Are small and medium-sized firms really credit constrained ? Evidence from a French targeted credit programme*

Laurent Bach**

ABSTRACT

This study aims to measure the extent to which French small and medium businesses lack adequate access to credit. In order to solve the endogeneity problem that made previous attempts unsatisfying, we use the "natural experiment" methodology.

We exploit variations of a French programme consisting in targeted bank loans. Using firm-level data, we find that the returns on supplementary debt issued thanks to these reforms range between 15% and 79%, implying severe credit constraints among French SMEs. Further evidence shows that the main beneficiaries of these reforms were firms on which banks' information is "soft" rather than "hard".

Keywords : Banking, Credit Constraints, Small Business Lending, Targeted Credit

JEL : G21, G28, L8

* I thank Thomas Piketty, Esther Duflo, Antoine Faure-Grimaud, Francis Kramarz, Jean-Laurent Rosenthal, Sébastien Roux, and seminar participants at the London School of Economics, the Paris School of Economics and CREST-INSEE for very helpful discussions. I also thank Frédéric Cherbonnier, Philippe Leroy and the French Treasury for their helpful practical insights on French banking regulations and provision of administrative data.

** Paris School of Economics (PSE), 48 Boulevard Jourdan, 75014 Paris, France; CREST-INSEE, 15 Boulevard Gabriel Péri, 92245 Malakoff CEDEX, France. E-mail : laurent.bach@ens.fr.

1 Introduction

Nowadays, the existence of limits to access to credit among firms is agreed upon by most of the theoretical economic literature (Stiglitz, Weiss (1981); Tirole (2006)). So much so that this has become a major ingredient of macroeconomic analysis in order to explain short term fluctuations (Bernanke, Gertler (1989)) or the dynamics of inequality and growth (Aghion, Bolton (1997); Piketty (1997)). This phenomenon has also been a major argument for policy-makers both in the developed and the developing world willing to help firms grow via direct or indirect financial aid.

Unfortunately, the empirics of credit constraints remains very controversial. The first generation of empirical models, the cash-flow-to-investment-sensitivity literature (Fazzari, Hubbard, Petersen (1987)), though very compelling at first glance, was faced with very consistent criticism showing the endogeneity bias present in most of that literature (Kaplan, Zingales (2000)). That's why a second generation of empirical tests used exogenous shocks to external financing ability as instruments for the estimation of cash-flow investment sensitivities; either shocks in internal capital markets (Lamont (1997)), or shocks in firms' retirement contributions (Rauh (2006)) were used as instruments, and led to convincing results, in that cash flows seem to have a causal and positive effect on investments. However this is not an ultimate proof of binding credit constraints, and could simply be induced by empire-building instead (Stein (2003)).

There's also a debate as to why banks would refuse to lend to profitable enough firms. The classical view is that moral hazard problems are so prevalent for those firms that they can't convince any investor to finance them, in which case only redistribution from rich to poor can fully eradicate credit rationing. However, recent studies have tried to go further and open the black box of banks' information-acquisition technology : in an influential paper, Stein (2002) made the distinction between "hard" information on potential loanees (financial statements, credit scoring) and "soft" information (past relationships with the bank), and argued that centralised banks were better at gathering the former kind of information, usually available for big enough firms, while decentralised banks had a comparative advantage in gathering the latter kind, often the only kind of information available for small and medium firms. As a result, decentralised banks should be better at reducing small and medium firms' access to finance. This clearly opens new avenues for policy design. However, evidence on the effects of bank concentration on access to finance is mixed (see Berger et al. (2004) for a review), and the distinction between "hard" and "soft" information might not be so crucial if we take into account the diversity of lending contracts using "hard" information to lend to informationnally opaque firms (Berger, Udell (2005)).

Along these two controversies lies the discussion over the opportunity for governments to reduce credit rationing. Informational asymmetries between firms and investors at the source of credit constraints may be replaced by lack of information aggregation on behalf of the state, leading to a possibly greater misallocation of capital. Moreover most of the empirical literature assessing the effects of direct state interventions on firms' access to credit seems to show that indiscriminate and direct action from the state is not welfare-enhancing at all (Bertrand, Schoar, Thesmar (2007)). But there's hardly been any empirical analysis of state interventions on access to credit going through the channel of financial intermediaries, supposed to be better than governments at aggregating information on firms.

In this paper, we evaluate the impact of reforms of the CODEVI targeted lending program in France. This programme channels French households' savings to firms with less than 76 million euros in annual sales and belonging to specific sectors of the economy, via the French banking system which collects these savings and allocates them to eligible firms. We estimate the effect of access to the program on the availability of bank finance for targeted firms by taking advantage of two successive changes in the sectoral perimeter of the program in 1994 and 1995. Then, we test the credit constraints hypothesis by looking at the changes in real outcomes of targeted firms following these reforms. The results indicate that firms from newly eligible sectors increased their total credit stock by 8 % on average thanks to the reform. Furthermore, the economic return on newly incurred debt ranges between 15% and 79%, well above the French cost of capital. This contradicts the empire-building hypothesis and shows the existence of sizable credit constraints among French small-and-medium sized firms in the mid-nineties. Finally, we turn to an analysis of banks' allocation of targeted funds and provide a test of Stein's hypothesis that it is primarily firms on which information is "soft" that suffer from credit constraints in a world where banks are centralised organisations : we find that banks favour middle-aged firms, irrespective of indicators of financial soundness and firm size. This is further evidence that loan officers are constrained in their use of "soft" information to make their lending decisions.

Our study is related to an article by Banerjee and Duflo (2004) measuring the magnitude of credit constraints in India, taking advantage of reforms in a directed lending program to make a differences-in-differences estimation of the effect of working capital bank loans on real outcomes. They first find a strong increase in output due to newly incurred bank debt, which is not due to substitution with other forms of finance. Then they estimate the profitability of debt by looking at the magnitude of the effect of bank debt on profits : if the estimated marginal return is well above the cost of funds, it is because there must exist credit constraints. The results of their estimations lead to the acceptance of the credit constraints hypothesis. In particular, they find that the private return to bank debt is equal to 90%, well above the cost of funds in India, whose highest bound is 42%.

We test the existence of credit constraints among small-and-medium sized French firms, which implies divergences with the Banerjee and Duflo (2004) analysis. Firstly, this paper deals with a developed country therefore the financial system should be better at addressing informational asymmetries and the magnitude of credit constraints lower than in India. The second divergence comes from the discontinuity in the lending program that supports the identification strategy. In Banerjee and Duflo (2004), the reforms affect the threshold of size eligibility, which is problematic if credit constraints are to be greater when firms are smaller. In this paper, the reforms affect sectoral eligibility in a fairly refined manner, allowing us to compare very comparable firms inside and outside the programme's perimeter. Finally, whereas Banerjee and Duflo (2004) use firm-level data coming from one branch of a big Indian bank, we use the French fiscal database at firm level (INSEE- Benefices Reels et Normaux (BRN)). Therefore, we have data points over a longer amount of time before and after the reforms and for a much greater number of firms. This strengthens the differences-in-differences analysis as it enables us to know the total amount of debt incurred by firms and to look at banks' screening behaviour.

