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EVALUATING A PROGRAM OF PUBLIC FUNDING OF PRIVATE INNOVATION ACTIVITIES. AN ECONOMETRIC STUDY OF FONTAR IN ARGENTINA

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Evaluating a Program of Public Funding of Private Innovation Activities. An Econometric Study of FONTAR in Argentina

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This working paper is one of several background reports to OVE's Science and Technology Thematic Evaluation carried out during the 2005-2006 Ex-Post Evaluation Cycle.

The team of this Thematic Study was comprised of Alessandro Maffioli, Bertha Briceño, and coordinated by Inder J. Ruprah. The findings and interpretations of the authors do not necessarily represent the views of the Inter-American Development Bank. The usual disclaimer applies. Correspondence to: Alessandro Maffioli, e-mail: alessandrom@iadb.org, Office of Evaluation and Oversight, Inter-American Development Bank, Stop B-719, 1300 New York Avenue, NW, Washington, D.C. 20577.

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INTRODUCTION

This work contains an evaluation of the *Non-Reimbursable Funds* (ANR) program of the *Argentinean Technological Fund* (FONTAR), which is managed by the *National Agency of Scientific and Technological Promotion* (ANPCYT), an organization that is part of the Argentine federal government. FONTAR's objective is to fund projects presented by private firms that aim at improving their competitive performance through technological innovation activities.

The main goal of this evaluation is to analyze the impact of the ANR program on the innovation activities of granted firms. We also try to ascertain whether ANR contributed to improve supported firms' innovative outcomes (i.e. launching of new products and/or process technologies) and productivity performance.

A counterfactual notion of causality would imply comparing the outcomes of funded firms with those that the same firms would have obtained in case they had not been funded. In order to estimate this unobserved potential outcome, we count with data from a group of firms which did not receive the grants, either because they never applied for them or because they applied but did not accomplish the basic requirements for being granted[1].

In order to carry out the evaluation we have taken advantage of a series of econometric techniques that allow us to identify the impact of having received a subsidy from the FONTAR on relevant outcomes, distinguishing this impact from other factors that could also be affecting firms' performance. In other words, we aim at establishing a causal relationship between firm's reception of the subsidy and its innovation activities, not a simple correlation between both phenomena.

Section 1 reviews the debates on the arguments that, according to the literature, could justify the existence of a policy of public funding of private innovation activities. Furthermore, the discussion on the ways that such policy should assume is also examined, as well as that on the appropriate mechanisms to evaluate its results. In section 2 the evolution of FONTAR and the main funding mechanisms that it offers are briefly outlined, with particular emphasis on the ANR. Section 3 includes a description of the methodology and data sources used to make the evaluation, as well as the main results. Finally, section 4 concludes.

[1] Only in the first call of the program there were firms with their projects approved but not financed due to budget restrictions, these firms are also part of our control group.

I PUBLIC PROMOTION OF INNOVATION ACTIVITIES: JUSTIFICATION, INSTRUMENTS AND EVALUATION OF ITS RESULTS

This section presents a discussion on the rationale for public promotion of private R&D[2], analyzes the main instruments that are employed to that end and introduces the main elements to be taken into account for the evaluation of those instruments.

Our focus lies on measuring the effect of public subsidies for private firms. With that aim, we survey the literature that evaluates, based on micro econometric evidence, the impact of public funding. In particular, we carefully analyze one of its branches, the studies that deal with the issue of additionality vs. substitutability (crowding out).

A. Justifications of Public Support to Commercial R&D

The rationale for governmental support to commercial R&D rests upon the traditional theory of "market failures". Two of those failures are the most mentioned by the literature: imperfect appropriability conditions and financial constraints.

The "incomplete appropriability problems" that arise in the production of scientific and technological knowledge were highlighted in the seminal works by Nelson (1959) and Arrow (1962). They are mainly related to the qualities of knowledge as a public good.

Perfect exclusion is not feasible in the case of knowledge. It is impossible to appropriate all the benefits that arise in the form of a multiplicity of applications and combinations from the same knowledge –i.e. you cannot prevent externalities arising out of the generation of new knowledge-. This is the source of the difference between the private and the social marginal return of new knowledge, which could lead to an underinvestment in innovation activities in the frame of competitive markets.

Furthermore, the non-rival and cumulative character of knowledge intensifies the difficulty to compensate for the non-appropriable profits, causing a greater

[2] Throughout this report the terms "private innovation activities" and "private" or "commercial R&D" are used interchangeably in general. Nevertheless, it is important to notice that the second one is subsumed in the first one, and that for developing countries commercial R&D expenditures could represent a small fraction of total private innovation expenditures. We come back to this distinction when we describe our outcome measures in section 3.

suboptimality in the allocation of resources, which is increased as well due to the high component of uncertainty and to the indivisibilities that entail knowledge investments.

On the basis of this argument, it is expected that, even considering the existence of intellectual property rights legislation, in the absence of policy intervention the social rate of return on R&D expenditure would exceed the private one, thus the level of investment in R&D would be too low from a social point of view. Griliches (1992) and Hall (1996) present evidence in this direction, highlighting the presence of important spillovers in knowledge generation activities[3].

Some qualifications have been made in relation to the underinvestment argument. On the one hand, Martin and Scott (2000) emphasize that the forces leading to underinvestment differ from sector to sector; depending on the particular sources of innovation failures. Mani (2004), in turn, posits that their relevance also differs from developed to developing countries.

On the other hand, as the works by Mansfield *et al*. (1981) and Levin *et al*. (1987) document, the high cost of imitating some innovations could mitigate the appropriability problem in some cases, especially when patent laws fulfill their theoretical role[4]. Finally, the rent seeking literature has also contemplated the possibility of overinvestment. As the games developed by Fundenberg and Tirole (1987) or Anderson *et al*. (1997) illustrate, the competition between firms seeking for a competitive advantage could lead to that phenomenon.

Hall (2002) is the appropriate reference for studying the financial market failures that may lead to underinvestment in innovation activities. This work draws upon Arrow (1962) that claims for the existence of an additional gap between the private rate of return and the cost of capital when the innovator and the financer of that innovation are two different entities. The main argument lies in the fact that "some innovations will fail to be provided purely because the cost of external capital is too high, even when they would pass the private returns hurdle if funds were available at a 'normal' interest rate" (Hall, p. 3).

[3] Jaffe (1998) distinguishes between three types of spillovers generated by R&D activities: i) the knowledge generated by one agent could be used by another agent without any compensation (knowledge spillovers); ii) the availability of a new product or process in the market with prices that do not fully reflect its improved properties could benefit the clients of the firm (market spillovers) and iii) the commercial or economic value of a new technology may strongly depend on a related group of technologies (network spillovers). Klette *et al*. (2000) develop a framework to test econometrically the presence of these different forms of spillovers.

[4] Patents would be efficient when knowledge can be easily codified and innovations cannot be circumvented (Mansfield *et al*., 1981).

Hall (2002) gives four possible explanations for the gap between external and internal costs of capital. The first one is the presence of asymmetric information between the inventor and the investor that would cause a higher cost of the external capital to generate an Akerlof's type of lemon's premium. The second one relates to moral hazard (a classical principal-agent problem between inventor and investor may appear). Finally, capital structure and different taxes for different sources of funds are mentioned as the third and fourth explanations of the financial cost gap.

Financial market failures have been a key rationale for R&D public funding. As we will see next, the econometric evidence shows that small and new R&D intensive firms often receive the most significant impacts of funding, and they are precisely the most affected by financial constraints.[5] However, the argument may not be so relevant for large and established firms, which are less likely to be financially constrained.

B. Incentive Mechanisms

If we accept that the consensus in the literature on the subject is that, by one or both reasons mentioned in the previous section, it is not possible to trust only in the market to guarantee a socially efficient volume of commercial R&D, the issue is how public support must be carried out. In this section we review the theoretical arguments for and against the main instruments that have been proposed in this regard.

As the survey by David *et al*. (2000) highlights, economists have suggested two main policy responses to the imperfect appropriability problem of commercial R&D (beyond the adoption of intellectual property rights regimes): direct procurement and incentives for private investment. While the first one relates to public research institutes and national laboratories, we will concentrate on the studies focused on the incentives for private investment.

David *et al*. (2000) contrasts the properties of the two main incentives for R&D: tax incentives and direct subsidies. In theory, tax incentives[6], whose effectiveness is surveyed in OECD (1998) and Hall and Van Reenen (1999), act by reducing the marginal cost of R&D, whereas direct subsidies also impact by raising the marginal rate of return on investment.

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[5] See for example Klette and Moen (1999), Hall (2002) and Duguet (2003).

[6] OECD (1998, chapter 7) distinguishes and analyzes several tax measures, such as a more rapid depreciation of investment in machinery, deductibility of current R&D expenditure from taxable income and tax credits.

Because of the way they act, some forms of tax incentives (in particular incremental schemes that reward marginal R&D) are not expected to generate "crowding out" effects (see below), but they mainly tend to affect the composition of R&D in favor of projects with short-run benefits, which frequently are the ones that generate fewer spillover effects. Private firms retain autonomy in deciding how to react to the reduction in their after-tax costs of R&D, and they use the credits to first fund those projects with the highest private rate of return and not necessarily the highest social one. Furthermore, tax incentives have low response elasticity in the short run, thus a modest decrease in the cost of R&D could stimulate little or no additional research in the first years (Klette *et al*., 2000; Hall and Van Reenen, 1999; Warda, 2002)[7].

On the other hand, subsidies sometimes are characterized by a governmental selection of projects or at least research areas; therefore, it is more probable that they may be targeted to those projects with larger expected marginal social rates of return. Provided that they are aimed at this goal, they should not be expected to displace private R&D investment either.[8] Nevertheless, subsidies are more prone to pressures to fund projects with high private marginal rates of return, both because of lobbying or to guarantee the "success" of public aid. In this case, the possibility of a crowding out effect increases.

Besides, an additional complication arises when agencies have to decide what kind of projects to target. They must search for those with large expected social benefits, but with inadequate private expected returns because in that case firms could carry out the projects by themselves. As Cohen and Noll (1991) and Yager and Schmidt (1997) show, the government is often unable to choose that kind of projects, either because of informational limitations or lobbying.

There are other kinds of public intervention. David *et al* (2000) discuss government R&D contracts, which mainly apply at aerospace and defense expenditure and often carry a future public commitment to purchase. Credit lines are another variant, which in contrast with subsidies, help firms create a credit reputation and reduce asymmetric information. This attribute could be significantly relevant to deal with the financial constraints problem[9].

Martin and Scott (2000) claim that the effective kind of intervention depends on the nature of innovation, which differs from sector to sector, therefore employing

[7] Hall and Van Reenen (1999) find that the response to an R&D tax credit is small at first, but increases over time.
[8] Both tax credits and direct subsidies could generate crowding out effects through an increase in inelastic R&D inputs prices. This argument is developed in David and Hall (2000).
[9] For more details on this instrument and its effectiveness, see Mansfield (1996).

a general analytic framework to find the right instrument would be incorrect. They develop a typology of innovation modes and sectoral innovation failures that distinguishes firms by their main mode of innovation and the corresponding sources of innovation failure.

According to their classification, the firms that develop inputs (software, instrument and equipment sectors) suffer especially from financial market transaction costs, so they recommend as policy instruments the support of venture capital markets.[10] On the other hand, those firms that develop complex systems or apply high-science content technology suffer mainly from limited appropriability of technologies; therefore, in that case they prescribe subsidies as one of the remedial solutions, but also R&D cooperation and high-tech bridging institutions to facilitate diffusion.

