



# Scientific Research and Scholarship Notes toward the Design of Proper Scales

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GERALD HOLTON

## Scientific Research and Scholarship

### Notes toward the Design of Proper Scales

THE FACT THAT an important social invention has occurred, one that is destined to transform a part of society, sometimes goes unrecognized for a surprisingly long time. A case of this sort was the nineteenth-century development of science as a small but worthy profession for individuals in its own right. Another case exists at present. It is to be found in the particular way by which scientists have come to organize and coordinate their individual research pursuits into a fast-growing commonwealth of learning.

The new pattern for doing basic research in science is worth studying for its intrinsic merits. This essay hopes to spell out what it now means to be active in basic scientific work; but it should also help to explain coherently individual elements of contemporary science, each of which is obvious enough by itself—the rate and excellence of research output, the size of the funds that are needed or made available, the spectacular applications of science in industry and the military establishments, and so forth.

There are more important reasons still for sketching here the apparent operations of this commonwealth of learning. One reason is the fact that this pattern carries specific lessons for the conduct and organization of effective scholarly work in any field, no matter how different or remote from science it may be and must remain; one such lesson should be in the definition of a scale for measuring the adequacy of support. The second reason is, conversely, the realization that scientific work may best be understood as one of the products of the general intellectual metabolism of society, and hence that in the long run the growth of science depends critically on the growth of all fields of scholarship. This interpretation (to be published separately) has specific implications for policy, particularly in the humanistic studies.

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Thus I deal here with two related preconceptions involving science: first, that its spectacular efflorescence is the result of forces so unique to science that other fields cannot hope to apply its lessons to their own benefit; and second, that science in turn is nourished by a system of its own whose health does not depend significantly on the state of all other scholarship. Both these conceptions are false.

### *The Stimuli for Growth*

As a profession, science has been remarkably little studied, except for a handful of books and reports that seem to be covered over by the growing flood of changing statistical data. I shall choose basic research in physics as carried on today in the United States to characterize some common features of all the sciences. The choice is quite appropriate from several points of view. For example, the number of its academic practitioners has not grown at an inordinate rate compared with other fields of study. In the year 1914 there were only 23 doctorates awarded in physics in the United States out of a total of 505 for all fields, 244 of which were in science.<sup>1</sup> Thus some fifty years ago the Ph.D. degrees granted in physics amounted to 4.6 percent of all Ph.D. degrees for the year, or 9.4 percent of those in all the sciences. Remarkably enough, the most recent year for which good figures are available, 1959, shows virtually the same proportions. The 484 Ph.D. degrees in physics accounted for 5.1 percent of all Ph.D. degrees, and for 9 percent of all those in science.\*

The great rise of research output in physics in the last half century did not entail a corresponding loss of numbers in other areas. Indeed, in a sense, there has been a relative decrease in the number of basic-research physicists, since now one-half of all new Ph.D.'s in physics are heading for governmental or industrial research and administration employment for which there was no equivalent in previous years and where a much smaller fraction of men are doing basic research than are in academic employment.<sup>2</sup> Thus the remaining group of Ph.D.'s in physics is of intermediate size—that is, it is comparable to the graduating Ph.D. classes in history, political science, mathematics, religion, or English literature.<sup>3</sup> Those who do not stay in the academic life serve, of course,

\* *Statistical Abstract of the United States, 1961* (82nd edn.), pp. 130, 539. The total number of Ph.D. degrees in science in 1959 was 57 percent of all Ph.D. degrees versus 48 percent in 1914—a noticeable but not unbalancing shift.

to link physics to applied science, as is also the case in the larger fields of chemistry and biology.

Physics is a good profession to choose for this analysis because there exists an immense variety in the group, from the man in the small college who, with two or three colleagues, does all the work of the department and still finds time to think about new physics, to the man whose full time is spent in the laboratory of a large research institute. Any two physics-research projects picked at random are likely to have less in common with each other than does the statistical average of all physics research compared with the statistical average of almost any other experimental science. And yet there is in this group, taken as a whole, a strong sense of cohesiveness and professional loyalty. Despite the variety, despite the specialization that makes it difficult to follow what is being done in the laboratory next door, despite the important differences between basic and applied, large and small, or experimental and theoretical physics, its practitioners still clearly conceive of themselves as doing in different ways work in one identifiable field. There are no large cleavages and disputes between sizable factions representing fundamentally different styles.

If we first focus specifically on the professional life of a representative physicist, it is essential to remember at the outset the brevity of time in which basic research on a significant scale has been done in this country, or, for that matter, anywhere. The word "scientist" itself did not enter the English language until 1840. Until about the turn of the century, the pattern was that of work done by isolated men. Experimental research was often financed with one's own funds. Even in a relatively large department, advanced students were rare. Thus, Harvard University, one of the earliest in the United States to grant Ph.D. degrees in physics, had a total of six theses before 1900, and thereafter an average of about two per year until World War I. During that war, it has been reported, "there was no classification of physicists. When the armed forces felt the need of a physicist (which was only occasionally), he was hired as a chemist."<sup>4</sup> Having an adequate laboratory space of one's own in most universities was an unfulfilled wish even for the outstanding experimentalist—though this applied more to Europe than to the United States. For example, in 1902, at the peak of their research, four years after their discovery of polonium and radium and after many years of pleading for more space in which to do their extensive chemical and physical work, the Curies still had only their old

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wooden shed at Rue Lhomond and two small rooms at Rue Cuvier. On being proposed for the *Légion d'Honneur*, Pierre Curie wrote to Paul Appell: "Please be so kind as to thank the Minister, and inform him that I do not feel the slightest need of being decorated, but that I am in the greatest need of a laboratory."

The growth of science between the wars needs little discussion. The driving force was in part the needs of an increasingly sophisticated, technologically oriented, competitive economy, and in part the sheer excitement induced in more and more students (drawn from a widening base in the population) by beautiful ideas ever more rapidly revealed, such as the quantum theory and early nuclear physics. But the rate at which exciting ideas are generated is correlated with the ability of a field of study to "take off" into self-amplifying growth.

To the economic and intellectual stimuli of earlier days the Second War added the new stimulus of the threat of power in the hands of the Germans, who had been the foremost nation in scientific achievement. Einstein's letter of 1939 to President Roosevelt (so like that of Leonardo to Sforza!) dates the moment after which the scale of research support changed in a surprisingly short time by more than an order of magnitude. Since 1940, Federal funds for science alone have grown over one hundred-fold.\*

What mattered here, however, was really not so much the hot war and the cold war, for wars by themselves had not in the past unambiguously promoted the growth of science. Rather, it was a development unprecedented in recorded history: the demonstration that a chain of operations, starting in a scientific laboratory, can result in an event of the scale and suddenness of a mythological occurrence. The wide-spread fascination and preoccupation with science—in itself an essential element in its continued growth—find here their explanation at the elemental level.

In our society there had always been a preoccupation with the scientific hero who comes back with a major revelation after having wrestled with his angel in self-imposed isolation (Newton, Röntgen) or in relative obscurity (Curie, Einstein). Now, a whole

\* Great care must be taken not to use any easily counted measure (money, persons, pages of articles, energy of accelerated particles) to stand for increases in what really "counts," namely, in the qualitative understanding and the qualitative rate of increase of that understanding. The numbers are useful to a degree, but the effects of numerical increases in hands, minds, and tools for science are highly nonlinear.

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secret army of scientists, quartered in secret cities, was suddenly revealed to have found a way of reproducing at will the Biblical destruction of cities and of anticipating the apocalyptic end of man that has always haunted his thoughts. That one August day in 1945 changed the imagination of mankind as a whole—and with it, as one of the by-products, the amount of support of scientific work, including accelerators, field stations, observatories, and other temples.

To a physicist, nothing is so revealing as relating qualitative changes to quantitative changes. Man can cope surprisingly well with large rates of change in his environment without himself changing significantly. His psyche can take in its stride rapid rearrangements in the mode of life—collectively, for example, those owing to a large increment in the life span, and, individually, those owing to great deterioration of health. Precipitous changes of condition during quite short periods are well-tolerated. But the traumatic experience of *one* brief, cataclysmic event on a given day can reverberate in the spirit for as long as the individual exists, perhaps as long as the race exists. Hiroshima, the flight of Sputnik and of Gagarin—these were such mythopoeic events. Every child will know hereafter that “science” prepared these happenings. This knowledge is now embedded in dreams no less than in waking thoughts; and just as a society cannot do what its members do not dream of, it cannot cease doing that which is part of its dreams. This, more than any other reason, is the barrier that will prevent scientific work from retreating to the relative obscurity of earlier days, even if some turn in our civilization should bring all other phases of our lives back to their earlier levels.

### *Who Are the Scientists? A Representative Case*

The element of discontinuity in the general experience of our time merely reinforces the discontinuities in the experiences of contemporary science. The rate at which events happen is again the important variable. For, when a field changes more and more rapidly, it reaches at some point a critical rate of activity beyond which one has to learn by oneself, not merely the important new ideas, but even the basic elements of one's daily work. This is now true of many parts of physics and of some other fields of science, not only for the most productive and ingenious persons, but for anyone who wishes to continue contributing. The recent past, the

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work of one or two generations ago, is not a guide to the future, but is prehistory.

