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The long-term effects of R&D subsidies on firm performance: Evidence from a regression discontinuity design

Innovation is the most important factor for economic growth, but the government's ability to affect innovation outcome is contested. This study contributes to the understanding of the long-term effects of one of the most frequently used innovation policy tools, R&D subsidies, for Swedish innovation policy as well as the international research and policy community.

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The long-term effects of R&D subsidies on firm performance: Evidence from a regression discontinuity design

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Abstract

Innovation is the most important factor for long-term economic growth in advanced economies, but the empirical results regarding the effects of the most common innovation policy, namely research and development (R&D) grants, are inconclusive. This paper presents quasi-experimental evidence on the effectiveness of R&D subsidies for small businesses. We use firm level data on Swedish applicants of the EU-financed Eurostars R&D program during the period 2008 to 2019. The program finances international collaborative R&D projects led by small- and medium-sized firms. Subsidies are awarded based on applicants' score points where applicants with scores above a certain threshold receive funding. Through the use of a list in ranking order it is possible to estimate the causal effect on, e.g., the employment and turnover of the subsidies using a sharp regression discontinuity design. Access to panel data on firms' financial information gives us a unique opportunity to evaluate the impact of the program up to 12 years after the granting of subsidies. The empirical analysis shows that these subsidies have a positive and significant effect on turnover, employment, the number of scientific and technology workers, and the propensity to export. The effect is stronger on firms that are expected to be financially constrained, such as small and younger firms. We find that the subsidy effect on turnover, employment and export last for more than 7 years after the end of the project, which is consistent with explanations based on long-term channels such as improved competitiveness through the market introduction of innovations. We show that the main result is robust to alternative model specifications.

1. Introduction

Innovation is the most important factor for long-term economic growth in advanced economies (Romer 1990, Bloom et al 2019). There are theoretical market failure arguments for innovation policy such as the underinvestment in R&D by firms due to the non-rival and non-excludable characteristics of knowledge (Solow 1956, Romer 1986) and the lack of collateral for young as well as innovative firms due to imperfect capital markets (Hall 2002). Governments therefore initiate various policies to correct these failures and increase the production of new knowledge, ideas and ultimately innovative products and services that in turn can increase productivity and social welfare. One of the most prevalent policy-tools is governmental grants for research and development (R&D). Empirical studies, however, show mixed results of governmental R&D-grants, and the evidence is therefore inconclusive.

Becker (2015) and Tillväxtanalys (2020) describe the development of research about R&D-grants. They find that econometric studies throughout the 1990s and 2000s showed various results. Some of these studies found that the public money crowded out private money and therefore had no effect on the innovation output of the participating organizations, while other studies found positive effects of R&D grant programs. Nevertheless, these studies are not experimental and therefore sensitive to potential selection between the treated group of firms and the control group. The treated firms had applied for the R&D grant program, and the control groups were picked on the basis that they shared similar observable traits, such as turnover, sector and so forth. There is, however, a risk that the firms that apply and receive an R&D grant could be different from the control firms in terms of unobserved attributes that are import for firm performance. In such cases, a simple comparison of the outcomes of treated- and control groups could be biased. This risk for selection bias and omitted variable bias has initiated a renewed interest in the effects of R&D-grants using a more careful identification approach for the control group, namely quasi-experimental and experimental research designs (Becker 2015, Bloom et.al 2019, Santoleri et al 2020).

These recent quasi-experimental and experimental studies have used regression discontinuity analysis (Howell 2017, Bruhn & McKenzie 2019, Santoleri et al 2020), natural experiments (Moretti et al 2019) and random control trials (Kleine et al 2022), and they have found that the grants have had significant positive effects on various outcome variables. The quasi-experimental study by Bronzini and Iachini (2014) did not, however, find any overall positive effects on investments, but concluded that the study showed a large heterogeneity where small firms experienced small positive effects. There is a variation in program design, sectors, time period, outcome variables and institutional environment concerning the studied programs, thus there is a need for more experimental studies to increase the evidence of the effects.

The objective of our study is to identify the casual effects of Eurostars[1], a large European collaborative R&D-grant program. Eurostars targets R&D-oriented SMEs, and the grant is conditioned on funding an R&D-project that is close to market entry. Many projects therefore involve both suppliers and customers in a joint international project. We have access to unique data showing us the scores of Swedish applicants to the grant program. The scores render the possibility to set up a regression discontinuity design (RDD). A second objective of the study is to evaluate regression discontinuity as an impact evaluation tool in innovation

[1] Section 3 in this report explains Eurostars and its selection process in detail.

policy. Many R&D support programs use scores to select between applicants. However, it is not frequently used in evaluating industrial policy.

Regression discontinuity design is one of the most credible empirical approaches for causal identification. This approach provides a way of estimating treatment effect in a non-experimental context where treatment is determined based on a continuous score in which only units above a certain threshold point receive a grant. In a sharp RD design, the average treatment effect is estimated by comparing units with score points just above (subsidized) and below (non-subsidized) the cut-off score. The empirical strategy for causal identification utilized the idea that the firms that scored just above the cut-off point are likely to be very similar to the firms with scores just below it. A sufficient condition for causal identification is that it should not be possible for firms to perfectly manipulate their scores around the cut-off point. In our context, we argue this is likely to be satisfied since the cut-off point is determined by the funding agencies depending on the available budget, and thus the cut-offs are not known by the evaluators.

Our data for the empirical analysis come from two sources. Information on the scores of the Swedish applicants is obtained from the EU secretariat at the Eureka Network, which organizes the call for proposals. The selection process at Eureka is conducted in three stages, and the data we have access to include projects that reached the final stage of the application process. These finalists receive scores from Independent Evaluation Panel (IEP) according to a set of criteria classified into two categories, approved and not-approved. Among the approved, the cut-off was set depending on the national budget of each member state. Our second data source is the internal database at Growth Analysis, which consists of micro data on the full scope of economic statistics and the firm population in Sweden. This database is annually updated with data from the structural business survey at Statistics Sweden. The database also consists of government support measures, which enabled us to not only link the specific firm to their specific economic data, but also to the amounts funded by the Swedish Innovation Agency (Vinnova). These datasets have thus provided us with the ability to construct the panel dataset that allowed us to follow the effect from the year of subsidy up to a maximum of 12 years.

The empirical analysis shows that subsidies have a positive and significant effect on turnover, employment, the number of scientific and technology workers, as well as the propensity to export. The effect is stronger for firms that are expected to be financially constrained, such as small and younger firms. We find that the subsidy effect on turnover, employment and export last for more than 7 years after the subsidy, which is consistent with explanations based on long-term channels such as improved competitiveness through the market introduction of innovative products and processes. We show that the main result is robust to alternative model specifications.

The uniqueness of this particular study is the long time series. Many studies only cover a few years after the grant is awarded; instead, this study shows casual effects up to 12 years after. This implies that the effect is due to the additional money for R&D and not a certification effect (see Howell 2017). It also implies that the effects are not only a so-called "sugar rush", which suggests that the long-term effects of R&D grants are minimal. It is moreover a non-sector specific grant, which distinguishes it from some other studies that are sector specific. In addition, it is important to stress that there needs to be many empirical studies before a consensus can be formed around the effects of R&D grants, and we found some heterogeneity among the firms. For example, younger firms are much more susceptible for these grants rather than older firms, which can be due to younger firms also tending to be smaller, something many prior studies have emphasized.

In section 2 we describe the research frontier on the effects of R&D-grants, and in section 3 we describe the Eurostars program and its selection process and provide an overview of the data. Section 4 describes the empirical strategy and present tests of the RD design's validity. Section 5 contains the estimation results, whereas section 6 reports heterogeneous treatment effects. Finally, section 7 shows our robustness checks and section 8 brings the paper to a close.

2. Literature review

In this section, we provide an introduction to the new stream of literature on quasi-experimental and experimental studies of R&D-grants. We look into which data sources, methods, time periods, outcome variables and results are used and discovered.

Bronzini and Iachini (2014) evaluate a multi-sectorial regional R&D subsidy program in northern Italy. They use a sharp regression discontinuity design to compare the investment spending of subsidized firms with that of unsubsidized firms. They find that small enterprises increased their investments by approximately the amount of the subsidy, which implies no crowding-out effect. However, on the overall sample of small and large firms they cannot find any positive effects of the grant on investment.

Howell (2017) conducts a large-sample quasi-experimental evaluation of R&D subsidies. It is a study into one sector, the energy sector, and she uses data on ranked applicants to the US Department of Energy's SBIR grant program. The study finds that an early-stage grant award approximately doubles the probability that a firm receives subsequent venture capital and has large, positive impacts on patenting and revenue. These effects are stronger for more financially constrained firms. In addition, the study finds that certification, i.e., quality signaling, where the award contains information about firm quality, likely does not explain the grant effect. Instead, the grants are useful because they fund technology prototyping.

Santoleri et al (2020) investigate the impact of a European public R&D grant program targeting small- and medium-sized enterprises (i.e., the SME Instrument) on a wide range of firm outcomes. They employ a sharp regression discontinuity design to provide the quasi-experimental evidence on R&D grants over both geographical and sectoral scopes. Results show that grants increase investment and innovation outcomes as measured by cite-weighted patents; they trigger faster growth in assets, employment and revenues; and they lead to higher likelihood of receiving follow-on equity financing and lower failure rates. These effects are larger for firms that are smaller and younger, or operating in sectors characterized by higher financial frictions. Furthermore, responses are stronger in countries and regions with lower economic development. The beneficial effects of R&D grants materialize through funding rather than certification effects.

Bruhn and McKenzie (2019) applied a regression discontinuity analysis and estimate the effects of R&D grants on participating firms in large publicly funded R&D-consortiums. It was concluded that these R&D-consortia depend on public funding because most projects that did not receive funding were cancelled. The grants therefore contributed with more connections between industry and academia, but the study also found an increase in patent applications and research publications due to the grants. The time period was 3-4 years between funding and the end of the study.

In a randomized control trial of innovation vouchers (the small allocation of money for a specific purpose, in this case to initiate cooperation with, e.g., university researchers or other R&D experts) for SMEs, it was found that 80% of the collaboration projects between SMEs and other research organizations depended on the public grant (Cornet et al 2006). In a similar RCT study on innovation vouchers (Kleine et al 2022) in the UK it was found that the innovation voucher program has an immediate, short-term impact on the execution of these innovation projects with positive effects on product and service development, internal processes, and intellectual property protection. However, it was also observed that these results fade out quite quickly, i.e., two years after the intervention many effects caused by the innovation voucher program have disappeared.

A recent study by Moretti et al (2019) uses a natural experiment design by measuring the changes in military R&D spending. These changes are often driven by exogenous political changes, and it therefore possible to estimate the casual effects of the spending on R&D outcomes. They find that a 10 percent increase in publicly-funded R&D to private firms results in a 3 percent increase in private R&D, suggesting that public R&D crowds in private R&D. In addition, the grants raise productivity growth for the beneficiary firms.

In conclusion, the evidence is still scarce but a majority of studies claim significant positive effects. There is a consensus around the heterogeneity of the effects, and it is clear that young and small firms receive the most positive effect. Also, firms in financially constrained sectors seem to gain an extra value from the grant, which is consistent with the theoretical underpinnings of this study area. However, long term effects of R&D grants are not measured by these studies.

3. The R&D grant program Eurostars

Eurostars is a grant program that provides funding and support to organizations collaborating on international R&D projects or wanting to explore international markets. In Sweden it is a funding-only scheme, and there is no business support. The aim of Eurostars is to contribute to European competitiveness, innovation, employment, economic change, sustainable development and environmental protection, and help to achieve EU objectives agreed to in the Treaty of Lisbon. It primarily targets small- and medium sized technology companies (SMEs) that can collaborate with research institutes, large companies, universities and other types of organizations. As a joint program between the EUREKA[2] network and the European Union, Eurostars has had different programs over the years, for example, the Eurostars 2 program, which until 2021 was funded by 36 members[3] countries, and EU Horizon 2020.

In July 2008, the European Parliament and the Council adopted a proposal providing for EU participation in financing the Eurostars Joint Programme ('Eurostars'). The first Eurostars program, Eurostars-1, launched in 2008. Over the period 2008-2013, the estimated public funding was EUR 472 million, giving a proportion of EU funding (EUR 100 million) to national funding (EUR 372 million) of 26.9 %[4]. The second program, Eurostars-2, ran between 2014 and 2020 with a total public budget of 1.14 billion euro and 1098 projects.[5] The private co-investment in R&D is done by the applying organizations, which means that they dedicate part of their R&D budget to this particular project. There are no other external financiers of the projects.

The EUREKA Secretariat governs the implementation of Eurostars. In the member countries, so-called national funding bodies (NFBs) coordinate the program. In Sweden, it is the governmental agency called the Swedish Innovation Agency (Vinnova). The Secretariat organizes calls for proposals, verifies the eligibility of applications and selects projects for funding. It is also responsible for allocating the EU financial contribution. In turn, the national funding bodies earmark the national contributions to Eurostars in their R&D budgets and thus finance their national participants.

The aim of the program is achieved by promoting innovation activities and international cooperation by enabling SMEs to conduct joint research and innovation. The program, up until Eurostars-2, was market oriented, and product market introduction was expected within two years after project completion. Eurostars projects are bottom-up, meaning that the applicants can innovate across all technological areas. However, the national funds could be directed at certain innovation areas. Eurostars is a competitive program where R&D projects are evaluated based on their potential and not by their field of technology.

There are eight criteria upon which the program, i.e., Eurostars-1 and Eurostars-2, is based. It is collaborative and international; in any project there should be at least two partners (firms, universities, research institutes or other types of organizations) from two different participating states. It is SME-oriented, meaning at least one partner should be a R&D-performing SME. In addition, it is market-oriented since they must have a maximum duration of three years, and within two years of project completion

[2] Eureka was established in 1985 as international non-profit legal entity based in Belgium. It was initially an agreement between 18 countries and the European Commission to foster competitiveness and market integration and to encourage R&D cooperation.

