Research Policy 47 (2018) 1344–1365

Contents lists available at ScienceDirect

Research Policy

journal homepage: www.elsevier.com/locate/respol

Partly risky, partly solid – Performance study of public innovation loans

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ARTICLE INFO

JEL classification:
H440
H540
O380

Keywords:
Innovation policy
Direct lending programs
Control groups
Asymmetric information

ABSTRACT

In this paper I attempt to measure the ability of a Norwegian publicly subsidized loan program to identify innovative firms that are victims of market imperfections. I apply three complementary control groups, which all have in common that they address specific unobservable characteristics of the program participants. The program participants perform better on a variety of growth measures compared to the firms rejected by the program. Compared with firms that receive private credit financing, I do not find that the program participants perform better in the upper quantiles of the contingent performance distribution despite a lower survival rate. The latter result suggests that the program does not seem to succeed in identifying a target group of firms with a sufficiently high growth potential. Firms with innovation loans are not outperformed by venture portfolio companies with respect to sales growth. The venture portfolio companies do, however, have higher survival rates as well as stronger growth in employment and assets. The latter result possibly indicates that the venture portfolio companies are more likely to succeed in the long run. The overall results indicate that the selection competency of the bureaucrats administrating the program is at level with that of private banks, and possibly also of that of venture funds. Still, in order for the program to provide the same level of welfare improvement as regular business credit provided by the private market, I find that the positive externalities from the program must be sufficiently large to compensate for the direct public subsidy element including risk adjusted return on equity and social costs of public funds.

1. Introduction

With the financial crisis of 2008–09, policies that intend to supplement private financial markets received renewed interest as a response to tightened bank credit lines. According to OECD (2009), government loan and credit guarantee schemes were the most frequently applied public measures to enhance SME liquidity in response to the financial crisis. Public credit programs appeal to policy makers as they leverage public funds, have limited up front costs, and the liabilities are contingent and pushed into the future (Honohan, 2010). This gives credit programs an advantage over schemes providing grants, equity and tax credits.

Following the global proliferation of publicly financed loan and guarantee schemes, there are numerous studies from different countries that try to measure the effectiveness of public credit programs (Warwick and Nolan, 2014; Valentin and Wolf, 2013; Samujh et al., 2012; Beck et al., 2008). The results are, however, ambiguous, partly due to differences in program scope and design across countries, but also likely due to varying methods.

As described by Curran (2000), the main challenge in evaluating small business policies is finding a proper control group. This challenge still remains to be solved, as private sector development programs rarely are designed with a component of random participation (Warwick and Nolan, 2014). As a second best approach, one can either try to find well-controlled comparisons and/or natural quasi experiments (Angrist and Pischke, 2008). There are severe methodological challenges related to sampling in non-randomized studies. Storey (1998) distinguishes between two types of sampling biases arising from selective public policy programs: (1) Self-selection bias arising from motivated firms applying to be part of the programs, and (2) the administrative bias arising from the scheme providers choosing which firms to finance.

Several effect studies of private sector development programs apply propensity score matching (PSM) to identify control groups that prior to treatment are as similar as possible to the program participants (see e.g. Oh et al., 2009; Norrman and Bager-Sjögren, 2010; Uesugi et al., 2010; Foreman-Peck, 2013; Autio and Rannikko, 2016). The control groups selected with PSM, however, fail to address non-observable firm characteristics that are potentially important for the self-selection into the program and/or being selected by the program administrators. In this paper, I approach the problem with non-observable sources of bias by applying three different control groups which all address potential

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https://doi.org/10.1016/j.respol.2018.04.018
Received 22 March 2016; Received in revised form 14 April 2018; Accepted 16 April 2018
Available online 27 April 2018
0048-7333/

problems with this kind of sample selection biases. Takalo (2009) emphasizes that any public innovation policy tool should be judged on whether it yields an expected net increase in social welfare. However, most impact studies aim at measuring the counterfactual outcome — what would have happened to the firms had they not received loan financing from the public program? I also try to measure the counterfactual outcome of not receiving an innovation loan. My main focus, however, is on output measures — such as survival, profitability and growth in sales, value added and employment — applying control groups that serve as benchmarks of the alternative use of resources outside the program.

This paper presents an effect study on the performance of firms with an innovative project receiving funding from the Norwegian publicly financed and administrated direct lending program — "the innovation loan program". The first control group contains firms which applied for innovation loans but were rejected. Program rejects are a popular control group because it indirectly controls for the firms' motivation to apply, cf. Storey's self-selection bias. Moreover, it is a cost effective control group as it constitutes an easily identifiable control group available for most programs. If there is no administration bias, this control group measures the counterfactual outcome had the firms not received an innovation loan. However, this is an imperfect control if the program administrators are able to make good judgements on which projects they choose to finance and which they do not. Hence, this comparison can only be considered as an upper bound of the program's effect, as the projects selected by the program administrators presumably are better than those rejected on average, even after controlling for observable characteristics.

The second control group consists of firms which received loans from a private credit institution. By comparing with a group that is in demand of credit and has been screened by an external loan officer, I implicitly control for several non-observable firm characteristics that otherwise could lead to self-selection and administrative biases. Such non-observable characteristics could be growth ambitions, the entrepreneur's quality, and the quality of the project. Still, the innovation loan program is designed in such a way that it attracts a group of firms which are perceived as too risky to receive credit in the private market. Thus, this source of self-selection is not controlled for by comparing with firms with private bank loans. However, since the average risk of the innovation loan portfolio compared to a regular bank portfolio is known, it is possible to draw expectations with regard to how the innovation loan portfolio firms should perform in order to be successful. In particular, as firms with private bank loans receive the same type of treatment as firms with innovation loans, i.e. credit financing, that creates a natural welfare benchmark for the innovation loan program.

A potential disadvantage of using firms which receive private bank loans as a control group is that these firms do not necessarily take on innovative projects. Thus, if innovative projects take longer time to develop and generate sales, this control group can lead to a false conclusion due to a too short post-treatment period. In order to address this potential measurement problem, I also compare the firms receiving innovation loans with firms with venture capital financing. These make up my third control group. The advantage with this control group is that venture capitalist funds invest in innovative projects which typically do not have debt financing in the private market. This comparison with the performance of venture capital portfolio companies is also a measure of the alternative use of resources for the innovation loan program. Moreover, it gives a benchmark regarding the time it takes before one should expect innovative projects to start generating sales and eventually surpluses. While the control group consisting of venture portfolio companies is not likely to contain a self-selection bias, there is probably an administrative bias due to a tighter selection of companies into the venture fund portfolios compared to the innovation loan program. Again, although this control group is not perfect either, since I know the control group's characteristics it is possible to formulate hypotheses on what observed relative performance of the innovation loan portfolio would suggest that the program is welfare-improving.

The outline of this paper is as follows: In Section 2 I discuss the rationale for public intervention in the capital markets, in particular in funding young innovative companies. Section 3 contains a literature review of former evaluations of policy schemes providing finance to young innovative companies. In Section 4 I present and discuss the mandate of the innovation loan programme, while in Section 5 I describe the data set and the variables included in this study. In Section 6 I first present the control groups' characteristics and what conclusions it is possible to draw based on the comparison with the innovation loan companies. Then I present the empirical strategy and the results from comparing the performance of firms with innovation loans with the firms in each of the different control groups. In Section 7 I discuss the welfare effects of the innovation loan program, and in Section 8 I summarize and conclude on the results.

2. Capital market imperfections for young innovative companies

The starting point for public intervention in capital markets is that there exist projects with a positive net present value that do not receive financing. There are several theoretical models that explain such capital market imperfections. The explanatory factors are typically due to asymmetric information between entrepreneur and investor and/or the presence of positive externalities that neither of them have incentives to account for.

In their seminal paper, Stiglitz and Weiss (1981) assume a situation where debt is the preferred instrument of entrepreneurs, and where there is asymmetric information between entrepreneur and lender. The result is underinvestment in equilibrium, as many projects with a positive net present value are not financed. Besanko and Thakor (1987) and Bester (1985) point to the fact that banks use collateral as a sorting criterion to solve this problem. Entrepreneurs with high quality projects and low risk of default will be willing to provide collateral, while entrepreneurs with low quality projects will not be willing to risk their assets. However, entrepreneurs with high quality projects but no securities available to serve as collateral will still be victims of credit rationing à la Stiglitz and Weiss.

By altering a single assumption of the model, De Meza and Webb (1987) come to the opposite conclusion of Stiglitz and Weiss (1983). De Meza and Webb's theoretical model shows that there should be no credit rationing, and in fact that there is too much investment in entrepreneurship in equilibrium. While in Stiglitz and Weiss the optimal policy would be to subsidize interest rates, in de Meza and Webb's model it is optimal to tax interest rates. De Meza and Webb, however, remove the interesting feature of the Stiglitz and Weiss model that lenders and borrowers have different perspectives on what a good type of project is. While Stiglitz and Weiss assumed that projects have different risk profiles but the same expected value, de Meza and Webb assume that the projects have the same outcome if they succeed, but that they differ in probability of success.

While suitable for some types of projects, the de Meza and Webb assumption does not seem realistic when it comes to comparing innovative projects with businesses applying well known standard technologies. The firms eligible for private bank credit are typically characterized by having a steady cash flow and access to collateral. The firms receiving innovation loans, however, might have completely different risk-return profiles. Interestingly, de Meza and Webb show that in the Stiglitz and Weiss model, equity would have been the preferred instrument as long as the projects' returns are costly to verify ex post and there are no particular transaction costs related to equity contracts.

Myers and Majluf (1984) formalize a pecking order theory, predicting that when financing new projects the firm will exhaust all equity before trying to access external financing. In need of external financing, however, the firm will prefer to issue debt financing, while external equity markets are a last resort when other sources of financing are

exhausted. The result builds on an assumption of asymmetric information between insiders and outsiders, which in turn shows that issuing new equity is a signal of an overvalued firm. Since external investors expect this, a signal that the firm wants to issue equity drives down the price of the firm. Thus, an equity issue will only happen at a discounted value. The pecking order theory might be particularly relevant for SMEs as the cost of equity for them may be higher than for larger firms due to stronger information asymmetries. Moreover, as the owner and manager is the same in most SMEs, it is reasonable to assume that the manager acts in the interest of the incumbent owner.

Berger and Udell (1998) provide a general framework predicting capital structures, tying together different partial theories into a "financial growth cycle". They argue that the financial needs and financing instruments available change as firms grow larger, become more experienced and informationally transparent. Opaque smaller firms in need of external capital require tailored, typically expensive, financial solutions involving screening, contracting and monitoring. This framework is in line with the fact that private equity and debt markets offer highly structured and complex contracts to small businesses that are informationally opaque.

The majority of projects financed with innovation loans can be categorized as young highly innovative companies (YICs). There are three important reasons why policy makers should be concerned about YICs. First of all, evidence points in the direction that YICs achieve higher growth in sales and employment than other firms (Schneider and Veugelers, 2010; Czarnitzki and Delanote, 2012; Vivarelli and Audretsch, 1998). An important explanatory variable for this is that young innovative firms are more committed to radical innovations, in contrast to their established counterparts focusing on incremental innovations.

Secondly, based on the theoretical arguments above, YICs are a subgroup of SMEs that is likely to face particular difficulties in financing their investments. YICs are young, and thus typically do not have any retained earnings (Berger and Udell, 1998). Moreover, credit is often not accessible as YICs have no track record and a risk-return profile that makes the moral hazard problem too large for standard debt contracts. In addition, YICs typically invest in non-tangible assets unsuitable as collateral for bank credit (Hall, 2010). Furthermore, financing from external equity may prove too costly due to high fixed costs related to screening, contracting and monitoring, making the remaining gains for the entrepreneur too small (Hall, 2005). Consequently, if the entrepreneur does not have any personal wealth from inheritance or previous successful ventures, then YICs are likely to face capital constraints. Although Parker (2009) concludes from his literature review that there is not sufficient evidence to claim capital constraints for entrepreneurs, recent well designed empirical studies find a significant relation between the propensity to start a business and personal wealth (Adelino et al., 2015; Fairlie and Krashinsky, 2012). Lee et al. (2015) find that the gap between demand and supply for financing is particularly large for innovative firms, suggesting that there is a structural problem in the market.

Finally, the intangible nature of investments in innovation and R&D activities makes it hard for the firms to appropriate the full benefits of the investment as they create positive knowledge spillover effects to successors, competitors and others (Arrow, 1962; Holbrook et al., 2000; Klepper and Sleeper, 2005; Audretsch and Lehmann, 2005). Thus, the combination of high growth potential, possible financial constraints and potential positive externalities makes YICs a relevant target group for public policies.

3. Literature review on policy schemes promoting entrepreneurship

Acs and Audretsch's (1988) finding that small firms were effective in innovation motivated governments to support innovative new firms through various policy schemes (Autio and Rannikko, 2016). Lerner (2009) and Parker (2009) later recognized the lack of knowledge based practice in the area of promoting entrepreneurship. As pointed out by Lerner (2009), there is evidence that innovation is important for economic growth, and that entrepreneurship is important for innovation; the big question is then whether public intervention can promote entrepreneurship. During the past 5–10 years, spurred by the financial crisis in 2008–09 and the increased popularity of public interventions in capital markets, a number of works have investigated public SME finance in the innovation space.

The financial assistance provided by policy schemes aiming at promoting entrepreneurship is offered in various forms: Credit guarantees, grants, equity investments or tax credits. In the remainder of this chapter I present results from evaluations of different types of policy schemes.

