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Do R&D Subsidies Cause Better Access to External Financing?

Empirical Evidence from the Research Council of Norway's Grant Programs

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Abstract

Public funding of R&D has become a popular policy instrument to alleviate financial constraints for innovation and entrepreneurship. This master's thesis analyses whether public R&D subsidies lead to better access to external financing. Using data on grant applications to the Research Council of Norway in the period between 2010 to 2020, we find grants to have a strong, positive impact on the growth in capital for financially constrained firms. We address endogeneity concerns by using a regression discontinuity design. In particular, we exploit ranks in application grades for as-if-random assignments around a threshold for grant approval. Receiving a grant more than doubles young and small ventures' probability of raising equity the first year after application, from 20 percent to 52.8 percent, while the likelihood of raising long-term debt in the same period increases from 18.1 percent to 33.4 percent. Testing for heterogeneous treatment effects, we also find grants to increase the probability of subsequent long-term debt financing for knowledge-intensive firms.

Keywords: Research and Development, R&D Subsidies, Innovation, Public Policy Instruments, Financial Constraints, Regression Discontinuity Design

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1. Introduction

R&D subsidies have become a popular policy instrument to encourage innovation and growth in modern economies. The rationale stems from a disbelief in the free-market ability to provide financing for new ventures and socio-economic projects (Hall & Lerner, 2010). Recent evaluations of Norwegian public subsidy programs contend that the current system is too complex and inefficient (Deloitte et al., 2019; NHO, 2019). While the direct input and output additionalities of R&D support programs have received much scrutiny, little empirical research has been devoted to how subsidies can stimulate better access to capital for financially constrained firms.

This master's thesis provides empirical evidence on Norwegian R&D support programs' effect on external financing for Norwegian companies in the period between 2010 and 2020. Using data from the Research Council of Norway (RCN), we find grants to significantly impact small start-ups while showing little effect on large and mature companies. Limiting the sample to new knowledge-intensive industries, like IT, we also discover a positive effect on long-term debt among grant recipients.

For causal inference, we apply a Sharp Regression Discontinuity Design (RDD). By exploiting ranking in applicants' grades, the quasi-experimental approach allows for an as-good-as-random estimate when comparing firms immediately around a threshold of grant approval. If a significant discontinuity between grantees and non-grantees is detected, the average effect of public grants can be estimated as a jump in the outcome variable. To the best of our knowledge, our study is the first to establish a causal effect of R&D support on external financing for grant recipients in Norway.

A grant from RCN increases the probability of receiving external equity financing for small start-ups from 20 percent to 52.8 percent the first year after grant approval. This effect is consistent the four following years as well. For the same firms, a grant also increases the probability of raising long-term debt from 18.1 percent to 33.4 percent the first year after grant approval. These findings show that the RCN support programs have a substantial impact on mitigating financial constraints for small start-ups. For IT firms, a grant from RCN increases the possibility of a rise in long-term debt from 23.1 percent to 44.3 percent the first year after grant approval.

2. Background

Subsidy programs are widely used in Europe and abroad, and are typically actualized through grants, tax credit schemes, loans, or loan guarantees. In Norway, there is an extensive network of government-backed initiatives to facilitate R&D activity in both the public and private sectors. The private sector alone has received more than 100 billion NOK in grants and tax credits from the Norwegian government between 2010 and 2020, according to data from Statistics Norway (2021). More than 70% of this funding is directed toward start-ups and business-oriented R&D through The Research Council of Norway (RCN), Innovation Norway (IN), and SkatteFUNN.

Despite billions of NOK being channeled through the Norwegian policy agencies, a commission from 2018 found several aspects of the risk capital markets to be limiting for entrepreneurship and innovation in Norway (NOU 2018:5, 2018). Most notably are the financial constraints facing small and early-stage businesses with capital requirements of up to 20 million.[1] Also, in a comprehensive review of the Norwegian public support system from 2019, reports criticized the system for being too complex and inefficient (Deloitte et al., 2019; NHO, 2019). While the review, initiated by the Ministry of Trade, Industry, and Fisheries, found potential for improvements, financial constraints among the subsidy participants received little scrutiny.

A more recent commission, assessing the economic prospects of post-pandemic Norway, calls for more and better empirical research on the public support measures (NOU 2021:4, 2021). Many profitable businesses have suffered heavy losses to their equity because of the pandemic and will require a capital build-up going forward. Simultaneously, there is an ongoing shift to restructure the Norwegian economy toward a climate-sustainable state. This involves a gradual downscaling of its multibillion-dollar petroleum industry. The Ministry of Finance points out that an effective capital market will be an essential catalyst for the transition (Meld. St. 14, 2021). Thus, an accurate public support system that stimulates private investments in R&D projects and new ventures seems more important than ever.

[1] In addition to risk capital, young firms are often in need of relevant expertise from external owners. A Norwegian study from 2010 found that among wealthy entrepreneurs, the wealthiest performed worse, implying a need for expertise from other external owners (Hvide & Møen, 2010).

3. Literature Review

The impact of public R&D subsidies on firms' ability to attract external capital has seen little research devoted to it. A substantial part of the evaluation literature is dedicated to measuring the impact on output additionality, like patenting activity, in subsidized firms. Studies from Japan, Italy, and New Zealand show that R&D grants significantly impact the number of patents filed by a firm (Branstetter & Sakakibara, 2002; Bronzini & Piselli, 2016; Le & Jaffe, 2017). Yet, these findings are not universal, as evidence from South American and Norwegian programs suggests no impact on patenting activity (Maffioli & Hall, 2008; Raknerud, Rybalka, & Cappelen, 2012). Other output additionalities, such as employment, productivity, and value creation, have also been addressed in a more recent study by Nilsen et al. (2020), where they find younger R&D firms to outperform larger and more mature firms.

Other studies have focused on firm input additionality of support programs, such as money spent on R&D, and address whether support schemes stimulate or potentially crowd out these types of investments (Lach, 2003; González & Pazo, 2008; Almus & Czarnitzki, 2003). Subsidies tend to have a positive impact on increased R&D expenditures in smaller firms, compared to mixed results and instances of crowding-out effects among larger enterprises, as found by Yu et al. (2016). Earlier research by Klette et al., (2000), addresses other potential spillovers effects while also highlighting the estimation problems of non-random samples in R&D subsidy programs.

This paper investigates financially constrained firms in Norway and how government-backed R&D programs can stimulate their ability to raise external financing, where we apply a quasi-experimental approach to measure the average effect of public R&D grants. A similar focus can be found in Meuleman and Maesenaire (2012). While they find strong evidence of a positive certification effect on debt-financing for Belgian SMEs, they only discover a positive effect on external equity financing in a subsample of start-ups. Following the footsteps of Meuleman and Maesenaire, Wu (2017) find Chinese private enterprises to be more likely to raise external debt financing than state-owned firms, despite the latter being able to receive more subsidies.

