Funding R&D Cooperation Between Firms and Universities

The Effectiveness of the Innosuisse Model

Florian Hulfeld

Andrin Spescha

Martin Woerter

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This paper investigates the causal effect of funding research and development (R&D) cooperation with public universities on the performance of firms. We use comprehensive firm-level panel data from Switzerland that spans from 2010 to 2022 and covers a broad set of variables including the innovation support received by firms. We examine the effectiveness of the most important public innovation agency Innosuisse, which funds R&D cooperation between firms and universities. To identify the causal effect, we use three newly developed difference-in-differences estimators that allow for arbitrary treatment effect heterogeneity. Because firms selectively apply for funding and Innosuisse selectively funds a subset of applicants, we have a twofold selection problem. To make the parallel trends assumption more plausible, we create a control group based on firms that never applied for Innosuisse funding but otherwise resemble the funded firms. The baseline results show that the funding increases firm sales by 21% and employment by 18% on average over a time window of 5 years. We use specification curve analysis to show that this result holds over numerous important alternative specifications. The funding of R&D cooperation between firms and universities can thus be a potent alternative to more traditional policy instruments like R&D subsidies or tax credits.

1 Introduction

Innovation is essential for economic growth. Nonetheless, innovation activities are below their socially desired levels due to market failures arising from the public good nature of innovations (Arrow, 1972; Eberhardt, Helmers, & Strauss, 2013), their uncertain outcomes (Coad & Rao, 2008; Mata & Woerter, 2013), and the financial constraints of (small) firms (Carpenter & Petersen, 2002; Hall & Lerner, 2010). Consequently, governments usually rely on three direct policy instruments to support innovation: R&D subsidies, patent boxes, and R&D tax credits (Bloom, Van Reenen, & Williams, 2019). Despite being one of the world's leading innovators, Switzerland lacks any of these policy instruments at the federal level and relies on a more targeted policy instrument: funding research and development (R&D) cooperation between private firms and public universities. Since the systematic reliance on this policy instrument is unique in an international context, it is crucial to understand how effectively it addresses the present market failures and thus contributes to Switzerland's innovation success. Hence, this study investigates the causal effect of funding R&D cooperation with public universities on the performance of firms.

In Switzerland, public innovation support at the federal level comes from the Swiss Innovation Agency - Innosuissse, which funds R&D cooperation between private firms and public universities. Innosuisse evaluates applications for joint innovation projects and covers the costs arising at universities. Firms do not receive any funds and must cover their own project costs. The entire project costs are usually split equally between firms and Innosuisse (see figure A.IIii). Innosuisse's annual budget for funding innovation projects varied from 2019 to 2023 between 140 and 170 million CHF. Half of the projects receive between a quarter and half a million CHF (see figure A.IIi).

To identify the effect of the funding on firm performance, we cannot just compare funded firms with non-funded firms. Since firms do not randomly apply for funding and Innosuisse does not randomly distribute its funding among applicants, the funding decision suffers from a twofold selection problem. Both selection problems likely lead to a positive selection bias, causing an overestimation in the effect of the funding on firm performance.

To address this problem, we rely on difference-in-differences (DiD) methods. The most important aspect of a DiD is the choice of a valid control group. The firms funded by Innosuisse need to be compared to firms that are very similar to them but did not receive the funding. We rely on three different control groups to diversify the risk of choosing an invalid control group. However, we do propose a baseline control group, where we take all firms in the Swiss economy that never applied for Innosuisse funding and then randomly exclude firms that are distinct from firms that received Innosuisse funding prior to their treatment. This distinction is relevant if the observable characteristics affect firms' employment and sales. By randomly excluding firms that have a distinct number of competitors (see figure 5-1p), have no R&D expenditures (see figure 5-1n), do not have any employees with a tertiary degree (see figure 5-1d), and have no exports (see figure 5-1h), we achieve balance in core variables like firm size, human capital, or R&D activities. Figure 5-1 shows that the distribution of all relevant covariates is very similar between funded and non-funded firms after these exclusions. The matching makes it more likely that both groups develop similarly over time and the parallel trends assumption becomes more plausible.

We start our estimations with the two-way-fixed-effects difference-in-differences model (TWFEDD) using OLS. However, recent contributions have emphasized potential bias of the TWFEDD in the presence of treatment effect heterogeneity (Goodman-Bacon, 2021). Because Innosuisse funding likely has dynamic effects on firm performance that increase over time, the average treatment effect on the treated (ATT) obtained from such a model will be underestimated. To address this bias, we rely on three recently developed DiD methods that allow for arbitrary treatment effect heterogeneity (Callaway & Sant'Anna, 2021; Borusyak, Jaravel, & Spiess, 2024; de Chaisemartin & D'Haultfoeuille, 2024).

The paper uses firm-level panel data from Switzerland that spans from 2010 to 2022. The core of our data comes from multiple waves of different firm-level surveys conducted by the KOF Swiss Economic Institute at ETH Zürich. These surveys run on the KOF Enterprise Panel (KEP); a stratified random sample representative of the entire Swiss economy. We extend the survey data with detailed funding data on the project-level of all Innosuisse applicants since 2017. This provides a comprehensive dataset with 920 unique firms, 383 funded and 537 non-funded firms, and 3220 firm-year observations.

The funding of research cooperation between firms and universities increases firm performance substantially. An innovation project funded by Innosuisse increases firm sales by about 21% and employment by about 18% on average over the next 5 years in our baseline specification. The funding exerts strong dynamic effects that increase over time. The effects on firm performance are around 4% and 8% after one year, 16% and 17% after three years, and increase to 30% and 42% after five years for sales and employment, respectively. Moreover, we show that these effects are not only heterogeneous over time and across treatment cohorts but also over different subgroups. When compared to small and large firms, medium-sized firms show smaller effects that do not evolve over time. Finally, we show that the effects on firm performance are different across different types of higher education institutions as well as across scientific disciplines underlying the respective innovation projects.

Empirical research always entails many difficult choices about data collection, processing, and analysis. Even though numerous choices may be equally defensible from a theoretical viewpoint, they can nonetheless have a large influence on the results. To combat this problem, researchers usually run sets of alternative specifications in the robustness section of their papers. Here we instead advocate the use of specification curve analysis (Simonsohn, Simmons, & Nelson, 2020), which simultaneously shows the results of thousands of specifications in a highly aggregated way.

We estimate 432 alternative specifications using 4 different estimators, 3 anticipation lags, 3 control groups, 3 fixed-effects structures, and include or exclude firms with international innovation funding as well as microfirms. The majority of these specifications show quite similar results. Our baseline specification is located between the median and the third quartile of all treatment effect sizes. No specification yields a sign reversal and about 75% of the specifications using employment as well as 50% of those using sales yield statistically significant results. We find two main drivers behind the insignificant specifications: First, TWFEEDD shows lower effect sizes since it suffers from a negative bias caused by the forbidden comparisons occurring in the presence of increasing treatment effects. Second, the control group that uses firms that applied for Innosuisse funding but were rejected shows near zero results. This is a direct consequence of the second type of selection bias with Innosuisse choosing which firms to fund. This bias is not purged by re-balancing here, in contrast to our baseline control group. Almost all specifications show statistically insignificant averages for the placebo estimates that are close to zero, supporting our view that the large majority of these specifications are theoretically valid.

The contribution of our study to the existing literature is fourfold: First, we analyze the large-scale, systematic funding of firm-university R&D-cooperation. Second, we assess the impact of this innovation policy funding when only few other innovation policies exist and provide one (among many other) possible explanation for Switzerland's position as a global innovation leader. Third, we use the most recently developed methods in the difference-in-differences literature that allow for arbitrary treatment effect heterogeneity and compare the estimators against each other. Fourth, to combat the problem of numerous researcher degrees of freedom in data analysis, which can lead to high variation in obtained results, we make use of specification curve analysis.

2 Literature review on innovation support

2.1 The rationale behind innovation support

An important question behind every policy instrument to support innovation is why we should implement it in the first place. In a perfect market economy, private firms know best how much they should invest in R&D, since they have the best knowledge about the costs of such investments. Intervention by the government would only distort their decisions and cause misallocations. However, there are many reasons why, under actual market conditions, a company's R&D investment decision may not be optimal from a social perspective and why political measures are therefore justified.

First, output from R&D is partly a public good that creates knowledge spillovers (Arrow, 1972). The knowledge behind the results of R&D investments is non-rivalrous (one company's use does not reduce another's) and non-excludable (Eberhardt et al., 2013). Other firms can use the results without themselves having to pay the full R&D costs. This means that the social returns to R&D are higher than the private returns. From a societal point of view, companies therefore underinvest in R&D.

Second, R&D projects are risky and their outcomes are uncertain (Silverberg & Verspagen, 2007; Mata & Woerter, 2013). The skewed returns of R&D projects (Åstebro, 2003; Coad & Rao, 2008) can make public support important. It allows firms to pursue risky projects that they would not have executed otherwise. From an economic point of view such an attitude is desirable since broadly applied funding can be overcompensated by the large gains of some firms (Mata & Woerter, 2013). Such major successes, which would not have been possible without public support, can also open up entirely new fields of knowledge and create new markets.

Third, start-ups and small firms in particular often face difficulties to raise capital for R&D due to market imperfections (Carpenter & Petersen, 2002; Hall & Lerner, 2010). The high uncertainty and complexity of R&D projects makes it difficult to assess their expected return. Access to financing through credit is thus more challenging than with fixed capital investments. Moreover, R&D projects are intangible investments and cannot serve as a collateral. Potential investors and creditors also face information asymmetries for most R&D projects and do not have access to all the necessary information about the projects (Aboody & Baruch, 2000; Baruffaldi, Simeth, & Wehrheim, 2024). They may therefore refrain from investing or lending credit. Public support for an R&D project acts here as a quality signal, indicating that the project has passed a certain level of scrutiny. This could reduce the perceived information asymmetry and trigger additional investment from private investors. However, the empirical evidence for such a signaling effect of innovation support is inconclusive(Kleer, 2010; Howell, 2017; Santoleri, Mina, Di Minin, & Martelli, 2022).

The next section lays out the main innovation policy instruments that address these market failures.

2.2 Innovation policy instruments

Governments have a wide range of measures at their disposal to increase a country's capacity to innovate. Some of these measures have a direct impact on innovation activity, for example by reducing the cost of innovation and thus allowing firms to undertake riskier projects. These include measures such as R&D subsidies, patent boxes, and R&D tax credits. In addition, there are a number of broader measures, such as the promotion of education, competition, or skilled immigration, which also have a positive effect on innovation (Bloom et al., 2019).

2.2.1 R&D tax credits

Tax credits for R&D enable companies to deduct more than 100% of their R&D expenses from their taxable income. This lowers the marginal cost of R&D investments and thus provides companies with an incentive to invest more in R&D. A potential issue with this measure is that companies have an incentive to declare as much expenditure as possible as R&D expenditure, which can lead to so-called 'relabeling' of expenditure (König, Storesletten, Song, & Zilibotti, 2022).

In a literature review, Becker (2015) concludes that R&D tax credits increase private R&D investments, with a negative elasticity of R&D with respect to user cost of capital of around unity. More recent studies using causal research designs confirm the positive impact of R&D tax credits on R&D investments (Bøler, Moxnes, & Ulltveit-Moe, 2015; Chen, Liu, Suárez Serrato, & Xu, 2021) and patents (Dechezleprêtre, Einiö, Martin, Nguyen, & Van Reenen, 2023). Nonetheless, Chen et al. (2021) show that about a quarter of the R&D investment (in China) is due to relabeling.

2.2.2 Patent boxes

Patent boxes provide lower tax rates for revenues from patents. The idea is to incentivize R&D leading to further patents. However, Bloom et al. (2019) argue that patent boxes only lead to tax competition, where firms shift the royalties of their patent to different geographical places. Firms use it to minimize their tax burden, while the effect on innovation is minimal (Gaessler, Hall, & Harhoff, 2021).

2.2.3 R&D subsidies

R&D subsidies (or grants) are direct transfers of money to specific companies. Unlike R&D tax credits, R&D subsidies allow governments to encourage the development of specific technologies. The cost to society can be very high, especially if the subsidised technologies do not succeed and deliver the expected breakthroughs. There is also a risk of a free-rider effect where the subsidized company would have made these investments anyway, meaning that the subsidy crowds-out the private investment. Ideally, subsidies lead to behavioural additionality (Dimos & Pugh, 2016). This would be the case, for example, if the company invested additional funds in the publicly funded project that it would not have done without the subsidy (crowding-in). An advantage of subsidies is that they provide targeted support to young businesses that are less able to benefit from other support measures such as tax credits.