3.1. Credit guarantees

The most common type of financial public policy measure directed toward SMEs is credit guarantees. Credit guarantee programs trigger bank credit by providing insurance to the bank against the risk of firm default. There are a number of studies from various countries such as Canada, UK, France, Italy, Japan and South Korea investigating the performance of national credit guarantee programs in terms of their additionality in the capital market (Riding et al., 2007; Cowling, 2010; Lelarge et al., 2010; Boschi et al., 2014; Uesugi et al., 2010; Ono et al., 2013a). In general, the results tend to be positive with respect to the credit guarantee schemes' effect in improving access to finance for small firms. Boschi et al. (2014), however, find that the additionality is only positive above a certain lower threshold of the guarantees' coverage ratio. Ono et al. (2013a) find that if the loan with the public guarantee is provided by a bank that already has an established relationship with the firm, the loan is offset (partially or completely) by a decrease in other loans from the same bank.

As emphasized by Riding et al. (2007), the schemes tend to differ in motivation and scope, making it hard to compare and interpret the results across countries. Some programs provide an overall increase in the supply of financing to small businesses (e.g. Canada, France, UK), other programs aim only to provide credit to firms that fail to obtain credit from other sources (e.g. US), while again other programs aim at preventing the failure of firms that would otherwise go under (e.g. Japan).

Although credit guarantee schemes often benefit innovative firms with limited collateral, most credit guarantee schemes are not directly targeted toward innovative firms. An exemption is the Korea Technology Credit Guarantee Fund (KOTEC), targeted at technology-based newly founded firms promoting the growth of technologically advanced SMEs and venture businesses. Oh et al. (2009) found that the credit guarantee helped firms maintain their size and improved survival during the financial crisis. However, the credit guarantee program did not increase R&D activities or investments, and they found no growth in productivity.

3.2. Grants

Direct subsidies, in the form of grants, are a commonly applied capital instrument to promote young and innovative firms. Quite a few recent studies analyze the effectiveness of such policy schemes, most of them applying matching techniques to identify the control group. Autio and Rannikko (2016) find that a Finnish policy scheme targeted at high-growth entrepreneurship more than doubled the growth rates of treated. Investigating a program providing R&D subsidies to Korean SMEs, Cin et al. (2016) conclude that the subsidies increase the expenditure on R&D as well as firm productivity. In the UK, Foreman-Peck (2013) finds that firms which received support from the UK state support programmes for innovation grew significantly faster than other innovative firms. The state-supported innovation programmes typically

involved grants from the state, but in addition, a system of tax credits for innovative activities was also implemented. Lerner (2000) found that the Small Business Innovation Research Program in the US was capital additional, and moreover that the program increased the sales and employment growth for high-technology firms. In contrast, based on an effect study of a programme providing subsidies to early stage innovative ventures in Sweden, Norrman and Bager-Sjögren (2010) conclude that the program has not been additional, and moreover that the scheme has not been successful in identifying potentially successful firms.

3.3. Venture capital

In the private market, venture capital funds are the most professionalized capital instrument targeted at financing young innovative companies with high growth potential. In return for equity stakes in companies, the venture funds offer development investment capital, professional business advice as well as access to business networks. The business model is that the fund should exit the company at the point where the largest growth potential is revealed to the market, hopefully with a high return on its investment.

Kortum and Lerner (2000) found that venture capital accounted for a large share of industrial innovations in the United States. The venture capital market in Europe was, however, far less developed (Bottazzi and Da Rin, 2002). Thus, public policy schemes creating hybrid venture funds or co-investment venture funds emerged as a popular financial instruments to promote high growth entrepreneurship.[1] Hybrid refers to funds backed by both private and public funding, while co-investment funds are publicly backed funds whose mandate is to "match" private venture capital investments.

The effects of such public policy initiatives are, however, ambiguous. Wonglimpiyarat (2016) is one of several papers that emphasize the establishment of the Yozma venture funds, a public backed fund co-investing in private venture funds, as a key factor in triggering the currently thriving venture industry in Israel. Investigating the sales growth of close to 800 European venture portfolio companies, Grilli and Murtinu (2015) find that government administrated venture capital funds underperform compared to private funds. However, when investigating the performance of young venture portfolio companies they find no statistical significant differences between government- and privately administrated funds. Based on the same data set, Cumming et al. (2017) find results indicating that private funds are more likely to have positive exits than government-backed funds. The authors do not find any statistically significant differences in the likeliness of a positive exit between private funds and that of mixed syndicates of private independent and governmental venture capital.

Studying six UK hybrid venture funds, Nightingale et al. (2009) identify a modest positive effect on portfolio company performance measured in terms of sales, employees and fixed assets relative to a matched control group. While they identify an interesting *j*-curve pattern over time with respect to the development in profit margins and labor productivity, the effects on these variables are however not statistically significant. In a comparative analysis of public and private venture capital funds in the UK, Munari and Toschi (2015) find that a significant reduction in private venture capital has been offset by increased access to public venture capital. However, their results indicate that the public venture funds are less likely to lead to a positive exit of the portfolio company, as well as being less effective in attracting additional venture capital investments to the company. This goes in particular for public venture funds that have constraints with respect to which geographical regions they may invest in. In a study of the Australian public Innovation Investment fund, Cumming (2007) finds that the program has improved the access to capital for innovative firms. Moreover, he does not find any statistically significant differences between the public venture capital and private venture capital in the likelihood of positive exits. Still, the conclusion with regard to exits is premature as the majority of investments had not been exited at the time of the study.

In Norway, the State Audit Institution finds that none of the 15 seed and venture funds, part funded with subsidized governmental loans, have succeeded in bringing forward high growth companies with more than 50 employees over the 16 year period 1998–2014 (Riksrevisjonen, 2016). In an effect study of eight Swedish co-investment venture funds backed by public funding, Damvad (2016) finds that the portfolio companies have weakly significant higher growth in sales, productivity and employees than the matched control group four years after the initial investment. The funds started investing as late as 2009, so the evaluation emphasizes that it is too early to conclude on performance.

3.4. Tax credits

Some countries also use the tax system to give incentives to R&D projects in the commercial sector. Based on a country level panel data study of nine OECD countries over a period of 19 years, Bloom et al. (2002) find evidence that tax incentives, reducing the cost of capital of R&D, are effective in increasing R&D intensity.

Recent firm level effect studies of country specific R&D tax credit schemes provide more ambiguous results. Cowling (2016) investigates the effect of the UK tax credit scheme to promote and support R&D. He finds that the program seems to have a positive effect on radical process innovations, while it does not seem to have any effect on the level of product and service innovations. In a similar study based on the Norwegian tax credit scheme for R&D activities, Cappelen et al. (2012) find the opposite, that the tax credits have a positive effect on process and (to some extent) incremental product-service innovation, but no effect on radical innovation or patenting. Evidence from Canada, however, points to that R&D tax credits have a positive impact on innovation outputs including product innovations and sales growth, as well as radical innovations (Czarnitzki et al., 2011). Lokshin and Mohnen (2012) find that the Dutch tax credit scheme leads to an increase in R&D investments, but that the crowding-out effect can only be rejected for small firms.

Tax credit policy schemes are typically allocated automatically, meaning that every firm that fulfills the requirements indicated by law is eligible for the tax credit. In comparison, credit guarantees, grants and venture capital investments are typically selective schemes, in the sense that they provide R&D subsidies directly based on careful assessment of an application for financial support. In a study of Italian young innovative firms, Colombo et al. (2013) compare employment growth in firms that have received support from selective schemes with firms receiving support from automatic schemes. They find a statistically significant higher employment growth for firms with selective support given that the finance is provided in the very early period of the firms' lives. The authors point out that one explanation for the differences could be that selective schemes have an additional "certification" effect for young firms, improving their access to finance. A challenge with this type of study is that most firms have received support via the automatic scheme, making it hard to find a proper control group.

4. The Norwegian innovation loan program

The empirical context of this paper is the Norwegian innovation loan program. The innovation loan program is established based on the assumption that the level and number of innovative projects is below the socially optimal in the sense that there are imperfections in the financial market, or that there are positive externalities from innovative projects which the private capital market does not take into account when considering whether a firm is eligible for credit. By providing

[1] According to Nightingale et al. (2009), Murray and Liu in their 2009 unpublished review of 16 developed economies found 31 hybrid schemes in operation.

credit to innovative projects, the program aims to solve this problem.

The innovation loan program is administrated by Innovation Norway. Its task as administrator is to provide loans to projects that are expected to be socially profitable.[2] The innovation loan is offered at an interest rate 1–2 percentage points above the average rate of regular fully secured market loans. Thus, the firms applying for innovation loans are a self-select pool of applicants that have innovative projects they need financing for, and have limited tangible assets available to serve as collateral. According to Innovation Norway' internal guidelines, one criterion to qualify for an innovation loan is whether the firm can be expected to be able to cover interest and capital payments out of its own cash flow at the latest six months after the loan has been paid out. Alternatively, if the cash flow is not likely to be sufficient, that the loan can be serviced by other means, e.g. that the owners pay interest from their own pockets.

As opposed to policy schemes providing grants, equity and tax credits, public authorities can in theory operate a direct lending program or a credit guarantee scheme without appropriating funds from public budgets. Fees and interest margins can cover running administration costs and losses. Both loans and credit guarantees, however, involve credit risk, and the government may be required to allocate funds at some point in time if the program's income is not sufficient to cover the actual losses.

In the case of the innovation loan program, approximately one third of the total credit portfolio is backed by equity placed in a loss fund to cover expected losses on the portfolio. Innovative projects are typically associated with risk capital in equity form, and normally you do not find these projects funded with debt (Audretsch and Lehmann, 2004). However, from an economic efficiency point of view, it is not a given whether debt or equity are the better option. The high probability of default related to innovative projects makes these projects unattractive to creditors as the creditor only receives a fixed income in the case of success, but takes a lot of risk related to a default. This negative distributional effect associated with debt in favor of the successful entrepreneur at the expense of the creditor, can be compensated for by the government's taxation of firm profits. However, the efficiency loss related to the transaction costs associated with equity financing of small unlisted firms, such as valuation of the company, are most likely considerably higher than those associated with debt financing. Thus, this can explain why a public agent would choose to provide debt to a high risk innovative firm, rather than equity. Moreover, from the perspective of the firm owners, an innovation loan can be a very attractive instrument. Although the project may not be generating revenue yet, paying interest out of their own pockets to handle the debt is a cheap insurance premium against an unsuccessful result of the project.

There are few differences in the economic realities of a public direct loan program, such as the innovation loan program, and a public credit guarantee program. Both types of schemes aim to increase lending to the private business sector by reducing the requirement for collateral compared to regular bank loans. While credit guarantee programs trigger bank credit by providing insurance to the bank against the risk of firm default, direct lending programs provide these loans directly. The unsecured part of the loan is analogous to a credit guarantee.

A distinct difference between a credit guarantee scheme and direct lending program is that the credit guarantee also involves a private lending institution (usually a bank). The advantage of a credit guarantee compared to a direct lending program is that it allows for the private bank to develop know-how and technologies so that it can reduce risk and transaction costs and increase profitability on SME lending (Valentin and Wolf, 2013). This is also why, according to internal guidelines, the innovation loans should preferably be granted in co-finance with other sources of credit, either provided by private banks or by other Innovation Norway programs. Analogous to a first-loss partial credit guarantee, the innovation loan will typically have lower priority than other loans. However, in a situation where the firm has limited tangible assets available for collateral, innovation loans can be provided without co-financing with other sources of credit. In practice, the majority of firms that are granted an innovation loan falls within the latter group.

5. Data and variables

5.1. The data

I construct a data set combining administrative records of the program with firm level accounting information from the Norwegian Register of Company Accounts. I focus on firms which received innovation loans during the time period 2004 to 2009. The database includes yearly accounting and employment figures covering the period 2002 to 2012, balance sheet figures as well as firm specific information such as industry affiliation, date of establishment and geographical location. The Register accounts for all firms that have been granted an innovation loan for a specific project. Moreover, a large database based on the same reporting standards is an advantage when searching for firm control groups. In the panel data analysis all figures are deflated with the Norwegian consumer price index.

5.2. Measures of firm performance

I measure firm performance by survival, growth and profitability.

Survival is measured by whether a firm is active in a given year. The firm is considered to be inactive if it does not have turnover or labor costs in consecutive periods. As firms that become inactive are likely to default on their debts it is highly relevant to see whether there are differences in survival rates between the firms with loans from Innovation Norway and firms with private bank financing. Moreover, survival is also an important measure inasmuch as it tells us whether the remaining results are likely to be plagued by survival bias.

To assess growth I measure the firm's development in sales, value added, number of employees and the (book) value of firm assets. Foreman-Peck (2013) argues that sales is a particularly relevant outcome variable as it is closely related to the surplus measures of well-being from welfare economics: consumers' surplus and profits. Moreover, Norrman and Bager-Sjögren (2010) argue that sales is a proxy for customer satisfaction with the project and the management's ability to commercialize the product. I also study the number of employees over time. If firms are able to employ more people, they contribute to the overall activity in the economy. However, if the project fails to commercialize, the employment is only a redistribution of resources. I interpret an increase in employment as a measure of the firm's ability to attract resources, which in turn is a signal of the quality of the project. I also study growth in value added. Similar to Norrman and Bager-Sjögren (2010) I also include asset growth. Assets is the sum of equity and debt and is a measure of the firm's ability to gather resources.

