Credit Guarantees and New Bank Relationships*

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Abstract

Credit guarantee schemes for bank loans are at the heart of most Governments' strategies to help firms, and often direct vast volumes of credit. This paper examines Chile's credit guarantee scheme for bank loans to small and medium enterprises (SMEs), which is structured like many OECD countries' schemes. We use a regression discontinuity around the eligibility cutoff and find that credit guarantees have large positive effects on firms' total borrowing without large increases in default rates, in contrast to the (limited) existing evidence. The scheme also has an amplification effect: firms increase borrowing from other banks in the eighteen months following a loan guarantee. Moreover, we show that the guarantees are used to build new bank relationships, an important process which is not well understood in the literature. Finally, we show that firms use the credit increase to significantly scale up their operations. These results provide evidence that credit guarantees are an effective policy tool for both boosting credit availability, and for establishing new bank relationships for SMEs.

Keywords: Credit Guarantees, Bank Relationships, Bank Lending, Entrepreneurial Finance, Collateral, Small Business

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1. Introduction

"Helping [Small Businesses] expand — to get their ideas off the ground — is one of the best ways to support economic growth and needs the continued focus of both elected officials and the private sector... Securing financing remains a major barrier to growth... Small business owners overwhelmingly rely on banks for funding"

L. Blankfein, M. Bloomberg, W. Buffett, M. Porter, USA Today, June 7, 2016

Governments around the world continue to take action to tackle longstanding SME financing difficulties. Credit guarantees remain the most widely used instrument, with many countries expanding and introducing novel features to their credit guarantee programmes.

Ángel Gurría, OECD Secretary General, OECD (2016)

Small businesses are widely held to be credit constrained, and as a result Governments across the world have portfolios of programs to support their access to finance. Government Credit Guarantee Schemes (CGS) are the most common – and often claimed as the most effective – policy tool to increase lending to small firms (Beck et al., 2010; Beck et al., 2008). CGS repay lenders a proportion of a loan's principal in case of default, reducing the need for the borrower to post collateral, and they often cover vast volumes of credit: in 2014 Government CGS guaranteed loans equivalent to 5.7% of GDP in Japan and 4.1% in Korea, while the US's SBA guaranteed nearly 24 billion of loans in 2015 alone (OECD, 2016; Dilger, 2016).

Moreover, in reaction to the 2008-09 financial crisis, enlarged Government CGS were at the forefront of the effort to stimulate lending to firms.[1] However, despite their size and ubiquity, concerns remain regarding the effectiveness and value of CGS (e.g. De Meza, 2002; Green, 2003; Gropp et al., 2014). Using a unique data set of the

[1] The lending covered by these schemes expanded, often massively, in every OECD country except Austria between 2007 and 2010, and the increased importance of CGSs has persisted over time. The inflation-adjusted median value of small and medium enterprises (SME) credit guarantees rose by 45% in the OECD between 2007 and 2014. 26 OECD countries have CGSs as of 2014 (OECD, 2016).

universe of Chilean firms and a regression discontinuity design, this paper examines the effectiveness of a CGS with a very similar design to those in place in many OECD countries.

Ideally, a CGS would direct the guarantee towards creditworthy firms with positive NPV projects. Further, firms benefiting from the scheme would be credit constrained, so the CGS would improve the allocation of funds across firms, and create financial additionality, that is, loans covered by the guarantee would not have been made (or would have been materially smaller) in the absence of the scheme. Finally, increased credit access for credit constrained firms would lead to real effects as the firm scales up, such as higher levels of capital, employment, or sales.

However, the effects of CGS could instead be markedly different. Firstly, CGS could direct lending towards firms without positive NPV projects by exacerbating the firm-level moral hazard and adverse selection problems faced by banks, leading to high default rates and undermining the sustainability of the scheme. Secondly, CGS could be used to shift bad loans to the Government balance sheet (Uesugi et al., 2010). Further, guaranteed loans may be assigned to firms that are unconstrained and would have received loans in any case, resulting in rents for participants, and potentially no real effects of the scheme. Thus, whether and in what ways CGS work is an empirical question.

Unfortunately, it is difficult to identify the causal impact of CGSs on firms because those firms that receive a guaranteed loan are not randomly *selected*: the scheme may be more attractive to certain types of firms (applicant self-selection), and the bank or guarantee agency is also likely to have incentives to apply the guarantee to firms with characteristics unobservable to the econometrician (selection by the guarantee distributor). Thus, the firms that actually receive the guaranteed loans will differ from the remaining firms along an unknown number of dimensions, making the construction of an appropriate counterfactual group extremely difficult, and rendering the bias from estimations that do not fully resolve this problem potentially very large, and of indeterminate sign. This selection challenge has meant that there is surprisingly little evidence based on a robust identification strategy regarding the most basic question about such schemes – is there financial additionality? – and still less on the real or other financial effects of CGS on borrowers.[2]

This paper overcomes the obstacles posed by selection bias and the absence of an appropriate control group by examining Chile's FOGAPE Credit Guarantee Scheme in 2011 and 2012 using a regression discontinuity design (RDD) together with a comprehensive administrative data set covering all the firms in the economy. FOGAPE has an average guarantee rate of almost 80% of the loan principal. Participating private banks choose which of their borrowers' new loans receive a guarantee, and perform all the credit screening, monitoring, and, if necessary, debt collection functions. The RDD compares firms that just missed out on being *eligible* for FOGAPE with firms that are just eligible. To do so, we make use of the fact that the variable determining a firm's eligibility is extremely opaque to both firms and banks, and is costly to manipulate, and so whether a firm is eligible for FOGAPE in a given month is plausibly random in a narrow range around the eligibility threshold.

Intuitively, firms are as-if-randomly assigned around the eligibility threshold, which naturally generates two groups free of selection bias: a "treatment group" of all the eligible firms near the threshold, and a "control group" of all the firms that narrowly missed being eligible. Comparison of the two groups, coupled with a rich dataset on the population of firms near the threshold provides a clean causal estimate of the impact of eligibility for FOGAPE on firm-level outcomes, because no selection bias is possible

[2]Udell (2015) writes: "SME loan guarantee programs are globally ubiquitous and countries have invested significantly in them... Unfortunately, it is my sense that academic research on the effectiveness of these programs has not matched their policy importance."

– *all* the nearby firms are in the data, not a selected subset. Moreover, the RDD permits a rigorous examination of whether firms are as-if-randomly assigned around the threshold.

We then move to estimate the effect on firms that actually receive a guarantee (referred to as *treated* firms henceforth), as opposed to the effect on all eligible firms, because the majority of eligible firms do not receive a guarantee. This is not direct, though, because reciept of treatment is endogenously chosen by both firms, which choose to apply for a loan, and by banks, which decide which of these firms receive the guarantee. This two-sided choice means that a simple comparison of treated and untreated firms would be subject to a double selection bias, so we use eligibility as an instrument for treatment in the region around the threshold, allowing the use of a fuzzy RDD design.[3]

Our estimates indicate that FOGAPE provides substantial financial additionality: treated firms approximately double their total bank debt. Moreover, debt at *other banks* also rises steadily over the subsequent year, and is causally attributable to the guarantee scheme, which we term the *amplification effect*. To our knowledge, this amplification effect is a novel empirical result. The mechanism behind the amplification effect may be that FOGAPE generates a positive information externality, whereby banks that did not assign a FOGAPE guarantee to the firm positively update their priors regarding the borrower's creditworthiness on observing increased leanding by the FOGAPE-granting bank (they are probably unaware the lending is guaranteed), and so lower their screening requirements for lending. For lending increases that arise over the year following FOGAPE treatment, banks also observe firms' recent history of non-default, which would also inform banks' estimate of firm creditworthiness.[4] Alternatively, the amplifi-

[3]This uses only the part of the variation in treatment that covaries with eligibility (the instrument) to estimate the effect on "compliers" in the region around the threshold, that is, firms that receive the guarantee only if they were eligible, and do not receive it otherwise (Angrist et al., 1996).

[4]An alternative mechanism might drive the aplification effect if FOGAPE frees up a fraction of

cation effect could be driven by an increase in firm size and net worth produced by the intial FOGAPE loan, and the subsequent firm scale-up.

An important and novel finding of this paper is that FOGAPE is used by firms and banks as a bridge to building new (or developing recently established) bank relationships. Rajan (1992) argues that while having few bank relationships has important advantages, it exposes the firm to the risk of hold-up by the lender because firms seeking an additional relationship are viewed as lemons. In the Detragiache et al. (2000) model firms with few bank relationships are more vulnerable to premature and suboptimal liquidation as a result of negative shocks to banks. In the light of the empirical literature on the transmission of negative bank capital and liquidity shocks to borrowers (for example, see Peek and Rosengren, 2000; Paravisini, 2008; Iyer and Peydro, 2011; Schnabl, 2012; Chodorow-Reich, 2014) it seems clear that the value to firms of having more than one established bank relationship, especially in crisis periods, is likely to be substantial. Further, Cahn et al. (2017) provide suggestive evidence that firms with only one bank relationship have limited access to credit. However, we know relatively little about how additional bank relationships are established except that switching costs must be high, given the costs imposed on firms by asymmetric information (Santos and Winton, 2008; Ioannidou and Ongena, 2010; Darmouni (2016)). We know even less about how policy might influence this process.[5]

We find that 24% of treated firms near the eligibility threshold have no debt with the bank that gives them a guaranteed loan twelve months before, and a further 10% have only very small loans from that bank throughout the year preceding the FOGAPE loan.

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the firm's collateral, which can be used to borrow from other lenders. However, conversations with participating banks suggest this is unlikely, because, they say, banks are generally unwilling to release existing pledged collateral, even if it is no longer required because of a Government guarantee. Further, when a firm fully pays off its loans from a bank it generally takes around six months for the collateral to be released.

[5]Calomiris and Himmelberg (1993) note that the Japan Development Bank historically emphasized its role as a "pump primer" to infant industries, which involved directed credit with the aim of helping constituent firms develop creditworthiness.

Thus, around a third of the FOGAPE loans in our sample are used to establish a new, or to develop a fledgling banking relationship. With our RDD framework we establish that FOGAPE causes firms to increase their number of bank relationships around the time the loan guarantee is granted, and in addition, this process extends over the eighteen months following the FOGAPE guarantee - that is, well after the guaranteed loan is made. Thus, the new relationships are not just with the FOGAPE-granting bank, but also with other banks - an amplification effect for bank relationships (an extensive margin) as well as for debt (the intensive margin). This not-directly-monetary benefit is potentially comparable to the alleviation of credit constraints we document in terms of its value to firms.

A natural concern in response to evidence of financial additionality is whether CGS distort incentives for borrowers and lenders, which could increase default rates enough to reduce overall welfare. The collapse of many CGS in the 1980s and 1990s due to unsustainable default rates makes clear that this is not a solely theoretical concern. Moreover, a recurring result in the extant literature on CGS is that firms are more likely to default after participating in a CGS (for example, Lelarge et al., 2010, and Uesugi et al., 2010). Unfortunately, data limitations reduce our ability to bring evidence to bear on this issue, because default events generally happen over a year after loan is granted, and our default data only extend to October 2013 - almost three years after our first treatment month, but only ten months after our last. In our RDD framework we do not find evidence of a large increase in defaults relative to controls. However, we find suggestive evidence that defaults are higher for treated firms than for untreated firms starting nine months after the loan is granted, meaning that there may be a moderate default effect that is masked by the low power of the experiment for detecting differential default. In addition, when we look far from the threshold at the smallest firms receiving FOGAPE we find a small increase in firms' default rate on loans from the FOGAPE-granting bank relative to loans from other banks to the same firms.

FOGAPE has been financially sustainable over time is well-suited for study because its relatively simple structure means it can be readily implemented elsewhere, and because many other countries' schemes are of similar design.[6] This means that the results we report for FOGAPE are of direct relevance to OECD countries, given their enduring interest in expanding credit to small business, the relatively large SMEs studied (firms with annual sales of around a million US dollars) and because Chile's developed financial sector makes comparisons with developed economies appropriate.[7]

Whether the recipients of FOGAPE guarantees were financially constrained ex ante is critical in evaluating the efficacy of the program, and also for the relevance of CGS in alleviating the credit constraints that are central to many models of business and credit cycles.[8] We provide evidence that the recipients of bank credit guarantees were constrained in the sense of wanting to borrow more at the bank interest rate, and being unable to do so. This is because, while we do not have data on loan interest rates, major participating banks' policies did not reduce the interest rate charged on guaranteed loans in this period, or change the loan maturity, relative to non-guaranteed loans. Given that we see large increases in total bank debt resulting from the guarantee, this indicates that these firms were credit rationed at the bank interest rate.

Importantly, this increase in bank debt cannot be explained by a shifting of liabilities away from likely more expensive non-bank funding sources, for which we do not have data. This story does not fit with the real effects we report: treated firms scale up,

[6]For example, the US Small Business Administration's 7(a) loan guarantee scheme is very similar, but imposes additional eligibility requirements on recipients.

[7]"The financial system is large, well diversified, and highly integrated into the global financial system. . . Banks are well capitalized (in terms of both quantity and quality of capital) and profitable" (IMF, 2011). "Chile's financial system is now well-developed by emerging market standards, and even by the standards of many OECD members" (OECD, 2011)

[8]For example, (Bernanke and Gertler, 1989); Kiyotaki and Moore (1997); Holmstrom and Tirole (1997)

growing their sales, input purchases, and number of employees. Moreover, treated firms increase their borrowing at banks that are not giving them FOGAPE - the amplification effect. Thus, both the real and the amplification effects are inconsistent with recipient firms being financially unconstrained ex ante.

In conclusion, we show clear causal evidence of the effectiveness of a CGS: direct financial additionality, an amplification effect at other banks, the development of new banking relationships, and a general increase in the size of the firm. The World Bank's Enterprise Survey routinely reports that large subsets of firms view access to finance as a barrier to growth (Kuntchev et al., 2013). Banerjee and Duflo (2014) provide clear evidence of financial constraints for firms with sales of around US $200,000 in India, a substantially poorer and less financially developed country. We also find evidence consistent with significant financial constraints for firms with sales of around $1m USD in Chile, a middle-income OECD country with a well-developed financial system. This convergence in well identified evidence using micro-data for non-public firms suggests that firm credit constraints may be widespread and associated with intrinsic features of SMEs rather than with characteristics of the financial system.[9] Importantly, the 2011-2012 period we consider was not a time of material financial stress for the Chilean economy. However, evidence is mounting that small firms (and young firms) are especially vulnerable to credit contractions (see Khwaja and Mian, 2008; Fort et al., 2013; Iyer et al., 2014; DeYoung et al., 2015). In turn, this means that the positive effects of the FOGAPE credit guarantee scheme reported here are likely a lower bound of the true effects for small and medium-sized firms over the business cycle.

