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The "Sugar Rush" from Innovation Subsidies - A Robust Political Economy Perspective*

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Abstract

The governments of most advanced countries offer some type of financial subsidy to encourage firm innovation and productivity. This paper analyzes the effects of innovation subsidies using a unique Swedish database that contains firm level data for the period 1997-2011, specifically information on firm subsidies over a broad range of programs. Applying causal treatment effect analysis based on matching and a diff-in-diff approach combined with a qualitative case study of Swedish innovation subsidy programs, we test whether such subsidies have positive effects on firm performance. Our results indicate a lack of positive performance effects in the long run for the majority of firms, albeit there are positive short-run effects on human capital investments and also positive short-term productivity effects for the smallest firms. These findings are interpreted from a robust political economy perspective that reveals that the problems of acquiring correct information and designing appropriate incentives are so complex that the absence of significant positive long-run effects on firm performance for the majority of firms is not surprising.

Keywords: Innovation subsidies, market failures, causal treatment effect evaluation, firm performance, CEM, robust political economy

JEL: H25, O38, P16

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1 Introduction

In most advanced economies, there are various types of government subsidies, many of them aimed at small and innovative firms (Becker, 2015). This is especially true in the European Union, where innovation is considered a key driver of the smart, sustainable, and inclusive growth required to meet challenges of unemployment, climate change, globalization, migration and an aging population. Innovation was a core element of the Lisbon summit and the Horizon 2020 strategies and will likely continue to be a central part of future strategies. There is a general consensus that innovative ventures will be necessary for accomplishing the structural change required to solve the problems we are facing today, and will face in the coming decades. One of the major policies employed to this end involves subsidies to small and medium-sized innovative firms. This is a key element of the "Innovation Union," the EU's strategy "to create an innovation-friendly environment that makes it easier for great ideas to be turned into products and services that will bring our economy growth and jobs" (EU, 2015). These initiatives are founded on at least three propositions. The first is that new, small firms are especially important for innovation, job creation, and long-term economic growth. The second is that markets, especially financial markets, fail to invest sufficiently in such firms, leading to these firms having insufficient incentive to engage in innovation activities. The third proposition is that government subsidies are an appropriate way of compensating for the problem of market failure to invest. Previous research supports the first proposition that new and small firms are especially important for innovation, job creation, and long-term growth (Soete and Stephan, 2004). Indeed, the sources of innovation have been the subject of extensive research in recent years. New firms are found to be more innovative than others (Acs and Audretsch, 1988) and entrepreneurs financed by private venture capital firms are more innovative than other new firms (Kortum and Lerner, 2000).

This paper evaluates the effects of Swedish subsidies and thus sheds light on whether the last proposition—that government subsidies are effective in increasing firm performance—finds support in a real-world context. If there are large market failures that prevent firms from accessing funding, there should be significant positive effects from direct subsidies. However, if market failures are small or the design of the public subsidy programs is inefficient, there should be a lack of positive effects, particularly in the long run. The causal chain, in short, is that firms receive subsidies, innovate, and, subsequently, becoming more productive.[1] Using firm-level data

[1]As we do not have reliable measures on innovation outcomes, we evaluate the effects of sub-

and a unique Swedish database of firm subsidies, combined with a qualitative analysis of Swedish innovation programs and recently developed techniques of treatment effect analysis, we provide empirical evidence of the short- and long-term effects of governmental subsidies on firm performance. The results show that innovation subsidies do not, on average, create long-lasting positive effects on firms' average wage costs, investments, labor productivity, or profits. We find positive short-run effects on human capital investments but negative effects on physical investments over the post-treatment period. In addition, we find short-term positive productivity effects from subsidies only for the smallest firms with only 2 employees.

The absence of lasting positive effects from governmental innovation subsidies makes sense when viewed from the perspective of robust political economy (RPE) (Leeson and Subrick, 2006; Pennington, 2011; Boettke and Smith, 2012). According to the RPE perspective, public programs developed to solve market failures must be able to both access sufficient information and provide public officials with appropriate incentives. This means, from a cost-benefit perspective, that the costs of such innovation support programs might exceed their benefits since it is costly to collect taxes and pay for the administration. In short, providing innovation subsidies involve two major problems. The first is the knowledge problem, or the impossibility of sufficient knowledge, widely regarded as Hayek's (1945) main contribution to economics. The second relates to public choice theory, promoted by Buchanan, Tullock, and others (e.g. (Tullock, 1967; Mueller, 1976; Buchanan and Tollison, 1984), which posits that the government is not, in fact, a perfect, benevolent entity, but is instead made up of officials and others who act in their own interest, thereby leading to potential incentive problems. The RPE perspective encompasses both of these literature strands and can therefore be viewed as a synthesis.

This paper contributes a new methodology for evaluating the effects of subsidies, over a longer time period, and uses a representative sample containing more firms than have other studies. We are able to do this because of a unique database containing information on innovation subsidies in combination with register data on firm statistics. This allows us to address the potential bias that stems from the nonrandom assignment of subsidy treatment to firms and to evaluate the effect of subsidies in both the short and long-run. More specifically, our panel allows us to track most firms for up to eight years after they have received a subsidy and some firms for more than 10 years. The large amount of register data, compared to survey data, allows for a richer analysis of the effects of subsidies than was possible

sidies on firm performance, as the final objective of granting subsidies to firms.

in most other studies and allows us to draw conclusions regarding the general effects of firm subsidies in Sweden. In addition, we address the general question of why innovation support programs do not create the intended beneficial long-term effects: we provide evidence that the agencies that grant subsidies to firms do not take into account the information and incentive problems that must be resolved in order to maximize the likelihood of success. Although there is a large literature evaluating the effects of public subsidies on innovation, to our knowledge, none have used the theoretical RPE framework. We believe this framework provides valuable insights into how public support programs should be designed: effective programs must be able to address information and incentive problems before they can result in efficient outcomes.

The remainder of this paper is organized as follows. Section 2 describes the political rationale for providing innovation subsidies and summarizes previous research. Section 3 describes the institutional design of Swedish agencies that grant innovation subsidies. Section 4 provides econometric results on the effects of innovation subsidies. Section 5 concludes.

2 The Economic and Political Rationale for Public Innovation Subsidies

A growing body of empirical research suggests that new firms, especially technology-intensive ones, may suffer from insufficient capital. The literature on capital constraints (reviewed in Hubbard, 1997) documents that an inability to obtain external financing limits many forms of business investment. Hall (1992), Hao and Jaffe (1993), and Himmelberg and Petersen (1994) find similar results, particularly in the case of smaller firms (Lerner, 2009).

The market failure argument in this context basically rests on two observations: incomplete capital markets and the existence of external effects. The incompleteness of capital markets is explained by the existence of informational asymmetries: those who seek financing know more about their business venture than do the potential investors (Akerlof, 1970). As a consequence it is difficult for investors to assess the business potential and risk involved, and hence the price, which, in turn, leads to credit rationing or an excessive interest rate. Moreover, there is a potential for principal agent and moral hazard problems involved, which increases the risk premium and financing costs even further (Kaplan and Strömberg, 2001). There is also the problem of double trust, noticed by Cooter and Schäfer (2012), which can prevent the creation of mutually beneficial contracts.