The literature on the effectiveness of R&D subsidies is very extensive and presents mixed findings. According to Zúñiga-Vicente et al. (2014) about 60% of all studies find that R&D subsidies lead to more R&D investments (crowding-in or additionality), while 20% report crowding-out and another 20% find no significant effects. Becker (2015) reports mixed evidence as well but emphasizes that recent research shows less crowding-out and more additionality, especially for small firms. Dimos and Pugh (2016) conduct a meta-analysis, finding no evidence of crowding-out but also no evidence of substantial additionality. They caution that studies not addressing the potential endogeneity of subsidies often report larger effects, possibly due to selection bias related to unobserved factors like skilled employees.

More recent studies using different methods, such as exploiting application rankings (Bronzini & Iachini, 2014; Bronzini & Piselli, 2016; Howell, 2017; Santoleri et al., 2022) or exogenous changes in laws (Azoulay, Graff Zivin, Li, & Sampat, 2019; Hünermund & Czarnitzki, 2019; Pallante, Russo, & Roventini, 2023), show that R&D subsidies can positively affect outcomes like R&D investments and employment, particularly for smaller firms. The mechanism is thereby mostly the relaxing of financial constraints. However, some studies, like Wang et al. (2017), also find no significant effects of subsidies on different firm outcomes. Recent studies using difference-in-differences methods mostly report positive impacts of R&D subsidies, too, especially for smaller firms (Vanino, Roper, & Becker, 2019; Mulier & Samarin, 2021), although there are exceptions, such as Lanahan et al. (2021), which find negative effects on employment.

In sum, the existing literature finds mostly positive effects of R&D subsides on innovation inputs and outputs of firms. However, it does also contain a non-negligible share of studies that show weaker or even negative effects of R&D subsidies. This emphasizes the importance of how R&D subsidies are designed and distributed; they can subsequently differ in their impact on firm outcomes.

2.3 Innovation support through research cooperation with universities

The common element of R&D tax credits, patent boxes, and R&D subsidies is that they target the cost side of R&D projects. Meanwhile, many companies rather lack the capabilities to carry out R&D projects on their own. For example, companies may not have the right R&D personnel or access to modern R&D laboratories. However, with the support and resources of public universities and research institutes, they could carry out these projects. This is one important focus of Switzerland's innovation promotion system.

The literature contains four recent contributions that investigate the effect of a funding of R&D cooperation on R&D spending. Bellucci et al. (2019) find that R&D subsidies targeted at individual research projects (without partners) yield more positive effects than of those targeted at collaborative research projects (with partners), whereby the latter refers to joint projects between SMEs and other firms, research centers, and universities. Engel et al. (2016) show that the impact of R&D funding on internal R&D investment is higher for business-business collaborations than for business-university collaborations. In contrast, the impact of R&D funding on the share of external R&D expenditure is higher for business-university collaborations than for business-business collaborations. This implies that companies involved in business-university collaborations tend to outsource more of their R&D to universities. Third, Beck, Lopes-Bento, and Schenker-Wicki, 2016 find that R&D expenditures induced by innovation support affect only radical innovations and not incremental innovations, but they do not find evidence that horizontal, vertical, or science collaborations alter this relationship. Finally, Kleine et al. (2022) run a randomized control trial in which they distribute vouchers worth 5000 GBP to a sample of small firms granting access to the service of experts from universities. Even though this treatment is comparatively small, Kleine et al. (2022) find a (short-lived) impact on the execution of innovation projects.

Hence, the existing literature already provides some estimates of the funding of research cooperation between firms and universities. However, while Bellucci et al. (2019) measure R&D cooperation more broadly than just with universities, Engel et al. (2016) and Beck et al., 2016 show particular results that are already given in our context. The institutional setup of the Innosuisse funding requires that about half of the R&D investment happens at the university, which is akin to a "free" outsourcing of R&D. This point also differentiates our study from Beck et al., 2016; firms are required to increase their own R&D investments to about half of the total project costs. In contrast to this existing literature, our study measures the combined effect of public R&D investments at universities, which are funded by Innosuisse, together with private R&D investments on the performance outcomes of firms, measured as sales and employment. The results of Kleine et al. (2022) are especially encouraging to measure the impact of funding R&D cooperation between science and business within a much broader economic context.

3 Innovation support in Switzerland: The Innosuisse model

3.1 The Swiss innovation support system

Switzerland is one of the most innovative countries in the world. The success of the Swiss innovation system rests on several pillars: high quality universities, good infrastructure, competitive product and factor markets, a technological/engineering focus based on vocational education, and political stability (SBFI, 2020). The innovation support system in Switzerland builds on this strong foundation.

The organisation and implementation of the innovation support in Switzerland is bottom-up. Firms and universities have to take the initiative. There are only few and small-scale top-down programs in Switzerland.[1] Public R&D funding is mainly in the hands of the

[1]For instance, since 2020 the Swiss Energy Research for the Energy Transitions (SWEET), which is a funding programme of the Swiss Federal Office of Energy (SFOE), or since 2021 the Flagship projects of Innosuisse targeting innovation for system-relevant and societal challenges.

federal government, with two important agencies: the Swiss National Science Foundation (SNSF), which supports basic research at universities, and Innosuisse, which supports innovation mainly through knowledge and technology transfer (KTT) between private firms and public universities (i.e., R&D cooperation, see next chapter). Other Innosuisse instruments are the direct support of start-ups, as well as coaching, networking, and project set-up, including idea generation. Both SNSF and Innosuisse grant their funds according to the quality of the applications rated by experts in the respective fields.

Importantly, apart from Innosuisse, Switzerland has no direct innovation support for private firms on the federal level. However, there are different initiatives on NUTS-3 level (the Swiss cantons). Since January 1st 2020, each of the 26 Swiss cantons can grant tax credits for R&D investments. They are free to allow companies to deduct up to 150% of their R&D expenses from their taxable income. Also since January 1st 2020, the cantons can make use of the "patent box"; up to 90% of the net income derived from patents can benefit from reduced taxation at the cantonal level. The patent box is less important for Switzerland though, as by 2023 less than 0.7% of firms above 5 employees have made use of it. In addition to these two measures, the cantons can support innovation through further measures such as corporate tax incentives, support for start-ups, and the creation of clusters and regional networks. Innovation promotion measures often also include the coordination of activities and the dissemination of information, but not the direct financing of R&D investments (SBFI, 2020).

Switzerland is also part of the EU framework programs for research and innovation, which in our time frame are Horizon 2020 and its successor Horizon Europe. These programs mainly fund (basic) research at universities. However, over the period 2015-2019, Horizon 2020 programs had also funded more than 200 million CHF for projects with small- and medium sized firms in Switzerland. This funding is substantial and corresponds to about 25% of the annual Innosuisse budget for the funding of R&D cooperation. Switzerland is only partially associated with the Horizon Europe programs. This means that it is excluded from certain European funding programs, like the European Research Council (ERC) grant.

In Section 4.3 we will outline how in our setup we control for the these alternative innovation policy instruments in Switzerland.

3.2 The Innosuisse model

Innosuisse is the most important public agency for innovation support in Switzerland. What makes it special in an international context is that its main funding instrument supports innovation through the financing of research cooperation between private firms and public universities[2]. Firms submit together with universities applications for a joint innovation project. Innosuisse will then evaluate the projects and, if approved, it will pay the project costs that arise at the public university, for example the salaries of the respective researchers. The private firms do not themselves receive any funding but instead have to contribute their own financial means to the innovation project. The split between the Innosuisse funding of the universities and the private means on behalf of the firms has over the relevant time period been around 50% (see figure A.IIii). The annual budget of Innosuisse for the funding of joint innovation projects ranged over the years 2019 to 2023 between 140 and 170 million CHF. This means that in this period the total annual joint innovation project costs (Innosuisse funding and corresponding private R&D investments) amounted to over 300 million CHF.[3]

The project funding of Innosuisse explicitly targets small and medium-sized established firms, but also start-ups and spin-offs (about 30%). It encompasses national and international university partners (e.g., Eurostars). In our sample ranging from 2019 to 2023, the financial contribution of Innosuisse for each innovation project has been about 360'000 CHF on average, while the total costs of the respective innovation projects have been about 710'000 CHF on average (these numbers are calculated without overhead costs). The distribution of the Innosuisse funding in CHF is shown in figure A.IIi. Given that the targeted firms are comparatively small firms, the size of the average innovation project is substantial. About half of Innosuisse's project funding goes to universities and institutes of technology, the other half to universities of applied sciences.

The R&D support of Innosuisse thus do not just provide funding, but instead allow for an extension of the knowledge capacity of firms. The subsidies come in the form of access to qualified research personnel and an accompanying infrastructure. Since the companies have to bear about half of the project costs, they have a considerable interest in the

[2] For a general overview of the knowledge and technology transfer in Switzerland, see Wörter et al. (2024)

[3] This stands in relation to the total amount of R&D investments in Switzerland of about 24.6 billion CHF, with 7.5 billion public and 16.8 billion private R&D investments (BFS, 2023). However, note that a large fraction of the private R&D investments in Switzerland is performed by a few (very) large multinational corporations, which are not a relevant target group for Innosuisse funding.

project and are thus under greater pressure to succeed than with a simple transfer of funds. Innovation projects with universities typically differ in some attributes from other innovation projects of the firms: they are more novel (i.e., more radical than incremental), more technologically complex, riskier, and have a focus on knowledge and technology exchange (Spescha, Tran, & Wörter, 2024). They are usually also more closely aligned with basic research than other innovation projects, which might enhance the knowledge spillovers from the projects. In general, Innosuisse aims to fund innovation projects that would otherwise not have been pursued. Note that next to carrying half of the project costs, the requirement to work together with a university also incurs costs for firms in the form of finding partners, building up trust, as well as coordination.

4 Research design

4.1 Endogeneity

4.1.1 A twofold selection problem

The Innosuisse funding is not randomly distributed among Swiss firms. We can therefore not just make a simple mean comparison between the performance of firms funded by Innosuisse and those not funded by Innosuisse. Such a comparison would suffer from a twofold selection problem: First, firms self-select into the application for Innosuisse funding. Second, Innosuisse selects only a subset of applicants (or projects).

The first selection problem yields a biased treatment effect in a simple mean comparison. The firms that self-select into Innosuisse funding likely differ from all other firms in the economy. If those differences influence how firms develop over time, this causes a bias in the estimated effect of the Innosuisse funding. For example, if only small high-tech start-ups that are on a fast growth path apply for Innosuisse funding, we would overestimate the average impact of the funding, as these companies would have grown faster than all other companies even without the funding. Conversely, if only firms that face large obstacles in their innovation activities applied for Innouisse funding, we would underestimate the effect of the funding. Hence, in a simple mean comparison, the direction of the bias is ambiguous and we cannot know whether these biases cancel out each other.

The second selection problem likely leads to an upward bias. Innosuisse does not select the project applications randomly. It rather evaluates all applications and tries to fund projects that are of higher quality and thus have a higher market potential. The higher quality of those projects would have allowed the funded firms to perform better than the non-funded firms even in the absence of funding. The strength of this bias depends itself on the quality of the selection process. If Innosuisse is able to distinguish well between high-quality and low-quality projects, the selection bias is greater.

4.1.2 The necessity of a valid counterfactual

We address this twofold selection problem by using a difference-in-differences (DiD) approach that builds a counterfactual state of funded firms without the funding by assuming parallel trends to a valid control group. The choice of a valid control group that identifies the desired treatment effect is the pivotal aspect of a DiD: firms funded by Innosuisse must be compared with firms that are very similar to them but did not receive funding. In order to obtain a valid control group, we need to eliminate significant differences between the control group and the funded firms prior to their funding. This makes a comparable evolution of those groups after the funding more credible.

