Dissecting the Effect of Financial Constraints on Small Firms[1]

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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.

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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, Demirgüç-Kunt, and Maksimovic, 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.[2] Our results provide *prima facie* evidence of small firms' direct need to finance labor (cf., Benmelech, Bergman, and Seru, 2015; Bakke and Whited, 2012; Pagano and Pica, 2011); and highlight how financial 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, Campello, and Weisbach, 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).[3] 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

[2] This evidence is supported by the result found in Campello, Graham, and Harvey (2010) that firms planned to cut employment during the crisis.

[3] 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).

of the effect of the CGS on corporate outcomes (cf., Angrist and Pischke, 2009). 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 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, Campello, and Weisbach, 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, Sraer, and Thesmar, 2010; D'Acunto, Tate, and Yang, 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, Schoar, and Severino 2015; Gopinath et al., 2017; Borio et al., 2016; Charles, Hurst, and Notowidigdo, 2018). Evidence also exists for layoffs being personally costly to managers and/or generating reputational concerns for firms (Agarwal and Kolev, 2016; 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 relief. These estimates point to sizable 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, McKenzie, and Woodruff, 2008; McKenzie and Woodruff, 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, Lopez-de-Silanes, and Shleifer, 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, Campello, and Weisbach, 2011). The results are especially consistent with theoretical and empirical work suggesting that firms face collateral constraints (Stiglitz and Weiss, 1981; Besanko and Thakor, 1987; Whited, 1992; Kiyotaki and Moore, 1997; Hennessy and Whited, 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.[4]

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, 2014; Duygan-Bump, Levkov, and Motoriol-Garriga, 2015; Benmelech, Frydman, and Papanikolaou, 2019).[5] 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 adjustments that would follow capital investment reactions to external finance because of the labor-capital complementarities in the production process (e.g., Benmelech, Bergman, and Seru, 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, 1986), or if labor market frictions make it costly for firms to adjust their labor force (e.g., Oi, 1962; Sharpe, 1994; Michaels, Page, and Whited, 2019). 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. They 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, Bergman, and Seru, 2015).

[4] Some of the papers in the collateral channel literature include: Chaney, Sraer, and Thesmar, 2012; Schmalz, Sraer, and Thesmar, 2017; Jensen, Leth-Petersen, and Nanda, 2014; Corradin and Popov, 2013; Adelino, Schoar, and Severino, 2015; Kerr, Kerr, and Nanda, 2015; and Bahaj, Foulis, and Pinter, 2017.

[5] Several papers provide evidence that financing frictions affect labor during non-recessionary periods: Agarwal and Matsa, 2013; Cantor, 1990; Sharpe, 1994; Matsa, 2010; Bakke and Whited, 2012; Benmelech, Bergman, and Enriquez, 2012; Agrawal and Matsa, 2013; and Michaels, Page, and Whited, 2019. Other papers in the literature include Almeida, Campello, and Weisbach, 2011 and Pagano and Pica, 2011.

We also contribute to the literature on the employment effects of the Great Recession.[6] 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).[7] 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, 2017) and "lenders' health" for US firms with syndicated loans (Chodorow-Reich, 2014). 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 during the crisis, whereas the majority of small firms have no access to public debt (Adrian, Colla, and Shin, 2012; Ivashina and Scharfstein, 2010; Chodorow-Reich, 2014). 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, 2000; 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, 2014; 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, Sraer, and Thesmar, 2010; Italy: de Blasio et al., 2014; US: Brown and Earle 2017; D'Acunto, Tate, and Yang, 2017; Chile: Mullins and Toro, 2017).

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

[7] 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.

The rest of this paper proceeds as follows. In Section 1, we describe the data in detail. In Section 2, we characterize the institutional setting of the UK CGS. We explain the empirical strategy in Section 3. Section 4 presents the results. In Section 5, we summarize a battery of robustness checks. Section 6 concludes the paper.

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; Michaely and Roberts, 2011; and 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.[8] 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: Δ *external debt*, Δ *inside debt*, Δ *trade credit*, and Δ *issued 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

[8] 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).

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' *total 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.[9] 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., Mullins and Toro, 2017).[10] The filings do not include information on loans' interest or default rates either. Hence, similar to other papers in the literature (e.g., Lelarge, Sraer, and Thesmar, 2010), we use the survival probability of companies—which we can track in the data—as the main measure of default.

We measure the year-to-year in employees: Δ *number 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: Δ *fixed 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 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: Δ *revenue*, Δ *cost of sales*, and Δ *profits*, 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.

[9] In addition, reporting on account components is sparser—for example, we have several missing observations for long-term debt (see Table 1).

[10] While they cannot be fully ruled out, there is little support for side effects in survey evidence (IFF, 2016; London Economics, 2017; BIS, 2013).

Finally, we present our main results using logarithmic transformations of our outcome variables (e.g., Δ *ln(external debt + 1)*) in order to mitigate the potential impact of outliers (which we show below can significantly affect results: see Section 4.2). However, we also present results using the untransformed variables in order to best interpret the magnitudes of results.

2 Institutional Context: UK Small Firms 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.[11] The EFG 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, 2017). 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.[12]

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).[13] 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.[14] 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.[15]

[11] 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, Innovation, and Skills.
[12] 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 £4,875 M (£11,419 M). Source: BBB, 2017b.
[13] Currently, there are over 40 participating lenders. For more details on the application process and the list of lenders see: BBB (2014).
[14] Term limits are also imposed: between 3 months and 10 years for term lending and between 3 months and 3 years for overdrafts.
[15] 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.

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.[16] Second, borrowers are required to pay a 2% annual premium over the costs and fees charged by the lender.[17] The premium is collected quarterly 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.[18] 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.[19] 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).[20] 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

[16] The cap was originally set at 9.75% but was revised in 2012 to 15% per lender.
[17] 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 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.
[18] See, for example, https://www.money.co.uk/business-loans.htm
[19] In 2012, eligibility was further extended to businesses with revenues of no more than £41M.
[20] Alternative forms of assistance for exports are provided by UK Export Finance.

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 sizable 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 policymakers is that these schemes are the most effective tool for increasing small firms' access to finance (Beck, Klapper, and Mendoza, 2010; OECD, 2018).

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 A.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 A.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).[21]

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 4.

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).

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

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 5.2). We also exclude firms in sectors that are not eligible for the EFG (see Section 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 make sure that firms report detailed financial statements the year pre-launch (see Section 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 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 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 1). 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%).[22]

[22] 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, Thesmar, and Sraer (2010) is also composed of smaller firms (i.e., 1.82 employees), as they focus on start-ups. Finally, the sample of Mullins and Toro (2017) 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.