The remainder of the paper is structured as follows. Section two provides an overview of the logic of credit guarantee schemes and of the related literature. Sec-

[9] A large body of work measures the existence of firm financial constraints in the US (for example Hadlock and Pierce 2010; Hoberg and Maksimovic 2014; Krishnan et al. 2014; Petersen and Rajan 2002).

tion three describes the data and empirical method. Section four presents the results, and section five concludes.

2. Credit Guarantee Schemes

2.1. The logic of credit guarantee schemes

A natural starting point is to consider why a CGS might be justified – why might positive NPV loans remain un-funded in the absence of a government guarantee of bank loans? There is extensive theoretical work on why small firms are particularly affected by credit constraints; the most well-examined answer is the existence of especially acute asymmetry of information between banks and small firms.[10] Further, even in the absence of information asymmetries, non-contractible effort or non-verifiable income at the firm could lead to firms being fully or partially rationed out of the market – Tirole (2006) presents several models of agency costs that lead to credit constraints.[11]

By contrast, the posting of collateral can provide an escape from credit rationing by: (i) inducing self-screening by firms, because only high NPV borrowers are willing to provide good collateral; and (ii) increasing effort and reducing strategic default by making default more costly for the borrower. As a result, a second reason why small firms may be credit constrained is that some firms lack sufficient collateral due to some

[10] For example, in Stiglitz and Weiss (1981) adverse selection and moral hazard mean that banks cannot adjust their interest rates upwards to fully compensate for the high baseline risk of SME borrowers, and credit rationing occurs in response. Higher interest rates could increase the riskiness of the borrower pool by, for example, (i) disproportionately dissuading borrowers with low risk projects; or inducing borrowers to (ii) select higher risk projects; (iii) reduce effort; or (iv) strategically default more often ex post.

[11] Lending relationships (Rajan, 1992; Petersen and Rajan, 1994; Berger and Udell, 1995) mitigate information asymmetry over time by developing soft information (Stein, 2002; Liberti and Petersen, 2017) and thus learning about fixed dimensions of borrower quality (Botsch and Vanasco, 2015). While relationships reduce the degree of credit rationing, relationship lending is still relatively costly, and the cost of post-loan monitoring is raised by the opacity of small borrowers. Of course, relationship lending is not a solution for new borrowers.

combination of wealth constraints, asset intangibility, and inability to pledge future cash flows. Even firms with substantial collateral may find themselves credit rationed if the collateral process (registration, enforcement, contracts, bankruptcy) works poorly, or if the cost of immobilizing collateral is high.[12]

A third source of small firm credit constraints is that the transactions cost of lending to SMEs is high relative to the size of their loans, and largely fixed. This makes for a low profit margin in comparison to larger borrowers: the smaller the prospective loan, the higher the likelihood that this will result in denial of credit, ceteris paribus. Transactions costs are high in lending to new borrowers because the information available to banks is limited, often of low quality, scarce (in the case of young firms), or costly to process due to the wide heterogeneity of SMEs, which makes automation difficult. The cost of evaluating a potential borrower is largely fixed, because it mainly consists of specialized loan officer time.

Credit guarantee schemes are a potential solution to some – but not all – of these inefficient small firm credit constraints. Because CGS in general do not generate additional information about borrowers, they do not reduce information asymmetry problems, and may instead exacerbate them, as discussed below. However, CGS do serve to increase lending to firms denied credit because they lack collateral (or because collateral institutions function poorly) by serving as a substitute for firm collateral. Furthermore, CGS are likely to reduce the impact of high and fixed transactions costs of lending to SMEs by reducing the risk to the lender, and so increasing expected profit. Given that the asymmetric information, transactions cost and limited collateral problems are more severe for young firms – they generally start small – and young firms are uniquely important for innovation and productivity (Haltiwanger et al., 2012), the social cost of

[12] Collateral is especially costly when the value of small firm collateral falls (e.g. in real estate downturns), because firms are then asked to put up new collateral or face immediate credit reductions. Also, see Tirole (2006) section 4.3.3 for a review of the deadweight costs of collateral.

credit rationing the youngest and smallest firms may be especially high.

However, CGS are not a panacea: costs may outweigh benefits, and it is, crucially, still unclear whether they are effective at all in meaningfully expanding lending. Government programs involve deadweight losses, incentive distortions, and potential agency problems due to political influence on lending (e.g. Khwaja and Mian, 2005; Zia, 2008; Banerjee and Duflo, 2014; Bhue et al., 2017). More directly, CGS may lead to riskier lending than average by worsening adverse selection and moral hazard problems, for example by directing lending towards firms without positive NPV projects by encouraging excessive entry (De Meza, 2002), or by reducing banks' incentives to screen (Gropp et al., 2014). Further, CGS could be used to shift bad loans to the Government's balance sheet – unsustainable default rates were a feature of many CGS in the 1980s and 1990s. Alternatively, guaranteed loans may be assigned to firms that are unconstrained and would have received loans in any case, resulting in rents for banks or firms, and no financial additionality. Thus, whether and how CGS work is an empirical question.

Finally, CGS are often temporarily enhanced in response to periods of macroeconomic or financial stress OECD (2013). Strong evidence exists that SMEs are disproportionately affected by changes in macroeconomic variables such as monetary policy and the business cycle (for example, Gertler and Gilchrist, 1994), and particularly by credit crunches (Khwaja and Mian, 2008; Fort et al., 2013; Iyer et al., 2014, DeYoung et al., 2015), making CGS more likely to be effective (and welfare enhancing) in such periods. This paper examines the effectiveness of a CGS in the absence of such stresses, which provides a sterner test of their value.

2.2. Existing literature on Credit Guarantee Schemes (CGS)

Many studies examine the impacts of credit guarantee schemes because of the large number of existing programs (Beck et al., 2010). Unfortunately, estimating the causal impact of CGSs on firms is very difficult, because firms that receive a guaranteed loan are not randomly selected. For example, the scheme may be more attractive to certain types (for example, risky or politically unconnected firms), leading to applicant self-selection. Further, the bank or guarantee agency often has incentives to apply the guarantee to firms with characteristics unobservable to the econometrician, leading to selection by the guarantee distributor. The interaction of these two sources of selection is complex. Thus, the firms that receive guaranteed loans differ from the remaining firms along an unknown number of dimensions, making the construction of an appropriate counterfactual group extremely difficult. As Gozzi and Schmukler (2015) note in an overview of CGS: *"rigorous evidence on the impact of these schemes is still scarce."*

Lelarge et al. (2010) provide perhaps the strongest evidence to date of the effects of a CGS by exploiting the new eligibility of certain industries to the French CGS in a difference in difference framework paired with a selection model. Our study differs from theirs in two key respects. Firstly, the plausibly exogenous variation they exploit is industry-level, whereas we exploit firm-level variation, allowing for greater power and a cleaner experiment. Secondly, their focus is on a CGS for newly established firms only, whereas there is no constraint on the ages of the firms we examine, making for potentially greater external validity. They find evidence of financial additionality and higher ex post firm growth rates, but also higher ex post bankruptcy rates for treated firms.

Lelarge et al. (2010) are not the only study to show a dark side to CGS. Uesugi et al. (2010), using a matching estimator that rests completely on the strong assumption that all selection is controlled for by variables they observe, show alarming evidence of Japan's CGS being used to shift bad loans on to the books of the government on a massive scale. Different but also clearly welfare-reducing effects are reported by Zia (2008) in the context of subsidized export credit in Pakistan, where nearly half of the subsidized loans went to financially unconstrained firms. In addition, Cowan et al. (2015) examine the early years of FOGAPE (2003 to 2006) using a fixed effects empirical design, and find that (i) FOGAPE clients were *not* more likely to default on their (guaranteed) FOGAPE loans than on their non-FOGAPE loans; but also (ii) FOGAPE clients defaulted more than non-FOGAPE firms, which they argue suggests that such firms are lower quality borrowers. Cowan and co-authors also use bank-level variation in access to guarantees to estimate positive financial additionality at the aggregate level. Our study has access to much more detailed data, and most importantly to exogenous variation at the firm level, allowing us to examine the causal effects of FOGAPE directly, and to examine many more dimensions of the program's effects on firms. Among the few papers finding largely positive effects, Hancock et al. (2007) take a wider perspective, and examine the effects of US Small Business Administration (SBA) loans on state-level outcomes. They report that SBA loans were less pro-cyclical and less affected by bank capital than non-guaranteed loans, suggesting a stabilizing effect of the SBA loan programme on SME outcomes.

3. Data and Empirical Strategy

3.1. Data

This paper uses a new data set that links banking and tax information for the universe of Chilean firms. The data are the administrative records from Chile's Internal Revenue Service, Unemployment Insurance Administrator, and Bank Supervisory Authority, and these institutions use the information for their own auditing or supervisory activities, which means that this is high quality data. Datasets are merged using a unique identifier equivalent to a social security number for both individuals (for unemployment insurance) and firms. The panel covers 2005 to mid-2013, although some datasets extend further.

Tax records from Chile's IRS are both annual (income tax declaration form) and monthly (value-added-tax declaration form), and cover all firms in Chile. Importantly, tax records provide monthly records of all firms' VAT eligible purchases and sales. Unemployment insurance data are monthly, and identify the number of workers at each firm, their wage, tenure and contract type. Unemployment insurance contributions are mandatory for all salaried workers in the private sector in Chile, so our data covers the universe of employer-employee matches for salaried jobs in the formal private sector. Banking data includes the universe of bank-firm pairs and contains the stock of outstanding debt for each pair in each month, as well as the debt's non-payment status in several categories. Loan characteristics such as the interest rate or maturity are unfortunately unavailable, as is a trustworthy investment variable.

Finally, the FOGAPE administrator provided the IRS with a variable (along with the appropriate identifier) indicating which firms had received a guaranteed loan, the date they received it and the granting bank. We separately obtained from FOGAPE a database containing detailed data on all FOGAPE loans, including date, amount, maturity, anonymized bank and firm identifiers, among other characteristics. However, due to legal constraints they were unable to share this in a way that would permit merging with the IRS-Unemployment Insurance-Banking data used throughout the paper.

3.2. Sample

We begin with the Chilean IRS's definition of a firm: all corporations, and individuals if they pay the corporate income tax or are an employer of another person. This very broad definition therefore includes many nonproductive "shell" firms and single-worker firms. To deal with this, we drop all firms that do not have at least one employee continuously – at the monthly frequency – for 12 months before and one month after the focal month, which is the month the guaranteed loan was made. This restriction drops many firms that appear intermittently in the data and distort firm dynamics. We then drop all firms that had less than three employees on average in the twelve months before the focal month to avoid results being driven by extremely small firms. In order to remove firms that are non-participants in the banking system we also drop firms whose average total bank debt during the six months before the focal month is less than 100 UFs, (approximately $4,000 USD).[13] Finally, we drop all firms in the financial sector to avoid potential double counting in the banking data, and firms in the Public Administration and Defense sectors.

The sample period is January 2011 to December 2012 inclusive. We begin in January 2011 and not before because FOGAPE's funding and rules changed substantially in the 2009-2010 period, making it unrepresentative of the scheme's normal operations, to which it returned in January 2011. We also focus the sample around the sales cutoff in eligibility for FOGAPE, as detailed in the next section.

3.3. FOGAPE's eligibility rules

For a firm to be eligible for FOGAPE credit guarantees the sum of its sales over the preceding twelve calendar months must not exceed 25,000 UF (approximately US 1 million), and it must be a borrower of "normal" risk according to the bank's internal rating. Sales are determined by a complex formula that is not public: banks (not customers) can query whether a potential customer is FOGAPE-eligible or not on a given day via a private web system that checks against an IRS database, but they do not see the value of the rolling twelve month sum, only an eligible/ineligible indicator.

[13]Unidad de Fomento (UF) is the inflation indexed unit of account widely used in Chile, worth approximately US $40 in 2011-12

Moreover, the database is of monthly VAT declaration forms that are uploaded into the web-query system with a lag of approximately three months.

The opacity of the sales formula, and the lag in updating the database, makes it hard for firms in the vicinity of the 25,000 UF eligibility threshold to anticipate on which side of the cutoff they will be at any given point in time. This opacity and uncertainty will be of value for the empirical strategy, as will be discussed in the following section.

Finally, two details of how FOGAPE operates are worth mentioning here. Firstly, in our period the guarantee rate, while set in a competitive auction, was very close to the maximum of 80%. Secondly, banks – not firms or FOGAPE itself – decide which firms receive a loan guarantee. For more details regarding how FOGAPE works we refer the reader to Cowan et al. (2015).

3.4. Empirical strategy

We estimate the causal effect of credit guarantees on firm outcomes using a regression discontinuity design (RDD). The intuition behind the RDD is as follows. Firms in a narrow bandwidth around the eligibility threshold are quasi-randomly assigned to be on either side of the 25,000 UF sales threshold of eligibility in any given month, because they do not know the value of the sales that the Chilean IRS is using to compute their eligibility status. In the language of the RDD, the assignment variable (the IRS's twelve month sum of sales) is not visible to the firms or to the banks (which is somewhat unusual in a RDD), but is available to the econometrician. When banks query the eligibility system they only observe whether the firm is eligible, while firms cannot query the system themselves.

This means that firms on one side of the cutoff are well suited to be controls for firms on the other side, because the only dimension along which they differ systematically is in their IRS sales measure, which in turn determines whether or not firms are eligible for

FOGAPE, and so can receive treatment. The difference in outcome variables between eligible (sales below 25,000) and ineligible firms (sales above 25,000) is what the RDD estimator measures.

The RDD's suitability for causal inference derives from the relatively mild assumptions it requires. RDDs rely on a key assumption of *imprecise* control Lee and Lemieux (2010) which is that companies cannot precisely control their IRS sales value, and thus cannot choose to be eligible for FOGAPE with certainty. This assumption implies that, in the absence of the eligibility threshold the outcomes of firms just below the cutoff and those above would have been similar, so the only reason that the actual outcomes are different is that some firms below the threshold are assigned FOGAPE guaranteed loans.

While it is true that firms could reduce their sales to such an extent that they could be certain to be eligible, this would be costly to the firm, given the uncertainty they face with regards to how far away they are from the threshold, and because if a firm needs credit, delaying sales is especially costly. Alternatively, the firm could attempt to delay the tax reporting of the sales. However, these sales are subject to value added tax (VAT), which has a built-in incentive structure that generates a third-party reported paper trail to facilitate tax enforcement, so delayed reporting would require active cooperation from the firm's customers, if they are firms themselves (for evidence that Chile's VAT enforcement is strong, see Pomeranz, 2015). In any case, whether firms are manipulating their reported sales is explicitly testable in our data by examining the density of observations on either side of the threshold using the McCrary (2008) test, as described below, and also by comparing the time paths of monthly sales of firms receiving loan guarantees with those of firms receiving loans without guarantees – they are identical, and show no evidence of sales being delayed, suggesting no manipulation.