One set of differences between the funded firms and a possible control group of all firms in the Swiss economy is accounted for by firm fixed effects. These eliminate time-invariant heterogeneity and accounts for any differences in firm-level characteristics between the treatment and control group that are stable over time. Differences in observable time-variant heterogeneity are typically addressed by using covariates. However, we cannot include covariates after the treatment has taken place, as they themselves might have been impacted by the treatment. Instead, to account for the remaining differences between the treatment and control group, we randomly exclude firms in the control group that are dissimilar to the funded firms prior to their funding. This makes it more likely that the two groups would have evolved in parallel without the Innosuisse support, enabling us to identify the funding effect. Section 5.2.3 explains in detail how we achieve this balance.

Because we have a selected sample of firms, we can at most identify the average treatment effect on the treated (ATT). Only firms that expect to benefit from Innosuisse funding will apply for it. If Innosuisse were to distribute funds to all firms in the economy, most firms would not be able to benefit because they lack critical capacities. We can only assess the impact of Innosuisse funding on the non-random sample of firms that actually applied. This is not a problem in itself, as these firms are also the relevant target group. However, the funding effect we identify unlikely generalizes to all types of sub-groups in the economy.

4.2 Empirical model

4.2.1 TWFEDD

The starting point of the analysis is the two-way fixed-effects difference-in-differences model (TWFEDD) estimated by OLS. This specification has been the standard in most empirical difference-in-differences (DiD) designs over the past two decades. The outcome variable y_{it} is regressed on a treatment variable D_{it} and on a set of firm dummies $\{\mathbb{1}[j = i]\}_j$ and time dummies $\{\mathbb{1}[s = t]\}_s$. The corresponding regression model is

$$y_{it} = \beta_0 + \beta_{TWFEDD} D_{it} + \mu_i + \tau_t + u_{it} \tag{1}$$

where β_{TWFEDD} is the parameter of interest and μ_i and τ_t are the firm and time fixed effects, respectively. The central assumptions for TWFEDD to identify the ATT in case of a single treatment group are no-anticipation, parallel trends, and i.i.d. sampling (Goodman-Bacon, 2021). However, our setup relies on three different treatment groups that are treated at different points in time. This requires the further assumption that treatment effects are constant over time and among groups. We likely have heterogeneous treatment effects that are dynamic (i.e., change over time) and differ between treatment groups, violating the identifying assumption of treatment effect homogeneity.

Dynamic treatment effects lead to "forbidden comparisons" (Borusyak et al., 2024). OLS also uses already treated units as control group for the treated units. If there are dynamic effects where the ATTs increase over time, it will cause downward biased effects induced by negative weighting, because OLS "subtracts" the increasing treatment-path of the already treated units from the estimated ATTs. In our setting, β_{TWFEDD} will thus be underestimated (de Chaisemartin & D'Haultfoeuille, 2020; Goodman-Bacon, 2021).

To address this issue, we rely on novel DiD approaches that can identify the ATT with multiple treatment groups even in the presence of treatment effect heterogeneity. The literature provides approaches for staggered treatments (Callaway & Sant'Anna, 2021; Borusyak et al., 2024; de Chaisemartin & D'Haultfoeuille, 2024), repeated treatments (de Chaisemartin & D'Haultfoeuille, 2024), and continuous treatments (de Chaisemartin, D'Haultfoeuille, Pasquier, & Vazquez-Bare, 2022; Callaway, Goodman-Bacon, & Sant'Anna, 2024). The approach of Callaway and Sant'Anna (2021) will provide the baseline in this study.

4.2.2 Callaway and Sant'Anna (2021)

Callaway and Sant'Anna (2021) focus on staggered treatments in a setup with multiple time periods "t" and multiple treatment groups "g". In our context, "t" refers to a particular year between 2010 2022 in a biennial cycle and "g" is the two-year lagged period where a treatment group is first treated (i.e., 2018, 2020, or 2022). The DiD estimator of Callaway and Sant'Anna (2021) focuses on the group-time average treatment effect on the treated ATT_{gt}. Each ATT_{gt} is a 2×2 DiD for each treatment group "g" at each time period "t" with baseline period "g-2". It serves as the building block for more aggregated parameters, which in our case are event study effects. The different ATT_{gt}'s are "averaged" (e.g., weighted by sample shares) across the different treatment groups to create ATT_t's. The parallel trends assumption must hold backward only up to "g-2" and is thus relatively weak. In contrast to TWFEDD, Callaway and Sant'Anna (2021) makes no "forbidden comparisons" and allows researchers to define the weighting. This means that the approach allows for arbitrary treatment effect heterogeneity over time and groups. Importantly, Callaway and Sant'Anna (2021) allows for the inclusion of pre-treatment covariates to achieve more balanced treatment and control groups. Callaway and Sant'Anna (2021) propose three different DiD estimands to recover the ATT_{gt}'s: outcome regression, inverse probability weighting according to Abadie (2005), and an improved doubly-robust method using inverse probability weighting and weighted least squares of Sant'Anna and Zhao (2020). The authors recommend the use of the doubly-robust method, as it is a combination of the other two and more robust to model misspecification.

4.2.3 Borusyak, Jaravel, and Spiess (2024)

Borusyak et al. (2024) derive an estimator that is efficient under unrestricted heterogeneity of treatment effects in event studies (i.e., has the smallest standard errors). Efficiency further requires the Gauss-Markov assumptions. The estimator has a representation as an imputation estimator. First, predict $\hat{Y}_{it}(0)$ from the sample of all untreated observations using a model with firm and time fixed effects. Second, calculate the individual treatment effects for each observation: $\tau_{it} = Y_{it}(1)$ - $\hat{Y}_{it}(0)$. Third, estimate the target parameter τ as a weighted sum of all τ_{it} with, for example, frequency weights. While the estimator of Callaway and Sant'Anna (2021) uses only "g-2" as counterfactual, Borusyak et al. (2024) use all periods "t=2010" to "g-2" as counterfactual. The latter can lead to bias if parallel trends do not hold over the whole pre-treatment period (e.g., group-specific trends).

In fact, the efficiency advantage of Borusyak et al. (2024) comes from this pre-periods extension. The estimator is also more efficient in the presence of heteroscedasticity and serial correlation. Note that Borusyak et al. (2024) can also incorporate non-binary treatments.

4.2.4 de Chaisemartin and D'Haultfoeuille (2024)

de Chaisemartin and D'Haultfoeuille (2024) propose an event-study-like estimator that is robust to heterogeneous treatment effects for staggered treatments but also for non-binary and/or non-staggered treatments. The proposed estimator DID_{gl} compares the evolution of the outcome for the treatment group "g" until period "l" to one or more control groups with the same period one treatment as "g" but that has not yet changed until period "l" or never changes. The DID_l estimator then calculates a weighted average over the different DID_{gl}. Comparing switchers to non-switchers with the same period one treatment is important. If period one treatments were to differ, that would require a stronger parallel trends assumption that imposes the same evolution of outcomes for all treated groups. In binary and staggered designs, DID_l reduce to the estimator of the event-study parameters of Callaway and Sant'Anna (2021). Because DID_{gl} requires that there exist groups with the same period-one-treatment, there must not be too many treatment values. In non-binary and/or non-staggered designs, DID_l has a more difficult interpretation. de Chaisemartin and D'Haultfoeuille (2024) propose three strategies that allow for a better interpretation: treatment-path-specific DID_l, normalized DID_l^n, and cost-benefit analysis. We rely on the normalized DID_l^n, which scales DID_l by the total treatment dose group "g" has received until "l" irrespective of when these doses occurred. DID_l^n is then a weighted average of the effects of the current "l" and the "l-1" lagged treatments. It can be used to test whether current and lagged treatments have the same effect and do not change over time.

4.3 Confounding innovation support policies

In addition to the funding of R&D cooperation between firms and universities (section 3.1), Switzerland has a number of other instruments to promote innovation. In principle, there is no direct public funding of R&D in Switzerland. However, the cantons (NUTS-3) have different strategies for promoting innovation. As of 2020, there are the above mentioned R&D tax credits and the patent box at the cantonal level. In addition, they rely on various other funding strategies, such as the coordination of innovation activities. This could pose a problem for our estimates if firms in cantons with more favourable innovation funding regimes also receive more Innosuisse funding. Figure A.Ii shows that application and funding rates differ across cantons and a failure to account for different cantonal trends likely biases our results. We thus use different linear trends per canton or include more rigorous canton-by-time fixed effects in our model. Another element of innovation funding in Switzerland, described in section 3.1, is the participation in the EU Framework Programs. The problem is that firms may have received support from EU Framework Programs and Innosuisse at the same time or that funding from both is more difficult to attain such that firms that are rejected at one agency more frequently receive funding from the other. This could also lead to biased treatment effect. Since our survey data has information for each firm on whether it also participated in the EU framework programs, we will drop firms that were funded by EU programs from both treatment and control groups.

4.4 Specification curve

In an empirical study, researchers have a great deal of discretion in specifying their empirical strategy. These 'researcher's degrees of freedom' (Simmons, Nelson, & Simonsohn, 2011) arise from an uncertainty in how to make the right choices, for example with regard to data collection, processing, or analysis. Often researchers make a certain set of choices, while they could equally well have made a quite different set of choices, because none of the available choices are clearly superior to each other. The problem is that such alternative choices usually lead to a large array of equally defensible but often very different results. Spescha, 2021 describes in detail this important problem and its consequences for empirical economic research.

One way of dealing with the wide range of researcher degrees of freedom is to use specification curve analysis (Simonsohn et al., 2020), which presents the results of thousands of specifications at once in a highly aggregated form. This method goes far beyond the presentation of tables of alternative results - as often found in the 'robustness checks' section of a paper - and makes it possible to examine all kinds of specification choices. This applies not only to the choice of control variables, but also to sample restrictions, estimation methods, or functional forms. Specification curve analysis involves three steps: 1) identifying the set of researcher degrees of freedom, 2) displaying the results, and 3) jointly testing all the specifications. The graphical display allows the researchers to identify consequential degrees of freedom. The joint test further shows whether all available specifications together can reject the null hypothesis of no effect.

To strengthen the credibility of our main specification, we use a specification curve analysis that includes six dimensions of researcher degrees of freedom. The first dimension includes four estimators (TWFEDD, Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfoeuille (2024), Borusyak et al. (2024)), the second three anticipation lags δ (none, 6 months, 12 months), the third three control groups (balanced never-applicants, unbalanced never-applicants, unbalanced rejected applicants), the fourth three fixed effects regimes (two-way, two-way and cantonal trends, two-way and canton-by-time), the fifth in- or excludes firms with international innovation funding, and the sixth in- or excludes microfirms with fewer than 5 employees.

5 Data

5.1 Sources

The data used in this study stems from multiple waves of firm-level surveys conducted by the KOF Swiss Economic Institute at the ETH Zürich (ETHZ) and mandated by the State Secretariat for Education, Research and Innovation (SERI). The firm-level surveys run on the KOF Enterprise Panel (KEP), which is a stratified random sample representative of the Swiss economy. It covers about 9'500 firms and is stratified on geography, industry (NACE Rev.2 two-digits), and firm size. It is drawn from the official Business and Enterprise Register (BER) of the Federal Statistical Office (FSO) and biannually updated to account for changes in the population of firms and to combat sample attrition.

The first part of the data consists of 6 waves of the Swiss Innovation and Digitalisation Survey (SIDS): 2011, 2013, 2015, 2017, 2019, 2021, and 2023. The SIDS is the official implementation of the CIS in Switzerland and its counterparts in the U.S.: the Annual Business Survey (ABS) and Business Enterprise Research and Development (BERD) Survey. In-between the years of the SIDS, other firm-level surveys have been conducted. They form the second part of the data. Namely, the Swiss Knowledge Transfer Survey (SKTS) in 2011 and 2018, and the Swiss Digitalisation Survey (SDS) in 2016 and 2020. The third part of the data consists of the 3 waves of the Innosuisse Survey (IS), which as been mandated by Innosuisse - The Swiss Innovation Agency and conducted in 2019, 2021, and 2023. This particular survey runs on the population of firms that have applied for funding of their innovation projects at Innosuisse. This data is further combined with detailed data on the innovation project level for all Innosuisse applicants, that is, funded and non-funded firms as well as research partners. This latter data has been provided by Innosuisse. Merged together these four different parts of the data provide one large, comprehensive firm-level panel dataset that covers the years 2010 to 2022.