[3] 36 members under Eurostars-2 and 37 under Eurostars-3 (Singapore joins the program). There are three editions of Eurostars and if a specific edition is not mentioned we refer to the Eurostar program as a whole.

[4] https://www.eumonitor.eu/9353000/1/j9vvik7m1c3gyxp/vjxz781j7uyx

[5] https://www.era-learn.eu/network-information/networks/eurostars-2

the product of the research should be ready for launch into the market. The project leader must be an SME, but large firms, universities, research institutes and any other type of organization may participate as project members. Other criteria relate to the program having to fit into the relevant national research and innovation programs.

Vinnova covers up to 50% of the project costs for SMEs, i.e., the private firm has to show that it will also use their own resources for the project. The maximum funding amount is a grant of SEK 5 million per project. Vinnova covers up to 30% of project costs for large companies. The maximum funding amount is a grant of SEK 2 million per project or a grant of SEK 5 million per project if there is a Swedish SME in the consortium. Vinnova covers up to 70% of project costs for universities or research organizations.[6]

Swedish companies must be registered as a limited company (aktiebolag) in Sweden and must have a permanent establishment there. Project activities must be conducted at sites that belong to a participating Swedish company. Furthermore, project costs must belong to the company and must have submitted in at least two annual reports to the Swedish Companies Registration Office. The most recent annual report/ financial statement should show that net sales or equity correspond to at least half of the amount of funding they are applying for.[7]

3.1 The selection processes

The selection processes are made in different stages and by different organizations in Eurostars-1 and Eurostars-2.

1. Selection based on formal and financial conditions. First the applicants are selected based on an eligibility check. All applications are reviewed and those applications that do not meet the formal conditions are sorted out.[8] In addition, the applicants are assessed by their financial status.[9] The national organizations support the Eureka Secretariat in checking the eligibility criteria while they are also responsible for the financial viability assessments. Vinnova checks the applicants credit ratings as well as determines that it is a Swedish registered company.

In the eligibility check the applicants must demonstrate that they have resources to co-fund their part of the project. Five financial ratios are used: Solvency, Liquidity, Net and Gross Profitability and Financial Autonomy. It is only the two most recently closed financial years that are assessed. For start-ups, since in most cases there are no objective financial ratios available yet, these organizations usually cannot provide the information requested in this check. They are strongly advised to get in contact with Vinnova to check their financial viability for funding.

2. Evaluation stage 1, expert evaluation individually. Each eligible application is assessed by three independent experts commissioned by the Eureka Secretariat. Each expert is matched to an application based on their technical expertise. Experts are of different nationality in relation to the home countries of the participating consortia, and they evaluate the projects according to set criteria. Each project is evaluated on a scale from 1 to 6 in 3 categories and several subcategories. [10] Under Eurostars-2 proposals needed to pass a certain threshold to reach the next phase.

[6] https://www.eurekanetwork.org/countries/sweden/eurostars/funding
[7] https://www.eurekanetwork.org/countries/sweden/eurostars/eligibility
[8] Eurostars Funding excellence in innovation.pdf
[9] Eurostars Financial Viability Guidelines for companies.pdf (eurekanetwork.org).
[10] Eurostars_Guidelines_for_Experts_20140226.indd (eurekanetwork.org)

Each expert assigns a score from 1 to 6 to each sub-criterion, where 1 is the lowest score and 6 is the highest. Experts perform their evaluations independently and give unique justifications and commentary. The scores of the sub-criteria are averaged to provide the score for the main criterion. Averages that result in fractional parts of 0.5 and above are rounded up to the nearest integer. Fractional parts lower than 0.5 are rounded down to the nearest integer. As there are three experts rating three criteria, nine values between 1 and 6 are generated.

The average value for each of the main criterion is calculated from the previously calculated scores. The average value for all criteria must be greater than or equal to 3.6, otherwise the application is rejected. In this way a threshold is established so that only the best, most competitive projects (those with the highest total scores) progress to the review by the Independent Evaluation Panel, with the remaining being removed from the process.

- In criterion 1, it is the quality and efficiency of the implementation that is assessed. The sub-criteria are the:
    - quality of the consortium,
    - added value through cooperation,
    - realistic and clearly-defined project management & planning, and
    - reasonable cost structure.
- In criterion 2, it is the potential impact of the R&D-project that is assessed. This was divided into the following sub-criteria:
    - market size,
    - market access and risk,
    - competitive advantage,
    - clear and realistic commercialization plans, and 5) economic, environmental and societal impact.
- In criterion 3, it is the excellence of the R&D-project that is assessed. The sub-criteria are:
    - degree of innovation,
    - new applied knowledge,
    - level of technical challenge, and
    - technical achievability and risk.

3. Evaluation stage 2, expert committee evaluation of the finalists. In the second stage of the evaluation process, each finalist application is evaluated by the Independent Evaluation Panel (IEP). The evaluation panel consists of experts appointed by the member countries and accepted by the High-level Group (the highest decision-making body in Eureka). The composition of the panel changes over time to ensure adequate coverage of technical and market fields and to ensure representation of a variety of countries that participate in the program. The number of selected experts depends on the number of applications that need to be evaluated. Normally 10 to 14 experts but it can be as many as 20. The IEP assesses each project and give it a score, each category up to 200 points, and the maximum score for a project is therefore 600 points. The project is rejected if it receives less than 120 points in one category or less than 402 points altogether.

4. Evaluation scores are sent to member countries. The list is sent to each member country, and the countries must follow the ranking list and finance the project from the highest rank and downwards until the limits of the national budget. This means that some of the final projects are below the minimum score for funding and thus not financed. However, there is also a group of applicants that are above the minimum

score and thus accepted for funding, but depending on the budget constraints this group will not receive funding. Lastly, there is a group which is above the minimum score and receives funding.

5. The projects run between 1 to 3 years. Projects can ask for extension on their deadlines. If there are good reasons this is often approved. It is often only extended by a few months according to Vinnova. There is no follow-up funding for Eurostars projects. In some of these schemes there are possibilities to apply for further funding but that is not the case in Eurostars.

3.2 Eurostars Sweden in numbers

Our dataset from the secretariat at the Eureka Network contains projects that reached to the second stage of the evaluation process, henceforth finalists. This means that our dataset does not include applications that are rejected by individual experts at the first stage. The number of applicants has steadily increased and in 2019 it was 121 applicants and out of them 63 became finalists. The dataset includes the name of the projects, the scores from the expert committee evaluation and the names of the firms leading the projects from the calls over the period 2008-2019. One year, 2013, is however missing due to technical reasons.

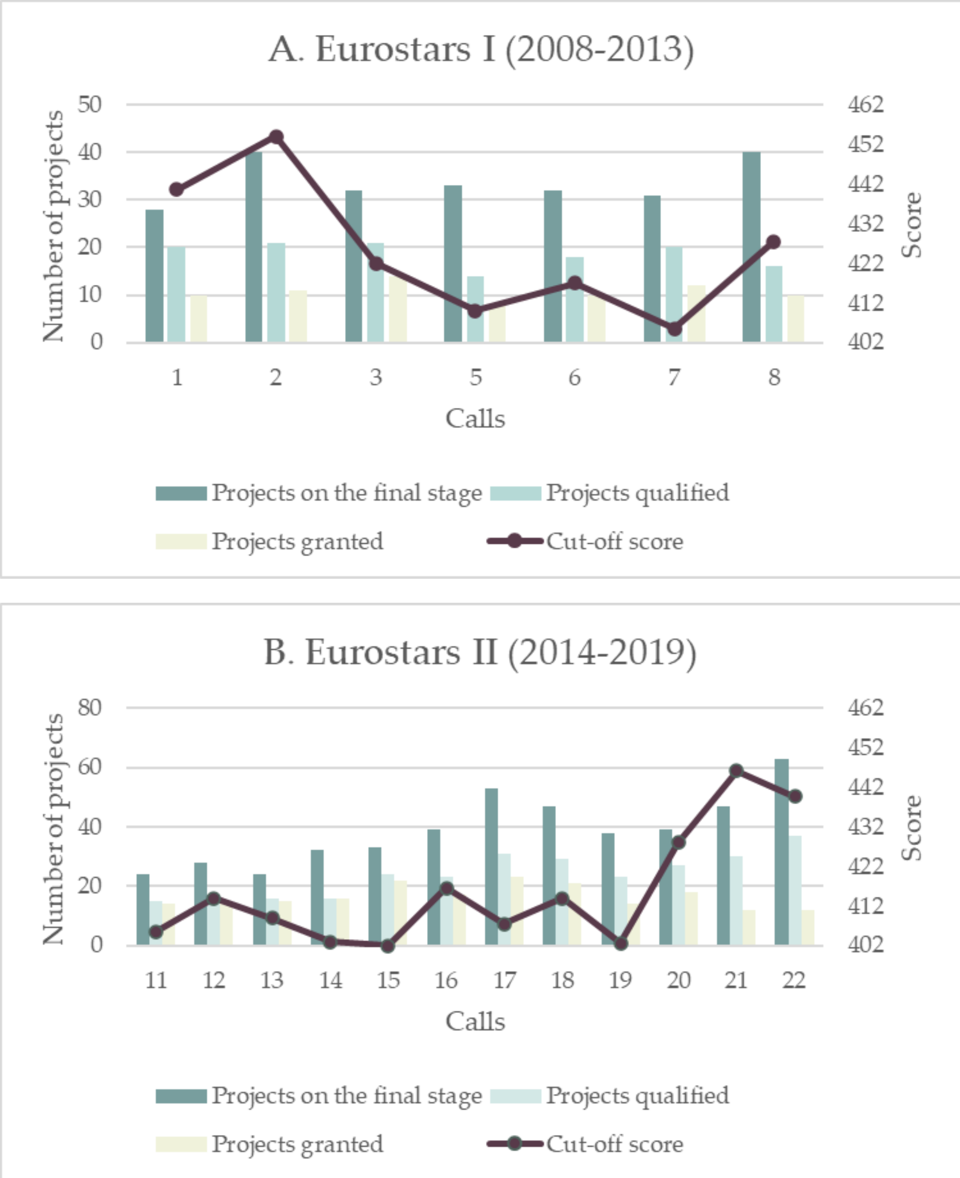
Since the inception of the Eurostars program, Swedish firms have increasingly applied to the program in collaboration with partners. From 34 projects in 2008 (call 1), the number of projects applied for has risen to 121 in 2019 (call 21 and 22) according to the Eureka website.

The projects – those successful and those rejected

Figure 1 shows the total number of projects at the final stage of the evaluation process, projects that are qualified for a grant (i.e., those with a score of 402 or above) and projects that received a grant and the minimum score point for receiving grant. Over the period 2008-2019 703 projects (excluding 2013) reached the final stage of the evaluation process, of which 59 percent were deemed to qualify for grant. As mentioned above, the fact that a project qualifies for grant award does not automatically mean that the projects receive funding. As described in the description of the selection process (Section 3.1), the most common reasons for failing to receive the subsidy are as follows: i) a limited budget in one of the Eurostar’s partner funding agencies (in our case the national budget allocated by Vinnova), and ii) a failure to satisfy one of the three minimum criteria for subsidy (Quality and efficiency, Impact and Excellence). Thus, depending on the number of qualified projects and the available funding, the minimum cut-off score among subsidized projects varies from call to call. Figure 2 shows the dynamics of the minimum cut-off scores staring from call 1 to call 22. The cut-off points range from the lowest cut-off score 402 points in call 15 to the highest cut-off score 454 points in call 2. On average, over the period 2008-2019, 65 percent of those that qualify for a grant (i.e., those with 402 points or above) received funding in the end.

It is not uncommon that one firm applies for more than one project during the same program period. In some extreme cases, a few firms have participated in up to 11 different project applications in one year and successfully received funding for 2-5 projects.

Figure 1. Number of projects in the final selection stage



Note: The sample in the above figure is based on projects in the second stage of the application process. Information for call 9 and 10 (year 2013) is missing from the time series due to a lack of data. The figures corresponding to the bar charts is reported on the left y-axis, while the figures for the line chart are shown on the right y-axis.

Participating firms

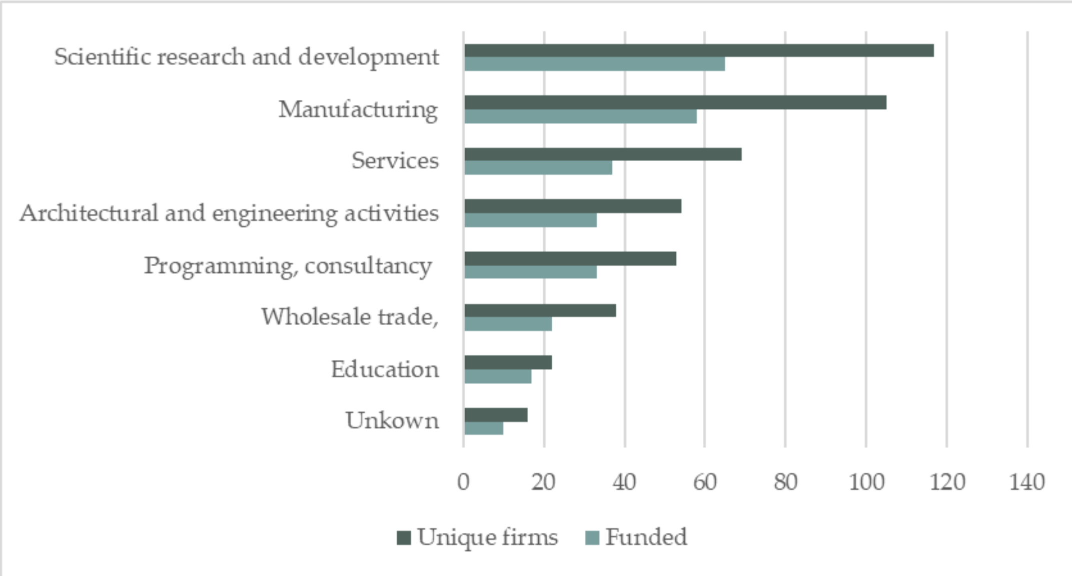
In our dataset, we have identified 487 unique firms of which we keep 474 for the initial description in this chapter. Our main interest lies in identifying whether there are any effects on private businesses due to the subsidies provided. As such we exclude government related institutes from our analysis below. The institutes constitute just below 3 percent of the number of employees and less than 1 percent of turnover in our population of firms (i.e., all applicants). It is important to note that in our analysis described in chapter 4 additional restrictions are made concerning our data material.