Another paper dealing with similar issues is Paravisini (2005). Using differences-in-differences methodology, he analyses the effect of a targeted lending program on small Argentinian firms. He finds that only 7 cents in each dollar lent with World Bank funds wouldn't have been provided to firms anyway. This result seems to imply that targeted lending programs are used by banks to reduce their lending costs without substantially increasing the amount of loans they provide. Our paper is complementary and not really in contradiction with Paravisini (2005).

Being unable to distinguish program bank loans from usual bank loans, we can't measure the exact degree of substitution between the two types of loans. However, the quantitative importance of the programme we're interested in is much bigger than the Argentinian program analysed by Paravisini, so that even with a high degree of substitution between programme loans and usual loans, access to the French programme may have a very significant influence on firms' real outcomes.

2 Institutions and Data

2.1 Banking Regulations in France

Until the beginning of the 80s, most of the loans were channelled to French firms via direct (Public banks) or indirect (Provision of money to banks) interventions of the State. It is now a well-assessed fact that this system entailed a huge misallocation of capital among firms due to a lack of price guidance. Since the 1984 reform, these interventions have been much more focused on helping very specific groups of firms supposed to suffer from an undue lack of finance, as is the case in most developed countries; for example, in the United States, a whole federal administration, the Small Business Administration (SBA), is dedicated to help finance SMEs, while the Community Reinvestment Act obliges banks to lend to specific communities.

In France, most of these policies helping SMEs are led by the Treasury. They include subsidized loans, targeted lending and loan guarantees. Since 1984, the State has progressively favoured the last two options. It has abandoned subsidized loans, arguing that their providers, when lending to firms, lack expertise and incentives, while targeted lending and loan guarantees take advantage of more efficient information processing by private financial intermediaries. According to the French Treasury, in 2003, 55% of loans awarded to less-than-three-year-old small-and-medium-sized firms were guaranteed by the State, while the figure is 15% for older SMEs. As for the targeted lending program (the CODEVI), its importance has evolved very frequently for reasons that we will explain in the next subsection. Statistics from the French Central Bank show that, in 2003, these targeted loans represented 4.2 % of long-term loans to firms in nominal value. However this figure is largely underestimating the importance of these loans as the French Central Bank doesn't provide data about total long term loans depending on firm size.

2.2 The CODEVI Program

The CODEVI (COmptes pour le DEVeloppement Industriel, i.e. Accounts for Industrial Development) savings accounts were created by the French government in 1983. In the fashion of other very popular savings accounts such as the Livret A, it is a very liquid account whose revenue is tax-free (up to a certain limit) but determined by the state (usually following the evolution of interest rates). The big difference for savers is that this account can be managed not only by not-for-profit banks (the Caisses d'Epargne, the Credit Mutuel, the Banques Populaires and the Credit Agricole) but also by private banks. The main variable influencing savers' decisions to invest their money into these accounts is the maximum amount of money that can be saved tax-free in CODEVI accounts. In 1983, the limit was 1500 euros, but it moved to 2250 euros in 1990, 3000 euros at the end of 1993 and finally 4500 euros at the end of 1994. The amount of money saved in CODEVI accounts is much more sensitive to that limit than to the rate of return of the accounts due to the subsidy implied by the tax deduction. In figure 1, we can see the effects of these changes on the stock of CODEVI loans made to SMEs.

Insert figure 1.

The CODEVI funds are to be invested along lines dictated by the French Treasury : a sizable minority of the funds (around 30%) are transferred to the main French public financial institution, the Caisse des Depots et Consignations (CDC), in order to fund its venture capital activities; but the most part are converted into loans to French firms. Many characteristics of these loans are fixed by the State. The most important of them are the duration of the loan, the interest rate, the size, the ownership status and the sector of the borrowing firms.

The loans must last more than one year, but the regulations for the interest rate are much looser : there is a maximum interest rate determined by the State, but it is not reviewed regularly and its level is too high to be binding for banks. In fact, the major influence on the interest rate of loans comes from the rate of return of the CODEVI accounts that is determined by the State. If it is low by comparison with Central Bank interest rates, the interest rates on CODEVI loans may be lower than average, but these may be higher than average if the rate of return of CODEVI funds is high in comparison with Central Bank interest rates.

According to state regulations, firms borrowing on CODEVI funds must have annual sales under 76 million euros, mustn't be owned by a firm whose annual sales are above this threshold and must belong to specific sectors of the economy : until the end of 1993, only firms belonging to industrial sectors were eligible, and after 1993 wholesale trade firms became eligible; and finally, in January 1996, retail trade firms were made eligible to CODEVI loans. We have obtained quarterly data from the Treasury about the sectoral destination of new loans made with CODEVI funds from 1994 to 1997 (see figure 2). They show that the 2nd reform took real effect very soon after its official implementation (see the kink between the last quarter of 1995 and the 1st quarter of 1996). Unfortunately, we don't have sectoral data dating from before the first sectoral reform, but there's no reason why loan officers should have been more reluctant to provide loans to wholesale trade firms.

Insert figure 2.

In order to enforce these regulations, the Treasury asks every bank detailed data on CODEVI loans for each quarter and Treasury officers have great powers of investigation whenever they suspect breaches to CODEVI regulations. There are financial penalties in case of non-compliance of banks : the unused funds must be deposited in an account of the CDC, with a return which is low enough to discourage banks.

The importance of the scheme for SMEs' bank lending therefore crucially depends on the amount of money banks have to convert into loans. This amount of money depends first on the maximum amount that can be saved tax-free in the CODEVI accounts. It also depends on how much of the CODEVI funds has already been lent to SMEs in the recent past. The production of new CODEVI loans is therefore cyclical. For instance in 1990-1991, many SMEs obtained CODEVI loans whose average duration was 4 years. During these 4 years (until 1994-1995), banks were thus unable to produce a sizeable amount of CODEVI loans, since the previous loans were not yet reimbursed. This cyclical pattern makes it difficult to know the exact importance of CODEVI funds for the production of new loans each year. However, according to Marc Vienot[1], who was CEO of the Societe Generale, one of the biggest French banks, at that time, the CODEVI loans represented 61% of the production of authorized new loans in 1994 (most of this coming from the 4th quarter, right after the increase in the limit of the tax-free savings account). Quarterly surveys by the Banque de France on firms' credit conditions , mentions from the 4th quarter of the year 1994 to the 3rd quarter of the year 1996 that most of the new loans to eligible SMEs were made with CODEVI funds.

In brief, wholesale trade SMEs were made eligible to the scheme as early as 1993 but it barely had an effect on their access to credit before the very end of 1994, when an unanticipated change in the limit of the accounts took place, as shown by figure 1. Wholesale trade SMEs had indeed a better access to credit after 1994 (this is what we will call the 1^{st} reform), while retail trade firms only had a better access to credit after 1995 (this is what we will call the 2^{nd} reform). It is important to insist on the fact that this 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 increased : indeed, these CODEVI funds might just represent cash transfers from un subsidized accounts. What is really crucial for our analysis is that it is the supply of funds to a specific set of firms that increased exogenously, while the global supply of funds to firms might have remained constant.

