NBER WORKING PAPER SERIES

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Working Paper 6947
http://www.nber.org/papers/w6947

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
February 1999

Draft prepared for "The economics of technology policy" -- A special issue of *Research Policy*. The paper has benefited from comments from John van Reenen and participants at the NBER productivity meting, Dec., 1998. This project has received partial financial support from the Norwegian Research Council.

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NBER Working Paper No. 6947
February 1999
JEL No. O30, O40, L10

ABSTRACT

A number of market failures have been associated with R&D investments and significant amounts of public money have been spent on programs to stimulate innovative activities. In this paper, we review some recent microeconomic studies evaluating effects of government sponsored commercial R&D. We pay particular attention to the conceptual problems involved. Neither the firms receiving support, nor those not applying, constitute random samples. Furthermore, those not receiving support may be affected by the programs due to spillover effects which often are a main justification for R&D subsidies. Constructing a valid control group under these circumstances is challenging, and we relate our discussion to recent advances in econometric methods for evaluation studies based on non-experimental data. We also discuss some analytical questions that need to be addressed in order to assess whether R&D support schemes can be justified. For instance, what are the implication of firms' R&D investments being complementary to each other, and to what extent are potential R&D spillovers internalized in the market?

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1 Introduction

The theoretical literature on market failures associated with R&D and technological innovations is vast, and there is also a steadily growing empirical literature verifying the importance of spillovers in R&D and innovative activities. There is consequently little controversy among economists about the desirability of governmental support to these activities[1], and all OECD countries have over several decades spent significant amounts of public money on programs intended to stimulate innovative activities. However, compared to the size of the programs and the emphasis put on technology policy by politicians, the effort to evaluate in quantitative terms the economic benefits and costs of R&D subsidies have been rather modest.

In this paper, we review some recent contributions to this evaluation literature that use econometric techniques based on microdata, in particular firm-level data. More specifically, we review the microeconometric literature evaluating the effects of *government sponsored commercial R&D*. This kind of government support to commercial R&D projects is supposed to target projects with large potential social benefits, but with inadequate expected returns to private investors. An important question is whether the government agencies are able to choose projects with high social returns that the private sector would not undertake on its own[2].

Evaluating the effects of government sponsored projects, one has to face the question of what would have taken place without the subsidies, and it is important to realize that evaluating large scale subsidy programs is an exercise in counter factual analysis. 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 our discussion to the recent advances in econometric methods for evaluation studies based on non-experimental data.

Most of the available evaluation studies of R&D programs have not been based on microeconometric techniques, but instead on case studies and interviews with program and project managers[3]. These key persons are typically asked to report the payoff from the projects, and similar questions might be asked also to downstream users of innovations emerging from the R&D program in question[4]. It is easy to conceive an upward bias in the payoff reported by project managers, not least because a high estimate typically increases the chances that the R&D program will be considered successful and continued or replaced by a similar program.

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[1] See, however, the heated and wide-ranging debate on this issue in *Research Policy*, starting with the review by David (1997) of Kealey's (1996) book on economic issues of scientific research. See also the exchange between Friedman (1994) and Griliches (1994).

[2] See e.g. Yager and Schmidt (1997) for a detailed and sceptical discussion of the government's ability to reduce market failures in R&D activities.

[3] Mansfield (1996) surveys this methodology and gives references to the previous literature.

[4] Cf. e.g. Link et al. (1996) and Link (1996). The 1996 book by Link is reviewed by Averch (1997).

Also, one should not underestimate the problems for the project managers in constructing an estimate of the payoff from individual projects, since such estimates are based on counter factual questions similar to those faced by the econometrician[5]. Another disadvantage of the case studies is that they have high marginal costs per case (project) considered, and case studies consequently tend to be quite selective and suffer from the objection that they may not be representative. Finally, evaluation studies not based on 'objective data' may more easily be biased e.g. by prior beliefs, which is a problem because evaluation studies typically are done by 'professional evaluators' who are part of the political process which formulates the programs, and who "are dependent on those commissioning the evaluation studies for further projects and studies, and risk losing future clients if they voice strong critisism" (Luukkonen, 1998).

It is outside the scope of this paper to discuss in detail evaluation studies based on interviews and case studies. Our study focuses on microeconometric studies of firm level data or similar data sources, as we pointed out above. It is also narrowly focused on the impact on manufacturing performance of direct government support to commercial R&D-projects, and it largely ignores closely related issues such as the impact of research in governmental labs, defense related R&D-contracts, support to basic research in universities and tax-breaks for R&D[6]. Furthermore, we do not review the literature that exclusively considers to what extent R&D subsidies crowd out privately financed R&D investments, but our discussion addresses this issue in the context of the more wide-ranging studies that we consider.

We start in section 2 by considering four microeconometric studies that directly try to evaluate the effects of government sponsored commercial R&D, and we refer to these studies at several points in the rest of the paper. Section 3 discusses some general issues considered in the recent econometric literature on evaluation studies when only non-experimental data are available, which is typically the case for R&D programs. Section 4 discusses more narrowly how the four studies address the essential issue of R&D spillovers, and proceed with a discussion of ways to extend the evaluation studies at this point. In section 5, we discuss some questions related to market imperfections and spillovers that needs to be addressed to evaluate whether the R&D support schemes can be justified. Our suggestions for future research are summarized in the last section.

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[5] Notice that the project manager might have less information e.g. about economic results of competing projects or firms than the econometrician does.

[6] See the survey by Hall and van Reenen (1999) on taxes and R&D, and Mowery and Rosenberg (1998) for a wide ranging discussion of the other issues and further references.

2 Four microeconometric studies of government-sponsored R&D

The SEMATECH research consortium in the US. Irwin and Klenow (1996) evaluated the SEMATECH program in the US, which was a research consortium established in 1987. SEMATECH was set up to promote US manufacturing's role in the development of technology for production of semiconductor products[7]. The consortium was initiated with fourteen firms but has since been somewhat restructured with a few of the initial firms pulling out. About half of the consortium's annual budget (about $200 Mill.) was financed through government subsidies from 1987-96[8].

In their study based on annual firm-level data for the period 1970-1993, Irwin and Klenow (1996) found that SEMATECH was successful in eliminating excessive duplication of R&D, which was a major objective of the consortium. At the same time, the SEMATECH firms had on average a more rapid growth in sales than non-member firms. Irwin and Klenow also compared the SEMATECH firms' performance in terms of physical investment, returns on assets and sales, and labor productivity growth, but found no systematic difference from non-member firms for these variables. Their analysis was based on running a set of similar regressions of the form

$$Y_{it} = \alpha_i + \beta_1 Y_{i,t-1} + \beta_2 D_i^{\mathrm{SMT}} + \text{Dummies} + e_{it}, \tag{1}$$

where Y_{it} is the performance measure of interest, e.g. private R&D to sales ratio, for firm i in year t, while D_i^{SMT} is a dummy which is one if the firm was a member of SEMATECH and zero otherwise. Their regressions include firm-specific parameters, α_i, which are treated as so-called firm fixed (or correlated) effects[9]. The dummies include time dummies and firm age dummies, while e_{it} is an error term. The 'experiment' in the data allowing Irwin and Klenow to identify the interest parameter β_2, is the observations for non-member firms in the same industry as the SEMATECH members (i.e. the electronic components industry, SIC 367). The presence of observations prior to the establishment of SEMATECH is useful to add precision to the estimates of the auxiliary parameters.

Irwin and Klenow focus on their estimate of β_2, which according to their computations

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[7] Link et al. (1996) evalute the returns to SEMATECH projects through interview studies.

[8] The government support to SEMATECH was ended in 1996, but the consortium has continued with private funding only. In August, 1998 it was announced that SEMATECH and the government will jointly sponsor new university-based research centers to "study new methodologies in designing, testing and connecting microchip components".

[9] However, their regressions do not account for the bias created by the presence of these fixed effects in combination with a lagged dependent variable. See Nickell (1981).

suggests savings in R&D around $300 Mill. But this estimate does not account for the dynamic effects captured by the lagged dependent variable in their model. The long run effect of R&D membership is given by $\beta_2/(1-\beta_1)$, which is about 75 percent higher, and their estimated model consequently indicates that the R&D saving from SEMATECH was substantially higher than $300 Mill.

The study by Irwin and Klenow convincingly suggests that SEMATECH has been a profitable project in terms of social costs and benefits, as the consortium has managed to eliminate wasteful duplication of R&D, while preserving the same or perhaps even better R&D output despite the cut in R&D spending. It would seem useful to repeat their exercise with a sample covering also the period after 1993. As recognized by Irwin and Klenow, the most important reservation one could raise against their analysis is probably the validity of the control group. Comparing the list of SEMATECH member firms to the non-member US firms, it is clear that the SEMATECH member are the leading US manufacturers in the electronic components industry, and this was true also when SEMATECH started. Irwin and Klenow try to account for the differences by incorporating the fixed effects, but even when they condition their analysis on such permanent differences, it remains questionable whether the non-members of SEMATECH in the same industry reveal what the members would have experienced without SEMATECH in place. We will return to this issue when we discuss methodological questions in evaluation studies based on non-experimental data in section 3 below.

