Innovation support and its effect on employment: the case of Brazil

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Abstract

Purpose – Theoretically, the effect of innovation on employment is ambiguous. There is a negative impact due to labor-saving efficiency and a positive impact through increasing competitiveness, leading to business expansion and new hiring. In this regard, we investigated the effect of innovation support on employment for Brazilian firms that received public support for innovation from Finep.

Design/methodology/approach – We use a novel database by merging Finep and RAIS microdata, which allows for a better identification of the causal effect. Moreover, we use a staggered difference-in-differences (DiD) method recently developed by Callaway and Sant'Anna (2021), specifically designed for situations where treatments occur in different years.

Findings – We found a statistically significant effect of innovation support on formal employment, especially on firms seeking financing for process innovations (but not product innovations).

Originality/value – Unlike previous studies assessing the causal effect of Brazilian government support on innovation, we used as a control group companies that applied for support but were not selected. This novel strategy provides a better identification of the causal effect.

Keywords Innovation support, Finep, Causal effect

Paper type Research paper

1. Introduction

Many scholars consider innovation a driving force for economic development and growth. Technical progress has accelerated over the past decades, with numerous innovations being introduced in organizations, requiring them to constantly review and reformulate their management practices and models.

Indeed, differentiation through innovation has been the path several companies take to compete and survive in highly competitive environments. As these activities involve higher risks due to the development of new products, processes, or services, and consequently, greater

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difficulty in accessing credit with an appropriate profile, public support through proper financial instruments becomes essential to encourage companies to innovate.

The *Financiadora de Estudos e Projetos* (Studies and Projects Financing Office) – *Finep* was established in 1967 as a state-owned enterprise (currently under the Ministry of Science, Technology, and Innovation - MCTI), with the initial mission of fostering scientific and technological development in Brazil, primarily by supporting academic and structural research and development projects. Over time, as innovation gained prominence as a driver of economic growth, Finep expanded its scope beyond financing scientific infrastructure to actively promoting innovation within companies. This shift allowed the institution to establish itself as a key enabler of technological risk, encouraging private investment in innovation and fostering collaboration between research institutions and the productive sector with financial support for innovation activities.

One of the key questions arising from the State's participation in supporting corporate innovation is its impact on firm growth, particularly regarding employment levels. Several studies have explored whether public financial support for innovation effectively translates into job creation (Maffioli, De Negri, Rodríguez, & Vázquez-Bare, 2017; Ferraro, Männasoo, & Tasane, 2023). However, the effects can vary depending on the funding mechanism and evaluation period, as highlighted by Santana (2016). Given these findings, we explore the case of Finep's reimbursable funding for corporate innovation activities, estimating its causal effect on employment at supported firms, compared to companies that did not receive funding.

Impact evaluations are part of a broader agenda for evidence-based policymaking (Gertler, 2018). This methodology shifts the focus from input evaluations to outcome evaluations, facilitating the verification of the effectiveness of policies and programs at various stages of implementation. It becomes possible to understand which policies, instruments, and programs are most effective by analyzing whether goals were met. Impact evaluation is a guiding tool to determine which initiatives and products should be maintained, modified, or discontinued, clarifying what works and what does not. The central challenge is identifying the causal relationship between the program or policy on outcomes of interest.

The goal of this paper is to gather compelling and comprehensive evidence that can be used to support decision-making regarding Finep's support for innovative companies through reimbursable funding by investigating its effects on employment levels in the supported firms. This will be achieved by integrating data from Finep and RAIS.

While studies have examined the relationship between public financial support for innovation and employment in Brazil (Maffioli *et al.*, 2017; Rauen, Saavedra, & Hamatsu, 2019), evidence on the specific impact of Finep's reimbursable funding remains limited. Rauen *et al.* (2019) indirectly assessed the impact of Finep's credit program on overall employment, highlighting its effects on firms' technological efforts and high-skilled job creation. However, a direct evaluation of how Finep's reimbursable funding influences employment levels across supported firms is still lacking, which this study aims to address.

We use a novel identification strategy to identify causal effects by including in the treated group companies that received support from Finep and in the control group companies that applied for support but were not selected during Finep's funding selection process. This approach ensures greater similarity in unobserved characteristics between treated and control firms, allowing for the identification of causal effects with greater credibility. Unlike studies that rely on innovation surveys, such as PINTEC in Brazil (Santana, 2016) and CIS in Europe (Peters, 2005), which provide extensive cross-sectional insights but face limitations in identifying direct causal relationships, our methodology leverages administrative data directly linked to the funding process. This enables a more precise impact assessment of Finep's reimbursable funding on firm-level employment, reducing biases associated with self-selection and omitted variables.