2.3 Data

In order to make a differences-in-differences analysis of those reforms, we needed to collect detailed firm-level data for a sufficiently large span of time before and after the reforms. The first problem is that because of their small size, targeted firms are usually not surveyed in a comprehensive way. This is why we have used the French annual fiscal database of firms' accounts (the Benefices Reels et Normaux, or BRN, database from the French National Institute of Statistics (INSEE)). It has the advantage of being comprehensive[2] and we can trace firms well before the date of the reforms but its administrative nature entails that many outliers must be removed from the base before proper use[3]. Moreover, we only have accounting data at our disposal, which means that we don't have loan-level data for each firm-year observation. In particular, we can't distinguish CODEVI debt from other kinds of debt; however, we can still distinguish long-term bank debt (including CODEVI) from other kinds of debt. Our second concern was that targeted firms must be independent, i.e. not under ownership of an ineligible firm. That is why we needed to use a second database from INSEE that details ownership relationships between firms (the Liaisons Financieres, or LIFI, database). In that case, the problem was that before 1992, information on ownership of small and medium firms is not reliable enough to be used for econometric purposes. Finally, in order to avoid any bias due to entry caused by the reforms in question, we decided to remove any firm entering the database for the first time after the 1^{st} reform, i.e. after 1994.

As a result, our database includes 160000 firm-year observations on retail and wholesale trade firms between 1992 and 1999[4]. Therefore, in comparison with Banerjee and Duflo (2004), the size of our sample is very large. However, we don't have loan-level variables such as the interest rate and the duration of each loan variables at our disposal.

In order to look at the effects of the reforms on labour, we have merged our database with the Donnees Annuelles-Donnees Sociales (DADS) dataset for the years 1994 to 1999. This dataset contains information on the number of hours worked yearly in each French firm, but it is only available from the year 1994. Our analysis will therefore only deal with the effects of the second reform on labour.

We have also added data on bankruptcies at firm-level, extracted from the database provided by French courts to the National Institute of Statistics. All bankruptcy filings are registered in that database, so that we are able to consider the effect of both reforms on default.

Finally, we have gathered data on firms' birth date from the INSEE-SIRENE register of French firms. Though this data suffers from a nonnegligible attrition rate (around 10%), we'll use it to look at the issue of loan officers' screening attitudes.

Descriptive statistics of the sample are to be found in Table 1. Financial variables are in thousands of euros 1995. The sample is almost equally divided into wholesale and retail trade firms. Note that more than 96% of the sample corresponds to firms whose size and ownership pattern make them eligible for CODEVI loans. But these 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. The stock of subsidisable bank debt for eligible firms is 160,000 euros on average. It is also important to note that wholesale trade SMEs are on average a little bigger than retail trade SMEs : the former have an average of 25 employees while the latter have an average of 20 employees, and this difference also exists for different size variables. That is why we have to control in our estimations for size effects.

Insert Table I.

3 Establishing Credit Constraints

3.1 Theory

The theory underlying our identification strategy is related to the one developed by Banerjee and Duflo (2004).

The policy change we analyze involved that the firms in question were offered additional bank credit. However this in itself does not imply that they would have borrowed more at the market interest rate : as the CODEVI accounts are tax subsidized, the cost of CODEVI funds for banks may be lower than usual and banks may be tempted to pass this cost saving on to firms, and this will increase the demand for loans. The eligible firms' debt increase following the reforms may then simply be the consequence of a change in firms' demand for credit : that's why disentangling price and quantity effects of the reforms is a crucial matter.

But if firms aren't credit constrained and if the reforms have significant downward effect on loan prices, then the first effect of the reforms for the firms in question will be that they will substitute market borrowing with bank loans, simply because they are cheaper. Therefore, *if following the reforms, firms increase their bank credit without substituting for their market borrowing, then it means that there is no change in demand and these firms are indeed credit constrained.*

This is a first way to distinguish supply from demand effects of the reforms. A second method of identification goes through the analysis of the profitability of newly incurred debt. If firms aren't credit constrained and the CODEVI rate is lower than the market rate, the profitability of newly incurred debt should be between the cost of CODEVI funds and the market cost of funds. Thus, *if following the reforms, firms increase their credit and increase their profits in such a way that the implied profitability of debt is greater than the market cost of funds, then it means that these firms are credit constrained.*

In addition, our natural experiment allows us to detail the identity of the loanees when loan officers suddenly have to make more loans and find credit constrained clients. In particular, we want to know whether they primarily use "soft" information, that is their past relationships with firms. According to Stein (2002), if banks are centralised organisations, then credit constraints may arise among those firms on which banks' information is mainly "soft". So *if we first find that there are some credit constrained firms, and if we observe that these firms were mostly those on which banks' information is "soft", then we can confirm the validity of Stein's hypothesis.*

This is why we develop an original method for the identification of banks' information on credit constrained firms. If the hypothesis that banks mainly use "soft" information is true, then sudden access to the CODEVI programme should have two effects : on the demand side, older firms should already have good access to credit before the reforms and therefore should ask for less additionnal credit than younger firms; on the supply side, loan officers should want to privilege the firms with whom they already had relationships in the past, that is older firms. As a consequence of these two contradicting effects, there may be a nonlinearity in the allocation of the CODEVI funds depending on age : older firms may not want to enter the programme while very young firms may be refused access to it, so that middle-aged firms are those that may take the most benefit from official eligibility to the programme. On the other hand, if banks use "hard" information in the form of indicators of financial soundness contained in financial statements, then we should observe that firms that are financially sound according to their statements should benefit from access to the CODEVI programme more than average. Therefore, i*f being a middle-aged firm is a good predictor of who benefits from the reforms while "hard" indicators of financial health aren't such good predictors, then credit constrained firms are those on which banks' information is "soft".*

3.2 Empirical strategy : Reduced Form Estimates

As was already explained, our empirical strategy takes advantage of two successive extensions of the targeted lending scheme, first to the wholesale trade small-and-medium firms (effectively as of the 4th quarter of 1994) and secondly to the retail trade small-and-medium firms (in December 1995). As noted above, previously eligible firms from industrial sectors obtained a very large part of their loans from CODEVI funds, so it is reasonable to think that extension to wholesale and retail trade sector firms reduced the shadow price of borrowing for these firms.

There may be endogenous entry of trade firms because of the reforms, so we decided to restrict our analysis to firms that existed before the reforms (e.g. in 1993) to avoid selection bias. Since debt incurred by firms and the outcome variables we will consider are very persistent and because much of the variation may come from firm size, we focus on the first-difference in logs of these variables, i.e. $\log(x_t)$-$\log(x_{t-1})$.

Our strategy is to use these two changes in policy as a source of shock on the availability of bank credit to the small and medium-sized trade firms, using each trade sector as a control group for the other trade sector. The first stage consists however in checking that there were indeed such shocks. To do this we estimate an equation of the following form in the sample of firms whose size and ownership patterns fit the criteria of CODEVI loans' eligibility :

$$\log k_{it} - \log k_{it-1} = a_k X_{it} + g_{1k} WS_i{*}POST_{1t} + g_{2k} RET_i{*}POST_{2t} + \varepsilon_{kit} \quad (1)$$

where we adopt the following convention for the notation : k_{it} is a measure of total credit to firm i in year t, WS_i is a dummy indicating whether the firm i belongs to the wholesale trade sector, RET_i is a dummy indicating whether the firm i belongs to the retail trade sector, $POST_{1t}$ (resp. $POST_{2t}$) is a dummy equal to one in the years after the 1^{st} reform in 1994 (resp. after the second reform in 1995)[5] and X_{it} is a set of controls including year dummies, a size variable (log of number of employees in period t-1) and their interactions, and 2-digit sectoral dummies. This means that $WS_i{*}POST_{1t}$ and $RET_i{*}POST_{2t}$ are dummies for the effects of the 1^{st} and 2^{nd} reform respectively. Because of year fixed-effects and sectoral fixed-effects, our estimations of the impact of the reforms account for potential trends for or against the use of credit and for potential structural differences among sectors in the use of credit. One should also note that because our sample is exclusively made of retail and wholesale trade firms, the interactions WS_i*$POST_{1t}$ and RET_i*$POST_{2t}$ account for differences in sectoral differences in debt and other outcomes between three periods of time : 1992 to 1994, 1995 and 1996 to 1999, so that the specification is not too restrictive as to what the data can tell us on differential sectoral trends. Moreover, because the period of time during which retail and wholesale trade had opposite eligibility status is reduced to one year, we can only look at short-term effects of the reforms.