Our research is closest to that of Howell (2017), both econometrically and in terms of research focus. She examines the US Department of Energy's Small Business Innovation Research (SBIR) program, in which firms can apply for a two-phase award program. By employing a

Sharp Regression Discontinuity Design comparing companies immediately around an award cutoff, she identifies a causal relationship between phase one grants and subsequent financing. Howell finds the likelihood of receiving venture capital investments to increase from 10 to 19 percent among grant recipients.[2] As opposed to the research of Howell, we include a heterogeneous data source with companies outside the energy sector. Also, data from RCN consists solely of earmarked grants for R&D-specific purposes that are monitored through consecutive project reports. The SBIR program has no such enforcement ex-post.

A more recent report from 2018 on Norwegian support schemes over the period 2010-2016 finds a strong correlation between public support to start-ups and growth in new equity capital (Grünfeld et al., 2018). Our analysis goes further and addresses the problem of identification in R&D subsidies applying a more recent and recognized approach for causal inference among economists. While our study also adds to the existing public R&D programs evaluation literature, we provide new evidence of a positive causal effect on external financing among Norwegian start-ups.

In the following section, we lay the framework of discussion with our hypothesis development. Next, we present the data and descriptive statistics before the empirical method is assessed. Then, results are reported with an evaluation of heterogeneity and internal validity before we conclude the paper with a discussion of our findings.

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[2] While Howell (2017) goes further in estimating effects on other outcomes, we concentrate our research to the impact on external financing. An important notion, however, is that by exploiting empirical evidence on uninformative ranks (given by DOE officials) and the grant effect on patents she argues the impact on equity financing being a "proof-of-concept" effect rather than a stand-alone certification effect.

4. Hypothesis Development

Public spending has been a popular discussion topic in economic literature, especially through the pioneering works of John Maynard Keynes and Milton Friedman. Keynes' advocacy for economic planning and its challenger, the free market, 'laissez-faire' regime, supported by Friedman and fellow monetarists, has been well-articulated by Rivot (2013), among others. While most of their work on monetary and fiscal policy go beyond the scope of this paper, we find their main opposing views on public stimulus to be helpful when assessing the effect of government support programs.

Keynesian economists believe that short-term policies involving public spending can increase private investments through a multiplier effect (Rivot, 2013). On the other hand, monetarists oppose this view, arguing the significance of a crowding-out effect that ultimately leads to less private investments (Blinder & Solow, 1973). This idea of how public funding can both mobilize and discourage private investment motivates us to explore the impact of Norwegian support programs.

Today, opposing arguments of Friedman's free-market ideas are widely used by policymakers to justify subsidy programs of various kinds (Hall & Lerner, 2010). In the context of R&D, higher financial constraints for R&D projects are a well-established argument, as first articulated by Arrow (1962). It means there exists a gap between internal investments and external financing for innovative projects. Hall and Lerner (2010) propose that higher information asymmetries, risks of moral hazard, and tax considerations between external and internal capital are the main reasons why this funding gap exists.

For public subsidies to mitigate these constraints and crowd in external financing, Lerner (1999) among others, suggests a certification mechanism in which the government acts as a certifier for private investors and banks. The presumption is that when faced with high uncertainty, such as high information asymmetries between an entrepreneur and investor, the investor sees the grant as a signal of quality in the recipient, which alleviates some of the investment uncertainty.

Howell (2017) devises an additional funding mechanism of governmental support that works through an equity channel and a prototyping channel. A grant enables the entrepreneur to retain a larger stake of equity that she would otherwise have to sell to finance a new project. Thus, her incentives to commence R&D are higher, potentially reducing the moral hazards on the entrepreneur's part. In the prototyping channel, the public grant enables R&D initiatives that ultimately result in proof-of-concept work. The resulting technology will then alleviate some of the uncertainty and information asymmetry between the entrepreneur and potential investors.

If the certification mechanism and the funding mechanism truly mobilize capital for firms conducting R&D, it should be reflected in their balances. This line of reasoning implies that R&D subsidy recipients will have a higher probability of receiving external financing than that of non-subsidized firms. This also implies the opposite; a lower probability of raising external capital would be expected in cases where the mechanisms fail and public grants discourage investor activity. To test this two-sided conjecture, we constitute our main hypothesis as follows:

Public grants lead to a change in external financing for grant recipients.

This hypothesis can be developed further by specifying the type of financing we want to measure, as well as the population of interest. Debt financing is typically out of reach for small start-ups due to less collateral and financial history than their older and more mature competitors (Hall & Lerner, 2010). These firms are also, in general, more constrained because of higher information asymmetries. This suggests that public grants are more likely to influence equity financing for young and small ventures. This also implies that for larger and more mature firms with sufficient access to capital, the effect should be negligible in terms of their balances.[3]

A third important implication follows the intuition of both the aforementioned. Industries with higher asymmetries and lower collateral are more likely to be more constrained than their counterparts. This is typically the case for new knowledge-intensive industries, such as information and communications technology, with much of their value in intangible assets.[4] Thus, public grants are more likely to affect the financing of these firms compared to more classical capital-intensive industries, like petroleum, construction, and shipbuilding.

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[3] For most firms, after-tax cost of capital drives the wedge between the different capital sources, as proposed by Auerbach (1984). Historically, retained earnings have been preferred over debt, and debt over equity, suggesting that new endeavors for bigger corporations will be financed with either debt or withheld profits.

[4] According to data from 2015, 'new industries' in Norway have an average ratio of 26% in interest-bearing debt, while for 'classical industries' the same amounts up to 50% (Menon Economics, 2017).

In the following sections, we will direct our focus on the equity financing of small start-ups. Based on economic theory and literature, public grants are most likely to influence this type of financing and firms. For heterogeneity purposes and a broader research contribution, we will also incorporate debt as a measure of raising capital. For different population subsamples, we include that of large and mature firms, as well as classical industries and new industries.

5. Data

To investigate the impact of Norwegian subsidies, we use data on grant applications from the Research Council of Norway (RCN). Established in 1993 and governed by the Ministry of Education and Research, the council receives commissions from more than 15 different ministries and manages a budget of more than 10 BNOK annually. With the purpose of financing projects supporting science and innovation, the goal of the council is to promote and connect Norwegian business and science (Research Council of Norway, 2016). This is mainly done by proposing calls for grants in sectors and areas that align with key policy targets.

The data provided to us by RCN comprise of all grant applications, both approved and rejected, submitted to the council between 2010 and 2020. The data consists of 5,223 unique applications for 291 different proposal calls, of which 2,064 applications were granted an average amount of 6 MNOK, totaling to 12.4 BNOK.

We combine the data from RCN with financial data on the applicants retrieved from Proff Forvalt (2021).[5] The financial data comprise of all annual company accounts in the period between 2010 and 2020. Due to subsequent dissolutions and some missing entries from 2020, not all applicants have a complete set of financial records in that period. This is addressed in more details in section 7.3.5.

5.1 Normalized Application Grades

All applications submitted to the RCN are evaluated based on several different assessment criteria. These criteria are individually graded on a scale from 1 to 7 or letters A to C for every application by external experts serving as council referees. Conditional on the individual proposal call, these criteria vary and are weighted differently before calculating an application's final assessment grade. Scientific advisors of the council will propose a recommended ranking of the grant applications based on the referees' individual assessment and the overall assessment of all applications with respect to the individual proposal call. After which, a council portfolio board decides on the final approval or rejection of the applications.