The existence of external effects of innovative activities and investments in new ideas—due to the public good-character of knowledge creation and positive spillovers to other firms and society in general (Arrow, 1962; Baldwin, 1969; Romer, 1990; Hausmann and Rodrik, 2003)—makes capital markets even more suboptimal. In other words, investments in innovative firms might result in informational externalities that further deflate the potential earnings of the initial investment (Baldwin, 1969; Hausmann and Rodrik, 2003; Pack and Saggi, 2006). Entrepreneurs also face the risk of others replicating their ideas (Cooter and Schäfer, 2012). In short, when investors cannot reap the full benefits of investment, underinvestment will ensue.

The results from previous empirical studies are mixed regarding the effectiveness of government innovation subsidies. Hall and Lerner (2010) demonstrate that, from a theoretical view, firms with a lack of external funding, but with plenty of good ideas, will increase their output if they receive public money. A number of studies find that government intervention in firm's innovation activities does not have long-lasting positive effects on firm performance (Acemoglu and Verdier, 2000; Datta-Chaudhuri, 1990; Krueger, 1990; Zerbe and McCurdy, 1999; Grossman, 1986; Brainard and Martimort, 1997). Other studies, however, argue that government subsidies can be effective but only if they are appropriately designed and administered (Pack and Saggi, 2006; Lerner, 2009). Bronzini and Iachini (2011) and González et al. (2005) find positive effects of R&D subsidies for small firms; Koski and Pajarinen (2013) do not. Some studies document crowding-out of private money when firms receive public money, particularly in the case of large firms (Wallsten, 2000; Lach, 2002; Görg and Strobl, 2007); other studies do not find crowding-out effects and, instead, find positive effects of subsidies to firms with innovative ideas but insufficient financial resources (Hussinger, 2008; Hottenrott and Peters, 2012). David et al. (2000) survey the literature and find a lack of consensus as to whether public R&D is a complement or a substitute for private R&D; the authors suggest matching techniques and randomized experiments as fruitful avenues for future research. Martin and Scott (2000) provide several suggestions for efficient subsidies and are skeptical of a direct approach: "Because governments typically have a poor record of identifying ultimately successful lines of technological development in advance, public support for innovating SMEs should not take the form of direct grants. Nor should it take the form of government debt or direct equity financing."

There are two papers that, like ours, use Swedish data in their evaluation of subsidies. Söderblom et al. (2015) evaluate the effect of subsidies from the Vinnova agency on acquiring additional human and financial capital and find positive results. However, their approach is likely to lead to biased treatment effect estimates as they compare firms that were rejected at the last stage of the application process to firms that were not rejected. Their claim that their identification strategy corresponds to a regression discontinuity design is not valid unless the decision made at the final stage was random. However, if it is assumed that Vinnova has the competence to select the better firms at the final stage, then the approach of Söderblom et al. (2015) leads to treatment effect estimates that are upward biased. The other study of note is Daunfeldt et al. (2014), which also uses coarsened exact matching to address the issue of selection bias. It finds no significant results from subsidies on firm growth, the skill level of employees, or firm productivity when evaluating two specific programs of Vinnova. Our study uses a similar approach and data, but we differ in two important aspects: first, we have access to a broader sample of firms for a longer time and we also evaluate several Vinnova programs instead of just the two included in Daunfeldt et al. (2014). In addition, we evaluate programs of SAERG and SEA. Finally, we evaluate different outcome variables and also use the RPE framework to explain the findings.

3 Swedish Institutional Design for Innovation Subsidies

Economic support for innovation, in the form of subsidies or subsidized loans to firms, is a common way of promoting innovation and growth. We studied three Swedish agencies whose purpose is to promote the innovation, growth, and competitiveness of Swedish firms: the Swedish Innovation Agency Vinnova, the Swedish Energy Agency (SEA) Energimyndigheten and the Swedish Agency for Economic and Regional Growth (SAERG). These agencies administer and implement the subsidy programs that are analyzed in our quantitative study. SAERG promotes entrepreneurship in general, Vinnova finances high-risk projects, and SEA focuses on projects within the energy industry. In total, we have information about many different innovation and firm support programs that were active during the years of 1997 to 2011 (Riksdag, 2008; Vinnova, 2015; SAERG, 2015d,e; Västernorrland, 2015; Energimyndigheten, 2011; SAERG, 2015a,b). This is a large fraction of the Swedish direct subsidies to firms, but we do not have information about subsidies granted by public venture capital funds. However, these funds operate differently, taking an active ownership role in firms they finance instead of granting subsidies.

The agencies have adopted the EU regulation on subsidies. In general, although the programs are of various design, firms that fulfill certain conditions regarding size and company form, and that propose projects judged to be relevant to the program, may apply for subsidies, normally up to 50 percent of eligible costs. Depending on the program, the money may be used to finance investments in human and/or physical capital, employee training, inventories, and costs associated with research and development. Given the large scope of the Swedish programs for innovation support, we are able to evaluate, in total, 8,518 firms that were subsidized between 1997-2011 and compare them to 261,476 firms that were not, to the best of our knowledge, subsidized. The total sample of 269,994 firms should include most of the firms in Sweden with more than two employees during 1997–2011, excluding dropped sectors and firms receiving multiple subsidies. While there might be firms that were subsidized during this period but were not included in the MISS database, we believe that this is a minor problem. The bulk of all subsidies in Sweden are handled by these three agencies, and the EU subsidies are channeled via SAERG. Therefore, there should be only a limited number of firms that we classify as being nonsubsidized that were in fact subsidized. The total amount of subsidies in this sample amounted to 46.7 billion in real SEK (1997 as base year). To put this sum in perspective, it is more than twice the cost of building the Öresund Bridge—around 20 billion SEK.

3.1 Assumptions Behind Governmental Interventions in Markets

There is a substantial literature questioning the presumption of welfare economics that government interventions are benevolent, rational, and based on perfect knowledge (Datta-Chaudhuri, 1990). Building on this literature, the more recent robust political economy (RPE) literature (Boettke and Leeson, 2004; Leeson and Subrick, 2006; Pennington, 2011) emphasizes that market failures are not, in themselves, sufficient to legitimize government subsidies to firms. In contrast to general interventions such as tax cuts, these kinds of selective intervention risk creating inefficient outcomes and market distortions that might be more severe than the problems they are intended to solve. To be robust and effective, policy measures must be designed to address the fundamental information and incentive problems discussed above (Coyne and Moberg, 2014). Moreover, as stressed in previous research, political decision-making may suffer from cognitive limitations and biases, imperfect self-control, framing effects, loss aversion, endowment effects, choice bracketing, information and choice overload, and a poor grasp of probability calculations (Tversky and Kahneman, 1974).[2]

Under the RPE approach, the first criterion of information compatibility can be met only when policymakers (i.e., the subsidy granters) have better information than market actors. For the case of innovation subsidies this means that the granting agencies must have better information regarding the potential grantee's business potential and risks than do private funding institutions. Otherwise, the innovation subsidies will not lead to better results in terms of innovation and performance than those that would have been obtained had the funding been provided by financial market participants. There are firms that will succeed even if they do not receive subsidies, and firms that will be unsuccessful even if they do receive a subsidy, and, lastly, firms that will be successful only if they receive a subsidy. Clearly, only the last group should receive subsidies.[3] According to Hall (2002), there is a difference between subsidizing an innovative firm with credit constraints and an innovative firm without credit constraints. A program that effectively targets the former group should be considered to have information compatibility. Hence, evidence that the subsidized firms are better than the average firm is not evidence that the subsidy has actually resulted in a socially optimal use of tax revenues.

