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The Effects of Small-Firm Loan Guarantees in the UK:

Insights for the COVID-19 Pandemic Crisis

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The Effects of Small-Firm Loan Guarantees in the UK:

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Loan guarantees are popular policy responses during the COVID-19 crisis. Despite their prevalence, evidence of their effectiveness is sparse. We estimate the impacts of UK guarantees during the Great Recession, by exploiting firm-size eligibility restrictions. Guarantees increased four-year performance, labour-productivity, and employment growth, but not investment. Results are driven by firms with high-training-costs employees. They are consistent with the guarantees enabling a small number of financially constrained firms to retain workers that helped rebuild the businesses post-crisis. The results suggest that COVID-19 responses based on guarantees alone can be regressive, because poorer workers are more likely to have low-training-costs jobs.

JEL codes: D04, D22, G21, G28, G38, H32, H81

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Governments around the world have enacted stimulus measures to minimize the economic impact of the COVID-19 crisis. One popular policy is loan guarantees that increase firms' access to credit. For example, the UK government unveiled in March 2020 a scheme for £330 billion of loan guarantees—equivalent to 15% of the country's GDP—to provide businesses with cash to pay wages and other expenses.[2]

Loan guarantees target businesses that otherwise could not access market loans because they cannot cover banks' collateral requirements.[3] These requirements are especially onerous when firms borrow to retain staff for the simple reason that employees cannot be pledged as collateral for loans.[4] Absent the guarantees, financially constrained firms will be forced to lay-off workers.

Despite the increasing prevalence of loan guarantees, evidence for the success of such schemes is still sparse. This is due, in large part, to difficulties in accessing detailed data for small firms. But it is also because constructing meaningful counterfactual scenarios is challenging: What would have been performance of firms absent the guarantees?

In this paper, we estimate the impact of the Enterprise Finance Guarantee (EFG), the small-firm loan guarantees implemented starting in 2009 as part of the UK's business policy response to the Great Recession (GR). We use micro data to estimate effects on several firm outcomes, and exploit firm-size eligibility restrictions for identification. Our results are consistent with the guarantees enabling a small number of financially constrained firms to retain workers that helped rebuild the businesses post-crisis. We conclude with a discussion of the relevant policy insights from the results for the COVID-19 crisis.

Why study the EFG? While loan guarantees were the main small business policy response to the GR (OECD, 2018; World Bank, 2015), their effect is contentious, making it an important empirical question. Critics argue that by allowing firms to borrow without pledging collateral, and by providing a guarantee, these programs lower lenders' incentives to screen and discipline borrowers.[5]

In responding to critics, the UK loan guarantees program has several design features to curtail lenders' and borrowers' risk-taking incentives. Lenders are incentivized by the partial guarantees on

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[2] Loan guarantees are also a significant part of the stimulus package in the US and other countries. See: https://www.imf.org/en/Topics/imf-and-covid19/Policy-Responses-to-COVID-19

[3] On average, 80% of commercial loans require collateral, the value of collateral needed is 200% of the loan amount. See: Enterprise Surveys (http://www.enterprisesurveys.org), The World Bank.

[4] Moreover, these requirements tend to increase using recessions. For example, the collateral posted on loans by low risk firms increased by 78% for UK small firms during the Great Recession (ESRC, 2011).

[5] A non-exhaustive of relevant literature includes: Smith and Warner, 1979; Stulz and Johnson, 1985; Boot, Thakor, Udell, 1991; Rajan and Winton, 1995; Park, 2000; Liberti and Sturgess, 2014; Lelarge, Sraer, and Thesmar, 2010; Acs et al., 2016; D'Acunto, Tate, and Yang, 2017.

individual loans (75% of outstanding balance), and by the caps on the overall amount of guarantees sought by each bank (9.75% of the scheme's size). Borrowers remain fully liable, and banks can request additional personal guarantees, thus incentivising banks to monitor.

Other features of the scheme also impact incentives. For example, the scheme is funded by charging a premium to the borrowers of 2% in addition to the chargers by lenders (on average, 5.8%).[6] The aim of the premium is to share the costs of guarantees between beneficiaries and tax payers, but an unintended consequence may be adverse selection. Perhaps partly as a result of this rationing, take-up was low relative to the target population. Less than 7,000 UK companies issued loans through the scheme in 2009, which correspond to fewer than 5% of eligible firms.

Our empirical strategy exploits variation in participation from the unexpected firm-revenue-size eligibility threshold. We measure causal effects of the program using a difference-in-difference approach that compares treatment and control firms that in 2008 had, respectively, revenues below and above the £25M threshold.[7] Our approach constructs meaningful counterfactuals because eligibility is "as good as random" for firms near the threshold, under the assumption that treatment and control groups would have evolved in parallel absent the guarantees. Under the additional assumption that the EFG had no effects on non-participants, we measure casual effects on EFG-borrowers, by instrumenting debt with eligibility and the program's launch. In support of the identification assumptions, we present suggestive evidence from several tests.

Our evidence shows that the guarantees increased average four-year profits, labour-productivity, survival, and employment growth, but not investment, for eligible firms near the threshold and relative to non-eligible firms. The relative increases occurred in lockstep with debt issuances, did not revert during 2010-2013, were absent prior to 2009, and mask large heterogeneity: The results are entirely driven by firms in industries that are in the top quartile of costs to train employees.[8]

Additional results suggest that the findings are mainly driven by effects on the minority of eligible firms that take-up the scheme, rather than by potential effects that the EFG can have on non-participants.[9] Under the assumption that no such non-participants' effects exist, we estimate annual

[6] Premiums are common in loan guarantee programs. In Europe, as well as in developing countries, the fee is typically about 1 percent of the loan amount.

[7] The alternative empirical approach of regression discontinuity design is not feasible in this setting because the low take-up implies that statistical power is enough to meet the high power demands of that method; see Schochet (2008) and Deke and Dragoset (2012).

[8] We note that even amongst these firms the take-up was lower than 10%.

[9] Potential effects on non-participants include: "anticipation effects" (the mere possibility of having the option to access to scheme in the future changes behaviour of eligible firms but not their take-up), "externality effects" (the scheme displaces lending by banks to non-eligible firms) or "general equilibrium" effects (the scheme alters the cost of capital).

returns to guaranteed debt that range between 16% and 20%. These returns exceed the above-market scheme rates (average of 7.8%), and are below the cost of outside-funding options (which can be as high as 50% for unsecured loans).

The results are consistent with the guarantees enabling some financially constrained firms to retain workers during the crisis, which were fundamental to rebuild the businesses in the recovery. Absent the guarantees, these businesses would have had to lay-off workers. To finance any investment during the crisis, these firms could borrow by pledging the fixed assets as security. Instead, securing loans for employee retention is harder, because employees cannot be pledged as collateral for loans. As a result, absent the guarantees, these firms will incur costs of rehiring and training workers post-crisis, making the recovery less profitable and less likely (cf., Oi; 1962; Rota, 1998). In addition, labour-productivity will decrease in these firms if labour and capital are complementary, and if the capital stock is irreversible (Caggese, 2007).

Alternative interpretations are unlikely to explain our findings. The macroeconomic environment makes labour-supply explanations unlikely, because workers in financially distressed firms had few options of outside jobs (cf., Baghai, Silva, Thell and Vig, 2020). Additionally, concerns of risk-shifting by banks and borrowers are mitigated by the profitability and survival results. Finally, the labour-productivity effects are inconsistent with guarantees preventing efficient labour reallocation, and instead demonstrate that not all employee retention occurring during the GR was necessarily unproductive (cf., Coulter, 2013).

Given the appealing returns, we ask why more eligible firms did not take-out EFG loans? Several issues may hinder the take-up of loan guarantee programs, and the available data does not allow us to rigorously distinguish between them.

First, there may be no failure in take-up at all: the businesses who opted in are the ones who could benefit, and those who did not opt in could not benefit. This could happen if for most firms, for example, expected benefits from retention were not enough to compensate for the 2% scheme premium. Whether the premium rationed-out potential beneficiaries is an important concern that must be weighed against the benefits of making participants chip in the scheme's costs. Note however that no failure in take-up does not imply no market failure: our return estimates suggest that a failure exists for those who did opt-in, in that they did not take out a loan at the market rate.

Alternatively, failure in take-up occurred, perhaps because features of the scheme deterred potential beneficiaries. For example, allowing banks to request personal guarantees pushed back demand of firms that had already exhausted all personal guarantees in securing other loans. However, this take-up failure may not necessarily be inefficient: restricting banks from requesting personal guarantees can decrease their incentives to monitor. Another possibility is that considerations other than economic benefit may keep firms at bay. For example, firms may be averse to government scrutiny, or may fear the stigma effect of partaking in the stimulus.

The question remains: was the EFG good value for money? To answer this question, we do a back-of-the-envelope calculation based on our estimates and publicly available data on the program's cost to perform a cost-benefit analysis.[10] Under plausible assumptions, results show that for the sub-sample of eligible firms close to the eligibility threshold, the economic benefits were 1.5 times the costs.

What do we learn from this evidence that is relevant for the COVID-19 crisis? The main objective in this crisis is to have people stay at home, and thus governments' main short-term aim is to insure workers. An important question is whether this aim is best achieved by policies that target firms or individuals. Our results show that policies that target firms, like loan guarantees, can achieve the insurance objective only partially. Eligible firms will have incentives to retain workers, but only for those workers who are costly to train and hire. These findings imply that COVID-19 stimulus packages based on firm guarantees alone can be regressive, because poorer workers are the more likely to have low-training-costs jobs. Other stimulus programs that either target individuals, or are aimed at firms' low-skill workers, are therefore warranted, such as the Job Retention Scheme program also sponsored by the UK government in this crisis.

Alternatively, governments can also consider major overhauls of loan guarantee schemes to increase take-up. Early informal evidence on the Coronavirus Business Interruption Loan Scheme (CIBLS) in the UK—the equivalent to the EFG for the COVID-19 Pandemic crisis—suggests that companies are struggling to get the funds. Prior research warns against widespread relaxation in scheme design (e.g., Lelarge, Sraer, and Thesmar, 2010; D'Acunto, Tate, and Yang, 2017). But for some, the unprecedented urgency of this crisis has made fast access to the funds a priority, and has pushed to a far second-level considerations of moral hazard. Only time will tell whether the potentially large benefits from streamlined guarantee programs favoured by countries like Switzerland, will compensate for the potential long-term difficulties when the loans come due for repayment.

We are not the first to estimate the effects of government stimulus programs. In terms of the GR, several studies have found positive employment effects of the "American Recovery and Reinvestment Act" (Wilson, 2009; Feyrer and Sacerdote, 2011; Chodorow-Reich, Feiveson, Liscow, 2012), although some of this evidence has been contested (see Conley and Dupor, 2012). These studies exploit cross-regional variation in exposure to the programs and cannot account for potential externalities between

[10] See: Allinson, Robson, and Stone (2013).

regions. By contrast, our identifying variation is at the firm level, and we find no evidence of sizable cross-firm externality effects.

Our approach is potentially applicable to many efforts to evaluate fiscal stimulus programs worldwide, because it harnesses the power of firm-level data without relying on private information about participants; instead, the method relies on exogenous restrictions in program access. Three important caveats require mention. First, eligible firms may differ from non-eligible firms on characteristics that could be responsible for the results we find. However, the precise patterns we witness—a sharp rise in debt financing, performance and employment coinciding with the EFG launch, are hard to reconcile with a story that is unrelated to the EFG. In addition, we cannot reject null-effects in several placebo tests using: (i) firms in non-eligible industries, (ii) fake launch years, and (iii) random thresholds.

Second, it is difficult to measure the effect of the program on eligible firms that did not borrow through the EFG program. Given our reliance on eligible firms as the treatment group, if the EFG program had an effect on eligible, yet non-EFG-borrowers (for example, anticipation effects that arise from the mere possibility to borrow in the future), our empirical strategy would be unable to detect it. However, any argument that the EFG had such an effect must explain how non-borrowers had a sharp relative increase in performance and employment immediately after the policy launch, during a macroeconomic environment with negative demand shocks and where securing finance was not trivial. Against their empirical relevance, we find no evidence of such effects among non-borrowers.

Third, it is difficult to measure the equilibrium effects of the program on the entire economy. However, such effects are unlikely given the program's small size. The small size also suggests that externality effects that can dampen or exacerbate the impact estimates are likewise improbable. In support of the exclusion restriction that no such effects exist, we show evidence of no differences in effects between areas with ex-ante high and low externality potential as measured by the geographical concentration of non-eligible firms.

We also contribute to the growing literature on the impacts of loan guarantees showing the asymmetric effects on employment and investment, and their role in enabling firms to retain productive workers during the GR.[11] Recent work on guarantees has focused on bank's and borrowers' moral hazard (Lelarge, Sraer, and Thesmar, 2010; D'Acunto, Tate, and Yang, 2017), bank relationship effects (Mullins and Toro, 2017), certification effects on borrowers (Bonfim, Custodio and Raposo, 2019), credit supply effects (Bachas, Kim and Yannelis, 2019), workers' earning trajectories (Barrot, Martin,

[11] Several recent studies explore the effects of loan guarantee programs. A non-exhaustive list includes: Lelarge, Sraer, and Thesmar, 2010; D'Acunto, Tate, and Yang, 2017;

Sauvagna and Vallee, 2019), job creation (Brown and Earle 2017) and lending terms (de Blasio et al., 2017). Relative to existing studies on EFG, we provide evidence of causal impacts. Prior work provides little guidance of causal effects as estimates are based on matching-on-observables. We show the inaccuracy of EFG take-up predictions based on observables, which is consistent with other work on credit access interventions (Karlan, Morduch and Mullanathain, 2010; Crepon, Devoto, Duflo and Pariente, 2015).

