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DO R&D SUBSIDIES STIMULATE OR DISPLACE PRIVATE R&D? EVIDENCE FROM ISRAEL

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ABSTRACT

In evaluating the effect of an R&D subsidy we need to know what the subsidized firm would have spent on R&D had it not received the subsidy. Using data on Israeli manufacturing firms in the 1990s we find evidence suggesting that the R&D subsidies granted by the Ministry of Industry and Trade stimulated long-run company-financed R&D expenditures: their long-run elasticity with respect to R&D subsidies is 0.22. At the means of the data, an extra dollar of R&D subsidies increases long-run company-financed R&D expenditures by 41 cents (total R&D expenditures increase by 1.41 dollars). Although the magnitude of this effect is large enough to justify the existence of the subsidy program, it is lower than expected given the dollar-by-dollar matching upon which most subsidized projects are based. This "less than full" effect reflects two forces: first, subsidies are sometimes granted to projects that would have been undertaken even in the absence of the subsidy and, second, firms adjust their portfolio of R&D projects-closing or slowing down non-subsidized projects-after the subsidy is received.

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1 Introduction

Does government policy play a role in influencing the rate and direction of technological change? Most governments appear to believe so. A wide variety of instruments are used by governments to foster technological change: tax cuts, subsidies to R&D, the formation of R&D consortia and national R&D laboratories are but a few examples. In this paper we focus on the relationship between government subsidies to R&D and company-financed R&D in Israel.

The Israeli experience is of interest because its high-tech sector boomed in the course of the last decade, both by national and international standards. Government R&D and innovation policies are perceived as crucial elements of this success story (Trajtenberg ,2000). Yet,

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there is no quantitative assessment of the effectiveness of these policies. This paper attempts to close this gap by focusing on the question: Are R&D subsidies stimulating or displacing company-financed R&D in Israeli manufacturing firms? The lessons learned from the Israeli case should be of interest to countries implementing or contemplating the use of subsidy schemes to promote R&D.[1]

An R&D subsidy can have a direct and an indirect effect on firm performance. The direct effect comes about through the increase in total R&D expenditures, holding company-financed R&D constant. Griliches and Regev (1998) estimate the separate effects of subsidized and company-financed R&D expenditures on output and productivity of Israeli manufacturing firms. Their findings point to significant and, in some cases, very large effects of subsidized R&D on output. The indirect effect operates through the response of company-financed R&D expenditures to the subsidy. If the R&D subsidy displaces own R&D expenditures, the total effect on productivity may be lower than what the Griliches and Regev estimates suggest. On the other hand, if it stimulates own R&D expenditures then the effects of the subsidy are magnified. Thus, an understanding of the relationship between R&D subsidies and company-financed R&D is necessary for a correct assessment of the role of R&D subsidies in boostering productivity.

The precise way in which R&D subsidies are administered is likely to make a difference. In Israel, the largest R&D subsidy program is the one implemented by the Office of the Chief Scientist (OCS) at the Ministry of Industry and Trade. Firms apply for an R&D grant on a project by project basis. All firms intending to export part of the outcome of the R&D project qualify for participation in the program. The vast majority of the subsidies granted represent 50 percent of the agreed-upon R&D budget. Thus, upon approval of the project the firm commits to match, dollar-by-dollar, the subsidy received by the OCS. If the project is commercially successful, the firm pays the subsidy back in the form of royalties. Thus, the grant becomes a loan conditional on the success of the project.

The R&D subsidy can be viewed as lowering the private cost of the project. Receiving the subsidy may therefore turn an unprofitable project into a profitable one to be pursued by the firm. Or it may speed-up the completion of a project already under way. If subsidized R&D involves setting up or upgrading research facilities (labs) then the fixed costs of *other* current and future R&D projects are lowered, increasing their probability of being undertaken. The learning and know-how gained in the subsidized project can also spill-over to *other* current and

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[1] For example, the R&D support given by the ATP in the U.S., by TEKES in Finland, by CDTI in Spain and by the Norwegian government operate in a somewhat similar manner.

future projects thereby enhancing their prospects of success. For all these reasons, the R&D subsidy can stimulate current and future private R&D expenditures.[2]

Indeed, the standard rationale for government support of R&D is rooted in the belief that some form of market failure exists that leads the private sector to underinvest in R&D (Arrow, 1962; Nelson, 1959). To a large extent, underinvestment in R&D occurs because the social benefits from new technologies are difficult to appropriate by the private firms bearing the costs of their discovery, and because imperfect capital markets may inhibit firms from investing in socially valuable R&D projects (Griliches, 1998; Romer, 1990). Publicly supported R&D ought to be augmenting or complementing private R&D efforts. It would therefore be surprising, and contrary to stated goals, if R&D subsidies were to substitute for private R&D.

Yet, some empirical evidence suggests that some substitution between private and government funded R&D does indeed occur. In the U.S., Wallsten (2000) showed that a subset of publicly traded, young, technological-intensive firms, reduced their R&D spending in the years following the award of a Small Business Innovation Research grant, while Busom (2000) finds that in about 30 percent of the Spanish firms in her sample, public funding fully crowds out privately financed R&D. On the other hand, Klette and Moen (1997) conclude that the R&D subsidies were successfully targeted at firms that have significantly expanded their R&D expenditures, and that there is little tendency for crowding out in their sample of high-technology Norwegian firms.[3]

One way to rationalize the possibility of "crowding out" is to argue that government bureaucrats are under strong pressure to avoid the appearance of "wasting" public funds and, therefore, may tend to fund projects with higher success probabilities and with clearly identifiable results, i.e., projects that are likely to have high *private* rates of return. These are projects that could have been financed by the firm either from internal or external funds suggesting that the R&D subsidies are in fact superfluous and may be crowding out private R&D resources. If, however, the funds released by the subsidy are invested in other R&D projects which, because of liquidity constraints, could not have been undertaken before these funds became available, the subsidy may be accomplishing its stated purpose, albeit in an indirect way.

Another channel through which publicly funded R&D projects may crowd out privately financed R&D is through their effect on the price of inelastically supplied R&D inputs (David

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[2] The terms "company-financed", "private" and "own" R&D expenditures are used interchangeably.

[3] David, Hall and Tool's (1999) review of recent studies suggest contradictory results on this issue. Drawing general conclusions is not easy because of the differences in samples and in methodologies among the studies reviewed.

and Hall, 1999). Suppose the subsidy does indeed turn an unprofitable project into a profitable one. Then, if the costs of hiring additional R&D personnel are high, the firm may decide to discontinue a previously profitable project. The commitment to undertake the subsidized project may come on account of other non-subsidized projects. This factor may be of importance in Israel because of the serious shortage of scientists and engineers in some high-tech areas.[4]

It is important to realize that from the firm's point of view, the R&D subsidy eases possible liquidity constraints because it is cheaper to apply for a government subsidy than to raise funds in the capital market. Thus, the firm views the R&D subsidy as a substitute source of financing rather than as a stimulating force to do more R&D. Once a subsidy is received, and the firm commits to undertake the subsidized R&D project, the firm can adjust its portfolio of R&D projects initiating new ones and/or closing old ones. Any analysis of the effect of the subsidy needs to take these changes into account.

As this discussion shows, the crux of the matter for evaluating the effect of the R&D subsidy is to know what the firm would have spent on R&D had it not received the subsidy. This counterfactual information, however, is not available. All the estimation methods used in this paper essentially attempt to estimate the missing expected counterfactual by the mean outcome of some group of firms.

Using data on Israeli manufacturing firms in the 1990s we find evidence suggesting that the R&D subsidies granted by OCS stimulated company-financed R&D expenditures: their long-run elasticity with respect to R&D subsidies is 0.22. At the means of the data, adding one dollar of R&D subsidy increases long-run company-financed R&D expenditures by 41 cents on average. Total R&D expenditures increase, of course, by 1.41 dollars. Although large enough to justify the existence of the OCS subsidy program, the estimated effect is lower than expected given the dollar-by-dollar matching upon which most subsidized projects are based. This "less than full" effect reflects two inherent aspects of the subsidy program: first, subsidies are sometimes granted to projects that would have been undertaken even in the absence of the subsidy and, second, firms adjust their portfolio of R&D projects–closing or slowing down non-subsidized projects–after the subsidy is received.

Section 2 defines what it is that we want to learn about the effects of R&D subsidies. Section 3 describes the main features of the data analyzed in this paper and Section 4 presents

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[4] David and Hall (1999) also identify a set of "second-order" crowding out effects (e.g., firms may decrease their own R&D in publicly funded areas because of anticipated lower returns due to the eventual disclosure of the outcomes of publicly funded R&D projects) which are more relevant to government R&D contracts that pursue specific (socially relevant) R&D projects than to R&D subsidies given to private firms to pursue their own, private, research agenda.

the empirical estimates of the effect of receiving an R&D subsidy on company-financed R&D expenditures. In Section 5, the analysis is extended to the effects of the level of subsidy in the context of a dynamic panel data model. Conclusions close the paper.

2 What is the R&D Subsidy Effect?

As stated in the introduction, this paper examines the effect of R&D subsidies on the level of company-financed R&D expenditures. Specifically, we ask whether receiving a subsidy stimulates or crowds out private R&D expenditures. In this section, we define what it is that we try to measure.

The subsidy scheme in Israel, and in many other countries, is such that firms apply for an R&D subsidy to a specific project. If the project is accepted, the firm must match the level of the subsidy with its own funds (see Section 3 for details). Let $D = 1$ represent the event of receiving a subsidy and let y denote the log of company-financed R&D expenditures. Let y^0 and y^1 be the log of company-financed R&D expenditures when the project is not subsidized ($D = 0$) and when it is subsidized ($D = 1$), respectively.[5]

Suppose subsidies are received at time $\tau > t_0$ and we wish to estimate their impact on time $t_1 (> \tau)$ R&D expenditures, y_{t_1}. The "gain" in company-financed R&D expenditures from receiving a subsidy is $\Delta_{t_1} \equiv y^1_{t_1} - y^0_{t_1}$. We would like to know Δ_{t_1} for each firm because it measures the percentage difference between the observed R&D outlay and the outlay that the firm would have incurred had it not received a subsidy–the "what if" outcome. Knowledge of Δ_{t_1} would answer the question: what is the effect on the firm's private R&D expenditures at t_1 of receiving a subsidy at τ?

