Benmelech et al. (2015)). In support of labor market frictions theories, we show in Panel C of Table 2.6 that the results are not entirely driven by firms with large mismatches between the payment of inputs and the ultimate generation of cash flow (as measured by accounts receivables over revenues before the crisis). Also in support of these theories are the relatively fewer lay-offs in eligible firms during 2009, which could be consistent with the practice of "labor hoarding" whereby firms optimally retain workers that may be unnecessary to meet shot-term demand given their high replacement costs (Oi, 1962; Giroud and Mueller, 2016). The increase in long-term debt shown in Table 2.2 is also consistent with labor market frictions theories, as presumably firms will need longer repayment schedules to repay fixed

costs than to address temporal mismatches.[24]

Finally, we note that we remain agnostic about the main drivers behind the financial constraints during the crisis. While the tightening of lending standards by banks during the Great Recession is well-documented (see Adrian et al. (2013); Ivashina and Scharfstein (2010)), the concomitant decreases in the values of pre-existing redeployable assets could also make latent collateral constraints more binding. While the latter cannot be fully ruled out, complementary results provide suggestive evidence for the former explanation. In particular, we find that results do not vary with the tangibility of firms' pre-crisis assets (Panel A, Table 2.6), which we use as a proxy for the value of firms' redeployable assets before the Great Recession.

2.4.4 Alternative explanations

In this section, we discuss alternative interpretations of our findings. While these alternative interpretations cannot be fully ruled out (e.g., as econometricians we only have partial information), we present evidence against their empirical relevance.

The main alternative interpretation of our findings is that the CGS did not relax financial constraints for eligible firms but rather set incentives for firm and banks to overinvest and pursue negative net present value projects. The CGS could potentially deteriorate firms' and banks' incentives for a number of reasons. For example, the financial literature has a long history of arguing that pledging assets as collateral allows banks to attract high-quality firms and discipline managers, and sets incentives for banks to monitor firms (e.g., Smith and Warner (1979); Stulz and Johnson (1985); Boot et al. (1991); Rajan and Winton (1995); Park (2000); Liberti and Sturgess (2014)). In addition, CGS critics contend that these schemes can distort banks' incentives to properly screen loan applications and monitor firms (cf., Lelarge et al. (2010); Kerr et al. (2015); Acs et al. (2016); D'Acunto et al. (2017)).

A similar alternative interpretation is that employment cuts during the crisis were

[24]The findings are different from the González-Uribe and Paravisini (2019), who studies angle investors investment in response to the change of tax relief for two reasons: 1) we look at different types of firms (González-Uribe and Paravisini (2019) look at micro firms with average size of £167,902, in our sample, the average SME firm has an asset level of £19M); 2) we analysis different sample period: González-Uribe and Paravisini (2019) focus on a period around 2012, this paper looks at pre and post crisis period of 2009).

corrections for the negative present value projects pursued before the crisis, and that access to guaranteed loans allowed eligible firms to avoid these efficient but privately costly corrections. For example, several papers show that the pre-Great Recession boom period led to market distortions Adelino et al. (2015); Gopinath et al. (2017); Borio et al. (2016); Charles et al. (2018). Evidence also exists about how layoffs can be personally costly to managers and/or generate reputational concerns for firms (Agarwal and Kolev, 2017; Folger and Skarlicki, 1998).

The difficulties of empirically discerning the quality of investment opportunities are well known, and, like previous papers in the literature, we are unable to fully ascertain whether firm behaviour is value-maximizing. Nonetheless, we use the rich and novel dataset for small firms in the UK to provide compelling evidence in support of our preferred interpretation. The distinguishing prediction between the interpretations is the net present value (NPV) of the marginal projects. Under the hypothesis that the CGS relaxed financial constraints, the marginal projects have a positive NPV, while the opposite is true if the CGS distorted incentives. We use several measures of accounting-based performance and survival indicators, which combined provide a proxy of the NPV of firms' projects. Our approach is made possible by the rich UK dataset, which includes information on several accounting variables that are not typically available for the private firms that make up the majority of our sample, and of small firms more generally (cf., Brown and Earle (2017)). The accounting measures we use include changes in revenues, gross profits, and costs of goods sold. Our focus on survival rates follows the standard approach in other CGS-related work to measure changes in the underlying risk of projects (e.g., Brown and Earle (2017); Lelarge et al. (2010)). Ideally, we would also look at default rates. However, as is common in CGS studies, we have no information on loan performance (see Section 2.2; for an exception, see William and Toro (2018) on the Chilean CGS).

Three additional results support the hypothesis that the CGS relaxed financial constraints and provide evidence against the alternative interpretation that firms and banks incentives deteriorated.

First, Table 2.4 shows that eligible firms performed better than non-eligible firms. Column 2 shows that gross profits for eligible firms increased by £208,711—a 65%

increase over the sample mean (Column 2, Panel A). This increase in profits is not associated with any financial effects (e.g., CGS loans are cheaper than non-CGS external debt sources), because gross profits are measured based on pre-interest expense (i.e., revenues minus costs). This increase in profits instead reflects real increases in sales: average revenues increased by 111% over the unconditional mean (£997,142 over £910,517; see Column 3 Panel A). This increase in sales is unlikely to be driven exclusively by potential output price changes (e.g., Gilchrist et al., 2017), but rather reflects increased production scale. Column 4 shows that the cost of goods sold increased by 107% over the sample mean (665,426 over 623,737; see Column 4, Panel A). The performance results are not driven by outliers. Columns 5-7 in Panel A show that results are similar when we consider log transformations (the point estimate of log profits is large, albeit not statistically significant given reduced statistical power). We report the results for log profits in Column 5 for the sake of completeness, but highlight the difficulty in interpreting these results, as by definition these results can only be estimated for firms with positive profits before and after 2009, which are likely to be a selected sample.

Second, the results in Table 2.4 show that the survival probability of eligible firms positively responds to the CGS. Column 1 in the table shows a small but significant survival increase for eligible firms (0.004; 0.47% over the sample mean). We estimate the effect of the EFG launch on firms' survival by running equation (1) using as dependent variable a dummy equal to one if a firm does not file financial accounts with CH in a given year, and excluding firm fixed effects from the estimation.[25] The positive result on survival implies that the relative increase in profits and earnings for eligible firms is unlikely to be a reflection of riskier strategies pursued by these companies in response to the CGS.

The final result that counters the alternative explanation that the CGS distorted incentives is the invariance of results in subsamples where moral hazard and adverse selection problems (as measured pre-CGS) are likely to be more pronounced, such as in young businesses (cf., Berger and Udell (1998); Oliner and Rudebusch (1992)) and highly levered firms (cf., Myers (1977)). While the over-investment hypothesis would predict a stronger reaction from firms where information frictions are potentially

[25]The results are similar if we refine the survival variable to indicate only firms that stop filing accounts altogether for the rest of the sample.

more pronounced, results in Panels B and D of Table 2.6 show that results do not vary with firms' leverage pre-CGS or firms' age. We note that this additional evidence is mostly suggestive because observable characteristics are endogenous to firms' innovation opportunities.

Outside of our estimates, the modest EFG take-up rate reported in official statistics (and hotly debated in the policy arena; e.g., IFF (2016)), provides compelling evidence against the hypothesis that the employment results reflect the distorted incentives of agents. A government investigation on the EFG reports that fewer than 5% of eligible firms issue any loans through the scheme.[26] Taken to its logical conclusion, the hypothesis on distorted incentives predicts instead excessive take-up (as firms and banks turn to invest in negative NPV projects).

Overall, we argue that the results are most supportive of the hypothesis that the EFG relaxed financial constraints in small firms during the crisis. This evidence adds to other studies documenting positive CGS effects on firm outcomes in the US and Chile (e.g., USA: Brown and Earle (2017); Chile: William and Toro (2018)), but contrasts with evidence found in other settings, most prominently France. The contrasting results for France can be traced (at least in part) to variations in policy design. For example, Lelarge et al. (2010) argue that the CGS in France sets incentives for borrowers to pursue risky projects by explicitly forbidding lenders to require additional private guarantees for scheme loans. By contrast, the EFG explicitly allows lenders to require additional private guarantees (except the borrower's main residences), which helps curtail borrowers' risk-taking incentives (see Section 2.1). This allowance was one of the major innovations in policy design for EFG when compared to its predecessor, The Small Firms Loan Guarantee (SFLG). Another innovation in the EFG was the cap on default payments, which was aimed at curtailing banks' risk taking. The cap was set at 9.75% of the scheme's value per bank, whereas under the SFLG the government covered 75% of the outstanding balance of all the loans that defaulted. Other factors explaining the difference in the results between the two settings include the macroeconomic conditions, types of firms, and regulatory regime surrounding the empirical analysis. Lelarge et al. (2010) study the mid-1990s, a decade of high economic growth, and focus on new

[26]The low take-up rate has also been the subject of policy debate (see, for example, IFF (2016)).

ventures, where information asymmetries and the scope of risk-shifting is likely larger than for older firms (cf., Berger and Udell (1998)). In addition, Davydenko and Franks (2008) document large differences in creditor rights between France and the UK, which lead French banks to require more collateral than UK banks, and also to rely more on collateral forms that minimize the statutory dilution of their claims in bankruptcy.

2.4.5 Estimating the costs of collateral constraints

In this section, we estimate the implied employment and profit sensitivities to the external debt issuance induced by the CGS using an instrumental variables (IV) strategy. Our main identification assumption is the exclusion restriction that the CGS affected firms' outcomes only by increasing external financing and not through any other mechanism.

There are strong reasons to believe that the exclusion restriction holds in our empirical setting, even though we cannot link employment changes specifically to EFG borrowers (recall that firms do not distinguish between EFG loans and other types of debt in their reports to CH; see Sections 2.2 and 2.4.1). For example, the small size of the scheme relative to the financial system implies that its launch is unlikely to have changed the aggregate cost of capital in the market (and thus non-borrowers' cost of finance). In addition, the pressing macroeconomic conditions at the time makes it unlikely that firms cut their firing in anticipation of borrowing through the EFG in the future. Finally, we show in the Appendix that the relative differences between the outcomes of eligible and non-eligible firms are not significant if we restrict the data to observations with negative changes in external debt (see Table A2.4, Panel A). However, we note that the identification assumption is fundamentally untestable, and our sensitivity estimates should be interpreted with this caveat in mind.

To estimate the sensitivities of firm outcomes to external financing, we use the system of equations comprised of equation 2.1 using $\Delta External\ Debt_{it}$ (and logs) as dependent variable, and the following equation:

$$\Delta k_{it} = \alpha_i + \gamma_t \times Industry\ FE + \beta \Delta External\ Debt_{it} + \epsilon_{it} \tag{2.2}$$

where we instrument $\Delta External\ Debt_{it}$ using. $Eligible_i \times Post_t$, and all variables are as defined in Section 3.3. The identification assumption is precisely that that EFG access affects firm outcomes only by increasing external financing and not through any other channel. Table 2.6 summarizes our results. By definition, the IV estimates are Local Average Treatment Effects (LATEs) for complier firms, or firms that are induced to issue external debt by the instrumental variable.

The IV estimates are shown in Table 2.7, together with the corresponding Ordinary Least Squares (OLS) and first stage estimates in both levels and logs. We note that the first stage varies across columns, as the number of observations varies across dependent variables (see Table 2.1. The point estimates of the IV specification in levels imply that a £100,000 increase in external debt leads to an additional 1.3 employees (Panel B, Column 1). The results from the specification in logs indicates that the implied elasticity of employment to external borrowing is 0.053 (Panel A, Column 2).

In contrast to the robust results for employment, the IV results in Table 2.7 for fixed assets are significant for changes in levels (Panel B), but not for logs (Panel A). Further, the point estimate in Column 4 of Panel A is very small (0.0012), suggesting that the significant point estimate in Panel B is driven by outliers, just as with the difference-in-differences estimates in Table 2.3.

A comparison between columns 1 and 2 in both panels of Table 2.7 shows that the OLS estimates are an order of magnitude smaller that the IV estimates. The higher magnitudes for the IV estimates can be explained by a number of factors, but a plausible explanation is that complier firms that issue debt in response to the scheme have particularly large sensitivities of employment to external finance relative to the population of small companies. In support of this interpretation is the low-take up rate of the scheme (IFF, 2016). Note that the higher IV magnitudes are instead unlikely to be explained by a weak instrumental variables problem, as indicated by the healthy first-stage F-statistics reported in the last rows of Panels A and B.

The sensitivity of 1.3 additional employees for an increase in £100,000 suggests that the types of workers retained during the crisis by EFG borrowers were "high-skill" employees (the average annual salary in UK is £28,677). This type

of employees can be very costly to hire and retain for constrained firms, especially during recessions. However, the counterargument would highlight the potential high returns associated to retaining/rehiring this type of employees: optimal wages should reflect employee's value added to the firm. Said differently, we would expect firms to incur in costly labor hoarding only if the expected returns of doing so are large enough to compensate such costs. Consistent with this notion, we estimate large returns to the external finance induced by the CGS, measured as additional gross profits per unit of external debt. This measure of returns is common in the development literature (see de Mel et al. (2008)), although it is clear that it constitutes only a rough approximation of true capital returns—i.e., all risk-adjusted, discounted, expected future cash flows from firms' investments are not captured. Note that any changes in profits stemming from changes in financing costs are not included in our calculation; gross profits are calculated before any adjustment of interest payments.

The IV estimates for marginal profitability suggest that returns to capital average 53% per year in the "complier" businesses that respond to the relief. The estimates are similar when we use logs—point estimate is large, albeit not statistically significant given reduced statistical power. Again we report the results for log profits for the sake of completeness, but highlight the aforementioned difficulty in interpreting these results (see Section 2.4.4). Overall, these estimates point to sizeable efficiency costs of financial constraints in small firms, as the annual marginal profitability is more than six times the average scheme interest rates (7.8%) and more than nine times the average UK loan rates (5.8%) for small businesses.

Our estimates of capital returns are, however, within the range of prior return to capital estimates in the economics literature, which are mostly based on microenterprises in developing countries. de Mel et al. (2008) find annual average returns to capital of 55-65% for microenterprises in Sri Lanka that were randomly allocated small (200 USD) capital grants. Using a similar experiment than the one in Sri Lanka, McKenzie and Woodruff (2008) find returns to capital in the range of 250-360% per year among microenterprises in Mexico. Banerjee and Duflo (2014) estimate that returns to capital average 74-100% per year among large enterprises in India that are eligible for earmarked credit from Indian banks. Udry and Anagol (2006)

estimate that annual returns to capital in Ghana average 50%-250% among small scale agricultural producers and 60% among purchasers of used auto parts in Accra.

Our estimates can be in the lower range of the estimates from prior work for several reasons. For example, our firms are substantially larger than in most prior work (the average total asset size in our sample is £19M), and McKenzie and Woodruff (2008) show a negative relation between firm size (as measured by capital stock) and returns to capital. Other potential explanations include differences in access to skilled labor, growth opportunities, as well as the degree of financial frictions among others, between the UK companies in our sample and the businesses in underdeveloped economies.

2.5 Identification Tests and Robustness

In this section, we provide a battery of tests using different controls, sub samples, and specifications. We divide the tests into two groups: potential identification issues and potential sample selection concerns.

2.5.1 Identification tests

The main identification concerns in our empirical strategy are: (1) the potential manipulation of EFG eligibility in 2009, and (2) the violation of the parallel trends assumption—i.e., that treatment and control firms would have evolved similarly absent the EFG. We discussed the evidence against the first concern in Section 2.3.2. We now turn to the second concern.

Table 2.5 presents results from the standard test for parallel trends, where we compare trends in outcomes across treatment and control firms during the pre-treatment period (see also Table A2.3 in the Appendix). The table presents the results found when estimating a more flexible version of equation 2.1, where we include a full set of interactions between year dummies and the variable $Eligible_i$. The table shows no significant differences across treatment and control groups before 2009 (with the exception of marginally significant differences in costs and profits in the year 2005).

We use complementary placebo and falsification exercises to provide further ev-

idence that our estimates are unlikely to be spuriously driven by differential trends across firms of different revenue size. First, we estimate 200 placebo regressions using randomly selected (fake) eligibility thresholds between £30M and £37M (so as not to include any data from the actual analysis). We define treatment and control firms as we do in the main analysis but use the placebo threshold instead of the actual one. Specifically, we restrict the sample to firms whose 2008 revenues fit within a £13M window on either side of the placebo threshold and classify firms into eligible and non-eligible if their revenues in 2009 are below or above this threshold, respectively. A summary of the results is presented in Table 2.8. As expected with randomly picked thresholds, we cannot reject the null of no effect in more than 95% of the cases (except for log changes in external debt, where we cannot reject the null in 92% of the cases).

Second, we run falsification tests using firms in non-eligible industries in our data. In particular, we replicate the analysis for companies with revenues in 2008 that were close to the £25M threshold but are in industries that do not qualify for the EFG program (see Section 2.2). Figure 2.3 shows there is no significant change in firm outcomes across the smaller and larger of these companies in non-eligible industries (see also Figure A2.2 in the Appendix). In the figure, we present results using several revenue bandwidths, including the £13M bandwidth of our main specifications.

2.5.2 Potential sample concerns

The central concern with the main analysis sample is that results may be sample specific—i.e., they hold only for the £13M bandwidth. To address this concern, in Figure 2.4 we show that the results are similar in twelve alternative sub-samples of companies with revenue within a bandwidth of £7.5M to £18.5M, around the £25M eligibility threshold (see also Figure A2.3 in the Appendix). Figure 2.6 shows that our estimates of the returns to capital in constrained small firms are not sample specific, as they are similar in the twelve alternative sub-samples.

A second concern is the potential bias from dynamic misclassification of firms that were non-eligible in 2008 but decreased their revenues in later years in order to qualify for the program. As evidence against this concern, in the Appendix we

show that the results are robust to excluding from the sample all companies that reported revenues in 2008 between £22.5M and £27.5M—that is, within a £5M window around the threshold (see Table A2.4, Panel B). Firms that had reported revenues in 2008 closely above the £25M threshold are more likely to be able to manipulate assets in order to qualify for the scheme after the EFG launch. This test also presents evidence against concerns that potential spillovers from eligible to non-eligible firms drive the results. Concerns about the substitution of funds away from non-eligible firms and towards small firms are more pronounced the closer that firms are to the eligibility threshold.

