EVOLUTION TOWARD A MATURE SCIENTIFIC LITERATURE[1]

W. MANSFIELD CLARK

Johns Hopkins University, Baltimore

I wish to thank the Society for a delightful memory and for a great honor. Nineteen years ago our former Secretary, Dr. Hitchens, officially sanctioned my first appearance before a national society and then took me to a café in old Philadelphia where I enjoyed the conversation of several famous men. That reception to the fellowship of scientific workers is typical of the fine spirit with which this Society has always welcomed its youngsters. For the great honor I owe more than I can express. There are aspects of its bestowal which I cannot mention without seeming to question your judgment; but I believe you will recognize my meaning when I say that its bestowal is evidence of the remarkable catholicity of this Society.

The discussions at our annual meetings broaden and deepen with the ever enlarging stream of scientific thought. This is inevitable since the study of life in any of its varied forms carries concerted thought of many kinds to ever harder tasks. It is a fact that in the lines of our printed programs, which describe investigations specific to our own science, our members write:—the languages of pathology and thermodynamics; the data of statistical and chemical analyses; the signs of potentiometric and optical measurements; the notations of enzymatic and genetic specificities; the symbols of chemical and cytological morphologies; names from the manuals of instrumentation and the catalogues of taxonomy. The contributions of morphologists, of physiologists, of chemists, of physicists, of mathematicians, of

[1] Presidential address delivered before the Society of American Bacteriologists at its Thirty-fifth Annual Meeting, Philadelphia, Pennsylvania, December 28, 1933.

those who are skilful in the numerous subsidiary subjects and in their recombinations to form the specialities and the contributions of those who deal with what may be called the special dimensions of biology are not of equal importance in the individual case but all are coördinated toward the comprehension of what, for lack of a better term, we call life. United within this broad meaning of biology the members of this Society come here from chairs and academies of theoretical learning and from institutes and field camps established to explore the frontiers of medicine and public health, the fields of agriculture and the provinces of industry. They come here to witness the confluence of many different streams of thought in solving problems of proven importance to the welfare of mankind or problems of importance to the whole of science, problems for which our specific science offers unique material. In such a comradeship there can be none of the confining jealousies of the old professional guilds but rather the recognition that it is the necessity for skill with the special tools of hand and mind that makes one of us a physiologist, another a taxonomist, another an immunologist. Indeed for the purposes of our specific science we would welcome within our fold ever wider interests, recognizing that those who huddle into narrow groups have pitifully failed to appreciate the great enlargement of those intellectual coöperations which would hold us had we no formal organization other than that which oils a simple machinery of meeting and of publication.

With the growing complexity of our science there grow not only those specific problems which must be solved by the labor and the insight of individuals but also problems of intellectual adjustment requiring the coöperation of all. A pressing problem of the second type arises from that necessity for breadth of knowledge which every student of life must feel and the tokens of which I have just noted. By these same tokens of our great desire and pressing need it is such as we who can judge, perhaps better than the devotees of some abstract science, the state of scientific literature. Of the scientific literature men have spoken in private with such despair that I feel compelled to raise their hopes by reviewing corrective and constructive forces displayed in history.

To do this with the frankness that the case demands I must betray a discord between my own words and deeds. I shall hang myself, but speak I must of crimes against the common weal.

To appreciate the forces which have been operating throughout the twentieth century let us look back to the first third of this century. Place yourself at the close of that period—1933—a date which now seems long ago and imagine that you were then a student of life.

For information upon current work you would have groped your way through about 30,000 yearly entries in Biological Abstracts, 37,000 yearly entries in Chemical Abstracts, and the 1269 pages of Quarterly Cumulative Index Medicus which itemized the articles in 1388 periodicals. It goes without saying that you would have ignored the greater part of these abstracts. Yet the hawk's eye was needed since quarry was to be found in distant fields. Undoubtedly a chemist, concerned with the theoretical advances and the biological applications of his science, would have glanced at no less than 20,000 of those abstracts which in Chemical Abstracts were then increasing at a linear rate of about 1900 a year. For anyone of your special interests there would have been a background whose literature had been at least a century in the making and upon this background would have converged the theory, the technical appliances and the detailed information from various branches of knowledge. In the effort to appreciate these you would have sought among the 15,000 scientific texts published annually[2] such selected lists as those published by Nature and from over 3000 publications there recorded[3] annually you might have glanced at the reviews of about 1000 in physics, chemistry, biology and bacteriology searching for texts and monographs which might help you to "keep up." You would have felt like Alice in Wonderland while the Red Queen cried, "Faster, faster. Here it takes all the running you can do to keep in the same place. If you want to get somewhere else, you must run at least twice as fast as that!" You would have sought current reviews scattered in some three or five hundred journals which

[2] Allan Gomme in The Uses of Libraries (Baker editor), 1930.

