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The Causal Effects of Climate Investment Subsidies: Regression Discontinuity Evidence from Swedish Firms*

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Abstract

This paper provides new empirical evidence on the firm-level economic impacts of a Swedish grant program that subsidizes firms' investments in greenhouse gas emissions-reducing projects. We exploit a discontinuity in the assignment mechanism in the program to estimate causal effects of investment subsidies on investment additionality and firm growth. Combining highly-detailed data on the details of each application with detailed firm-level balance sheet data on investment and other aspects of firm performance provides a range of new results. Our estimates suggest that the grant scheme led to increases in investment, turnover and the number of employees. The results indicate a substantial crowding-in effect on investment.

Keywords: Abatement, additionality, investment grants, subsidies, climate policy.

JEL Codes: D22, Q52, Q54.

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1 Introduction

Increasing concern over climate change and the political difficulties associated with the feasibility of pricing carbon have led some governments to introduce alternative approaches, such as subsidy schemes, in their policy portfolio (Fabra and Reguant, 2024). However, it is an open question whether abatement subsidies are an effective climate policy tool. A common concern regarding abatement subsidies is that they may be a relatively inefficient method of reducing greenhouse gas (GHG) emissions if they are subsidizing investments that would have occurred without the subsidy, known as a lack of "additionality" (Criscuolo et al., 2019). Despite its obvious importance, there is a dearth of studies evaluating the additionality of abatement subsidies.

In an effort to fill this important research gap, this study evaluates the causal effects of a Swedish subsidy scheme named Klimatklivet (Swedish for "the climate leap"), historically one of Sweden's major policies targeting GHG emissions. The scheme provides grants to financially support local climate investments made by Swedish businesses and other organizations, subsidizing a fraction of the investment cost. By the end of 2018, the government had already spent 4.7 billion SEK on grants to about 3,200 different local projects aimed at decreasing GHG emissions.

There are often two main problems in evaluating the additionality of investment subsidy programs. One difficulty is to disentangle the impact of the subsidy from other confounding factors. Another problem lies in the selection bias of the firms that are awarded a subsidy stemming from the policy assignment mechanism. For example, if the assignment mechanism targets firms with characteristics that consistently differ from the firms not receiving financial aid, the estimated effect by a comparison of the groups would be biased. Alternatively, if the financed firms would have made the investment regardless of the subsidy, the effects would most likely be overestimated. The latter is often of high interest for the policymakers to ensure that public funds are used efficiently.

In this study, we identify the causal effects of emission-reducing investment subsidies on firm-level investment and other aspects of firm performance. This is possible by exploiting the assignment mechanism in the Klimatklivet program as a quasi-experiment.

The assignment mechanism ranks all applications based on the projects' predicted carbon dioxide equivalent (CO2e) reduction per invested SEK. Only projects with a CO2e/SEK above a specified cutoff received funding. The cut-off was unexpectedly changed after the closing of three application periods in 2016 and 2017. This allows us to exploit a sharp regression discontinuity (RD) approach in the analysis. We show that our design satisfied the required validity assumptions, lending itself well to a RD analysis and allowing us to estimate the Local Average Treatment Effects (LATE) of the firms with grant applications close to the threshold.

Our analysis combines highly-detailed data on the details of each application with detailed firm-level balance sheet data on investment and other aspects of firm performance. Crucially, we observe both approved and rejected grant applications in the data, and each application's ranking in terms of emissions reductions per invested SEK. The timing of the application rounds that we study in 2016 and 2017 permits the use firm-level data to control for pre-treatment characteristics and also follow the impacts until completion of the construction phase.

Our analysis provides a range of new results. We find that the subsidy scheme led to increases investment, the number of employees, and turnover. Our most conservative estimates suggest that the program increased firm-level total net investment by 12–15 percent and firm-level machinery net investment by 12–17 percent. Our results are robust to a variety of specifications and robustness checks. We show that our analysis satisfies the criteria required for a RD design, implying that our estimates can be given a causal interpretation.

We compare our estimates of investment impacts with the size of the grants and the total investment costs of the projects, performing some simple back-of-the envelope calculations. We find that the estimated magnitude of the investment effects exceed the value of the grants by a factor of three on average. We also find that the investment effects exceed the total cost of the projects by at least 50 percent on average, suggesting a sizable crowding-in of investment.

We contribute to a small and recent literature evaluating the additionality of subsidized investment grants directed at businesses and organizations for the purpose of reducing

GHG emissions. A prominent example is the work by Calel et al. (2024), which evaluates the additionality of the Clean Development Mechanism (CDM) in the context of wind power projects in India. The authors find that a majority of projects would have gone ahead even without funding. In contrast to our work, their study lacks data on rejected applications, but convincingly infers additionality by studying nearby wind power projects that were not funded by the CDM offset scheme. Marino et al. (2021) uses data on manufacturing firms receiving an abatement subsidy in the Netherlands between 1999 and 2011, and finds significantly increased abatement and environmental R&D investment, again without data on rejected applications.

Our work is complementary to a vast literature on subsidies directed at households and individuals, including studies on the impact of subsidies for electric vehicles (Li et al., 2017; Springel, 2021; Muehlegger and Rapson, 2023; Sheldon et al., 2023) and residential energy efficiency programs (Allcott and Greenstone, 2012; Fowlie et al., 2018; Gilbert et al., 2022). A general finding across all these studies is that investment subsidies are not cost effective, with implied abatement costs per tonne far in excess of existing carbon pricing schemes.

