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1 Introduction

The essays collected in this volume cover most of my work on the relationship of productivity growth to R&D expenditures in industry. That technical change was a major source of measured productivity growth was already clear to me while I was a graduate student at the University of Chicago in the mid-1950s. This was the message emerging from the agricultural data developed by the U.S. Department of Agriculture (USDA) and the national data constructed by the NBER (see, e.g., Barton and Cooper 1948; Schmookler 1952; Fabricant 1954; Abramovitz 1956). It was also clear that such technological changes were not purely "exogenous." They were the result of economic activity, especially where its main purpose was to generate such changes, as in organized public and private research. My teacher, Theodore Schultz, actually attributed *all* the productivity growth in agriculture to public investments in agricultural research (Schultz 1953). While the idea that the rate and direction of technical change were both influenced by economic incentives was neither surprising nor new, there was almost no quantitative evidence for this view then. It was my belief (and presumption) that one could use the newly available econometric techniques to establish such facts and to provide measures of their magnitude. This belief provided much of the inspiration for my work that was to follow.

Technical change is usually measured by changes in some index of total factor or, more precisely, multifactor, productivity (TFP, or MFP) at the firm, industry, or economy-wide level. Besides pervasive measurement errors, there are three circumstances that govern these changes that may be wholly or partially "endogenous" to the economic system: (1) New knowledge spreads through training and the adoption of new equipment that embodies the current "state of the art." Whether one thinks of the diffusion of new technology (and organizations and institutions) as a disequilibrium and learning phenomenon, or as an equilibrium process that is affected by adjustment costs and asymmetric information, is largely a semantic issue. What is important is that this process of diffusion is influenced by economic forces and incentives. (2) New techniques, inputs, and products can all be thought of as outward shifts in the "production possibilities frontier." They result from conscious efforts by scientists, engineers, entrepreneurs, and various other tinkerers, both formal and informal, to improve the existing state of technology. (3) The production of such economically valuable new knowledge depends, at least in part, on the generation of new scientific knowledge in universities and other institutions, both at home and abroad, which is itself subject to economic constraints and influences.

My earliest research concentrated on the first two topics: an econometric analysis of the diffusion of a new technology, hybrid corn (Griliches 1956, 1957b), and the measurement of the returns to public and private research investments in this and related technologies (Griliches 1958b). Central to the first paper was the concept of a diffusion curve or path. For the purposes of this analysis, I used the logistic curve in which "time" is essentially exogenous (as it was also to be in the concurrent and subsequent theories of technological change). I was not entirely happy with such a formulation and had explored in an appendix to my thesis an alternative model that made the rate of adoption a direct function of profitability, with improvements in the "quality" of the technology (rising relative yields of hybrid versus open pollinated corn) and the fall in its price as its major driving forces. The arrival of partial-adjustment distributed-lag models led me to try them as an alternative framework for the analysis of technical change in my work on the demand for fertilizer in agriculture (Griliches 1958a). That work interpreted the growth in fertilizer use as a lagged response to the continued decline in its real price.

At about the same time, I started working on the direct measurement of technological change using output over input indexes. This was based on the earlier work in agriculture summarized for me by Schultz (1953; see also Ruttan 1954, 1956) before the topic was transformed by Solow's (1957) elegant reformulation. The stylized facts that had emerged were quite troublesome: the lion's share of the observed growth in output was attributed to "technical change" or, more correctly, to the "residual."