Two issues arise when considering the computation of Δ_{t_1}. First, Δ_{t_1} cannot be computed for any firm because data on the counterfactual are missing: for the same firm we observe either y^0 or y^1 but never both variables at the same time. Thus, Δ_{t_1} has to be estimated. Let us therefore assume that, conditional on the firm not having a subsidy at time t_0, receiving a subsidy at τ shifts expected R&D expenditures at t_1 by α. Then,

$$E(y^1_{t_1} | D_{t_1} = 1, D_{t_0} = 0) \equiv E(y^0_{t_1} | D_{t_1} = 1, D_{t_0} = 0) + \alpha$$

and,

$$\alpha = E(y^1_{t_1} - y^0_{t_1} |, D_{t_1} = 1, D_{t_0} = 0) = E(\Delta_{t_1} | D_{t_1} = 1, D_{t_0} = 0) \tag{1}$$

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[5] A drawback of using the binary indicator variable D to estimate the subsidy effect is that it does not reflect the size of the R&D subsidy. The use of logs is motivated, in part, by this scale problem. In Section 5, we also estimate the effect of the *level* of R&D subsidies on the level of own R&D expenditures

Thus, even though we cannot compute the gain Δ_{t_1} for each firm because of the missing counterfactual we can measure an *average* gain for the firms that received an R&D subsidy. This effect is known in the evaluation literature as the "effect of treatment on the treated". It measures the average percentage change in company-financed R&D expenditures between what was actually observed among firms that received a subsidy at time τ and what these firms would have spent had the subsidy not been received.[6],[7]

The estimation problem is that data on firms receiving support identify $E(y^1_{t_1}|D_{t_1} = 1, D_{t_0} = 0)$ but cannot identify the counterfactual $E(y^0_{t_1}|D_{t_1} = 1, D_{t_0} = 0)$. In Section 4 we present different estimates of the parameter α. All the estimation methods essentially attempt to estimate the expected counterfactual by the mean outcome of some group of firms. Doing this requires additional information and assumptions.

The second issue is that of interpretation of the subsidy effect α. In defining α, we implicitly assumed that the firm performs a single R&D project or that y represents R&D expenditures at the project level. In practice, however, firms are simultaneously involved in several R&D projects and the available data are usually on firm-level R&D expenditures, i.e., y comprises expenditures on *all* (subsidized and non-subsidized) R&D projects performed by the firm. Is there anything useful that we can learn from estimates of α based on firm-level R&D data?

In order to answer this question we need to take into account the possibilities of substitution across different R&D projects within the firm. We do this by performing an accounting decomposition that helps to trace out the effect of the R&D subsidy on the expenditures of subsidized and non-subsidized projects. Although not a model in the usual sense, the accounting framework is also helpful in emphasizing various economic factors affecting the performance of the subsidy program and in providing a way to interpret the empirical estimates of Sections 4

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[6] These expectations can be defined conditional on firms' characteristics (e.g., industry affiliation, size, technological area, etc.). The subsidy effect may, therefore, vary with these characteristics.

[7] Another possibility for assessing the effect of the R&D subsidy is by looking at the performance of firms *after* the subsidy has been discontinued. Comparing the R&D expenditures of a firm without a subsidy to the expenditures the firm would have incurred had the subsidy been continued is, however, uninformative regarding the effectiveness of the subsidy program. To see this, assume that the flow of R&D subsidies stops because the project is completed. Thus, comparing the (firm-level) R&D expenditures of a non-subsidized to a subsidized firm reflects the expenditures on other non-subsidized R&D projects. Firms having completed their subsidized projects may now be in a better technological and financial position than before the project was completed. This would stimulate R&D and would imply a positive change in R&D expenditures. Alternatively, they may realize that their efforts are not going to bear fruit and decide to cut-down on their R&D program. This would imply a negative change in R&D. In any case, the effect being estimated is the effect of the outcome of the R&D project, and not the effect of the subsidy itself. Knowledge of what a firm would have done were the subsidy to be continued tells us nothing on whether the subsidized project would have been undertaken in the first place in the absence of the subsidy.

and 5.

Let us assume for analytical convenience that the size of the R&D projects is fixed.[8] The only decision the firm makes is whether to undertake the project or not. The firm has n potential projects each one of size a_i, $i =, 1..., n$. It is convenient to work with R&D expenditures in levels–not in logs–so that a_i and Y are in levels. Company-financed R&D expenditures are

$$Y^0 = \sum_{i=1}^{n} a_i \chi_i^0$$

where χ_i^0 is a binary variable indicating whether project i is undertaken or not when a subsidy is not received.

Assume that the firm applies for a subsidy only to the n^{th} project.[9] If the subsidy is received the cost of the n^{th} project is $a_n = \lambda a_n + (1-\lambda)a_n$ where λ is the subsidized proportion and λa_n is the amount of the subsidy. Company-financed R&D expenditures are

$$Y^1 = \sum_{i=1}^{n-1} a_i \chi_i^1 + (1-\lambda)a_n$$

Note that receiving a subsidy can change the decision to operate any of the first $(n-1)$ projects, and that the subsidized project (project n) is always implemented, $\chi_n^1 = 1$, because of the subsidy contractual agreement. The increase in company-financed R&D expenditures from receiving a subsidy is

$$\widetilde{\Delta} = Y^1 - Y^0 = \sum_{i=1}^{n-1} a_i(\chi_i^1 - \chi_i^0) + (1-\lambda)a_n - \chi_i^0 a_n \tag{2}$$

When is the likely sign of $\widetilde{\Delta}$? Suppose first that the subsidy does not change the decision on the unsupported projects, $\chi_i^1 - \chi_i^0 = 0$ for $i = 1, \ldots n-1$. Then,

$$\widetilde{\Delta} = \begin{cases} (1-\lambda)a_n \text{ if } \chi_n^0 = 0 \\ -\lambda a_n \text{ if } \chi_n^0 = 1 \end{cases}$$

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[8] This is probably not a bad assumption. Indeed, what constitutes a "project" is a moot point as the packaging of R&D activities into projects is not well-defined and strategic considerations may affect this packaging when applying for subsidy support.

[9] Our analysis can be easily extended to cover cases where firms apply for subsidies to more than one project. What we do no have is a model of the firm's decision on which projects to submit for R&D subsidies. Are firms submitting their (privately) best projects for subsidies in the hope of using these funds to finance less atractive projects or other non-R&D activities? An understanding of these issues can help in evaluating the subsidy program and in designing more effective subsidy schemes.

Clearly, $\widetilde{\Delta}$ is positive only when the subsidy causes the subsidized project to be implemented, and $\widetilde{\Delta}$ is negative if the subsidized project would have been undertaken even in the absence of the subsidy. A significantly negative estimate of $\widetilde{\Delta}$ would then mean that the subsidy crowds-out private R&D expenditures, whereas a significantly positive estimate means that the subsidy stimulates private R&D.

When the decision to implement the other non-subsidized projects can change as a result of receiving the subsidy, the mapping between the sign of $\widetilde{\Delta}$ and the effect of the subsidy is not so clear-cut. Without loss of generality, let us assume that only the decision on the $(n-1)^{th}$ project can be changed. Then

$$\widetilde{\Delta} = \begin{cases} 1. \quad a_{n-1} + (1-\lambda)a_n \text{ if } \chi_n^0 = 0 \text{ and } (\chi_{n-1}^1 - \chi_{n-1}^0) = 1 \\ 2. \quad -a_{n-1} + (1-\lambda)a_n \text{ if } \chi_n^0 = 0 \text{ and } (\chi_{n-1}^1 - \chi_{n-1}^0) = -1 \\ 3. \quad a_{n-1} - \lambda a_n \text{ if } \chi_n^0 = 1 \text{ and } (\chi_{n-1}^1 - \chi_{n-1}^0) = 1 \\ 4. \quad -a_{n-1} - \lambda a_n \text{ if } \chi_n^0 = 1 \text{ and } (\chi_{n-1}^1 - \chi_{n-1}^0) = -1 \end{cases}$$

The gain from the subsidy $\widetilde{\Delta}$ is definitely positive when both projects are implemented as a result of receiving the subsidy as in case $1(\chi_n^0 = \chi_{n-1}^0 = 0$ and $\chi_n^1 = \chi_{n-1}^1 = 1)$. This is the best-case scenario: the R&D subsidy turns not only the subsidized project, but also the non-subsidized one, into profitable projects. This may happen when the subsidized project involves setting up or upgrading research facilities lowering the fixed costs of other current (and future) non-subsidized R&D projects. There may also be a spillover of learning and know-how gained in the subsidized project to other current (and future) R&D projects increasing their prospects of success and thereby their profitability. Thus, spillover and "cost-sharing" effects may encourage further R&D expenditures in other non-subsidized R&D projects.

On the other hand, an opposite effect may occur when the firm lacks enough skilled R&D workers or faces liquidity constraints that make it very costly to implement the $(n-1)^{th}$ project along with the subsidized project to which it is committed. The firm may find it profitable to discontinue the non-subsidized project (case 2). Company-financed R&D expenditures may decrease or increase as a result of the subsidy depending on the relative size of both projects.

Cases 3 and 4 involve cases where the subsidized project would have been undertaken even without the subsidy $(\chi_n^0 = 1)$. In this respect, the subsidy is superfluous and this alone contributes a negative amount (equal to the subsidy) to the R&D gain $\widetilde{\Delta}$. If, however, the funds released by the subsidy λa_n are used to implement an additional project which could not have been previously financed because of liquidity constraints (say), and if this project is large enough, then the subsidy effect may become positive. The size of the non-subsidized project (a_{n-1}) may be larger than the subsidy (λa_n) if receiving the R&D subsidy has some signal value that lowers the costs of financing.[10] The last case, where the $(n-1)^{th}$ project is closed down ($\chi^1_{n-1} = 0$) as a result of receiving the subsidy is difficult to rationalize on economic grounds and so we rule it out as a feasible possibility.