Our work is also builds on existing studies that evaluate the impact of R&D and regional development grants on firm-level investment using RD designs. Santoleri et al. (2022) find that the Small and Medium Enterprise Instrument led to sizable increases in R&D spending and patenting among European firms. Howell (2017) finds significant effects of the US Department of Energy's SBIR grant program on firms' future venture capital funding, patenting, and revenue. Bronzini and Iachini (2014) evaluate Italian R&D firm subsidies implemented after Law no. 7/2002, art. 4', and find positive investment effects on small firms. Bronzini and de Blasio (2006) evaluate an Italian investment subsidy, finding evidence of increasing investments in the short run but a substantial decrease in long-term investment rates. Evaluating the same Italian investment scheme, Cerqua and Pellegrini (2014) find positive subsidy effects on employment, investment, and turnover, and also find that the subsidy satisfies the criteria of "additionality". Decramer and Vanormelingen (2016) use an RD design to estimate the effects of a firm investment subsidy policy in Flanders designed to increase the competitiveness of small and medium-sized firms in Flanders, and find small positive effects on investment, employment, output, and productivity for the smaller subsidized firms but not the medium-sized firms.[1]

Our analysis provides the first rigorous study of the causal effects of a granting scheme that subsidizes firms' investments in greenhouse gas emissions-reducing projects. There is a dearth of evidence on the impacts of such granting schemes, despite similar programs in other countries.[2] Isberg et al. (2017) evaluated Klimatklivet by order of the Swedish EPA using a descriptive approach, and found that firms receiving grants reduced their GHG emissions. Isberg et al. (2017) also found an overall improvement of air quality, acidification, ozone layer, over-fertilization, living forests, and health. Klimatklivet has also been evaluated using anonymous survey evaluations by both the Swedish National Audit Office (RiR 2019:1) and Pädam et al. (2021). These studies did not make use of the quasi-experimental setting in the assignment mechanism of Klimatklivet to estimate causal effects using an RD design. Moreover, the data from the subsidy program has not been combined with panel data of financial statements of the firms applying for Klimatklivet, contributing to the novelty of our approach.

The rest of the paper is organized as follows. We first describe the details of Sweden's climate investment subsidy scheme in section 2. We then describe the data and provide descriptive statistics in section 3. Our empirical strategy and its validity are explained in section 4. The main results and further robustness exercises are described in section 5. A discussion of the results, potential limitations, and a back-of-the-envelope calculation of the program's value-for-money in terms of investment additionality are provided in section 6. Conclusions follow in section 7.

[1] In a related vein, Dechezleprêtre et al. (2023) and Chen et al. (2021) evaluate the impact of tax incentives for R&D in the U.S and China respectively using RD designs, and find positive effects on R&D spending.

[2] Examples of similar granting schemes in other countries includes the Carbon Credit Unit (ACCU) Scheme in Australia, the Low Carbon Economy Fund (LCEF) in Canada, the Energy Aid Program in Finland, the National Climate Initiative (NKI) in Germany, and clean energy grants at the federal and state levels in the U.S.

2 Sweden's climate investment subsidy scheme

The Swedish government established the Klimatklivet policy in 2015 to support local climate initiatives with financial aid to eligible organizations. Between 2015 and 2023, the program had awarded grants with a total value of SEK 14.8 billion to over 20 thousand projects (Swedish Environmental Protection Agency, 2024). Klimatklivet subsidized 41 percent of the total investment costs on average during this period, implying that the program was associated with local climate investments totaling SEK 35.7 billion.

The goal of the Klimatklivet policy is to fund investment projects with the highest efficiency by selecting the projects with the highest reduction of CO2e emissions per invested SEK. All types of organizations are eligible to apply for a subsidy in Klimatklivet, (i.e. firms, municipalities, housing cooperatives, etc) if they want to make a GHG emission-reducing investment. Organizations across all sectors are eligible to apply[3]. There are multiple calls for applications each year, ranging from 2–4 application rounds per year. Organizations can submit their applications when a call is open, and each round is roughly open for four weeks. The timing of the application rounds in the first three years of the program are provided in Table 1.

Table 1: Threshold levels and timing of application rounds, 2015–2017

Application round	Threshold, kg CO2e/SEK	Application period
2015:1	1.00	Jun 29 – Sep 15
2015:2	1.00	Nov – Dec 17
2016:1	**0.75**	**Feb 15 –Mar 14**
2016:2	**1.00**	**May 16 – Jun 13**
2016:3	1.00	Aug 29 – Sep 26
2017:1	1.00	Jan 9 – Jan 31
2017:2	**0.75**	**Mar 10 – Apr 3**
2017:3	0.75	Aug 7 – Sep 11
2017:4	0.75	Oct 9 – Nov 9

Notes: Bold indicates the application rounds used in the analysis. Source: Swedish EPA.

Applications are only taken into consideration if the following conditions are fulfilled: i) the investment project cannot be profitable in less than five years; ii) the investment

[3]The empirical analysis in this study will exclusively use applications from firms. Firms receive more than 77% of the funds allocated by the policy.

project cannot qualify for the electricity certificate system for renewable electricity production; iii) the investment project cannot commence before being granted funding under Klimatklivet; and iv) the organizational activity cannot be covered by EU Emissions Trading System (ETS).[4] Organizations are allowed to apply for multiple projects in the same application round and apply multiple times. They are also allowed to re-apply for projects that have been denied subsidies in previous application rounds.

The Swedish EPA and the County Administrative Boards administer the application process and the County Administrative Boards assist the organizations with their applications. The organizations submit detailed information about the project for which they seek funding, including investment costs, the estimated lifespan of the investment, and the estimated annual emission reductions. Applying is free of charge, but the administrative process of putting an application together is time-consuming. All applications are assessed by the Swedish EPA and any misreported calculation errors are corrected by the agency. When evaluating the applications, the EPA sometimes adjusts the CO2e reduction and lifespan numbers before calculating each project's expected CO2e reduction per invested SEK.

The Swedish EPA administer the selection process to award the most efficient investments based on the CO2e reduction per invested SEK. All applications are ranked only based on this metric. All applications with a CO2e/SEK above a determined cut-off point are financed and the projects below the cut-off point are not. The threshold is determined after the call for applications is closed.

The threshold levels are listed in Table 1. The threshold has never been pre-determined nor has it been announced before any application round has closed. However, information about the threshold level in the previous application rounds has been available for those organizations seeking the information.

As presented in Table 1, the cut-off was set to be 1 kg of CO2e reduction per invested SEK in the first three consecutive application rounds. When the application for the third round (2016:1) closed, the Swedish EPA decided to lower the cut-off to 0.75 kg

[4] Additionally, less common reasons for not being eligible are: v)Lack of implementation capacity, vi) Pilot study or research, vii) The investment project must be carried out in accordance with law, iix) The investment project starting date has already taken place, and ix) Insufficient firm financing.

CO2e/SEK. The cut-off was again suddenly increased to 1 kg CO2e/SEK in round 2016:2, then indefinitely lowered back to 0.75 kg CO2e/SEK beginning with round 2017:2, where it has remained ever since.[5] Applicants were not informed about changes to the cut-offs in advance. This analysis is thus restricted to the three rounds experiencing a change to the cutoff (2016:1, 2016:2, and 2017:2).