Finally, we contribute to the labour and finance literature by showing that financial constraints were particularly binding for employment in small firms during the crisis (Giroud and Mueller, 2017; for a review see Matsa, 2018). By contrast, most prior estimates on the returns to capital in small firms trace the effects of financial constraints to investment (de Mel, McKenzie, and Woodruff, 2008; McKenzie and Woodruff, 2008; Banerjee and Duflo, 2014). The implication is that the main margin of adjustment for constrained firms during recessions may be employment rather than the capital stock. As such, a corollary for empirical work is that the focus of the cash-flow sensitivity literature on fixed asset investment can lead to erroneous perceptions regarding the degree to which firms are financially constrained (cf., Almeida, Campello and Weisbach, 2004).

The rest of this paper proceeds as follows. In Section 1, we describe the institutional context and the extant research on EFG impacts. In Section 2, we describe the data. We explain the empirical strategy in Section 3. Section 4 presents the results. In Section 5, we interpret the findings, perform a cost-benefit analysis, and discuss the policy implications. In Section 6 we summarize a battery of robustness checks. Section 7 concludes the paper.

1 The Enterprise Finance Guarantee: Description and Extant Research

In this section we first provide the institutional background, and then we describe the extant research on the EFG impacts.

1.1 Description of the EFG

The Enterprise Finance Guarantee (EFG) is the largest UK government program targeted at small firms.[12] The EFG provides lenders with a government-backed guarantee for 75% of the value of each individual loan given out through the scheme.[13] Borrowers can access a maximum of £1.2M in loans

[12] 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, Energy and Industrial Strategy (former: Department for Business, Innovation, and Skills).

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

through the scheme, and remain liable for 100% of loan balances. No restrictions are imposed in the usage of funds, except the financing of specific export orders.

EFG lenders have full decision-making control.[14] They perform all the credit screening and monitoring functions and decide upon all commercial matters, including the type of facility (e.g., new loans, conversion of overdrafts into loans), interest rates, and other fees.[15] 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.[16] Finance terms are from three months up to ten years for term loans and asset finance, and up to three years for revolving facilities and invoice finance.

The EFG was launched in January 2009. The launch was part of a worldwide trend of guarantee programs' expansions as countercyclical policy tools in the aftermath of the financial crisis (see Gozzi and Schmukler, 2016). In the UK, the program's launch was also motivated by the below-par performance of the EFG's predecessor: The Small Firms Loan Guarantee (SFLG).

The effect of loan guarantees is contentious making it an important empirical question. Critics argue that by allowing firms to borrow without pledging collateral, and by providing a guarantee, these programs lower lenders' incentives to screen and discipline borrowers (cf., Lelarge, Sraer, and Thesmar, 2010; Kerr, Kerr, and Nanda, 2015; Acs et al., 2016; D'Acunto, Tate, and Yang, 2017).

There are two main levers introduced by the EFG to curtail lenders' risk-taking incentives. First, individual loan guarantees are subject to a cap of 9.75% on the total exposure across a lender's annual portfolio of EFG-backed lending. This cap means that banks are exposed to all of the remaining bad debts after this limit.[17] Second, the EFG explicitly allows lenders to insist on additional private guarantees (except the borrower's main residences). By contrast, neither of these levers existed under the SFLG. The EFG also curtails borrowers' risk-taking by keeping them liable for 100% of the loan.

Other features of the scheme also impact incentives. For example, the scheme is funded by charging borrowers the 2% premium in addition to the chargers by lenders (on average, 5.8%). The aim of the

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[14] This characteristic of the EFG contrasts the design of the US guarantee program ran by the Small Business Administration and discussed at length by Brown and Earle (2017) and D'Acunto, Tate, and Yang (2017).
[15] Term limits are also imposed: between 3 months and 10 years for term lending and between 3 months and 3 years for overdrafts.
[16] 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.
[17] The cap was originally set at 9.75% but was revised in 2012 to 15% per lender.

premium is to share the costs of the scheme between beneficiaries and tax payers, but an unintended consequence may be adverse selection (cf., Stiglitz and Weiss, 1981).[18]

Fees are common in loan guarantee programs, and range between 1 to 2 percent of the loan amount.[19] The premium is collected quarterly in advance throughout the life of the loan, and is assessed based on the loan's outstanding capital balance. Relative to the costs of unsecured loans—a natural outside option for small firms with no collateral—the fee is low. 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), which is one order of magnitude smaller than that of an unsecured loan for the same amount that fluctuates between 22.8% (subject to revenue conditions) and 49% (subject to no restrictions) outside of the scheme.[20] However, relative to the average scheme rate of 7.8%, the fee is sizable.

Perhaps partly as a result of the premium, take-up was low relative to the target population. Less than 7,000 UK companies issued loans through the scheme in 2009, which correspond to fewer than 5% of eligible firms. Eligible firms in 2009 consisted of small firms operating in the UK that had revenues of no more than £25M, and that operated in a targeted industry, which corresponded to roughly 60% of UK firms that year. Almost all industries were targeted by the EFG, and where exclusions applied they arose from EU State Aid rules. Sectors with partial or full restrictions include agriculture (including horticulture); banking, finance, and associated services; membership organizations (including professional, religious, and political) and trade unions; coal; education; fisheries, and aquaculture; insurance and associated services; public administration; national defence and compulsory social security; and transport.

The revenue-based eligibility threshold was not disclosed pre-launch, and the exact value was unexpected as it did not coincide with any of the small-firm definitions across government programs. 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

[18] The premium was agreed after consultation with the banks, and a 25% discount was implemented for all premiums due and successfully collected during 2009.
[19] 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.
[20] See, for example, https://www.money.co.uk/business-loans.htm

(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.

In 2009, EFG participants borrowed roughly £600M worth of loans through the scheme (see Figure A.1 in the Appendix). The average loan was £100K, the pre-fee interest rate was 5.8%, and the loan term was 76 months. After three years, 17% per cent of the value of 2009 EFG loans had defaulted, with most defaults occurring for the lower-value loans (Allinson, Robson and Stone, 2013). This default rate is likely higher than in SME secured loans, but, perhaps because of the design innovations, it is smaller than the rate of defaults of SFLG loans.[21]

1.2 Extant Research on EFG Impacts

What was the effect of EFG on small firm performance? The fundamental empirical challenge to answering this question is that counterfactual outcomes in the absence of the program are unobserved. To explore the effects of the program, a research design must form a reasonable estimate of the pattern of small firm performance if the program had not been implemented.

Previous research on this question estimates counterfactual outcomes using samples of non-borrowers that are matched to scheme borrowers based on observable characteristics such as firm size (Allinson, Robson, and Stone, 2013; Muller, Devnani, and Julius, 2017). The validity of this approach relies on the identification assumption that borrowers and matched non-borrowers do not differ in unobservable dimensions that are correlated with their decision to borrow. The main concern is the high likelihood that this assumption does not hold. A classic example concerns firms with relatively better investment opportunities selecting or being selected into loan contracts and thereby confounding any causal effect of access to credit with the casual effects of growth opportunities (that may change unobservable over time). Selection can work in the opposite direction as well; e.g., if firms borrow in anticipation of needing to smooth upcoming negative shocks that are unobservable to the econometrician.

Selection on unobservables is a common concern when constructing counterfactual outcomes to assess the impact of credit access interventions (cf., Karlan, Morduch and Mullanathain, 2010). For the evaluation of guarantee programs, this concern looms large given the low take-up rates of these programs and the difficulty of predicting who will take up guaranteed loans (OECD, 2018). In a survey of 76 guarantee programs across 46 countries, Beck, Klapper, and Mendoza (2010) find that most

[21]SFLG default rates averaged 35%. See: Graham Review of the Small Firms Loan Guarantee Scheme Interim Report 2004, available at: http://researchbriefings.files.parliament.uk/documents/SN00827/SN00827.pdf

guarantee programs in their survey have a stock of less than 1,000 new loan guarantees per year, even though the programs are typically designed to cover large sets of businesses.

In auxiliary analysis we show that selection on unobservables plays an important role in the EFG setting, which raises concerns that approaches that use matching/selection-on-observables will be severely biased. We estimate a model of credit demand based on past borrowing behaviour and several firm characteristics. We then use firms' predicted borrowing propensity scores as predictors of EFG take-up. Borrowing propensities have very little explanatory power, and what is more, exhibit a non-linear relation: firms in the top quartiles of the score have lower estimated borrowing than firms with median borrowing propensity scores. For the model, we use firm data prior to the EFG launch (that we explain more detail in Section 1).[22] Results are summarized in Appendix 2.

Because of selection-bias concerns, our approach to form counterfactual outcomes does not rely on matching by observables the EFG borrowers with other firms. Instead, we use the cross-firm variation in access to the scheme as induced by the revenue-based eligibility threshold of the EFG at its launch, as we explain in more detail in Section 3.

The main advantage of this approach is that under plausible assumptions, it constructs meaningful counterfactuals as we explain in more detail below. Another advantage is that we can estimate the scheme's impact by harnessing the power of firm-level data without needing to access data on EFG borrowers from government records. This is an important consideration, as the EU General Data Protection Regulation (GDPR) limits the retroactive access to programs' users that did not explicitly provide consent to use their data in evaluations. Our approach is thus potentially applicable to evaluate many other small firm fiscal stimulus programs that limit participation based on size-based eligibility thresholds, even if the data on users is not readily available to researchers.

2 Data

In this section, we describe our data sources, outcome measures, and sample.

2.1 Main Data Source

The main 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. This information was originally extracted by BVD from Companies House (CH), the business register in the UK.

[22] In unreported exercises, we show results are similar if we also use data post the EFG launch for the prediction model.

This data source has been validated by prior research, including the early work by Brav (2009) and Michaely and Roberts (2011). The main novelty in our setting is that we use the data to measure changes in a variety of outcomes for small firms in the UK, which constitute a subgroup of businesses that we seldom observe in academic research given the limitations in accessing data for private firms with traditionally no publicly available financial statements. Our original extract from FAME encompasses a 9-year period from 2004 to 2013. We exclude firm-year observations with missing or negative values of total assets, and we winsorize variables at the most extreme 2% in both tails of the distribution.

2.2 Data on Employee Training Costs

We use information from the Occupational Information Network (O*NET), the successor of the US Department of Labor's *Dictionary of Occupation Titles* (DOT), to classify firms into those facing relatively high or relatively low costs to train their employees (cf., Autor, Levy and Murname, 2003).

The O*NET provides information on job characteristics at the occupational level, and information on costs to train employees, and on the skills of workers is reported in the "Education, Training, and Experience" file. This file provides a mapping of occupations classified using the O*NET-SOC codes to ratings of required levels of Education, Training, and Experience.

Our focus is on the "On-the-Job Training" ratings that rate each occupation depending on the amount of on-the-job-training required.[23] Ratings go from 1 "none or short demonstration" to 9 "Over 10 years".

We aggregate these ratings at the industry level by weighting the frequency of each occupation within each industry. We then match these data to the firm-level information by matching the O*Net industry classification to the UK SIC-2007 industry classification, effectively assigning each firm a training cost index.

2.3 Outcome Measures

We measure five types of firm outcomes using the FAME data. First, we construct four broad types of capital sources: *external debt, inside debt, trade credit*, *lease*, and *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.

[23] In unreported exercises we also focus on the "Related Work Experience" ratings that rate each occupation depending on the related work experience required. Ratings go from 1 "none" to 11 "over 10 years". The related work experience is defined as the skills and know-how that a worker receives in another occupation which is usually considered necessary by employers or is a commonly accepted substitute for more formal types of training or education. See: https://www.bls.gov/ooh/about/occupational-information-included-in-the-ooh.htm.

Trade credit corresponds to loans from suppliers. Lease corresponds to the type of asset finance known as “hire, purchase and leasing”, which allows firms to possess and control an asset during an agreed term, while paying instalments covering depreciation of the asset, and interest to cover the capital cost.

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.

We note that the filings do not distinguish between government guaranteed loans and other sources of external loans, so we use changes in external debt to proxy for issuances of guaranteed loans. We detail the advantages and issues of this proxy in Section 4.1, where we discuss results. Firms’ filings also include no information on loans’ interest or default rates either. We proxy default by tracking *survival* similar to other papers in the literature (e.g., Lelarge, Sraer, and Thesmar, 2010).

The second type of variable we construct are measures of firm performance including *revenue*, *cost of sales,* and *profits*. 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.

The third type of variable we construct is employment at the firm as measured by the *number of employees.* Data on employee wages is not available in FAME, and data on managerial compensation is not well populated for small firms.

The fourth type of variable we measure is capital investment, namely changes in *fixed assets*. In unreported results that we discuss in Section 4, we also keep track of changes in total assets, and current assets that include cash and accounts receivables. We note that FAME does not have information on research and development expenses.

The final type of variable we measure is labour *productivity*. We present our main results using as proxy the value of revenues per employee. In unreported regressions, we show that results are qualitatively similar if we use profits per employee.

We note two properties of our variables and sample that require special statistical treatment. First, most of the outcome variables we consider are highly persistent, such as fixed assets. Second, some of our main variables of interest are highly skewed, such as external debt. To address these two issues, all our models are based on changes in the logarithmic transformations of our outcome variables (e.g., Δ *ln(external debt + 1))*, and we back out the implied effects on the untransformed variables by using medians values rather than means, as we explain in more detail in Section 3.

One concern with this approach is that a logarithmic transformation of profits would introduce selection bias because of the negative values this variable can take. This concern is in practice mitigated by the low occurrence of negative profits in the data (7% of the observations). Following Banerjee and Duflo (2012), and as we explain detail in Section 4, we nevertheless address this issue by backing out the effects on profits based on the estimates on revenue and costs, which always have positive values.

2.4 Sample

We restrict the analysis sample to firms that report revenues in 2008 (one year prior to the EFG launch) ranging between £12M and £38M—a £13M bandwidth around the EFG revenue-based eligibility threshold. This restriction ensures that we are comparing eligible and non-eligible firms of similar revenue size. In Section 5.2, nevertheless, we show that our results are robust to alternative bandwidth selection.