It is also very important to acknowledge the fact that both the rationale for public support to private innovation activities as well as the most appropriate instruments for granting that support could differ between developing and developed countries. One of the reasons for this difference is the fact that firms in developing countries are seldom engaged in formal R&D activities aimed at producing radically new products or processes.

Instead, they are more oriented to imitate, adapt and/or improve technologies that are already available in the market and they do that often through more informal innovation activities, since they rarely have a formal R&D department. Furthermore, technology imports usually play a much larger role than domestic innovation activities. Large firms are often more prone to undertake formal R&D and at least in the more industrialized countries of Latin America and Asia there are firms that are engaged in advanced innovation projects. However, these cases are more the exception than the rule[11].

While some authors have stated that, in this situation, non-fiscal instruments could be more important than fiscal ones (see Mani, 2004), the fact is that market failures are more frequently present in developing countries than in developed ones, specially regarding financial constraints. Moreover, neither imitation nor adaptive innovations are exempt from uncertainty, since they are far from being trivial activities.

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[10] Hall (2002) sustains that venture capital is not a solution for financial market failures, especially in countries where public equity markets are not well developed. However, Martin and Scott (2000) suggest that it could work for this type of firms if its support is implemented as part of a wider design aimed at dealing with financial market failures.

[11] This is not the case only in developing countries, since even in developed countries small firms seldom do have an R&D department and they rarely declare explicit R&D expenditures (Hujer and Radic, 2005).

Hence, there are arguments in favor of fiscal public support to innovation activities in developing countries. Given the limited role of R&D in those countries, this support could well be more general, and involve also informal innovation activities as well as some types of technology acquisition activities. As seen below, this is the case of the ANR that are under analysis in this report.

C. Relevant Outcomes

Beyond the discussion about the modality that must assume the public support to commercial R&D, if we accept that such support is desirable, the question on how to evaluate its results naturally arises. In this sense, it is possible to distinguish between three kind of relevant outcomes that one would like to measure in order to ascertain the effects of public funding.

First of all, funding should have an effect on the amount the benefited firm spends in R&D (or other kind of innovation activities which could be promoted by the public policy). This leads to the analysis of the relationship between public and privately financed R&D investments and to determine whether that relationship is one of complementarity (often called additionality) or substitution (crowding out).

The literature that deals with this problem is vast and has been continuously growing in the recent years. Annual expenditure in R&D is the most often-used dependant variable. Sometimes it is measured in logs in order to compensate for skewness in the distribution of R&D[12]. Several studies acknowledge the fact that it is important to avoid measuring size effects in the outcome variable. Since that effect may no be eliminated by the use of size proxies as explanatory variables, then expenditure in R&D per employee or divided by total sales (which measures innovation intensity) are used[13]. Other studies also consider R&D personnel as a proxy for R&D effort.

Nevertheless, this is only the first step, since R&D expenditures do not necessarily reflect the success of R&D efforts (Hujer and Radic, 2005; Patel and Pavitt, 1995). Therefore, it is necessary to consider variables that represent innovative outcomes.

[12] See for instance Czarnitzki and Licht (2006).
[13] See Görg and Strobl (2005). Other studies that utilize R&D intensity are Czarnitzki and Licht (2006), Kaiser (2004), Czarnitzki and Fier (2002) and Busom (2000).

The innovative outcomes variables employed in the literature are patents, sales of new products and the introduction of a new or improved product or process[14]. The first ones might be appropriate for developed countries[15], but patents are not always relevant for measuring innovative outcomes in developing countries where the percentage of firms that patent is low. Moreover, many public programs directly aim at the introduction of new products or processes, which is why a measure of new or improved products could be a better outcome variable.

Lastly, since innovation is not an end by itself, it is relevant to ascertain whether innovative outcomes impacts on firm's performance. Sales, employment, productivity and profitability are usually considered with this purpose. The work by Klette *et al*. (2000) extensively surveys the studies on the productivity impacts of public and private R&D and we will draw upon it in the following sections.

It is important to take into account that data is not usually available to estimate long term impacts of the programs under analysis. This is important since one should notice that even if short-run performance is not improved by the availability of public funds, funded projects can develop capabilities that in the long run may lead to an increase in R&D investment and to an improvement in innovative outcomes and/or in productivity/profitability performance. Therefore, a complete analysis of an instrument may need to be developed several years after it has been put into action[16].

D. Outcome Determinants and their Channels of Impact

David *et al*. (2000) develop a theoretical framework in order to understand how public intervention (through direct innovation activities or incentives) affects private R&D investment decisions. It consists basically of a downward-sloping

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[14] For a survey of patents as a measure of innovation outcome see Griliches (1992). In the strand of the evaluation literature we are analyzing, Czarnitzky and Licht (2006) and Czarnitzki and Hussinger (2004) among others use patents as an outcome, whereas Hujer and Radic (2005) take into account new or improved products. Other works have also considered management, marketing or commercialization innovations.

[15] Nevertheless, as it is well known, not all innovations are patented and not all patents protect marketable innovations. Furthermore, patents effectiveness levels vary considerably among sectors and firms often employ patents in "strategic" forms that have not necessarily to do with the intention of protecting innovations that are going to be launched to the market (Levin *et al*, 1987; Cohen *et al*, 2000).

[16] Although it is not a paper aimed at evaluating the impact of public funding programs, it is useful to take into account the results of Benavente *et al* (2005), who show that, for a panel of innovative Chilean firms, the rate of return for R&D expenditure was close to 54% during the nineties –a value more than three times larger the return obtained for physical capital - 17%. However, R&D expenditure caused contemporaneous negative impacts over firms' profits suggesting that a learning process was in place.

marginal rate of return on investment curve (MRR curve that represents derived demand for R&D) and an upward-sloping marginal cost of capital curve (MCC curve that accounts for the opportunity cost of R&D). The positive slope of the later reflects the fact that the firm will pass from financing projects with its own funds to demand external funds when it increases its R&D investment volume (up to the point that the firm needs external funding, the curve would be actually flat).

Each curve is represented by one equation that has the level of R&D expenditure as an argument. The MRR equation is also affected by variables that reflect technological opportunities, the state of demand, the institutional framework and others factors that affect appropriability conditions. On the other hand, the MCC equation has as additional arguments variables that account for technology policy measures that affect costs, macroeconomic conditions, expectations and capital market conditions. The intersection of both curves would give the optimal level of R&D investment and its corresponding equilibrium rate of return.

In this framework, R&D subsidies would shift the position of the MCC schedule to the right increasing the firm's optimal level of total R&D investment. Furthermore, as it was originally suggested by Blank and Stigler (1957), the grant-funded innovation activity could give place to learning and training spillovers and cause a shift in the MRR schedule since the efficiency of the whole R&D activity of the firm could be improved. This last effect may also be generated by the use of the new available equipment or specialized team formation, thanks to the subsidy, in the other innovative projects of the firm.

The literature generally ignores the effects of subsidies on the MRR curve, and focus on the MCC shifts they generate. David *et al*. (2000) distinguish three analytical cases. The first one involves a credit-constrained firm, which cannot finance its optimal level of R&D neither with internal nor with external funds. In this case, the subsidy in a form of a public grant would increase the firm R&D expenditure by its full amount (there would be a rightwards shift of the vertical MCC curve), making possible for the firm to approach its unconstrained optimum level.

A second possibility arises when the grant shifts an upwards-sloping MCC curve (that is to say, when a firm cannot finance its optimal level of R&D with its own funds, but it is able to get external financing). In this case there would be an increase in the private R&D investment, but by less than the amount of the subsidy.

Finally, when the subsidy is granted to a firm facing a horizontal MCC curve (when the firm is at its unconstrained optimum level of R&D expenditure) a grant would not generate any additional private investment[17].

In any of these three cases, the granting of publics funds may send a signal to investors and equity holders that cause a reduction in the firm's internal cost of funds. It is also possible that the public review process leads non-public sources to fund other projects of the firm, hence allowing an increase in its total research expenditure[18].

The first two cases and the signaling effect give theoretical fundament for a complementary (additionality) relationship between private and public funded R&D. On the other hand, the third case introduces theoretical reasons for the possibility of the existence of a crowding out effect.

A displacement of funds (crowding out effect) would be observed when those projects that would have been carried out anyway by firms are granted[19]. Moreover, as Lach (2002) posits, even if firms needed the funds to develop the projects, the accomplishment of the subsidized ones could lead to dismiss some of the non-funded projects when firms lack skilled R&D personnel or are financially constrained. What is worse, public subsidies could generate an upward pressure on R&D input's prices and therefore, they may increase the costs of the non-subsidized projects and reduce the privately-funded R&D investment[20,21].

On the other hand, even if we determine the existence of an additionality effect of the subsidy, this does not mean that the funding would be socially beneficial. We would still have to show that it generates a positive effect on the innovative

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[17] As Wallsten (2000) states, if R&D investment has short-run diminishing returns and the firm has an equilibrium level of R&D investment funded with its own funds, the public funding will cause the firm to reduce its own expenditure in the whole amount of the subsidy, thus the total R&D investment would remain unchanged.

[18] This is known in the literature as the "halo" effect.

[19] As we have previously noted, this is likely to be the case when bureaucrats fund the projects with greater evidence of future success. However, as Lach (2002) suggests, the funds released by the funded projects could fund other projects of the firm and the complete crowding out effect could be avoided.

[20] An analysis of the forces that would end up prevailing can be found in David and Hall (2000).

[21] In turn, David and Hall (2000) suggest that R&D crowding out may not necessarily be bad. This could be the case, for instance, when racing behaviors and business stealing strategies exist, leading to R&D overinvestment. However, they admit that these cases are rarely found in practice and that the instruments to correct its potential damaging effect are not those related to public funding of private research.

outcomes variables[22]. This is important because one cannot presume that more R&D leads automatically to new products or processes, since innovation is an activity with a high degree of uncertainty. In turn, innovation, as said before, is not an end by itself, so it would be necessary to show that public subsidies had a positive net private and/or social impact.

Thus, we would have a three-step model in which first the public subsidy should have an impact on R&D expenditures, next increased R&D should foster more innovative outcomes and finally those outcomes should generate private and/or social benefits in terms of productivity, spillovers, etc. This is the same kind of logic that is behind the so-called CDM model (Crepon, Duguet and Mairesse, 1998).

In practice, variables affecting innovation inputs and outcomes are mostly the same. Hujer and Radic (2005) divide the determinants of innovation in those that are related to market factors and those that correspond to firm factors. Among market factors are the ones related to the supply side such as competition intensity, market concentration, exposure to international trade; and those of the demand side: profitability, expected success, etc. On the other hand, firm factors are divided between those that affect firm's internal capabilities like technical expertise, technology, existence of R&D department, share of high qualified employees or employees devoted to R&D; and the ones related to external capabilities such as R&D cooperation with other entities.

Since Schumpeter's work, firm's size and industry concentration have been the two most discussed determinants of innovation[23,24]. The evaluation literature, which will be reviewed in the next section, often uses the number of employees and the amount of sales, both in levels and squares, as proxies of firm's size. Due to lack of data availability, in few cases a concentration measure (such as the

[22] One could draw upon the spillovers argument and assume that the additional R&D expenditure is beneficial *per se* since it would approach the private R&D expenditure level to the social one.

[23] The most common assumption in the case of size is that it has a positive effect both on innovation inputs and outcomes. Cohen (1995) claims that size is not a determining factor *per se*, but it reflects appropriability conditions. See also Acs and Audretsch (1990) and Rothwell and Dogson (1994) for arguments that justify that the sign of the size effect depends on other particular characteristics of the firms.

[24] Two arguments have been explored in the received literature in this regard: that there is a positive impact of ex-ante market power on innovation and that successful innovation leads to higher concentration (in the latter case, market structure is endogenous) –see Scherer (1992)-. On the other hand, the model developed by Dasgupta and Stiglitz (1980) implies that both market concentration and innovations depend on demand elasticity and their positive association is not more than a correlation driven by that common factor.