In this framework, firms may react differently to the R&D subsidy. The subsidy's impact depends essentially on the budget constraint faced by the firm and on the effects of relaxing it, as described above. This means that the average effect of the subsidy on the subsidized firms–the effect of treatment on the treated–may differ from the average effect of giving an R&D subsidy to a randomly chosen firm (Heckman, 1995).

In light of this discussion, how do we interpret a finding of a positive α? When $\alpha > 0$ the subsidy stimulates company-financed R&D on average, either because new projects that would not have been undertaken without the subsidy are presently undertaken, as in case (1), or because even if some non-subsidized projects are closed-down there is still a positive net effect of the subsidy. α can even be positive when the subsidy is superfluous and the released funds are used to fund a larger project that could not have been implemented before the subsidy funds became available as in case (3).

When $\alpha = 0$, the subsidy does not, on average, displace nor stimulate private R&D expenditures. The firm adjusts its portfolio of R&D projects to accommodate the subsidized project which it is committed to perform. The trade-off between the subsidized and non-subsidized projects balances-off on average. On the other hand, $\alpha < 0$ means that the subsidy is displacing–crowding out–private R&D effort, either because not *all* of the released resources from subsidizing a superfluous project are directed to other R&D project but to other activities such as marketing, production, etc., as in case (3), or because the subsidized project purely crowds out other non-subsidized projects, as in case (2).

Thus, the sign of α gives us information on the qualitative aspect of the relationship between subsidies and private R&D. α is estimated in Section 4.[11] The magnitude of this

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[10] Note also that there may be spillover and cost-sharing effects between the two projects but these cannot be attributed to the subsidy because the subsidized project would have been undertaken anyway.

[11] Note that when $\alpha = 0$, *total* R&D expenditures (private + subsidized) increase by the size of the subsidy, whereas when $\alpha > 0$ ($\alpha < 0$) total R&D expenditures increase by more (less) than the subsidy.

relationship is also of considerable interest. Recall that in most cases the subsidy is matched dollar-by-dollar by the firm. If nothing else changed, we should observe an increase in company-financed R&D expenditures relative to the non-subsidy case equal to the amount of the subsidy provided, of course, that the subsidy is not superfluous. This is probably the rationale behind the subsidy schemes in Israel and in other countries. But things can go “wrong”: the subsidy may be superfluous and/or the firm may adjust its R&D portfolio in response to the subsidy. In Section 5, we estimate the marginal effect of the R&D subsidy.

Note that we restricted ourselves to the effect of the subsidy on the firm’s own R&D expenditures. Subsidies may also carry implications towards other non-R&D activities, both contemporaneously and over time, and, through interfirm spillovers or rivalry channels, subsidies to one firm may have effects on other firms’ R&D activities. These, however, are all *indirect* effects which are not the main goal of the R&D subsidy program (except for its effects on R&D employment). If the *direct* effects on the subsidized R&D project are negative or not significant, the economic justification for continuing with the subsidy program in its present form is considerably undermined even if the indirect effects are quantitatively more important than the direct effects. There are more effective ways of generating the indirect effects than through R&D subsidies.

3 R&D Support in Israel

3.1 R&D Programs

The Israeli government funnels its support of R&D projects through several channels. The most important venue are the R&D grants given by the Office of the Chief Scientist (OCS) at the Ministry of Industry and Trade as mandated by the “Law for the Encouragement of Industrial Research and Development” from 1984.[12] Sixty percent of all government support to R&D is implemented by the OCS (Avnimelech and Margalit, 1999).

Trajtenberg (2000) analyzes the operation of the OCS in detail. The volume of grants administered by the OCS was 120 current million dollars in 1988, it increased steeply up to the mid 1990’s and then leveled off at about 310 current million dollars per year. The per year number of firms applying for subsidies varied between 580 and 780 during 1991-1999, and over 6500 projects were approved since 1995. The OCS approves a firm’s application if the project satisfies some specified criteria based on technological and commercial feasibility. About 70

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[12] The purpose of the law is “to encourage and support industrial R&D in order to enhance the development of local-based industry..., to improve Israel’s balance of trade..., and to create employment opportunities in industry...”.

percent of all applications are approved. In fact, the OCS is mandated by law to subsidize *all* eligible proposals; there is no ranking of the proposals. Moreover, the principle of "neutrality" precludes the OCS to select projects according to fields or any other such considerations.

Grants from the OCS are provided as a percentage of the estimated project-specific R&D expenditures. This percentage varies between 30 and 66 depending on the circumstances. If the goal of the R&D project is to create a new project or industrial process or to make significant improvements in existing ones, the grant is 50 percent of the approved R&D expenditures. If it is just to improve an existing product, the grant is 30 percent. Exceptions to this rule are start-up companies which receive 66 percent of the approved R&D expenditure (up to $250,000 per year) during the initial two years, and firms in "preferred" development areas receiving 60 percent of the approved R&D budget. The vast majority of the projects are supported at 50 percent: essentially, firms match the R&D subsidy dollar-by-dollar.

When a government-assisted R&D project results in a commercially successful product, the developers are obliged to pay royalties. The royalties are a percentage of the revenues derived from the project going from 3 percent during the first three years, to 4 percent over the next three years, and remain at 5 percent in the seventh year and any year thereafter.[13]

The OCS uses the proceeds of the royalties to fund future R&D projects. The share of royalties received out of total grants has been increasing very rapidly from about 10 percent in 1990 to 19 percent in 1995 and 41 percent in 1999, and is therefore becoming a very important element in the OCS annual budget for R&D support.

In addition to the "standard" R&D grant, the OCS also gives grants for the execution of detailed feasibility studies regarding the marketing potential of R&D projects, and also funds the formation of business plans for start-up and young companies based upon the conclusions of the feasibility studies. Grants are also given to assist in the creation of beta-sites (mostly) overseas to test the new product in "real-life" situations.

The OCS and the Israel Center for Research and Development (Matimop) also implement bi-national programs supporting joint projects between companies or individual researchers. Although the most important program is the BIRD (US-Israel Bi-national Foundation), Israel also has bi-national R&D agreements with a number of countries and several agreements with the European Union (e.g., participation in the Eureka network).

Another two channels used by the government to fund R&D activities is through the Magnet Program which supports the establishment of R&D consortia to carry out research in generic pre-competitive technologies, and through the establishment of technological incu-

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[13] In any case, the royalties shall not exceed the amount of the grant plus interest.

bators that enable novice entrepreneurs with innovative concepts to translate their ideas into commercial products. Starting in 1992 the government also proved instrumental in developing venture capital funds that play an increasingly pivotal role in the evolution of the high-tech industry in Israel (Avnimelech and Margalit, 1999)

3.2 Description of the Data

The data used in this paper are a subset of the data analyzed in Griliches and Regev (1999). The dataset is restricted to manufacturing firms doing R&D, i.e., to firms appearing at some point in the Surveys of Research and Development in Manufacturing conducted by the Central Bureau of Statistics during the period 1990-1995. It includes firm-level data on sales, exports, employment, total R&D expenditures, R&D subsidies, and other characteristics on approximately 180-190 R&D-doing firms per year.

The data on R&D subsidies are the data obtained directly from the Survey of Research and Development questionnaire. The survey breaks down the external sources of R&D financing into three categories: 1) grants from the OCS at the Ministry of Industry and Trade, 2) financing from the bi-national Israel-American Fund, and 3) financing from other government sources. We consider all three sources together and label them "R&D subsidies". As mentioned in the introduction, the OCS subsidy program is the largest form of subsidization. During the sample period, grants from the OCS accounted for about 87 percent of all government support.

It is important to realize that the R&D expenditures and R&D subsidy data is at the firm level and may involve one or more projects. Moreover, there is no information in our dataset on firms that applied for subsidies and were denied. These firms cannot be distinguished from those that do not apply for subsidies.[14]

Tables 1-6 describe the main features of the sample data as pertain to R&D subsidies. In Table 1 we observe that company-financed R&D expenditures increased in every year through out the 1990-95 period, even though most of their increase occurred between 1992 and 1993. Their annual rate of growth was 7.2 percent. This pace was matched, on average, by the growth in R&D subsidies at 8.4 percent at an annual rate. As a result, the ratio of R&D subsidies to total R&D expenditures remained stable at about 20 percent.

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[14] The OCS database contains project level information and a list of denied applicants. Regretfully, these data have yet to be matched to the R&D surveys in a coherent manner.

Table 1: Aggregate R&D Expenditures and Subsidies in Manufacturing

Year	Company R&D (1)	Subsidies (2)	Subsidy ratio $(3) = \frac{(2)}{(1)+(2)}$
1990	739.4	188.4	0.20
1991	776.7	206.1	0.21
1992	867.1	198.8	0.19
1993	1029.5	246.1	0.19
1994	1039.2	288.3	0.22
1995	1048.2	281.5	0.21

Figures in millions of 1990 NIS aggregated from firm-level data using sampling weights.

The subsidy ratio in Table 1 does not differentiate among firms receiving and not receiving subsidies. The number of firms with positive R&D in the sample hovers around 165-195 per year and about 60 percent of them receive some kind of subsidy (Table 2).

Table 2: R&D Performers

Year	No. of firms doing R&D	% of firms receiving subsidy	Mean $\frac{\text{subsidy}}{\text{Total R\&D}}$ ratio for firms with subsidy > 0	Mean $\frac{\text{subsidy}}{\text{Private R\&D}}$ ratio
1990	183	59.6	0.31	0.58
1991	196	56.6	0.32	0.63
1992	185	63.2	0.29	0.46
1993	190	59.5	0.27	0.48
1994	186	57.5	0.27	0.57
1995	163	60.1	0.26	0.48

Among the supported firms, the mean subsidy ratio is about 30 percent in the first years of the sample but appears to be declining over time.[15] Median subsidy ratios (not shown) are almost identical to the mean ratios. Subsidized R&D represented, on average, 63 percent of company-financed R&D in 1991, and was down to 48 percent in 1995. Thus, R&D subsidies constitute a significant portion of the R&D effort of manufacturing firms. Evidently, subsidies are not a marginal source of funding.