The second criterion, incentive robustness, requires that the agencies implementing the policy in question have stronger incentives to act in the public interest and increase economic welfare than do market actors. However, as Niskanen (1975) hightlights, free-riding, principal-agent, and moral hazard problems can also occur in governmental agencies and, moreover, affect the relationship between the granters and the receivers of firm subsidies. Furthermore, special interest group lobbying may direct money to the wrong groups (Baldwin and Robert-Nicoud, 2007). Political willingness to intervene in markets and grant subsidies to firms might also be motivated by the strong desire of politicians to "do something" related to promoting economic growth as creating more innovation in the economy is generally perceived as something good by most voters.

[2]For comprehensive presentations of behavioral economics, see Kahneman (2003); Camerer (2004); Wilkinson and Klaes (2012). For an argument in favor of incorporating bounded rationality into economic analysis, see Conlisk (1996); Berggren (2012).

[3]One can compare information compatibility with the medical system of triage implemented by Napoleon. In performing triage, patients are divided into three groups: those who will survive without care, those who will not survive even if they do receive care, and those who will survive if they do receive care. Only the last group should be treated to maximize efficiency of the limited resources (Iserson and Moskop, 2007).

3.2 Evaluation of Swedish Innovation Subsidy Agencies from the Robust Political Economy Perspective

To qualitatively assess whether political decision makers consider the importance of the fundamental RPE-problems of information and incentives, when they attempt to solve market failures in venture financing we conducted a comprehensive review of our three agencies' appropriation directions,[4] given by the Ministry of Enterprise and Innovation, so as to shed some light on how these agencies work in practice. We scrutinized all of the digitally available appropriation directions for the three agencies, a total of 32 appropriation directions between 2003 and 2015.[5] Moreover, we analyzed 16 program evaluations (1,287 pages) by independent parties that were published between the years of 2008 and 2015 (e.g., Eurofutures, 2009, 2010, 2008; Ramboll, 2013; Sweco, 2009, 2010a,b; Kempinsky et al., 2011).

Next, we analyzed our agencies' assessment criteria and their specified program goals. Each of the agencies' criteria involved innovativeness, commercial potential, expected effects on firm and regional growth, and sustainability of the projects, the evaluation of which all require a thorough understanding of the firm, the market, and their future development. The goals advanced by the agencies are also difficult to measure. SEA, for example, wants to promote an improved environment, lower energy costs, and increased energy awareness in the companies supported, whereas SAERG's operations are intended to contribute to more attractive business conditions, as well as to sustainable regional growth and a stronger development capacity in all parts of the country. In an internal evaluation, these goals were defined as new customers, new markets, strengthened product portfolios, better results, improved market position, more stable revenues, and increased value added (SAERG, 2015c). Vinnova concludes, in its annual report for 2013 under "Goal achievements," that its activities yield results in the very long run, normally after 10 years minimum (Vinnova, 2014, p.11). The expected short-term results of its programs include new knowledge and new collaborative relationships, whereas the expected long-term results include increased understanding of research and development, skill develop-

[4] In the Swedish government's management of its agencies, performance management is carried out via instructions (or equivalent), appropriation directions and other decisions. The instructions include the agency's objectives and tasks, as well as periodic reporting requirements. Where necessary, the appropriation direction sets out annual targets, tasks and reporting requirements. Once the Riksdag has adopted the central government budget, the Government issues appropriation directions for all appropriations. The appropriation directions set out the agencies' financial conditions and priorities for the budget year.

[5] For SEA (Näringsdepartementet, 2003-2015a) and Vinnova (Näringsdepartementet, 2003-2015b), but between 2009 to 2015 for SAERG (Näringsdepartementet, 2009-2015).

ment, new processes, and increased competitiveness (Vinnova, 2014, p.22), which, in turn, is expected to improve firm performance and, subsequently, economic growth.

Our conclusion from the qualitative study of subsidy-granting agencies is that there is no indication that these agencies understand the information and incentive problems, nor is there any evidence that the programs have been designed to mitigate these problems. In other words, taking the RPE perspective, we find no evidence that the Swedish programs for innovation support attempt to minimize the risk of policy failure. One justification for supporting firms with innovation subsidies is that banks and other private actors such as venture capital firms are too risk averse to do so themselves given the great uncertainty of innovative projects. However, the existence of government innovation support programs implies that the agencies administering these programs are able to identify those innovative firms suffering from financial constraints, and have appropriate incentives to implement the policies required. The RPE perspective that public programs must be information and incentive compatible to be efficient suggests that programs designed to increase innovation should be evaluated often to see if they are actually have the intended effect. Interestingly, although active labor market programs are evaluated frequently using state-of-the-art treatment effect analysis (e.g., Bennmarker et al., 2013), public innovation subsidy programs in Sweden are rarely the subject of such analyses.

4 Econometric Treatment Effect Evaluation

In this section, we provide empirical evidence of the effect of the subsidies, defined in our study as a treatment. In this context, simple regression analysis could suffer from selection bias as one can presume that the treated firms would have performed better even without treatment if the agencies are picking the better firms for their programs. Randomization of treatment across firms would solve the selection problem, but is currently not available in the context of innovation subsidies (Angrist and Pischke, 2008). We address this issue by defining a group of matched firms that are similar to the treated ones. This approach is also known as selection on observables and relies on the conditional independence assumption, which means that given the observable characteristics, treatment assignment becomes independent of the treated subject's characteristics. Hence, after the matching processes our regression estimates of the treatment effect are assumed to be unbiased similar to those one would obtain from an experimental setting (Rubin, 1974).

It could be argued that it is unrealistic to expect long-term effects from innovation subsidies. For example, a firm that creates an innovation giving it a first-mover advantage will in time lose this advantage because, at least in a properly functioning market, other firms will copy its successful products and strategies (Schumpeter, 1934). Nevertheless, the motivation for agency intervention is that the subsidy will create a long-lasting effect for the firm itself (and in turn for the entire economy). Thus, we investigate whether these agencies are, indeed, fulfilling their own raison d'être. Likewise, the outcome variables in the econometric analysis do not include innovation output measures but, instead, the economic impact from the innovation. This is both due to a lack of data on innovation outcomes, such as patents, and also because the point of the subsidies is to produce long-lasting performance effects.