Herfindhal-Hirschman index) is introduced or, when available, a market share variable.

Given the importance of appropriability conditions and its different nature across sectors[25], the literature tries to control for them by including industry and regional dummies. Industry dummies could also control for different technological opportunities when there is no proxy variable available for the technology state of the firm. There is also some support for the idea that linkages with users or other institutions could impinge positively on innovation success, thus a dummy for the presence of cooperation agreements is frequently included.

Other relevant factors are firm's internationalization (often measured by the amount of exports), trade regime, the availability of complementary inputs (proxied by the percentage of qualified workers in the total labor force), the age of the firm and the internal funding capacity. This last factor is particularly relevant when financial market failures are present; and it is often measured by debt to sales ratio. Foreign capital participation can also be a relevant indicator in this sense.

E. Measuring R&D Subsidies Impacts

In this section we review the econometric evidence presented in the literature on the effects of R&D subsidies. Two studies, David *et al*. (2000) and Klette *et al*. (2000), do an excellent work summarizing the voluminous literature that analyzes the impact of public funding on R&D investment, innovative outcomes and firms' performance.

However, we could say that their most relevant role was to emphasize the importance of counting with rigorous econometric techniques that help distinguish the causal effects of public funding, and to suggest the keys for their application. Up to the year 2000, descriptive case studies and other non-econometric quantitative assessments had characterized R&D programs evaluations, together with some econometric works that did not attempt to estimate causal effects. But since then there was a burst of new studies that introduced the evaluation literature based on treatment effects, which has been widely used in labor economics, to the evaluation of R&D programs.

Perhaps fostered by the above-mentioned works, the search for crowding out or additionality effects with these new techniques spread along developed countries,

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[25] The number of previous patents could measure the expected appropriable benefits of the firm up to some extent, but patents are not relevant incentives for some sectors.

and is becoming to be common practice in developing countries introduced by the demands of international evaluation agencies.

This new literature is committed to confront the problems of endogeneity generated by selection bias, simultaneity or omitted variables. In the particular case of R&D programs evaluations this problems are relevant, as David *et al*. (2000) and Klette *et al*. (2000) state. The main issue is that the funded firms are not selected randomly, but on the basis of some not always clear criteria that could be related to their past performance or R&D expenditure. This generates that the group of funded firms differ in average from the group of the non-funded firms, thus it is not possibly to simply use the outcome of the last one to estimate what the supported firms would have experienced in the absence of the funding (the counterfactual outcome)[26], at least without further adjustments.

Selection bias arises when differences between funded and non-funded groups are reflected in variables that affect both treatment (the granting of the subsidy) and the relevant outcome. Among these variables, there might be proxies for demand, appropriability conditions, and technical opportunities. Not taking them into consideration could generate an upward bias when the funded firms are the ones that would have the largest expected outcome in the absence of funding (Wallsten, 2000, Jaffe, 2002), but it could also cause a negative bias if firms with more profitable inventions are less likely to apply due to higher opportunity costs (Takalo *et al*. 2005).

The simplest method[27]to estimate treatment effects would be to include available relevant variables as controls in a regression of the relevant outcome on a dummy variable for the treatment, and to suppose that only this observable variables influence both outcome and treatment. An alternative that rests upon the same assumption is to match funded and non-funded firms on the basis of those variables or on the probability of receiving the subsidy (propensity score)[28].

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[26] Brown *et al*. (1995) suggest using only the firms with rejected applications for the program, and not all non-supported firms, to estimate the counterfactual outcome. They argue that that group is more likely to be similar to the treated group.

[27] For a formal description of these methods applied to R&D programs evaluation, see the methodological section of this work and Jaffe (2002), who exposes them in a clear way.

[28] Several works use this procedure. For details see Lerner (1999) for the SBIR in the United States, Lach (2002) for Israel, Almus and Czarnitzky (2003), Czarnitzky and Fier (2002), Aerts and Czarnitzky (2004), Czarnitzky and Hussinger (2004), Hujer and Radic (2005) for Germany, Duguet (2003) for France, Kaiser (2004) for Denmark, González and Pazó (2005) for Spain, Lööf and Hesmati (2005) for Sweden, and Görg and Strobl (2005) for Ireland. For developing countries we can mention the paper of Benavente and Crespi (2003) that analyses the impact of an enterprises R&D consortiums in Chile, in the spirit of Branstetter and Sakakibara (1998) paper for Japan.

The main problem of methods that rest upon the "selection on observables" assumption is that selection bias could also be originated by unobservable variables such as management ability or technical opportunities. The difference in differences (DID) and fixed effects approaches remove the possible bias generated by time-invariant unobservable variables[29]. These methods can be carried out in the matched sample, as Görg and Strobl (2005) do, to improve their performance.

Nevertheless, they could also fail, for example, when the reception of a subsidy is associated with a particular good project that would lead to a higher outcome (in this case the DID estimates would be overestimated) or when firms are selected into the program when they are performing exceptionally badly[30]. For example, Klette and Moen (1999) find that restructuring large IT firms were more likely to receive subsidies, and therefore the DID estimates would be underestimated. Furthermore, as Klette *et al*. (2000) notice, the "innovational opportunity sets" may undergo differential alterations among distinct sectors and region areas, representing unobservable time-varying variables that invalidate the assumptions of DID and fixed effects estimators.

Another possibility is to construct a simultaneous equation model with at least one equation that models the selection process and another one to model the effect of the subsidy on the relevant outcome. The identification strategy would depend on the existence of an exclusion restriction, which is provided either by a variable that affects the probability of being selected but does not affect the outcome, or by an assumption about the functional form of the relationship between the unobservable variables and the outcome. In the case that it is possible to find such a restriction, the presence of time-varying unobservable variables would not generate an estimation bias.

Besides being utilized as an identification strategy, the structural modeling approach is recommended by David *et al*. (2000) and Klette *et al*. (2000) as it could be a contribution to a proper interpretation of the reduced-form results. By modeling the government agency selection process and firm R&D responses, it is possible to achieve a more precise knowledge of what is actually happening than the mere treatment effect would indicate. In spite of the difficulty for identifying

[29] In the R&D program evaluation literature, they are used by Branstetter and Sakakibara (1998), Lach (2002), Benavente and Crespi (2003), Hujer and Radic (2005) and Görg and Strobl (2005) among others.
[30] In the evaluation literature this situation is known as the Ashenfelter-dip.

the equations of this type of models and the arbitrary assumptions needed to employ this method, several works in the received literature have used it[31].

As we have previously noted, identification could be granted by a variable that affects selection into the program but not the outcome (once selection process is accounted for), that is to say, an instrumental variable. Unfortunately, this kind of variable is not easy to find. Wallsten (2000), inspired on Lichtenberg (1988), uses the variations in the available budget of the funding agency. In particular, the available budget for the section of the program in which the firm applied is used as an instrument for the reception of the funding. Ali-Yrkkö (2005) also utilizes the amount that was requested (and not necessary granted) by firms, finding results different from those of Wallsten –as seen below, this is a frequent case in the literature on this subject-.

Jaffe (2002) also discusses the advantages of regression-discontinuity designs and randomized studies over all previous techniques. The first ones need an indicator variable determining exclusion or selection into the program, which requires that the selection process be made on the basis of at least one exclusive characteristic of a group of firms. On the other hand, the second ones need random selection of the funded firms (at least after eliminating some non-qualifying firms). These complicated requirements have prevented these methods from being applied yet in the program R&D evaluation literature.

When it comes to surveying the conclusions of this literature, one finds, as David *et al*. (2000) recognize, that to formally aggregate and synthesize the results of available works is not a simple task. The employed strategies are different across studies and often not comparable among them. Furthermore, studies analyze data from a different range of years, technological fields and countries; and they also contemplate distinct R&D funding mechanisms (even if we restrict to R&D subsidies, the form they take vary from country to country).

Furthermore, it could be the case that there is no such thing as one universal relationship between R&D funding and the outcomes we want to analyze. As David *et al*. put it when they describe the relationship between public R&D spending and private R&D investment: “Some considerable doubt must surround the very idea that there is a universal relationship of that kind, and so it would be better to avoid causal comparisons and juxtapositions of findings, striving to compare like to like where that is feasible” (David *et al*. 2000, pp. 510).

[31] See Toivanen and Niininen (2000), Busom (2000), Wallsten (2000), Czarnitzky and Hussinger (2004), Kaiser (2004), Hujer and Radic (2005) and Takalo *et al*. (2005).

Taking that claim into consideration, it is not surprising that concluding on the review of the literature that search for the presence of crowding out or additionality, from the last 30 years up to the year 2000, David *et al*. (2000) accept that the evidence remains inconclusive. They compare the results of studies that consider the same unit of analysis and distinguish four cases: line-of-business, firm, industry, and national economy and even inside each category they find contrasting results –the same conclusion applies even considering the studies that were published after David *et al.'*s survey-.

However, they explain that one-third of the studies (11 of 33) assert that public R&D funding is a substitute for private R&D investment. The frequency of the finding of a substitution effect is higher for works based on firm and lower levels of aggregation (9 of 19), and most of this is due to papers based on data from the United States (only 2 of 12 studies with data from other countries find a substituibility relationship). They claim that these differences could be attributed to distinct objectives of U.S. and European R&D programs, and to bias occasioned by inter-industry differences in the technological opportunity set that could artificially generate a positive correlation in the higher aggregation level studies.

David *et al*. (2000) suggested that more rigorous studies had to be carried out in order to validate the previous claims. However, six years later, and with plenty of new, at least more "rigorous" (but still probably suboptimal in relation to randomized studies) works, the controversy is still present.

García Quevedo (2004) develops a meta-regression of the econometric evidence, considering all the papers surveyed in David *et al.* (2000) and several more recent ones. The analysis concludes that there are no characteristics of applied studies that lead more frequently to a complementarity or substituibility result. However, weak some evidence is found in favor of the presence of a crowding out effect in firm level studies.

With regards to studies that evaluate the social returns of subsidies to commercial R&D activities, Klette *et al*. (2000) find that four out of five of the studies they analyze present positive effect of subsidies on the performance of funded firms. Nevertheless, they also highlight that more works introducing modern program evaluation techniques should be introduced to derive a robust conclusion.

Finally, it is important to signal some limitations of modern studies and highlight the real relevance of treatment effect estimates. As Jaffe (2002) notices, the R&D program evaluation literature is not directly looking at spillovers effects, which in some cases may be the principal rationale for these programs, and is not measuring the long term and general equilibrium effects. Available short run and partial equilibrium treatment effect estimates could guide decisions about program modification at the margin, but not those related to large changes or its elimination[32].

[32] The estimation of long term and general equilibrium treatment effects has already been carried out by program evaluation literature, thus it is likely that R&D program evaluation literature could follow that path when more compressive, long-term data be available (although it is not easy to evaluate the impacts of innovation in a general equilibrium framework due to some intrinsic features of knowledge as an economic good).

II FONTAR AND THE ANR PROGRAM

The Argentinean Technological Fund (FONTAR) is one of the two funds[33] of the National Agency of Scientific and Technological Promotion. The Agency is a federal organization created in 1996 that belongs to the Ministry of Education, Science and Technology. It was formed with the main objective of separating the promotion and funding functions from those directly related to the execution of scientific and technological activities and to policy setting.

