

Are Small Businesses Worthy of Financial Aid?

Evidence From a French Targeted Credit Program*

Laurent Bach[†]

Stockholm School of Economics

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Abstract

We ask whether public financial aid reduces small businesses' credit constraints. To answer the question, we analyze a policy of bank loans made from subsidized funds. Extensions of this large program are plausibly exogenous and help identify its effects. Using firm-level data, we find that the program substantially increases debt financing without substitution between subsidized and unsubsidized finance. Returns on subsidized debt are significantly above its market cost, with no subsequent surge in default risk. We interpret this as evidence that targeted firms are credit-constrained and underline the implied welfare differences between upfront financial aid and public guarantees.

Keywords : Banking, Credit Constraints, Small Business Lending, Financial Aid

JEL : G21, G28, L8

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[†]Stockholm School of Economics, Sveavägen 65, Stockholm, Sweden. Tel.: +46 8 736 9157. E-mail : laurent.bach@hhs.se.

1 Introduction

The existence of financial constraints is the major argument for financial aid given to small and young firms. Such policies are widespread in both developed and developing countries: they include targeted loans, public guarantees, and start-up and innovation grants. What is more, the scope of these policies is substantially widened in times of recession. For instance, in the midst of the prolonged slump that followed the 2008 financial crisis, President Obama proposed, on February 2, 2010, that \$30 billion be transferred to a new small business lending fund, which would help small banks access capital in exchange for higher lending to small businesses. There is however surprisingly little we know about such public interventions and, while there is a growing consensus that small firms are particularly credit-constrained¹, it is still unclear whether existing public policies actually reduce these constraints.

In this paper, we aim to evaluate the efficiency of one of these policies, that is, targeted loans, taking advantage of a natural experiment in France that consisted of a sudden increase in the volume of a lending program named CODEVI taking place between 1994 and 1996.² We show that this liquidity shock represents a rare opportunity for the identification of credit constraints because some sectors were not eligible for these targeted loans, even though they were similar in other respects to eligible sectors. Furthermore, the program we are dealing with in this paper has real economic significance, since it subsidized a little less than 10% of the *stock* of all corporate long-term loans at the time of the shock, while more than 60% of *new* long-term loans made to then-eligible firms came from CODEVI funds.

The CODEVI program consists of letting banks collect French households' savings into tax-free accounts and then lending these funds to firms belonging to specific sectors of the economy that make less than €76 million in annual sales. This description puts the program in the category of upfront financial aid, by which we mean policies that subsidize interest payments or provide funds to firms deemed to be financially constrained. As opposed to public guarantee programs, in which there is public spending only in the case of a default on the subsidized loan or when the value of outside equity goes below a certain threshold, upfront financial aid implies that public funds are

¹One can cite, among many others, Gertler and Gilchrist (1994), Kashyap et al. (1994), Almeida et al. (2004), Ashcraft (2005), Beck et al. (2005), and Khwaja and Mian (2008).

²CODEVI stands for COMptes pour le DEVELOppement Industriel.

spent either as long as the company is doing well, as is the case for subsidies on interest payments, or at the time of firms' initial investment, as is the case for public lending policies and grants. This distinction matters because most modern financial aid to small and young firms is indirect rather than direct. Since they are ill-equipped to pick "winners" and monitor firms, public authorities typically delegate the task of selecting and monitoring subsidized firms to private or semi-private entities such as banks, bank consortiums or venture capital funds. This in turn raises a new kind of moral hazard situation between such intermediaries and public authorities that is only reinforced if the program consists of providing subsidies only in "bad" states of the world, as is the case for public guarantees but not for upfront financial aid.

Using exhaustive firm-level data drawn from fiscal receipts over the years 1992 to 1999, we estimate the effect of eligibility for the CODEVI program on the availability of bank finance with a differences-in-differences approach. We test the credit constraints hypothesis by looking at the changes in real and financial outcomes of targeted firms following reforms of the lending policy. Results indicate that firms from eligible sectors increased the growth of their loan stock by 8 percentage points, on average, due to the reforms. In the meantime, targeted firms did not reduce their use of unsubsidized finance whatever maturities we consider, and the economic return on newly incurred debt was significantly greater than 14.5% a year, well above the French market cost of bank debt as measured by the average interest rate for similar loans. These policy shocks did not significantly affect the probability of default. Taken together, these results suggest the existence of sizeable credit constraints among French small businesses in the mid-nineties.

Evidence on small business lending programs using microeconomic techniques is narrow and recent. This is partly because the policies are very diverse and country-specific (see, e.g., Beck et al., 2010). Another problem is that the pool of entrepreneurs who benefit from these programs is not randomly drawn, which may bias OLS and matching estimators in all sorts of directions. This is the main problem that studies on SBA programs, such as Lerner (1999), have been facing.

Solving this problem typically involves analyzing "natural experiments." This is what Lelarge et al. (2008) attempt to do with loan guarantees in France. Using the extension of public guarantees to new sectors as a source of identification, they find that loan guarantees substantially increase firms' debt capacity but also significantly raise the probability of future bankruptcy. Regarding targeted loans, we know of only three papers that evaluate such programs with a formalized identification

strategy. The one closest to our study is by Banerjee and Duflo (2008), who analyze an extreme form of targeted loans: in India, banks are required to lend a large fraction of their funds to small businesses. Using changes in the defining sales threshold as a source of identification, they find large effects of being “prioritized” on bank credit and profit growth. However, in contrast to our contribution, the data they use are limited to clients of a single bank and their identification strategy has a major shortcoming in that it cannot control for size-specific macroeconomic shocks. A second related contribution is Paravisini (2005), who analyzes the effect of a targeted lending program organized by the World Bank on small Argentinian firms. He finds that funds awarded to local banks do not allow eligible firms to substantially increase their borrowing because of the fungibility of subsidized and unsubsidized funds in banks’ balance sheets³. One last related paper is Zia (2008), which studies the impact of the withdrawal of the yarn sector from subsidized export loans in Pakistan. Zia finds that this event had a significantly negative effect on yarn exports of private firms but no effect at all on the exports of listed firms, although half of the subsidies used to be channelled toward the latter group.

In this paper, we use a similar “natural experiment” methodology to evaluate credit policies. But since previous results are very specific to the policy and financial context under study, the evaluation of a slightly different policy in a financially developed country can deliver new critical insights. In our case, the CODEVI policy was not fully captured by unconstrained firms, and it led to the realization of positive NPV projects, in stark contrast to the existing literature and the prior that lending policies should have much less of an effect in a developed country. We argue that this difference comes from the fact that when the reforms we look at were implemented, subsidized funds were so sizeable that they were only imperfectly fungible with regular sources of funds at the bank level, and that unlike the system of loan guarantees, the policy did not reduce banks’ incentives to monitor borrowing firms.

The main implication of these results is that upfront interventions in lending markets may actually work and have less perverse effects than currently popular public guarantees. We conjecture that the latter kind of intervention is often preferred for political economy reasons: at the time of their enactment, upfront interventions have an instantaneous and certain negative effect on public

³Note that some results from this piece of research were subsequently published in a different paper (Paravisini, 2008) whose focus is on bank-level credit constraints rather than on the targeted credit policy itself.

finances, while public guarantees carry these costs only in the future and in fewer states of the world. In the absence of a correct public accounting system for these liabilities and with short-sighted policymakers, this makes loan guarantees appear to be a cheap way to lever up public interventions helping finance small and young firms.

Our paper also contributes to the literature dedicated to the measurement of credit constraints initiated by Fazzari et al. (1988). What makes this literature controversial is a fundamental identification problem in the sense that, theoretically, a simple positive correlation between higher internal resources and higher investment may not reflect a purely causal relationship (see, e.g., Altı, 2003). A large literature has tried to get around this problem through either structural estimation of financial market imperfections (e.g., Hennessy & Whited, 2007; Aubier and Cherbonnier, 2007) or the use of exogenous credit supply or wealth shocks as natural experiments (e.g., Blanchard et al., 1994; Lamont, 1997; Rauh, 2006; Chaney et al., 2009). However, in the former case, the results remain very dependent on strong assumptions regarding financial institutions and firms' behavior, while in the latter case the results are restricted to very specific contexts, such as US listed firms or businesses in developing countries. Therefore, using French evidence is particularly useful, since France is one of the few places in which extensive data on private businesses are available and which can be considered financially developed: according to Djankov et al. (2007), France is in the highest quintile of countries in terms of the ratio of private credit to GDP.

The remainder of the paper is as follows. In section 2, we present the functioning and evolution of the French targeted lending program called CODEVI. In section 3, we describe the data collection as well as some descriptive statistics. In section 4, we detail our identification and estimation strategy for the measurement of credit constraints, while section 5 gives the results of these estimations and their interpretation. Section 6 concludes the paper.

2 Institutional Background

The CODEVI policy gives incentives to individual savers to lend to small businesses, through the intermediation of banks. The following description clarifies how reforms of the program have engendered a very significant positive liquidity shock for some groups of small businesses circa 1995.

2.1 The CODEVI Savings Accounts

The first component of the CODEVI policy is an account similar to Roth individual retirement accounts (IRA) in the US. Withdrawals and interest revenues are free of income tax: the implied fiscal loss was equal to about 11 cents per euro accrued in 2006, which is why there is a contribution limit, equal to €6000 in 2011. CODEVI funds benefit from the same deposit guarantee system as current accounts, thus providing very high liquidity, but there is a major drawback in that savers cannot choose how funds are invested or the rate of return. In the period we are interested in, the interest rate was always set by the state in such a way that savers would quickly hit the contribution constraint: the only significant changes in volumes came from changes in contribution limits in the last quarters of 1993 and 1994,⁴ which amounted to a sudden doubling in the maximum volume of CODEVI accounts. This increase in CODEVI savings does not seem to have caused a reduction in other kinds of savings. In Figure 3 of Appendix A, we show the monthly evolution of deposits in CODEVI accounts on the one hand, and in unsubsidized yet similarly liquid savings accounts⁵ on the other hand. As it turns out, the latter form of savings was not negatively affected by the increased availability of subsidized savings accounts around 1994-1995. This suggests that the reforms increased the total volume of savings and did not just lead to a reshuffling of deposits within the banking system. In turn, these reforms sharply increased the amount of funds that banks had to use according to CODEVI-specific guidelines.