2.6 Conclusions

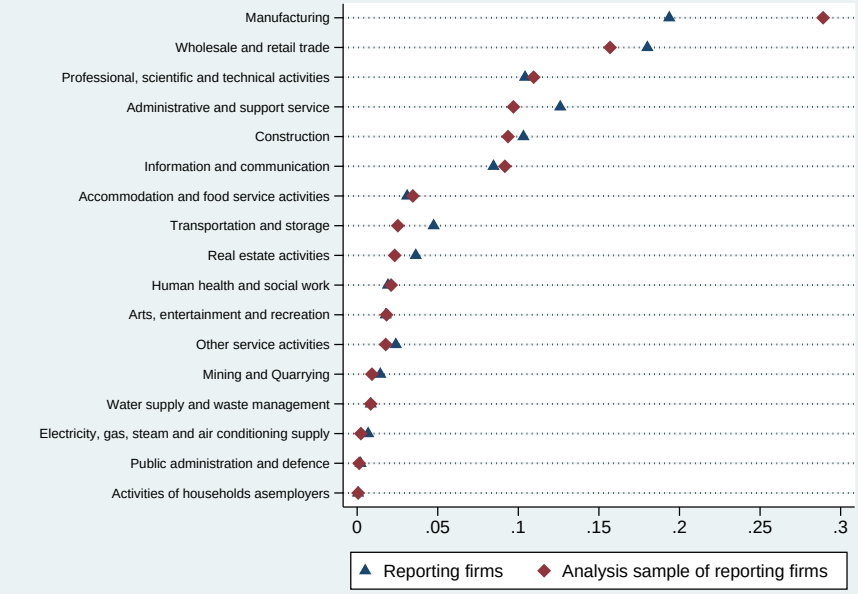
In this paper, we use the Great Recession as a laboratory to dissect the implication of financial constraints for small firms. We exploit firm-level eligibility requirements for a credit guarantee scheme launched in the UK during 2009 as an exogenous determinant of external finance during the crisis. Using a difference-in-difference methodology, we show that eligible firms relatively increased their borrowing, employment, sales, profits, and survival, but disinvested as much as non-eligible businesses. The low take-up rate of the government guarantee programs has been the subject of policy debate (see for example, IFF (2016)). Though the analysis in this paper uses the universe of firms close to the threshold, findings in this paper may be context specific. Moreover, the results should be carefully interpreted under the financial crisis time. It is difficult to extrapolate our results beyond SME firms and non-crisis period. For example, in the non-crisis period, firms may have better investment opportunities and more available positive NPV projects, thus marginal adjustment could be different. Our results show compelling evidence that financial constraints during the crisis prolonged the real effects of negative demand shocks, chiefly by affecting small firms' ability to finance employment rather than capital investments. They provide evidence of small firms' direct need to finance labor and provide new insights about how firms adapt to financial constraints. In particular, they highlight how employment, and more generally intangible assets, can be more sensitive than investment to financial constraints in small firms, likely because fixed

assets have collateral value whereas employees do not.

Figure 2.1: Industry Distribution of UK SMEs

Panel A shows the distribution of firms across industries as determined by their SIC 2007 2 digit code. Panel B shows the distribution of firms across the top 40 industries as determined by their SIC 2007 5 digit code. The Reporting Firms sample includes all firms with more than 50 employees in 2008 and total assets above £3.26M in 2008 (see Section 2.1 for an explanation on the filing requirements for UK firms of different sizes). The analysis sample includes reporting UK firms with revenues in 2008 between £12M and £38M (i.e. $+/-$ £13M window around the revenue threshold of £25M) that survive at least until 2009.We exclude firms in sectors that are not eligible to EFG program.

Panel A: Industry Distribution(2-digit SIC 2007)



Panel B: Mean Leverage Ratio

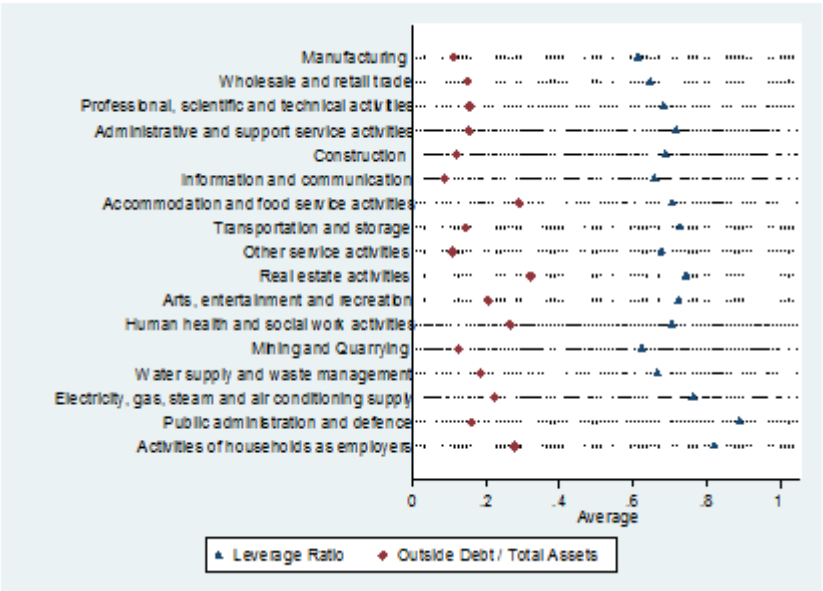


Figure 2.2: Distribution of Firms by Revenue Values in 2008

The figure plots the distribution of revenues in 2008 for the firms in the sample. The sample includes UK firms with revenues in 2008 between £12M and £38M (i.e. $+/-$ £13M window around the revenue threshold of £25M) that survive at least until 2009, and with more than 50 employees in 2008 and total assets above £3.26M in 2008—so as to make sure that firms report detailed financial statements the year pre-launch (see Section 2.1 for an explanation on the filing requirements for UK firms of different sizes). Results from the McCrary test for discontinuity in the distribution of firm revenues at the revenue threshold of £25M are summarized in the x-axis title of the plot. We cannot reject the hypothesis that the distribution of firms is continuous at the £25M threshold: the discontinuity estimate (log difference in density height at the £25M threshold) is -0.049 with standard error of 0.091.

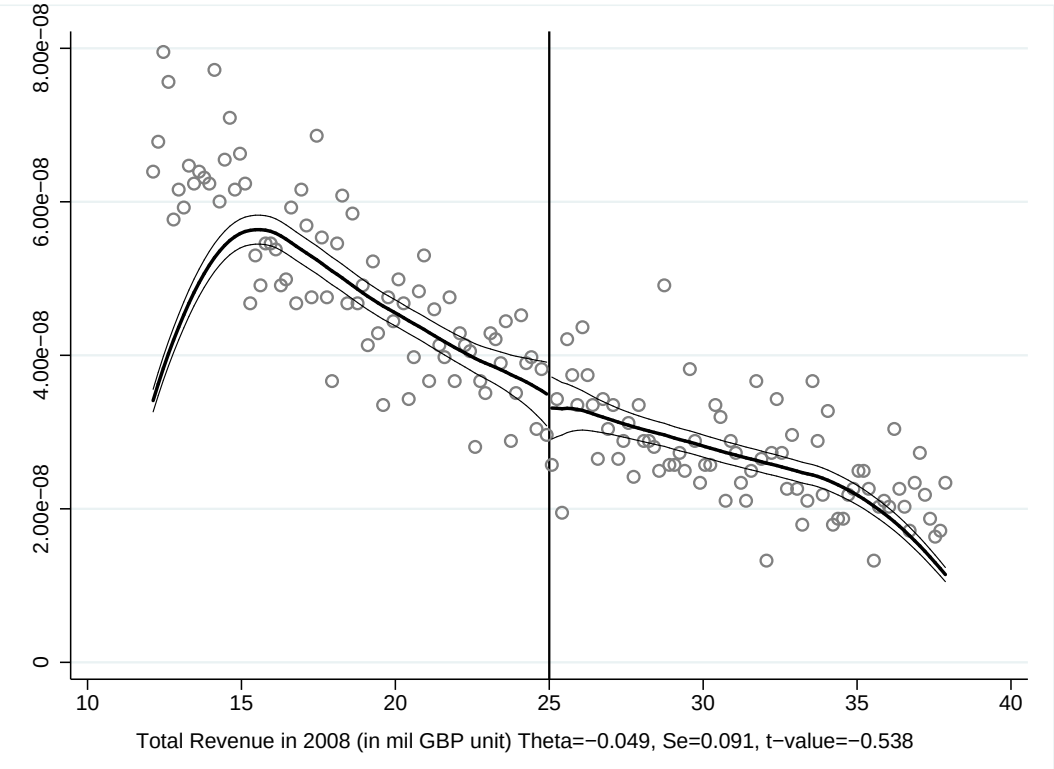


Figure 2.3: Placebo Test with Firms in Non-eligible Industries

The plot presents results from estimating equation 2.1 using different sub-samples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008, but in non-EFG-eligible industries. The dependent variable is specified in the top of each plot. The solid black line plots the estimated coefficients and the red dashed line the 90th percent confidence interval. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The solid vertical line represents results using our preferred bandwidth of £13M (i.e., an estimation window of $+/-$ £13M around the revenue threshold of £25M).

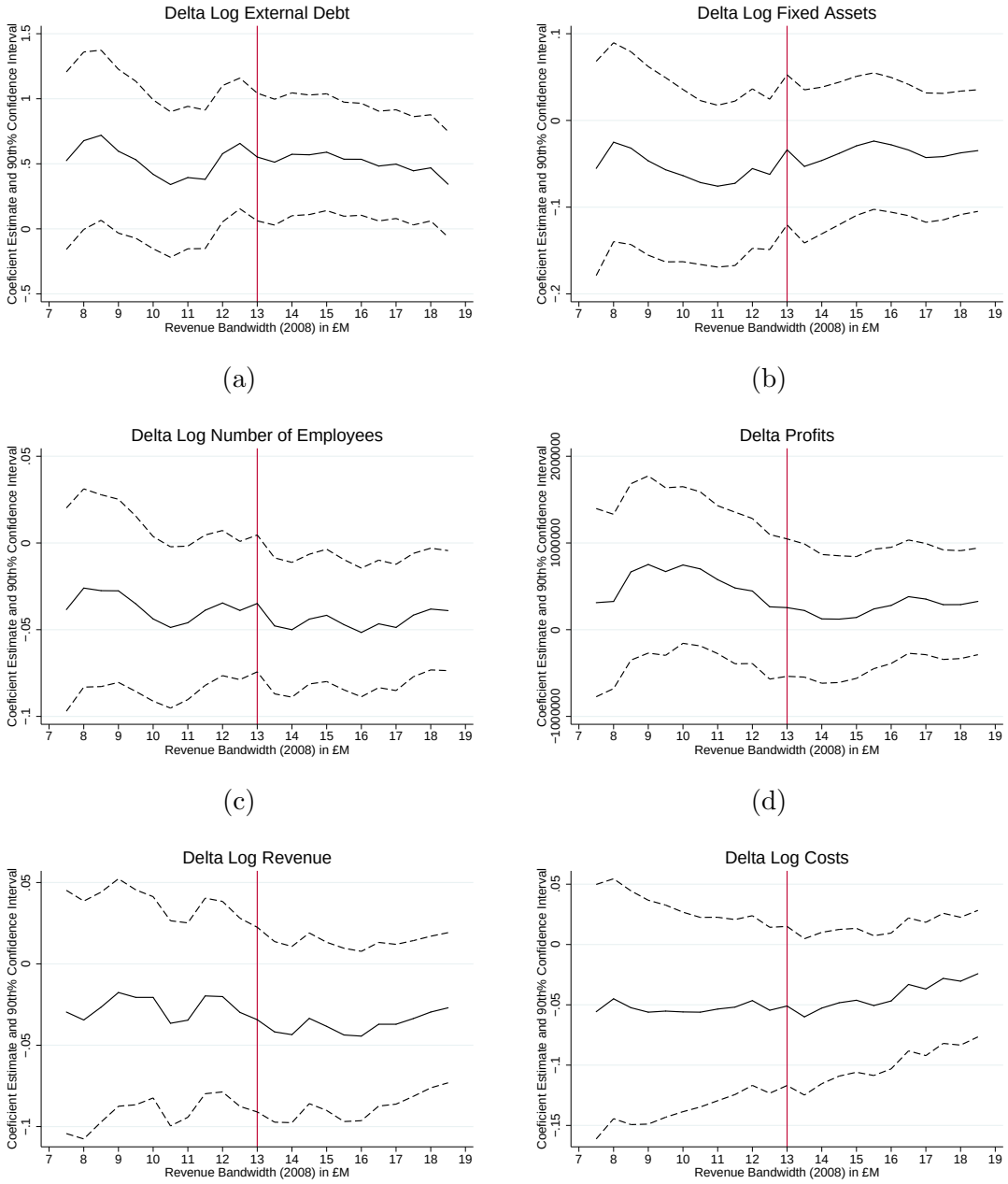


Figure 2.4: Robustness checks with different revenue (2008) bandwidths

The plot presents results from estimating equation (1) using different sub-samples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008. The dependent variable is specified in the top of each plot. The solid black line plots the estimated coefficients and the red dashed line the 90th percent confidence interval. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The solid vertical line represents results using our preferred bandwidth of £13M (i.e., an estimation window of $+/-$ £13M around the revenue threshold of £25M). There are 2,472 (1,717) and 3,480 (8,204) control (treatment) firms in the windows with £7.5M and £18.5M revenue bandwidth, respectively.

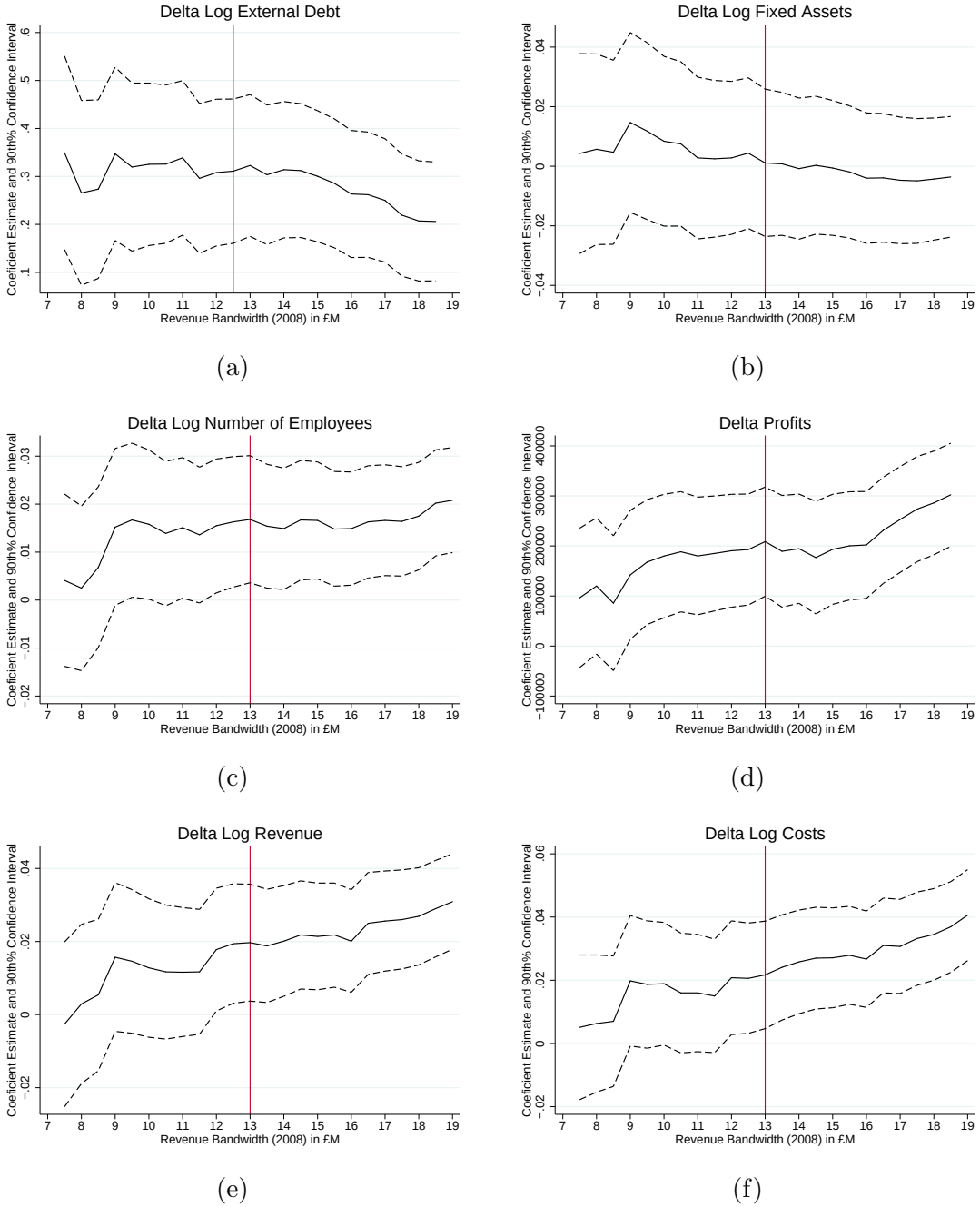


Figure 2.5: Dynamic Change: Eligible vs Non-eligible Firms

This figure plot the dynamic change of levels for eligible firms (Treatment) and non-eligible firms (Control) firms. Panel A shows the issuance of external debt and other sources of finance. Panel B presents the investment, labor and profitability changes. All estimates are based on regressions with change in levels as outcome variables. The point estimates are plotted with the 95% confidence intervals. There are 5,044 eligible firms with revenues below the £25M SME threshold in 2008. The control groups of firms whose eligibility status did not change in 2008 is made up of 2,679 firms with revenues in 2008 above the £25M threshold. All variables are winsorized at the top and bottom 2%.