Additionally, we only consider firms that in 2008 had more than 50 employees, and more than £3.26M in total assets above in 2008. Filing abbreviated financial accounts with no information on revenues is the prerogative of smaller firms in the UK. By excluding firms with smaller employee and assets sizes, we mitigate potential biases from self-selection of firms into reporting revenue information.

Finally, we also exclude firms in sectors that are not eligible for the EFG (but we use them in Section 5 as a placebo test). We note that the quality of the data is high as the firms' financial statements are audited (given their revenue size).[24] Moreover, 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.

There are 5,044 eligible firms (47,558 firm-year observations) and 2,679 non-eligible firms (25,044 firm-year observations) in our final sample (7,723 firms in total and 72,602 firm-year observations).

The sample firms are representative of the universe of UK firms in 2008 in the targeted industries. Appendix 3 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). Relative to the universe of 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%).

[24] 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).

Relative to prior work studying guarantee programs, 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 guarantee programs in Chile they study, our empirical strategy based on larger firms near the eligibility threshold, as well as the average size differences between the UK and Chilean firms.

Panel A in Table 1 presents summary statistics for the sample. In 2008, the median firm has £21.05M in revenue, £13.26 in total assets, and 147 employees. Over the sample period (2004-2013), median revenue, total assets, total equity, and total non-equity liabilities are: £19.96M, £12.31M, £4.51M, and £6.23M, respectively. Annual profits and employees at the median are £5.35M and 137, respectively. The main source of capital for the firms in the sample is non-equity liabilities (51% of total assets; £6.23M/£12.31M), and compares to the mean historical leverage ratio for public firms of 60% reported by Graham, Leary, and Roberts (2015). More than 15% of the sample firms do not issue external debt over the sample period. The importance of external debt is also heterogeneous among external debt borrowers. Among firms with positive external debt, external debt corresponds to 33% of total assets (£7.42/£22.6M) at the, and 14% of total assets at the median (£1.78M/£12.9M).

Panel B in Table 1 shows that the borrowing, performance, employment and fixed assets trends of eligible and non-eligible firms look very similar prior to the EFG launch. The table reports average outcome trends across eligible and non-eligible firms in 2008. The similarities in the trends of eligible and non-eligible firms support the parallel trends assumption behind our research design, which we now explain in detail.

3 Empirical Strategy

In this section, we describe the empirical approach. We defer the presentation and interpretation of results to Section 4.

3.1 Intention-to-Treat Estimates

Our empirical strategy exploits the eligibility threshold as plausible exogenous variation in participation to identify EFG effects on businesses near the eligibility threshold. We form treatment and control groups using firms that in 2008 had revenues below and above the unexpected £25M eligibility cut-off, respectively. For the group formation, we use the value of revenues the year before the EFG launch so as to minimize concerns of firms manipulating their revenues to become eligible (below we show evidence that such manipulation was not prevalent).

Imperfect compliance with the assignment to the treatment group motivates an "intention-to-treat" (ITT) estimator. For example, the majority of eligible firms did not comply with the assignment because they did not take-up the scheme (either by choice or because they were rejected by lenders), as we discussed in Section 1.

The main advantage of the ITT is that it produces an unbiased estimate of average treatment effects even when there is substantial noncompliance, as long as the parallel trends assumption holds. Under this condition, the offer to apply was "as good as random" for firms near the revenue threshold, even if the decision to participate is endogenous. Below, we show evidence in support of the parallel trends assumption.

To estimate the ITT, we use a *difference-in-difference* approach that compares outcome trends across eligible and non-eligible firms near the threshold. We estimate equations of the following type:

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

where $Eligible_i$ is an indicator variable of eligible firms and $Post_t$ is a dummy equal to one in the years 2009-2011.

We control for varying macroeconomic conditions and industry shocks with year dummies for each industry using the 5-digit SIC classification. Industry controls are important given the heterogeneity in external debt issuance across industries (see Appendix 3). Finally, firm fixed effects account for differential firm-specific trends in all variables. The standard errors in all regressions are adjusted for heteroskedasticity and clustered at the firm level.

The coefficient of interest in (1) is β, which measures the average change in the dependent variable after the EFG launch for treatment over control firms. A positive β would imply that the average of the dependent variable increased for eligible firms relative to non-eligible firms after the EFG launch.

Several facts suggest that the parallel trends assumption is likely satisfied. Figure 1 shows no evidence of sorting of firms around the eligibility threshold during the launch, as the distribution of revenues in 2008 appears continuous at £25M. 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.

This lack of sorting mitigates concerns of eligibility manipulation that would make the parallel trends assumption invalid—for example, if "savvy" firms modify their revenues in order to be EFG eligible, and thus treatment firms would likely had evolved differently from the control firms because they are less savvy. It is also consistent with the limited ability of firms to predict the EFG eligibility threshold. As explained in Section 1, pre-launch the threshold was not disclosed, and hard to predict as the definition of small and medium sized firms vary widely across the different UK fiscal stimulus programs.

Also in support of the parallel trends assumption, Panel B in Table 1, and Figure 2, show that treatment and controls firms had similar trends pre-EFG launch. Figure 2 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 $Eligible_i$.

In Section 6.1 on robustness, we present further evidence in support of parallel trends using several placebo tests. In Section 6.2, we also show evidence against the empirical relevance of other methodological concerns including spurios trends and bandwidth dependence.

Our ITT estimates measure percentage changes as they are based on first differences of log transformations of the variables of interest. To back-out the ITT estimates on the untransformed variables, we add the estimated β to the unconditional median growth rate of the variable, and then multiply the resulting number with the median value of the variable, so as to mitigate the impact of outliers. For example, a β of 0.296 when we use changes in logs of external debt issuance as dependent variable in equation (1), translates into an estimated increase of £25.7K in external debt in levels (based on the median values of the growth rate, and level of external debt: 0 and £86.7K, respectively).

3.2 Heterogenous Treatment Effects

The average ITT is captured by β in equation (1). We also estimate heterogeneous treatment effects by splitting the sample on characteristics of interest. Looking at the costs to train employees is interesting, because retaining employees during the crisis will be most important for firms that will find it costly to train new hires in the future.

Interesting too is looking at the mismatches between input payments and the ultimate generation of cash flow (as measured by accounts receivables over revenues) (e.g., Greenwald and Stiglitz, 1988), because the need to finance productive labour is presumably higher in these firms.

Asset tangibility is a standard cut in financial constraints studies, but the predictions of where EFG impacts should be strongest are not straightforward. On the one hand, asset tangibility can proxy for potential collateral that firms can pledge for new projects, if these assets are not already backing existing debt. Eligible firms with high-tangibility may therefore need to rely less on costly guaranteed loans to access finance during the GR. On the other hand, EFG lenders also request private guarantees. As such, asset tangibility may proxy for higher EFG eligibility, and EFG usage is thus expected to be higher for this subset of firms.

Family firms are also an interesting sub-sample to focus on. These firms' cost of debt can be lower than for non-family businesses in recessions, and therefore respond less to the EFG (cf., Tsoutsoura and Lagaras, 2020). At the same time, these firms may have higher (or even non-pecuniary) incentives to provide insurance to workers, and therefore demand more EFG loans.

Finally, looking at areas with differing densities of non-eligible firms is important to gauge the practical relevance of possible externality effects. If potential EFG impacts are driven by externality effects then these impacts will be higher in areas where the potential for externalities is highest—i.e., those area with more ex-ante non-eligible firms.

3.3 Treatment on the Treated Estimates

We also estimate "treatment-on-the-treated" (TOT) effects by using an instrumental variables (IV) approach that estimates equations of the following type:

$$\Delta \log(k_{it}) = \alpha_i + \gamma_t \times Industry\ FE + \varphi \Delta \log(external\ debt_{it}) + \varepsilon_{it} \qquad (2)$$

where we instrument $\Delta \log(external\ debt_{it})$ with $Eligible_i \times Post_t$. The first stage corresponds to the estimation of equation (1) with $\Delta \log(external\ debt_{it})$ as dependent variable.

Because these estimates are based on first differences of log transformations of the variables, they measure the elasticity of the dependent variable to external debt for complier firms—i.e., the percentage change in the dependent variable to a one percentage change in external debt.

To back-out the sensitivity on the untransformed variables, we multiply the IV estimate with the unconditional median ratio of the variable to external debt. For example, an IV estimate of 0.082 for changes in log employees, translates into an estimated increase of 0.67 employees for every £100K of external debt in levels, based on the unconditional median ratio of 8.2 employees per £100K in external debt. We use the median ratio rather than the ratio of the medians because for a large fraction of observations in the sample (45%) the value of external debt is zero. We also provide alternative estimates based on the ratio of the medians using the subsample for which the values of external debt are not zero, and we find similar estimates.

The IV estimates measure the causal effects on EFG borrowers near the threshold, under the exclusion restriction assumption that the EFG had no effects on non-participants. The exclusion restriction would be violated if eligible firms are affected by the EFG even if they do not take guaranteed loans, for example, if eligible firms change their behaviour in anticipation that they will be able to apply for EFG in the future (i.e., anticipation effects). The exclusion restriction would also be violated if non-eligible firms are affected by the EFG, for example, if the loans to borrowers displace funds for non-eligible firms (i.e., externality effects). Finally, the exclusion restriction will also be violated if the EFG changes the market cost of capital via equilibrium effects.

In Section 6 of robustness, we present suggestive evidence in support of the exclusion restriction, and against the empirical relevance of potential anticipation, externality and equilibrium effects. However, we note that the exclusion restriction is an identification assumption and cannot be fully tested. Therefore, the IV estimates should be interpreted with this caveat in mind.

4 Results

Tables 2, 3, and 4 and Figure 2 summarize ITT effects from estimating different versions of equation (1). TOT estimates based on equation (2) are presented in Tables 3 and 4. Table 5 summarizes heterogeneity of ITT results from different sample cuts.

4.1 External Debt and Design Validation

This section reports effects of EFG on external debt. While external debt is not necessarily the ultimate outcome of interest, estimating this effect is an important step in the analysis because access to EFG is unlikely to affect eligible firms materially unless credit constraints bind. If eligible firms can simply obtain a loan from a different lender at similar terms (or cheaper given commercial loans do not include the EFG premium), then we will not find a treatment effect on external debt issuance, and hence would not expect to find treatment effects on employment, fixed assets or profitability under the exclusion restriction. Similarly, In addition, a comparison of our take-up and debt estimates with EFG official statistics helps validate our empirical approach.

Table 2 reports EFG treatment effects on financing outcomes. Column 1 reports the treatment effect estimate on the extensive margin of external borrowing. The probability of issuing external debt increased by 0.032 after the EFG launch for eligible relative to non-eligible businesses. The estimated size of take-up is consistent with official statistics: which as explain in Section 1 is lower than 5% relative to the target population of businesses satisfying the eligibility threshold. Our estimate is also consistent with take-up statistics by borrower size: only 331 (out of 6,700) EFG borrowers in 2009 had turnover above £5M (see Allison, Robson and Stone, 2013).

Eligible firms also borrowed more intensively that non-eligible firms. The coefficient in Column 2 implies that eligible firms on average borrowed £26K more than non-eligible firms after the EFG launch. We obtain this estimate by adding the 0.296 estimated increase in the growth rate of external debt (Table 2, Column 2) to the unconditional median growth rate of external debt (0; Table 1), and then multiplying the resulting number with the unconditional median debt (£87K; Table 1).

This estimated effect on average external debt issuance is low relative to the average loan size of £100K reported by government, because of the low take-up rate among eligible firms. To measure the size of the external debt issuance conditional on take-up, we obtain the *conditional* estimate based on the ratio between the implied ITT from Column 2 and the take-up estimate in Column 1. The results imply that conditional on issuing new external debt, eligible young firms issued on average £800K in external debt after the EFG launch (Column 1). The average conditional issuance is below the maximum loan value of £1.2M and has a 95th confidence interval ranging between £454K and £1.5M (estimated using bootstrap; see Efron and Tibshirani 1986). The results imply that eligible firms that do issue debt, issue close to the maximum guaranteed loan size.

Our estimated loan size is higher than the average EFG loan size likely because our sample is comprised of the largest eligible firms, which have larger than average debt capacities and are a minority of EFG borrowers (less than 5% of all EFG borrowers in 2009; see Allison, Robson and Stone, 2013). While access to guaranteed loans may help companies attract other lenders outside the scheme (see for example Mullins and Toro, 2018), evidence from a formal assessment by the government suggests this is not a main driver of our conditional take up estimate. The assessment revealed that only 9% of total finance raised by EFG borrowers came from sources other than the EFG loans, and that less than 5% of borrowers applied for other sources of finance (Allison, Robson and Stone, 2013).

We note that the conditional estimate may also underestimate the size of EFG loans, if EFG borrowers use the proceeds to pay-off existing debts. Perhaps because of the premium this behaviour was not apparent in practice as only a minority of the surveyed EFG recipients in 2009 (2.6%) reported the payoff/consolidation of existing debts as the main reason for seeking the guaranteed loans.

The relative external debt issuance response to the EFG was immediate, and did not revert during the sample period. Figure 2 shows a relative increase in external debt issuance the year of the launch, and no differing patterns between eligible and non-eligible firms before 2009. Increases in both long- and short-term debt (Columns 3 and 4) are responsible for the relative effects in external debt issuance. Both of these increases are consistent with the EFG design, because guarantees are available for a wide range of products, both short- and long-term, including term and asset finance facilities.