2.2 Banking Characteristics of the CODEVI Funds

For banks, the main advantage of CODEVI accounts is that they can be managed not only by not-for-profit banks, as is the case for many tax-free savings accounts, but also by private banks. However, this comes at a cost for banks, since the CODEVI funds are to be invested along the lines dictated by the French treasury: a sizeable portion of the funds is transferred to a public venture capital agency, but most of the funds are converted into corporate loans, many characteristics of which are determined by the state. Due to these restrictions, the program essentially matters only for small businesses' financing.

⁴Starting from €1500 in 1983, the contribution limit moved to €2250 in 1990, €3000 at the end of 1993, €4500 at the end of 1994, and €6000 in 2007.

⁵These accounts are grouped under the category "Livrets soumis à l'impôt" in money aggregates provided by the French central bank. Both kinds of accounts belong to the "M2" money aggregate.

The loans must have terms longer than two years, but the regulations for the interest rate are looser: there is a maximum interest rate determined by the state, but it was not regularly updated. As a result, around 1995, its level was too high to be binding for banks. In fact, the major influences on the interest rate of loans come from the cost of collecting savers' money into specific accounts and from the rate of return on the CODEVI accounts that is determined by the state. Only if interest rates are low by comparison with central bank interest rates may the interest rates on CODEVI loans be lower than average. Firms borrowing on CODEVI funds must be small businesses, which means annual sales below €76 million and not being owned by a firm whose annual sales are above that threshold, and belong to specific sectors. Initially, only firms belonging to industrial sectors were eligible, and after 1993, wholesale trade firms became eligible, but with no real effects until the end of 1994 as will be explained in the next subsection. Finally, in January 1996, retail trade firms (excluding supermarkets) became eligible for CODEVI loans. Quarterly data from the treasury about the sectoral destination of new loans made with CODEVI funds, as shown in Figure 4 of Appendix A (available online), show that the widening of eligibility criteria at the beginning of 1996 took real effect very soon after its official implementation. This suggests that rules dictated by public authorities as to the destination of the loans are relatively well enforced. Indeed, treasury officers have great powers of investigation whenever they suspect breaches to CODEVI regulations and there are substantial financial penalties in case of non-compliance. Because banks still bear the losses in case of default, they have an incentive to provide these CODEVI funds to eligible firms that would have obtained a loan in the absence of the policy. It is only when subsidized funds are sufficiently abundant and restricted in their destination that they may alleviate small businesses' credit constraints.

2.3 Impact of the CODEVI Program on Small Businesses

The amount of money banks have to convert into loans to small businesses depends on the contribution limit of CODEVI accounts. In Figure 1, we show the evolution of the stock of CODEVI loans; unsurprisingly, the major shock comes from the increases in the contribution limits in 1993 and 1994. It takes some time however before these changes have an effect on the supply of loans. Figure 1 shows that it is only in 1995 that the supply of CODEVI loans reacts to the increase

in the supply of CODEVI funds, in spite of substantial financial penalties⁶ in case banks do not use the money from CODEVI accounts to make loans. The main reason is that CODEVI funds are first centralized within each bank at the national level and then reallocated to bank branches across France for the purpose of providing CODEVI loans. This process is not integrated into banks' common routines and for this reason it has been estimated that banks were finally able to authorize a new generation of CODEVI loans only by the very end of 1994⁷.

Figure 1 about here.

According to central bank statistics, the stock of CODEVI loans at the end of 1996 represented only 9.4% of the total stock of *all* more-than-one-year loans to *all* French firms. This suggests that changes in CODEVI contribution limits could only have a small *upwards* effect, if any, on market interest rates and that we should not expect large general equilibrium effects contaminating our interpretation of the effects of the reforms. If anything, money aggregates suggest that these reforms increased total savings, so we should expect market interest rates on company loans to go *down* or remain *constant*⁸, in which case general equilibrium effects would lead us to underestimate credit constraints.

What remains is that the importance of the CODEVI reforms for *eligible* firms is large. One of the biggest French banks, Société Générale, revealed in a parliamentary report that CODEVI loans represented 61% of the production of authorized new loans to CODEVI-eligible firms by the end of 1994, i.e., after the last increase in the contribution limit for the account (Loridant and Marini, 1995). In addition to this, quarterly qualitative surveys by the Banque de France on firms' credit conditions mention that from the fourth quarter of 1994 to the second quarter of 1996, most of the new loans to eligible small firms were made with CODEVI funds.

In brief, at the very end of 1994, an unanticipated change in the limit of the CODEVI accounts triggered a huge increase in the supply of subsidized loans that were at the time unavailable to retail trade businesses. Therefore, small wholesale trade firms had much better access to credit

⁶Essentially, the unused funds must then be transferred to the State against interest payments below the rate of return awarded to savers in CODEVI accounts.

⁷See Loridant and Marini (1995) for further explanations and bank executives' accounts on this issue.

⁸This last point was clearly made in the seminal theoretical contribution on credit rationing by Stiglitz and Weiss (1981).

after the last quarter of 1994, but this specificity with respect to retail trade lasted only until December 1995, when the CODEVI funds were also opened to retail trade firms. Such an increase in the supply of funds to eligible firms does not imply that the supply of funds to the banking system as a whole has significantly increased, even though money aggregates suggest it did. What is really crucial to our analysis is that it is the *relative* supply of funds to a specific set of firms that increased exogenously, as in Peek & Rosengren (1995).

2.4 The Economic and Political Context of the CODEVI Reforms

Our identification strategy relies on the interaction of two sets of changes, the new eligibility of wholesale trade and the expansion of CODEVI savings accounts, each of which must receive a proper explanation in order to give some causal credibility to our results. First of all, it may appear intriguing that wholesale trade firms were awarded eligibility to CODEVI loans in 1993 while they became eligible to loan guarantees under the SOFARIS program only after 1995⁹. Both programs were initially designed for industrial sectors because it was commonly believed in the eighties in France that industry needed a specific boost from the state, at the expense of service sectors typically likened to “mom and pop” stores unlikely to provide many jobs¹⁰. However, the CODEVI program originally had a more flexible perimeter in that it could be changed by a simple private letter sent by Treasury authorities to French banks¹¹, while the perimeter of the SOFARIS program always had to be determined by law¹². This explains why, given the general goal of helping small business owners further, the CODEVI programme saw its perimeter extended more quickly than was the case for the SOFARIS loan guarantees. Eventually, service sectors came to be seen as important sources of jobs and were subject to more and more benevolence from the state. This recognition came first to wholesale trade because retail trade firms used to epitomize the “mom and pop” store more than any other service sector; this in turn explains why the CODEVI was first extended to wholesale trade. This also means that any differences we observe between wholesale and retail trade firms around 1995 do not reflect the effect of the SOFARIS loan guarantee program

⁹The latter policy change was analyzed in detail by Lelarge et al. (2008).

¹⁰See Cohen (1989).

¹¹See Loridant and Marini (1995).

¹²This difference is in the end due to public accounting differences between the two schemes: the CODEVI is considered as a tax expenditure to the benefit of savers, while SOFARIS is labelled as direct public spending to the benefit of small businesses.

since both sectors were simultaneously excluded and then included in its perimeter.

It remains to be understood why there was such an expansion of CODEVI accounts leading to a sharp increase in the supply of CODEVI loans after the last quarter of 1994. The main underlying economic fact is the 1993 recession, during which it was commonly believed that small firms were badly hit. In this context, the incoming 1995 presidential election and the competition between two politicians belonging to a party traditionally favored by small business owners triggered a series of measures in their favor until the end of 1995¹³. Thus, because special interest groups pushing for the CODEVI reforms were defined by firm size, and because small and large firms have different exposure to business cycles, firm size cannot help us distinguish businesses that exogenously had better access to finance thanks to the reforms. In our case, it is then only in the sense that wholesale trade firms are, on average, slightly bigger than retail trade firms that the two categories may have had different growth paths in the absence of our identifying reforms. That is why our differences-in-differences assumption will be valid only conditional on initial size and therefore requires using firm-level data.

Another potential problem is that wholesale and retail trade sectors may react differently to business cycles¹⁴. However, figures on sectoral averages of debt growth drawn from our firm-level sample (as shown in Figure 2) show that these two sectors' business cycles are, in fact, similar except for the impact of the CODEVI reforms in 1995. French national accounts (as shown in Appendix A available online, Figure 5) are consistent with the hypothesis that retail and wholesale trade had naturally quite similar business cycle patterns before the reforms.

Figure 2 about here.

¹³The most conspicuous ones are the bankruptcy law of 1994, the expansion of loan guarantees at the end of 1994, and the introduction of a reduced corporate tax rate for small businesses starting in 1997.

¹⁴This is not however the first paper considering these sectors as comparable: Costa (2000) also uses these two sectors as treatment and control groups, albeit in a US context.

3 Data

3.1 Data Collection

Our identification strategy implies that we gather detailed firm-level data. That is why we use the annual French fiscal database of firms' accounts called BRN and provided by the French National Institute of Statistics (INSEE). This data set is described in Bertrand et al. (2007). It gives detailed balance sheet data on about 600,000 firms a year, covering about 95% of total sales in the private sector.¹⁵ It allows us to distinguish different kinds of debt stocks according to the identity of the lender and the duration of the loans. A unique identification number allows us to track each firm from one year to another. Inside this file, we selected all firms that belonged to the trade sectors, except supermarkets, since these firms have traditionally been subject to very specific regulations in France. Since the CODEVI program's eligibility requirements include ownership restrictions, we also made use of the LIFI database from INSEE that details ownership relationships between firms.