3.2 Methodology

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

$$(1)\ \Delta\, k_{it} = \alpha_i + \gamma_t \times Industry\ FE + \beta Eligible_i \times Post_t + \varepsilon_{it}$$

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) 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 (1) is β, which measures the average change in the dependent variable ($\Delta\, k_{it}$) 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 relatively 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, 2009), 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. 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 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 5, we discuss these issues in more detail and show suggestive evidence from several robustness checks against their empirical relevance.

4 Results

Tables 2, 3, and 4 summarize results from estimating different versions of equation (1). Panels C and D in Table 2, and Panel B in Tables 3 and 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, Duflo, and Mullainathan, 2004). Table 5 presents results from estimating a more flexible version

of equation (1), where we include a full set of interactions between year dummies and the variable $Eligible_i$.

4.1 External Debt

Table 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 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 1 and Table 1), and short-term debt results are less robust (see Table 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.[23] 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., Mullins and Toro, 2017). 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), we cannot assess the practical relevance of these explanations empirically. By the same token, we cannot measure take-up rates and adjust ITT estimates to obtain TOT estimates (cf., Angrist and Pischke, 2009). Under our

[23] For identification purposes we restrict attention to eligible firms close in size to the £25M eligibility threshold; see Section 2.

identification assumptions, however, the economically significant increase in external debt reported in Table 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. 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).

4.2 Employment and Investment

Table 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 3). This relative increase is economically significant, as it corresponds to a 78% increase in employment over the unconditional mean (3; see Column 1, Panel A, Table 3). The employment response to the EFG was quick. Figure 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).[24]

In contrast to the robust employment adjustments, Table 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 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

[24] 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).

adjustments. We find only weak evidence of any relative adjustments in these variables for eligible firms following the EFG launch (see Table A.2 in the Appendix). Further, Figure 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).

4.3 Interpretation of the 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, 2014; Duygan-Bump, Levkov, and Motoriol-Garriga, 2015; Benmelech, Frydman, and Papanikolaou, 2018). 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, Bergman, and Seru, 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, 1988) 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, Page, and Whited, 2019). 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 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, Bergman, and Seru, 2015). In support of labor market frictions theories, we show in Panel C of Table 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 layoffs 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, 2017). The increase in long-term debt shown in Table 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.

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, Colla, and Shin, 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 6), which we use as a proxy for the value of firms' redeployable assets before the Great Recession.

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, Thakor, Udell, 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, Sraer, and Thesmar, 2010; Kerr, Kerr, and Nanda, 2015; Acs et al., 2016; D'Acunto, Tate, and Yang, 2017).

A similar alternative interpretation is that employment cuts during the crisis were 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, Schoar, and Severino, 2015; Gopinath et al., 2017; Borio et al., 2016; Charles, Hurst, and Notowidigdo, 2018). Evidence also exists about how layoffs can be personally costly to managers and/or generate reputational concerns for firms (Agarwal and Kolev, 2016; 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, Sraer, and Thesmar, 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 1; for an exception, see Mullins and Toro, 2017 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 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 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 overinvestment hypothesis would predict a stronger reaction from firms where information frictions are potentially more pronounced, results in Panels B and D of Table 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

[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.

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

Earle 2017; Chile: Mullins and Toro, 2017), 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, Sraer, and Themsar (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). 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, Sraer, and Thesmar (2010) study the mid-1990s, a decade of high economic growth, and focus on new 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 bankrutpcy.

4.5 Estimating the Costs of Financial 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 1 and 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 A.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 (1) using $\Delta External\ Debt_{it}$ (and logs) as dependent variable, and the following equation:

$$(2)\quad \Delta k_{it} = \alpha_i + \gamma_t \times Industry\ FE + \beta\ \Delta External\ Debt_{it} + \varepsilon_{it}$$

where we instrument $\Delta External\ Debt_{it}$ using. $Eligible_i \times Post_t$, and all variables are as defined in Section 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 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 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 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 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 3.

A comparison between columns 1 and 2 in both panels of Table 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, McKenzie, and Woodruff, 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 4.4). Overall, these estimates point to sizable 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. McKenzie, De Mel, and Woodruff (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 (2006) 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.

5 Identification Tests and Robustness

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

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 3.2. We now turn to the second concern.

Table 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 A.3 in the Appendix). The table presents the results found when estimating a more flexible version of equation (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 evidence 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 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). Figure 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 A.2 in the Appendix). In the figure, we present results using several revenue bandwidths, including the £13M bandwidth of our main specifications.

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 4 we show that the results are similar in twelve alternative subsamples of companies with revenue within a bandwidth of £7.5M to £18.5M, around the £25M eligibility threshold (see also Figure A.3 in the Appendix). Figure 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 subsamples.

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 A.4, Panel A). 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.

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. 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.

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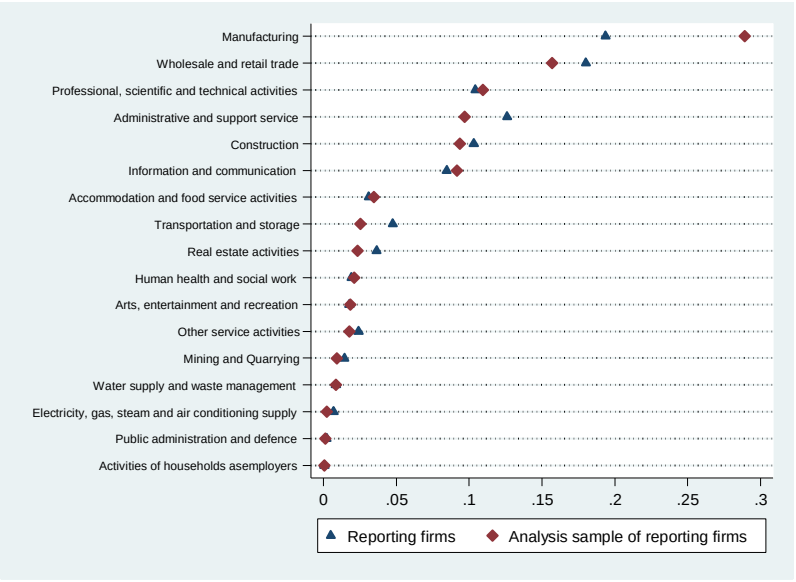
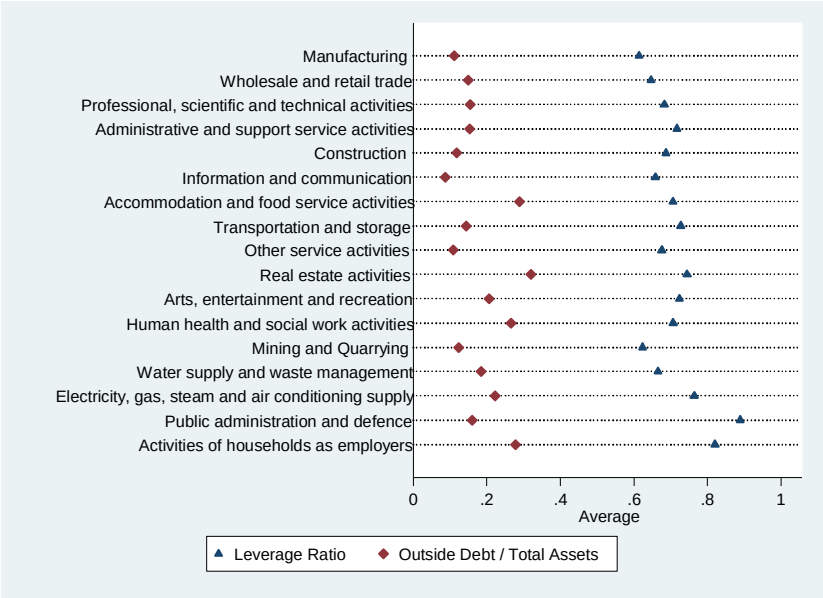
Figure 1: Industry distribution of UK SMEs