Firm profitability is measured by operating return on assets (OROA). This measure is used by Bennedsen et al. (2007) and Becker and Hvide (2013). A potential problem with OROA as a measure of profitability is that it may be biased over time in favor of knowledge intensive firms. As the firms take on investment projects, I expect their capital intensity to change over time. If it is the case that investments in knowledge is not included in "total assets", then the OROA estimates may be biased in over time in favor of knowledge intensive firms. In the firm accounts there is an accounting record for "intangible assets",

[2] Previous evaluations of Innovation Norway and its programs point out that there is a deficiency of explicit measurable objectives related to the individual programs (Pöyry and Kaupang, 2010; Grünfeld et al., 2013). In its internal guidelines, Innovation Norway has operationalized the definition of 'socially profitable' as projects with an annual expected nominal return on assets of 6% or more. For the purpose of this study it is less relevant how Innovation Norway has defined socially optimal projects. Rather, I argue that a more relevant benchmark is to compare the performance of the firms with alternative uses of credit.

which includes investments in R&D, patents and goodwill. Investments in human capital should therefore be included in the denominator of OROA, and thus there should there be no bias in favor of knowledge intensive firms. We do, however, know that the book value of unlisted firms deviates from their true value. This problem is likely to be larger for knowledge intensive firms.

An alternative measure of profitability could be operating margins. This measure of profitability is independent of the firm's book value. The problem with this measure, however, is that it is extremely volatile as the firms we are looking at are in the early stage of commercialization with very low and variable sales. Thus, small absolute changes in sales may have an extreme impact on changes in operating margins. Thus, I do not use operating margins as a performance measure. However, I do include a dummy for whether the firm is running with operational deficits. This variable allows me to study the probability of running operational deficits which is interesting as an indicator of the firm's ability to handle its debt obligations.

The performance variables are interesting to study in context of each other as they are likely to develop differently over time. For investing firms we are likely to see asset and employment growth preceding sales growth which again precedes growth in profits. Survival is also an important performance measure since if the project does not succeed commercially it will eventually drop out of the sample affecting the estimated average development of the performance other variables. In the short term, profit growth is generally not a suitable variable to measure the success of young firms that invest heavily in innovative projects. Rather, as observed by Nightingale et al. (2009), firms that make investments are likely to have increasing operational deficits for some time in return for expectations of strong growth and success in the future. However, if a firm is going to be a success, at some point in time it must come out of the *j*-curve, and one should expect to see the profitability to improve over time.

6. Empirical approach and estimation results

This study applies three types of control groups trying to deal with different sources of bias: First I compare the innovation loan program participants with firms which applied for the program but which were rejected, then I compare them with firms with private bank financing and finally I compare them with venture fund portfolio companies.

Each of the control groups has its advantages and disadvantages, and it is not possible ex-ante to state which of the control groups is the better. Rather, the control groups complement each other and add up to a more nuanced impression on the performance of the innovation loan program. The possible biases of the different control groups provides directions regarding which implications can be drawn from the relative performance of the firms with innovation loans. Since each of the control groups has distinct characteristics, the interpretation of the comparison with the firms with innovation loans will be different. Still, as we know the direction of the selection bias, I can develop hypotheses on which results I would expect in order for the program to be successful, unsuccessful or when further tests are needed. Thus, before I present the empirical analyses for each of the control groups, I first describe the characteristics of each group and how this impacts the interpretation of results from the empirical analyses.

6.1. Control group characteristics

Table 1 summarizes characteristics of the control groups applied in this study: Program rejects, companies with private credit financing and venture portfolio companies. The most obvious difference between the control groups is that while the rejected applicants do not get financing, the other two control groups get an alternative treatment. This difference has important implications for the interpretation of the results. While the rejected applicants provide a benchmark of what would have happened if the firms had not received an innovation loan, the comparison with the firms with private loan financing and venture capital financing serves as benchmark of the alternative use of capital. Hence, the latter two control groups are benchmarks of the *welfare effects* of the program, while the first is a benchmark for the *additionality* of the program.

Table 1
Summary of control groups' characteristics.

	Rejected applicants	Companies with private credit financing	Venture portfolio companies
Benchmark additionality	Yes	No	No
Benchmark welfare effects	No	Yes	Yes
Captures elements of motivation	Yes	Yes	Yes
Innovative investment projects	Yes	n/a	Yes
Self-selection bias	No	Yes	No
Administrative bias	Yes	No	Yes
Easily identifiable	Yes	No	No
Sample size	Small	Large	Small

The three control groups have in common that they consist of firms that have projects they need to finance. Thus, all control groups capture important unobservable elements of *motivation* to undertake an investment project. Similar to the firms that received innovation loans, both the rejected applicants and the companies with venture capital financing are likely to have *innovative* investment projects, while we do not know whether the firms with private loan financing have an innovative investment project. If innovative projects take longer time to develop and generate sales and have a more volatile performance distribution, comparing with the control group consisting of firms with private credit financing can result in a false conclusion if the post-treatment period is too short or the methodological design does not investigate the tail of the performance distribution.

In order to measure the counterfactual outcome it is important to control for *self-selection* into the innovation loan program. The control group consisting of program rejects by definition controls for this bias as these firms also apply for financing through the program. Similar to Cowling and Siepel (2013), in their evaluation of the UK Small Firms Loan Guarantee Scheme, I also use firms with private credit financing as a control group. This control group does, however, not control for the self-selection bias. As explained in Section 4, innovation loans are offered at an interest rate which is higher than the average rate offered by private banks. Thus, in contrast to the program rejects, the sample of firms with innovation loans is by design a self-selected group of firms which otherwise would not have received private bank financing.[3] Venture capitalist funds invest in innovative projects which typically do not have debt financing in the private market. Thus, firms that apply for innovation loans are also relevant for venture capital financing. In fact, 12% of the firms with innovation loans already had private equity fund investors at the time the loan was granted by Innovation Norway. This implies that there is no self-selection bias between the two control groups.

The innovation loan program participants are not randomly selected among the pool of applicants. Rather, the program administrators have a mandate to identify and finance the projects with the most potential. This suggests that compared to the program rejects there is an *administrative bias* which will lead to an overestimation of the treatment

[3] An alternative control group could be firms that applied for private credit, but were rejected. These firms are likely to be more similar to the firms receiving innovation loans than the firms that actually received credit financing. Still, I would not know if the rejected firms would actually qualify for an innovation loan. Moreover, the rejected firms are not necessarily representative as many firms are discouraged from applying for private loans.

effect. Wallsten (2000), however, argues that program administrators have incentives to select projects with moderate risk in order to avoid negative publicity related to failures. This line of reasoning suggests an administrative bias in the opposite direction. Still, keeping in mind that the innovation loan program is designed in such a way that it does not attract moderate risk firms that otherwise could be financed in the private credit market, the administrative bias is consequently most likely to inflate the estimated effect compared to the program rejects.

Firms with private bank loans and firms with innovation loans have in common that they have been assessed by external creditors with regard to their eligibility for loan financing. Examples of characteristics available to creditors but which are not directly observable from the data I have at hand, could be qualitative information on the entrepreneur's quality and growth ambitions, or the size of contracts on future sales. If the program administrators are good judges of the quality of the investment project and firms' ability to handle future debt payments, there will be no administrative bias between the companies with innovation loans and the companies with private loan financing. In fact, if the administrators are good judges of project quality and credit eligibility their screening will compensate for the self-selection bias arising from the fact that the sample of firms applying for the program are firms considered ineligible for credit financing by private banks.

I also expect there to be an administrative bias in the comparison with venture capital portfolio companies. Firms with venture capital financing go through a very tight screening process with respect to growth potential, and thus I expect that the venture portfolio companies on average have a higher expected growth potential compared to the firms with innovation loans. Another source of administrative bias arises from the fact that firms which receive innovation loans are expected to be able to handle their debt obligations at the latest six months after the loan is paid out. Venture portfolio companies are rarely financed by debt financing and do not face the same obligations.

In addition to the control groups' conceptual characteristics there are also some practical aspects that are relevant in assessing and comparing the quality of the control groups. The program rejects is an *easily identifiable* control group available for most programs. Thus, this control group is a natural place to start for most evaluations as it is cost effective and gives valuable information about the performance of the program. Comparisons with firms with private debt financing as well as venture capital portfolio companies, however, require data from external sources and are usually more resource demanding to get hold of. In addition to identifying companies with private debt financing or venture capital financing, one also needs information about the timing and amount of financing, as well as other firm characteristics such as industry affiliation, pre-treatment size and age. The fact that the data are normally gathered from different sources may also pose a challenge with respect to measurement error biases.

The control groups may also vary with respect to *sample size*. For example, there are fewer program rejects than there are firms receiving innovation loans. The same goes for the number of venture portfolio companies. This poses a challenge with respect to the generalizability of the results since it is not possible to control for all observable differences between the samples, such as industry affiliation.

6.2. How to interpret results

The comparison with program rejects is to be seen as a first test with respect to whether the program is successful in improving welfare. If the innovation loan program is to be considered welfare-improving, it is a necessary but not sufficient condition that it performs better than the rejects. This means that if the program participants perform on the same level or more poorly than the rejected firms, this is a strong indication that the program is not successful in promoting firm and socially profitable innovative projects. However, if I find that the program participants perform better than the control group consisting of rejects, then further tests are required to be able to conclude on whether the program has been successful.

I expect that there is more volatility in the group of firms with innovation loans compared to firms with private bank financing. The reason for this is that the innovation loan firms are self-selected based on having higher risk. Thus, I expect firms with innovation loans to have higher default rates but also more growth successes. If I do not find that the non-defaulting firms with innovation loans have more and larger growth successes than the firms with private loan financing, then this is an indication that the program is not successful in promoting firm and socially profitable innovative projects. If I find that the program participants have the same level of survival, but that they perform on the same level or better than the firms with private credit, then this would be an indication that the program is socially beneficial. This result is, however, unlikely as the firms with innovation loans are a self-selected group of firms with higher default probability. Finally, if I find that the program participants have a higher default rate but also higher growth than the control group consisting of firms with private credit, then further tests are required to conclude on whether the growth among the survival firms is sufficiently large to compensate for the higher default rate.

In contrast to the comparison of firms with innovation loans with firms with private bank loans, I expect the venture-backed firms to have a more volatile growth but a higher expected value than the firms with innovation loans. This hypothesis follows from the fact that firms which receive innovation loans are expected to be able to handle their debt obligations at the latest six months after the loan is paid out. Venture portfolio companies are rarely financed by debt financing and do not face the same obligations. The firms with venture capital financing do, however, go through a tighter screening process with respect to growth potential compared to the firms with innovation loans. If, however, there are no differences, then this suggests that the innovation loan program gives the same return on resources as the venture funds. In a well functioning capital market one would on average expect that venture capital investments are socially profitable (defined as returns above risk adjusted capital costs). Thus, given that the capital markets are well functioning, such a finding would suggest that the innovation loan program is socially beneficial. If I find that the venture portfolio companies experience higher growth at an earlier stage than the firms with innovation loans, then this would indicate that the time span I look at is sufficient to capture growth from innovative projects. Moreover, this result would indicate that the program is not successful in promoting firm and socially profitable innovative projects at the level of Norwegian venture funds. That the program participants have weaker performance than the venture capital portfolio firms is, however, not sufficient to conclude that the program is not welfare-improving. Thus, this control group is not suitable for rejecting the alternative hypothesis that the program is welfare improving.

6.3. Comparison with program rejects

In this sub chapter I compare the performance of firms with innovation loans with those firms rejected by the program. Some firms receive innovation loans more than one time related to different projects. For these I use the first innovation loan as the treatment year. Other firms apply for innovation loans more than once and are rejected every time. For these firms I consider the first rejection as the year of rejection. Firms that experienced both successful and unsuccessful applications are excluded from the sample.

Innovation loans are project specific financing, while the firm is the unit of analysis. Thus, if the project is relatively small related to the firm's total activity it is hard to identify whether the performance of the firm is due to the innovation loan project or some other project within the firm. In order to handle this measurement problem I exclude firms for which the loan amounts to less than 20% of the total assets the year before the loan was paid out from the analysis. In the analysis where I compare firms with innovation loans with firms with private bank

Table 2
Summary statistics: firms with innovation loans compared to firms rejected by the program.

	Treated (119 obs.)					Control (21 obs.)				
	Mean	sd	p25	p50	p75	Mean	sd	p25	p50	p75
Sales	14,799	30,257	734	5284	13,318	19,789	54,868	436	1824	8781
Employees	12	19	2	5	12	13	32	1	2	8
ValueAdded	4344	10,811	−110	1123	5593	7168	21,370	−508	851	5324
TotalAssets	21,658	64,209	2154	6430	15,848	32,692	109,095	1777	4076	14,941
YearTreatment	2007.7	1.6	2007	2008	2009	2007.8	1.7	2007	2009	2009
FirmAge	9.0	6.9	4.0	7.0	12.0	8.0	5.6	4.0	6.0	10.0
InnovationLoan	2925	3808	1000	2000	3000	0	0	0	0	0

Note: This table displays summary statistics the year before the firms had their innovation loan application accepted or rejected during the period 2004–2009. Figures are in 1000 2004-NOK.

loans, the same criterion applies for the control group.

Moreover, I exclude firms that were not alive two years before receiving treatment. I do this in order to be able to control for pre-treatment differences, and in particular whether the treated and controls are likely to follow the same trend growth.

Table 2 presents summary statistics on the firms which were granted innovation loans (the treated) and the firms which had their application rejected (controls). The summary statistics are based on the year before treatment. The treatment year is the same as the year the loan was granted or rejected. The sample contains 119 firms which received innovation loans and 21 rejects in the period 2004 to 2009.[4] From the table we see that the average level of sales, total assets, labor costs and employees is quite similar for those firms which receive an innovation loan and for those that had their application rejected. However, the median firm among the firms that were granted an innovation loan is larger than the median rejected firm. The median firm among the firms which were granted an innovation loan had five employees and sales of 5.2 million NOK (EUR 0.7 million) the year before receiving an innovation loan, while the median rejected firm had two employees and sales of 1.8 million NOK (EUR 0.2 million).