The assumption of imprecise control over firms' IRS sales implies firms are randomly assigned to eligibility for FOGAPE. Unlike an instrumental variable's exclusion restriction, this assumption has three main testable implications: 1) Observed pre-determined characteristics should be identically distributed on either side of the sales threshold; 2) the density of firms on either side of the threshold should be the same; 3) RDD estimates estimate should not vary materially when we include baseline covariates, as these are not required for consistent estimation of the treatment effect. We provide evidence for the first two in the following section, and for implication three in the results section.

The first and most direct specification we estimate is the reduced form comparison of all firms in the bandwidth and below the cutoff, with all such firms above the cutoff. This is an unbiased estimator of the average effect of being eligible for FOGAPE – not necessarily receiving treatment – in the region around the threshold. We pool all firm-month observations, including year-month fixed effects, and estimate the following regression, where ρ is the coefficient of interest:

$$Outcome_{it} = c + \rho Eligible_{it} + \gamma_1 Sales_{it} + \gamma_2 EligibleXSales_{it} + \delta_t + \epsilon_{it} \qquad (1)$$

Where i indexes firms, and t indexes months. Eligible is an indicator equal to one if the firm's IRS sales are below 25,000 UF. Sales is our observed firm IRS sales value for that month – the assignment variable – minus 25,000 to center the data on the cutoff. Equation 1 is estimated on a bandwidth of 1,500 UF to either side of the cutoff (i.e. a bandwidth of∼\$60,000 US around a cutoff of ∼\$1 million), and corresponds to a local linear regression discontinuity estimate with a uniform kernel. Reduced form estimates should be interpreted as the causal effect of FOGAPE eligibility on the outcome variables. That is, the average effect of the program across the subpopulations present around the eligibility threshold (i.e. "compliers, always takers and never takers" in theAngrist et al. (1996) terminology) .

We then move to estimate the effect of actual treatment, as opposed to eligibility. For this we estimate a Fuzzy not a Sharp RDD for two reasons. Firstly and most importantly, not all eligible firms are treated, because banks do not have unlimited guarantee funds and so must choose which firms to assign the guarantee to. Further, a substantial fraction of firms above and below the eligibility threshold have no demand for credit on any given month, which materially reduces the frequency of treatment (above and below the threshold). Figure 1 presents the local averages of treatment probability around the cutoff. Secondly, some treated firms appear to have sales above the threshold, which would make them ineligible. After extensive conversations with officials managing FOGAPE, we are certain this is due to measurement error. In particular, the lagged updating of the assignment variable (IRS sales) means that the sales value that was visible at the time on the web query system is being mis-assigned in our data to the month before or the month after treatment.[14]

Fuzzy RDD resolves both the slight mismeasurement of the assignment variable, and more importantly, the endogenous selection of the firms that receive FOGAPE by the banks. In particular, for fuzzy RDD we use the observed value of the assignment variable to generate an eligibility indicator, which is in turn used as an instrument for actual receipt of treatment. Thus, while the reduced form RDD provides estimates of the impact of FOGAPE eligibility, the fuzzy RDD estimates the effect of actual treatment i.e. receiving a guaranteed loan. Fuzzy RDD estimates should be interpreted as the average causal effect (LATE) on the subpopulation of compliers, that is, firms that always receive a (guaranteed) loan when they are eligible for FOGAPE, and never otherwise.[15]

[14] Additionally, there are a very small number of exporters using FOGAPE that are not subject to the 25,000 UF cutoff, but that appear in our data as recipients of treatment, when in reality the treatment they received was different.

[15] Fuzzy RDD requires two further assumptions to those required by sharp RDD (Hahn et al., 2001), which seem extremely likely to hold in our setting. The first is monotonicity (i.e. crossing the cutoff cannot cause some units to take up the treatment and others to reject it) and the second, excludability

We implement the fuzzy RDD following the standard procedure using instrumental variable estimation. As before, we pool all firm-month observations, including year-month fixed effects, and estimate the following instrumental variables regression, where β is the coefficient of interest:

$$Treatment_{it} = c + \gamma_0 Eligible_{it} + \gamma_1 Sales_{it} + \gamma_2 EligibleXSales_{it} + \delta_t + u_{it} \quad (2)$$

$$Outcome_{it} = a + \beta Treatment_{it} + \phi_1 Sales_{it} + \phi_2 TreatmentXSales_{it} + \eta_t + \nu_{it} \quad (3)$$

Where i indexes firms, and t indexes months. Equation (2) is the first stage: Treatment is an indicator equal to one if the firm receives a guaranteed loan; Eligible is an indicator equal to one if the firm's IRS sales is below 25,000 UF, and is used as an instrument for treatment. As for the reduced form estimate, the specification is estimated on a small bandwidth of 1,500 UF (relative to the 25,000 UF cutoff), with a uniform kernel and robust standard errors, following standard practice: local linear regression fuzzy RDD.

Using a wider bandwidth provides additional statistical power at the cost of introducing greater bias, because the RDD's randomization result is local: as one moves away from the cutoff it becomes increasingly less true that the firms on either side are similar ex ante. An alternative is to use a wider bandwidth and to control for increasing heterogeneity on either side of the boundary using a flexible polynomial function. However, in their benchmarking of the RDD against experimental data, Black et al. (2007) report that local linear regressions have lower bias and less specification-sensitivity than polynomial regressions. Accordingly, the local linear regression above is the preferred specification throughout. The bandwidth choice of 1,500 UF of sales (~US $60,000) on either side was chosen for simplicity and comparability of the sample across estimates,

(i.e. crossing the cutoff cannot impact the outcome variable other than through impacting receipt of treatment).

but we show estimates are robust to both smaller and larger bandwidths (as well as polynomial functions with larger bandwidths).

The above specification includes year-month fixed effects. While fixed effects (of any type) are not required for consistent inference in the RDD, they mitigate concerns that certain months may be different from others. In robustness checks, we show that we obtain similar results if we vary the bandwidth size, if we include a variety of covariates, or if we remove the controls for the assignment variable, and instead control non-parametrically for the assignment variable by making the bandwidth extremely small (e.g. 500 UF or 750 UF). It is worth recalling, however, that a valid RDD with a local linear specification and a small bandwidth – our main specification – does not require the inclusion of covariates beyond the assignment variable for identification or consistency, and is not subject to omitted variable biases.

3.5. Tests for Quasi-Randomized Assignment

Our identification strategy relies on quasi-random assignment to eligibility for the treatment (i.e. a FOGAPE guaranteed loan). As mentioned in the previous section, this assumption has testable implications, akin to the tests of effective randomization in experimental data.

The first testable implication is that the distribution of the assignment variable should not exhibit any bunching around the discontinuity, as this constitutes prima facie evidence that firms can manipulate their value of the assignment variable, suggesting a violation of the key assumption of imprecise control. Figure 2 does not provide evidence of bunching. Furthermore, we perform the McCrary (2008) and Cattaneo et al. (2016) tests for discontinuities in the density of the assignment variable: IRS sales.

Given our empirical design where we observe the same eligibility experiment repeated every month, the first test is run for every month from 2011 to 2012. Panel A of

Table 1 shows that the average monthly t-statistic for the McCrary test of discontinuity in the density around the eligibility threshold for a bandwidth of 10,000 UF at each side is -1.09. For a larger bandwidth with values of the running variables ranging from 20,000 UF to 45,000 UF it is -1.1; none are statistically significantly different from zero at any conventional level. The second generation manipulation test of Cattaneo et al. (2016) is presented in Panel B of Table 1. For both uniform and triangle kernels all versions of the test cannot reject the null of a continuous density across the threshold: in the theoretically preferred, highest power version of the test the p-value is 0.92. In short we are unable to reject the null of continuity of the density function around the eligibility threshold, suggesting firms are not manipulating their reported sales in order to become eligible for the program.

The second testable implication is that firms to the left and to the right of the cutoff should be similar on the basis of ex ante characteristics. If they differ, then the treatment would not appear to be randomized and we would infer that companies are able to predict their eligibility and sort themselves accordingly. In Table 2 we present summary statistics for a series of covariates several months before the focal month, separately below and above the eligibility cutoff. The difference-in-means test provided in the last column of each summary statistics table confirms that the average difference in each characteristic across the bandwidth is statistically insignificant, except for those correlated with size, which is to be expected given the assignment variable determining eligibility is firm sales. For all dependent variables of interest we also run RDD tests in periods before the focal month and report them in the corresponding tables: there are no differences across the threshold for periods earlier than three months before the focal month, which is again consistent with quasi-randomized assignment of firms to either side of the threshold.[16]

[16]Finding statistically significant effects in the three months immediately preceding the focal month is to be expected, because the assignment variable is constructed from monthly VAT declaration forms

A third testable implication of random assignment to treatment is the relative invariability of estimates to the inclusion of baseline values of covariates and fixed effects. If the RDD is valid, covariates beyond the assignment variable (and functions thereof) are not required for identification or consistency, and serve simply to reduce sampling variability, especially with a local linear specification in a narrow bandwidth. Thus they should not change the value of the coefficient materially on average, although some fixed effects could reduce the available variation to such an extent that little remains for estimation. In the results section we show this is the case for the main results of the paper by including a battery of control variables. In unreported results we re-estimate the RDD for a number of placebo thresholds instead of 25,000 UF in sales; no discrete jumps are observed.

4. Results

4.1. Financial Additionality

Panels A and B in Table 3 show the causal effect of FOGAPE on the change in firms' total debt. Here we use the Davis et al. (2006) growth measure, using as the base period the average of six months before the focal month (t) in which the guaranteed loan was made. This growth measure divides the difference between the future month t+x and the base period (the average of t-6 to t-1) by the average of the two, and is especially well-suited to dealing with large heterogeneity, as it bounds growth between -2 (exit) and 2 (entry). We also show results using the traditional definition of the growth rate and obtain somewhat larger estimates; the difference is driven by firms with very low debt either the following month or the month before, resulting in a very

that are uploaded into the IRS-run web-query eligibility system with a variable lag of one to three months, so we lag this variable by three months accordingly.

low divisor, and consequently a very high rate of change. The Davis et al. measure is much less affected by these outliers and as a result is preferable in our view.

Recall that the main specification has a bandwidth of 1,500 UF around the threshold (∼US $60,000, relative to the cutoff at $1 million) and runs a local linear regression RDD, i.e. the debt increase is pure financial additionality attributable to the CGS. The reduced form estimates of Table 3 show that debt for eligible firms grows 2.6% on average in the focal month with respect to ineligible firms in the bandwidth. That is, there is an average 2.6% increase in one month relative to the average of the preceding six months, causally attributable to being eligible for FOGAPE.

The fuzzy RDD estimate that measures the causal effect of actually receiving a guarantee, for the subpopulation of complier firms, shows much bigger effects. On average, treated firms increase their total debt by almost 95% at the moment of receiving the guarantee, with respect to ineligible firms in the bandwidth. Importantly, these estimates are robust to a wide variety of different specifications, providing support for the validity of the RDD. In particular, including firm level controls such as lagged total debt, number of banks, percentage of debt with main bank and industry and main bank fixed effects, does not have a material effect on the estimates, which supports the local exogeneity of the eligibility rule. Furthermore, we run a specification without the running variable in a smaller bandwidth without the assignment variable to rule out that results are driven by the correlation of debt growth with the assignment variable and find similar results. The use of a triangle kernel does not change the estimates either. Results are consistent with local linear regressions on smaller and larger bandwidths of 1,250 and 1,750 UFs; and with a larger bandwidth of 10,000 UF using a fourth degree polynomial of the assignment variable. For completeness we also report an estimate using the robust bias-corrected estimator of Calonico et al. (2014) and their bandwidth selection procedure. This estimator provides very similar, but slightly larger effects, and the optimal bandwidth determined following their procedure is 1,789 UF, which is very close to our main specification.

To study the persistence of the effects of FOGAPE, we run regressions for leads of the dependent variable. Figures 3, 4 and 5 show the reduced form estimates graphically for 6 months before the focal month, the focal month, and 6 months after, respectively. Panels A and B in Table 4 show the cumulative effect on debt growth over time. Here, the estimate on the lead six months after the focal month should be interpreted as the cumulative growth in the firm's total debt over six months. Given the term structure of the average loan with principal repayments during the life of the loan, we expected to find a persistent but diminishing differential effect of Fogape over time. However, as shown by both reduced form and fuzzy RDD estimates, the effect increases over the first year, reaching a peak at around twelve months after the focal month. When we examine, in our RDD setup, the growth in debt excluding debt held by treated firms at the bank that gave them the FOGAPE guarantee (but retaining their debt at other banks) we find that the debt of treated firms at banks that did not extend FOGAPE to them increases also (see Table 5).

This later increase in debt at banks that are not extending the guarantee to treated firms suggests a positive information externality, whereby banks not giving the firm a FOGAPE guarantee observe the borrower is more creditworthy than previously estimated (and are likely unaware the lending is guaranteed), and so lower their screening requirements for lending. For lending increases that arise some time after FOGAPE treatment, banks also observe firms' recent history of non-default, which would also inform banks' estimate of firm creditworthiness. Informal conversations with officers in the Chilean banking industry and the administrators of Fogape suggest that this is likely. The fact that the effect grows through the first year after the focal month has two important implications. Firstly, the effect of the program on firms' debt capacity extends beyond the amount of the guaranteed loan, leading to an amplification effect on firms' access to credit. In turn, this means that any attempt to measure the effects of CGSs should evaluate the impact on total borrowing, rather than just on individual loans.

Second and more importantly, this results suggests that treated firms are indeed credit constrained. Following the logic in Banerjee and Duflo (2014), if after receiving FOGAPE firms are willing to accept higher rates from other institutions and increase borrowing, this implies that in fact firms are credit constrained because they want to borrow more at the highest offered rate – in other words, additionality of FOGAPE cannot be explained by firms accessing a lower interest rate on the guaranteed loan due to a subsidy component. Moreover, while we do not have data on loan interest rates, participating banks have told us that their policy did not reduce the interest rate charged on guaranteed loans in this period, or change the loan maturity, relative to non guaranteed loans. Given that we see large increases in total bank debt resulting from the guarantee, this indicates that these firms were credit rationed at the bank interest rate. This further strengthens the evidence that FOGAPE is reducing credit constraints rather than redistributing or increasing the debt of unconstrained firms.