The results from the different sample cuts reveals some interesting patterns as shown in Table 5. First, Column 2 in Panel A shows that the effects on debt issuance are driven by the firms with high costs to train employees. Panel B shows that the effects are stronger for firms with a higher need to finance labour because of mismatches between sales (revenue) and cash-flow (receivables). Panel C shows that the effects on debt are driven by high-tangibility firms that are more likely to have the additional private guarantees that are requested by EFG lenders. Panel D shows family firms do not react to the EFG, likely because they have better access to finance than non-family businesses during recessions. Finally, Panel E shows that results are similar across areas with high and low number of non-eligible firms, which suggests that externality effects are not a first order concern.

To sum up, the debt results show that the large majority of eligible firms close to the eligibility threshold do not react to the EFG, which is consistent with official EFG statistics. However, those that do, borrow on average close to the maximum guaranteed debt amount allowed by the scheme. Loan issuance is driven by eligible firms that face high costs to train employees.

4.2 Impact on Other Financing Sources

We explore potential substitution and/or complementarity between EFG loans and capital sources other than external debt in the remaining columns of Table 2. This exercise is made possible by the detailed UK data. As explained in Section 2, UK firms distinguish *between* different funding sources including outside debt, but also inside debt, trade credit and equity in their filings.

This exercise is important because access to EFG loans is unlikely to affect eligible firms materially unless firms were financially constrained such that the costs of alternative financing options (other than external debt) is prohibitively high. If eligible firms can simply obtain a loan from a director or issue equity, then we would not expect to find treatment effects on real outcomes.

We find no evidence suggestive of substitution or complementarity between guaranteed debt and other types of financing sources. There are no robust, significant changes in internal debt (Column 5), trade credit (Column 6), asset finance (Column 7) or issued equity (Column 8) for eligible firms, relative to non-eligible firms, after the EFG launch.

Overall, the results in Table 2 suggest that some eligible businesses in our sample were financially constrained at the time of the EFG launch. This interpretation is consistent with survey evidence in official EFG reports. Muller Devnani and Julius (2017) show that approximately one in six participants reported they were facing challenging financial circumstances at the time they received the EFG loan. We now turn to examining effects on performance to provide further evidence of financial constraints.

4.3 Firm Performance

Table 3 summarizes ITT estimates of the effect of EFG on firm performance. The coefficient in Column 1 shows that the growth of profits in eligible firms was on average significantly higher than in non-eligible firms after the EFG launch.

The relative increase in profit growth corresponds to real effects rather than financial effects, because profits are measured based on pre-interest expenses (i.e. revenue minus costs of goods sold), and Column 2 shows that revenue relatively increased for eligible firms after the EFG launch. The increase in profit growth is also unlikely to reflect output price changes only (for example, Gilchrist et al., 2017 show that some firms increase output prices to cover falls in demand during the GR), because proxies of production scale also increase: Column 3 shows positive and significant effects on costs.

Finally, the results on profits are not driven by surges in risk. Column 4 shows that the survival probability increases for eligible relative to non-eligible businesses. 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.[25]

There are some interesting patterns of heterogeneous treatment effects in performance reported in Table 5. First, consistent with the effects on debt, Panel A shows that the effects on profits are driven by the firms with high costs to train employees. Panel B shows that the point estimate is highest for firms high mismatches between revenue and cash flow. Panel C shows that the performance effects are also concentrated in firms with high tangibility. Panel D shows higher effects on non-family firms. Finally, Panel E shows no monotonic increase in the EFG effects with the number of non-eligible firms, which add credence to the assumption that externality effects are not first order.

Encouraged by the finding that effects on non-participants do not seem very important (we present further evidence on this regard in Section 6), we present suggestive estimates of the impact of EFG take-up on real outcomes using eligibility and the launch as an instrument for borrowing as explained in Section 3.

Table 3 presents results from the IV estimation of the effect of EFG debt on profitability. We use these elasticity estimates to back-out the average increase in profits caused by every pound borrowed using the methodology explained in Section 2.

[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.

The estimate we obtain in Column 6 implies a median increase in profits of £16 per £100 increase in the external debt stock. This estimate on the return on external debt is plausible, in light of the loan prices faced by EFG borrowers (on average 7.8%) and the potentially high price of pre-existing outside options such as unsecured commercial debt (possibly 50% or higher)[26].

We obtain this estimate by multiplying the estimated sensitivity of 0.06 to the unconditional median ratio of profits to external debt in the sample where profits are positive (i.e., the sample for which we can use the change in log profits as dependent variable; 2.79). We use the median ratio of profits to external debt because many firms in the sample have no external debt (45% of the observations have zero external debt). An alternative estimate multiplies the estimated sensitivity to the median value of profits (£5.3M) and divides it by the median value of external debt (£1.8M) for the sub-sample of firms with external debt. This approach estimates a median increase in profits of £18 per £100 increase in the external debt stock.[27]

The estimate on the returns to debt in column (6) may be biased, since we do not take into account firms with negative profits. Following Banerjee and Duflo (2012), we address this concern by computing an indirect estimate of the effect on profits through the unbiased estimates we obtain in Columns 7 and 9 for revenue and costs. This estimate points to an increase in profits of £20 for a £100 increase in the external debt stock, which is £4 more in profits per £100 of external debt than in the selected sample. We calculate this indirect estimate by subtracting the per-pound-of-loan increase in the cost from per-pound-of-loan increase in the revenue, implied by the estimates in Columns 7 and 9. We estimate the per-pound-of-loan increases in revenue and costs, by multiplying the respective estimated sensitivities (0.068 and 0.072) with the unconditional median ratios of revenue to external debt (11.31) and costs to external debt (7.95), respectively.

A comparison between these return-to-debt estimates and the average scheme rates suggests that on average, returns to the EFG loans were more than enough to pay their cost for complier borrowers.

A comparison between Columns 5 and 6 shows that the OLS estimate of the relation between external debt and profits is an order of magnitude smaller than the IV estimate. The higher magnitudes for the IV estimate can be explained by a number of factors, but a plausible explanation is that "complier firms" who issue debt in response to the scheme have particularly large average returns to external debt relative to the population of small companies. Note that the higher IV magnitudes are instead unlikely

[26] See: https://www.money.co.uk/business-loans.htm

[27] The implied IV estimates based on the ratio of mean profits to mean debt is 0.11.

to be explained by a weak instrumental variables problem, as indicated by the healthy first-stage F statistics.

We return to the interpretation of the results in Section 5 when we discuss in more detail the implication of the findings, and provide different benchmarks against which to compare the magnitudes of the results.

4.4 Employment and Fixed Assets: Exploring the Mechanisms

We now explore the effects on employment and fixed assets to shed light on the mechanisms at play behind the EFG effect on small firm performance. Table 4 summarizes results, and shows a large asymmetry in employment and investment responses.

Column 1 in Table 4 shows that the difference in employment growth is positive between eligible and non-eligible firms after the EFG launch. Results in Figure 3 reveals that this positive difference is explained by a smaller employer contraction in eligible businesses during the crisis, rather than net employee growth in these firms. Figure 3 plots average changes in employment and fixed assets (relative to 2008) separately for eligible and non-eligible firms around the EFG launch. Note that for ease of exposition the estimates for eligible firms are plotted slightly forward in the time-axis than those for non-eligible firms.

In contrast to the employment results, Column 2 in Table 4 shows no difference in the fixed assets' investment growth for eligible versus non-eligible firms: the point estimate is very close to zero and is not statistically significant. Results in Figure 3 also show that both eligible and non-eligible businesses made no significant capital adjustments on average during the sample period around the GR. In unreported regressions, we show that total and current assets show similar imperceptible adjustments.

Figure 2 corroborates the asymmetric response of employment and fixed assets. The figure shows that eligible firms increase employment but not fixed-assets after the EFG launch relative to non-eligible firms. Figure 2 plots the dynamic ITT estimates on employment and fixed assets based on the flexible estimation of equation (1).

The asymmetric response of employment and fixed assets adds to previous work that finds similar asymmetries in firms' reactions to financial shocks (cf., Bakke and Whited, 2012). The response is also consistent with survey evidence. Relative to surveyed non-EFG borrowers, surveyed EFG recipients are significantly less likely to seek external finance for the purpose of purchasing assets (22.8% and 12.1%, respectively; see Allison, Robson and Stone, 2013).

Figure 2 shows that the increase in employment occurred in lock-step with debt issuance and did not revert during the sample period. Table 5 shows that employment effects are entirely driven by industries with high costs to train employees. The employment effects are also stronger for firms with mismatches in cash-flow and sales, are not present for family firms and do not monotonically increase for areas with higher externality potential.

Under the assumption of no meaningful EFG impacts on non-participants, we quantify the impact on EFG borrowers' employment and investment using the IV approach. We estimate that a £100K increase in external debt leads to an additional 0.67 employees. We obtain this estimate by multiplying the estimated sensitivity of 0.082 in column 5 of Table 4 to the unconditional median ratio of 8.2 employees per 100K of external debt.

This estimated sensitivity suggests that the types of workers that EFG borrowers retained were relatively high-skill, as the average annual salary in UK is £28,677. Consistent with this interpretation we show in unreported analysis that within the sub-sample of industries with high training costs, the significant effects concentrate on those industries that additionally also require high experience of their workers, and therefore likely command higher wages.

By contrast, the IV estimate for investment of Column 7 in Table 4 implies that investment did not significantly respond to the EFG launch, as is nevertheless consistent with the ITT results of Column 2 in the table. Results in Columns 2 and 7 suggest that the positive and significant OLS estimate in Column 6 is biased: the positive association between debt and fixed asset investment reflects larger borrowing needs of firms with high investment opportunities, rather than necessarily a causal effect of external debt access on fixed asset investment during the recession.

5 Discussion

In this section we discuss the interpretation of results, the potential reasons behind the limited take-up of the EFG, conduct a cost-benefit analysis, and discusses the external validity of results. We finalize this section by discussing the implication of our results for the COVID-19 pandemic crisis.

5.1 Interpretation of Results

The results are consistent with the guarantees enabling a small number of financially constrained firms to retain workers that were fundamental in rebuilding the businesses post-crisis, likely because they already possessed firm-specific skills. Skilled staff are harder and more expensive to hire and to lay off than others, and plenty of companies were complaining of skills shortages before the recession hit (Lambert, 2010).

Absent the guarantees, these businesses would have had to lay-off workers to absorb the negative demand shock. Consistent with this explanation, Confederation of British Industry (CBI) surveys conducted during and immediately after the recession noted that two-thirds of companies cited employee engagement as their top concern.[28]

To finance any investment during the crisis, these firms could borrow by pledging the fixed assets as security. Instead, securing loans for employee retention would be harder, because employees cannot be pledged as collateral for loans. As a result, with no government guarantees these firms would incur in costs of rehiring and training workers post-crisis, making the recovery less profitable and less likely (cf., Oi; 1962; Rota, 1998). And would have lower labour-productivity if capital and labour are complementary and the capital stock is irreversible (Caggese, 2007).

Alternative interpretations for the results are less consistent with the findings. Insurance provision by firms to workers against idiosyncratic risk is an explanation that is less relevant in our context given the aggregate scope of the GR (cf., Guiso, Pistaferri and Schivardi, 2005).

The macroeconomic environment also helps rule out supply-side explanations, as workers were unlikely to voluntarily retire from non-eligible firms with tighter financial constraints given the fewer outside options available during the crisis (cf., Baghai, Silva, Thell and Vig, 2020). Additionally, results are driven by industries with high training costs, where worker skills are more likely to be firm-specific, and less transferable to outside jobs.

Risk shifting by banks and borrowers is also an unlikely explanation. Risk shifting cannot explain the asymmetric responses by employment and fixed assets, is not consistent with the performance and survival increases, and was likely largely mitigated by several scheme features.

Finally, the labour-productivity effects are not consistent with concerns that guaranteed loans kept workers in unproductive firms, and/or prevented efficient reallocation of labour. These findings demonstrate that not all labour retention occurring during the Great Recession was unproductive (cf., Coulter, 2010).

5.2 Why More Eligible Firms did not Borrow through the EFG?

Several issues may hinder the take-up of loan guarantee programs in spite of appealing average returns, but as one limitation that we share with previous research, we note that we cannot rigorously distinguish between them with our current data (cf., Beck, Klapper, and Mendoza, 2010).

[28] CBI Industrial Trends Survey 2010.

First, there may be no failure in take-up at all: the businesses who opted in are the ones who could benefit, and those who did not opt in could not benefit.

This could happen if most firms rationally opted-out, because their expected benefits from employee retention were not enough to compensate for the 2% scheme premium. However, in that case, an important concern is whether the premium rationed-out potential beneficiaries that had no access to other sources of funds (for example, firms exempted from the government programs with no premium, such as the Working Capital Scheme). Consistent with this concern, survey evidence shows sizable take-up sensitivities to the premium: four out of ten participants reported that increases in 1% of the premium would have deterred them from drawing down the EFG loan (Allinson, Robson, and Stone, 2013). Lowering the fee may not be efficient, though. The optimal premium trade-offs the costs from potential rationing, with the benefits from having users chip in the expenses of the scheme that would otherwise would be transferred to non-participant tax payers.

Note as well that no failure in take-up does not imply that no market failure exists. Our return estimates suggest that a credit failure exists for those who did opt-in, in that they did not take out a loan at the market rate.

Alternatively, take-up failure exists for either of two general reasons.

First, features of the scheme repelled potential beneficiaries. Allowing banks to request personal guarantees pushes back demand of firms that had use-up all personal guarantees to secure funds during the crisis. We note that this take-up failure may not necessarily be inefficient, for example, restricting banks from requesting personal guarantees can decrease their incentives to monitor.

Second, considerations other than economic benefit may keep firms at bay. For example, firms may be averse to government scrutiny. The latter is however unlikely, given the limited role of the government in the EFG execution. Firms may also be averse to taking on debt, especially during a recession, if entrepreneurs are risk averse. Firms may also fear potential stigma from partaking in stimulus programs. Finally, take-up may also be hindered by excessive red-tape to access the funds. In support of this issue, survey evidence shows that EFG borrowers reported significantly longer times for lending decisions to be made than firms borrowing under normal borrowing conditions (Allinson, Robson, and Stone, 2013).