Japanese research consortia. SEMATECH was inspired by the success of Japanese research consortia in the semiconductor industry and other high-tech industries. Branstetter and Sakakibara (1998) have examined the performance of the Japanese research consortia in these industries, combining econometric techniques with an interview study. The Japanese research consortia were heavily subsidized by the Japanese government; government subsidies covered on average two thirds of the research costs for the projects carried out within the consortia. Branstetter and Sakakibara argue that the Japanese research consortia were primarily aimed at bringing together complementary R&D projects, thereby making the R&D projects more productive and also more profitable[10]. In this view, the research consortia have raised the learning opportunities and thereby stimulated to more R&D. Notice that this situation is different from the SEMATECH case discussed above, where the consortium eliminated excessive duplication of parallel research rather than promoted complementary research. Branstetter and

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[10] They motivate this focus with a reference to Cohen and Levinthal (1989), who emphasized that firms typically undertake R&D to learn about competitors' innovative activities. See section 5.1 for further remarks on this issue.

Sakakibara's econometric results show that a membership in the Japanese research consortia typically stimulated private R&D spending, and also made the research effort more productive.

Branstetter and Sakakibara's result on R&D spending is obtained by estimating a model slightly different from Irwin and Klenow's non-structural model (cf. eq. 1 above):

$$\log(R\&D_{it}) = \alpha_i + \beta_1 \log(\text{Capital}_{it}) + \beta_2 C_{it} + \text{Dummies} + e_{it} \tag{2}$$

The left hand side variable is private R&D spending in firm i in year t, while the first explanatory variable on the right hand side is physical capital added to control for size effects. C_{it} is the number of research consortia in which the firm is involved in year t and β_2 is the parameter of interest. Branstetter and Sakakibara present estimates where they treat α_i as random, firm specific effects, as well as firm fixed effects. The dummies include both time and industry dummies as their sample covers several high-tech industries. The equation is estimated on an unbalanced sample of 226 firms over the period 1983-89, with 141 firms participating in at least one research consortium during the sample period, while the remaining 85 firms did not. As pointed out above, their results revealed a positive and statistically significant value for the interest parameter β_2.

To examine whether the research consortia created spillovers and thereby made the research effort more productive, Branstetter and Sakakibara estimated several patenting equations. Using data on patents granted to Japanese firms in the US, Branstetter and Sakakibara started by estimating an equation with the log of patents as dependent variable[11]:

$$\log(P_{it}) = \alpha_i + \beta_1 \log(R\&D_{it}) + \beta_2 \log(\text{ Capital}_{it}) + \beta_3 C_{it} + \text{Dummies} + e_{it}. \tag{3}$$

Their point estimate of the consortia coefficient β_3 suggests that membership in an additional consortium tends to raise patenting by 5 percent, and this effect is statistically significant irrespective of whether α_i is treated as a random firm specific effect, or as a firm fixed effect. Branstetter and Sakakibara consider several alternative specifications of equation (3), and conclude that the positive effect of membership in research consortia is robust.

The final part of Branstetter and Sakakibara's analysis focuses more closely on the R&D spillovers associated with membership in a research consortium. This analysis is carried out by augmenting equation (3) with two additional terms representing spillovers. The basic spillover

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[11] They avoid the problems with zero patents by using $\log(P_{it}+1)$ as their left hand side variable. There is also an issue of the dating of the left hand side variable, and Branstetter and Sakakibara follow the patent literature and date each patent according to the patent's year of *application*.

variable is constructed as a weighted sum of other firms' R&D, where the weights reflect the 'technological distance' between the firm in question and each of the other firms. The primary additional term representing spillovers in Branstetter and Sakakibara's analysis is this spillover variable interacted with a dummy variable reflecting membership in research consortia. Their parameter estimate for this interaction term is positive and statistically significant when α_i is treated as a random effect, and Branstetter and Sakakibara conclude that membership in research consortia augment knowledge spillovers.

One final, interesting aspect of Branstetter and Sakakibara's study is their use of interviews to supplement the econometric analysis. The responses in their interview study are consistent with their finding that government funds did not substitute for private R&D spending, but rather tended to increase the firms' own R&D spending. Interestingly, the interviews also suggested that selection into the research consortia were *not* biased towards the best projects; firms which are technology leaders in a field tend to be reluctant to participate in projects which will spread their superior knowledge and where they have little to gain. We will discuss how this selection effect affects the interpretation of the estimated parameters in section 3.

Government support to commercial R&D projects in Israeli firms. The study by Griliches and Regev (1998) illustrates how the production function framework widely used to study returns on R&D, can easily be adapted to study the effects on private firm performance of government-funded R&D[12]. Their study covers the overall effort by the Israeli government to promote R&D related to manufacturing activities, incorporating a number of governmental programs[13]. They estimate the private returns accrued to the supported manufacturing firms, created by the government-funded R&D. Their preliminary results suggest that this *private* rate of returns to the firms carrying out R&D funded by the government is large, even if one incorporates both the private and social R&D costs. The social rate of returns has been even higher if these R&D programs generated any spillovers as presumably was expected.

Griliches and Regev estimate production functions incorporating R&D capital (K_{it}), allowing for a separate coefficient on the share of R&D capital accumulated with government funding (s_{it})[14]:

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[12] See Griliches (1979). The production function approach has previously been used to study the impact of goverment funded R&D in US manufacturing firms in Griliches (1986). A very large share of this research in the US has, however, been related to defense contracts and there are a number of reasons why it is hard to measure the real effects of defense related R&D projects, as discussed by Griliches (1979).

[13] This governmental support to R&D include commercial R&D projects, support to consortia engaged in 'generic' technologies (Magnet-program), National S&T Infrastructure program, USA-Israel binational program, and defense related contracts.

[14] This specification is based on the observation that

$$\ln\left(\frac{Q}{L}\right)_{it} = \alpha_i + \beta_1 \ln\left(\frac{C}{L}\right)_{it} + \beta_2 \ln\left(\frac{M}{L}\right)_{it} + \beta_3 \ln\left(\frac{K}{L}\right)_{it} + (\beta_3\delta)\, s_{it} + \text{Dummies} + e_{it}, \quad (4)$$

where Q, L, C and M are output, labor, physical capital and materials. As above, the subscripts refer to firm i in year t. The dummies include a number of control variables in addition to year and industry dummies. The parameter of interest is δ, which can be interpreted as the effective premium or discount on government supported R&D. As mentioned above, their preliminary results suggest quite a high, positive and statistically significant premium on government supported R&D, based on a sample of more than 11 000 firm-year observations covering the period 1990-95. One could suspect that the high premium is due to the government picking the best firms, and that the estimated rate of return therefore is upward biased. However, this does not seem to be the case. The premium is particularly high when fixed effects are accounted for, suggesting a *negative* selection bias where firms with a high share of R&D capital accumulated with government funding typically have low average productivity levels. We will return to this issue in section 3.

Their finding of a high premium on the R&D projects funded by the government suggests that the projects should have been profitable also for the firms themselves[15]. According to their estimates, the government picks good projects in commercial terms, but the projects seems to be too profitable to justify government support. The question then emerges in what way such studies are useful to evaluate governmental support to commercial R&D, given the ambiguity of the interpretation of rate of return estimates. That is to say, a low rate of return estimate suggests that the projects might have been unsuccessful, while a high rate of return estimate suggests that the firms should have been able to fund the R&D activities themselves, unless there are significant capital market imperfections affecting R&D investments. One or two additional steps are consequently required to draw any conclusions about the social value of these R&D programs. First, their study should be supplemented by a study of how private R&D spending tend to respond to R&D subsidies, and second, spillover benefits should be estimated as we will discuss in section 4.

$$\begin{aligned}\ln\left[K_1 + (1+\delta)\, K_2\right] &= \ln\left[K_1 + K_2 + \delta K_2\right] \\ &= \ln K + \ln(1+\delta s) \simeq \ln K + \delta s,\end{aligned}$$

where K_1 and K_2 are two types of capital with different efficiency, and $K = K_1 + K_2$. δ is the efficiency premium of the second type, while s is the share of this kind of capital. The last approximation is good if δs is small.

[15] As discussed in Griliches and Regev (1998), a high premium does not necessarily imply a high rate of return on the supported projects, as the support typically went to R&D intensive firms and their model assumes diminishing marginal returns to R&D capital.