4.2. FOGAPE and New Lending Relationships

CGSs can have non-monetary benefits that go beyond the relief of credit constraints due to lack of collateral. One such benefit, virtually ignored by the literature on CGSs, is their potential ability to incentivize the development of new banking relationships for small firms, which are generally opaque and thus costly to screen initially. In this section we present novel evidence showing that FOGAPE helps SMEs to form and develop new banking relationships. Further, we show that there are two levels to this effect: the first is that FOGAPE is often used to begin a new banking relationship; the second is that the guarantee causes an increase in the number of bank relationships over the subsequent 18 months with banks that did not provide the guarantee.

There is a large literature on the benefits of relationship banking. At the firm level, early empirical studies like Petersen and Rajan (1994) and Berger and Udell (1995) show that longer banking relationships improve access to credit for bank-dependent firms, athough they expose firms to hold up costs (Santos and Winton, 2008; Ioannidou and Ongena, 2010). At the bank level a well-established literature shows how sticky banking relationships are key to the transmission of both positive and negative bank-level shocks to firms[17]. The fact that banks' health may have a direct and economically important effect on their clients highlights the value to firms of having several banking relationships, particularly in times of aggregate financial distress (Detragiache et al. (2000))

Borrowers and lenders form relationships to overcome inefficiencies caused by asymmetric information. Through a relationship the lender develops private information (Soft information - see Stein, 2002), which reduces the lender's expected cost of providing capital. However, this learning process entails risk for the lender, which can be reduced by a guarantee. Given that banks are free to assign FOGAPE among their borrowers with very few limitations, the question is what types of risk the bank would prefer to reduce. Banks may find it more profitable to assign guarantees to resolve the uncertainty about the type of new clients, rather than bounding the potential losses on the projects of risky clients about which there is little asymmetric information. Because banks in Chile are not allowed to hold equity there is limited upside in financing risky projects; however, the potential value of new "good" clients is high, as the bank can

[17] For negative shocks to bank liquidity or capital the literature is extensive; see for example Peek and Rosengren (2000); Khwaja and Mian (2008); Paravisini (2008); Iyer and Peydro (2011); Schnabl (2012); Chodorow-Reich (2014). Toro (2015), using the same data set for Chilean firms finds that a positive bank-level shock during the 2008-09 crisis has a temporary financial effect at the firm level, which in turn translates into persistent real effects in the medium term.

extract rents from several loans over time[18].

Figure 6 shows the frequency of FOGAPE guarantees as a function of the length of the banking relationship between the bank assigning the guarantee and the firm receiving it. The histogram is constructed using all guarantees from 2007 to 2013. It shows that almost 15% of the guarantees are assigned to firms that do not have a relationship with the lender, that is, to firms completely new to the bank[19]. Furthermore, around a third of this fraction corresponds to firms that are completely new to the banking system – Figure 7 – which suggests that the effect is larger on firms with fewer banking relationships. Including the first six months of a relationship, a time frame where the uncertainty about the firm's quality is still very high, the fraction of guarantees assigned to new clients goes up to almost 25%. These figures suggest that guarantees are often used by banks and firms to form new relationships. Furthermore, in our sample bandwidth of 1,500 UF in 2011-2012, a full 24% of firms had no borrowing with the bank that gave them a FOGAPE loan 12 months later, and a further 10% have trivially small loans from that bank throughout the year preceding the FOGAPE loan. Thus, around a third of the FOGAPE loans in our sample are used to establish an entirely new, or to develop a fledgling banking relationship.

We then run several RDD regressions examining a firm's number of banking relationships at the eligibility threshold. Since firms around the eligibility threshold are relatively well established, with an average (and median) of two relationships, the effect of the program on the formation of new relationships is likely to be lower than it is for

[18]This argument assumes that the lender has some market power, which is consistent with the empirical evidence on sticky banking relationships, and with models such as Rajan (1992) and Detragiache et al. (2000). Acquiring information not available to the market provides the lender with this market power and the ability to extract future rents from the firm. For a discussion of the effects of bank competition on SME's access to credit see Petersen and Rajan (1995).

[19] We observe every firm-bank pair in Chile from 2005 to 2013, so our data set allows us to measure the length of banking relationships from 2005. Since for all figures with the complete sample of FOGAPE loans we are using data between 2007 and 2013, a firm is considered new to the system if it did not get any loans throughout 2005 and 2006.

smaller and less established firms. Thus, we run these regressions for the subsample of firms that have a single banking relationship four months before the focal (treatment) month - there are no effects on the total number of bank relationships for firms that enter our sample with with two or more banks. Panels A and B in Table 6 shows the results of these regressions for the reduced form RDD specification and the Fuzzy RDD specification respectively. The program has an economically and statistically significant impact on the formation of new bank relationships.

We focus on economically meaningful bank relationships, by which we mean banks with which the firm has at least 20% of its total debt. The reduced form estimates show that eligibility for program has a causal effect of generating .011 new bank relationships in the focal month, and this effect peaks at nine months, with an estimate of .036 new bank relationships; coefficients for before the focal month are not statistically distinguishable from zero. Panel B shows the estimates for compliers, that is, firms that received a guaranteed loan only because they were eligible. The effects of eligibility for the program are statistically and economically important: the estimates imply that at the time of receiving the guarantee, compliers increase their number of bank relationships by half a bank around the FOGAPE month, suggesting that half the complier firms are receiving FOGAPE from a new bank.

Over the year following treatment the estimated RDD effect on number of bank relationships increases substantially. These additional bank relationships must be coming from a bank that is not using FOGAPE with this firm - an *amplification effect* whereby the additional lending that FOGAPE enables makes the borrower a better prospective client to unrelated banks.

An alternative way to see this effect is to examine the dynamics of bank relationships on the treated subsample, as we do in Figure 8, where we show the average number of bank relationships over time (relative to the treament month - 0). Figure 9 shows how the median, 75th and 90th percentiles of treated firms' number of bank relationships gradually increase over time. Moreover, this is not in response to declining lending at the bank that lent the firm FOGAPE, as is shown by Figure 10. Rather, treated firms sem to be leveraging their improved ability to obtain credit to obtain further loans with other banks, as is evidenced in Figure 11.

These patterns are consistent with FOGAPE causing a positive information externality, as mentioned earlier for the amount of borrowing from existing relationships. In this setting the externality induces the formation of new bank relationships, as new banks update their prior regarding the firm's credit quality based on the additional credit the firm has obtained.

4.3. The Cost of FOGAPE: Default

A major concern with credit guarantee schemes is that they may increase default rates, as reported by some existing papers (for example Lelarge et al., 2010; Uesugi et al., 2010). In the case of FOGAPE, its explicit requirement that firms be of at least "normal" risk according to the bank's internal rating, and a rule that reduces future allocation of guarantees if the bank's default rate is too high seem to prevent the phenomenon documented for Japan by Uesugi et al. (2010): shifting of firms about to default into the CGS. Nevertheless, credit guarantees can have an effect on the types of firms and projects that get funded – a selection effect – and may also distort incentives at the bank and the firm level that affect repayment behavior – moral hazard. On the one hand, banks could choose to assign the guarantee to projects more costly to monitor (relative to the amount of the loan), which would in turn decrease monitoring efforts by the bank and increase default rates. On the other hand, once assigned a guarantee, firms with more than one banking relationship can choose to default on lenders with insured loans if defaulting on those lenders is relatively less costly. This could occur if, on average, relationships with lenders that provide guarantees are less developed from the firm's perspective.

Cowan et al. (2015) address the question of incentive distortions caused by FOGAPE in its early years in a sample of firms that is not focused around the eligibility threshold, but instead spans the distribution of annual sales for eligible firms. Comparing delinquency rates for different loans to the same firm from the same bank (using firm-bank-time fixed effects), they find that firms are more likely to *miss payments* on loans guaranteed by the program, defined as payments are between 60 and 90 days late. Nevertheless, they find no difference in the *default rate* (payments delayed by more than 90 days) of these loans, and thus conclude that firms' long-term performance is not affected by the guarantees.

Using the identification provided by the eligibility rule and data on repayment behavior by treated firms, in this section we focus on the effects of the program on the default rate both at the firm and the firm-bank level using two different approaches. Firstly, we run the RDD and examine default rates for firms around the eligibility threshold. Secondly, we extend and complement the analysis in Cowan et al. (2015) and use a firm-bank fixed effects approach to analyze observations away from the eligibility threshold.

4.3.1 Loan Defaults

We begin by comparing repayment behavior for firms that are eligible for FOGAPE versus the repayment behavior of ineligible firms around the 25.000 UF threshold. Unfortunately, data limitations reduce our ability to bring evidence to bear on this issue, because default events generally happen over a year after loan is granted, and our default data only extend to October 2013 - almost three years after our first treatment month, but only ten months after our last. In our RDD framework we do not find evidence of a large increase in defaults relative to controls.

Panel A in Table 7 shows the reduced form RDD estimates on repayment behavior six months before eligibility, and twelve and eighteen months after eligibility. The fact that estimates of the program's effect on all three measures are not statistically different from zero at a 95% confidence level six months before, supports the view that assignment to either side of the threshold is plausibly random within the bandwidth. The estimates for twelve and eighteen months later for the three measures of default are not statistically significant either, which seems likely to be the result of the low power of the RDD in our sample to detect small effects that occur well over a year after the loan is granted. Panel B in Table 7 also shows the Fuzzy RDD estimates, and therefore the causal effect on the subpopulation of compliers. Again, the standard errors are large, and so the 95% confidence intervals do not rule out potentially large delinquency effects. Indeed, in the time series of defaults for treated firms in comparison to untreated firms in Figure **??** the default rate is higher for treated firms starting nine months after the loan is granted, suggesting that there may be a default effect that is masked by the low power of the experiment for detecting differential default.

4.3.2 Do firms default on banks providing FOGAPE more than on banks that do not provide guaranteed loans?

To further explore the effects of FOGAPE on repayment behavior we adopt a complimentary approach, in the spirit of the "within borrower" specification in Cowan et al. (2015), and for which we extend our sample period backwards to start in 2005. We ask if, conditional on getting a guarantee and having more than one lender, firms default more frequently on bank relationships with guaranteed loans, than on bank relationships without guarantees.

We use two measures of default. First, we measure the probability that at least one loan defaults or becomes non-performing (that is, at least ninety days overdue for payment) at the firm level, conditional on the firm having no non-performing loans in the previous period. This measure is intended to capture the extent to which firms selectively choose to default on guaranteed loans. We also use the frequency of a firm having at least one non-performing loan at any point in time.

We run the following specification on firm-bank pairs from 2005 to 2013 at a monthly frequency, where β is the coefficient of interest:

$$Default\ Frequency_{ibt} = c + \beta fog24_{ibt} + \gamma_1 Rel.Length_{ibt} + \gamma_2 Rel.Importance + \delta_{it} + \epsilon_{ibt} \tag{4}$$

Where i denotes firm, b bank and t time. The dependent variables are the two measures of default frequency described earlier. We run each regression on the subpopulation of firms that have at least two bank relationships and received at least one loan guaranteed by FOGAPE in the previous twenty four months[20]. We include firm-month fixed effects, which means that identification of the effect of FOGAPE in this setting comes from firms choosing to default differentially between those lenders that provide a loan with a guarantee to the firm and other no-guarantee lenders. In other words, estimated coefficients on fog24 are not explained by either time varying firm-specific characteristics, such as demand shocks, or static characteristics.

An important difference between our approach and that in the "within borrower" strategy in Cowan et al. (2015) is that we observe a much longer panel for the universe of firm-bank pairs in Chile, which allows us to include crucial controls at the firm-bank level to capture the effect on default of characteristics specific to the firm-bank relationship. Specifically, we control for the length of the relationship (months since the first loan from that bank) and a measure of how important the bank is to the

[20] We observe the firm and lender pair when a new loan is assigned the guarantee. Unfortunately we do not observe the maturity or other characteristics of the loan, and therefore we do not observe if an outstanding loan has the guarantee. A twenty four month time frame provides an upper bound on the effect, as the average FOGAPE loan has a maturity of 16 months.

firm (average proportion of total lending to the firm over the last 2 years provided by each bank). Our other findings suggest that controlling for these two characteristics is particularly important, because FOGAPE is used by banks in part to attract new clients, and to develop the relationship when the bank is not the main lender. These two controls together should capture the depth of the lending relationship.

Estimates of the coefficient on fog24 in equation (4) do not, strictly, provide a causal interpretation of the effect of the program on repayment behavior, because unobservable characteristics of the firm-bank relationship could also determine the assignment of a guarantee. However, the inclusion of firm-time fixed effects and firm-bank relationship controls greatly reduces the set of potential explanations for our finding. In short, it seems likely that whatever we find (or fail to find) is causally attributable to incentive distortions (or their absence) caused by the program. Importantly, although not directly comparable with the estimates from the Fuzzy RDD (because here the sample is for firms with at least two bank relationships in a longer time period), estimates from these regressions provide an alternative estimate of the effect of the program at the threshold. Additionally, we obtain estimates away from the threshold, and can compare average default rates for firms of different size.

Table 8 shows the estimates from these regressions which use the whole range of the data, as opposed to just around the threshold, and therefore include firms with all levels of annual sales. Results are consistent for both measures of repayment behavior. Column (1) in Panel A shows that the average probability of becoming non-performing is approximately 0.57% for non-FOGAPE bank-firm pairs and it is 4.6% higher for FOGAPE bank-firm pairs at 0.6% (0.569% + 0.026%). Column (1) in Panel B shows that, on average, 2.2% of non-FOGAPE bank-firm pairs have at least one non-performing loan at any point in time, and that this probability increases by almost 10% for FOGAPE -bank-firm pairs. The lower than average default rates in both panels is explained by the characteristics of our subsample: firms with more than one banking relationship that have received a guaranteed loan in the past twenty four months.

In Panels A and B, Column (2) adds firm-bank fixed effects and bank relationship controls. The estimates show that firms are more likely to have their loan become non-performing (i.e. default) on the bank that provided a FOGAPE loan than they are on their other bank(s) in our sample. Although the numbers are not large in magnitude, they show that the probability of becoming non-performing is 14% higher (0.079%/0.569%) for FOGAPE bank-firm pairs (Panel A), and that FOGAPE bank-firm pairs are approximately 20% more likely to have at least one non-performing loan (Panel B).

Columns (3) to (6) for Panels A and B show results for the same specification run on firms in buckets of the assignment variable used in the Fuzzy RDD – cumulative sales over the past twelve months. For both measures of repayment behavior, the higher likelihood of default on debt to FOGAPE banks as opposed to other banks is driven by smaller firms: Columns (5) and (6) in both panels.[21] The fact that default for FOGAPE firm-banks rises and the difference becomes significant as we move towards smaller firms may be evidence that the program reduces monitoring or collection efforts by lenders for smaller loans, for which the likely fixed cost of these activities is relatively larger. Importantly however, column (4) in both panels shows no evidence of differential repayment behavior for firms around the eligibility threshold, which supports the findings of the Fuzzy RDD.