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[5] Proff Forvalt is a subscription service available through the NHH Library that enables exporting extended company and accounting information on Norwegian firms registered in The Brønnøysund Register Centre.

This especially comes into play when there are more satisfactory grant applications than grants available. In some cases, applicants are also interviewed before a final decision is met.

For most proposal calls, a clear threshold determines acceptance or denial of grant for the given call. E.g., an application with an overall grade above the threshold yields an acceptance, while an overall grade below the threshold results in a rejection. For proposal calls where limited funds dictate the rejection of some satisfactory final application grades, similar grades may be subject to both acceptance and denial. To ensure a consistent set of ranking across different proposal calls, we normalize the grades conditional on the individual proposal calls. The highest-ranked rejection is scaled so that the application grade for firm *i* at the year of grant decision t is $X_{it} = -1$, and the lowest-ranked approval is scaled so that $X_{it} = 0$. Correspondingly, lower-ranked rejections further from the threshold of acceptance are scaled to $X_{it} = -2, -3, \ldots$, and $X_{it} = 1, 2, \ldots$ for higher-ranked approvals.

5.2 Measures of External Financing

As a measure of equity financing, we use accumulated paid-in capital of all firms for $\tau = 1, \ldots, 5$ years after the year of grant decision. Furthermore, we compute changes in paid-in capital from the year of grant decision t to τ years after, denoted as $y_{i\tau}$. Due to a large array of absolute changes in paid-in capital across firms, we find a binary variable indicating an increase/no increase in equity capital to be more appropriate. This can be denoted as $Y_{i\tau}$, where:

$$Y_{i\tau} = \begin{cases} 1 \text{ if } y_{i\tau} \geq z \\ 0 \text{ if } y_{i\tau} < z \end{cases} \tag{1}$$

To measure *external* equity financing, we employ a lower bound, z, to only regard $y_{i\tau}$ that are “high enough” to be increases in external equity. Paid-in capital is made up of capital stock, treasury stock, and paid-in capital excess of par, and an increase in this account suggests the company has issued equity for new capital or for existing debt.[6] In some cases, however, firms commence bonus issues, where unrestricted equity is transferred to the capital stock. While this is rarely the case for start-ups, a bonus issue will cause retained profits, including grants,

[6] Restructuring debt for equity does not contribute new capital but may increase paid-in capital significantly. We find this to be extremely rare in our data.

to show up in paid-in capital. [7] The lower bound z can mitigate these events and ensure a valid measure of external equity.

To identify z, we use data on risk capital investments provided by the Argentum Centre for Private Equity at NHH. Cross-checked with financial data from Proff Forvalt, we find an average increase in Norwegian targets' paid-in capital balances to be approximately 15% in the period between 2010 and 2016. This is consistent with a lower quartile of 15% for all stock issue sizes in Norway between 2011 and 2015 (Grünfeld, Grimsby, Hvide, & Høiseth-Gilje, 2018). To the best of our knowledge, a lower bound of $z = 15\%$, provide us with a good proxy for external equity financing. [8]

To measure external debt financing, we compute changes in long-term debt from grant decision year t to $\tau = 1, \dots, 5$ years after, denoted as $l_{i\tau}$. Like our proxy for equity, we account for the high variation in absolute changes by employing a binary variable for increases in long-term debt, denoted as $L_{i\tau}$. Note that long-term debt behaves differently from paid-in capital, in that it will decrease as installments and the principal amount matures. In addition, long-term debt is by definition *external* so that we can employ lower bound of zero.[9] The variable can be denoted as follows:

$$L_{i\tau} = \begin{cases} 1 \text{ if } l_{i\tau} > 0 \\ 0 \text{ if } l_{i\tau} \leq 0 \end{cases} \tag{2}$$

[7] Public grants are posted to the income statement and will affect the total equity account through retained earnings. Also, bonus issues are not considered as new equity.

[8] A complete set of VC and seed funding investments in the grant recipients would provide for an appropriate measure of external equity financing, as seen in Howell (2017). Unfortunately, such records are expensive and time-consuming to retrieve. Our measure will be at least as good a proxy, if not better, as it will also capture the effect of business angels and other types of investors. Note that we do not distinguish between existing owners and new owners, as both are regarded as external capital for the firm.

[9] We do not distinguish between loans from parent companies and third-party institutions, yet this should not be a concern as most of the observations are either the parent company or are not organized with subsidiaries.

5.3 Sample Selection and Descriptive Statistics

We limit the data to only comprise of commercial proposal calls. Additionally, to ensure an unbiased main sample of commercial firms, we only include the first application for every firm.[10] As our primary research focus is on young, small ventures conducting R&D, we distinguish between *young* and *mature* firms in the data set by applying a median split of 7 years. Similarly, we employ a ceiling of 5 MNOK in assets for *small* firms.[11] We will refer to this group as our *primary* subsample, consisting of small start-ups. Additionally, we create three *secondary* subsamples to test for heterogeneity. One group for large and mature firms, applying the same median split of 7 years and 5 MNOK in assets. Another two is categorized as classical or new industries, independent of firm size and age.[12] In Table 1, a detailed overview of the sample selection process is shown for the main sample with all firms (Panel A) and the primary subsample (Panel B).

Table 1: Sample Selection

Step	Description	Dropped	Sample size
Panel A: All firms			
1	Unique applications 2010-2020		5223
2	Remove applications without grade	41	5182
3	Remove applications without org.nr.	83	5099
4	Keep first application for each firm	2897	2202
5	Keep proposal calls with grant threshold*	792	1410
6	Keep commercial proposal calls	150	1260
Panel B: Small start-ups			
7	Keep firms ≤ 7 years of age	621	639
8	Keep firms with ≤ 5 MNOK in assets	280	359

**We allow for similar grades that are subject to approval/denial.*

[10] Some firms apply to several proposal calls. This is addressed further in section 7.2.2.

[11] Number of employees is also a popular measure of firm size. In our case, however, limiting the selection to low employee count would be less representative of the population in question. We use assets, since low-asset companies are more likely to face financial constraints than firms with few employees. For instance, capital-intensive industries like construction and real-estate have few employees and high assets, and will typically have easy access to new capital, primarily due to the pledgeability of these assets.

[12] Classical industries are typical capital-intensive firms, like petroleum, construction, and shipbuilding. New industries are the typical silicon-valley ventures, like information-, communications-, and computer technology. These are identified using NACE-codes 5-35 for the first group, and 62,63 and 71 for the latter.

In Table 2 we show the descriptive statistics for both the main sample of 1260 observations (panel A) and of the primary subsample consisting of 359 observations (panel B). The samples are grouped by application status (Grantees and Non-grantees), showing differences in application- and financial variables. Note that some financial records are missing, and observation count will consequently differ across some of the variables.