Table 3 shows that most of the R&D activity in the manufacturing sector is undertaken by subsidized firms, highlighting the role of the OCS in the development of the Israeli high-tech sector. Non-subsidized firms–about 40 percent of all R&D firms–account for only 10-15 percent of total R&D expenditures. The largest share (about 40 percent) of total R&D expenditures corresponds to the 20 percent of all firms that are medianly subsidized.

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[15] Small firms (up to 100 employees) have mean subsidy ratios between 30 and 35 percent, while the mean ratio for larger firms (above 300 employees) is 25 percent.

Table 3: Distribution of Total R&D by Subsidy Ratio

Year	Subsidy Ratio (S)			
	$S = 0$	$0 < S \leq 0.15$	$0.15 < S \leq 0.3$	$0.3 < S$
1990	0.11	0.24	0.38	0.27
1991	0.16	0.25	0.25	0.34
1992	0.16	0.19	0.38	0.28
1993	0.11	0.23	0.46	0.20
1994	0.09	0.18	0.43	0.30
1995	0.07	0.23	0.37	0.33
Class Size	0.41	0.13	0.20	0.26

Class size is the proportion of firms in each subsidy class in all firm-years observations.

Subsidized firms are larger than non-subsidized firms (Table 4). They spend, on average, about 5.5-8.5 millions of 1990 NIS in R&D, and employ around 400 employees. Non-subsidized firms, on the other hand, spend considerably less in R&D–1.5-2.0 millions of 1990 NIS–and employ about half the number of workers than their subsidized counterparts. The differences persist, although less significantly, after controlling for firm size. Although suggestive, these differences are likely to be biased estimates of the subsidy effect because they do not account for the endogeneity of the R&D subsidy (see Section 4).

Table 4: Firm Characteristics by Support Status

Year	Firms with subsidy	Firms without subsidy	Difference
Mean *Own* R&D expenditures			
1990	5609.1	1302.4	4306.7*
1991	5365.2	1815.3	3549.9*
1992	5816.5	2381.9	3434.6*
1993	7337.2	1755.8	5581.2*
1994	7891.4	1403.4	6488.0*
1995	8663.3	1299.7	7363.6*
Mean Employment			
1990	392.0	143.9	248.1*
1991	379.6	147.3	232.2*
1992	370.4	168.3	202.2*
1993	388.6	174.8	213.8*
1994	399.5	196.1	203.4*
1995	442.2	220.6	221.8*
Mean *Own* R&D Expenditures per Worker			
1990	19.3	14.2	5.1*
1991	18.5	18.0	0.4
1992	22.1	15.9	6.2
1993	22.0	11.5	10.6*
1994	24.1	9.1	14.6*
1995	24.6	9.1	15.5*

Own R&D in thousands of 1990 NIS. *Rejects the null hypothesis of equality of means against one-sided alternative at 5% significance level.

R&D subsidies are not distributed equally among R&D performers. Indeed, the distribution of R&D subsidies is highly skewed. Table 5 indicates that the largest firm quartile–around 25-30 firms–receives about 70-80 percent of all subsidies.

Table 5: Distribution of R&D Subsidies by Firm Size

Year	Employment			
	0 – 50	51 – 100	101 – 300	301+
1990	0.06	0.06	0.22	0.66
1991	0.05	0.07	0.21	0.67
1992	0.04	0.06	0.22	0.68
1993	0.04	0.06	0.10	0.80
1994	0.02	0.05	0.12	0.81
1995	0.03	0.03	0.13	0.82
Class Size	0.30	0.18	0.29	0.23

Employment is the number of man-hours. Class size is the proportion of firms in each employment class in all firm-year observations.

On the other hand, small firms–employing less than 100 workers–receive at most 12 percent of all R&D subsidies, even though they represent about half the firms doing R&D. This suggests that the performance of the R&D subsidy program as a whole is tied to the fortunes of these 25-30 firms. It is, therefore, of interest to allow for a differential effect of R&D subsidies by firm size.

Table 6 shows the distribution of R&D subsidies by industry. At first glance it may appear that R&D support is biased towards electronics and chemical firms but, as the numbers in parentheses show, 95 percent of all R&D is performed by firms in these two industries.

Table 6: Distribution of R&D Subsidies (Total R&D Expenditures) by Industry

Year	Electronics	Chemicals	Machinery	Others
1990	0.82 (0.80)	0.17 (0.14)	0.01 (0.03)	0 (0.03)
1991	0.86 (0.80)	0.12 (0.14)	0.02 (0.03)	0 (0.02)
1992	0.85 (0.82)	0.12 (0.14)	0.02 (0.03)	0 (0.01)
1993	0.81 (0.75)	0.17 (0.20)	0.01 (0.03)	0 (0.02)
1994	0.85 (0.77)	0.13 (0.18)	0.02 (0.03)	0 (0.02)
1995	0.83 (0.78)	0.15 (0.17)	0.01 (0.03)	0 (0.01)
Class Size	0.48	0.28	0.13	0.11

Others inlcude the Food, Paper and Printing,Textiles and Light industries.
Class size is the proportion of firms in each industry class in all firm-year observations.

In short, about 60 percent of the R&D performers receive some kind of subsidy, which on average represents 30 percent of the firm's total R&D expenditures and, therefore, constitutes a significant source of funding for R&D projects. Subsidized firms are on average larger (in terms of employment and R&D size) and more R&D intensive than non-subsidized firms, and almost all subsidized firms belong to the Electronics and Chemical industries. About 85 percent of the R&D activity in the manufacturing sector is conducted by firms receiving some R&D subsidy, but the distribution of subsidies is highly skewed with about 75 percent of all the subsidies going to the 20 percent largest firms.

4 The Effect of Receiving an R&D Subsidy

4.1 Simple Difference Estimator

A straightforward approach to estimating α is to argue that mean R&D expenditures of the non-supported firms, $E(y_t^0 | D_t = 0, D_{t-1} = 0)$, may be a reasonable estimate of the counterfactual $E(y_t^0 | D_t = 1, D_{t-1} = 0)$. This implies that an estimator of α could be the simple difference in mean own R&D expenditures by support status

$$\widehat{\alpha}^D = \overline{y}_t^{01} - \overline{y}_t^{00} \tag{3}$$

where the means are taken over the two groups of firms defined by the subsidy status in period t, conditional on *not* having received a subsidy at $t-1$.[16]

Table 7 shows the estimated means of log own R&D expenditures for the two groups of firms (columns (1) and (2)) and their difference in column (3). The subsidy effects are very imprecisely estimated and vary considerably in sign and magnitude over the years.[17]

Table 7: Difference by Support Status

Mean Own R&D Expenditures (number of firms)

Firms without subsidies in year $t-1$

	(1)	(2)	(3)
Year	Firms with subsidy at t	Firms without subsidy at t	$\widehat{\alpha}^D$ (s.e.)
1991	5.75 (11)	6.00 (54)	-0.25 (.51)
1992	6.30 (2)	6.20 (59)	0.10 (1.22)
1993	5.79 (11)	6.18 (40)	-0.39 (.60)
1994	6.57 (8)	5.83 (54)	0.74 (.67)
1995	5.92 (11)	5.92 (51)	0.00 (.54)

Number of firms in parenthese in cols. 1 and 2. Standard errors in parentheses in col. 3.

The R&D subsidy appears to have no significant effect on company-financed R&D expenditures of the supported firms: total R&D expenditures increase by the amount of the subsidy.

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[16] The superscripts denotes the subsidy status in period $t-1$ and t, respectively.

[17] The difference (in sign) between column 3 and the last column in Table 4 is due to the conditioning on last period subsidy status.

This conclusion holds provided the identifying assumptions underlying the simple difference estimator are valid. This qualification begs the question: does this simple procedure give an unbiased estimator of α? The expectation of (3), leaving implicit the conditioning on $D_{it-1} = 0$, is

$$
\begin{aligned}
E(\widehat{\alpha}^D) &= E(y_{it}|D_{it} = 1) - E(y_{it}|D_{it} = 0) \\
&= E(y^1_t|D_{it} = 1) - E(y^0_{it}|D_{it} = 1) + E(y^0_{it}|D_{it} = 1) - E(y^0_{it}|D_{it} = 0) \\
&= \alpha + E(y^0_{it}|D_{it} = 1) - E(y^0_{it}|D_{it} = 0) \qquad (4)
\end{aligned}
$$

The simple difference of means, therefore, identifies α *plus* a potentially non-zero *bias* term reflecting differences in R&D outlays between subsidy recipients and non-recipients. This bias disappears if, conditional on $D_{it-1} = 0$, y^0_{it} is mean independent of D_{it}. That is, if

$$E(y^0_{it}|D_{it} = 1, D_{it-1} = 0) = E(y^0_{it}|D_{it} = 0, D_{it-1} = 0) \qquad (5)$$

Under this assumption, the difference by support status estimator is an unbiased and consistent estimator of α. If the subsidy were randomly assigned to the firms, D would be independent of y^0 by definition, and the bias disappears.

The subsidy, however, is not randomly assigned to firms and we therefore need to question the validity of assumption (5). The identifying assumption (5) means that having received a subsidy does not affect the level of the R&D project the firm would have undertaken had the subsidy not been received. Only in this case, the mean R&D expenditures of the non-supported firms$-E(y^0_{it}|D_{it} = 0, D_{it-1} = 0)-$is an unbiased estimator of the counterfactual level of R&D outlays-$E(y^0_{it}|D_{it} = 1, D_{it-1} = 0)-$the level of R&D expenditures the supported firms would have incurred had the subsidy been removed.