Government support to commercial R&D projects in Norwegian high-tech firms. Klette and Møen (1998a) have studied the impact of a series of governmental programs aimed at supporting commercial R&D projects in Norwegian manufacturing related to information technology. These IT-programs were intended to stimulate complementary R&D activities, especially in high-tech manufacturing, and the effort peaked in the four years 1987-90. The econometric analysis revealed few significant differences between the supported firms and the non-supported firms in the same industries despite the large amounts of R&D support provided. Similarly at a more aggregated level, the study found that targeted industries did not show any outstanding performance compared to the rest of the manufacturing sector in Norway, nor in comparison to the same industries in other OECD countries[16]. The study concluded that the effort to promote IT-related manufacturing has been largely unsuccessful, and the study proceeds by examining why the IT-programs had so poor coordinating performance.

In terms of the performance measure used by Griliches and Regev (1998), i.e. total factor productivity growth, Klette and Møen (1998a) found that the supported firms did significantly *worse* than the non-supported firms. Considering this performance measure alone, one is led towards the conclusion that governmental support is associated with significantly poorer performance, but the systematic difference between supported and non-supported firms disappear when considering a broader set of performance measures. It is difficult to conceive that there is a *causal* relationship between government support and poor performance in terms of total factor productivity growth, and it seems more plausible that the relationship runs the other way; the government tried to save some of the main high-tech firms as they encountered problems when the IT-industry was restructured towards the end of the 1980s. This possible interpretation illustrates why there might be a *negative* selection bias in the parameter estimates capturing the effect of government support, and we will discuss how this selection bias can be reduced or eliminated in section 3.

The microeconometric part of the study by Klette and Møen is similar to Irwin and Klenow (1996) in that the estimating equations are reduced form equations with a number of different performance measures as left hand side variables; private R&D spending and physical investment, growth in sales, employment and productivity, and returns on assets and sales. The estimating equations do not include lagged dependent variables in contrast to equation (1), but the main results are based on models including fixed, firm level effects. The first, microeconometric part of the analysis was based on firm and plant level data for the period 1982-95.

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[16] The Norwegian governmental support to R&D in the targeted industries seem to have been high in relative terms also in an international perspective. See below.

As mentioned, the study also contained a more aggregated analysis, based on industry level data for Norway and other OECD countries. This part of the analysis examined the overall performance of the targeted high-tech industries[17]. The motivation for this was that some of the benefits from the program could spill over to non-supported firms with the result that the comparison between the supported firms and the non-supported firms would underestimate the effect of the program. To the extent that these spillover effects were important, these effects should show up in the performance at a more aggregated level. At the more aggregated level, it is, however, difficult to identify a control group, i.e. a similar non-supported industry, and Klette and Møen considered two alternatives. The first comparison is between the targeted high-tech industries and the rest of the manufacturing sector as a whole. This is clearly not a clean quasi-experiment, but it is nevertheless interesting to compare e.g. the profit rates and the returns to investments (R&D and physical) in the targeted industries to other industries in a cost-benefit perspective. The second comparison at the industry level is based on OECD-data for the targeted high-tech industries in Norway and other OECD countries. Once more, the contrast between industry performance in Norway and the other OECD countries is far from a clean quasi-experiment, as the same high-tech industries also received considerable governmental support in the other OECD countries. As far as the OECD data goes, they suggest that the increase and perhaps also the level (relative to private R&D spending) of governmental support to these industries were significantly larger in Norway than in most of the other countries in the second half of the 1980s.

In a companion study, Klette and Møen (1998b) examine more closely the effect of the R&D subsidies on private R&D spending in the supported firms. The first part of their analysis uses a non-structural econometric approach similar to Branstetter and Sakakibara (1998), as specified in equation (2) above. The analysis suggested that governmental R&D support did not crowd out private R&D spending, but neither did the firms increase their own R&D spending as was (formally) expected in the 'matching grant' contract scheme that was widely used. In the second half of their study, they introduce a structural model for R&D investment which incorporates a 'learning-by-doing effect' in R&D, where accumulated R&D-capital (past R&D effort) has a positive impact on the productivity of current R&D[18]. This framework suggests that temporary R&D grants might have had a more lasting, positive effect on private R&D spending after the support had expired, but the empirical results at this point are more suggestive than conclusive.

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[17] Two alternative defintions of the targeted industries were considered: a widely defined group ISIC 382, 383 and 385, and a more narrowly defined group; ISIC 3825 and 3832.

[18] The same framework was used in a different way in Klette (1996) and Klette and Johansen (1998).

3 Estimating counter factual outcomes from non-experimental data

As we will clarify below, the results in the studies presented in section 2 are based on the assumption that R&D subsidies to a large extent are allocated randomly to firms and projects. With enough randomness in the allocation process, data for the firms receiving R&D subsidies as well as for similar non-supported firms provide us with quasi-experiments and a basis for causal, econometric analysis. Given the many factors involved in the political economy process that determines the allocation of R&D subsidies, random allocation may not be too misleading in some cases. However, assuming that governments' deliberate selection process is largely random is at least suspect, and there might be a significant bias involved in the estimated impact parameters. This section tries to clarify the potential biases involved and explain how the methodology can be improved by drawing on some recent advances in econometrics associated in particular with evaluation of labor market programs[19].

Selection and the problem of the counter factual. Both the studies by Irwin and Klenow (1996) and Klette and Møen (1998a) use the outcome of the non-supported firms to estimate what the supported firms would have experienced had they not been supported, and the studies from Japan and Israel use their econometric models as devices to generate similar counter factuals[20]. The difference in performance between supported and non-supported firms is the estimated gross impact of the R&D support schemes. 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 schemes, and this is the selection bias problem that we referred to above[21].

To aid the discussion, let us address the issue somewhat formally, and assume that the performance of a firm i in period t, denoted Y_{it}, is given by

$$Y_{it} = \alpha_i + \lambda_t + \beta_i D_i \; + u_{it} \tag{5}$$

where D_i is a dummy variable which is one if the firm has received R&D support and zero

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[19] See Angrist and Krueger (1998) and Heckman et al. (1998) and references cited there. Blundell (1998) gives a simple introduction to the econometric literature.

[20] We will not consider the evaluation literature more relevant for the last two studies on estimating 'treatment effects' with various levels of 'treatment', as this complicates the analysis considerably. See Angrist and Krueger (1998).

[21] An interesting analysis of the choice of comparison groups in evaluating technology innovation programs is Brown et al. (1995). They suggest that it would be useful to consider the performance of only the firms with rejected applications to construct the counter factual instead of the the performance of all the non-supported firms. The reject project applications are hardly a random group of projects, but they may get as close to a control group as is possible.

otherwise[22]. α_i is a firm specific intercept, λ_t reflects shocks common across firms, while u_{it} represents temporary fluctuations in unobservables. We have abstracted from other (observable) regressors for simplicity. To be concrete, α_i represents permanent differences in firm performance while u_{it} represents temporary fluctuations in performance around the firm specific means, due to effects specific for individual R&D-projects. Equation (5) incorporates heterogenous responses to the R&D support (*ex post*) as indicated by the subscript i on the β-coefficient, and the distribution of these coefficients may differ systematically between the supported and the non-supported firms. Indeed, the agency allocating the R&D support might try to allocate their funds on the basis of anticipated differences in the β_i's. As we shall argue below, such a systematic difference does not make the evaluation results uninteresting, but limits the kind of counter factual questions the evaluation results can answer.

Most of the studies above present estimates where α_i is treated as a firm specific parameter, i.e. where α_i is allowed to be correlated with D_i . In this way, the estimated impact parameter is *not* biased even if the supported firms are non-randomly selected, as long as the selection is based on firm characteristics that are largely invariant over time. Assuming that data are available before and after the supported firms have received their support, i.e. at times t_0 and t_1, this gives the estimator[23]

$$\begin{aligned} \widehat{\beta}_{\text{did}} &= \left(\overline{Y}^s_{t_1} - \overline{Y}^s_{t_0}\right) - \left(\overline{Y}^n_{t_1} - \overline{Y}^n_{t_0}\right) \\ &= \Delta\overline{Y}^s - \Delta\overline{Y}^n, \end{aligned}$$

where $\Delta\overline{Y}^s$ and $\Delta\overline{Y}^n$ are the average changes in performance from before to after the R&D support scheme was operating, and the superscripts s and n refer to the supported and the non-supported firms, respectively. In the econometric literature, this estimator is now commonly referred to as the *'difference-in-differences'* estimator. Assuming that D_i and u_{it} are uncorrelated, we have that

$$\text{plim } \widehat{\beta}_{\text{did}} = E\left(\beta_i | D_i \ = 1\right) \equiv \beta^S$$

which is a parameter of interest, representing *the average impact of the R&D-support on the supported firms*[24]. This is the parameter of interest if we want to do a cost-benefit analysis of

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[22] We have ignored the time subscript on D_i for simplicity, since we will focus the discussion on situations were the econometrican uses only data from before and after the program has taken place.