5.3 Cost-Benefit Analysis

Given the limited take-up, the question remains, was EFG good value for money?

To answer this question we perform a back-of-the-envelope calculation based on our estimates and publicly available data on the program's cost to perform a cost-benefit analysis. Under plausible assumptions, results show that for the sub-sample of eligible firms close the eligibility threshold, the economic benefits were 1.5 times the costs.

Our calculation is as follows: First, we estimate the average benefits per borrower in our sample at £128K. To produce this estimate we multiply the average loan size of £800K (see Section 4.1) in our sample, with the 0.16 sensitivity of profits to the guaranteed debt (see Table 3).

Next, we estimate the average costs per borrower for the borrowers in our sample at £82K. To produce this estimate, we use publicly available information on: the losses from loan defaults, the administration costs of running the scheme, and the opportunity costs of the loans based on information from a government evaluation of the EFG.

In detail, we estimate the average per borrower value of loan defaults at 13.6K (£92M/6,700 borrowers) and the average administration costs at £119 (£800K/6,700). These estimates are based on actual default and administration cost data, and assume they are equal across borrowers (for more details see: Allinson, Robson, and Stone, 2013). In addition, we estimate an opportunity cost of £68,000 (£800K×8.5%), based on a private sector opportunity cost of capital of 8.5% that is used in the government report.

The cost-benefit analysis likely underestimates the benefit-cost ratio for a number of reasons. The expected cost of defaults in our subsample can be smaller because in practice defaults concentrated in smaller borrowers (see Section 1), although loss given default is higher for our subset of companies. In addition, profits only measure short-term benefits, and it is possible that longer-term benefits exist given the positive productivity results.

5.4 External Validity

In terms of external validity, there are two main issues to consider when interpreting the findings.

One external validity issue is the representativeness of our sample. As with most empirical work, the findings may apply only to the sample used to estimate them. Our sample is a subset of larger populations of interest: principally, firms that are perceived by policy makers to be at higher risk of financial constraints during the crisis. But, our estimates do not measure impacts on smaller firms who are far below the revenue-size threshold, nor on new businesses incorporated after the EFG launch.

In terms of magnitude, our estimates on returns to new debt are comparable to those in prior work on small firms' financial constraints. Our estimates are close to the 15% lower bound estimates reported in the paper by Bach (2013), which exploits the introduction of a targeted credit program in France as an exogenous determinant of debt access.

However, our return estimates are lower than return to capital estimates for underdeveloped economies, which typically exceed 50% (e.g., Banerjee and Duflo, 2014). Several explanations exist for why our estimates are in the lower range. Perhaps more prominently, we trace the debt returns to employment rather than investment. Additionally, our sample firms are substantially larger than the microenterprises of development work, and a negative relation has been documented for firm size and returns to capital (McKenzie and Woodruff, 2006).

The second external validity issue relates to the representativeness of the EFG. There is a large heterogeneity in the design of loan guarantees. Our results are most representative of other programs that like the EFG take steps to align lenders' and borrowers' incentives with the policy objective.

For example, variations in policy design can help explain why our results contrast those for the French guarantees reported in Lelarge, Sraer and Thesmar (2010). The evidence in that study shows that the French program increased risk taking incentives of borrowers because it explicitly forbid lenders to require additional private guarantees. By contrast, EFG lenders likely have higher incentives, as well as higher ability to screen and discipline borrowers because they can request personal securities (cf., Smith and Warner, 1979; Stulz and Johnson, 1985; Boot, Thakor, Udell, 1991; Rajan and Winton, 1995; Park, 2000; Liberti and Sturgess, 2014).

5.5 Insights for the COVID-19 Crisis

The main objective in the COVID-19 crisis is to have people stay at home, and thus governments' main short-term aim is to insure workers. An important question is whether this objective is best achieved by policies that target individuals or firms.

The idea behind targeting firms is for governments to pay employees through corporate payrolls. The intention is to use companies as a way to get money to the individuals that need it to feed their families and pay their rent. Most of the criticism against this idea has focused on the self-employed. The concern is that independent workers, which represent 17% of the work force in UK, will be missed by such policies.[29]

Our results offer a novel insight into this debate. They demonstrate that recessionary loan guarantees targeting companies can achieve the insurance objective only partially, even amongst the non-self-

[29] See https://www.bloomberg.com/news/articles/2020-03-26/u-k-s-sunak-pledges-coronavirus-support-for-self-employed

employed. Eligible firms will have incentives to retain workers, but, only for those workers that are costly to hire and train. For firms with low-training-costs workers that are easy to hire, paying the above-market rates of these schemes to retain staff will not be good value for money.

The main implication of these findings for the COVID-19 crisis is that policy responses based on firm guarantees alone can be regressive because poorer workers are the more likely to have low-training-costs jobs. Other stimulus programs targeting lower skill workers are therefore warranted, such as the Job Retention Scheme program also sponsored by the UK government in this crisis.

Alternatively, governments can also consider major overhauls of loan guarantee schemes to increase take-up. As explained in section 5.2., survey evidence of EFG borrowers suggests that other scheme features that deterred take-up where banks' requests of personal guarantees and perceived red-tape in accessing the funds.

Whether policy makers should consider tweaking the scheme design to increase take-up during the COVID-19 crisis is a valid question. Early informal evidence on the Coronavirus Business Interruption Loan Scheme (CIBLS) in the UK—the equivalent to the EFG for the COVID-19 Pandemic crisis—suggests that companies are struggling to get the funds. Companies seeking CIBLS funding have criticised the scheme for imposing harsh conditions on borrowers and for its slow delivery, with widespread reports of delays and confusion among banks.

The main criticism of tweaking the scheme design to improve access are concerns regarding borrowers and banks' incentives to take the scheme. Prior research warns against widespread relaxation in scheme design (e.g., Lelarge, Sraer, and Thesmar, 2010; D'Acunto, Tate, and Yang, 2017). However, the unprecedented urgency of this crisis has made fast access to the funds a priority among policy makers, and has pushed to a far second-level considerations of moral hazard. Only time will tell whether the potentially large benefits from streamlined guarantee programs—such as the SFr40bn package of emergency loans in Switzerland—will compensate potential long-term difficulties when the loans come due for repayment.

6 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.

6.1 Identification Tests

The main identification concerns in our empirical strategy are potential: (1) manipulation of EFG eligibility in 2009, (2) violation of the parallel trends assumption and (3) violation of the exclusion restriction assumption. In Section 3, we discussed the evidence against the first concern in detail, and discussed standard evidence against the second concern summarized in Panel B of Table 1 and in Figure 2. We now present a more detailed discussion of the evidence against the second and third issues, which complements the discussion on this subject in Section 4.

We use complementary placebo and falsification exercises in support of the parallel trends asusmption. 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 thresholds. 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 2008 are below or above this threshold, respectively. A summary of the results is presented in Table 6. As expected with randomly picked thresholds, we cannot reject the null of no effect in more than 95% 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). Table 8 shows there is no significant change in firm outcomes across the smaller and larger of these companies in non-eligible industries.

While we cannot fully rule out any violations because as econometricians we have limited information, when taken together, results from: the standard pre-trends test, the placebo regressions and the falsification exercise, provide compelling evidence in favour of the parallel trends assumption.

Regarding the exclusion restriction assumption of no EFG effects on non-participants, the results in Table 5, and the patterns in Figure 3 suggest potential externalities are not first order as discussed in Section 4. Panel E in Table 5 shows that the results are not increasing in the ex-ante density of non-eligible businesses, as would be expected if externalities in non-eligible businesses were the main explanation of the results. Additionally, Figure 3 shows that the employment effects are not due to employee movements from non-eligible to eligible businesses.

We run two complementary placebo regressions to provide additional supportive evidence of the exclusion restriction, and summarize results in Appendix 4. First, 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. 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. Second, we show evidence of no-effects on non-borrowers: Appendix 4 shows that no changes in profits, survival or employment are visible for the sub-sample of firms with negative external debt changes. This additional test provides suggestive evidence against the practical relevance of potential anticipation effects.

When taken together, results in Table 5 (Panel E), Figure 3 and Appendix 4, together with the unlikely equilibrium effects because of the small size of the scheme, constitute evidence against violations of the exclusion restriction (e.g., externality, anticipation, and general equilibrium effects).

6.2 Potential Sample Concerns and Serial Correlation

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 Table 7 we show that the results are similar in two other alternative subsamples of companies within smaller revenue bandwidths around the £25M eligibility threshold.

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. We note that the main advantage of the ITT is that it is consistent even if this type of non-compliance exists. Nevertheless, the tests in Appendix 4 that show robustness to exclusion of firms with revenues in 2008 within a £5M window around the threshold, alleviate this concern, and these are the firms that are more likely to be able to manipulate assets in order to qualify for the scheme after the EFG launch.

One final concern is that the potential serial correlation in outcomes may lead to inconsistency in standard errors. To address this concern, in Appendix 4 we show that results are robust to collapsing the data to two observations per firm (one before and one after the EFG launch) (cf., Bertrand, Duflo, and Mullainathan, 2004).

7 Conclusions

In this paper, we explore the effects of the EFG—a guarantee program targeting small firms that was implemented during the Great Recession in UK. We exploit time series variation from the launch of the scheme in 2009, and cross-sectional variation from the unexpected size-based eligibility requirements. We calculate ITT estimates using a difference-in-difference methodology, and find that eligible firms relatively increased their borrowing, profits, survival, productivity and employment, but not investment, after the program launch. Additional results suggest that the findings are mainly driven by effects on the minority of eligible firms that take-up the scheme, rather than by externalities or other general equilibrium effects. Under this assumption, and using an IV approach, we estimate returns on guaranteed debt that comfortably exceed the above-market scheme rates. Results are driven by firms with high-training-costs employees. The results are consistent with the guarantees enabling financially constrained firms to retain key workers that helped rebuilt their businesses post-crisis. The main implication for the COVID-19 crisis is that policy responses based on guarantees alone can be regressive, because poorer workers are the more likely to have low-training-costs jobs. Other support aimed at low-skill workers is therefore warranted, such as the Job Retention Scheme program also sponsored by the UK government in this crisis.

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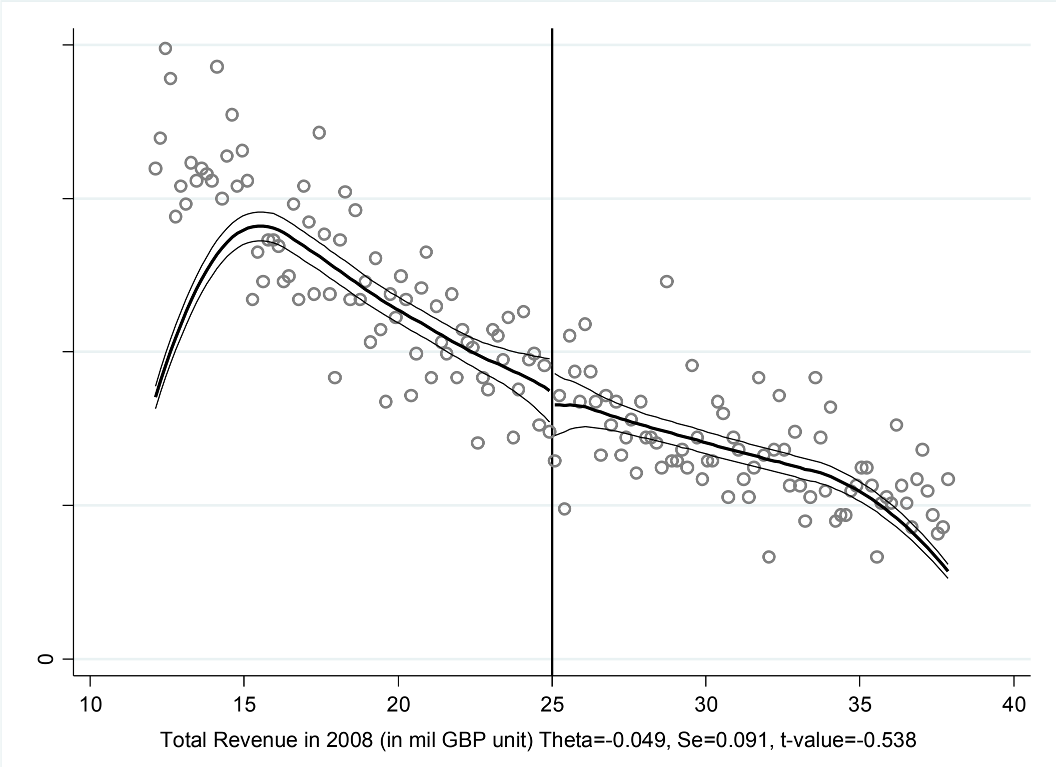
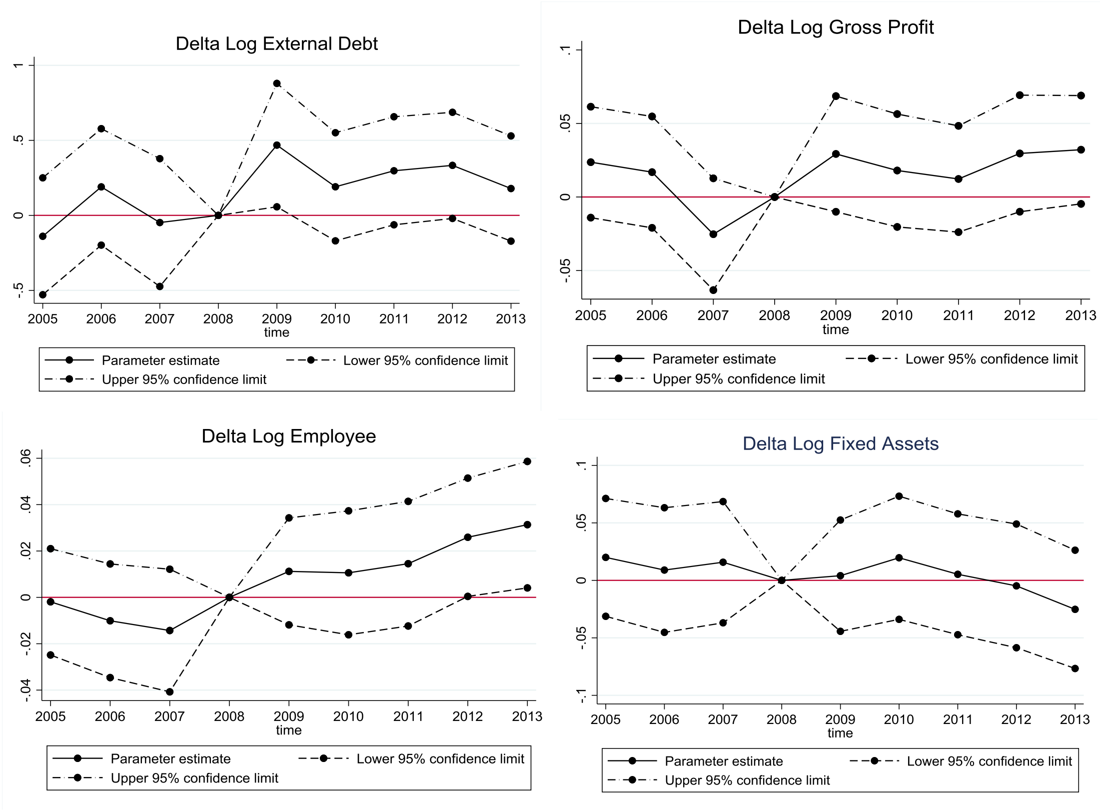
Figure 1: Distribution of Firms by Revenue Size in 2008