We use all reimbursable fund requests from 2011 to 2017. As the treatment (receiving funds for an innovation project from Finep) is observed in all periods, we use recently developed methods for staggered Difference-in-Differences (DiD), namely, Callaway and Sant'Anna (2021).

This document is structured as follows. Section 2 addresses the theoretical framework and related literature. Section 3 discusses empirical methods and the data used. Section 4 presents empirical results. Finally, Section 5 collects concluding remarks.

2. Theoretical framework and relevant literature

Faced with the continuous challenges posed by globalization and financial interconnection, companies and economies must maintain a competitive position and keep pace with the leading global players. This necessitates a structured national effort toward technological advancement that can make the Brazilian economy competitive. It is widely acknowledged that one of the paths to increasing national industry competitiveness is the support, promotion, and investment in research, development, and innovation (R&D&I) activities.

According to the Oslo Manual (OECD, 2018), innovation is "the implementation of a new or significantly improved product (good or service), process, marketing method, or organizational method in business practices, workplace organization, or external relations."

Given the economic changes driven by innovation and technological progress, how do innovative activities impact firm employment? These activities may have different effects, creating new job opportunities or changing the workforce profile. Technological progress generates job losses and gains, rendering specific skills, goods, markets, and manufacturing processes obsolete while driving economic growth (Schumpeter, 1982; Solow, 1957; Romer, 1990; Lucas, 1988).

Economic theory has reached a consensus on the impacts of innovation on employment. In a synthesis, Van Reenen (1997) argues that the effect of innovation on employment is ambiguous. While innovation may reduce the need for labor per unit of product, it also lowers production costs, potentially increasing output and competitiveness, leading to business expansion and new hiring. Using the case of the UK in the 1980s and 1990s, the study found a strong positive correlation between innovation and employment.

The result may depend on the type of innovation. Product innovation is more likely to increase labor demand than process innovation (Duhautois, Erhel, Guergoat-Lariviere, & Mofakhami, 2022; Calvino & Virgillito, 2018; Fioravante, 2007). Process innovation tends to negatively impact employment levels since the productivity gains generated by new processes generally reduce the labor required for a given production level. Nevertheless, the total effect may be positive on employment as such cost savings are passed on to prices and boost product demand. Vivarelli (2014) emphasizes that product innovation is generally associated with positive employment effects, particularly when it leads to the development of entirely new markets and is supported by patenting activity, which enhances the appropriability of returns. Other studies that found positive effects of innovation on employment, primarily from product innovation, are Peters (2005) for Germany, Jaumandreu (2003) for Italy, Pereira, Martínez Correa, and Scattolo (2018) for Argentina, and Zuniga and Crespi (2013) for a sample of Latin American Countries.

Recent studies on the effects of public subsidies for innovation in employment provide additional insights. Maffioli *et al.* (2017) analyzed the impact of public credit programs in Brazil and found that access to public funding significantly increased firms' employment growth. Similarly, Ferraro *et al.* (2023) and Banai, Lang, Nagy, and Stancsics (2020) assessed the effects of EU subsidies on SMEs in Estonia and Hungary, respectively, and concluded that such support positively impacted employment.

Innovation subsidies can alleviate financial constraints for SMEs, indirectly supporting employment by facilitating access to bank financing and reducing investment barriers, as Chiappini, Montmartin, Pommet, and Demaria (2022) found in their study.

As noted by Mérida, Hasenclever, and Carvalho (2022), the employment impacts of innovation may differ depending on a country's level of development. Developed countries generally feature companies engaging in highly original innovations, supported by strong R&D structures and highly skilled labor. In contrast, developing countries like Brazil focus more on acquiring machinery and equipment to improve processes and increase production efficiency rather than creating radical innovations. Empirical evidence from Brazil suggests that process innovations are more prevalent than product innovations, reflecting the country's industrial structure, which is concentrated in low and medium-low-technological-intensity sectors. This may lead to weak employment effects of innovation in the country.