The participation of the firms in the different surveys is voluntary. The response rates of the different surveys range between 25% and 40%. To assess non-response bias, KOF always conducts a series of telephone interviews to a random sample of 500 non-responding firms. They have to answer to the most important questions from the SIDS. Comparisons of these answers with the survey data shows that non-response bias is not an issue. However, because of the non-response, the panel is unbalanced. For our baseline specifications, we have 3220 unique firm-year observations and 920 unique firms that comprise 383 funded firms and 537 control firms.

5.2 Variables

5.2.1 Outcomes

The outcome variables for this study are firms' sales and employment figures (y_{it}). Both variables stem from the different survey waves outlined in section 5.1. Firms report the nominal values of their sales in CHF and the full-time equivalent numbers of their employees at the end of and two years before the last completed fiscal year prior to the survey wave. For example, the 2021 SIDS provides sales and employment figures for 2020 and 2018. We do not analyze further outcome variables, because we would lose too many observations of switchers due of the unbalanced nature of the data. Sales and employment enter the specifications in natural logarithms. Since we rely on a DiD method, we will essentially look at percentage growth in both sales and employment.

5.2.2 Treatment group

The main treatment D_{it} is a binary variable indicating if a firm received project funding from Innosuisse in the two years prior to year "t". This information stems from the project-level data provided by Innosuisse, which contains the universe of applications to Innosuisse from 2017 to 2022. The applications are split into approved project funding and refused project funding. The data also provides information on approved and refused innovation cheques. These consist of a direct funding of 15'000 CHF to firms. Because this funding is very small compared to the funding for innovation projects, we do not include approved innovation cheques as treatments, except if the innovation cheque amounts to more than 25% of prior firm sales. However, this latter step concerns only a handful of firms. While firms with approved project funding constitute our treatment group, firms with refused project funding will form one of the possible control groups.

The baseline specification uses a staggered treatment variable $D_{it} = \mathbb{1}[t \geq g_i]$ where $g_i \in \{2018, 2020, 2022\}$ is the first period where firm "i" receives Innosuisse funding. The project-level data also contains the start date of each innovation project and we create treatment groups according to the two-year window prior to year "t" as shown in figure A.Iii. For example, firms that are funded between the start of 2017 and the end of 2018 belong to treatment group $g_i = 2018$ and will have D_{it} set to "0" for all years before 2018 and set to "1" for 2018 and all years after 2018. For the estimator of de Chaisemartin and D'Haultfoeuille (2024), we assume a non-staggered treatment $D_{it} = \mathbb{1}[t = g_i]$ that is "1" only in the period where the firm actually receives the funding. For the other estimators, we use the staggered variable. The intuition behind all approaches is that only one treatment occurred in the first funding period.

As evident in figure A.Iii, the distribution of the start dates of funded projects roughly resembles a uniform distribution within each treatment window with the exception of 2018 and 2019. To rule out that the analysis crucially depends on a specific start date, we use alternative cut-offs for the start date that shift the two year window by $\delta = 6$ months and $\delta = 12$ months. If we set $\delta = 6$, the treatment variable will receive the value "1" in the year 2018 if the funded project started at between July 2016 and June 2018. Importantly, many funded innovation projects follow a funded innovation cheque. Since cheques aim to kick-start an innovation project, this means that the plans for the innovation project are in many cases older than the actual start date. As such, δ allows us to assess the impact of different anticipation lags.

The KOF surveys include only firms with at least 5 employees. In contrast, the Innosuisse Survey also includes firms with less than 5 employees. Many of those firms are start-ups or spin-offs. Hence, a potential control group of non-funded firms created from the KOF Surveys never contains firms with less than 5 employees. The treatment group, on the other hand, contains many of them. To make firms in treatment and control group more comparable, we drop firms with less than 5 employees from the treatment group in our baseline specification.

5.2.3 Control group

We implicitly defined the control group in the previous section as firms that have not received any project funding from Innosuisse between 2010 and 2022. However, the sample of firms that have not received any project funding is heterogeneous. In order to identify the ATT we have to specify the right control group that evolves in parallel to the treatment groups. To hedge against the possibility that we choose the wrong control group, we will run specifications with several different control groups.

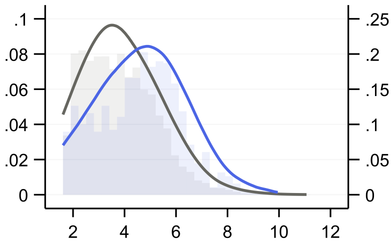
The baseline control group **A** consists of all firms in the Swiss economy that did not apply for any project funding from Innosuisse between 2010 and 2022 and exhibit similar characteristics as the firms in the treatment groups. To create this control group, we start with all firms in the Swiss economy that did not receive any funding from Innosuisse in the given horizon. We then subtract the firms that applied for funding but were rejected. As evident from the distribution of the pre-treatment variables in the left-hand side in figure 5-1, these firms are quite dissimilar from the group of treated firms prior to the treatment. If we fail to account for this heterogeneity, our results are likely biased due to the twofold selection problem described in 4.1.1. To approximate the distribution of the pre-treatment variables of the treatment groups, we randomly exclude firms in the control group that are dissimilar in these potential confounders.

First, we randomly exclude firms that have no exports in more than half of the observed years.[4] As evident in subfigure g, a far higher share of non-applicants has no exports than when compared to funded firms. We thus randomly drop firms from the control group until they show a similar share of no exports as the treated group. Second, we randomly exclude firms that have no employees with a tertiary degree (academics) in more than half of the observed years. Again, subfigure c shows that they are far more frequent in the control group. Third, we randomly exclude firms that have no R&D expenditures in more than half of the observed years. Subfigure m shows the respective imbalance between the two groups. Last, we randomly exclude firms that have a different number of competitors in more than half of the observed years. More specifically, we randomly drop firms that have more than 5 competitors until the two distributions are similar. This matched group of firms provides the baseline control group **A** for this study.

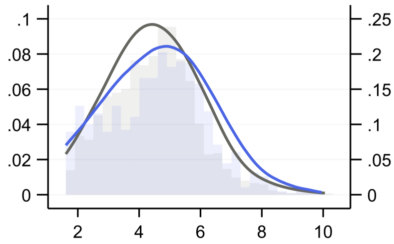
[4] Note that because we do not have a treatment year for the never-treated firms, all years are pre-treatment years for them. Furthermore, the balancing applies to the median of the pre-treatment variables for each firm such that the respective exclusion criteria must apply to more than half of those years. For example, when using the export variable, a firm must lack exports in more than half of the pre-treatment years, which are are all observed years for the control and the years $t_g : t < g$ for the treated groups g.

Figure 5-1: Balancing of pre-treatment variables between treatment and control

$ln(Employees_{it})$

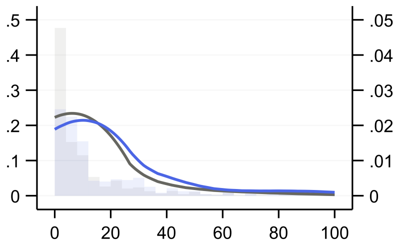


(a) Unbalanced

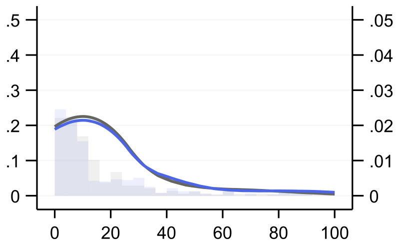


(b) Balanced

$Academics_{it} \backslash Employees_{it}$

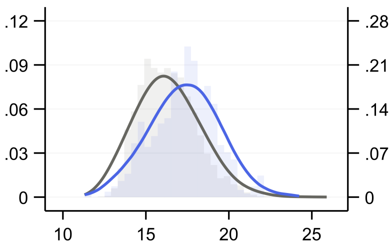


(c) Unbalanced

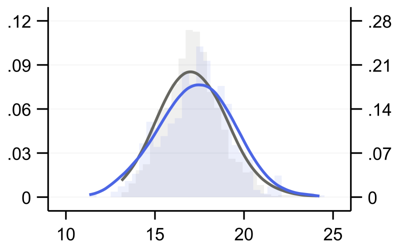


(d) Balanced

$ln(Sales_{it})$

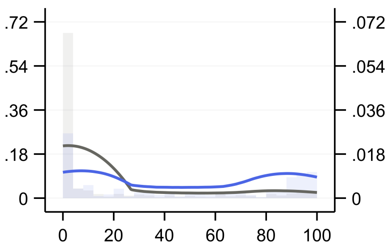


(e) Unbalanced

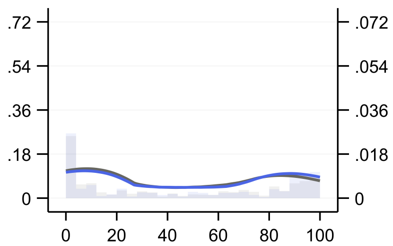


(f) Balanced

$Exports_{it} \backslash Sales_{it}$



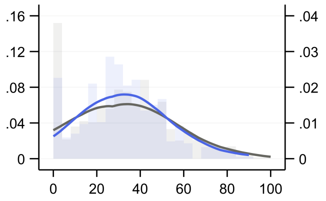
(g) Unbalanced



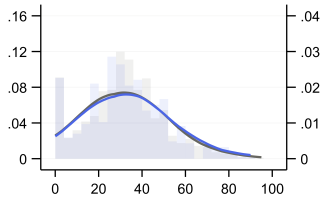
(h) Balanced

Figure 5-1: Balancing of pre-treatment variables between treatment and control (ctd)

$Costs_{it}^{Labor} \backslash Sales_{it}$

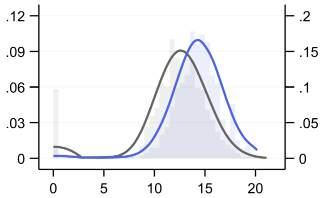


(i) Unbalanced

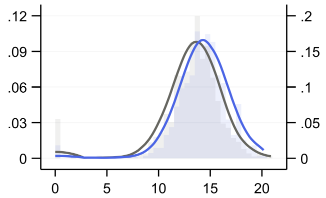


(j) Balanced

$ln(Investments_{it} + 1)$

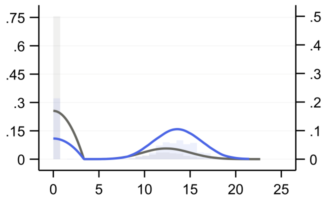


(k) Unbalanced

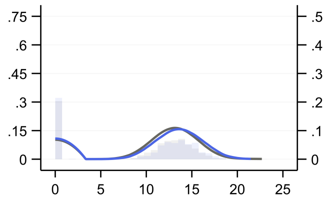


(l) Balanced

$ln(expenditures_{it}^{R\&D} + 1)$

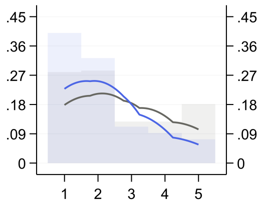


(m) Unbalanced

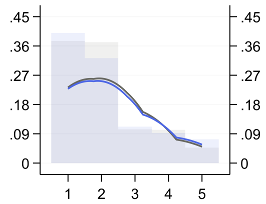


(n) Balanced

$Competitors_{it}$



(o) Unbalanced



(p) Balanced

Note: The figure shows the pre-treatment distributions of key variables for the control and treatment groups. Specifically, we use all periods $t < g$ for each treatment group g and all periods for the control groups as they are not treated (yet) in the observed horizon. The histograms are lightly shaded in the background and measured as fractions on the left axis. The solid lines are corresponding kernel densities using Epanechnikov kernels measured on the right axis. All treatment groups are pooled and presented in blue —. Control groups are in grey —. The left panels report the unbalanced controls that include all firms that never applied for Innosuisse funding (control group **C**). The right panel reports the balanced control group that excludes firms randomly if they do not resemble the treated units (control group **A**).

Figure 5-1 shows the distribution of potential confounders for the treatment group as well as the control group before (control group **B**) and after the balancing (control group **A**). We refer the interested reader to table A.I that contains further covariates and reports the mean, standard deviation, minimum, first quartile, median, third quartile, maximum, and the number of observations behind those statistics. The figure shows that group **A** is much more similar to the group of treated firms than group **B**. This successful balancing should limit the selection bias, especially since the balance also applies to potential confounders that were not part of the random exclusions like the labor share in figure j, investments in figure l, and even to our outcome variables of interest, employees in figure b and sales in figure f.