I perform a differences-in-differences panel regression comparing the firms with innovation loans with the control group of firms that had their project applications rejected by the program. The equation estimated is the following:

$$\text{Performance}_{i,t} = \alpha + \beta_1\text{*treated}_i + \beta_2\text{*after}_{i,t} + \beta_3\text{*treated}_i\text{*after}_{i,t} + \gamma X_i + \epsilon_{t,i}, \quad (1)$$

where $Performance_{i,t}$ varies depending on the application for firm i at time t, β_1 measures the pre-treatment difference between treated and controls, β_2 measures the common growth for treated and controls, β_3 is the treatment effect on the treated (double difference), X is a vector of estimated coefficients for the control variables and ϵ is the error term. β_3 is our main coefficient of interest. The control variables include dummy variables for the year the loan application was accepted or rejected as well as the pre-treatment values at t-1 of log-sales, log-total assets, and log-employees. The latter variables are included to control for pre-treatment size differences. In addition I control for the effect of the Global Financial Crisis 2008-09 by including a dummy variable for the year 2009.[5]

By using a differences-in-differences model I allow for unobserved heterogeneity between treated and controls as long as this heterogeneity is time invariant.[6] For this assumption to be fulfilled the treated and controls must be on the same trend (parallel-trend assumption). This means that there must be reason to believe that the treated and the control group would be likely to follow the same time trend without treatment. Although this assumption is hard to test explicitly, I investigate pre-treatment behavior to substantiate that the trends are the same. Running a regression comparing pre-treatment growth from $t-2$ to $t-1$, measured in log differences, I do not find statistically significant differences in pre-treatment growth patterns for treated and controls.

Table 3 displays the results from the differences-in-differences regression comparing firms with innovation loans and firms that were rejected by the program. I consider a window from two years before to as much as eight years after being granted or rejected an innovation loan. The only exemption is the regression with *alive* as the dependent variable. Here I only estimate post-treatment differences as the firms are all alive before receiving treatment. The estimates measure the average effects before and after treatment. The *Treated* estimates tell us that there are no statistically significant pre-treatment differences in levels between the groups with respect to any of the dependent variables.

Starting with Column 1, the *Treated*After* estimate is not significantly different from zero. This indicates that there are no differences in the probability of survival between treated and controls, and thus that there is no survival bias in the sample. From Column 2 we see that there are no statistically significant differences between the groups with respect to running with operational deficits. The point estimates displayed in Column 1 and Column 2 are estimated based on a probit model, and should be interpreted as marginal probabilities conditional on the mean value of the independent variables. The remaining regressions are estimated with OLS.

The estimated coefficients for *Treated*After* tell us that the firms receiving innovation loans have significantly higher growth in sales, value added, employees, and total assets compared to the rejects. We see for example that the estimated average post-treatment development in sales is 29 log-points. For relatively small changes, log-points is a good approximation for percentage points. In this case one must be careful with the interpretation since I have added a constant of two million NOK before taking logs. Thus, the percentage growth will be somewhat underestimated, in particular for the smallest firms.

The table shows that there is no statistically significant common growth for treated and controls. The only exception is for employees. Here we see that there is a (weakly) statistically significant negative development. Taking into account that we do not see any positive

[4] Innovation Norway operates with three ways of rejecting a loan application: return the application, request for withdrawal, or decline. The control group only includes firms for which the application was declined. This way we are assured that the control group only contains firms that have gone through a similarly thorough screening by Innovation Norway as those that had their application accepted.

[5] Although Norwegian firms were also negatively affected in the later half of 2008, this is not possible to detect in the data since the first half of 2008 was booming, which averages out the effect for that year.

[6] A Sargan–Hansen test was used to test whether a random effects model would be more efficient than a fixed effects model. The consistency of the random effects model was rejected at the 10% level when estimating the effect on growth in employees. The estimated treatment effects of the fixed effect and the random effects model are very similar, and qualitatively the same. Thus, I chose only to report fixed effects model estimates.

Table 3
Survival, growth and profitability of firms with innovation loans compared to firms that were rejected by the program.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) ln(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
Treated		.042	−.044	−.222	−.001	−.077	.052
		(.11)	(.08)	(.13)	(.08)	(.06)	(.07)
After		.010	−.084	−.072	−.233*	−.087	.076
		(.10)	(.11)	(.13)	(.14)	(.11)	(.08)
Treated*After	−.040	−.037	.291**	.377**	.414***	.469***	−.029
	(.04)	(.11)	(.13)	(.15)	(.15)	(.13)	(.09)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			95.23	33.1	99.16	76.32	3.925
Adjusted *R*-squared			.7777	.5495	.7418	.7086	.0977
Log-likelihood	−206	−489	−665	−804	−736	−766	−217
Chi-Square	16	52					
No. of obs.	632	807	807	737	808	807	789

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: This table displays differences between firms that had their innovation loan application accepted and rejected during the period 2004–2009. The regressions have a differences-in-differences setup. The data is panel for the period 2004–2012 covering a window of two years before and until eight years after the application was accepted or rejected. The exemption is the regression in Column 1, *Survival*, which only estimates post-treatment differences. The year of treatment is excluded from the sample because it cannot be clearly assigned to either before or after treatment. The coefficients displayed in Column 1 and Column 2 should be interpreted as marginal probabilities at mean values, the estimates in Columns 3–6 are log-points, while OROA in Column 7 estimates growth. See Table B.3 for a definition of the dependent variables in the regressions. For numerical values I have added two million NOK before taking the natural logarithm, while for employees I added the number 1. In the regression I control for business cycle, log-sales at $t-1$, the squared of log-sales at $t-1$, log-employees at $t-1$, the squared of log-employees at $t-1$, log-total assets at $t-1$, the squared valued of log-total assets at $t-1$, log-loan size, pre-treatment growth in sales and employees from $t-2$ to $t-1$ and the Global Financial Crisis (dummy for the year 2009).
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

development in assets, in addition to a negative development in employees, this suggests that the firms that do not receive innovation loan financing do not succeed in finding alternative financing at a later point in time.

This analysis shows that the firms that are granted innovation loans experience higher growth than those rejected by the program. By comparing with rejected applicants self-selection into the program is controlled for. Unfortunately, I cannot separate the effect from receiving a innovation loan from the possible administrative bias stemming from the screening process by Innovation Norway's case workers selecting the best projects. Although I perform a differences-in-differences regression controlling for certain pre-treatment characteristics, some administrative bias is likely to remain in the sample. This implies that the firms which received innovation loans would have had a better development than the firms which did not receive an innovation loan also in case none of them had received a loan. In fact, in a separate analysis attached in Appendix A, I show that the administration at Innovation Norway is able to operate regular lending activity on the same level as private banks. Although regular bank screening and innovation project screening are not equivalent, this supports an assumption that Innovation Norway has a screening competency which enables them on average to select the better among projects.

However, if the firms with innovation loans had not received loan financing the growth would most likely also have been lower, and at least delayed. This is also supported by a survey among participants for which a vast majority report that the innovation loan program was important for the realization of their project (Grünfeld et al., 2013). The fact that the asset growth is at level with growth in sales and value added, suggests that financing is an important part of the firms' growth. Finally, I find no statistically significant differences in profitability between the groups. The latter is interesting because one could expect that the average profitability would go down when the asset volume increases. One explanation could be that the firms with innovation loans are more likely to receive other types of support such as grants, which would improve profitability.

Table B.4 in Appendix B displays a regression on the same sample but replacing the aggregate before and after treatment dummies with period specific time dummies as well as the interaction of the *Treated* variable with these time dummies. The detailed time estimates are interesting because they allow for non-linearities in the development both before and after treatment, possibly revealing sub-trends not captured by the more general pre- and post-treatment variables.

The results in Table B.4 are generally very similar as displayed in Table 3. The estimates suggest that there is a tendency that firms with innovation loans have a lower probability of surviving with time. Particularly, one should be careful about the interpretation of the estimates five to eight years after treatment as there is likely some survival bias in these estimates. Although the differences-in-differences estimates for value added post-treatment are positive, they are not statistically significant at the 10% level. There is a tendency of firms with innovation loans having weaker results than firms which do not receive innovation loans five to eight years after treatment. This is likely because many of the firms that do not receive innovation loans never really get started with their project, and that they consequently are less likely to run with operational deficits. Table B.4 can also give us information about the parallel trend assumption. The fact that the coefficients *Treated*2 years before treatment* and *Treated*1 years before treatment* are not statistically different from zero, indicates that the pre-treatment growth is about the same for firms with innovation loans and firms which had their project rejected.

6.4. Comparison with firms with private bank loans

In this analysis I compare the performance of innovation loan program participants with firms which applied and received private long term credit the same year as the treatment group. Similar to many other recent effect studies of policies for private sector development, I apply the method of propensity score matching (see e.g. Oh et al., 2009; Norrman and Bager-Sjögren, 2010; Uesugi et al., 2010; Foreman-Peck, 2013; Ono et al., 2013b). In propensity score matching (PSM), each of the treated firms is matched with an unsupported firm selected contingently on having the same observable pre-treatment characteristics

as the participating firm (see Caliendo and Kopeinig, 2008 for a survey on PSM in an entrepreneurship context). Based on the matched sample it is then possible to measure the average treatment effect among the treated (ATT) by comparing with the non-treated firms. For the ATT to be observable, the propensity score matching must, however, satisfy two crucial assumptions: the conditional independence assumption (CIA) and common support (CS). For the CIA to hold we must believe that we are able to identify a twin for each of the treated firms by matching the firms based on observable characteristics. That is, had the supported firm not received finance from Innovation Norway, then the matched firms would on average have had the same development.

The assumption of common support requires that there exists a good match for the program participant within the total population of unsupported firms. In practice this is assured by matching each of the firms from the group of supported firms with one more unsupported firms with similar propensity scores. If such a firm exists in the group of unsupported firms, then the treated firm's counterfactual outcome can be estimated.

Firms tend to differ in more ways than what is measurable. If the unmeasurable differences are not randomly distributed between treated and controls, and these differences have an effect on outcome, then the estimates will remain biased. In fact, in this matching I know that the CIA is violated as the firms with innovation loans are a group of firms which is perceived as too risky to be granted private bank financing, while the control group is a group with private bank financing. The advantage, however, is that I have clear expectations on what the bias between the groups is. This enables me to make clear predictions on what type of results I would expect for the innovation loan program to be successful. The latter separates this study from most other studies applying propensity score matching exclusively on observable variables.

When searching for matches among the population of firms with private bank financing I match with respect to a variety of standard quantitatively measurable control variables. Some of the variables are matched exactly, such as industry (NACE A-V), geography (centrality 1–4) and loan vintage. Pairing with respect to firms receiving long term loan financing the same year controls for business cycle effects. Exact matching, also called stratification, means that I only search for matches within the same industry-region-vintage as the firm which received an innovation loan. The propensity scores are estimated based on a probit model including the following pre-treatment characteristics: log-sales, log-total assets, number of employees, firm age and log-loan size. These are potentially important characteristics when comparing firm performance of credit finance. The square of the log transformed variables and the square of the number of employees are also included in the propensity score matching. The latter is to control for possible second order effects. Moreover, in order to improve the likelihood of a common trend assumption, I also match the firms' pre-treatment growth in sales and employees in the period $t-2$ to $t-1$ before receiving loan financing.

I have a sample of 132 firms with innovation loans during the period 2004–2009 for which I try to find a match. Some firms received more than one innovation loan related to different projects during that period. I use the year of the first loan in that period as the treatment year. I apply a one-on-one nearest neighbor matching with replacement. Replacement means that the same firm may be used as a match more than one time. From the propensity score matching, 99 of the firms found common support within a probability radius of 0.05.

Table 4 displays the pre-treatment statistics on the matched sample of firms with innovation loans and firms with private bank loans. The table shows that the control group is a good match with respect to quantifiable pre-treatment firm characteristics. From Column 1 and Column 2 we see that the pre-treatment mean values of sales, total assets, and number of employees are similar for the firms that received innovation loans compared with the group which received private bank loans. This is also confirmed by the *t*-test which fails to reject any of the mean pairs as significantly different. The size of the loan that the firms receive, measured by the size of long term credit in the firm's accounts, is higher among the controls. The difference is, however, not statistically significant between the groups. Sales, total assets, employees and loan size are not normally distributed variables. Thus, the *t*-test may not be a good test for comparing means. However, a plot of the distribution for treated and controls reveals that the distributions are similar for treated and controls. Due to brevity, these graphs are not displayed. Similar pre-treatment growth is essential in order to substantiate the parallel trend assumption for treated and controls. Table 4 shows that the mean value of pre-treatment sales and employee growth is similar among treated and controls. Running a regression comparing pre-treatment growth for all my selected performance variables for treated and controls, I find that the alternative hypothesis that the pre-treatment growth levels are different is highly insignificant. This result supports the assumption that the treated and controls are on a parallel trend.

Table 4
Comparison of pre-treatment means of matched variables for firms with innovation loans and control group with loans from private banks.