Although firms participating in Klimatklivet are not covered by the EU ETS, they are exposed to the Swedish carbon tax, which has been in place in 1991 and is levied on all fossil fuels in proportion to their carbon content. At the beginning of our study period in 2015 the general tax rate and the nominal tax rate for industry outside the EU ETS was SEK 1050/tonne and SEK 630/tonne respectively.[6] As of 2018 the industry rate converged with the higher general rate. The Swedish carbon tax was SEK 1190/tonne at the end of our study period in 2020.

3 Data and descriptive statistics

The empirical analysis in this paper relies on data from two databases that are combined at the application level. The first dataset includes information about all applications to the Klimatklivet policy. The data is retrieved from the application software (KlivIT), which is generated and managed by the Swedish EPA. The second dataset is longitudinal firm-level register data retrieved from the *Longitudinal Integrated Database for Health Insurance and Labour Market Studies* (LISA) from Statistics Sweden. LISA includes all firms in Sweden with at least one employee, and contains annual balance sheet information on firm characteristics including investment, profit, fixed assets, turnover, number of employees, sector, and firm age (truncated at 30). We use this firm-level data for the years 2015–2020, allowing us to follow firm-level outcomes for at least three years after the grant decision. Attrition is very low in among the firms included in the analysis.[7]

[5]Fast charging stations used by the general public and all charging stations for workplaces and residences were subject to a lower CO2e/SEK threshold, equal to 75 percent of the normal threshold (0.6 kg CO2e/SEK in 2016:1 and 2017:2, and 0.8 kg CO2e/SEK in 2016:2).

[6]Swedish plants in the EU ETS are exempt from the Swedish carbon tax. See Martinsson et al. (2024) for more details on the Swedish carbon tax and the its impact on firm-level emission intensity.

[7]Firm-level emissions are only available for a small number of observations in another register, making it impossible to estimate the impact on emissions.

The KlivIT data consists of detailed data from all Klimatklivet applications, including both granted and rejected applications. The application software records the date of application submission and details about the climate investment project provided by the firms. The data includes information on the firm organization number of the applicant, whether or not a grant was awarded, the expected emission reductions per year in kg CO2e, investment costs in SEK, expected lifespan of the investment, the share of the total investment cost that is to be subsidized, and the expected date of project completion. Specific details on the type of investment the firm wants to make are also included in the application.

The analysis is restricted to applications from three application periods (2016:1, 2016:2, and 2017:2) that can be matched with the firm characteristics data and that meet several other conditions. We restrict the sample to applications that are either granted or rejected solely on the basis of their expected CO2e reduction per invested SEK. This means that we do not include applications that were rejected for other reasons that disqualified them, nor do we include poorly written applications. A small number of applications were withdrawn prior to decision or awarded but later rescinded by the EPA and repayment was demanded; these cases were also removed from the analysis. Finally, we restrict the analysis to firms that are active and have at least one employee one year before and one year after applying for funding. This last restriction reduces the number of observations from 447 to 406. For the subset of observations close to the threshold (running variable between -0.249 and $+0.249$), this last restriction reduces the number of observations from 125 to 115. Employment is the outcome with the fewest non-zero observations, while there are more observations taking a value of zero in the case of investment.

Summary statistics for the application data in the analysis are reported in Table 2. Of the 406 applications in the data, 198 were granted and 208 were rejected. The average subsidy request is SEK 1.4 million, and is roughly similar between granted and rejected applications. Applicants requested about half of the total investment cost from the program. The CO2e reductions per invested SEK for granted applications had a mean of 4.96 kg/SEK and a median of 1.25 kg/SEK.

Descriptive statistics for firm-level characteristics in the data are reported in Table 3.

Table 2: Application descriptive statistics

Variables	Obs.	Mean	SD	p10	Median	p90
Requested subsidy, SEK thousands						
Granted	198	1235	2739	26	300	2970
Rejected	208	1562	3233	70	291	4100
Total	406	1403	3003	40	300	4100
Total cost of investment, SEK thousands						
Granted	198	2405	6090	53	562	5940
Rejected	208	2934	6903	120	568	6100
Total	406	2676	6517	80	568	6100
Subsidy fraction of investment in percent						
Granted	198	55	18	40	50	92
Rejected	208	57	21	37	50	100
Total	406	56	19	40	50	100
CO2e reductions per invested SEK						
Granted	198	4.96	10.46	0.72	1.25	14.11
Rejected	208	0.34	0.23	0.00	0.29	0.65
Total	406	2.59	7.65	0.11	0.65	3.47

Notes: Based on observations included in Panel A column (1) of Table 7.

Firms that applied for funding are small and medium-sized on average. A t-test of equal means reveals that the firm-level characteristics of granted versus rejected applications are not statistically difference from each other in all respects except age in the year before applying. Firms submitting rejected applications are about three years younger than granted firms on average.

Table 3: Firm descriptive statistics

Variables	Granted		Rejected		T-test, equal means	
(SEK millions unless noted)	Mean	Median	Mean	Median	Difference	p-value
Total Investment	66.9	17.5	62.5	11.4	-4.40	0.781
Machinery Investment	31.0	6.6	31.1	4.4	0.06	0.992
Employees	198.7	40.0	224.8	40.0	26.10	0.571
Turnover	1035.9	239.9	1286.4	155.8	250.46	0.247
Profit	76	16	138	13	61.93	0.060
Fixed Assets	1235.0	129.0	1788.2	36.9	553.25	0.297
Age	18.4	21.0	15.8	17.0	-2.67**	0.001
Observations	198		208		406	

Notes: Firm-level summary statistics for all applications included in the analysis one calendar year prior to applying. Based on observations included in Panel A column (1) of Table 7.

A tabulation of the number of rejected and granted applications by project category is reported in Table A.1 in the Appendix. All 10 main categories used by the Swedish EPA are represented in the analysis. The top three project types were charging stations (223 applications), followed by energy conversion (82 applications) and energy efficiency (28 applications).

Firms' applications contain details about the estimated completion date of construction of their proposed project. We use this information for calculating the outcome variables, such as accumulated investment during the project, as well as other outcomes in the year of project completion. A tabulation of the number of applications by construction time, in calendar years, is provided in A.2 in the Appendix. About one third of projects are competed in the same calendar year. Most projects are completed during the next calendar year. Very few projects take three years or more to complete.