Assumption (5) will hold when there are no common or correlated factors determining the probability of receiving a subsidy and the level of R&D expenditures. Therefore, assumption (5) is overly strong and is bound to fail in the data. As observed in Section 3, the two groups of subsidized and non-subsidized firms differ in many aspects (e.g., in size, in industry affiliation) that are most likely to affect both the level of R&D expenditures directly and the probability of receiving a subsidy. Thus, the difference in mean R&D by support status is not only capturing the causal effect of the subsidy but also part of the effect of the excluded determinants of R&D and D.

For example, if R&D subsidies are biased towards firms in electronics, and in this area R&D expenditures are much larger than in other research fields then the bias term would be positive and the simple difference in means by support status overestimates the casual effect of the R&D subsidy. In the same vein, suppose that liquidity-constrained firms are more likely to apply for–and to receive–an R&D subsidy *and* to tighten their R&D expenditures. Then we would expect the bias term to be negative and the simple difference in means by subsidy status will underestimate the causal effect of the R&D subsidy. It may also be the case that, after realizing that the particular R&D area being explored is unlikely to bear significant fruits, the firm decides to cut down its R&D activities. This would imply that the firm is likely to have its subsidies removed *and* to reduce its own R&D outlays and (3) would overstate the subsidy effect.

In these examples, the independence assumption of R&D expenditures and subsidy support status cannot be sustained. The correlation between subsidies and R&D is not causal; it is due to a third factor affecting both decisions. The examples therefore suggest that a potential alternative to random assignment of the R&D subsidy could be to "control" or "account" for the firm's industry, or for the firm's cash-flow and technological position so as to make R&D expenditures and subsidy support independent, conditional on a set of firm characteristics.

4.2 Simple Difference Estimator Conditional on Covariates

If controlling for firms' characteristics eliminates *all* the differences in potential own R&D expenditures among supported and non-supported firms then the missing counterfactual can be consistently estimated by the mean R&D expenditures of the non-subsidized firms (after controlling for differences in firms' characteristics). This is the "selection on observables" assumption whereby selection into the R&D subsidy program is based on a set of observable variables and possibly on unobserved variables uncorrelated with potential R&D expenditures,

$$E(y_{it}^0|x, D_{it}=1, D_{it-1}=0) = E(y_{it}^0|x, D_{it}=0, D_{it-1}=0) \tag{6}$$

where x is a vector of covariates.

Assumption (6) says that, given x, selection into the subsidy program is not based on variables correlated with y_{it}^0.

To implement this approach we need to compute (3) at each value of x. Assuming linearity of the conditional expectation of y given x and D, an equivalent procedure is to estimate α from

$$y_{it} = x_{it}'\beta + D_{it}\alpha + \varepsilon_{it} \tag{7}$$

with an OLS regression (see the Appendix).

Table 8: Difference by Support Status given Covariates

Firms without subsidies in previous year

Year	No. of firms	$\widehat{\alpha}^D$ (s.e.)
1991	65	-0.55 (.37)
1992	52	-0.82 (1.22)
1993	49	-0.75 (.44)
1994	61	0.65 (.47)
1995	60	-0.41 (.42)

Standard errors in parentheses

Regression includes log employment, log sales and industry dummies.

In Table 8, we include industry affiliation, employment size, and sales to control for the effect on R&D of observable firm characteristics that may be correlated with the probability of receiving an R&D subsidy. The size variables–employment and sales–may capture some of the effect of liquidity constraints.[18] Adding covariates makes the estimated parameters $\widehat{\alpha}^D$ smaller (more negative) and improves their precision, but they remain insignificantly different from zero.

In general, however, there are *unobserved* characteristics that cannot be controlled for which may lead to failure of (6). The technological position of the firm, for example, will fit into this class of variables: it affects R&D expenditures and may also affect the probability of receiving an R&D subsidy. Because R&D subsidies are not randomly assigned to firms, and because it is likely that factors affecting both R&D expenditures and the probability of receiving an R&D subsidy (and its level) remain uncontrolled for, the OLS estimator of α from (7) may not be a consistent estimator of the effect of the R&D subsidy on the supported firms. In order to overcome this identification problem inherent in non-experimental data, we impose restrictions on the process generating the data.

4.3 Difference in Differences (DID) Estimator

A first restriction is to assume that the unobserved characteristics (ε_{it}) potentially correlated with the subsidy status can be decomposed into a firm-specific and time-specific effect. This leads to an error-component specification of ε_{it}. Model (7) becomes,

$$y_{it} = x'_{it}\beta + \alpha D_{it} + \theta_i + \lambda_t + \eta_{it} \tag{8}$$

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[18] Klette and Moen (1997) relate optimal R&D expenditures to expected profitability (proxied by sales) and subsidies. Because employment and sales are highly collinear usually only one of the regressors comes in positive and significant. The sum of the estimated coefficients hovers around 0.7-0.8.

where θ_i is the firm-specific effect , λ_t is a time-specific component common to all firms, and η_{it} is an *i.i.d.* zero mean random variable assumed to be uncorrelated with x_{it}.

Applying model (8) to firms without a subsidy at $t-1$, $D_{it-1} = 0$, and taking first differences to remove firm-specific effects results in,

$$\Delta y_{it} = \Delta\lambda_t + \Delta x'_{it}\beta + \alpha D_{it} + \Delta\eta_{it} \tag{9}$$

which relates the growth rate in own R&D expenditures to the growth rates in the observed and unobserved covariates and the subsidy dummy.

From (9) it follows that

$$\begin{aligned} E(\Delta y_{it}|\Delta x, D_{it} &= 1, D_{it-1} = 0) - E(\Delta y_{it}|\Delta x, D_{it} = 0, D_{it-1} = 0) \qquad (10) \\ &= \alpha + E(\Delta\eta_{it}|\Delta x, D_{it} = 1, D_{it-1} = 0) - E(\Delta\eta_{it}|\Delta x, D_{it} = 0, D_{it-1} = 0) \end{aligned}$$

It is clear now that, conditional on Δx_{it} and on $D_{it-1} = 0$, the expected difference between the *growth rates* of subsidized and non-subsidized firms identifies α provided $\Delta\eta_{it}$ is mean independent of D_{it},

$$E(\Delta\eta_{it}|\Delta x, D_{it} = 1, D_{it-1} = 0) = E(\Delta\eta_{it}|\Delta x, D_{it} = 0, D_{it-1} = 0) \tag{11}$$

The difference between assumptions (5) and (11) is that the latter allows for firm-specific unobserved effects θ_i (e.g., unobserved managerial skills or time-invariant efficiency levels) and economy-wide shocks λ_t to affect both the level of company-financed R&D expenditures and the support status of the firm. We can do this because the additivity assumption in (8) implies that "same-firm" differences eliminate the firm-effects terms while "same-period" differences eliminate the time-effects from the bias. In other words, the panel features of the data and the error component assumption permit us to relax the selection on observables assumption to allow for correlation between (time-invariant) firm-specific and time-specific effects and the subsidy dummy variable D.

This issue is closely related to the bias in the estimation of α attributed to the self-selection of firms into the application process of the R&D subsidy program (Busom, 2000). Not all firms apply for a subsidy. Firms decide to apply for a subsidy on the basis of their expected profitability of applying relative to not applying. Projects satisfying a technological and commercial feasibility criterion are eligible for a subsidy and, in fact, the OCS is mandated by law to subsidize *all* eligible proposals. Seventy percent of all applications are approved. As explained in Section 3, the procedure is non-competitive so that there is no ranking of the proposals. Because grants are repaid only if success is achieved, it is possible that firms working in riskier R&D areas will be more likely to apply for subsidies. For this and other reasons, firms receiving an R&D subsidy do not constitute a random sample of firms from the population of R&D doers. Because the characteristics that make a firm a recipient of an R&D subsidy are likely to be correlated with the determinants of own R&D effort, we need to control for this source of correlation.[19]

DID with covariates goes some ways towards solving this problem. First, it accounts for common observed covariates affecting the decisions to apply for a subsidy, to be granted one, and to do R&D. Second, it also takes account of permanent (time-invariant) differences between successful and unsuccessful applicants, and non-applicants. Thus, if one believes that part of the self-selection mechanism works through the observed covariates (e.g., industry, size) 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 during the sample period (such as the degree of risk in the R&D area in which the firm is involved), then the DID estimator is an acceptable estimation procedure. DID fails, however, to control for idiosyncratic factors affecting simultaneously the level of R&D expenditures and the probability of receiving a subsidy.[20]

The difference-in-difference (DID) estimator is the sample version of (10),

$$\widehat{\alpha}^{DID} = \left(\overline{y}_t^{01}(x_t) - \overline{y}_{t-1}^{01}(x_{t-1})\right) - \left(\overline{y}_t^{00}(x_t) - \overline{y}_{t-1}^{00}(x_{t-1})\right) \tag{12}$$

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[19] We only observe recipient and non-recipient firms in the data. A recipient firm is one that applied and was accepted to the subsidy program, the non-recipient firm may have applied for a subsidy and been denied or may not have applied at all. The data at hand do not allow us to distinguish between denied applicants and non-applicants.

[20] This approach handles the problem caused by the notion that more "successful" firms may be receiving more R&D subsidies and doing more R&D. If the "success" profile of the firm is more or less constant during the sample period then it differences-out in the *DID* estimator. If, however, receiving an R&D subsidy is associated with the unexpected development of a particularly good idea which also leads to more R&D expenditures, then the *DID* estimator is likely to be upward biased (more on this in Section 5).

where the means are taken over the two groups of firms defined by the subsidy status in period t, conditional on the subsidy status at $t-1$ and on the value of x.