Table 2: Descriptive Statistics

		Grantees				Non-grantees			
Variables	Type	Mean	Median	SD	Obs.	Mean	Median	SD	Obs.
Panel A: All firms									
Paid-in capital (Y_{i1})	0-1	0.274	0	0.447	354	0.224	0	0.417	749
Long-term debt (L_{i1})	0-1	0.337	0	0.473	356	0.326	0	0.469	745
Raw grade	1-7	5.498	6	0.609	417	3.769	4	0.957	843
Several appl. (C_i)	0-1	0.456	0	0.499	417	0.409	0	0.492	843
Age (years)	Count.	12.68	9	15.26	407	10.23	6	13.39	827
Assets*	Cont.	3646	27.15	36315	364	827	8.13	6857	786
Grant amount*	Cont.	5.360	4.7	3.842	417	0	0	0	843
Panel B: Young and small firms									
Paid-in capital (Y_{i1})	0-1	0.520	1	0.503	75	0.275	0	0.447	273
Long-term debt (L_{i1})	0-1	0.293	0	0.458	75	0.267	0	0.443	270
Raw grade	1-7	5.526	6	0.663	76	3.675	4	0.871	283
Several appl. (C_i)	0-1	0.421	0	0.497	76	0.357	0	0.480	283
Age (years)	Count.	1.395	1	1.424	76	1.664	1	1.784	283
Assets*	Cont.	1.199	0.773	1.204	76	1.055	0.538	1.234	283
Grant amount*	Cont.	4.469	4	3.066	76	0	0	0	283

**In millions (NOK)*

6. Empirical Method

A valid method for identification must be employed to accurately estimate the effect of public grants on subsequent capital. Only identifying a correlation between grants and capital changes could give some insights, but it does not suggest whether the grant affects capital or if capital affects the grant decision.

One plausible method to discern the effect of public grants on private financing could be to compare the capital of firms before and after the treatment. However, as young companies tend to raise capital as they scale up, it may prove difficult to detect a treatment effect. One possible approach is to use a control group of companies not receiving treatment, but with similar characteristics as those that did receive treatment. Although some grants serve specific purposes, it is reasonable to assume that public business-oriented R&D programs grant financial support to the companies with the best outlooks. This rationale is also applicable for private investors looking for profitable projects to invest in. Hence, such a matching approach could be biased, as the companies most likely to receive capital regardless of the grants are more likely to be part of the treatment group.

To solve for the possible selection bias, we employ a Regression Discontinuity Design (RDD). An RDD uses a defining characteristic for a discontinuous change in the probability of receiving treatment as a function of an underlying assignment variable. The defining characteristic is given by a cutoff value, denoted as c, determining whether the observation receives treatment or not. Looking at firms immediately around the cutoff will mitigate selection bias as these observations should have similar outlooks. Measuring the local treatment effect on the outcome variable around the cutoff makes it possible to establish causal inference as the treatment is as-if random. This, in addition to easily testable underlying assumptions, has increased the RDD's popularity over the years, and it is widely recognized among modern economists for estimating program effects.[13]

An integral part of the RDD approach is the assignment variable. Using normalized ranking of application grades, as discussed in section 5.1, with a clear cutoff value c, we can employ

[13] See Lee and Lemieux (2010) for a good overview of RDD and why the approach have gained favorable traction among economists since the 1990s. Also note that contributions by Joshua D. Angrist and Guido W. Imbens on RDD and methods for causal relationships have most recently (2021) been recognized with the Nobel Prize in Economic Sciences.

a Sharp RDD.[14] Conditional on the overall application grade, X_{it}, firm *i* receives a grant, D_{it}, when X_{it} is above the cutoff value, c, thus:

$$D_{it} = \begin{cases} 1 \text{ if } X_{it} \geq c \\ 0 \text{ if } X_{it} < c \end{cases} \tag{3}$$

A causal treatment effect is only estimated when a discontinuous change in the outcome variable is observed around the cutoff of the assignment variable. Our binary outcome variables, increase in external equity, $Y_{i\tau}$, and increase in long-term debt, $L_{i\tau}$, is regressed on grant status $D_{i\tau}$ and normalized application grade $X_{i\tau}$ for estimation. We allow for different slopes of the regression by including the interaction term $D_{it} \cdot X_{it}$. In practice, two linear regressions run on either side of the cutoff, and we can write the model for $Y_{i\tau}$ as follows:

$$Y_{i\tau} = \alpha + \beta_1(D_{it}) + \beta_2(X_{it}) + \beta_3(D_{it} \cdot X_{it}) \tag{4}$$

The coefficients are estimated using ordinary least squares (OLS). Also, given our binary outcome variable, the resulting model is a linear probability model (LPM). While this allows for an easy interpretation of the coefficients as probabilities of the outcome variable, error terms of LPM are inherently heteroskedastic. Thus, we employ heteroskedasticity-robust standard errors as proposed by Halbert White (1980).

As the assignment variable is discrete, identification requires running linear regressions of some functional form. Lee and Card (2008) recommend a goodness-of-fit test to assess the correct functional form for the regression. Employing their test, we find the first-order polynomial to outperform higher-order polynomials for all versions of our model.

Causality of treatment in RDD is identified at the discontinuity. This means there exists a tradeoff between a narrow bandwidth (the distance from the discontinuity point of which to fit the regression function) and keeping enough observations to ensure informative estimates. Imbens and Kalyanaraman (2012) propose a method for choosing the optimal bandwidth, taking both the density around the cutoff and the conditional variance into account. Although it is derived for the purpose of finding the optimal bandwidth for a local linear model, it is also applicable for a global linear model restrained by the local bandwidth. Applying their method

[14] In a Sharp RDD the probability of treatment for each observation is discrete at the cutoff (0 to 1), while it is continuous for its counterpart, the Fuzzy RDD. The latter requires more assumptions to hold in order to be regarded as valid (Imbens & Lemieux, 2008)

yields an optimal bandwidth of one or two for either side of the cutoff, depending on the choice of τ for our model.

We also employ the more traditional leave-one-out cross-validation approach, comparing the Mean Square Error (MSE) of different bandwidth sizes. This supports a bandwidth of one for all values of τ. Imbens and Kalyanaram argue that such a method is sensitive to the actual distribution and regression method. For our purpose, however, it serves as support for choosing a consistent bandwidth of one or two, independent of τ. The method also supports the choice of a first-order polynomial, when cross-validating different functional forms. In addition, testing for the optimal bandwidth with $L_{i\tau}$ as the outcome variable yields identical results for both methods. We will also address the sensitivity of bandwidth choice further in section 7.3.3.

Finally, a bias-variance tradeoff seems unavailing, considering the optimal bandwidth, as the discreteness of the assignment variable dispenses a linear regression with few observations on each side of the cutoff. For our primary subsample of young and small firms, a bandwidth of one results in a sample of only 121 observations at $\tau = 1$.[15] Although a larger sample size is desirable, such a narrow bandwidth ensures a coherent RDD where the average treatment effect is the only effect displayed.

For the following section, we proceed with a bandwidth of one, and first-order polynomial and OLS estimation for the RDD-model in equation (4).