[23] With observations for more than two years, a preferable estimator might be the 'within'-estimator widely used in the panel data literature. The 'within'-estimator is closely related to the 'differences-in-differences'-estimator. Alternatively, observations for several periods during or after the support has been provided might be used to examine the time profile of the impact by considering a number of 'difference-in-differences' estimates, as is done in Heckman et al. (1998).

[24] In the econometric literature, this parameter is often termed *the mean impact of the treatment on the treated.*

the R&D support scheme. Notice, however, that this parameter may not be informative of what would happen if the R&D support scheme was extended to previously non-supported firms, when there are systematic differences in the responses to R&D support between the supported and the non-supported firms[25].

As mentioned, most of the estimates presented in the four studies discussed above are based on the *'difference-in-differences'* estimator or similar estimators, and the study by Heckman et al. (1998) suggests that this method is preferable to alternatives such the widely-used parametric selection-correction method introduced by Heckman (1979) and the more recent matching methods discussed in Heckman et al. (1998).

The econometric evaluation literature has noticed that there may remain a serious problem due to correlation between the temporary shocks (u_{it}) and the probability of being selected into the program[26]. Discussing the results from the study by Klette and Møen (1998a), it was observed that the poor growth performance in terms of total factor productivity for the supported firms, might have been due to the government supporting some large firms that were facing particularly severe problems when the IT-industry was restructured towards the end of the 1980s. In that case, there was a positive relationship between receiving R&D support and the prospect of growing more slowly than the average, and the growth performance of the non-supported firms was not very useful for estimating what the supported firms would have experienced had they not been supported. Consequently, the 'difference-in-differences' estimator underestimated the impact of the R&D-support on the supported firms in this case. Similarly, in the Japanese case, Branstetter and Sakakibara (1998) found from their survey-study that firms with the most promising projects in a technological field were reluctant to participate in research consortia, which might have created a similar downward bias in the 'difference-in-differences' estimator.

On the other hand, it is easy to conceive that the bias can go the other way in cases where firms apply for support when they have discovered particularly promising R&D projects. The screening of projects in the government agencies will also tend to create a selection bias in the estimated impact. More precisely, if there is a positive correlation between a firm hitting partic-

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We have that

$$\beta^S = E(\beta_i|D_i = 1) = \overline{\beta} + E\left[(\beta_i - \overline{\beta})\,|D_i = 1\right],$$

where $\overline{\beta}$ is the *population mean impact effect.*

[25] One could, however, extrapolate the impact analysis to the non-supported firms also in this case if one adds assumptions about the functional form for the β_i-distribution, along the lines in Heckman (1979).

[26] In studies of training programs, it has been observed that individuals tend to be selected into the program during periods when they perform particularly badly, i.e. have particularly low income. This is the so-called 'Ashenfelter-dip'.

ularly promising projects that tend to generate above average performance growth in subsequent years, and the chance of the firm receiving R&D support, the 'difference-in-differences' estimator will *over*estimate the impact of the R&D-support on the performance of the supported firms. Previous studies of the effectiveness of R&D subsidies in stimulating private R&D spending has been criticized by Kauko (1996) along these lines.

The econometric literature has suggested that such biases can be reduced or eliminated by augmenting the 'difference-in-differences' estimator, incorporating conditioning variables reflecting the pre-program performance[27]. That is, differences in longitudinal changes in performance between supported and non-supported firms should control for pre-program, temporary shocks that influence the probability of being supported, e.g. pre-program changes in R&D or firm growth. Similarly, one would also like to control for *anticipated* future temporary shocks that influence the probability of being supported by conditioning on forward looking variables, in particular physical and R&D investment and perhaps also hiring or firing.

In the review of the study of SEMATECH in section 2, we raised the issue that the members and non-members in SEMATECH were to a large extent quite different firms in terms of size and closeness to the technological frontier. As emphasized in Heckman et al. (1998), such differences make the evaluation results critically dependent on assumptions about functional forms, both in terms of the performance equation and the selection equation, and Heckman et al. find that this tend to generate substantial biases in the case they examine. Exploring various matching-procedures as well as regression methods, Heckman et al. conclude that evaluation results are only reliable when they are based on 'treated' units (cf. supported firms) which are similar to some of the 'non-treated' units (cf. non-supported firms). For the supported firms that can not be adequately 'matched', the comparison to non-supported firms can give quite misleading inference of the impact.

Spillovers and the counter factual: 'Catch-22'? Using the non-supported firms to evaluate what would have happened to the supported firms if they had not been supported assumes that there is no spillover effects of the R&D support scheme to the non-supported firms, which is clearly a strong assumption. The question is whether the performance of the non-supported firms can be considered independent of the support given to the supported firms[28]. One could argue both ways in terms of the bias this problem introduces in the estimated impact of the

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[27] See Angrist and Krueger (1998), Heckman et al. (1998) and Blundell (1998) for details.

[28] Manski (1993) considers a closely related problem; the assumptions required for identification of spillover effects, when we want to condition on regressors that tend to eliminate independent variations in the spillover variable. This is largely the reverse of the question we discuss in this section. See also Griliches (1998, ch. 12).

R&D program; the impact will be *under*estimated if the non-supported firms tend to benefit e.g. from pure knowledge spillovers from the R&D in the supported firms, while the impact will be *over*estimated if the non-supported firms are hurt as they loose relative competitiveness to the supported firms.

This spillover issue is particularly problematic since spillovers to technologically related firms are often a major justification for such programs in the first place. This implies a 'Catch-22' problem: If the program is successful in creating innovations that spill over to technologically related firms, it will be very difficult to find similar non-supported firms that can identify the counter factual outcome for the supported firms. This problem is particularly transparent if one tries to evaluate the performance of the supported firms by means of the matching procedure, as described in Blundell (1998, section 5.4.2). The matching estimator suggested by Blundell is given by

$$\widehat{\beta}_{\text{mm}} = \frac{1}{N_S} \sum_{i \in S} \left(Y_i - \sum_{j \in N} \omega_{ij} Y_j \right)$$

where Y_i and Y_j are the *post*-program outcomes for a supported and a non-supported firm, respectively, while N_S is the number of supported firms. ω_{ij} is a weight indicating the 'similarity' between the two firms before the R&D-support was provided. Our point is that similar weighting schemes have been used to identify 'technologically related' firms when estimating the impact of R&D spillovers, as in studies by Jaffe (1986), Branstetter and Sakakibara (1998) and others that we will discuss in the next section. This suggests that the better a firm seems to satisfy the conditions required to identify the counter factual outcome in the absence of spillovers, the worse might this spillover problem be.

The motivation for introducing the matching estimator into the econometric tool box has been that it requires only weak assumptions about functional forms, as we noted above (see Heckman et al., 1998). This argument suggests therefore that it might be difficult to identify the impact of R&D programs more generally, without imposing strong functional form assumptions[29]. As is so often the case in economics, one does not get very far in causal inference with non-experimental data unless a significant amount of structure is imposed on the analysis.

To conclude, we face the paradoxical situation that if an evaluation study finds little difference between the supported firms and the non-supported firms it could either be because the R&D program was unsuccessful and generated little innovation, or because the R&D program was highly successful in generating new innovations which created large positive spillovers to the

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[29] See Manski (1993) for a formal analysis of the functional form assumptions required for identification in a closely related context, and within a regression framework.

non-supported firms.

Focus on a few successes?

> [T]he economic value of one great industrial genius is sufficient to cover the expense of the education of a whole town. (Marshall, 1920, p.179)

It has been widely recognized that the economic benefits from research projects tend to have a highly skewed distribution, with a median return which might not be very high but a few projects generates a high mean return; see e.g. Scherer (1999)[30]. This represents a further challenge to regression analysis of the impact of R&D subsidies, and such skewness might be particularly pronounced for the outcome of government sponsored R&D projects to the extent that government tends to support high-risk R&D. This observation raises the question of whether the main parameter of interest is the *average* impact of the R&D-support on the supported firms. More precisely, we might be interested in the average rate of return to the whole R&D subsidy program, but the weighted average estimates provided by the *'difference-in-differences'* estimator or similar estimators will typically not apply the economically relevant weights to the individual observations, and we may want to pay more attention to the economically interesting outliers than such estimation procedures tend to encourage.

To the extent that the estimated impact parameter is driven by a few high-return observations, the confidence intervals for the impact parameter will be large and poorly approximated by the routinely reported intervals based on asymptotic normal distributions. Even if calculated correctly[31], the confidence interval obtained will be large, reflecting the substantial uncertainty that prevails in trying to infer the impact parameter when the outcomes are characterized by a highly skewed distribution with long right tails. This suggests that we might need to consider a number of independent evaluation studies, say through meta-analysis, before we can provide an estimate of the impact of the R&D subsidies with much precision.