[3] Nature, **129**, 370 (1931).

were to be found in an average medical library[4] and which represented the cream of about 18,000 periodicals[5] which were increasing at such a rate that one new journal of interest to chemists was created each fortnight[6] and 150 new titles of publications in 16 different languages came within the field of "Botany: Current Literature" in the period 1920 to 1926.[7]

Of the despair of critical reviewers there is no lack of proof. One instance will suffice. In 1933 appeared a second volume of Annual Review of Biochemistry. The editor noted that the reviews covered a period of only one year and only 25 subjects and yet required the summary of 3000 papers. He estimated that these 3000 papers represented less than half of the papers which might have been of sufficient merit to have deserved treatment. Ten of the 25 authors were frank enough to state either that they had placed restrictions upon their already special topics or that they despaired of a complete analysis of the work for the preceding year.

Whence came the flood of scientific papers? The Industrial Revolution of the nineteenth century had created a demand for the scientific control and the investigation of industrial processes. It had also brought wealth and leisure, conditions conducive to the support of abstract investigations. Consequently old and new, academic and commercial institutions in the older industrial countries increased their production of scientific works enormously. In 1933 Gowland Hopkins[8] estimated that 8 to 10 individuals were engaged in research where one was engaged twenty years before. But this was only the beginning. By the end of the first third of the 20th century scientific reports had begun to be abundant from the old cultural centers in China, Japan, India, and from educational, agricultural and industrial frontiers on every continent and great island of the earth. When a severe economic crisis made little impression upon the acceleration of the

[4] Cunningham, Science, **77**, 410.
[5] Allan Gomme, l. c.
[6] Crane, Ind. Eng. Chem. News Ed., **8**, 5 (1930).
[7] Atwood, Science, **65**, 255 (1927).
[8] Science, 1933, p. 229.

output and the circumstances of this crisis made it evident that ultimately there would be still more leisure for cultural pursuits throughout the World, it became evident that scientific literature at the close of the first third of the 20th century was growing like an infant in its second week after birth and that only the centuries which are to be its years of youth can witness all its changes on the way toward maturity.

Even now, near the close of the twentieth century, we cannot foresee the nature of a mature scientific literature, but it is well that we should examine the practices of the earlier period to appreciate the origins of the Scientific Reformation and problems that it left unsolved.

Previous to the world war it had been the custom to introduce a subject by reviewing its literature, preferably to the time of Aristotle. Increasing familiarity with ancient literature made continued reference to the priority of Aristotle somewhat embarrassing; but this was settled after the manner of the old decisions of taxonomy. An apparent end to the introductory literature review came after the world war when, in conjunction with the increased production already mentioned, there was a temporary increase in the cost of printing. Both of these forces made the *individual* journal cultivate a new type of scientific paper which has been called "the glorified abstract." It became the prevailing style. A wag maintained that he had seen in one editorial office an automatic machine geared to chop each comprehensive article at each sign of a change in topic. At any rate "the glorified abstract," with its tense confinement to the immediate business at hand, seemed to have eliminated the literature review and therewith a traditional, a clumsy but withal a useful way of giving the reader perspective in his placement of the subject. However, this condensation of the world's literature was apparent only. In the original literature exposition became so cryptic as to require review of each new group of original contributions and in a short time review journals so expanded as to open to second-hand reviewers their opportunity to learn a subject's content by compiling annotations of extensive bibliographies. Space that the "glorified abstract" saved one journal was recaptured by the review journal.

The unsatisfactory natures of both the short, cryptic article and the second-hand review soon would have elevated to prominence that more mature style of writing which thoughtful authors were developing had there not been powerful forces opposing change. These we must examine.