Finep, the primary governmental agency responsible for supporting and fostering innovation in Brazil, operates three main instruments to support corporate innovation projects: reimbursable funding, economic subsidies, and direct investment. This study focuses on the reimbursable funding line, which aims to support the innovation of Brazilian companies, and outlines specific goals and objectives during the financing period. This financial product is like credit provided by a commercial bank, but with subsidized rates and generally more attractive terms for the firm. Finep supports all stages and dimensions of the scientific and technological development cycle: basic research, applied research, innovation, and the development of products, services, and processes. It also supports incubating technology-based companies, the establishment of technology parks, structuring and consolidating research and innovation processes in established companies, and market development.

The existing literature on Finep's activities focuses primarily on evaluating its programs and the impact on innovation financing. Still, it does not directly investigate the relationship between the public funding it provides and the employment levels of beneficiary companies. Studies such as Zackiewicz (2005) analyze impact assessment methodologies in S&T&I programs, while Furtado *et al.* (2008) examine the effects of the Basic Sanitation Research Program (PROSAB) from the perspective of knowledge generation and transfer. Gallon, Reina, and Ensslin (2010) study the impact of the Juro Zero Program (PJZ) on the economic and financial performance of companies in Santa Catarina, and Rauen *et al.* (2019) assess the influence of Finep's reimbursable funding on the technological efforts of beneficiary firms. Only the latter refers to employment effects on a broad sample of firms. They found that companies benefiting from Finep's reimbursable funding increased their performance by hiring qualified personnel associated with R&D expenditures. The study employed a quasi-experimental approach with Propensity Score Matching (PSM) to compare firms that received Finep's credit with similar firms that did not. The findings indicate that public financial support was complementary in fostering technological efforts. Additionally, although the study primarily assessed technological effort and high-skilled employment, its results indirectly positively impact overall employment levels.

Despite the importance of these studies, they do not comprehensively address the relationship between Finep's reimbursable funding and employment generation. The available evidence suggests that, while public financing for innovation effectively supports technological advancements and R&D expansion, its short-term effects on job creation remain uncertain. While Rauen *et al.* (2019) indicate a positive impact on high-skilled relative employment, Santana (2016) suggests that these effects may require a longer evaluation horizon to be fully captured. However, the methodology used by Rauen *et al.* (2019) and Santana (2016) presents limitations in addressing selection bias, as their control group included all firms that did not use the funds. The PSM and DiD methods only partly control for confounding characteristics and likely fail to account for unobserved factors influencing the likelihood of receiving Finep support. Moreover, firms that obtained Finep funding may have already had stronger growth expectations, introducing an upward bias in the estimated employment effects if common trends before the treatment are observed.

Recent advances in impact evaluation highlight the importance of considering the timing of treatment across different cohorts, a factor not fully accounted for in previous studies. To address these limitations, our study employs a more refined identification strategy, using a control group composed of firms that applied but were not selected, ensuring greater comparability in both observable and unobservable characteristics. Furthermore, we adopt advanced econometric techniques designed for staggered treatment effects, following Callaway and Sant'Anna (2021), which explicitly account for treatment timing across different cohorts. By refining the identification of causal effects, this study provides a more reliable estimate of the impact of Finep's reimbursable funding on employment generation.

Thus, this study seeks to bridge this gap in the literature by analyzing whether Finep's reimbursable funding influences employment generation in beneficiary firms through innovation support. Focusing on firm-level employment outcomes, this research contributes to a deeper understanding of the role of public innovation policies on labor market dynamics in Brazil.

3. Methodological framework

3.1 The dataset and descriptive statistics

We used data from Finep and the RAIS (Annual Social Information Report), provided by the Ministry of Labor. RAIS, also used in Rauen *et al.* (2019), is an administrative record compiled annually by the Federal Government. It serves as an instrument for legal compliance, monitoring, and characterizing the formal labor market. The number of employment contracts (number of employees) yearly, from 2011 to 2017, is used.

Finep's database, on the other hand, contains information on companies' funding requests, including both approved and rejected applications. Examples include the amount of funds requested, funding approval status, the company's location, industry classification (CNAE), and the year the support was granted. As of July 2024, the database comprises 3,478 funding requests submitted between November 2003 and July 2024, made by 2,232 distinct companies. Among these, 1,376 proposals were approved and 2,102 were rejected. This dataset refers exclusively to reimbursable funding applications and does not include other instruments such as non-reimbursable grants or economic subsidies. The sample effectively used is smaller, as cases with missing information were excluded, rules to organize the panel were imposed, and firms had to be matched with RAIS data.