Recent econometric advances suggest that it might be possible to estimate the distribution of the subsidy impacts across firms[32], but we believe that these methods should only provide a first step in a closer investigation of the economic benefits of the most important innovations generated by the R&D subsidy programs. It would be useful to merge econometric studies of the kind discussed in this paper with more detailed case studies of the most successful projects, and perhaps also some of the less successful projects. In the 1960s, the 'Traces'-project illustrated

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[30] This observation is closely related to finding that the distibution of the value of patents is highly skewed with a long right tail, see e.g. Pakes (1986).

[31] Whatever that means, but say from bootstrap estimates for argument's sake.

[32] See Heckman et al. (1997) and Abadie et al. (1998).

the second half of this strategy, as the project tried to trace the economic benefits, including spillovers, from some of the most important innovations and civilian spin-offs generated by NASA and defense related R&D.

4 Identifying spillovers and the social benefits of R&D projects

We noted above that spillovers tend to invalidate the non-participants as a control group. However, measuring the magnitude of the spillovers generated is by itself a crucial part of evaluating the programs. The studies discussed in section 2 covered quite well the benefits to the private firms receiving the support, while in most of the studies the spillovers to non-supported firms and pecuniary externalities to customers and consumers were not extensively addressed.

A full cost benefit analysis of an R&D support scheme would involve estimating the expression

$$w(s) = \sum_{i \in S} \widetilde{\Delta}\, \pi_i\,(s_i, \underline{s}) + \sum_{j \in N} \widetilde{\Delta}\, \pi_j\,(s) + \sum_{l \in R} \widetilde{\Delta}\, \pi_l\,(s) + \sum \widetilde{\Delta}\ (\mathrm{CS}) - d(s) \tag{6}$$

where $\widetilde{\Delta}$ is used to indicate the (counter factual) shift in the various variables as follows[33]: The first sum covers the change in profits in the group of supported firms, S, due to the R&D support scheme. Note in this respect that the benefit for each firm belonging to S, can be decomposed into a direct effect capturing the increase in profits due to the support they have received themselves, s_i, and an indirect effect capturing the change in profits due to the support received by other firms, $\underline{s}$. The latter component may be positive, negative or zero, depending on what kind of spillovers are present. The second summation term on the right hand side of (6) captures the change in profits in the group of non-supported firms, N, in the same industry as the supported firms[34]. The sign of this term is also ambiguous. We will refer to it as the indirect effect on the non-supported firms, and it may be a mixture of knowledge and rent spillovers. The next two terms represent rent spillovers alone. That is, the third sum captures the change in profits in firms in the rest of the economy, R, due e.g. to pecuniary externalities as inputs become cheaper or better. The fourth term is the increased consumer surplus in the economy. The last term is trivial and represents the deadweight loss associated with the funding of the program.

The treatment of spillovers in the evaluation studies. Using equation (6) to fix ideas, we will now briefly discuss how far the various evaluation studies reviewed in section 2 go

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[33] All variables on the right hand side of (6) should be interpreted in terms of present values of current and future benefits.

[34] Our concept of industry is at this point loosly defined as firms which are technologically related.

towards incorporating the full welfare effects of the programs they examine. With respect to estimation techniques, the most ambitious attempt to estimate spillovers among these studies is Branstetter and Sakakibara (1998). Still, this study explores only the effect of the programs, i.e. the subsidized research consortia, on the participating firms and other firms in the same industry. In other words, they deal roughly with the first two sums in (6), while ignoring pecuniary externalities to firms in other industries and consumers. With respect to the first sum, the change in profits for the supported firms, it is not relevant to distinguish between the direct and the indirect effect of subsidies, as the subsidies are given to consortia and not to individual firms.

Branstetter and Sakakibara find evidence that participation in research consortia rises research output, even after controlling for research input and firm specific effects. It seems reasonable to interpret this as a pure knowledge spillover, but comparing their framework to equation (6), we should note that they have not considered how the increased research output affect the consortia participant's *profits*. Depending on how close competitors the participants are in the output markets, there may be negative rent spillovers between them, and the innovative gains may partly accrue to customers and suppliers. However, focusing on innovative output seems like a reasonable strategy when the total welfare effect cannot be measured, since increased research efficiency necessarily increase total welfare. Turning next to the indirect effects of the program on non-supported but technologically related firms, they find clear evidence of general R&D spillovers, but they do not identify the extent of spillovers from the subsidized consortia to the non-members.

Irwin and Klenow (1996) resemble Branstetter and Sakakibara in that they consider membership in a subsidized research consortium and not individually received R&D subsidies under a program. Referring back to equation (6) one could say that Irwin and Klenow sign the first term on the right-hand side, i.e. the effect of the program on the participants, as they find increased profitability for SEMATECH members. The level of profitability is obviously affected by other factors than SEMATECH membership, but their results suggest that members have increased profitability relative to non-members also when they control for such factors. The difference could in principle be due to non-members facing stronger competition after the introduction of SEMATECH, i.e. a negative pecuniary externality belonging in the second term on the right hand side of (6), but the authors' interpretation is that it is most likely due to increased research efficiency within the consortium[35].

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[35] This is based on their finding that SEMATECH members significantly reduced their R&D-intensity relative to non-members, and the assumption that non-members R&D spending was not affected by SEMATECH. This

Klette and Møen (1998a), like Branstetter and Sakakibara (1998), try to capture both the effect on the supported firms and the effect on the non-supported firms in the same industry, but they ignore possible rent spillovers to other industries and to consumers. The empirical part of the study starts out estimating the effect of the support on the supported firms[36], but find no significant impact neither in the short nor in the long run. This could, as mentioned in section 2, be due to strong spillovers from the supported to the non-supported firms, and they investigate this issue by comparing the growth of the supported high tech industry (including the non-supported firms) both to growth in overall manufacturing, and to growth in similarly defined high tech industries in other OECD countries.

Griliches and Regev (1998) do not explicitly deal with spillovers. However, the framework they use is one which easily lend itself to incorporate such effects the way they are usually treated in the more general literature on R&D spillovers. We will now turn to this larger literature as it is obviously of great relevance both with respect to methodology and R&D policy. First, we take a closer look at the frameworks available to study R&D spillovers and then we briefly review the main findings and rise some concerns.

Traditional approaches to the study of R&D spillovers. There are two main strands of literature investigating the empirical importance of R&D spillovers. First, there are case studies which try to estimate the social return to particular research projects by extensively tracing the effects of the resulting innovations. This approach was first used to evaluate public investments in agricultural research, but private R&D investments have also been studied. The most famous example of the latter is Mansfield et al. (1977) finding a median social rate of return of 56 percent, more than twice the comparable median private rate of return[37]. The detailed information provided by case studies have been extremely valuable for understanding the mechanisms at work in technologically advanced industries and markets. However, as pointed out in the introduction, case studies always suffer from the objection that they may not be representative. This has motivated econometric work, which is the other main approach.

Most econometric studies have been performed within a production function framework where a 'pool' of outside knowledge is included in the production function of a firm or an

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assumption is of course crucial, as we discussed in section 3 under the heading of 'Selection and the problem of the counter factual.'

[36] Since the subsidies in the programs they evaluate are given to individual firms they could in principle have distinguished between direct and indirect effects of the support on the supported firms, but their focus is (implicitly) on the sum of the two effects.

[37] More recent studies include Bresnahan (1986) on computers, Trajtenberg (1983, 1989) on CT scanners and the Bureau of Industry Economics (1994) on 16 innovations in Australia.

industry. This is the idea utilized in the study by Branstetter and Sakakibara (1998)[38]. The R&D pool is constructed as a weighted sum with weights (ideally) representing the relevance of R&D undertaken elsewhere in the economy, i.e.

$$S_{it} = \sum_j w_{ijt} K_{jt} \tag{7}$$

where S_{it} is the spillover pools, and w_{ijt} is the effective fraction of knowledge in firm j which is freely available to firm i at time t. The weights are usually considered a measure of the proximity between the firms, and have been constructed in a number of ways. According to the survey by Mohnen (1996), both product fields, types of R&D, patent classes, input-output flows, investment flows and patent flows have been utilized[39], and he suggests other possibilities such as flows of R&D personnel, qualifications of R&D personnel and R&D cooperation agreements.

As pointed out by Griliches (1979), there are two different concepts of spillovers behind these measures. First, a firm may benefit from research undertaken elsewhere to the extent that changes in the market prices of its inputs do not fully reflect the value of the innovations. From a production function point of view it is not really a spillover, but a measurement problem. If price indexes fully reflect quality adjustments, R&D embodied in inputs will not be relevant as a separate variable. Lacking quality adjusted price indexes, however, one can try to trace the effect of R&D rents not appropriated through the product prices by including the R&D investments of the producers in the production function of the buyers in proportion to the purchases done[40]. We have followed several previous writers and used the term 'rent spillovers' for this effect. True knowledge spillovers, however, are ideas borrowed from other researchers, and one would think that these spillovers increases with the technical relatedness and geographical closeness of firms. According to this view, measures based on product fields, patent classes, types of R&D, R&D cooperation or qualifications of R&D personnel seems most suited to constitute the weights in equation (7), maybe augmented with geographical distance[41].