Thus the representative physicist is far more his own constantly changing creation than ordinary persons have ever been. His sense of balance and direction cannot come from the traditional past. It has to come from a natural sure-footedness of his own—and from the organism of contemporary science of which he strongly feels himself a part. None of the novels or the representations in the mass media which I have seen have portrayed him with success, perhaps because they missed the fact that this is the component that really counts.

Though I am referring to statistical data, the man I have selected to typify my comments is not a statistical average but rather a summary of traits, each of which is well-represented in the profession and all of which, taken together, will be generally agreed to among physicists as representing a worthy and plausible specimen. I go into some detail, partly because not only the novelists but even the anthropologists have so far failed to penetrate this part of the forest to provide a good description of the new tribe. But I also want to make a basic point about the humane qualities of training and professional life. First of all, I note that our man, like the majority of his colleagues, is young, perhaps thirty-five years old, or just three years short of the median age of fulltime-employed scientists in the United States.<sup>5</sup> Even so, he has already had nine years of professional experience and increasingly creative work, having finished his thesis at the age of twenty-six, after a study period of about four and a half years.<sup>6</sup> In completing this work, like 25 percent of all physics graduate students in the country,<sup>7</sup> he was supported by fellowships, in his case by National Science Foundation fellowships for the first two years.\* During his last two years he worked on a research assistantship, helping an experimental group in the construction of a new type of beta-ray spectrometer and submitting as his thesis early measurements he made with it in connection with this work.\*\* Thus, like the majority of graduate students in physics,

\* The present number of such National Science Foundation awards for graduate study, offered in all the sciences, is 2,500, at stipends of \$1,800, \$2,000, and \$2,200 for successive years, plus full tuition, a family and travel allowance, and a cost-of-education allowance to the institution; in addition, there are a number of other substantial fellowship programs in science.

\*\* More than one-third of all graduate students in experimental or theoretical physics held research assistantships in 1959-1960, and 31 percent were holding teaching appointments while studying. This, with fellowships and

his education was financed from the outside and proceeded without significant delays.

After graduation, he hoped to obtain a postdoctoral fellowship—perhaps the best way for the really good scholar to consolidate his grasp of his material and to map out a field for himself before plunging into the routine of professional life. But there are not yet enough such programs, and he did not receive an award. He therefore chose among the two or three suitable offers of a job and went into a middle-sized university. In selecting academic life—the only aspect of the profession to be treated here—he has become one of approximately 8,000 physicists in colleges and universities, as against twice as many working in industry and half as many in the government.<sup>8</sup>

He knows the pull toward industry to be strong because he compares offers that regularly reach him. The present median income for all college and university physicists of his rank and approximate age is \$8,000<sup>9</sup> (somewhat better in his particular case), whereas the median income in industry is \$3,000 higher,<sup>10</sup> and the usual offer he receives from industry is higher than that. Moreover, the pull may be expected to be much stronger over the next years. While the needs for physicists in educational institutions and in the Federal government are expected to grow by 66 percent respectively, the figure for industry is 130 percent.<sup>11</sup>

These facts help to explain his lack of deep concern as to whether the forthcoming discussion of his promotion to a post having tenure in the present university will go well. He knows from folklore that fifteen years ago there were only a few really good departments of physics in the United States; but now there are some thirty universities with research programs lively enough to yield between five and forty-three theses each year,<sup>12</sup> and there are many more good small departments. The availability of funds has helped to spread excellence in basic research widely and rapidly. He would have an even larger choice in liberal-arts colleges, but he has become rather used to cooperative experimental research of the size and with the tools that are usually associated with the larger universities. Significantly enough, there are twice as many physical scientists on the faculties of universities as in liberal-arts colleges; they form a larger fraction of the total faculty on campus; and—most important for this par-

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scholarships, means that only a relatively small fraction was not helped one way or the other, though 30 percent reported that inadequate finances were still a retarding factor in their graduate work. Source: Interim Report, Ref. 6.



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ticular experimentalist—whereas the average liberal arts college employs one nonfaculty professional staff person in physical science for every ten physical-science faculty members, at the average university the proportion is better than one to one.<sup>13</sup> This implies much better backing from technical personnel in universities, particularly for those inclined to do large-scale experimentation; however, this ratio will probably soon improve, when the regional joint facilities now being developed among colleges in several areas are completed.

Our physicist has to his credit a number of publications—several short papers and one long review paper. He is considered a productive person, interested in one of the main excitements (which to him has recently become an experiment in the field of high-energy physics), and, to some degree (less than one would perhaps like) in his undergraduate students. These he meets relatively rarely by the standards of his predecessors. One course, more rarely two, is a typical class schedule for a physics professor at a major university; it allows sufficient time for work, for contact with graduate students, and for the long seminars with colleagues in which one carries on one's continuing self-education. For the same purpose, his research leaves, sponsored by one of several national programs, come rather more frequently than sabbatical years used to do. Summers are given by the members of his small group to research on the same contract with the government agency that sponsors the project. During these months there is extra salary for faculty and assistants. When necessary, there are trips to one of the seven major laboratories sponsored by the Atomic Energy Commission but administered for unclassified academic research by a regional group of universities.\*

These circumstances, to repeat, are not typical of all scientists, but representative of a type of new scientist now often encountered. What is emerging is the picture of a research-minded scholar who lives in a world that has arranged fairly adequate support to help him carry through his ideas wherever such help is possible.

\* One example is the Brookhaven National Laboratory, where approximately half the operating time of the principal accelerators is reserved for the resident staff, and the rest is for visiting groups from universities and other domestic and foreign institutions. The present budget of the Brookhaven National Laboratory is \$18,700,000; the total budget for all seven such laboratories in the United States for the next fiscal year is \$135,000,000. (See *Background Information on the High Energy Physics Program and the Proposed Stanford Linear Electron Accelerator Project*. Report of the Joint Committee on Atomic Energy, 87th Congress, 1st Session, 1961, p. 38.)

This help shows up in a number of other important (or even quite trivial) ways. For example, postdoctoral fellowships bring good research talent at no extra cost to the project, for a year or two at a time. Or when an important-looking article in a foreign-language journal appears (one not among the many journals regularly translated by the American Institute of Physics and other organizations), funds for a translating service can be found.

Our physicist's current research grant happens to have been negotiated with the Office of Naval Research, after some extended discussion and troubled waiting. An insight into the sources from which basic-research sponsorship usually comes and the places where the work is done may be obtained by a quick count of the acknowledgments cited in the program abstracts for the most recent meeting of the American Physical Society.<sup>14</sup> Of the 480 papers contributed, 18 percent are from colleges and universities without indication of foundation or government support; 43 percent acknowledge such support (from the Atomic Energy Commission, the United States Air Force, the United States Naval Research, the United States Army, the National Science Foundation, the National Aeronautics and Space Administration, or others); 21 percent are papers on basic research done in and largely financed by industry; 16 percent were done in government (including national) laboratories by persons employed there; and the remaining 2 percent include sponsorship from private foundations such as Sloan and Ford.

Our man's Navy-sponsored contract, therefore, is financed quite typically; it is not a large contract, and of course no part of the work is hampered by restrictions on publication nor, indeed, does it have any directly foreseeable applications to Navy activities.\* The amount of the grant available to our man and to a senior colleague and collaborator who is acting as "principal investigator" is perhaps \$46,000 for a two-year period. About half this sum is for the purchase and construction of equipment; the rest is largely for serv-

\* Basic-research sponsorship by the Navy, Army, Air Force, Atomic Energy Commission, and other branches of the government (and in other countries by their equivalents) is generally justified in such terms as these: the project is one "with which the Navy should be in communication lest a breakthrough of vital importance occur. A classic example of the latter was early Navy work in nuclear physics which ultimately permitted more rapid utilization of nuclear power for ship propulsion. It is not possible to define firm boundaries as to Navy interest because of the unpredictability of basic research results and the complex interrelationships between fields of science." (*Basic Research in the Navy*, vol. I, p. 53.)

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ices, including graduate-student research assistants. Though the Navy cut down the original request for funds, there is still enough for the machine shop, electronics technicians, secretarial help, work by the draftsman's office or the photographer, and for publication and reprint charges. The contract support, therefore, is adequate.

Our physicist is better off than a considerable number of other academic physicists in less convenient circumstances. Many, in smaller colleges particularly, are hard-pressed. And, on the other hand, this man is perhaps not differently situated from many an equally talented and productive young man or woman in fields outside the sciences. Nonetheless, it is clear by the standards of the recent past in physics itself that here is a new type of scholar. Indeed, he and each of many colleagues like him has available for life the security, means, and freedom to do research that Alfred Nobel hoped to give by his prize to the few outstanding persons in the field. Most significantly, our new scientist is new in that he does not regard himself as especially privileged. The facilities for doing creative work are being accepted and used by him without self-consciousness and with the same naturalness as one accepts the convenience of a telephone.

This is the point. For whatever reasons, right or wrong, that society has chosen to make this possible, the circumstances exist for getting scholarly work done by more people than might otherwise do it, and for providing humane conditions of training for the oncoming generation.