We now have to check whether there was a negative effect of the reform on the demand for kinds of credit not supplied with CODEVI funds, such as bond credit, loans from financial institutions other than banks, and less-than-one-year bank loans. Assuming that these sources of credit are good substitutes for more-than-one-year bank loans, if the reform has had no effect on the demand for these kinds of credit, then the existence of a positive effect of the reforms on total credit implies the existence of credit constraints.

That is why we estimate the following equation, similar to equation 1 :

$$\log k_{oit} - \log k_{oit-1} = a_{k_0}X_{it} + g_{1ko}WS_i{*}POST_{1t} + g_{2ko}RET_i{*}POST_{2t} + \varepsilon_{koit} \quad (2)$$

where k_{oit} is the amount of credit other than more-than-one-year bank loans incurred by firm i in year t. We'll say that firms are credit constrained if g_{1k} and g_{2k} are positive on one hand, and if g_{1ko} and g_{2ko} are equal to zero on the other hand.

3.3 Empirical Strategy : Structural Estimates

According to our identification strategy presented in the first subsection, firms are credit constrained if the return on capital is higher than the market interest rate. We try now to estimate the size of this differential.

We will first use an instrumental variables estimation of the effect of credit on value added, using WS_i*$POST_{1t}$ and RET_i*$POST_{2t}$ as instruments for [$\log k_{it} - \log k_{it-1}$]. This will allow us to estimate the elasticity of output with respect to total credit incurred.

But the impact of total credit on value added does not directly inform us on the marginal benefit of the extra investment : entrepreneurs who incurred more debt thanks to the reforms might simply be empire-builders wishing to invest any available euro as long as it increases firm size. That is why a final piece of evidence comes from looking at profits : we make another instrumental variables estimation of the effect of credit on profits, also using WS_i*POST_{1t} and RET_i*POST_{2t} as instruments for [log k_{it} − log k_{it-1}], in order to see the impact of additional credit on profitability.

The problem is that the logarithm of profits is only defined when profits are strictly positive, leading to potential sample selection. To solve that problem, we do a separate instrumental variables estimation for the effect of bank credit on value added and on costs and compute an effect on profits : in order to check that our direct estimate doesn't suffer from selection bias, we compare it with that indirect estimate.

In order to analyse the effect of credit constraints on employment, we take advantage of the availability in our dataset of variables such as hours worked each year. We will compute the same instrumental variables estimations as above to analyse the effect of credit on the number of hours worked per year.

3.4 Empirical strategy : Robustness Checks

The interpretation of the results crucially depends on the assumption that the error term is not correlated with the regressors, especially for WS_i*POST_{1t} and RET_i*POST_{2t}. in equation (1). But there are many reasons why that assumption may not hold. In particular, wholesale and retail trade SMEs may be differently affected by other measures of economic policy.

We use several parallel methods to address this concern. The first one consists in checking that there were no differential trends before and after the reforms. In order to do so, we estimate an equation similar to (1) where we replace WS_i*POST_{1t} and RET_i*POST_{2t} by interactions of year dummies with the wholesale trade dummy and we perform a F-test of equality of these year-sector interactions excluding the year 1995. If the test fails to reject this hypothesis, then we can argue that our results do not come from differential trends before and after the reforms.

The second method takes advantage of the fact that there were two experiments affecting different sets of firms. This will allow us to test whether each reform has had exactly the same impact for two different targets or not. In more econometric terms, we use an over identification test following each of our Instrumental Variable Estimates. If these tests do not reject the hypothesis of exogeneity of our Instrumental Variables, then it is highly implausible that our results arise from differential trends between wholesale and retail trade firms.

Our third method takes advantage of the fact that firms whose size or pattern of ownership do not fit the criteria of eligibility of the CODEVI loans are not affected by the two experiments, whatever their economic sector. Then if there are differential trends between wholesale and retail trade firms, we should expect to see them also 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. If the estimates for WS_i*POST_{1t} and RET_i*POST_{2t}. aren't significant, then it is even more implausible that our results for the effect of the reforms arise from differential trends between wholesale and retail trade sectors.

Our last set of robustness checks as to the exogeneity of our reforms concerns the potential effects of another regulatory change, the enactment of the Raffarin law in July 1996. It lowers the size threshold above which the building of new supermarkets has to be authorised by a commission composed of politicians and consumers' representatives, from 1000 square metres to 300 square metres. As shown by Bertrand and Kramarz (2001), this system constitutes a stringent barrier to entry in the retail trade sector, but probably not as much in the wholesale trade sector. These changes in the regulation of the retail trade sector may partly account for the sizeable effect of credit on output we analyse in this paper. In order to check whether this is the case or not, we take advantage of two aspects of the Raffarin law. First, as the law slows down the process of decision to open new supermarkets from the end of 1996, it should have gradual effects on retail trade incumbents' output, so we compute our estimates of the effects of credit on output with data up to 1996 and then up to 1997 and then up to 1998. If the Raffarin law explains part of our estimates then the effect of credit on output should be significantly bigger and bigger as we extend our data longer and longer after the enactment of the Raffarin Law. Secondly, we know[7] that the Raffarin Law aimed at slowing down the development of hard discounters such as LIDL and ALDI, whose business is essentially retailing food products; so if the Raffarin law explains part of our estimates, then these estimates should be lower when we control for yearly changes in the food retail sector. That is why we compute our estimates of the effects of credit on output including in our regressions dummies for the food retail sector interacted with year dummies and compare them to our previous estimates.

Another concern about the results we may obtain is potential autocorrelation of errors across time, as shown by Bertrand, Duflo and Mullainathan (2004). Our estimations may not be as subject to this kind of criticism as many other studies using differences-in-differences estimates because the number of years before and after the reforms is not very large. However, we check that our standard errors are not biased downwards in the following way : we repeat our set of Instrumental Variables regressions with a sample restricted to 3 years chosen in order to represent each period of the programme (before the first reform, between the two reforms, and after the second reform) and to maximise the distance between each year, assuming that autocorrelation of errors across time decreases as the time distance between observations increases. We then check that the results we obtained earlier are still significant in that new set of regressions.

One last criticism we address about the results we may obtain is that very profitable investments may be driven by an increase in risk-taking by investors. The increase in output and profits may then reflect more risky strategies pursued by firms who incur CODEVI debt, instead of the existence of credit constraints.