The Agency is an organism that, in spite of depending administratively on the Secretariat of Science, Technology and Productive Innovation, is autonomously governed by a Directory integrated by nine members that have a four-year mandate and are removed by halves every two years. Although its headquarters are located in Buenos Aires, it counts with a web of institutions called Technology Linkage Units (UVTs), which are disseminated, all around the country. These UVTs accomplish an important role by promoting the program, assisting firms in the preparation of project proposals and helping the Agency with the distribution of funds.

Available funds come from the National Treasure, Loans of the Inter-American Development Bank (I.A.D.B.), the recovery of reimbursable funds and from cooperation agreements with national and international institutions. Public resources are partially granted to the Agency in the frame of the Law 23,877 aimed at Technological Innovation Promotion. Between 2001 and 2005 the Agency executed an annual average of 51.4 million dollars, which increased to US$89.7 million between 2004 and 2005.

While FONTAR has been functioning since 1995, initially it only worked on the basis of soft credit lines that aimed directly at the financial constraint problem. In 1998, funding in the form of fiscal credits applied to income taxes was introduced. But it was not until the year 2000 that the ANR program started to grant projects with non-reimbursable funds by means of a matching grant scheme.

Specifically, the ANR program finances up to 50% of the cost of technological innovative projects. The funds are only disbursed when FONTAR approve, technically and financially, the completion of the corresponding stage of the projects. That means it only gives funds as a repayment of firm effective innovation investments.

[33] The other is the Fund for the Scientific and Technological Research (FONCyT). For its description and evaluation see Chudnovsky *et al*. (2006a).

As a general, though flexible, rule, the maximum project execution horizon comprises a 2 years period. The maximum amount for the subsidy in charge of the Agency is either 100.000 or 300.000 Argentinean pesos depending on the nature of the project (these are the amounts established in the 2005 call).

The ANR program is mainly aimed at funding projects oriented to research and development, pilot scale technologies, applied knowledge generation, innovative products and process development, management improvements or human capital training (when these are related to product and/or process innovations) and to the creation of technology based start ups.

For these projects, an Ad-hoc Evaluation Committee analyzes: a) project's technical quality and feasibility (probability of technical success, rationality in projected stages, coherence of planned budget, etc.), b) firm's technical capacity (infrastructure and personnel related to the project, firm's antecedents, etc.), c) firm's economic and financial conditions (capacity to invest its corresponding 50% of the project) and d) project's economic viability (projected impact, coherence of expected results).

The calls contemplate the possibility of establishing a quality ranking (based on the abovementioned evaluation criteria) among the approved projects in order to set funding priorities in case of budget restrictions. However, with the exception of the first call for projects in 2000, up to the moment that potential ranking has not been utilized since budgetary resources have been enough to fund all admitted projects.

FONTAR is involved in all the stages of an innovative project, from its formulation to its execution and performance. That is, it provides assistance in the design of innovation projects, it evaluates applications in terms of their technical, economic and financial conditions, it helps in their execution and finally it supervises their performance.

Besides the ANR program and the fiscal credit for R&D, FONTAR currently counts with several lines of credits for firms and institutions with innovation projects. All of them are granted through public calls and on the basis of projects proposals.

In table 1a we can appreciate that almost 83 million pesos were compromised between 2001 and 2005 to fund ANR projects. That amount represents a 20% of the total budget devoted to FONTAR projects in that period. The average project signed a contract for a subsidy of $95.782. Due to the economic crisis, only 10% of the compromised budget was executed in 2001. In 2002 those projects approved in the previous year received the corresponding funds, and only 13 new projects were approved. Since 2003 the activity of FONTAR entered into a more "normal" regime[34].

Table 1a. Approved ANR projects by year (number and amounts)

Year	APPROVED FONTAR PROJECTS(*) (in Arg $)	TOTAL ANR WITH CONTRACT ($)	Number	$ per project	TOTAL DISBURSED ANR ($)
2001	19,127,999	15,662,006	151	103,722	1,675,773
2002	11,239,407	1,015,960	13	78,151	9,918,559
2003	47,457,592	8,804,562	109	80,776	3,092,187
2004	190,537,760	37,084,683	380	97,591	7,082,130
2005	126,044,577	20,380,224	213	95,682	16,271,700
TOTAL	394,407,335	82,947,435	866	95,782	38,040,349

** It includes all the projects approved by FONTAR under its different programs.*
Source: FONTAR.

Table 1b gives an idea of the role of FONTAR, and ANR in particular, *vis à vis* Argentinean total expenditures on R&D. The significance of FONTAR increased between 2001 and 2004 when its approved projects[35] represented almost a 10% of all Argentinean R&D investments. In particular, approved ANR projects also become more significant in the total from 2002 to 2004, although their participation in FONTAR sources of finance decreased. It is important to notice that FONTAR finances innovation activities and not only R&D investments; this would imply that the importance of ANR reflected in table 1b is overrated, since only a fraction of its budget is devoted to finance R&D.

Table 1b. FONTAR and Argentinean R&D investment

	2001		***2002***		***2003***		***2004***	
	millions of pesos	%	Millions of pesos	%	Millions of pesos	%	Millions of pesos	%
Total Investment in R&D	1140.9	100	1215.5	100	1541.7	100	1958.7	100
FONTAR (approved projects)	19.13	1.68	11.24	0.92	47.46	3.08	190.5	9.73
ANR (approved projects)	17.29	1.52	6.86	0.56	11.79	0.76	52.03	2.66

Source: Secretaría de Ciencia y Tecnología para la Innovación Productiva (SeCyT) and FONTAR.

[34] Note must be taken that disbursed funds are systematically lower than approved projects amounts because the ANR reimburse expenditures effectively made by benefited firms.
[35] See footnote 34.

III IMPACT EVALUATION OF THE ANR PROGRAM

A. Econometric Methodology

Evaluating subsidy programs is an exercise in counterfactual analysis, i.e., the evaluator faces the question of what would have taken place without the subsidies. In addition, neither the firms receiving support, nor those not applying, can be considered random draws. Constructing a valid control group in this setting is quite challenging and we relate the discussion to the recent advances in econometric methods for evaluation studies based on non-experimental data.

Much, if not all, of the empirical work evaluating the effectiveness of sponsored projects has been what is usually called 'after-the-fact' evaluation—an evaluation in which a researcher comes along sometime after a set of grants has been made and attempts to infer the effect of those grants using observational data collected at that time.

Recently, some researchers have proposed that grant agencies, anticipating the need for such evaluation, may build certain features into the grant process to facilitate later evaluation. The proposed procedure consists in using 'randomization' in order to award grants. Under this procedure the control group is constructed as a randomized subset of the eligible population. That is, the agency would identify a group of potential grantees and randomly award grants within this group, meaning that the probability of receiving the grant would be the same for all members of the group (Jaffe, 2002).

A necessary pre-condition to implement randomization is that the subsidy has to be designed taking the future evaluation into account. In the case of the ANR of FONTAR these necessary previous steps were not taken. Thus, in this paper we will evaluate the impact of FONTAR's ANR program by using after-the-fact evaluation.

When performing after-the-fact evaluation to evaluate the impact of a subsidy program the outcome of the non-supported firms is used to estimate the counterfactual scenario, i.e., what the supported firms would have experienced had they not been supported. The difference in performance between supported and non-supported firms is the estimated gross impact of the support scheme.

The performance of the non-supported firms may, however, differ systematically from what the supported firms would have experienced in the absence of the support scheme. This is the selection bias problem widely discussed in the evaluation literature.

In what follows we present more formally the selection bias problem in the context of the evaluation of the impact of the ANR of FONTAR. We also discuss the methodology, including variables and econometric models, to be used in order to evaluate the impact of the program. Finally, we summarize the difference in difference approach used in this study to deal with the selection bias problem in the context of after-the-fact evaluation.

In a previous work (on the FONCYT program, see Chudnovsky *et al*, 2006a) we discuss a number of alternative approaches available to deal with the selection bias problem, namely (i) regression with controls, (ii) instrumental variables, and (iii) regression discontinuity. Regression with controls is a second-best approach compared to difference-in-difference and it is used in the literature when panel data is not available. Since we have access to panel data, we will not follow a regression with controls approach. Besides, we will not use the instrumental variables methodology since we have not found adequate instruments[36]. Furthermore, given that selection into the FONTAR program is not based on an observable variable –at least, not on a variable observable by us-, or on some deterministic threshold process we cannot perform a regression-discontinuity analysis.

1. Selection Bias

The selection problem that arises in attempting to assess the impact of a program like the ANR of FONTAR is widely recognized (see Klette et al., 2000, or Heckman *et al*., 1998). In few words, selection bias (or evaluation bias) consists of the difference between the adjusted outcomes of the non-participants and the desired counterfactual mean.

As a basis for a formal discussion, consider the following version of the standard model:

$$Y_{it} = \beta_i D_{it} + \lambda X_{it} + \alpha_i + \mu_t + \varpi_{it} + \varepsilon_{it} \qquad (1)$$

where Y_{it} is the outcome of interest (I&D expenditure intensity) of applicant *i* in time *t*, D_{it} is a dummy variable that takes the value of 1 if applicant *i* receives the subsidy in time *t*, β_i is the impact for candidate *i* from receiving the subsidy, and X_{it} is a vector of observable determinants of outcome (i.e., proportion of qualified labor force on total labor force).

[36] We were not able to argue in favor of a variable that affects selection into the program but not the outcome (once selection process is accounted for).

The unobservable determinants of firms' outcome are reflected by the last four terms. There is a time-invariant 'applicant effect' (the individual effect, α_i, which represents permanent differences in performance among candidates) and a time-period effect common to all applicants (μ_t). The usual error term, which is assumed to be uncorrelated with the *X*'s and *D*, is represented by ε_{it}. This term represents temporary fluctuations in performance around the specific mean of each applicant and it is neither observed by the econometrician nor by the granting agency. The only non-standard entry in Equation (1) is ϖ_{it}, which represents period- and applicant- specific shocks that are un-observable by the econometrician, but observable by the granting agency –for example, through the evaluation of the quality and potentiality of the project.

The effect of the subsidy program is allowed to vary by applicant, and our goal is to measure the average impact. For the purpose of benefit/cost analysis, we would like to know $E\left(\beta_i / D=1\right)$, the average effect of the grant program for those firms receiving the subsidy.[37] In this way, the question being examined is the classic one of determining the effectiveness of a treatment that is given to a non-random fraction of some population. We wish to determine the average effect of treatment on the treated group. The obvious way to do this is to estimate some version of Equation (1) on a sample of applicants who did and did not receive grant funding, and use the regression coefficient on the treatment dummy as our measure of the treatment effect.

The selection bias problem arises because we presume that β_i is correlated with α_i and ϖ_{it} across *i*. That is, the projects that are the best candidates for funding are also the projects that would have the largest expected outcome in the absence of funding. This means that selection on β_i makes $E\left(\alpha_i+\varpi_{it} / D=1\right)>0$, which biases the regression estimate of $E\left(\beta_i / D=1\right)$[38].

The difference-in-difference estimator is the most widely used estimator in the evaluation literature (see Heckman *et al*., 2000). It consists of the difference between the before-after difference in outcome for participants and the before-

[37] Note at this point that the impact of the program is associated with a dichotomous grant (D=1) or no grant (D=0) condition. Thus, we are assuming that the magnitude of the grant does not matter.

[38] Recall that the assumption needed in order to get unbiased estimates for β in an Ordinary Least Square regression is that the conditional expected value of the error term has to be equal to zero.

after difference in outcome for non-participants –hence the name difference in differences.

In terms of Equation (1) the time-invariant unobservable α_i is eliminated by taking the difference in performance after treatment as compared to the performance before treatment. If such a difference is taken also for untreated entities, then common time effects μ_t are also eliminated.