[15] Note that different bandwidth sizes will dictate the number of observations used in the regressions, and thus, the observation count visible in the tables will consequently deviate some throughout this paper.

7. Results

This section is divided into three parts. The first part presents the main results of grant effect on equity and debt financing for small start-ups. The second part addresses possible heterogeneous treatment effects, where we present the effect on the same outcomes for the three secondary subsamples: i) large and mature firms, ii) classical industries and iii) new industries. In the third part, we assess the results' internal validity. We will also discuss the results and their implications in more detail in section 8.

7.1 Grant Effect on Small Start-ups

Figure 1 shows the mean outcomes (dots) and 90 percent confidence interval (line segments) for every normalized application grade of the variable $Y_{i\tau}$ the year leading up to and after grant decision. A discontinuity between $X_{i1} = -1$ and $X_{i1} = 0$ implies a positive relationship between the grant status and external financing around the threshold.

Figure 1: Mean Outcomes of Equity Financing for Small Start-ups

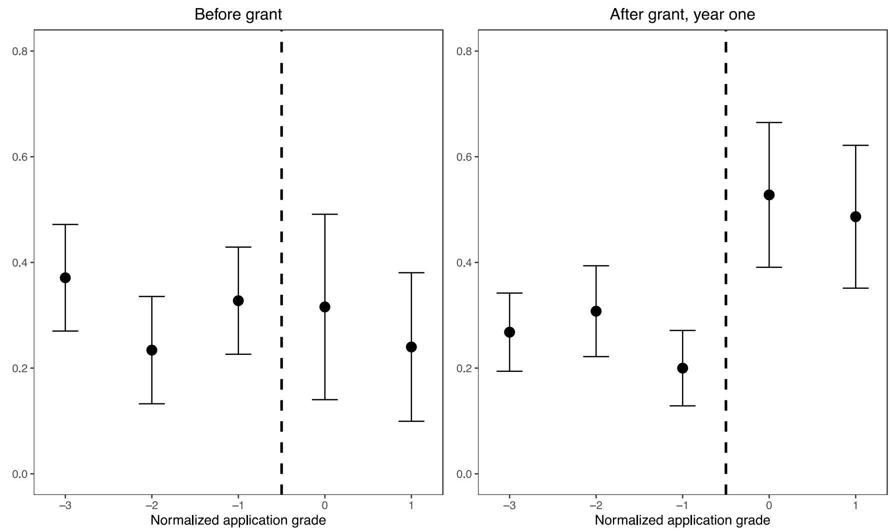


Table 3 reports the regression estimates of the OLS model using a bandwidth of one for both outcome variables. Grant coefficient is statistically significant and positive for both paid-in capital and long-term debt in year one, which reflects the impression from Figure 1. While the effect on debt fades off in the following years, the effect on equity remains and is significant all years throughout $\tau = 5$. The coefficients can be interpreted as the probability of receiving subsequent external financing, $P(Y_{i\tau} = 1|D_{it})$ and $P(L_{i\tau} = 1|D_{it})$. For example, the overall probabilities (independent of grant status) for external equity financing in year one and five are 20 percent and 51.5 percent. With grant approval, $D_{it} = 1$, they increase by 32.8 and 37.4 percentage points to a total probability of 52.8 percent and 88.9 percent, respectively.

Table 3: Grant Effect on External Financing for Small Start-ups

Bandwidth = 1					
Year (τ):	1	2	3	4	5
Panel A: Grant effect on paid-in capital					
Dependent variable:			$Y_{i\tau}$		
Grant	0.328***	0.217**	0.267**	0.273**	0.374***
	(0.094)	(0.106)	(0.113)	(0.114)	(0.119)
Constant	0.200***	0.408***	0.453***	0.509***	0.515***
	(0.044)	(0.059)	(0.063)	(0.070)	(0.090)
Observations	121	103	89	76	51
Panel B: Grant effect on long-term debt					
Dependent variable:			$L_{i\tau}$		
Grant	0.153*	0.126	0.083	-0.093	0.116
	(0.091)	(0.100)	(0.121)	(0.121)	(0.146)
Constant	0.181***	0.217***	0.333***	0.365***	0.273***
	(0.043)	(0.050)	(0.060)	(0.068)	(0.080)
Observations	119	101	87	74	51

Heteroskedasticity-robust standard errors are reported in parentheses.

7.2 Heterogeneity of Results

Results may differ considerably across different subsets of the main sample. Some firm characteristics are more prone to financial constraints than others, and thus more likely to experience a grant effect. To address the heterogeneity of grant effect, we investigate the three secondary subsamples as explained in section 5.3. We also include a subsection addressing the possible influence of firms applying for grants more than once.

7.2.1 Grant Effect on Secondary Subsamples

The regression results for the three groups are displayed in Table 4. Regressions are estimated using the same model as for small start-ups, with a bandwidth of one. For large and mature firms, there is little to no significant effect from grants on subsequent financing. The same applies to classical industries. Hence, little inference can be made for these two groups, other than the overall probability, of which large and mature firms have a 10.3 percent probability of raising external equity and 40.2 percent probability of raising debt the year after application, independent of grant status. For classical industries, the same probabilities are 15.9 percent and 38.1 percent, respectively. For new industries, the grants have little to no effect on equity as well, with an overall probability of 32.9 percent. However, grants yield a significant and positive effect on debt financing for this group, increasing the probability by 21.2 percentage points.

Table 4: Grant Effect on External Financing for Secondary Subsamples

Bandwidth = 1

Year (τ) = 1

Panel A: Grant effect on paid-in capital			
Dependent variable:		$Y_{i\tau}$	
	Large and mature	Classical industries	New industries
Grant	0.021	0.062	-0.002
	(0.044)	(0.072)	(0.085)
Constant	0.103*	0.159**	0.329***
	(0.030)	(0.047)	(0.054)
Observations	212	122	131
Panel B: Grant effect on long-term debt			
Dependent variable:		$L_{i\tau}$	
	Large and mature	Classical industries	New industries
Grant	0.002	0.026	0.212**
	(0.068)	(0.090)	(0.085)
Constant	0.402***	0.381***	0.231***
	(0.048)	(0.062)	(0.048)
Observations	212	122	130

Heteroskedasticity-robust standard errors are reported in parentheses.

7.2.2 Controlling for Several Applications

Some firms with first-time rejected applications and subsequent financing may file several applications and end up receiving grants at a later time. While these firms could potentially strengthen our results, they end up biasing against. All companies with first-time rejections, but succeeding grants, file at least one successive application. We add a binary control variable, C_i, to our model to control for firms that have applied several times. The results are displayed in Table 5. While there is little improvement in the overall grant effect, we see that the probability of receiving external equity financing, without grant and successive applications, drops across all years. Also, with some significance in the coefficients of C_i, it implies that the probability of receiving external equity financing increases if applied more than once. Obviously, this does not invalidate our results, but rather provides some explanation to the variation in grant effect over the five-year estimation window.