There is at once a number of urgent objections, of course. One might say that it is not difficult to construct utopias for any field, given enough money. On the one hand, the money involved is easily afforded, the amount small on any scale except that of depression-reared experience or the starvation-oriented practices in all too many other equally worthy fields of scholarship;<sup>15</sup> on the other hand, this is not a paper utopia, but a working system for employing people's minds and hands in the time-honored mission of adding to the sum of the known.

Alternatively, the opposite objection may be heard: that really good ideas do not flourish without an element of personal hardship. But, despite the support intended by well-known stories (true, false, and sentimentalized), the evidence now is altogether the other way round. The once-in-a-generation ideas may still, as always, come from the most unexpected places; yet, throughout history, trans-

forming ideas, as well as great ideas only one magnitude less high, have not appeared in science at a rate equal to a fraction of the present rate. The sacrifice implied by the sum of thousands upon thousands of wretched student and research years under inadequate conditions in the past can surely be no source of satisfaction, even if the additional expenditures had not, after all, shown a better yield in science. I suspect that another Marie Curie, a Kepler, even a Roger Bacon, would not be damaged by more help, or by the availability of cooperative research facilities for those inclined to use them.<sup>16</sup>

There remains a third major objection. Has this useful and often pleasant arrangement not been bought at too high a price? It is popularly suspected that somewhere in the background there is a group of high military officers whose interest and decision ultimately control, from year to year, whether or not academic research shall flourish, just as the Renaissance patron determined whether the studio would continue or not. If tomorrow it were discovered how to destroy multitudes by reciting poems, the physicists would have to move into the garrets, and poets would be enticed into the laboratory space. It is not, after all, only the intrinsic merit of the subject that now gives it vigorous life, but also the weapons-aspect of its occasional by-product, vigorously exploited by applied scientists and engineers in industry and government. As the student newspaper, *The Tech*, at the Massachusetts Institute of Technology said not long ago in a plaintive editorial: "Most of the students at M.I.T. will, at some time in their lives, work for the government on military projects."

This is of course frightening and confusing ground. In part these widely held conceptions are not true, or at least no longer true. The influence of government (particularly that of the military branch) on science has not been without an effect in the opposite direction. As some scientists have become increasingly effective and trusted in their roles as advisors, a noticeable educative influence has made itself felt in Washington. The rising role of certain agencies such as the National Science Foundation and the National Institutes of Health have vastly improved the picture in the last decade. The research effort, when carried on above a certain minimum level, becomes an autonomous part of the system, as certain long-resisting industries are also beginning to discover. Even if any group now wanted to turn off the Federal support of science, it could not be done. On the contrary, it is nowadays more typical for scientific

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advisors to try to turn off what appear to be hastily conceived projects initiated by the Pentagon.

And yet, the deeper intent of the objection cannot be either disproved, or evaded, or sustained. It is at the same time bitterly true and false, as would be a refusal to sanction the rising standard of living in our present, artificially sustained economy. The problem posed is at bottom the same for the academic scientists as it is for anyone from grammar school teacher to legislator who participates in the life of a nation which is so closely geared to an arms race with a determined antagonist. And while the hope of gaining indirect or long-range benefits from basic science motivates those agencies that support physics, the large majority of academic scientists themselves have clearly declared again and again their eagerness to work toward a peaceful resolution of the crisis that is to a degree responsible for the high level of their support. In fact, it is largely from the work of such scientists that one may hope for the development of ideas, understanding, and techniques that will help in achieving what mankind never before took to be a serious task, the control of armaments and of inter-national aggression.

### *Requirements for Growth*

#### *Mobility, Organization, Leapfrogging*

While it would not be either possible or necessary in this context to describe in detail the research project that engages our physicist's attention, let us turn from his personal background to the general rules of action of the profession. We leave him as he is contemplating a possible modification in the use of a liquid-hydrogen bubble chamber, a device for making apparent the passage of elementary particles such as those generated in accelerators. The triggering event for this thought was a brief article, the heading of which is duplicated in Figure 1.

It will be instructive to study this figure with care. It contains a great deal of information about the metabolism of a lively field of scholarship, denoted even in the very name of the journal. The *Physical Review* is perhaps *the* definitive physics journal in America, though it is only one of the many good journals in which basic research in physics is published. In 1958, the sheer bulk (7,700 pages in that year), the continuing rate of expansion, and the delay between the receipt and publication of articles made it necessary to detach from the *Physical Review* the "Letters to the Editor," in

which brief communications are made. This resulted in the separate, quickly printed, semimonthly publication, *Physical Review Letters*. The article indicated in Figure 1 came out a month after its receipt; under the older system it might have taken twice as long.

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 HELICITY OF THE PROTON FROM A DECAY\*

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Figure 1. The heading of a short announcement of results in *Physical Review Letters*, 1961, 7: 264.

Why is this speed so important? One explanation could be that this profession is made up of fiercely competitive people. It is true that egos are strong and competition naturally present. But in the United States, at least, it proceeds in a low key; personal relationships, though perhaps lacking some color and warmth, are almost invariably friendly.

There are three explanations for this fact. First, the authority of scientific argument does not lie in personal persuasiveness or in personal position but is independently available to anyone. Second, there is the general loyalty to the common enterprise, mentioned previously. And most importantly, scientists as a group seem to be self-selected by a mechanism that opposes aggressive competition. Anne Roe, in summarizing her long studies in this field, reports in an essay, "The Psychology of Scientists,"<sup>17</sup>

Their interpersonal relations are generally of low intensity. They are reported to be ungregarious, not talkative—this does not apply to social scientists—and rather asocial. There is an apparent tendency to femininity in highly original men, and to masculinity in highly original women, but this may be a cultural interpretation of the generally increased sensitivity

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of the men and the intellectual capacity and interests of the women. They dislike interpersonal controversy in any form and are especially sensitive to interpersonal aggression.

Thus the theory of aggressive competition is not likely to be correct in explaining the speed often felt to be necessary. Rather, one must look to other causes. I will select two quite obvious ones, which seem to me among the most important. One is the intense interest in what has been found. The other is the natural desire not to be scooped by other groups known to be interested in the same topic. And here it is important to note a major cause for this possibility—the fact that research is usually carried out in the open. It would be inconceivable for a typical academic physicist not to instruct any visitor who shares his interests on the detailed current status of his research, even if, and precisely because, this same visitor is working on the same “hot” lead. This principle of openness is one of the basic aspects of the scientific ethos.

We now read the names of the authors given in Figure 1, and are perhaps surprised by their number. To be sure, a commoner number of collaborators would be two, three, or four, although ten percent of the authors of the other papers in the same issue of the journal are sole authors. Yet it is neither the longest list of authors to be found, nor is it unrepresentative. Here let me signal three points. One is the cooperation in research that is implied within each group, as well as among widely dispersed groups; another is the distribution in this country (and indeed internationally) of the cooperating enterprises (some long established, others not known as little as twenty years ago to have had strong research interests in physics); the third is the authors' remarkably heterogeneous backgrounds that are implied. The list of names makes the point more bluntly than could any comment of mine.

This last point is perhaps the most important of these factors in explaining the growth of science in our time. Nowhere else can one find a better *experimental* verification of the general worth of the democratic doctrine, which is often uttered but rarely tested seriously. Social and geographic mobility in a field of work, as in society itself, is the essential prerequisite for a full exploitation of individual talent. The success of contemporary science all over the world despite the great variety of social and political settings is merely a striking case study of this proposition.

It is somewhat ironical to note how the need for talented individuals in science is discussed by people who speak about it in very

different ways. For example, Academician A. N. Nesmeyanov, president of the Soviet Academy of Sciences, said in closing a celebration on the first anniversary of the launching of Sputnik: "We may be confident that in the name of the great ideals of humanity, our people, under the leadership of the Communist Party, will accomplish new and ever more notable feats. Our guarantee of this is the socialistic structure of our country, which gives wide rein to the development of science and ensures the bringing out of the notable talents of our people."<sup>18</sup>

This comment was cited by L. V. Berkner,<sup>19</sup> president of Associated Universities, Inc., which administers the Brookhaven National Laboratory. He added: "We have one great advantage, and that is the immense freedom that is enjoyed by each of our citizens. This freedom challenges the individual, without being pressed by his government, to do his part in bringing the free society of men in which we live to a position of unquestioned leadership. For in a free system, it is the individual, not the government, that determines the competence of the system."

The important fact is of course that regardless of their deeper differences the two systems share a preoccupation with the nurturing of individual scientific talent, and as a result are more or less on a par with regard to the quality of their scientific output.\*

The gathering of talent brings not merely rewards proportionate to the amount of talent but also rewards that are, at least in the early stages of a new field, nonlinear and disproportionate. In other words, the contributions of  $n$  really good persons working in related areas of the same field are likely to be larger (or better) than  $n$  times the contribution of any one of them alone in the field. This is true of a group as well as of individuals who do not work in physical proximity to one another.

\* It follows that any relaxing of social, economic, or other barriers which prevent talent from finding its proper scope is to be encouraged. Physicists would do well to ponder whether the amazingly low number of women in physics (2½ percent) in the United States is not indicative of such barriers, particularly in view of the larger fraction typical of other technically advanced countries. Disturbing and not unexpected difficulties of another kind are discussed in Russell Middleton, "Racial Problems and the Recruitment of Academic Staff at Southern Colleges and Universities," *American Sociological Review*, 1961, 26: 960. On the other hand, the obvious distribution of the authors' names in Figure 1 sets a certain norm for any field. The standard of social mobility implied by this case has very little to do with respect to science *per se*, but everything with respect to the seriousness of one's interest in the excellence of scholarship.