Having come to this problem with a background in econometrics, I found the spectacle of economic models yielding large residuals rather uncomfortable, even when the issue was fudged by renaming them technical change and then claiming credit for their "measurement." My interest in specification analysis (Griliches 1957a) led me to a series of questions about the model used to compute such residuals and also, especially, about the ingredients—the data—used in the model's implementation. This led me to a research program that focused on the various components of such computations and alternatives to them: the measurement of the services of capital equipment; issues of deflation, quality change, and appropriate depreciation concepts; the measurement of labor input and the contribution of education to its changing quality; and most relevantly for this volume, the role of left-out variables (inputs) such as public and private investments in R&D. I also worried about formula misspecification issues, especially economies of scale and other sources of disequilibria, which led me to a continued involvement with production function estimation. This program of research, which was announced, implicitly, in "Measuring Inputs in Agriculture" (Griliches 1960) and found its fullest expression in my two papers on agricultural productivity (Griliches 1963, 1964), has served me well. It was similar, in certain aspects, to the line pursued by Denison (1962) at about the same time, except that I put more emphasis on its econometric aspects, that is, on the explicit testing of the various proposed adjustments and attributions to sources of growth.

It was in this context that I turned to an analysis of the contribution of public expenditures on agricultural research to overall productivity in agriculture, using U.S. state data. By the mid-1960s I was moving to more general analyses of the productivity growth puzzle in manufacturing and in the economy at large (Griliches 1967; Jorgenson and Griliches 1967) and beginning my search for microdata with the hope that such processes could be better studied at lower levels of aggregation (see, e.g., Griliches and Ringstad 1971). More recently, I have started looking at the third aspect of this puzzle: the role of science and the productivity of the social resources invested there (Adams and Griliches 1996). The essays collected in this volume are, however, limited largely to the second range of topics, chronicling and describing the quest to understand and measure the contribution of R&D to the growth of productivity, both at the firm and industry levels.

I should note that by the time I shifted my attention to the contribution of industrial R&D a few others were already studying some of its aspects. The pioneers were Minasian (1962), who analyzed the relationship of productivity growth to R&D expenditures in eighteen chemical firms; Mansfield (1965), who analyzed the growth of productivity for ten chemical and petroleum-refining firms and ten two-digit-level manufacturing industries; and Terleckyj (1958), who used two-digit SIC-level industry data to attack the same question (see the summary of his work in Kendrick 1961, 181–84). Related work was also being done by Conrad (Brown and Conrad 1967), Nelson (1959, 1962), Scherer (1965), and Schmookler (1962).[1] I viewed my own contribution as pushing this line of research forward using much larger and more representative databases and employing more advanced econometric techniques.

This volume starts with my 1979 essay, "Issues in Assessing the Contribution of Research and Development to Productivity Growth," which provides a framework for much of the research that was to come and also identifies and

1. At one point, Conrad, Mansfield, Scherer, Schmookler, and I all belonged to the same study group at Princeton, which was organized by Jesse Markham with the support of the Ford Foundation. Nelson and I also served on the planning committee for the 1960 Minnesota conference, cosponsored by the NBER and the Social Science Research Council, on the rate and direction of inventive activity, under the chairmanship of Simon Kuznets.

describes many of the difficulties that haunt this line of research to this day.[2] It exposits the R&D capital model which has become a standard tool in this field and outlines the spillovers problem and suggests several approaches to its solution, some of which were taken up later by my students (e.g., Jaffe 1986) and/or reinvented (Romer 1990). Chapters 3 and 4 report the results of a major, two-decades-long effort to gain access to the detailed individual firm–level data on R&D collected by the Bureau of the Census for the National Science Foundation (NSF) and to match them to parallel company data in the censuses of manufactures and enterprise statistics. Chapter 3 was started in the mid-sixties, presented at the NBER Conference on Research in Income and Wealth in 1975, but not published until 1980. It analyzed the 1957–65 growth rates and the 1963 levels of productivity for 883 U.S. manufacturing firms and related them to their past R&D expenditures (among other variables). It concluded that the various estimates "indicate an overall elasticity of output with respect to R&D investments of about .07, which can be thought of as an average of .1 for the more R&D-intensive industries such as chemicals and .05 for the less intensive rest of the universe. . . . [It implies] .27 as the overall estimate of the average gross excess rate of return to R&D in 1963. . . . It is 'gross' because . . . our measures [do not] allow for any depreciation of past R&D investments, and it is 'excess' because the conventional labor and fixed capital measures already include the bulk of the current R&D expenditures once" (71).