Variable	Mean			*t*-test	
	(1) Treated	(2) Control	(3) %*bias*	(4) *t*	(5) $p \geq \lvert t \rvert$
Sales	8858	7429	10.2	0.7	0.484
Employees	7.8	6.7	10.1	0.69	0.489
ValueAdded	2538	2988	−7.8	−0.53	0.6
TotalAssets	10,443	12,817	−6.9	−0.48	0.63
Loan	3191	4541	−10	−0.7	0.482
SalesGrowth	0.074	0.0733	0.5	0.03	0.973
EmployeeGrowth	0.101	0.0814	7.3	0.5	0.618
FirmAge	8.2	13.6	−66.8	−4.6	0

Note: Column 1 displays the mean value of the matched variables at $t-1$ for the firms with innovation loans (treated). Similarly, Column 2 displays the mean value for the control group at $t-1$. The mean values of the variables are in 1000 1998-NOK. In the matching I use log transformed variables and the square of the log transformed variables, while the table displays the absolute values. Sales growth and employee growth are measured by differences in logs from $t-2$ to $t-1$. Column 3 displays the bias in the sample. The %bias is the percentage difference of the sample means in the treated and non-treated as a percentage of the square root of the average of the sample variances in the treated and non-treated groups. Column 4 and Column 5 display the *t*-tests for equality of means in the two samples. The null-hypothesis is that the means are equal, thus a low *p*-value will reject this hypothesis.

Based on the matched sample I perform a differences-in-differences panel regression comparing firms with innovation loans with the matched control group of firms with private bank loans from the propensity score matching (PSM).[7] This is the same model as described in Eq. (1), the only exemption being that I also control for loan size.[8] The controls adjust for any residual bias between treated and controls which remains after the matching and increase estimation efficiency. Notice that the treatment year is the year the loan was paid out, not the year the loan was granted. This is an important difference which improves the accuracy of treatment as there is usually some lag between the date when the loan was approved, and the time when the project is initiated and the loan paid out.

In the matching analysis I implicitly control for much of the systematic risk by matching with respect to industry-region-vintage cohorts as well as firm size and amount of credit financing. Thus, given

[7] The combination of matching and difference-in-differences was first proposed by Heckman et al. (1997).

[8] A Sargan–Hansen test was used to test whether a random effects model would be more efficient than a fixed effects model. The consistency of the random effects model was rejected at the 5% level for the model estimating the treatment effect on growth in value added, assets and OROA.

Table 5
Survival, growth and profitability of firms with innovation loans from Innovation Norway compared to firms with private bank loans: Overall performance.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
Treated		.294***	.015	−.207***	−.000	−.011	−.267***
		(.06)	(.04)	(.07)	(.04)	(.04)	(.04)
After		.074	.116*	.106*	.078	.213***	−.055**
		(.06)	(.07)	(.06)	(.07)	(.06)	(.02)
Treated*After	−.066*	−.130*	.111	.243**	.069	.220**	.142***
	(.03)	(.07)	(.10)	(.10)	(.11)	(.11)	(.04)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			91.26	40.58	124.7	118.1	6.17
Adjusted *R*-squared			.737	.5174	.7547	.7478	.1243
Log-likelihood	−300	−726	−954	−1020	−990	−979	−171
Chi-Square	31	63					
No. of obs.	932	1201	1167	1116	1172	1167	1145

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: The regression is based on a matched sample of firms with innovation loans and firms with private bank loans. See Table 3 for a detailed description of the table.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

that the remaining difference between treated and controls is predominantly unsystematic risk, this should according to standard financial theory imply that the required rate of return is the same for firms with innovation loans and the control group of firms with private bank loans. Assuming that firms with private credit financing have an expected return above or equal to the required return on equity, the innovation loans will be an efficient use of resources if the portfolio of firms with innovation loans is at least as successful as the firms with financing from private credit institutions.

Table 5 presents the regression results from comparing the firms with innovation loans with the matched group of firms with regular private bank financing. As expected we see from the *treated* estimates that there are pre-treatment differences between the two groups with respect to the share of firms with operating deficits, Column 2, the level of valued added, Column 4, as well as the level of operating returns relative to total assets, Column 7. That is, at the time of applying for loan financing, the firms that receive innovation loans are less likely to have a sufficient cash flow to handle debt payments. This is in accordance with our expectations, as we know that firms that are granted innovation loans need only to be expected to handle debt payments within six months after the loan has been paid out to be eligible for loan financing.

The estimate in Column 1 tells us that the firms with innovation loans are significantly less likely to survive in the period after the loan has been paid out, see coefficient *Treated*After*. This implies that the remaining post-treatment estimates, Column 2–Column 7, must be interpreted with caution as there is a tendency of survival bias in the sample. For example, the table shows that the firms with innovation loans have higher post-treatment growth in value added relative to the firms with private bank financing. This may, however, be because the firms which had the poorest value added, e.g. due to poor profitability, went out of business. To illustrate the potential size of the survival bias, assume that the firms that do not survive have a value added growth of −100%. We see from the table that the remaining innovation loan firms have an average value added growth of 24.3% more than the firms with private bank financing after the loan was paid out. If we take the survival bias into account, performing the following simple back of the envelope calculation $(0.066 * (-100) + (1 - 0.066) * 24.3)$, then valued added growth after treatment is still 16.1% higher for the firms with innovation loans. Given the same standard errors this estimate would have a *p*-value of 0.054. Thus, even when controlling for sample survival bias, the difference in value added growth is still most likely positive and statistically significant at the 10% level.

The estimates tell us that there is a statistically significant positive growth in sales and value added for both treated and controls in the period after receiving loan financing, see coefficient *After*. Employee growth is also almost statistically significant at the 10% level. This suggests that loan financing facilitates growth, although the analysis does not tell us what the growth would have been for these firms without credit financing. Looking at the *Treated*After* estimates it is interesting that although both groups have a statistically significant asset growth, the growth is significantly larger for firms with innovation loans. This suggests that the firms with innovation loans receive more follow up financing.

As a robustness test I run a regression on the same sample of firms as in Table 5, but with a different model specification. In this model the overall *After* and *Treated*After* variables are split into more detailed time periods. The results are displayed in Table B.5 in the appendix. The more detailed time period estimates reveals that the difference in share of surviving firms decrease over time. In fact, after 5–8 years the share of non-surviving firms is 25.8% higher. The analysis also suggests that the difference with respect to the higher share of innovation loan firms running with operational deficits is persistent also after treatment. This illustrates that many of the innovation loans firms have problems handling their debt obligations. From the regression analysis we see that the firms with innovation loans have a higher sales growth than firms with private bank debt 5–8 years after treatment. However, if we take the survival bias into account $(0.258 * (-100) + (1 - 0.258) * 42.7)$, then sales growth 5–8 years after treatment is 6%. Assuming the same standard errors, this estimate would have been highly insignificant with a *p*-value of 0.39. It should be noticed that the panel is not balanced in the sense that the large 2009-cohort of innovation loans only has three years of observations after receiving an innovation loan, while the 2004-vintage is the only one that has eight years. Thus, the estimates for performance after five years or more are based on the vintages of 2004–2007.

In accordance with expectations, the results presented in Tables 5 and B.5 suggest that firms with innovation loans are less likely to survive and to run with operational deficits. Since the firms with innovation loans have a higher operational risk than firms with private bank loans, the surviving innovation loan firms should have a higher growth than the firms with private banks. In particular one should expect the distribution of firms with innovation loans to have higher growth in the

upper quantiles of the distribution compared to a group of firms with regular bank loans. The regression results presented in Tables 5 and B.5 provide some indication that firms with innovation loans experience higher growth in sales after five years or more, that they have a higher increase in employment after 1–2 years after treatment, and that they accumulate more assets.

To investigate whether the upper tail of the distribution is different for the treated group I follow the method of Athey and Imbens (2006) constructing a quantile difference-in-difference estimate. If the innovation loan program is successful in screening firms and their projects I expect the group of firms with innovation loans to outperform the firms with private bank loans in the right tail of the performance distribution.

There are some essential differences between the linear regression model and the quantile regression model. For the conditional mean in the linear regression model to be unbiased the error term is assumed to have an expected value of zero. In the quantile regression model the error term is required to be zero at the quantile I am interested in.[9] For example, when we look at the 0.75-quantile, we must put a restriction which says that 75% of the residuals should be negative and 25% should be positive. Thus, in a single covariate case the regression line will pass through a pair of data points where one quarter of the observations will be above the estimated regression line and three quarters will be below the regression line. There are typically multiple solutions satisfying the zero error term property. The quantile regression estimate is derived by minimizing the sum of the absolute values of the residuals, weighted according to the quantile. For example, with the 0.75th quantile the positive residuals are given larger weight (0.75), while the negative residuals are given a smaller weight (0.25) in the minimization problem.

Table 6 displays the results of quantile regression at the 75th, 90th, and 95th percentile of the contingent performance distribution respectively. To keep it brief the table only displays the differences-in-differences estimates from the regressions. Missing observations are given the value zero. This is because I want the quantile regressions to capture the fact that going out of business is poor performance, and not just missing variables.

Starting from the top of the table, we see that generally there are no statistically significant differences between the growth of the firms with innovation loans and private bank loans. However, measured in sales, we see that the 75th percent best firm among the firms with innovation loans has a 50 log-point higher growth in sales after 5–8 years. The result is statistically significant at the 10% level. Moreover, 1–2 years after treatment the firm at 75th percentile have 35 log-points higher growth in employment. This is similar to what we saw for the average performance estimates in Table B.5. With respect to OROA, we see that the 75th best firm with innovation loans have a statistically significant weaker profitability before receiving loan financing, but that the difference gradually decreases over time. 3–4 years after the loan was paid out there are no differences between the groups.

At the 90th percentile there are few significant differences between the group of firms with innovation loans and the firms with private bank loans. Most of the post-treatment coefficient estimates are in disfavor of the firms with innovation loans, although few are statistically different from zero. The exception is employment where I find a statistically significant weaker growth in employment for the firms with innovation loans. The results on OROA follow the same pattern as at the 75th percentile. Generally, quantile regression estimates are less stable the further away the percentile is from the median. This is because a large weight in the regression is put on a few observations at the tail of the distribution. In this case the normal distribution may not be an appropriate assumption (see Chernozhukov and Fernández-Val, 2011). Still, at the 95th percentile we see the same pattern as at the 90th percentile. We have negative but statistically insignificant estimates for post-treatment growth in sales. For value added we see that there are statistically significant pre-treatment differences in levels which last until 1–.2 years after treatment. For employment we find higher growth for firms with private bank loans. At the 95th percentile of the contingent distribution, the firms with innovation loans have a statistically significant lower employment growth compared to firms with private bank loans after five years or more.

Overall, the quantile regression results do not seem to support the hypothesis that the surviving firms with innovation loans outperform the group of firms with regular bank loans in the upper tail of the distribution. Thus, it does not seem as if innovation Norway succeeds in selecting a group of firms with a higher growth potential than firms with regular bank loans.

Typically, innovative projects are expected to take a longer time to develop compared to standard projects, but if they succeed they can give high returns. Thus, given that innovative projects have a different time profile with respect to development and commercialization, the comparison with projects financed with regular bank loans may falsely give the impression that the firms with innovation loans underperform although it is really an issue of timing. For this reason I also estimated post-treatment differences between the groups over time. In Table B.5 I found some evidence that firms with innovation loans have a stronger development in sales five to eight years after treatment. However, if the surviving innovation loan projects would outperform the private bank financed projects we would also expect to see early indications of future commercial success in the form of higher employment and asset growth compared to regular projects financed by private banks. To some extent the results in Table B.5 support this. However, the comparison of firms with innovation loans and private bank loans in the far right tail of the contingent performance distribution, see Table 6, does not indicate that firms with innovation loans outperform those with bank loans.

6.5. Comparison with venture portfolio companies

As an alternative to firms with private bank financing I compare the firms with innovation loans with firms that received venture fund financing during the period 2004–2009.

Table 7 displays summary statistics the year before receiving financing for the sample of firms with innovation loans and venture portfolio companies respectively. Firms that have received both venture fund financing and innovation loans are excluded from the sample. The reason for this is that I cannot separate the effects of the two sources of capital from each other. The final sample contains 128 firms with innovation loans and 34 firms with venture fund financing. From the table we see that the average and median pre-treatment size and age of the groups are similar. While the average size of the venture portfolio company is larger than that of the firms receiving innovation loans, the opposite is the case for the median firm. This implies that the sample of venture portfolio companies contains some larger firms.

As both treated and controls are engaged in innovative projects I expect the average development and commercialization period to be similar. The time of treatment for the firms with innovation loans is measured as the year the innovation loan is paid out, while the time of treatment for the control group is when the venture fund makes its first investment in the portfolio company. I find that among the firms which received both venture financing and innovation loans the venture financing is provided on average one year before the loan. Thus, if anything, we should expect the firms with innovation loans on average to be more mature compared to the firms with

[9] In the standard linear regression model the error term is also assumed to have a constant variance (homoscedasticity). In the quantile regression model the only assumption on the error term is that it is zero at the relevant quantile. For more on quantile regressions see e.g. Hao and Naiman (2007) or Khandker et al. (2010).

Table 6
Firms with innovation loans compared to firms private bank loans: quantile regressions.