Although we cannot distinguish CODEVI debt from other kinds of debt in the data, we are able to distinguish long-term bank debt (including CODEVI loans) from other kinds of debt, so that, instead of knowing the amount of *subsidized* debt, we know the amount of *subsidizable* debt. Such measurement error should only lead to underestimating the real effect of the policy changes. In addition, the administrative nature of the BRN data set entails removing many outliers from the base before we can properly use the data. We selected the firms based on the following criteria over the years 1991-2000 : they have existed at least three years, have had more than 10 employees for at least one year, have had for each year the following ratios between the first and the 99th percentile: ROA, EBITDA over sales, financial debt over fixed assets, fixed assets growth. In addition to this, using logarithms in our regressions mechanically removes firms that have zero employment, value added, costs, fixed assets or financial debt other than subsidizable debt. This selection disproportionately removes very small firms (i.e., fewer than 10 employees) from the sample. To the extent that those firms are likely to be more credit constrained than the average firm, this should bias our results *against* finding significant credit constraints. In addition, the CEO of Crédit Lyonnais (in Loridant and Marini, 1995), one of the major French banks, stated

¹⁵Any firm from the trade sector with annual sales higher than €533,000 is included in the data set. Another tax file, the BRN-RSI, includes firms with sales below that level but is less accurate and gives considerably less detail on firms' financial structure.

that firms between 10 and 100 employees represented 63% of CODEVI loans in number and 67% in amount in 1994. Thus firms with fewer than 10 employees are unlikely to have very significantly benefited from the program.

Finally, in order to avoid any bias due to heterogeneous entry caused by the reforms in question, we decided to remove any firm entering the database for the first time after the first reform, i.e., after 1994. This ensures that the firms present in the sample after the reforms are drawn from the same pool as the ones present before the reforms, which is essential for an unbiased differences-in-differences strategy. In unreported regressions, we also run our analysis on a sample that includes entering firms and none of our results are affected, either statistically or economically.

As a result of this sampling procedure, our database includes 161,170 firm-year observations on trade firms between 1992 and 1999.

3.2 Descriptive Statistics

3.2.1 General Statistics

In Table 1, we present summary statistics samples spanning the period from 1992 to 1999.

Table 1 about here.

The use of working capital credit is unusually high: it is never below 50% of all liabilities on average. This is probably because trade credit is a means to smooth cash outlays in the face of uncertain delivery dates (see, e.g., Ferris, 1981) and, as such, is an integral part of a trade firm's production function. This statement is supported by the fact that the rate of working capital credit is of equivalent size among small (panel A) and big (panel B) firms, even though the latter are supposed to use less trade credit, since they have better access to external finance (see, e.g., Biais et al., 1995).

Note as well that more than 96% of the sample corresponds to firms whose size and ownership patterns make them eligible for CODEVI loans. The remaining 4% is evenly composed of firms that are directly ineligible because their sales are too high and firms that are indirectly ineligible because, even though they have less than €76 million in sales, they are in fact owned by a firm

whose sales are above this threshold. As a result, eligible firms are much smaller than ineligible ones, with an average number of 23 employees for the first group and 286 for the second group.

Among small businesses, the sample is almost equally divided into wholesale trade on the one hand, and retail and car trade on the other hand. Industry classifications do not allow us to distinguish those firms in the car trade sector that belong to retail trade from those that belong to wholesale trade, so we will focus our further analyses on wholesale and retail trade only. Furthermore, this sector benefited from large car scrapping subsidies in 1994 and 1995, which is further reason for caution. All our results have the same economic and statistical significance when we leave them out of the sample.

One should readily observe that wholesale trade small businesses (panel C) are, on average, bigger than those in retail trade (panel D): the former have an average of 25.5 employees while the latter have an average of 18.3 employees, and this difference is even bigger for assets, value added and sales. That is why we have to control for year-to-year size effects in our estimations, since smaller firms were subject to several beneficial policy changes during the period of study and since such a policy bias is actually confirmed by our differences-in-differences statistics. Regarding capital structure, the main fact is that all trade sectors share a tendency to incur massive amounts of working capital credit. However, retail trade firms have a higher financial leverage than wholesale trade firms, most of the difference coming from a larger appetite of retail trade businesses for long-term debt. This per se does not put into question our identification strategy, as the latter is perfectly compatible with differences in *levels* between our treatment and control groups. In addition, nothing in our description of the political decision process suggests that such differences in financial behavior triggered the specific treatment awarded to wholesale trade, let alone the expansion of CODEVI accounts. Yet, those differences impose that we pay a lot of attention to period-by-period statistics.

3.2.2 Differences-in-differences Statistics

In Table 2, we present sample means for the main variables of interest across different subsamples in terms of period, size and sector. Table 2 focuses on the patterns of financial debt growth over the period 1992-1999.

Table 2 about here.

The period is one of intense deleveraging in the French economy: the ratio of financial debt to fixed assets decreased by 35% in French nonfinancial corporations, according to estimates from the French central bank¹⁶. This was primarily due to very high interest rates following the 1992 currency crisis: annualized interest rates on 3-month French T-bills reached 12% in January 1993 and only slowly went down to a plateau of 3.5% in 1997-1999. This deleveraging shows in our own sample as financial debt of small firms went down by little less than 30% from 1991 to 1999.

Yet for our purpose, the most striking feature of the data is that a very significant difference arises in 1995 between the firms considered as sufficiently small to be eligible for CODEVI and the firms left out of the program because they are too large. Indeed, in 1995, average credit growth was 12.6 points higher among smaller firms. This gap is significant among both retail and wholesale trade firms, albeit it is bigger in the latter group. We take this as a reflection of the large array of policies that were enforced in favor of small business finance after the 1993 recession. It also suggests again that using the small vs. large distinction as an identification strategy is unlikely to lead to a clear exclusion restriction. This is why in the rest of the paper, we focus on the sample of small businesses. In this group, the impact of the CODEVI policy on financial debt is reflected in the data: while small wholesale trade firms and small retail trade firms had no significant difference in average credit growth before and after 1995, there was a 7.3-point credit growth gap in favor of wholesale trade firms during the year 1995, i.e., the year when this group was the sole beneficiary of CODEVI loans.

The other elements of Table 2 allow us to go further and analyze the patterns of unsubsidizable debt, value added and profits across sectors and periods. While unsubsidizable debt increases sharply in 1995 among small businesses relative to large businesses, we do not detect any significant difference between wholesale and retail trade for this credit component. This is further evidence that wholesale and retail trade differ in 1995 only because of the new CODEVI loans the former group of firms received. It also shows that the reform did not lead newly eligible firms to substitute CODEVI loans for other unsubsidized sources of credit. The results regarding value added and profits are as compelling: while there was no difference between wholesale and retail during the

¹⁶Detailed figures are available in Durant and Girard (2004).

periods 1992-1994 and 1996-1999, we can detect at the end of 1995 a 2.9-point and a 6.3-point annual growth gap in favor of wholesale trade in terms of value added and profits, respectively. These sample means hint at the substantial financial and real effects of the CODEVI reforms that took place in 1994 and 1995. However, in order to get to more substantive conclusions, we must provide a more formalized identification strategy and run a regression analysis.

4 Establishing Credit Constraints

4.1 Theory

Even though our differences-in-differences approach allows us to control for differences in the *level* of the loan demand curve between treated and control firms, it does not *per se* allow us to control for potential movements *along* targeted firms' loan demand curve that would be triggered by access to the targeted lending policy. In particular, one could easily imagine that treated firms increased their bank credit simply because banks decided to lower interest rates *specifically* for those firms. To disentangle the price and quantity effects of the reforms and assess the existence of credit constraints using shocks to the availability of subsidized financing, we distinguish two different methods that are backed by a partial equilibrium model of the credit market presented in appendix B (available online), along the lines of Banerjee & Duflo (2008).

4.1.1 Reduced-form Identification

The first method consists of estimating the joint evolution of subsidized and unsubsidized financing of targeted firms following an expansion in the availability of subsidized loans. The intuition is that in the absence of financial constraints, such an expansion should either have no effect on total external financing or have a positive effect on total external financing together with a negative effect on unsubsidized financing. Therefore, *if following the reforms, firms increase their subsidized sources of finance without substituting for unsubsidized finance, then it must be that these firms are indeed credit constrained.*

This should provide a very straightforward test for the existence of credit constraints, relying on reduced-form effects of the CODEVI policy. This test is different from the one proposed by Banerjee and Duflo (2008). They have data on interest rates but not on other forms of lending by

firms; this is the reverse in our contribution, which is why our reduced-form identification strategy differs from theirs. However, this relies on the assumption that unsubsidized sources of finance, such as loans from non-financial institutions, less-than-two-year bank loans or working capital credit, are substitutable with the CODEVI loans to a significant degree. To address this potential weakness, our baseline estimation of the effect of the reforms on unsubsidized credit excludes those sources of finance that could be considered poor substitutes for CODEVI loans, such as working capital credit.

4.1.2 Structural Identification

The second identification strategy is more structural in the sense that it is derived from a more refined exclusion restriction: scenarios in which firms are not credit constrained imply that the profitability of additional investments linked to the CODEVI reforms is lower than the unsubsidized cost of funds, while the reverse is true when targeted firms are credit constrained. Thus, *if following the reforms, firms increase their bank credit and increase their profits in such a way that the implied marginal profitability of debt is greater than the market cost of bank debt, then it means that those firms are credit rationed.*

It should be clear that we do not compare *average* profitability with the *average* cost of capital. We compare instead the *marginal* effect of additional debt on before-interest profits with the *marginal* cost of this debt, which is the market cost of bank debt. The exact comparison is between before-interest marginal profits conditional on not going bankrupt, and the ex-ante market interest rate for company loans. This implicitly takes into account default probabilities, since in equilibrium the market interest rate for company loans should be equal to the opportunity cost of funds for outside investors divided by the probability of default. This may pose a problem if firms receiving CODEVI loans pick riskier projects and have higher bankruptcy rates than average, thus requiring a higher risk premium than the one implicit in the measure of the average cost of debt we will use. However, we can test this hypothesis by looking at the evolution of bankruptcy rates following eligibility for the policy.

4.2 Reduced-form Estimates

4.2.1 Effect of the Reforms on Debt

The empirical strategy takes advantage of a sudden difference in access to subsidized loans between small wholesale trade firms and small retail trade small businesses, from the last quarter of 1994 to December 1995. However, the first stage consists of checking that there was indeed such a differential shock. In order to do so, we estimate an equation in the sample of small businesses of the form:

$$\log(d_{it}) - \log(d_{it-1}) = \alpha_d X_{it} + \gamma_{1d} WS_i * POST_{1t} + \gamma_{2d} WS_i * POST_{2t} + \varepsilon_{dit} \quad (1)$$

where d_{it} is a measure of total financial debt for firm i in year t , WS_i is a dummy indicating whether firm i belongs to the wholesale trade sector, $POST_{1t}$ (resp. $POST_{2t}$) is a dummy equal to one in the years after 1994 (resp. after 1995)¹⁷ and X_{it} is a set of controls, including year dummies, a size variable¹⁸ with year interactions, and 2-digit sectoral dummies. We also include in our regressions dummies for trade in car-related goods interacted with year dummies in order to control for imprecisions in industry classifications and car-specific policy measures.