I started discussing the updating of this study with the NSF and the Bureau of the Census in 1976. What looked at first like a reasonably simple job became a major effort when it turned out that the old project tapes had been inadvertently blanked by the Census and that the data for the 1958 and 1963 census of manufactures could not be retrieved in machine-readable form. Luckily, most of the original R&D schedules could still be found, though they had to be repunched from scratch. After much work a new data set was created, covering 652 U.S. manufacturing firms for which growth rates could be computed for the 1967–77 period and which could also be matched to 1972 Census schedules. This took much effort and time, as did the analysis stage, so that the final results were published only in 1986. The major advance in this study was the ability to distinguish between basic and other R&D expenditures, and between privately and federally financed R&D expenditures. The interesting findings were (1) R&D contributed positively to productivity growth and seemed to have earned a relatively high rate of return. Moreover, there was no evidence of a decline in returns between the two studies, the first covering 1957–65 and the second 1966–77. (2) Basic research appeared to be more important as a productivity determinant than other types of R&D. (3) Privately financed R&D expenditures were more effective, at the firm level, than federally financed ones. The analysis of these data was updated in the 1980s by

2. Most of my relevant earlier work has been reprinted in Griliches (1988a).

Lichtenberg and Siegel (1991) and the earlier results have stood the test of time. Additional work using this database has been done recently by Adams and Jaffe (1996).

The difficulties and long delays involved in working with confidential Census data led us to look at other, more "open" data sources. In the 1970s detailed firm income and balance sheet data became available in machine-readable form as part of the Compustat tapes. At the same time the U.S. Patent Office had begun to computerize and make available its records. In the early 1960s I had become friends with Jacob Schmookler (see Griliches and Schmookler 1963) and after his untimely death in 1967 thought that it would make sense to extend some of his research program using the newly available and more easily accessible data and more advanced econometric techniques. All of these considerations led to the initiation of a large, NSF-sponsored research project at the NBER in 1978. The primary task of this project was to match data for U.S. manufacturing firms in the various Compustat tapes (including those on the over-the-counter market) with the records of their patenting activity. That also turned out to be a project of larger size and greater difficulty than we had anticipated, both because of the large amount of merger activity among U.S. firms and because the patent office records did not contain comparable firm identifiers. The latter problem led us to write a complicated lexicographic matching program, which worked, but not perfectly, and required much additional manual checking and correction. Ultimately we constructed a large panel data set, based on the Compustat population in 1976 and covering (to varying degrees) about 900 firms for the years 1958–78. This panel was later extended to 1984 and then again to 1989, and it may be extended again in the near future (see Hall et al. 1988). The effort to analyze these data brought together a number of first-rate students and collaborators. Many of the first round results of this work were summarized in a series of papers presented at the 1981 NBER conference at Lenox, Massachusetts, and published subsequently in Griliches (1984). Three papers from that volume, focusing primarily on the productivity-R&D relationship, are reproduced here.

Some of the results of that project are not covered in this volume, though most of the patent work is discussed in chapter 13, "Patent Statistics as Economic Indicators." Also missing here is work that focused on market value as an alternative measure of success for a firm's innovative endeavors, such as Griliches (1981) and Cockburn and Griliches (1988), and papers whose primary purpose was the development of appropriate econometric methodology (Hausman, Hall, and Griliches 1984; Hall, Griliches, and Hausman 1986).