It is obvious that, as the number of investigators in a field increases, the individual's chance of establishing a priority decreases. The defense left to the individual is increased frequency of publication. As late as 1933 there was a strong traditional sanction for this. It had its elements of genuine helpfulness and of lively interest. It protected, not against invasion of a reserved field, but the peace of the competent investigator who wished only notice that his work had been started long ago and was not to be confused with that of those who rushed to press. The worthy and the selfish motives kept alive the description of each, little advancing step long after simultaneity of publication had become so common that the prior claim had lost much of its distinction. By 1933 certain journals already had become virtually only a means of announcing claims or virtually only newspapers, and soon the volume of this newsy literature became so great as to make impracticable its inclusion in seriously critical reviews. This evolution to a neglect of the newsy literature inevitably led to a differentiation between professional scientists and a new sort of amateur. In the period 1935–40 there occurred a series of disputes centered about the claim that priority and "publication" are established by a radio broadcast *and* the circulation of its electrical transcription. The disputes were so lively that only newspapers could provide their adequate vehicle. Thus the daily press became the *traditional* vehicle. In 1938 a popular periodical called the *Daily Scientific Preview* made capital of this change in custom and by 1940 it was printing most of the more urgent, preliminary papers. But its greatest stroke came in 1942 when it opened a column and offered cash prizes for successful guesses upon the outcome of novel experiments. The fun of this new game quenched hot disputes. A new and lively sort of popular science had been left in the wake of serious scientific advance.

A distinctive part of the literature was the contribution of students to whom publication meant an initiation into a privileged class. The important contributions from this source and their true functions in the scheme of educational and professional life were somewhat obscured by a practice best illustrated as follows. An investigator so noted for the number of his papers that experts in his own field had no time to read his work is reported to have made the following remark.[9] "You complain," said he, "that I publish too many papers. But each paper represents an accepted means by which a student can acquire credit. That credit and his degree are certificates that the man is capable of earning his living in a highly organized system. I cannot deny my students their opportunity to earn their bread and butter and I am but following an accepted means of increasing the army of workers needed to exploit the resources of nature." The low standards thus inculcated were doubtless the cause of the remark that the army of graduates was like the army of Xerxes of which Thucydides said that "there were many men but few warriors." This was unfair alike to the university system at its best and even to its average product. But it is a commentary upon the times that certain universities let themselves be so impressed by the growing opinion that the doctorate had lost its significance that they did away with its bestowal. By the time a considerable number of their graduates were insisting upon the punctilious use of their title "Mr." other universities had already given enlarged meaning to the ancient traditions and their students were seeing to it that their theses would become creditable parts of their records.

Closely associated with the temporarily distorted attitude toward the function of publication in the training of graduate students was a strange opinion which developed in the rank and file of employees. This opinion was that an investigator's promotion depended upon the number of his publications. Professor Tobias

[9] Confidential report.

Smith displayed discernment by the manner in which he investigated this matter. He first searched the minutes of faculty meetings. There he found unmistakable evidence that candidates did emphasize the number of their papers and no evidence that any committeeman ever read the *contents* of the papers. Thus it *seems* that the candidate was wise in his emphasis of number. But, suspecting that these records might not divulge all, Professor Smith searched the private correspondence of the time and discovered that members of the faculties, after requiring an indefinite minimum of publication, rated a candidate in *inverse* proportion to the number of his papers.

The prevailing attitudes had an obvious effect upon the literature of that period. Brief publication, sufficient to set forth the brilliancy of an idea, was at a premium. Indeed it appears to have been a period of exceptional brilliancy. But it should not be forgotten that the plodder and the master alike saw the cream of new developments skimmed and only the drudgery of careful, comprehensive work left to them. Out of this arose the well-known epithet "cream skimmer." It was a difficult psychological situation which was to be adjusted only as the purposes of a mature scientific literature developed. The development was by evolution in the course of which there were not lacking conscious efforts toward reform. These we must examine briefly.