4.1 Data and Descriptive Statistics

The data used are based on the MISS database of firms receiving subsidies and were collected by Growth Analysis[6] from the three different agencies that grant the subsidies. These data are matched with register data on Swedish firms from Statistics Sweden. Firms with fewer than two employees are excluded so as to reduce noise due both to small firms being volatile and because there is a lack of register data for such firms (this very small firms are not required to report data in the same manner as larger firms). In another step to reduce unwanted heterogeneity in the control group, firms with a NACE code related to agriculture, restaurants, and publicly funded industries were excluded.[7] These sectors are seldom targeted for these types of subsidies and one would not expect comparable effects from the subsidies. This creates an unbalanced panel from 1997 to 2011. The total number of firms, both subsidized and nonsubsidized is presented in Table 1. Descriptive statistics for the main variables of interest, depending on whether a firm will be treated or not, are presented in Table 2. Because the inclusion of firms that receive more than one subsidy from these agencies might interfere with the post-treatment analysis, only firms that receive one subsidy are included. If the effect from treatment is temporary, the effect from multiple subsidies will be different from that arising from a single subsidy. There are around 1,400 firms in our database that receive multiple subsidies, whereas almost 8,000 receive a single subsidy, meaning that even the removal of these multiple-subsidy firms leaves us with a sufficiently large sample for analysis. Moreover, the multiple-subsidy firms are significantly

[6]Growth Analysis is a Swedish government agency tasked with analyzing and evaluating Swedish growth policy.

[7]More specifically, firms related to SNI2002-codes 1-5, 55 and 75-99 were excluded.

larger than the single-subsidy firms and so including them would not only lead to a more complex estimation, but also bias the result due to the difference between the groups. All variables, except number of employees and the share of high-skill workers per total labor, are in real SEK. Descriptive statistics for firms subsided by an agency relative to the control group are available in the Appendix.

Table 1: Number of subsidized and non-subsidized firms

Firm type	Number of unique firms
Subsidized by SEA	133
Subsidized by Vinnova	727
Subsidized by SAERG	7,781
Control group (non-subsidized)	276,021

Notes: SEA=Swedish Energy Agency, Vinnova=Swedish Innovation Agency SAERG=Swedish Agency for Economic and Regional Growth

4.2 Methodology

The nonrandom assignment of firms to receiving subsidy makes evaluation more difficult (Klette et al., 2000). Our approach is based primarily on Heyman et al. (2007), and others (Girma and Görg, 2003; Greenaway et al., 2005; Görg and Strobl, 2006; Bandick et al., 2014) with the proviso that we match using CEM rather than propensity score matching. The advantage of CEM is that it does not require the balancing property that must hold for propensity score matching (PSM). To identify the effects of the innovation subsides we run fixed-effect panel regressions on four different outcome variables, with two different methods, and three subsamples. Thus we conduct six regressions for each outcome variable. One advantage of the panel fixed-effects model is that it can account for unobserved time-invariant heterogeneity. However, given that innovation subsidies are not randomly assigned to firms, estimates using the sample may suffer from selection bias. The significant differences between the subsidized and nonsubsidized firms in observable characteristics displayed in Table 2 indicates that this is, indeed, a problem. To correct for this, we match the treated firms using coarsened exact matching (CEM) (Iacus et al., 2009, 2011; King and Nielsen, 2016). The CEM matching is based on number of employees, log of capital stock, industry (single digit) NACE-codes and region. These variables should capture the heterogeneity of firms size, location and industry and the non-linear effects of treatment in these sectors, making the control group much more accurate. The combination of matching and difference-in-difference with further covariates estimation is able to capture the treatment effect without the risk of confounding bias. Given that we use wages, investments and indirectly sales as outcome variables in the regressions, we cannot use these as matching variables as well even though there are large differences between the treated and non-treated firms regarding these variables.

The CEM algorithm, unlike the PSM one, does not estimate the probability of being treated. Instead, it coarsens the variables in strata and weights firms depending on how close they are to the treated firms. Exact matching would require assigning a treated firm with, say, a capital stock of 350,001 SEK, a firm having exactly the same size capital stock. Since this might be impossible to achieve, coarsing the variables makes matching possible (Iacus et al., 2009). While CEM primarily uses continuous variables, we match exactly regarding categorical variables industry and region.

Table 2: Summary statistics treated vs. non-treated

	Observations[1]	Mean	Median	Std. Dev.
Non-treated				
Number of employees	1,686,869	14	4	107
Wage costs per employee	1,686,869	130	124	122
Share of high skill labor	1,545,187	.44	0	1.7
Gross investments	1,686,869	1,388	45	27,923
Net sales	1,686,869	29,015	3,982	318,829
Capital stock	1,686,869	39,946	2,220	872,429
Total amount support received	1,686,869	0	0	0
Treated				
Number of employees	81,141	38	7	392
Wage costs per employee	81,141	134	130	88.4
Share of high skill labor	76,076	.42	0	1.21
Gross investments	81,141	4,070	130	64,582
Net sales	81,141	75,840	7,310	827,830
Capital stock	81,141	132,273	4,135	3,117,791
Total amount support received	81,133	312,141	88,568	1,808,829

Notes: Summary statistics for treated and non-treated firms between 1997-2011. All monetary figures in real SEK.

[1]Firm-year.

Note that the treated firms are not smaller than the non-treated firms on average, nor do they seem to have access to less capital as indicated by their investment level. While this is not in line with the recommendations from the literature discussed in Section 2, we cannot draw any conclusions from summary statistics alone.

The matching reduces the distance between all variables, as can be seen by the comparing the $L1$ column in Table 3 with the same column in Table 4. Both positive

Table 3: Imbalance measurement, univariate imbalance

Variable	L1	Mean	Min	25%	50%	75%	Max
Employees	0.179	17.81	0	1	2	6	-23,887
Log capital stock	0.154	.495	3.03	0.456	0.469	0.533	.156
Region	0.415	4.99	1	7	5	7	0
Industry	0.335	-1.06	0	-2	-1	-2	0

Table 4: Matching summary

Number of strata	Number of matched strata
2,748	925

	0	1
All	1,686,869	5,935
Matched	1,484,654	5,915
Unmatched	202,215	20

Variable	L1	Mean	Min	25%	50%	75%	Max
Employees	0.095	4.92	0	0	1	2	-613
Log capital stock	0.081	0.016	0.131	0.009	0.014	0.011	.
Region	8.8e-13	6.3e-11	0	0	0	0	.
Industry	9.0e-13	1.1e-11	0	0	0	0	0

and negative results for the imbalance measurement regarding the variables should be interpreted as a difference between the two groups. A zero therefore indicates a lack of difference between the groups in that category. For all variables, the coefficient is smaller, that is, better matched, in Table 4. Therefore, the matching was successful in making the groups more similar to each other and thus reducing the bias in the estimations. We also perform regression solely on the treated sample and on the entire population of firms. The regressions on only the treated sample thus have no control group but nonetheless provide useful information. When regressing against the full population without matching, the result of the treatment effect is likely to be upward biased compared to the CEM results due to the selection of different firms into treatment, but will provide us with a robustness check. If all three subgroups are showing the same pattern this will indicate that the results are relevant.

The following model is estimated for each outcome variable, for $i = 1, \ldots N$, and $t = 1, \ldots T$:

$$y_{it} = \alpha + \beta X_{it}^{'} + \gamma Treatmentdummy_{it} + \tau Postdummy\, d_{it} + \delta_j + \rho_k + \theta_t + \varepsilon_{it} \quad (1)$$

where $\beta X_{it}^{'}$ is a vector of control variables, δ_j are two-digit industry control dummies, ρ_k are regional control dummies, θ_t are year dummies and ϵ_{it} is an error term. A further control is the share of employees with a university degree to capture various degrees of knowledge intensity across firms. In the first regression we use a treatment dummy for the year when the firms receive a subsidy and a post treatment dummy that takes the value of 1 the year after the subsidy, and remains 1 for the rest of the panel. This allows us to measure the average effect of the treatment using all available information.