Chapter 5 was the result of the beginning of my collaboration with Jacques Mairesse of the Institut National de la Statistique et des Études Économiques (INSEE), Paris, which has now continued for close to twenty years. Besides a keen mind and strong econometrics and statistics training, Mairesse brought to the collaboration an interest in microdata and access to parallel data sets collected by French statistical organizations. We also went on, with the help of Fumio Hayashi, to extend our research to parallel data sets on Japanese firms, and to worry about many methodological problems connected with the analyses we were pursuing (see Mairesse and Griliches 1990; Griliches and Mairesse 1998). The major contribution of chapter 5 was to break out of the straitjacket of the confidential Census-NSF data and show that, without imposing reasonable values on other coefficients, the "within" time dimension of the data is not rich enough to deliver a clear estimate of the R&D effect on productivity. We also tried to deal with the simultaneity problem by developing a "semi-reduced-form" system of equations which yielded rather high estimates of the contribution of R&D capital to productivity growth relative to that of physical capital. And it was the first paper to raise the possibility, in this context, that the non–fully competitive environment in which some of our R&D-intensive firms operate may affect the interpretation of such results. This issue would reappear in the literature spawned by R. E. Hall's 1988 paper and was reanalyzed by Klette and Griliches (1996). I shall come back to this topic under the rubric of "unfinished business" in chapter 12.

One of the problems with firm-level data is that firms, especially large U.S. firms traded on national stock markets, are often not homogeneous entities but rather conglomerates of various types of activity. In this sense, our data were still not "micro" enough. Chapter 6, written with Kim Clark, tried to get around this problem by using proprietary data at the "business unit" level, a more homogeneous subdivision of the firm. The analysis was based on 1970–80 data for 924 such business units in manufacturing. It found a significant relationship between TFP growth and R&D expenditures, implying a rate of return on the order of 18 percent, and no evidence of any deterioration in the productivity of R&D during the 1970s. It did find some evidence of spillover effects in the sense that returns to R&D were higher in those businesses where major technical changes had occurred within the recent past.

Chapters 7 and 8, written jointly with Jacques Mairesse, represent our effort to extend such analyses beyond the United States and to provide them with a comparative perspective. Chapter 7 focuses on the French-U.S. comparison; chapter 8 extends it to parallel Japanese data. In doing that we encountered many measurement and comparability issues, but the basic message kept coming through: R&D is important, but the differences in the productivity growth experience across countries do not seem to be connected to the R&D process—the estimated coefficients are largely the same. Nor can R&D account for the worldwide slowdown in productivity growth that occurred in the late 1970s and early 1980s. The estimated effects are just too small for that.[3]

Chapters 9 and 10, written with Frank Lichtenberg, take us back from the

3. The issue of the worldwide slowdown in the growth of productivity has preoccupied me and other researchers through much of the past two decades. For a further discussion of the role of R&D in this see Griliches (1988b).

micro level to the more aggregate industry level. The problem with such analyses is the difficulty of matching industry definitions across data sources. As part of this effort, I was instrumental in bringing the Penn-SRI-Census four-digit SIC-level database to the NBER where it has been continually updated (see Bartelsman and Gray 1996 for the latest revision). Industry-level R&D series have been available only at the two-digit SIC level for total R&D and at the more relevant product-field level, for a mixture of two- and three-digit-level industries, for applied R&D expenditures only. The focus in chapter 9, which uses time series on 28 two-and-a-half-digit manufacturing industries, is again on whether there was a decline in the "fecundity" of R&D expenditures over time and on the role of federally financed R&D expenditures in this story. It finds a strong relationship between TFP growth and private (but not federally financed) R&D expenditure intensity, and no observable decline in this relationship through the period of observation (which ended in 1976). A more detailed industrial look is taken in chapter 10, using the three-digit SIC business-line level R&D data collected by the Federal Trade Commission (FTC) in 1974 under Scherer's leadership. Using the NBER four-digit-level productivity database, we constructed TFP measures for 193 manufacturing industries for the years 1959–78 and used alternative measures of product and process R&D and own (used in the same industry) and imported R&D (based on the 1974 "technology flows" matrix constructed by Scherer [1984]) to reexamine some of Scherer's (1982) conclusions. We found that own R&D has a relatively large and "significant" estimated rate of return, on the order of 0.30, and that it does not decline significantly between 1959–68 and 1974–78, while the contribution of imported R&D, which appears to be sizable, cannot be estimated with much precision. This may be due, in part, to our use of manufacturing industries only, while imported R&D may have its largest effects in some of the nonmanufacturing sectors such as agriculture, transportation, and finance.