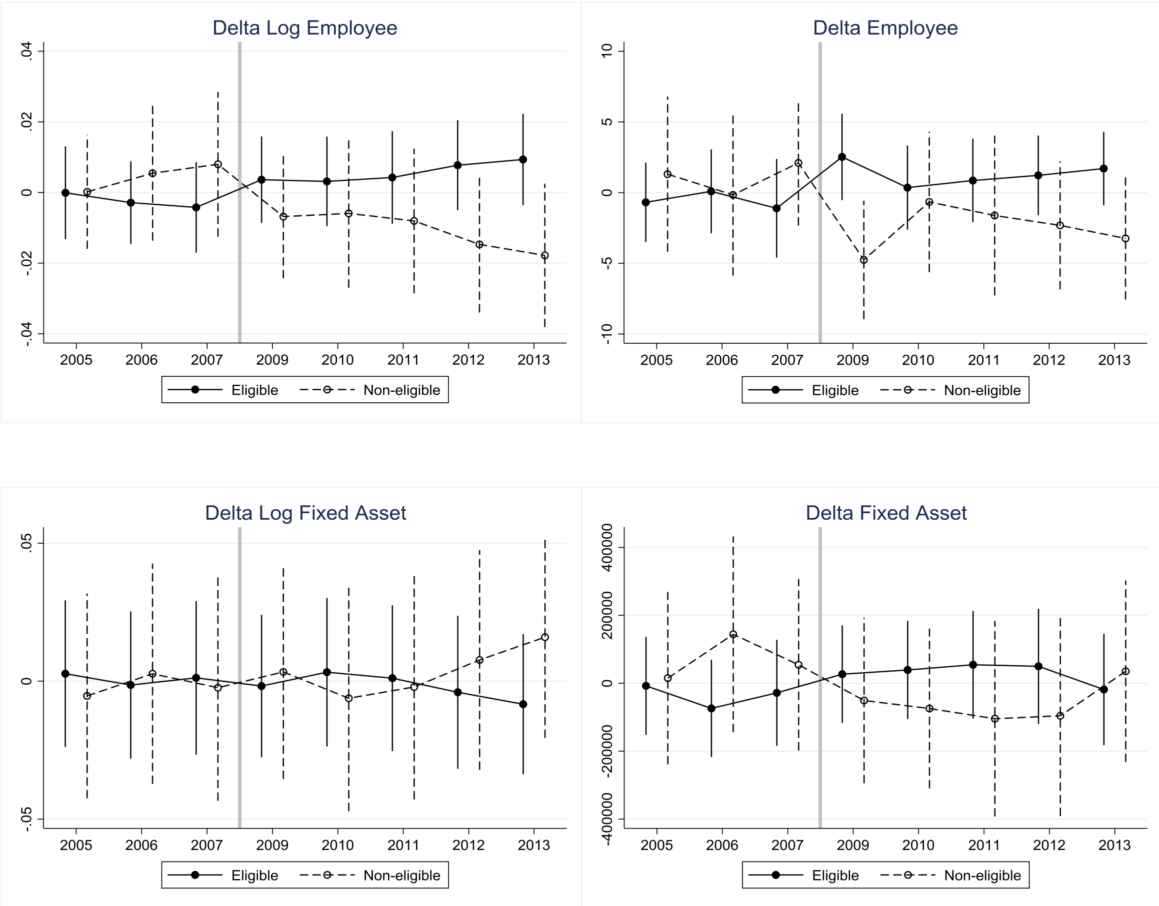
The figure plots the distribution of revenues in 2008 for the firms in the sample. The sample includes UK firms with revenues in 2008 between £12M and £38M (i.e. +/-£13M window around the revenue threshold of £25M) that survived until at least 2009, and with more than 50 employees in 2008 and total assets 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 2: EFG and Dynamic Changes



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 3: Hiring and Investment across Eligible and Non-eligible Firms



The plot presents estimates of the residualized annual means in the trends of employees and fixed assets. The dependent variable is specified in the top of each plot. The solid (dashed) horizontal line plots the difference in the estimated coefficients and the coefficient of 2008 for eligible (non-eligible businesses). The solid (dashed) vertical lines plot the 95^{th} percent confidence interval of the differences.

Table 1: Summary statistics

Panel A: Full sample

Variables	(1) Obs.	(2) Median	(3) Mean	(4) Std. Dev.
Revenue 2008	7,723	21,052,000	22,327,815	7,296,620
Receivables 2008	7,186	2,674,000	3,134,158	2,517,395
Receivables/revenue 2008	7,186	0.14	0.14	0.11
Total Assets 2008	7,723	13,255,000	20,892,576	22,229,984
Employees 2008	7,723	147	208	219
Employees/Total Assets 2008	7,723	0.000011	0.000017	0.000027
Leverage Ratio 2008 (Total Non-equity Liabilities /Total Assets)	7,133	0.24	0.35	0.74
Tangibility 2008 (Fixed Assets/Total Assets)	7,585	0.24	0.32	0.26
Issued Equity	60,724	127,700	1,571,142	2,988,149
Total Equity	72,377	4,512,000	8,179,506	10,507,180
Total Non-Equity Liabilities (Total Assets- Total Equity)	72,602	6,232,000	12,082,742	18,471,259
External Debt	72,602	86,766	4,030,337	14,905,831
Internal Debt	63,741	475,000	4,558,915	12,256,930
Trade Debts (Receivables)	64,968	2,575,978	3,229,399	3,177,983
Trade Credit	67,708	1,382,000	1,960,181	2,216,519
Survival	74,825	1	0.96	0.19
Total Assets	72,602	12,312,000	20,236,899	23,841,066
Fixed Assets	69,971	2,700,000	8,246,435	15,417,855
Current Assets	72,357	7,838,000	11,636,285	12,172,120
Employees	69,354	137	203	242
Profit	58,465	5,345,000	6,909,197	6,077,595
Revenue	58,524	19,956,652	22,638,605	13,917,606
Cost of Sales	58,524	13,513,000	15,762,021	11,716,138
Productivity	57,833	140,645	183,867	331,592
Δln(External Debt)	64,308	0.00	-0.20	4.99
Δ ln(Internal Debt)	54,581	0.00	0.16	3.74
Δ ln(Total Assets)	64,552	0.04	0.06	0.78
Δ ln(Fixed Assets)	61,929	0.01	0.02	0.71
Δ ln(Current Assets)	64,278	0.05	0.07	0.83
Δ ln(Employees)	61,051	0.01	0.00	0.36
Δ ln(Trade Credit)	59,357	0.02	0.02	0.68
Δ ln(Issued Equity)	52,884	0.00	0.04	0.75
Δ ln(Profit)	49,865	0.04	0.04	0.46
Δ ln(Revenue)	51,058	0.04	0.04	0.39
Δ ln(Cost of Sales)	51,058	0.04	0.04	0.42
Δ ln(Productivity)	50,264	0.02	0.03	0.31
ΔExternal Debt	64,552	0.00	-7,518	7,520,392
ΔInternal Debt	54,581	0.00	474,525	6,599,626
ΔTotal Assets	64,552	427,000	967,142	7,577,468
ΔFixed Assets	61,929	-20,000	288,246	4,403,744
ΔCurrent Assets	64,278	384,000	677,074	5,173,831
Δ Employees	61,051	1.00	3.00	80.00
ΔTrade Credit	59,357	22,000	72,173	1,196,787
ΔIssued Equity	52,884	0.00	64,841	754,143
ΔProfit	51,001	163,000	307,488	2,429,063
ΔRevenue	51,058	709,000	968,195	6,970,083
ΔCost of Sales	51,058	419,000	658,084	5,763,474
ΔProductivity	50,264	2,476	7,752	232,678

Panel B: Treatment and control in 2008

Variable	N	Mean	sd	Median	N	Mean	sd	Median	Mean Difference	T-statistics
	Treatment Sample				*Control Sample*					
Δln(External Debt)	4,882	0.053	5.080	0.000	2,600	0.127	4.965	0.000	-0.075	0.609
Δ ln(Internal Debt)	4,292	0.260	4.100	0.000	2,339	0.173	3.919	0.000	0.087	-0.840
Δ ln(Total Assets)	4,883	0.124	0.776	0.047	2,600	0.112	0.754	0.040	0.012	-0.643
Δ ln(Fixed Assets)	4,773	0.053	0.677	0.020	2,539	0.061	0.705	0.012	-0.007	0.437
Δ ln(Current Assets)	4,874	0.136	0.830	0.061	2,596	0.112	0.844	0.051	0.024	-1.180
Δ ln(Employees)	4,762	0.047	0.311	0.020	2,552	0.050	0.321	0.020	-0.002	0.320
Δ ln(Trade Credit)	4,620	0.008	0.668	0.007	2,456	0.020	0.662	0.020	-0.012	0.711
Δ ln(Issued Equity)	3,874	0.076	0.788	0.000	2,131	0.055	0.737	0.000	0.022	-1.044
Δ ln(Profit)	3,803	0.050	0.490	0.040	2,096	0.060	0.430	0.050	-0.004	0.313
Δ ln(Revenue)	3,883	0.070	0.320	0.050	2,135	0.090	0.310	0.060	-0.019	2.208
Δ ln(Cost of Sales)	3,883	0.080	0.360	0.060	2,135	0.100	0.340	0.060	-0.020	2.127
Δ ln(Productivity)	3,841	0.034	0.305	0.027	2,117	0.055	0.277	0.040	-0.022	2.693

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). Panel A summarizes the full sample, Panel B compares the treatment versus control sample in year 2008. 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

	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(External Long Debt)	(4) Δln(External Short Debt)	(5) Δln(Internal Debt)	(6) Δln(Trade Credit)	(7) Δln(Lease)	(8) Δln(Issued Equity)
$Eligible_i \times Post_t$	0.032***	0.296***	0.156**	0.249***	0.024	0.022*	-0.065	-0.003
	(0.008)	(0.075)	(0.066)	(0.072)	(0.065)	(0.012)	(0.058)	(0.016)
Obs.	63,779	63,539	63,705	63,627	63,779	58,551	50,940	51,903
R-squared	0.316	0.105	0.117	0.103	0.100	0.155	0.159	0.189

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 2009-2013. 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 and performance

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dep. Var.	Δln(Profits)	Δln(Revenue)	Δln(Costs)	Survival	Δln(Profits)	Δln(Profits)	Δln(Revenue)	Δln(Revenue)	Δln(Costs)	Δln(Costs)
Method	ITT	ITT	ITT	ITT	OLS	IV	OLS	IV	OLS	IV
$Eligible_i \times Post_t$	0.021**	0.023***	0.024***	0.008***						
	(0.010)	(0.009)	(0.009)	(0.003)						
Δln(External Debt)					0.000	0.060*	0.002***	0.068**	0.002***	0.072**
					(0.001)	(0.032)	(0.000)	(0.030)	(0.000)	(0.033)
Obs.	48,967	50,152	50,152	73,984	48,773	48,773	49,953	49,953	49,953	49,953
R-squared	0.220	0.257	0.249	0.105	0.220	-0.449	0.258	-0.806	0.250	-0.755
First Stage										
Δln(External Debt)						0.336***		0.335***		0.335***
						(0.088)		(0.086)		(0.086)
F-stat excluded instrument						14.66		15.04		15.04

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 2009-2013. 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 4: EFG, Employment, Investment and Productivity

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dep. Var.	Δln(Employees)	Δln(Fixed Assets)	Δln(Productivity)	Δln(Employees)	Δln(Employees)	Δln(Fixed Assets)	Δln(Fixed Assets)	Δln(Productivity)	Δln(Productivity
Method	ITT	ITT	ITT	OLS	IV	OLS	IV	OLS	IV
$Eligible_i \times Post_t$	0.025***	-0.008	0.012*						
	(0.007)	(0.013)	(0.006)						
Δln(External Debt)				0.005***	0.082***	0.014***	-0.036	-0.001**	0.037*
				(0.000)	(0.031)	(0.001)	(0.057)	(0.000)	(0.021)
Obs.	60,255	61,138	49,347	60,022	60,022	60,907	60,907	49,149	49,149
R-squared	0.231	0.179	0.179	0.236	-1.290	0.188	-0.124	0.180	-0.377
First Stage									
Δln(External Debt)					0.297***		0.244***		0.330***
					(0.077)		(0.076)		(0.088)
F-stat excluded instrument					14.72		10.41		14.15