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With respect to the former, the particular way group work or cooperative research functions was long ago discovered and exploited by industrial laboratories and by medical researchers. Although some group research existed as far back as the seventeenth century, and beginnings of cooperative research even on something like the present scale of groups had been made, notably in the Cavendish Laboratory and E. O. Lawrence's laboratory at Berkeley, physicists did not really understand its full merits until the creation of the World War II laboratories (the Manhattan District, the Massachusetts Institute of Technology Radiation Laboratory, the Harvard Radar Countermeasures Laboratory, and others). Not only did they learn what it meant to do science when the rest of society is really backing science (a lesson not forgotten); more particularly, they discovered how to work together in groups, despite the fact that a member may be neither particularly inclined to gregariousness nor even informed in detail on the subject of his neighbor's specialization.

What took place here was analogous to impedance matching, the method by which an electronics engineer mediates between the different components of a larger system. That is, special coupling elements are introduced between any two separately designed components, and these allow current impulses or other message units to pass smoothly from one to the other. Similarly, in these quickly assembled groups of physicists, chemists, mathematicians, and engineers, it was found that the individual members could learn enough of some one field to provide impedance matching to one or a few other members of the group. They could thus communicate and cooperate with one another somewhat on the model of a string of different circuit elements connected in one plane, each element being well enough matched to its immediate neighbors to permit the system to act harmoniously. While an applied organic chemist, say, and a pure mathematician, by themselves, may not understand each other or find anything of common interest, the addition of several physicists and engineers to this group increases the effectiveness of both chemist and mathematician, *if* each scientist is sufficiently interested in learning something new.

That this system worked was a real discovery, for the individual recruits had come largely without any experience in group research. And while during the war the system of cooperative research was tried out successfully on applied, or "mission-directed,"<sup>20</sup> research on a large scale, it was continued after the war in many places in

basic science, at first on a much smaller scale—and it was still found to work to great advantage.

Another and even more important effect of group work on the growth of a field exists among eager groups in the same field who are, however, not side by side but located at some distance from one another. One research team will be busy elaborating and implementing an idea—usually that of one member of the group, as was the case with each of the early accelerators—and then will work to exploit it fully. This is likely to take from two to five years. In the meantime, another group can look, so to speak, over the heads of the first, who are bent to their task, and see beyond them an opportunity for its own activity. Building on what is already known from the yet incompletely exploited work of the first group, the second hurdles the first and establishes itself in new territory. Progress in physics is made not only by marching, but even better by leapfrogging.

To be sure, this method of locomotion is the way that interplay in the work of individuals can help to assure rapid advances; and the most valuable scientists are precisely those who can leapfrog by themselves farther than groups can. Yet, in sum, the presence of groups assures that their imagination and combined follow-up potential will allow frequent long jumps ahead into qualitatively different territory.

We can turn for a specific illustration to accelerators, not because they are glamorous or unique, but because quantitative data are easy to find there. Ernest Rutherford suggested in 1927 that the nucleus should be explored by bombarding it with artificially accelerated particles, because the natural projectiles available from radioactive sources are neither continuously controllable in speed nor of high enough energy. This gave rise at the Cavendish Laboratory in the early 1930's to the design and construction by J. D. Cockcroft and E. T. S. Walton of an accelerator for protons. Its first successful operation is represented by a black circle near the left edge of Figure 2.<sup>21</sup> Improvements since then have increased the top operating energy, e.g., in the proton linac, from the original one million electron volts (1 Mev) to about 60 Mev (note the nonlinear, i.e., logarithmic scale on the ordinate). But in the meantime, a profusion of new machines of quite different types have made their appearance, one after another. The cyclotron of E. O. Lawrence and M. S. Livingston (1932) was a radically different machine, and it immediately rose to higher operating energies; but this curve later

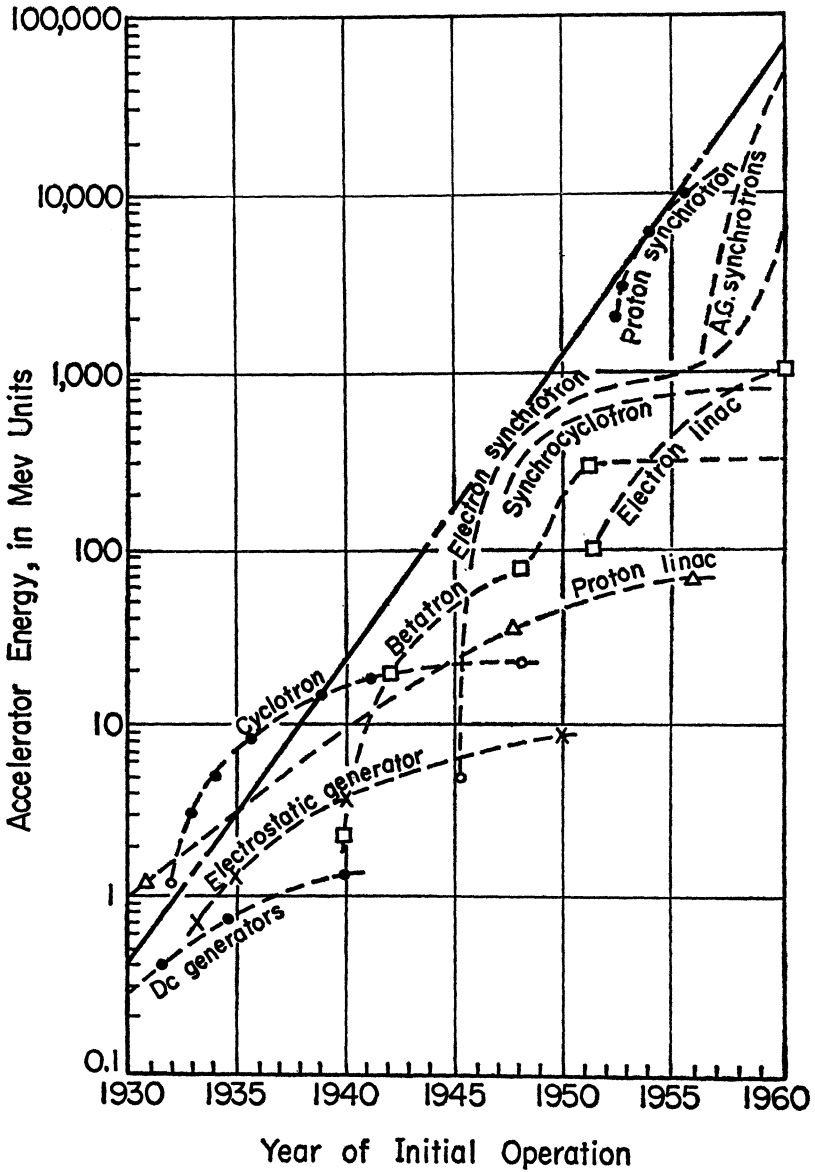


Figure 2. The rate of increase of operating energy in particle accelerators. (Courtesy of M. S. Livingston.)

flattened out (owing to the impossibility of a fixed-frequency resonance accelerator of this type to impart effectively more energy to particles when these have already achieved a significant relativistic mass increase).

The electrostatic generator, initiated by Van de Graaff at the Massachusetts Institute of Technology, entered the situation at about this time, with less energy but with useful advantages in other ways. It differed from its two main predecessors qualitatively (i.e., in the fundamental method of achieving the accelerating voltage), as indeed these differed from each other. In 1940 the betatron—again a fundamentally different machine—started with a design by D. W. Kerst at the University of Illinois, and then entered regions of higher and higher energies, where new phenomena could be expected to occur. New machines are continuing to come from different groups and widely dispersed laboratories; the leapfrogging process is clearly at work and opens up more and more spectacular fields for basic research.

One cannot help noticing an unexpected but crucial result in Figure 2. The heavy straight line (which would be an exponentially upturned curve if it were on an ordinary plot instead of on the semilog coordinates) of course indicates roughly the approximate maximum accelerator energy available to physicists in any year. This line shows that the top energy increased on the average by a factor of about ten every five years—for example, from about 500 Mev in 1948 to about 5,000 Mev (i.e., 5 Bev) in 1953. At this rate, the 33,000 Mev Alternating-Gradient Synchrotron at the Brookhaven National Laboratory, first operated on 29 July 1960, was ready none too soon. The possibility of going into the next higher range by means of two large accelerators whose particle beams will collide with one another is now being discussed.