Most of the work discussed up to now, except for the papers with Lichtenberg, is based on firm- or business-unit-level microdata, measuring largely *private* returns to R&D, rather than the possibly more interesting *social* returns. I was well aware of the importance of knowledge spillovers in the generation of technological change (see the lengthy discussion in chap. 2), but had no access to data which would allow an entry into this topic. I did, however, send a number of my students on the search for such spillovers and some of them were able to produce evidence for their existence (Evenson 1968; Evenson and Kislev 1973; Schankerman 1979; Jaffe 1986). Chapter 11 was written partly in response to the appearance of the "new growth theory" and the rather limited acquaintance of its practitioners with the previous empirical literature on this topic. It recycled parts of the 1979 "Issues" paper, to remind them of its existence, and surveyed the accumulated literature on spillovers and the conceptual and econometric problems associated with their estimation. The evidence that had accumulated in the meantime was actually quite impressive and, if taken at face value, would assign a greater role to R&D in the generation of productivity growth and possibly also in its slowdown.

There were several components to the new growth theories relevant to our story: the emphasis on endogenous technical change, the emphasis on R&D spillovers, and the importance of imperfect competition in the R&D context. That technical change was endogenous was not "news" to me. That is what we had been studying all along in our work on diffusion (Griliches 1957b), on the role of purposive R&D expenditures as generators of such change (Griliches 1964), and on the impact of the economy on inventive output (Griliches and Schmookler 1963). And the theory was not particularly new either (see the citations in chap. 11).

The other main component of this literature was the emphasis on the importance of R&D spillovers and their incorporation into aggregate growth models.[4] While this was not great news to us, the increased interest in this area generated by the literature revitalized both the theoretical and the empirical research on such spillovers, especially at the aggregate level (Coe and Helpman 1995 is a leading example of such work). But estimating such spillovers is very hard and the empirical results to date do not justify, in my mind, the original claims made for such theories. The estimated magnitudes are just not large enough to explain major differences in growth rates over time and over countries. Knowledge and knowledge generation is indeed the major source of productivity growth in the long run. But our ability to describe and quantify its flows is still quite rudimentary (see chap. 14 for further reflections on this topic).

A major advance brought about by these new theories was the explicit integration of the R&D process with market equilibrium in imperfectly competitive markets. While the older literature was well aware of the "appropriability" problem due to the "nonrivalrous" nature of R&D output, this had not been fully integrated into the earlier growth models or reflected adequately in empirical work (though there is already some discussion of it in chap. 5; see also Klette and Griliches 1996 for a more detailed exposition of the implications for empirical estimation of such models). The new theories also revived interest in "creative destruction" (see, e.g., Aghion and Howitt 1992) of knowledge-based quasi-rent positions, a topic discussed earlier under the label of "depreciation" of knowledge or R&D stocks (see Pakes and Schankerman 1984), and they gave impetus to a whole new range of studies (such as Caballero and Jaffe 1993 and Putnam 1997).

It is beyond the scope of this introduction to survey all the parallel and subsequent work by others on this range of topics. Chapters 2 and 11 already do

4. See Romer (1986, 1990) for the original statement and also chap. 2 in this volume for the simple spillover equation, then Barro and Sala-i-Martin (1995) and Grossman and Helpman (1991) for excellent expositions of this literature.