4 Empirical strategy and validity

4.1 Empirical strategy

To credibly estimate the effects of subsidies it is crucial to disentangle the effect of the subsidy from possible confounding effects and control for potential selection bias induced by the assignment mechanism. This is often challenging due to the design of the firm subsidy programs. For example, it is often the case that the policy is designed to only subsidize the most efficient firms or projects to make the most use of taxpayer money. In the Klimatklivet context, this implies that the climate initiatives that mitigate the greatest amount of CO2e emissions per invested SEK receive the subsidies and the least efficient investments are not subsidized. In general, the assignment of treatment is not random; therefore, the financed and non-financed firms potentially have differences in the unobserved characteristics that are correlated with the outcome of interest.

This study aims to investigate if a climate investment subsidy affects firm-level investments and economic performance. The ideal way to explore this relationship would be if some firms are randomly assigned to receive a climate investment subsidy while the other firms do not. Comparing the mean outcomes of the funded firms with the non-funded firms would yield causal effects of the climate investment aid. Since the selection processes of the policy award the investment projects with the most cost-efficient GHG reductions, it is not credible to assume random assignment of treatment. The firms that are awarded a subsidy also have the most cost-efficient GHG mitigation scores. It is probable that the financed and non-financed firms differ in unobserved characteristics other than treatment status, and a comparison of mean outcomes would not yield the causal effects of the climate investment aid.

Fortunately, we are able to exploit sudden changes to the assignment rule in order to identify causal effects, using a sharp RD approach and comparing the firms of the investment projects close to the cut-off. Thistlethwaite and Campbell (1960) introduced RD where treatment is approximately assigned randomly close to a cut-off (Imbens and Lemieux, 2008; Lee and Lemieux, 2010).

The firms scoring on either side of the cut-off can be assumed to be similar for application rounds 2016:1, 2016:2 and 2017:2. Despite their similarities, firms scoring above the threshold are granted funding and the firms scoring below the threshold are denied funding, and the potential difference in outcomes between funded and non-funded firms can be explained by the grant.

We estimate the following equation using an OLS estimator:

$$y_i = \beta_0 + \beta_1 granted_i + f(CO2perSEK_i) + \beta_2 y_i^{pre} + \lambda_{round} + \lambda_{category} + \lambda_{ind} + \epsilon_i \quad (1)$$

where y_i denotes the outcome for application i, $granted_i$ is an indicator variable taking a value of one if application i is funded and zero otherwise, and $CO2perSEK_i$ is the centered CO2e reductions per investment cost of application i. Importantly, $f(CO2perSEK_i)$ is continuous at the threshold and is a linear or quadratic polynomial allowed to differ on each side of the threshold.[8] y_i^{pre} is included to control for the outcome the year prior to the application. λ_{round} control for application round-specific fixed effects, $\lambda_{category}$ control for project category fixed effects, and $\lambda_{industry}$ control for industry fixed effects. ϵ_i is the error term.

Following Lee and Lemieux (2010), the regressions are estimated using different bandwidths. Running the regressions with infinite bandwidth, i.e. using the full sample, will not result in credible estimates. Since the previous threshold was public information, it is possible that some firms self-select into treatment by submitting an application just above the threshold used in the previous application period. A narrow bandwidth close to the threshold also yields less biased estimates (Calonico et al., 2020). The main concern with this approach is the limited number of observations potentially lowers the precision of the estimate, especially if it is combined with a small sample as in the case of the Klimatklivet data. Restricting the bandwidth to exclude firms that attempted to self-select into treatment (based on cutoff in the previous application round) implies a bandwidth of ±0.249 centered around the threshold.

[8]This approach followsLee and Lemieux (2010) and Gelman and Imbens (2019).

4.2 Validity

To assess if there are any differences between firms above the threshold and thus obtaining the subsidy, and the firm below the threshold, we test if there are any differences in observable variables prior to treatment. Table 4 reports the results of the balancing tests. The results indicate that the sample is balanced with respect to all of the outcome variables, as well as with respect to firm age.

Table 4: Balancing tests

	(1) Total Invest.	(2) Machinery Invest.	(3) Turnover	(4) Employees	(5) Profit	(6) Fixed Assets	(7) Age
RD_Estimate	1.415	0.931	-0.434	-0.320	0.581	0.281	-3.802
	(0.919)	(0.900)	(1.616)	(0.893)	(1.942)	(0.965)	(3.975)
Observations	86	85	115	115	88	112	115

Notes: The dependent variable in each column is logged value in year $t-1$. All columns include a quadratic polynomial of the running variable. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Validity also rests on the assumption that firms cannot self-select into the group of granted applications. This could occur if some firms obtain information about the change to the threshold and submit applications with a GHG reduction per invested SEK that is just above the threshold. Knowing the threshold would allow firms to apply for the largest grants possible for a given project. In the data, any such manipulation would reveal itself through a heaping of applications just above the threshold. The McCrary (2008) manipulation test does not reject the null of no manipulation with respect to the running variable based on original applications to Klimatklivet, illustrated in Figure A.1 in the Appendix. However, the manipulation test may suffer from a lack of power. Nonetheless, visual inspection of the histogram in Figure A.1 suggest no such manipulation.

In its evaluation of prospective projects, the Swedish EPA sometimes adjusts the expected CO2e reduction to better reflect reality. A histogram of the running variable after EPA adjustments is illustrated in Figure A.2 in the Appendix. The CO2e/SEK for several approved charging station projects were adjusted by evaluators at the EPA to equal exactly the cut-off value. Importantly, this bunching is not caused by firms' manipulation