This section has examined whether firms are more likely to default on their loans from banks that give them FOGAPE in comparison to loans provided by other banks. This requires that we focus exclusively on firms with loans from at least two banks,

[21]Firms above the threshold column (3) were eligible for FOGAPE if they were exporters, and under different conditions, although there are relatively few such firms. In 2009-2010 firms above the 25,000 UF cutoff were also temporarily eligible for FOGAPE.

unlike in the RDD framework examining default. We find no evidence that firms around the eligibility threshold are more likely to default on their loans to the banks that gave them FOGAPE. However, for smaller firms we do find evidence of default being differentially higher for banks providing FOGAPE, in comparison to default on other banks.

4.4. Real Effects

The FOGAPE program increases credit availability, as per the results in Section 4.1. However, the loan guarantee program does not reduce the price of credit for treated firms: while we do not observe interest rates, participating banks have told us that their policy was to set interest rates at the same level for both non-guaranteed and guaranteed loans. However, treated firms may use the increased bank credit exclusively to replace non-bank debt, such as trade credit, with the guaranteed bank debt, because non-bank debt is generally more expensive. If this were the case the effect of the program on firms' total debt could potentially be negligible, and because we do not observe non-bank debt we cannot check this directly. Although this seems unlikely, given the large size of the effect on firms' bank debt, and the often limited availability (and high cost) of non-bank debt, we address this issue by exploring the effect of the program on firms' employment, sales and purchases. A positive effect on these real variables would indicate that treated firms are indeed increasing total borrowing – the program would be changing the firms' assets as well as the structure of its liabilities.

In addition, exploring the magnitude of the real effects reveals the extent to which firms around the eligibility threshold are credit constrained. In particular, the elasticity of these real variables to bank-debt is an informative measure of credit constraints in our setting. This is because firms that are not credit constrained do not value a dollar of debt at more than its cost, and so choose an optimal level of employment, sales and purchases, conditional on the interest rate. Any additional credit made available to such firms at the bank interest rate will have no effect on these real variables. However, financially constrained firms cannot increase employment and sales to reach their optimal levels because they cannot access additional credit, even though they would be willing to pay for it. Thus, the size of the increase in employment or sales in response to the program's increase in bank lending reveals how financially constrained treated firms are ex ante.

We obtain estimates of the elasticity of firms' employment, sales and purchases over a twelve month period by running the following instrumental variables specification:

$$Outcome_{it} = c + \beta Debt\ Growth_{it} + \eta_t + \nu_{it} \tag{5}$$

Essentially, we are interested in the effect of debt growth on each outcome variable (e.g. employment growth). To ensure that the estimate is for the effect of the guarantee program we make use of our RDD framework (in the same narrow bandwidth we use throughout) and use program eligibility as an instrument for debt growth. The parameter of interest is β: the elasticity of the outcome variable to growth in bank debt.

The estimates in table 9 show that for an increase of 10% in bank debt, firms' employment, sales and input purchases increase by 4.8%, 5% and 5.6% respectively a year after receiving a guaranteed loan, and these effects are causally attributable to the program. Thus, the program indeed has a positive effect on firms' debt capacity, which confirms our results on additionality. As a comparison, Banerjee and Duflo (2014), for a sample of firms in India with average sales below US $200,000, report elasticities of sales(cost) with respect to subsidized bank credit of 0.75 (0.70), substantially larger than those we estimate.

The magnitude of the estimated elasticities is nonetheless large, which suggests that firms receiving a guarantee operate under substantial financial constraints despite the fact that they are not especially small, and that the Chilean financial system is well-developed (OECD, 2011; IMF, 2011). In turn, the fact that treated firms experience substantial credit constraints indicates that the program is achieving a core objective, and is well-targeted. Further, the similar magnitudes of the estimated elasticities suggest a general scaling up of the firm after receiving a guarantee, which supports the idea of binding financial constraints.

5. Discussion

CGSs are ubiquitous across the world, and mobilize significant resources. Yet selection challenges have meant that there is surprisingly little clean evidence on the causal effects of these programs on firms' access to credit, potential incentive distortions and other dimensions relevant to assess the effectiveness and value of CGSs. Moreover, CGSs provide a window into the effects of credit constraints on firms, especially if the CGS is effective in alleviating them.

Using a comprehensive data set and a regression discontinuity design, we have provided clear causal evidence of the effects of FOGAPE, Chile's CGS program, on firms' borrowing, repayment behavior, the formation of new bank relationships, and on firms' ex post scale up in terms of sales, input purchases and employment. Our results indicate that FOGAPE provides substantial financial additionality. In fact, the program seems to boost overall debt capacity, as borrowing increases not only at the lender assigning the guarantee, but at other banks as well.

An additional and novel finding, to our knowledge, is that guarantees are used by banks to develop new banking relationships, a costly process for firms and a risky one for banks. In particular, FOGAPE is used to develop new bank relationships for firms that, ex ante, had only a single existing bank relationship. This non-monetary benefit is potentially of greater value than the additional lending we document, given the key role of bank health in periods of financial stress.

The fact that there are large real effects, and that we observe increased borrowing from banks other than the one assigning a guarantee provides strong evidence that FOGAPE is indeed helping firms that are credit constrained, as opposed to firms that would have received credit in any case. That is, the program is serving its main stated objective. On the other hand, we found evidence that firms to default more on loans with the guarantee than on loans without, suggesting that the CGS introduces some moral hazard. However, this seems to be true only at smaller firms, and the magnitude of this effect does not seem to be large.

Banerjee and Duflo (2014) find causal evidence of financial constraints for Indian firms, providing perhaps the first cleanly identified evidence, using micro-data, of major credit constraints for small firms outside of credit crunches. We provide similar evidence for firms with five times the level of average annual sales, in a much more developed financial system, in good times. This suggests that credit constraints are pervasive for small firms, instead of being a temporary consequence of tightening bank credit standards in periods of financial stress.

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Figure 1: Probability of receiving a FOGAPE guaranteed loan