The dataset was structured such that each firm appears only once in the panel, with one observation per period. Specifically, each firm is associated with a single treatment status and timing. If a firm submitted multiple funding requests and was only approved in a later round, we retained the year of first approval as the treatment period (first treatment), and the firm was considered treated in all subsequent years. If a firm submitted multiple proposals but was never approved, we used the year of its first request as the baseline year and considered it untreated throughout the analysis period. This approach guarantees that all observations are comparable across units and periods, avoids duplication, and aligns with the assumptions of the staggered Difference-in-Differences framework.

As this study focuses on Finep's reimbursable funding, it is appropriate to present its main characteristics briefly. Data from 2011 to 2017 will be used, as these years provide complete information for the Finep and RAIS databases. Tables A1 and A2 in the Supplementary material present sample statistics for this data.

From the data presented, 898 reimbursable funding applications were submitted to Finep, with a 42.5% approval rate. This suggests a relative balance between the treated (supported) and control (not supported) units. Additionally, during Dilma Rousseff's presidency, a significant increase in applications was observed in 2013 and 2014, likely due to public policies such as the Investment Support Program (PSI). In the latter years, the number of applicants and the total supported amount decreased, but with a slower decrease in the conversion rate. This suggests that a larger share of firms that did not apply after 2014 were likely not to have been approved.

Based on the information on average project size, one sees that supported firms tend to present/have approved larger projects. As for the number of firms, support amounts were concentrated in 2013, where additional funding and lower rates were used (PSI program). Throughout the entire period, approximately R$ 23 billion in reimbursable funding applications for innovation projects were analyzed, of which around R$ 16 billion were approved, representing a conversion rate (support approval) close to 70%.

3.2 Impact assessment on employment in supported firms

Since innovative companies have characteristics that differentiate them from non-innovative ones, a simple analysis of employment differences across Finep-supported (innovative) and non-supported firms would not adequately capture the effect of innovation, as other factors influence these variables, some observable and others not. De Blasio, Fantino, and Pellegrini (2015) noted that it is challenging to isolate the impact of innovation subsidies from other induced effects, making it necessary to use evaluation methods that can infer causal relationships.

Hussinger (2008) states that the most common solutions for estimating the behavior of subsidized companies in the absence of a subsidy (the treatment) are the DiD method, propensity score matching, instrumental variables (IV), and selection models. A more recent approach is the Regression Discontinuity Design (RDD).

However, as Gertler *et al.* (2018) highlight, RDD should only be used in programs based on a continuous index that ranks potential participants and includes a cutoff point determining program eligibility. Since Finep's approval process lacks a transparent and objective eligibility index, RDD is not viable.

Therefore, given the impossibility of defining a cutoff point, a combination of conspicuous control group selection and estimation techniques will be used to mitigate selection bias. Specifically, we only employ as control group firms that applied for support and a DiD method, recognizing staggered treatment timing (Callaway & Sant'Anna, 2021).

3.3 Empirical strategy

The panel data structure, with innovative companies receiving Finep support in different years, required using a staggered DiD model. In fact, in the database composed of Finep and RAIS data, companies receive support (treatment) at different points in time, and the evaluation focuses on differences in employment level trajectories between similar firms, some treated and others not.

Since the allocation of Finep's reimbursable funding to innovative firms is not randomized, the methodology takes into account key characteristics of credit allocation, namely: (1) the different moments of reimbursable funding approval (treatment) for distinct groups of treated units; (2) the availability of data for various pre-treatment periods for different treated and untreated groups; and (3) the assumption of parallel trends after conditioning on observable variables.

The method used is Callaway and Sant'Anna (2021), who developed a flexible estimator, expanding the application of the Difference-in-Differences method. This approach differs from the conventional Difference-in-Differences model, which estimates the Average Treatment Effect on the Treated (ATT) by comparing the average variation in outcomes between treated and control groups, focusing on a single-period treatment start for all treated units, as discussed by Angrist and Pischke (2010), among others.

In contrast, the estimator proposed by Callaway and Sant'Anna (2021) [1] allows for the identification of heterogeneous effects of interventions, as treatment may occur on different dates for different treated units. The key idea is to group companies based on when they first participated in the treatment. These groups, referred to as entry cohorts, are denoted by g. For this study, $g = 2013, \ldots, 2017$.

The central objective is to identify the effect of Finep support on the employment level of the supported companies for each combination (g, t), where g represents the entry cohorts and t denotes calendar time.