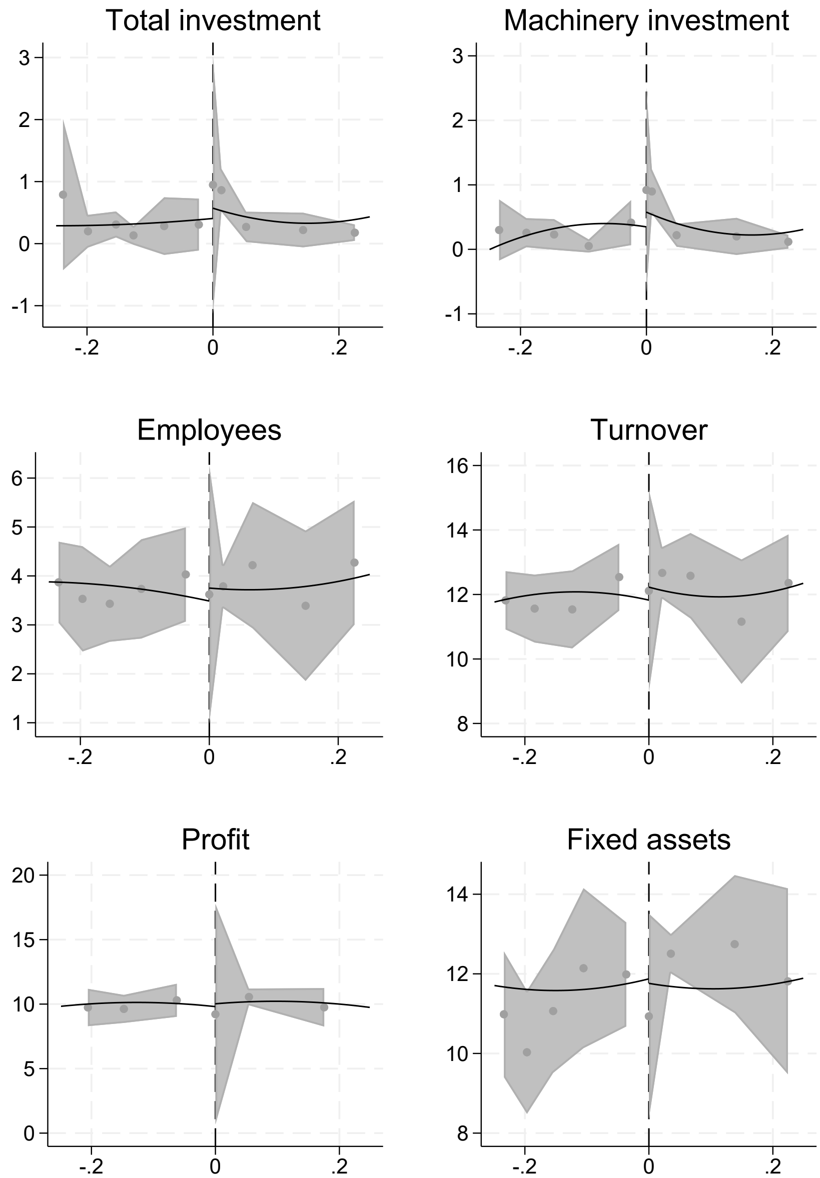

Figure 1: RD plots for outcome variables

Notes: RD plots for applications with a running variable between -0.249 and $+0.249$. The dependent variable for total investment and machinery investment is the is the accumulated net investment between t and up to $t+3$ (depending on each proposed project's expected completion date), divided by fixed assets in year $t-1$. The dependent variable for is the log of turnover in the year the investment project ends. Dots represent means of the firm-level outcomes within each quantile bin, following Calonico et al. (2015). Fitted lines from local polynomial regressions with a quadratic fit and 95 percent confidence intervals are also reported.

by firms. We base the running variable on the EPA-adjusted CO2e/SEK throughout the entire analysis.

5 Results

5.1 Main results

Before presenting the econometric results we first present graphical evidence of discontinuity in post-grant outcomes. Figure 1 uses a bandwidth of -0.249 and $+0.249$ and quadratic polynomial regressions estimated separately on both sides of the threshold. The graphs suggest positive effects of receiving a grant on investment, employees and turnover, although the effects appear small relative to the underlying variability of the outcome variables.

Table 5 reports the estimated RD results of the investment grants on growth in total investment. We calculate investment growth in two ways. The dependent variable in columns (1) and (2) is the change in the log of total investment between year $t-1$ and $t+1$. The dependent variable in columns (3) and (4) is the is the accumulated net investment between t and up to $t+3$ (depending on each proposed project's expected completion date), divided by fixed assets in year $t-1$. Using accumulated investment scaled by fixed assets follows the approach used by Santoleri et al. (2022). Odd-numbered columns include a linear polynomial of the running variable, while even-numbered columns include a quadratic polynomial. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Finally, Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ± 0.249 bandwidth. All specifications include lagged $(t-1)$ total investment and application period, project category, and 5-digit industry fixed effects.

The results in Table 5 provide evidence of a positive impact of receiving a grant on growth in terms of log growth and as a share of fixed assets. The estimates in column (2), Panel C of Table 5 suggest a 0.49 log point increase in total investment due to receiving a grant, equivalent to a $[exp(0.49) - 1] \times 100 \approxeq 63$ percent increase in investment. The estimates in column (4), Panel C of Table 5 suggest a 0.14 log point increase in total

Table 5: Effects on total investment

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	-0.06	0.15	0.05	0.07
	(0.13)	(0.17)	(0.07)	(0.07)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	295	295	316	316

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.34***	0.38***	0.11*	0.09
	(0.12)	(0.14)	(0.06)	(0.08)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	87	144	82	145

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.35***	0.49***	0.11*	0.14**
	(0.12)	(0.16)	(0.06)	(0.07)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	82	82	86	86

Notes: The dependent variable in columns (1)–(2) is the change in the log of total net investment between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the accumulated total net investment between t and up to $t+3$ (depending on each project's expected completion date), divided by fixed assets in year $t-1$. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) total investment and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

investment due to receiving a grant, equivalent to a $[exp(0.14) - 1] \times 100 \approxeq 15$ percent increase in investment.

The estimates using accumulated investment scaled by fixed assets yield the most conservative results compared to the estimates using log investment growth. Using accumulated investment also leads to slightly more observations in columns (3) and (4), as non-positive values are included. In contrast, the estimates in columns (1) and (2) require positive investment in years $t-1$ and $t+1$. The estimates using a strict bandwidth between -0.249 and $+0.249$ are the most conservative in the sense that they ensure that they exclude observations that may have self-selected on the previous year's CO2e per invested SEK threshold. For these reasons, the estimates in columns (3) and (4) of Panel C are thus our preferred specifications.

Table 6 reports the estimated estimated RD results of the investment grants on growth in machinery investment, using the same structure as the previous table. The results suggest a positive impact of receiving a grant on growth in the value of machinery investment, but only in terms of log growth. The estimates in columns (2) and (4) of Panel C of Table 6 suggest a 17 percent to 77 percent increase in machinery investment due to receiving a grant, depending on the choice of outcome variable. Since many of the projects supported by Klimatklivet involve machinery investments, this result makes intuitive sense.