The DID estimator equals the difference between the mean R&D change between $t-1$ and t among the supported and non-supported firms, conditional on x. In Table 9 we present first the DID estimates assuming $\beta = 0$. Because of its simplicity this estimator is widely used in practice.[21]

Table 9: DID Estimates of α
Mean Own R&D Expenditures (number of firms)

Firms without subsidies in $t-1$

Year	$D_{it} = 1$	$D_{it} = 0$	$\widehat{\alpha}_{01}^{DID}$ (s.e)
1990	6.02 (11)	6.18 (54)	
1991	5.75 (11)	6.00 (54)	
Difference	-0.27	-0.18	**-0.09** (.27)
1991	6.84 (2)	6.25 (59)	
1992	6.30 (2)	6.19 (59)	
Difference	-054	-0.06	**-0.48** (.47)
1992	5.95 (11)	6.39 (40)	
1993	5.79 (11)	6.18 (40)	
Difference	-0.16	-0.21	**0.05** (.26)
1993	6.55 (8)	5.82 (54)	
1994	6.57 (8)	5.83 (54)	
Difference	0.02	0.01	**0.01** (.27)
1994	6.17 (11)	5.97 (51)	
1995	5.93 (11)	5.92 (51)	
Difference	-0.24	-0.05	**-0.19** (.33)

The sample in each pair of years includes firms with positive R&D in both years.

The sample in each pair of consecutive years consists of all firms not receiving a subsidy during year $t-1, D_{it-1} = 0$, and having positive R&D expenditures in *both* years. This requirement ensures that the difference in the averages equals the average of the firm-specific differences.[22] For example, 65 firms doing R&D in 1990 and 1991 did not receive a subsidy in 1990 (out of a total of 183 firms doing R&D). Eleven of them received a subsidy in 1991, and reduced their own R&D expenditures by 27 percent on average. The average reduction in own R&D expenditures among the remaining 54 firms was 18 percent. Thus, if the identifying assumptions hold, the R&D subsidy caused a decrease of 9.0 percent in own R&D outlays.

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[21] These estimates were computed from a least squares regression of Y_t on a firm-dummy, a year t dummy, and D_{it} for each pair of consecutive years; equation (8) imposing $\beta = 0$.

[22] Strictly speaking, this requirement is unnecessary. Some firms that did R&D and did not receive a subsidy in year $t-1$ stopped doing R&D the next year. Because the log of zero is not defined we excluded these firms from the averages in *both* years. Including them in the average of the first year, or setting their R&D expenditures in the second year to a very small positive number does not change the qualitative nature of the results.

Repeating this exercise for every pair of consecutive years produces widely varying estimates of the subsidy effect. Most estimates, however, are negative while the positive ones are very small in magnitude. All of them are very imprecisely estimated. This is not surprising given the small number of transitions in the data. For example, in 1992 only 2 firms received an R&D subsidy out of the 61 firms that did not receive any subsidies in 1991.

In the more reasonable case of $\beta \neq 0$, the estimates in Table 9 can be justified only when the covariates do not vary much across the two groups of firms. If the values of the covariates differ among the two groups, and this is ignored, the DID estimator picks up the correlation between these covariates and D.

A convenient way to control for different values of x, is to estimate α from (8) in two consecutive years, using panel data estimation methods. Specifically, we need data for two periods, a "pre-treatment" period and a "post-treatment" period. A fixed-effects estimator of (8) generates consistent estimators of β and α provided the identifying assumptions hold.[23] To improve the precision of the estimator we pool the data over the 5 years of data in the sample (assuming time-invariant parameters) using only observations for which $D_{it-1} = 0$. Table 10 presents these results.

Table 10: Pooled DID Estimates of α, 1990-95

Equation (8)

	(1)	(2)	(3)	(4)
$D_t = 1$	-0.194 (.156)	-0.192 (.159)	-0.167 (.159)	-0.299** (.168)
$D_{t-1} = 1$	–	–	–	0.378* (.190)
Emp.	–	0.154 (.195)	0.146 (.204)	-0.108 (.239)
Sales	–	–	0.224 (.158)	0.285 (.208)
Within R^2	0.040	0.042	0.052	0.087
N (*firms*)	326 (136)	324 (136)	319 (134)	214(103)

Standard errors in parentheses. Year dummies included. A * (**) indicates different from 0 at 5% (10%) significance level.

In cols. (1)-(3) we use only firms with $D_{it-1} = 0$, while in col. (4) we use only firms with $D_{it-2} = 0$.

In columns (1)-(3), the subsidy effect comes in negative and of considerable size, about -19 percent. This estimate is, by and large, consistent with the results in Table 9. Even though pooling the data gives more precise estimates, they are still insignificantly different from zero at conventional significance levels. Not surprisingly, larger firms spend more on R&D expenditures but the size elasticity of R&D is well below unity implying that company-financed R&D by employee decrease with the size of the firm.[24]

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[23] In section 5, we present GMM estimators of the paramteres based on the first-difference equation.

[24] Note also that the low $R^{2\prime}s$ indicate that most of the within-firm variation in own R&D expenditures is largely unrelated to changes over time in scale.

Usually (subsidized) R&D projects are conducted over a number of years and the grant money is transferred to the firm accordingly. This means that the event "$D_{it} = 1, D_{it-1} = 1, \ldots, D_{it-q} = 1$" is probably a more accurate representation of the event "receiving a subsidy" than the single indicator D_{it}. All this suggests that lags of D_{it} should be added to model (8) in order to capture the full effect of the subsidies. Column (4) adds one lag of the subsidy indicator to the regression. The results change dramatically. Subsidies do indeed displace private R&D expenditures by 30 percent but this substitution is completely reversed later on. When the actual implementation of the subsidized project is delayed or spans more than one year, the firm uses the subsidy funds to substitute for private funds during the first year of the project–when the project is being run at a slow pace–but then compensates with increased private funds as the project's pace picks up. Overall, however, the level of private R&D expenditures is not significantly changed from what it would have been in the absence of the subsidy: the sum of both subsidy coefficients is positive but small and not significantly different from zero (8 percent with a standard deviation of 75 percent).

Allowing for a differential effect of the subsidy on the R&D expenditures of firms with more than 300 employees (about 20-25 percent of the sample in any given year) does not affect these results: the "large firm" dummies are individually and collectively not significantly different from zero.

Along with the timing explanation, the large reduction in the estimated coefficient of D_t when D_{t-1} is added to the regression is consistent with a model where current R&D expenditures respond (positively) to the know-how generated by past R&D projects. In this case, η_{it} in (8) is positively correlated with lagged subsidies implying that, given some persistence in the subsidy status D, the estimates of the subsidy effect in columns (1)-(3) of Table 10 are biased upward. When D_{t-1} is included in the regression the coefficients of D_t should be reduced as in column (4) of Table 10. This type of model, however, also implies that lagged company-financed R&D ought to be added to the regressions in Table 10. This is done in the next section where we formulate a dynamic panel data model and estimate the full effects of the R&D subsidy on company-financed R&D expenditures.

5 Dynamics and the Effect of the Subsidy Level

We start with an extension of model (8) using q lags of s, the log of the amount of subsidies,

$$y_{it} = x'_{it}\beta + \sum_{\tau=0}^{q} \alpha_\tau s_{it-\tau} + \theta_i + \lambda_t + \eta_{it} \tag{13}$$

Besides adding dynamics, this section abandons the use of the binary indicator D in favor of the *level* of the R&D subsidy. If the effect of the subsidy kicks in only after a certain subsidy level is attained, using a binary variable D might miss this type of effect.

The motivation for focusing on the dynamic effects of the R&D subsidy is that subsidies can affect future expenditures on other R&D projects for a number of reasons mentioned in Section 2. First, R&D can increase because of within-firm spillovers and the sharing of common R&D equipment and other fixed costs. Second, when the subsidy is superfluous the private R&D funds released by the subsidy can be invested in future R&D projects. Third, receiving a subsidy may signal good news leading to lower costs of financing other R&D projects. On the other hand, more R&D activity in the present may slow down future projects if the firm is facing steep R&D costs. The net impact of these effects on future R&D expenditures is captured by the lagged subsidy terms in (13).

At the end of Section 4 we mentioned an additional channel through which subsidies can have dynamic effects. The know-how obtained from the firm's R&D projects accumulates over time making the firm better at doing R&D in a learning-by-doing sense: the productivity of R&D increases. Moreover, the manner in which R&D is financed may matter. If projects were subsidized by the OCS in the past, firms may find it cheaper to finance new R&D projects in the future because of the signal value associated with OCS support. OCS support enhances the firm's R&D reputation and this lowers financial costs. Thus, the level and composition of R&D expenditures today can have lasting effects on future R&D expenditures.

Let ω_{it} represent the unobserved know-how and R&D reputation of the firm which is a part of η_{it}, $\eta_{it} = \omega_{it} + \xi_{it}$ where ξ_{it} is a true *i.i.d.* idiosyncratic zero mean shock, and ω_{it} is determined in part by past R&D expenditures as follows,

$$E(\omega_{it}|s_{it-1}, y_{it-1},\ldots) = \sum_{\tau=1}^{p} \beta^{s}_{\tau} s_{it-\tau} + \rho_{\tau} y_{it-\tau} \tag{14}$$

with the parameters β and ρ presumably non-negative. In practice we will set $p = q$ and therefore write,

$$y_{it} = x'_{it}\beta + \sum_{\tau=0}^{q} \delta_{\tau} s_{it-\tau} + \sum_{\tau=1}^{q} \rho_{\tau} y_{it-\tau} + \theta_i + \lambda_t + v_{it} \qquad t = q+1, \ldots, T_i \tag{15}$$

$$\text{with } \delta_{\tau} = \begin{cases} \alpha_0 & \tau = 0 \\ \alpha_{\tau} + \beta^{s}_{\tau} & 1 \leq \tau \leq q \end{cases}$$

and $v_{it} = \omega_{it} - E(\omega_{it}|s_{it-1}, y_{it-1},\ldots) + \xi_{it}$ has zero mean.