Figure 2 below visualizes the industry groups of the firms as opposed to the number of projects in Figure 1. It describes in descending order the total number of unique firms applying for funding over the period 2008-2019, those receiving funding and which industry group they are identified with.

Most firms are identified with scientific research and development and manufacturing comes a close second. Interestingly, over the period 2008-2019 there was an almost equal number of firms receiving funding between these two industries, even though more firms in the scientific sector applied for grants. They received approximately SEK 215 million (scientific research) and SEK 200 million (manufacturing) each.

Within education, larger medical academic research universities and other universities located throughout Sweden have applied to the program. They are on average more successful in their applications compared to other firms. Between 2008-2019 they received a total of SEK 232 million.

Figure 2. Total number of firms applying and those receiving funding by industry (NACE11)

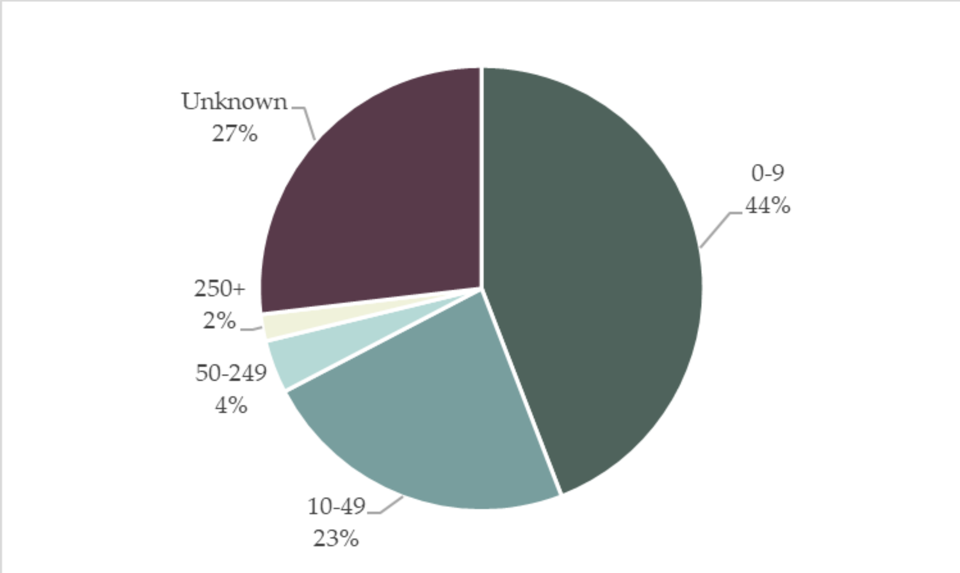


Size of the firms and grants paid

The Eurostars program is aimed at small- and medium-sized companies, but larger firms can participate as partners. According to our data, approximately 3 percent of the number of companies receiving funding are large companies, i.e., they employ more than 250 persons, see Figure 3. The majority of these belong to the education sector thus consisting of universities and medical academic research facilities. Worth noting is that the projects are applied for through various departments and are most likely seen as separate entities within the Eurostars program. As we link the dataset to the organization number and match up with our registers on employment, we see the result of the entire university staff.

[11] Harmonised European statistical classification, in Swedish Svensk näringsgrensindelning.

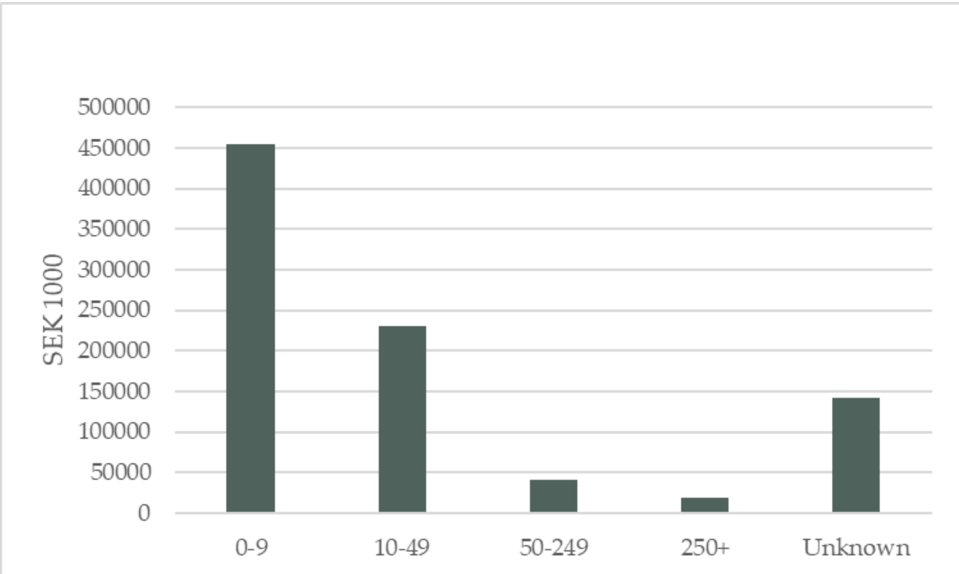
Figure 3. Percent of firms receiving funding, size by employment, 2008-2019



Noteworthy in Figure 3 is that 27 percent of the firms that receive funding cannot be matched up with our economic data. These are firms mostly within the education industry. The reason we do not have access to their economic data is due to the statistical system in Sweden. The authority collecting the financial statistics on these firms does not submit these to Statistics Sweden, and it is they who provide access to us.

Over the period 2008-2019, our data indicate that Vinnova financed projects within Eurostars were worth just below SEK 900 million. The overall majority went to small- and medium-sized firms.

Figure 4. Funding received from Vinnova by size of firm, 2008-2019



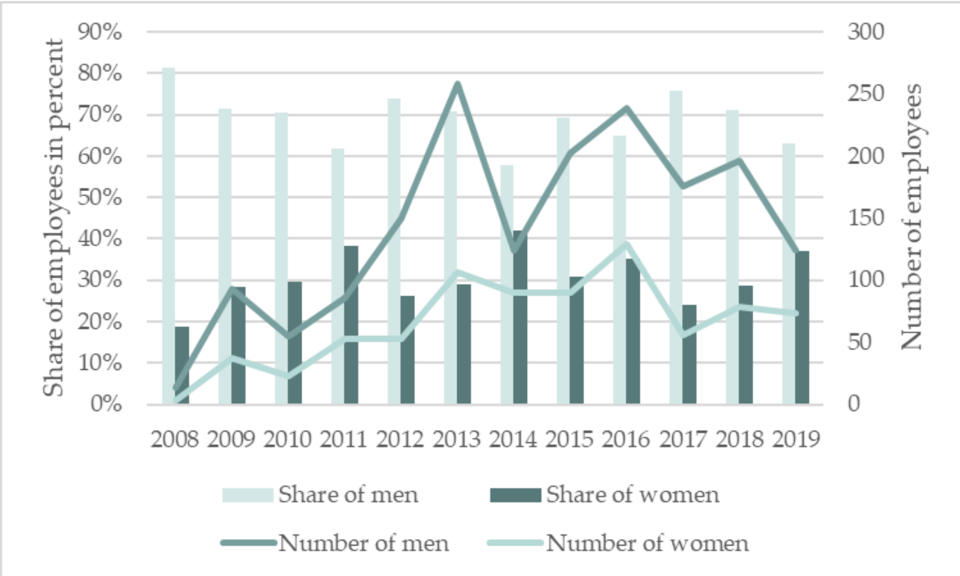
The firms gender structure

In order to finance the up-and coming projects, Vinnova asks the applicants for a gender equality plan. They thus need to describe how gender equality is integrated into the project group composition and in the intended result of the project.[12] This was not part of the Eurostars-1 and 2 programs, but we nevertheless look into this aspect to shed light on the situation as it was.

Thus, we examine the structures of our target group, the SMEs (0-49 employees), in terms of employment by gender. Our data indicate that the firms receiving funding employ more men than women (the lines in Figure 6). This does not differ from the unsuccessful firms; they also employ more men than women. Interestingly, the number of men employed in the firms of interest have plummeted in recent years, and the number of men and women are converging more and more.

The share of employees has varied over the years (the bars in Figure 6) with an incremental increase. Overall, the share has on average remained at a 70/30 share.

Figure 5. Number of employed men and women within the SME firms receiving funding



Note: The figure excludes government institutes

[12] https://www.vinnova.se/en/calls-for-proposals/eurostars-cut-off-3/eurostars-fall-2022/

4. Empirical Strategy

4.1 Identification strategy

As mentioned above, the objective of this study is to provide causal evidence on the impact of research and development subsidies on firm performance, in our case the Eurostars program for Swedish companies.

Like most intervention programs in the social sciences, the allocation of Eurostars research and development support is non-random. Ideally, identification of effects on growth and employment, which we are looking for, would be simpler if innovation subsidies were randomly assigned across firms. Randomization ensures that every firm has an equal chance of treatment and thus generates a comparison group that is similar in many dimensions except for the treatment status. In the absence of the randomization of subsidies, the main concern is that subsidized firms could be different from non-subsidized firms on unobservable attributes that are correlated with the outcome of interest. For instance, innovation support programs tend to be competitive, and only the best firms are subsidized, using criterion that may or may not be observable to the researcher. In this context, failure to account for such unobserved confounders may lead to a bias on the estimated parameters, a classical omitted variable bias.

To address the potential problems with omitted variable bias we employ a quasi-experimental method[13] based on a regression discontinuity (henceforth RD) design. This approach provides a way of estimating a treatment effect in a non-experimental context where treatment is determined based on continuous score points (also known as running variables) in which only units above a certain cut-off point receive treatment.
The empirical strategy for causal identification utilizes the idea that, although score points are not randomly awarded, under certain conditions whether a unit receives a score point just below or above the cut-off point can be viewed as if they are randomly assigned. That is, units with score points just below the cut-off (non-treated) can be a good comparison for units with scores just above the cut-off (treated). A sufficient condition for identification is that it should not be possible for units to perfectly manipulate their scores around the cut-off point. In the absence of the perfect manipulation of scores, treatment effect is obtained by comparing outcomes of units just above and below the cut-off score points (see Imbens and Lemieux (2008), Lee and Lemiuex (2010), DiNardo and Lee (2011) and Cattaneo, Idrobo & Titiunik (2020) for a review of the literature).

The Eurostars and an RD design

This study employs an RD design by exploiting the selection criteria for Eurostars where the decision to award the grant (treatment) is a deterministic function of the applicants' score point that is awarded by an independent external review panel (score points range between 0-600). Applications with minimum qualifying score points, i.e., 402 or above, are ranked according to their score points and granted funding, in descending order, until the budget is depleted or all firms above the threshold have received funding. This means that the minimum score point for a grant is not the same for every call, and it varies depending on the number of qualified applicants and the available budget per call. For the empirical analysis, the score points are normalized to zero at the midpoint[14]

[14] The midpoint is obtained by taking the average of the maximum score point for the non-subsidized firms and the minimum score point for the subsidized firms per call.

between the maximum score point for non-subsidized firms and the minimum score point for the subsidized ones within each call.[15] The selection criteria described above allow us to employ a regression discontinuity design where the treatment effect is identified by comparing the outcome of firms with score points just above cut-off point (subsidized) and firms with score points just below the cut-off point (non-subsidized).

As discussed in the description of the selection process (Section 3.1), not all firms above the threshold received funding in the end. This has implications for the RD design since passing the minimum threshold does not necessarily lead to a discontinuous jump in the probability of treatment from 0 to 1. In this context, one option is to use a fuzzy RD design, which allows for smaller jump. Another alternative is to implement a sharp RD design after the exclusion of projects that did not meet the selection criteria (also known as non-compliers). In this study, we consider the latter option due to the limited number of non-compliance (less than 5 percent) and its simplicity. Most importantly, the reason for non-compliance is largely due to factors outside the control of the applicants (i.e., sample selection is not a major concern in this context). As will be shown latter, in the robustness section, we find a similar result when using a fuzzy RD design.

4.2 Implementation of RD Design

An important feature of RD design is that treatment effects are identified at/near the cut-off point (Cattaneo, Idrobo & Titiunik, 2020). A practical challenge for researchers is that the number of observations near the cut-off point tend to be few (Green, Leong, Kern, Gerber & Larimer 2009). Although relying on observations near the cut-off reduces the risk of bias, it increases the variability of the estimates, making the result less informative. Thus, in practice one must rely on observations further away from the cut-off. The risk, however, is that this may introduce bias if treated firms further away from the cut-off point differ from non-treated firms in ways that are correlated with the outcome of interest. To account for such bias, a common approach is to control the polynomial function of the running variable. An alternative approach is to use a local polynomial model (Cattaneo, Idrobo & Titiunik, 2020). This approach uses observations within a narrowly defined bandwidth around the cut-off (i.e., excluding observation further away from the cut-off), while at the same time controlling for the polynomial function of the running variable. Unlike the model that uses the full population, this approach is less sensitive to the choice of the polynomial functions.