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

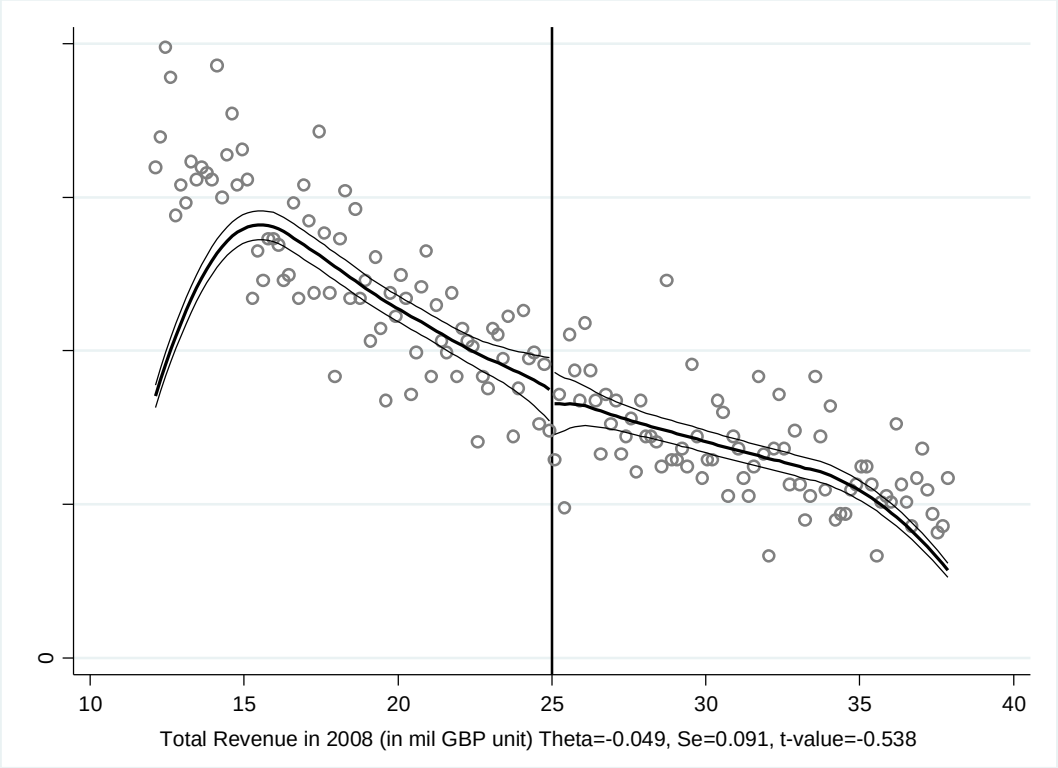


Panel B: Mean leverage ratio



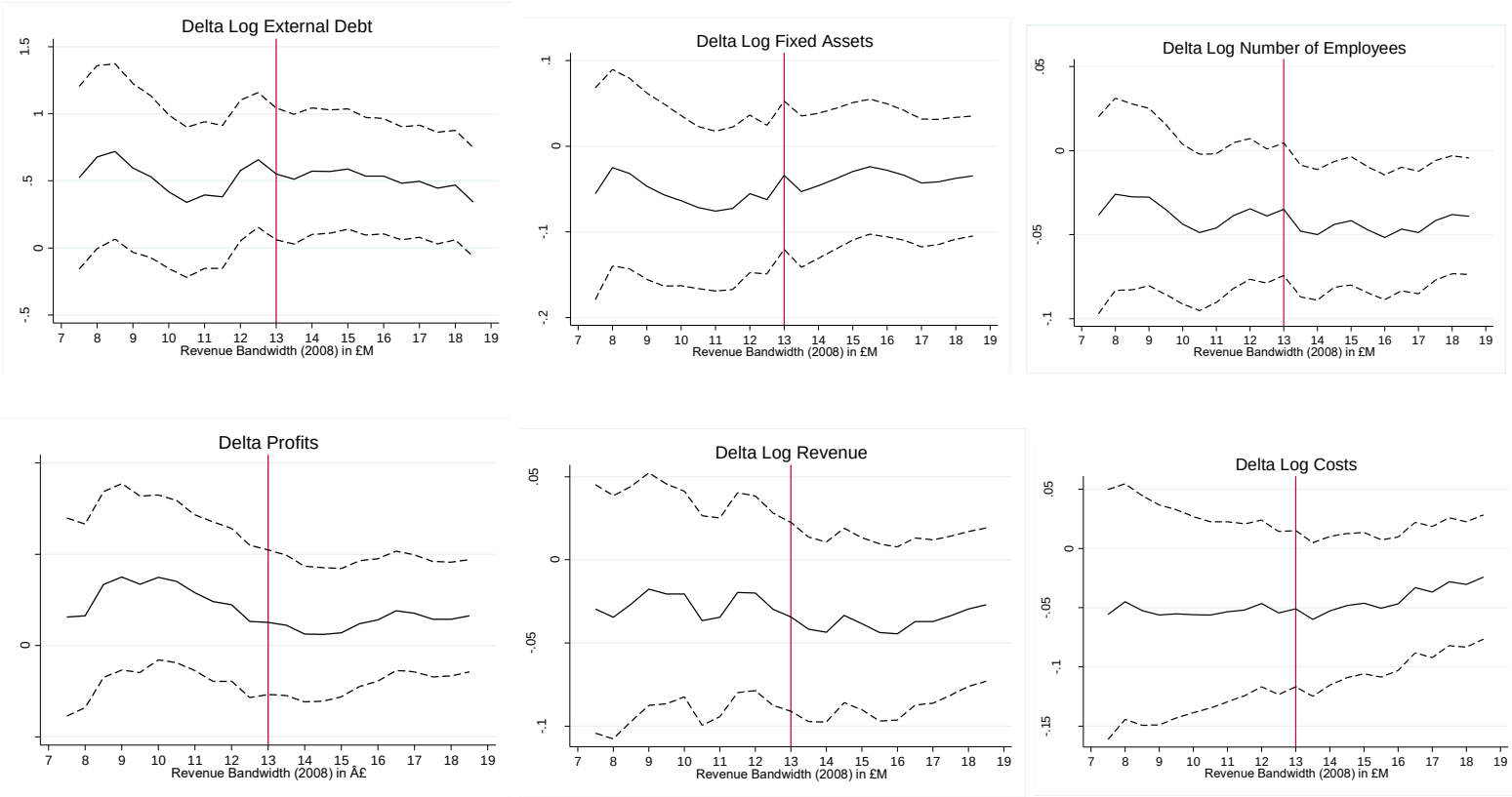
Panel A shows the distribution of firms across industries as determined by their SIC 2007 2-digit code. Panel B shows the distribution of firms across the top 40 industries as determined by their SIC 2007 5-digit code. The Reporting Firms sample includes all firms with more than 50 employees in 2008 and total assets above £3.26 M in 2008 (see Section 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 survived until at least 2009.We exclude firms in sectors that are not eligible for the EFG program.