The coefficients δ reflect two types of subsidy effects: the *direct* effects of the subsidy mentioned in Section 2 and captured by the $\alpha's$, and the *indirect* effects operating through the know-how and reputation acquired by the firm reflected in the $\beta's$. We can only identify the total subsidy effect $\delta = \alpha + \beta$. Without additional information on the relative size of β and ρ, we cannot hope to decompose the subsidy effects into its direct and indirect components. However, in order to assess the effectiveness of the subsidy program, knowledge of δ suffices.

We assume that v_{it} is independently distributed across firms but can have arbitrary heteroskedasticity across firms and time. Importantly, we also assume that v_{it} is serially uncorrelated within the firm. The idiosyncratic term ξ_{it} is assumed to be uncorrelated with current and lagged variables in the model and therefore v_{it} is also uncorrelated with lagged private and subsidized R&D expenditures,

$$E(v_{it}|s_{it-1}, y_{it-1},...) = 0 \quad \text{for } t \geq 2 \tag{16}$$

The additional covariates x_{it} are assumed to be predetermined in the sense that

$$\begin{aligned} E(x_{is}v_{it}) &= 0 \quad s \leq t \\ &\neq 0 \quad s > t \end{aligned} \tag{17}$$

Taking first-differences in (15) to get rid of the firm-specific effects results in,

$$\Delta y_{it} = \Delta x_{it}'\beta + \sum_{\tau=0}^{q} \delta_\tau \Delta s_{it-\tau} + \sum_{\tau=1}^{q} \rho_\tau \Delta y_{it-\tau} + \Delta\lambda_t + v_{it} - v_{it-1} \quad t = q+2, ..., T_i \tag{18}$$

OLS estimates of (18) will be inconsistent because y_{it-1} and possibly x_{it} are correlated with v_{it-1}. The identifying assumptions (16) and (17) guarantee that subsidies and own R&D expenditures lagged two or more periods, $s_{it-2}, y_{it-2}, ..., s_{i1}, y_{i1}$ are orthogonal to $v_{it} - v_{it-1}$. Thus, levels of s and y lagged two or more periods can serve as instruments. In addition, the predetermined covariates $x_{it-1}, ..., x_{i1}$ are also valid instruments in the differenced equation for period t. The validity of all these instruments rests on the assumption that v_{it} is serially uncorrelated. To ensure that this condition is met, we will test for serial correlation of the first-differenced residuals.

It is also possible that current subsidies s_{it} are correlated with v_{it} because there may still be some correlation left between firm-specific but time-varying unobservables (ξ_{it}) and the subsidy variable. For example, a firm unexpectedly comes up with a new idea for an R&D

project that leads it to spend more resources on R&D and to request more subsidies. Then, if this event cannot be controlled for, the subsidy variable s_{it} is correlated with v_{it}. We will use the firm's export history (log of exports) as an instrument for s_{it}. Subsidies are given to firms intending to export the outcomes of the subsidized R&D project. This "intention" is likely to be correlated with past export experience, but this experience is probably not correlated with unpredicted effects on R&D activities such as the arrival of "new ideas".

We estimate the parameters in (15) by GMM (see Arellano and Bond, 1991), and report only one-step estimates for which inference based on its heteroskedasticity robust estimated asymptotic variance matrix is generally thought to be more reliable than the one based on the more efficient two-step GMM estimator (Blundell and Bond, 1998). The data consist of all firm-year observations having non-missing data on own R&D, subsidies, employment, sales, and exports.[25] Table 11 presents results for $q = 1$.

Table 11: Estimates of the Subsidy Level Effect

$q = 1$

	(1)	(2)	(3)	(4)
Coefficient of	OLS	Within	First Δ	First Δ
y_{t-1} (ρ_1)	0.779* (.035)	0.082 (.050)	-0.029 (.140)	-0.003 (.123)
s_t (δ_0)	-0.006 (.017)	-0.024 (.021)	-0.080 (.064)	-0.119** (.063)
s_{t-1} (δ_1)	0.054* (.016)	0.044* (.016)	-0.089 (.057)	-0.061 (.051)
Employment	0.139* (.053)	0.233** (.141)	0.202 (.321)	0.243 (.297)
Sales	0.053 (.044)	0.130* (.052)	-0.222 (.140)	-0.181 (.131)
m_1	-2.87	-3.96	-0.96	-1.29
m_2	0.75	-0.26	-0.75	-0.52
Instruments	–	–	*A*	*B*
Sargan test $(p - value, df)$	–	–	0.80 (35)	0.91 (45)
N $(firms)$	766 (221)	545 (193)	545 (193)	545 (193)
Years	1991-1995	1992-1995	1992-1995	1992-1995

Industry and year dummies included. Asymptotic robust standard errors in parentheses.

$A=\{y_{t-2},...,y_1,s_{t-2},...,s_1,emp_{t-2},...,emp_1,sales_{t-2},...,sales_1,YD's,ID's\}$

$B=A\cup\{exp_{t-2},...,exp_1\}$.

A * (**) indicates different from 0 at 5% (10%) significance level. m1 and m2 are asymptotically N(0,1) tests for first-order and second-order serial correlation of first-differenced residuals except in the OLS regression where the residuals are in levels.

Column (1) presents OLS estimates of model (15). These estimates show small, but significant, lagged subsidy effects. Note that the autoregressive parameter ρ_1 is quite high indicating, as expected, a lot of persistence in the R&D series. These estimates, however, are likely to be biased upward because of the presence of firm-specific effects.

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[25] We use DPD98 for Gauss (see Arellano and Bond, 1998). All variables are in logs and in cases where the original variable was zero the log of that variable was set to zero.

In column (2), these firm-effects are removed using a (fixed-effect) within estimator. Estimated subsidy effects are indeed a bit smaller. The estimated autoregressive parameter ρ_1 is now, however, too small (and not significantly different from zero) as expected due to the downward bias in the fixed-effect estimator of ρ_1 for small T and positive ρ_1 (Nickell, 1981).

To correct for this inconsistency and for the potential endogeneity of the subsidy regressors, column (3) presents GMM estimates of the parameters of interest using own R&D, the level of subsidies, employment and sales lagged two or more periods as instruments for y_{t-1}, s_t and s_{t-1}. Subsidy effects are estimated to be more negative but still not significantly different from zero. The Sargan test of over-identifying restrictions indicates that the instruments are valid. Adding the firm's exporting history to the list of instruments in column (4) improves the validity of the instruments and gives marginally significantly negative estimates of the contemporary subsidy effect.

The results in columns (3) and (4) are problematic because the estimated autoregressive parameter ρ_1 is even smaller that the one obtained by fixed-effects. In addition, the low values of m_1 (and m_2) indicate that first differenced residuals are serially independent suggesting that v_{it} is a random walk. This is taken as evidence of a misspecified model.

In the context of GMM estimation of short panels there is, however, another explanation. Recently, Blundell and Bond (1998) showed that when ρ_1 is moderately large and T is moderately small, the GMM estimator based on the first-differenced equation can be seriously biased and, in particular, the estimator of ρ_1 is downward biased. When the y series is very persistence, lagged levels of the series are only weakly correlated with the first differences: there may be a problem of "weak instruments". This would explain the low estimated ρ_1. Blundell and Bond (1998) suggest using *additional* moment restrictions–the orthogonality between $(\theta_i + v_{it})$ and $y_{it-1} - y_{it-2}$—which is valid under stationarity of the initial conditions process y_{i1}. In practice, we add equations for the level of y_{it} using the first differences of y and other regressors as instruments to the level equations. Table 12 implements this approach.

Table 12: GMM Estimates of the Subsidy Level Effect
First Differences and Level Equations

Coefficient of	(1)	(2)	(3)
y_{t-1} (ρ_1)	0.449* (.086)	0.428* (.094)	0.454* (.103)
y_{t-2} (ρ_2)	–	0.085 (.060)	0.097 (.061)
s_t (δ_0)	0.034 (.061)	-0.078 (.057)	-0.076 (.058)
s_{t-1} (δ_1)	0.002 (.047)	0.114* (.044)	0.049 (.060)
s_{t-2} (δ_2)	–	0.061** (.033)	0.125* (.049)
Employment	0.703* (.234)	0.397 (.283)	0.224 (.315)
Sales	-0.099 (.116)	0.182 (.246)	0.227 (.252)
Net effect (p-value)	0.036 (.110)	0.097* (.023)	0.098** (.061)
m_1	-5.85	-3.72	-3.63
m_2	0.00	-1.61	-1.95
Instruments	*C*	*C*	*D*
Sargan test (p-value,df)	0.29 (65)	0.08 (58)	0.27 (45)
N (firms)	738 (193)	495 (143)	495 (143)
Years	1991-1995	1992-1995	1992-1995

Unweighted data. Industry and year dummies included. Asymptotic robust standard errors in parentheses.

A * (**) indicates different from 0 at 5% (10%) significance level.

$C=\{y_{t-2},...,y_1,s_{t-2},...,s_1,emp_{t-2},...,emp_1,sales_{t-2},...,sales_1,$
$exp_{t-2},...,exp_1,YD's,ID's\}\cup\{\Delta y_{t-1},\Delta s_{t-1},\Delta emp_{t-1},\Delta sales_{t-1}\}$

$D=\{y_{t-2},...,y_1,emp_{t-2},...,emp_1,sales_{t-2},...,sales_1,$
$exp_{t-2},...,exp_1,YD's,ID's\}\cup\{\Delta y_{t-1},\Delta emp_{t-1},\Delta sales_{t-1}\}$

m1 and m2 are asymptotically N(0,1) tests for first-order and second-order serial correlation of first-differenced residuals.

The autoregressive coefficients are sensible in all regressions and the serial correlation tests suggest the presence of first-order serial correlation only in the first-differenced residual, as expected when v_{it} is serially independent. When using two lags, the list of instruments C is only marginally valid possibly because lagged subsidies are included.[26] Excluding them from the instrument list (for the differenced and level equations) gives a more satisfactory set of instruments according to the Sargan test (list D in column (3)).