Table 5: Grant Effect on Equity Financing with Controls

Bandwidth = 1					
Dependent variable:			$Y_{i\tau}$		
Year (τ):	1	2	3	4	5
Grant	0.340^{***}	0.245^{**}	0.300^{***}	0.288^{**}	0.408^{***}
	(0.097)	(0.108)	(0.112)	(0.112)	(0.114)
Several applications	0.106	0.177*	0.206**	0.128	0.210*
	(0.083)	(0.099)	(0.104)	(0.112)	(0.127)
Constant	0.153^{***}	0.319^{***}	0.337^{***}	0.440^{***}	0.388^{***}
	(0.050)	(0.074)	(0.083)	(0.094)	(0.119)
Observations	121	103	89	76	51

Heteroskedasticity-robust standard errors are reported in parentheses.

7.3 Internal Validity of Results

The strength of RDD as a quasi-experimental approach lies in the "as good as randomly assigned" treatment status near the cutoff (Lee & Card, 2008). There are generally two main concerns to the validity of this feature, according to Imbens and Lemieux (2008). These are (i) other possible changes at the same cutoff of the assignment variable, and (ii) manipulation of the assignment variable. In the following subsections, we will address these two general concerns, as well as other potential threats to the validity of our results.

7.3.1 Other Changes at the Same Cutoff

If the RDD is valid, we should observe no treatment effect on outcomes where a treatment effect is not expected (Imbens & Lemieux, 2008). This could happen in cases where there is a discontinuity in baseline covariates at the same cutoff of the assignment variable. While this should not be a problem, as the assignment variable (application grades) are unique to this specific treatment (grant), we test for changes in pseudo-outcomes that are known not to be affected by the treatment. We use ex-ante measures of $Y_{i\tau}$ and $L_{i\tau}$, in addition to a binary variable for increase in assets (denoted as $A_{i\tau}$) to test for grant effect. The model from equation (4) with a bandwidth of one is applied to all three variables, all of which are measured in the year leading up to grant application. In Table 6, we show the results for small start-ups. Grant effect is small and insignificant for all three outcomes, as anticipated. Repeating the test for firms in new industries shows close to identical results.

Table 6: Grant Effect on Pseudo Outcomes for Small Start-ups

Bandwidth = 1			
Year (τ) = 0			
Dependent variable:	$Y_{i\tau}$	$L_{i\tau}$	$A_{i\tau}$
Grant	-0.012	-0.175	-0.053
	(0.129)	(0.133)	(0.136)
Constant	0.328**	0.754***	0.632***
	(0.063)	(0.058)	(0.065)
Observations	77	76	76

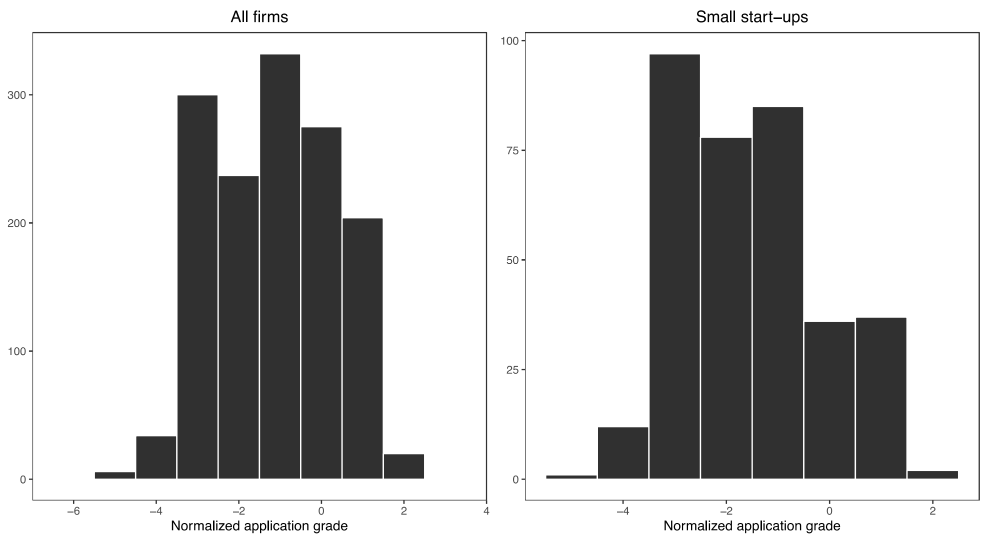
Heteroskedasticity-robust standard errors are reported in parentheses.

7.3.2 Manipulation of the Assignment Variable

If individuals can precisely influence the assignment variable, it violates the non-manipulation assumption, and thus treatment status around the cutoff cannot be regarded "as random" (Lee and Lemieux, 2010). RCN has several extensive measures in place to ensure impartiality of the referees and the integrity of the project evaluation process. Yet, since the cutoff for many projects is known ex-ante, the possibility for manipulation exists. A discontinuity in the density around the cutoff of the assignment variable may suggest manipulation (Imbens & Lemieux, 2008).[16] The density of applicants with normalized grades are shown in Figure 2, in which we observe no obvious signs of manipulation above the cutoff.

The jump in frequency below the cutoff from $X_{it} = 0$ to $X_{it} = -1$ for small start-ups looks suspicious on the other hand. Such a change in density around the cutoff could imply some influence over treatment. Some explanation can be derived from the observations with a raw grade of zero that are scaled to minus one, because of normalization (see section 5.1). In addition, inspecting the distribution of the main sample with all firms, density looks more equally distributed, as shown in Figure 2. Also, considering the occasional limited funds of RCN, the grades are not distributed equally on either side of the threshold, as only around a quarter of all applicants receive grants. Hence, some discontinuity in the density above the cutoff should be expected. In sum, the concern does not seem to invalidate our results.

Figure 2: Density Plot



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[16] McCrary (2008) proposes a test of the continuity of the density of the assignment variable. Unfortunately, the test is not applicable in this case, due to the discrete application grades.

7.3.3 Sensitivity of Bandwidth Choice

Results that critically depend on a particular bandwidth size may be less credible. Following the recommendations of Imbens and Lemieux (2008), we test for bandwidth sizes of two and all values on both sides of the cutoff. Estimates of the coefficients and standard errors are obviously affected by the bandwidth size, but the results should be fairly consistent across different bandwidth choices. Results of all subsamples are robust to changes in bandwidth. In Table 7, we document the bandwidth tests for small start-ups.