This ten-fold (i.e., order-of-magnitude) increase in energy every five years entails a corresponding opening up of interesting results and new fields of work, each of which will keep research projects going for a long time. The multiplication of fields and results constitutes a graphic example of what is meant by an increase in scientific activity in one area. This, too, is a particular and peculiar pattern of physical science—although, of course, the time for a doubling of range or scale is not so short in most other areas of physics.\* The driving force here is in large part a simple and

\* Exponential increases in range or accuracy have long been a part of scientific advance, but the doubling rate was smaller. Thus between 1600 and 1930,

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general psychological one: Particularly when the more onerous material constraints on the realization of an ingenious new idea are removed, the really original person is not likely to be interested in spending his creative energy on something that produces much less than a three-fold, five-fold, or preferably an order-of-magnitude change. This has always been true, even when the financial considerations prohibit the realization of the idea, or when costs are inherently no great factor. A five- to ten-fold increase in accuracy of measurement or of prediction; an extension of the accessible pressure range from 2,000 atmospheres to 10,000, then to 50,000, then to above 200,000; an eight-fold increase in the volume of space seen by a new telescope—these are obviously interesting and worthy goals. On the other hand, to increase the precision or range in an area by, say, 30 percent is good, but is not likely to generate special enthusiasm in an individual or a particular group.

The natural pace, therefore, is that of doubling (or more), and of doing so rapidly. As in developments in the military missile field, the urge is strong to design an accelerator which will be beyond the one now being readied for its first tests. Leapfrogging has become somersaulting. But not all physics is accelerator-bound, just as not all science is physics, and so a balance is preserved in the large.

These considerations apply directly only to experimental physics, and even then only to those research projects that go after an extension of knowledge that can be associated with an increase of some numerical index such as range or accuracy. It therefore does not refer to such experimentation as the investigation of G. P. Thomson, which was intended to confirm whether or not an electron beam exhibits wave properties, and it also does not refer to much theoretical work. Models to deal with these cases are nevertheless possible—for example, by using as a quantifier the criterion of the inclusion in one framework of previously unrelated elements, and the production of new, unrelatable entities—and such models produce the same general conclusions concerning the increase of pace.

### *Diffusion Speed and Critical Rates*

Nothing is more striking in a high-metabolism field such as

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approximately, the accuracies of measuring time and astronomical angular distance each increased fairly consistently at an average doubling time of about 20 years. For data, see H. T. Pledge, *Science since 1500* (New York: Harper & Brothers, 1949), pp. 70, 291.

physics or experimental biology than the usefulness of the present. For example, M. M. Kessler<sup>22</sup> has found that 82 percent of the references cited in research papers published in the *Physical Review* during the last few years are references to other recent articles in scientific journals. Half of these articles cited are less than three years old! Reference to the more distant past decreases quite sharply; only 20 percent of all references are seven years old or more.

After journal citations, the next most frequent references (about 8 percent) are to private communications, unpublished or to be published; if the latter, they are usually in preprint form, the old standard method of communicating in a specialty field, a method which has now grown markedly. References in *Physical Review* articles to books turn out to rank only third, or 6½ percent (the remaining 3½ percent of references being to industrial reports, theses, etc.). Even these books seem increasingly often to be edited volumes of various articles. The net effect, then, is that of the diffusion and use of information at high speed.\*

There are other ways in which scientific information diffuses and is used. Nothing, surely, is a more viscous medium for diffusion than the educational system of college and high school. How do the advances of science fare there? We know that the situation is not yet satisfactory, and we can understand the difficulty that must arise whenever the diffusion time is radically different from the natural pace of research. An example is the treatment of special relativity theory in a long-established senior-level physics text, such as F. K. Richtmyer's *Introduction to Modern Physics*. In the first edition (1928) the theory of relativity occupied about a page. Six years later came the second edition, with twelve pages on this topic, gathered in an appendix. The third edition, eight years later, had a separate, regular, thirty-page chapter in the text. And in subsequent editions of this outstanding text the material has properly spread throughout the book so that it is meaningless to make an estimate of the actual space given it. But then, little had been added to special relativity theory as a separate research topic since long before 1928.

\* Not surprisingly, the speed of advance implies a degree of waste, and a number of simultaneous efforts along virtually identical lines. I have discussed elsewhere other reasons for the necessity of some wastefulness and for synchronicity in scientific work; for example, in the *American Scientist*, 1953, 41: 89-99, and the *American Journal of Physics*, 1961, 29: 805-810. Nothing here should be taken as a defense of much that is merely expensive large-scale gadgetry, but which passes for science under such labels as "Space."

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Alternatively, by making a cut through the educational system another way, one can follow the progress of ideas as they move from the research desk down to the schoolroom. The emanation electroscope was a device invented at the turn of the century to measure the rate at which a gas such as thorium emanation loses its radioactivity. For a number of years it seems to have been used only in the research laboratory. It came into use in instructing graduate students in the mid-1930's, and in college courses by 1949. For the last few years a cheap commercial model has existed and is beginning to be introduced into high-school courses. In a sense, this is a victory for good practice; but it also summarizes the sad state of scientific education to note that in the research laboratory itself the emanation electroscope has long since been moved from the desk to the attic. The high rate of turn-over of ideas in science presents almost insoluble problems for a conventional educational system in which information about the events at the top are propagated slowly and without a short-circuiting of any of the intermediate elements below.

In order to have a better model of the process by which knowledge in a research field advances, we must think about the rate of diffusion along yet another dimension. In all fields of scholarship, the inputs for a lively research topic are not restricted to a narrow set of specialties, but can come from the most varied directions. In physical science it is easy to document this process of the diffusion of knowledge from many sides, over a period of time, into one research area—on the part of individuals, and quite independently of the effectiveness of groups dealt with earlier. Figure 3 is a schematic design intended to give, in rough approximation, both a feeling for what may be meant by the "growth of a field" and an overview of the cumulative effects of contributions from various scientific specialties.<sup>23</sup>

The field chosen is that of shock waves. It is a "classical" research subject that originated in 1848 when the British mathematician and physicist G. G. Stokes and the astronomer and mathematician J. Challis communicated their struggles with solutions of the equation of motion in a gas as developed by Poisson in 1808. Stokes was led to propose, on theoretical grounds, that a steep gradient in velocity and density should exist in the gas if a large disturbance were propagated in it. Both their contributions are represented by the two arrows at the far left, the directions of the arrows indicating the specialty fields involved.

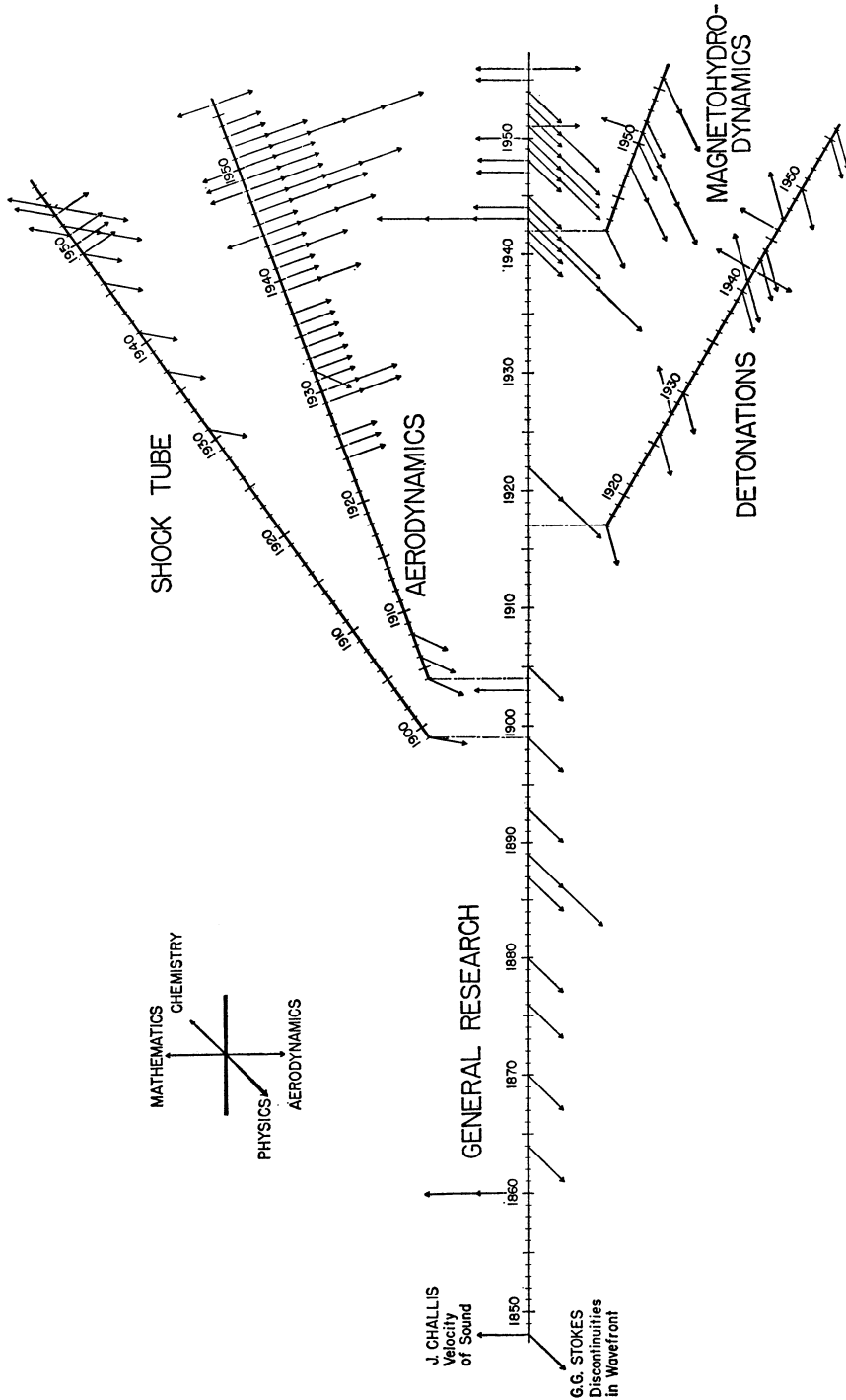


Figure 3. A representation of the development of basic research and of some applications. Each arrow represents a major contribution. Its direction indicates the specialty field involved (see coordinate system at the top left); for example, an arrow rising perpendicularly from the time axis represents mathematics.