In order to check this, we look at the effect of debt on the probability of filing for bankruptcy one year later, using the same kind of IV estimation as above, but with a dummy equal to one when firm i files for bankruptcy in year t +1 as a dependent variable. If the effects of debt on probability of filing for bankruptcy are not significantly positive then an increase in output and profits may hardly be explained by the undertaking of riskier projects.

3.5 Empirical Strategy : Bank screening analysis

According to our identification strategy, loan officers use "soft" information for the marginal credit constrained borrower if middle-aged firms benefit from the CODEVI reforms more than average, irrespective of indicators of financial health. This is why we estimate the following equation :

$$\log k_{it} - \log k_{it-1} = a_s X_{it} + g_s REF_{it} + b_{1s} REF_{it}{*}YNG_{it} + b_{2s} REF_{it}{*}OLD_{it} + b_{3s} REF_{it}{*}M_{it-1} + c_{1s} YNG_{it} + c_{2s} OLD_{it} + c_{3s} M_{it-1} + \varepsilon_{kit} \quad (3)$$

where REF_{it} is a dummy equal to 1 when either $WS_i{*}POST_{1t}$ or $RET_i{*}POST_{2t}$ is equal to 1, YNG_{it} is a dummy equal to 1 when firm i in year t belongs to the 10 percent youngest firms in the sample (e.g. less than 6 years old), OLD_{it} is a dummy equal to 1 when firm i in year t belongs to the 50 percent eldest firms in the sample (e.g. more than 15 years old), and M_{it} is a vector of "hard" indicators of financial health such as its insolvency ratio (financial costs over EBITDA) and its operating return on assets, and an indicator of firm size (the log of the number of employees). X_{it} is the same set of controls as above plus firm age interacted with 3-digit sectoral fixed-effects in order to control for differences in firms' life cycle across sectors. We'll say that credit constraints arise mostly among firms on which banks' information is "soft" if b_{1s} and b_{2s} are negative while b_{3s} is not significantly different from 0.

4 Results

4.1 Credit

Table II, 1^{st} column, presents the results of estimating equation (1) in the sample of independent firms whose sales don't exceed 76 million euros. The coefficients for $WS_i{*}POST_{1t}$ and $RET_i{*}POST_{2t}$ are equal to 0.076 and 0.087 respectively and both estimates are significant at the 1% level. Therefore we can say that both reforms had approximately the same effects on their targets, when we compare the credit outcome of each target with the relevant control group.

This means that access to the CODEVI scheme allowed newly eligible firms to increase their total credit stock by 8% on average.

Insert Table II.

4.2 Evidence of Credit Constraints

4.2.1 Reduced Form Estimates

In table III, 1^{st} column, we look at the impact of the reforms on total credit excluding more-than-one-year bank credit, in the sample of independent firms whose sales don't exceed 76 million euros. The coefficients for $\mathrm{WS}_i{*}\mathrm{POST}_{1t}$ and $\mathrm{RET}_i{*}\mathrm{POST}_{2t}$ are equal to 0.009 and 0.021 but aren't significantly negative, showing that there wasn't a significant amount of substitution between subsidized credit and other kinds of credit.

This result and the one in the previous subsection suggest that these SMEs were credit constrained at the time of the reforms.

Insert Table III.

4.2.2 Instrumental Variables Estimates

In this section, we present in table IV the instrumental variables estimates of the effect of credit on value added, profits, costs, and hours worked.

Row (1) presents the Instrumental Variables estimate of the effect of credit on value added using the instruments $\mathrm{WS}_i{*}\mathrm{POST}_{1t}$ and $\mathrm{RET}_i{*}\mathrm{POST}_{2t}$ in the sample of independent firms whose sales don't exceed 76 million euros. The coefficient is 0.41 with a standard error of 0.11.

Row (2) presents the Instrumental Variables estimate of the effect of credit on operating costs (equal to value added minus EBITDA). The estimates we obtain are significantly smaller than the ones for value added, which suggests that our direct estimate for profits may not be very biased.

Row (3) presents the Instrumental Variables estimate of the effect of credit on operating profits (equal to EBITDA). The estimates we obtain are very significant, as the elasticity of profits with respect to credit is equal to 0.83 with a standard error of 0.28.

We can use these estimates to get a sense of the average increase in profit caused by every euro in loan. In the subset of the sample where EBITDA is strictly positive, the average credit stock (averaging across years and SMEs) is 454,934 euros and the average EBITDA is 258,681 euros, so the estimate we obtained in row (3) of table IV allows us to calculate that an increase of 100 euros in the credit stock corresponds to an increase of 47 euros in profits with a standard error of 16 (so the 95% confidence interval is between 15 and 79 euros).

But, as already mentioned above, it may be that our estimate is biased as we don't take into account firms whose EBITDA is negative. This is why 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 credit stock (averaging across years and SMEs) is 460,786 euros, while the average value added and operating costs are equal to 953,326 euros and 755,236 euros respectively; therefore, using the coefficients in column (1) and (2), an increase of 100 euros in the credit stock corresponds to an increase of 85 euros in value added, and 38 euros in operating costs. This implies a 47 euros increase in EBITDA for the average firm. So our direct estimate of the effect of credit on profits is not biased.

We now need to assess whether such a return on debt can be completely explained by a subsidy in the form of reduced interest rates. In order to support this hypothesis, the potential subsidy on interest rates for these SMEs should be close to the return of debt we unveiled in our analysis. But, according to the Banque de France, the average interest rate for more-than-one-year bank loans in 1995-1996 was equal to 8%, which is significantly below 47 %. This reinforces our conclusion that targeted firms were severely credit constrained at the time of the reforms.

For obvious public policy reasons, we would like to know the effect of the reduction of credit constraints on labour. This is why we look now at row (4) where we present the IV estimates of the effect of credit on hours worked. The coefficient is significant at the 5% level. Using this coefficient, we find that an increase of 100 euros in the credit stock corresponds to an increase of 5.59 hours worked during the year. As in 1995, according to the French Ministry of Labour, the average annual work duration was equal to 1773 hours, this means that 31150 euros in credit are needed to create one additional job. And knowing that the hourly minimum wage was equal to 5.64 euros in 1995, we can compute that an increase of 100 euros in credit stock leads to an increase of at least 32 euros in wages.

Insert Table IV.

4.2.3 Robustness checks

Firstly, the F statistic for the test of equality of interactions between the wholesale dummy and year dummies excluding 1995 is equal to 0.76 with a p-value of 0.60 , which shows that there weren't observable differential trends in the use of external credit before and after the reforms. A graph in figure 3 shows the estimates for these interaction dummies across time and their 95% confidence intervals. It clearly confirms our claim that the shocks to credit were exogenous.

Then we check that the Sargan test doesn't reject the hypothesis of exogeneity of our Instrumental Variables, by looking at the Sargan statistics p-values for each IV estimation in table IV. We observe that the p-value is always large enough to confirm the quality of our instrumental variables. Intuitively, this means that both reforms had the same effect on two different groups, which helps us believe in the good quality of the "natural experiment".

As planned above, we also look at tables II and III to check that when firms aren't eligible to the program for reasons of size or pattern of ownership, their behaviour is exactly the same during the period whatever the sector they belong to. We can see that in this sample, the estimates for WS_i*POST_{1t} and RET_i*POST_{2t} are always insignificant. This shows that the results we get for the sample of SMEs are not driven by differential sectoral trends.