Professor Goodview has recently examined the archives of several of the more reputable journals and has uncovered the vast, unselfish work of editors and their referees. The records show that editors agreed then as now that a scientific article should be treated as a potentially permanent stone in the edifice of a science. Avoiding evaluation of its final function, an editor must also regard an article as a potential object of scrutiny by innumerable readers so that a modest estimate of five or twenty thousand reader-hours for a paper of average importance would seem to call for at least a few hundred hours of careful composition. Yet there is some evidence, subsequent to 1935, that manuscripts were relayed to editorial offices by the teletypewriter directly after dictation. Professor Goodview finds that previous to 1935 there is abundant documentary evidence that editors and referees spared no time in carefully compiling suggestions and corrections which were frequently ignored in authors' revisions. In return for their pains the editors received innumerable letters which they must have filed in grim silence since they are found filed without notes. Apparently to express his feelings one editor stored these letters in a disinfectant. Professor Goodview finds in the private correspondence of the editors evidence that they kept the faith, dreamed of clarity of exposition and even of literary beauty but resigned themselves to await the only effective remedy—the development of a general appreciation of what a mature scientific literature should be.

An attempt to solve the difficulty was made in 1946 by a committee of the League of Nations. It drafted resolutions entitled *Categories of Permissible Publications.* Unfortunately the subtitle, *Suggestions to Authors,* was printed in small type and was either overlooked or purposely ignored by the editors of commercialized scientific journals of the type that had started in Germany. Had the *Suggestions to Authors* been followed, practically none of these journals could have survived. Doubtless this is the reason for the propaganda against the League's *Suggestions* and the reason for their slow adoption. Containing nothing that could be called usurpation of individual freedom, the *Categories of Permissible Publications* set forth principles having universal appeal. For instance, the fourth category, subheading B, stated the principle that gratuitous speculation unsupported by experimental evidence or theoretical basis is unethical. Only gradually did this principle of ethics acquire force but it finally did so by way of the more conservative scientific societies until now it has such "teeth" that its violation is sufficient basis for exclusion from membership.

As the League of Nations' promulgation of the *Categories of Permissible Publications* failed of immediate effect so did the theoretical treatise by McKensie. McKensie shows that in the systems we study, the possible phenomenal relations among the components are practically infinite in number. It follows that if these are described according to even *one* code the possible record of so-called facts is practically infinite. The problem of scientific publication, said McKensie, is not to provide *unlimited* facilities for the recording of "facts" important as they are. The very complex problem of publication must be governed by the two-fold principle that while the potential number of facts is infinite the intellectual abilities for their assimilation have inherent limitations. Even a cataloguing department of a science can ultimately assimilate only such facts as it can use in a practically available system of record and if the records become too elaborate their detail becomes less available than that supplied directly by nature. In many instances instruments have evolved to make qualitative comparisons and quantitative measurements so easy and so available to a particular purpose as to obviate the necessity for those written records which are necessarily limited in scope. By means of specific cases McKensie shows the ultimate futility of publishing what he calls the incidental, qualitative test. This is the sort of test which an investigator finds invaluable in quickly obtaining "leads" and "hunches" for systematic work. Any large, published accumulation of these defeats the purpose of a scientific literature, since their bulk becomes too great for fresh review and the apparent trend of their indications leans on habitual opinion.

With regard to the discovery of really new categories of relations McKensie shows that they are so rare as to impose upon the announcer the duty of a most exhaustive effort to demonstrate absence of correlation with the facts of established categories.

McKenzie recognizes clearly that during the early study of a subject a good plan of work may be missed, an adequate working hypothesis is seldom available and all pertinent relations may not be within the possibility of appreciation. But it then becomes the more important to publish only accurate and comprehensive data adequate for the purposes of a systematic organization which should be devoid of the generalized treatment for which the time is obviously not ripe. When a subject becomes stabilized the highly specialized ideas and confused intricacies which it has accumulated can and should be revised, and its generalization reduced to the simplest, most universal, most rigid and most basic terms possible. As Whitehead remarked "The paradox is now fully established that the utmost abstractions are the true weapons with which to control our thought of concrete fact." This by no means implies stagnation or dogma for even the ordinary intellect now appreciates that generalizations are conveniences and that if there remains in any generalization lack of rigour or adequacy these are best revealed by scrutiny of the basic concepts. In the words of J. J. Thomson "a theory is a policy rather than a creed."