Table A.3 in the Appendix reports the estimates with respect to property investment (which includes land and buildings). The sign and magnitude of the estimates varies greatly between columns. The vast majority of observations are zero in the case of land and building investment, leading to a very small number of observations and volatile estimates. The results reported in Table A.3 should thus be interpreted with caution.

Tables 7, 8, 9, and 10 report the estimated effects with respect firm-level turnover, employees, profits and total fixed assets respectively. The dependent variable in columns (1) and (2) is the change in the log growth of the outcome between year $t-1$ and $t+1$. The dependent variable in columns (3) and (4) is the change in the log of the outcome between year $t-1$ and the year the investment project is completed. The dependent variable in columns (5) and (6) is the log of the outcome in the year the investment project is completed. Odd-numbered columns include a linear polynomial of the running variable,

Table 6: Effects on machinery investment

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.11	0.26	0.05	0.08
	(0.15)	(0.21)	(0.07)	(0.06)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	283	283	308	308

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.69***	0.83***	0.11*	0.10
	(0.11)	(0.17)	(0.06)	(0.10)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	80	143	86	136

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.66***	0.57***	0.11*	0.16**
	(0.11)	(0.11)	(0.06)	(0.08)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	78	78	85	85

Notes: The dependent variable in columns (1)–(2) is the change in the log of machinery net investment between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the accumulated machinery net investment between t and up to $t+3$ (depending on each project's expected completion date), divided by fixed assets in year $t-1$. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) machinery investment and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

while even-numbered columns include a quadratic polynomial. Panel A presents the estimates using all available observations, Panel B presents the estimates using bandwidths selected using Calonico et al. (2020), and Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ± 0.249 bandwidth. We use the same controls as the investment regressions.

We find large positive effects of receiving a grant on turnover and the number of employees when focusing on observations near the cutoff, (Panels B and C of Tables 7 and 8) with relatively stable point estimates across specifications. The point estimates across all columns of Panel C of Table 7 suggest that receiving a grant increased employment by between 52 percent and 60 percent, depending on the choice of outcome variable. Similarly, the point estimates across all columns of Panel C of Table 8 suggest that receiving a grant increased turnover by between 49 percent and 97 percent, depending on the choice of outcome variable. In contrast, we find no effects on profits and fixed assets.

The results in Panels A and C of Tables 7–10 suggest that attrition bias is not an issue, as the number of observations is stable despite using outcomes at time $t+1$ versus the year of project end (up to $t+3$). In Panel C of Tables 7 and 8, for example, the number of observations is 115 for all specifications.

5.2 Robustness

As demonstrated above, the results for investment turnover and employees are robust to the measure used. Reassuringly, our findings of robust effects with respect to investment employment and turnover suggest that the study does not critically suffer from problems of low power. However, it is well-known that RD designs typically require more observations than standard Difference-in-difference approaches to reach the same level of statistical power, which could nonetheless be a factor in the less precise results for other outcome variables.

In the Appendix we perform falsification tests with placebo thresholds 0.05 above and below the true threshold. In the case of total investment and machinery investment (Table A.4), 3 out of 16 estimates are statistically significant and with the opposite sign. It is evident that the estimates using scaled investment perform better in the placebo tests, as

Table 7: Effects on number of employees

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log employees at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.12	0.05	0.08	0.06	0.10	0.10
	(0.15)	(0.09)	(0.13)	(0.08)	(0.13)	(0.09)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	406	406	403	403	403	403

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log employees at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.49***	0.43***	0.43***	0.41***	0.45***	0.46***
	(0.14)	(0.16)	(0.13)	(0.14)	(0.15)	(0.16)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	106	115	115	116	116	116

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log employees at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.45***	0.43***	0.43***	0.42***	0.45***	0.47***
	(0.15)	(0.16)	(0.13)	(0.14)	(0.15)	(0.16)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	115	115	115	115	115	115

Notes: The dependent variable in columns (1)–(2) is the change in the log of the number of employees between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the change in the log of employment between year $t-1$ and the year the investment project ends. The dependent variable in columns (5)–(6) is the log of employment in the year the investment project ends. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) employment and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 8: Effects on turnover

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log turnover at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.12	0.15	0.13	0.18	0.16	0.20
	(0.12)	(0.12)	(0.11)	(0.12)	(0.11)	(0.12)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	404	404	403	403	403	403

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log turnover at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.39***	0.43***	0.51***	0.47***	0.59***	0.62***
	(0.11)	(0.14)	(0.09)	(0.13)	(0.11)	(0.15)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	116	126	95	125	95	126

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log turnover at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	0.40***	0.50***	0.43***	0.53***	0.50***	0.68***
	(0.11)	(0.12)	(0.11)	(0.12)	(0.13)	(0.13)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	115	115	115	115	115	115

Notes: The dependent variable in columns (1)–(2) is the change in the log of turnover between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the change in the log of turnover between year $t-1$ and the year the investment project ends. The dependent variable in columns (5)–(6) is the log of turnover in the year the investment project ends. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) turnover and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 9: Effects on profits

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log profit at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.03	-0.04	0.05	-0.04	0.00	-0.08
	(0.08)	(0.11)	(0.06)	(0.08)	(0.07)	(0.10)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	328	328	325	325	325	325

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log profit at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.18*	-0.14	0.11	0.14	0.12	0.09
	(0.11)	(0.13)	(0.09)	(0.10)	(0.09)	(0.10)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	101	137	87	82	87	78

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log profit at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.15	-0.09	0.12	0.14	0.13	0.15
	(0.11)	(0.11)	(0.09)	(0.11)	(0.09)	(0.11)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	82	82	82	82	82	82

Notes: The dependent variable in columns (1)–(2) is the change in the log of profit between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the change in the log of profit between year $t-1$ and the year the investment project ends. The dependent variable in columns (5)–(6) is the log of profit in the year the investment project ends. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) turnover and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table 10: Effects on total fixed assets

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log fixed assets at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.02	-0.02	0.03	0.09	0.08	0.14*
	(0.05)	(0.05)	(0.07)	(0.06)	(0.08)	(0.09)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	375	375	375	375	375	375

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log fixed assets at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.03	0.01	-0.15	-0.16	-0.19	-0.13
	(0.06)	(0.07)	(0.11)	(0.14)	(0.12)	(0.15)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	93	114	112	122	103	181

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log fixed assets at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
RD_Estimate	-0.03	0.00	-0.15	-0.17	-0.17	-0.19
	(0.06)	(0.07)	(0.11)	(0.14)	(0.12)	(0.14)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic
Observations	111	111	112	112	112	112

Notes: The dependent variable in columns (1)–(2) is the change in the log of fixed assets between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the change in the log of fixed assets between year $t-1$ and the year the investment project ends. The dependent variable in columns (5)–(6) is the log of fixed assets in the year the investment project ends. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) turnover and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

none of the estimates in columns (3) and (4) of Table A.4 are statistically significant. In the case of other outcomes (Table A.5), none of the estimates are statistically significant, regardless of the choice of outcome variable.