In a second regression, we specify the same model, but allow the treatment dummy to vary over time. That is, we introduce dummy variables d_{itl} for $l = 1, \ldots, 8$ for each year following the receipt of a subsidy:

$$y_{it} = \alpha + \beta X_{it}^{'} + \gamma Treatmentdummy_{it} + \sum_{l=1}^{8} \tau_l Postdummy\, d_{itl} + \delta_j + \rho_k + \theta_t + \varepsilon_{it} \quad (2)$$

This is done in order to capture the subsidy's time-varying effects. The results from Model (1) are presented in Tables 6–9. In Figures 1–4, the treatment effect from Model (2) is plotted year by year, with year 0 being the year the firm receives the subsidy and ending eight years later. For clarity, only the CEM-based regressions are plotted. Full regression tables, including the two other control groups, are available in the online Appendix. Since the panel is unbalanced, with firms receiving subsidies on a yearly basis there is a loss of information when extending the post-treatment analysis. Although we have 8,173 observations on firms one year after they received the subsidy, only 2,815 observations remain in year 8. This is due to firms going bankrupt, being bought by others firms, and the like. The main reasons for firms dropping out, however, is the ending of the panel and the distribution of when firms receive subsidies. For example, a firm that receives a subsidy in 2010 is observed for only one year, a firm that receives a subsidy in 2009 is observed for two years, and so on (right truncation). Therefore, the choice of time span for the post-treatment analysis becomes a tradeoff between the increasing information due to the subsidy having more time to take effect and the loss of information due to firms not having been in the panel long enough.

The loss of firms for reasons other than the timing of the subsidy is a problem. Since we have no information on reasons for early exit, and two potential reasons—bankruptcy and takeovers—have widely different implications, our results could be biased. The firms that potentially span our entire post-subsidy evaluation period are those that received subsidies between 1997–2002. Of the 5,396 firms that received a subsidy during this period, we have only 2,815 observations on eight-year post-treatment dummies, implying a loss of 2,581 firms, or 48 percent of the sample. This loss is mainly driven by firms with fewer than 10 employees. Of the 3,703 small firms that received a subsidy during this period (69 percent of the sample), we have only 1,590 observations on eight-year post-treatment dummies. Out of a total of 2,581 lost firms, only 991 had more than 10 employees. Although the large loss of small firms is perhaps not surprising given the volatility of small firms, it could affect our results. First, firms that leave the panel due to mergers and acquisitions will produce a bias. These firms were successful, but their success is captured by other firms and therefore missing in the regressions. Firms leaving the panel due to bankruptcy will produce a survival bias. Unfortunately, we cannot separate these two types of bias. However, the loss of firms is steady over the studied time period, suggesting that there were no asymmetric shocks that might effect the evaluation.

The endogenous and exogenous variables are defined in Table 4.2. The gross operating profit variable follows OECD protocol and is used in a similar way by Vandenberghe (2013). Both average wage costs and gross investments are input variables for the firm, whereas value added and gross profits are output variables. Although it is reasonable to assume that the subsidy will have a positive effect on the input variables, it is unclear how the output variables will react. The average wage costs also work as a proxy for labor quality. If the subsidy induces the firm to hire more highly skilled labor, this should result in higher average wage costs. Note that subsidies for investments in current assets are not reported as a separate item of revenue but as a lower acquisition cost for the assets. This means that depreciation over forthcoming years will be lower, which, ceteris paribus, means higher yearly profits for companies, thus creating a potential upward bias in productivity since the value added might be caused by accounting rules and not higher productivity per se.

Table 5: Dependent and independent variables

Variable	Definition
Average wage costs	Log of total wage costs divided by the number of employees
Gross investments	Log of gross investments
Productivity	Log of total value added divided by the number of employees
Gross operating profit	Log of total value added divided by the total wage costs
Share of high skill labor	Number of tertiary educated workers divided by the total number of workers
Capital stock	Log of current assets plus fixed assets

The number of control variables varies in each of the four regressions. When regressing the wage cost per labor, the control variables also need to be divided by the labor size, that is, capital and sales per labor. When regressing investments, we do not divide by the labor force and hence the control variables are also not per labor. When regressing the value added per labor, we also use per labor control variables. Since value added is dependent on sales, we exclude this as a control. Likewise, gross operating surplus is also dependent on sales and the latter is thus excluded.

4.3 Results

Table 6: Outcome variable: log labor costs per employee

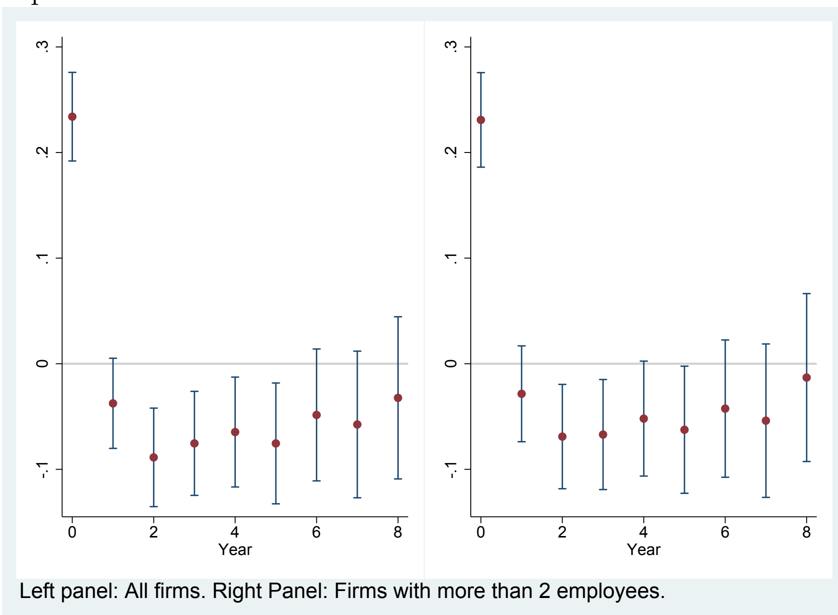
	Only treated	CEM	Full pop
Treatment effect	0.0064	0.017***	0.0018
	(0.0073)	(0.0064)	(0.0069)
Post treatment dummy	0.023***	0.0042	0.0062
	(0.0075)	(0.0058)	(0.0056)
Share of high skill labor	0.016***	0.014***	0.0079*
	(0.0038)	(0.0040)	(0.0044)
Log sales per employee	0.23***	0.28***	0.33***
	(0.011)	(0.0054)	(0.0029)
Log capital stock per employee	0.038***	0.028***	0.031***
	(0.0069)	(0.0033)	(0.0017)
Constant	3.05***	2.60***	2.18***
	(0.099)	(0.11)	(0.16)
Observations	75,602	1,517,379	1,609,051
Adjusted R^2	0.131	0.119	0.132

Cluster robust standard errors (firm level) in parentheses.* p<0.1, ** p<0.05, *** p<0.01
Firm, year, regional, and industry fixed effects included.