But no generation ever follows the thought of its own philosophers. McKensie's theoretical treatment, like the formal resolutions in *Permissible Publications*, was without immediate effect. Likewise without effect were the occasional examples of excellent scientific writing. An effective example is a reflector which focuses the light from an excited state upon its cause. Such an example came in 1965. Its true origin lay in that evolving change of attitude which I have reviewed; but its immediate incitement was a comparatively trivial incident which arose as follows.

As science became more highly specialized the coinage of new words became so rapid that in 1933 the rate was over one new word a day in medicine alone.[10] At first this caused no worry. It was the general opinion that only the more useful words would survive to enrich the language of science. Then too, as Simeon Strunsky said, "Man has always had a capacity for adding to his happiness or to his terrors by substituting long words for short ones." As specialization became more refined there were more refined uses of the principle admirably expressed by Lavoisier: "Every physical science is formed, necessarily, of three things; the series of facts which constitute the science, the ideas which recall and the words which express these facts. The word ought to call forth the idea, the idea depict the fact; they are three impressions of the same seal." Now an elaborate terminology may prove invaluable to the specialist and so long as he is profitably occupied it would seem presumptuous to demand of him the abandonment of any convenience which serves his purpose. But, as I have already noted, the early part of the 20th century saw the property

[10] Stedman, Practical Medical Dictionary, 12th ed.

of the specialist no longer exclusively his own. There was hardly a special development in any science which failed to find application in the practical affairs of the world. There was hardly a special development in an exact science which failed of theoretical use in such comprehensive subjects as biology or medicine. Failing to recognize this, specialists had made little or no effort to integrate their terminologies with those of contiguous subjects. As Stuart Mudd remarked, "The fact that the phenomena of bacteriology and immunity have been described in a special terminology has been a serious obstacle to the much needed collaboration of chemists." It was as if Jahveh had repeated his ancient words: "Behold the people is one and they have all one language—and now nothing will be restrained from them, which they have imagined to do. Go to, let us go down and there confound their language, that they may not understand one another's speech." As early as 1933 a biologist passing from a meeting of biochemists to a meeting of cytologists might have heard discussions of the same subject in languages very different.

By the nineteen forties the situation had become of some importance in the higher training of students. Perhaps it was trivial that students wasted time in learning words that contributed nothing to precision of thought or that lecturers wasted time in labeling each idea as it was developed. There will always be wastes of this or similar kinds arising from the conditions that make for progress. But what was serious was the fact that youths who came to learn subjects complained that they listened to logodaedalists. Let no one trifle with the idealism of youth or treat lightly those manifestations of its reaction to offense which he may think strange. The reaction to offense became apparent in 1947 when a class in immunology became riotous. In 1948 a large group of students at a prominent medical school conspired to write their examination papers in *Basic English*. They were failed. The furor which this caused in a sympathetic public precipitated the numerous student rebellions of 1949. The Federal Dictator, misunderstanding the situation, and believing that the cause was inadequate facilities for students to learn the languages of the sciences, ordered each national society to prepare a dictionary of its terms. The Government was to pay for clerical work and publication but the society was to pay for intellectual direction. There was precedent for this division of cost. Industry had need for numerous compilations such as those of the older "Beilstein" and *International Critical Tables,* and, since these had grown so large as to exhaust the financial resources of contributing philanthropists, the Government had had to take over their publication. However, the Government had continued to use the volunteered services of "cöoperating experts."

In outlining the project of a dictionary of bacteriology the committee of this Society discovered that no adequate dictionary could be written without the coöperation of experts from each exact and contiguous science. It also estimated that immediate publication would be impracticable since there was required of the lexicographer the formal definition of some special word for each, slight distinction. Not only would long, historical researches be necessary to find distinction between such terms as *microaerophilic aerophobe* and *prosaerotactic microaerophobe* but the utmost care would be required in defining little words such as *rough* and *smooth.* A legal outlet was found in the writing of a set of elementary texts the simplified language of which was declared the official language of the Society. The committee used this dodge in no mean spirit but only as the immediate and admittedly inadequate solution of a more fundamental difficulty which it had learned. For the solution of this difficulty it asked and received the support of this Society in a long-time, serious project, initiated by the committee's labor of 1952–1965.