Table 7: Bandwidth Tests for Small Start-ups

Dependent variable:			$Y_{i\tau}$		
Year (τ):	1	2	3	4	5
Panel A: Bandwidth = 2					
Grant	0.435***	0.235	0.329*	0.355*	0.536**
	(0.134)	(0.159)	(0.170)	(0.183)	(0.214)
Application grade	-0.108	-0.018	-0.063	-0.081	-0.162
	(0.069)	(0.083)	(0.090)	(0.103)	(0.125)
Grant * Application grade	0.066	0.079	0.143	0.240	0.182
	(0.139)	(0.146)	(0.154)	(0.150)	(0.176)
Constant	0.092	0.390***	0.391***	0.428***	0.353*
	(0.103)	(0.132)	(0.141)	(0.159)	(0.199)
Observations	236	213	178	137	93
Panel B: No bandwidth					
Grant	0.369***	0.209*	0.242*	0.198	0.250*
	(0.106)	(0.118)	(0.131)	(0.134)	(0.148)
Application grade	0.369***	0.209*	0.242*	0.198	0.250*
	(0.106)	(0.118)	(0.131)	(0.134)	(0.148)
Grant * Application grade	0.096	0.092	-0.038	0.117	-0.033
	(0.111)	(0.104)	(0.144)	(0.117)	(0.134)
Constant	0.134**	0.403***	0.507***	0.585***	0.639***
	(0.066)	(0.083)	(0.090)	(0.099)	(0.126)
Observations	348	317	258	201	142

Heteroskedasticity-robust standard errors are reported in parentheses.

7.3.4 Placebo Tests

Discontinuities elsewhere than at the threshold for treatment may also imply a less credible RDD (Imbens & Lemieux, 2008). By design, only those individuals that make it above the assignment variable cutoff are treated, and thus, a jump in the outcome variables should only come into play there if there is a treatment effect. We apply two placebo tests for discontinuities in the assignment variable elsewhere than at the threshold for treatment. Using model (4 and a bandwidth of one for all subsamples, we test for discontinuities one value above and one below the actual cutoff. In Table 8 these are shown in Panel A and B, respectively, for the subsample of small start-ups. The results suggest no falsification for all subsamples, and it appears that the discontinuities in the outcome variables are very much caused by the treatment for small start-ups and firms in new industries.

Table 8: Placebo Tests of Equity Financing for Small Start-ups

Bandwidth = 1					
Dependent variable:			$Y_{i\tau}$		
Year (τ):	1	2	3	4	5
Panel A: Artificial cutoff at grade = 1					
Grant	-0.041	0.061	0.080	0.159	0.020
	(0.120)	(0.120)	(0.125)	(0.108)	(0.123)
Constant	0.528***	0.625***	0.720***	0.783***	0.889***
	(0.086)	(0.088)	(0.094)	(0.090)	(0.078)
Observations	73	67	50	40	29
Panel B: Artificial cutoff at grade = -1					
Grant	-0.108	-0.018	-0.063	-0.081	-0.162
	(0.069)	(0.083)	(0.090)	(0.103)	(0.125)
Constant	0.308***	0.427***	0.516***	0.591***	0.677***
	(0.053)	(0.058)	(0.063)	(0.076)	(0.087)
Observations	163	146	128	97	64

Heteroskedasticity-robust standard errors are reported in parentheses.

7.3.5 Check for Dropped Observations

Finally, we want to address the concern of influential observations dropping out of the sample. Since our model depends on available data on paid-in capital and long-term debt, applicants will drop out when financial data is missing. This is especially the case for more recent applicants as we increase the estimation window.[17] In Table 9, we list the distribution of results for small start-ups, with dropouts in parentheses. Row 1-4 relate to the outcomes in external equity financing, given grant status ($Y_{i\tau}|D_{it}$), and columns 1-5 to years after grant decision.

For most years, there is a fairly equal distribution among both outcome pairs that bias for (1 and 4) and against (2 and 3) the results. At $\tau = 2$ however, there are twice as many dropouts in row 1 and 4 than 2 and 3. This will obviously reduce the grant effect in year two, as seen in Table 3, but most of these dropouts are due to missing financial records from 2021 and cannot be accounted for.[18] More influential are the large number of dropouts from row 1, year one, that show up as observations in row 3, year two. While these observations contain important information about the effect on non-grantees, further investigation reveals that half of these firms actually receive support from RCN within the second year of their first application. An adjustment to include only first-time approved applicants, and otherwise rejected ones, would obviously bias our sample. An option, however, is to control for successive applications like we do in section 7.2.2.

Table 9: Distribution of Observations and Dropouts (in parentheses)

Year (τ):		1	2	3	4	5
1	$Y_{i\tau} = 0 \mid D_{it} = 0$	68 (0)	42 (10)	35 (4)	26 (4)	16 (10)
2	$Y_{i\tau} = 0 \mid D_{it} = 1$	17 (0)	12 (2)	7 (3)	5 (1)	2 (2)
3	$Y_{i\tau} = 1 \mid D_{it} = 0$	17 (0)	29 (4)	29 (3)	27 (7)	17 (10)
4	$Y_{i\tau} = 1 \mid D_{it} = 1$	19 (0)	20 (2)	18 (4)	18 (1)	16 (3)
	Total	121 (0)	103 (18)	89 (16)	76 (13)	51 (25)

[17] Company accounts for 2021 are obviously unattainable, as well as some missing records for 2020.

[18] Some companies are also dissolved during the five-year estimation window, but little information can be interpreted from this, as there is no clear pattern of dissolutions due to acquisitions or bankruptcies. A large number of acquisitions among the approved applicants that drop out could arguably be in favor of a positive grant effect on equity. Many bankruptcies among those that did not receive grant could imply the same, but other way around; not receiving a grant is negative for equity.

8. Discussion

8.1 Implications

In the period of 2010 to 2020, R&D support from RCN contributed to an increase in the probability of receiving external equity with about 30 percentage points on average for small start-ups. It goes to show that this form of public support not only enables the inception of new projects, but also acts as a powerful tool to stimulate private capital for small start-ups. The numbers coincide well with a 2017-survey on RCN recipients in which 31% of SMEs claimed access to capital produced significant challenges for commercialization and growth (Research Council of Norway, 2019). In addition, the same firms experienced an increase in the likelihood of receiving debt capital by 15 percentage points the first year after grant approval. The combination of these findings provides firm evidence that public grants lift some of the uncertainty associated with investing in small start-ups.

How RCN grants alleviate this uncertainty can be difficult to infer. While the effect is present across the entirety of the five-yar estimation window, there are some discrepancies with respect to the effect's magnitude, which can arguably be used in describing how the investors respond. The significant effects in year one and two (column 1 and 2 in Table 3) rules out any stand-alone proof-of-concept effect through the equity channel, as the average project time is close to three years. The idea is that an investor would wait for a proven prototype before investing in a new technology. The small jump in the overall probability of financing after three years, from 62,5% to 72%, could call for such a prototype effect, but this cannot be regarded as strong evidence, however, as an increase of 10 percentage points is fairly consistent for all five years.

A combination of certification and equity mechanisms provide for a more reasonable explanation for the grant effect on start-up's financing. Subsidies have the highest effect on increases in equity in year one and five after grant approval, with an increase in probability of 32,8 and 37,4 percentage points, respectively (column 1 and 5 in Table 3).

The early increase supports a signal effect, in which the government (RCN) acts as a certifier of possible investment opportunities. Through the assessment by industry experts and knowledge of RCN, it sends a powerful signal of innovation quality in grant recipients to potential investors. Additionally, as part of the application process, the applicants must convey valuable information about the research project and prospects of the company. This is information the common investor not necessarily have access to, and thus, an investor may be tempted to exploit the implicit information found in the application status. In addition, the grant money may trigger some loan facilities, like requirements of support program enrollment or the ability to pledge cash collateral. Thus, the early effect is arguably proof of a certification mechanism for both equity and debt.