Estimating knowledge and rent spillovers. It is widely acknowledged in the empirical literature that it is hard to distinguish knowledge spillovers from rent spillovers. Jaffe's (1986)

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[38] The basic idea is most completely spelled out in Griliches (1979, 1995), but was first applied by Brown and Conrad (1967) at industry level data. Branstetter and Sakakibara (1998) build on Jaffe (1986) in their particular implementation of the framework.

[39] Many of the studies reviewed by Mohnen (1996) use industry level data rather than firm data.

[40] Note that this way of getting around the lack of quality adjusted price indexes, will miss out on 'spillovers' in final product markets, i.e. the increases in consumer surplus represented by the fourth term on the right-hand side of equation (6).

[41] Cf. e.g. Jaffe (1989), Jaffe et al. (1993) and Adams and Jaffe (1996) for the relevance of the geographical dimension.

methodology is probably the one that comes closest to looking for the first type. Following suggestions in Griliches (1979), he links an outside pool of R&D to firm performance. Jaffe also extends the basic framework by controlling for differences in technological opportunities across different sectors and by allowing for the amount of spillovers received to depend on the firms' own R&D investments. His key contribution, however, lies in the implementation of equation (7). To isolate pure knowledge spillovers, he uses the degree of overlap in the distribution of firm's patents to construct the proximity weights, since patents are classified according to *technological* criteria. Furthermore, he uses the constructed spillover pool as an explanatory variable in a *knowledge* production function, utilizing count data on patents as a proxy for output. The coefficient on the spillover pool is therefore quite likely to represent pure knowledge spillovers. By studying the effect of the same spillover pool on profits and market value, he also shed light on the effect of negative rent spillovers due to increased competition, as the estimated coefficient then is likely to be a mixture of this effect and knowledge spillovers. Without underplaying the methodological difficulties associated with his work, Jaffe argues that the sum of 'circumstantial evidence' brought out is enough to make a good case for the existence of spillovers.

Positive rent spillovers from research embodied in intermediate inputs may best be investigated using a spillover pool whose weights are based on intermediate input flows. There are several weaknesses associated with this approach, however. First, as technical information may be exchanged between suppliers and customers, such a measure may pick up some pure knowledge spillovers as well. Second, data on firm level input-output flows are extremely rare and we know of only one microeconometric study of this type, namely Fecher (1992)[42]. Finally, rent spillovers to final consumers, i.e. increased consumer surplus associated with new goods or production techniques cannot be measured using this framework.

In theory, rent spillovers should be measured as the area under the final good's demand curve. As noted by Bresnahan (1986) this may be done either by econometric techniques or by index-number techniques[43]. Bresnahan uses index numbers to measure the rent spillovers from the computer industry to consumers through the effect of computers as inputs in the financial sector. The computer industry is chosen because quality adjusted input prices are needed, and these have been estimated for this industry using hedonic techniques. However, hedonic techniques are not suited to handle large product changes, i.e. the introduction of qualitatively new product characteristics. Partly for this reason Bresnahan only covers the period up to 1972

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[42] This is according to the survey of Mohnen (1996). Note, however, that there exists a large number of studies based on industry level data which use input-output matrices to construct the weights.

[43] Cf. Mansfield et al. (1977) for an early study utilizing this idea.

when traditional mainframes were challenged by software advances and large mini computers[44]. Trajtenberg's (1983, 1989) study of computed tomography scanners, on the other hand deals explicitly with the problem of measuring the welfare gain from the introduction of qualitatively new goods, and the challenges involved in correctly measuring the welfare gains from new goods are also discussed in a recent book edited by Bresnahan and Gordon (1997). The evidence gathered there indicate that the increase in consumer surplus associated with the introduction of new goods may be substantial, and that ordinary price indexes are likely to underestimate the welfare gains.

Surveying surveys and adding a grain of scepticism. Griliches (1997, ch. 5) summarizes available econometric studies to find social rates of returns to R&D which tend to be several times larger than the private ones, and Mohnen (1996) listing more than 50 studies, concludes that "spillovers exist and have to be taken into account when evaluating the returns of government-financed R&D". Other surveys, such as Griliches (1992), Nadiri (1993), the Australian Industry Commision (1995), Hall (1996) and Jaffe (1996) agree, and their conclusions are not controversial.

There is no reason to doubt the existence of positive spillovers, but considering the difficulties involved simply in constructing a measure of the stock of knowledge and next the uncertainty over what is an appropriate lag length, it is somewhat remarkable that almost all studies trying to estimate something as intangible as knowledge spillovers actually report significant results[45]. There are at least three possible pitfalls which justify some concern. First, the results may be subject to what Griliches (1992) calls a publication filter, self-imposed by researchers working in the field or imposed by editors and referees considering non-significant coefficients to be of little interest. Second, some of the effects interpreted to be spillovers, may actually be knowledge transfers which are internalized in the market, e.g. through cooperative agreements. Third, the reported significant coefficients could to some extent be spurious, reflecting correlated unobservables across technologically related firms. Griliches (1998, p. 281) mentions in particular common technological opportunities, but correlated productivity shocks or measurement errors would have the same effect. This potential bias is closely related to the problem of estimating the counter factual outcome in the presence of spillovers, that we discussed in section 3.

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[44] Another reason was that the regulation regime in the financial sector changed about that time.

[45] Cf. Geroski (1991) and Geroski et al. (1998) for studies which do not find significant spillovers. These studies differ from others in that they base the spillover pool on innovation count data rather than on R&D investments. The Bureau of Industry Economics (1994) also find rather modest spillovers in their 16 case studies.

5 R&D spillovers and the case for government support

As emphasized already, the concerns raised above do not imply that we have doubts about the existence of spillovers, but there remain some questions concerning the existing estimates. In this section we rise another set of questions concerning R&D spillovers, now taking a closer look at what policy implications can be drawn, *given* that spillovers exist. If spillovers can be received costlessly, it is quite obvious that the arguments in favor of subsidies are valid. Firms performing R&D do not reap the whole benefit, and as they equate marginal cost to marginal *private* benefit, their investments will be below the social optimum. There is, however, a number of reasons why this argument is incomplete, and we will below discuss four issues that deserve further attention in the evaluation of the net welfare gains associated with R&D subsidies. In section 5.1, we consider how private investment in R&D is affected by spillovers when a firm cannot receive such spillovers without incurring own R&D activity, and section 5.2 discusses some of the recent insights from studies of R&D spillovers when such spillovers affect foreign as well as domestic firms and consumers. In section 5.3 we give some remarks on coordination through R&D joint ventures and similar market arrangements, while section 5.4 considers implications of spillovers transmitted through the mobility of research workers. Discussing these issues, we hope to make clear why and how evaluation studies often need to go beyond the topics reviewed in sections 2 and 4.

5.1 Costless spillovers *versus* complementary R&D activities

Geroski (1995) points out that even if one accepts that involuntary diffusion of knowledge happen and that this knowledge have commercial value to some of the recipients, it is still one thing to argue that spillovers exist and another to argue that they undermine incentives to innovate. Geroski's point is that firms must typically invest in research themselves in order to benefit from external knowledge pools. This argument was emphasized in Branstetter and Sakakibara (1998), cf. section 2 above, and has perhaps been most forcefully stated by Cohen and Levinthal (1989)[46]. Cohen and Levinthal discussed in detail how a firm's own R&D activity tend to enhance the absorptive capacity of R&D results produced in other firms. If such a complementary relationship exists, "the analogy between spillovers and manna from heaven" is misleading and it is "not clear exactly what 'bits' of knowledge have been produced by one's own learning efforts and which have spilled over from rivals" (Geroski, 1995). In this situation, spillovers may actually

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[46] Important empirical evidence is presented by Mansfield (1981) finding that imitation costs on average are about 65 percent of the original innovation costs.

stimulate R&D[47]. The returns to own R&D increases in the size of the spillover pool, and this creates a positive feedback mechanism between the R&D investments in technologically related firms[48]. A negative effect due to imperfect appropriability still exists, but it is counteracted by an 'absorption' incentive, and consequently the net effect of spillovers on R&D investments is ambiguous[49].

The empirical evidence on the relationship between own and others R&D suggests that complementarities in R&D is important in many cases. In addition to the empirical results presented by Cohen and Levinthal (1989) and Branstetter and Sakakibara (1998), Jaffe (1986) and Geroski et al. (1993) find a complementary relationship between own and others R&D[50]. Despite the rapid growth in the theoretical literature on R&D investment, spillovers and welfare, however, little attention has been paid to the role of such complementarities and no results seems to be available discussing to what extent a market equilibrium will lead to too little investment in R&D in this case[51]. A rather bold suggestion is that technology policies may be too focused on sectors such as aircraft, semi-conductors, computers, electronics components and communication equipment, where innovations tend to be complementary according to Levin (1988). The apparent paradox, that one observes coinciding high spillovers and high R&D investments in industries like these (Spence, 1984), may indicate that these are industries where spillovers do not undermine the incentives to innovate, and where rivalry and strategic interaction may even lead to excessive R&D investments.

