veloped for the reader the intellectual environment in which Boas worked. Three essays follow which are devoted exclusively to Boas. The first of these, based largely on the Boas papers in the American Philosophical Society, deals with his intellectual development as a student, his change from geographer to ethnographer, his rejection of a materialist philosophy, and his growing emphasis on the role of historical phenomena in the development of given cultures. When Stocking deals with Boas' physical anthropology, he emphasizes its many analogues with the present-day activities in that subdiscipline. But though Boas' work in physical anthropology was effective in separating the biological from the cultural, and thus may be considered transitional in the formulation of the modern culture concept, I do not see how we can attribute the same role to him with regard to present-day views of race and biological evolution. Boas was rediscovered by physical anthropologists long after their theory and methods had developed from other roots. Stocking is particularly impressed with Boas' use of physical anthropology to elucidate historical problems, an approach not typical of later American physical anthropology. To further emphasize Boas' contribution to the final separation of race and culture as distinct concepts, Stocking includes an essay on the tenacity of Lamarckian social thinking, which continued to blur the distinction.

In the final essay Stocking proposes some reasons why Boas' importance is not generally recognized today: It is easy today to take Boas for granted; without an awareness of the intellectual environment in which he worked, it is difficult fully to appreciate his contribution. Moreover, the cultural-evolutionary thinking that has had a recent resurgence in anthropology is of course fundamentally contrary to Boas' historical approach. Yet it is Stocking's argument that during the professionalization of anthropology in America, at the turn of the century, the Boasian culture concept became established; it not only set the basis for modern anthropology, but also diffused into the other behavioral sciences.

Stocking's discussion of his own methodology as a historian should produce some appreciation of the complexity of historical interpretation and thus some apprehension about accepting simplistic attempts to use history to legitimize current points of view. Stocking chooses to write intellectual history in terms of the content of ideas. The problem is to understand what a man thought, why an idea, perhaps now rejected or irrelevant, once seemed reasonable to a given individual. Quite naturally, which ideas and what men the historian chooses to deal with is ultimately influenced by an interest in the present. For Stocking the challenge is to understand the development of the modern concept of culture, because in his opinion "much of the social sciences of the 20th century may be seen as a working out in detail of the implications of the culture idea." Although anthropology is, in Kuhnian terms, in a preparadigmatic state in which historical interpretations are easily determined by competing contemporary points of view, the closest thing the field has to a paradigm is the concept of culture.

But why should we trust Stocking's historical interpretation over that of historian X or anthropologist Y? Does Stocking validate his conclusions by the same rigorous means expected of the natural scientist? I think not. Initially he leads us to be hopeful by making explicit his concern for sampling and analytic procedure. Yet in writing these essays he found it necessary to revert to "an approach in more traditional intellectual historical terms." If Boas was responsible for something approaching a paradigm shift in anthropology, culminating perhaps in *The Mind of Primitive Man*, Stocking has not helped us to understand this break with traditional thought.

The question of methodology is important, for, as Stocking admits, part of the kinship of historians and anthropologists is based on their common concern with the evident impossibility of subsuming their subject matter within the framework of nomothetic explanation.

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Explaining the Growth of Output

Industrial Research and Technological Innovation. An Econometric Analysis. EDWIN MANSFIELD. Published for the Cowles Foundation for Research in Economics at Yale University by Norton, New York, 1968. xx + 235 pp., illus. $7.50.

The Economics of Technological Change. EDWIN MANSFIELD. Norton, New York, 1968. x + 260 pp., illus. $6.95.

The economist's deepest concerns are with the evolution of output, especially in relation to the labor force and to population, its allocation in use between consumption and investment, and its distribution among the population. One central empirical fact is the rise over time in per capita output, at least in the countries that we like to think of as "advanced"; a second is the enormous disparity in per capita output among the nations of the world. Classically, economic theory has one general hypothesis that contributes to the explanation of these two phenomena. The output of a nation depends not only on its labor force but also on the material resources at its disposal, capital goods and natural resources; the American worker produces more because he has more tools. Probably most economists at any time would have agreed that, as technological knowledge expanded, the productivity contribution of a given quantity of capital goods was increasing; but the problem was first put into sharp quantitative perspective by the empirical work of Abramovitz and Solow (*1*) over a decade ago, which showed strikingly that on any reasonable assumption the growth of capital and labor as conventionally measured was totally inadequate to explain the growth in output in the United States. It appeared that there must have been an increase in the efficiency with which given resources (capital and labor) were being utilized in production. The measure of this efficiency is termed *total factor productivity* (*2*). Later studies made it similarly clear that total factor productivity differed very considerably from country to country, that is, that

international differences in per capita income are far from being explained by differences in capital per capita.

It was natural to assume that the increase in total factor productivity was in good measure due to technological progress, to the increase in knowledge represented by new and better machines. (An additional explanation is the increased skill of the labor force, due in good measure to educational levels which are rising over time and differ across countries; but this factor probably explains no more than half the historically observed growth in factor productivity.) The question then arose, To what extent could technological progress be affected by policy? More specifically, to what extent was it the result of research and development decisions by firms and by nonprofit organizations, the government, and the universities; and to what extent are research and development decisions, in turn, the result of economic incentives which would be affected by economic policy? Closely related, and perhaps especially important for underdeveloped countries, what are the factors, particularly the economic factors, in the diffusion of existing technological knowledge?