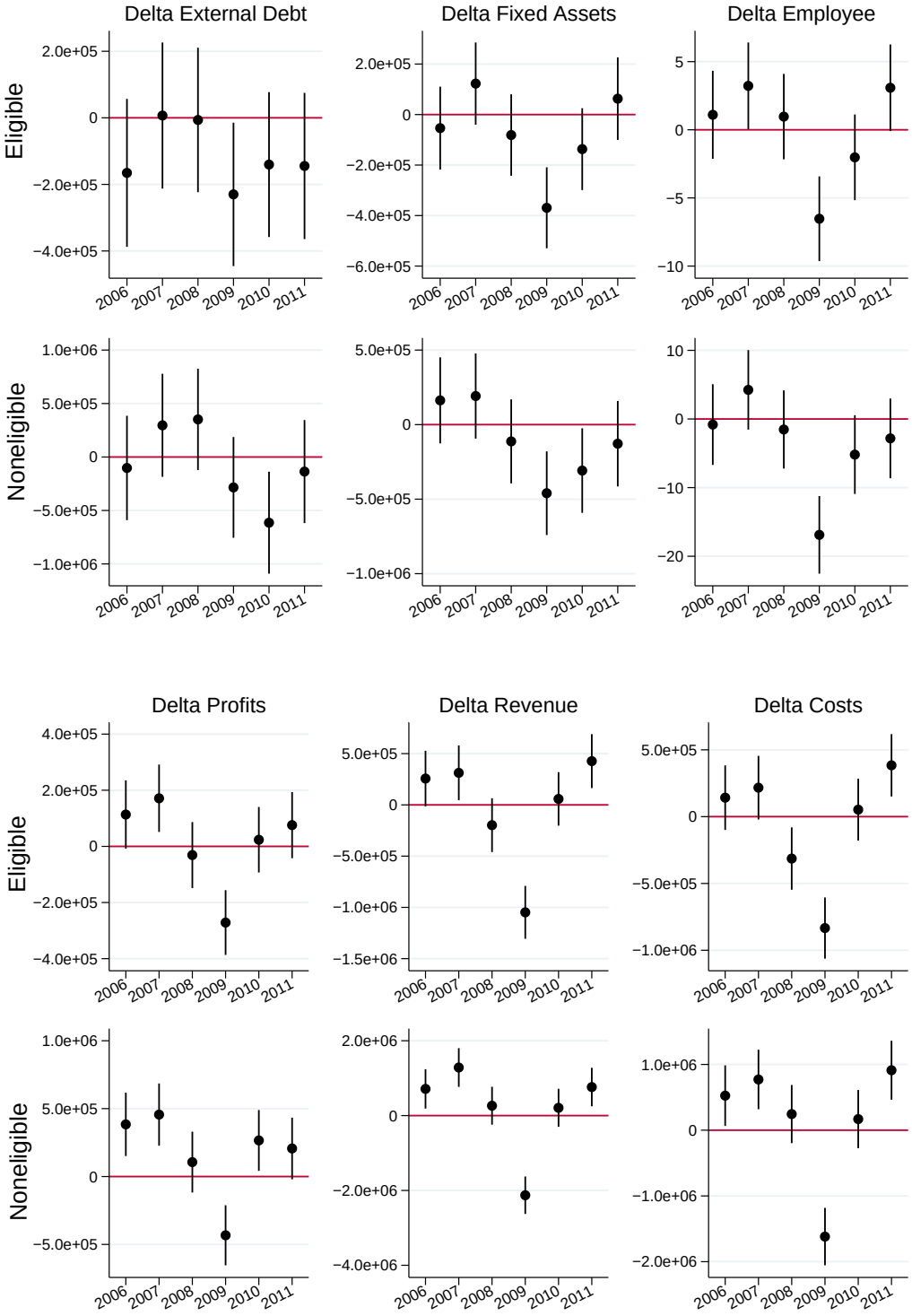


Figure 2.6: Estimation Returns to Capital for Different Revenue (2008) Bandwidths

The plot presents results from estimating the system of equations 2.1-2.2 using different subsamples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008. The dependent variable is changes in gross profits. The solid black line plots the estimated coefficients and the red dashed line the 90th percent confidence interval. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The solid vertical line represents results using our preferred bandwidth of £13M (i.e., an estimation window of $+/-$ £13M around the revenue threshold of £25M). There are 2,472 (1,717) and 3,480 (8,204) control (treatment) firms in the windows with £7.5M and £18.5M revenue bandwidth, respectively.

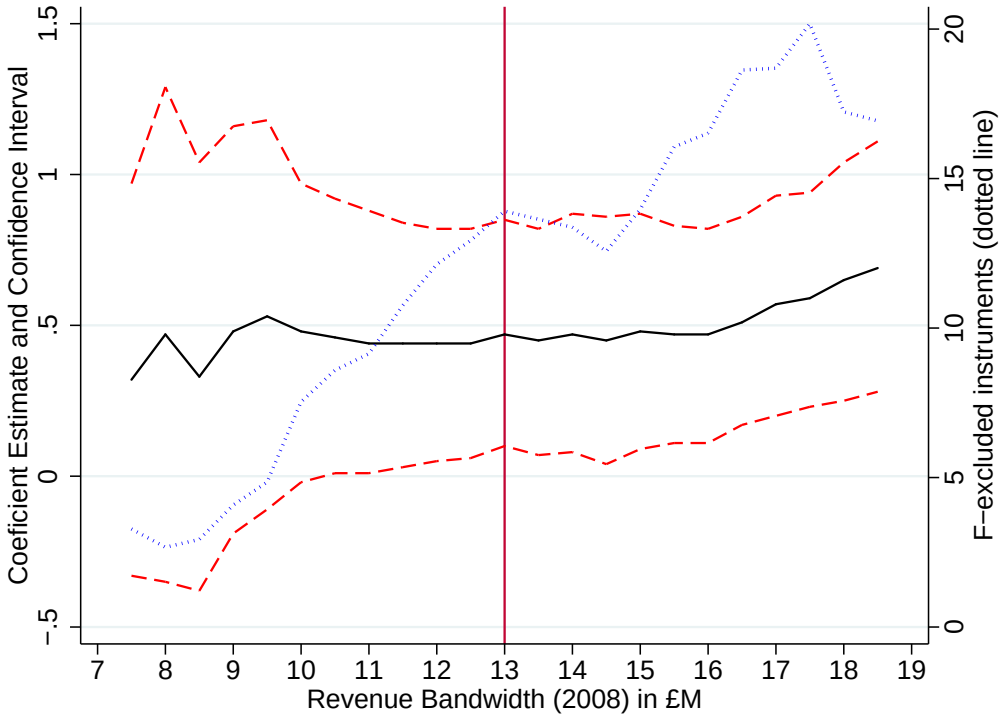


Table 2.1: Summary Statistics

The table presents summary statistics for the main variables in the analysis sample. The sample includes UK firms with revenues in 2008 between £12M and £38M (i.e. +/-£13M window around the revenue threshold of £25M) that survived until at least 2009, and with more than 50 employees in 2008 and total assets above £3.26 M in 2008, so as to make sure that firms report detailed financial statements the year pre-launch (see Section 2.1 for an explanation on the filing requirements for UK firms of different sizes). We also exclude firms in sectors that are not eligible for the EFG (see Section 2.1 for more details). There are 5,044 eligible firms with revenues below the £25M SME threshold in 2008. The control groups of firms whose eligibility status did not change in 2008 is made up of 2,679 firms with revenues in 2008 above the £25M threshold. All variables are winsorized at the top and bottom 2%.

	(1)	(2)	(3)	(4)
Variables	Obs.	Mean	Std. Dev.	Median
Revenue 2008	7,723	22,327,815	7,296,620	21,052,000
Receivables 2008	7,723	2,916,232	2,555,813	2,522,000
Long Term Debt 2008	7,723	0.15	0.47	0.02
Receivables/revenue 2008	7,723	0.13	0.11	0.13
Number of Employees 2008	7,723	208.34	218.91	147
Number of Employees/Total Asset 2008	7,723	0.00	0.00	0.00
Leverage Ratio 2008 (Total Non-equity Liabilities) /Total Assets	7,723	0.68	0.27	0.69
Tangibility 2008 (Fixed Assets/Total Assets)	7,723	0.31	0.25	0.24
Issued Equity	48,516	1,504,981	2,813,451	129,000
Total Equity	58,345	6,308,472	8,398,987	3,425,000
Total Non-equity Liabilities (Total Assets–Total Equity)	58,513	13,015,830	17,981,735	6,976,000
External Debt	52,864	4,509,596	14,914,647	370,000
Internal Debt	52,839	3,977,377	9,191,606	377,000
Trade debts (Receivables)	52,836	3,076,979	2,820,666	2,519,000
Trade creditors (Operational Liabilities)	55,057	1,889,222	1,987,877	1,360,000
Profits (Revenue – Cost of Sales)	47,233	6,568,658	5,792,620	5,201,000
Revenue	55,294	21,517,343	11,770,288	19,351,000
Cost of Sales	47,252	15,065,214	10,171,767	13,164,000
Survival	58,513	0.86	0.34	1.00
Total Assets	58,513	19,306,189	22,376,226	11,916,000
Fixed Assets	56,784	7,902,125	14,607,939	2,639,500
Current Assets	58,367	10,993,692	11,145,551	7,548,000
Number of Employees	56,289	199.79	229.46	137.00
ΔExternal Debt	50,560	179,769	6,120,164	0.00
ΔInternal Debt	50,560	296,836	4,408,833	0.00
ΔTotal Assets	50,560	1,071,223	7,330,854	533,000
ΔFixed Assets	48,861	331,775	4,271,874	-25,000
ΔCurrent Assets	50,396	726,446	4,928,536	471,000
ΔNumber of Employees	48,120	2.86	83.45	1.00
ΔOperational Liabilities	46,868	77,716	1,153,591	40,000
ΔIssued Equity	41,396	68,732	751,682	0.00
ΔProfits	39,928	320,726	2,890,538	216,000
ΔRevenue	47,154	962,042	7,091,248	882,000
ΔCost of Sales	39,944	663,844	5,740,636	535,000

Table 2.2: EFG and Financing

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Panel Data Change in Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	ΔExternal Debt	ΔExternal Long Debt	ΔExternal Short Debt	ΔInternal Debt	ΔOperational Liabilities	ΔIssued Equity
$Eligible_i \times Post_t$	502,560***	521,992**	194,149***	-188,905*	32,392	-59,000***
	(126,425)	(223,693)	(70,630)	(112,457)	(25,800)	(19,338)
Obs.	49,957	22,723	41,538	43,307	46,219	40,582
R-squared	0.176	0.234	0.143	0.207	0.181	0.235
Mean Dep. Var.	179,669	174,764	42,744	470,536	141,800	134,801

Panel B: Panel Data Change in Logs

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	Δln(External Debt)	Δln(External Long Debt)	Δln(External Short Debt)	Δln(Internal Debt)	Δln(Operational Liabilities)	Δln(Issued Equity)
$Eligible_i \times Post_t$	0.323***	0.237*	0.178**	0.028	0.024*	-0.011
	(0.090)	(0.134)	(0.089)	(0.075)	(0.014)	(0.018)
Obs.	49,853	22,686	41,483	43,307	46,219	40,582
R-squared	0.119	0.212	0.142	0.14	0.178	0.227
Mean Dep. Var.	-0.11	-0.12	-0.15	0.15	0.03	0.05

Panel C: Collapsed Data Change in Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	ΔExternal Debt	ΔExternal Long Debt	ΔExternal Short Debt	ΔInternal Debt	ΔOperational Liabilities	ΔIssued Equity
$Eligible_i \times Post_t$	532,110***	515,973**	232,664***	-127,316	40,718	-61,779***
	(144,510)	(255,551)	(85,308)	(111,284)	(28,178)	(20,439)
Obs.	14,922	7,084	12,892	13,358	14,016	11,992
R-squared	0.191	0.203	0.192	0.187	0.229	0.174

Panel D: Collapsed Data Change in Logs

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	Δln(External Debt)	Δln(External Long Debt)	Δln(External Short Debt)	Δln(Internal Debt)	Δln(Operational Liabilities)	Δln(Issued Equity)
$Eligible_i \times Post_t$	0.292**	0.310**	0.144	0.05	0.028	-0.007
	(0.097)	(0.152)	(0.098)	(0.087)	(0.017)	(0.023)
Obs.	14,961	7,080	12,886	13,358	14,016	11,992
R-squared	0.144	0.203	0.157	0.158	0.171	0.192

Table 2.3: EFG and Investment in Labor and Assets

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Panel Data

	(1)	(2)	(3)	(4)
Dep. Var.	ΔFixed Assets	ΔEmployees	Δln(Fixed Assets)	Δln(Employees)
$Eligible_i \times Post_t$	228,814**	5.354***	0.001	0.017**
	(97,065)	(1.743)	(0.015)	(0.008)
Obs.	48,239	47,482	48,239	47,482
R-squared	0.242	0.244	0.211	0.269
Mean Dep. Var.	434,209	3	0.03	0.01

Panel B: Collapsed Data

	(1)	(2)	(3)	(4)
Dep. Var.	ΔFixed Assets	ΔEmployees	Δln(Fixed Assets)	Δln(Employees)
$Eligible_i \times Post_t$	248,769**	5.988***	0.007	0.018*
	(105,524)	(1.944)	(0.018)	(0.010)
Obs.	14,398	14,448	14,398	14,448
R-squared	0.151	0.168	0.148	0.188

Table 2.4: EFG and Performance

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. Column 10 in Panel A estimates equation 2.1 excluding the firm fixed effect. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Panel Data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Survival	ΔProfits	ΔRevenue	ΔCosts	Δln(Profits)	Δln(Revenue)	Δln(Costs)
$Eligible_i \times Post_t$	0.004*	208,711***	997,142***	665,426***	0.016	0.019**	0.022**
	(0.002)	(66,307)	(151,931)	(127,218)	(0.012)	(0.010)	(0.010)
Obs.	57,833	39,180	46,499	39,196	38,273	46,499	39,196
R-squared	0.913	0.261	0.265	0.249	0.243	0.28	0.283
Mean Dep. Var.	0.86	321,110	910,517	623,737	0.04	0.05	0.05

Panel B: Collapsed Sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Survival	ΔProfits	ΔRevenue	ΔCosts	Δln(Profits)	Δln(Revenue)	Δln(Costs)
$Eligible_i \times Post_t$	0.005	292,409***	1,255,431***	827,299***	0.02	0.021*	0.031**
	(0.003)	(78,624)	(171,645)	(145,144)	(0.015)	(0.013)	(0.012)
Obs.	15,282	11,858	14,090	11,858	11,640	14,090	11,858
R-squared	0.904	0.59	0.65	0.557	0.213	0.233	0.212

Table 2.5: Dynamic Effects of EFG

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Change in Logs

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	Δln(External Debt)	Δln(Fixed Assets)	Δln(Employee)	Δln(Profits)	Δln(Revenue)	Δln(Costs)
$Eligible_i \times 2005$	-0.15	-0.00028	-0.0024	0.030	0.024	0.051***
	(0.20)	(0.036)	(0.012)	(0.019)	(0.015)	(0.016)
$Eligible_i \times 2006$	0.18	-0.022	-0.0100	0.0077	0.00091	0.022
	(0.20)	(0.033)	(0.012)	(0.021)	(0.015)	(0.016)
$Eligible_i \times 2007$	-0.055	-0.055*	-0.014	-0.017	-0.022	0.0079
	(0.22)	(0.032)	(0.013)	(0.021)	(0.015)	(0.017)
$Eligible_i \times 2009$	0.46**	0.0039	0.01	0.033*	0.022	0.038***
	(0.21)	(0.023)	(0.012)	(0.020)	(0.014)	(0.014)
$Eligible_i \times 2010$	0.19	0.0016	0.009	0.018	0.018	0.047***
	(0.18)	(0.024)	(0.014)	(0.020)	(0.015)	(0.016)
$Eligible_i \times 2011$	0.30	0.029	0.012	0.0064	0.015	0.034**
	(0.18)	(0.029)	(0.014)	(0.020)	(0.016)	(0.017)
Obs.	48,853	49,957	47,482	38,273	46,499	39,196
R-squared	0.119	0.242	0.269	0.243	0.281	0.283

Panel B: Change in Levels

	(1)	(2)	(3)	(4)	(5)	(6)
Dep. Var.	ΔExternal Debt	ΔFixed Assets	ΔEmployee	ΔProfits	ΔRevenue	ΔCosts
$Eligible_i \times 2005$	335,982	29,274	-2.90	165,329	305,780	456,663*
	(229,421)	(279,946)	(3.48)	(122,825)	(288,385)	(247,666)
$Eligible_i \times 2006$	194,156	-298,149	-0.20	-127,431	-104,210	136,193
	(240,768)	(288,270)	(3.61)	(132,326)	(293,393)	(262,730)
$Eligible_i \times 2007$	23,101	-447,543	-3.91	-160,565	-530,727*	58,351
	(259,269)	(283,725)	(3.15)	(126,788)	(280,117)	(256,184)
$Eligible_i \times 2009$	560,491**	1.07e+06***	7.48***	448,765***	1.77e+06***	1.44e+06***
	(284,483)	(289,661)	(2.84)	(129,277)	(264,766)	(243,592)
$Eligible_i \times 2010$	915,149***	224,472	0.78	-7,117	547,011**	632,438**
	(285,400)	(281,625)	(3.33)	(135,247)	(269,921)	(246,589)
$Eligible_i \times 2011$	415,242	466,013	2.33	44,997	279,778	255,686
	(281,652)	(305,200)	(3.45)	(138,219)	(302,367)	(265,027)
Obs.	49,957	49,957	47,482	39,180	46,499	39,196
R-squared	0.176	0.231	0.244	0.262	0.266	0.25

Table 2.6: Heterogeneity

The table presents results from estimating equation 2.1, within different sub-samples. Panel A splits the sample based on median Tangibility in year 2008, calculated as fixed assets over total assets in year 2008. Panel B compares sub-samples above and below the median leverage in 2008 (total debt/total assets). Panel C is based on sub-samples above and below the median receivable levels. Panel D splits the sample by median age, and Panel E compares manufacture and non-manufacture sectors. At the end of each panel, a comparison between coefficients is displayed. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Above and Below Median Tangibility in 2008 (Fixed Assets/Total Assets)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	ΔIn(External Debt)	ΔlnFixed Assets)	ΔIn(Employees)	Survival	ΔProfits	ΔIn(Revenue)	ΔIn(Costs)
High Tangibility							
$Eligible_i \times Post_t$	0.406**	0.008	0.026*	0.005	322,126***	0.041**	0.022
	(0.137)	(0.021)	(0.014)	(0.004)	(118,719)	(0.017)	(0.016)
Observations	21,416	21,124	20,395	24,886	16,577	19,889	16,585
R-squared	0.169	0.275	0.277	0.938	0.294	0.294	0.322
Low Tangibility							
$Eligible_i \times Post_t$	0.329***	0.005	0.016	0.002	138,619*	0.007	0.02
	(0.124)	(0.022)	(0.010)	(0.004)	(815,734)	(0.013)	(0.015)
Observations	27,567	26,238	26,187	31,958	21,695	25,742	21,704
R-squared	0.139	0.225	0.310	0.924	0.288	0.319	0.307
Difference	0.078	0.0035	0.011	0.003	183,507	0.034	0.0025
	(0.18)	(0.03)	(0.017)	(0.0055)	(143,639)	(0.021)	(0.022)