In general, the implementation of RD design often requires the choice of bandwidth as well as the order of polynomial function. However, due to the difficulty of knowing the correct functional form and the bandwidth, the practice in the literature is to present results using alternative bandwidth choices and order of polynomials. In this study, we follow the literature and present results using alternative bandwidth choices and polynomial orders.

[15] A natural alternative is to normalize the scores at the cut-off point, i.e., the minimum score values for the treated, so that the scores for every call will have a value of zero exactly at the cut-off point. However, given the small number of observations per call, requiring all calls to have a value of zero at the cut-off point will distort the score distribution by increasing the number of observations just at the cut-off point. Similar issue arises if we normalize at the maximum score point among the non-treated firms. This motivates our choice to use the midpoint between the maximum score for the non-treated and the minimum for the treated within calls (See Fort et al., 2022 for more discussion on the problem and solutions).

To investigate the impact of R&D subsidy, we start by estimating the following equation using a pooled sample of firms for all post-subsidy years:

$$Y_{it} = \alpha + \beta Subisdy_i + f(Score_i) + Z_{ti}\delta + \varepsilon_{it}, \quad (1)$$
$$\text{For } -h \leq Score_i \leq +h$$

where Y_{it} represent the outcomes of interest for firm i and post-subsidy decision year t. The outcome variables that are investigated in this study are log turnover, employment, the number of science and technical (henceforth S&T) workers and export. The variable $Score_i$ represents the score points awarded by external evaluators, where the points are normalized to zero at the cut-off point for every call. The main variable of interest is $Subsidy_i$, which is a dummy variable that takes a value of 1 if a firm receives subsidy ($or\ Score_i \geq 0$), otherwise it takes a value of zero ($Score_i < 0$). The expression $f(Score)$ is a polynomial function controlling for the relationship between the outcome variable and score points. To improve the precision of the estimates, our preferred model specification adds controls, denoted by the vector Z_{it}, such as year fixed effect, sector fixed effect (2-digit), firm age, and pre-treatment capital and wage expenditure measured the year before subsidy.[16] The parameter β represents the causal treatment effect. The values -h and +h represent the minimum and maximum score point (bandwidth). We use a triangular kernel weight in all regressions, which assigns a higher weight to observations closer to the cut-off point.

Next, we take advantage of the panel structure of our registered data and estimate the subsidy effect for each year after the subsidy:

$$Y_{it} = \alpha + Subisdy_i * \sum_{j=1}^{12} D_{it}^{j} \beta^{j} + \sum_{j=2}^{12} D_{it}^{j} \mu^{j} + f(Score_i) + Z_{ti}\delta + \varepsilon_{it} \quad (2)$$
$$\text{For } -h \leq Score_i \leq +h$$

The above model is similar to equation 1, with the exception that we now add an interaction variable between the subsidy and the dummies for the number of years since the subsidy decision (year - year of subsidy decision), denoted by D_{it}^{j}. The superscript j represents the number of years since the subsidy decision. Depending on the year of the subsidy, we can observe the effect of the subsidy up to 12 years. The main parameters of interest are β^{j}, representing the causal treatment effect by the years since the subsidy decision, i.e., $for\ j = 1,2 \ldots 12$.

An important assumption for the identification of treatment effect in the context of RD design is that it should not be possible for firms to precisely manipulate the score points near the cut-off. Although it should be possible for firms to improve their score point, e.g., by writing a better-quality proposal, it should not be possible for firms to precisely determine the score points awarded by the external reviewers. We argue that the above assumption is likely to be satisfied in our case since the exact cut-off point is not known by both evaluators and applicants before grant award. As discussed above, the exact cut-off point is determined based on the number of applicants with a score above 402 points and the available budget for each application round. In addition, it is very difficult to imagine applicants perfectly manipulating the score points as the reviewers are expected to be independent. In section 4.4, we will provide indirect tests for manipulation.

[16] As will be shown later, excluding the above controls does not affect the main result, except the precision of our estimates.

4.3 Further data development for our analysis

As described in section 4, we have access to data regarding subsidized and non-subsidized Swedish firms for every call during 2008-2019. An important feature of our data is that we have access to the score points awarded by external reviewers as well as the minimum score among awarded firms. In addition, we can link this dataset with various firm level administrative registered data sources, which is our main source of information on variables such as turnover, employment, firm age, industry classification, capital, total wage expenditure and others. Through this data, we can follow every firm from the year of the subsidy decision up to 2019. This makes it possible to evaluate the impact of the program both in the short and long-run, i.e., up to 12 years after the subsidy.

As noted above, over the last two decades, it has not been uncommon for firms to make several attempts to receive funding or receive more than one grant at a different point in time with another project application. To avoid contamination effects from past grant awards, this study focuses on the impact of the first subsidy award. For instance, if a firm received a grant in 2008 and 2014, then we consider only the year 2008 as the year of subsidy. Similarly, we consider the first application year for the non-subsidized firms as the year of a failed application.[17] Restricting the sample to the first-time subsidy awarded and non-awarded firms leave us with a sample of 462, out of which 282 are subsidized firms.[18]

To the population of first-time subsidy awarded and non-awarded firms, we make the following restrictions. First, we exclude firms with no financial data and firms that are state-owned (54 firms). Second, for reasons discussed in Section 4.1, we exclude 22 firms with score points above the minimum threshold but did not receive a subsidy. Third, we restrict our analysis to small firms, i.e., with pre-treatment turnover below 100 million kronor and less than 50 employees, which constitutes 89 percent of the sample. This restriction is necessary to avoid the possibility of the results being driven by outliers. This is especially important in an RD design where identification relies on few observations near the cut-off.[19] Finally, from the distribution of firms by score points, we note that 95 percent of the firms in our sample have scores within a range of 110 points below or above the cut-off (See Appendix Figure A1), while the rest of 5% have scores outside the above range and mostly skewed to the left of the distribution. To maintain a balance in the left and right of side of the score distribution, we restrict the analysis to firms with +/- 110 score points. As a result of the above sample restrictions, our final sample constitutes 212 subsidized and 148 non-subsidized firms. Depending on the year of the first application/award, firms can be observed from the first year of application/award up to a maximum of 12 years. Thus, the panel data constitute 1835 firm-year observations, with 1142 subsidized and 693 non-subsidized firms. The summary statistics for the years after the subsidy decision is presented in Appendix Table A1.

The summary statistics on characteristics of subsidized and non-subsidized firms for the year before a subsidy decision is shown in Table 1. The pre-subsidy characteristics of 25 firms are not reported in the table below due either to the fact that they were newly founded on the year of the subsidy (15 firms) or the information is missing (10 firms). **Error! Reference source**

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[17] In a few cases, firms win their first award in their second or further attempts. In this case, we allow such firms to be part of the control group from the year of the first application up to the year before their first grant award. Starting from the first grant year, they become part of the treatment group.

[18] We will show that the main result is robust when excluding firms with multiple subsidy awards.

[19] In the Robustness section, we will present the result by relaxing the restriction on net turnover to 500 million and employment to 250 employees. The main results are not affected by our choice of size restriction, except its effect on the precision of the estimates.

not found. shows that subsidized and non-subsidized firms are similar in several of the pre-subsidy characteristics such as the number of employees, turnover, age, number of scientific and technical workers, share of firms in the manufacturing sector, total wage expenditure and capital. For instance, the average number of employees is 8 for both subsidized and non-subsidized firms. Almost half of the employees in both subsidized and non-subsidized firms can be classified as scientific and technical workers. We also see that the two groups are similar in terms of age, with an average of 8 years. On average, the pre-treatment turnover for non-subsidized firms is slightly higher than subsidized firms, but this difference is not statistically significant.

A possible reason for such similarity between subsidized and non-subsidized firms could be that the firms that are promoted to the final stage of the application process are highly competitive, and the differences in the awarded score points may not predict real differences. This is also reflected in the distribution of the awarded score points (Figure 6), where about 40 percent of the firms lie within +/- 30 points and about 70 percent within +/- 60 points.

Table 1. Summary statistics in the year before the subsidy decision

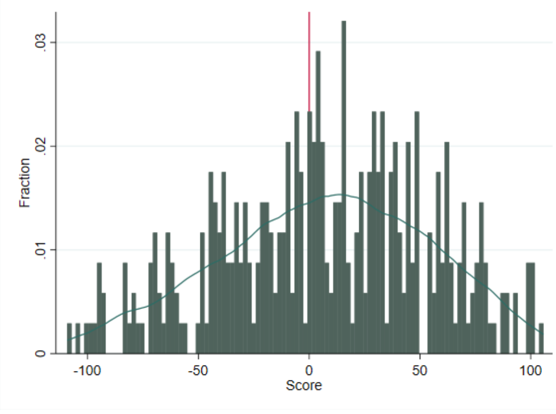
	Non-subsidized	**Subsidized**
	Mean (SD)	Mean (SD)
Number of employees	8.24	8.26
	(9.38)	(9.49)
Scientific and Technical workers	4.12	3.91
	(5.35)	(5.25)
Age	9.67	9.45
	(8.17)	(8.19)
Turnover (log)	6.81	6.71
	(3.25)	(3.59)
Share of exporting firms (%)	3.62	3.55
	(18.7)	(18.6)
Share in manufacturing sector (%)	19.6	23.3
	(39.8)	(42.4)
Wage expenditure (log)	6.71	6.77
	(2.54)	(2.53)
Capital (log)	3.94	3.82
	(2.91)	(2.97)
Observation	138	197

4.4 Test for manipulation

Although it is not possible to directly test the assumption for no manipulation of scores, the literature provides indirect ways of testing the above assumption. One way of testing the presence of the manipulation of scores is through the inspection of the distribution of the firms around the cut-off point. A jump in the distribution of firms at the cut-off point can be indicative of the potential manipulation of scores. Figure 6 shows the frequency distribution of firms by normalized score points. A visual inspection of the figure shows no clear shift in the distribution of firms around the cut-off.[20]

[20] McCrary (2008) provides a formal test of manipulation using the density function of the running variable (score). However, such tests are not suitable when the running variable is discrete and the sample size is small.

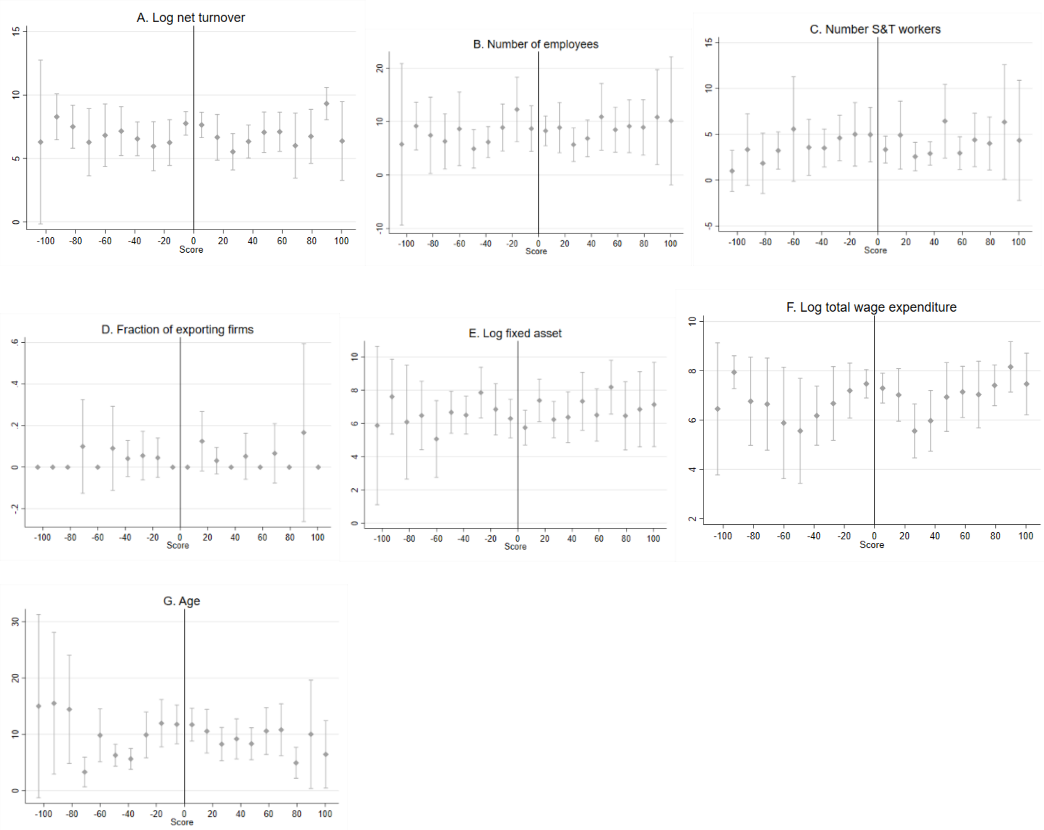
Figure 6. Distribution of firms by score point



Note: The bell-shaped line represents kernel density distribution

Another indirect way of testing for no manipulation is to examine the similarity in pre-treatment characteristics of subsidized and non-subsidized firms near the cut-off areas. The idea is that if the allocation of score near the cut-off is as good as random (no manipulation), then the pre-subsidy outcomes and characteristics of the subsidized and non-subsidized firm should be similar. We test this in two ways. First, we show that there is no significant jump at the cut-off point in terms of pre-determined characteristics such as turnover, employment, number of science and technology workers, fraction of exporting firms, fixed assets, total wage expenditure and age (see Figure 7).

Figure 7. Pre-subsidy decision outcomes and characteristics



Notes: Panel A-G show binned scatter plots of pre-subsidy firm characteristics by normalized scores. The dots represent average values within bins ($1\ bin = 11\ score\ points$) and the vertical lines corresponding to each point is the 95 percent confidence interval.