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 2009-2013. 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 5: Heterogeneity

Panel A: Sample Cuts by Training Costs

Dep. Var.	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(Profit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Q1								
$Eligible_i \times Post_t$	0.040**	0.261	0.026	0.017	0.012	0.020***	0.022	-0.005
	(0.020)	(0.179)	(0.026)	(0.021)	(0.027)	(0.007)	(0.020)	(0.034)
Observations	10,892	10,845	8,045	8,289	8,289	12,656	10,209	10,290
R-squared	0.279	0.048	0.166	0.169	0.191	0.030	0.175	0.149
Q2								
$Eligible_i \times Post_t$	0.021	0.196	-0.004	-0.017	-0.011	0.001	0.011	-0.022
	(0.014)	(0.125)	(0.014)	(0.012)	(0.014)	(0.005)	(0.011)	(0.023)
Observations	18,808	18,746	14,860	15,049	15,049	21,802	17,848	18,208
R-squared	0.274	0.042	0.168	0.196	0.196	0.034	0.188	0.131
Q3								
$Eligible_i \times Post_t$	0.025*	0.174	0.005	0.012	0.021*	0.003	0.027***	0.005
	(0.013)	(0.126)	(0.014)	(0.012)	(0.013)	(0.005)	(0.010)	(0.017)
Observations	20,700	20,633	16,832	17,233	17,233	23,779	19,922	20,141
R-squared	0.241	0.039	0.121	0.183	0.179	0.023	0.189	0.121
Q4								
$Eligible_i \times Post_t$	0.039**	0.381**	0.080***	0.082***	0.061**	0.010	0.040**	-0.012
	(0.017)	(0.162)	(0.027)	(0.026)	(0.026)	(0.007)	(0.019)	(0.030)
Observations	14,147	14,078	10,016	10,378	10,378	16,588	13,048	13,268
R-squared	0.259	0.043	0.148	0.194	0.175	0.035	0.178	0.136
Difference: Q4 vs. rest	0.012	0.178	0.077***	0.081***	0.055**	0.001	0.020	-0.004
	(0.019)	(0.180)	(0.029)	(0.027)	(0.028)	(0.007)	(0.021)	(0.033)

Panel B: Sample Cuts by Receivables/ Revenue

Dep. Var.	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(Profit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Q1								
$Eligible_i \times Post_t$	0.008	-0.213	0.018	0.038*	0.023	0.004	0.002	-0.023
	(0.018)	(0.160)	(0.027)	(0.021)	(0.023)	(0.007)	(0.017)	(0.028)
Observations	13,684	13,619	10,522	10,788	10,788	16,085	12,901	13,044
R-squared	0.358	0.164	0.289	0.260	0.279	0.167	0.276	0.244
Q2								
$Eligible_i \times Post_t$	0.032*	0.367**	-0.006	0.004	-0.001	-0.004	0.019	-0.031

	(0.019)	(0.168)	(0.020)	(0.016)	(0.017)	(0.006)	(0.016)	(0.028)
Observations	14,154	14,100	10,903	11,123	11,123	16,413	13,307	13,626
R-squared	0.385	0.198	0.306	0.373	0.360	0.181	0.301	0.232
Q3								
$Eligible_i \times Post_t$	0.043**	0.347**	-0.010	-0.003	0.014	0.005	0.019	-0.016
	(0.018)	(0.169)	(0.017)	(0.017)	(0.016)	(0.006)	(0.016)	(0.027)
Observations	14,085	14,037	11,367	11,482	11,482	16,260	13,340	13,622
R-squared	0.364	0.187	0.304	0.318	0.312	0.197	0.274	0.204
Q4								
$Eligible_i \times Post_t$	0.055***	0.577***	0.031	0.018	0.022	0.025***	0.045**	0.011
	(0.018)	(0.173)	(0.024)	(0.022)	(0.023)	(0.006)	(0.018)	(0.033)
Observations	14,115	14,055	11,024	11,138	11,138	16,323	13,302	13,652
R-squared	0.362	0.172	0.290	0.326	0.306	0.171	0.265	0.245
Difference: Q4 vs. rest	0.025	0.337*	0.016	-0.003	-0.001	0.018***	0.028	0.026
	(0.020)	(0.190)	(0.026)	(0.023)	(0.025)	(0.007)	(0.020)	(0.035)

Panel C: Sample Cuts by Tangibility

Dep. Var.	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(Profit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Q1								
$Eligible_i \times Post_t$	0.002	0.099	0.010	0.000	-0.004	-0.007	0.015	-0.043
	(0.016)	(0.163)	(0.023)	(0.019)	(0.020)	(0.007)	(0.014)	(0.031)
Observations	15,141	15,075	11,552	11,741	11,741	17,703	14,167	14,379
R-squared	0.362	0.152	0.287	0.312	0.306	0.175	0.310	0.215
Q2								
$Eligible_i \times Post_t$	0.018	0.374**	0.031	0.039**	0.053**	0.003	0.024*	0.002
	(0.017)	(0.160)	(0.021)	(0.017)	(0.022)	(0.006)	(0.015)	(0.028)
Observations	14,877	14,796	11,652	11,813	11,813	17,241	14,064	14,471
R-squared	0.371	0.183	0.273	0.325	0.315	0.181	0.314	0.257
Q3								
$Eligible_i \times Post_t$	0.047***	0.205	-0.019	-0.000	0.000	0.011*	0.023	-0.006
	(0.017)	(0.156)	(0.019)	(0.017)	(0.016)	(0.006)	(0.015)	(0.024)
Observations	15,008	14,959	11,615	11,871	11,871	17,325	14,275	14,698
R-squared	0.354	0.191	0.303	0.311	0.313	0.177	0.273	0.247
Q4								
$Eligible_i \times Post_t$	0.065***	0.466***	0.054**	0.063***	0.046*	0.018***	0.049**	0.031
	(0.020)	(0.163)	(0.025)	(0.022)	(0.025)	(0.006)	(0.020)	(0.027)
Observations	14,359	14,311	10,551	10,779	10,779	16,727	13,484	14,105
R-squared	0.381	0.190	0.312	0.376	0.332	0.163	0.261	0.273
Difference： Q4 vs. rest	0.043**	0.232	0.046*	0.050**	0.029	0.015**	0.029	0.045

	(0.022)	(0.181)	(0.027)	(0.023)	(0.026)	(0.007)	(0.021)	(0.031)

Panel D: Sample Cuts by Ownership Structure

Dep. Var.	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(GrossProfit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Family firms								
$Eligible_i \times Post_t$	0.024	-0.052	0.007	0.001	0.006	0.003	0.000	0.003
	(0.018)	(0.143)	(0.016)	(0.014)	(0.014)	(0.004)	(0.013)	(0.024)
Observations	15,007	14,936	12,023	12,122	12,122	16,864	14,290	14,651
R-squared	0.344	0.191	0.264	0.271	0.282	0.166	0.280	0.215
Nonfamily firms								
$Eligible_i \times Post_t$	0.029***	0.401***	0.024*	0.027**	0.028**	0.004*	0.031***	-0.008
	(0.009)	(0.090)	(0.012)	(0.011)	(0.012)	(0.002)	(0.009)	(0.016)
Observations	47,637	47,457	35,727	36,821	36,821	53,626	44,808	45,343
R-squared	0.336	0.117	0.232	0.282	0.268	0.112	0.245	0.191
Difference:								
Familv.s	-0.005	-0.453***	-0.023	-21,585	-0.026	-0.001	-0.031*	0.011
Nonfamily firms	(0.020)	(0.166)	(0.018)	(107,502)	(0.018)	(0.004)	(0.016)	(0.029)

Panel E: Sample Cuts by Density of Non-eligible Firms

Dep. Var.	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(GrossProfit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Q1								
$Eligible_i \times Post_t$	0.028	0.254	0.017	0.022	0.019	-0.009	0.013	-0.032
	(0.018)	(0.157)	(0.025)	(0.021)	(0.019)	(0.006)	(0.016)	(0.028)
Observations	15,910	15,855	12,410	12,613	12,613	18,436	14,902	15,298
	0.377	0.170	0.289	0.292	0.314	0.192	0.304	0.235
Q2								
$Eligible_i \times Post_t$	0.029*	0.349**	0.052**	0.012	-0.011	0.010*	0.024	-0.040
	(0.016)	(0.163)	(0.022)	(0.019)	(0.022)	(0.006)	(0.018)	(0.033)
Observations	16,230	16,152	11,638	11,861	11,861	18,851	15,193	15,299
R-squared	0.378	0.163	0.286	0.351	0.324	0.194	0.275	0.201
Q3								
$Eligible_i \times Post_t$	0.034*	0.219	-0.015	0.012	0.030	0.023***	0.013	-0.026
	(0.018)	(0.167)	(0.022)	(0.017)	(0.022)	(0.007)	(0.016)	(0.024)
Observations	14,193	14,145	11,127	11,354	11,354	16,494	13,457	13,678

R-squared	0.380	0.197	0.310	0.323	0.301	0.178	0.291	0.243
Q4								
$Eligible_i \times Post_t$	0.036**	0.289*	0.017	0.045**	0.056**	0.003	0.043***	0.044
	(0.018)	(0.162)	(0.023)	(0.022)	(0.022)	(0.007)	(0.016)	(0.031)
Observations	14,037	13,983	10,789	11,009	11,009	16,475	13,300	13,530
R-squared	0.370	0.191	0.316	0.333	0.319	0.202	0.284	0.272
Difference: Q4 vs. rest	0.004	-0.027	-0.002	0.027	0.039	-0.009	0.021	0.067**
	(0.020)	(0.180)	(0.026)	(0.024)	(0.024)	(0.014)	(0.018)	(0.033)

The table presents results from estimating equation (1) using different subsamples, as specified on the top of each panel. 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 2009-2013. The 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 6: Placebo Tests Using Random Revenue Thresholds

Dep. Variable	(1) Average coefficient	(2) Non-rejection rate at 5% level
Δ ln(External debt)	0.035	2.33%
Δ ln(Internal Debt)	-0.018	0.00%
Δ ln(Trade Credit)	-0.020	0.00%
Δ ln (Issued Equity)	-0.015	0.00%
Δ ln(Fixed Assets)	-0.001	0.00%
Δ ln(Employees)	0.003	0.00%
Δ ln(Total Assets)	-0.046	0.00%
Δ ln(Current Assets)	-0.020	0.00%
Δ ln(Profit)	-0.003	0.00%
Δ ln(Revenue)	0.002	0.00%
Δ ln(Costs)	0.007	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.

Table 7: Robustness Checks Using Alternative Bandwidth

	(1)	(2)		(3)	(4)	
	Window [10M, 40M]			Window [13M, 37M]		
Dep. Variable	Coefficient	95% confidence interval		Coefficient	95% confidence interval	
Δ ln(External debt)	0.293	[0.170,	0.415]	0.306	[0.169,	0.443]
Δ ln(Fixed Assets)	-0.002	[-0.023,	0.018]	-0.004	[-0.028,	0.019]
Δ ln(Employees)	0.022	[0.011,	0.034]	0.020	[0.007,	0.034]
Δ ln(Profit)	0.020	[0.004,	0.035]	0.021	[0.004,	0.039]
Δ ln(Revenue)	0.025	[0.011,	0.039]	0.021	[0.005,	0.037]
Δ ln(Costs)	0.029	[0.014,	0.044]	0.023	[0.006,	0.040]

The table presents results from estimating equation (1) using different subsamples of companies with revenue levels within a bandwidth of £15M (Columns 1 and 2) and £12M (Columns 3 and 4) around £25M in 2008.The standard errors are presented in brackets and are adjusted for heteroskedasticity and clustered at the firm level.