As for our concern about other simultaneous regulatory changes, we can consider that the Raffarin Law has no effect on our estimates, since they are constant across time (see table V, column 4,5, and 6) and the inclusion of year-dummies for the food retail sector doesn't significantly change the size of our effects of the CODEVI reform on profits (see table V, column 3).

Insert Table V.

Finally, we look in table VI at the results of the IV estimation of the effect of debt on bankruptcy filing one year later. It appears that newly incurred debt had a positive but insignificant effect on the probability of filing for bankruptcy one year later. This result tends to show that the superior profitability of investments made by newly eligible firms is not due to more risk-taking on the lender side.

Insert Table VI.

4.3 Bank screening analysis

In table VII, we detailed the results of our estimations of equation (3) in the subsample of observations where firm age is a nonmissing variable. We observe that middle-aged firms were twice as likely to benefit from access to the CODEVI programme as were younger and older firms (with total debt increasing by 11% among middle-aged firms instead of 5% for younger and older firms thanks to access to the programme), while "hard" indicators of financial health and firm size do not seem to be dimensions along which decisions to allocate CODEVI funds were made. This result, together with the previous result that there exist credit constraints among firms in our sample, suggests that it is the prevalence of "soft" information on them which prevented these profitable firms from getting credit before the CODEVI programme took action.

Insert Table VII.

5 Conclusion

This paper takes its inspiration from the analysis of a public policy in a developing country, and yet we find that the effects of a similar French policy are not much lower than those of the Indian policy analysed by Banerjee and Duflo (2004).

Part of this paradox may be explained by the existence of increasing returns to scale in the French trade sector circa 1995, as opposed to industrial sectors : there are important fixed costs of advertising and stocking and yet the delay between investments and returns is much shorter.

Still, one has to come up with an explanation of why banks were reluctant to lend to these firms before they had access to the program and how the program managed to remove this reluctance.

It certainly has to do with the fact that the CODEVI programme targets small firms. As argued by Stein (2002), the inability to lend to small firms is consubstantial to being a bank : banks have to be large enough to spread out idiosyncratic risk, but this comes at the expense of having a long distance between the owners of a bank and its loan officers, thus creating agency problems within the bank. In order to reduce these agency problems, bank owners will usually have to limit the amount of funds a loan officer can provide to the clients whose good prospects are not easily verifiable, which will be more often the case for small firms.

With this story in mind, the existence of the CODEVI programme should lead to more lending to small firms : following Stein's explanation, bank owners will then have an incentive to allow their loan officers to make more loans to these clients on whom they have good "soft" information in order not to pay penalties : we were able to test this hypothesis, and evidence from the CODEVI reforms consistently showed that this mechanism was the main responsible for the existence of credit constraints in our sample.

In terms of policy design, this paper has two different implications. First, it has to be pointed out that on a normative basis, it is not sufficient for welfare analysis to assess whether this programme helped small firms to invest profitably. This programme consists in channeling savings to some small and medium firms via fiscal incentives, so it has to be proved that other firms would have invested these funds less profitably. We know from Aghion and Bolton (1997) that in a world with decreasing returns to scale and imperfect capital markets, the distribution of income will have an effect on the extent of credit rationing in the whole economy : rich people may have too much money to invest in their own productive activities and be too reluctant to lend this money to poor people which are thereby made unable to use their productive abilities. In such a world, redistribution of income from rich to poor will be welfare-improving as it reduces the extent of credit rationing. In some sense, the CODEVI programme has some of the characteristics of a redistribution scheme : assuming that the average CODEVI saver is richer than the average CODEVI borrower, the fiscal incentive to save in CODEVI accounts may act as a welfare-improving system of redistribution from rich to poor. In a companion paper (Bach

(2007)) we develop a model along these lines; we show that there exists a large set of tax and subsidy rates that will substantially reduce credit rationing and allow for the realisation of very profitable projects, as observed in this empirical study. Moreover, we show that such a policy is very cost effective for the state, as foregone money due to subsidies will be more than recovered through taxes on newly implemented projects. In particular, using the average estimate of the net return of debt that we obtained in this study as a parameter in the model, and calibrating our model in the France of 2006, we show that allowing savers to put one additional euro in CODEVI accounts will provide the state more tax revenues than tax losses by about 40%, and increase GDP in the same time.

Secondly, our results cast new light on the organisational mechanisms that lead to credit constraints and it's not clear whether the problem has deepened or not ever since the time of the reforms we analysed in this paper. On the one hand, though we do not have any aggregate data on banks' credit policies in France, one can argue that banks' information on small firms has increased a lot since 1995 thanks to improvements in communications and storage technologies; but on the other hand, the liberalisation of the European banking sector in 1993 and the introduction of the euro in 1999 have triggered a series of mergers and acquisitions in the French banking sector that may have increased centralisation of loan decisions : firm-level evidence from the United States (Sapienza (2002)) and Belgium (Degryse et al. (2005)) seems to confirm this point.

6 References

Adda, Jérôme, and Russel Cooper. (2000). "Balladurette and Juppette : A Discrete Analysis of Scrapping Subsidies." *Journal of Political Economy* 108, 778-806.

Aghion, Philippe, and Patrick Bolton. (1997). "A theory of trickle-down growth and development." *Review of Economic Studies 64*, 151-172

Allain, Marie-Laure, and Claire Chambolle. (2003). "Les relations entre la grande distribution et ses fournisseurs : bilan et limites de trente ans de régulation." *Revue Française d'Economie* 17, 169-212

Bach, Laurent. (2007). "Optimal investment subsidy with credit constraints : theory and evidence." *Mimeo Paris School of Economics*

Banerjee, Abhijit, and Esther Duflo. (2004). "Do firms want to borrow more ? Testing Credit Constraints Using a Directed Lending Program." *CEPR Discussion Paper* 4681

Berger, Allen.(2004). "Bank concentration and competition: An evolution in the making." *Journal of Money, Credit and Banking 36*, 433-451

Berger, Allen, and Gregory Udell. (2005). "A more conceptual framework for financing of small and medium enterprises." *World Bank Policy Research Working Paper* 3795

Bernanke, Benjamin, and Mark Gertler.(1989). "Agency Costs, Net Worth, and Business Fluctuations". *American Economic Review 79*, 14-31

Bertrand, Marianne, and Francis Kramarz. (2002). "Does Entry Regulation Hinder Job Creation? Evidence from the French Retail Industry." *Quarterly Journal of Economics 107*, 1369-1413

Bertrand, Marianne, Antoinette Schoar, and David Thesmar. (2007). "Banking Deregulation and Industry Structure : Evidence from the French Banking Reforms of 1985." *forthcoming Journal of Finance*

Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. (2004). "How much should we trust differences-in-differences estimates ?" *Quarterly Journal of Economics 119*, 249-275

Degryse, Hans, Nancy Masschelein, and Janet Mitchell.(2005). "SMEs and bank lending relationships : the impact of mergers". *Federal Reserve Bank of Chicago Proceedings May* 148-165

Fazzari, Steven, Glenn Hubbard, and Bruce Petersen. (1988). "Financing Constraints and Corporate Investment." *Brookings Papers on Economic Activity 1, 141-195*