Next, we present a balancing test by regressing a subsidy dummy (=1 if subsidized, otherwise zero) on pre-subsidy decision outcomes and firm characteristics. Table 2 provides results using observations near the cut-off (with a standardized score of +/- 30 points around the cut-off) and in the full sample (a standardized score of +/- 110 points around the cut-off). We can see that none of the pre-subsidy firm characteristics predict the probability of being treated, when using observation near the cut-off as well as the full sample.

Table 2. Balancing test, using pre-subsidy characteristics

	Dependent variable: Subsidy (1/0)	
	Full sample (+/- 110 points)	Full sample (+/- 110 points)
Variables	(1)	(2)
Number of employees	0.001	-0.003
	(0.005)	(0.008)
Turnover (log)	-0.005	0.016
	(0.011)	(0.017)
Age	-0.001	-0.005
	(0.004)	(0.006)
Manufacturing (1/0)	0.058	0.016
	(0.071)	(0.116)
Total wage expenditure (log)	0.004	-0.012
	(0.025)	(0.039)
Capital (log)	-0.000	-0.007
	(0.010)	(0.015)
Export (1/0)	0.001	0.240
	(0.152)	(0.232)
# of science and technology (S&T) workers	-0.001	-0.007
	(0.008)	(0.011)
Dummy for missing S&T workers (1/0)	-0.020	0.076
	(0.138)	(0.210)
Constant	0.595***	0.680***
	(0.162)	(0.252)
Observations		
R-squared	335	141
Test of join significance	0.004	0.034
F-value	0.13	0.52
P-value	0.999	0.859

Note: Column 1 and 2 report estimated coefficients from the OLS regression of the subsidy dummy on pre-subsidy firm characteristics listed above. The missing observations for S&T workers (53) are replaced by the mean value of S&T workers for the full sample, and we add a dummy taking a value of 1 for missing observations and zero otherwise. Robust standard errors are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

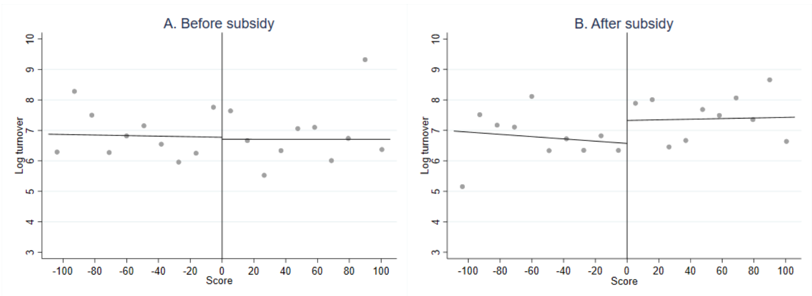
5. Results

We evaluate the effect of the Eurostars program in terms of its contribution to i) firm growth (measured by turnover), ii) job creation (total employment), iii) strengthening the research and innovation capacity of companies (measured by the number of scientific and technical workers), and iv) internationalization (measured by entry into the export market). Sub-section 5.1 presents the results on turnover; Sub-section 5.2 shows result on total employment and the number of scientific and technical workers; and finally Sub-section 5.3 provides result for export.

5.1 Subsidy effect on turnover

Before presenting the regression results, we provide graphical evidence using a binned scatter plot of logarithms of turnover by scores, shown in Figure 8. The score point in the x-axis is normalized to zero at the cut-off point measuring the distance from the cut-off. The firms to the left of the cut-off point (with negative scores) are not subsidized while firms to the right of the cut-off point (with zero or positive scores) are subsidized. The lines that pass through the scatter plots represent fitted linear regression lines estimated separately for the left and right side of the cut-off. For ease of comparison, we present graphs for the year/s before (Panel A) and after (Panel B) the subsidy decision. Before the subsidy, the graph shows no evidence of a discontinuous jump at the cut-off confirming the finding in the previous section. After the subsidy decision, we see that log turnover increases discontinuously when firms cross the minimum threshold for a subsidy. This shows that most of the firms that benefited from a subsidy experience a higher turnover growth.

Figure 8. Log turnover before and after subsidy



Notes: Panel A shows log turnover 1-year before the subsidy decision (N=335). Panel B shows log turnover for a pooled sample of post-subsidy years (N=1823). The dots in the above graph represent local average log turnover within bins (1 bin =11 score points). The average number of observations within bins is 30 in Panel A and 165 in Panel B.

It is interesting to note that the linear regression lines in the above figures are flat, especially for the year before subsidy decision. It indicates that the score points awarded by reviewers have no power in predicting firms' turnover. This motivates our choice of a lower order of polynomials in the following regression analysis. In particular, we consider a model with zero polynomials as our preferred model, although we will also report results using a polynomial order of one and two. In terms of bandwidth choice, we will show results using the full sample (+/- 110 points) as well as results using a narrow bandwidth of +/- 30 points.

The results from the estimation of equation 1 using a pooled sample of the post-subsidy years are reported in Table 3. The estimated coefficients can be interpreted as the average subsidy effect on turnover for the post-subsidy period (on average 4 years after subsidy). Column 1, which is our baseline model specification, presents coefficients from an OLS regression of equation 1 with a polynomial order of zero, i.e., a constant, and by using the full sample with a triangular kernel weight. Although RD design does not rely on model controls for identification, the following covariates are added to improve the precision of the estimates: age, age squared, pre-treatment capital (log), pre-treatment wage expenditure (log), 2-digit industry fixed effect and year fixed effect. The result shows a positive and significant effect of subsidy on firms' turnover, with an estimated effect of 0.7 log points. This means that the average turnover of the firms that receive a subsidy almost doubles during the years after a subsidy.[21] If we compare this with the pre-subsidy median (average) turnover of subsidized firms, the subsidy effect is equivalent to an average increase in turnover by about 1.96 (10.6) million kronor per year. Although the magnitude of the effect appears large, this result should be interpreted in relation to the amount of subsidy the firms received, which is about 2.5 (2.2) million kronor for a median (average) firm.

Column 2 to 6 of Table 3 show estimates using alternative model specification, estimation methods and bandwidth choice. Column 2 shows that dropping the covariates from the model do not change the estimated coefficient. The only consequence is that the coefficients are now less precisely estimated as indicated by the increased standard errors shown in the brackets. Column 3 and 4 present results by adding linear and quadratic polynomial controls. The coefficients become larger for these models, but they are imprecisely estimated. In addition, in line with the graphical evidence, none of the estimates for the linear and quadratic polynomial controls are statistically significant. Column 5 shows that our baseline result is stable when using a Tobit model, while column 6 shows that our baseline result is stable for a narrower bandwidth choice, i.e., a distance of +/-30 points from the cut-off.

Table 3. Effect of subsidy on turnover

	Dependent variable: Log turnover					
	Full sample (+/- 110 points)					Narrow bandwidth (+/- 30)
	OLS				Tobit	OLS
Variables	(1)	(2)	(3)	(4)	(5)	(6)
Subsidy	0.698**	0.673*	0.966*	1.455*	0.742**	1.082**
	(0.318)	(0.397)	(0.531)	(0.741)	(0.344)	(0.506)
Score			-0.013	-0.053		
			(0.009)	(0.034)		
Score*Treated			0.018	0.060		
			(0.012)	(0.041)		
Score squares				-0.000		
				(0.000)		
Score squared*Treated				0.000		
				(0.000)		
Controls	Yes	No	Yes	Yes	Yes	Yes
Pre-subsidy median (average) turnover for subsidized (million kronor)	1.96 (10.6)	1.96 (10.6)	1.96 (10.6)	1.96 (10.6)	1.96 (10.6)	1.96 (10.6)
Observations	1,823	1,823	1,823	1,823	1,823	767

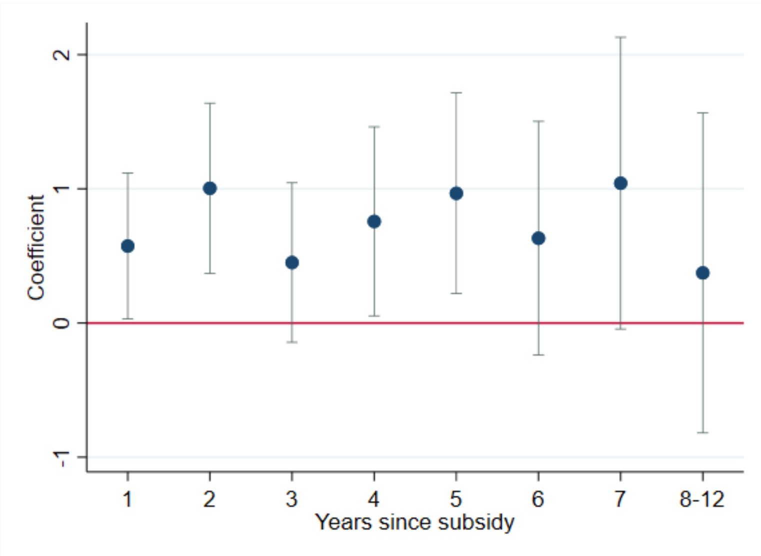
Note: All regressions include triangular kernel weights. Controls are: age, age squared, pre-treatment wage expenditure (log), pre-treatment fixed asset (log), 2-digit industry fixed effect and year fixed effect. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

[21] In terms of percentage, the 0.7 log point increase is equivalent to a 101% increase in in turnover, which is obtained as follows: $(exp(0.7) - 1) * 100 = 101\%$

Figure 9 present results on the effect of a subsidy by years since subsidy decision. That is, we estimate a version of equation 2 but by using our preferred model controls similar to column 1 of Table 4. The figure shows an immediate subsidy effect starting from the year of treatment (around 0.57 log points) and remains at a higher or same level for the years after subsidy.

The immediate effect on turnover is rather unexpected considering the program's expected time laggard for the development and commercialization of new products, processes and services.[22] One explanation that is consistent with a short-term effect is a certification/signaling effect. Firms may receive initial boost in turnover if winning the grant in itself signals firm quality, thereby creating more opportunities in terms new business contracts and attracting external investors (Lerner, 1999). Second, the requirement for international collaboration attached to the subsidy award may also have an immediate effect on turnover if such collaborations create more opportunities to expand the market to other countries or firms. Although the above factors are expected to explain the immediate effect on turnover, it would be too farfetched to imagine them having an effect as far as 7 years after the subsidy. Thus, it is reasonable to expect that the observed pattern arose due to a combination of immediate channels (explained above) followed by long-term channels, such as improved competitiveness through the market introduction of innovate product/process.

Figure 9. Effect of subsidy on turnover, by years since subsidy



Note: The dots in the above figure show the estimated coefficients of the effect the subsidy has on turnover using a model specification similar to equation 2. The model is estimated using OLS after adding baseline model controls and triangular kernel weight. The vertical lines connected to the estimated coefficients represent a 90 percent confidence interval.

5.2 Subsidy effect on employment

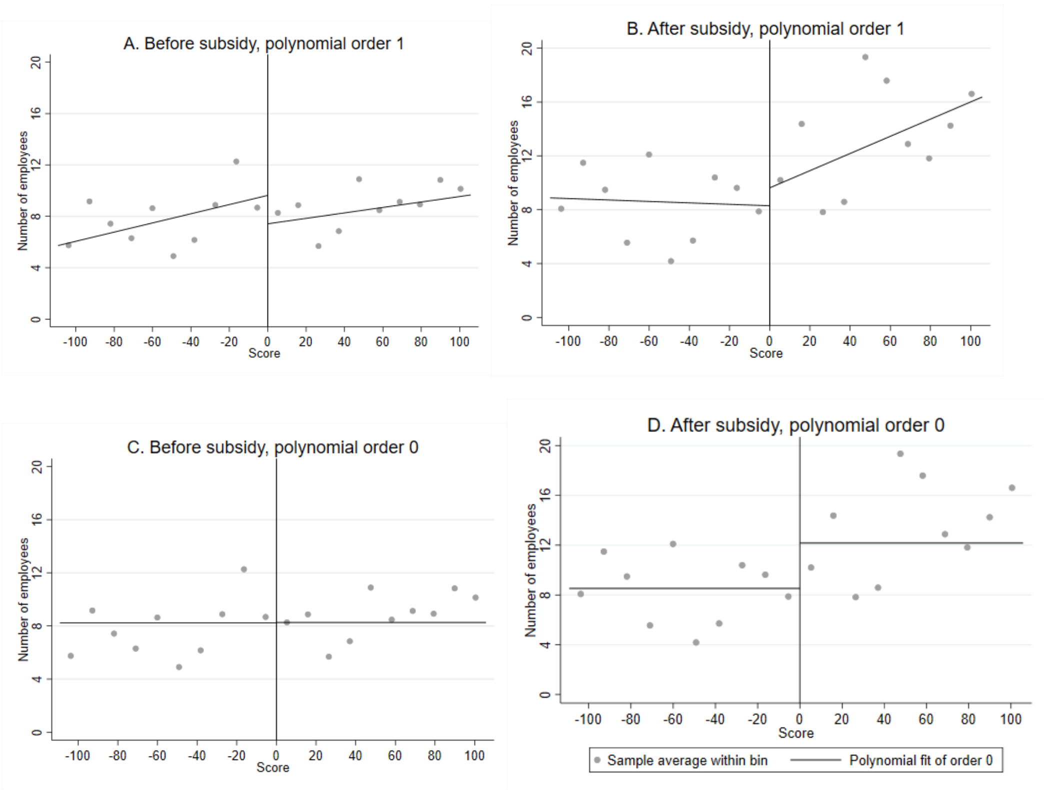
This section investigates the impact a subsidy has on total employment and the number of scientific and technology workers. We start by presenting the result for total employment followed by result on the number of scientific and technology workers.