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The successive events are similarly indicated. For example, further basic work in the mathematics of wave propagation by Riemann and by Earnshaw follows in 1860, and other arrows placed on the "General Research" line refer to contributions in mathematics by men such as Hadamard (1903), Chandrasekhar (1943), and Kantrowitz (1951), or in physics by Mach (1876, 1887, 1889), Bethe (1942), von Neumann (1943), and Truesdell (1951). New specialty fields branch off as shown from time to time, some having pronounced technological orientation; but it is illustrative of the difficulties of clear separation that a branch such as magnetohydrodynamics (where the initial arrow indicates the work by Alfven in 1942) now plays a fundamental part in both basic and applied fusion research. The increasing activity is evident throughout. As these lines go forward, one may well expect further branchings at the growing edge from any of the five present lines, and fundamental contributions along any of the four dimensions. It is becoming more and more evident that departmental barriers are going to be difficult to defend.

Another illustrative interpretation of cumulative growth is obtained by following, on a shorter time scale than Figure 3, the effect and interrelationship of a few particularly creative and stimulating persons within a field. Figure 4 represents the results of a recent study,<sup>23</sup> tracing in general terms the rise of the fields of molecular beams, magnetic resonance, and related work in pure physics. In particular, it is focused on one part of the extensive achievement of I. I. Rabi, both in developing the original molecular beam techniques, and in selecting and stimulating a group of productive associates or students (whose names are underlined on the chart in Figure 4).\*

This description is analogous to making a large magnification of a small part of the previous figure to determine its "fine structure." After working with Otto Stern in Hamburg, Rabi in 1929 effected a branching-off from previous lines of research (analogous therefore to Alfven's arrow for 1942 at the head of the magnetohydrodynamics line in Figure 3, or the arrow on the aerodynamics line for Prandtl in 1904). It can be seen that soon after, both in independent laboratories as well as in those of Rabi and his associates, the applicability of the early techniques, and the originating of new questions

\* It should be understood that this chart does not pretend to an exhaustive description of all work in this field, and in particular does not indicate any work by these persons in other fields.





now suggesting themselves in neighboring parts of the same fields, provoked a rapid branching into several new directions. The excitement of this field as a whole and its fruitfulness are attested by the large rate of inflow of new persons, including many outstanding experimental and theoretical physicists.

The course of the future is clearly going to be a continuing multiplication on the same general pattern. And although the growth is more eye-catching at the end portion of each branch, there is still a fruitful harvest in many of the lower boxes in Figure 4. Thus, molecular beams themselves remain important in current research. Finally, the connections with the technological exploitations of these advances have not been represented; but one should be aware that such connections almost invariably exist, and in this case they could be shown at several points (for example, maser, atomic clock).

### *A Simple Model for the Growth of Research in Science*

We may now correlate the descriptive details in a simple qualitative model of the growth process of scientific research. It is too ambitious to expect such a model to tell us "how science works," but it should help us to understand its more bewildering and spectacular aspects.

A hypothetical construction should start with a "zeroeth-order" approximation; that is, we know it to be inadequate from the beginning, but we also know how to improve it to attain a first-order approximation and, if possible, higher-order approximations later. Such a start is provided by Newton's analogy of having been on the shore of the known, "while the great ocean of truth lay all undiscovered before me." Scientists do indeed seem generally to think about basic research in terms of some such picture. They often have described it as if it were a voyage of discovery launched on uncharted waters in the hope of reaching a new shore, or at least an island. To be sure, neither research nor a sea voyage is undertaken without some theory that serves as a rough chart. Yet such vague terms are used, even when the promise of end results would strengthen the cause of the hopeful explorer. Thus during a recent Congressional inquiry to ascertain the large financial needs for future accelerator constructions, the scientists—quite properly—gave Congress no more definite commitment of returns on the considerable investment it was asked to undertake than this:\*

\* "A Ten-year Preview of High Energy Physics in the United States.

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It is, therefore, likely that the next decade will see the discovery of unexpected phenomena as well as the development of hitherto unknown techniques of particle detection and identification, and new means of particle acceleration and containment. Since it is impossible to predict the nature of these developments, it is very difficult to take their effect into account in any ten-year cost preview.

Taking the analogy of the voyage of exploration as sufficiently suggestive for the moment, we see that on the average a single searcher will expect the number of new islands he discovers to increase with time, perhaps more or less linearly. The same will be true if his is not the only ship that has started out, and if we assume the expeditions to be still few and not yet in contact with one another so as to affect the individual search patterns.

Hence the number of unknown islands yet to be found in a finite ocean (that is, the number of interesting ideas—not “the facts”—supposed to be still undiscovered in this pool) will be expected to drop off in time, somewhat as line I in Figure 5(a) does. In devel-

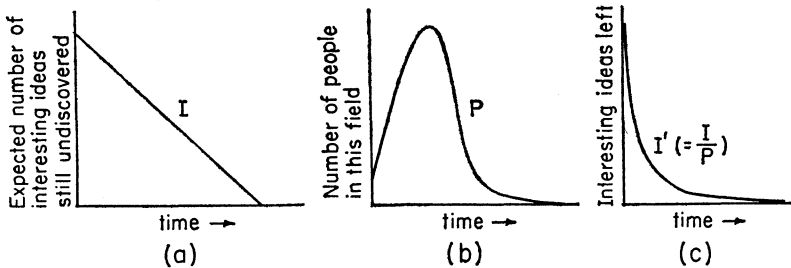


Figure 5. Zeroeth-order approximation for a model of research in a specified area.

oping a model for discovery, we shall now build a series of simple graphs on Figure 5(a) to summarize in an easily perceived form some qualitative trends.

But if Figure 5(a) itself were a proper model for discovery, science, like geographical exploration or gold-mining, would sooner or later be self-terminating. In fact, the end should come sooner rather than later, because the news of discoveries in a fruitful ocean spreads interest in them. New explorers will rush in, as shown by the

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Detailed Backup for Report of Ad Hoc Panel of the President's Science Advisory Committee and the General Advisory Committee to the A. E. C., December 12, 1960," in *Background Information on the High Energy Physics Program*, *op. cit.*, p. 24 (see p. 369 above).

early part of curve P in Figure 5(b). This influx by itself will assure that the quantity of ignorance remaining decreases with time in a manner shown not by curve I but by curve I' in Figure 5(c); that is, it will drop more nearly exponentially than linearly. If one also takes into account the fact that communication among the searchers shown on curve P improves the effectiveness of each one's search (a main function of communication, after all), then the middle portion of curve I' should really drop off even more steeply, causing I' to have the shape of an inverted sigma; and this is precisely what the data presented in Figure 2 indicate. In either case, however, the specified field will in time become less attractive, and the number of investigators will be decreasing somewhat as shown. Curve P thus indicates directly the size of the profession at any time, and indirectly—by the steepness of the slope of P—the intensity of interest or attractiveness of the field with respect to net recruitment (the inflow minus the outflow of people).

We shall soon have to add some mechanism to explain why science as a whole increases in interest and scope instead of deteriorating, as in Figure 5. Nevertheless, we already recognize that for some specific and limited fields of science this model is useful. Thus in 1820 Oersted's discovery of the magnetic field around wires that carry direct current, and the theoretical treatments of the effect by Biot, Savart, and Ampère in the same year, sparked a rapidly rising number of investigations of that effect; but it was not long before interest decreased, and by the time of Maxwell's treatise (1873) no further fundamental contributions from this direction were being obtained or even sought.

In fact, the same statement now applies (even in a good program) to virtually every topic presented in depth to physics students throughout their undergraduate training, and to a number of their typical graduate courses—except for students' own thesis fields. So, while Figure 5 may also be applicable to other areas of scholarship, the impressively different feature in physical science is that the time span for curve I' has become quite short when compared with the time span of an active researcher's professional life, and frequently even when compared with the new recruits' period of training.