Panel B: Above and Below Median Leverage in 2008 (Debt/Total Assets)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	ΔlnFixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
High Leverage							
$Eligible_i \times Post_t$	0.333**	0.009	0.018	0.007	249,443**	0.015	0.032*
	(0.136)	(0.024)	(0.013)	(0.005)	(110,538)	(0.017)	(0.018)
Observations	21,802	21,015	20,674	25,368	17,089	20,361	17,099
R-squared	0.172	0.258	0.308	0.924	0.296	0.303	0.315
Low Leverage							
$Eligible_i \times Post_t$	0.265**	-0.003	0.016	0.001	198,215**	0.023*	0.012
	(0.125)	(0.021)	(0.011)	(0.003)	(87,456)	(0.012)	(0.013)
Observations	27,170	26,338	25,890	31,471	21,110	25,239	21,115
R-squared	0.140	0.258	0.279	0.935	0.299	0.309	0.303
Difference	0.068	0.012	0.0017	0.0059	51,228	-0.008	0.02
	(0.18)	(0.032)	(0.017)	(0.0057)	(140,655)	(0.021)	(0.022)

Panel C: Above and Below Median Receivables in 2008 (Receivables/Revenue)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	ΔlnFixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
High Receivables							
$Eligible_i \times Post_t$	0.460***	0.004	0.028**	0.004	293,158***	0.017	0.007
	(0.135)	(0.022)	(0.012)	(0.004)	(87,235)	(0.014)	(0.013)
Observations	23,383	22,847	22,335	27,112	18,859	21,793	18,863
R-squared	0.150	0.231	0.284	0.944	0.296	0.303	0.296
Low Receivables							
$Eligible_i \times Post_t$	0.241*	-0.002	0.005	0.003	124,653	0.018	0.021
	(0.127)	(0.022)	(0.011)	(0.004)	(102,044)	(0.014)	(0.017)
Observations	25,521	24,423	24,168	29,651	19,333	23,714	19,346
R-squared	0.154	0.23	0.301	0.918	0.295	0.302	0.321
Difference	0.22	0.0056	0.023	0.00085	168,505	-0.00074	-0.014
	(0.19)	(0.031)	(0.017)	(0.0055)	(134,230)	(0.020)	(0.021)

Panel D: Above and Below Median Age

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	ΔlnFixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
Old firms							
$Eligible_i \times Post_t$	0.366**	-0.003	0.018*	-0.001	68,348	0.026**	0.017
	(0.145)	(0.017)	(0.009)	(0.003)	(89,600)	(0.011)	(0.012)
Observations	25,142	23,096	23,071	27,207	18,960	22,405	18,966
R-squared	0.165	0.225	0.264	0.948	0.243	0.283	0.287
Young firms							
$Eligible_i \times Post_t$	0.299**	-0.001	0.018	0.011**	421,581***	0.022	0.032*
	(0.126)	(0.026)	(0.014)	(0.004)	(105,770)	(0.017)	(0.019)
Observations	24,729	24,275	23,522	29,653	19,317	23,229	19,327
R-squared	0.158	0.243	0.305	0.916	0.322	0.323	0.338
Difference	-0.028	-0.0022	0.00012	-0.012**	-353,233**	0.0039	-0.016
	(0.18)	(0.031)	(0.017)	(0.0056)	(138,643	(0.021)	(0.022)

Panel E: Manufacture vs non-manufacture firms

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	ΔlnFixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
Manufacture							
$Eligible_i \times Post_t$	0.259	0.010	0.021*	0.006	45,464	0.003	0.014
	(0.169)	(0.024)	(0.011)	(0.004)	(101,371)	(0.015)	(0.015)
Observations	14,535	14,277	14,095	16,823	12,167	13,765	12,169
R-squared	0.135	0.212	0.301	0.945	0.281	0.283	0.291
Non-manufacture							
$Eligible_i \times Post_t$	0.349***	-0.003	0.015	0.004	281,789***	0.026**	0.025*
	(0.106)	(0.019)	(0.01)	(0.003)	(84,366)	(0.012)	(0.013)
Observations	35,318	33,962	33,387	41,010	27,013	32,734	27,027
R-squared	0.112	0.211	0.262	0.921	0.255	0.28	0.281
Difference	-0.090	0.013	0.0064	0.002	-236,325*	-0.023	-0.011
	(0.20)	(0.03)	(0.015)	(0.0055)	(131,191)	(0.019)	(0.02)

Table 2.7: Sensitivities of Employment, Fixed Assets and Profit to External Debt

The table presents results from estimating the system of equations 2.1-2.2. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is gross profits. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Change in Logs

	(1) OLS	(2) IV-2SLS	(3) OLS	(4) IV-2SLS	(5) OLS	(6) IV-2SLS
Dep. Var.	Δln(Employment)	Δln(Employment)	Δln(Fixed Assets)	Δln(Fixed Assets)	Δln(Profit)	Δln(Profit)
$Eligible_i \times Post_t$	0.0043***	0.053*	0.015***	0.0012	0.00005	0.048
	-0.00048	-0.029	-0.001	-0.055	-0.00068	-0.04
Obs.	47,378	47,378	48,135	48,135	38,175	38,175
R-squared	0.272	-0.131	0.221	0.213	0.243	0.048
Δln(External Debt)		0.31***		0.27***		0.33***
		-0.092		-0.091		-0.1
F-stat		10.56		10.56		10.79

Panel B: Change in Levels

	(1) OLS	(2) IV-2SLS	(3) OLS	(4) IV-2SLS	(5) OLS	(6) IV-2SLS
Dep. Var.	ΔEmployment	ΔEmployment	ΔFixed Assets	ΔFixed Assets	ΔProfit	ΔProfit
$Eligible_i \times Post_t$	7.5e-07***	0.000013**	0.12***	0.54**	0.0098	0.47**
	-1.80E-07	-5.60E-06	-0.017	-0.24	-0.013	-0.19
Obs.	47,482	47,482	48,239	48,239	39,180	39,180
R-squared	0.247	-0.421	0.267	-0.053	0.261	0.206
ΔExternal Debt		410,980***		421,515***		440,898***
		-126,476		-129,720		-118,221
F-stat		10.56		15.8		13.91

Table 2.8: Placebo Tests-Random Revenue Thresholds

This table presents summary results from 200 placebo tests, were we randomly select 200 thresholds in the interval £30M-37M of revenues in 2008. We restrict the sample to firms with revenues in 2008 within a window of £13M to the right and £13M, to the left of the random threshold. We classify firms into "placebo small" and "placebo non-eligible" if their revenues in 2008 are below or above the random threshold, respectively.

Dep. Variable	(1) Average coefficient	(2) Non-rejection rate at 5% level	(3) Non-rejection rate at 5% level for positive coefficients
Δln(External debt)	0.103	7.30%	7.30%
Δln(Internal Debt)	-0.089	0.00%	0.00%
Δln(Operational Liabilities)	-0.023	0.00%	0.00%
Δln(Issued Equity)	-0.022	19.90%	0.00%
Δln(Fixed Assets)	-0.003	0.00%	0.00%
Δln(Employees)	0.014	0.70%	0.70%
Δln(Total Assets)	-0.035	24.50%	0.00%
Δln(Current Assets)	-0.004	0.00%	0.00%
Δln(Profits)	-0.017	2.00%	0.00%
Δln(Revenue)	0.008	0.00%	0.00%
Δln(Costs)	0.004	0.00%	0.00%

2.7 Appendix

Figure A2.1: Official EFG Statistics
This plot shows quarterly EFG statistics loans from January 2009 until June 2017. Panel A shows the total number of loans offered (blue bar) and drawn (red line). Panel B displays the total value of loans offered (blue bar) and drawn (red line). Panel C shows the average loan size offered (blue bar) and drawn (red line). For more official EFG statistics see source: British Business Bank `https://british-business-bank.co.uk/ourpartners/supporting-business-loans-enterprise-finance-guarantee/latest-enterprise-finance-guarantee-quarterly-statistics/`

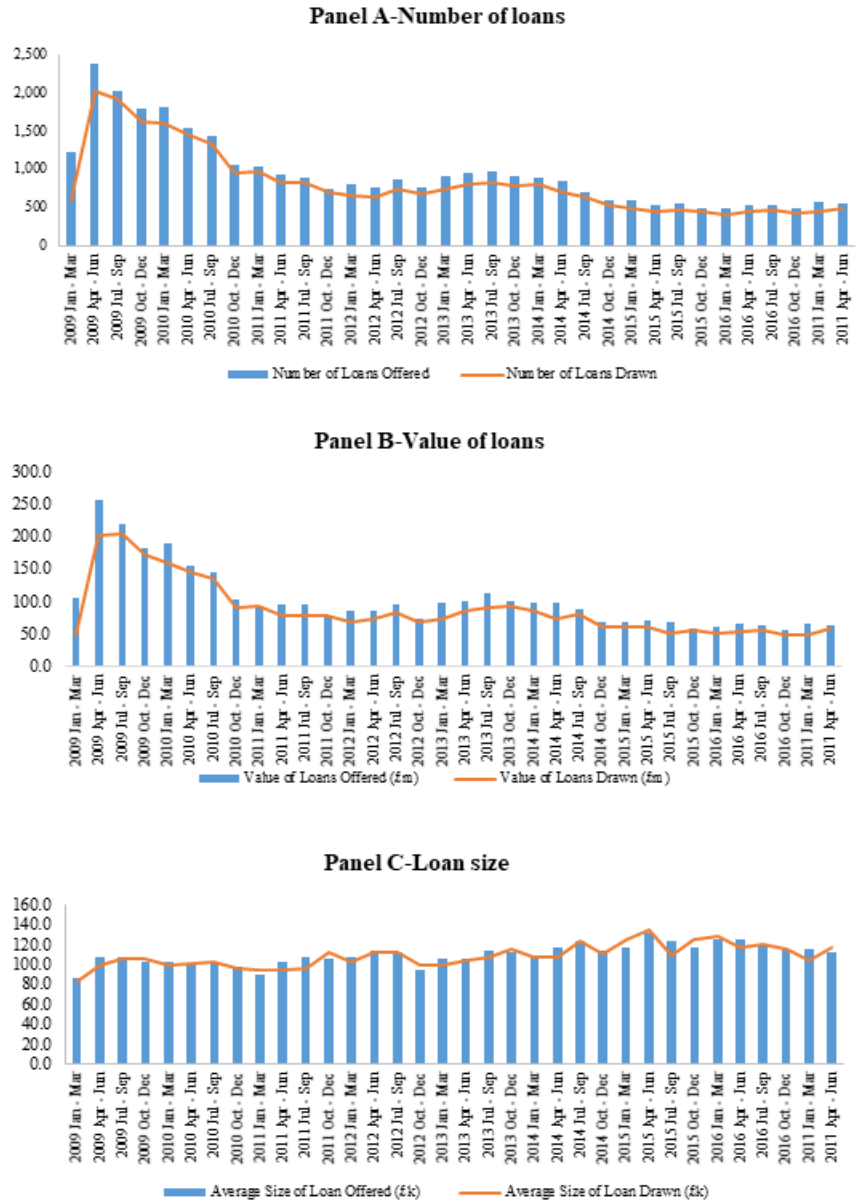


Figure A2.2: Placebo Test with Firms in Non-eligible Industries

The plot presents results from estimating equation 2.1 using different sub-samples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008, but in non-EFG-eligible industries. The dependent variable is specified in the top of each plot. The solid black line plots the estimated coefficients and the red dashed line the 90th percent confidence interval. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The solid vertical line represents results using our preferred bandwidth of £13M (i.e., an estimation window of $+/-$ £13M around the revenue threshold of £25M).

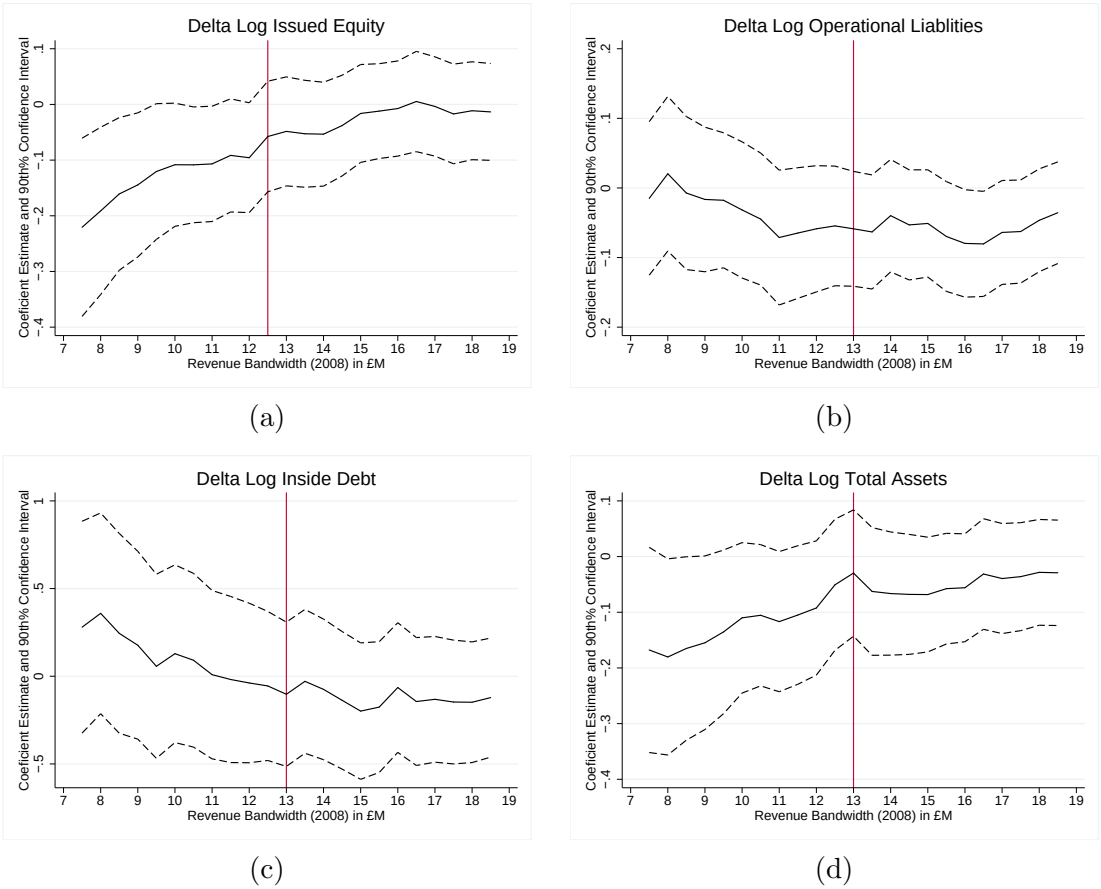


Figure A2.3: Robustness Checks with Different Revenue (2008) Bandwidths

The plot presents results from estimating equation 2.1 using different sub-samples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008. The dependent variable is specified in the top of each plot. The solid black line plots the estimated coefficients and the red dashed line the 90th percent confidence interval. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. The solid vertical line represents results using our preferred bandwidth of £13M (i.e., an estimation window of $+/-$ £13M around the revenue threshold of £25M). There are 2,472 (1,717) and 3,480 (8,204) control (treatment) firms in the windows with £7.5M and £18.5M revenue bandwidth, respectively.

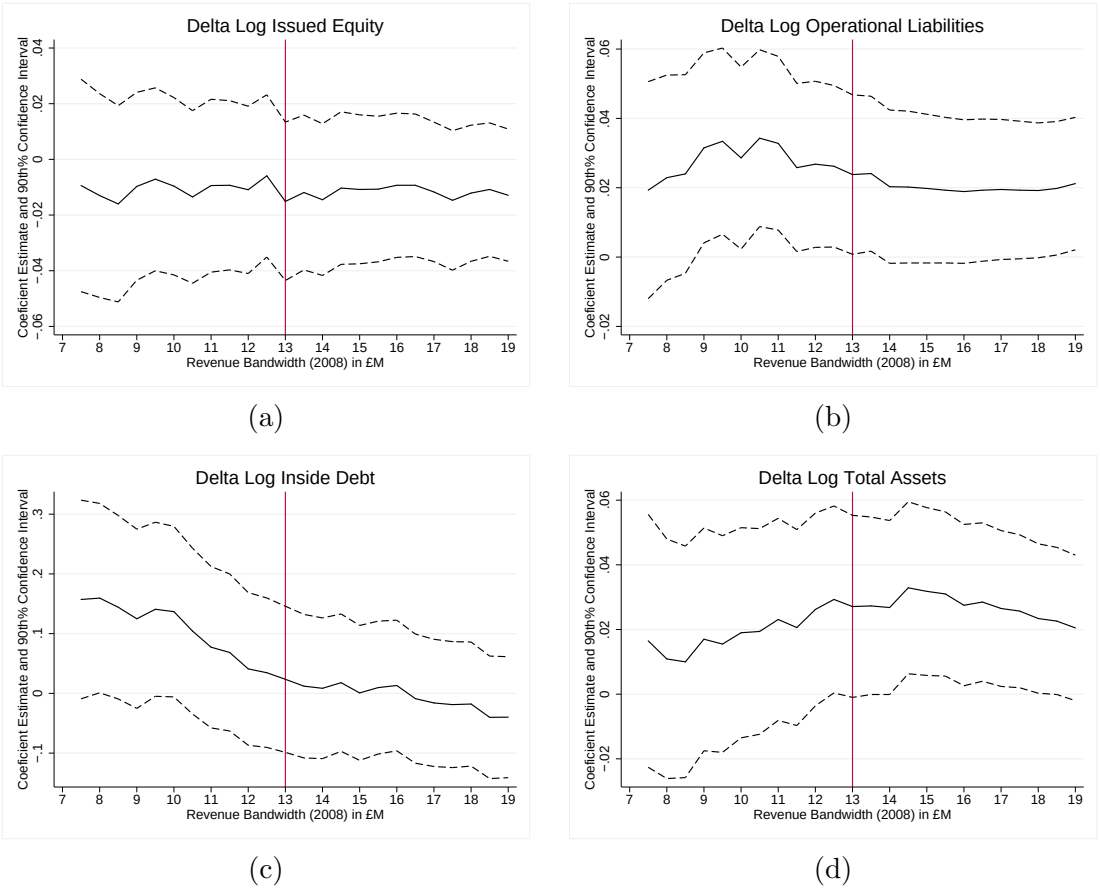


Table A2.1: Terms of Borrowing by Amount Borrowed for EFG-backed Loans in 2009

The table presents average conditions on EFG-backed loans issued in 2009. The source is the BIS 2013 report based on CfEL loan portfolio data available at: `http://fenjoyl.com/pdf/13-600-economic-evaluation-of-the-efg-scheme.pdf`