Without a knowledge of the situation which I have attempted to outline it is impossible to appreciate the overwhelming task of that famous committee which began the work of the Scientific Reformation.

With splendid logic it attempted, first, to describe in simple language the basic relations in several branches of our science. But what were then considered the criteria by which a relation could be judged to be basic? The committee did well, first to indicate the difficulty in this question, and then frankly to dodge the answer. It chose to select, in the light of advanced information, those relations which seemed at the time to be generally accepted as stabilized parts of knowledge. Type cases were presented as delineations of the subject. There was great difficulty in eliminating from many of these cases the very special hypothetical treatments in which they had become embedded, but this separation was essential to the better reselection which has been going on since.

Somewhat easier was the preparation of treatises upon the microscope, balance, potentiometer and numerous other instruments which were in common use and for which theory and practice had become fairly well stabilized. But here there remained the difficult task of tracing all concepts and mathematical formulas to their basic origins and building from these a well ordered theoretical treatment which raced smoothly to its conclusions. There were also many curious omissions to be made good. For instance, I myself have examined at random texts available in 1933 and have found: innumerable formulas for culture and staining media in which the specifications of materials were so indefinite that a chemist would not have been certain of the materials demanded; elaborate discussions of the mechanics of apparatus used in heat sterilization with no mention of the principles of heat penetration or of "thermal death rates;" descriptions of anaerobic methods with nothing concerning their physics or chemistry. Among ten elementary texts, two dictionaries, and two monographs dealing with the polariscope only one (an elementary text) defined the meaning of dextro rotation of the plane of plane polarized light and none mentioned the change in convention which had occurred in the nineteenth century! Among many texts which dealt with what was then called "pH" none gave its experimental origin and significance so clearly as to encourage the elimination of this convenient but unnecessary symbol and the adoption of the basic energy data which it crudely symbolized.

Perhaps the most difficult of all intellectual tasks is the construction of an elementary treatise on basic theory. Many of our ideas originate not in logical completeness but in response to repeated impacts of humble impressions. Theory then grows by accretion and when completed it is often, as Eddington says, like *The House that Jack Built.* Its basic definitions run in circles. To recast a body of theory, to reveal its basic postulates and its arbitrary beginnings, to construct its logical progression free from the entanglements occasioned by the accidents of history involves a heavy responsibility. If it is not met the teaching of advanced subjects suffers. Indeed, it is noteworthy that in a period when radical experiments in elementary education were numerous and when interest in the philosophy and methods of teaching was at its height, practically nothing had been done to so analyze the higher branches of learning that their greater and more *permeating* concepts would filter through to the elementary schools and there become the central themes of a consistent, systematic training. The reorganization of advanced thought was painfully slow for the simple reason that this task, which is essential to the organization of knowledge and to the development of a true pedagogy, is incomparably the most difficult of all intellectual tasks—a thousand times more difficult than the exposition of an advanced subject in terms which have become to the expert the parts of his daily speech.

An example which I give with temerity, because I am unfamiliar with its detail, will illustrate the sort of task which the committee undertook. There had been implicit in the early formulas for gravitational phenomena and in the early concepts of electrical phenomena a function which George Green in 1828 named *the potential function.* The concept of *the potential,* which in its origin at least is an extremely simple concept (that of a unit of potential energy) became invaluable in those branches of mathematics which were associated with physics. It was used throughout the higher physics in describing gravitational, electrostatic, electromagnetic and mechanical phenomena. In 1875 Willard Gibbs adapted the concept to the uses of chemistry; but accidents of history prevented it becoming well known among chemists. Consequently the biologist who took his views from texts of physical chemistry knew of *the potential* only as electrical potential. In his study of the distribution of water and of electrolytes in living things the biologist drew his ideas from very special and not always appropriate laboratory devices, his ideas of chemical equilibria from an entirely different category of thought, his consideration of electrical potential differences at the phase boundaries of the living cell and at the phase boundaries of his electric cells from still other sources. Seldom was he guided to the view that his frequent measurement of an electrical potential illustrated the application of a generalized concept useful in each of these other subjects. Perhaps this is not a particularly fortunate illustration but it serves to indicate the sort of task the committee undertook. It searched for powerful, widely applicable and permeating principles which would serve in the coördination of theoretical subjects. It restored and adapted these to a well ordered elementary instruction which could progress smoothly into advanced subjects. It enlisted the services of the highest authorities and by irrefutable logic persuaded them to abandon many of the special conventions of their subjects which contributed little to precision of thought and nothing to general usefulness. No rigidity was sacrificed to mere simplicity, but simplicity was attained by clarity of thought and excellence of exposition. Perspective and proportion were preserved and each topic was integrated with those of contiguous subjects. Brevity and adequate completeness were joined in beauty of exposition. Not a principle was mentioned that was not carefully developed from its experimental origins or its basic postulates. Not a word was allowed that did not have its roots in the fundamentals of science. Not a book came to its binding before twelve years of coöperative and intensive labor.