Is 'a big push' from government R&D subsidies needed? Complementarity in R&D activities, as discussed above, is related to the discussion of governmental support to emerging industries. A significant portion of the support to commercial R&D are targeted towards new, high-tech businesses and emerging technologies, and it seems to be based on infant industry arguments. That is, support to targeted high-tech sectors is often rooted on the view that government support is needed to get emerging industrial activities to 'take off' and reach 'a critical mass'.

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[47] Cf. Cohen and Levinthal (1989) for a formal analysis.

[48] It has been argued that a similar complementarity exists between the knowledge stock and new investments in R&D within individual firms, cf. Klette (1996), Klette and Johansen (1998) and the references cited therein.

[49] Note, however, that if R&D investments can be divided into innovative research on one hand and imitation costs on the other, it may be that only imitation costs are complementary to the spillover pool. If this is the case, there will be no positive feedback, i.e. it might be that firms with a deliberate imitation strategy contribute little or nothing to the spillover pool.

[50] Bernstein (1988) finds a complementary relationship in R&D intensive sectors while firms in sectors performing little R&D tend to substitute spillovers for own R&D.

[51] A study that comes close is Kamien and Zang (1998), which contains a model emphasizing the complementarities in R&D-activities across firms.

Perhaps surprisingly, this view might be entirely consistent with the discussion of complementary R&D activities above, where it was argued that such complementary spillovers may encourage investments in R&D. The point is, as emphasized by Matsuyama (1995), that complementarities tend to create multiple equilibria where e.g. one equilibrium corresponds to little or no R&D activity in each of the firms while another equilibrium corresponds to high R&D activities in several or all firms[52]. That is, with an emerging industry or new technology, the firms might get trapped in a low-level equilibrium where the lack of complementary spillovers render R&D unprofitable in all firms with the result that the emerging industry never reaches 'the critical mass' and 'takes off'. This suggests that the government might play a coordinating role by triggering higher activity e.g. through an R&D program, until the firms and the industry have reached the high R&D-activity equilibrium.

In the study by Klette and Møen (1998a), they argue that the rationale for government funding of IT-related research programs in Norway can be well understood in these terms. As pointed out in section 2, their findings suggest, however, that the Norwegian IT-programs were not very successful in initiating new manufacturing activities related to IT, and their case study elaborates on the informational difficulties involved. Inspired by Matsuyama (1997), they conclude:

> In contrast to the situation with illustrative and simplistic game theoretic models, in real coordination problems, information is a serious obstacle; - what is the nature of the game, which players are involved, what do the pay-off structure look like and how rapidly is it likely to change? Or in less formal terms; exactly which firms and what activities should be coordinated and in what way? These serious questions are very hard to answer in a rapidly developing field such as information technology and might be particularly hard to solve in a small open economy where a large majority of the innovations take place abroad. We believe that industrial innovation is an activity where coordination problems and 'market failure' often are pervasive, but it is probably also an activity where policy makers and bureaucrats often lack the information needed to improve on the market solution.

Hence, even though complementarities in theory make it possible to improve on the market solution, it is necessary to analyze whether governments in practice have the necessary capabilities to do so before initiating coordination programs.

5.2 International spillovers and high-tech policy

Complementarity between firms in R&D and other activities is a central idea in the 'new trade theory' and 'new economic geography' literature of the last two decades. Much of the policy

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[52] Klette and Møen (1998a) elaborate on this point and gives further references.

debate over support for R&D and innovation is concerned with international competition and 'dynamic comparative advantage', and those in favor of public technology programs are clearly inspired by the infant industry arguments discussed above. The work of Grossman and Helpman (1991) is of particular relevance to technology policy. Grossman and Helpman (1991, ch. 8) show that if spillovers are geographically bounded, history matters, and countries with a head start in accumulation of knowledge can widen their lead over time. Moreover, they show that governments of lagging countries can improve their growth prospects by offering a temporary R&D-subsidy. This may eliminate these countries' disadvantage in high tech industries. Similar results are obtained by Krugman (1987) in the context of learning-by-doing spillovers. However, as demonstrated by Grossman and Helpman (1991, ch. 7), the scope for national policies disappears if knowledge spillovers are perfectly international, i.e. if ideas flow as easily between nations as they do within nations. The extent to which spillovers are 'intranational' or 'international' is therefore an important empirical question. Inspired by these findings, Branstetter (1996) undertook a microeconometric investigation using panel data for US and Japanese firms, and he found evidence that spillovers are stronger within each of the two countries than between them. These results are supported by Narin et al. (1997) finding substantial 'excessive' self-citation when comparing citations across countries, and by Eaton and Kortum (1994) finding that technology diffusion is considerably faster within than between countries[53]. Branstetter concludes that "the idea that promotion of R&D can have an impact on comparative advantage is one that trade economists should take more seriously".

Trade economists working on growth and development are, naturally, focused on export oriented and import competing sectors. However, it is not obvious that these are industries where the case for government support is particularly strong. The total gain from national R&D investments include not only knowledge spillovers, but rent-spillovers to customers and buyers of intermediate goods as well. As argued in section 3 these may be considerable, and in the extreme case of monopolistic competition, often assumed in theoretical models, all profits are competed away such that only rent spillovers are relevant for policy. If a substantial part of the spillovers created through R&D subsidy programs are to the rest of the world, e.g. because the targeted R&D intensive industries are highly export oriented, one may question why the government of the source country should bear the financial burden. This reservation seems particularly relevant to small open economies, but it has also been emphasized by several commentators in the debate over the funding of the ATP-program in the US[54].

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[53] Cf. Mohnen (1998) for recent a review of the literature on international R&D spillovers.

[54] See e.g. Yager and Schmidt (1997).

At a general level, it is not difficult to outline the implications of international R&D spillovers. Governments should only subsidize R&D up to the point where the marginal cost equals the marginal social benefit accruing to its own nationals. When evaluating the marginal social benefit, potential negative repercussions from increased competition due to unintentional spillovers received by foreign firms should be included[55], but also potential positive effects of economic growth abroad[56]. Such positive effects could e.g. be larger export markets and increased political stability in developing countries. Empirical results have obviously not been accumulated to a level which makes it possible to determine what amount of subsidies are optimal according to this theoretical criteria.

Empirical results suggests, as noted above, that spillovers to some extent are geographically bounded[57]. This may justify national technology programs, but the point we want to emphasize is that a careful analysis of the likely distribution of spillovers is necessary as the share of spillovers accruing to non-nationals may be substantial in some sectors. Note also that the existence of international spillovers give scope for increased *global* efficiency through R&D cooperation between countries. The fact that technology policy and R&D programs within the European Union to some extent have been moved from individual member states to the union level since the 1980s, can be interpreted as a response to this understanding[58].

5.3 R&D joint ventures and the Coase theorem

Klette and Møen (1998a) argue that firms seems to internalize spillovers through various market arrangements largely ignored in many of the theoretical models of R&D investment[59]. One aspect of this is that the empirical findings emerging from the literature reviewed in section 4 might be quite misleading, since some of the effects interpreted to be spillovers, may actually be knowledge transfers which are internalized in the market, e.g. through cooperative agreements.

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[55] This effect need not be negative, as increased competition in the home market benefit consumers and other industries through input linkages.

[56] There is a large a theoretical literatur on optimal R&D policies, exploring what may happen under various assumptions regarding degree of competition, degree of intra- and interindustry spillovers, degree of openness, degree of international spillovers, whether there is strategic behavour or not, whether there is R&D cooperation or not and whether R&D of the firms in question are strategic complements or substitutes. Cf. Leahy and Neary (1997a), Leahy and Neary (1997b) and Neary (1998) for recent reviews of this literature and further extentions. The not surprising policy advice in this literature, as we read it, is that it all depends on the assumptions. An important challenge for empirically minded economists, therfore, is to sort out what assumptions are the relvant ones. Alternatively, one can follow Neary (1998) and many others, and conclude that the detailed information required to improve on the market solution is unlikely ever to be available to the policy maker.

[57] This is not only a finding of the literature on international spillover, there is also a literature utilizing national data which strongly support the view, cf. e.g. Jaffe (1989), Jaffe et al. (1993) and Adams and Jaffe (1996).

[58] Well known examples of such programs include ESPRIT, EUREKA and TSER among others.

[59] See Leahy and Neary (1997a) and references cited in that study for a review of recent theoretical studies of R&D joint ventures.