This means that $WS_i * POST_{1t}$ and $WS_i * POST_{2t}$ are dummies for the respective effects of granting a privilege to wholesale trade and of removing this privilege. Because of year fixed effects and sectoral fixed effects, our estimations of the impact of the reforms account for common trends in the demand and supply of debt and for structural differences among sectors in the demand and supply of debt. We can only look at the short-term effects of access to CODEVI because the period during which retail and wholesale trade had opposite eligibility status is reduced to one year.

Since debt stocks are very persistent and have fat-tailed distributions, we focus on the first difference in logs of debt stocks. For similar reasons, this transformation will be applied to most of the other outcomes we look at in this paper. Finally, we choose to focus on total financial debt, rather than just subsidizable financial debt, because our theory predicts that by looking only at the evolution of subsidized debt one may overestimate credit constraints, due to substitution effects between subsidized and unsubsidized components of financial debt. Moreover, the estimates of the

¹⁷Both reforms started to have real effects at the beginning of the calendar year, so that our data take into account what happens the year after the reforms.

¹⁸The logarithm of number of employees in period $t - 1$.

implied return on debt that we will compute later on have to be compared to the market interest rate applying to financial debt when it is not subsidized, and not just to the interest rate that applies to subsidized debt.

4.2.2 Effect of the Reforms on Alternative Sources of Credit

Our first identification strategy suggests that we check whether the extension of the CODEVI program had a negative effect on the demand for the kinds of financial debt not supplied with CODEVI funds, such as bond credit, loans from financial institutions other than banks, and less-than-two-year bank loans.

That is why we estimate an equation parallel to (1), again in the sample of small businesses:

$$\log(d_{it}^{ns}) - \log(d_{it-1}^{ns}) = \alpha_{d^{ns}} X_{it} + \gamma_{1d^{ns}} WS_i * POST_{1t} + \gamma_{2d^{ns}} WS_i * POST_{2t} + \varepsilon_{d^{ns}it} \quad (2)$$

where d_{it}^{ns} is the amount of debt other than more-than-two-year bank loans incurred by firm i in year t . We argue that firms are credit constrained if γ_{1d} and γ_{2d} , from Equation (1), are positive on the one hand, and if $\gamma_{1d^{ns}}$ and $\gamma_{2d^{ns}}$ are equal to zero on the other hand. One might, however, not be convinced that the forms of alternative financing we analyze are the only ones that may react to a change in the price of long-term bank debt. This is why we also run an estimation of Equation (2), including equity and working capital credit in our measure of unsubsidized financing means.

4.3 Structural Estimates

In order to be more conclusive, these first estimates should be completed by an analysis of the effect of subsidized debt on real outcomes.

We will first use an IV estimation of the effect of debt on value added, using the CODEVI reforms as a source of instruments for the evolution of debt. The first-stage equation in this setup is Equation (1), where the instruments are the sector-period interactions. It will allow us to estimate the elasticity of output with respect to total debt incurred. The impact of total debt on value added does not, however, inform us about the marginal benefit of the extra investment: entrepreneurs who incurred more debt thanks to the reforms might simply be empire-builders (see, e.g., Stein, 2003). That is why we also perform an instrumental variables estimation of the effect of debt on

profits. The higher the implied marginal profitability of debt is, the likelier it will be that firms in the sample are credit constrained.

Because the logarithm of profits is only defined when profits are strictly positive, there might be a sample selection bias: in our sample, about 19% of observations are then excluded from the analysis. It is not clear, though, how this exclusion should bias our results, since this crucially depends on whether profits are *persistently* or *temporarily* negative. In order to solve that problem, we do a separate instrumental variables estimation for the effect of bank credit respectively on value added and on costs, which always have positive values. The combination of those two estimates allows us to estimate the effect of debt on profits indirectly and check whether there is a selection bias.

4.4 Robustness Checks

4.4.1 Exogeneity of the Reforms

Given our explanation of the political process leading to the natural experiment, we are confident that the inflow of subsidised credit that affected small wholesale trade firms is, by and large, exogenous. However, this assumption should be put to more formalized testing.

Our first test takes advantage of the fact that firms whose size does not fit the criteria of eligibility for the CODEVI loans are not affected by the experiment, whatever their economic sector. If there are differential trends between wholesale and retail trade firms, we should also expect to see them in the case of these bigger firms. That is why we estimate the same Equations (1) and (2) in the sample of non-eligible firms belonging to the trade sectors, which is akin to running a “placebo” experiment. However, this alternative sample is much smaller (5,799 observations instead of about 155,000 in the sample of small firms), so the statistical power of this “placebo” procedure is admittedly weaker than in our “real” experiment; this is why we complement this approach with other robustness checks.

The second method takes advantage of the fact that there were two experiments: a subsidy privilege was awarded to wholesale trade and then removed. This allows us to test whether these symmetric experiments had symmetric effects, which would not be the case if wholesale and retail trade firms were subject to different business trends. In econometric terms, this means that we

perform a Sargan over-identification test following each of our estimations. If there is indeed symmetry in the effects of the natural experiment, then one can reject the hypothesis that our effect is fully driven by unobservable differential trends between wholesale and retail trade as long as such trends go in the same direction during the years 1994 to 1996. This includes the potential differential effect of all general policy changes that took place after 1994 and were sustained thereafter, such as reductions in employer payroll contributions that really started in 1995 (see, e.g., Crépon and Desplatz, 2001).

In a third step, we run differences-in-differences specifications, including linear sectoral trends or lagged sales growth interacted with year dummies, as a further way of testing for unobservable, yet distinct, time trends.

Our last set of robustness checks specifically deals with the potential effects of another regulatory change, the enactment of the Raffarin and Galland laws in July 1996, which imposed heavy regulations on supermarkets. Retail trade firms might have indirectly benefited from the anti-competitive effects of those laws even though, according to Askenazy and Weidenfeld (2007), the main beneficiaries were the existing supermarkets, which are not included in our sample anyway. In order to check further whether this is the case, we take advantage of two aspects of those regulations. First, according to Askenazy & Weidenfeld (2007), the anti-competitive effects of those laws started to take place at the end of 1996 but only very gradually. This smooth profit pattern allows us to test whether these laws strongly bias our results: we compute the estimates of the effects of debt on profits with data up to 1996 and then up to 1997 and then up to 1998, and check whether the difference between the treated and control groups grows over time. Second, the laws were aimed at slowing down the development of hard discounters whose business is essentially retailing food products; so if the new laws explain our estimates, then these should be significantly lower when we control for yearly changes in the food retail sector.

4.4.2 Inference Issues

For differences-in-differences estimates, Bertrand, Duflo and Mullainathan (2004) recommend clustering standard errors at the level of the treatment status. Since, here, treatment status is defined at the 2-digit industry level, this leads to clustering with only four different groups, which may severely bias standard errors downwards. According to Cameron & Miller (2010), this bias may be

very important when the number of groups is smaller than 50. That is why we take a conservative approach to standard errors, which consists of choosing the greatest standard error estimate among a variety of potential estimates defined by their cluster level:¹⁹ either the conventional estimate, the heteroskedasticity-robust one, and the ones correcting for clustering at firm-level, at treatment-period/industry-level, at industry-level, at “région”/industry-level, or at “département”/industry-level. The last two estimates are correct if the within-treatment-status error-dependence comes from a smaller geographic level than the whole country. This is likely to be the case if bank-firm relationships are localized, as is usually the case for small businesses (see, e.g., Degryse et al., 2009).

4.4.3 Default Risk

Our identification strategy may address one last reservation, namely that an increase in output and profits may reflect riskier strategies pursued by firms incurring CODEVI debt, instead of reflecting the existence of credit constraints. In order to address this, we look at the effect of the reforms on the probability of filing for bankruptcy over either the next year, the next three years, or all following years, using the same dependent variables as in Equation (1) in both a Probit and a linear probability model.

5 Results

5.1 Effect of the Reforms on Debt

The first column of Table 3 presents the results of estimating Equation (1) in the sample of small businesses. Giving a privileged access to CODEVI loans to wholesale trade firms increased their financial debt growth by 7.6 points relative to retail trade firms, and the end of this privilege reduced their debt growth by 8.7 points in relative terms. These coefficients are not significantly different from one another: the relative effect of opening the CODEVI program to wholesale trade in 1994 was fully offset a year later when the policy was extended to retail trade firms.

Table 3 about here.

¹⁹Note that such a procedure has been formalized by Yang et al. (2005).

These results confirm the limited aggregate evidence we have that the program was of utmost importance for small businesses around 1995. It also suggests that the policy did not provide pure windfall gains to firms that would have also taken on debt in the absence of these subsidized loans, in opposition to estimates made by Paravisini (2005) in the Argentinian case. This imperfect fungibility of subsidized funds into the pool of unsubsidized funds probably has several causes. French authorities might have been better equipped for monitoring banks, and the restrictions on how subsidized funds could be lent were more stringent than in Argentina. Also, in the Argentinian case, World Bank funds represented only 0.1% of loans, while in our case, CODEVI loans could represent up to 60% of new loans to eligible firms. Therefore, there is a much greater chance that subsidized finance was not inframarginal for banks' lending decisions. Finally, while Paravisini's policy takes place during an economic expansion, the reform we analyze takes place just after what had, by then, been the biggest recession in France since World War II. In this context, banks' lending decisions might have been severely constrained by the level of deposits, so that the large inflow of CODEVI funds at the end of 1994 might have eased banks' constraints on lending to informationally sensitive firms.

5.2 Evidence of Credit Constraints

5.2.1 Reduced-form Estimates

In Table 3, third and fourth columns, we look at the impact of the reforms on total financial debt, excluding more-than-two-year bank credit. The growth of these debt instruments *increased* by 0.9 point in wholesale trade and then *decreased* by 2.1 points following the first and the second reform, respectively, albeit not significantly in statistical terms. So there was no significant substitution between subsidized debt and other kinds of debt. When we include working capital credit and equity in the set of alternative financing means, as is done in the last columns of Table 3, we still do not find a significant slowing down of unsubsidized liabilities following greater access to subsidized loans. These two results and the one in the previous subsection suggest that these small businesses were credit constrained at the time of the reforms, assuming that the reduced-form identification strategy we proposed in section 4 is valid.