Surprising as it may seem, the links among research and development, innovations, and increases in total factor productivity are far from clear. The enormous acceleration in R & D (even apart from governmental support) over the last 30 years has been accompanied by only a mild increase in the rate of increase of total factor productivity and by no increase at all in inventions, at least as measured by patents. There is considerable direct evidence that productivity can increase without any change in process big enough to be termed an invention. The effectiveness of R & D in increasing inventions is at any rate difficult to measure.

What is needed is a whole series of investigations at various levels of generality in the economic system and with various methods. Though qualitative analysis of inventions is an old staple of economic historical research, quantitative methods capable of giving more exact knowledge and of assessing the relative importance of different factors only dates back some 15 years. Since 1961, Edwin Mansfield has been conducting a wide variety of econometric investigations into different aspects of these interrelations, now collected into the volume *Industrial Research and Technological Innovation.* At the same time, he has prepared a survey of the entire area, covering his work and an amazing variety of other sources (the works of industrial engineers, operations researchers, and sociologists, as well as economists, are drawn upon), in *The Economics of Technological Change.*

The latter is undoubtedly the most comprehensive, fairest-minded discussion of the field in being. The author neither gives excess prominence to his own research nor slights it. A large variety of topics are covered and illustrated with both statistical analyses and case studies. Formal theoretical analyses are referred to and summarized, but the developments are not given in detail. An inevitable consequence of the wide coverage is a certain flatness. All topics and points of view are represented in a few paragraphs each. The author, in a laudable effort to avoid premature judgments on disputed matters, has prevented himself from giving emphases which would have lent contour and direction. But as a guide to the literature, there is no substitute.

His research book is an invaluable addition to the literature and indeed constitutes a major portion thereof. In the nature of the case, the data require a great deal of interpretation to yield any results and in any case are not so abundant or so well defined as to be compelling. Mansfield is scrupulous in indicating the limits of his results. But taken together they are impressive. He has documented very well the role of the profit motive in governing initiation of R & D projects, the allocation of resources in a large firm among competing R & D projects, and the acceptance of inventions introduced by other firms. In no case are his results at variance with sociological and psychological theories, which stress communication flows and the like; rather the intensity of certain parameters is related to profitability. For example, the diffusion of an innovation follows a logistic curve, as postulated by social psychologists; but the parameters of the curve are such as to imply more rapid acceptance if profitability is high (*3*) and if the risk, as measured by the ratio of needed initial investment to total assets of the firm, is low.

Another series of results, of great interest for policy, shows that there is no tendency for large firms to have proportionately more R & D than middle-sized firms have. For a given level of R & D activity, they actually seem to produce less inventions (weighted by importance). For a given size of firm, inventions seem to be produced about proportionately to R & D activity, with little evidence of increasing returns to scale. These results have important implications for policy with regard to industrial concentration; they remove the basis for a widespread attack on antitrust policy based on the assumption that large firms are inherently more progressive and contribute more proportionately to technological advance.

Finally, there is a group of studies which seek to estimate the rate of return on investment in R & D and its influence on the rate of technological change. As estimated, the rates of return vary very considerably by industry; interestingly, furniture and apparel have the highest returns. Mansfield also finds a positive relation between technological change and cumulated R & D. However, he is very properly cautious about all these results; very strong a priori assumptions have to be made to make the data yield any inferences at all. The characteristic difficulties of inductive inference from nonexperimental data are strongly displayed here.

The author has not sought to develop general hypotheses about either the motivation for R & D or the impact of R & D on productivity. Doubtless, his caution is well advised in view of the poverty of knowledge and the difficulty of acquiring and interpreting relevant data. There is no question that his work will be a major point of reference and orientation for subsequent work on industrial research and its relations to the capitalist system and the growth of productivity.

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References and Notes

1. M. Abramovitz, "Resource and output trends in the United State since 1870," *Amer. Econ. Rev. Papers Proc.* **46**, 5 (1956); R. M. Solow, "Technical change and the aggregate production function," *Rev. Econ. Statis.* **39**, 312 (1957).
2. Total factor productivity may be thought of as the ratio of output to an index number of total resources, that is, a suitably weighted sum of labor and capital. It thus differs from labor productivity, which is output divided by labor alone, a magnitude convenient for measurement but of relatively little significance. If capital is increasing more rapidly than labor, as has of course been the case historically, then total factor productivity is increasing less rapidly than labor productivity. All these measures, which aggregate over wide varieties of commodities, are affected by very considerable conceptual and technical difficulties of measurement.
3. This had earlier been demonstrated for the introduction of hybrid corn by Z. Griliches in his classic paper "Hybrid corn: an exploration in the economics of technological change," *Econometrica* **25**, 501 (1957).