Figure 6 shows again in curve I' the decrease of ignorance, together with a time scale (T, 2T, 3T, etc.) along the abscissa, drawn in such a way that the amount I' has dropped roughly to half the initial value when period T has elapsed, to one-quarter

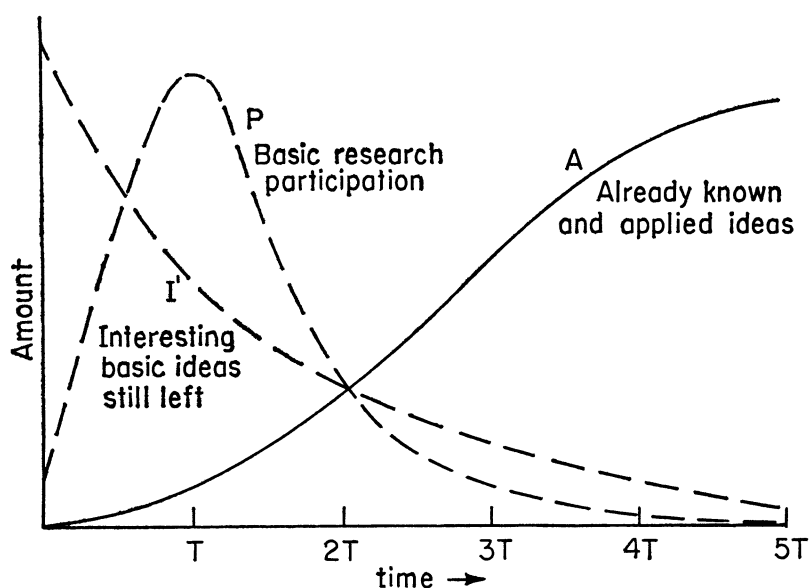


Figure 6. Inverse relationship between the accumulation of application and the interest in a basic-research field.

after total time  $2T$ , to one-eighth after  $3T$ , etc.  $T$  is thus the “half life” of the suspected pool of interesting basic ideas. The statements of the last paragraph imply that  $T$  is now short, perhaps between five and fifteen years for a specific, lively field on the frontiers of physical science.\* This is also in accord with the data cited earlier, which showed that in reports of new research the references to published work fall overwhelmingly within the most recent years.

While it is not intended here to give an accurate idea of the absolute scales, the relative positions, or the detailed shape of the curves,  $P$  has been placed so as to indicate that the number of active researchers will reach a maximum when a large part of the presumed total of interesting ideas has already been discovered. This suspicion and the sense of dwindling time also contribute to the evident pressure and the fast pace. It appears to me that a critical slope for  $I'$  exists. When the rate of decrease indicated by  $I'$  for the specific research field is not so large (that is, when  $T$  is of the order of the productive life span of individuals, or longer),

\* Needless to say, one might cite a number of interesting research fields in physics in which the time scale is longer.

the profession organizes its work, its training methods, and its recruitment quite differently than if the value for  $T$  is only a few years. There are recent examples, as the case of oceanography, of a science passing from the first phase into the second, taking on many of the sociological characteristics of physics as a profession.

By means of Figure 6 we can briefly consider the application of new findings in basic research, as indicated in curve A. Such applications include use in other fields (for example, radioisotopes in medicine), and use for applied research and development. Curve A is meant particularly for the last of these, for example in the development of an industrial product. Clearly, a curve  $P'$  that would be similar to  $P$  could be drawn to show how the number of people engaged in applied research is likely to grow and ultimately to diminish, for it is their work which A traces out.

Such a curve  $P'$  would have the same general shape as  $P$ , but it would be displaced to the right of curve  $P$ . For it is clear that the longer one waits before beginning to apply fundamental ideas, the more nearly one's work will seem to be based on complete knowledge. Today, however, curve A does not wait to rise until  $I'$  has reached very small values. We can readily understand this in terms of three factors: the competitive pressures within an industry, the natural curiosity of talented people, and the needs of basic research itself—which, in experimental physics at least, is now closely linked with the availability of engineering developments of basic discoveries. A curve  $P'$  for applied research participation will therefore overlap curve  $P$  for basic-research participation, and indeed these two populations will often draw on the same sources. For example, Kessler<sup>22</sup> reminds us that articles in the *Proceedings of the Institute of Radio Engineers* refer with considerable consistency to the publication of basic research in physics; in the case of a relatively new applied field, such as transistors, such references to articles in the *Physical Review* occur not much less frequently than citations to *Physical Review* articles in basic-research journals. In the past much blood has been shed over distinctions between pure and applied research. It may be fruitful to assume that a critical difference lies in the relative positions on the time axis of curve  $P$  showing the basic-research population and a corresponding curve  $P'$  that could be drawn for the applied research population. The fruitful interaction of basic and applied science will be indicated by the overlap of these two populations, in time as well as in the sources from which they draw their material.



A First-order Approximation

We are now ready to attempt a first-order approximation to improve our model for the progress of scientific research. For this purpose we examine Figure 7(a), where curve D is simply the mirror

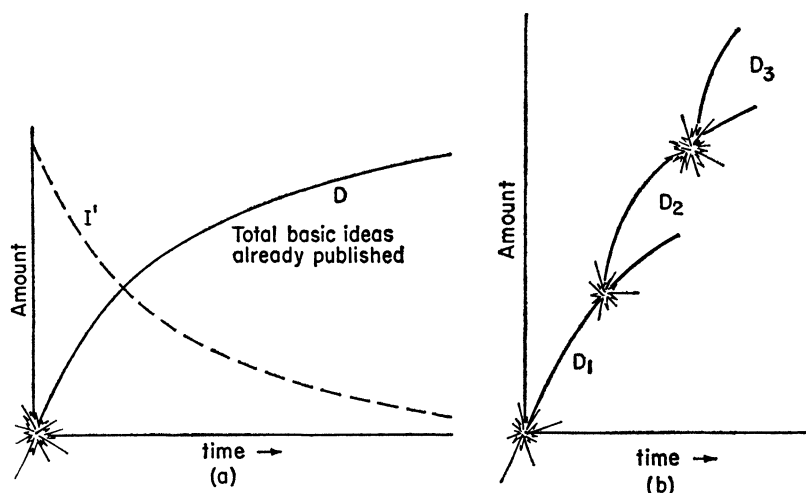


Figure 7. The escalation of discovery lines.

image of  $I'$ , plotted in the same plane. That is, whereas  $I'$  presented the decrease of ignorance,  $D$  presents the increase of total basic “discoveries” made in the finite pool of interesting ideas. The beginning of curve  $D$  indicates necessarily the occasion that launched the expeditions in this field, say the discovery in 1934 of artificial radioactivity by the Joliot-Curies while they were studying the effect of alpha particles from polonium on the nuclei of light elements. Up to this point their research had followed a fruitful line, originating in Rutherford’s observation in 1919 of the transmutation of nitrogen nuclei during alpha-particle bombardment.

The new Joliot-Curie observation, however, inaugurated a brilliant new branch of discovery. We suddenly see that the previous model (Figure 5) was fatally incomplete because it postulated an *exhaustible* fund of ideas, a limited ocean with a definite number of islands. On further exploration, we now note that an island may turn out to be a peninsula connected to a larger land mass. Thus in 1895 Röntgen seemed to have exhausted all the major aspects of X-rays, but in 1912 the discovery of X-ray diffraction in crystals

by von Laue, Friedrich, and Knipping transformed two separate fields, those of X-rays and of crystallography. Mosely in 1913 made another qualitative change by showing where to look for the explanation of X-ray spectra in terms of atomic structure, and so forth. Similarly, the Joliot-Curie findings gave rise to work that had one branching point with Fermi, another with Hahn and Strassmann. Each major line of research given by line D in Figure 7(a) is really a part of a series  $D_1$ ,  $D_2$ ,  $D_3$ , etc., as in Figure 7(b). Thus the growth of scientific research proceeds by the *escalation* of knowledge—or perhaps rather of new areas of ignorance—instead of by mere accumulation.

By means of this mechanism, we can understand at the same time the pace, the proliferation, and the processes of diffusion and branching shown in Figures 3 and 4. When an important insight (including a “chance” discovery) causes a new branch line  $D_2$  to rise, fruitful research usually continues on the older line  $D_1$ . But many of the most original people will transfer to line  $D_2$ , and there put to work whatever is applicable from their experience along  $D_1$ . (Perhaps now the most important thing to know is when to drop  $D_1$  and go to  $D_2$ .) If the early part of  $D_2$  rises steeply—because there is now a new area of ignorance that can be filled in at a rapid initial rate—then  $D_2$  will appear as an exciting field, and will be very attractive to researchers. Many will switch to  $D_2$ ; but the largest source of ready manpower is the new recruits to the field. Hence the lively sciences have a constant need to “grow,” or at least to enlarge the profession. This may well run eventually into difficulties as the limitation of available talent sets an upper boundary to possible growth.<sup>24</sup>

The newest recruits, therefore, are likely to be serving their apprenticeship at the newest and most rapidly growing edges. This is an excellent experience for them. But rapidity of growth depends on the inflow of research talent, and at the same time it is also *defined* by the output achieved. Thus there appears a danger of self-amplifying fashions: from a long-range point of view, too many people may be crowding into some fields and leaving others undermanned. One partial remedy has been for the less fashionable fields to set up their own professional specialty organizations and their own training and recruitment programs—a process which, once initiated, further polarizes the narrowing subsections within science as a whole.

With the concept of escalation in mind, let us finally re-examine

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Figure 2, where we found a leapfrogging progress to ever higher accelerator energies. The two concepts are intimately related; Figure 2 indicates the application of the escalation process of Figure 7(b) to a narrow and particularly vigorous specialty, that of accelerator design.<sup>25</sup> The same analysis may be applied directly to other experimental fields which do not have such strong increases in the value of an easily identifiable variable. But it should again be stressed that advances in most theoretical aspects of science, and in not a few experimental ones, do resist quantification, and that then no analogue to Figure 2 can be readily drawn. Nor should this be unexpected. In the end, what any advance must be judged by is not some quantifiable improvement in a specialty, but the qualitative increase in the depth of understanding it contributes to a wide field. For this reason, our model for the growth of science must, and should, remain qualitative.