The lagged subsidy coefficients change considerably relative to those in Table 11: they are now positive and more precisely estimated. The immediate impact of the subsidy remains negative (columns (2) and (3)) or very small, column (1). The sum of the subsidy coefficients is about 0.10 and significantly different from zero. Adding a third lag of y and s results in the same point estimate of the net effects (about 0.11) but is less precisely estimated. [27] Note that

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[26] The form the instruments take are levels lagged two or more periods for the first differenced equations, and lagged first-differences for the level equations.

[27] The third lag of s and y is not significant.

the pattern of the coefficients resembles the pattern in column (4 of Table 10 except that now the net effect is significantly positive.

The long-run effect of the subsidy takes account of the autoregressive component in y and is approximately equal to $\frac{0.098}{1-0.454-0.097} \approx 0.22$. That is, a 10 percent increase in the current subsidy level leads to an increase of 2.2 percent in long-run company-financed R&D expenditures. Government subsidies did not crowd out private R&D expenditures in the long-run. On the contrary, private expenditures were increased by the subsidy. Dividing 0.22 by $\frac{s}{y}$ gives the marginal effect of a subsidy dollar. Using the average subsidy-private R&D ratio in the data–about 54 percent (Table 2)–we obtain a marginal effect of 0.41. [28]That is, an extra subsidy dollar increases company-financed R&D expenditures by 41 cents on average in the long-run. We cannot reject the hypothesis that large firms have the same subsidy effect as small firms (p-value 0.17) but large firms have, on average, a smaller $\frac{s}{y}$ ratio (38 percent). Thus, an additional dollar of R&D subsidy to large firms increases their long-run private R&D expenditures by 58 cents on average. In this sense, the subsidy program elicits a stronger response from large than from smaller firms.

Although significantly positive, the subsidy effect is substantially lower what could have been expected a-priori given the dollar-by-dollar matching upon which most subsidies are based. The reasons for this "less than full" effect lie in that the subsidies are sometimes granted to projects that would have been undertaken even without the subsidy, and in that firms adjust their portfolio of R&D projects–closing or slowing down non-subsidized projects–after the subsidy is received.[29]

6 Conclusions

This paper analyzes the effects of R&D subsidies on company-financed R&D using data on Israeli manufacturing firms during 1990-1995. The R&D subsidy effect is defined as the average

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[28] We use a log-log model because it allows for variation in the marginal effect whereas a linear model assumes the same effect at any level of subsidies.

[29] If we assume that the funding source of R&D does not affect the know-how and reputation state ω then $\beta^s_\tau = \rho_\tau$, and we find, in general, that $\widehat{\alpha}_\tau = \widehat{\delta}_\tau - \widehat{\rho}_\tau$ is significantly negative. Were we to assume that subsidized R&D is half as productive as private R&D, $\beta^s_\tau = \frac{1}{2}\rho_\tau$, we would still find negative $\alpha's$. These results, albeit crude, suggest that R&D subsidies work in subtle ways: on the one hand they crowd out company-financed R&D expenditures–the negative $\alpha's$–possibly because in some cases the subsidies are superfluous while in other instances the implementation of the subsidized project forces the firm to close or scale down other non-subsidized projects. But, on the other hand, the know-how obtained from R&D projects to which the subsidies contribute and the R&D reputation boosted by the OCS support have strong effects on future company-financed R&D expenditures. Overall, the effect on company-financed R&D is positive. This suggests that empirically identifying the channels through which subsidies affect R&D activities can be an interesting and fruitful research project.

percentage change in company-financed R&D expenditures between what was actually observed among firms that received a subsidy and what these firms would have spent had the subsidy not been received.

We explore a variety of channels through which the subsidy operates. Some of these channels affect private R&D positively and some affect it negatively. For example, the subsidy can turn a privately unprofitable project into a profitable one and, through spillovers and cost-sharing effects, it may even prompt the firm to implement additional non-subsidized projects. On the other hand, the commitment to implement the subsidized project may crowd-out other previously profitable projects when the firm is constrained by its stock of skilled R&D labor or by liquidity considerations. Subsidizing projects that would have been undertaken by the firm even in the absence of the subsidy contribute to a negative subsidy effect. But, even if the subsidy is superfluous, the funds released by the subsidy can finance other R&D projects so that the subsidy only partially crowds-out private R&D expenditures. The paper focuses on estimating the net effect of the subsidy on private R&D; the distribution of these effects through the different channels is left for future research.

The first empirical approach borrows from the "treatment effect" literature using a DID estimator to estimate the effect of receiving an R&D subsidy. This results in positive but insignificant subsidy effects on private R&D. These results also highlight the importance of dynamics in the effect of R&D subsidies on private R&D as discussed in Section 2. The DID approach, although very clear in its interpretation of the parameters and in its capability of handling selection biases, is less well suited to tackle the estimation of dynamic models. In Section 5, we estimate a dynamic panel data model which takes account of the endogeneity of current subsidies. After some diagnostic tests are passed, the final specification implies that OCS subsidies do not crowd out company-financed R&D expenditures. Their long-run elasticity with respect to R&D subsidies is 0.22. At the means of the data, an additional dollar of R&D subsidy increase company-financed R&D expenditures by 41 cents on average in the long-run. Total R&D expenditures increase, of course, by 1.41 dollars.

This is a large effect. Even though it is large enough to justify the existence of the OCS subsidy program, the subsidy effect is lower than expected given the dollar-by-dollar matching upon which most subsidized projects are based. This "less than full" effect reflects two inherent aspects of the subsidy program identified in the paper: first, subsidies are sometimes granted to projects that would have been undertaken even in the absence of the subsidy and, second, firms adjust their portfolio of R&D projects–closing or slowing down non-subsidized projects–after the subsidy is received. These considerations should be taken into account when formulating a coherent R&D subsidy policy.

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A Appendix

In this appendix we show the equivalence in estimating α between the standard linear econometric model and the difference by support status.

Assume that R&D expenditures for non-supported and supported firms at t satisfy

$$y_{it}^0 = x_{it}'\beta + \varepsilon_{it}^0 \qquad\qquad y_{it}^1 = x_{it}'\beta + \delta_i + \varepsilon_{it}^1 \tag{19}$$

where the error terms are assumed uncorrelated with x, a $kx1$vector of firm characteristics, $E(\varepsilon^0|x) = 0$ and $E(\varepsilon^1|x) = 0$.[30] Note that the subsidy impact is allowed to differ across firms.

Following Heckman's (1995) general treatment of this issue, observed R&D expenditures of firm i in the group of firms defined by $D_{it-1} = 0$ can be written as

$$y_{it} = D_{it}y_{it}^1 + (1 - D_{it})y_{it}^0 = x_{it}'\beta + D_{it}(\delta_i + \varepsilon_{it}^1 - \varepsilon_{it}^0) + \varepsilon_{it}^0$$

which resembles a regression model with a random coefficient for the regressor D.

The effect of the subsidy–the coefficient of D_{it}–varies across firms and time depending on $\varepsilon_{it}^1 - \varepsilon_{it}^0$ and δ_i. When the error term is the same in both subsidy states, $\varepsilon_{it}^1 - \varepsilon_{it}^0 = 0$ and the subsidy effect is homogeneous, the effect of the subsidy is always the same for all firms and in all years. Then the model reduces to the standard dummy-variable framework used in most applied work.

Let $v_{it} = \varepsilon_{it}^1 - \varepsilon_{it}^0$ and $\delta_i = \overline{\delta} + \xi_i$. Observed R&D can then be written as

$$\begin{aligned} y_{it} &= x_{it}'\beta + D_{it}\overline{\delta} + \varepsilon_{it}^0 + D_{it}\left[\xi_i + v_{it}\right] \\ &= x_{it}'\beta + D_{it}\delta^* + \varepsilon_{it}^0 + D_{it}\left[\xi_i + v_{it} - E(\xi_i + v_{it}|x, D_{it} = 1, D_{it-1} = 0)\right] \\ &= x_{it}'\beta + D_{it}\delta^* + \varepsilon_{it}^0 + \omega_{it} \end{aligned} \tag{20}$$

where

$$\begin{aligned} \delta^* &= \overline{\delta} + E(\xi_i + v_{it}|x, D_{it} = 1, D_{it-1} = 0) \\ \omega_{it} &= D_{it}\left[\xi_i + v_{it} - E(\xi_i + v_{it}|x, D_{it} = 1, D_{it-1} = 0)\right] \end{aligned}$$

Note that (20) has the same form as equation (7).

But what is δ^* measuring? The effect of treatment on the treated is by definition $\alpha = E(y_{it}^1 - y_{it}^0|x, D_{it} = 1, D_{it-1} = 0)$. Taking expectations of (19) we obtain,

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[30] A linear projection argument would make ε^0(ε^1) and x uncorrelated by construction. In this case, however, β has no causal interpreation.

$$\begin{aligned}\alpha &= E(y_{it}^1 - y_{it}^0 | x, D_{it} = 1, D_{it-1} = 0) \\ &= E(\delta_i + v_{it} | x, D_{it} = 1, D_{it-1} = 0) \\ &= \overline{\delta} + E(\xi_i + v_{it} | x, D_{it} = 1, D_{it-1} = 0) = \delta^*.\end{aligned}$$

Thus, δ^* is measuring the effect of treatment on the treated, conditional on x. The effect of treatment on the treated can be decomposed into the change in R&D expenditures for the average firm in the population, $\overline{\delta}$, plus a term $E(\xi_i + v_{it} | x, D_{it} = 1, D_{it-1} = 0)$ indicating how much the change for recipient firms differs from the average change that would be experienced by a firm picked up at random from the entire population of firms.

We just showed that the parameter of interest α can be expressed as the coefficient of D in a regression model relating R&D expenditures to a vector of covariates and to the subsidy dummy. Note that ω_{it} in (20) is by construction always mean independent of D_{it} so that the only source of correlation between the error term and the subsidy dummy may arise from ε_{it}^0. Thus, a sufficient requirement for consistency of the OLS estimator of $\overline{\delta}$ (and of β) in (20) is that, conditional on x and $D_{it-1} = 0$, ε_{it}^0 and D_{it} be mean independent as required by (6).