	(1) ln(sales + 2) Coef./SE	(2) ln(va + 2) Coef./SE	(3) ln(emp. + 1) Coef./SE	(4) ln(assets + 2) Coef./SE	(5) OROA Coef./SE
75 percentile					
Treated*2 years before treatment	.253	.043	.648***	.210	−.188***
	(.27)	(.24)	(.18)	(.19)	(.05)
Treated*1 year before treatment	.184	.022	.522**	.178	−.185***
	(.23)	(.15)	(.25)	(.25)	(.03)
Treated*(1–2) years after treatment	.390	.150	.511***	.267	−.074***
	(.24)	(.15)	(.16)	(.18)	(.02)
Treated*(3–4) years after treatment	.191	.190	.223	−.012	−.045
	(.41)	(.21)	(.31)	(.18)	(.03)
Treated*(5–8) years after treatment	.515*	−.087	.097	−.490	−.028
	(.29)	(.25)	(.47)	(.36)	(.04)
R-squared	.043	.029	.041	.040	.057
No. of obs.	1464	1464	1464	1464	1464
90 percentile					
Treated*2 years before treatment	.505	.494**	−.023	.171	−.179**
	(.32)	(.21)	(.27)	(.25)	(.08)
Treated*1 year before treatment	.379	.513***	.300	.247	−.207***
	(.28)	(.18)	(.29)	(.32)	(.05)
Treated*(1–2) years after treatment	.228	.236	−.246	.059	−.124**
	(.26)	(.18)	(.17)	(.24)	(.05)
Treated*(3–4) years after treatment	.010	.176	−.158	.173	−.110**
	(.24)	(.20)	(.20)	(.34)	(.05)
Treated*(5–8) years after treatment	−.473	−.257	−.578*	.171	−.011
	(.50)	(.35)	(.34)	(.40)	(.06)
R-squared	.028	.010	.024	.021	.051
No. of obs.	1464	1464	1464	1464	1464
95 percentile					
Treated*2 years before treatment	.565	.490**	.486**	.348	−.115
	(.72)	(.21)	(.22)	(.30)	(.10)
Treated*1 year before treatment	.226	.264	.532***	.143	−.267**
	(.43)	(.28)	(.20)	(.31)	(.13)
Treated*(1–2) years after treatment	.197	.511***	.113	−.014	−.024
	(.33)	(.16)	(.34)	(.44)	(.13)
Treated*(3–4) years after treatment	−.137	.169	−.049	−.395	−.047
	(.28)	(.17)	(.19)	(.68)	(.06)
Treated*(5–8) years after treatment	−.431	−.010	−.340	.003	.036
	(.59)	(.22)	(.27)	(.61)	(.13)
R-squared	.013	.019	.026	.018	.018
No. of obs.	1464	1464	1464	1464	1464

Standard errors (SE) are reported in parentheses.
Note: The data set and the control variables are the same as in Table 5. The only difference is that I replace missing values with zero in order to avoid bias, e.g. due to firms falling out of the sample. In the estimation I use the program qreg2 in STATA developed by (Machado and Santos Silva, 2013). Using qreg2 the standard errors and t-statistics are asymptotically valid under heteroskedasticity and misspecification.

* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

Table 7
Summary statistics: firms with innovation loans compared to venture fund portfolio companies.

	Treated (128 obs.)					Control (34 obs.)				
	Mean	sd	p25	p50	p75	Mean	sd	p25	p50	p75
Sales	14,088	29,390	518	5107	12,088	19,925	43,102	559	3157	15,349
Employees	11	18	2	5	11	14	27	2	6	14
ValueAdded	4206	10,359	−211	1163	4566	3681	9944	−394	1409	6246
TotalAssets	20,521	62,113	1689	5779	15,301	57,779	269,145	2989	6917	14,329
YearTreatment	2007.7	1.5	2007.0	2008.0	2009.0	2006.8	1.7	2005.0	2007.0	2008.0
FirmAge	8.3	6.8	3.5	6.0	10.0	9.0	6.4	4.0	7.5	12.0
InnovationLoan	2848	3755	900	2000	3100	0	0	0	0	0

Note: This table displays summary statistics the year before the firms received an innovation loan or venture fund financing during the period 2004–2009. The figures are in 1000 1998-NOK.

Table 8
Survival, growth and profitability of firms with innovation loans compared to firms with venture capital financing: Overall performance.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
Treated		.057	.096*	−.015	−.013	.012	−.044
		(.08)	(.05)	(.11)	(.06)	(.04)	(.05)
After		.274***	.329***	.216	.402***	.543***	−.141***
		(.09)	(.11)	(.13)	(.11)	(.13)	(.04)
Treated*After	−.081**	−.321***	−.119	.102	−.227*	−.234	.220***
	(.03)	(.10)	(.13)	(.15)	(.13)	(.14)	(.05)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			211.1	39.86	141.1	154.1	5.933
Adjusted *R*-squared			.8087	.5484	.7607	.7616	.1485
Log-likelihood	−244	−541	−745	−984	−835	−772	−200
Chi-Square	25	79					
No. of obs.	729	931	931	861	933	931	907

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: See Table 3 for a detailed description of the table content and the analysis.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

venture financing.

Table 8 displays the results from a regression analysis comparing firms with innovation loans with a control group of firms which received venture fund financing for the first time during the same period.[10] The firms with venture fund financing are already a selected group of firms. Thus, I do not use propensity score matching on this sample. As before I apply a differences-in-differences model. Column 1 and Column 2 are estimated with a probit model, while the remaining are OLS. See Eq. (1) for details on the estimation model. I study the assumption of parallel trends by running a regression comparing pre-treatment growth in my selected performance variables for treated and controls. I find that the alternative hypothesis, that the pre-treatment growth levels are different, is highly insignificant for all my performance variables. This supports the assumption that the treated and controls are on a parallel trend, and thus that the post-treatment estimates are unbiased.

From Table 8 Column 1 we see that the firms with innovation loans are less likely to survive compared to the venture portfolio companies. To some extent this is surprising as I would have expected the firms with innovation loans on average to be less risky than the firms with venture capital financing. On the other hand, the venture portfolio companies have owners with financial muscles which are able to keep the firm running for a longer period of time given that they keep their faith in the project. The estimated difference in the share of surviving firms is, however, not large, and consequently the survival bias in the remaining coefficients is limited. Column 2 shows that the firms with innovation loans are less likely to run their operations with deficits. This is not surprising as venture fund portfolio companies typically increase their operational deficits when they find investors. This is part of the so-called *j*-curve with increasing operational deficits over some time in the hope of cashing out on the investment in the end. Firms with innovation loans should on their side be able to handle their debt obligations, something which is challenging if running with operational deficits. Interestingly, in contrast to the results we saw in the comparison with firms with bank financing, there seem to be no pre-treatment differences with respect to the share of firms running with operational deficits before receiving loan financing. This suggests that the control group of venture portfolio companies has a development profile that is more similar to the firms with innovation loans compared to the firms with private bank financing.

Table 8 reveals a statistically significant positive growth in sales for both treated and controls after treatment of 37.4 log-points, see coefficient *treated*. I do, however, not find statistically significant differences between the two groups. Similarly, with respect to value added I find no statistically significant differences between the groups after treatment, see Column 4. For employees I find a statistically significant positive development after treatment for both groups, although significantly weaker for firms with innovation loans. From Column 6 we see that both groups have a strong common growth in assets. It appears that the asset growth is weaker for the firms with innovation loans, although the difference is at the margin not statistically significant at the 10% level. We see that the firms with innovation loans are generally more profitable in terms of OROA. This is consistent with these firms being selected based on the expectation that they can handle debt obligations at the latest six months after the loan is paid out. In comparison, even successful venture backed portfolio companies typically run their operations with operating deficits for some years before their technology is commercialized.

Table B.6 in Appendix B displays regressions on the same sample of firms as the regressions in Table 8, but with a different model specification splitting the overall *After* and *Treated*After* variables into more detailed pre- and post-treatment time periods. Interestingly, the share of non-surviving firms is 25.2% higher 5–8 years after treatment for the firms with innovation loans compared to the venture portfolio companies. The remaining estimates for performance 5–8 years after treatment should thus be interpreted in the light of a survival bias. To illustrate the potential impact of the survival bias I assume that the non-surviving firms have a sales growth of −100%. Based on this assumption the estimated difference in sales growth 5–8 years after treatment would be −30% $(0.252 * (-100) + (1 - 0.252) * -0.77)$. Given the same standard errors, this estimate would be statistically significant at the 10% level. A similar analysis for the post-treatment differences in employee growth, correcting for sample survival bias, also suggests that there is statistically significant lower growth in employees for firms with innovation loans 5–8 years after treatment. A plausible interpretation of the development in the dependent variables is that the firms with venture financing put their resources into expanding with respect to more employees and assets, while the firms with innovation loans focus more on handling their debts by putting more emphasis on running their business with an operating surplus.

Analogous to the comparison of firms with innovation loans with

[10] A Sargan–Hansen test was used to test whether a random effects model would be more efficient than a fixed effects model. The consistency of the random effects model was rejected at the 5% level for the model estimating the effect on growth in all performance variables except OROA.

Table 9
Firms with innovation loans compared to firms with venture capital financing: Quantile regressions.

	(1) ln(sales + 2) Coef./SE	(2) ln(va + 2) Coef./SE	(3) ln(emp. + 1) Coef./SE	(4) ln(assets + 2) Coef./SE	(5) OROA Coef./SE
75 percentile					
Treated*2 years before treatment	.035	−.042	−.067	.003	−.029
	(.05)	(.08)	(.05)	(.04)	(.06)
Treated*1 year before treatment	−.000	−.021	−.029	−.000	−.014
	(.01)	(.06)	(.03)	(.01)	(.05)
Treated*(1–2) years after treatment	−.029	−.082	−.249**	−.116	.065**
	(.17)	(.09)	(.11)	(.10)	(.03)
Treated*(3–4) years after treatment	−.066	−.200**	−.178	−.503***	.069**
	(.12)	(.09)	(.24)	(.14)	(.03)
Treated*(5–8) years after treatment	.042	−.285	−.227	−.329***	.061**
	(.12)	(.18)	(.22)	(.12)	(.03)
R-squared	.658	.526	.546	.531	.123
No. of obs.	1053	1053	1053	1053	1053
90 percentile					
Treated*2 years before treatment	−.209**	−.159	−.125	.018	.012
	(.10)	(.19)	(.16)	(.04)	(.07)
Treated*1 year before treatment	.007	−.053	−.061	−.000	.024
	(.02)	(.04)	(.05)	(.01)	(.07)
Treated*(1–2) years after treatment	.038	.039	−.282**	−.074	.090*
	(.11)	(.12)	(.13)	(.16)	(.05)
Treated*(3–4) years after treatment	.071	−.060	−.454***	−.268	.009
	(.11)	(.29)	(.17)	(.20)	(.09)
Treated*(5–8) years after treatment	−.020	−.046	−.378	−.638***	.127**
	(.39)	(.31)	(.27)	(.23)	(.05)
R-squared	.615	.491	.506	.479	.057
No. of obs.	1053	1053	1053	1053	1053
95 percentile					
Treated*2 years before treatment	−.199*	−.120	−.074	.039	−.044
	(.12)	(.08)	(.06)	(.05)	(.08)
Treated*1 year before treatment	−.005	−.133*	−.037	−.015	.075
	(.02)	(.08)	(.05)	(.02)	(.05)
Treated*(1–2) years after treatment	−.039	.100	−.324***	−.321	.160*
	(.18)	(.26)	(.12)	(.30)	(.10)
Treated*(3–4) years after treatment	−.116	−.238	−.513**	−.163	.044
	(.40)	(.27)	(.22)	(.18)	(.08)
Treated*(5–8) years after treatment	−.649	−.699	−.477*	−.995**	.155*
	(.65)	(.61)	(.27)	(.43)	(.09)
R-squared	.579	.456	.471	.441	.014
No. of obs.	1053	1053	1053	1053	1053

Standard errors (SE) are reported in parentheses.
Note: The data set and the control variables are the same as in Table 8. The only difference is that I replace missing values with zero in order to avoid bias, e.g. due to firms falling out of the sample. See also Table B.4 for interpretation of the estimated coefficients and Table 6 for more details on the quantile regression estimation.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

firms with private bank financing, I do not expect the control group of venture portfolio companies to have the same performance distribution as the firms with innovation loans. I expect the firms with venture investments to have a higher growth in the right tail of the distribution. This is based on the assumption that venture backed firms go through a tighter selection process with respect to growth potential compared to the firms with innovation loans.

Table 9 compares the performance distribution of the firms with innovation loans and the venture portfolio companies at the 75th percentile, the 90th percentile, and the 95th percentile respectively. To keep it brief only the differences-in-differences estimates are displayed. Note that the control group only contains 34 firms. Thus, estimates at the 95th percentile contain at most two firms with venture financing. It is important to look at the different quantile regressions in context. If the results at the different percentiles all seem to go in the same direction, then this strengthens the result. For employment and asset growth, however, the quantile regressions seem to suggest that the venture portfolio companies have a stronger growth in the right tail of the distribution. Firm with innovation loans have a statistically significant higher profitability, measured by OROA, at the 75th, 90th, and 95th percentile of the distribution.

Overall the quantile regressions suggest that there are no differences in sales or value added growth 1–8 years after treatment. The venture portfolio companies do, however, have a stronger growth in employment and assets. This may indicate that some of the venture portfolio companies are more likely to succeed in the long run.

7. Welfare implications of the innovation loan program

Based on the result from the firm level effect study it is interesting to explore how large the positive spillover effects from the innovative projects should be in order for the innovation loan program to have the same welfare effect as regular business loans.

From the comparison with the program rejects, see Table 3, the average treatment effect on sales growth from an innovation loan was 0.29 log-points, which is approximately 29%. Among the firms

Table 10
Summary table: estimated treatment effects from innovation loans relative to control groups.