The advantage of the difference-in-differences approach is that of eliminating any need to find observable correlates for the unobserved productivity difference. That is, a procedure of regression with controls is based on the selection-on-observables assumption. In the difference-in-difference approach we allow selection on un-observables, as long as these un-observables are candidate specific and time invariant.

This means that one important limitation of this approach is that it only controls for time-invariant candidate's un-observables. To the extent that the agency can and does evaluate the proposed project distinctly from the proposing firm, differencing does not eliminate the resulting selection bias.

In addition, one could imagine other sources of unobserved performance differences that vary across individuals and time. For example, applicants may decide to enter the grant competition when they have been enjoying unusually good (or bad?) recent performance. Any unobserved variation of this kind biases the estimator.

The validity or not of the difference-in-differences identification assumptions depends on the particular case under analysis: if one believes that part of the self-selection mechanism work through the observed covariates and that, given these covariates, what determines whether or not a firm is granted a subsidy are firm characteristics that stay more or less constant across time, then the difference-in-difference estimator is an acceptable estimation procedure.

Finally, in this study we will apply the difference-in-differences approach not only to the whole sample but also to the common support (the region of common support includes those values of the propensity score that have positive density within both the D=1 and D=0 distributions. For more details, see below)[39].

[39] In this case, the estimated treatment effect must then be redefined as the treatment impact for program participants whose propensity scores lie within the common support region.

a) Matching methods

Traditional matching estimators pair each program participant with an observably similar non-participant and interpret the difference in their outcomes as the effect of the program.

Matching estimators are justified by the assumption that outcomes are independent of program participation conditional on a set of observable characteristics. This is not a trivial assumption. It requires that all variables that affect both selection and outcome in the absence of treatment be included in the matching. In other words, matching methods are based on the selection-on-observables assumption, as in the regression with controls approach. The key difference between matching and the regression with controls approach is that regression makes the additional assumption that simply conditioning linearly on X suffices to eliminate selection bias. Matching is non-parametric and, as such, avoids the functional forms restrictions implicit in running a linear regression (Smith, 2000). Avoiding functional forms restriction can be important to reducing bias (Smith and Todd, 2003). Moreover, matching highlights the so-called common support problem, in the sense that it makes it easy to see when there is no non-participant to match with for some participants.

The main purpose of matching is to re-establish the conditions of an experiment when no randomized control group is available. The matching method aims to construct the correct sample counterpart for the missing information on the treated outcomes had they not been treated by pairing each participant with members of non-treated group. Under the matching assumption, the only remaining difference between the two groups is program participation. All that is required in order to estimate the average treatment effect on the treated is that there are analogues for each treated firm in the non-treated sample.

In the literature on the matching samples construction one can find several approaches to construct the control group. Supposing that X contains only one variable, it would be intuitive to look for an individual as control observation that has exactly the same value in X as the corresponding participant. If the number of matching criteria is large, however, it would hardly be possible to find any control observation. To solve this problem, Rosenbaum and Rubin (1983) developed the so-called 'propensity score matching'. The idea is to estimate the propensity score of participation, *P(X),* for the whole sample and find pairs of participants and non-participants that have similar probability value of participation. In this way, by using the propensity score matching one reduces the multidimensional problem of several matching criteria to one single measure of distance.

A key prerequisite for the application of propensity score matching is that individuals with equal probability of being selected as beneficiaries of the program are left out of the program (Heckman, Ichimura, and Todd, 1997).[40]

When using the propensity score the comparison group for each treated individual is established by a pre-defined measure of proximity. Having defined the common support, the next issue is that of choosing the appropriate weights to associate the selected set of non-treated observations for each participant one. Several possibilities are commonly used, from a unity weight to the nearest observation and zero to the others, to equal weights to all, or kernel weights, which account for the relative proximity of the non-participants' observations to the treated ones in terms of P (X).

In this paper we use the popular 'nearest neighbor' matching, i.e. after the estimation of a (probit) regression model of the participation dummy on important criteria, one selects the control observation with the closest estimated probability value to the participant.[41]

In general the form of the matching estimator is given by

$$\beta^{M} = \sum_{t \in T} \left\{ Y_i - \sum_{j \in C} W_{ij} Y_j \right\} w_i ,$$

where *T* and *C* represent the treatment and comparison groups respectively, W_{ij} is the weight placed on comparison observation *j* for individual *i* and w_i accounts for the re-weighting that reconstructs the outcome distribution for the treated sample. In the nearest neighbor matching case the estimator becomes

$$\begin{aligned} &\beta^{M}_{NN} = \sum_{i \in T} \left\{ Y_i - \sum_{j \in C_i} W_{ij} Y_j \right\} \frac{1}{N_T} \\ &W_{ij} = \frac{1}{N_{C_i}} \, if \, j \in C_i ; \; W_{ij} = 0 \;\; if \, j \notin C_i ; \;\; C_i = \min_j \left\{ \left| P_i - P_j \right| \right\} \end{aligned} \tag{2}$$

[40] If there are values of X such that P(X) =1, then participants with such values necessarily lie outside the common support because their probability of not participating is zero.

[41] Asymptotically, all the different matching estimators produce the same estimate, because in an arbitrarily large sample, they all compare only exact matches. In finite samples, different matching estimators produce different estimates because there are systematic differences between them, in the observations to which they assign positive weight and in how much weight is assigned.

where, among the non-treated, *j* is the nearest neighbor to *i* in terms of P(X).[42]

For a better understanding of the matching algorithm, we briefly summarize the procedure applied (see Czarnitzki and Hussinger, 2004):

1. Estimate the propensity score and restrict the sample to common support, deleting all observations on treated firms with probabilities larger than the maximum of the one of the controls and all control units with an estimated propensity score smaller than the minimum propensity score in the treated group.[43]
2. Select a firm *i* that received a grant.
3. Take the estimated propensity score $P\left(X_i^{'}\hat{\beta}\right)$ and calculate the distance $d_{ij} = P\left(X_i^{'}\hat{\beta}\right) - P\left(X_j^{'}\hat{\beta}\right)$ for every combination of the treated firmer *i* and every firm from the potential control group *j*.
4. The firm *j* from the potential control group with the smallest distance serves as control observation in the following outcome analysis. The comparison observation is drawn randomly if more than one firm attains the minimum distance.
5. Remove the *i-th* observations from the pool of firms that received grants but return the selected control observation to the pool of control observations. This is done because of the relatively limited number of control firms.[44]
6. Repeat steps 2 to 5 to find matched pairs for all recipients.
7. Once a control observation has been picked for each subsidized firm, calculate the mean difference between the treatment group and the selected control group.[45]

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[42] In general, kernel weights are used for W_{ij} to account for the closeness of Y_j to Y_i.

[43] We are adapting the definition in Czarnitzki and Hussinger in order to achieve the actual intersection between both supports. Notice that since it may exclude some treated, the estimate obtained with this definition of common support is not exactly an average treatment on the treated estimate.

[44] Matching with replacement involves a tradeoff between bias and variance. Allowing replacement increases the average quality of the matches (assuming some re-use occurs), but reduces the number of distinct non-participant observations used to construct the counterfactual mean, thereby increasing the variance of the estimator (Smith and Todd, 2003). Nearest neighbor matching without replacement has the disadvantage that the estimate depends on the order in which the observations get matched.

[45] When using matching methods, it is important to get the correct standard errors. In the case of nearest neighbor matching, treating the matched sample as given will understate the standard

b) Difference-in-differences matching estimator

The difference-in-differences matching estimator combines the matching estimator and the difference-in-differences estimator. According to the literature, this should be a more efficient strategy than the two estimators separately applied (Blundell and Costa-Dias, 2002).

In the nearest neighbor case the effect of the treatment on the treated can now be estimated over the common support using an extension to (2),

$$\hat{\beta}^{M-DID} = \frac{1}{N_{T_S}} \sum_{i \in T_S} \left(\left[Y_{it} - Y_{it'} \right] - \sum_{j \in C_S} w_{ij} \left[Y_{jt} - Y_{jt'} \right] \right), \quad (3)$$

where t is the time period after treatment and t' is the time period before treatment, *Ts and Cs* indicate, respectively, the treatment and the matched control groups in the common support and w_{ij} represent the weights, calculated as in the simple matching estimator, corresponding to the researcher j matched to a treated researcher i.

As discussed above, matching methods are based on the selection-on-observables assumption. The difference is that now the main matching hypothesis is stated in terms of the before-after evolution instead of levels. It means that controls have evolved from a pre- to a post-program period in the same way treatments would have done had they not been treated (Blundell and Costa-Dias, 2002).

Summing up, the procedure to obtain the difference-in-differences matching estimator is as follows:

1. Estimate the propensity score.
2. For each treated firm find the non-treated firm that matches the treated firm according to the propensity score (see matching procedure).
3. Calculate before/after difference for each participant.
4. Calculate before/after difference for each non-participant chosen as a match. Notice that some non-participants may appear more than once.
5. Evaluate difference-in-differences using Equation (3).

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errors. As pointed out by Smith (2000), in practice most researchers report bootstrapped standard errors. However, Abadie and Imbens (2006) show that bootstrapping is not valid for nearest neighbor matching due to lack of smoothness, whereas it works for kernel matching, therefore we will present kernel matching estimates as well.

B. Data

Our database was constructed from a tailor-made survey conducted by INDEC (National Institute of Census and Statistics). We count with data from 414 firms for 4 successive years (2001-2004) and for 1998. From the total sample of 414 firms, 136 have been granted a non-reimbursable subsidy (ANR) from the FONTAR, 62 firms applied but did not receive the ANR, and 216 firms did not apply for the subsidy.

Descriptive statistics of the variables used later in this work are presented in Table 2.

The outcomes we consider are: innovation intensity (total and private), new product sales and labor productivity. Our first regression deals with “innovation intensity”, a variable constructed as the ratio of total innovation expenditures to total sales. Among the items included in total innovation expenditures are: intramural and extramural R&D expenditures, purchase of software, hardware and capital goods, technology contracts, in house industrial design and management, and personnel training. This variable has been chosen and constructed in the vein of the Oslo manual (for more details see OECD 2005). The use of this variable is also justified on the basis that ANR finance not only R&D but also all those abovementioned items.

We also consider “private innovation intensity”, which nets out the amount of the subsidy received from total innovation expenditure and divide this by total sales. As we do not count with information on the annual funds received from FONTAR, we take the total sum and prorate it equally among the years that the funded project was in practice.

It is possible to appreciate in table 2 that the mean value of the two innovation intensity variables declines throughout the sampled period. However, one should note that between 2003 and 2004 (the years with the largest amounts of subsidies granted) the mean for funded firms grows, while the one for non-funded firms declines.

In the second place, as an innovation outcome, we consider domestic sales of new products.[46] Actually, this variable represents the real amount[47]of sales of

[46] We have also used new products exports, and the sum of new products domestic sales and exports. The results obtained were qualitatively identical as the ones we show in the following section.
[47] All real values were obtained by dividing each nominal value by the corresponding Producer Price Index (1993=100) disaggregated at the two digits level of the ISIC Rev.3. For service sectors

new or significantly improved products for the firm. We count with two temporal observations of this variable in our data; the one for 2004 includes the sales of products of innovations developed from 2001 to 2004, while the one for 2001 considers the period 1998-2001.

The number of missing values is significantly high for this variable; it represents 60% and 50% of total observations for 2001 and 2004 respectively. While for the year 2001 the proportion of missing values is similar for funded and not funded firms (42% and 40% respectively), this is not the case for 2004 with percentages of 35% for funded firms and 55% for non-funded ones. We also considered other innovation outcomes such as a dummy variable, which indicates if the firm innovated during the sampled period or the number of patens, but missing values were even higher for these variables[48].

The mean amount of sales grows between both observations. This happens both for funded and non-funded firms, but while for the first ones it grows a 74%, in the case of non-funded firms the increase is only 35%.