Figure 2: Distribution of firms by revenue values in 2008



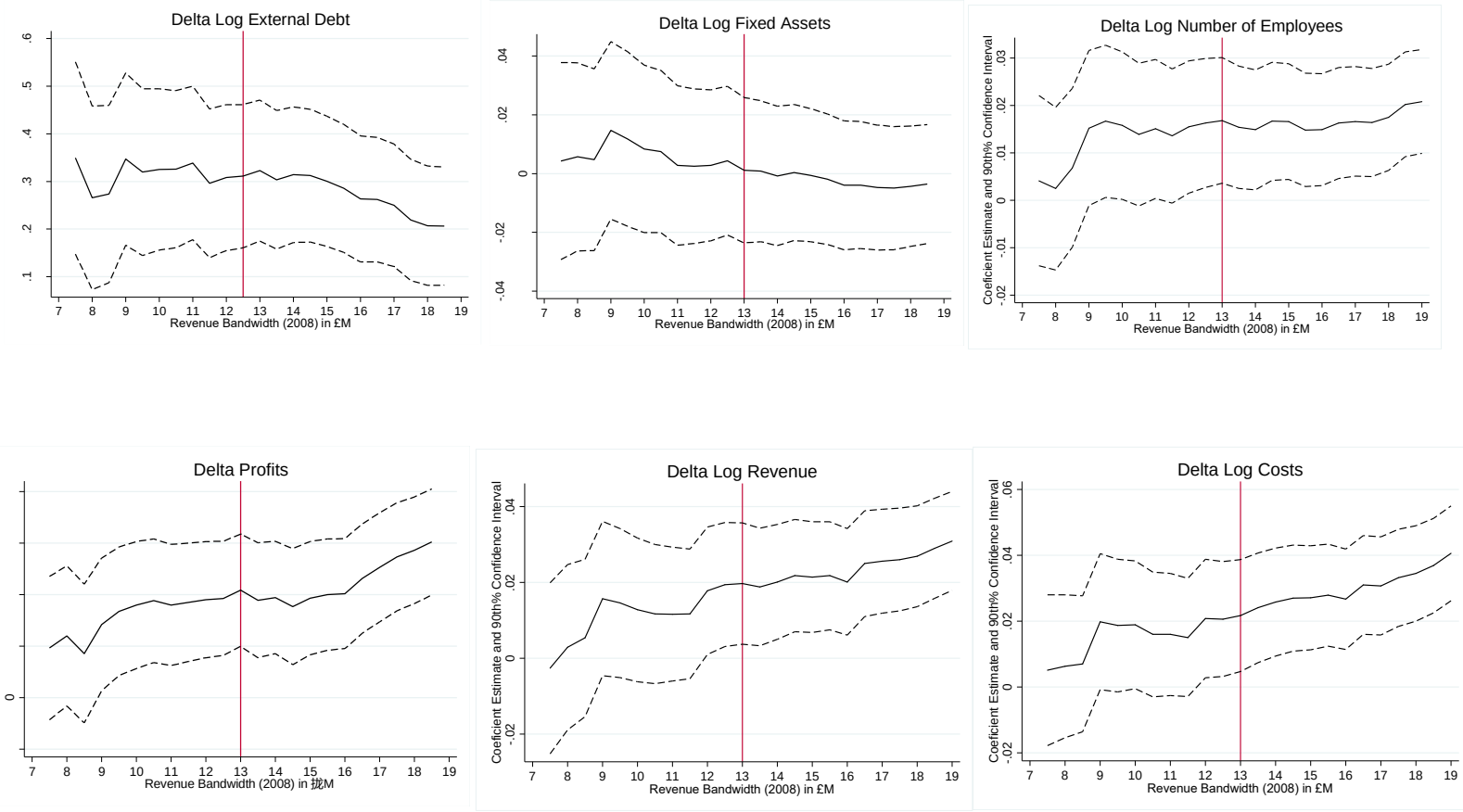
The figure plots the distribution of revenues in 2008 for the firms in the sample. The sample includes UK firms with revenues in 2008 between £12M and £38M (i.e. +/-£13M window around the revenue threshold of £25M) that survived until at least 2009, and with more than 50 employees in 2008 and total assets above £3.26 M in 2008, so as to make sure that firms report detailed financial statements the year pre-launch (see Section 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 a standard error of 0.091.