The importance of these basic treatises lay not so much in their intrinsic worth, great as this was. The effort to write them was the first large scale, coöperative and pedagogically effective effort which had been made to organize the teaching of science, the first real recognition of an *impasse* facing teachers in the higher schools, the first recognition of an intellectual idealism on the part of students which had been ignored, scorned, offended by those who had forgotten the origins of universities in the will and idealism of students and who thought only of their own "papers."

The immediate enthusiasm for these basic treatises was slight as it may well have been in an age which was only slowly recovering from an enthusiasm for the measure of greatness in terms of volume. But the enthusiasm was so sustained as soon to sweep through all departments. Then the effect upon the literature of science in general was remarkable. The series of basic treatises gave to the succeeding generation an almost universal appreciation of established type relations, of established theoretical principles and of permeating concepts of proven worth, so that authors could be confident that the basic origins of their own investigations would be appreciated and the language of their expositions understood. Theoretical investigations began to acquire a somewhat universal terminology and an ordered purpose. Gaps in knowledge which had been obscured by volume of publication, neglect of basic assumptions and the dodging of essential issues were more clearly seen and hence the more quickly filled. Dogmas were made transparent and the bodies of data which they had purported to describe were reorganized as bodies of data to await the perspicacity of genius. The vision of an integrated science makes trivial the newsy article, the preliminary paper, the incomplete investigation, the non-typical miscellaneous datum, the long table of unorganized results, careless reorganization of theory, unnecessary terms, the gratuitous speculative idea. The designer of instruments now leaves something to the common sense and ingenuity of the technician. There is now no occasion for an abstract such as the following, which was published in 1933. "The shield is made from a tin can."

A feeling for the foundations of science has merged with a new attitude toward the functions of publication. As the years pass, as the outline of the whole of science begins to take form, as the perspective of the past grows clearer and the hopes of the unlimited future grow brighter men find less satisfaction in the scrambles of the moment and a deeper joy in the ordered work of a lifetime. Few articles are now published which are not the work of long years—often the work of a lifetime, the results of comprehensive experiments thrice redistilled in redesign of method, in reorganization of data and in reflection. Scientific literature is on its way toward maturity.

I have given this brief historical review to remind you of the forces of evolution and of fitting example and to remind you of the prospect that science a hundred or a thousand years from now may suffer from our defects as we have suffered from the blindness of a preceding age.

The change of emphasis from factual detail to factual type, from theory suitable for "vanishing particulars" to theory as a permeating matrix has made us careless of the "conflicting fact" and neglectful of the genius who speaks an obscure language. Our assurance of an ordered future is too much like that which closed the nineteenth century. When the discoveries of the early twentieth century wrecked that old assurance men tried to "cut the wreck loose with an ax." They declared that Einstein completely replaced Newton, that Rutherford completely replaced Dalton, that Debye completely replaced Arrhenius, that DeVries completely replaced Darwin. Our renewed confidence in the irreversible achievements of science may be as smug as the attitude in 1933 which was to the effect that each advance upset all that had gone before. We shall doubtless have with us always those lesser ones who will expound only in terms of their own convenience and those others who will explain only by degrading the niceties of scientific thought to the style of a fairy tale. But this the Scientific Reformation has taught us—not to trifle with a literature that is to remain the record of emerging order. Henceforth we shall brook no compromise between simplicity and rigidity. Both qualities we shall carry to the highest ground of intellectual endeavor and there strive for a maturity of scientific literature commensurate with our vision of its beauty in the centuries to come.