In all regressions, the CEM-based estimates tend to be lower than the treated-only and full population results. This suggests that after matching, the potential bias, which stems from the fact that subsidized firms perform better than the average firm before receiving treatment, has been reduced. Considering the reduction in potential bias, the CEM-based estimates on subsidy effects should be more accurate than the other estimates. There is also a clear difference between the average effect in Tables 6–9 and the time-dependent effect in Figures 1–4.

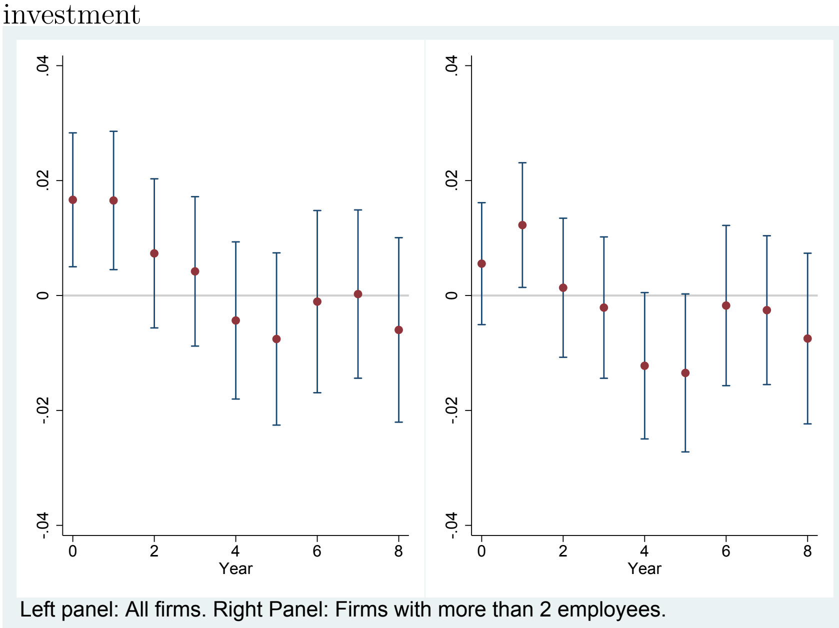
Figure 1 shows an increase in wage costs during the year the firm receives the subsidy and during the following year. However, there is no long-run effect, as the coefficients are insignificant for all years after the second. We interpret this increase

Figure 1: Yearly diff-in-diff treatment effect estimates for outcome variable wage expenditures



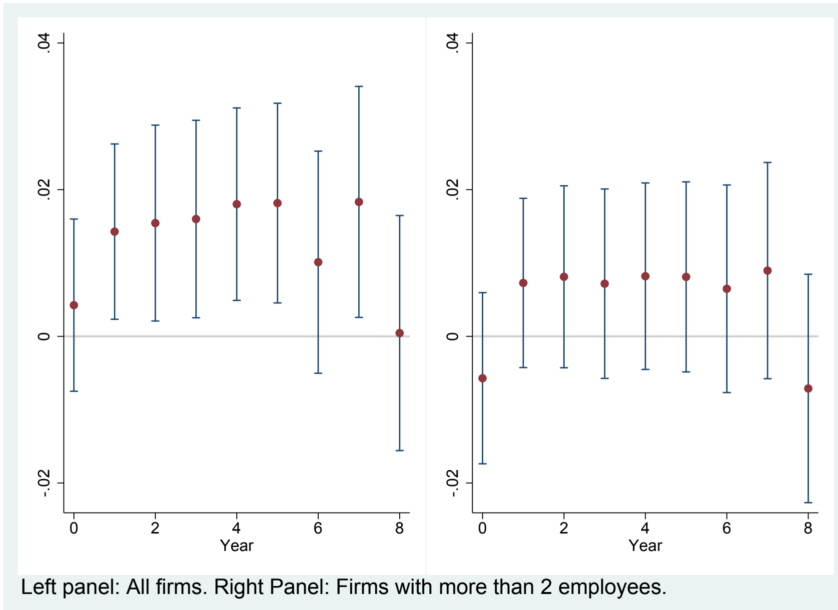
Notes: CEM-based point estimates and 95 % confidence intervals based on cluster robust standard errors.

Figure 2: Yearly diff-in-diff treatment effect estimates for outcome variable gross investment



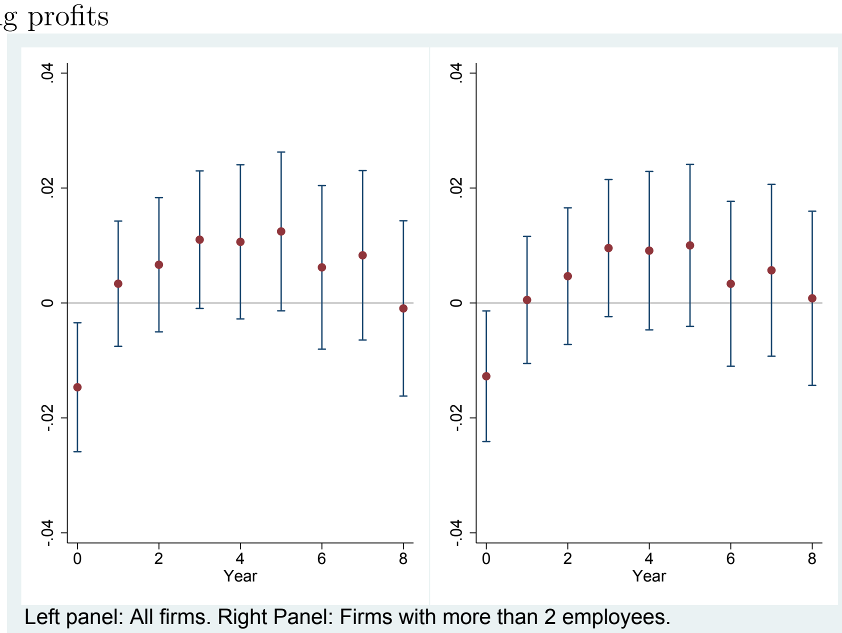
Notes: CEM-based point estimates and 95 % confidence intervals based on cluster robust standard errors.

Figure 3: Yearly diff-in-diff treatment effect estimates for outcome variable productivity



Notes: CEM-based point estimates and 95 % confidence intervals based on cluster robust standard errors.

Figure 4: Yearly diff-in-diff treatment effect estimates for outcome variable gross operating profits



Notes: CEM-based point estimates and 95 % confidence intervals based on cluster robust standard errors.