The second, broadest control group **B** consists of all firms that never applied for project funding from Innosuisse. It resembles control group **A** but neglects balancing. This control group allows a comparison of our treatment group to the "average firm" in the Swiss economy and assesses the effect of the pre-treatment balancing. The DiD estimate of the treatment group with control group **B** will be a combination of the ATT and both types of selection bias identified in 4.1.1.

The third control group **C** consists of all firms that applied for Innosuisse funding and neither received project funding nor an innovation cheque. It excludes all other firms in the Swiss economy. It is affected by the second type of selection bias mentioned in 4.1.1, because it may contain only firms whose applications of projects and cheques are of a lower quality. The DiD with this control group will thus likely overestimate the effect of the Innosuisse funding. The use of control group **C** is interesting to evaluate whether Innosuisse funding mainly picks "winners".

6 Results

6.1 Baseline Results

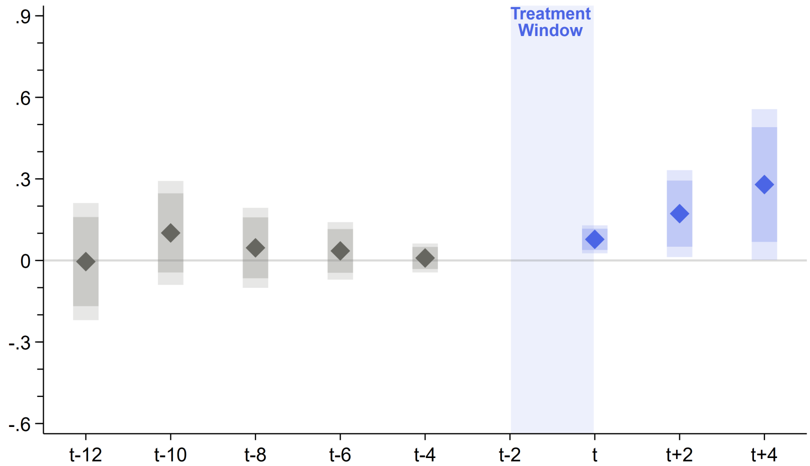
Our baseline results rely on the estimator from Callaway and Sant'Anna (2021). We use a binary treatment indicating whether a firm received an Innosuisse grant or not and exclude any firm that receives funding more than once. Furthermore, we assume that there is no anticipation lag, $\delta = 0$. We use the manually balanced control group **A** and include cantonal trends to account for different R&D tax rates and patent boxes. Furthermore, we exclude firms from the control or treatment groups if they receive any international innovation funding, and we also exclude micro-firms with fewer than five employees. The last point is related to the fact that the sample from which we derive our dependent variable is based on firms with more than five employees.

Figure 6-2 shows the baseline results, subfigure 6-2a for employment and subfigure 6-2b for sales. In the case of employment, the Innosuisse funding shows a statistically significant effect on the 1% level that strongly increases over time. As visible in Table 6-1, the point estimate increases from 7.8% in t to 27.9% in $t + 4$. In the case of sales, the effect of the Innosuisse funding is not yet significantly different from zero in t. However, the effect increases strongly over time and it becomes statistically significant at the 1% level in $t + 4$. This last point estimate is 41.9%.

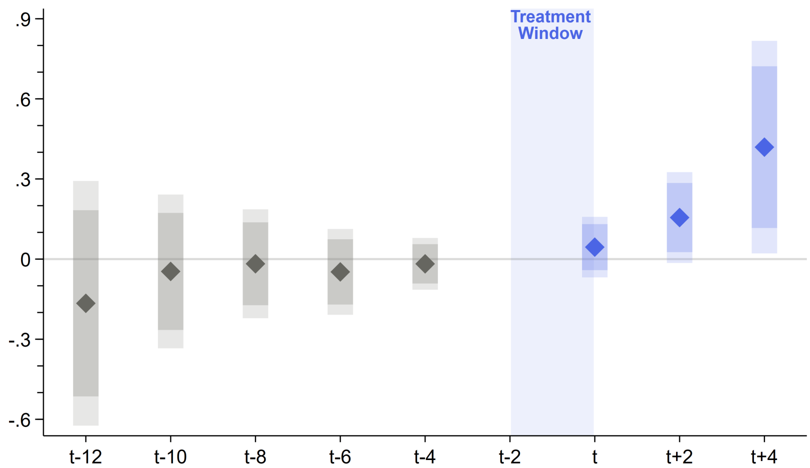
The fact that sales need a longer time lag to react to the Innosuisse funding makes sense intuitively. When an innovation project starts, firms tend to hire new employees already at the outset. Sales, on the other hand, will only react once the project has been completed and the product/service has been launched. The increased sales may then in turn cause firms to hire further employees. This may be the lagged effect of the Innosuisse funding on employment we observe in $t + 4$.

It is important that the prior trends are flat and are not significantly different from zero in any pre-period. This applies to both sub-figure 6-2a and sub-figure 6-2b. This makes the assumption of parallel trends more credible in our context. In terms of employment and sales, funded and non-funded companies were similar before Innosuisse funding and only differ from each other after funding.

Figure 6-2: Treatment effect dynamics of Innosuisse funding



(a) $\ln(Employment_{it})$



(b) $\ln(Sales_{it})$

Note: The figure shows the baseline event study estimates using the estimation procedure of Callaway and Sant'Anna (2021). We use residualized dependent variables that purge cantonal trends, assume an anticipation lag of $\delta = 0$, use control group **A**, and exclude firms that received innovation funding from international sources as well as all microfirms with less than 5 full-time equivalents. All estimates are forward looking and compare the evolution of the treated against the control group from $g - t_{lag}$ to $g - 2$ for pre-treatment periods $t_{lag} \in \{4, 6, 8, 10, 12\}$ or from $g - 2$ to $g + t_{lead}$ for post-treatment periods $t_{lead} \in \{0, 2, 4\}$. For better readability, we use t in the figures although it actually refers to the relative time to the treatment period g, which varies across treatment groups. As suggested by Borusyak et al. (2024), we use different colors for the estimates, namely ◆ for the pre-treatment placebo estimates and ◆ for the dynamic treatment effects. The lighter shaded bands (▭) correspond to 99% CIs and the darker shaded ones (▭) to 95% CIs.

Table 6-1: Treatment effect dynamics of Innosuisse funding

	$\ln(Employment_{it})$	$\ln(Sales_{it})$
	Effects	
t	0.078	0.045
	(0.020)	(0.044)
t+2	0.172	0.156
	(0.062)	(0.066)
t+4	0.279	0.419
	(0.108)	(0.154)
Average	0.176	0.207
	(0.049)	(0.063)
	Placebo	
t-4	0.009	-0.018
	(0.021)	(0.038)
t-6	0.035	-0.048
	(0.041)	(0.062)
t-8	0.046	-0.018
	(0.057)	(0.079)
t-10	0.101	-0.046
	(0.074)	(0.112)
t-12	-0.004	-0.166
	(0.084)	(0.178)
Average	0.038	-0.059
	(0.045)	(0.077)
Firm-years	3220	2866
Treated	1144	944
Control	2076	1922
Firms	920	805
Treated	383	310
Control	537	495

Note: This table shows the baseline event study estimates corresponding to figure 6-2. The averages are calculated by simple unweighted averages of the 5 placebo and 3 treatment effects. `Firm-years` reports all unique observations $(i - by - t)$ used in the estimation while `Firms` reports the unique number of firms used in the estimation. Standard errors clustered at the firm-level are in parentheses.

6.2 Treatment Effect Heterogeneity

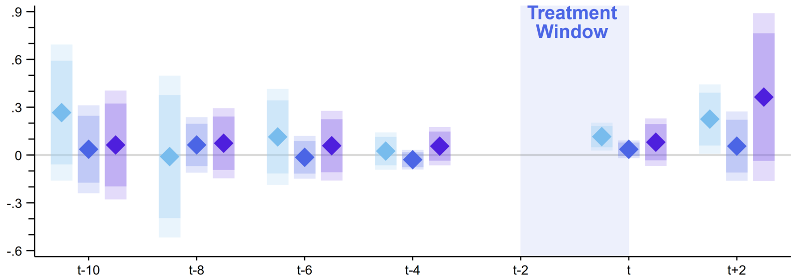
Section 6.1 showed the ATT for the entire sample of firms. We now look at sample splits to study the heterogeneity of the Innosuisse funding effects. First, we split by firm size and partition into three sets based on the cutoffs 50 and 250 full-time equivalents in pre-treatment periods. Figure 6-3a shows the results for this sample split. Innosuisse funding has an increasingly positive impact on employment over time, in both small and large firms. Medium-sized firms seem to profit somewhat less. However, the effects are statistically significant only for small firms. This is not surprising, as they can rely on a much larger sample size than the medium-sized or large firms, which increases the precision of the estimates. Medium-sized firms show only slightly increasing funding effects, but given the positive estimates for small and large firms, this may be a sample effect that can revert with a longer time series. Importantly, the pre-trends are again flat and not significantly different from zero for all three groups sizes, implying that prior to the treatment funded firms developed similarly to non-funded firms.

Second, we split by higher education institutions: 1) the ETH domain, 2) universities, 3) universities of applied sciences, and 4) research institutions and government agencies. This partition only affects funded firms and we use control group **A** for all splits. The results in figure 6-3b show increasing effects of the Innosuisse funding for the ETH domain and the universities of applied sciences. For both universities and research institutions and government agencies we observe statistically insignificant effects. The pre-trends are once again flat and near zero for all higher education institutions.

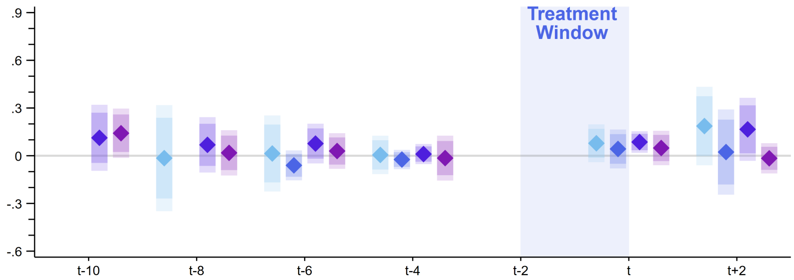
Third, we split by scientific fields: 1) social and health sciences, 2) information and communication technologies, and 3) engineering sciences.[5] This partition again applies only to funded firms. Figure 6-3c shows increasing effects of the Innosuisse funding for information and communication technologies (ICT) as well as for engineering sciences. However, the effect is statistically significant only for the engineering sciences. The social and health sciences show a strong initial increase that does not carry on to the next

[5]This partition bases on the UNESCO's International Standard Classification of Education (ISCED-F 2013), a taxonomy of scientific fields. The cluster of projects in *social and health sciences* comprises the one digit codes 1 (education), 4 (business, administration, and law), 9 (health and welfare), and 10 (services). The cluster of projects in *information and communication technologies* comprises the one digit code 6 (information and communication technologies). The cluster of projects in *engineering sciences* comprises the one digit codes 7 (engineering, manufacturing, and construction) and 8 (agriculture, forestry, and fisheries). As such, *social and health sciences* roughly align with Innosuisse's internal categorization of Social Sciences & Business Management and Life Sciences, *information and communication technologies* with Information & Communications Technology, and *engineering sciences* with Engineering and Energy & Environment

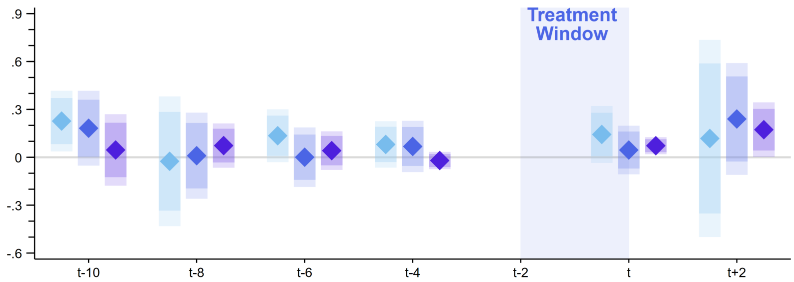
Figure 6-3: Heterogeneous treatment effect dynamics of funding on employment



(a) across firm sizes



(b) across higher education institutions



(c) across scientific fields

Note: The figure shows event study estimates of Innosuisse funding on employment across different subsamples using the baseline specification from figure 6-2. Due to low precision in the estimates from lower observational counts in the subsamples, we do not report the edge periods $g-12$ and $g+4$. The lighter shaded bands correspond to 99% CIs and the darker shaded ones to 95% CIs.
In subfigure a, ◆ marks small firms with FTEs between 5 and 50, ◆ medium-sized firms with FTEs between 50 and 250, and ◆ large firms with more than 250 FTEs. To prevent the funding from affecting the partition, we exclusively group firms based on pre-treatment periods $t < g$.
In subfigure b, ◆ marks the ETH domain, ◆ universities, ◆ universities of applied sciences, and ◆ research institutions of national importance and government agencies. This partition only affects firms that receive funding and refers to the type of higher education institution with whom they collaborate in R&D.
In subfigure c, ◆ marks social and health sciences, ◆ information and communication technologies, and ◆ engineering sciences. This partition only affects firms that receive funding and refers to the scientific field of their innovation project. All splits use the same control group **A** without any balancing to the respective treatment partition.

period. However, the pre-trends for the social and health sciences tend to deviate from zero, making this particular effect less credible. In contrast, the pre-trends for ICT as well as for engineering sciences are again flat and near zero.