6 Discussion and investment additionality

Overall, we find positive results on investment, turnover and the number of employees. We do not find conclusive evidence for an impact on fixed assets or profits. The positive estimates for investment suggest evidence for additionality from the program. Effects on turnover and employment suggest that the grants lead to firm expansion. The lack of effect on fixed assets is somewhat puzzling, but may stem from the fact that the stock of fixed assets in firms tends to be relatively large compared to annual total investment (SEK 1.24 billion versus SEK 67 million in Table 3), and is driven mainly by factors unrelated to receiving a grant. The lack of an effect on firm profits is not surprising, given that projects that are profitable within five years are not eligible.

Using these most conservative estimates of investment additionality from columns (3) and (4) of Panel C in Tables 5 and 6 suggests that the program increased firm-level total net investment by 12–15 percent and firm-level machinery net investment by 12–17 percent. We can perform some back-of-the-envelope calculations to gauge the value-for-money provided by the program. We first combine the estimated percent change in investment with the pre-grant mean investment levels from Table 2. Using the mean machinery investment (SEK 31 million) implies that investment increased by between SEK 3.7 million and SEK 4.7 million for the average firm. Comparing this with the mean requested subsidy among granted applications from Table 2 (SEK 1.24 million) suggests that our most conservative estimates imply investment additionality far in excess of the subsidy itself. Comparing instead to the average total cost of the investment among granted firms (SEK 2.4 million) suggests a sizable crowding-in of investment in the order of at least 50 percent.

Our analysis does not include emissions as an outcome variable due to a lack of observations with available firm-level emissions data for the three application periods used in the analysis. The reason is that it is only mandatory for industrial firms (manufacturing and mining) with 10 employees or more to complete the *Energy Use in Industry Survey*, which is necessary to calculate firms' CO2e emissions. In contrast, the data on balance sheet outcomes that we use (investment, turnover, number of employees, etc.) is readily available for all Swedish firms.

7 Conclusion

The purpose of this study is to evaluate the additionality of the Klimatklivet climate investment grant program. Using a sharp RD design, the results suggest that receiving a grant resulted in a significant increase in total investment and machinery investment. Our estimates also suggest large positive effects on firm growth, as measured by turnover and the number of employees. Given the validity of the RD design that we employ, we argue that the robust effects on investment, employment and turnover are indeed causal.

The results have important policy implications for climate granting schemes not only in Sweden but also in other countries. Our results suggest that the policy intervention provides investment additionality, with a substantial crowding-in effect on investment. This result implies that such granting schemes have the potential to encourage investment needed to reduce GHG emissions.

One potential shortcoming of this analysis is that we lack data on firm-level emissions for firms exposed to this quasi-experiment. Our conclusions regarding additionality of the investment grant scheme are thus restricted to investment additionality. A study of the impact on emission outcomes would facilitate an analysis of the cost-effectiveness of the program in terms of the cost per tonne CO2e, which is an important policy question. We thus leave an evaluation of the impact of Klimatklivet on emissions and its implications for the cost-effectiveness of the program for future work. As evidence on the effectiveness of climate investment granting schemes is scarce at present, we hope that our work inspires future research on this particular policy instrument.

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A Appendix

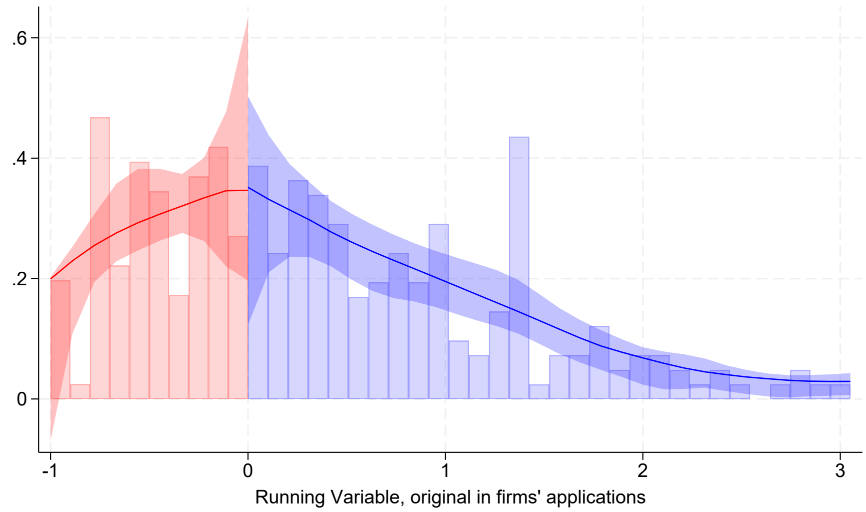

Figure A.1: Histogram of running variable and manipulation test based on original applications to Swedish EPA.
Notes: Based on observations included in Panel A column (1) of Table 7. Implements manipulation testing and graphical procedures proposed in Cattaneo et al. (2020) and Cattaneo et al. (2021).