[22] In robustness section, we show results from estimation of a model similar to a difference in difference, where we estimate equation 2 by adding observations from three years before a subsidy. As shown in Figure 16, we find no significant effect on the year before a subsidy, and the effect only emerges after a subsidy decision.

Subsidy effect on total employment

Figure 10 provides graphical evidence on total employment for the years before and after a subsidy decision. For the year before a subsidy, the fitted regression lines in Panel A show a small drop in the number of workers at the cut-off, although this drop is not statistically significant. This result is also somehow sensitive to the choice of polynomial approximation. For instance, we find no signs of drop in the number of workers when we look at the mean values, i.e., the polynomial order of zero, on the left and right side of the cut-off (Panel C). For the years after a subsidy, we find some indications of an increase in the number of works. Again, it appears to be sensitive to the choice of polynomial approximation. A mean comparison of the subsidized and non-subsidized firms show that employment seems to have increased for most of the subsidized firms (Panel D).

Figure 10. Employment before and after a subsidy



Notes: Panel A and C show the number of employees 1-year before a subsidy decision (N=335). Panel B and D show the number of employees for a pooled sample of post-subsidy years (N=1823). The dots in the above graph represent the local average number of employees within bins (1 bin =11 score points). The average number of observations within a bin is 30 in Panel A and 165 in Panel B.

Table 4 shows the estimated coefficients of the effect of a subsidy on employment. In line with the graphical evidence, the models with no polynomial controls (column 1 and 2) show a positive and significant subsidy effect on employment, with an estimated effect of 3.1. This is equivalent to a 62 (39) percent increase in employment compared to the pre-subsidy median (average) employment among the subsidized firms, i.e., 5 (8) employees. Adding linear and quadratic controls of the running variable, respectively, reduces the estimated effects to 1.7 and 1.5, and they are no longer significant. However, none of the estimates for linear and quadratic controls are statistically significant, indicating the low power of the scores in predicting total employment. This motivates our choice of the model without polynomial controls. The last column shows the result using a narrow bandwidth. We find a subsidy effect of 2.6, but the estimate is not statistically significant.

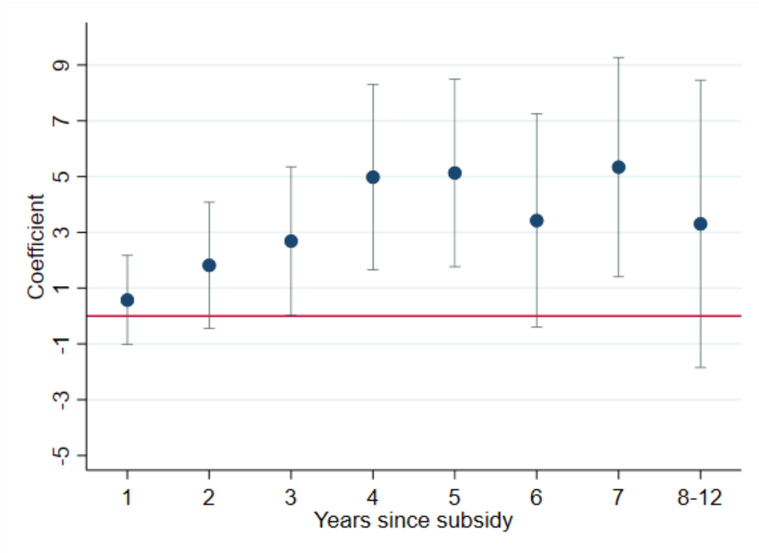
Table 4. Effect of subsidy on employment.

	Dependent Variable: Number of employees				
	Full sample (bandwidth of +/-110)				Narrow bandwidth of +/-30
Variables	(1)	(2)	(3)	(4)	(5)
Subsidy	3.139**	2.992*	1.720	1.469	2.590
	(1.352)	(1.537)	(2.338)	(3.300)	(2.315)
Score			-0.011	-0.079	
			(0.033)	(0.108)	
Score*Treated			0.073	0.245	
			(0.054)	(0.189)	
Score squares				-0.001	
				(0.001)	
Score squared*Treated				-0.001	
				(0.002)	
Controls	Yes	No	Yes	Yes	Yes
Pre-subsidy median (average) employment for subsidized	5 (8)	5 (8)	5 (8)	5 (8)	5 (8)
Observations	1,823	1,823	1,823	1,823	767

Note: All regressions include triangular kernel weights. Controls are: age, age squared, pre-treatment wage expenditure (log), pre-treatment fixed asset (log), 2-digit industry fixed effect and year fixed effect. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure 11 examines how the effect of a subsidy on employment changes by years since subsidy using our preferred model. We find an increasing trend on the effect of a subsidy, where the effect increases from 0.5 employees on the year of a subsidy to a maximum of 5 employees at the 4th year after subsidy. The magnitude of the effect remains at about 3-5 employees for 5-12 years after a subsidy. The presence of a long-term effect on employment indicates that subsidy has an effect beyond the direct effect, i.e., using the subsidy money to finance new recruitment. This finding is in line with explanations based on the long-term channels such as improved competitiveness through the market introduction of innovative products or services.

Figure 11. Effect of a subsidy on employment, by years since the subsidy



Note: The dots in the above figure show the estimated coefficients of the effect a subsidy has on employment using a model specification similar to equation 2. The model is estimated using OLS after adding baseline model controls and triangular kernel weight. The vertical lines connected to the estimated coefficients represent a 90 percent confidence interval.

Subsidy effect on human resources in science and technology

Human resources devoted to science and technology (S&T) is a key ingredient for innovation and new product development. We examine whether subsidies contribute to the development of human capital in science and technology in the short- and long-term.[23] Table 5 shows the estimated coefficient on the impact of a subsidy on the number of science and technology workers. The estimate for our preferred model, column 1, shows that subsidized firms hire about 1.9 more science and technology workers compared to the non-subsidized firms. This is a large increase considering the size of firms in our sample with median and mean S&T workers of 2 and 4, respectively. Column 2 shows that dropping the model controls does not change the main result, except the loss in precision of the estimate. Like the turnover and total employment equations, linear and quadratic polynomial controls of the running variable have no power in predicting employment in scientific and technical activities (see column 3 and 4). The last column shows that the result for our preferred model specification (column 1) is robust to the choice of a narrower bandwidth.

Table 5. Effect of a subsidy on employment in science and technology activities

	Dependent Variable: Number of S&T workers				
	Full sample (bandwidth of +/-110)				Narrow bandwidth of +/-30
Variables	(1)	(2)	(3)	(4)	(5)
Subsidy	1.919***	1.606*	2.067	2.484	2.326*
	(0.697)	(0.820)	(1.315)	(1.841)	(1.350)
Score			-0.002	-0.036	
			(0.015)	(0.053)	
Score*Treated			-0.001	0.036	
			(0.023)	(0.087)	
Score squares				-0.000	
				(0.001)	
Score squared*Treated				0.000	
				(0.001)	
Controls	Yes	No	Yes	Yes	Yes
Pre-subsidy median (average) employment for subsidized	2 (4)	2 (4)	2 (4)	2 (4)	2 (4)
Observations	1,823	1,823	1,823	1,823	767

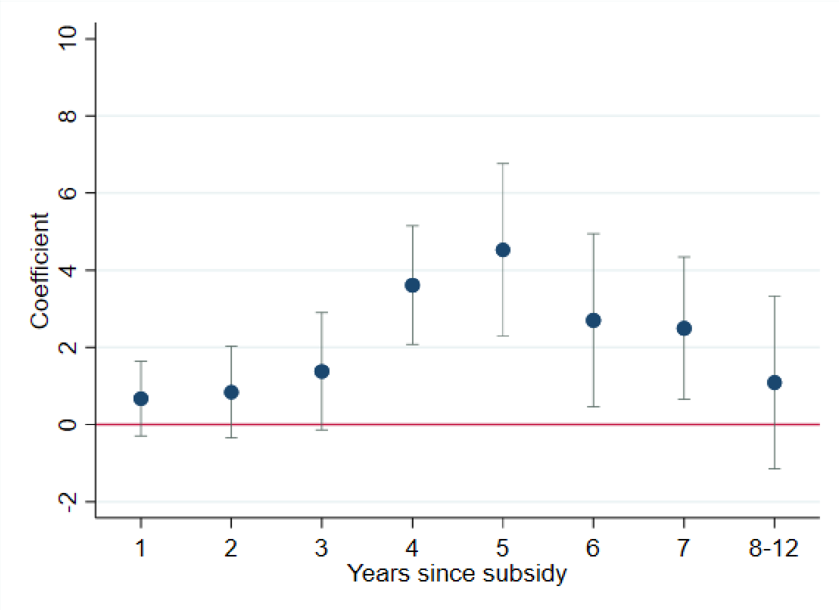
Note: All regressions include triangular kernel weights. Controls are: age, age squared, pre-treatment wage expenditure (log), pre-treatment fixed asset (log), 2-digit industry fixed effect and year fixed effect. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Finally, we present results similar to those found in column 1 of Table 5, but for each year after a subsidy. The estimated coefficients are presented in Figure 12. The figure shows

[23] We follow the definition adopted by Eurostat and classify a person as a scientific and technology worker if the person: i) has a university education and ii) is employed in either "Professionals" (ISCO major group 2) or "Technicians and Associated Professional" (ISCO major group 2).

an inverted U-shaped effect where the maximum effect is observed at the 5th year before it starts to diminish slowly. This pattern may arise, for instance, if the subsidy effect is weaker for those cohorts that can be followed beyond five years after a subsidy (e.g., firms subsidized during 2008-2013, i.e., within the Eurostars 1 program) compared to cohorts that can only be followed for a few years (e.g., firms subsidized during 2014-2019, i.e., within the Eurostars 2 program). However, we found no empirical support for this since a similar inverted U-shape pattern was observed when we restricted the sample to Eurostars 1 only (see Appendix Figure A3). Thus, a likely explanation for the above pattern seems to be a tendency for non-subsidized firms to catch-up with the subsidized firm.

Figure 12. Effect of a subsidy on the number of scientific and technical workers, by years since the subsidy.



Note: The dots in the above figure show the estimated coefficients of the effect a subsidy has on the number of S&T workers using a model specification similar to equation 2. The model is estimated using OLS after adding baseline model controls and triangular kernel weight. The vertical lines connected to the estimated coefficients represent a 90 percent confidence interval.

5.3 Subsidy effect on export

In this section, we evaluate the impact of a subsidy on export performance, which is an important indicator of the innovativeness and international competitiveness of a firm/country. Before presenting the results using the RD design, we provide descriptive evidence on the fraction of firms with a positive export performance and the amount of export by years since the subsidy decision. It is clear from Figure 13 Panel A that a small fraction of applicants, about 4 percent, export their product before the year of the subsidy decision. There is, however, an interesting development over time. The fraction of firms that are exporting has increased over time, and the increase is particularly larger for subsidized firms. A similar pattern can be seen when looking at the export value in Panel B. Considering that there are only a few firms that are exporting and strong variability in export value, in the subsequent analysis, we will only focus on the impact of the program on internationalization, i.e., the probability of exporting. In addition, the ability of a firm to penetrate the export market can be a good indicator of the market introduction of unique products or services.

Figure 13. The fraction of exporting firms and the amount of export

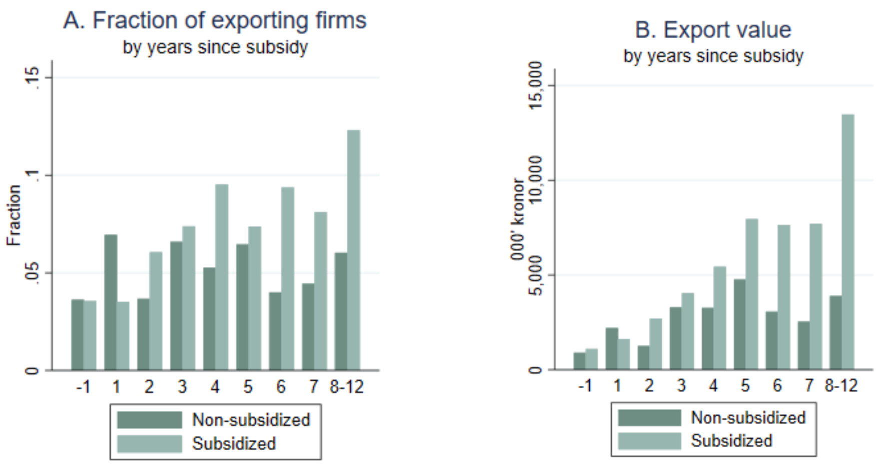
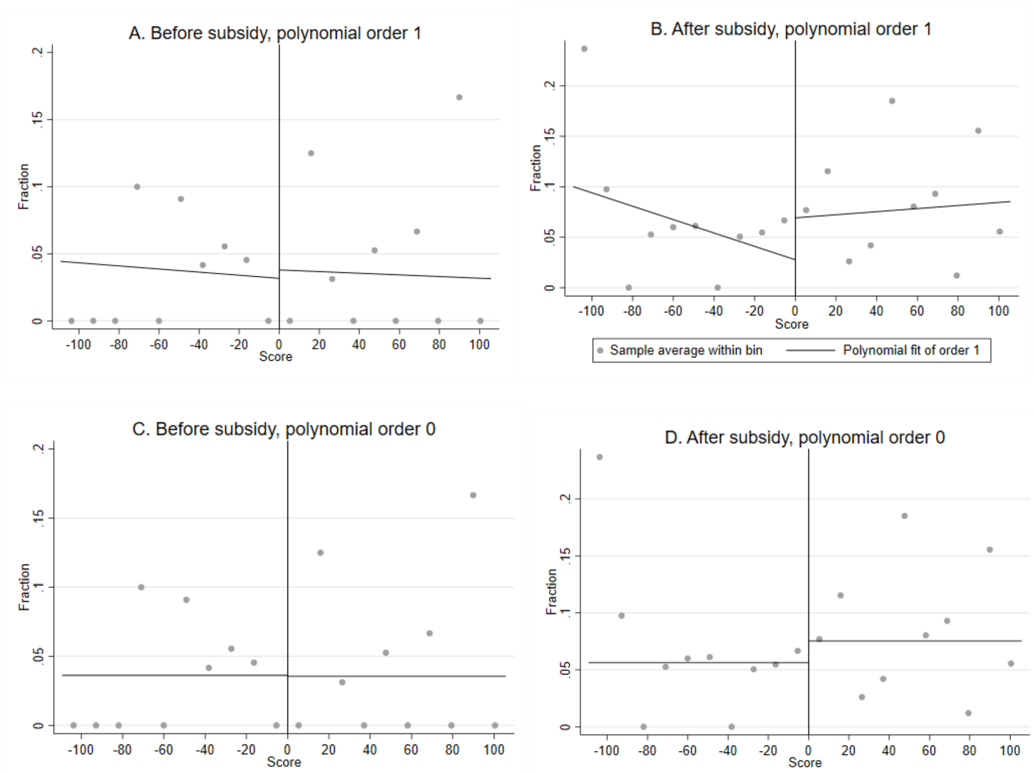


Figure 14 shows the RD graphs on the probability of exporting before and after a subsidy. Panel A and C report the pre-subsidy share of exporting firms by adding polynomial approximation of the order 1 and 0, respectively. Although there is a large variation in the data, the subsidized and non-subsidized firms on average look very similar. Panel B and D show the post-subsidy share of exporting firms for a polynomial order of 1 and 0, respectively. In general, the share of exporting firms increased for both subsidized and non-subsidized firm. There is also evidence, especially in Panel D, that the share of exporting firms among subsidized firms becomes larger than non-subsidized firms, although there is high variability in the data. Like the result for employment, a first order approximation performs poorly and its slope seems to be affected by the right or left extreme values (Panel B).