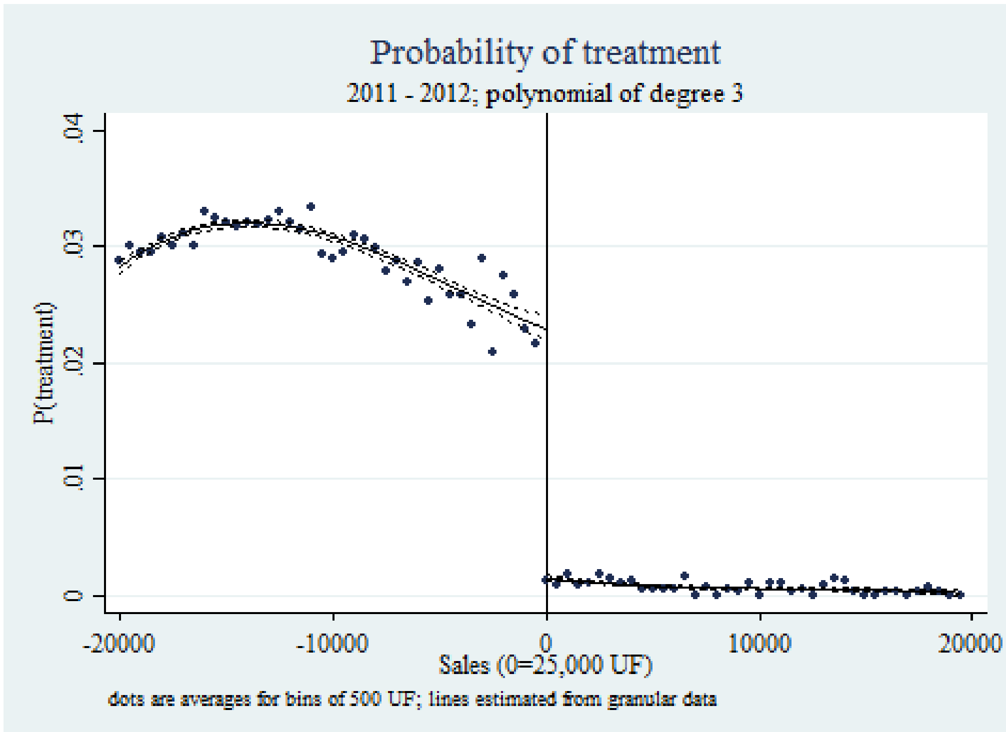


Figure 2: Histogram of assignment variable

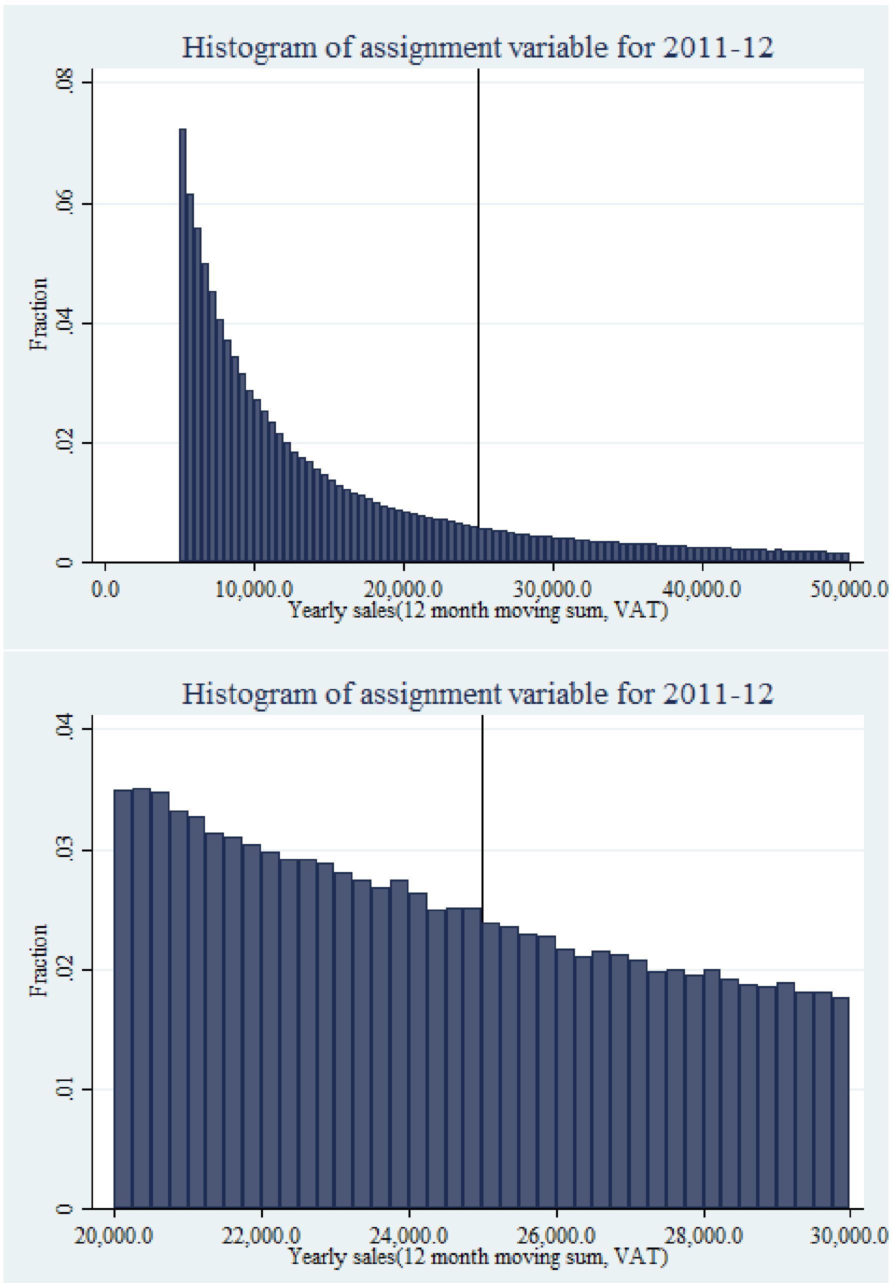


Figure 3: Debt growth 6 months before focal month

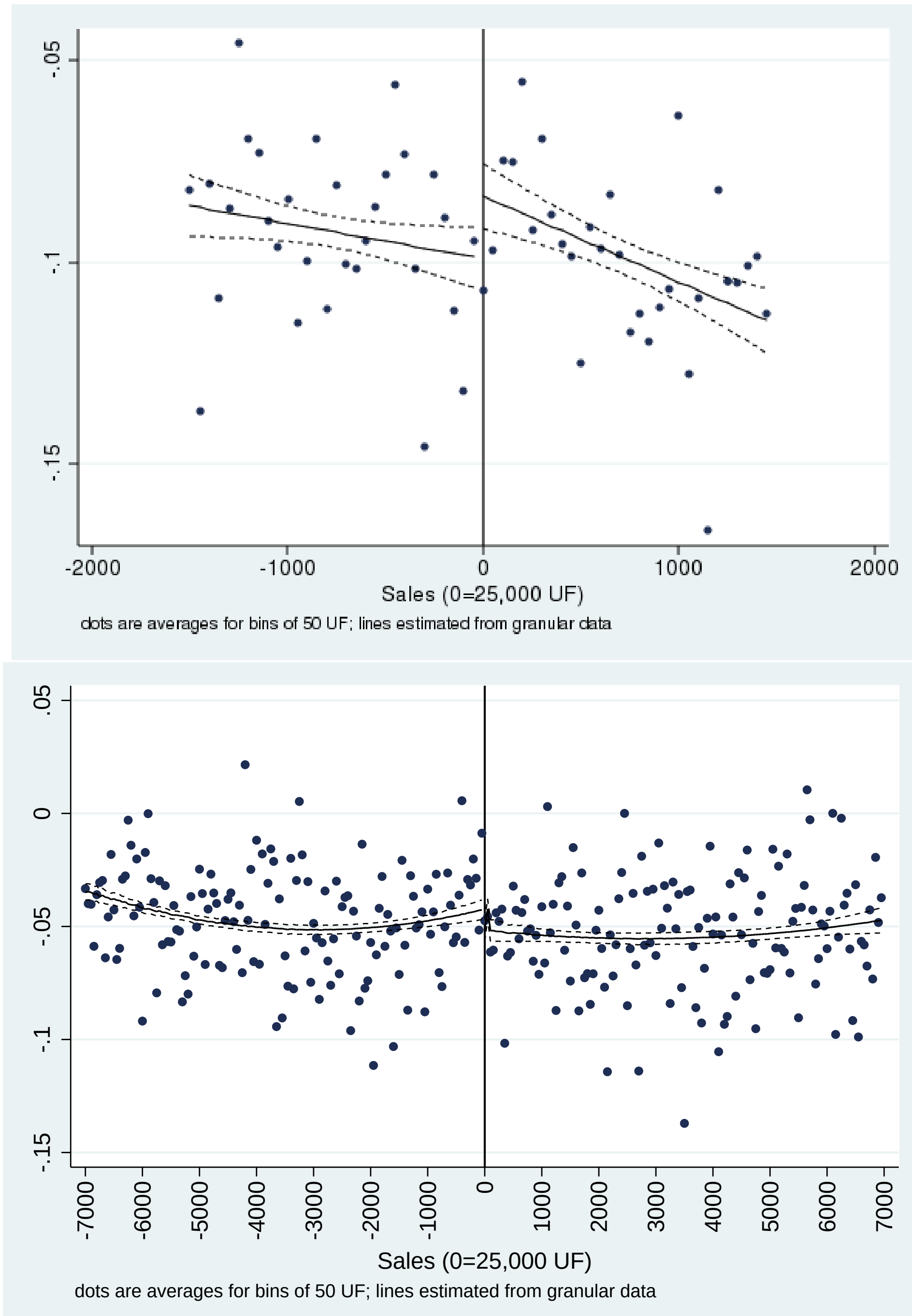
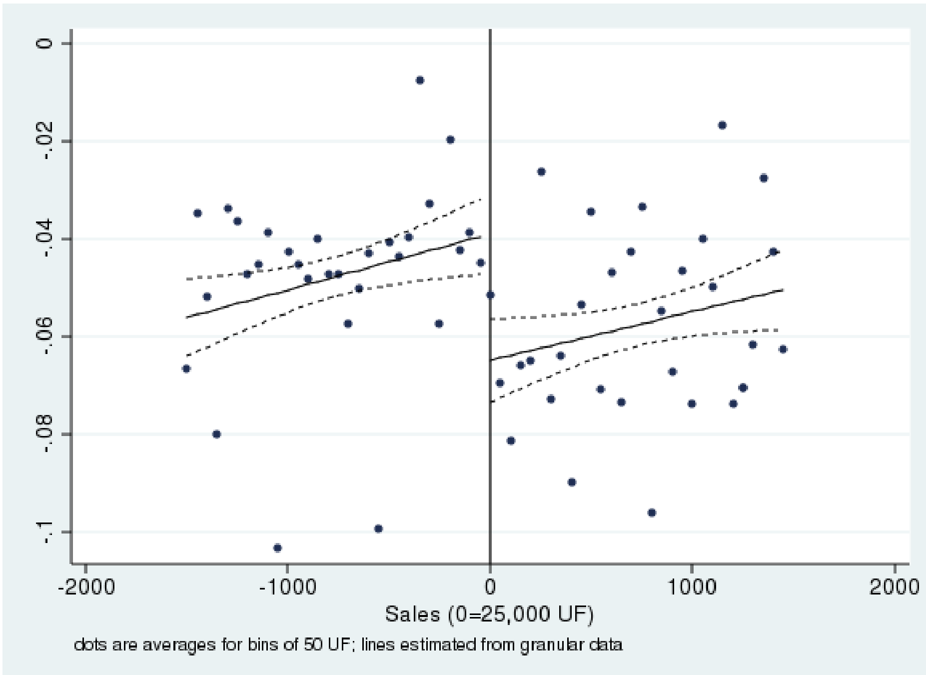


Figure 4: Debt growth in focal month



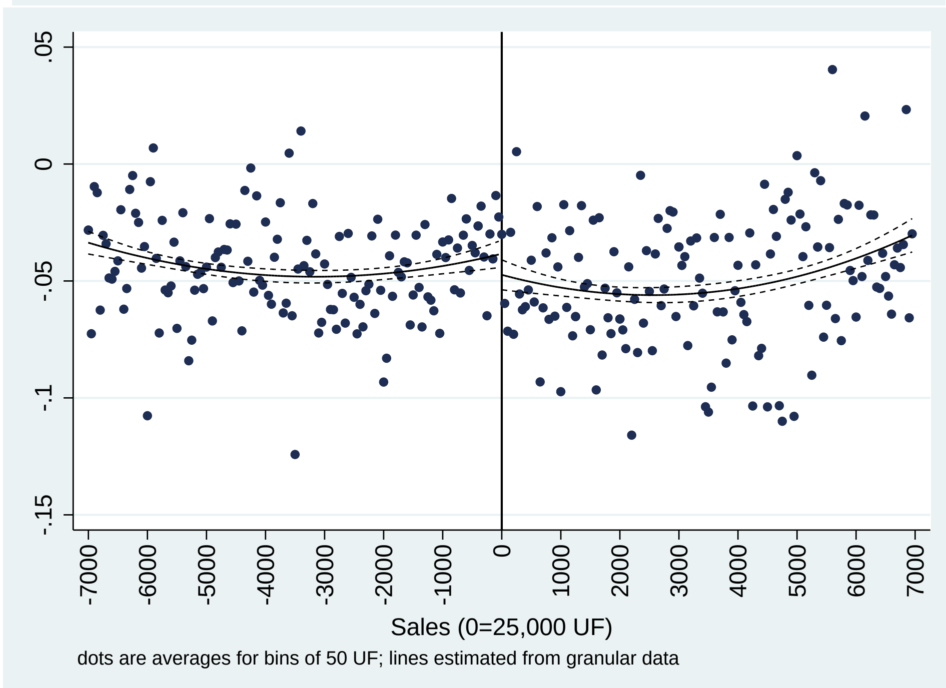


Figure 5: Debt growth 6 months after the focal month

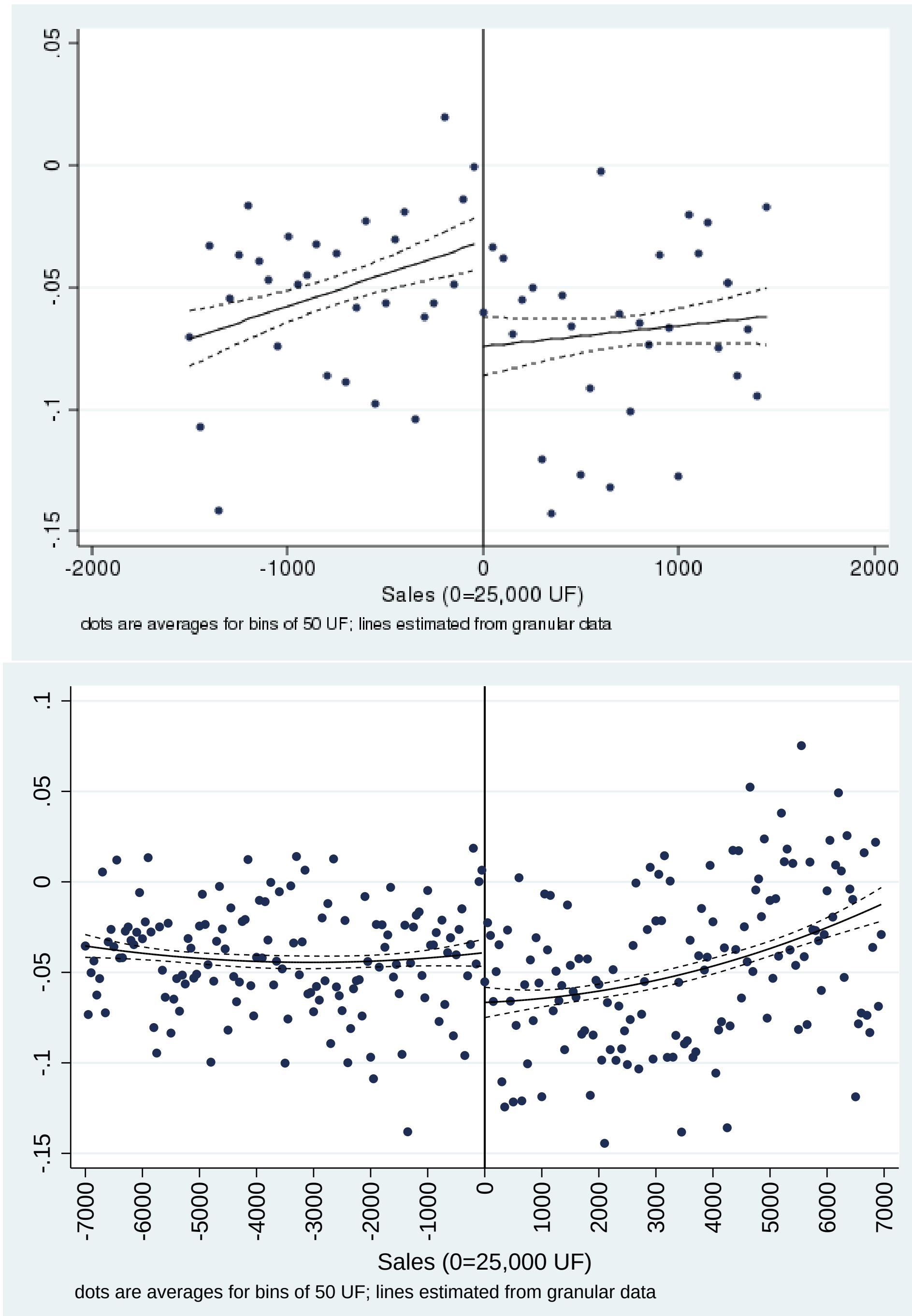


Figure 6

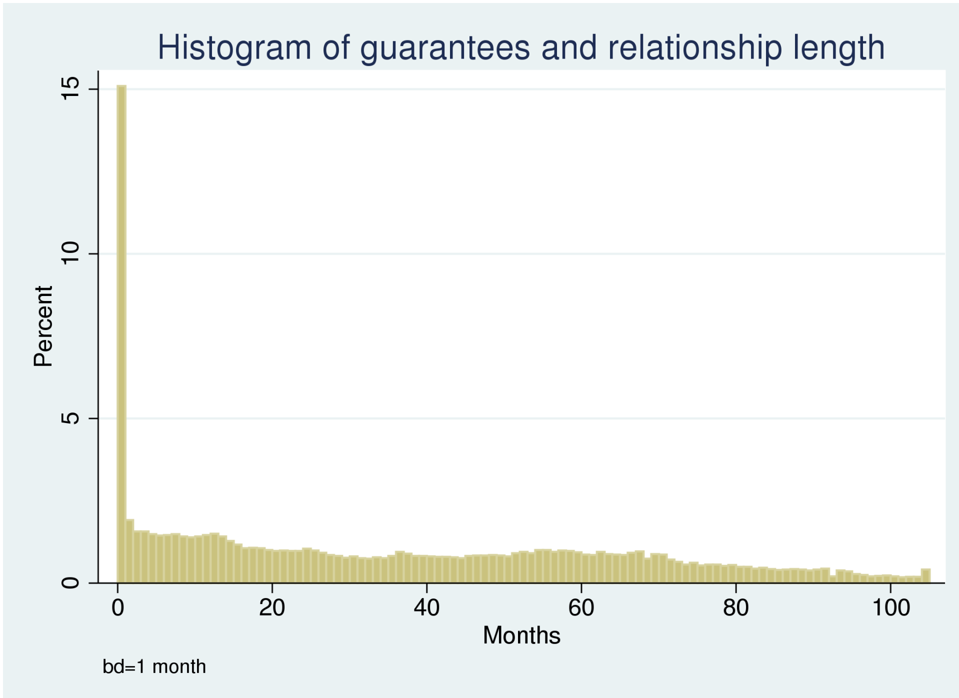
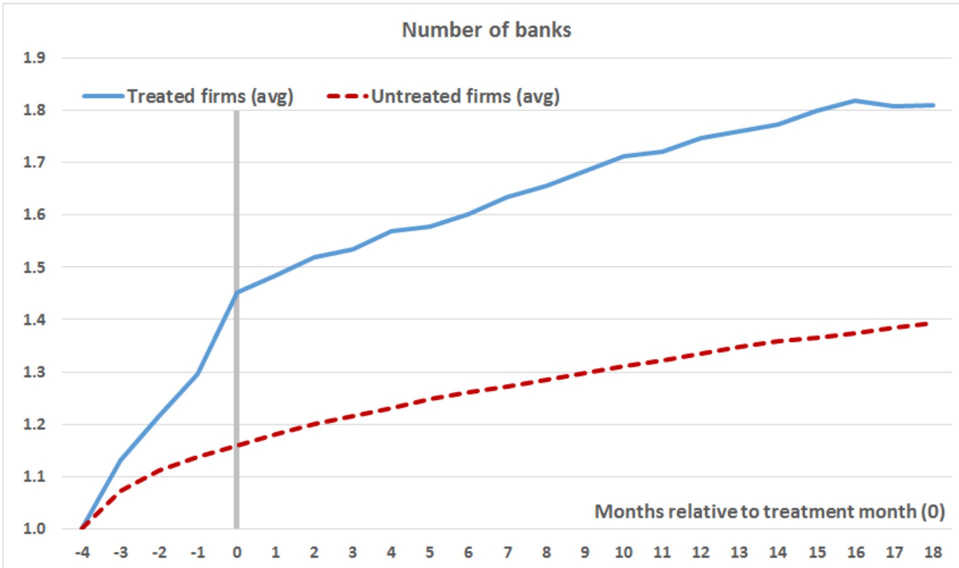