Figure 3: Placebo test with firms in non-eligible industries



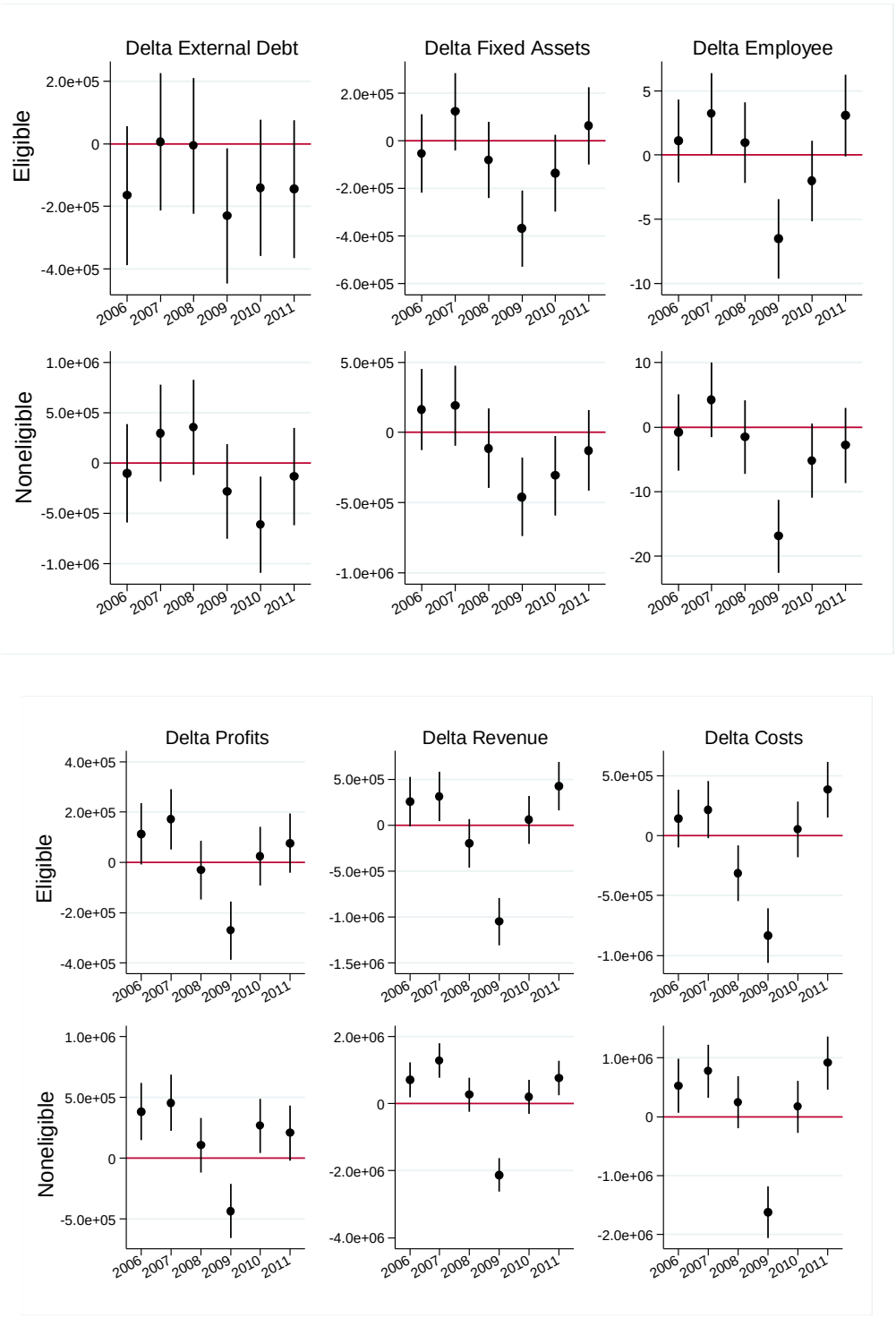
The plot presents results from estimating equation (1) using different subsamples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008, but in non-EFG-eligible industries. The dependent variable is specified in the top of each plot. The solid black line plots the estimated 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 4: Robustness checks with different revenue (2008) bandwidths



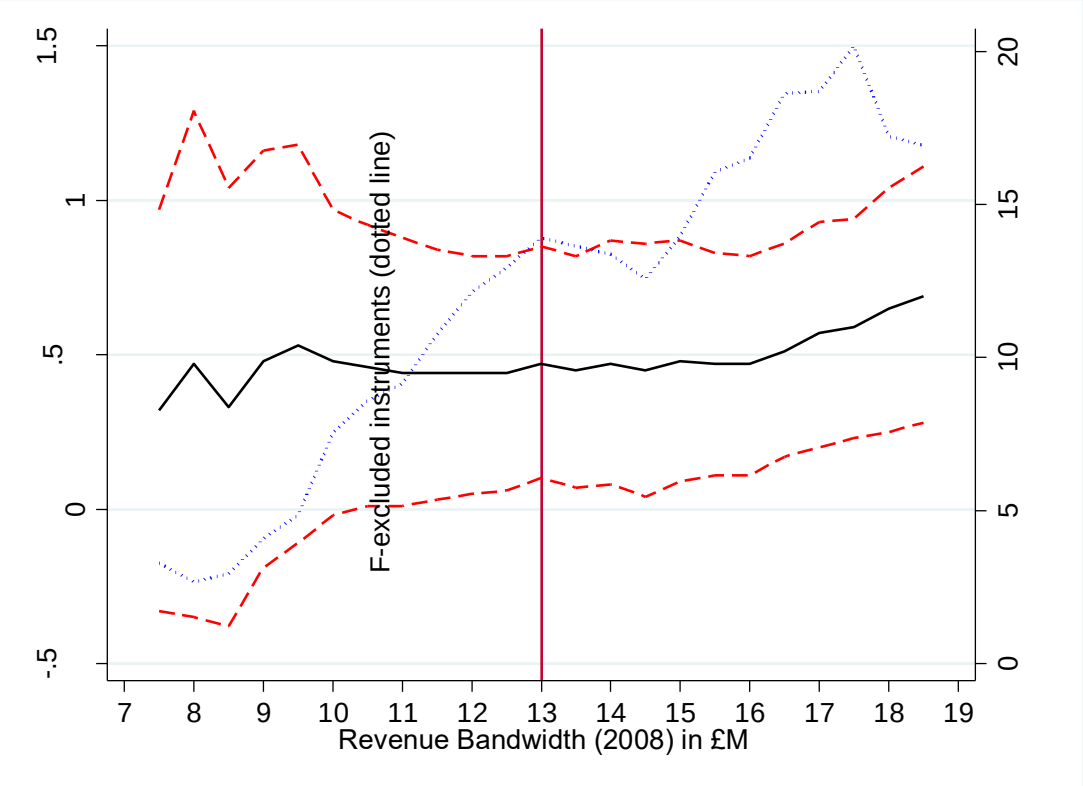
The plot presents results from estimating equation (1) using different subsamples of companies with revenue levels within a bandwidth of £7.5M to £18.5M of £25M in 2008. The dependent variable is specified in the top of each plot. The solid black line plots the estimated coefficients and the red dashed 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 5: Dynamic patterns: Eligible vs non-eligible firms



This figure plots the dynamic change of levels for eligible firms (Treatment) and non-eligible firms (Control). 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 6: Estimation returns to capital for different revenue (2008) bandwidths



The plot presents results from estimating the system of equations (1)-(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 plots 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 1: Summary statistics

Variables	(1) Obs.	(2) Mean	(3) Std. Dev.	(4) 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	147.00
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 Credit	55,057	1,889,222	1,987,877	1,360,000
Profit (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	6,286,735	0.00
ΔInternal Debt	50,560	296,836	4,375,744	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
ΔTrade Credit	46,868	77,716	1,153,591	40,000
ΔIssued Equity	41,396	68,732	751,682	0.00
ΔProfit	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

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 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 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%.