In its broad application, the estimator uses observable variables from the treated and control units, defined as companies that applied for Finep support but were rejected, to ensure that control units are as similar as possible to treated firms regarding participation probability and observable characteristics. The propensity score is estimated using matching models with observables such as internal ratings for the firm on credit, project characteristics, firm innovation ability, project, and firm size. The rating data is available from 2013, so the matched estimates start with the 2014 cohort (compared with the unmatched data that begins with the 2013 cohort) [2].

The core assumption of Callaway and Sant'Anna's (2021) estimator is that the control group provides an appropriate counterfactual for the average trajectory of the outcome variable of interest after the financing approval for treated groups, had they not received support, like other DiD estimators of causal effects. Since this assumption is not directly testable, its validity is assessed by testing the similarity of pre-treatment mean trajectories of the outcome variable between treated and control groups.

Given the availability of data for the periods $t = 1, 2, \ldots, \tau$, the ATT estimator for group g in period $t \geq g$ proposed by Callaway and Sant'Anna (2021) is given by:

$$ATT(g,t) = E\left[w_g^G(Y_t - Y_{t-1})\middle|G_g = 1\right] - E\left[w_g^C(Y_t - Y_{t-1})|C = 1\right], \tag{1}$$

where G_g is a binary variable assuming value 1 if the firm receives support from Finep in period g (and zero otherwise), C is a binary variable whose value is 1 if the firm belongs to the control group (and zero otherwise) and Y_t and Y_{t-1} represent the outcome variable in period t and in the period immediately preceding Finep support for group g. The difference $(Y_t - Y_{t-1})$ is calculated separately for each group g and its corresponding control group, weighted by w_g^G and w_g^C.

The $ATT(g,t)$ estimator enables the evaluation of a broad range of heterogeneous effects by identifying treatment effects for each group g in periods $t \geq g$. An interesting issue with the Callaway and Sant'Anna (2021) and other staggered treatment models is that no cohort control groups exist. All controls are potentially used in contrast to each treated cohort.

Our data set includes firms that applied for funding and were not supported. This allows us to consider a traditional DiD model for each cohort. The conditional C in equation (1) has no cohort indicator. Our data allows us, using panel data fixed effects methods, to calculate:

$$ATT(g,t) = E\left[(Y_t - Y_{t-1})\middle|G_g = 1\right] - E\left[(Y_t - Y_{t-1})\middle|C_g = 1\right]. \tag{2}$$

To include the common trend hypothesis, we specify an event-study specification (Angrist & Pischke, 2010) that simultaneously estimates treatment effects and tests common trends:

$$Y_{it} = \sum_{t=1}^{g-2} \delta_t D_{it} + \sum_{t=g}^{T} \beta_t D_{it} + \theta X_{it} + \varepsilon_{it}, \tag{3}$$

where $t = 1, \ldots, (g\text{-}1)$ indicates pre-treatment periods, with the period.

g-1 being omitted, and $t = g, g+1, \ldots, T$ are post-treatment periods. The treatment periods are indexed by the cohort identifier g. The coefficients δ_t provide a common trend hypothesis, and the β_t captures causal effects. The controls X_{it} include time and firm fixed effects. The regression is estimated for each cohort g. Given the dataset's specificities, we examined Finep support's effect on the number of employees over four consecutive years (2014–2017), using data from 2011–2017.

As mentioned, the estimates have been carried out using propensity score matching sample weight adjustment, for common support guarantee. The propensity score was calculated using explanatory variables such as the project, firm, and credit ratings, as well as the size of the support requested and whether the firm has an R&D unit. The ratings are calculated internally by FINEP as additional evidence for the request evaluation process. The ratings are calculated from 2013, so the first cohort to be evaluated is the one with credit granted in 2014. First-stage results are available upon request. Non-matched data is also used for robustness analysis, and results are relegated to section B of the Supplemental material, available on the journal website. Table A2 of the Supplemental material shows that the propensity score can be considered successful in balancing the sample compared to the unmatched data, where supported firms are larger and better rated by FINEP.

Once the contract is signed and financial guarantees are presented, the first installment of resources is transferred, marking the formal start of treatment. This study defines the treatment year as the year the contract with Finep was signed. Accordingly, each firm's entry cohort corresponds to the year of contract signature, which reflects the actual timing of support implementation and anchors the subsequent impact evaluation.

4. Empirical results

Table 1 depicts results from two models. Model 1 considered the logarithm of the number of employees as the dependent variable, aiming to capture the treatment effect over time, adjusting for panel data structure and treatment timing with no covariates but time and firm fixed effects. Model 2 does the same, but also controls for observed differences between the treatment and control groups by incorporating the scores from a Propensity Score Matching (PSM) as sample weights.