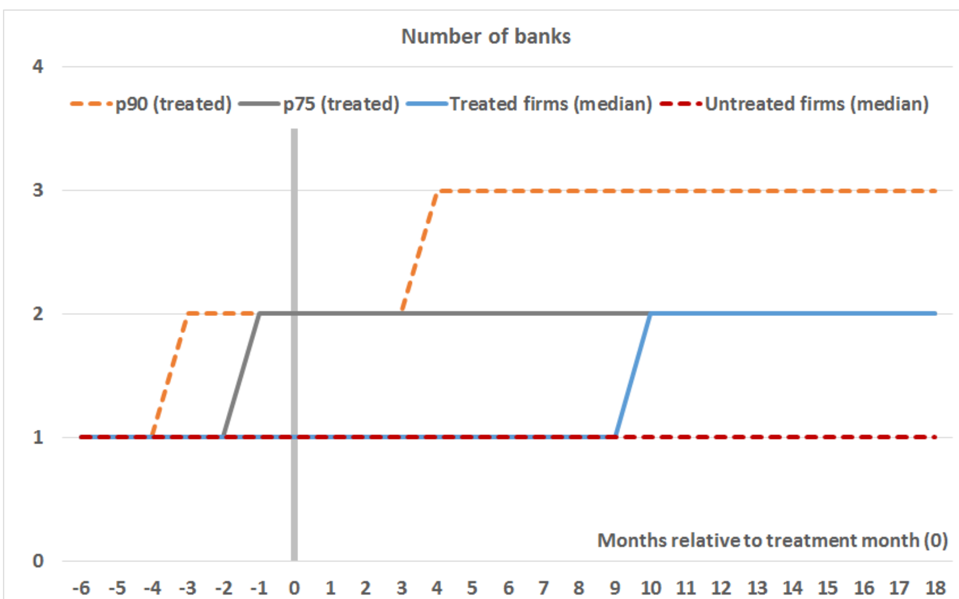
Figure 7

Figure 8: Average Number of Bank Relationships for treated firms



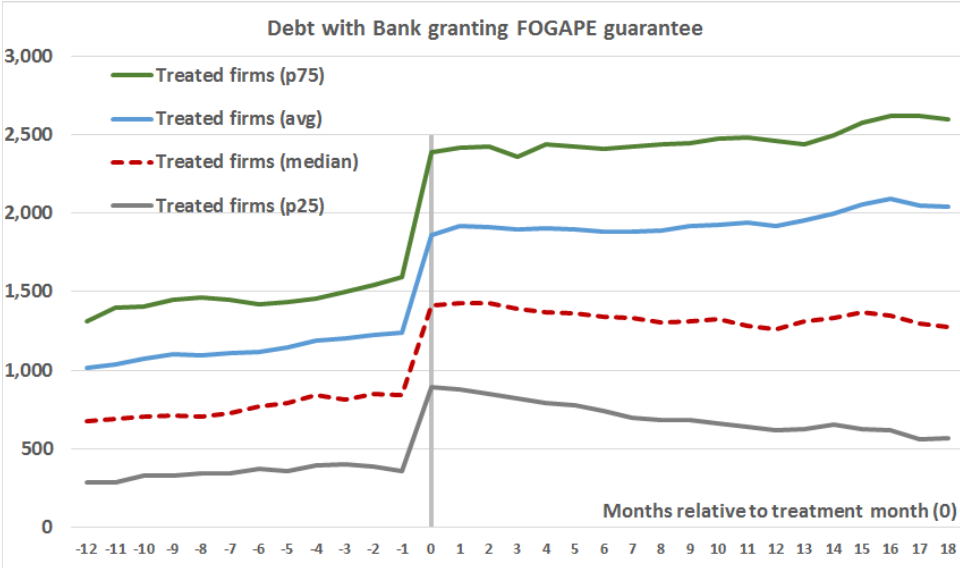
This graph displays the average number of bank relationships of firms that actually receive the FOGAPE credit guarantee ("treated firms"). This figure is for firms in a sales range of 15,000 to 25,000 UF (the latter is the eligibility cutoff) in 2011 and 2012 (the graph is noisier but very similar if we restrict the range to 1,500UF around the cutoff), and is for firms that had only one bank relationship four months prior to the treatment month.

Figure 9: Distribution of Number of Bank Relationships for treated firms



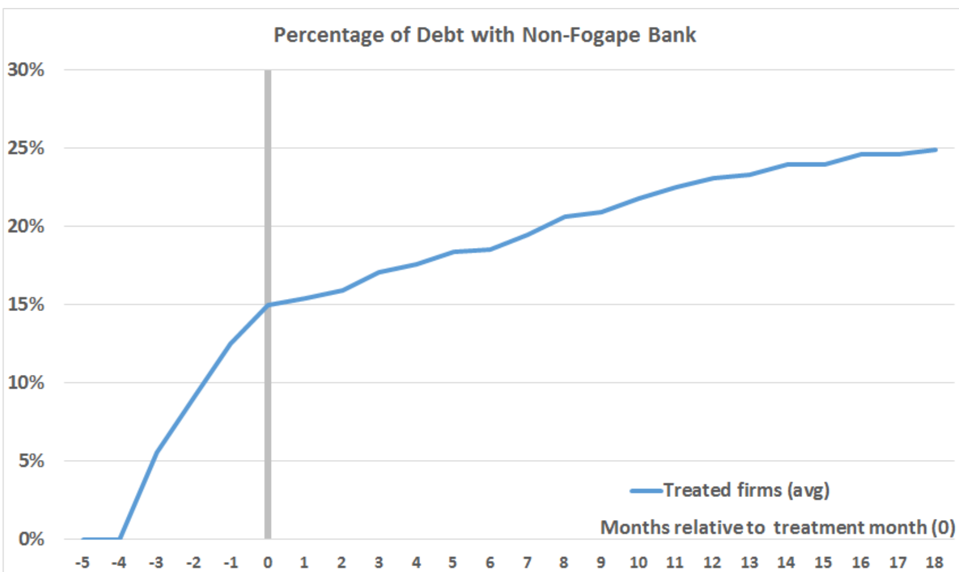
This figure displays percentiles of the distribution of the number of bank relationships of firms that actually receive the FOGAPE credit guarantee ("treated firms"). This figure is for firms in a sales range of 15,000 to 25,000 UF (the latter is the eligibility cutoff) in 2011 and 2012 (the graph is noisier but very similar if we restrict the range to 1,500UF around the cutoff), and is for firms that had only one bank relationship four months prior to the treatment month. The median for the untreated firms is included for reference.

Figure 10: Debt of treated firms with FOGAPE-granting bank



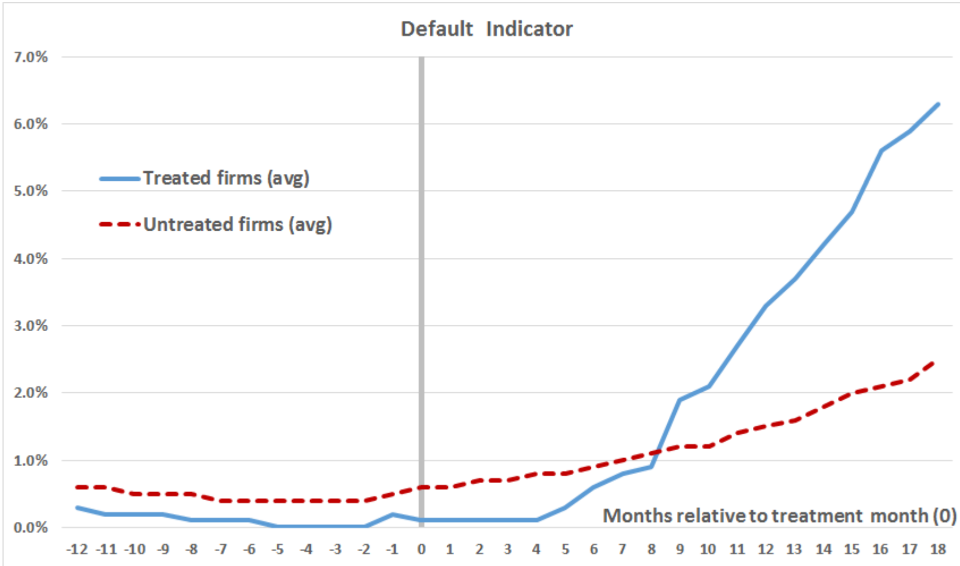
This figure displays four components of the distribution of the total debt with the FOGAPE-granting bank of firms that actually receive the FOGAPE credit guarantee ("treated firms"). This figure is for firms in a sales range of 15,000 to 25,000 UF (the latter is the eligibility cutoff) in 2011 and 2012 (the graph is noisier but very similar if we restrict the range to 1,500UF around the cutoff), and is for firms that had only one bank relationship four months prior to the treatment month. p75 refers to the 75th percentile.

Figure 11: Proportion of treated firms' debt with banks other than the FOGAPE-granting Bank



This figure displays the proportion of the total debt of firms that actually receive the FOGAPE credit guarantee ("treated firms") that is held by banks other than the bank that granted the firm the guarantee. This figure is for firms in a sales range of 15,000 to 25,000 UF (the latter is the eligibility cutoff) in 2011 and 2012 (the graph is noisier but very similar if we restrict the range to 1,500UF around the cutoff), and is for firms that had only one bank relationship four months prior to the treatment month.

Figure 12: Proportion of firms defaulting on their bank debt each month



This figure displays the proportion of firms that default on one of their loans (i.e. are more than 90 days late in paying) in each month for treated and untreated firms. This figure is for firms in a sales range of 15,000 to 25,000 UF (the latter is the eligibility cutoff) in 2011 and 2012 (the graph is noisier but very similar if we restrict the range to 1,500UF around the cutoff).

Table 1 - Density tests

Panel A - McCrary (2008) Density test

L- limit	R- limit	theta	s.e.	t-stat
20,000	30,000	-0.009	0.008	-1.09
20,000	35,000	-0.009	0.007	-1.25
20,000	40,000	-0.008	0.008	-1.11
20,000	45,000	-0.009	0.008	-1.10

This table shows results for the McCrary (2008) test for discontinuities in the density of firms' sales around the eligibility threshold (25,000 UF). The null hypothesis is continuity in the density (i.e. no manipulation). The test is run every month in 2011-2012 for different bandwidths around the eligibility threshold: L-limit is the lower bound of the assignment variable used in the test, R-limit is the upper bound. These test results suggest no manipulation of the assignment variable around the threshold

Panel B - Cattaneo, Janssen and Ma (2016) Density test

	Left of c	Right of c		
# obs.	207,614	106,156	# obs.	313,770
Order loc. Poly.	2	2	Model:	unrestricted
Order BC	3	3	BW method:	comb
			VCE method:	jackknife

	Uniform kernel		Triangle kernel	
	L	R	L	R
Eff. # obs.	61,672	35,393	53,803	34,825
Bandwidths (hl,hr)	estimated	estimated	estimated	estimated
Bandwidth values	3,807	2,767	3,369	2,721
Bandwidth scales	0.5	0.5	0.5	0.5
Method	**T**	**P>|T|**	**T**	**P>|T|**
Conventional	-1.42	0.16	-0.88	0.38
Undersmoothed	-0.22	0.83	-0.06	0.95
Robust Bias-Corrected	0.02	0.98	0.10	0.92

This table displays the results of the Cattaneo, Jansson and Ma (2016) RDD manipulation test, where the null hypothesis is continuity of the density (i.e. no manipulation). The result under "Robust Bias-Corrected" is preferred theoretically and has greater asymptotic power. "Undersmoothed" reduces the bandwidth to provide an approximately valid test, but has lower power as a result. "Conventional" is likely to present a possibly first-order bias according to Cattaneo et al. (2016), but is presented for completeness. Results are given for both uniform and triangle kernels. These test results suggest no manipulation of the assignment variable around the cutoff (c).

Table 2

Summary Statistics

	Firms with FOGAPE sales between 23,500 and 25,000						Firms with FOGAPE sales between 25,000 and 26,500						*p*-value (difference in means)
	Mean	Std. Dev.	10th	Median	90th	Number	Mean	Std. Dev.	10th	Median	90th	Number	
Fogape sales	24,246	457.82	23,639	24,229	24,851	16,129	25,733	434.11	25,141	25,724	26,337	14,460	0.00
Total assets (1yr lag)	699.8	2,781	80.3	310.3	1,256	16,129	783.7	3,578	87.0	340.7	1,369	14,460	0.01
Fixed assets (1yr lag)	188.7	680.8	0.0	39.5	401.2	16,129	207.4	689.1	0.0	43.1	434.9	14,460	0.09
Investment (1yr lag)	29.2	359.4	-10.7	7.1	103.0	16,046	32.8	494.0	-10.3	8.0	111.7	14,227	0.43
Firm age (yrs)	10.9	5.9	3.0	10.0	19.0	16,129	10.9	5.9	3.0	10.0	19.0	14,329	0.93
Debt Δ (12m lag)	-0.19	0.73	-1.32	-0.02	0.55	15,420	-0.20	0.74	-1.36	-0.02	0.55	13,507	0.63
Debt Δ (6m lag)	-0.09	0.50	-0.59	0.02	0.27	15,982	-0.10	0.50	-0.63	0.02	0.26	13,941	0.25
Debt Δ (5m lag)	-0.09	0.45	-0.47	0.02	0.20	16,046	-0.09	0.45	-0.47	0.02	0.20	14,006	0.69
# Bank rel. >20% (6m lag)	1.4	0.5	1.0	1.0	2.0	15,982	1.4	0.5	1.0	1.0	2.0	13,941	0.97
# Bank rel. (5m lag)	2.1	1.1	1.0	2.0	4.0	16,142	2.1	1.1	1.0	2.0	4.0	14,127	0.77
% Non-performing debt(5m lag)	0.9%	8.8%	0.0%	0.0%	0.0%	16,142	0.9%	8.5%	0.0%	0.0%	0.0%	14,127	0.75
% Non-performing debt lag 4m	1.0%	9.2%	0.0%	0.0%	0.0%	16,198	0.9%	8.7%	0.0%	0.0%	0.0%	14,199	0.37
1{Non-performing debt} (5m lag)	0.05	0.21	0.00	0.00	0.00	16,671	0.04	0.21	0.00	0.00	0.00	14,564	0.78
1{New bank rel.} (5m lag)	0.11	0.32	0.00	0.00	1.00	16,671	0.11	0.32	0.00	0.00	1.00	14,564	0.94
# of workers (5m lag)	20.8	28.9	5.0	13.0	42.0	16,671	21.7	29.4	5.0	14.0	43.0	14,564	0.05
Δ # workers (5m lag)	-0.02	0.21	-0.19	0.00	0.14	16,671	-0.02	0.20	-0.18	0.00	0.14	14,564	0.35
% Temporary workers(5m lag)	28.8%	30.3%	0.0%	20.0%	81.0%	16,671	28.9%	30.6%	0.0%	19.4%	83.3%	14,564	0.78
Δ Sales (6m) lag 5m	-0.03	0.22	-0.20	-0.01	0.14	15,694	-0.03	0.21	-0.19	-0.01	0.14	13,707	0.22
Δ Purchases (6m) lag 5m	-0.03	0.19	-0.22	-0.01	0.16	15,718	-0.02	0.18	-0.22	-0.01	0.16	13,727	0.58

This table presents summary statistics for the sample; the block on the left presents these statistics for firms just eligible for FOGAPE (a bandwidth of 1,500 UF), while the block to the right presents the statistics for firms just ineligible because their FOGAPE sales exceed the threshold. The rightmost column shows the p value from a two sided difference in means test. FOGAPE sales are the firm's sales as registered by FOGAPE's specialized system, and are what is used to determine a firm's eligibility for the credit guarantee program. These sales are measured in UF (Chile's Unidad de Fomento, equivalent to approximately $40 USD in the period) and are as of the focal month. Total assets, fixed assets and investment are from the Chilean IRS's annual data, and are in million pesos (approximately US $2,000). All Δ variables are constructed with the Davis et al. (2006) growth measure, using as the base period the average of six months before the focal month, t. This growth measure divides the difference between t and the base period (the average of t-6 to t-1 months) by the average of the two. # of Bank rel. >20% refers to the number of bank relationships the firm has that also include at least 20% of the firm's total debt. % Non-performing debt is the fraction of the firm's total debt that is non-performing. A fraction of a loan is considered non-performing if a principal payment is overdue by more than three months. 1{non-performing debt} is an indicator variable that equals one if the firm has some non-performing debt. 1{New bank rel.} is an indicator equal to one if the firm has established a new bank relationship in the last three months. % Temporary workers is the proportion of workers at the firm that are on temporary contracts that can last a maxiumum of one year. Δ Sales and Δ purchases are six month moving sums of firm sales and purchases that are subject to VAT.