Table 2: EFG and financing

Panel A: Panel data change in levels

Dep. Var.	(1) ΔExternal Debt	(2) ΔExternal Long Debt	(3) ΔExternal Short Debt	(4) ΔInternal Debt	(5) ΔTrade Credit	(6) 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

Dep. Var.	(1) Δln(External Debt)	(2) Δln(External Long Debt)	(3) Δln(External Short Debt)	(4) Δln(Internal Debt)	(5) Δln(Trade Credit)	(6) 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	0.140	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	ΔTrade Credit	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(Trade Credit)	Δln(Issued Equity)
$Eligible_i \times Post_t$	0.292**	0.310**	0.144	0.050	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

The table presents results from estimating equation (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.

Table 3: EFG, employment and investment

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

The table presents results from estimating equation (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.

Table 4: EFG and performance

Panel A: Panel data

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dep. Var.	Survival	ΔProfit	ΔRevenue	ΔCosts	Δln(Profits)	Δln(Revenue)	Δln (Costs)
$Eligible_i$	0.004*	208,711***	997,142***	665,426***	0.016	0.019**	0.022**
$\times Post_t$	(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	0.280	0.283
Mean Dep. Var.	0.86	321,110	910,517	623,737	0.04	0.05	0.05

Panel B: Collapsed data

	(1)	(2)	(3)	(4)	(5)	(6)	
Dep. Var.	Survival	ΔProfit	ΔRevenue	ΔCosts	Δln(Profits)	Δln(Revenue)	Δln (Costs)
$Eligible_i$	0.005	292,409***	1,255,431***	827,299***	0.020	0.021*	0.031**
$\times Post_t$	(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	0.590	0.650	0.557	0.213	0.233	0.212

The table presents results from estimating equation (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 (1) excluding the firm fixed effect. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table 5: EFG and financing, employment and investment dynamics

Panel A: Change in logs

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employee)	(4) Δln(Profits)	(5) Δln(Revenue)	(6) Δln(Costs)
$Eligible_i * 2005$	-0.15	-0.00028	-0.0024	0.030	0.024	0.051***
	(0.20)	(0.036)	(0.012)	(0.019)	(0.015)	(0.016)
$Eligible_i * 2006$	0.18	-0.022	-0.010	0.0077	0.00091	0.022
	(0.20)	(0.033)	(0.012)	(0.021)	(0.015)	(0.016)
$Eligible_i * 2007$	-0.055	-0.055*	-0.014	-0.017	-0.022	0.0079
	(0.22)	(0.032)	(0.013)	(0.021)	(0.015)	(0.017)
$Eligible_i * 2009$	0.46**	0.0039	0.0100	0.033*	0.022	0.038***
	(0.21)	(0.023)	(0.012)	(0.020)	(0.014)	(0.014)
$Eligible_i * 2010$	0.19	0.0016	0.0090	0.018	0.018	0.047***
	(0.18)	(0.024)	(0.014)	(0.020)	(0.015)	(0.016)
$Eligible_i * 2011$	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.	49,853	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

Dep. Var.	(1) ΔExternal Debt	(2) ΔFixed Assets	(3) ΔEmployee	(4) ΔProfits	(5) ΔRevenue	(6) ΔCosts
$Eligible_i * 2005$	335,982	29,274	-2.90	165,329	305,780	456,663*
	(229,421)	(279,946)	(3.48)	(122,825)	(288,385)	(247,666)
$Eligible_i * 2006$	194,156	-298,149	-0.20	-127,431	-104,210	136,193
	(240,768)	(288,270)	(3.61)	(132,326)	(293,393)	(262,730)
$Eligible_i * 2007$	23,101	-447,543	-3.91	-160,565	-530,727*	58,351
	(259,269)	(283,725)	(3.15)	(126,788)	(280,117)	(256,184)
$Eligible_i * 2009$	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)
$Eligible_i * 2010$	915,149***	224,472	0.78	-7,117	547,011**	632,438**
	(285,400)	(281,625)	(3.33)	(135,247)	(269,921)	(246,589)
$Eligible_i * 2011$	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	0.250

The table presents results from estimating a more flexible version of equation (1), where we interact year indicator variables with $Eligible_i$, a dummy indicating whether the firm had revenue below £25M in year 2008. 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.

Table 6: Heterogeneity

Panel A: Above and below median tangibility in 2008 (fixed assets/total assets)

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) Δln (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)	(118,718.613)	(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	0.020
	(0.124)	(0.022)	(0.010)	(0.004)	(81,573.825)	(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	0.0030	183,507	0.034	0.0025
	(0.18)	(0.030)	(0.017)	(0.0055)	(143,639)	(0.021)	(0.022)

Panel B: Above and below median leverage in 2008 (debt/total assets)

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) 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)	(110,537.521)	(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)	(87,456.034)	(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	-0.0080	0.020
	(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)

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) 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	0.230	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

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) 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: Manufacturing vs non-manufacturing firms

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) Δln (Costs)
Manufacturing							
$Eligible_i \times Post_t$	0.259	0.010	0.021*	0.006	45,464	0.003	0.014
	(0.169)	(0.024)	(0.011)	(0.004)	(101,371.173)	(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-manufacturing							
$Eligible_i \times Post_t$	0.349***	-0.003	0.015	0.004	281,789***	0.026**	0.025*
	(0.106)	(0.019)	(0.010)	(0.003)	(84,365.903)	(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	0.280	0.281
Difference	-0.090	0.013	0.0064	0.0020	-236,325*	-0.023	-0.011
	(0.20)	(0.030)	(0.015)	(0.0055)	(131,191)	(0.019)	(0.020)

The table presents results from estimating equation (1) using different subsamples. 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 subsamples above and below the median leverage in 2008 (total debt/total assets). Panel C is based on subsamples above and below the median receivable levels. Panel D splits the sample by median age, and Panel E compares the manufacturing and non-manufacturing 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 2009-2011The dependent variable is specified at 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.