Figure 14. Fraction of exporting firms before and after a subsidy



Notes: Panel A and C show the fraction of exporting firms 1-year before a subsidy decision (N=335). Panel B and D show the fraction of exporting firms for a pooled sample of post-subsidy years (N=1823). The dots in the above graph represent the local average number of employees within bins (1 bin =11 score points). The average number of observations within a bin is 30 in Panel A and C, and 165 in Panel B and D.

Table 6 investigates whether subsidies increase the probability of firms selling their product in a foreign market. In line with the graphical evidence, we find that subsidies increase the probability of joining the export market by about 2.5 percentage points (see column 1 and 2), but the coefficients are not statistically significant.[24] In line with the results for the other outcome variables, linear and quadratic polynomials have no prediction power (Column 3 and 4). The baseline result is stable when using a narrow bandwidth shown in column 5.

Table 6. Effect of a subsidy on the probability of export

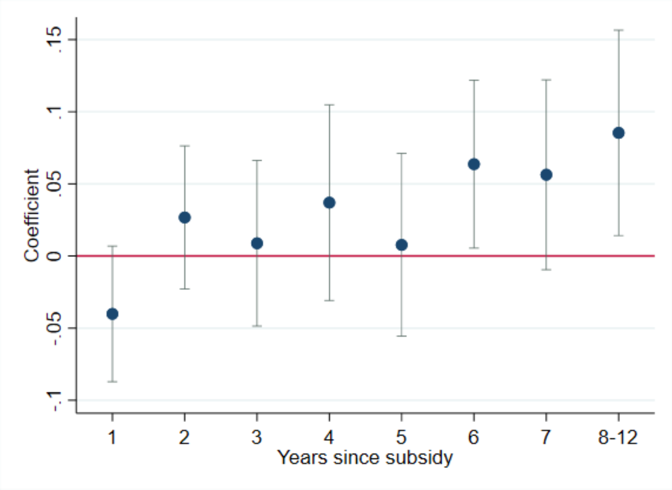
	Dependent Variable: Export dummy (1/0)				
	Full sample (bandwidth of +/-110)				Narrow bandwidth of +/-30
Variables	(1)	(2)	(3)	(4)	(5)
Subsidy	0.025	0.026	0.017	0.001	0.016
	(0.022)	(0.022)	(0.041)	(0.059)	(0.043)
Score			0.000	0.001	
			(0.001)	(0.002)	
Score*Treated			0.000	-0.001	
			(0.001)	(0.003)	
Score squares				0.000	
				(0.000)	
Score squared*Treated				-0.000	
				(0.000)	
Controls	No	Yes	Yes	Yes	Yes
Fraction of exporting firms on the year before subsidy (%)	3.6	3.6	3.6	3.6	3.6
Observations	1,823	1,823	1,823	1,823	767

Note: All regressions include triangular kernel weights. Controls are: age, age squared, pre-treatment wage expenditure (log), pre-treatment fixed asset (log), 2-digit industry fixed effect and year fixed effect. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Estimated coefficients on the effect of a subsidy for every year after a subsidy is reported in Figure 15. Except for the year of subsidy, the estimated coefficients are positive, and the magnitude of the effect increases over time. In contrast to the results for the pooled sample, shown in Table 6, the estimates become statistically significant at the 10 percent level starting from the sixth year since the subsidy. The progressive increase in export probability coincides with the expected time lag required for the market introduction of new products and services.

[24] We get a more precise estimate when excluding observation for the first year of subsidy.

Figure 15. Effect of a subsidy on the probability of export by years since the subsidy



Note: The dots in the above figure show the estimated coefficients on the effect a subsidy has on export probability using a model specification similar to equation 2. The model is estimated using OLS after adding baseline model controls and triangular kernel weight. The vertical lines connected to the estimated coefficients represent a 90 percent confidence interval.

6. Heterogeneous effects

The main rationale in favor of government subsidies for research and innovation activities of SMEs is to correct for market failures, associated with positive externalities. Although there is less dispute on the presence of market failure, critics against public intervention mention that the possibility of policy failure is overlooked (Karlson et al., 2021). For instance, it could be difficult for program managers of a public subsidy to identify the right project that would not have been financed in the absence of a subsidy. That is, there is a risk that public money could be spent on projects that would have been financed with or without the project. However, the empirical analysis so far shows that subsidies have causal effects on a number of measured outcome variables, which implies that at least part of the subsidized projects would not have been undertaken in the absence of the subsidy. To further examine whether subsidies finance firms that are likely to be financially constrained, we conduct heterogeneity analysis by financial vulnerability. The idea behind this exercise is that if subsidies reduce financial constraint, then the subsidy effect should be stronger on firms that are likely to be financially constrained.

We utilize two commonly used indicators for financial vulnerability, namely, firm age and size. It is generally assumed that small firms face financial constraint because of information asymmetry (lack of reputation), lack of collateral and high risk (linked with a low level of product diversification). Young firms are also assumed to face financial constraint due to a lack of reputation and the high bankruptcy rate. Table 7 presents heterogeneous subsidy effects by age and firm size for our preferred model specification. Panel A confirms our expectation that subsidy effect is larger among young firms (age <10 years) than old firms. We also find a stronger subsidy effect for small firms on turnover outcome. Although the absolute subsidy effect on employment and number of S&T workers is higher for large firms, in terms of percentage the effect is higher for small firms. The subsidy effect on employment for large and small firms is approximately 30 and 57 percent, respectively. For the S&T workers, the effects are 44 and 60 percent, respectively, for large and small firms.[25] In sum, the results indicate that a subsidy is more effective on firms that are likely to be financially constrained and face higher financing cost.

Table 7. Heterogeneous effects by firm size and age

	Log turnover	Total employment	Employment in S&T	Export (1/0)
	(1)	(2)	(3)	(4)
Panel A: Effect by firm age				
Subsidy effect				
Age<10	1.288***	3.520**	2.209**	0.012
	(0.445)	(1.485)	(0.927)	(0.023)
Age >=10	-0.203	2.613	1.580	0.044
	(0.407)	(2.381)	(1.014)	(0.039)
Panel B: Effect by firm size				
Subsidy effect				
# employees <10	0.982***	1.743**	1.202**	0.009
	(0.377)	(0.842)	(0.554)	(0.010)
# employees >=10	-0.206	6.105*	3.564**	0.064
	(0.442)	(3.556)	(1.574)	(0.070)

Note: N=1,823. All regressions include baseline controls and triangular kernel weights. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

[25] That is obtained by taking the ratio of the estimated coefficients and the median employment (S&T workers). The median employment (S&T workers) for small firms is 3 (2) workers and for large firms it is 20 (8).

To further improve our understanding about the effectiveness of a subsidy, we provide additional heterogeneity analysis by industry, size of subsidy and program (Eurostars I and II). Panel A of Table 8 shows that the subsidy effect is stronger in manufacturing, bioscience, engineering, and natural science. However, we find a generally weaker subsidy effect in ICT and trade sectors. Panel B of Table 8 provides heterogenous effects by grant size, i.e., whether a firm receives a grant below or above the median in our sample. The results show that larger grants do not necessarily increase the effectiveness of the subsidy. In fact, the effect of larger grants is smaller on turnover, while we find no heterogeneity for the other outcomes. It is, however, important to consider that the firms with larger grants tend to be larger in firm size than those with a smaller grant. Thus, the results could to some extent be driven by the strong effect that the subsidies have on smaller firms.

Table 8. Heterogeneous effect by grant size and sector

	Log turnover	**Total employment**	**Employmnet in S&T**	**Export (1/0)**
	(1)	(2)	(3)	(4)
Panel A. Effect by sector				
Subsidy effect				
Manufacturing	0.471	5.705	3.231***	0.067*
	(0.529)	(3.548)	(0.992)	(0.034)
Bioscience	1.179	4.919	4.176	0.073
	(1.689)	(5.663)	(5.359)	(0.067)
Engineering and natural science	2.127**	4.938*	3.618**	0.002
	(0.909)	(2.574)	(1.769)	(0.022)
ICT	0.243	-0.080	0.364	-0.049
	(0.368)	(3.051)	(1.364)	(0.067)
Trade and other sectors	-0.157	1.879	0.670	0.038
	(0.426)	(1.712)	(1.076)	(0.042)
Panel B. Effect by grant size				
Subsidy effect				
Bellow median (<2.48 million kronor)	0.876**	3.038*	1.922**	0.030
	(0.364)	(1.746)	(0.971)	(0.025)
Above median (>= 2.48 million kronor)	0.458	3.274*	1.916**	0.017
	(0.391)	(1.694)	(0.762)	(0.028)

Note: All regressions include baseline controls and triangular kernel weights. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Finally, we investigate the presence of cohort-specific effects by looking at heterogenous effects on firms that receive a grant award in phase 1 (2008-2013) and phase 2 (2014-2019) of the Eurostars program. Appendix Figure A3 shows the subsidy effect for each year since the subsidy. In general, we find no major difference on the effect of the subsidy between the two cohorts.

7. Robustness checks

In this section we investigate the sensitivity of our results to alternative estimation methods and sample selection.

The results in the main analysis excluded non-subsidized firms with score points above the minimum cut-off (22 unique firms), for reasons explained in section 4.3. A natural alternative is to employ a fuzzy regression discontinuity design by using all observations. The estimated coefficients based on a fuzzy RD design are presented in the second row of Table 9. For comparison, we also report the estimates from our baseline model in the first row. It is evident from the comparison of the coefficients in the first two rows that the main result is robust to alternative RD designs.