Table 2. Descriptive statistics

	FULL SAMPLE			ANR=1			ANR=0		
Variable	**Obs**	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**	**Obs**	**Mean**	**Std. Dev.**
Outcomes									
Innovation intensity 2001	258	0.17	0.52	104	0.30	0.77	154	0.08	0.21
Innovation intensity 2002	254	0.16	0.48	97	0.26	0.56	157	0.11	0.42
Innovation intensity 2003	284	0.14	0.45	118	0.15	0.27	166	0.13	0.54
Innovation intensity 2004	299	0.11	0.28	114	0.18	0.38	185	0.06	0.18
Private innov. int. 2001	258	0.13	0.36	104	0.21	0.49	154	0.08	0.21
Private innov. int. 2002	254	0.13	0.45	97	0.17	0.49	157	0.11	0.42
Private innov. int. 2003	284	0.12	0.43	118	0.12	0.21	166	0.13	0.54
Private innov. int. 2004	299	0.09	0.25	114	0.13	0.34	185	0.06	0.18
Sales new products 2001	167	13026	24176	57	8598	14304	110	15321	27738
Sales new products 2004	213	18293	42825	89	14969	39952	124	20679	44781
Laborproductivity2001	376	930	1238	121	1038	1630	255	878	999
Laborproductivity2002	386	985	2831	124	1264	4612	262	852	1321
Laborproductivity2003	396	912	2551	125	1108	4221	271	821	1147

we used the Implicit Price Deflator for the private consumption component of Gross Domestic Product (1993=100).

[48] The survey explicitly instructed firms to put zeros where they correspond. While we think that many of registered missing values are actually "zeros", we have no way of distinguishing between them and missing values.

Variable	FULL SAMPLE Obs	Mean	Std. Dev.	ANR=1 Obs	Mean	Std. Dev.	ANR=0 Obs	Mean	Std. Dev.
Laborproductivity2004	400	897	2356	127	1066	3957	273	818	932
Determinants									
ANR	414	0.33	0.47	136	1.00	0.00	278	0.00	0.00
Applied To ANR	414	0.48	0.50	136	1.00	0.00	278	0.22	0.42
Group	411	0.17	0.37	136	0.06	0.24	275	0.22	0.41
Innovation intensity 1998	195	0.08	0.17	59	0.08	0.13	136	0.07	0.19
Laborpoductivity1998	316	1099	1433	94	1230	1708	222	1044	1299
Real total sales 1998	342	81315	321268	106	26720	41302	236	105836	383482
Real total sales 2001	392	59859	188999	125	26942	50026	267	75270	224927
Real total sales 2002	402	57918	216565	130	26200	66300	272	73078	258056
Real total sales 2003	408	54496	150994	132	27277	57163	276	67514	177934
Real total sales 2004	407	65347	193439	131	31652	60626	276	81339	229584
Expo 1998	168	1454427	3867859	56	690143	1286419	112	1836569	4609324
Expo 2001	198	1328436	3442899	71	835069	1571108	127	1604255	4116767
Expo 2002	202	3727334	11900000	75	1622573	3583590	127	4970304	14700000
Expo 2003	217	2903201	9710509	78	1610813	3020560	139	3628425	11900000
Expo 2004	217	3857763	11300000	77	2274213	4152471	140	4728715	13600000
GI 1998	270	445618	1917490	88	118580	226796	182	603747	2315708
GI 2001	309	203084	564003	109	95928	151287	200	261485	685709
GI 2002	304	308363	1270669	109	166828	579858	195	387477	1522183
GI 2003	314	472877	2142248	116	222936	635541	198	619306	2645258
GI 2004	320	589597	2586689	119	291623	754632	201	766010	3201872
Qualified labor share 1998	226	0.16	0.23	69	0.25	0.28	157	0.13	0.20
Qualified labor share 2001	273	0.20	0.24	91	0.26	0.27	182	0.16	0.22
Qualified labor share 2002	280	0.20	0.24	95	0.26	0.25	185	0.17	0.23
Qualified labor share 2003	296	0.21	0.24	101	0.25	0.24	195	0.19	0.24
Qualified labor share 2004	304	0.20	0.23	104	0.25	0.24	200	0.18	0.23
Total employees 1998	329	68.85	113.71	101	34.68	62.70	228	83.99	127.29
Total employees2001	385	68.66	161.61	126	35.51	61.32	259	84.79	190.41
Total employees2002	394	67.41	189.53	130	32.29	58.50	264	84.70	226.04
Total employees2003	403	71.84	194.03	131	37.95	64.70	272	88.16	230.24
Total employees2004	406	77.80	178.50	132	44.45	73.22	274	93.86	209.52

Finally, labor productivity (constructed as the ratio of total sales to total employees) is used as our performance variable. In general, this variable presents a downward trend for both groups of firms.

The set of controls used in the regressions includes: ANR –which is our variable of interest- (a dummy variable that takes the value of 1 if the firm was carrying out a project funded with an ANR in that year), Applied to ANR (dummy that takes the value of 1 if the firm applied for an ANR during the sampled period), GROUP (1 if the firm is part of a group of firms controlled by a holding), qualified labor share (ratio between qualified employees and total employees), exports, total employees, gross investment (GI) and real sales. We also count with 9 regional variables and 17 industry divisions -for details see annex 1.

C. Results

1. Additionality vs. Crowding Out

In Table 3.a we present the results corresponding to the test of additionality vs. crowding out hypothesis.

Table 3.a. Additionality vs. crowding out – control group includes all available firms

	Dependent variable: Innovation intensity				***Dependent variable: Log(innovation intensity)***			
	Dif-in-dif		Dif-in-dif In common support		Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ANR	0.123	0.067	0.084	0.052	0.614	0.430	0.582	0.567
	(0.052)**	(0.022)***	(0.045)*	(0.025)**	(0.112)***	(0.156)***	(0.182)***	(0.216)***
Qualified labor share		0.293		0.181		1.360		1.917
		(0.148)**		(0.407)		(0.845)		(1.632)
Exports/ Sales		-0.029		0.062		0.047		-0.183
		(0.138)		(0.169)		(0.519)		(0.589)
Employees		.00007		.0011		.0002		.0014
		(.00007)		(.0009)		(.0007)		(.0040)
Obs.	1095	580	509	347	1047	557	490	337
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level.*

Column (1) presents results corresponding to the baseline model, including ANR as the only explanatory variable in a fixed effects specification. The coefficient associated to ANR is significant at the 2% level, and its value suggests an increase in total innovation intensity in those firms receiving the subsidy.

Column (2) incorporates the proportion of qualified employees on the total number of employees, the ratio of exports to total sales, and the total number of employees as additional explanatory variables. Only the first of these variables is significant at the usual levels of confidence. ANR, on the other hand, remains significant, now at the 1% level, thus supporting the conclusion that the program has had a positive impact on innovation intensity of those funded firms.

We also include the interaction between ANR and the year dummies in order to explore the possibility that the average impact of ANR on innovation intensity depends on the year in which the subsidy was awarded. In all cases the interaction terms are jointly not significant at the usual levels of confidence suggesting thus, that the impact of the subsidy is constant across time[49].

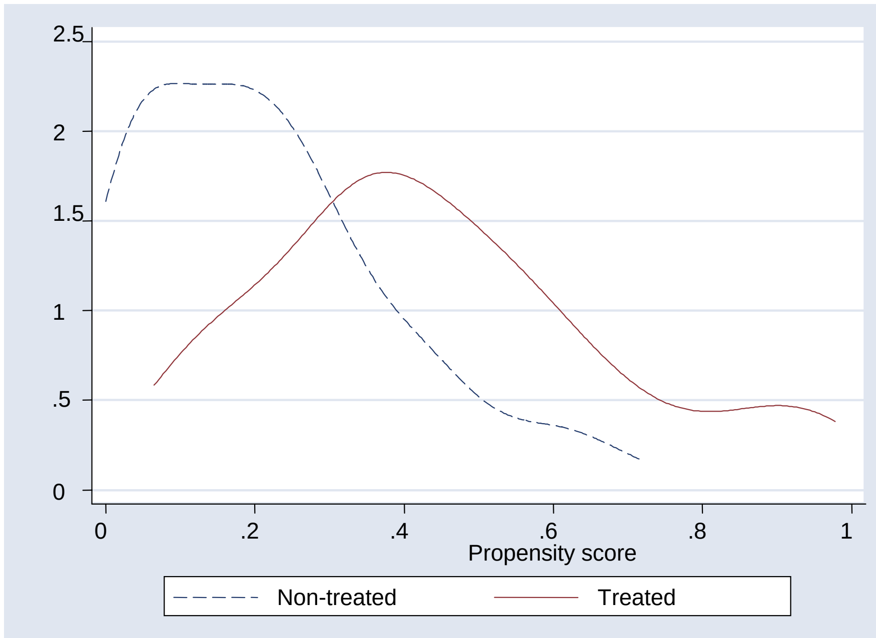
Columns (3) and (4) replicate the difference-in-differences estimation on the sample restricted to the common support (defined as the intersection of the distribution of propensity scores for treated and non-treated firms). The propensity score used to obtain the common support sample was obtained by means of a Probit model for the probability that a firm received the subsidy at some point during the period 2001 to 2004 as a function of the following pre-treatment (measured in 1998) characteristics: Group, Real Total Sales, the ratio of qualified employees to the total number of employees, industrial branch, and the region in which the firm operates (see Annex 2).[50]

The restricted sample, considering only those observations in the intersection of the support of the distribution of propensity scores for both funded and non-funded firms, has 206 observations (over a maximum of 414 firms). Figure 1 shows a non-parametric estimation of the distribution of the propensity score for the two groups of firms -the size of the common support can be appreciated as the intersection of the support of these distributions-.

[49] All regressions mentioned but not shown are available from the authors upon request.

[50] This specification of the probit model passes the tests for the “balancing property”, that is, the balancing of pre-treatment variables given the propensity score.

Figure 1. Kernel non-parametric estimation of the distribution of the propensity scores for treated and non-treated firms.



An important reduction in the sample size is due to missing values in the estimation of the propensity score. However, results obtained using the restricted sample, are consistent with the previous ones: the coefficient on ANR remains positive and significant in the innovation intensity equation.

Columns (5)-(8) replicate results in columns (1)-(4) but using the natural logarithm of innovation intensity as the dependent variable (this is a convenient way to normalize the innovation intensity variable due to its biased distribution). In all cases the coefficient on ANR is significantly different from zero at the 1% level. The value of the coefficient suggests that funded firms increased their innovation intensity after the subsidy in a range from 54% (exp (0.43) -1)[51] to 79% compared to those firms that were not funded.

In Table 3.b and Table 3.c we replicate results from Table 3.a but separating the control group in two groups, one with firms that applied for the subsidy and the

[51] See Halvorsen and Palmquist (1980) for the interpretation of dummy variables in semi logarithmic equations as the one we are analyzing. If we take into account Kennedy (1981) correction, the range would be from 52 to 76%.

other with those firms that never applied for the subsidy. In both cases results are remarkably similar to the ones shown in Table 3.a.