Overall, we can say that the Innosuisse funding has a positive and increasing effect on employment when analysed by different subgroups, even though the sample sizes of these subgroups are decidedly lower. Figures 6-3a, 6-3b, and 6-3c in the appendix show these same analyses of treatment effect heterogeneity for the firm sales variable. The results for the three firm size groups are very similar, even if the precision of the estimates is different from that of the employment effects. In contrast, the results for the four higher education institutions are less convincing, because the estimates are less precise and the pre-trends likely violated. One possible explanation here is that the four groups rely on the same control group **A**, which is balanced for the entire economy, and the four groups may thus not each have balanced control groups. The results for the three scientific fields show the same problems. Although the pattern of the funding effects is similar to the pattern in the case of employment, the estimates are less precise and the pre-trends are not parallel.

7 Robustness

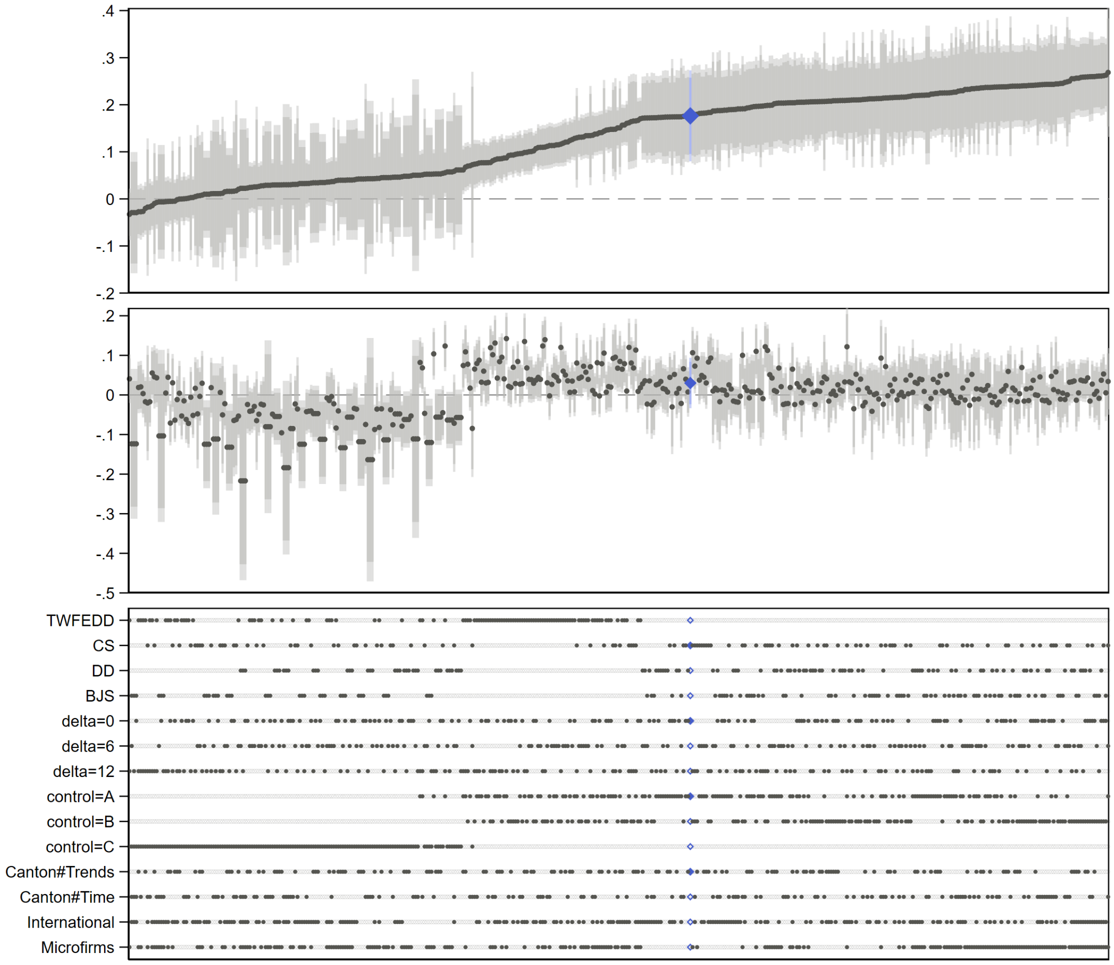
7.1 Specification Curve Analysis

Figure 7-4 shows the specification curves for employment (subfigure a) and sales (subfigure b). The results for employment in subfigure a reveal several interesting patterns. The majority of specifications show positive and statistically significant effects of the Innosuisse funding. We can see only a handful of sign reversals. Moreover, the pre-trends are almost always near zero and statistically insignificant, which implies that our research design is valid. Our baseline specification is located close to the median of all effect sizes.

All of the lower and statistically insignificant effects in subfigure a stem from the use of control group **C**, which is the unbalanced group of firms with rejected innovation projects. These specifications suffer from the second type of selection bias, with Innosuisse choosing which firms to fund. The results indicate that applicants funded by Innosuisse do not outperform applicants not funded by Innosuisse. The funded applicants show only slightly higher employment than the non-funded applicants, with slightly negative pre-trends. This implies either that the non-funded applicants nonetheless carried out their innovation projects or that Innosuisse tends to fund applicants that would otherwise show lower employment growth than non-applicants. However, because control group **C** is unbalanced, it might also be a composition effect.

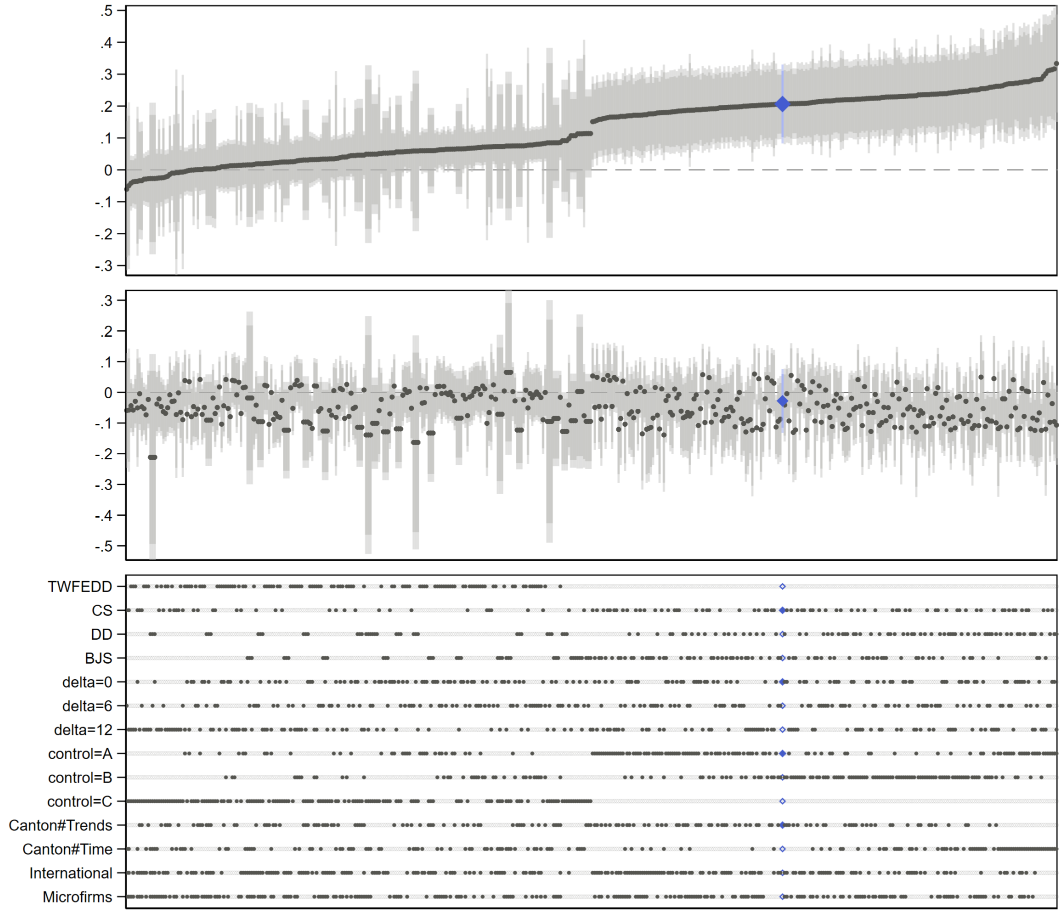
The band of lower but more precisely estimated effects in the middle of subfigure a stems from the TWFEDD. Notably, none of the higher effect sizes to the right of the subfigure stem from the TWFEDD. This is a consequence of the negative weighting of TWFEDD in the presence of treatment effect heterogeneity. Whereas the TWFEDD uses more data and thus delivers more precise estimates, it suffers from a negative bias due to increasing treatment effects. This result is evidence for the superiority of the estimators

Figure 7-4: Specification Curves



(a) $\ln(Employment_{it})$

Figure 7-4: Specification Curves (ctd)



(b) $\ln(Sales_{it})$

Note: The figure shows the average pre-treatment (lower panel) and post-treatment effects (upper panel) over multiple specifications. We follow Callaway and Sant'Anna (2021) and build unweighted averages over the 3 periods pre, $g-4$, $g-6$, and $g-8$, and post treatment, g, $g+2$, and $g+4$, where g indicates the first year after each firm in treatment group g is treated. We only use the 3 placebo effects $t-4$, $t-6$, and $t-8$ because the estimator of de Chaisemartin and D'Haultfoeuille (2024) cannot produce more placebos than effects. The specifications include 6 dimensions. The first dimension includes the four different estimators two-way fixed effects difference-in-differences (TWFEDD), Callaway and Sant'Anna (2021) (CS), de Chaisemartin and D'Haultfoeuille (2024) (DD), and Borusyak et al. (2024) (BJS). The second dimensions includes the three anticipation lags of zero months (delta=0), 6 months (delta=6), and 12 months (delta=12). The third dimension includes our three different control groups **A**, **B**, and **C** as specified in section 5.2.3. The fourth dimension includes different fixed effects that control for canton trends (Canton#Trend), canton-by-time fixed effects (Canton#Time) or none of them. The fifth dimension includes or excludes firms that applied for international innovation support (International). The sixth dimension includes or excludes microfirms with less than 5 full-time equivalent employees (Microfirms). The main specification reported in section 6.1 is labeled in blue (⋄)

of Callaway and Sant'Anna (2021), de Chaisemartin and D'Haultfoeuille (2024), and Borusyak et al. (2024) in our context. These estimators may be less efficient in the (unlikely) case of homogeneous treatment effects, but remain unbiased in the case of heterogeneous treatment effects.