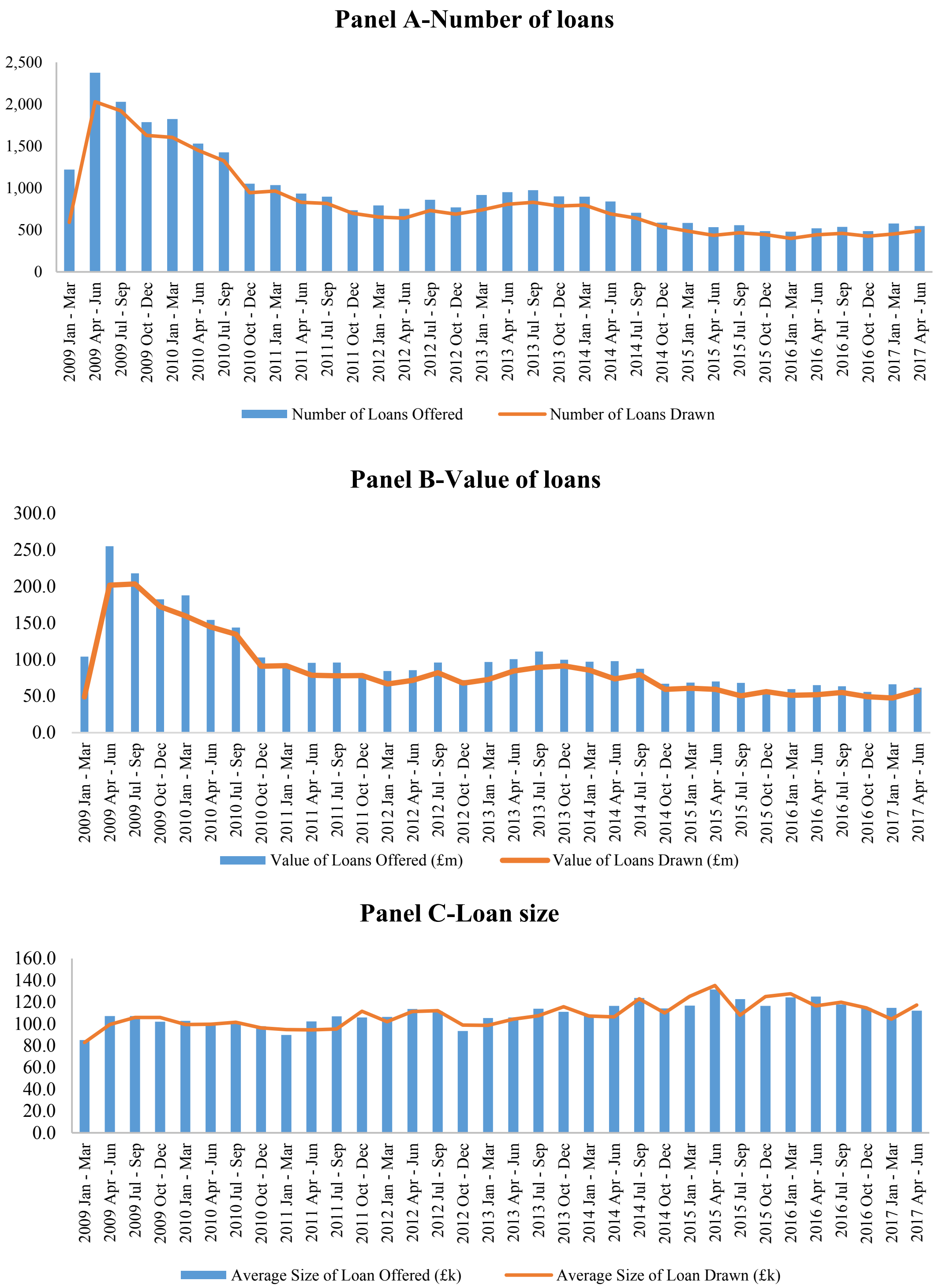
Table 8: Placebo Tests Using Firms in Non-eligible Industries

	(1)	(2)		(3)	(4)	
	Window [10M, 40M]			Window [13M, 37M]		
Dep. Variable	Coefficient	95% confidence interval		Coefficient	95% confidence interval	
Δ ln(External debt)	0.044	[-0.259,	0.347]	0.044	[-0.301,	0.389]
Δ ln(Fixed Assets)	-0.032	[-0.101,	0.036]	-0.074	[-0.155,	0.007]
Δ ln(Employees)	-0.045	[-0.076,	-0.013]	-0.034	[-0.070,	0.002]
Δ ln(Profit)	-0.030	[-0.091,	0.032]	-0.019	[-0.093,	0.056]
Δ ln(Revenue)	-0.004	[-0.064,	0.056]	0.006	[-0.064,	0.077]
Δ ln(Costs)	-0.045	[-0.113,	0.024]	-0.066	[-0.148,	0.016]

The table presents results from estimating equation (1) using different subsamples of companies with revenue levels within a bandwidth of £15M (Columns 1 and 2) and £12M (Columns 3 and 4) around £25M in 2008, but in non-EFG-eligible industries. The standard errors are presented in parentheses and are adjusted for heteroskedasticity and clustered at the firm level.

Appendix 1: Official EFG Statistics

Figure A1: Number and Value of EFG loans



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/

Table A1 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

Appendix 2: Predicting EFG Take-up

Table A2.1-Take-up Prediction

	D(ΔExternal Debt > 0)
Age	-0.002*
	(0.001)
Lag(Tangibility)	0.734***
	(0.054)
Lag Leverage	0.347***
	(0.052)
Lag Issued Capital	-0.000
	(0.000)
Lag Trade Creditors	0.000**
	(0.000)
Lag Total Assets	0.000
	(0.000)
Age^2	0.000
	(0.000)
Lag Leverage^2	0.097***
	(0.038)
Constant	-1.017***
	(0.193)
Observations	20,836
Pseudo R2	0.0792

This table presents the probit regression predicting the take up rate using firm level characteristics. The sample is based in year from 2004 to 2008, before the launch of EFG program. In the probit regression, 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

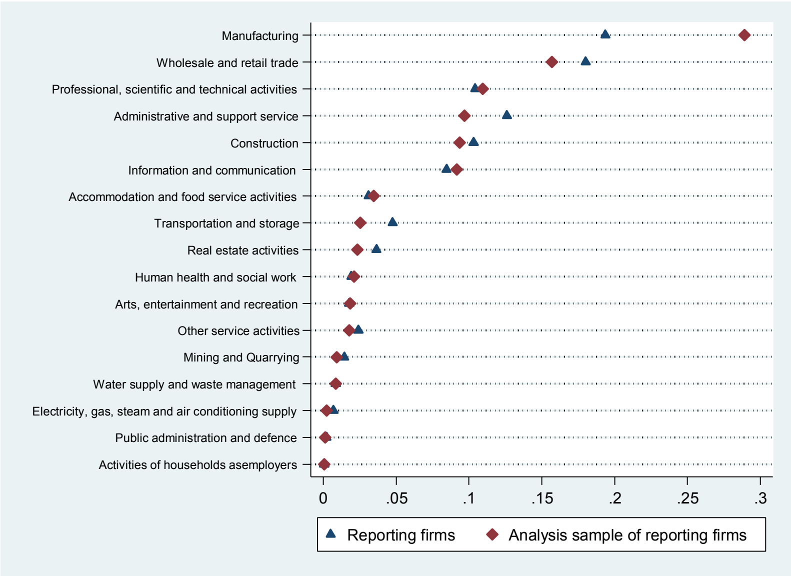
Table A2.2: Sample Cuts by Ex-ante Predicted Take-up Rate

Dep. Var.	(1) D(ΔExternal Debt > 0)	(2) Δln(External Debt)	(3) Δln(Profit)	(4) Δln(Revenue)	(5) Δln(Costs)	(6) Survival	(7) Δln(Employees)	(8) Δln(Fixed Assets)
Q1								
$Eligible_i \times Post_t$	0.021	0.147	0.013	0.017	0.015	0.009	0.013	-0.024
	(0.016)	(0.179)	(0.021)	(0.016)	(0.019)	(0.006)	(0.014)	(0.034)
Observations	12,817	12,782	10,287	10,129	10,129	14,675	12,236	12,428
R-squared	0.327	0.132	0.221	0.287	0.269	0.124	0.252	0.193
Q2								
$Eligible_i \times Post_t$	0.024	0.308*	0.018	0.001	-0.008	-0.005	0.041**	-0.063**
	(0.018)	(0.176)	(0.021)	(0.020)	(0.018)	(0.007)	(0.016)	(0.027)
Observations	12,520	12,452	10,266	10,048	10,048	14,457	11,859	12,116
R-squared	0.329	0.161	0.266	0.316	0.309	0.170	0.283	0.217
Q3								
$Eligible_i \times Post_t$	0.036*	0.285	-0.008	0.028	0.048**	-0.010	0.039**	0.000
	(0.019)	(0.176)	(0.022)	(0.019)	(0.022)	(0.007)	(0.019)	(0.026)
Observations	12,202	12,159	10,091	9,773	9,773	14,179	11,585	11,907
R-squared	0.344	0.195	0.322	0.343	0.317	0.186	0.285	0.234
Q4								
$Eligible_i \times Post_t$	0.073***	0.237	0.008	0.030	0.032	0.014*	0.031	0.034
	(0.022)	(0.167)	(0.021)	(0.021)	(0.021)	(0.008)	(0.020)	(0.028)
Observations	11,973	11,926	9,639	9,449	9,449	14,039	11,230	11,666
R-squared	0.368	0.229	0.351	0.316	0.324	0.197	0.284	0.302
Difference： Q4 vs. rest	0.044*	-0.076	0.005	0.020	0.021	0.019**	0.005	0.066**
	(0.023)	(0.188)	(0.023)	(0.023)	(0.023)	(0.008)	(0.021)	(0.031)

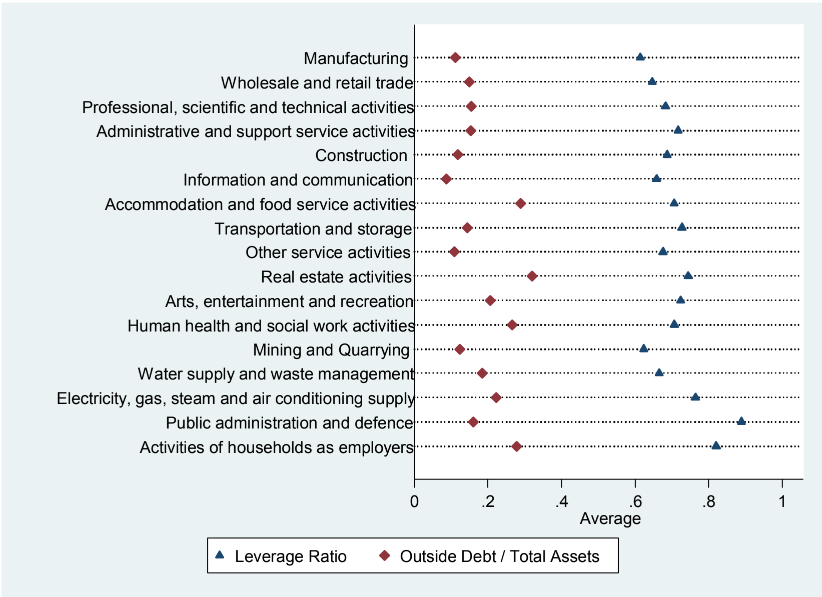
Appendix 3: Leverage Ratio by Industry

Figure A3: 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.

Appendix 4: Robustness Checks

Table A4.1 : Robustness checks: Collapsed Sample

Panel A—ITT Financing

	(1) D(ΔExternal Debt>0)	(2) Δln(External Debt)	(3) Δln(External Long Debt)	(4) Δln(External Short Debt)	(5) Δln(Internal Debt)	(6) Δln(Trade Credit)	(7) Δln(Hire Purchase Lease)	(8) Δln(Issued Equity)
$Eligible_i \times Post_t$	0.032***	0.280***	0.188**	0.236***	0.008	0.026	-0.064	0.001
	(0.008)	(0.082)	(0.076)	(0.082)	(0.075)	(0.016)	(0.064)	(0.022)
Obs.	14,922	14,922	14,922	14,922	14,922	14,026	13,476	12,046
R-squared	0.103	0.137	0.139	0.138	0.142	0.178	0.139	0.209

Panel B—ITT and IV Performance

Dep. Var.	(1) Δln(Profits)	(2) Δln(Revenue)	(3) Δln(Costs)	(4) Survival	(5) Δln(Profits)	(6) Δln(Profits)	(7) Δln(Revenue)	(8) Δln(Revenue)	(9) Δln(Costs)	(10) Δln(Costs)
Method	ITT	ITT	ITT	ITT	OLS	IV	OLS	IV	OLS	IV
$Eligible_i \times Post_t$	0.025**	0.029***	0.034***	0.004						
	(0.013)	(0.011)	(0.012)	(0.008)						
Δ(External Debt)					0.003	0.074*	0.009***	0.093**	0.008***	0.107**
					(0.003)	(0.042)	(0.002)	(0.043)	(0.003)	(0.048)
Obs.	11,692	11,874	11,874	15,282	11,692	11,692	11,874	11,874	11,874	11,874
R-squared	0.160	0.237	0.213	0.568	0.160	-0.226	0.239	-0.412	0.214	-0.524
First Stage										
Δ(External Debt)						0.353***		0.311***		0.335***
						(0.088)		(0.089)		(0.086)
F-stat excluded instrument						16.15		12.11		12.11

Panel C—ITT and IV Employment and Investment

Dep. Var.	(1) Δln(Employees)	(2) Δln(Fixed Assets)	(3) Δln(Productivity)	(4) Δln(Employees)	(5) Δln(Employees)	(6) Δln(Fixed Assets)	(7) Δln(Fixed Assets)	(8) Δln(Productivity)	(9) Δln(Productivity
Method	ITT	ITT	ITT	OLS	IV	OLS	IV	OLS	IV
$Eligible_i \times Post_t$	0.028***	-0.002	0.017*						

	(0.010)	(0.016)	(0.009)						
Δ(External Debt)				0.014***	0.101**	0.028***	-0.007	-0.002	0.055*
				(0.002)	(0.043)	(0.004)	(0.057)	(0.002)	(0.033)
Obs.	14,462	14,408	11,762	14,462	14,462	14,408	14,408	11,762	11,762
R-squared	0.174	0.153	0.189	0.185	-0.552	0.169	-0.011	0.189	-0.308
First Stage									
Δ(External Debt)					0.279***		0.292***		0.313***
					(0.082)		(0.081)		(0.090)
F-stat excluded instrument					11.66		13.01		12.16

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 2009-2013. 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 A4.2 : Robustness checks: Alternative sub-samples

Panel A: Firms with negative external debt changes

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Δln(External Debt)	Δln(Profit)	Survival	Δln(Employees)	Δln(Fixed Assets)
$Eligible_i \times Post_t$	0.075	-0.019	-0.001	0.016	-0.000
	(0.191)	(0.020)	(0.004)	(0.016)	(0.027)
Observations	12,906	14,867	20,110	18,130	18,421
R-squared	0.594	0.430	0.156	0.401	0.403

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

	(1)	(2)	(3)	(4)	(5)
Dep. Var.	Δln(External Debt)	Δln(Profit)	Survival	Δln(Employees)	Δln(Fixed Assets)
$Eligible_i \times Post_t$	0.406***	0.024**	0.011***	0.033***	-0.001
	(0.108)	(0.011)	(0.004)	(0.008)	(0.015)
Observations	35,443	40,154	61,365	49,536	50,335
R-squared	0.145	0.226	0.118	0.239	0.189

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 2009-2013. 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.