The late surge in equity capital on the other hand, advocates for an effect through the equity channel. As the public funding enables project inception, it will ultimately lead to a greater demand for capital the closer to maturity and commercialization of the project. A late 2018-survey on RCN recipients found that more than half of the projects that were finished in 2014 had reached commercialization by the time of questioning (Research council of Norway, 2019). Translated to this context, it means that by year five after grant approval, more than half of the projects have yet to reach commercialization. There are typically high costs associated with bringing new technology to market, hence it is likely that firms will raise capital in periods leading up to commercialization.

Our findings show little to no grant effect on the external financing of big and mature firms. The results are almost identical to that of classical industries.[19] This supports the general idea that these firms typically face little financial constraints due to less information asymmetries and high pledgeability in the form of real assets. While the grant effect is negligible, some inference can be made from the overall probabilities. Independent of application results, large and mature firms are four times as likely to raise debt (40%) as equity (10%) within the first year after applying for a R&D grant. Likewise for classical industries with a probability of 38% for debt and 16% for equity. This aligns well with the tax considerations we discussed earlier in section 4, of which firms tend to favor debt over equity for external financing.

In 2019, 100 of the biggest corporations undertaking R&D in Norway accounted for more than half of all commercial R&D-expenditures. The second half was shared among 2,900 smaller businesses. While our study shows that subsidizing big firms mobilize little to no external capital, we cannot leave out an internal mobilization of capital. Grants may arguably serve a

[19] Norway has a long tradition of big corporations in capital-intensive sectors, like petroleum and shipping. Many of these firms will consequently show up in both subsamples, forcing similar results.

different purpose for the big R&D-players, like guiding the investment choices and promoting social beneficial projects.

Grants nearly double the probability of receiving debt financing the first year after grant approval for new industries, from 23% to 55%. While there is no effect on financing beyond this, it supports similar debt certification and trigger mechanisms to that of small start-ups. In addition, before grant approval, new industries have more than double the probability of issuing equity and half the chance of raising debt in the year after application, compared to the larger and more capital-intensive firms. After the new industry businesses receive grant approval the likelihood of raising debt is close to equal. This proves that grants can level the short-term debt frictions for the typical new industries firm. As equity remains unchanged, it also supports the theory of tax wedging between debt and equity financing.

Considering the classical economic conflict on public spending, as derived in the beginning of section 4, our findings clearly favor a multiplier effect over the crowding-out mechanism. Obviously, in a policy framework, the mechanisms of public funding are far more complex than those we have discussed in this section. In addition, subsidy spending may potentially crowd out private investment in other areas as a mere result of budget allocation. Nonetheless, for the purpose of this study where we target the direct causal effects of public support programs, the mechanisms laid out above are generally in support of a multiplier effect of public grants.

The grant effect is not universal, however, as the results only show a positive impact on small start-ups and businesses in new industries. In addition, while we can conclude that the effect is causal, we are unable to conclude on the underlying mechanisms that facilitate the effect. Both the certification and the equity channel present reasonable explanations, but we cannot rule out a prototype mechanism for the equity effect on young and small ventures. Their debt effect on the other hand, as well as that of new industry businesses, make for a more stand-alone certification effect.

8.2 External Validity

The results presented in this thesis join a line of similar findings in other recent studies. Meuleman and De Maeseneire (2012) suggest that Belgian R&D grants have a positive signal effect on SMEs' access to long-term debt. Howell (2017) proposes a prototype effect and finds that the US' SBIR program doubles the probability of receiving venture capital for early-stage grant recipients. Both studies also link grants with a stronger effect on the access to capital for young firms, compared to older ones. Our findings also identify the relationship suggested in the domestic study by Menon Economics (2018), where they found a positive correlation between grants and growth in equity for start-ups. Considering the statistical validity and previous research in line with our results, we believe our findings to represent a close to true picture of the actual effect of public grants in Norway.

8.3 Additionality

In light of recent evaluations of the Norwegian public support system, our findings provide some basis for strategies going forward, especially concerning the access to capital for entrepreneurs and innovative projects. Deloitte et al. (2019) recommend restructuring the system to better meet four growing challenges. In the comprehensive report, they argue that i) general market failures, ii) system failures, iii) transformation failures, and iv) cyclical challenges can be corrected through the support system, partly by directing subsidies to specific companies and sectors.

Our study addresses the first challenge in particular, and show that public subsidies directed at financially constrained firms can correct for such market failures. More so, if the benefits of mitigating financial constraints outweighs the socio-economic benefits of subsidizing firms with a sufficient access to capital, it becomes apparent that allocating funds from large, mature and capital-intensive industries, to young, small firms and knowledge-intensive industries, is favorable.

System failures are market failures that arise because of geographical challenges and may be especially present in Norway where long distances make it difficult to connect some market players. Firms in certain remote locations may be less disposed to external financing than competitors in more urban areas for instance. The evidence provided in thesis show that R&D

subsidies can relieve some financial frictions. Thus, targeting some key areas that are more prone to such constraints may also prove beneficial.

The third type of market failure, the transformational failure, occurs when market players make short-term decisions that do not necessarily take long-term consequences into account. For example, the environmental externalities of fossil fuel production. To encourage a transition away from a petroleum-dependent economy, RCN and the other policy agents can direct subsidies toward innovation and investments in other industries. Our research implies a multiplier effect of public funding in new industries with possibly higher information asymmetries. New markets are likely to be unknown territory for a majority of investors, compared to the entrepreneurs in those markets, and alleviating some of this uncertainty through the support system may help accelerate the green transition in Norway.

For the fourth challenge, Deloitte et al. (2019), suggest that the public subsidy programs can work as countercyclical instruments as well. Many extraordinary support schemes have been facilitated through the public subsidy system to combat the economic downturn of the COVID-19 pandemic. In light of our study, subsidizing financially constrained firms may prove valuable to preserve access to capital and R&D investments in times of volatile markets and high degrees of uncertainty.

Obviously, this thesis is concentrated at studying external financing for subsidy recipients and pay little attention to the socio-economic benefits beyond that. We hope our findings can complement previous and future research on both Norwegian and international subsidy programs. Knowledge on firms' financing- and investor behavior towards public funding can serve as valuable basis for the design and strategy of these programs going forward.

9. Conclusion

Small new ventures commencing R&D are likely to face financial constraints due to high information asymmetries, moral hazard, and little collateral in real assets. This thesis has presented empirical evidence on the relationship between R&D grants from the Research Council of Norway and the financing of small start-ups in Norway. We have found grants to have a positive effect on long-term debt financing, and an even stronger effect on external equity financing for these firms. The effect is *causal*, thus showing the importance and accuracy of the RCN programs to mitigate the financial constraints for young and small businesses. Our study has also revealed an effect on the debt of new industries, like information and communications technology, while grants seem to have no effect on the financing of larger and more mature firms as well as those in more classical, capital-intensive industries.

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