5.2.2 Structural Estimates

We present the IV estimates of the effect of debt incurred thanks to the CODEVI reforms in Table 4, column 1.

Table 4 about here.

We can use these estimates of debt elasticities to compute the average increase in profit caused by every euro on loan. In the subset of the sample where EBITDA is strictly positive, the average debt stock (averaging across years) is €454,934 and the average EBITDA is €258,681, so the estimate we obtained in row (3) of Table 4 allows us to calculate that an increase of €100 in the debt stock corresponds to an increase of €47.1 in profits. This is arguably a large point estimate, but one should readily point out that our IV strategy involves large standard errors: the implied change in EBITDA of 47 cents per one euro of additional debt has a standard error of 16.6 and the lower bound of its 95% confidence interval is €14.5, a number that is more reasonable but yet suggests the existence of strong credit constraints.

This estimate may also be biased, since we do not take into account firms whose EBITDA is negative. For that reason, we compute an indirect estimate of the effect on profits through the unbiased estimates we obtained in rows (1) and (2) for value added and operating costs. In the whole sample, the average debt stock (averaging across years) is €460,786, while the average value added and operating costs are equal to €953,326 and €755,236, respectively; therefore, using the coefficients in rows (1) and (2), an increase of €100 in the debt stock corresponds to an increase of €85.7 in value added, and €36.9 in operating costs. This implies an increase of €48.7 in EBITDA for the average firm, which is 3.4% *more* than in the selected sample; it is then plausible that our direct estimate of the effect of debt on profits is not biased upwards.

We now need to assess whether such a return on debt can be explained by a subsidy in the form of reduced interest rates. In order to support this hypothesis, the return of debt we unveiled in our analysis should be equal to or smaller than the interest rate that those firms usually pay for their loans. But according to quarterly surveys on firm loans' interest rates run by the Banque de France, the average interest rate for more-than-two-year bank business loans in 1995-1996 was equal to 8.2%, which is significantly below 14.5%, the lowest bound of the 95% confidence interval

for the marginal return on debt. It could, of course, be the case that the average interest rate measured by the Banque de France corresponds to firms requiring lower risk premia than those in our sample. However, as we will see *infra*, the data do not support this hypothesis. This makes it plausible that targeted firms were credit constrained at the time of the CODEVI reforms.

5.2.3 Robustness Checks

Overall, the claim that the reforms we analyze in this paper are exogenous is backed by our robustness checks.

First, the Sargan statistics shown in Table 4 do not reject the hypothesis of exogeneity of our instrumental variables. This means that awarding and removing the subsidy privilege to wholesale trade firms had symmetric effects, which gives more credence to our assumption of common trends. The regressions with linear trends and lags in sales growth shown in Table 5 do not invalidate this assumption either.

In Table 3, one can check that when firms are not eligible for the program for reasons of size or pattern of ownership, their behavior is not affected by the reforms: the estimates for time-sector interactions are always insignificant. This shows that the results we obtain for the sample of small businesses are not driven by differential sectoral trends.

Table 5 about here.

We can also argue that the Raffarin and Galland laws had no significant effect on our differences-in-differences estimates, since the latter are constant across time (see Table 5, columns 4,5, and 6) and since the inclusion of year dummies for the food retail sector does not significantly change the size of the effects of the CODEVI reform on profits (see Table 5, column 3).

Finally, we look at the effects of the reforms on bankruptcy filing in Table 6.

Table 6 about here.

It appears that neither of the two reforms had any significant impact on the probability of filing for bankruptcy, whatever the window of observation one chooses to look at. This result shows that the superior profitability of investments made by newly eligible firms is not due to more risk-taking

on the lender's side. It comforts us in the decision to compare the marginal profitability of debt with the average interest rate charged for loans similar to the CODEVI loans. It is also in stark contrast to Lelarge et al. (2008) who find very negative effects of loan guarantees on firm survival. In a sense, this was to be expected since public loan guarantees, in effect, provide banks with limited liability. In comparison, subsidies on interest payments included in targeted loan programs do not limit banks' downside risk and therefore are not subject to the same moral hazard problem. In addition, those two French programs did not target the same companies: loan guarantees analyzed by Lelarge et al. (2008) target start-up companies which are very small (less than two employees on average) and have very high bankruptcy rates (up to 24% in their analysis), while the CODEVI program does not put an emphasis on those firms and in our sample, targeted firms are significantly bigger (around 23 employees on average) and thus have smaller average bankruptcy rates (up to 9.5%). One cannot then rule out that these two populations of firms just react differently to the same program.

6 Conclusion

Using shocks to the availability of subsidized credit in France, we estimate the level of credit constraints among small businesses targeted by a lending policy. Overall, the empirical evidence points to large financial constraints among small French businesses circa 1995: these firms do not substitute unsubsidized finance with unsubsidized sources of credit, and the marginal profitability of their incurring an additional euro of debt is much higher than the market cost of debt they face.

These surprising results may have to do with the fact that the program we look at targets small firms. As argued by Stein (2002), in order to reduce internal agency problems, bank owners usually limit the amount of funds a loan officer can provide to clients with barely verifiable prospects, which is often the case for small firms. Unfortunately, we could not find a proper test of that hypothesis, given that we do not have bank-level data. Another aspect is that the natural experiment we took advantage of took place in the aftermath of an important economic recession. If banks' and firms' net worth had been higher, it is possible that the uncovered credit constraints would have been lower. On the contrary, in times of recession, especially when banks' capital is badly hit, our estimates may have strong external validity.

The paper also provides original results regarding the effects of credit subsidy programs. Contrary to existing results, we do not find that French banks were able to perfectly sterilize funds earmarked for small businesses. Above all, our results point to sharp contrasts between upfront financial aid and public guarantees. It has been found that the latter policy increases bankruptcy rates in very high proportions. We could not detect such an effect of targeted loans on bankruptcy rates, probably because such loans do not *per se* discourage banks from monitoring their loan portfolio. This raises the question of why loan guarantees are at present so popular among policymakers, as the number of countries implementing this policy testifies. One of the reasons may be found in political economy, as suggested by Lucas et al. (2004): the budgetary cost of subsidies on interest payments is instantaneous, while the cost of guarantees reveals itself only slowly; so, myopic politicians may be more willing to implement loan guarantees. It might also be that among financially constrained projects, the riskier ones, which are significantly more encouraged by loan guarantees, may also be the ones that deliver more positive externalities, in particular in terms of R&D. This is an empirical issue that we leave for future research.

Table 1: Summary Statistics

| | PANEL A : SMALL FIRMS | | | | | PANEL B : BIG FIRMS | | | | |
|------------------------------|-----------------------|------|------|------|----------|---------------------|-------|-------|--------|----------|
| | Mean | Q25 | Q50 | Q75 | Nb. Obs. | Mean | Q25 | Q50 | Q75 | Nb. Obs. |
| Equity Share | 0.25 | 0.13 | 0.25 | 0.40 | 155371 | 0.25 | 0.12 | 0.25 | 0.38 | 5799 |
| Subsidizable Debt Share | 0.09 | 0 | 0.04 | 0.12 | 155371 | 0.03 | 0 | 0 | 0.03 | 5799 |
| Unsubsidizable Debt Share | 0.13 | 0.02 | 0.08 | 0.18 | 155371 | 0.16 | 0.01 | 0.08 | 0.25 | 5799 |
| Working Capital Credit Share | 0.53 | 0.38 | 0.52 | 0.65 | 155371 | 0.56 | 0.40 | 0.55 | 0.70 | 5799 |
| Financial Debt | 460 | 61 | 162 | 398 | 155380 | 9388 | 290 | 1552 | 7221 | 5799 |
| Capital | 7615 | 1427 | 3057 | 7056 | 155371 | 161859 | 11395 | 44941 | 162547 | 5799 |
| Employment | 23 | 9 | 14 | 24 | 155379 | 289 | 33 | 98 | 279 | 5799 |
| Sales | 5443 | 1296 | 2447 | 5632 | 155380 | 141050 | 11076 | 74607 | 149071 | 5799 |
| Value Added | 953 | 329 | 521 | 988 | 155380 | 15799 | 1687 | 5385 | 16601 | 5799 |
| EBITDA | 198 | 30 | 89 | 211 | 155380 | 4289 | 195 | 984 | 4323 | 5799 |

| | PANEL C : WHOLESALE TRADE (SMALL) | | | | | PANEL D : RETAIL TRADE (SMALL) | | | | | PANEL E : CAR TRADE (SMALL) | | | | |
|------------------------------|-----------------------------------|------|------|------|----------|--------------------------------|------|------|------|----------|-----------------------------|------|------|------|----------|
| | Mean | Q25 | Q50 | Q75 | Nb. Obs. | Mean | Q25 | Q50 | Q75 | Nb. Obs. | Mean | Q25 | Q50 | Q75 | Nb. Obs. |
| Equity Share | 0.27 | 0.14 | 0.26 | 0.40 | 72081 | 0.24 | 0.11 | 0.25 | 0.42 | 47318 | 0.23 | 0.12 | 0.23 | 0.36 | 35972 |
| Subsidizable Debt Share | 0.06 | 0 | 0.02 | 0.08 | 72081 | 0.12 | 0 | 0.07 | 0.19 | 47318 | 0.10 | 0.00 | 0.06 | 0.15 | 35972 |
| Unsubsidizable Debt Share | 0.12 | 0.02 | 0.07 | 0.18 | 72081 | 0.14 | 0.02 | 0.07 | 0.19 | 47318 | 0.12 | 0.02 | 0.08 | 0.18 | 35972 |
| Working Capital Credit Share | 0.55 | 0.41 | 0.54 | 0.67 | 72081 | 0.49 | 0.32 | 0.47 | 0.62 | 47318 | 0.54 | 0.42 | 0.54 | 0.65 | 35972 |
| Financial Debt | 597 | 65 | 183 | 486 | 72084 | 316 | 53 | 134 | 296 | 47324 | 379 | 67 | 169 | 415 | 35972 |
| Capital | 10607 | 1877 | 4137 | 9738 | 72081 | 4578 | 1118 | 2232 | 4384 | 47318 | 5612 | 1279 | 2746 | 6562 | 35972 |
| Employment | 26 | 10 | 15 | 28 | 72084 | 18 | 8 | 11 | 17 | 47323 | 23 | 10 | 14 | 28 | 35972 |
| Sales | 7116 | 1846 | 3458 | 7510 | 72084 | 2547 | 968 | 1505 | 2448 | 47324 | 5899 | 1206 | 2942 | 7538 | 35972 |
| Value Added | 1213 | 407 | 672 | 1283 | 72084 | 617 | 267 | 388 | 595 | 47324 | 875 | 327 | 514 | 1042 | 35972 |
| EBITDA | 269 | 40 | 118 | 288 | 72084 | 124 | 23 | 66 | 147 | 47324 | 153 | 29 | 78 | 181 | 35972 |