Amount borrowed	Average Interest Rates	Average Fees	Fees as % of loan value	Average loan terms (months)
£1K-£25K	8.10%	£560	3.30%	65
£25K-£50K	6.20%	£880	2.40%	76
£50K-£100K	5.30%	£1,650	2.30%	83
£100K-£250K	4.70%	£2,770	1.80%	79
> £250K	4.10%	£8,290	1.70%	76
Average	5.80%	£1,980	2.00%	76

Table A2.2: Assets and Receivables: Eligible vs Non-eligible Firms

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. Column 10 in Panel A estimates equation 2.1 excluding the firm fixed effect. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Panel Data

Dep. Var.	(1) ΔTotal Assets	(2) ΔCurrent Assets	(3) ΔReceivables	(4) Δln(Total Assets)	(5) Δln(Current Assets)	(6) Δln(Receivables)
$Eligible_i \times Post_t$	777,447***	212,424*	100,062	0.031*	0.024	-0.009
	(168,646)	(109,594)	(35,770)	(0.018)	(0.019)	(0.014)
Obs.	49,957	49,789	44,031	49,957	49,789	44,031
R-squared	0.231	0.193	0.206	0.242	0.233	0.189
Mean Dep. Var.	1,177,457	754,343	234,991	0.09	0.1	0.04

Panel B: Collapsed Data

Dep. Var.	(1) ΔTotal Assets	(2) ΔCurrent Assets	(3) ΔReceivables	(4) Δln(Total Assets)	(5) Δln(Current Assets)	(6) Δln(Receivables)
$Eligible_i \times Post_t$	812,834***	229,613**	-0.01	0.032	0.033	19,249
	(177,240)	(111,899)	(0.017)	(0.023)	(0.024)	(38,088)
Obs.	14,922	14,886	13,360	14,922	14,886	13,360
R-squared	0.147	0.135	0.198	0.192	0.188	0.295

Table A2.3: Dynamic Effects of EFG

The table presents results from estimating equation 2.1. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is specified in the top of each column. The dependent variable is specified in the top of each column. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Change in Logs				
	(1)	(2)	(3)	(4)
Dep. Var.	Δln(Internal Debt)	Δln(Trade Credit)	Δln(Issued Equity)	Δln(Total Assets)
$Eligible_i \times 2005$	-0.22	0.014	-0.072*	-0.00028
	(0.16)	(0.026)	(0.037)	(0.036)
$Eligible_i \times 2006$	-0.22	0.013	0.028	-0.022
	(0.16)	(0.025)	(0.036)	(0.033)
$Eligible_i \times 2007$	-0.22	0.0053	-0.061*	-0.055*
	(0.18)	(0.028)	(0.032)	(0.032)
$Eligible_i \times 2009$	-0.23	0.018	-0.045*	0.0039
	(0.17)	(0.029)	(0.026)	(0.023)
$Eligible_i \times 2010$	-0.054	0.043	-0.036	0.0016
	(0.16)	(0.027)	(0.027)	(0.024)
$Eligible_i \times 2011$	-0.084	0.036	-0.070**	0.029
	(0.15)	(0.027)	(0.028)	(0.029)
Obs.	43,307	46,219	40,582	49,957
R-squared	0.14	0.178	0.227	0.242

Panel B:Change in Levels				
	(1)	(2)	(3)	(4)
Dep. Var.	ΔInternal Debt	ΔTrade Credit	ΔIssued Equity	ΔTotal Assets
$Eligible_i \times 2005$	-79,188	-37,351	-38,065	29,274
	(180,259)	(44,741)	(32,837)	(279,946)
$Eligible_i \times 2006$	-34,411	-68,079	-59,310**	-298,149
	(159,125)	(47,773)	(29,526)	(288,270)
$Eligible_i \times 2007$	45,997	-88,871*	-27,572	-447,543
	(174,698)	(52,989)	(26,236)	(283,725)
$Eligible_i \times 2009$	-202,609	102,864*	-105,361***	1.07e+06***
	(193,478)	(53,439)	(30,308)	(289,661)
$Eligible_i \times 2010$	-120,873	-103,699**	-46,972	224,472
	(188,175)	(48,527)	(30,065)	(281,625)
$Eligible_i \times 2011$	-288,395	-55,950	-115,109***	466,013
	(225,180)	(55,427)	(32,887)	(305,200)
Obs.	43,307	46,219	40,582	49,957
R-squared	0.207	0.182	0.236	0.231

Table A2.4: Robustness Check—Alternative Sample

This table presents the robustness checks using alternative samples. In panel A, we focus on sub-sample of firms with negative external debt. In Panel B, we drop firms that are close to the threshold of £25M by excluding firms with revenue in between £22.5M to £27.5M in year 2008. $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008 and $Post_t$ is a dummy equal to one in the years 2009-2011. The dependent variable is gross profits. All columns include firm fixed effects and separate year effects for each 5-digit 2007 SIC industry. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Panel A: Firms with negative external debt

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	Δln(Fixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
$Eligible_i \times Post_t$	0.075	0.004	0.003	-0.003	60,092	0.014	0.031
	(0.191	(0.030)	(0.018)	(0.006)	(147,747)	(0.019)	(0.019)
Observations	12,906	12,734	12,501	12,906	10,385	12,014	10,385
R-squared	0.594	0.48	0.456	0.959	0.478	0.486	0.491

Panel B: Exclude firms with revenue in between £22.5M to £27.5M in 2008

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Δln(External Debt)	Δln(Fixed Assets)	Δln(Employees)	Survival	ΔProfits	Δln(Revenue)	Δln(Costs)
$Eligible_i \times Post_t$	0.406***	0.007	0.025***	0.002	254,559***	0.023**	0.027**
	(0.108)	(0.017)	(0.009)	(0.003)	(78,376)	(0.011)	(0.012)
Observations	35,443	39,729	39,059	47,673	32,173	38,275	32,186
R-squared	0.145	0.221	0.278	0.928	0.259	0.288	0.288

Chapter 3

Manager Turnover and Product Market Competition: Evidence from UK Family Firms

Family firms are important players in the economy around the world, especially in Europe and Asia (Porta et al., 1999; Claessens et al., 2000; Faccio and Lang, 2002). In the UK, family business made a 26% contribution to GDP in 2017, providing 35% of total employment(Oxford Economics, 2018). From the Santander bank's succession gap to the Samsung group's "soft succession", family control has drawn much attention from the public. Despite the prevalence of family firms, the corporate governance, especially the succession decisions in family firms remain understudied.[1] This paper empirically investigates how family firms balance the benefits and costs of the promotion of family members to key corporate management positions. Specifically, I examine the change of the structure of top executive management teams of family firms in response to the intense product market competition from foreign markets.

[1]Recent literature focuses on comparing performances of firms managed by family CEOs relative to those managed by non-family CEOs, and find ambiguous results. For instance, Anderson and Reeb (2003) find that the valuation of family firms listed in S&P 500 is higher than non-family firms. Villalonga and Amit (2006) study the Fourturn 500 companies and show that the higher valuation of family firms is only restricted to firms that are under management of founders. Morck et al. (2000); Adams et al. (2009) find similar worse performance of family firms. Moreover, Pérez-González (2006) compares the family succession with the unrelated replacement of family CEOs and finds that interited control perform worse, and Bennedsen et al. (2007) show causal evidence that family succession destroys firm value. Bandiera et al. (2017) provide evidence that family CEOs work less hours than professional CEOs and that explains 18% of the performance gap.

Deciding upon the succession of a senior executive manager is a critical challenge in determining a firm's survival in difficult times and growth prospective in the long run. It is a particularly topical issue for family companies. The choice of either appointing a connected family member or an unrelated person to a key managerial position can affect a firm's corporate policy, such as its investment decision, and hence influence a firm's performance (Bennedsen et al., 2007). Family firms are found to have lower debt financing costs: Lagaras and Tsoutsoura (2015) show that creditors value family ownership and control by explicitly requiring family members to stay in such firms. Family control brings several advantages to family firms according to theoretical models. Firstly, family management mitigates the agency problem between owner and mangers in family firms, where the ownership and control are more likely to be aligned (Shleifer and Vishny, 1986; Bolton and Von Thadden, 1998). Secondly, compared with unrelated professional managers, family managers have more incentives to make long-term investments (Cadbury, 2000). Lastly, family managers facilitates the firm-specific investments by using the firm-specific knowledge which is difficult to obtain(Donnelley, 1988; Barnes and Hershon, 1994). However, family managers may distort the objective of maximizing a firm's value and have incentives to search for private benefits at the cost of minority shareholders (Morck et al., 1988). Furthermore, family managers are generally inferior to professional managers, provided that talented managers are more likely to be selected from the outside labor pool than from within family kinships (Burkart et al., 2003; Pérez-González, 2006).

This paper aims to investigate the economic factors that would change the family succession decisions. Previous studies show that several factors can affect such transition decisions. Bennedsen et al. (2007) show that the gender of first born child is strongly correlated with family CEO succession decisions. Policy shifts can also affect family firm succession decisions, as Tsoutsoura (2015) finds that inheritance tax changes have influence on family succession decisions and investment strategies. In this paper, I examine how pressures from product markets affect top executive manager transitions. Specifically, I focus on the product market competition from foreign markets.

Product market competition has long been viewed as a vehicle to mitigate man-

agerial slack in corporations (Hart, 1983; Schmidt, 1997). While G-index (Gompers et al., 2003) is widely used as a proxi for good corporate governance, it is not easily appliable to the private firm sample. Bloom et al. (2015b) use survey measure of management practices and find that private equity firms are better managed than family firms and private firms. Private family firm provides a unique setting to study how product market competition influences the corporate governance. One advantage of studying family firms is that succession plan decision, as one of the most important aspect of the corporate governance, is one of the most observable corporate governance decisions. It also provides evidence of how product market competition works for private firms. Different from public firms, private firms have illiquid shares and more aligned separation of ownership and control, shareholders have relatively more incentive to monitor managers. Therefore, we should expect less pronounced effect in private firms if competition mitigates managerial slacks.

In theory, product market competition changes the benefits and costs of appointing a family manager. On the one hand, as product market competition intensifies, the good reputation and business relationships built up by the founder or previous family managers add to the advantages of passing the firm on into family hands. However, the advantage of family managers having less myopic investment strategies (Cadbury, 2000) becomes less important. When the competition increases, the priority is more likely be driven by the short-term survival as incumbent firms are more likely to be liquidated. On the other hand, the higher level of competition causes a decreased mark-up thus reduce the managerial slack, discipline the manger's behaviour. However, at the mean time, less rents also leave less rewards for managers and lowers the provision of managerial effort.[2] Together, the product market competition changes the mapping between manager's effort (skill) and firm's profit, and the overall effect is ambiguous. Moreover, unrelated managers with better skills selected from a larger talent pool are more likely to be capable to manage the firm (Burkart et al., 2003; Caselli and Gennaioli, 2013), which is precisely important when the competition is intensive, thus favouring the choice of an unrelated manager. Therefore, the variation in product market competition changes the relative benefits and costs of both forcing a current manager to leave a firm and appoint-

[2]Raith (2003) finds mixed results once allowing for the free entry of firms and changes the elasticity of substitution with larger size of the market.

ing a family member to a key managerial position. The net effect is ultimately an empirical question.

The UK is the fifth largest import country in the world, especially in manufactured goods. This makes foreign competition a crucial channel through which the product market competition changes the rewards for exerting effort or for hiring more talented managers. Furthermore, foreign competition is an important force that can have a substantial impact on firm level outcomes. It has been shown in the empirical literature that competition from foreign markets spurs innovation, improves productivity, changes ownership structures, and improves management quality (Cuñat and Guadalupe, 2009; Bloom et al., 2015a, 2016; Bena and Xu, 2017). As recognized by previous research, measurements of product competition, such as concentration ratio, Herfindahl-Hirschman-Index (HHI) or price-cost margins are difficult to compute or interpret, and also raise the concern of endogeneity (Schmalensee, 1989; Cuñat and Guadalupe, 2009). To avoid the flaws of these measurements, I compute the import penetration at industry level to measure the product market competition from foreign markets. To address potential endogeneity concerns because of either measurement error or family firms' anticipation of import penetration movements, I follow the literature and use import weighted average exchange rates at industry level as an instrumental variable for the import penetration (see Bertrand (2004); Cuñat and Guadalupe (2009)).

This study draws on historical managerial data from the Orbis database provided by the Bureau van Dijk (BvD). I define family firms by matching the family names of senior executive managers. Using the appointment and resignation dates of senior executive managers, I am able to track changes in senior executive managerial positions for each firm over time. In addition, the comprehensive database includes financial information on most of the private family firms in the sample, allowing me to control for firm characteristics. Trade data and domestic production information at the industry level is collected from the Eurostat database. I focus on a sample of UK manufacturing family firms because of the availability of domestic production data. The analysis sample covers the period from 2004 to 2014, including a period of Pounds depreciation in 2009 and appreciation in 2006 and 2012 [3], generating the

[3]See: Effective exchange rate index of Sterling from the Bank of England. `https://www.bankofengland.co.uk/boeapps/database/fromshowcolumns.asp?Travel=NIxAZxSUx&`

time varying exchange rates at the industry level.

My findings show that, jointly with firm characteristics such as firm size and age, product market competition from foreign markets plays an important role in senior executive managerial position changes. Foreign competition substantially increases the frequency of senior managers' resignations (with or without replacements), and, specifically, the number of family-manager departures. My results also show that family firms fire more family managers without any replacements and that they change current unrelated managers for new unrelated ones. In addition, I find that the results are more pronounced in the family firm sample run by founder family members. However, family firms without the family founder as a current manager are quieter when faced with high levels of foreign competition.

This paper adds to two strands of literature. Firstly, this paper contributes to the literature studying family firms. Instead of looking at family control on firm performance (Anderson and Reeb, 2003; Villalonga and Amit, 2006; Ellul et al., 2010; Bennett et al., 2016), this paper seeks to answer the question of what factors drive managerial position changes. This paper complements the existing research by showing that apart from the gender of first born child (Bennedsen et al., 2007) and the heritage tax rate (Tsoutsoura, 2015), the product market competition is an important economic factor that has a significant impact on succession decisions.

Secondly, this paper adds to the literature of examining the product market competition and corporate governance. Several papers show that product market competition can substitute for internal governance: Cuñat and Guadalupe (2005, 2009) find that product market competition changes the pay structure of U.S. executives. Bloom et al. (2010) show that the product market competition leads to a decentralized decision making to employees. Similarly, Guadalupe and Wulf (2010) provide evidence that trade liveralization reduces the corporate hieracchies. Giroud and Mueller (2010) find that comepeittion lowers the mangerial slack by presenting that BC law passage only causes signifiant decline of performance in firms operating in the non-competitive industry. Guadalupe and Pérez-González (2010) show that the product market competition lowers the private benefit of control based on anal-

FromSeries=1&ToSeries=50&DAT=RNG&FD=1&FM=Jan&FY=2000&TD=31&TM=Dec&TY=2025&FNY=Y&CSVF=TT&html.x=66&html.y=26&SeriesCodes=XUDLBK67&UsingCodes=Y&Filter=N&title=XUDLBK67&VPD=Y

ysis of 16 countries. In addition, Giroud and Mueller (2011) find that companies in more competitive industries benefit less from corporate governance. Chhaochharia et al. (2017) analyse the Sarbanes-Oxley Act of 2002 in US and show that corporate governance is more important when the product market competition is less. This paper extends the literature in evaluating the economic impacts of competition on corporate governance from public firms to private firms by focusing on a sample of private family firms in the UK, which represents an important part the economy.[4]My findings are consistent with the notion that product market competition from foreign markets plays an disciplinary role in corporate governance.

The rest of the paper is as follows: Section 3.1 describes the data; Section 3.2 develops the hypothesis; Section 3.3 outlines empirical strategy; Section 3.4 presents the results. In Section 3.5, I conduct robustness checks and Section 3.6 concludes.

3.1 Data

In this section, I briefly introduce the data collected from Bureau van Dijk (BvD) and Eurostats. Then, I put forward the definitions of family firms and successions. A summary of the sample is given at the end of this section. The details of variable names and definitions are available in Appendix A3.1.

3.1.1 Management and financial data

The Orbis database of the Bureau van Dijk (BvD) provides comprehensive historical management information on public and private firms from all over the world, and covers the population of private firms in the UK.[5] For each senior executive manager in the database, I collect information on age, gender, full name, and level of responsibility, as well as on the appointment and resignation dates.[6] Firm level

[4]In 2018, there were 5.7 million private sector businesses in the UK, and over 96% of businesses are (private) Micro Sized businesses with less than 10 employees. The Micro Sized businesses, however, represent about 33% of employment and 22% of turnover(Rhodes, 2018)

[5]The UK government requires that every private firm incorporated in the UK report the director's information, to the registry—Companies House (see: `https://www.gov.uk/government/publications/life-of-a-company-annual-requirements/life-of-a-company-part-1-accounts`). Bena and Xu (2017) also report that the majority of their firms from UK in an international analysis.

[6]In this paper, I focus on the change in senior executive managerial positions. One advantage of the database is its good coverage. I have detailed information on recorded senior executive

financials from 2004 to 2014 are obtained from the Fame database (see Brav (2009); Michaely and Roberts (2012); González-Uribe and Paravisini (2019)). The financial data is then matched with management data using the unique identifier generated by BvD. Based on the individual senior executive manager information collected from Orbis, I next construct the family firm sample and measure senior manager departures and successions.

3.1.2 Definition of family firms

The literature on family firm research uses a broad definition of family firms.[7] Considering the nature of the research and the availability of management information, I classify a company as a family firm if at least one family member is in the senior executive management team. A senior executive manager is defined as a family member if at the appointment date, at least one current or previous senior executive manager has the same family name. A founder-manager is defined as a senior executive manager who is appointed when the firm is incorporated.[8] And finally, a family manager is defined as a family founder if at least two founders have the same family names.

