is not a prohibitive price for a compendium of this sort. Is it worthwhile for those who already own the 1956 *Handbook*? My answer is "yes." If for no other reason, the inclusion of literature references (there are none in the 1956 *Handbook*) makes this volume considerably more useful than its predecessor. Essentially this is an expansion of sections 2 and 3 and parts of section 8 of the 1956 *Handbook*. The expansion has been accomplished in various ways: the data is presented in a more readable form (the type is larger and the tables are less compressed), more data are presented in most cases, and literature references are included. For example: In this volume data on chromosome numbers of organisms is presented in 57 pages and includes 1861 literature citations. In the 1956 *Handbook* equivalent material was covered in five pages and did not have literature citations. In addition, the data is presented, with respect to taxonomic classification, in a much more usable way.

The index is different. It is based largely on taxonomy and the scientific names of organisms. At first this sort of indexing seemed awkward to me, but when I used it in conjunction with the table of contents, as the editors advise, it proved quite workable.

This is not the sort of book which one can "read" and criticize in a comprehensive way. Its value will be ascertained only after repeated searches have been made for specific information. It is quite easy to wonder why certain types of information are not included, but it is probably equally easy to realize that not everyone approached for data compilation was willing to cooperate; the compiling of data is tedious. Even so, I should like to make some criticisms. It seems to me that data on hybrid vigor in corn were somewhat better organized in the older volume—and that it is somewhat less easy for a nonexpert on corn to extract meaningful information from this one. If data on mitotic indexes were to be included at all, why were they restricted to mammals and amphibia? I fail to see the significance of the inclusion of some data on the growth, in organ culture, of tibial rudiments from several animal embryos. In the first place, considerably more data is available about cultured bone rudiments; in the second place, this and a list of tissue culture cell lines is all that is provided about animal tissue culture (the heading of the table on tibial growth in culture contains the single typographical error which I detected: "floating lens technique" instead of "floating lens paper technique"). Data on the effect of temperature on chick development could have included correlative data on humidity involvement. This is an important consideration, and the data is readily available.

Lest I appear carping, let me hasten to indicate that most of the material seems to be quite good. I spent several evenings browsing through the volume, and my general impression is that, in view of what was intended, the volume is quite satisfactory and that it will be useful to have around. I suspect, again, that some of the criticisms which I have raised result from the probable fact that the committee was turned down by those who could have provided the information. These few grumbles are not intended to be destructive of a worthy project which, in most respects, is very well done.

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HISTORY AND PHILOSOPHY OF SCIENCE

On Laws That Govern the Growth of Science

A good deal of the substance of Derek Price's **Little Science, Big Science** (Columbia University Press, New York, 1963. 144 pp. $4.50), which is based on the 1962 Pegram lectures, has been previously published in a shorter form, entitled "Diseases of science," chapter 5 of Price's *Science Since Babylon*. That book was itself based on a series of lectures, given in 1959; so we now have a book based on a series of lectures which were based on a chapter of a book that was also based on a series of lectures. It is, to that extent, a book twice removed from a lecture—and it suffers for it. This apparently accounts for the paucity of credits.

In *Science Since Babylon*, Price put forth two "laws" of the growth of science. It is to these laws that he devotes a major portion of this latest effort. Price attempts to treat science as a measurable quantity. His aim is to elucidate what is new in the age of Big Science that makes Big Science different from Little Science.

Throughout, we find references to "this mathematical analysis"; yet right at the start we are told, "My approach will be to deal statistically in a *not very mathematical fashion*, with general problems of the shape and size of science and the rules governing growth and behavior of science-in-the-large" (p. viii, emphasis added). One worries about a study which is statistical in a not very mathematical way. And one is further confounded by an ominous mixing of metaphors in the next paragraph. In Price's view, the methods he is going to use are similar to those used in thermodynamics. But then we read that, "according to this metaphor [the four lectures are concerned, respectively] with the volume of science, . . . the velocity distribution of its molecules, . . . the way in which the molecules interact with one another, and . . . the political and social properties of this gas" (p. viii).

What Price here describes is, however, nothing that resembles thermodynamics. The analogy, that is closer than any other one, would be statistical mechanics. But the kinds of answers one gets in statistical mechanics depend entirely on the kinds of assumptions that one makes about the interactions and velocity distributions; analogously, the kinds of answers that Price comes up with should be largely dependent on his assumptions. The mathematical machinery used (whatever its status) is a tool that serves only a secondary function.

The first of Price's "laws"—that there is a doubling of science every 10 or 15 years, stretching back to the 17th century—looks, at first glance, like a straightforward empirical determination. It should be pointed out, however, that there has been little agreement among those who attempt to collect such data. But more important, until we know what Price considers to be "science," it is hard to know just how seriously to take this "law." Price himself has worried about this, but he assures us that "Even with a somewhat careless and uncritical choice of the index, taken as a measure, one has little trouble in showing that general exponential growth has been maintained for two or three centuries." It is this very lack of sensitivity to the choice of the variable which so impresses Price, and it is just this lack of sensitivity which raises my doubts. What Price never considers directly, in the context of these logarithmic increases, is the *significance* of the variables he uses as indices.