Table 1. Treatment effects estimates and pre-treatment trends (all, product and process innovation), with matching

	All innovation	Product innovation	Process innovation
Pre-treatment average	−0.070	−0.049	−0.019
	(0.084)	(0.084)	(0.109)
Post-treatment average	0.310***	0.291	0.319
	(0.105)	(0.190)	(0.225)
Pre-treatment (t−5)	−0.244	−0.316**	−0.048
	(0.158)	(0.142)	(0.360)
Pre-treatment (t−4)	−0.169**	−0.189	−0.063
	(0.082)	(0.125)	(0.123)
Pre-treatment (t−3)	0.036	0.175	−0.012
	(0.094)	(0.166)	(0.108)
Pre-treatment (t−2)	−0.130	−0.078	−0.218
	(0.090)	(0.109)	(0.303)
Pre-treatment (t−1)	0.155**	0.162	0.248
	(0.071)	(0.104)	(0.227)
Treatment	0.117***	0.127	0.161*
	(0.045)	(0.079)	(0.091)
Post-treatment (t+1)	0.274***	0.183	0.394**
	(0.105)	(0.201)	(0.155)
Post-treatment (t+2)	0.378***	0.332	0.419
	(0.136)	(0.244)	(0.303)
Post-treatment (t+3)	0.469***	0.520*	0.302
	(0.168)	(0.301)	(0.410)
Post-treatment (t+4)	−0.070	−0.049	−0.019
	(0.067)	(0.086)	(0.109)
Post-treatment (t+5)	0.310***	0.291	0.319
	(0.105)	(0.190)	(0.225)
Time fixed effects	Yes	Yes	Yes
Firm fixed effects	Yes	Yes	Yes
PSM	Yes	Yes	Yes
Observations	2,471	1,071	574

Note(s): The table reports treatment effects estimated using the approach developed by Callaway and Sant'Anna (2021) – see equation (1). All regressions include time and firm fixed effects (omitted for convenience). In parentheses are the standard deviations of the estimated parameters. The symbols *, **, and *** indicate parameters statistically different from zero at 10%, 5%, and 1% significance level, respectively
Source(s): Authors' calculations based on Finep and RAIS data

Based on average before and after treatment estimates, in the first two lines of the table, the pre-treatment average effect is statistically non-significant in the first line, suggesting that the common trends hypothesis is not rejected overall. The post-treatment average effect is statistically significant regardless of innovation type. Being supported by Finep increases employment by 36.3% on average (compared with the first pre-treatment year) [3]. In its peak (third year after treatment), the impact on employment reaches 59.8%. Unfortunately, the pre-trend hypothesis was violated in two years for the pooled innovation type sample. The post-treatment coefficients are much larger than the pre-trend differences, and persistent, but the common trend violation recommends caution in extrapolating the results as causal.

The remaining columns in Table 1 show treatment effects for supported firms by innovation type, as classified by Finep staff in the application processing process. We target projects that will generate product or process innovations. Other categories are not included due to smaller sample sizes and a general categorization. Unlike the previous literature, Finep's support seems to increase employment for process innovators (but not product innovators). Interestingly, here, the common trend hypothesis does not seem to be violated, but for a very early difference that disappears in later periods, closer to the treatment year, giving more credence to causal effect interpretations of the positive employment mean differences estimated in the model. The unweighted results – see Tables B1 and B2 in the Supplementary material – show that the yearly employment effects are significant only post-treatment, and more strongly for process innovation projects. The presence of positive employment effects is robust to matching or not.

As a rule, project classifications at Finep were categorized as either product or process innovation. Within this framework, budget allocation was one of the key parameters used for classification. Due to system limitations in place during part of the analyzed period, it was not possible to formally classify a project under more than one type of innovation, even if multiple innovation components were present. This limitation refers to the project classification process and not the analytical framework Finep adopted, which fully adheres to the Oslo Manual. This may partially explain why process innovation projects exhibit clearer impacts on employment levels, as the classification reflects financial predominance rather than a specific type of innovation, not necessarily contradicting existing literature.