	Rejected applicants	Companies with private credit financing	Venture portfolio company
Survival	(0)	(−)	(−)
Deficit	(0)	(−)	(−)
Sales	(+)	(0)	(0)
Value added	(+)	(+)	(0)
Employees	(+)	(0)	(−)
Assets	(+)	(+)	(0)
OROA	(0)	(+)	(+)

Note: (+)/(−) are positive and negative statistically significant differences at 10% or lower, while (0) implies that there are no significant differences between treated and controls. The table is based on estimates presented in Tables 3, 5 and 8.

receiving innovation loans the median sales at $t-1$ before receiving an innovation loan is 5.3 million NOK, see Table 2. Thus, for the median firm the average sales increase with 1.6 million NOK per year. In comparison we know that the program operates with an expected loss of one third of the portfolio. Thus, given a median loan of 2 million NOK, see Table 2, the expected total loss for Innovation Norway on the median firm is 0.7 million NOK. Unfortunately we cannot draw welfare implications from this as we (1) do not know what the counterfactual development really is, due to a likely administrative bias, or (2) what the alternative use of these resources would give us.

The results from the analysis in Section 6.4 suggest that the innovation loan firms are on the same level with respect to growth as firms with private bank loans, but they are more likely to go out of business. Still, even if there is a positive effect on the firms' performance from the program, the program might involve losses and/or transaction costs leading to a net welfare loss for the economy as a whole (Honohan, 2010). In fact, debt losses and administration costs are considerably higher for the innovation loan program compared to that of private banks. The annual administration costs of the program are above 2% of total assets. This is more at the level of a venture capital fund rather than a bank. According to the Norwegian central bank the average annual losses relative to the total portfolio of Norwegian business loans were 0.5% during the period 2002–2010. In comparison, the annual average loss rate for the innovation loan program was above 3%.

Private banks should cover administration costs, credit losses,[11] and return on their owners' equity from the income of their services. The innovation loan program, however, need only cover administration costs. Thus, the net difference in costs between the innovation loan program and the private bank is the size of the losses on the innovation loan portfolio plus the missing return on this equity.

During the period 2004–2009, 1 733 million NOK (EUR 217 mill.) were paid out in innovation loans. Given an expected average loss probability of one third, the government needs to set aside 578 million NOK (EUR 72 mill.) into a loss fund in order to cover future expected losses on the loans granted during the period. Since the losses are covered by the government through taxes, one must also add the social costs of public funds. For example, the Norwegian ministry of Finance operates with a social cost of public funds of 20% in their calculations. Given this rate the total extra costs of the innovation loan program compared to regular business loans are 692 million NOK (EUR 87 mill.).[12]

The loss fund is the government's equity. Unlike regular banks, the innovation loan program does not deliver return on this equity.[13] Thus, the government does not only lose the equity, but also the potential return on this equity compared to e.g. investing this equity into regular banking equity. The average risk free rate, measured by the 10-year Norwegian government bond rate during the period 2004–2009, is approximately 4%. Based on US data the average beta-value for banks during the same period was 1.18.[14] Assuming a market risk premium of 5%, from the capital asset pricing model, this gives a required return on equity of approximately 10%.

Thus, in order for the innovation loan program to be as welfare enhancing as regular business credit financing, the spillover effects from the innovative projects must be large enough to outweigh the 692 million NOK in expected losses covered by the government, plus the required return on equity on the loss fund.

Positive externalities arising from investment in R&D and innovation are an important part of the rationale for governments to have an innovation policy. The main challenge with this argument is that it is hard to measure the size and effect of these spillovers with any precision (see e.g. Honohan, 2010; Wieser, 2005).

The main source of knowledge spillovers from innovative projects is likely to come from labor mobility. Based on a sample of Norwegian subsidized IT-failures during the 1980s, Møen (2007) investigates whether there are spillover effects as scientists and engineers from the failed subsidized firms start working at other businesses or start new ventures themselves. Møen (2007) finds that firms which engage former employees from the subsidized firms do not perform any better than the average. Moreover, he finds that the spin-offs from the subsidized firms seem to perform below average. This study, based on Norwegian data, suggests that knowledge spillover effects are highly limited. If the results are generalizable for other innovation policy programs in Norway, the projects supported by the innovation loan program are not likely to have spillover effects which give rise to a welfare gain large enough to compensate for the 692 million NOK plus returns.

That said, spillover effects are likely to vary between regions, programs and over time depending on factors such as culture, technology shifts, population densities, labor mobility and industry composition. Stucchi et al. (2014) are the first to conduct an evaluation of an innovation policy program which also measures knowledge spillover effects. As part of the evaluation of the Argentinean public innovation program FONTAR, Stucchi et al. (2014) use a similar methodology as Møen (2007), measuring knowledge spillover effects through labor mobility using a panel of employer–employee data. In this case the study's results suggest that the indirect effects on employment, real

[11] During the 2008–09 financial crisis many private banks needed public assistance in order to avoid insolvency. Although the Norwegian government provided important measures to improve liquidity, no Norwegian bank needed any direct public funding.

[12] For simplicity I disregard that the transfers into the loss fund are made at different periods in time. Thus, the amount is not an accurate present value.

[13] The program has delivered small surpluses during the period but well below any normal rate of return on equity.

[14] See link http://people.stern.nyu.edu/adamodar/.

wages and probability of exports for the firms that employ highly skilled labor from the program participants are almost at size with the direct effects on the firms participating in the program. They find the average direct effect on real wages for the participants of the program to be 6.1%, while the indirect effect on real wages from hiring staff from participating firms is 3.6%. Based on the sample sizes in their matching analysis it seems as if there are about 20% more firms that experience knowledge spillover effects compared to firms participating in the program. Assuming that the firms experiencing direct and indirect effect on average are of equal size, the aggregate indirect effect on real wages is 70% of the total effect.[15] Hence, this study suggests that the indirect effect on labor productivity is quite large.

8. Conclusion and discussion of results

The research question I seek to answer is the following: How do the loan program participants perform relative to a set of relevant and complementary control groups? I consider relevant control groups to be either firms that are similar to the firms that receive treatment, or firms that receive a similar type of treatment as the firms receiving innovation loans. This research question distinguishes itself from most other effect studies which are mainly interested in measuring what would have happened to firms had they not received loan financing from the public program. I also try to measure the counterfactual outcome of not receiving an innovation loan. My main focus, however, is on measuring performance of the treated firms relative to control groups that serve as benchmarks of the alternative use of resources outside the program. This type of control group is particularly interesting in effect studies that aim to identify net welfare effects of the policy program. In this study I am able to provide a benchmark in nominal amounts on how large potential positive externalities should be in order for the program to provide welfare benefits on the same level as a particular alternative use of resources.

I apply three different control groups to assess the performance of the firms receiving innovation loans: Companies that applied for financing through the program but were rejected by the program administrators, companies with private credit financing and venture portfolio companies. The latter two control groups are benchmarks of the welfare effects of the program, while the first is a benchmark for the additionality of the program.

Comparing with program rejects I find that program participants perform better on a variety of growth measures (see Column 2 in Table 10). Although the sample is likely to be affected by an administrative bias, this result suggests that the program is additional and that receiving an innovation loan has a positive effect on firm growth. Comparing the treated firms with the program rejects can be considered a first test with respect to whether the program is successful in improving welfare. If there had been no differences between the treated and the rejects, then this would be a strong indication that the program is redundant with respect to financing innovative projects. Since I do find that the program participants perform better, further tests are needed to assess the welfare effects of the innovation loan program.

When comparing with firms receiving private credit financing, I find some weak evidence that the firms with innovation loans have higher sales growth after 5–8 years (see Table B.5 Column 1). A simple back of the envelope calculation shows that the difference in sales is most likely due to survival bias in the sample. Despite a lower probability of survival (see Table 10 Column 3), I do not find results suggesting that the firms with innovation loans perform better in the upper quantiles of the distribution (see Table 6). The comparison with the control group consisting of companies with private loan financing suggests that the innovation loan program does not succeed in financing the target group of innovative projects with a high growth potential.

However, applying venture capital portfolio companies as a control group, I find only weak indications that the performance of firms with innovation loans is poorer than that of the venture portfolio companies. In particular, I find no statistically significant differences with respect to the growth in sales (Column 4 Table 10). This could indicate that the time period I look at, 3–8 years after the loan was paid out, is a too short time interval to detect successful commercialization of innovative projects. An alternative explanation for this result is that neither the innovation loan firms nor the sample of venture portfolio companies will end up as commercial successes. I do find that venture portfolio companies have higher rates of survival, as well as stronger growth in employment and assets (Column 6 Table B.6). The finding that the venture portfolio companies have more human and capital resources put into their projects compared to companies with innovation loans may indicate that the venture portfolio companies are more likely to succeed in the long run compared to the firms with innovation loans. However, even if the innovation loan companies would turn out to have weaker performance than the venture portfolio companies the program could still be welfare improving. That is, while a finding that program participants perform at level with or even better than venture portfolio companies would have supported the alternative hypothesis that the innovation loan program is welfare improving, an indication of weaker performance does not enable us to reject the alternative hypothesis.

The fact that the selected firms perform better than the rejects as well as the fact that I do not find significant differences in terms of sales growth between the innovation loan firms and the venture portfolio companies suggests that Innovation Norway's selection competency is adequate, at least compared to private alternatives. Moreover, when comparing the performance of firms with market based loans from Innovation Norway with that of private market based loans I do not find statistically significant differences. This suggests that Innovation Norway's bank competency is at level with that of private institutions, although that may not be sufficient to make the program contribute to a net improvement in welfare. In fact, debt losses and administration costs are considerably higher for the innovation loan program compared to that of private banks. One third of the innovation loans are expected to end up as losses, and the administration costs are on the same level as for venture funds. Thus, I find that the knowledge spillover effects from the projects with innovation loans must amount to as much as one third of the amount of credit provided by the program plus risk adjusted return on equity and social cost of public funds in order for the program to provide the same level of welfare as regular credit activity toward the business segment. The actual knowledge spillover effect from the innovation loan program has not been investigated. However, a previous study from Norway on subsidized IT-failures suggests limited positive spillover effects, while an evaluation of the Argentinean public innovation program FONTAR suggests that the spillover effects can be large.

During the period for which I measure firm performance, from 2005–2012, the total number of bankruptcies in Norway was more than 25% higher during the final part of the period compared to the first half. Hence, I cannot rule out that the period which I investigate was a period with particularly adverse macro economic conditions making it difficult for innovative projects to succeed. The weighted average return of early stage European venture funds is about zero over the period 1980–2013 (EVCA, 2014). While the average return was positive until the mid nineties, it has been negative for most cohorts since. This suggests that it is a difficult environment for innovative projects to succeed in general, and not only for the innovation loan program in particular. A part of the explanation for poor performance of Norwegian venture funds might, however, also be that that much of the venture capital in this period has been provided through government-backed funds, which may have led to an overinvestment, and in turn low returns. The Norwegian State Audit Institution documented in a recent evaluation that none of the 15 publicly subsidized seed and venture

[15] This is calculated by multiplying the indirect effect on real wages with a factor of 1.2, according to the number of firms affected and dividing it by the direct effect on real wages (3.5 * 1.2/6.1 = 0.7).

funds have succeeded in bringing forward high growth companies during the period 1998–2014. On a general basis I consider venture portfolio companies a highly relevant control group for publicly supported young innovative firms. If there has been an overinvestment by venture capital funds in Norway, then venture capital portfolio companies is, however, not a good benchmark for the alternative use of resources. There seems to be strong evidence that this has been the case in Norway for the years 2005–2012.

Acknowledgements

I thank Professor Hans Hvide and my supervisors Professor Steinar Holden and Leo A. Grünfeld for valuable comments. I also would like to thank the two anonymous referees for their instructive input. Parts of the analysis presented in this paper are part of an evaluation of Innovation Norway's loan and guarantee schemes on behalf of the Norwegian Ministry of Trade and Commerce and the Norwegian Ministry of Local Government and Modernisation (Grünfeld et al., 2013). The research is also part funded by the Research Council of Norway.

Appendix A. The low-risk loan program

The screening processes of market based loans, commonly referred to as low risk loans, and the innovation loans are performed by the same institution and the same case workers. Thus, it adds information to see the results from the effect study of the low risk loan program and the innovation loan program in context. Since the low risk loan program in many respects is run and administrated like a private bank, the performance study of the low risk loans isolates the effect of whether bureaucrats are able to operate regular credit institutions.

What I measure here is basically what Storey (1998) refers to as administrative selection. That is, whether the bureaucrats have the competency to select firms eligible for debt financing. Consequently, when measuring the effect of the innovation loan program on firm survival and growth, I have information about the quality of the administrative competency of the lending institution based on the low risk loan program. The results regarding the innovation loan portfolio can be interpreted in light of this.

Given that the low risk loan program is a scheme not much different from any other bank, I expect that firms receiving low risk loans perform on the same level as firms with private bank financing. If the firms with low risk loans perform on the same level this would suggest that Innovation Norway is successful in their screening.

In the period 2004 to 2009 there were 371 service and industry related projects split on 304 firms which received low risk loans from Innovation Norway. Excluding firms for which the loan financing is smaller than 20% of the firm's assets the year before the loan was paid out the sample is reduced to 218 firms. Of the 218 firms 149 had common support. 62 did not find a match with the caliper set at 0.05, and 7 were excluded due to missing data points.

Table A.1 contains pre-treatment statistics on a matched sample of firms with innovation loans and firms with private bank loans. The table shows that the firms with low risk loans and the control group of firms with private bank loans have similar means. The *t*-tests do not find statistically significant differences in the two samples. Although not displayed I have also made a graphical comparison of the distribution of the samples with respect to sales, value added, total assets and loan size. All of this points in the direction that the control group is a good match. To the extent that the differences between treated and controls are not removed I control for the same pre-treatment characteristics in the regression on the matched sample.