Table 7: Outcome variable: log gross investments

	Only treated	CEM	Full pop
Treatment effect	0.26***	0.22***	0.28***
	(0.021)	(0.022)	(0.021)
Post treatment dummy	-0.18***	-0.082***	-0.13***
	(0.021)	(0.019)	(0.017)
Log employees	0.17***	0.16***	0.11***
	(0.024)	(0.016)	(0.0059)
Share of high skilled labor	0.021**	0.0083***	0.0014
	(0.0097)	(0.0031)	(0.0011)
Log sales	-0.033*	-0.058***	-0.052***
	(0.019)	(0.011)	(0.0050)
Log capital stock per employee	0.89***	0.85***	0.84***
	(0.021)	(0.016)	(0.0048)
Constant	-2.01***	-1.25*	-0.73
	(0.31)	(0.70)	(0.79)
Observations	61,433	1,077,132	1,140,030
Adjusted R^2	0.123	0.095	0.097

Cluster robust standard errors (firm level) in parentheses. * p<0.1, ** p<0.05, *** p<0.01
Firm, year, regional, and industry fixed effects included.

in wage cost not as a temporarily increase in the employees' wages, but as the firms using their subsidies to hire consultants and educate and train their employees. This increase thus could be seen as investment in human capital, which is an explicit goal of many of the subsidy programs. The effect on physical investment (see Figure 2) is similar but more drastic. Investments are significantly higher the year the firm receives a subsidy, but drop afterward and even become negative. The fact that the increase occurs the same year as the firm receives the subsidy indicates that the firms have correctly anticipated the subsidy and made plans in advance. The negative effects the following year might be due to the subsidy triggering early investing. Productivity, as measured by value added per labor (see Figure 3), exhibits a different pattern. Here, the effects are small but significant for years 1 to 5 and 7, but not for year 8. We interpret this as meaning that investments in human and physical capital are paying off. Firms that receive a subsidy invest and, after some time, this investment pays off and we observe slightly higher productivity compared to the control group for a while. But since the increase in investment was short-lived, the effects on productivity also diminish as other firms catch up, or the investment depreciates.[8] However, this finding no longer holds when firms

[8]This effect could also stem from the accounting rules discussed previously.

Table 8: Outcome variable: log value added per labor.

	Only treated	CEM	Full pop
Treatment effect	-0.012*	0.0076	-0.014**
	(0.0066)	(0.0063)	(0.0064)
Post treatment dummy	0.025***	0.019***	0.025***
	(0.0070)	(0.0058)	(0.0055)
Share of high skilled labor	0.014***	0.014***	0.0090**
	(0.0027)	(0.0038)	(0.0042)
Log capital stock per employee	0.32***	0.34***	0.35***
	(0.0073)	(0.0034)	(0.0016)
Constant	3.87***	3.73***	3.58***
	(0.11)	(0.10)	(0.18)
Observations	74,272	1,494,407	1,584,842
Adjusted R^2	0.149	0.151	0.163

Cluster robust standard errors (firm level) in parentheses.* p<0.1, ** p<0.05, *** p<0.01
Firm, year, regional, and industry fixed effects included.

with only two employees are dropped from the sample, because then the treatment effects on productivity become insignificant (see right panel in Figure 3). Hence, the entire effect on productivity is driven by these very small firms with two employees.[9] Turning finally to the effects on profits (see Figure 4), the only significant result is that firms that receive subsidies have lower profits than the control group the year they receive the subsidy, but afterward there is no significant difference. It could indicate that firms that are financially constraint by having less internal financial resources (low profit) are more likely to receive the subsidy. However, a trend similar to that for productivity is visible and much stronger when firms with two employees are included. The summary of results in Table 10 clearly shows that, on average, the long-term result from innovation subsidies is either zero or, in some cases, even negative. The average effect captured in Tables 6–9 is misleading; the effects shown in Figures 1–4 are more accurate since the treatment effect diminishes over time. The positive results for productivity are also completely dependent on the inclusion of firms with two employees.

4.4 Robustness Checks

To ensure the robustness of the results we run the same regressions on three different size categories and also by which agency the subsidies were granted. Using

[9]The share of these very small firms in the total sample is around 11 percent, and sample size drops from 8,636 to 7,689 firms when those firms are excluded.

Table 9: Outcome variable: Log of gross operating surplus

	Only treated	CEM	Full pop
Treatment effect	-0.018***	-0.017***	-0.018***
	(0.0059)	(0.0060)	(0.0057)
Post treatment dummy	-0.0051	0.0026	-0.0012
	(0.0060)	(0.0051)	(0.0048)
Share of high skilled labor	-0.0043**	-0.0036***	-0.0018*
	(0.0020)	(0.0012)	(0.0011)
Log capital stock per employee	0.17***	0.17***	0.17***
	(0.0052)	(0.0023)	(0.0011)
Constant	-1.06***	-1.04***	-1.06***
	(0.078)	(0.14)	(0.089)
Observations	72,971	1,469,326	1,557,927
Adjusted R^2	0.059	0.061	0.061

Cluster robust standard errors (firm level) in parentheses.* p<0.1, ** p<0.05, *** p<0.01
Firm, year, regional, and industry fixed effects included.

Table 10: Summary of short- and long term treatment effects from innovation support

	Inputs (ATE)		Performance (ATE)	
	Wages	Investments	Productivity	Gross Margin
Short term	+	+	+	0
Long term	0	-	0	0

+ means a significant positive, - a significant negative effect at 1 percent level, 0 no effect.
Based on Tables 6-9 and Figures 1-4 with CEM matching results, ATE=average treatment effect

OECD's definition of firm size, we run regressions based on whether the firm has 10 or fewer employees, more than 10 but fewer than 250, or more than 250. The results are summarized in Table 11 and reveal positive productivity effects for the micro firms only when firms with exactly two employees are included. Interestingly, we find no long-run productivity results for firms with 3–10 employees. We test for differences in the two major agencies by running regressions for firms receiving subsidies from either Vinnova or SAERG. There were too few firms receiving subsidies from SEA to analyze on their own. The results, in Table 12, show no long-term differences between the two agencies. However, perhaps it is the size of the subsidy that matters. We thus rerun the regressions with the size of the subsidy divided by the firm's capital stock instead of the treatment dummy. The results for the post-treatment dummies do not change, indicating an absence of nonlinear effects.

Table 11: Long-term effects from innovation subsidies for various firm sizes

	Inputs (ATE)		Performance (ATE)	
	Wages	Investments	Productivity	Gross Margin
Micro firms≤ 10 empl	0	0/-[a]	0/+[b]	0
SME:s≤ 10 ≥ 250empl	0	0	0	0
Large firms≥ 250empl	0	0	0	0

Notes from the previous Table 10 apply. ATE=average treatment effect. [a]Negative years 1-6, zero otherwise. [b]Positive if firms with 2 employees are included, insignificant otherwise.

Table 12: Long-term effects from innovation subsidies for different granting agencies

	Inputs (ATE)		Performance (ATE)	
	Wages	Investments	Productivity	Gross Margin
Vinnova	0	0	0	0
SAERG	0	-	0	0

Notes from the previous Table 10 apply. ATE=average treatment effect. Vinnova=Swedish Innovation Agency. SAERG=Swedish Agency for Economic and Regional Growth

4.5 Discussion

The political rationale for providing financial subsidies to increase firm performance is based on the assumption that market failures hamper the development of innovative firms. One such so-called market failure is that banks and other private actors such as venture capital funds are too risk averse, given the great uncertainty of innovative projects, to invest in these firms. However, the existence of government innovation support programs implies that these agencies are able to identify those innovative firms suffering from financial constraints, and have appropriate incentives to implement the policies required. The robust political economy perspective (RPE) that public programs must be information and incentive compatible in order to be efficient suggests that programs designed to increase innovation should be evaluated often to see if they are meeting these information and incentive requirements.

Our treatment effect analysis using a unique database on innovation subsidies in Sweden shows a lack of robust long-run effects, although there are positive short-run effects. Positive results on productivity are visible only when the smallest firms are included in the analysis. Regarding investments, the effects are in some cases even negative. Our qualitative study of the agencies involved finds that they have little or no understanding of the information and incentive problems, and have taken no action to remedy these difficulties, findings that make the econometric results not surprising. The combination of theory and qualitative investigation, along with traditional robustness checks, strengthen our results.