The result that innovation projects classified as process innovation generate employment growth can be better understood by considering the specific characteristics of developing countries, such as Brazil. These countries are marked by industrial sectors with low and medium-low technological intensity, emphasizing adapting production processes rather than creating new products (Mérida *et al.*, 2022). In such contexts, process innovations are more frequent. Still, they can also generate positive effects on employment levels, particularly by enabling the expansion of productive capacity and the diffusion of new technologies adapted to local conditions. Given the system-driven constraint that allowed only a single classification per project during the period analyzed, even applications involving more than one type of innovation were formally recorded under a single category, typically the one associated with the largest budget allocation. This may have led to an overrepresentation of process innovation. Therefore, the results observed in this study do not necessarily contradict the international literature but rather reflect the national production structure and the specific criteria used in the technical evaluation of proposals by Finep.

Firms can be heterogeneous in the unobservable shock that gives them confidence to apply for a grant in a given year (e.g. by anticipating an industry-specific positive shock). Such an unobserved shock may bias the estimates as the control group includes untreated firms that applied in different periods. As mentioned above, a solution would be to use the cohort of untreated firms as a control: firms that applied for support in a given year but were not granted funds. This requires estimating a Two-Way Fixed Effects (TWFE) model for each cohort. In this case, treatment is uniquely timed on the treatment group, so the staggered DiD concerns do not apply.

Table 3 presents the results of these cohort DiD models with matching, for all applicants in the sample and by innovation type, respectively. Unmatched sample results may be seen in the Supplementary material. Unlike Callaway and Sant'Anna (2021), each cohort is evaluated in a separate regression, as described by equations (2) and (3). The regressions consider different treatment years, with the treatment year indicated in the corresponding column (2013 to 2017). For example, the first column represents firms that were treated in 2013 and applied for funding in 2012. Thus, 2012 serves as the pre-treatment year, and it is omitted from the event study analysis. The control group for this column consists of firms that applied for funding in 2012 but were not selected. The same logic applies to the other columns.

In turn, each line indicated the treatment effect in a specific year. Therefore, coefficients before the blank space indicate tests for common trends. If the coefficient is not statistically significant, we conclude that employment growth before treatment was similar between treated and untreated firms, as required for causal identification using DiD. The causal effect coefficients for each cohort are in the cells after the blank space.

The total number of observations and the number of observations per year vary as indicated in the tables, reflecting the sample available for each year. The assumption of common trends is not violated except for firms treated in 2016 (Table 2). The coefficients are all positive, and most are statistically significant, confirming the results from Table 2.

In turn, Table 3 presents the results from the event studies for product and process innovators, and their interpretation is analogous to Table 2. Confirming Table 2 results, employment impacts are more relevant for process innovators, particularly for the firms treated in 2014.

Table 2. Event study – regressions by treatment year (with matching)

Dependent variable: ln (employment)		Treatment year 2014	2015	2016	2017
Treatment effect	2011	0.435	−0.018	−0.711	0.039
		(0.402)	(0.234)	(0.483)	(0.921)
	2012	0.181	−0.230	−0.761*	−0.498
		(0.302)	(0.171)	(0.386)	(0.645)
	2013		−0.113	−0.651	−0.471
			(0.077)	(0.418)	(0.636)
	2014	0.413		−0.346	−0.533
		(0.290)		(0.311)	(0.629)
	2015	0.763**	0.157*		−0.039
		(0.315)	(0.085)		(0.085)
	2016	0.870**	0.211	0.106	
		(0.348)	(0.141)	(0.102)	
	2017	0.969	0.151	0.312	0.066*
		(0.250)	(0.185)	(0.150)	(0.115)
R^2		0.09	0.04	0.13	0.06
Observations		469	1,197	385	245

Note(s): The table reports results from separate regressions for each cohort, as described by equations (2) and (3). All regressions include time and firm fixed effects (omitted for convenience) and are weighted by scores from a PSM. The coefficients above the blank space are pre-treatment cells. A significant coefficient may be interpreted as a violation of the common trend hypothesis. The coefficients below the blank cells are post-treatment cells. A significant coefficient may be interpreted as a causal effect, under the usual DiD assumptions. In parentheses are the standard deviations of the estimated parameters. The symbols *, **, and *** indicate parameters statistically different from zero at 10%, 5%, and 1% significance level, respectively
Source(s): Authors' calculations based on Finep and RAIS data

Table 3. Event study – regressions by treatment year (with matching)