Two concerns arise with this "founder" definition: one is that two "founders" may have the same family name coincidently, and the other is that managers are not necessarily shareholders of a firm. However, considering that most firms in the sample are small, private and young firms in the manufacturing sector, then it reduces the probability that two senior managers have the same family name by chance, as compared to large and complicated conglomerates. Moreover, I combine the manager's information with shareholders details from the Orbis database, family firm managers own the majority of their shares.

managers for the population of private firms in the UK. However, firms do not have report the exact titles of managers, for example, "Chief Executive Officer" and "Chief Technology Officer" are not distinguishable and they both enter the database under the category of senior executives. For the majority of firms in sample, there is usually no specification that one person is at the highest level, and decisions are made collectively. I keep information on all senior executive managers if several managers are at the same level of position.

[7]For instance, Anderson and Reeb (2003) use founding family's ownership and (or) the presence of family members on the board of directors to identify family firms. Bennedsen et al. (2007) rely on the personal and family information. Villalonga and Amit (2006), Pérez-González (2006) use both personal and the ownership information.

[8]The management record does not necessarily date back to the incorporation date. In this case, a founder is defined as a manager who is the first to join the firm when the earliest data is available.

3.1.3 Definition of successions

Before I define succession events, an accurate specification of resignation or departure is required. A senior executive manager leaves a firm if he or she is not reappointed to the same position within one year. Starting with the firm manager level data, I match each departure date with all other appointment dates, and the family names of departing and new managers in the same firm. A "succession" is classified whenever the following three criteria are met: (1) the departing manager must have stayed in his or her position for at least one year; (2) the departing manager cannot be matched to him- or herself; and (3) the new manager is appointed around one year of time of the departure date. When there are several appointments matched with one departure, I chose the one with the minimal gap in between the appointment and departure dates.

Two different outcomes could happen following the departures. If there is no match of new managers within one year window of the departing manager, then it is classified as departures without replacement; otherwise there is a replacement. For replacements, I also distinguish in between the replacements by other family managers and those by unrelated managers. I count the number of both scenarios as two measures of outcome variables following the departure. Matching the family names of departing and appointed managers, I separate family replacements from unrelated replacements. For the subsequent analysis, I include measures of managerial changes in the following different dimensions: the number of senior executive manager departures, the number of family/unrelated senior executive manager departures, and the number of departures replaced by family managers/unrelated managers/without replacements.

3.1.4 Competition measurement

This paper focuses on product market competition from foreign competitors. Following Bertrand (2004) and Bloom et al. (2010), I use the import penetration as a proxy for product market competition from foreign markets. Import penetration is defined as the ratio of import value over the total value of import and domestic production. A time series of domestic production data at industry level for the manufacturing sector is obtained from the Eurostats Structural Business Statistic-

s database (SBS). International bilateral trade data between the UK and its 200 major trading-partner countries from 2004 to 2012 is from the Comext database in Eurostat.

3.1.5 Sample statistics

Starting with all UK firms in the manufacturing sector from the Orbis dataset, I exclude firms incorporated before 2004. I further restricted my interest to family firms with available financial data and in industries for which industry-country level trade data is avaialble. The sample includes a time period from 2004 to 2014.[9] Overall, these restrictions bring the number of the sample down to 13,478 companies. 10,788 of these family firms have family founder managers and 2,690 are not run by any family founder manager.

The characteristics of the family companies in the sample are summarized in Table 3.1. The first two columns are based on the full sample, and the next four columns present the summary of founder managed and non-founder managed family firms. The average age of family firms in my sample is 18, and the average size ($Total\ Assets$) is £0.72M. On average, family firms have 2.92 managers, 2.21 family managers and 1.60 family founder managers. The yearly average manager departure is 0.06 (0.04 family manager departures and 0.02 unrelated manager departures). Then I decompose the three different outcomes after departures: number of manager departure with no replacement, with family succession and with unrelated succession. For both family manager departures and unrelated managers, no replacement is most frequent compare to the cases with successions. On average, 0.021 (0.006) family manager departures are followed by no replacement (family succession) on average. Compared with family successions after the family manager departures, the number of unrelated replacements is slightly higher, with an average of 0.008. Relative to the founder-family firms, non-founder family firms are slightly larger (£0.76M v.s £0.71M) and more mature (19.24 v.s 17.73). By definition, founder manager family firms also have more family senior managers (2.50 v.s. 2.74).

[9]The industry code changed from NACE Rev1.1 to NACE Rev2 in 2007, so I follow Bena and Xu (2017) to link these two versions of the code. To construct instrumental variable, I then match the SIC industry code with the Prodcom industry code. The first 4 digits of the Prodcom code is the same as the NACE code throughout the sample.

Figure 3.1 includes the plots of industry distribution in Panel A, the average import penetration, and the average industry level exchange rates in Panel B and C respectively. In the first panel of Figure 3.1, we see that industries are evenly distributed, with no single industry in the manufacture sector has a portion of more than 0.4%. The second plot shows that there is an increasing trend in the industry average import penetration, with a high growth rate before the crisis and a slight drop after 2008. The last panel plots the time varying exchange rates. We can observe that there was an depreciation of Pounds around year 2008 and appreciations in 2006 and 2012. The identification is from the industry-time varying import weighted exchange rates at the industry level, which can not be directly shown in figures, therefore, I randomly select 10 industries from the 188 industries in my sample and report the import penetration and exchange rate index in Table 3.2 to show some variation of this index across. Moving from the top to the bottom of the table, there is a variation of import penetration and the import weighted exchange rates across industries. The bottom row in the table that the average of import penetration in analysis sample is 0.34 and the exchange rate index average is 0.90. The standard deviations of 0.18 and 0.15 are large compared to the mean.

3.2 Hypothesis

In theory, product market competition has an ambiguous effect on the managerial incentives in a principal agent setting (Schmidt, 1997). On the one hand, competition reduces the firm's markup, meaning that managers have less incentives to exert efforts. On the other hand, greater competition leads to a higher marginal return to managerial effort when the product demand function is more elastic. In addition, firms are more likely to be liquidated in face of more competition, and thus generates costs to managers of losing their jobs. Therefore, the product market competition disciplines managerial behavior. Overall, the net effects of these two countering effects are ambiguous. Raith (2003) allows for free firm entry into the market with substitutable products, and the profits of surviving firms do not change as more firms exit the market with lower profitability, so the prediction of competition and effort is unambiguously positive.

Competition may also change the market transparency—the managers actions are more closely tied to the other managers' in the rest of the same market. Therefore, increased competition reduces managerial slacks and results in lower intensity of agency problems (see Hart (1983); Hermalin (1992)).

It is also worth noting that a firm can improve its performance by either inducing more managerial efforts or replacing managers with higher skilled ones; similar theoretical predictions apply to the relationship between the product market competition and managerial skills. Higher skilled managers are more capable of managing firms though hard times and the marginal return to higher skill is also higher when competition is high. However, the absolute change of profits may be lower to relative higher managerial talents.

How would product market competition change the relative advantage of family managers versus unrelated managers? Holding product market competition unchanged, hiring family managers brings advantages to firms: 1) aligned ownership and control reduces agency problem; 2) less myopic investment decisions; 3) lower cost of borrowing for family firms; 4) firm-specific knowledge. However, disadvantages also exist: 1) private benefits of control does not necessary maximizing firm's profits; 2) poor managerial skills relative to professional unrelated managers. If the net effect of competition on effort/skill is zero, then competition favours the family manager as lower cost of capital and long-run business relationships are important, especially during hard times. However, long term investment strategies may not as important as in normal times as the priority is the short run survival. If there is a positive relationship between competition and effort/skill, then competition incentivize the unrelated managers to exert effort, mitigate the agency problem. It is also rewarding to hire a manager with higher managerial skills, thus favouring the choice of an unrelated manager. If there is a negative correlation of competition and managerial effort/skill, meaning that lower rent reduces the incentive to exert managerial effort, then hiring more capable unrelated managers brings less advantage.

In sum, the theoretical implication of competition on number of family managers departure and appointments is ambiguous, and it is ultimately an empirical question. However, differentiating the reward to skill channel from the reward to effort channel

through which the competition works is not straightforward and out of the scope of this paper. The joint hypotheses that this paper tests are: 1) family managers obtain the private benefit of control; 2) product market competition changes the managerial effort/skill-profits relationship.

3.3 Empirical strategy

3.3.1 Empirical specifications

To estimate the baseline correlation between competition and the change of senior managerial positions, I run the following OLS regression:

$$y_{f,t+2} = \beta_0 + \beta_1 \ Import \ Pen_{i,t} + x_{f,t-1}\beta_2 + Year \ FE_t + Ind \ FE_i + \epsilon_{ft} \qquad (3.1)$$

where f denotes a firm, i denotes an industry and t denotes time. $y_{f,t+2}$ is the outcome variable in two years window from year $t+1$ to year $t+2$.[10] I denote by $Import \ Pen_{i,t}$ the import penetration for industry i at time t. In all regressions, firm characteristics $x_{f,t-1}$ controls for lagged firm age and size.[11]

To investigate the effect of foreign competition on senior executive managerial position changes, I examine two sets of outcomes. In the first set of regressions, I look at the total number of senior manager departures ($Number \ of \ departures$). Then I examine the number of family manager departures ($Number \ of \ fam \ departures$) and the unrelated manager departures ($Number \ of \ unrel \ departures$) separately. In the second set of regressions, I focus on the outcomes of vacant managerial positions. For both family and non-family manager departures, there are three different outcomes: 1) the departing manager is not replaced (managerial position is left vacant); 2) the departing manager is replaced by a family manager; and 3) the departing manager is replaced by an unrelated manager. I run regressions on 6 scenarios with outcome variables including: $Family \ no \ replacement$ $Family \ succession$ and $Fam - unrelated \ replacement$ for the family manager

[10] The real effect on firm performance and managerial changes takes time to observe in response to the intensified product market competition.

[11] Alternatively, unobservable firm level characteristics can be controlled for by adding firm fixed effects. I show that all results are robust with firm fixed effects in Session 3.5.

departures; *Unrelated noreplacement*, *Unrelated replacement* and *Unrelated − fam replacement* for the unrelated manager departures respectively.

3.3.2 Endogeneity problem and instrument variables

Equation 3.1 estimates the impact of the foreign competition measured by import penetration on senior executive managerial position changes. However, a common concern with the OLS specification is that the omitted variables may be correlated with the independent variable of interest—*Import* $Pen_{i,t}$. The endogeneity can be attributed to several reasons. The import penetration may be reversely determined by executive manager position changes through a shift in managers' operating strategies. Furthermore, the import penetration for each industry is calculated as the ratio of imports over the total value of imports and domestic production, thus generating the measurement error.[12] In addition, the concerns of endogeneity arise when family firms anticipate import penetration fluctuations, especially in post-recession periods when governments have carried out stimulus packages both in the UK and within Europe. All of these factors tend to bias the estimates down towards zero.

To address these concerns of endogeneity, I follow Bertrand (2004), and Cuñat and Guadalupe (2009) to implement import weighted exchange rates as instrumental variables. For each industry in each year, I construct an industry specific exchange rates index, calculated as bilateral exchange rates between the UK and other countries (measured as foreign currency against 1 £), weighted by the import share of total imports in each industry in the base year of the sample period.

Specifically, I include both the current and lagged exchange rate indices as our instruments considering that it usually takes time for import penetration to have an impact. The regression at firm-year level is as follows

$$\begin{aligned} Import\ Pen_{i,t} = \alpha_0 + \alpha_1 Exch\ Rate_{it} + \alpha_2 Exch\ Rate_{i,t-1} + x_{f,t-1}\alpha_3 \\ + Year\ FE_t + Ind\ FE_i + \xi_{ft} \end{aligned} \tag{3.2}$$

where *Import* $Pen_{i,t}$ is the import penetration for industry i in year t. *Exch* $Rate_{i,t}$

[12]For some of our industries, due to privacy policies or data collection restrictions, the domestic production at NACE 4-digit level is missing for some years.

and $Exch\ Rate_{i,t-1}$ are industry specific exchange rate indices in the current and lagged year. $x_{f,t-1}$ controls for firm level characteristics and is defined similarly as in Equation 3.1. Year and industry fixed effects are controlled for and error terms are clustered at the industry level.

The first stage regression results are presented in Table 3.3 in detail. Columns (1) to (3) report the first stage regression results over the full sample, sub-sample with and without non-founder family senior managers in firms respectively. Starting with the full sample, we observe that a 1 unit of appreciation in the pound against trading partner currencies is associated with a 0.79 decrease in contemporary import penetration and a 0.61 increase in import penetration one year later. The delayed positive reaction of import penetration to the appreciation of a local currency is a reflection of the "J- curve" effect in the related trade literature.[13] The import weighted exchange rate index becomes higher with an appreciation of the pound, and the import penetration will also becomes higher as it is cheaper to import from other countries, and foreign goods are competitive to domestic products. Year fixed effects and the industry fixed effect control for industry specific and time varying unobservable shocks. The identification of the outcome variable comes from the industry-time level exchange rates fluctuation. First stage F-test results are all significant at a 1% level. A Hansen J-test confirms that the over identification problem is not a concern.

The second stage regression is:

$$y_{i,t+2} = \beta_0 + \beta_1 \widehat{Import\ Pen}_{i,t} + x_{f,t-1}\beta_2 + +Year\ FE_t + Ind\ FE_i + \xi_{ft} \qquad (3.3)$$

where $\widehat{Import\ Pen}_{i,t}$ is the fitted value from Equation 3.2, $x_{f,t-1}$ is the same set of control variables as before. The coefficient β_1 is of our main interest; it captures the change in the characteristics of senior managerial positions as foreign competition increases through the appreciation of the pound. A positive estimates of β_1 indicates that increased competition results in an increase in outcome variables in the subsequent two years.

The coefficients in Columns (2) and (3) show similar results as those in the first column. In unreported tables, the first stage results including the lagged two period

[13]See Bahmani-Oskooee and Ratha (2004) for a literature review.

exchange rates yields higher F-test statistics. The results are similar and robust under both specifications, but for the brevity, I present all results based on the current and lagged exchange rates for all the analysis in this paper.

The exclusion restriction assumption for the identification strategy is that the import weighted exchange rates only affect the senior manager changes in family firms through changes in industry level import penetration. Import weighted exchange rates, which is not only dependent on domestic prices, but exchanges rates with all trading partners at the industry level, are less likely to be predicted by companies. Moreover, using the static import weights in 2002 and the nominal exchange rates reduce the impact of time-varying import demand from different countries, and reduces the explanatory power for potential confounding factors such as exports. In fact, following Cuñat and Guadalupe (2009), I find that the current and lagged industry level exchange rates are not significantly correlated with the export openness (see Table 3.3), which lends support to the exclusion restriction assumption.

3.4 Results

3.4.1 Senior executive manager departure

In this section, I provide the baseline OLS and IV-2SLS regression results of product market competition on the senior executive manager departures.

Table 3.4 presents the estimates of the impact of foreign competition on the number of management departures. The regressions are estimated over the full sample with three sets of dependent outcome variables: total number of departures, the number of family departures and the number of unrelated departures. We observe that the association between the number of senior executive manager resignations and import penetration in Column (1) is weak. In contrast, once the import penetration is instrumented with the industry specific exchange rate indices, the estimates are significant at 1% level: one unit increase in import penetration leads to 0.29 more senior manager departures. The economic effect is also sizeable, one standard deviation increase in import penetration causes a jump in number of departures by 0.052 (0.287*0.182) in the future two years, approximately 44% (0.052/(2*0.059)) of the sample average.

Next, I move to the different outcomes after the different types of manager departures, i.e. the number of family departures and unrelated departures. The results are presented in Columns (3) to (6). OLS regressions in Columns (3) ((5)) implies a positive (negative) correlation between family (unrelated) departures and foreign competition, but the estimates are not statistically significant. The IV-2SLS regression in Column (4) shows that one unit increase in import penetration causes a 0.21 increase in the number of family manager departures, significant at the 5% level. The coefficient is also economically significant, as one standard deviation increase in import penetration leads to 0.04 (0.207*0.182) more family managers leaving firms in the following two years, 50% (0.038/(0.038*2)) of the sample average. However, the 0.08 increase in the number of unrelated manager departures is not significant as a result of intensified competition from foreign markets.

As we can see from Table 3.4, the OLS estimates are substantially biased towards zero, which could be because of the correlation between the import penetration measurement of competition and/or the omitted variables. In addition, firm characteristics are also strongly correlated with the managerial position changes. Larger and older firms appear to increase the decisions to fire managers compared to the smaller or younger firms, given the import penetration level. But the magnitude of coefficients are much smaller than the competition measure, import penetration (0.015 and 0.001 in Column (2), Table 3.4).

Overall, Table 3.4 shows that higher level of competition from foreign markets reduces the private benefit of family control and leads to more senior managers, in particular family managers, leave their positions (no matter if the outgoing manager is replaced by a new manager or not). These results suggest that more significant increase in family managers departing is consistent with the notion that they are less qualified compared to the unrelated professional managers.

3.4.2 Outcomes of family manager departures

Table 3.5 presents the regressions results of the outcomes after family manager departures. There are three outcomes after a family manager departure: the departing manager is not replaced by any new managers/ replaced by a family manager/ replaced by an unrelated manager. Similarly, OLS and IV-2SLS regression results are

shown for each outcome variable in Table 3.5.

We observe that the correlation between the product market competition from foreign markets and the family departures without replacement is not significant from zero (Column (1)). However, the IV results show that greater competition causes more managers departing the firm without anyone to replace them. Specifically, one standard deviation increase in import penetration results in a 0.037 (0.206*0.182) increase in the unrelated succession of unrelated managers in the future two years. This accounts for about 89% (0.037/(0.021*2)) of the sample average.

The combined results in Tables 3.4 and 3.5 suggest that increased import penetration leads to more resignations of family managers. Furthermore, departing managers are not replaced by either another family manager or unrelated manager. Instead, previous positions held by family managers are left vacant. This is consistent with the notion that positions that family managers held are likely to be redundant. When the competition is high, the benefits of private control of families are smaller because the mark-up is lower. To cut costs, the vacancies will not be therefore filled in with any new managers.

3.4.3 Outcomes of unrelated manager departures

Table 3.6 presents the regression results for different outcomes of unrelated manager departures. Similar to the case when a family manager leaves a firm, there are three outcomes after the unrelated manager leaves: he or she is not replaced by any new managers, replaced by an unrelated manager or replaced by a family manager.