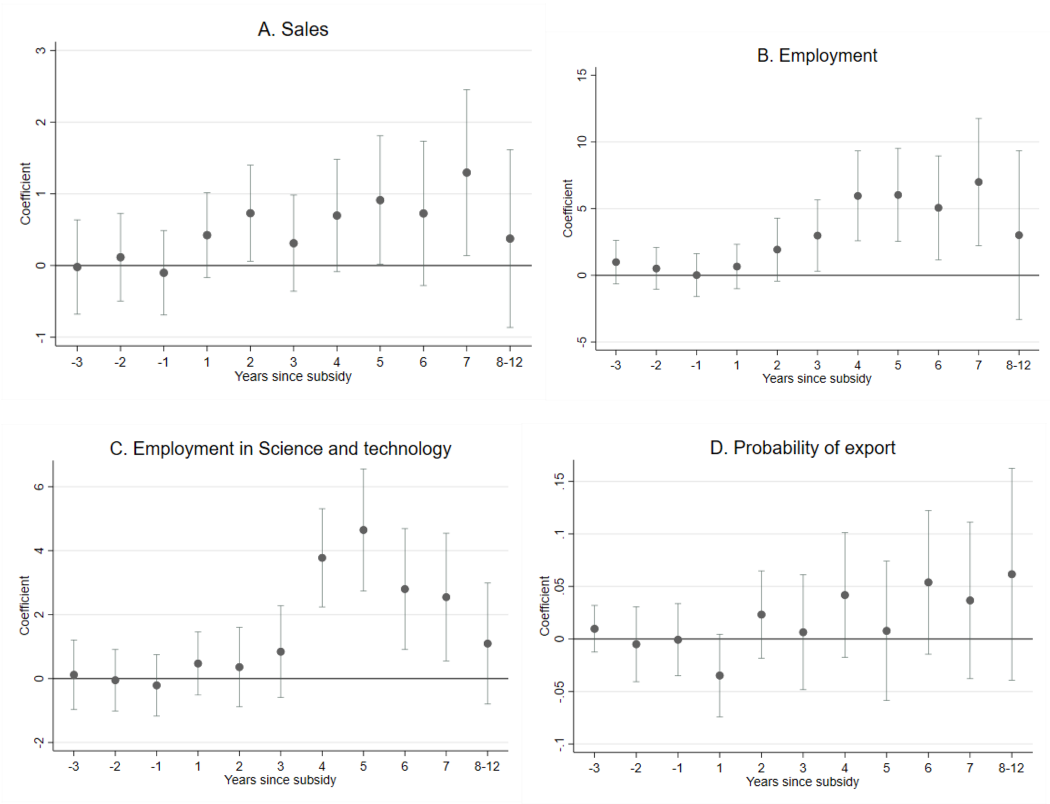
Table 9. Robustness checks

	Log turnover		Total employment		Employmnet in S&T		Export (1/0)	
	Full sample	Narrow BW	Full sample	Narrow BW	Full sample	Narrow BW	Full sample	Narrow BW
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Subsidy effect								
i. Baseline result	0.698**	1.082**	3.139**	2.59	1.919***	2.326*	0.025	0.016
	(0.318)	(0.506)	(1.352)	(2.315)	(0.697)	(1.350)	(0.022)	(0.043)
ii. Fuzzy RD design	0.713**	1.131**	2.975**	2.294	1.966***	2.469**	0.022	0.010
	(0.347)	(0.555)	(1.351)	(1.962)	(0.657)	(1.077)	(0.021)	(0.034)
iii. Controlling for before subsidy outcomes	0.588**	0.764*	3.451***	3.573**	1.873***	2.743**	0.02	0.018
	(0.255)	(0.427)	(1.164)	(1.686)	(0.539)	(1.086)	(0.034)	(0.039)
iv. Dropping firms with 2 or more subsidies	0.659**	0.788*	3.778***	2.234	1.872***	1.613	0.032	0.014
	(0.313)	(0.487)	(1.461)	(2.218)	(0.694)	(1.157)	(0.024)	(0.048)
v. Adding medium sized firms	0.634**	1.095***	3.906	7.551	3.159	7.619**	0.032	0.038
	(0.296)	(0.455)	(3.172)	(6.192)	(2.030)	(3.488)	(0.028)	(0.050)
vi. Controlling industry * year FE								
	0.729**	0.995**	3.167**	2.569	1.988***	2.386*	0.027	0.012
	(0.309)	(0.478)	(1.352)	(2.241)	(0.725)	(1.406)	(0.021)	(0.043)

Note: Each cell in the above table is obtained from a separate regression. N=1,823 in panel (i) and (iii), N=1,910 in panel (ii); N=1,627 in panel (iv); and N=1,987 in panel (v). All regressions include baseline controls and triangular kernel weights. Robust standard errors clustered on scores are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Next, we exploit the panel structure of our data and perform the following two robustness checks. First, we estimate the baseline model after controlling for pre-subsidy outcomes measured one year before the subsidy decision. These controls are expected to remove any remaining, if it exists, pre-exiting heterogeneity between subsidized and non-subsidized firms. The results, reported in row (iii), show no major changes compared to the baseline estimates. Second, in the spirit of a difference-in-difference (DiD) design, we estimate the subsidy effect for every year before and after subsidies. The coefficients are presented in Figure 16. It is interesting to note that there is no significant pre-subsidy difference between subsidized and non-subsidized firms as far as three years before subsidy decision, which is a strong indicator of the validity of our identification strategy. For the post-subsidy decision years, the estimates are very similar to the results found using the RD design.

Figure 16. Subsidy effect using the Difference-in-Difference method



Note: The dots in Panel A-D show the estimated coefficients on the effect a subsidy on turnover, employment, S&T workers and export. The model is estimated using OLS after adding baseline model controls. The vertical lines connected to the estimated coefficients represent a 90 percent confidence interval.

In section 4.3, we indicated that some firms receive more than one grant at a different point in time with another project application. In our sample, about 8 percent of firms receive more than one grant. This makes it difficult to isolate the effect of the first subsidy from the effect of subsequent subsidies. In row (iv) of Table 9, we estimate the baseline model after drop firms with two or more grants. As can be seen from row (iv), the main result is stable to the above sample restriction indicating that our result is not driven by firms that receive more than one subsidy.

To avoid the estimated coefficients being driven by extreme outliers, the main analysis is restricted to small firms, which represents more than 88 percent of the applicants. However, since the Eurostars program is mainly designed for small- as well as medium-sized firms, it is interesting to evaluate the impact of a subsidy after adding medium-sized firms, i.e., those with a pre-subsidy turnover of between 100,000 and 500,000 kronor and 50-249 employees. As can be seen from row (v) of Table 9, adding medium-sized firms does not change the main result in a major way, except concerning the precision of the estimates. In particular, we now see that the subsidy effect on employment is larger in absolute terms, and the estimates are less precisely estimated compared to the baseline model. Finally, the last row shows that the baseline result is robust to industry-specific time trends.

8. Conclusion and discussion

The future welfare and economic growth of advanced economies depend on innovations. Governmental grants for R&D are one of the most used innovation policy tools globally. There are theoretical arguments for supporting R&D (Solow 1956, Hall 2002), but the direct and selective policy tools have been especially questioned due to problems with "picking winners" (Lerner 2009). The empirical evidence of the effects has been mixed, and no consensus about the effects of R&D grants has emerged. In recent years, however, there has been a stream of literature using clearer causal identification strategies on the effects of R&D grants. This stream includes quasi-experimental and experimental studies, which have mostly shown positive effects. There is, however, still no consensus among researchers about the effects of the grants, and many studies show that different factors such as program designs and institutional environments affect the outcome. One important aspect is the long-term effect of the R&D grants, because otherwise the effects may be similar to a "sugar rush" (Gustafsson et al 2016) and have little impact on the long-term development of the economy. In this study, we have used a quasi-experimental study design to measure the casual effects and the time-series runs 12 years after the grant was awarded.

The empirical analysis shows that subsidies have a positive and significant effect on turnover, employment, and the number of scientific and research workers. The propensity to export shows a positive effect but it is not significant. The effect is stronger on firms that are expected to be financially constrained, such as small and younger firms. We find that the subsidy effect on turnover, employment and export last for more than 7 years after the subsidy, which is consistent with explanations based on long-term channels such as improved competitiveness through the market introduction of innovative products and process. We show that the main result is robust to alternative model specifications.

We believe this study contributes to the growing evidence of the casual effects of innovation policies and is a step closer toward more robust evidence of different innovation policy tools (Bloom et al 2019). In our study, we specifically contribute to the understanding of the effects of R&D grants. We confirm the recent quasi-experimental studies' results that identify a positive impact (Howell 2017, Santoleri 2020), but we add a new institutional setting (Sweden) and new outcome variables (skilled technical labor and exports). We also contribute with a long time-series which, perhaps most importantly, shows that the effects are long term.

There are a couple of policy implications. First, this particular program has shown a long-term effect for innovative SMEs in Sweden, which means that there is strong support for this particular program and similar R&D grant programs with the caveat that we do not know if the positive results displayed are larger than the costs of the program. Second, there is a need to continue using quasi-experimental and experimental effect designs to evaluate innovation policy tools. With the help of ranking scores these types of evaluations are made possible.

We further suggest that governments and their agencies set a research agenda based on evaluations of the different innovation policy tools to improve the understanding of the effects. Regression discontinuity design is an analytic instrument that does not disturb the funding process but gives reliable results. However, randomized control trials are an even better analytical instrument to provide information about casual effects. There is considerable evidence that the impact of R&D grants are heterogenous, i.e., participating firms are affected differently. This information can make R&D grants more effective if they are targeted towards the firms with the greatest potential for changing behavior. Furthermore, the budget for the Swedish applicants between 2008 and 2019 amounts to SEK 900 million. The question for policy-makers is to understand if this investment has higher societal benefits compared to SEK 900 million in taxes. The next step is to make a cost-benefit analysis and include the direct benefits. In addition, knowledge spillovers into different sectors and temporal spillovers of innovation are important to investigate.

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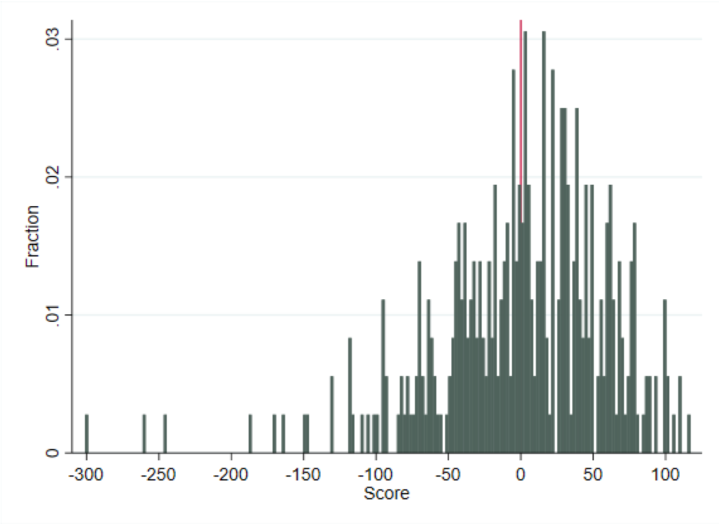
Appendices

Appendix Figures

Figure A1 Distribution of firms by score point



Note: The variable score in the x-axis is normalzied to zero at the cut-off point, representing the distance from the cut-off.

Figure A2 Number of workers in science and technology before and after subsidy

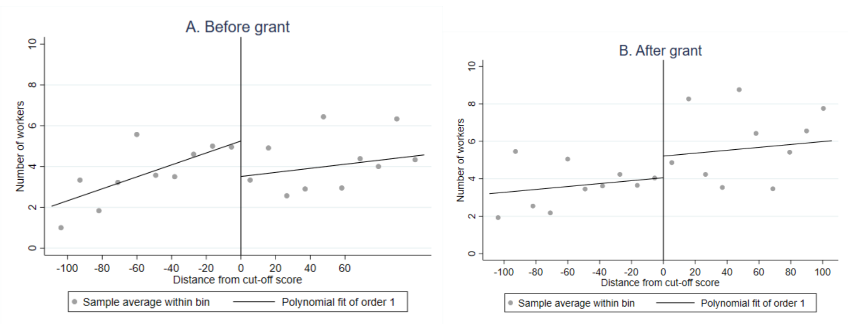
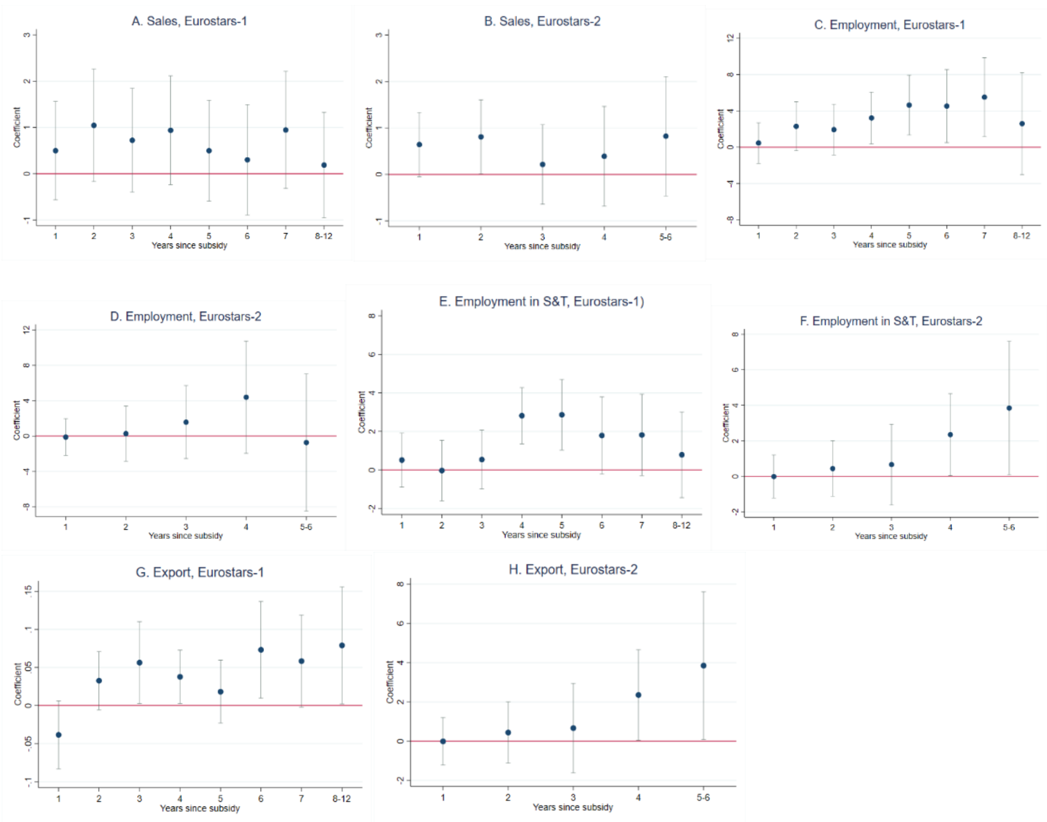


Figure A3 Heterogeneity by eurostars 1 and 2.



Appendix Tables

Table A1 Summary statistics for the pooled sample of post-subsidy years.

	Observation	Mean	Median	SD	Minimum	Maximum
Number of employees	1835	10.80	6.00	15.30	0	224.00
Net turnover (in million kronor)	1835	15.69	2.81	31.55	0	402.31
# of scientific and Technical workers	1549	4.89	3.00	6.77	0	63.00
Age	1835	13.65	12.00	8.81	0	35.00
Manufacturing (1/0)	1835	0.23	0.00	0.42	0	1.00
Wage expenditure (million kronor)	1835	4.93	2.20	7.46	0	71.19
Capital (in million kronor)	1835	28.93	2.19	188.14	0	4030.79
Years since subsidy decision	1835	4.11	4.00	2.71	1.00	12.00
Year of observation	1835	2016	2016	2.8	2008	2019
Year of subsidy decision	1835	2013	2012	3.0	2008	2019