Note: Small firms are independent firms whose annual sales are below 500 million francs. Big firms are either firms with sales above that threshold or firms whose parent company has sales above this threshold. Equity is the book value of equity. Subsidizable debt is more-than-two-year bank debt stock. Unsubsidizable debt is all financial debt stock minus more-than-two-year bank debt. Working capital credit is measured as the stock of trade payables plus bank overdrafts plus other sources of working capital credit. Liability shares are computed over Equity plus Subsidizable debt plus Unsubsidized debt plus Working Capital Credit. Debt stock is the sum of Subsidizable and Unsubsidized Debt. Capital is the sum of fixed assets and net working capital. Ratios are winsorized at the 1% level. Values are expressed in thousands of euros 1995 where applicable. Source: BRN, LIFI (1992-1999).

Table 2: Descriptive Statistics: Differences-in-Differences Sample Means

| Subsample Means of Financial Debt Growth (Log) | | | | | | |
|--|----------------------------|---------------------------|-----------------------------|---------------------------|----------------------------|--------------------------|
| | (1992-1994) | (1995) | (1996-1999) | (1995)-(1992-1994) | (1996-1999)-(1995) | (1996-1999)-(1992-1994) |
| <u>Small firms :</u> | | | | | | |
| Wholesale trade | -0.050** (0.006) | 0.085** (0.011) | -0.047** (0.006) | 0.135** (0.012) | -0.132** (0.013) | 0.004 (0.008) |
| Retail trade | -0.055** (0.007) | 0.012 (0.013) | -0.051** (0.006) | 0.067** (0.013) | -0.063** (0.014) | 0.004 (0.009) |
| (Wholesale trade)-(Retail trade) | 0.004 (0.009) | 0.073** (0.017) | 0.004 (0.009) | 0.068** (0.019) | -0.068** (0.019) | 0.000 (0.013) |
| <u>Big firms :</u> | | | | | | |
| Wholesale trade | -0.094** (0.027) | -0.061 (0.066) | -0.032 (0.030) | 0.033 (0.068) | 0.029 (0.069) | 0.062 (0.045) |
| Retail trade | -0.057 (0.092) | -0.115 (0.144) | -0.068 (0.069) | -0.058 (0.178) | 0.047 (0.157) | -0.010 (0.112) |
| (Wholesale trade)-(Retail trade) | -0.037 (0.086) | 0.054 (0.168) | 0.035 (0.076) | 0.091 (0.176) | -0.019 (0.176) | -0.072 (0.112) |
| <u>Whole sample :</u> | | | | | | |
| All Small firms | -0.052** (0.004) | 0.056** (0.009) | -0.048** (0.0064) | 0.108** (0.009) | -0.105** (0.009) | 0.004 (0.006) |
| All Big Firms | -0.088** (0.031) | -0.069 (0.060) | -0.038 (0.028) | 0.019 (0.064) | 0.029 (0.069) | 0.050 (0.042) |
| (Small firms)-(Big firms) | 0.036 (0.023) | 0.126** (0.044) | -0.011 (0.021) | 0.089 (0.047) | -0.136** (0.047) | -0.047 (0.032) |

| Subsample Means of Alternative Variables (Small Firms) | | | | | | |
|--|----------------------------|---------------------------|----------------------------|---------------------------|----------------------------|---------------------------|
| | (1992-1994) | (1995) | (1996-1999) | (1995)-(1992-1994) | (1996-1999)-(1995) | (1996-1999)-(1992-1994) |
| <u>Unsubsidizable Debt Growth (Log) :</u> | | | | | | |
| Wholesale trade | -0.049** (0.008) | 0.124** (0.016) | -0.076** (0.006) | 0.173** (0.017) | -0.200** (0.017) | -0.027* (0.011) |
| Retail trade | -0.042** (0.007) | 0.138** (0.019) | -0.067** (0.009) | 0.180** (0.021) | -0.205** (0.020) | -0.025 (0.014) |
| (Wholesale trade)-(Retail trade) | -0.007 (0.013) | -0.014 (0.025) | -0.009 (0.006) | -0.007 (0.027) | 0.005 (0.027) | -0.002 (0.018) |
| <u>Value Added Growth (Log) :</u> | | | | | | |
| Wholesale trade | 0.000 (0.002) | 0.029** (0.003) | 0.004 (0.002) | 0.028** (0.004) | -0.025** (0.004) | 0.004 (0.003) |
| Retail trade | 0.003 (0.003) | 0.000 (0.004) | 0.003 (0.002) | -0.002 (0.005) | 0.003 (0.005) | 0.001 (0.003) |
| (Wholesale trade)-(Retail trade) | -0.002 (0.003) | 0.029** (0.005) | 0.000 (0.003) | 0.031** (0.007) | -0.028** (0.006) | 0.003 (0.004) |
| <u>Profit Growth (Log) :</u> | | | | | | |
| Wholesale trade | -0.032** (0.027) | 0.059** (0.009) | 0.006 (0.030) | 0.091** (0.011) | -0.053** (0.010) | 0.038** (0.007) |
| Retail trade | -0.033** (0.092) | -0.005 (0.012) | 0.014* (0.069) | 0.028* (0.014) | 0.018 (0.012) | 0.046** (0.009) |
| (Wholesale trade)-(Retail trade) | 0.001 (0.009) | 0.063** (0.015) | -0.008 (0.007) | 0.063** (0.018) | -0.072** (0.016) | -0.009 (0.011) |

Note: Standard errors in parenthesis. * : significant at 5% level ** : significant at 1% level. Small firms are independent firms whose annual sales are below €76 million. Big firms either have sales above this threshold or belong to firms with sales above the threshold. Averages across sectors do not include car trade. Financial debt is comprised of bank loans, bonds and loans from institutions other than banks. Equity is the book value of equity. Subsidizable debt is more-than-two-year bank debt stock. Unsubsidizable debt is all financial debt stock minus more-than-two-year bank debt. Working capital credit is measured as the stock of trade payables plus bank overdrafts plus other sources of working capital credit. Liability shares are computed over Equity plus Subsidizable debt plus Unsubsidized debt plus Working capital credit. Financial debt is the sum of Subsidizable and Unsubsidized Debt. Values are expressed in thousands of euros 1995 where applicable. Sample means are drawn from a global sample with 161170 firm-year observations. Source: BRN, LIFI (1991-1999).

Table 3: Effect of the Reforms on Corporate Financing Decisions

| Dependent variable : | $\Delta\log(\text{Financial Debt})$ | | $\Delta\log(\text{Financial Debt} - \text{Long-term Bank Debt})$ | | $\Delta\log(\text{Financial Debt} + \text{Working Capital Credit} - \text{Long-term Bank Debt})$ | | $\Delta\log(\text{Total Liabilities} - \text{Long-term Bank Debt})$ | |
|--|-------------------------------------|--------------------------|--|--------------------------|--|-------------------------|---|--------------------------|
| | Small Firms | Big Firms | Small Firms | Big Firms | Small Firms | Big Firms | Small Firms | Big Firms |
| $WS_i * POST_{1t}$ <i>(Lending privilege awarded)</i> | 0.076** (0.021) | 0.084 (0.215) | 0.009 (0.031) | 0.305 (0.221) | 0.003 (0.009) | 0.031 (0.046) | 0.001 (0.006) | -0.010 (0.047) |
| $WS_i * POST_{2t}$ <i>(Lending privilege removed)</i> | -0.087** (0.020) | -0.004 (0.174) | -0.021 (0.031) | -0.256 (0.217) | 0.006 (0.008) | 0.007 (0.041) | 0.001 (0.005) | 0.037 (0.037) |
| No Observations | 155380 | 5799 | 155380 | 5799 | 155368 | 5799 | 155280 | 5797 |

Note: Small firms are independent firms with sales below €76 million. Big firms either have sales above this threshold or belong to firms with sales above the threshold. WS_i is a dummy for the wholesale trade sector. $POST_{1t}$ and $POST_{2t}$ are dummies for post-1995 and post-1996 observations, respectively. The operator $\Delta\log$ is the difference between the logarithm at the end of year t and the logarithm at the end of year $t - 1$. Financial Debt is equal to short-term plus long-term bank loans plus bonds plus loans from non-bank institutions. Working Capital Credit is equal to bank overdrafts plus trade payables plus other working capital debt. Total Liabilities is equal to Financial Debt plus Working Capital Credit plus Equity. All regressions include controls for 2-digit sectoral fixed effects, year-size fixed effects with size measured by the log of the number of employees at time $t-1$ and year-sector fixed effects for the car trade sector. Standard errors are in parenthesis. They are the maximum of the following standard error estimates: conventional, heteroskedasticity-robust, clustered at firm level, at 2-digit-industry and year level, at 2-digit-industry level, at 2-digit-industry and “région” level, and at 2-digit-industry and “département” level. * : significant at 5% level ** : significant at 1% level. Source: BRN, LIFI (1991-1999).