The first two columns show that intense competition results in an increasing number of no replacement after unrelated senior executive manager departures in the coming two years, but the coefficient is not statistically significant. The next two columns report that there is a significant increase in the number of unrelated manager successions at the 5% level. Specifically, one standard deviation increase in import penetration results in a 0.01 (0.0475*0.182) increase in the number of unrelated managers being replaced by another unrelated manager in the future. This accounts for about 108% (0.0086/(0.004*2)) of the sample average. For the unrelated to family manager replacement, the estimates are positive, but not significant.

In sum, foreign competition causes an increase in the departures of unrelated

managers, who are thereafter replaced by other unrelated managers in the future two years. Combining the findings in Table 3.6 with the results from Table 3.5, the preferred interpretation is that greater foreign competition leads to more departures of redundant family managers, and the replacement of important unrelated managers with other outside managers. The fact that more family managers are kicked out while the estimates on the number of unrelated manager departures is insignificant is supportive of the first hypothesis that family control brings private benefits of control to the families, which is less valuable when the competition is higher. Moreover, family firms replace unrelated managers with other unrelated managers, indicates that unrelated mangers have relative advantage over family managers, either because they are more capable or because the disciplinary role of product market competition—unrelated managers exert more effort and generates a higher marginal return. In addition, competition disciplines unrelated managers since they have to work hard to prevent the firm from going bankrupt. Altogether, this finding provides evidence that is consistent with the second hypothesis that the product market competition changes the correlation between effort/skill and firm profits and can serve as an external corporate governance discipline.

3.4.4 Founder v.s non-founder managed family firms

Previous literature provides mixed evidence of the performance of firms run by founder-CEOs versus non-founder-CEOs. For instance, Fahlenbrach (2009) shows that founder-CEO firms spend more in Research and Develpopment, investments, and have higher market valuations based on a sample of large US public firms. Adams et al. (2009) use the propotion of dead founder-CEOs as instrument of founder-CEO status and provide causal evidence that the control of founder-CEOs adds to the positive firm performance. However, using survey data for 13,345 firms across 32 countries, Bennett et al. (2016) show that founder-CEO-managed firms have lower management scores and the firm's performance improve once the ownership is changed. Villalonga and Amit (2006) show that although family firms under perform compared with other firms, founder-CEOs firms have higher valuation, the descent family CEOs destroy family firm value. The empirical evidence indicates that the conflicts between family shareholders and non-family shareholders are more

costly for non-founder-managed family firms than the founder-managed family firms.

In my analysis sample, 10,788 family firms are run by the founder family managers in the base year, and 2,690 firms are not managed by founders. From Table 3.2, we observe that the number of departures, manager replacements are similar for founder and non-founder managed firms. However, the change of managers team may not be the same in response to the intense competition.

I repeat the previous analysis on two sub-samples of family firms run by founders and non-founders. The results are presented in Tables 3.7 to 3.9. The dependent variable in the first two columns of Table 3.7 is the total number of senior manager departures in a forward two-year window. The next two columns show the impact of competition on the number of family manager departures, and the estimates on the number of unrelated departures are reported in the last two columns. For the founder-CEO managed firms, the significant increases in both the total number of manager departures and family manager departures, with 0.05 (0.2698*0.182) and 0.04 (0.2130*0.182) increases respectively due to a one standard deviation increase in the import penetration (Columns (1) and (3)). In contrast, for the non-founder run family firms, we only observe a significant increase in the total number of manager departures. However, when I decompose the total number of departures to family manager departures and unrelated manager departures, the estimates are not significant.

Similarly, Table 3.8 presents the regression results with dependent variables including: the number of no replacements, the number of family successions, and the number of unrelated successions after family manager departures. Most of the results for the sub-sample within founder run family firms carry what we already known for the full sample. In particular, we observe that a one standard deviation increase in import penetration causes 0.04 (0.221*0.182) more family manager departures without replacement. The result is significant at the 5% level. Columns (4) and (6) report a decrease in family replacements and an increase in unrelated replacements after a family manager departure respectively. However, the estimates are insignificant and the magnitude is smaller than the coefficient in the first column.

Table 3.9 includes the IV regression results for dependent variables of no replacement, unrelated replacement, and unrelated to family replacement after unrelated

managers respectively. Column (3) shows a significant increase in the number of unrelated replacements following unrelated manager departures caused by increased competition. However, we observe insignificant estimators in the rest of regressions for non-founder family firms.

To sum up, the main results presented in the previous section remains in the sub-sample of founder-managed family firms, however, the non-founder family firms seem to be quieter and reluctant to change the current manager team structure. One explanation of these results is that founder-run family firms are more vulnerable than the successor-run family firms in face of the intense competition, so they need to change the management team accordingly (the necessary management skills to set up a firm may not fit the skills needed when competition is high.). Another possibility is that non-founder managed family firms are already at the best manger-firm match stage, so no changes can improve the performance. It can also be the case that the non-founder managed firms implement worse corporate governance policies, which makes difficult to change the senior management teams.

3.5 Robustness Check

The identification strategy in this paper is to include both the current and lagged exchange rate indices as the instrumental variables of import penetration. However, the current and lagged exchange rates may be autocorrelated and bring up potential concerns. Therefore, it is worth checking the reduced form regressions. The results in Tables A3.2 to Table A3.4 are similar to the findings in the IV-2SLS specifications. We observe that the appreciation in lagged exchange rates causes more family managers to be replaced by unrelated managers or be without replacement after they leave a firm. Unrelated managers are more likely to be replaced by unrelated managers. However, the current exchange rate works in the opposite way, as the coefficients are mostly negatively significant. When I only include the sub-sample with founder running family firms, we observe similar patterns in estimated coefficients, shown in Table A3.3. And for non-founder run family firms, the estimates are not significant any longer, consistent with the heterogeneity findings in Section 3.4.4.

For all the regressions in this paper, I control for the firm level heterogeneity by adding firm level control variables: firm age and size. Alternatively, I can add firm fixed effect to better control for more unobservable firm characteristics. I examine all analysis by adding firm fixed effects. Table A3.5 presents the results on manager departures, and Table A3.6 and Table A3.7 display the different consequences of family manager departures and non-family departures respectively. All the results are similar compared to the main tables in the paper. Coefficients of estimates are at same significance levels and the magnitudes of coefficients are very close.

3.6 Conclusion

In this paper, I use detailed senior manager information on UK family firms in the manufacturing sector to examine the impact of foreign competition on family firm executive managerial position changes. I look at the specific channel, the foreign competition through which the market competition works. I measure foreign competition using import penetration at the industry level and instrumented it with current and lagged import weighted exchange rates. I find that increasing product market competition from foreign countries causes a significant increase in the number of senior manager resignations in family firms, especially the number of family senior manager resignations. I then examine the outcomes after a manager departs and show that foreign competition also leads to an increase number of family manger departures without a replacement and an higher number of unrelated replacements after an unrelated manager leaves a firm.

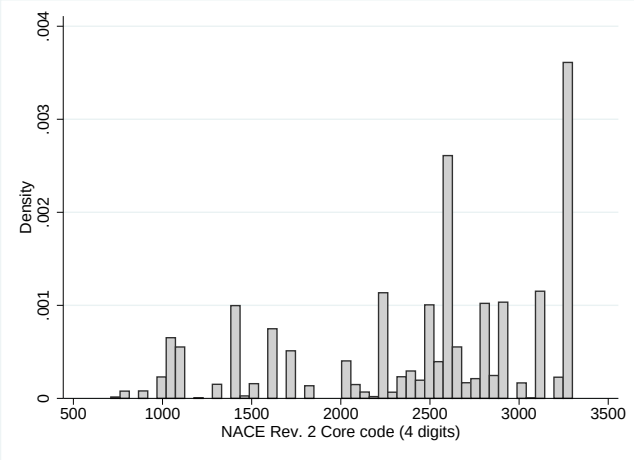
This paper is build on the notion that family managers add less value to the firm than the unrelated managers. Previous studies found that family related managers work less hours (Bandiera et al., 2017), and the family successions causes worse firm performance (Bennedsen et al., 2007). In the chapter 1, I show a negative impact of manager departures on young firm's growth induced by exogenous changes of managers' outside options, suggesting costly frictions to replace managers in those firms. In comparison, family firms in my analysis sample are larger and more established compared to the young and small firm sample in the first chapter. Even when the manager is not replaced after departure, there is not necessary harmful for the

firm as previous positions may be redundant if the managerial skill/effort provided was low. Therefore, replacing family related managers with unrelated managers represents an improvement of corporate governance.

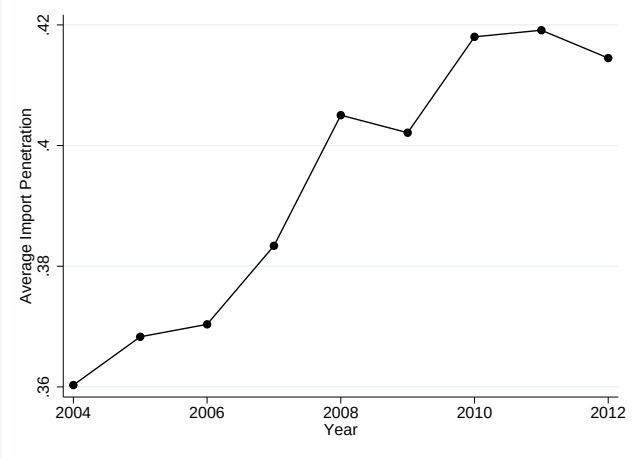
Overall, the findings in this paper suggest that we cannot reject the joint hypotheses that 1) within family transitions provide private benefits of family control and 2) product market competition changes the rewards for managerial effort (or) skill, and can serve as an external discipline of corporate governance. While this paper has not fully distinguished the effort from the skill channel, it is of interest for future research. In the sub-sample tests, I show that family firms without founders are quieter in the face of foreign competition. My results highlight that foreign competition has a statistically significant and sizeable causal effect on senior manager position changes in family firms. This paper adds an important economic force, foreign competition, to prior known factors, such as firm characteristics, institutional environments and tax policies, that affect family firm succession plans. Future work is needed to investigate the mechanisms through which family firm succession decisions are driven.

Figure 3.1: Industry Distribution, Import Penetration and Exchange Rate Index

Panel A: Industry Distribution



Panel B: Industry Average Import Penetration



Panel C: Industry Average Import-weighted Exchange Rate Index

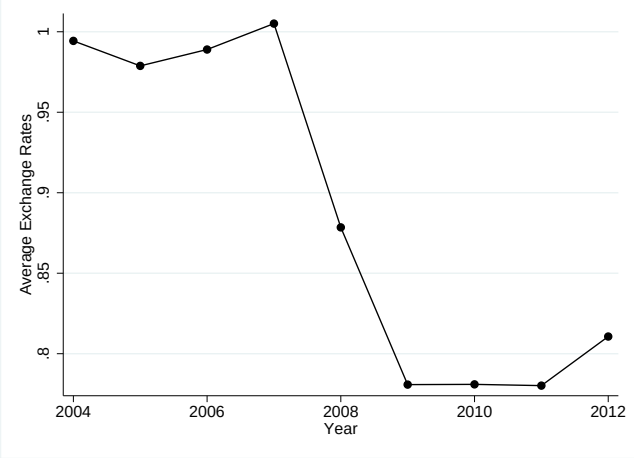


Table 3.1: Summary Statistics: Dependent Varibles

This table summaries the managers number and different types of managers in family firms. All statistics are reported for family firms with at least one year financial data available from year 2004 to 2014. Further restrictions requires the firms are operating in industries with trade data non-missing. The first two columns report the statistics over full sample, and the next two columns present the summary statistics the sub-sample of firms with family founder and the last two columns corresponds to sub-sample with non founders. The reported numbers of observation varies across variables due to the availability. *Total Assets* refers to total assets (in £M) in the balance sheet by the end of year 2004.

		Full sample		Founder Sample		Non-founder Sample	
		Mean	S.d	Mean	S.d	Mean	S.d
Num. of senior managers		2.922	1.284	2.908	1.262	2.962	1.367
Num. of family senior managers		2.207	0.963	2.499	0.853	1.740	1.123
Num. of family founder senior managers		1.607	0.991	2.187	0.731	0	0
Num. of manager departures		0.059	0.182	0.057	0.278	0.067	0.296
Num. of Family manager departures		0.038	0.226	0.041	0.235	0.028	0.184
Num. of Nonfamily manager departures		0.021	0.153	0.016	0.135	0.039	0.208
Family manager departure	Num. of No replacement	0.021	0.166	0.027	0.182	0.018	0.146
	Num. of family succession	0.005	0.080	0.005	0.078	0.004	0.069
	Num. of unrelated succession	0.008	0.107	0.009	0.111	0.007	0.092
Unrelated manager departure	Num. of No replacement	0.015	0.184	0.012	0.116	0.028	0.178
	Num. of unrelated succession	0.004	0.063	0.003	0.057	0.006	0.081
	Num. of family succession	0.002	0.044	0.001	0.039	0.004	0.071
Age in base year		18.029	16.715	17.725	17.052	19.236	15.251
Size (Aseets in £M)		0.718	1.313	0.709	1.319	0.755	1.287
Num. of firms		13,478		10,788		2,690	

Table 3.2: Summary Statistics: Independent Variable

In this table, 10 industries are randomly selected out of 177 industries in the analysis sample in the manufacturing sector with non-missing trade data. Import penetration and the exchange rates are both measured at the industry level. For each industry, the average and the standard deviation of the import penetration and corresponding exchange rate indices are displayed. Import penetration is defined as the ratio of import value over the sum of import and domestic production.Industry specific exchange rates is the bilateral exchange rates between UK and its main trade partners, weighted by the import share from each country in the base year.

Selected Industries	Import Penetration		Exchange Rate	
	Mean	S.d.	Mean	S.d.
2042	0.4530	0.0740	0.8754	0.0963
2052	0.1991	0.0250	0.8770	0.0991
2053	0.4936	0.0160	0.8691	0.0935
2059	0.4366	0.0380	0.9096	0.1054
2060	0.5016	0.1081	0.8777	0.0894
2110	0.5763	0.1893	0.8679	0.1184
2120	0.4618	0.1278	0.8753	0.0962
2211	0.4678	0.0494	0.8517	0.1054
2219	0.3814	0.0473	0.8894	0.1135
2221	0.3157	0.0277	0.8477	0.0970
Aggregated	0.3415	0.1815	0.9027	0.1531

Table 3.3: First Stage Regressions

The table shows the first stage regression results of equation 3.2. The dependent variable is *Import Penetration*. Column (1), (2) and (3) present the results across full sample, the sub-sample with and without founder family managers respectively. *Import Penetration* is import divided by import plus domestic production at 4-digit NACE code industry level. Column (4) to (6) show the exclusion restriction tests over different samples, where the dependent variable is the export openness. Export openness is defined as export value divided by the domestic production value. Industry specific exchange rates is the bilateral exchange rates between UK and its main trade partners, weighted by the import share from each country in the base year. *Total Assets* is the total assets (in £M) in the balance sheet each year. Firm age, year and industry fixed effects are also controlled for in all regressions. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Import Penetration			Export Openness		
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged Exch.Rates	0.6110**	0.6140**	0.5955**	1.5291	1.3852	2.1409
	(0.2485)	(0.2466)	(0.2600)	(1.5267)	(1.4634)	(1.8861)
Exch.Rates	-0.7895***	-0.7834***	-0.8135***	-2.1379*	-2.0326*	-2.5791*
	(0.1713)	(0.1664)	(0.1961)	(1.1814)	(1.1271)	(1.4485)
Total Assets	0.0000	0.0001	-0.0001	0.0009*	0.0009*	0.0009
	(0.0001)	(0.0001)	(0.0001)	(0.0005)	(0.0005)	(0.0010)
Age	-0.0000	-0.0000	0.0000	-0.0000**	-0.0000**	-0.0000
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Num. of Obs	117,051	93,598	23,453	116,138	92,901	23,237
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
R squared	0.0699	0.0692	0.0724	0.0102	0.0099	0.0119
F test	11.0583***	11.5811***	8.8138***	2.0563	2.1087	1.8453
Sample	Full	Founder	Non-founder	Full	Founder	Non-founder

Table 3.4: Competition and Manager Departures

The table shows the OLS and IV-2SLS regression results on manager departures. The dependent variable include the total number of manager departures, the number of family manager departures, and the number of unrelated manager departures. All dependent variables are calculated in two years forward window. Column (1),(3) and (5) present the OLS regression results; column(2),(4) and (6) present the baseline IV-2SLS estimations. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry specific exchange rates is the bilateral exchange rates between UK and its main trade partners, weighted by the import share from each country in the base year. *Total Assets* is the total assets (in £M) in the balance sheet each year. Firm age, year and industry fixed effects are also controlled for in all regressions. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures		Number of fam departures		Number of unrel departures	
	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	-0.0045	0.2870***	0.0030	0.2070**	-0.0075	0.0800
	(0.0268)	(0.1022)	(0.0232)	(0.1040)	(0.0157)	(0.0530)
Size	0.0150***	0.0150***	0.0023**	0.0023**	0.0128***	0.0128***
	(0.0025)	(0.0025)	(0.0010)	(0.0009)	(0.0020)	(0.0020)
Age	0.0010***	0.0010***	0.0007***	0.0007***	0.0003***	0.0003***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Num. of Observations	117,051	117,051	117,051	117,051	117,051	117,051
R-squared	0.005	0.004	0.002	0.001	0.007	0.007
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	Full	Full

Table 3.5: Competition and Outcomes of Family Departures

The table shows the OLS and IV-2SLS regression results on different outcomes of family manager departure. The dependent variable is the number of family departures with no replacement in first two column; the number of family successions in column (3) and (4), the number of unrelated replacements in last two columns. All dependent variable numbers are calculated in forward two years window. Column (1), (3) and (5) present the OLS regression results; column(2),(4) and (6) present the baseline IV-2SLS estimations with import penetration instrumented by current and lagged industry specific exchange indices respectively. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Family noreplacement		Family Succession		Fam-unrelated replacement	
	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	-0.0084	0.2063**	0.0092	-0.0055	0.0094	0.0207
	(0.0203)	(0.0963)	(0.0062)	(0.0306)	(0.0083)	(0.0315)
Total Assets	0.0020***	0.0020***	0.0001	0.0001	0.0015***	0.0015***
	(0.0007)	(0.0007)	(0.0003)	(0.0003)	(0.0005)	(0.0005)
Age	0.0004***	0.0004***	0.0002***	0.0002***	0.0001***	0.0001***
	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Num. of Observations	117,051	117,051	117,051	117,051	117,051	117,051
R-squared	0.001	-0.000	0.001	0.001	0.000	0.000
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	Full	Full