Table 7: Sensitivities of employment, fixed Assets and profit to external debt

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	0.000050	0.048
	(0.00048)	(0.029)	(0.0010)	(0.055)	(0.00068)	(0.040)
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)		(0.10)
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**
	(1.8e-07)	(5.6e-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		15.80		13.91

The table presents results from estimating the system of equations (1)-(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.

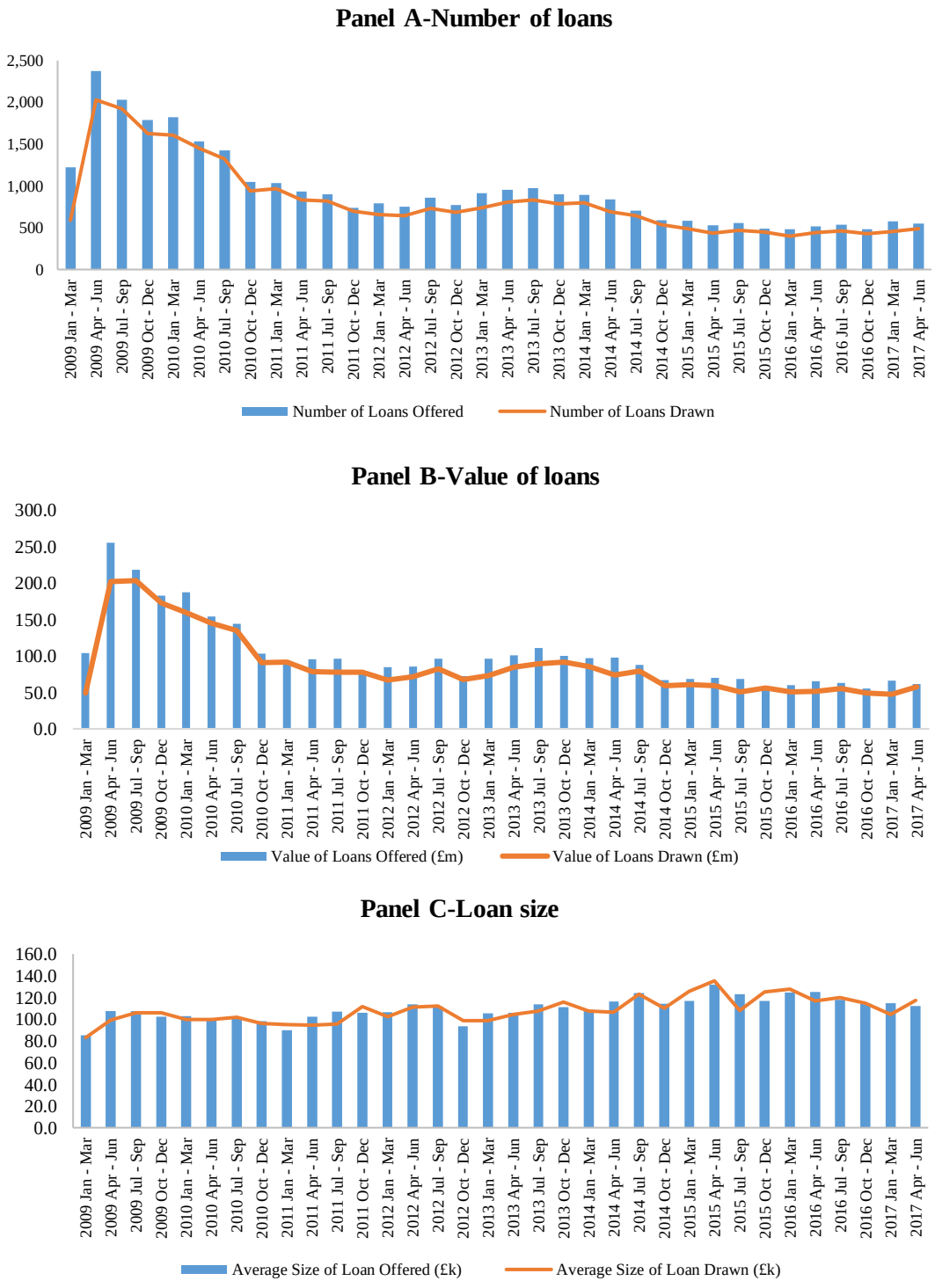
Table 8: Placebo tests-random revenue thresholds

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

This table presents summary results from 200 placebo tests, where 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.