Then too, Price fails to take note of disturbing discrepancies in his data. For example, it may be true that the number of scientists has been increasing exponentially. But if we assume with Price (Fig. 3 and p. 52) that a fixed number of these people become scientists via the Ph.D. route—this agrees with the findings of researchers at Arthur D. Little, Inc. [*Basic Research in the Navy: A Report to the Secretary of the Navy* **1**, 63 (1959)], how does Price account for the fact that the number of Ph.D.'s in science has remained constant relative to the total number Ph.D.'s that are produced? [Ibid.]. In the same context, one might ask what the growth curves for journals in the humanities look like.

The second "law" of scientific growth is that ". . . all the apparently exponential laws of growth must ultimately be logistic [S-shaped] . . ." (p. 30). We are given many examples—the growth of the bean plant, the production of coal, of zinc, and of copper, the discovery of new chemical elements, and the increase in particle accelerator energy. We could add others—the growth of bacteria cultures, the growth of a human being from an egg, the magnetization of an iron rod, and the like. Assuming that we passed through the midperiod of our logistic growth sometime in the 1940's or the 1950's, Price argues that saturation is already beginning. In *Science Since Babylon* Price saw the approaching saturation as some kind of "doomsday," however he sees here the chance of a new logistic curve rising "phoenixlike on the ashes of the old"—that is, an "escalated" logistic curve [see G. Holton, *Daedalus* (March 1962)]. Then, ". . . saturation seldom implies death, but rather that we have the beginning of new and exciting tactics for science operating on new ground rules" (p. 32).

Though it is true that logistic curves are very frequently a part of our description of nature, nature is not the *description* of nature and nature is not science. Price never makes this distinction clear, and he never justifies the facile identification he has made between logistic curves and the growth of science. It is a guess. But it is not the only possible interpretation. Assuming that we are approaching such a limit, this does not mean, ipso facto, that Big Science will change. The *rate* at which science grows may decrease, but the turnover in problems that constitute science may change at the same old rate.

Price is so impressed with the power of these laws that he is led to make some rather startling revelations. In his view "there cannot have been any great gain or loss to science during the war . . . science is just where it would have been, statistically speaking, ... if there had been no war" (p. 17). This conclusion is reached on the basis of a curve which shows that the rate of increase in physics abstracts was the same before and after the war. Even Price cannot maintain the absurd implications of this, for we later find him ascribing to the war, effects that are quite important in his scheme of things —the tendency for "elite" scientists to form tight-knit groups and the tendency for the growth of multiple authorship.

Price says that to enunciate these curves which describe the growth of science is in some sense to explain the growth (p. 7 and p. 17). But what Price is saying is that Big Science is bigger than Little Science. To explain a phenomenon, we would expect some reasons. For example, Galileo's description of *how* an object falls does not explain *why* the object falls. It is a kinematical description. Indeed, free fall is explained in Newtonian physics only in the sense that it becomes a special case of a still more general description. If one "explains" free-fall simply by the use of the word "gravity," the result is unsatisfactory. In the same way, for Price to "explain" the scientific revolution or the transition to Big Science by the use of the term "exponential" is to explain very little.

In the third chapter, Price does attempt to provide some dynamical descriptions. For example, the approaching saturation is in part ascribed to the fact that the doubling of "eminent" scientists proceeds at a slower rate than the doubling of "less eminent" scientists: "We are scraping the bottom of the barrel." At one point, Price argues that the number of papers a man publishes is a significant measure of relative eminence even though "it be freely admitted at the outset that this is a bad scale" (p. 40). He assures us that after we make this approximation we can come back and refine it. Price does attempt such refinement and observes that he would much prefer to employ the amount of *use* a paper receives rather than the quantity of papers published (p. 75). Unfortunately no such data are available, and Price merely states that undoubtedly (!) the same conclusions would be reached in any event.

Although there are some nice descriptions of the kinds of activity that Big Science has created, others (for example, Weinberg) have already provided us with these characterizations. Price has made a startling and ingenious synthesis of the sources from which he has culled the data of the last two lectures, but in the end he must again cry "exponential." One fails to see the promised essential difference between Little Science and Big Science.

In my opinion, Price has tangled two questions. He has confused the organization of science with the performance of science. One still needs a notebook, a fairly healthy dose of physical intuition, and a rather critical attitude to do science. It is true that the well-worn simile of a little sealing wax and a piece of string may no longer fit; but the tools of science may change without significantly or necessarily changing the way science is done, or the personality traits necessary to working scientists.