Kaplan, Steven, and Luigi Zingales (2000). "Investment-Cash Flow sensitivities are not valid measures of Financing Constraints." *Quarterly Journal of Economics 115*, 707-712

Lamont, Owen. (1997). "Cash flow and investment : Evidence from Internal Capital Markets". *Journal of Finance 52*, 57-82

Loridant, Paul, and Philippe Marini. (1995). "Rapport d'information sur les Comptes pour le Développement Industriel (CODEVI)." *Rapport d'information du Sénat* 298 (1994-1995)

Paravisini, Daniel. (2005). "Targeting Credit Through the Banking System : does it work ?" *Columbia University GSB Working Paper*

Piketty, Thomas. (1997). "The dynamics of the wealth distribution and the interest rate with credit rationing." *Review of Economic Studies 64*, 173-189

Rauh, Joshua. (2006). "Investment and Financing Constraints : Evidence from the funding of corporate pension plans". *Journal of Finance 61*, 33-72

Sapienza, Paola. (2002). "The effects of banking mergers on loan contracts". *Journal of Finance 57*, 329-367

Stein, Jeremy. (2002). "Information Production And Capital Allocation : Decentralized Versus Hierarchical Firms." *Journal of Finance* 57, 1891-1921

Stein, Jeremy. (2003). "Agency, information and corporate investment" in *Handbook of the Economics of Finance*, edited by George Constantinides, Milton Harris, and René Stulz, pp. 111-165. Elsevier Science B.V.

Stiglitz, Joseph, and Andrew Weiss.(1981)."Credit Rationing in Markets with Imperfect Information." *American Economic Review 71, 393-410*

Tirole, Jean. (2006). *The Theory of Corporate Finance.* Princeton, NJ.: Princeton University Press.

7 Notes

1. As quoted by Loridant and Marini (1995).

2. Any firm with annual sales superior to 533000 euros is included in the dataset.

3. Each firm had to respect the following criteria over the years 1991-2000 : exist at least 3 years, have at least one year more than 10 employees, have each year the following ratios between the 1st and the 99th percentile : ROA, EBITDA/Sales, Credit stock/Assets, and finally have strictly positive value added and credit stock.

4. Each accounting year begins the 1st of January and ends on the 31st of December.

5. Note that both reforms started to have real effects at the beginning of the calendar year, so that the data we have take into account what happens during around one year after the reforms.

6. We also include in our regressions dummies for trade in car-related goods interacted with year dummies in order to take into account a specific regulatory change for trade in car-related goods (a sector that is included in the retail trade sector) : in 1993 and 1995, the government implemented used cars' scrapping subsidies. The effect on car sales was significant, as documented by Adda and Cooper (2000).

7. See Allain and Chambolle (2003).

Table I : Descriptive Statistics

Firm type :	Retail SMEs	Wholesale SMEs	SMEs	Big Firms
Credit stock	**343** *(3)* *83295*	**597** *(12)* *72084*	**461** *(6)* *155380*	**9388** *(352)* 5799
More-than-one-year bank debt	**156** *(1)* 83295	**160** *(2)* 72084	**158** *(1)* *155380*	**1752** *(101)* 5799
Sales	**3994** *(22)* 83295	**7116** *(38)* 72084	**5442** *(22)* *155380*	**141050** *(3695)* 5799
Value Added	**729** *(4)* 83311	**1213** *(7)* 72084	**953** *(4)* *155380*	**15799** *(445)* 5799
EBITDA	**137** *(1)* 83295	**269** *(2)* 72100	**198** *(1)* 155380	**4289** *(152)* 5799
Firm age (in years)	**17.68** *(0.04)* 75283	**19.50** *(0.04)* 65040	**18.52** *(0.03)* 140323	**23.07** *(0.18)* 4907
Hours worked annually	**38871** *(255)* 60848	**48870** *(289)* 51692	**43479** *(192)* 112540	**544073** *(19512)* 4279
Nb. of employees	**20.34** *(0.12)* 83295	**25.53** *(0.13)* 72100	**22.74** *(0.09)* 155380	**288.56** *(8.64)* 5799

Note : Each column presents the mean level of each variable, with standard errors in parentheses and the number of observations on the third line. Values are expressed in thousands of euros 1995 where applicable.

Table II : Effect of the reforms on credit

Dependent variable : ln(credit stock$_t$) - ln(credit stock$_{t-1}$)

Firm type :	Small and medium firms	Big firms
$\mathbf{WS_i}$*$\mathbf{POST_{1t}}$ *(1^{st} reform)*	**0.076**** *(0.02)*	**0.084** *(0.188)*
$\mathbf{RET_i}$*$\mathbf{POST_{2t}}$ *(2^{nd} reform)*	**0.087**** *(0.02)*	**0.004** *(0.177)*
Nb. Observations	155380	5799

Note : Robust standard errors in parenthesis.

* : significant at 5% level ** : significant at 1% level

All regressions include controls for 3-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.

Table III : Effect of the reforms on unsubsidized credit

Dependent variable :	Δlog(credit without long-term bank debt)	
Firm type :	SMEs	Big firms
$\mathbf{WS_i{*}POST_{1t}}$	**0.009**	**0.305**
(1st reform)	(0.029)	(0.221)
$\mathbf{RET_i{*}POST_{2t}}$	**0.021**	**0.256**
(2nd reform)	(0.028)	(0.217)
Nb. Observations	155380	5799

Note : Robust standard errors in parenthesis

* : significant at 5% level ** : significant at 1% level

All regressions include the same controls as those presented in table II.

Table IV : Effect of credit on output, costs, profits and labour

(Sample of SMEs)

	Regressor : Δlog(credit stock)	
Dependent variables	IV	OLS
Δlog(value added)	**0.41****	**0.02****
	(0.11)	(0.00)
Sargan p-value	0.15	
Observations	155380	155380
Δlog(operating costs)	**0.23****	**0.02****
	(0.07)	(0.00)
Sargan p-value	0.10	
Observations	155380	155380
Δlog(EBITDA)	**0.83****	**-0.00**
	(0.28)	(0.00)
Sargan p-value	0.19	
Observations	126059	126059
Δlog(hours worked)	**0.37****	**0.02****
	(0.10)	(0.00)
Sargan p-value		
Observations	90703	90703

Note : Robust standard errors in parenthesis

* : significant at 5% level ** : significant at 1% level

All regressions include the same controls as those presented in table II.

Table V : Robustness checks on IV estimates

(Sample of SMEs)

	Regressor : Δlog(credit stock)					
Dependent variables	(1)	(2)	(3)	(4)	(5)	(6)
Δlog(value added)	**0.41****	**0.50****	**0.44****	**0.38****	**0.37****	**0.41****
	(0.11)	(0.18)	(0.13)	(0.11)	(0.12)	(0.13)
Observations	155380	68989	155380	137287	118439	99447
Δlog(operating costs)	**0.23****	**0.18**	**0.24****	**0.21****	**0.17***	**0.14***
	(0.07)	(0.10)	(0.08)	(0.07)	(0.07)	(0.07)
Observations	155380	69242	155380	137287	118439	99447
Δlog(EBITDA)	**0.83****	**0.90****	**0.96****	**0.86****	**0.82****	**0.85****
	(0.28)	(0.32)	(0.33)	(0.29)	(0.29)	(0.31)
Observations	126059	56532	126059	111160	95888	80700
Δlog(hours worked)	**0.37****	**0.36****	**0.36****	**0.37****	**0.33****	**0.25***
	(0.10)	(0.13)	(0.10)	(0.09)	(0.10)	(0.10)
Observations	90703	47134	90703	73251	55173	37274
Sample reduced to years 1992, 1995 and 1999	No	Yes	No	No	No	No
Food retail-year effects	No	No	Yes	No	No	No
Last year in sample	1999	1999	1999	1998	1997	1996

Note : Robust standard errors in parenthesis.