The highest and most precise effects in subfigure a are a consequence of the use of control group **B** together with the inclusion of microfirms. First, control group **B** is the unbalanced sample of firms that have never applied for Innosuisse funding. When compared to those firms, the funded firms have performed even better than when compared with the balanced control group **A**. This means that the full sample of firms that never applied for funding showed less employment growth than the counterfactual group of firms that is similar to the funded firms without the funding. Moreover, control group **B** is much larger than control group **A**, leading to higher precision in the estimated effects. Second, microfirms are funded firms with less than 5 employees. Their inclusion makes the funding effects more precise as well, since the group of funded firms now has more observations. The reason for the larger effect sizes may be that the funding has even bigger impacts for microfirms, as the innovation projects are larger relative to their firm size. However, while control group **B** and microfirms deliver higher effect sizes, not using them in the baseline specification is better justifiable from a theoretical point of view. Both specification choices make treatment and control groups more dissimilar and thus the parallel trends assumption less credible.

The use of canton trends or canton-by-time fixed effects shows quite similar results. In fact, the use of canton-by-time fixed effects leads to somewhat higher estimated effects. Because the use of canton-by-time fixed effects is arguably a stricter control, it underscores the validity of our research design: stricter versions deliver higher funding effects. The inclusion or exclusion of international innovation funding makes only little difference for the estimated effects. The estimated effects seem to be a bit lower when international innovation funding is included. This makes sense insofar as it eliminates potential double-funding, which could overestimate the Innosuisse funding effect.

The 0, 6, and 12 months anticipation lags in subfigure 7-4a show no clear pattern. They are quite evenly distributed over the different specifications, meaning that the anticipation lags have no particular impact on the results. One exception is the 12 months anticipation lag, which is associated with the set of lowest effect sizes.

The results for sales in subfigure b show similar patterns as in subfigure a for employment. The most important difference is that the estimated effects of the Innosuisse funding are in general lower and less precisely estimated. Our baseline specification is located at the third quartile of all effect sizes. Compared to the patterns we have observed in subfigure a, the most important difference is that the inclusion of microfirms is not associated with the highest but rather with the lowest effect sizes. Thus, while microfirms show stronger effects on employment, they show weaker effect on sales. This may be because very small firms generally have more difficulty than larger firms in gaining significant market shares through their innovations. The more positive effect sizes in the case of the canton-by-time fixed-effects is even more pronounced in the case of sales. Otherwise subfigure b shows about the same patterns as subfigure a.

Overall, the specification curves in figure 7-4 show evidence for a positive effect of the Innosuisse funding on employment and sales. Compared to our baseline specification, the set of lower and less statistically insignificant as well as the set of higher and more statistically significant funding effects both have theoretically meaningful interpretations. Moreover, the generally flat and near zero pre-trends are evidence that our difference-in-differences setup is valid.

7.2 Goodness of balancing

Table 7-2 provides evidence that our manual balancing of funded and non-funded firms delivers similar results as the balancing implemented in the Callaway and Sant'Anna (2021) estimator. One reason we have implemented the manual balancing is that this way we can retain more firm-year observations. In every survey wave we have the current as well as the two-year lagged values for both sales and employment. In contrast, for all other variables (i.e., our covariates), we have values only for the current period. Because our panel is unbalanced, it implies that if we restrict our estimation sample to only those firms that have information on covariates, we lose many firm-year observations. This is especially problematic for the smaller sample of funded firms. As laid out in section 5.2.3, the manual balancing matches those firms that have never applied for funding and have covariates to those firms that have received funding and have covariates. However, for the estimations we then retain all funded firms, that is, we also include those firms that have no covariates. In contrast, if we applied the balancing of the Callaway and Sant'Anna (2021) estimator, we would lose all funded firms that have no covariates. To assess whether our balancing procedure is valid, we therefore take control group **A** and drop all firms without covariates (see column 2 in Table 7-2 for employment) and compare it to control group **B** relying on the balancing of the Callaway and Sant'Anna (2021) estimator (see column 4 in Table 7-2 for employment).

The two different balancing procedures show quite similar effect sizes, for both the post- and pre-treatment periods. The effects obtained from the Callaway and Sant'Anna (2021) estimator are only slightly higher. This provides evidence that our manual balancing procedure is valid. Importantly, both procedures show lower post-period effect sizes than our baseline estimations using the balanced control group **A** or the estimations using the unbalanced control group **B**. This implies that the sample restriction to only those firms with covariates affects the results. However, we retain our baseline control group **A** because it allows using many more firm-year observations and there is no theoretical reason for why firms with or without covariates should differ from each other. The effect of such a sample restriction is not a priori clear. The sixth and eight columns of Table 7-2 show the same comparisons of the balancing procedures for the sales variable. The message of this second comparison is again the same. The manual balancing is valid, but the sample restriction to only those firms with covariates leads to lower post-period effect sizes. Importantly, note that the Innosuisse funding has lower, but still statistically significant effects also in the case of the restricted samples, for both sales and employment.

8 Conclusion

This study has found a causal effect of funding R&D cooperation with public universities on firm performance. In the context of the Swiss innovation funding system, we found that public funding from Innosuisse has a positive effect on both firm sales and employment. A joint innovation project funded by Innosuisse increases firm sales by about 21% and employment by about 18% on average over the next 5 years. The funding exerts strong dynamic effects that increase over time.

The specification curve analysis shows that our baseline estimates reside within the median and the third quartile of all alternative specifications. The large variation in effect sizes produced by the alternative specifications has a theoretically meaningful interpretation. The specification curve analysis demonstrates the importance of using a suitable estimation method, the correct control group, and controlling for possible confounders. Such a comprehensive presentation of numerous alternative specifications provides maximum transparency for the data analysis.

Table 7-2: Manual versus Callaway and Sant'Anna (2021) balancing

	$\ln(Employment_{it})$				$\ln(Sales_{it})$			
	A		B		A		B	
	Base	Adjust	None	Control	Base	Adjust	None	Control
				Effects				
t	0.078 (0.020)	0.048 (0.025)	0.083 (0.018)	0.054 (0.023)	0.045 (0.044)	0.087 (0.029)	0.059 (0.043)	0.110 (0.027)
t+2	0.172 (0.062)	0.113 (0.071)	0.201 (0.058)	0.137 (0.068)	0.156 (0.066)	0.086 (0.059)	0.198 (0.061)	0.147 (0.055)
t+4	0.279 (0.108)	0.090 (0.059)	0.342 (0.104)	0.117 (0.057)	0.419 (0.154)	0.186 (0.104)	0.483 (0.151)	0.236 (0.103)
Average	0.176 (0.049)	0.083 (0.039)	0.209 (0.047)	0.103 (0.035)	0.207 (0.063)	0.120 (0.047)	0.247 (0.060)	0.164 (0.044)
				Placebo				
t-4	0.009 (0.021)	-0.002 (0.021)	0.019 (0.017)	0.010 (0.019)	-0.018 (0.038)	-0.025 (0.038)	-0.007 (0.036)	-0.012 (0.037)
t-6	0.035 (0.041)	0.024 (0.041)	0.035 (0.034)	0.046 (0.034)	-0.048 (0.062)	-0.059 (0.061)	-0.034 (0.057)	-0.024 (0.058)
t-8	0.046 (0.057)	0.033 (0.057)	0.023 (0.049)	0.043 (0.050)	-0.018 (0.079)	-0.031 (0.079)	-0.046 (0.072)	-0.027 (0.074)
t-10	0.101 (0.074)	0.084 (0.073)	0.057 (0.057)	0.077 (0.058)	-0.046 (0.112)	-0.065 (0.111)	-0.080 (0.102)	-0.063 (0.105)
t-12	-0.004 (0.084)	-0.008 (0.084)	-0.024 (0.055)	-0.008 (0.057)	-0.166 (0.178)	-0.172 (0.178)	-0.204 (0.166)	-0.192 (0.168)
Average	0.038 (0.045)	0.026 (0.044)	0.022 (0.032)	0.034 (0.033)	-0.059 (0.077)	-0.070 (0.077)	-0.074 (0.070)	-0.064 (0.072)
Firm-years	3220	2808	16047	15529	2866	2594	14765	14408

Note: This table shows the effect of different balancing specifications. The 4 columns on the left correspond to employment and the 4 columns on the right to sales. Within those columns the first two use control group **A** while the latter two use control group **B**. Within control group **A**, the first column refers to the baseline assumption that balances control group **B**, the firms that never applied for Innosuisse funding, according to the procedure of section 5.2.3, which randomly excludes firms based on their similarity to the median of pre-treatment variables of the treated. The second column further excludes all treated firms from the estimation if they do not have those pre-treatment variables. Within control group **B**, the first column uses all treated firms and all firms that never applied for Innosuisse funding. The second column uses the same balancing variables as in the baseline control group **A**, but automatically controls for them in the Callaway and Sant'Anna (2021) estimation rather than manually before. Otherwise, the specifications follow those of the baseline design in figure 6-2. Standard errors clustered at the firm-level are in parentheses.

When we compare our results with the performance effects of R&D subsidies found in the existing literature, we see certain differences. The two regression discontinuity designs of Howell (2017) and Santoleri et al. (2022) find effects of R&D subsides on sales of around 35% on employment and of around 30% on sales. These are clearly higher than our estimated effect sizes. The two DiD designs of Vanino et al. (2019) and Mulier and Samarin (2021) show effects more similar to ours. Vanino et al. (2019) find that funded firms grow more in both sales and employment, 6% in the short term and 22% in the medium term. Mulier and Samarin (2021) find that R&D subsides lead to 11% more sales and 7.5% more employment. Consequently, our own estimates closely align with Vanino et al. (2019) and lie between the other estimated effects of Howell (2017), Santoleri et al. (2022), and Mulier and Samarin (2021). In fact, the two larger effects sizes of Howell (2017) and Santoleri et al. (2022) could be due to a difference in the applied methodological approaches. They both use cut-offs in the application rankings of funding agencies and thus compare funded applicants to other applicants and not funded applicants to (balanced) non-applicants, like in the case of the DiD designs here.

The study shows that funding R&D cooperation between private firms and public universities is a potent alternative next to more common policy instruments such as direct R&D subsidies, patent boxes, or tax credits. Instead of targeting the cost side of innovation with monetary transfers, funding R&D cooperation enhances the capacities of firms to generate innovations. The firms obtain access to knowledge residing within universities. In contrast to the monetary transfers of other instruments, applicants for Innosuisse funding are bound to R&D cooperation with universities. Despite this restriction, the funding of R&D cooperation seems to deliver similar or even higher performance effects for firms than more common policy instruments like R&D subsidies.

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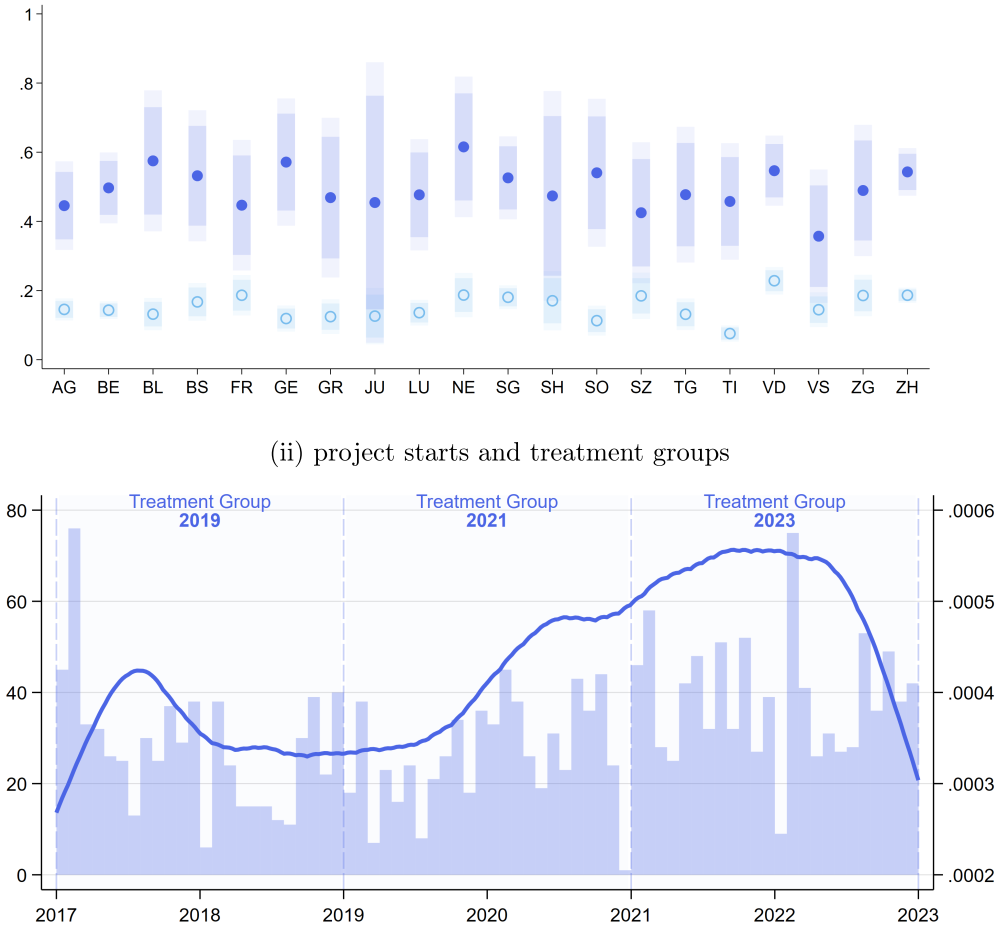
A Appendix