Whether the main drivers behind the results are information problems, incentive problems, or both is something that cannot be addressed by this analysis. It should be pointed out, however, that we estimate only the direct effects on the subsidized firms. There may be positive spillover effects on other firms, cluster effects, or other nonobservable positive effects from the subsidies that were not discovered in the econometric analysis. The results of our study suggest that treatment effects should be evaluated over a longer time and provide for the possibility of a changing treatment effect over time. The positive average effect is misleading since the results diminish with time. Thus, the use of time-dependent effects is important when evaluating the effects of subsidies and should be used in future research. Our results also suggest that only the smallest firms should be subsidized.

Given the lack of evidence of positive effect from these subsidies, it seems clear that new innovation support programs should be designed differently than the current versions. There is a risk that firms will become proficient at accessing money rather than creating value through innovation in the market, which further reduces the benefits of these programs (Baumol, 1990). Cost-benefit analyses of these programs need to take into account the deadweight loss associated with taxation required to raise the necessary funds (Feldstein, 1999) and the opportunity costs incurred by firms when filling out applications for the subsidies. Instead of relying on subsidies, innovation policy should focus more heavily on entrepreneurial processes in which existing (or new) knowledge is combined with individual abilities in the search for new market opportunities (Braunerhjelm and Henrekson, 2015).

5 Conclusions

In the European Union, innovation is considered the key driver of economic growth, which is why increasing innovation was a core element of the Lisbon summit and the Horizon 2020 strategies. Many governments have programs that provide subsidies to firms to spur their innovation activities. The political rationale for giving public money to private firms is based on the assumption that, due to information asymmetries, market failures hamper innovative firms' access to private funding such as bank loans or venture capital. Against this backdrop, this study evaluates whether such policies are successful in improving economic performance of firms. We use Sweden as a case study within the European Union and employ treatment effect analysis using a unique database on firms and subsidies. The results show positive effects from subsidies on firms' short-run wage expenditures, and negative effects on investment during the post-treatment period. A positive effect on firm productivity is found only for smallest firms with two employees, but not for the rest of the firms. Thus, drawing an analogy with the fitness industry, rather than providing firms with protein supplements to create stronger and more efficient firms, subsidies are more like candy, giving the firm a short-term sugar rush with no long-lasting effects.

We draw the following conclusions from our analysis. First, agencies are apparently not aware that their programs suffer from information and incentive problems, and thus the absence of long-lasting positive effects on economic performance for the majority of firms is not surprising. Second, this implies that the current Swedish innovation subsidy programs are not successful and need to be either carefully redesigned or abandoned. Third, all innovation programs should be evaluated by external, independent parties using state-of-the-art techniques. Overall, our study casts doubt on the widespread belief that innovation subsidies boost firm performance in the long run, and therefore direct innovation subsidies to firms should not be emphasized in future EU strategies for increasing growth. One exception is the group of smallest firms, though a detailed analysis of this is left for future research.

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Appendix

Tables

Table 13: Summary statistics treated vs. non-treated by Vinnova

	Observations[1]	Mean	Median	Std. Dev.
Non-Vinnova				
Number of employees	1,686,869	14	4	107
Wage costs per employee	1,686,869	130	124	122
Share of high skill labor	1,545,187	.44	0	1.7
Gross investments	1,686,869	1,388	45	27,923
Net sales	1,686,869	29,015	3,982	318,829
Capital stock	1,686,869	39,946	2,220	872,429
Total amount support received	1,686,869	0	0	0
Vinnova				
Number of employees	6,976	111	12	586
Wage costs per employee	6,976	171	159	187
Share of high skill labor	6,641	1	1	1.38
Gross investments	6,976	8,664	216	86,563
Net sales	6,976	246,225	13,147	1,889,777
Capital stock	6,976	564,831	11,492	7,488,790
Total amount support received	6,968	531,838	256,778	862,769

Notes: Summary statistics for treated and non-treated firms between 1997-2011. All monetary figures in real SEK. [1]Firm-year.

Table 14: Summary statistics treated vs. non-treated by SAERG

	Observations[1]	Mean	Median	Std. Dev.
Non-SAERG				
Number of employees	1,686,869	14	4	107
Wage costs per employee	1,686,869	130	124	122
Share of high skill labor	1,545,187	.44	0	1.7
Gross investments	1,686,869	1,388	45	27,923
Net sales	1,686,869	29,015	3,982	318,829
Capital stock	1,686,869	39,946	2,220	872,429
Total amount support received	1,686,869	0	0	0
SAERG				
Number of employees	72,793	27	6	349
Wage costs per employee	72,793	130	128	61.1
Share of high skilled labor	68,136	.34	0	.58
Gross investments	72,793	1,833	122	32,548
Net sales	72,793	41,824	6,970	438,544
Capital Stock	72,793	32,551	3,770	352,576
Total amount support received	72,793	221,232	79,792	696,564

Notes: Summary statistics for treated and non-treated firms between 1997-2011. All monetary figures in real SEK. SAERG = Swedish Agency for Economic and Regional Growth. [1]Firm-year.

Table 15: Summary statistics treated vs. non-treated by SEA

	Observations[1]	Mean	Median	Std. Dev.
Non-SEA				
Number of employees	1,686,869	14	4	107
Wage costs per employee	1,686,869	130	124	122
Share of high skill labor	1,545,187	.44	0	1.7
Gross investments	1,686,869	1,388	45	27923
Net sales	1,686,869	29,015	3,982	318,829
Capital stock	1,686,869	39,946	2,220	872,429
Total amount support received	1,686,869	0	0	0
SEA				
Number of employees	1,339	211	19	491
Wage costs per employee	1,339	173	159	281
Share of high skill labor	1,267	1.3	1	6.78
Gross investments	1,339	99,897	936	382,198
Net sales	1,339	897,993	31,324	2,972,483
Capital Stock	1,339	3,227,531	50,384	1.67e+07
Total amount support received	1,339	3,236,547	693,799	1.08e+07

Notes: Summary statistics for treated and non-treated firms between 1997-2011. All monetary figures in real SEK. SEA = Swedish Energy Agency. [1]Firm-year.

Table 16: Correlations matrix

	Support relative to C	Log of w/l	Log of inv.	Log of va/l	Profits	High skill l	Log capital /l	Log of l	Log of sales /l
Support relative to capital stock	1								
Log of wage cost per employee	-0.00234**	1							
Log of gross investments	0.00165	0.165***	1						
Log of value added per labor	-0.00841***	0.516***	0.342***	1					
Log of gross operating surplus	-0.0118***	-0.332***	0.202***	0.466***	1				
Share of high skilled labor	0.00108	0.0508***	-0.00922***	0.0512***	0.00307***	1			
Log of capital stock per employee	-0.00778***	0.213***	0.410***	0.547***	0.368***	0.0415***	1		
Log of employed	-0.00184*	0.229***	0.526***	0.205***	0.00534***	-0.00837***	0.0944***	1	
Log of sales per employee	-0.00720***	0.359***	0.255***	0.586***	0.248***	0.00851***	0.549***	0.200***	1