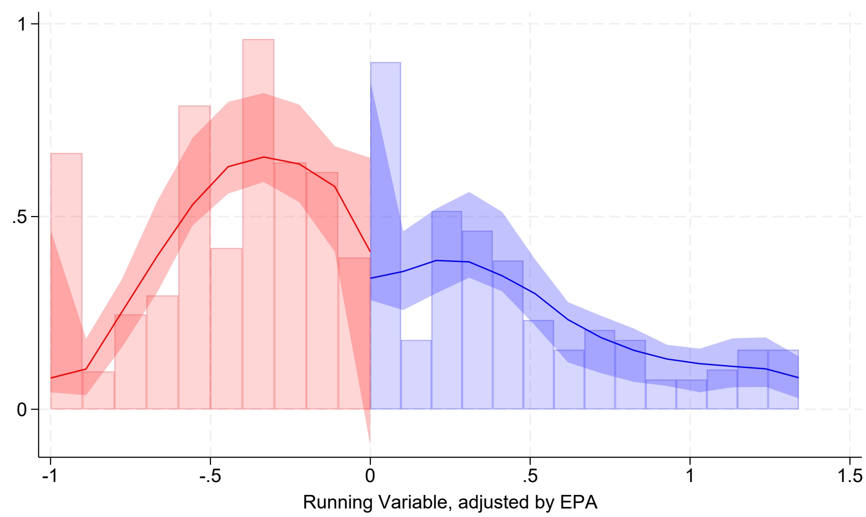

Figure A.2: Histogram of running variable and manipulation, adjusted by Swedish EPA.
Notes: Based on observations included in Panel A column (1) of Table 7. Implements manipulation testing and graphical procedures proposed in Cattaneo et al. (2020) and Cattaneo et al. (2021).

Table A.1: Number of rejected and granted applications by project category

Category description	Rejected	Granted	Total
Charging stations	120	103	223
Energy conversion	21	61	82
Energy efficiency	25	3	28
Transport	1	25	26
Vehicles	7	0	7
Information	6	0	6
Gas emissions	1	3	4
Biogas production	2	2	4
Infrastructure	2	1	3
Other	23	0	23
Total	208	198	406

Notes: Based on observations included in Panel A column (1) of Table 7.

Table A.2: Number of applications by project length, in calendar years

Project length (years)	Number of applications
0	130
1	143
2	111
3	18
4	3
5	1
Total	406

Notes: Project length is calculated as expected calendar year that construction is completed for the investment project, minus the calendar year of the project start. Based on observations included in Panel A column (1) of Table 7.

Table A.3: Effects on property investment

Panel A: All observations

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.62	1.98***	0.09***	0.11***
	(0.43)	(0.53)	(0.02)	(0.02)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	58	58	82	82

Panel B: Calonico et al. (2015) bandwidth selection

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	0.16	90.38***	0.01**	-0.42***
	(0.20)	(0.09)	(0.00)	(0.01)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	24	19	24	28

Panel C: ±0.249 bandwidth

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
RD_Estimate	-8.96***	56.48***	-0.03***	-0.20***
	(0.07)	(0.00)	(0.01)	(0.00)
Polynomial	Linear	Quadratic	Linear	Quadratic
Observations	17	17	28	28
R_squared				

Notes: The dependent variable in columns (1)–(2) is the change in the log of property (land and buildings) net investment between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the accumulated property net investment between t and up to $t+3$ (depending on each project's expected completion date), divided by fixed assets in year $t-1$. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. Panel A presents the estimates using all available observations. Panel B presents the estimates using bandwidths selected using Calonico et al. (2020). Panel C presents the estimates for firms with applications 0.249 around the cut-off, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) property investment and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.4: Placebo tests, investment outcomes

Panel A: Total investment

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
runningvar−0.05	0.16	0.25	-0.05	0.03
	(0.31)	(0.36)	(0.07)	(0.09)
runningvar+0.05	-0.14	-0.63***	0.05	-0.08
	(0.14)	(0.19)	(0.08)	(0.13)
Polynomial	Linear	Quadratic	Linear	Quadratic

Panel B: Machinery investment

	Log growth, $t-1$ vs $t+1$		As share of fixed assets	
	(1)	(2)	(3)	(4)
runningvar−0.05	0.17	-1.10**	-0.02	0.09
	(0.37)	(0.46)	(0.07)	(0.10)
runningvar+0.05	-0.22	-0.12	-0.02	-0.06
	(0.13)	(0.27)	(0.10)	(0.15)
Polynomial	Linear	Quadratic	Linear	Quadratic

Notes: This table presents the results of implementing placebo running variables that are shifted 0.05 above or below the true running variable. The dependent variable in columns (1)–(2) is the change in the log of total investment (Panel A) or machinery investment (Panel B) between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the sum of total or machinery investments in years t and $t+1$ (not logged), divided by fixed assets in year $t-1$. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. All specifications include applications 0.249 around the placebo running variables, i.e. ±0.249 bandwidth. All specifications include lagged ($t-1$) dependent variable and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.

Table A.5: Placebo tests, other outcomes

Panel A: Employment

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log employees at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
runningvar−0.05	0.12	0.28	0.03	0.12	0.05	0.16
	(0.16)	(0.20)	(0.14)	(0.18)	(0.15)	(0.20)
runningvar+0.05	0.06	-0.14	0.07	-0.07	0.05	-0.09
	(0.22)	(0.21)	(0.19)	(0.19)	(0.21)	(0.20)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Panel B: Turnover

	Log growth, $t-1$ vs $t+1$		Log growth, $t-1$ vs project end		Log employees at project end	
	(1)	(2)	(3)	(4)	(5)	(6)
runningvar−0.05	0.04	0.33	-0.10	0.05	-0.10	0.11
	(0.14)	(0.23)	(0.13)	(0.20)	(0.15)	(0.25)
runningvar+0.05	-0.01	-0.02	0.05	0.09	-0.01	0.04
	(0.17)	(0.19)	(0.16)	(0.17)	(0.18)	(0.20)
Polynomial	Linear	Quadratic	Linear	Quadratic	Linear	Quadratic

Notes: This table presents the results of implementing placebo running variables that are shifted 0.05 above or below the true running variable. The dependent variable in columns (1)–(2) is the change in the log of the number of employees (Panel A) or turnover (Panel B) between year $t-1$ and $t+1$. The dependent variable in columns (3)–(4) is the change in the log of employment or turnover between year $t-1$ and the year the investment project ends. The dependent variable in columns (5)–(6) is the log of employment or turnover in the year the investment project ends. Odd-numbered and even-numbered columns include linear and quadratic polynomials of the running variable respectively. All specifications include applications 0.249 around the placebo running variables, i.e. ± 0.249 bandwidth. All specifications include lagged ($t-1$) dependent variable and application period, project category, and 5-digit industry fixed effects. Based on observations included in Panel A column (1) of Table 7. Standard errors, clustered at the firm level, in parentheses. *** p<0.01, ** p<0.05, * p<0.1.