Table A.1
Comparison of pre-treatment means of matched variables for firms with low risk loans within industry and services and control group of firms with loans from private banks.

Variable	Mean			*t*-test	
	(1) Treated	(2) Control	(3) %*bias*	(4) t	(5) $p \geq \lvert t \rvert$
Sales	9062	11,053	−8.7	−0.74	0.462
Employees	7.1	6.3	5.7	0.47	0.637
ValueAdded	3275	4149	−10.2	−0.86	0.392
TotalAssets	13,306	17,970	−11	−0.93	0.355
Loan	6478	7355	−5.2	−0.44	0.662
SalesGrowth	.020	.025	−2.4	−0.18	0.857
EmployeeGrowth	−.002	.013	−5.1	−0.38	0.702
FirmAge	9.1	10.1	−10.5	−0.86	0.388

Note: Column 1 displays the mean value of the matched variables at $t-1$ for the firms with low risk loans. Similarly, Column 2 displays the mean value for the control group at $t-1$. In the matching I use log transformed variables and the square of the log transformed variables, while the table displays the absolute values. Column 3 displays the bias in the sample. The %bias is the percentage difference of the sample means in the treated and non-treated as a percentage of the square root of the average of the sample variances in the treated and non-treated groups (formulae from Rosenbaum and Rubin, 1985). Columns 4 and 5 display the *t*-tests for equality of means in the two samples. The null-hypothesis is that the means are equal and thus a low *t*-value will not reject this hypothesis.

Table A.2 displays the results from the regression analysis of firms with low risk loans compared to a control group of firms with private bank loans. The *Treated* estimates in Table A.2 tell us that the treated and the control group are not at statistically significant different levels pre-treatment. The only exception is operating returns on assets (OROA), where the return is significantly weaker at the 10% level for the group of firms with low risk loans. A separate regression on the matched sample, not displayed due to brevity, tells us that the matched sample does not have a statistically significant different pre-treatment growth in any of the performance variables. This suggests that the treated and the firms are on the same trend growth, and that the differences-in-differences estimates are not biased.

Table A.2
Survival, growth and profitability of industry and service firms with low risk loans from Innovation Norway compared to firms with private bank loans.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) ln(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
Treated		.026	.014	−.020	−.009	−.002	−.040*
		(.05)	(.05)	(.04)	(.04)	(.03)	(.02)
After		−.067*	.320***	.208***	.134***	.349***	−.018
		(.04)	(.06)	(.04)	(.05)	(.05)	(.02)
Treated*After	−.022	−.010	−.008	.021	.035	.215**	.028
	(.04)	(.05)	(.09)	(.06)	(.08)	(.08)	(.02)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			129.5	142.5	229.9	91.23	5.143
Adjusted *R*-squared			.7745	.638	.8105	.7057	.0611
Log-likelihood	−607	−1112	−1560	−1291	−1473	−1553	714
Chi-Square	67	117					
No. of obs.	1476	2004	1687	1676	1693	1687	1687

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: This table displays pre- and post-treatment differences between firms with low risk loans from Innovation Norway and a control group of firms with private credit. The sample is constructed by using propensity score matching and contains firms receiving long term credit financing during the time period 2004–2009. The data is a panel covering a window of two years before and until seven years after treatment. See Table 3 for a more detailed description of the table.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

The *Treated*After* estimate of the survival variable, see Column 1, displays no statistically significant differences between the firms with low risk loans and regular private bank loans with respect to survival in the period after the loan was paid out. There are also no statistically significant differences between the two groups of firms with respect to the probability of running operational deficits, Column 2, after the loan was paid out. These results suggest that there are no differences between the low risk loan portfolio and the private bank loan portfolio with respect to bankruptcy or rates of debt defaults.

The *After* estimates shows a statistically significant positive growth in sales, value added, number of employees and total assets for both the firms with low risk loans and the control group of firms with private credit. The differences between the firms with low risk loans and the firms with private bank loans post-treatment are generally small and insignificant. The exception here is growth in total assets where I find that the firms with low risk loans have a significantly stronger growth in total assets. Asset growth signals an ability to gain resources, either from running profits or from additional loan uptake or equity issues. It is surprising that the firms with low risk loans have a stronger growth in assets without also having either a stronger growth in e.g. sales, or a weaker development in profitability.

Appendix B. Variable definitions and robustness results

Table B.3
Definitions of regression variables.

Variable	Definition
Sales	Firm sales (1000 NOK).
Employees	Number of employees registered with the firm.
TotalAssets	The firm's total assets (1000 NOK).
ValueAdded	The firm's gross value added (sum of operating results, labor costs, write offs and write downs) (1000 NOK).
YearTreatment	Binary dummy variable equal to one for the respective year the firm received treatment.
FirmAge	Number of years since the firm was established at the time of treatment.
Loan	The change in long term loan financing at the time of treatment (1000 NOK).
InnovationLoan	The firm's amount of long term loan financing from a credit institution (1000 NOK).
SalesGrowth	Difference in ln(sales + 2) from year $t-2$ to $t-1$. Winzorized at the top and bottom 2.5 percentiles.
EmployeeGrowth	Difference in ln(employees + 2) from year $t-2$ to $t-1$. Winzorized at the top and bottom 2.5 percentiles.
Treated	Binary variable equal to one if the firm receives an innovation loan, and equal to zero otherwise.
After	Binary variable equal to one in the period after the firm has received an innovation loan, and equal to zero otherwise.
Survival	Binary variable equal to one if the firm has labor costs or sales, and equal to zero otherwise.
Deficit	Binary variable equal to one if the firm has operational deficits, and equal to zero otherwise.
ln(sales + 2)	Natural logarithm of sales plus NOK 2 million.
ln(va + 2)	Natural logarithm of value added plus NOK 2 million.
ln(employees + 1)	Natural logarithm of number of employees plus 1.
ln(assets + 1)	Natural logarithm of total assets plus NOK 2 million.
OROA	The firm's operating results on assets. Winzorized at the top and bottom 2.5 percentiles.

Table B.4
Survival, growth and profitability of firms which were granted innovation loans compared to firms that were rejected by the program (detailed estimates on performance pre- and post-treatment).

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
2 years before treatment		−.054	−.025	−.029	−.037	.045	.010
		(.10)	(.09)	(.10)	(.12)	(.09)	(.07)
(1–2) years after treatment		.138	−.124	−.180	−.135	−.005	−.044
		(.12)	(.10)	(.13)	(.10)	(.11)	(.06)
(3–4) years after treatment		−.047	−.111	−.062	−.309**	−.026	.083
		(.10)	(.12)	(.14)	(.12)	(.15)	(.09)
(5–8) years after treatment		−.262**	−.008	.075	−.416*	−.232	.351***
		(.13)	(.15)	(.17)	(.21)	(.18)	(.11)
Treated*2 years before treatment		.003	−.056	−.237	−.038	−.155*	.089
		(.12)	(.12)	(.16)	(.13)	(.09)	(.09)
Treated*1 year before treatment		.080	−.024	−.202	.036	.011	.025
		(.13)	(.07)	(.14)	(.07)	(.06)	(.08)
Treated*(1–2) years after treatment	.060	−.128	.256**	.236	.316**	.271*	.125*
	(.04)	(.13)	(.12)	(.17)	(.12)	(.14)	(.06)
Treated*(3–4) years after treatment	−.070	.041	.183	.099	.404***	.379**	.039
	(.06)	(.11)	(.13)	(.17)	(.15)	(.18)	(.08)
Treated*(5–8) years after treatment	−.214**	.164	.380*	.088	.635**	.708***	−.191**
	(.11)	(.11)	(.19)	(.19)	(.25)	(.24)	(.09)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			66.48	23.6	74.7	63.66	5.217
Adjusted *R*-squared			.7806	.5477	.7423	.7102	.1187
Log-likelihood	−185	−483	−656	−803	−732	−761	−205
Chi-Square	93	68					
No. of obs.	632	807	807	737	808	807	789

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: The regression is run on the same sample with the same control variables as the regressions displayed in Table 3. The only difference is that this regression includes detailed estimates on pre- and post-differences between firms that were granted innovation loans from Innovation Norway and those rejected. The *X years before/after treatment* estimates are the common level differences for both groups compared to the reference year $t-1$. The interacted dummy variables *Treated*X years before/after treatment* should be interpreted as the innovation loan firms' deviation from the common level at each point in time. The regression in Column 1, *Survival*, only estimates post-treatment differences.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

Table B.5
Survival, growth and profitability of firms with innovation loans compared to firms with private bank loans: Firm performance over time.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
2 years before treatment		−.035	−.061*	−.019	−.027	−.127***	−.014
		(.05)	(.04)	(.04)	(.06)	(.03)	(.03)
(1–2) years after treatment		.056	.063	.090	.036	.154***	−.052*
		(.07)	(.05)	(.06)	(.06)	(.05)	(.03)
(3–4) years after treatment		.101	.097	.095	.072	.182***	−.069**
		(.07)	(.06)	(.06)	(.07)	(.06)	(.03)
(5–8) years after treatment		−.029	.133	.120	.129	.077	−.077*
		(.09)	(.14)	(.12)	(.13)	(.11)	(.04)
Treated*2 years before treatment		.279***	.006	−.256**	−.037	−.017	−.245***
		(.07)	(.06)	(.10)	(.07)	(.05)	(.05)
Treated*1 year before treatment		.319***	.025	−.156**	.036	−.004	−.287***
		(.06)	(.03)	(.08)	(.03)	(.03)	(.04)
Treated*(1–2) years after treatment	.037	.215***	.090	.026	.134**	.133*	−.169***
	(.03)	(.07)	(.07)	(.07)	(.07)	(.07)	(.04)
Treated*(3–4) years after treatment	−.085*	.093	.024	−.013	−.041	.170	−.100**
	(.05)	(.08)	(.09)	(.09)	(.11)	(.11)	(.04)
Treated*(5–8) years after treatment	−.253***	.206*	.428**	.157	.117	.481*	−.062
	(.09)	(.11)	(.21)	(.18)	(.25)	(.25)	(.06)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			74.85	28.69	98.73	92.42	4.573

(*continued on next page*)

Table B.5 (*continued*)

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
Adjusted *R*-squared			.7429	.5174	.7556	.7504	.1243
Log-likelihood	−282	−723	−938	−1017	−985	−970	−168
Chi-Square	71	75					
No. of obs.	932	1201	1167	1116	1172	1167	1145

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: The table depicts detailed estimates on pre- and post-differences between firms with innovation loans from Innovation Norway and firms with private bank loans. The *Treated*Before/Treated*After* should be interpreted as the innovation loan firms' deviation from trend at each point in time. For example, the *Treated*Before* ($t-2$) estimate is the estimated difference for the innovation loan firms from the *Before* ($t-2$) estimate. The regression is run on the same sample with the same control variables as the regressions displayed in Table 5. The year before treatment, $t-1$, is the reference year for the *before/after* estimates.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

Table B.6
Survival, growth and profitability of firms with innovation loans compared to firms with venture capital financing: Firm performance over time.

	(1) Survival Coef./SE	(2) Deficit Coef./SE	(3) ln(sales + 2) Coef./SE	(4) log(va + 2) Coef./SE	(5) ln(employees + 1) Coef./SE	(6) ln(assets + 2) Coef./SE	(7) OROA Coef./SE
2 years before treatment		−.127	−.144*	.112	−.166*	−.108**	.080*
		(.11)	(.07)	(.13)	(.09)	(.05)	(.04)
(1–2) years after treatment		.233***	.166*	.092	.299***	.400***	−.124***
		(.09)	(.09)	(.17)	(.08)	(.09)	(.04)
(3–4) years after treatment		.147	.181	.354*	.327***	.453***	−.069
		(.11)	(.13)	(.20)	(.11)	(.15)	(.06)
(5–8) years after treatment		.259**	.559***	.471*	.351	.722***	−.123
		(.13)	(.17)	(.28)	(.26)	(.19)	(.09)
Treated*2 years before treatment		.073	.109	−.126	−.018	−.010	−.072
		(.11)	(.09)	(.12)	(.09)	(.06)	(.06)
Treated*1 year before treatment		.042	.048*	.079	−.006	.021	−.027
		(.09)	(.03)	(.15)	(.04)	(.04)	(.05)
Treated*(1–2) years after treatment	.020	−.252***	.008	.235	−.180*	−.134	.162***
	(.04)	(.09)	(.09)	(.17)	(.10)	(.11)	(.05)
Treated*(3–4) years after treatment	−.122*	−.236**	−.004	−.021	−.304**	−.217	.159***
	(.06)	(.10)	(.14)	(.17)	(.14)	(.15)	(.06)
Treated*(5–8) years after treatment	−.250**	−.346***	−.074	.001	−.266	−.407*	.247**
	(.11)	(.12)	(.22)	(.33)	(.32)	(.23)	(.10)
Estimation method	Probit	Probit	OLS	OLS	OLS	OLS	OLS
F-value			159.2	27.75	97.29	110.4	5.499
Adjusted *R*-squared			.8138	.5497	.7617	.7635	.1502
Log-likelihood	−228	−538	−729	−980	−830	−766	−196
Chi-Square	74	85					
No. of obs.	729	931	931	861	933	931	907

Clustered standard errors (SE) at the firm level are reported in parentheses.
Note: The regression is run on the same sample with the same control variables as the regressions displayed in Table 8. The year before treatment, $t-1$, is the reference year for the *X years before/after treatment* estimates. See Table B.4 for more on the interpretation of the estimated coefficients.
* Significance at 10%.
** Significance at 5%.
*** Significance at 1%.

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