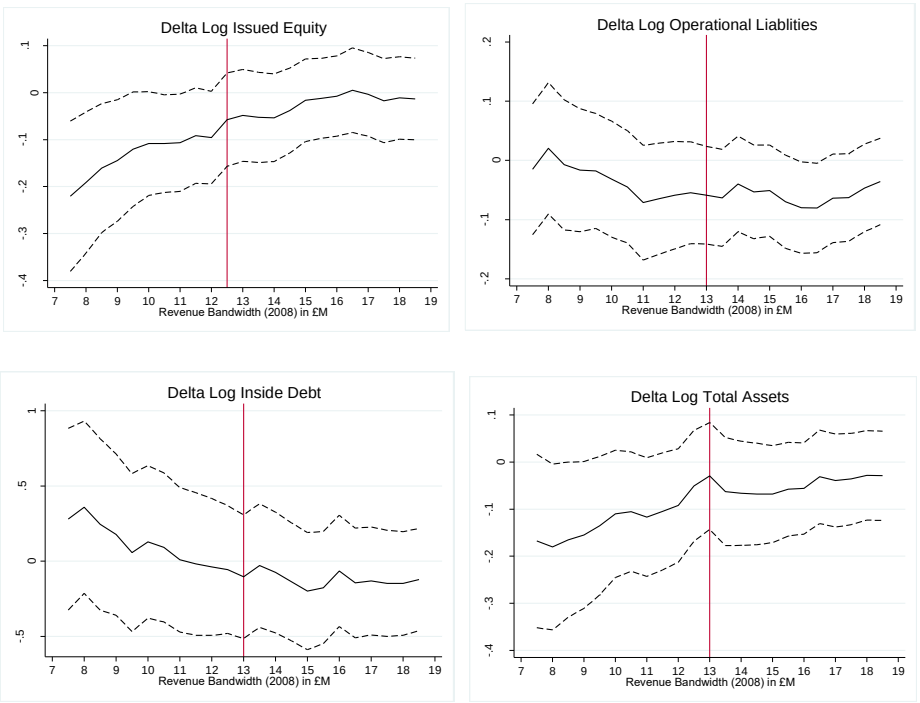
Appendix

Figure A.1: Official EFG Statistics



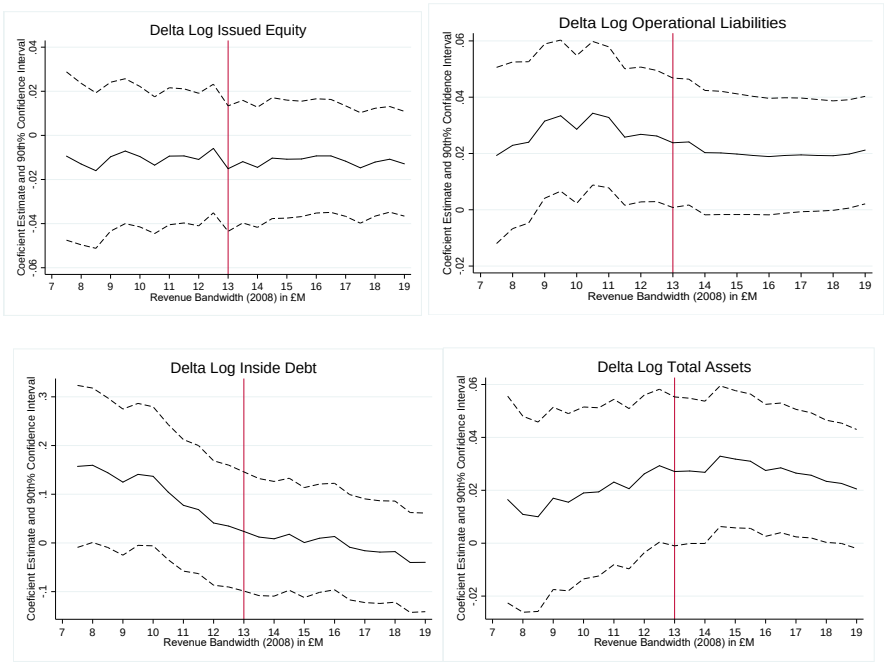
This plot shows quarterly EFG statistics loans from January 2009 until June 2017. Panel A shows the total number of loans offered (blue bar) and drawn (red line). Panel B displays the total value of loans offered (blue bar) and drawn (red line). Panel C shows the average loan size offered (blue bar)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

Figure A.2: Placebo test with firms in non-eligible industries



The plot presents results from estimating equation (1) using different subsamples 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 A.3: Robustness checks with different revenue (2008) bandwidths



The plot presents results from estimating equation (1) 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 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 A.1 Terms of borrowing by amount borrowed for EFG-backed loans in 2009

Amount borrowed	Average Interest Rates	Average Fees	Fees as % of loan value	Average loan terms (months)
£1K-£25K	8.1%	£560	3.3%	65
£25K-£50K	6.2%	£880	2.4%	76
£50K-£100K	5.3%	£1,650	2.3%	83
£100K-£250K	4.7%	£2,770	1.8%	79
>£250K	4.1%	£8,290	1.7%	76
Average	5.8%	£1,980	2.0%	76

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

Table A.2: Extension EFG and assets

Panel A: Panel data

	(1)	(2)	(3)	(4)
Dep. Var.	ΔTotal Assets	ΔCurrent Assets	Δln(Total Assets)	Δln(Current Assets)
$Eligible_i \times Post_t$	777,447***	212,424*	0.031*	0.024
	(168,646)	(109,594)	(0.018)	(0.019)
Obs.	49,957	49,789	49,957	49,789
R-squared	0.231	0.193	0.242	0.233
Mean Dep. Var.	1,177,457	754,343	0.09	0.10

Panel B: Collapsed data

	(1)	(2)	(3)	(4)
Dep. Var.	ΔTotal Assets	ΔCurrent Assets	Δln(Total Assets)	Δln(Current Assets)
$Eligible_i \times Post_t$	812,834***	229,613**	0.032	0.033
	(177,240)	(111,899)	(0.023)	(0.024)
Obs.	14,922	14,886	14,922	14,886
R-squared	0.147	0.135	0.192	0.188

The table presents results from estimating equation (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 (1) excluding the firm fixed effect. *, **, and *** indicate statistical significance at the 10%, 5%, and 1% levels, respectively.

Table A.3: Extension dynamic effects of EFG

Panel A: Change in logs

	(1)	(2)	(3)	(4)
Dep. Var.	Δln(Internal Debt)	Δln(Trade Credit)	Δln(Issued Equity)	Δln(Total Assets)
$Eligible_i * 2005$	-0.22	0.014	-0.072*	-0.00028
	(0.16)	(0.026)	(0.037)	(0.036)
$Eligible_i * 2006$	-0.22	0.013	-0.028	-0.022
	(0.16)	(0.025)	(0.036)	(0.033)
$Eligible_i * 2007$	-0.22	0.0053	-0.061*	-0.055*
	(0.18)	(0.028)	(0.032)	(0.032)
$Eligible_i * 2009$	-0.23	0.018	-0.045*	0.0039
	(0.17)	(0.029)	(0.026)	(0.023)
$Eligible_i * 2010$	-0.054	0.043	-0.036	0.0016
	(0.16)	(0.027)	(0.027)	(0.024)
$Eligible_i * 2011$	-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	0.140	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
$Eligible_i * 2005$	-79,188	-37,351	-38,065	29,274
	(180,259)	(44,741)	(32,837)	(279,946)
$Eligible_i * 2006$	-34,411	-68,079	-59,310**	-298,149
	(159,125)	(47,773)	(29,526)	(288,270)
$Eligible_i * 2007$	45,997	-88,871*	-27,572	-447,543
	(174,698)	(52,989)	(26,236)	(283,725)
$Eligible_i * 2009$	-202,609	102,864*	-105,361***	1.07e+06***
	(193,478)	(53,439)	(30,308)	(289,661)
$Eligible_i * 2010$	-120,873	-103,699**	-46,972	224,472
	(188,175)	(48,527)	(30,065)	(281,625)
$Eligible_i * 2011$	-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

The table presents results from estimating equation (1). $Eligible_i$ is a dummy indicating whether the firm had revenue below £25M in year 2008. 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.

Table A.4 : Robustness checks

Panel A: Firms with negative external debt

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) 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	0.480	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

Dep. Var.	(1) Δln(External Debt)	(2) Δln(Fixed Assets)	(3) Δln(Employees)	(4) Survival	(5) ΔProfit	(6) Δln(Revenue)	(7) Δln (Costs)
$Eligible_i \times Post_t$	0.406***	0.007	0.025***	0.002	254,559.035***	0.023**	0.027**
	(0.108)	(0.017)	(0.009)	(0.003)	(78,376.388)	(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

This table presents the robustness checks using alternative samples. In panel A, we focus on a subsample 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.