Table 4: Effect of Debt on Output, Costs and Profits

| Dependent variables | Regressor : $\Delta\log(\text{Financial Debt})$ | |
|--------------------------------------|---|---------------|
| | IV | OLS |
| $\Delta\log(\text{value added})$ | 0.41** | 0.02** |
| | (0.12) | (0.00) |
| Sargan p-value | 0.17 | |
| F of excluded IV | 11.9 | |
| Observations | 155380 | 155380 |
| $\Delta\log(\text{operating costs})$ | 0.23** | 0.02** |
| | (0.08) | (0.00) |
| Sargan p-value | 0.27 | |
| F of excluded IV | 11.9 | |
| Observations | 155380 | 155380 |
| $\Delta\log(\text{EBITDA})$ | 0.83* | -0.00 |
| | (0.29) | (0.00) |
| Sargan p-value | 0.13 | |
| F of excluded IV | 10.6 | |
| Observations | 126059 | 126059 |

Note: IV estimates are computed using sector-period interactions as instruments. The estimates are computed in the sample of independent firms with sales below €76 million. The operator $\Delta\log$ is the difference between the logarithm at the end of year t and the logarithm at the end of year $t - 1$. Operating Costs are equal to Value Added - EBITDA. All regressions include controls for 2-digit sectoral fixed effects, year-size fixed effects with size measured by the log of the number of employees at time $t-1$, and year-sector fixed effects for the car trade sector. Standard errors are in parenthesis. They are the maximum of the following standard error estimates: conventional, heteroskedasticity-robust, clustered at firm level, at 2-digit-industry and year level, at 2-digit-industry level, at 2-digit-industry and “région” level, and at 2-digit-industry and “département” level. Sargan p-value corresponds to the displayed estimator of standard errors. F-test of excluded instruments is computed using the same assumptions as for the Stock & Yogo (2002) weak instrument critical values. * : significant at 5% level ** : significant at 1% level. Source : BRN, LIFI (1991-1999).

Table 5: Robustness Checks on IV Estimates

| | Regressor : $\Delta\log(\text{Debt stock})$ | | | | | | |
|--------------------------------------|---|---------------|--------------|---------------|---------------|---------------|---------------|
| Dependent variables : | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| $\Delta\log(\text{value added})$ | 0.41** | 0.38** | 0.41* | 0.44** | 0.38** | 0.37** | 0.41** |
| | (0.12) | (0.13) | (0.13) | (0.13) | (0.12) | (0.13) | (0.14) |
| Observations | 155380 | 155380 | 143849 | 155380 | 137287 | 118439 | 99447 |
| $\Delta\log(\text{operating costs})$ | 0.23** | 0.19* | 0.21* | 0.24** | 0.21* | 0.17 | 0.14 |
| | (0.08) | (0.09) | (0.08) | (0.08) | (0.08) | (0.09) | (0.09) |
| Observations | 155380 | 155380 | 143849 | 155380 | 137287 | 118439 | 99447 |
| $\Delta\log(\text{EBITDA})$ | 0.83* | 0.78* | 0.79* | 0.96** | 0.86* | 0.82** | 0.85* |
| | (0.29) | (0.29) | (0.31) | (0.34) | (0.30) | (0.31) | (0.34) |
| Observations | 126059 | 126059 | 117162 | 126059 | 111160 | 95888 | 80700 |
| Sectoral linear trends | No | Yes | No | No | No | No | No |
| Past sales growth effects | No | No | Yes | No | No | No | No |
| Food retail-year effects | No | No | No | Yes | No | No | No |
| Last year in sample | 1999 | 1999 | 1999 | 1999 | 1998 | 1997 | 1996 |

Note: IV estimates are computed using sector-period interactions as instruments. The estimates are computed in the sample of independent firms with sales below €76 million. The operator $\Delta\log$ is the difference between the logarithm at the end of year t and the logarithm at the end of year $t - 1$. Operating Costs are equal to Value Added minus EBITDA. All regressions include controls for 2-digit sectoral fixed effects, year-size fixed effects with size measured by the log of the number of employees at time $t-1$, and year-sector fixed effects for the car trade sector. Inclusion of linear trends means that the specification includes a linear time trend as well as interactions of 2-digit sectors with linear time trends. Inclusion of past sales growth effects means that the specification includes lagged logarithm of sales growth with year interactions. Standard errors are in parenthesis. They are the maximum of the following standard error estimates: conventional, heteroskedasticity-robust, clustered at firm level, at 2-digit-industry and year level, at 2-digit-industry level, at 2-digit-industry and “région” level, and at 2-digit-industry and “département” level. * : significant at 5% level ** : significant at 1% level. Source: BRN, LIFI (1991-1999).

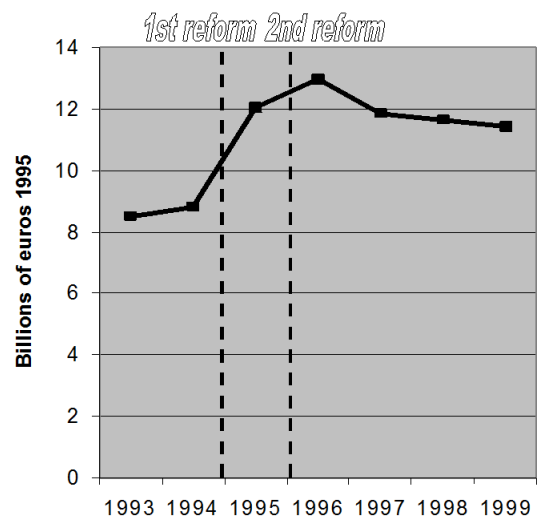
Table 6: The Effect of Access to Targeted Credit on Default

Dependent variable : Probability of filing for bankruptcy for firm i after year t

| | <i>After one year</i> | | <i>After three years</i> | | <i>Anytime after year t</i> | |
|---|----------------------------|----------------------------|---------------------------|----------------------------|-----------------------------|----------------------------|
| | Probit | OLS | Probit | OLS | Probit | OLS |
| WS_i*POST_{1t} <i>(Lending privilege awarded)</i> | 0.0032 (0.0022) | 0.0033 (0.0023) | 0.0058 (0.0037) | 0.0039 (0.0036) | 0.0094 (0.0057) | 0.0032 (0.0058) |
| WS_i*POST_{2t} <i>(Lending privilege removed)</i> | -0.0008 (0.0021) | -0.0029 (0.0021) | 0.0038 (0.0039) | -0.0012 (0.0050) | 0.0031 (0.0055) | -0.0066 (0.0053) |
| Average Default Probability | 0.012 | | 0.036 | | 0.095 | |
| Observations | 155380 | 155380 | 155380 | 155380 | 155380 | 155380 |

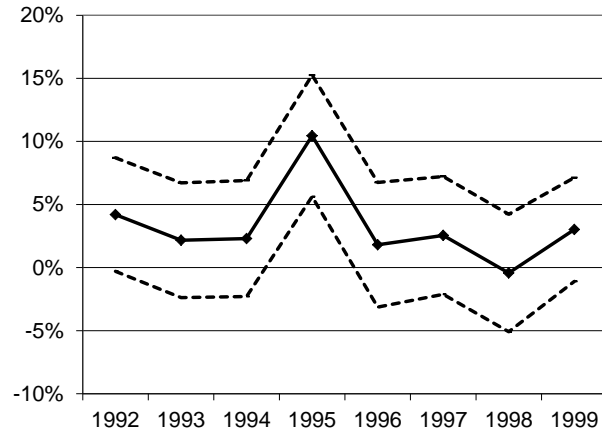
Note: Marginal effects reported. The estimates are computed in the sample of independent firms with sales below €76 million. WS_i is a dummy for the wholesale and retail trade sectors, respectively. $POST_{1t}$ and $POST_{2t}$ are dummies for post-1995 and post-1996 observations, respectively. All regressions include controls for 2-digit sectoral fixed effects, year-size fixed effects with size measured by the log of the number of employees at time t-1, and year-sector fixed effects for the car trade sector. Standard errors are in parenthesis. They are the maximum of the following standard error estimates: conventional, heteroskedasticity-robust, clustered at firm level, at 2-digit-industry and year level, at 2-digit-industry level, at 2-digit-industry and “région” level, and at 2-digit-industry and “département” level. * : significant at 5% level ** : significant at 1% level. Source: BRN, LIFI, SIRENE (1991-2007).

Figure 1: The Evolution of the Stock of CODEVI Loans



Source: Banque de France

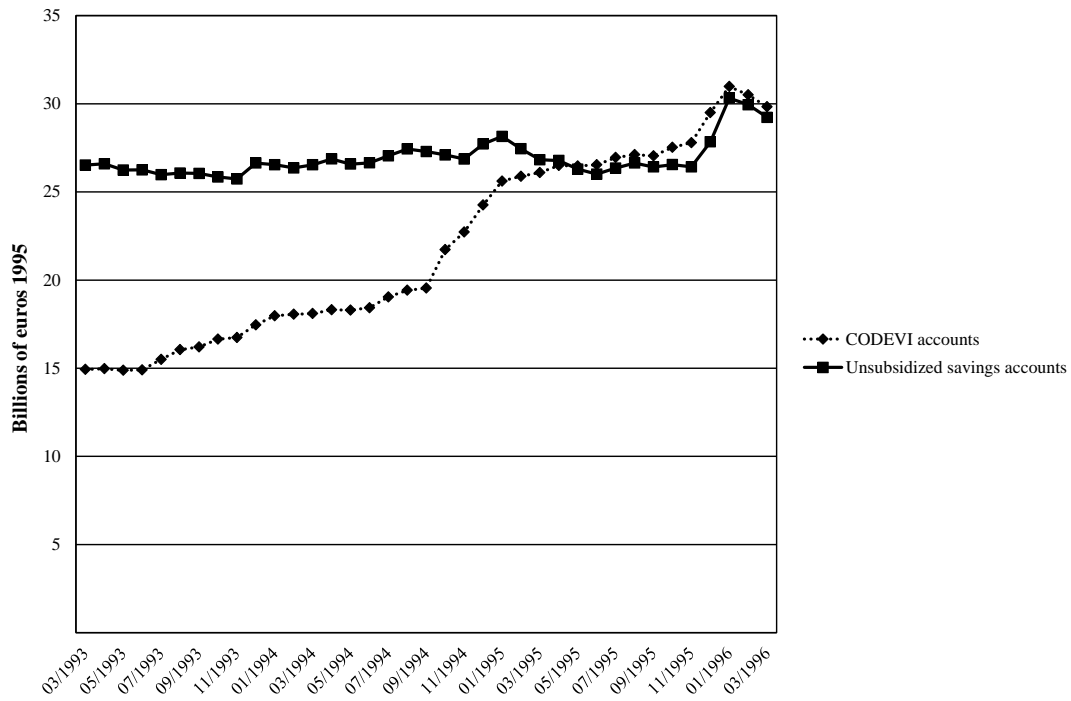
Figure 2: Estimated Difference in Financial Debt Growth Between Small Wholesale and Small Retail Trade Businesses



Note: This graph plots the point estimates of a regression of the logarithm of financial debt growth on interactions between wholesale trade dummies and year dummies, in the sample of firms whose size and ownership status make them eligible for CODEVI loans. These regressions included controls for 2-digit sectoral fixed effects, year-size fixed effects with size measured by the log of the number of employees at time t-1, and year-sector fixed effects for the car trade sector. The dashed lines represent the 95% confidence intervals. Source: BRN, LIFI (1991-1999).