Table 3.b. Additionality vs. crowding out – control group includes only firms that applied to the subsidy

	Dependent variable: innovation intensity				*Dependent variable: Log (innovation intensity)*			
	Dif-in-dif		Dif-in-dif In common support		Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ANR	0.125	0.062	0.082	0.053	0.623	0.443	0.584	0.580
	(0.052)**	(0.023)***	(0.045)*	(0.027)**	(0.114)***	(0.164)***	(0.186)***	(0.222)***
Qualified labor share		0.269		-0.185		2.393		1.435
		(0.198)		(0.599)		(0.802)		(2.273)
Exports/sales		0.032		0.169		0.173		0.215
		(0.201)		(0.211)		(0.563)		(0.664)
Employees		0.001		0.003		-0.001		-.0017
		(0.001)		(0.003)		(.003)		(.0079)
Observations	562	279	229	169	535	262	222	164
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 3.c. Additionality vs. crowding out – control group includes firms that did not apply to the subsidy

	Dependent variable: innovation intensity				*Dependent variable: Log (innovation intensity)*			
	Dif-in-dif		Dif-in-dif In common support		Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ANR	0.121	0.065	0.084	0.047	0.611	0.421	0.579	0.517
	(0.052)**	(0.022)***	(0.045)*	(0.023)**	(0.112)***	(0.155)***	(0.182)***	(0.216)**
Qualified labor share		0.209		-0.037		1.053		1.712
		(0.129)		(0.286)		(0.945)		(1.741)
Exports/ Sales		-0.145		-0.072		-0.534		-1.329
		(0.108)		(0.055)		(0.611)		(0.649)**
Employees		.00003		.0002		.0003		.0018
		(.00003)		(.0002)		(.0007)		(.0043)
Observations	966	511	449	301	926	492	431	292
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

The significant and positive impact of the subsidy upon total innovation intensity brings evidence that lead us to reject the crowding out hypothesis. However, it does not directly allow us to test for the additionality hypothesis. Therefore, table 3.d replicates the previous analysis considering only private innovation expenditures intensity in order to see if once the subsidy is netted out, we still find a positive effect of the funding, which would indicate an additionality effect.

As one can see in table 3.d, the impact of the subsidy is not significantly different from zero in this case, suggesting that it is not possible to conclude that the subsidy stimulated additional privately funded innovation expenditures.

Table 3.d. Additionality vs. crowding out – private I&D intensity (net of the subsidy)

	Dependent variable: private innovation intensity				*Dependent variable: Log(private innovation intensity)*			
	Dif-in-dif		Dif-in-dif In common support		Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ANR	0.022	0.026	0.009	0.020	0.164	0.106	0.281	0.284
	(0.039)	(0.019)	(0.014)	(0.018)	(0.112)	(0.161)	(0.171)	(0.197)
Qualified labor share		0.263		0.524		0.957		2.734
		(0.118)**		(0.306)*		(1.050)		(1.595)*
Exports/ Sales		-0.019		0.067		0.047		-0.169
		(0.140)		(0.169)		(0.542)		(0.608)
Employees		.00007		.0011		.0005		.0053
		(.00007)		(.0010)		(.0007)		(.0040)
Observations	1073	571	499	343	1025	548	480	333
Year dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Tables 4.a and 4.b present the results of extending the analysis by performing a difference-in-differences strategy on the matched sample restricted to the common support. Difference-in-difference matching estimates reaffirm our previous findings; there is a significant effect of the subsidy on total innovation expenditure intensity, but not on the privately funded one.

Table 4.a. Differences-in-difference matching estimator for innovation intensity

	Nearest neighbor matching	Kernel Matching
TREATED (n=37)	**Average**	
Innovation intensity after ANR	0.2091	
Innovation intensity before ANR	0.0815	
Difference	**0.1276**	
CONTROL(n=37)		
Innovation intensity after ANR	0.1493	
Innovation intensity before ANR	0.2238	
Difference	**-0.0745**	
Difference-in-differences	**0.2021**	**0.184**
Analytical standard errors	(0.1034)*	
Bootstrap standard errors	(0.0864)**	(0.0916)**

Notes: standard errors are shown in parentheses, for bootstrapped standard errors 500 replications were performed. The years considered for "after ANR" and "before ANR" were the year the firm was granted the subsidy and the immediate previous one, respectively (the same years were considered for each matched control). We only considered observations in the common support and without missing values for the corresponding year.

*The bandwidth used for the kernel estimator was 0.06, for other bandwidths from 0.04 to 0.24 bootstrapped standard errors vary between 0.08 and 0.092, and the estimates from 0.172 to 0.184, not changing the significance of the results. The average difference in propensity score between treatments and matched controls is 0.004, with a maximum difference equal to 0.05. The average propensity score is 0.369. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 4.b. Differences-in-difference matching estimator for private innovation intensity

	Nearest neighbor matching	*Kernel Matching*
TREATED (n=33)	**Average**	
Innovation intensity after ANR	0.1271	
Innovation intensity before ANR	0.0726	
Difference	**0.0545**	
CONTROL(n=33)		
Innovation intensity after ANR	0.1208	
Innovation intensity before ANR	0.2144	
Difference	**-0.0936**	
Difference-in-differences	**0.1481**	**0.125**
Analytical standard errors	(0.1035)	
Bootstrap standard errors	(0.0991)	(0.087)

*Notes: id. table 4.a. The bandwidth used for the kernel estimator was 0.06, for other bandwidths from 0.04 to 0.24 bootstrapped standard errors vary between 0.078 and 0.101, and the estimates from 0.108 to 0.126, not changing the significance of the results. The average difference in propensity score between treatments and matched controls is 0.004, with a maximum difference equal to 0.05. The average propensity score is 0.369. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

2. Heterogeneous Impacts for New and Established Innovators.

A relevant issue to explore is that related to the possibility that the ANR could have heterogeneous impacts on different groups of firms. In our case, we tested whether the ANR impact was different between firms that performed innovation activities before receiving the grant *vis a vis* those which did not undertake those activities –and hence, we could assume that the ANR was important to push those firms to begin to make expenditures in R&D-.

To test the hypothesis that the effect of the subsidy was not the same for all firms, we created a new variable, "Innov", which is a dummy variable equal to 1 if the firm did not declare a positive amount of innovation expenditures in 1998, and 0 otherwise. In this way, we attempt to see if the impact of the subsidies differs from "new innovators"[52] to "established innovators"-in our case, firms that already had positive innovation expenditures before the granting of the subsidies-.

In fact, the evidence shows that there was a different effect for these two groups of firms. On the one hand, the interaction between ANR and "Innov" is significant, indicating that the impact of the subsidy on innovation intensity (table 5.a) and private innovation intensity (table 5.d when restricting to the common support) was heterogeneous.

The evidence presented in tables 5.b and 5.c, in which the sample is divided in these two groups of firms, indicates that there was a crowding out effect for "established innovators", but not for "new innovators". Furthermore, tables 5.e and 5.f bring some evidence in favor of an additionality effect for "new innovators".

[52] This group also includes firms that did not exist in 1998.

Table 5.a. Additionality vs. crowding out. Interaction with new innovators -full sample-

	Dependent variable: Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR*innov	0.192	0.095	0.184	0.136
	(0.092)**	(0.034)***	(0.076)**	(0.061)**
Qualified labor share		0.283		0.0997
		(0.151)*		(0.425)
Exports/Sales		-0.042		0.038
		(0.140)		(0.169)
Employees		0.00006		0.0001
		(0.00007)		(0.0009)
Obs.	1095	580	509	347
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 5.b. Additionality vs. crowding out -including only "new innovators"-

	Dependent variable: Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR	0.187	0.092	0.157	0.117
	(0.093)**	(0.034)***	(0.067)**	(0.048)**
Qualified labor share		0.245		1.523
		(0.159)		(0.595)**
Exports/ Sales		-0.319		-0.098
		(0.254)		(0.147)
Employees		0.0001		-0.0005
		(0.0002)		(0.0008)
Obs.	428	189	106	69
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 5.c. Additionality vs. crowding out -including only "established innovators"-

	Dependent variable: Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR	0.047	0.045	0.046	0.017
	(0.041)	(0.027)*	(0.052)	(0.023)
Qualified labor share		0.163		-0.014
		(0.343)		(0.422)
Exports/ Sales		0.029		0.046
		(0.157)		(0.204)
Employees		0.00005		0.0012
		(0.00008)		(0.09)
Obs.	667	391	403	278
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

*Notes: robust standard errors are shown in parentheses. * Significant at the 10% level.*

Table 5.d. Additionality vs. crowding out, private innovation intensity. Interaction with new innovators -full sample-

	Dependent variable: Private Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR*innov	0.061	0.038	0.076	0.087
	(0.070)	(0.027)	(0.036)**	(0.038)**
Qualified labor share		0.257		0.484
		(0.117)**		(0.305)
Exports/Sales		-0.023		0.058
		(0.140)		(0.168)
Employees		0.00006		0.0012
		(0.00007)		(0.0009)
Obs.	1073	571	499	343
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 5.e. Additionality vs. crowding out, private innovation intensity -including only "new innovators"-

	Dependent variable: Private Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR	0.058	0.033	0.064	0.068
	(0.073)	(0.029)	(0.033)*	(0.031)**
Qualified labor share		0.093		1.173
		(0.121)		(0.529)**
Exports/ Sales		-0.336		-0.095
		(0.289)		(0.146)
Employees		0.0001		-0.0005
		(0.0002)		(0.0007)
Obs.	414	184	101	68
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.
*** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.

Table 5.f. Additionality vs. crowding out, private innovation intensity -including only "established innovators"-

	Dependent variable: Private Innovation intensity			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR	-0.018	0.019	-0.014	-0.009
	(0.031)	(0.025)	(0.012)	(0.02)
Qualified labor share		0.455		0.393
		(0.267)		(0.322)
Exports/ Sales		0.038		0.048
		(0.153)		(0.204)
Employees		0.00006		0.0014
		(0.00008)		(0.012)
Obs.	659	387	398	275
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

Notes: robust standard errors are shown in parentheses.

3. Innovative outcome

Table 6 presents OLS estimates for the innovative outcome. We have data for only two points in time (2001 and 2004) for our dependent variable, sales of new products. Therefore, a first differences regression was performed instead of classic fixed effects. Results show that it is not possible to reject the absence of any effect of the funding on firms innovation outcome.

When we perform the same strategy but after having matched treated with similar control firms, the results do not change. The outcome is presented in table 7.

Table 6. Innovative outcome

	Dependent variable: Δ (sales of new products)			
	Dif-in-dif		Dif-in-dif In common support	
	(1)	(2)	(3)	(4)
ANR	-107.56	-5886.47	-126.09	-905.97
	(3804.92)	(4814.10)	(3527.84)	(4832.99)
Δ qualified labor share		47437.87		68697.65
		(30769.63)		(34221.72)*
Δ innovation intensity		1523.20		-2717.53
		(6152.13)		(6893.82)
Δ Exports/ Sales		-45566.76		-38671.99
		(25523.62)*		(19863.32)*
Δ Employees		382.95		106.61
		(205.91)*		(90.64)
Observations	157	63	83	47

*Notes: in all cases Δ refers to the difference calculated between the years 2004 and 2001. Robust standard errors are shown in parentheses. * Significant at the 10% level.*

Table 7. Differences-in-difference matching estimator for innovative outcome

	nearest neighbor matching	*Kernel Matching*
TREATED (n=22)	**Average**	
Difference in new sales (2001,2004)-(1998,2001)	3388.20	
CONTROL(n=22)		
Difference in new sales (2001,2004)-(1998,2001)	2400.92	
Difference-in-differences	**987.28**	**1013.34**
Analytical standard errors	(3463.05)	
Bootstrap standard errors	(3830.29)	(3859.35)

Notes: id table 4.a. The bandwidth used for the kernel estimator was 0.06, for other bandwidths from 0.04 to 0.24 bootstrapped standard errors vary between 3268 and 3860, and the estimates from 886 to 1119, not changing the significance of the results. The average difference in propensity score between treatments and matched controls is 0.0002, with a maximum difference equal to 0.038. The average propensity score is 0.379.