Table 3.6: Competition and Outcomes of Unrelated Departures

The table shows the OLS and IV-2SLS regression results on different outcomes of unrelated manager departure. The dependent variable is the number of unrelated departures with no replacement in first two column; the number of unrelated replacements in column (3) and (4), the number of family replacements in last two columns. All dependent variables are calculated in two years forward window. Column (1), (3) and (5) present the OLS regression results; column(2),(4) and (6) present the baseline IV-2SLS estimations with import penetration instrumented by current and lagged industry specific exchange indices respectively. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Unrelated noreplacement		Unrelated replacement		Uunrelated-fam replacement	
	OLS	IV-2SLS	OLS	IV-2SLS	OLS	IV-2SLS
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	-0.0182	0.0361	0.0005	0.0475**	0.0030	0.0103
	(0.0134)	(0.0329)	(0.0082)	(0.0217)	(0.0056)	(0.0169)
Total Assets	0.0091***	0.0091***	0.0030***	0.0030***	0.0011***	0.0011***
	(0.0013)	(0.0013)	(0.0006)	(0.0006)	(0.0003)	(0.0003)
Age	0.0002***	0.0002***	0.0000	0.0000	0.0000**	0.0000**
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Num. of Observations	117,051	117,051	117,051	117,051	117,051	117,051
R-squared	0.005	0.005	0.002	0.002	0.001	0.001
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	Full	Full

Table 3.7: Competition and Manager Departures—Founder Firms vs. Non-founder Firms

The table shows the 2SLS regression results on manager departures over sample with and without family founder as senior manager in base year 2004. The dependent variable is the number of total senior manager departures in two years window after the competition shock in first two columns. The last two columns show the regression results of family senior manager departures in two years forward window. All the columns report IV-2SLS estimations with import penetration instrumented by current and lagged industry specific exchange rates respectively. All the dependent variable numbers are calculated in two years forward window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures		Number of family departures		Number of unrelated departures	
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	0.2698**	0.4143*	0.2130**	0.2082	0.0568	0.2061
	(0.1060)	(0.2240)	(0.0968)	(0.2031)	(0.0401)	(0.1726)
Total Assets	0.0127***	0.0228***	0.0019*	0.0032	0.0108***	0.0196***
	(0.0020)	(0.0080)	(0.0010)	(0.0021)	(0.0015)	(0.0063)
Age	0.0013***	-0.0004	0.0010***	-0.0005***	0.0003***	0.0001
	(0.0001)	(0.0003)	(0.0001)	(0.0001)	(0.0001)	(0.0002)
Num. of Observations	93,598	23,453	93,598	23,453	93,598	23,453
R-squared	0.005	0.004	0.002	-0.000	0.007	0.007
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Founder	Non-founder	Founder	Non-founder	Founder	Non-founder

Table 3.8: Competition and Outcomes of Family Departures—Founder Firms vs. Non-founder Firms

The table shows the IV-2SLS regression results of competition on the different outcomes of family manager departures over different sub-samples. The dependent variables are the number of family successions in first two columns, the number of non family-successions in column (3) and (4), the number of no replacement in column (5) and (6) and the number of unrelated successions in last two columns. All the dependent variable numbers are calculated in two years forward window. Column (1), (3) and (5) present the regression results over sub-sample of founder family firms. Column(2),(4) and (6) present the regression results over sub-sample without family founder firms. The import penetration instrumented by current and lagged industry specific exchange indices respectively. All the dependent variable numbers are calculated in two years forward window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	No replacement		Family Replacement		Fam-unrelated Replacement	
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	0.2206**	0.1705	-0.0042	-0.0182	0.0131	0.0545
	(0.0942)	(0.1472)	(0.0313)	(0.0489)	(0.0375)	(0.0481)
Total Asset	0.0015**	0.0036*	-0.0001	0.0004	0.0017***	0.0007
	(0.0007)	(0.0020)	(0.0004)	(0.0006)	(0.0006)	(0.0009)
Age	0.0006***	-0.0003**	0.0002***	-0.0001	0.0002***	-0.0001
	(0.0001)	(0.0001)	(0.0000)	(0.0001)	(0.0000)	(0.0001)
Num of Observations.	93,598	23,453	93,598	23,453	93,598	23,453
R-squared	-0.000	-0.001	0.001	0.000	0.001	0.001
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Founder	Non-founder	Founder	Non-founder	Founder	Non-founder

Table 3.9: Competition and Outcomes of Unrelated Departures—Founder Firms vs. Non-founder Firms

The table shows the IV-2SLS regression results of competition on the different outcomes of unrelated manager departures over different sub-samples. The dependent variables are the number of family successions in first two column, the number of non family-successions in column (3) and (4), the number of no replacement in column (5) and (6) and the number of unrelated successions in last two columns. All the dependent variable numbers are calculated in two years forward window. Column (1), (3) and (5) present the regression results over sub-sample of founder family firms. Column(2),(4) and (6) present the regression results over sub-sample without family founder firms. All the dependent variable numbers are calculated in two years forward window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	No replacement		Unrelated Replacement		Unrelated-fam Replacement	
	(1)	(2)	(3)	(4)	(5)	(6)
Import Penetration	0.0016	0.1274	0.0341**	0.1064	0.0232	-0.0450
	(0.0313)	(0.1323)	(0.0165)	(0.0686)	(0.0163)	(0.0437)
Total Asset	0.0080***	0.0133***	0.0026***	0.0044***	0.0006**	0.0025**
	(0.0011)	(0.0046)	(0.0005)	(0.0017)	(0.0003)	(0.0012)
Age	0.0002***	0.0002	0.0001**	-0.0001**	0.0000**	0.0000
	(0.0001)	(0.0002)	(0.0000)	(0.0001)	(0.0000)	(0.0001)
Num. of Observations	93,598	23,453	93,598	23,453	93,598	23,453
R-squared	0.004	0.005	0.002	0.002	0.000	0.001
Year FE	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES
Sample	Founder	Non-founder	Founder	Non-founder	Founder	Non-founder

3.7 Appendix

Table A3.1: Variable Definitions

Variable	Definitions
Dependent Variable	
Leave	Leave is a dummy variable defined at firm level. This variable takes the value of 1 if the firm have at least one manager leave the firm, and takes the value of 0 otherwise.
Transition	Transition is a dummy variable defined at firm level. This variable takes the value of 1 if the firm have at least one resigned manager matched to a new coming manager within 1 year gap, 0 otherwise.
Family Succession	Family transition is a dummy variable defined at firm level. This variable takes the value of 1 if the Transition is 1 and the new manager has the same family name as the leaving manager.
Competition Measurement	
Import Penetration	Import penetration is defined as Import value/(Import value+Domestic production value) at each industry level. Import value comes from Eurostat's Comext database and it is measured as the aggregate import (in thousands EUR) from all partner countries around the world. Domestic production value is obtained from Eurostat's Structural Business Statistics database (SBS) (in thousands EUR) .
Export Open.	Export Openness is defined as Export value / (Export value +Domestic production value) for each industry. Export value comes from Eurostat's Comext database and it is is measured as the aggregate import (EUR thousands) from all partner countries around the world. Domestic production value is obtained from Eurostat's SBS database.
Exch. Rate	The exchange rate is measured at industry level. It is the weighted average of bilateral exchange rates between UK and its main trading partners. The weight is the import share of total import value.
Control Variables	
Age	Firm age is measured in years since incorporation.
Ln Assets	Natural logarithm of total assets (in £M) .

Table A3.2: Robustness Tests: Reduced Form Regressions Full Sample

The table shows the reduced form regression results over full sample. The dependent variables are the number of departures in first three columns, the outcome of family manager departures in the next three columns, and the outcomes of unrelated manager departures in last three columns. All the dependent variable numbers are calculated in two years forward window. Column (1) shows the regression results with total number of departures, and column (2) and (3) displays the number of family departures and unrelated departures respectively. The dependent variables in column (4) and (7) are number of no replacement after family manager departure and after unrelated manager departure. Similarly, column (5) and (8) shows the results when the dependent variables are family replacement after family or unrelated managers departures. Column (6) and (9) includes the regression results with dependent variables of unrelated replacement number after family and unrelated departures respectively. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures			Outcomes of family departures			Outcomes of unrelated departures		
	Total	Family	Unrelated	No rep	Family suc	Unrelated suc	No rep	Family suc	Unrelated suc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged EX	0.1677**	0.1487**	0.0189	0.0931*	-0.0015	0.0488**	-0.0522*	0.0277	0.0447***
	(0.0812)	(0.0608)	(0.0433)	(0.0475)	(0.0200)	(0.0220)	(0.0289)	(0.0181)	(0.0156)
Exch. Rates	-0.2260***	-0.1651***	-0.0609	-0.1603***	0.0042	-0.0192	-0.0114	-0.0374**	-0.0111
	(0.0570)	(0.0618)	(0.0439)	(0.0545)	(0.0238)	(0.0239)	(0.0256)	(0.0176)	(0.0121)
Total Assets	0.0150***	0.0023**	0.0128***	0.0020***	0.0001	0.0015***	0.0091***	0.0030***	0.0011***
	(0.0025)	(0.0010)	(0.0020)	(0.0007)	(0.0003)	(0.0005)	(0.0013)	(0.0006)	(0.0003)
Age	0.0010***	0.0007***	0.0003***	0.0004***	0.0002***	0.0001***	0.0002***	0.0000	0.0000**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Num. of Observations	117,051	117,051	117,051	117,051	117,051	117,051	117,051	117,051	117,051
Adjusted R^2	0.005	0.002	0.007	0.001	0.001	0.000	0.005	0.002	0.001
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Full	Full	Full	Full	Full	Full	Full	Full

Table A3.3: Robustness Tests: Reduced Form Regressions: Founder Family Firm Sample

Notes: The table shows the reduced form regression results over sub-sample of founder family firms. The dependent variables are the number of departures in first three columns, the outcome of family manager departures in the next three columns, and the outcomes of unrelated manager departures in last three columns. All the dependent variable numbers are calculated in two years forward window. Column (1) shows the regression results with total number of departures, and column (2) and (3) displays the number of family departures and unrelated departures respectively. The dependent variables in column (4) and (7) are number of no replacement after family manager departure and after unrelated manager departure. Similarly, column (5) and (8) shows the results when the dependent variables are family replacement after family or unrelated managers departures. Column (6) and (9) includes the regression results with dependent variables of unrelated replacement number after family and unrelated departures respectively. All the dependent variable numbers are calculated in two years forward window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures			Outcomes of family departures			Outcomes of unrelated departures		
	Total	Family	Unrelated	No rep	Family suc	Unrelated suc	No rep	Family suc	Unrelated suc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged EX	0.2251***	0.1905***	0.0346	0.1198**	-0.0072	0.0533**	-0.0285	0.0230	0.0410***
	(0.0711)	(0.0599)	(0.0367)	(0.0505)	(0.0223)	(0.0255)	(0.0278)	(0.0210)	(0.0153)
Exch. Rates	-0.2150***	-0.1705***	-0.0445	-0.1718***	0.0036	-0.0130	0.0005	-0.0269**	-0.0198*
	(0.0665)	(0.0615)	(0.0315)	(0.0558)	(0.0245)	(0.0282)	(0.0244)	(0.0124)	(0.0117)
Total Assets	0.0127***	0.0019*	0.0108***	0.0015**	-0.0001	0.0017***	0.0080***	0.0026***	0.0006**
	(0.0020)	(0.0010)	(0.0015)	(0.0007)	(0.0004)	(0.0006)	(0.0011)	(0.0005)	(0.0003)
Age	0.0013***	0.0010***	0.0003***	0.0006***	0.0002***	0.0002***	0.0002***	0.0001**	0.0000**
	(0.0001)	(0.0001)	(0.0001)	(0.0001)	(0.0000)	(0.0000)	(0.0001)	(0.0000)	(0.0000)
Num. of Observations	93,598	93,598	93,598	93,598	93,598	93,598	93,598	93,598	93,598
Adjusted R^2	0.006	0.003	0.007	0.001	0.002	0.001	0.004	0.002	0.001
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Founder	Founder	Founder	Founder	Founder	Founder	Founder	Founder	Founder

Table A3.4: Robustness Tests: Reduced Form Regressions Non-founder Family Firm Sample

The table shows the reduced form regression results over sub-sample of non-founder family firms. The dependent variables are the number of departures in first three columns, the outcome of family manager departures in the next three columns, and the outcomes of unrelated manager departures in last three columns. All the dependent variable numbers are calculated in two years forward window. Column (1) shows the regression results with total number of departures, and column (2) and (3) displays the number of family departures and unrelated departures respectively. The dependent variables in column (4) and (7) are number of no replacement after family manager departure and after unrelated manager departure. Similarly, column (5) and (8) shows the results when the dependent variables are family replacement after family or unrelated managers departures. Column (6) and (9) includes the regression results with dependent variables of unrelated replacement number after family and unrelated departures respectively. The import penetration instrumented by current and lagged industry specific exchange indices respectively. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. The independent variable import penetration is the value of import divided by the sum of import and the domestic production. *Total Assets* is the total assets in the balance sheet each year in £M. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures			Outcomes of family departures			Outcomes of unrelated departures		
	Total	Family	Unrelated	No rep	Family suc	Unrelated suc	No rep	Family	Unrelated suc
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Lagged EX	-0.0855	-0.0341	-0.0514	-0.0172	0.0187	0.0287	-0.1574	0.0484	0.0603
	(0.2202)	(0.1319)	(0.1335)	(0.0890)	(0.0509)	(0.0543)	(0.1057)	(0.0382)	(0.0479)
Exch. Rates	-0.2885*	-0.1463	-0.1422	-0.1214	0.0105	-0.0438	-0.0696	-0.0843	0.0239
	(0.1538)	(0.1347)	(0.1457)	(0.0976)	(0.0381)	(0.0383)	(0.1100)	(0.0584)	(0.0324)
Total Assets	0.0228***	0.0032	0.0196***	0.0036*	0.0004	0.0007	0.0133***	0.0044***	0.0025**
	(0.0080)	(0.0021)	(0.0064)	(0.0021)	(0.0006)	(0.0009)	(0.0047)	(0.0017)	(0.0012)
Age	-0.0004	-0.0005***	0.0001	-0.0003**	-0.0001	-0.0001	0.0002	-0.0001**	0.0000
	(0.0003)	(0.0001)	(0.0002)	(0.0001)	(0.0001)	(0.0001)	(0.0002)	(0.0001)	(0.0001)
Num. of Observations	23,453	23,453	23,453	23,453	23,453	23,453	23,453	23,453	23,453
Adjusted R^2	0.005	0.001	0.008	0.001	0.001	0.000	0.005	0.003	0.002
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sample	NF	NF	NF	NF	NF	NF	NF	NF	NF

Table A3.5: Robustness Tests: Competition and Manager Departures with Firm Fixed Effects

The table shows the IV-2SLS regression results on manager departures. The dependent variables include the total number of manager departures, the number of family manager departures, and the number of unrelated manager departures. All the dependent variable numbers are calculated in forward two years window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is the weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £M) is the total assets in the balance sheet. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Number of departures (1)	Number of fam departures (2)	Number of unrel departures (3)
Import Penetration	0.2781***	0.2076**	0.0705
	(0.1010)	(0.1057)	(0.0547)
Num. of Observations	117,032	117,032	117,032
R-squared	0.001	0.001	0.000
Year FE	YES	YES	YES
Firm FE	YES	YES	YES
Sample	Full	Full	Full

Table A3.6: Robustness Tests: Outcomes of Family Manager Departures with Firm Fixed Effects

The table shows the IV-2SLS regression results on different outcomes of family manager departures. The dependent variable is the number of family departures with no replacement in first three columns; the number of family successions in Columns (3) to (6), the number of unrelated replacements in last three columns. All dependent variable numbers are calculated in forward two years window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £ million) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Family noreplacement			Family Succession			Fam-unrelated replacement		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Import Pen	0.2040**	0.2156**	0.1752	-0.0041	-0.0031	-0.0162	0.0218	0.0147	0.0523
	(0.0960)	(0.0930)	(0.1501)	(0.0307)	(0.0317)	(0.0483)	(0.0313)	(0.0371)	(0.0475)
Num. of Obs.	117,032	93,581	23,451	117,032	93,581	23,451	117,032	93,581	23,451
R-squared	-0.002	-0.002	-0.002	0.000	0.000	0.000	0.000	0.000	0.000
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Founder	Non-founder	Full	Founder	Non-founder	Full	Founder	Non-founder

Table A3.7: Robustness Tests: Outcomes of Unrelated Manager Departures with Firm Fixed Effects

The table shows the IV-2SLS regression results on different outcomes of unrelated manager departures. The dependent variable is the number of unrelated departures with no replacement in first three columns; the number of unrelated replacements in Columns (3) to (6), the number of family replacements in last three columns. All dependent variable numbers are calculated in forward two years window. The independent variable *Import Penetration* is the value of import divided by the sum of import and the domestic production. *Import Penetration* is instrumented by current and lagged industry specific exchange indices. Industry exchange rate is weighted average of exchange rates at the industry level, where the weight is the import share from each country in 2002. *Total Assets* (in £ million) is the total assets in the balance sheet each year. All regressions control for year and industry fixed effects and are estimated at firm-year level. * significant at 10%; ** at 5%; *** at 1%. Standard errors (in brackets) are clustered at industry level for all the regressions.

	Unrelated noreplacement			Unrelated replacement			Uunrelated-fam replacement		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Import Penetration	0.0107	-0.0045	0.1020	0.0483**	0.0147	0.1074	0.0109	0.0109	-0.0480
	(0.0322)	(0.0313)	(0.1344)	(0.0221)	(0.0371)	(0.0700)	(0.0171)	(0.0171)	(0.0444)
Num. of Observations	117,032	93,581	23,451	117,032	93,581	23,451	117,032	117,032	23,451
R-squared	0.000	0.000	0.001	0.001	0.000	0.001	0.000	0.000	0.001
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Firm FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Sample	Full	Founder	Non-founder	Full	Founder	Non-founder	Full	Founder	Non-founder

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