Product innovators

Dependent variable: ln (employment)		Treatment year 2014	2015	2016	2017
Treatment effect	2011	0.539	−0.031	−1.039	−0.541
		(0.735)	(0.328)	(1.109)	(1.126)
	2012	0.717	−0.269	−1.117*	−0.566
		(0.717)	(0.242)	(0.559)	(1.054)
	2013		−0.058	−0.601	−0.535
			(0.100)	(0.699)	(1.049)
	2014	0.800		−0.268	−0.555
		(0.719)		(0.778)	(1.038)
	2015	0.820	0.049		0.095
		(0.718)	(0.098)		(0.180)
	2016	0.785	0.151	0.309	
		(0.732)	(0.182)	(0.303)	
	2017	1.145	0.023	0.914	−0.006
		(0.728)	(0.276)	(0.682)	(0.557)
R^2	0.10	0.05	0.21	0.11	
Observations	196	553	133	119	

Process innovators

Dependent variable: ln (employment)		Treatment year 2014	2015	2016	2017
Treatment Effect	2011	0.392	0.336	−0.349	−0.493
		(0.380)	(0.397)	(0.917)	(0.539)
	2012	−0.760	0.213	−0.857	−0.287
		(0.836)	(0.369)	(0.967)	(0.220)
	2013		−0.048	−1.057	−0.341**
			(0.213)	(1.048)	(0.117)
	2014	0.162		−0.285	−0.393***
		(0.264)		(0.243)	(0.104)
	2015	1.197*	0.134		−0.291***
		(0.612)	(0.111)		(0.046)
	2016	1.357*	−0.045	0.146	
		(0.724)	(0.465)	(0.128)	
	2017	1.068	0.019	0.289	0.061
		(1.149)	(0.473)	(0.223)	(0.055)
R^2		0.11	0.06	0.12	0.12
Observations		315	315	161	91

Note(s): The table reports results from separate regressions for each cohort, as described by equations (2) and (3). All regressions include time and firm fixed effects (omitted for convenience) and are weighted by scores from a PSM. The coefficients above the blank space are pre-treatment cells. A significant coefficient may be interpreted as a violation of the common trend hypothesis. The coefficients below the blank cells are post-treatment cells. A significant coefficient may be interpreted as a causal effect, under the usual DiD assumptions. In parentheses are the standard deviations of the estimated parameters. The symbols *, **, and *** indicate parameters statistically different from zero at 10%, 5%, and 1% significance level, respectively
Source(s): Authors' calculations based on Finep and RAIS data

5. Concluding comments

We investigate the effectiveness of Finep's reimbursable funding on employment in supported companies. We use a unique dataset, including firms that received Finep support and those that applied for support but were not selected. This approach ensures a more closely matched sample between treated firms and a control group, at the very least through their willingness to innovate or perceived similarity in innovation-related characteristics, thus minimizing selection bias in treatment and control groups. Nevertheless, we also control for observable characteristics using propensity score weighted Difference-in-Difference (DiD) methods.

Treatment occurs in multiple years within our database, requiring recent methods for estimating causal effects via DiD, as Callaway and Sant'Anna (2021). We complemented this estimation by using cohort-specific models, restricting the control group to untreated companies within each cohort. The Callaway and Sant'Anna (2021) method pools these untreated companies across cohorts.

The literature points out that the effect of innovation financing cannot be identified as positive or negative a priori due to two opposing effects: innovation may reduce labor requirements in the production process, decreasing employment to increase productivity and efficiency. However, innovation can also make products more competitive and attractive to consumers, leading to demand and production expansion, positively affecting company employment.

We find a positive impact of innovation support from Finep on employment, particularly for process innovators, but not for product innovators, and it is concentrated in some cohorts. The previous literature indicates that product innovation is usually more likely to increase employment than process innovation (Duhautois *et al.*, 2022; Calvino & Virgillito, 2018; Fioravante, 2007). However, the classification approach Finep uses is distinct from the ones adopted in most innovation surveys worldwide. For this reason, our results do not necessarily contradict existing literature and may result from a different classification approach for innovation.

The sample used is relatively small, given Finep's limited budget, even though it is Brazil's largest innovation funding agency. Future studies with a broader time frame of Finep's activities should revisit these estimates to verify long-term effects and address other firm outcomes (particularly innovative activities and innovation outcomes).

Notes

1. We use the csdid script for Stata.
2. Unmatched data results are available in section B of the Supplementary material.
3. Since the treatment is a discrete variable and the dependent variable is expressed in logarithms, the percentage impact on employment of being supported by Finep is $100(e^{\beta} - 1)$, where $\beta = 0.310$ is the coefficient associated with the treatment.

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Supplementary material

The supplementary material for this article can be found online.

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