some of that. Moreover, a number of high-quality, comprehensive surveys have appeared recently: Australian Industry Commission (1995, vol. 3, app. QA); Mairesse and Sassenou (1991); Mairesse and Mohnen (1995); Nadiri (1993); and Hall (1996) (see also Griliches 1995). The first reference alone lists 27 studies estimating the returns to R&D at the firm level, 28 at the industry level, 10 at the country level, and 20 studies for agriculture alone. By and large they confirm the results of our earlier studies and I feel comfortable reproducing that work in this volume. There are, however, a number of methodological problems afflicting most of these studies. Some of them have already been outlined in chapter 2 and the output measurement problem will be reemphasized in chapter 14. Chapter 12 describes some problems that arise in defining and measuring R&D "capital" and its depreciation and the econometric problems involved in getting credible estimates of the returns to it. The chapter also presents new estimates of the contribution of R&D to production at the firm level, using some of the latest econometric techniques. The basic results are not changed much by updating either the data or the estimation technology, but their credibility remains shaky, since the causal interpretation of R&D affecting subsequent productivity levels is based almost entirely on the assumed lag structure. Ideally we should have some "natural" experiments which would produce exogenous changes in R&D expenditures. An analysis of the consequences of unanticipated R&D tax credits which impinge differentially on different firms depending on their tax situation might prove useful in this context. Another source of identifying assumptions could be the various governmental R&D subsidy programs pursued at different times and with different intensities in France, Israel, Japan, Norway, and other countries. Changes in such programs could serve as a source of exogenous shifts that would help us to identify the productivity effects of R&D more credibly. In the meantime we have to be careful and modest in interpreting such results. They are the best we have, but much remains to be done to pin them down more securely.

All of the work discussed above focuses primarily on the role of R&D as an *input* into the productivity-growth-generating processes. But by the time R&D effects are measured in the subsequent data on productivity growth, they are rather difficult to trace, both because of long and variable lags and because productivity measures are subject to many other influences and measurement difficulties, such as business-cycle-induced fluctuations in capacity utilization. It would be nice if one had a direct measure of the *outputs* of inventive activity to use in evaluating the effectiveness of various incentives for investing in R&D. Such considerations and hopes led us to invest heavily in the study of patent data and what they could teach us about the process of invention. A number of good students and research associates were involved in several projects arising out of that data collection effort and we published much on this topic. Chapter 13 does several things: (1) It summarizes much of our own work on this range of issues. (2) It sets it in the historical context of the field and also provides brief summaries and references to the rest of the relevant literature, not just to our own. (3) It discusses a major substantive issue, namely, whether one can take the declining ratio of patents received per R&D dollar as an indicator of diminishing returns and a decline in the fecundity of R&D. I will not summarize here what is already a long survey summarizing many other studies except to note that the answer to the implicit question in (3) is "not proven."

When I wrote chapter 13, I was under the illusion that we had mined out this topic and that I was closing the subject. Luckily, I was wrong. A number of very interesting studies of patents, their role in innovation, and what they can teach us about it have followed, many of them done by my students and their students. A short list of these studies, beyond those already mentioned above, would include Cockburn and Henderson (1997); Henderson and Cockburn (1996); Jaffe, Trajtenberg, and Henderson (1993); Kortum (1993); Kortum and Lerner (1997); Lach (1994); Lanjouw, Pakes, and Putnam (1996); and Schankerman (1997). (See also Scherer 1996 and Van Reenen 1996.)

The final essay in this volume, my presidential address to the American Economic Association, returns to a major theme of chapter 2: the difficulty of estimating the productivity returns to R&D in a world of imperfect output measurement. Many of the current technological changes are occurring in sectors where we have almost no decent measures of output, such as health and finance, or where these measures are flawed, such as the increasingly complicated high-tech computer and communication sectors. Moreover, these sectors have been growing in importance, accounting currently for close to two-thirds of the overall U.S. economy. This essay is a call for a review and expansion of the national income accounts to include more and broader measures of economic welfare, especially those that would bring the value and quality of human time into this framework. It is also a meditation on why we are not getting better data and a plea for a lowering of expectations of what economics can deliver in this area. Inventive activity is a truly creative and highly uncertain act. We can study it, try to comprehend it better, and support it, but we are unlikely to be able to control it finely, nor should we try.

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