A.I Information on Innosuisse funding

Figure A.I: Application, funding, and project start

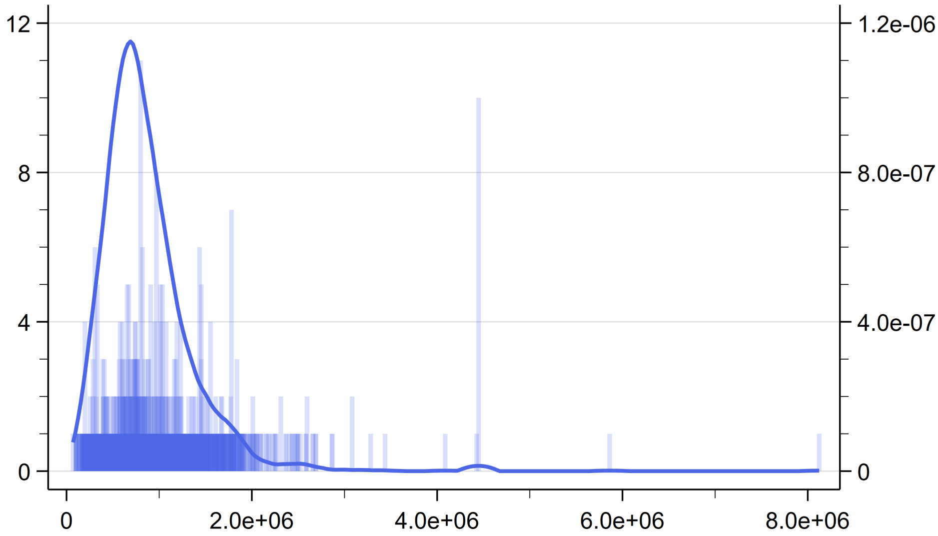
(i) application and funding, by canton



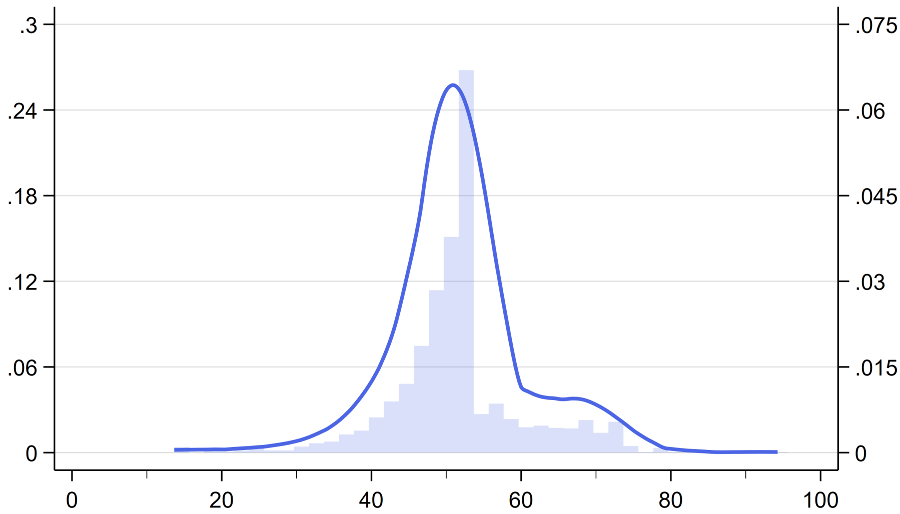
Note: This figure shows the application rates for and funding rates of Innosuisse innovation projects between 2017 and 2022. The upper panel (i) shows the application (∘) and funding rates (•) by canton (NUTS-3). Application rates rely on all firms while funding rates are conditional on application. The lighter shaded bands ( ) correspond to 99% CIs and the darker shaded ones ( ) to 95% CIs. We only report cantons where we observe more than 100 firms and label them with their official ISO 3166-2:CH code. The lower panel (ii) shows the project start dates over time and the corresponding treatment groups. The histogram shows the monthly frequency of start dates (left axis). The corresponding kernel density estimate uses a quarterly bandwidth and an Epanechnikov kernel (right axis). The light shaded areas in the background indicate the horizons we consider to create treatment groups assuming no anticipation lag ($\delta = 0$).

Figure A.II: Funds

(i) Total funds volume



(ii) Share of funds covered by Innosuisse



Note: This figure shows the volume of funds from Innosuisse's innovation projects between 2017 and 2022. The upper panel (i) shows the volume covered by Innosuisse and the firms (total funds). The lower panel (i) shows the share of the total volume covered by Innosuisse. The left axis corresponds to the histograms and shows the frequency for the upper panel and the fraction for the lower panel. The right axis corresponds to the kernel density plots using Epanechnikov kernels.

A.II Endogeneity

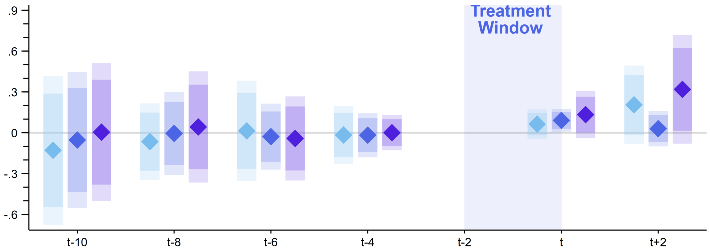
Table A.I: Pre-treatment balancing of the treatment and control groups

	N	Mean	Std. Dev.	Min	Q_1	Q_2	Q_3	Max
				$ln(Employees_{it})$				
Control: Unbalanced	18404	3.83	1.38	.588	2.71	3.69	4.75	11.1
Control: Balanced	2890	4.47	1.39	1.1	3.5	4.52	5.31	10.1
Treated	949	4.61	1.6	1.61	3.4	4.73	5.72	9.95
				$Academics_{it} \backslash Employees_{it}$				
Control: Unbalanced	12106	10.4	16.6	0	0	4	12	100
Control: Balanced	1947	15.3	18.5	0	4	10	20	100
Treated	472	16.6	21.2	0	4	9	22	100
				$ln(Sales_{it})$				
Control: Unbalanced	16959	16.3	1.68	11.3	15.1	16.2	17.4	25.9
Control: Balanced	2681	17.2	1.62	13.1	16.1	17.1	18.1	24.1
Treated	819	17.3	1.86	11.3	16.1	17.4	18.6	24.2
				$Exports_{it} \backslash Sales_{it}$				
Control: Unbalanced	11955	16	29.9	0	0	0	14	100
Control: Balanced	1913	43.2	38.1	0	3	35	82	100
Treated	469	46.2	39.4	0	3	45	90	100
				$Costs_{it}^{Intermediates} \backslash Sales_{it}$				
Control: Unbalanced	12102	32.2	24.5	0	10	31	50	100
Control: Balanced	1946	37	22.6	0	20	39	52	100
Treated	455	38.3	22.5	0	24	39	53	98
				$Costs_{it}^{Labor} \backslash Sales_{it}$				
Control: Unbalanced	12103	32.3	21.7	0	15	32.4	46	100
Control: Balanced	1946	31.4	17.8	0	20	31	41.3	95
Treated	464	32.7	18.8	.0402	20	31.5	44.5	90
				$ln(Investments_{it} + 1)$				
Control: Unbalanced	12100	12	3.7	0	10.9	12.5	14	21.1
Control: Balanced	1945	13.1	3.22	0	12.1	13.6	14.8	20.9
Treated	459	14.2	2.65	0	12.9	14.3	15.6	20.2
				$ln(Expenses_{it}^{R\&D} + 1)$				
Control: Unbalanced	13930	3.05	5.47	0	0	0	0	22.7
Control: Balanced	2292	9.15	6.3	0	0	11.9	13.8	22.7
Treated	581	9.22	6.6	0	0	12.2	14.4	21.5
				$Competition_{it}^{Price}$				
Control: Unbalanced	12106	3.87	1.04	1	3	4	5	5
Control: Balanced	1947	3.92	.963	1	3	4	5	5
Treated	474	3.94	1.01	1	3	4	5	5
				$Competition_{it}^{Non\text{-}price}$				
Control: Unbalanced	12106	3.07	.961	1	3	3	4	5
Control: Balanced	1947	3.18	.925	1	3	3	4	5
Treated	473	3.2	.934	1	3	3	4	5
				$Competitors_{it}$				
Control: Unbalanced	11637	2.65	1.46	1	1	2	4	5
Control: Balanced	1900	2.07	1.14	1	1	2	3	5
Treated	465	2.12	1.24	1	1	2	3	5

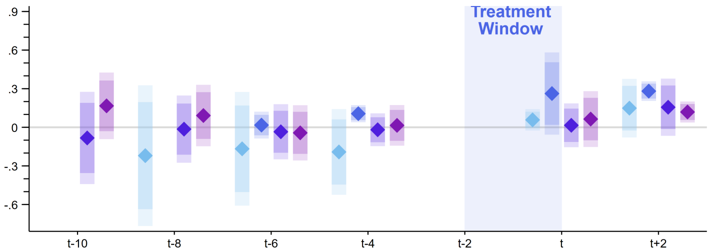
Note: This table shows distributional statistics of key variables between control and treatment groups in pre-treatment periods. Specifically, we use all periods $t < g$ for each treatment group g and all available periods for the control groups as they are not treated (yet) in the given horizon. The unbalanced control group includes all firms that never applied for Innosuisse funding, while the balanced control group randomly excludes firms that do not resemble the treatment group according to the procedure of section 5.2.3

A.III Heterogeneity

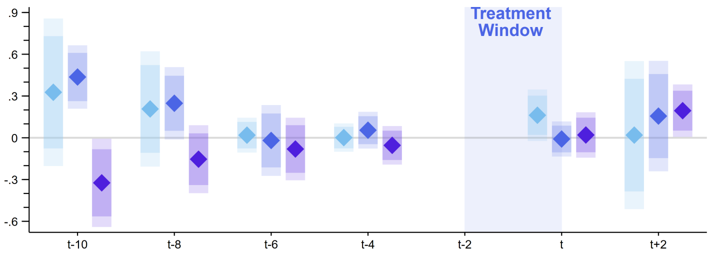
Figure A.III: Heterogeneous treatment effect dynamics of Innosuisse funding on sales



(i) across firm sizes



(ii) across higher education institutions



(iii) across scientific fields

Note: The figure shows event study estimates of Innosuisse funding on sales across different subsamples using the baseline specification from figure 6-2. Due to low precision in the estimates from lower observational counts in the subsamples, we do not report the edge periods $g - 12$ and $g + 4$. The lighter shaded bands correspond to 99% CIs and the darker shaded ones to 95% CIs.
In subfigure a, ◆ marks small firms with FTEs between 5 and 50, ◆ medium-sized firms with FTEs between 50 and 250, and ◆ large firms with more than 250 FTEs. To prevent the funding from affecting the partition, we exclusively group firms based on pre-treatment periods $t < g$.
In subfigure b, ◆ marks the ETH domain, ◆ universities, ◆ universities of applied sciences, and ◆ research institutions of national importance and government agencies. This partition only affects firms that receive funding and refers to the type of higher education institution with whom they collaborate in R&D.
In subfigure c, ◆ marks social and health sciences, ◆ information and communication technologies, and ◆ engineering sciences. This partition only affects firms that receive funding and refers to the scientific field of their innovation project. All splits use the same control group **A** without any balancing to the respective treatment partition.