It may well be that we are approaching the point in the development of science where the scientific organizer—

Price would count him among the "eminent" men of science—will no longer be the man who actually does science. If this is the case then indeed Big Science does differ in an essential way from Little Science. But it is the words "big" and "little" and not the term "science" which accounts for the difference. Unfortunately, headcounting of the sort that Price provides can shed little light on the reasons for the difference.

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India in the 1960's

Quiet Crisis in India. Economic development and American policy. John P. Lewis. Brookings Institution, Washington, D.C., 1962. xiv + 350 pp. $5.75.

"Nothing could so utterly demolish the effectiveness of United States economic policy toward India as would its commitment to an extreme laissez-faire position." One would wish that this sentence and much more in Lewis's thoughtful, perceptive, and lucid book could be read and taken to heart by all leaders of American opinion. The Clay Committee in its recent recommendations on foreign aid took a stand against assistance to government projects that compete with private enterprise; the chairman in subsequent testimony applied this doctrine to the projected Bokaro steel mill in India. As a result, the image of an America interested less in development than in imposing its own economic dogmas on other countries was again projected to a world only susceptible to Communist charges of American economic and ideological "imperialism."

In this book Lewis first examines the basic strategy of India's development plans and then the issues and techniques of American aid. His analysis is technically competent and illuminating, in language that need not repel a noneconomist. He is particularly successful in highlighting and clarifying such key issues as the pivotal foreign exchange scarcity, the need to mobilize idle manpower and put it to use, the division of investment between public and private sectors and the outlook for domestic and foreign private enterprise, the export problem and its implications for American commercial policy, and the crucial problem of rural development. For professionals in the development field, his most important contribution is chapter 7, "The role of the town in industrial location," in which he notes the grave disadvantages of overgrown metropolitan centers, finds the counter policy of "village-centered" industrial orientation futile, and thoughtfully develops an impressive argument for "town-centered" industrial development.

India launched the first of a series of 5-year development plans in 1951, a few years after independence. The current plan is the third, and 1963 is its middle year. "By all odds the most distinctive feature of the Indian effort," according to Lewis, is "its deep commitment to an orderly, peaceful procedure under which personal rights are respected. . . ." India is attempting an economic revolution, a rise from deepest poverty, within a framework of constitutional, representative government.

Will this effort succeed? The 1960's are the critical years. India must use its own resources to the maximum, and it must also import heavily from abroad during this decade in order to build up the investment in productive power and acquire the momentum that will—hopefully by 1975—enable it to continue progressing, but on a self-supporting basis.

At the same time, India's democratic system faces critical political tests: a successor to an "indispensable" prime minister; the problem of an aging majority party; divisiveness along regional, communal, and factional lines; and on top of everything else the Chinese aggression. Lewis justifiably doubts that, for underdeveloped countries in general, economic progress can assure orderly democratic evolution. But, rightly in my view, he argues that India is a special case. "She already has such a political evolution well established," and the thing she most needs in order to confirm and sustain her commitment to constitutional democracy through the severe trials ahead is "a sustained, clearly perceptible, widely shared surge of material advance." Along with dedicated Indian effort, this will require considerable outside help. America and other countries interested in the fate of freedom in this shrinking world should see that this help is forthcoming. Lewis speaks of "the unique importance" of the Indian experiment in a constitutional mode of economic development. Its fate will strongly influence the course of other Asian and African countries and "should be a primary concern of American foreign policy in the years just ahead."

"The test that India of the nineteen-sixties poses for Americans is whether they have the good judgment to recognize a monumental crisis while it still remains quiet. . . . It will be kept that way only through extraordinary effort, including American effort."

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Lippincott Geography Series

Geography in World Society. A conceptual approach. Alfred H. Meyer and John H. Strietelmeier. Lippincott, Philadelphia, Pa., 1963. xviii + 846 pp. Illus. $8.75.

This is a big book, and one may well question whether 846 pages, 600,000 words, and 4¼ pounds are not too much for an introductory text. Furthermore, it attempts a philosophic analysis of so much of geography, from astronautical man to urban planning, that there is little common focus. The basic organization is an areal interpretation and evaluation of earth realities, largely in regional terms.

"To facilitate the conceptual approach to the consideration of problems . . . all material in this book . . . has been organized on what might be called the 'self-tutorial plan.' The text is constructed, then, to be teachable as well as readable." Hundreds of quotations enrich the text. Each chapter ends with a set of problems entitled "Application of geographic understanding"; the following are illustrative of these problems: "Would it have been possible for the Mississippi River to have carved a valley like the Grand Canyon"; "Why do we produce so little rice in our country?"

The authors begin with two basic questions: "What is man in terms of *ecesis* (earth-habitat relationship)? What is his *ethos* (earth-steward responsibility)?"

The volume has six parts. The first deals with how scholars have developed the "geographic facts of life." The second reviews the basic classification of natural earth phenomena, and the third analyzes the processes by which man appropriates areal resources. Part