4. Productivity

In Table 8 and 9 we present results for one variable that capture firms' performance, labor productivity (ratio of total sales to total labor).[53] In these models ANR is not significant at the conventional levels of confidence suggesting that the subsidy does not have any impact on labor productivity. This conclusion is obtained from the fixed effects estimates and the difference-in differences matching ones as well.

Table 8. Productivity

	Dependent variable: Sales/Employees			
	Dif-in-dif		Dif-in-dif in common support	
	(1)	(2)	(3)	(4)
ANR	100.201	-105.843	20.013	-42.389
	(114.951)	(67.896)	(62.428)	(67.701)
Qualified labor share		1316.088		2812.126
		(1279.959)		(1537.825)*
Exports/Sales		240.482		244.424
		(217.153)		(249.504)
Employees		-1.440		-5.022
		(0.322)***		(1.759)***
GI		4.36e-06		.00002
		(.00001)		(6.15e-06)***
Observations	1558	622	652	357
Year dummies	Yes	Yes	Yes	Yes
Firm dummies	Yes	Yes	Yes	Yes

*Notes: robust standard errors are shown in parentheses. *** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

Table 9. Differences-in-difference matching estimator for labor productivity

	nearest neighbor matching	*Kernel Matching*
TREATED (n=51)	Average	
Labor productivity after ANR	728.54	
Labor productivity before ANR	810.05	
Difference	**-81.31**	
CONTROL(n=51)		
Labor productivity after ANR	677.97	
Labor productivity before ANR	666.88	
Difference	**11.09**	
Difference-in-differences	**-92.4**	**-85.76**
Analytical standard errors	(98.21)	
Bootstrap standard errors	(105.93)	(102.43)

Notes: id table 4.a. The bandwidth used for the kernel estimator was 0.06, for other bandwidths from 0.04 to 0.24 bootstrapped standard errors vary between 96.93 and 106.5, and the estimates from -80.1 to -88.9, not changing the significance of the results. The average difference in propensity score between treatments and matched controls was 0.001, with a maximum difference equal to 0.005. The average propensity score is 0.38.

[53] We replicated the procedure for other performance variables: total employment, total sales, growth of sales, qualified labor share and expo/sales ratio, obtaining the same non significant results to the ones presented in this section. Results are available upon request.

D. Some Qualitative Results

From our tailor-made survey conducted by INDEC we can draw upon answers of the 136 receptors of the subsidy to extract interesting conclusions. To begin with, two questions are related to the crowding-out vs. additionality discussion. Granted firms were asked if the ANR allowed them to develop a project that they would not have developed without the subsidy, or if it at least allowed them to speed up a project. These questions made us possible to estimate the counterfactual outcome using granted firms' assertions of what they would have done in the case they had not received the ANR, without having to find other comparable control group.

Only 5% of the firms answered both questions negatively. If firms' answers were not biased, these would be the firms that would have done their projects even without the subsidy, and thus, for them, ANR funds would have completely crowded out their own innovation expenditures. Then, 40% of the firms answered positively only the second question, which would mean that the ANR allowed them to speed up a project that they would have done anyway but in a longer span of time. For this group of firms, it would be possible to detect a partial crowding out effect of the funds, since in spite of the fact that the project would have been done even without the ANR, the later probably accelerated the project or speeded up its rate of performance.[54] Finally, the other 55% of the granted firms stated that the ANR allowed them to develop a project that they would not have carried out otherwise. Since ANR funds must be matched by a 50% counterpart investment financed by granted firms, this result can be interpreted as an indicator of some crowing-in or additionality effect of the funds.

These results must be taken with caution, since firms' answers could be biased, particularly in the direction of answering that they would have not done the projects without the ANR. Were that the case, the crowding out effect would probably be underestimated. Nevertheless, what these qualitative results probably indicate is that there could have been a heterogeneous impact of the ANR, and thus our econometric finding showing neither a crowding-out effect nor an additionality effect could be the product of aggregation. Further studies are needed in this direction. The distinction between ANR impact for new and established innovators, as explained previously, is a good initial step, but research on this issue must be reinforced when better data be available.

[54] These kinds of benefits are taken into account in a cost-benefit analysis of ANR developed in Chudnovsky et al. (2006 c). See also Gallagher et al (2002) for a theoretical framework that justifies the inclusion of those benefits.

Two other answers from the survey could be relevant to understand our finding of no significant effect of the subsidies on innovative outcomes and firms' performance. On the one hand, the most mentioned (28% of granted firms) drawback of the FONTAR program was that the amount of the ANR was not enough, followed by excessive red tape (24%), and little support to the introduction of the innovation in the market (19%). On the other hand, firms mentioned (57% of positive answers) the devaluation of Argentinean currency and the impact of 2001/2002 crisis as the main obstacle for developing their projects. Problems related to the quality of human capital and technological infrastructure were detected but only with 15 and 13% of positive answers respectively.

The combination of insufficient funding and the crisis impacts could have been crucial impediments for the existence of a link between innovation expenditures and innovative outcomes or firms' commercial performance. However, as we assert below in the conclusions, it is probable that this link can only be appreciated when more time has passed since the conclusion of the projects.

IV CONCLUSIONS

The main outcome of our evaluation is that firms that received the ANR had a higher level of expenditures in innovation activities than those firms that did not receive the subsidy. Hence, as we assume that the situation of the latter group is a good counterfactual for analyzing what would have happened to treated firms in case they had not received the ANR, we can state that the subsidies had a positive impact on the total level of innovation expenditures of those firms. In other words, there was no total crowding out effect.

Since we find a positive, but not significant, effect of ANR on private innovation intensity, we could say that there was not even a partial crowding out effect for treated firms. However, at the same time, evidence indicates that ANR did not generate an additionality effect, since it seemingly did not foster benefited firms to spend more money of their own on innovation activities. In this connection, our findings are not very different from those studies carried out in other countries as discussed in section 2.

Nevertheless, when we distinguish the effect of ANR program for firms that had some innovation expenditures several years before the beginning of the program and for those firms that had no innovation expenditures at that time, we find different results. For firms that already had innovation expenditures we see a crowding out effect of ANR funds, while for the other firms no crowding out is appreciated and some evidence in favor of an additionality effect is present.

Both the estimation of the effect of subsidies on innovative outcomes and firms' performance did not result in statistically significant results. However, in addition to the abovementioned factors -insufficient amount of funding and the effects of the crisis period- two other things must be taken into account in relation to this finding: i) since a large part of subsidized projects concluded in 2005, it could be the case that not enough time had passed as for us to detect a positive impact on firms' performance; ii) in another study we have shown that innovation expenditures have a positive impact on the probability of a firm of becoming an innovator and that innovators performed better than non-innovators in Argentina during the period 1998-2001 (Chudnovsky *et al*, 2006b). Hence, we could expect that, given time, firms benefited by ANR, which augmented their R&D expenditures, would experiment a positive impact on their innovative and productivity performance.

In this paper we have limited ourselves to analyze the private impact of ANR, although there are many ways in which that instrument could have had a positive social impact. This kind of analysis was done in Chudnovsky et al (2006c) in which ten “promising” projects (chosen by FONTAR) receiving subsidies were studied following a social cost-benefit analysis. We found that the spillovers of those projects (mainly on the users adopting the innovation) were substantial. In this way the results of the other report clearly complement those presented in this study.

While these findings suggest that the subsidies given by FONTAR had a positive effect, at the margin, on the innovating firms and on their users, more research is required to assess other issues related to the selection of firms, allocation of resources and evaluation of the results of public funding to innovation activities –including the need of following the performance of benefited firms to learn about the long term impacts of FONTAR subsidies-. Finally, given the fact that innovation activities –in particular R&D- are still very weak in Argentina, it is crucial to think in strategies aimed at giving a new impulse to this fundamental input for attaining a sustainable development process.

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ANNEX 1: DISTRIBUTION OF FIRMS BY ACTIVITY AND REGION

Table A. 1. Number of funded firms by principal activity and region

	Region									
Activity	**Pamp.**	**Centre**	**Cuyo**	**Chaco**	**GBA**	**Mesop**	**Capital**	**NOA**	**Patag**	**Total**
Agriculture, hunting and fishing	1	1	1	0	0	0	0	0	1	4
Forestry, logging, manufacture of wood	0	2	0	0	1	1	0	0	0	4
Services and business activities	1	3	0	1	1	0	11	0	1	18
Manufacture of food products and beverages	0	1	1	0	1	1	2	1	1	8
Manufacture of textiles	0	0	0	0	0	0	1	0	0	1
Manufacture of paper, publishing and printing	0	0	0	1	1	0	0	0	0	2
Manufacture of chemicals	0	4	0	0	1	0	6	0	0	11
Manufacture of rubber and plastics products	0	1	0	0	1	0	3	0	0	5
Manufacture of fabricated metal products	1	1	0	0	2	0	1	0	0	5
Manufacture of machinery and equipment	3	10	1	0	5	1	4	0	0	24
Manufacture of electrical machinery	1	3	0	0	1	0	3	0	0	8
Manufacture of medical, precision and optical instruments	1	2	0	0	2	0	6	0	0	11
Manufacture of motor vehicles	0	5	0	0	1	0	0	0	0	6
Manufacture of furniture	0	0	0	1	0	0	1	1	0	3
Wholesale and retail trade	0	1	1	0	0	0	2	0	0	4
Transport and communications	1	1	0	1	0	0	1	0	1	5
Computer and related activities	2	1	1	0	0	0	12	0	1	17
Total	11	36	5	4	17	3	53	2	5	136

Table A. 2. Number of non-funded firms by principal activity and region

	Region									
Activity	**Pamp**	**Centre**	**Cuyo**	**Chaco**	**GBA**	**Mesop.**	**Capital**	**NOA**	**Patag**	**Total**
Agriculture, hunting and fishing	1	1	1	0	1	1	1	4	1	11
Forestry, logging, manufacture of wood	0	1	0	0	2	4	2	0	0	9
Services and business activities	0	1	0	0	0	0	1	1	0	3
Manufacture of food products and beverages	4	3	3	0	3	11	5	1	0	30
Manufacture of textiles	0	0	0	1	1	1	7	3	0	13
Manufacture of paper, publishing and printing	1	1	0	0	2	0	3	0	0	7
Manufacture of chemicals	3	3	2	0	13	1	10	0	0	32
Manufacture of rubber and plastics products	4	4	0	1	5	1	6	1	0	22
Manufacture of fabricated metal products	2	4	1	0	12	0	5	0	0	24
Manufacture of machinery and equipment	3	18	0	0	7	0	15	2	1	46
Manufacture of electrical machinery	2	5	0	0	9	0	3	0	0	19
Manufacture of medical, precision and optical instruments	0	1	0	0	1	2	2	0	0	6
Manufacture of motor vehicles	3	5	0	0	1	0	1	0	0	10
Manufacture of furniture	2	1	0	0	2	0	1	1	0	7
Wholesale and retail trade	0	0	9	0	0	0	0	0	0	9
Transport and communications	0	0	0	0	0	0	2	1	0	3
Computer and related activities	0	3	0	0	0	0	23	1	0	27
Total	25	51	16	2	59	21	87	15	2	278

ANNEX 2: ESTIMATION OF THE PROBABILITY OF BEING GRANTED AN ANR PROBIT REGRESSION

Dependent Variable: ANR	Coefficient
Group	-0.672**
	(0.313)
Real Total Sales 1998	-0.000411**
	(-0.000192)
Qualified Labor Share 1998	1.705***
	(0.600)
Obs.	206
Industry Dummies	Yes
Region Dummies	Yes
Pseudo R2	0.2082
Log likelihood	-96.95

Notes: standard errors are shown in parentheses.
**** Significant at the 1% level; ** Significant at the 5% level; * Significant at the 10% level.*

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