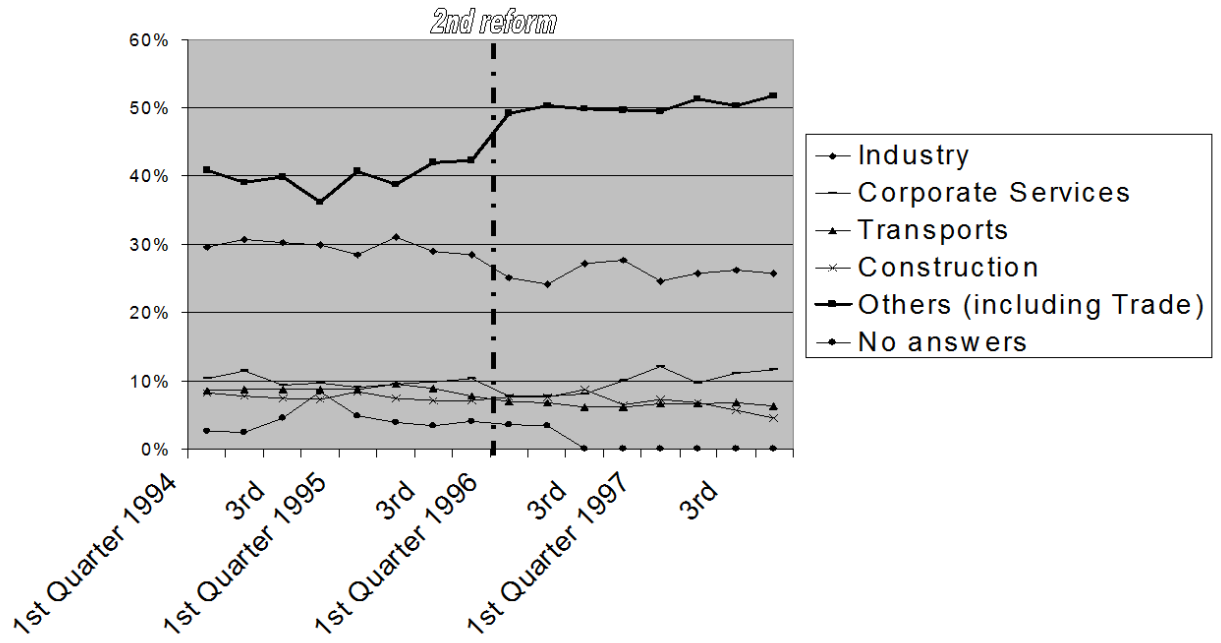
Appendix A: Additional Figures (meant for online publication)

Figure 3: The Evolution of Liquid Savings Accounts Around the CODEVI Reforms



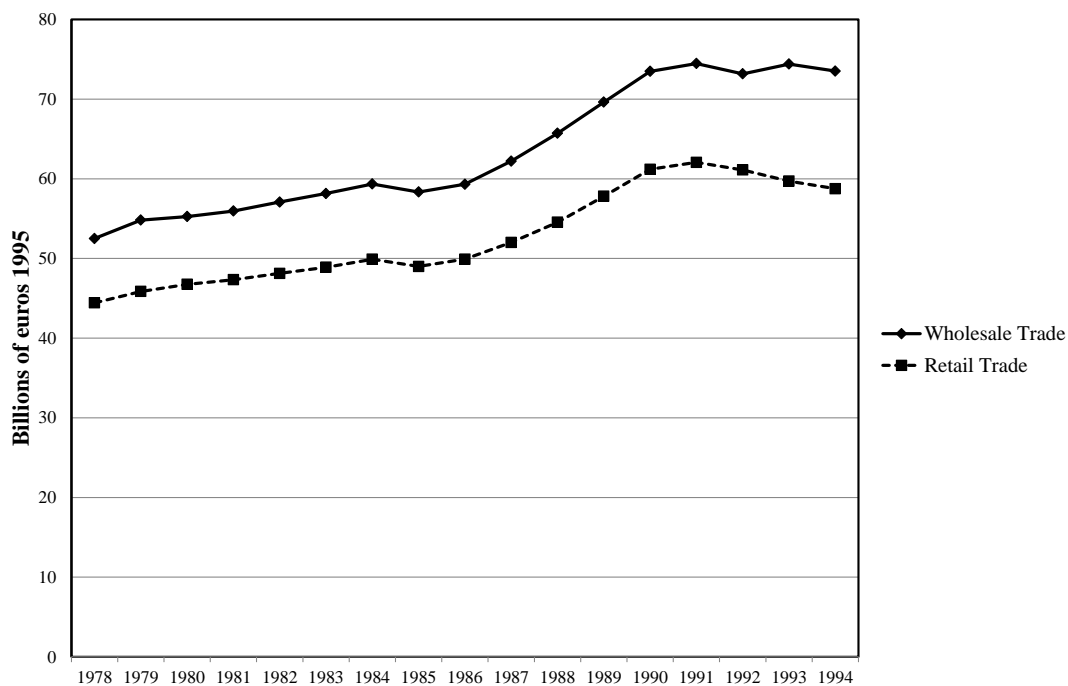
Note: Unsubsidized savings accounts include all savings instruments whose liquidity characteristics place them in the M2 - M1 money aggregate comprised of all “close substitutes” to money, and yet do not benefit from any tax exemption. Other components of M2 - M1 include the CODEVI savings accounts. *Source:* Banque de France

Figure 4: The Evolution of the Sectoral Distribution of CODEVI Loans



Source: French Treasury

Figure 5: The Evolution of Trade Sectors' Total Production over the Long Run



Note: Sectoral definitions were significantly altered in 1993. Source: French National Accounts, SCN 95

Appendix B: A Model of the Effect of Subsidized Credit on Constrained and Unconstrained Firms (meant for online publication)

Model setup K units of capital inputs allow entrepreneurs to produce $AK^\alpha = f(K)$ units of output with probability p and 0 otherwise. A is a constant, α is between 0 and 1. Every agent in the economy is risk-neutral. Entrepreneurs have limited liability and no personal wealth so they can finance their investment K with two different sources: subsidized finance (with quantity K_s) or unsubsidized market finance (with quantity K_{ns}). In both cases the supply of funds is assumed to be infinitely elastic. Results hold even when the market credit supply curve is upward-sloping.

The interest rate r_s on subsidized finance is equal or lower than r_{ns} , the interest rate for unsubsidized finance. Each firm is allowed to incur a maximum of $\overline{K_s}$ units of subsidized finance and $\overline{K_{ns}}$ units of unsubsidized finance. While the constraint on unsubsidized finance is only the result of a market failure, the limits to incurring subsidized finance are also the results of policy choices. In the context of the CODEVI program, $\overline{K_s}$ heavily depends on the maximum amount of money that the state allows French savers to hold in their CODEVI accounts.

Before-interest-profits are represented by Y , while Π represents after-interest-profits. The optimization problem for the individual entrepreneur is the following:

$$\max_{K, K_s, K_{ns}} E(\Pi) = p[AK^\alpha - r_s K_s - r_{ns} K_{ns}] \quad (3)$$

$$s.t. \quad K_{ns} \leq \overline{K_{ns}} \quad (4)$$

$$K_s \leq \overline{K_s} \quad (5)$$

A firm is said to be credit constrained as soon as the constraint (4) is binding.

Predicted effects of policy changes The theoretical analogue of the policy changes we evaluate in this paper is a marginal increase in $\overline{K_s}$, the maximum level of subsidized debt that can be incurred by an individual firm. We distinguish three different scenarios.

The fully unconstrained case If no source of finance is constrained, then firms should be able to invest so as to equate the marginal product of capital with the *subsidized* interest rate:

$$K^* = \left(\frac{\alpha A}{r_s}\right)^{\frac{1}{1-\alpha}} \quad (6)$$

A marginal increase in subsidized finance capacity \overline{K}_s would have no effect on total external financing, unsubsidized debt or before-interest profits.

The partially constrained case If unsubsidized finance is unlimited, yet subsidized finance is constrained, then for \overline{K}_s small enough, firms should equate the marginal product with the *unsubsidized* interest rate :

$$K^{**} = \left(\frac{\alpha A}{r_{ns}}\right)^{\frac{1}{1-\alpha}} < K^* \quad (7)$$

Then the optimal level of unsubsidized debt should be equal to $K^{**} - \overline{K}_s$ and before-interest-profits should not depend on \overline{K}_s . Small increases in \overline{K}_s crowd out unsubsidized finance and do not lead to higher investment and before-interest profits. Once \overline{K}_s grows bigger than K^{**} , subsidized credit rises enough to entirely crowd out unsubsidized finance, yet does not allow the entrepreneur to equate the marginal product of capital with the subsidized interest rate. Only then does an increase in \overline{K}_s bring about an increase in investment as well as an increase in before-interest-profits, but this profit increase cannot match the cost of capital in the unsubsidized credit market :

$$r_s = f'(K^*) \leq \frac{\delta f}{\delta \overline{K}_s} < r_{ns} = f'(K^{**}) \quad (8)$$

The fully constrained case If constraints on both subsidized and unsubsidized finance are binding, then the optimal level of investment is equal to $\overline{K}_{ns} + \overline{K}_s < K^{**}$. An increase in \overline{K}_s would increase the total external financing of the firm but may not lead to a reduction in the level of unsubsidized finance taken on by firms, because the marginal product of capital remains higher than the unsubsidized interest rate r_{ns} . For precisely this reason, increases in the subsidized finance capacity trigger increases in before-interest-profits over and above the cost of capital in the

unsubsidized credit market :

$$\frac{\delta f}{\delta \bar{K}_s} = f'(\bar{K}_{ns} + \bar{K}_s) > r_{ns} = f'(K^{**}) \quad (9)$$

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