We may now summarize. We have described what is considered an adequate system designed to support the pursuit of interesting ideas that add to man's basic knowledge—a system that aids researchers to do this sooner rather than later, and with work and luck to make a large difference to the state of knowledge of their field. We have noted the availability of means, of time, of collaborators, of encouragement by one's fellow-men, and of the stimulus of new results, all of which keep morale high; the open invitation to talent, no matter what real or imagined barriers to it may exist elsewhere; the aid given from student days forward for continued education; the predominantly youthful character of the profession, with the inflow of bright young people that is steadily growing; and the sense of building on the contribution of others. This, I believe, would be a fair description of the major features of most basic-research sciences as professions in the United States today, whatever their faults may be in detail.

*But none of these traits is inherently and necessarily restricted to the profession of science.* (Indeed, in the past, perhaps the majority of these traits did not describe any science well.) The description in the paragraph above might well apply to most fields of scholarship, but it does so only in some cases. In this sense I regard this contemplation of the physical sciences as useful, not because their methods are to be imitated, but because they, more than many other fields, have achieved a state of operation that need have nothing to do with science as science, but only with academic science as a profession.

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The description of it given above should allow us to distinguish between what is unique to science and what is not. It is not to the point to say that historians must be mature men before they can be historians, or that the Romance languages did not help build bombs and have no need of cyclotrons, and that the Navy is not waiting for break-throughs in theology. It is also not to the point to say that science is unique in its attention to quantifiable knowledge, in its need for cumulative growth, or in its luck or its ability to survive periods of acceleration in growth. Certainly, the clamor for more money and more manpower for its own sake is always wrong, even in science, and it can be fatal outside science. Perhaps, indeed, we need no increase in the rate of scholarly production of studies in Byzantine art or even in the history of biology. *But even in science*, the quantitative aspects of "growth" are merely indices of deepening understanding. Therefore, the question now must be: Given these differences between the needs of special fields of scholarship, and given agreement with the descriptive paragraph above as a worthy goal for any field of scholarship, what can we do to achieve this aim for each of the particular fields?

REFERENCES AND NOTES

1. R. T. Birge, "Physics and Physicists of the Past Fifty Years," *Physics Today*, 1956, 9: 23.
2. Byron E. Cohn (editor), *Report of the Conference on Curricula for Undergraduate Majors in Physics* (University of Denver, August 1961), p. 12.
3. *Statistical Abstract of the United States, 1961* (82nd edn.), p. 130.
4. Birge, *op. cit.*
5. *Scientific Manpower Bulletin No. 12*, National Science Foundation 60-78, December 1960.
6. Interim Report of the American Institute of Physics Survey of Graduate Students (mimeographed). With fellowship or scholarship aid, the median time now taken for the doctoral degree in physics is 4.31 years; without such aid, 4.84 years.
7. *Ibid.*
8. *The Long Range Demand for Scientific and Technical Personnel*, National Science Foundation 61-65, 1961, pp. 42 and 45. All too often the sciences are discussed as though they were all physics. To obtain perspective, the total number of United States physicists of all kinds (under 30,000) should be measured against the total number of professional scientists and

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engineers in industry alone: without counting scientists in government or physicians who do medical research, the number is 850,000. (See *Scientific and Technical Personnel in Industry, 1960*, National Science Foundation 61-75, 1961.) If government scientists, research-minded physicians, and science teachers are added, the total is 1,400,000. (See *Investing in Scientific Progress*, National Science Foundation, 1961, p. 18.) It had been estimated that only 27,000 in this large group are basic-research scientists, and that 15,000 of the latter are particularly active—the “real” scientists, as it were. (See *Basic Research in the Navy*, vol. I, Naval Research Advisory Committee, 1959, p. 29.)

9. *Scientific Manpower Bulletin No. 14*, National Science Foundation 61-44, 1961, p. 6.
10. *Ibid.*, p. 5. Also A. E. S. Green, “Scientific Salaries,” *Physics Today*, 1962, 15: 41.
11. *The Long Range Demand for Scientific and Technical Personnel*, *op. cit.*, p. 43. The reasons why he will probably continue in a university are significant. The mobility of a good man within the academic system in the United States plays a major part in the decision.
12. C. B. Lindquist, “Physics Degrees during the 1950’s,” *Physics Today*, 1962, 15: 20.
13. *Scientific Manpower Bulletin No. 13*, National Science Foundation 61-38, 1961, pp. 3 and 6.
14. *Bulletin of the American Physical Society*, Series II, vol. 7, 24 January 1962.
15. No fact of science has ever been as difficult to verify as the figure given out as basic-research expenditure. For example, the budget submitted by the President on 19 January 1962 contains \$12.4 billion for “Research and Development” (including that for the Department of Defense, and Space Research and Technology). Of this sum \$1.6 billion is said to be for “basic” research and training, including the programs of the National Institutes of Health, the National Science Foundation, and Agricultural Research, as well as large sums for the Atomic Energy Commission, Space, and unspecified items for the Department of Defense. Since in the past years the total sum spent for basic research from all sources has been about twice what the Federal government supplied, one might arrive at a total bill of from \$2.5 to \$3 billion for basic research in all sciences for the fiscal year 1962, or about half of one percent of the Gross National Product. However, a more likely figure for 1963, particularly if use is made of a stricter interpretation of “basic research,” is half this sum (or an average of about \$8 per person living in the United States) for all basic scientific research in physics, metallurgy, experimental psychology, medicine, etc.

For comparison, note the latest data in “Funds for Basic Research,” *Statistical Abstract*, *op. cit.*, p. 534. The total expenditures in 1959-1960 are (preliminary) \$1.149 billion, and the details list \$0.225 billion for basic research performed by the Federal government, \$0.344 billion for that performed by industry, \$0.500 billion for that performed by colleges and

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universities (including research centers they administered), and \$0.080 billion for that performed by other nonprofit institutions.

16. I will refrain from elaborating on the point that the new scientist now seems to have at least as much time and energy for other socially valuable activities as previous generations did. For example, a large number of distinguished scientists participate prominently in national voluntary advisory or citizens' groups, or give thought and help to one of about ten current national elementary and high-school or college programs for improving curricula in science.

Because in themselves they are either not new or not intrinsically unavoidable parts of the present pattern of science, I shall equally refrain from elaborating here some of the persistent and well-known complaints raised by scientists themselves: the volume of material to digest, the imbalance between different special fields, the encroachment of bureaucracy and of military technology, the need for keeping some "big science" efforts big and in the news by artificial means, the poverty of many teaching efforts. (For brilliant discussions of some of these and related points, see the essays by Merle A. Tuve in *Symposium on Basic Research*, edited by Dael Wolfe [Washington: American Association for the Advancement of Science, 1959], and by A. M. Weinberg, "Impact of Large-Scale Science on the United States," *Science*, 1961, 134: 161.)

I shall also neglect here the occasional pirate who is drawn to the scientific field, as in earlier times a man of talent with a like soul would have found scope for his aspirations in the service of a queen or a Boniface III. The obligations and opportunities of power and all it entails now lie on many of the most outstanding scientists. A thorough and sympathetic study of the situation is in *Conflict of Interest and Federal Service* (Cambridge: published for the Association of the Bar of the City of New York by the Harvard University Press, 1960), chapter 7.

17. *Science*, 1961, 134: 456.
18. A. N. Nesmeyanov, "A Great Victory of Soviet Science," *Vestnik, Akademiia Nauk SSSR*, 1959, 28: no. 11, pp. 3-9.
19. *Proceedings of the Institute of Radio Engineers*, 1959, 47: 1052.
20. This is the place to mention (without entering into it) the debate on the difficult problem of distinguishing among basic research, applied research, development, technology, quality control, and technical services. These form a continuous spectrum, and precise definitions do not survive the test of using them and talking about them. Suffice it to say that different panels of physicists and engineers working together usually manage to discriminate between these activities on the basis of brief descriptions. For a discussion, see the essay by D. Wolfe, in *Symposium on Basic Research, op. cit.*, Ref. 16.
21. From M. S. Livingston, *The Development of Particle Accelerators*. New York: Dover Publishing Company (to be published). Reproduced by permission. A similar chart is in M. S. Livingston and J. P. Blewett, *Particle Accelerators* (New York: McGraw-Hill Publishing Company, 1962), p. 6.

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22. *Proceedings of the Western Joint Computer Conference*, 1961, pp. 247-257, and private communications.
23. Based on data presented in *Basic Research in the Navy*, *op. cit.*, vol. 1. I thank Dr. Bruce S. Olds for arranging the release of the material for use here.
24. See Dael Wolfe, *America's Resources of Specialized Talent* (New York: Harper and Brothers, 1954), p. 192.
25. The model here proposed may be elaborated so as to deal with other features of scientific growth, for example, the manner in which work along lines  $D_2$  and  $D_3$  reflects on continued progress along  $D_1$ . Thus, after the early falling-off of contributions along the original lines of electrodynamics, interest was revived first by Maxwell's and Hertz's work in electromagnetic waves, then later by Lorentz's and Einstein's, and most recently by plasma physics.