It seems reasonable to believe that firms know who their customers, suppliers and rivals are, and according to the 'Coase theorem' firms would tend to sign contractual arrangements governing the knowledge flows between them[60]. A large number of cooperative agreements observed in the market indicate that this may be an aspect of the externality issue which has been grossly underemphasized. Freeman (1991) reports that "almost all of the top 20 information technology (IT) firms in US, EU and Japan made more than 50 cooperative arrangements of various kinds in the 1980s and some made more than a hundred". With respect to smaller companies, Aakvaag et al. (1996) report that about 60 percent of Norwegian electronic firms participate in technological cooperation schemes. Partner firms often have an interrelated ownership structure, and this is obviously a simple and basic market mechanism for internalizing externalities[61].

Related evidence is provided in the two studies by Zucker et al. (1998b) and Zucker et al. (1998a). In the (1998b) study, Zucker and her co-authors demonstrate that the location of academic experts at the leading edge of basic bioscience strongly influenced the location of new biotechnology enterprises in the US. Further exploring this in the (1998a) study, it was revealed that firms and star scientists were not merely located in the same area, but that the scientist were deeply involved in the operations of the firms. Hence, what might have been interpreted as localized knowledge spillovers using standard methodologies and data sets (cf. e.g. Jaffe 1989), was to a large extent a matter of market exchange.

Our point is not that spillovers are fully taken care of by complete contracting, and we recognize that it is notoriously hard to write complete contracts for uncertain and unpredictable activities such as R&D. What we argue is that both in theoretical and empirical analysis, more attention should be paid to the many contractual arrangements utilized and invented by the firms to overcome the potential spillover problems generated in innovative activities.

5.4 Spillovers and the mobility of research workers

We will end our review of spillover issues related to R&D policy by turning to the labor market. A number of authors have pointed to mobility of labor as an important mechanism for knowledge diffusion[62], and it is most often thought of as a spillover mechanism. Jaffe (1996) making a clear distinction between market spillovers and knowledge spillovers, considers mobility of researchers to be of the second type, writing that "[k]nowledge spillovers also occur when researchers leave

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[60] More precisely, firms will perfectly internalize externalities in the absence of information and transaction costs. See Usher (1998) for a critical view.

[61] See Klette (1996) for a study of spillovers between firms with an interlocking ownership structure.

[62] Cf. e.g. Geroski (1995), Jaffe (1996), Almeida and Kogut (1996) and Zucker et al. (1997) for some recent statments on the importance of labor mobility. Almeida and Kogut (1996) studying patent holders, is particularily interesting, showing empirically that ideas are spread through the mobility of key scientists.

a firm and take a job at another firm". Defining knowledge spillovers as "benefit leakages that occur in absence of a market interaction between the innovator and the spillover beneficiary", this seems a bit inconsistent since mobility of researchers takes place in the labor market. Jaffe implicitly acknowledges this point, writing that "important innovative successes are likely to increase the incentive for researchers to capitalize on their tacit knowledge by moving to another firm or starting their own". We will argue below that from a theoretical point of view, it is not entirely clear to what extent labor mobility really is a spillover, but if it is, we believe it is most correct to analyze it as a market (i.e. rent) spillover.

Our point of departure is that R&D investments not only increase the firms' stock of innovations, it also increases the human capital of the research workers. After all, research is a learning process. This perspective introduces two interesting questions. First, who captures the value of the human capital from R&D activities, and second, how is the firms' investment incentives affected by the possibility that research workers may quit. With perfect labor and credit markets, the answer to the latter question is that the investment incentives are not affected. To the extent that research work has a 'general training' element and increases the researchers future marginal product, they can look forward to corresponding future wages increases (cf. Becker, 1964). This gives the research workers an incentive to bear the cost of the training through lower wages in the beginning of their career, and consequently, a research worker who quits does not impose a cost on his or her employer[63]. If the fairly steep wage profile thus associated with a research career does not suit the researchers' consumption preferences, they can borrow for current consumption towards future wage increases. With respect to the question about who capture the value of the human capital from R&D, this analysis imply that it is the research workers, but they also pay the investment costs. The flip side of this conclusion is that labor mobility is not a mechanism which cause underinvestments in R&D, and should not be considered a spillover channel, either.

A first objection to the analysis above is that the credit markets are not likely to deliver all the necessary services given the moral hazard problems involved in borrowing on future income. This market failure will, evaluated in isolation, cause underinvestment in R&D[64]. If there is larger uncertainty over the future gains from research work than it is over future income from alternative career paths, risk aversion at the individual level will magnify the underinvestment

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[63] See Pakes and Nitzan (1983) for a related, formal analysis.

[64] The utility loss associated with low consumption in the beginning of the career will shift the supply-of-research-labor curve downwards, increase the equilibrium wage of research workers and thereby the price on R&D investments. This will result in R&D investments below the level associated with a perfect credit market.

problem[65]. Imperfections in the labor market may, on the other hand, increase firms' incentives to invest in research work by reducing the mobility of researches across firms. Such labor market imperfections include search costs and asymmetric information about the human capital of the employees. These effects will result in wages being below marginal product, and hence give firms an incentive to invest in workers' general (i.e. non-firm specific) human capital[66].

Determining the total effect of the 'training aspect' of R&D on investments and wages is in the end an empirical task, and little can at this moment be said. In order to investigate the issue, a framework explicitly linking R&D investments of firms with human capital accumulation in research workers, must be developed. Given the increasing number of matched employer-employee data set now becoming available, we think future research in this direction will prove fruitful. It might be essential, however, that these data sets are able to trace the mobility of researchers across establishments, as such mobility and entrepreneurship can be a major component of the pay-off for successful researchers.

6 Conclusions

> We have not succeeded in answering all our problems. The answers we have found only serve to raise a whole set of new questions. In some ways we feel we are as confused as ever, but we believe we are confused on a higher level and about more important things (Øksendal, 1985)

Estimates of the economic returns to R&D projects have gone a long way since this line of research started more than 40 years ago (cf. Griliches, 1958). We have in this paper focused on a relatively small number of recent studies that try directly to evaluate the social returns from subsidies to commercial R&D activities and we have discussed some of the shortcomings in the available studies. When discussing similar problems in making causal inference from observational studies, Cochran (1965) notes that a reader, "if later asked for a concise summary of the paper, may quite properly report: 'He said it's all very difficult'." We recognize that our paper may leave the same impression on our readers, but we also believe, as Cochran emphasized, that "[a] listing of common difficulties is ... helpful in giving an overall view of the problems that must be overcome if this type of research is to be informative." Furthermore, we have tried to emphasize that many of the unresolved questions are ready for further research with tools and data sets within our reach. On the methodological side, a more careful inference of

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[65] It might be the individual's aversion towards high *skewness* rather than high variance which is the more important issue here. Notice that risk-neutrality at the firm level is irrelevant for the argument.

[66] Cf. Acemoglu and Pischke (1998) for a review of some relevant literature.

the magnitude of the impact parameters of interest can be made drawing inspiration from the recent advances in the evaluation literature in labor market econometrics. A more ambitious approach would be to go beyond these largely non-parametric techniques and try to merge the model of performance and subsidy impact with a structural model of how the government allocates the R&D subsidies. A structural model of the allocation of R&D subsidies should address the question of how the government can construct operational procedures to identify R&D projects with high social returns[67], and the empirical analysis based on such a structural model can help us to identify to what extent the government agencies succeed in implementing these procedures. These are clearly interesting and worthwhile research tasks in themselves, and if completed successfully they give us an alternative handle to eliminate the potential selection biases that we discussed at some length in section 3.

A large number of research papers on R&D and spillovers have emerged over the last decade, but we have argued that several theoretical and empirical aspects of spillovers deserve more attention before conclusions about R&D policy can be drawn. Many of the issues we have raised seem to require a more detailed investigation of the nature of the spillovers and also a more detailed investigation into the various contractual arrangements that prevail in the market between firms and between the researchers and their firms.

Finally, an attempt to evaluate the economic returns to R&D subsidy programs seems to require a combination of empirical investigations at different levels of observation. We have argued that the microeconometric approach that has been the focus of this paper, should be supplemented with more detailed case studies to get a more precise estimate of the economic returns from the few, outstanding innovations that might typically generate a very large share of the economic benefits emerging from risk-oriented R&D subsidy programs. On the other hand, in order to estimate the impact of a R&D subsidy program in the presence of knowledge spillovers, we need to look beyond the direct impact of the subsidies on the performance of targeted firms and consider changes in performance of the industries or 'technological clusters' to which the supported firms belong. This may lead us to more aggregated, industry-level analysis. It is encouraging to observe that economic researchers already have many of these elements in the tool box, but they have not yet been fully tied together.

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[67] The practical difficulties in selecting R&D projects with high social returns are discussed in some detail in Yager and Schmidt (1997). The ongoing ATP/NBER project is particularly noteworthy in its attempt to draw on the insights from the econometric literature to resolve some of these difficulties.

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