* : significant at 5% level ** : significant at 1% level

All regressions include the same controls as those presented in table II.

Table VI : Marginal effect of credit on probability of survival

(Sample of SMEs)

Dependent variable : Probability of filing for bankruptcy for firm i in time t+1

	IV	OLS
Δlog(credit stock)	**0.035**	-0.000
	(0.026)	(0.000)
Sargan p-value	0.58	
Observations	155380	155380

Note : Robust standard errors in parenthesis

* : significant at 5% level ** : significant at 1% level

All regressions include the same controls as those presented in table II.

Table VII : Determinants of the allocation of CODEVI funds

(Sample of SMEs)

Dependent variable : Δlog(credit stock)

Regressors :	(1)	(2)	(3)	(4)	(5)
REF	0.11**	0.18**	0.11**	0.10**	0.17**
	(0.02)	(0.06)	(0.02)	(0.02)	(0.06)
REF*YNG	-0.05*	-0.05*	-0.05*	-0.05*	-0.05*
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
REF*OLD	-0.04**	-0.04**	-0.04**	-0.04**	-0.04**
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
REF*Firm size$_{t-1}$		-0.03			-0.03
		(0.02)			(0.02)
REF*Insolvency ratio$_{t-1}$			-0.00		-0.00
			(0.00)		(0.00)
REF*Return on assets$_{t-1}$				0.01	0.01
				(0.01)	(0.01)
Nb. Observations	140323	140323	140258	140323	140258

Note : Robust standard errors in parenthesis

* : significant at 5% level ** : significant at 1% level

REF is a dummy equal to one when firm i in year t eligible to the CODEVI programme; YNG is a dummy equal to one when firm i in year t belongs to the 10 percent younger firms in the sample; OLD is a dummy equal to one when firm i in year t belongs to the 50 percent older firms in the sample. Firm size is defined in Table II and the insolvency ratio is defined as the ratio of interests over EBITDA. All regressions include the same controls as in Table II plus age-sector fixed effects.

Figure 1 : Evolution of the scheme around the reforms

(source: Banque de France)

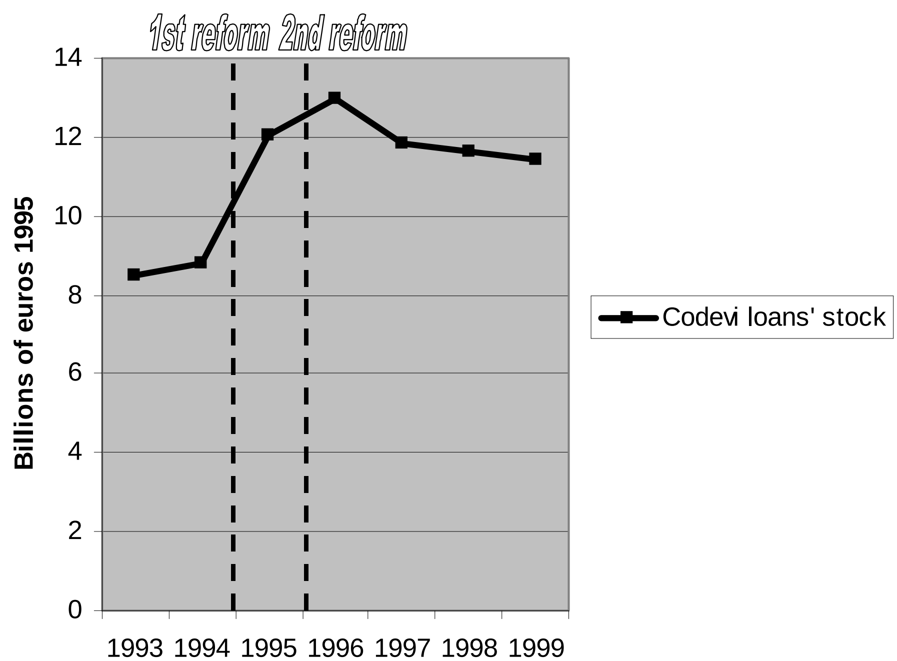

Figure 2 : Part of each sector in new CODEVI loans

(source: French Treasury)

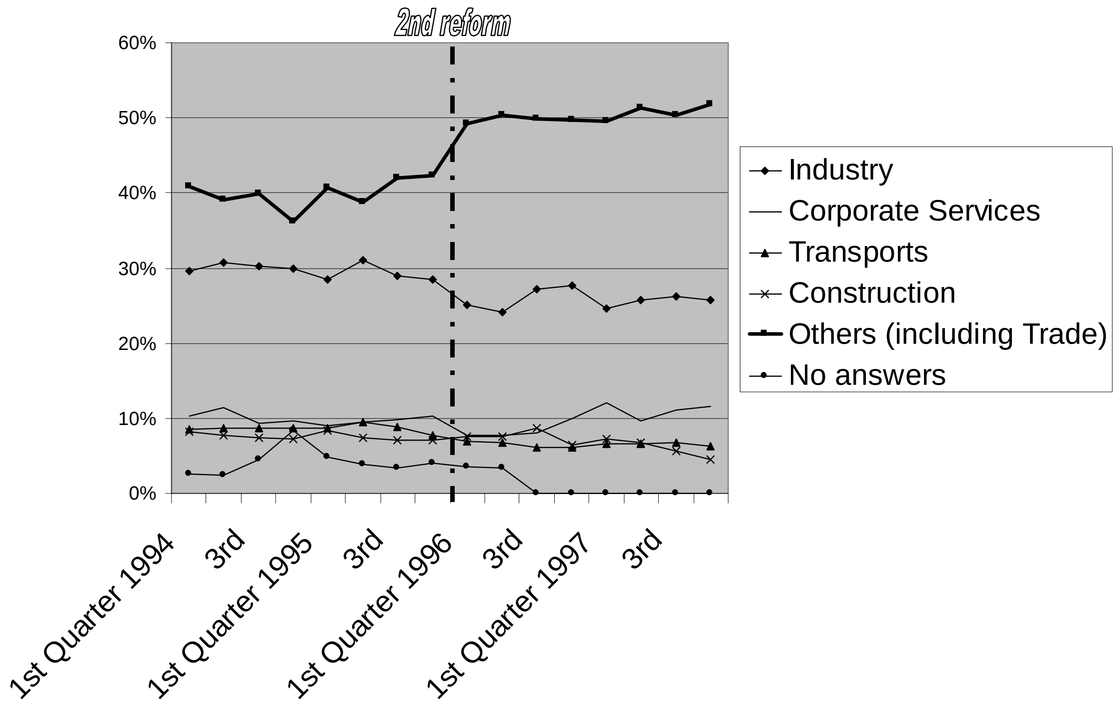

Figure 3 : Estimated difference in credit growth between wholesale and retail trade SMEs per year