Table 3 - RDD estimates for debt growth in focal period

	Panel A: Debt growth in focal period (Reduced form)								Alternative debt Δ
	Baseline			**No assignment**	**Controls**	**Triangle kernel**	**Poly.(4th°)**	**CCT(2014)**	
	1500UF	**1250UF**	**1750UF**	**500UF**	**1500UF**	**1500UF**	**10,000UF**	**bias-c+robust**	**1500UF**
Coefficient	**0.026****	**0.028****	**0.025****	**0.027*****	**0.033*****	**0.029****	**0.021***	**0.029****	**0.061****
s.e.	[0.012]	[0.013]	[0.011]	[0.010]	[0.011]	[0.013]	[0.011]	[0.012]	[0.025]
# obs.	30,937	25,857	36,066	10,379	29,843	30,937	229,055	36,845	30,937

	Panel B: Debt growth in focal period (Fuzzy RDD)								Alternative debt Δ
	Baseline			**No assignment**	**Controls**	**Triangle kernel**	**Poly.(4th°)**	**CCT(2014)**	
	1500UF	**1250UF**	**1750UF**	**500UF**	**1500UF**	**1500UF**	**10,000UF**	**bias-c+robust**	**1500UF**
Coefficient	**0.947****	**1.034****	**0.928****	**0.987*****	**1.213*****	**1.057****	**0.771***	**1.163****	**2.254****
s.e.	[0.431]	[0.489]	[0.420]	[0.366]	[0.422]	[0.475]	[0.415]	[0.466]	[0.929]
# obs.	30,937	25,857	36,066	10,379	29,843	30,937	229,055	36,845	30,937

Panel A reports estimates of the effect of *eligibility* for Fogape on the growth rate of firms' total debt in the month the FOGAPE loan is made (focal period). Panel B reports estimates of the effect of *actually receiving a Fogape-guaranteed loan* in the focal month (t). Total debt is the firm's debt across all banks. We use the Davis et al. (2006) growth measure, which divides the difference in firm debt between t and the base period (the average of t-6 to t-1) by the average of the two periods. "Alternative debt Δ" uses a standard growth rate (which is inflated by firms with small initial debt levels). Baseline estimates come from RDD (for Panel A) and Fuzzy RDD (for Panel B) local linear regressions around the eligibility threshold of 25,000UF in the specified bandwidths. We use White-Huber standard errors and a uniform kernel. "No assignment" runs the RDD in a very small bandwidth, and without controlling for the assignment variable: it is the difference between the average levels to the left and to the right of the threshold. "Controls" includes as additional variables: lagged total debt, number of banks, percentage of debt with main bank, and fixed effects for industry and main lender. "Triangle kernel" runs the baseline specification with a triangle kernel. "Poly.(4th)" runs a fourth degree polynomial (separately on each side of the cutoff) RDD specification in a very large (10,000) UF bandwidth. "CCT (2014)" presents the results of the Calonico et al. (2014) robust and bias corrected RDD estimator at the CCT optimal bandwidth of 1,789UF.

Table 4 - Dynamics of total debt growth

Panel A: total debt growth dynamics (Reduced form)

	lags and leads from focal period (months)											
	-6	**-4**	**-1**	**0**	**3**	**6**	**9**	**12**	**15**	**18**	**21**	**24**
Coefficient	**-0.013**	**0.003**	**0.016***	**0.026****	**0.039*****	**0.042****	**0.036****	**0.051****	**0.051****	**0.048***	**0.047**	**0.038**
s.e.	[0.011]	[0.009]	[0.010]	[0.012]	[0.014]	[0.017]	[0.018]	[0.021]	[0.024]	[0.027]	[0.031]	[0.037]
# obs.	30,154	30,409	30,808	30,937	30,509	30,256	30,056	27,267	23,204	19,304	15,650	11,825

Panel B: total debt growth dynamics (Fuzzy RDD)

	lags and leads from focal period (months)											
	-6	**-4**	**-1**	**0**	**3**	**6**	**9**	**12**	**15**	**18**	**21**	**24**
Coefficient	**-0.490**	**0.113**	**0.605**	**0.947****	**1.379****	**1.514****	**1.302***	**1.837****	**1.713****	**1.611***	**1.553**	**1.353**
s.e.	[0.418]	[0.333]	[0.369]	[0.431]	[0.539]	[0.612]	[0.666]	[0.786]	[0.837]	[0.917]	[1.040]	[1.314]
# obs.	30,154	30,409	30,808	30,937	30,509	30,256	30,056	27,267	23,204	19,304	15,650	11,825

Panel A reports estimates of the effect of eligibility for FOGAPE on firms' total debt growth rate at different horizons with respect to the baseline period of the six months before the loan. Panel B does the same for actually receiving a FOGAPE loan. Total debt is the firm's debt across all banks. We use the Davis et al. (2006) growth measure, using as the base period the average of six months before the focal month, t. This growth measure divides the difference between t and the base period (the average of t-6 to t-1) by the average of the two periods.

Table 5 - Dynamics non-FOGAPE debt growth

Panel A: Non-FOGAPE debt growth dynamics (Reduced form)

	lags and leads from focal period (months)							
	-6	**-3**	**0**	**3**	**6**	**9**	**12**	**18**
Coefficient	**-0.003**	**0.003**	**0.016**	**0.018**	**0.026***	**0.024**	**0.042****	**0.065****
s.e.	[0.01]	[0.01]	[0.01]	[0.01]	[0.02]	[0.02]	[0.02]	[0.03]
# obs.	27,856	29,038	30,458	29,076	28,374	27,758	24,804	17,107

Panel B: Non-FOGAPE debt growth dynamics (Fuzzy RDD)

	lags and leads from focal period (months)							
	-6	**-3**	**0**	**3**	**6**	**9**	**12**	**18**
Coefficient	**-0.193**	**0.207**	**0.821**	**0.887**	**1.225**	**1.075**	**1.915****	**2.628****
s.e.	[0.64]	[0.47]	[0.57]	[0.70]	[0.77]	[0.81]	[0.91]	[1.06]
# obs.	27,856	29,038	30,458	29,076	28,374	27,758	24,804	17,107

Panel A reports estimates of the effect of eligibility for FOGAPE on firms' non-FOGAPE debt growth rate at different horizons with respect to the baseline period of six months before the loan. Panel B does the same for actually receiving a FOGAPE loan. Non-FOGAPE debt is the firm's bank debt excluding debt held by treated firms at the bank that gave them the FOGAPE guarantee (but retaining their debt at other banks). We use the Davis et al. (2006) growth measure, using as the base period the average of six months before the focal period, t. This growth measure divides the difference between t and the base period (the average of t-6 to t-1) by the average of the two periods.

Table 6 - Number of bank relationships for single bank firms

Panel A: Reduced form RDD estimates of number of bank relationships

	lags and leads from focal period (months)									
	-2	**-1**	**0**	**1**	**3**	**6**	**9**	**12**	**15**	**18**
Coefficien	**0.007**	**0.007**	**0.011***	**0.025*****	**0.021*****	**0.025*****	**0.036*****	**0.025****	**0.027****	**0.005**
s.e.	[0.005]	[0.006]	[0.006]	[0.007]	[0.008]	[0.009]	[0.010]	[0.011]	[0.012]	[0.015]
# obs.	80,539	80,731	81,303	80,252	79,146	77,883	76,844	69,170	58,515	48,405

Panel B: Fuzzy RDD estimates of number of bank relationships

	lags and leads from focal period (months)									
	-2	**-1**	**0**	**1**	**3**	**6**	**9**	**12**	**15**	**18**
Coefficien	**0.397**	**0.439**	**0.539***	**1.208*****	**1.010****	**1.156*****	**1.576*****	**1.277**	**1.630**	**0.703**
s.e.	[0.26]	[0.29]	[0.33]	[0.37]	[0.40]	[0.44]	[0.56]	[0.89]	[2.12]	[1.35]
# obs.	80,539	80,731	81,303	80,252	79,146	77,883	76,844	69,170	58,515	48,405

Panel A reports RDD estimates of the effects of *eligibility* for Fogape on a firm's number of bank relationships, conditional on having only 1 bank relationship four months before the focal period. Panel B reports estimates of the effects of *receiving a loan guaranteed by FOGAPE* in the same sample. Number of bank relationships refers here to relationships that contain at least 20 percent of the firm's total debt. Estimates are presented starting two months before because the requirement that firms have only 1 bank four months before renders estimates before -2 uninformative. The bandwidth for these regressions is 10,000 UF, and the regression discontinuity is estimated with a third degree polynomial on either side. While the dependent variable in this regression is discrete (a count), we obtain very similar results using an indicator for having more than one bank relationship, but we present these results as they are more informative regarding the dynamics after 6 months.

Table 7 - Probability of non-performing loans

Panel A: Reduced form RDD estimates of probability of delinquency

	P(become non-			P(non-performing)			% non-performing		
	-6	**12**	**18**	**-6**	**12**	**18**	**-6**	**12**	**18**
Coefficient	**-0.002**	**-0.003**	**0.000**	**-0.004**	**-0.002**	**0.000**	**0.002**	**0.000**	**0.002**
s.e.	[0.004]	[0.005]	[0.005]	[0.005]	[0.006]	[0.006]	[0.002]	[0.004]	[0.006]
# obs.	31,235	31,235	31,235	31,235	31,235	31,235	30,141	27,409	19,401

Panel B: Fuzzy RDD estimates of probability of delinquency

	P(become non-			P(non-performing)			% non-performing		
	-6	**12**	**18**	**-6**	**12**	**18**	**-6**	**12**	**18**
Coefficient	**-0.089**	**-0.131**	**-0.016**	**-0.174**	**-0.085**	**-0.021**	**0.071**	**-0.028**	**0.060**
s.e.	[0.166]	[0.182]	[0.172]	[0.184]	[0.232]	[0.222]	[0.073]	[0.163]	[0.201]
# obs.	31,235	31,235	31,235	31,235	31,235	31,235	30,141	27,409	19,401

Panel A shows RDD estimates of the effects of eligibility for Fogape on measures of repayment behavior on loans six months before the focal period, and twelve and eighteen months after. Panel B shows fuzzy RDD estimates of the effects of receiving a loan guaranteed by Fogape on repayment behavior. A fraction of a loan is considered non-performing if a principal payment is overdue by more than90 days, which is also referred to as default in the text. P(become non-performing) is an indicator equal to one if at least one of the firm's loans becomes not performing, given that the firm had no non-performing loans in the previous period. P(non-performing) is similar, but is not conditional on the firm having no non-performing loans in the previous period. % non-performing is the fraction of the firm's total bank debt that is non-performing.

Table 8 - Default behavior conditional on having received a FOGAPE loan

Panel A: P(become non-performing)

	All		Bandwidth			
			30k+	**20k-30k**	**10k-20k**	**0k-10k**
	(1)	(2)	(3)	(4)	(5)	(6)
Fogape bank	0.026%**	0.079%***	0.179%***	0.008%	0.079%***	0.087%***
s.e.	[0.00012]	[0.00015]	[0.00067]	[0.00040]	[0.00022]	[0.00023]
As % of P(.)	4.6%	14.0%	31.5%	1.3%	13.9%	15.3%
Constant	0.569%					
s.e.	[0.00011]					
Firm-Month FE	No	Yes	Yes	Yes	Yes	Yes
Bank relationship controls	No	Yes	Yes	Yes	Yes	Yes
Observations	1,608,330	1,608,330	54,360	163,102	635,959	754,909
Firms	31,264	31,264	2,268	6,259	16,971	23,934
Adj. R2	0.00	0.06	0.08	0.07	0.05	0.06

Panel B: P(non-performing)

	All		Bandwidth			
			30k+	**20k-30k**	**10k-20k**	**0k-10k**
	(1)	(2)	(3)	(4)	(5)	(6)
Fogape bank	0.206%***	0.434%***	0.398%**	0.155%	0.308%***	0.597%***
s.e.	[0.00053]	[0.00056]	[0.00202]	[0.00127]	[0.00072]	[0.00089]
As % of P(.)	9.5%	20.0%	18.4%	7.2%	14.2%	27.5%
Constant	2.168%					
s.e.	[0.00050]					
Firm-Month FE	No	Yes	Yes	Yes	Yes	Yes
Bank relationship controls	No	Yes	Yes	Yes	Yes	Yes
Observations	1,710,282	1,710,282	57,890	172,653	672,527	807,212
Firms	31,954	31,954	2,344	6,456	17,423	24,717
Adj. R2	0.00	0.27	0.24	0.25	0.24	0.28

This table presents coefficients from regressions of frequencies of non-performing loans from banks granting a guarantee versus from other banks, given that the firm received a guaranteed loan in the previous 24 months and that the firm borrows from at least two banks. P(become non-performing) is an indicator equal to one if at least one of the firm's loans becomes not performing, given that the firm had no non-performing loans in the previous period. P(non-performing) is similar, but not conditional on the firm having no non-performing loans in the previous period. A loan is considered non-performing if a principal payment is overdue by more than three months. Bandwidth regressions divide the sample into different categories depending on the firm's annual sales, using the IRS' FOGAPE measure. Bank relationship controls are (i) number of months since the firm obtained its first loan from the bank, and (ii) the fraction of debt with the bank over the firm's total debt. All regressions are estimated by OLS with robust standard errors.

Table 9 - Elasticity of real variables with respect to bank debt

	Employment	Permanent workers	Temporary workers	Cumulative sales	Cumulative input purchases
Coefficient	**0.48****	**0.45***	**0.06**	**0.50***	**0.56***
s.e.	[0.24]	[0.24]	[0.80]	[0.28]	[0.29]
# obs.	14,059	13,691	9,110	23,596	23,624

This table reports instrumental variable estimates of the effect of firms' total bank debt growth on the growth rate of several real variables twelve months after receiving a guaranteed loan, in a bandwidth of 1,500 UF, for 2011-2012. Eligibility for FOGAPE is used as an instrument for bank debt growth. Total debt is the firm's debt across all banks. We use the Davis et al. (2006) growth measure, using as the base period the average of six months before the focal period, t. This growth measure divides the difference between t and the base period (the average of t-6 to t-1) by the average of the two periods.