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THE RÔLE OF RESEARCH IN MEDICINE[1]

In the old-fashioned, proprietary type of medical school which flourished in this country during the nineteenth century scientific research was neglected. Even where nominally associated with a university, the medical school was usually dominated by a rather narrow professional purpose, in which investigation was largely ignored. Though incidental research might be tolerated as a harmless diversion, it was more often discouraged as a mere waste of time and of no practical value. Contributions to the medical literature were usually superficial in character. As a rule, the highest ambition of the professor was to publish a text-book, which might establish or extend his fame, even though it represented no advancement of the subject through original research.

The revolution in medical education at the close of the past century brought many radical changes. The proprietary type of organization has been abandoned. We now recognize that the medical school must necessarily be an integral part of a strong university. In the reorganization of medical schools, the university ideal has become dominant. This ideal involves the search for the unknown, as well as the dissemination of the known. The modern medical schools have accepted this responsibility, and have made systematic efforts to provide for research as well as for teaching. They have recognized their duty to advance the science of medicine, in addition to the training of practitioners. Vast expenditures of money have been made to provide the personnel and facilities necessary for this purpose.

But revolutions tend to be followed by counter-revolutions. Especially in this restless, post-war period of discontentment, when all our social institutions are being challenged, it is not surprising that our medical schools are again subjected to criticism. Their efficiency has been questioned and their methods closely scrutinized. Among other things, doubts have been expressed concerning their present policies in the promotion of research. A reconsideration of the whole question of medical research and its relation to medical education therefore appears desirable.

Scientific research in general may be considered in two different aspects. Its first purpose is the increase of knowledge. With all our boasted progress, how little as yet we really know of the physical universe. Recent developments have shattered our former ideas

[1] An address at the commencement exercises of the Medical Department, University of Georgia, Augusta, June 2, 1924.

about even the structure of matter, the fundamental theories we had vainly supposed to be firmly established. And if our knowledge of the relatively simple phenomena of physics is still so inadequate, how much greater is our ignorance of the vastly more complex living organisms with which biology and medicine have to deal! Yet even the little we know forms our only basis for progress. Though still groping in darkness we have occasional glimpses of a promising future. The only hope for improvement is through continued investigation, which therefore demands our earnest attention and our continued support. The need is beyond question.

The scientific method is generally recognized as the road leading toward the desired goal. Yet there is apparently a prevalent misconception concerning the manner in which knowledge actually grows. This misunderstanding hampers the progress of all science, including medicine. To make adequate provision for continued progress, it is highly important to understand the actual mode of the growth of knowledge, which is really a process of evolution.

We may perhaps profitably compare the general process of the growth of knowledge to the development of America. First came Columbus and the other discoverers and explorers, who led the way and revealed the main features of the country and its various regions. Next were the colonizers, who established settlements at numerous convenient or strategic points. From these sources, the pioneer settlers slowly spread in various directions. The subsequent actual occupation of the country and the development of its resources were accomplished through the enterprise and energy of thousands of leaders in agriculture, industry, commerce and associated activities. But the efforts of all these numerous leaders would have been quite fruitless without the assistance and support of the millions of individual workers. Each of these played his part in making the latent resources actually available for us all. Even to this day every citizen participates to some extent in the making of America, a continuous, never-ending process. Thus leaders and followers alike are indispensable for social progress.

A somewhat similar process occurs in the extension of the bounds of knowledge. A popular but erroneous idea is that advancement is accomplished solely by the inspiration of a few great geniuses. These are, of course, essential and invaluable, but their discoveries are never entirely independent. They always represent the culmination of a series of steps or stages, including the thoughts and efforts of numerous previous workers in the same or related fields. In a very real sense, "there is nothing new under the sun." So interrelated and interwoven are the infinite parts of our common body of knowledge that they seem to form a vastly complicated mechanism, like a living, growing organism. The new develops from the old. From the very nature of things, an entirely new discovery would be quite incomprehensible to our minds. It could not be assimilated, and is therefore a practical impossibility. Every apparently new idea has its roots reaching far back into the past, and also its branches extending into the future.

By extension into the future, I mean that no discovery is completely established on first appearance. By sad experience, we have learned that caution is necessary in accepting even the most plausible new theory. Before its merit as truth is finally determined, it must be repeatedly tested and tried in its various relations. Its range and its limitations must be determined. It must run the gauntlet of skepticism by its opponents on the one hand, and of unwarranted enthusiasm and credulity by its advocates on the other. Through the inevitable test of experience, every new idea or discovery, whether great or small, thus gradually passes from the realm of uncertainty, and as confirmed approaches (but never quite reaches) the goal of absolute certainty. This applies to abstract truths or general principles and likewise to their applications, to both discoveries and inventions. The evolution of human knowledge is thus a process in which we all participate, consciously or unconsciously. In admiring the achievements of genius, we should not forget the important aid of the many plodders of lesser talent.

Nowhere is this more true than in the field of medicine. No discovery in the basic medical sciences, no advancement in the art of healing is to be credited to any single individual. Even the greatest heroes of medicine, those most richly endowed with the precious gift of creative imagination, are indebted to their predecessors for instruction and inspiration, to their contemporaries for criticism, and to their successors for the final adaptation and evaluation of their most original products. The pages of the history of medicine are crowded with examples with which you are all familiar.

The important discovery of the hormone secretin by the English physiologists, Bayliss and Starling, may serve as an instance. In referring to this work, Starling recently said:

> It was of no practical use to any one, but a source of much gratification to ourselves, since it seemed to open up a new chapter in our knowledge of the body. But there were at that time half a dozen workers skating along the edge of the discovery, and it is difficult to comprehend why, for example, Wertheimer and Lepage did not take the one further step which would have made them and not us the discoverers of secretin. . . . Every discovery, however important and apparently epoch-making, is but the natural and inevitable outcome of a

vast mass of work, involving many failures, by a host of different observers, so that if it is not made by Brown this year it will fall into the lap of Jones, or of Jones and Robinson simultaneously, next year or the year after.

Similarly, credit for the recent discovery of insulin belongs to no one man, nor even to any small group of men. Banting and Macleod, to whom the Nobel prize in medicine was awarded, very generously and properly shared it with their colleagues, Best and Collip. While these Toronto investigators fully deserve the greatest praise for their achievement, it is nevertheless well known that many others had previously worked on this problem, clearing up various preliminary stages and even coming very close to the final solution. It is unnecessary to rehearse the interesting story, which involves the unconscious coöperation of hundreds of workers, even to go back no farther than the discovery of the pancreatic islets by Langerhans in 1869. The subsequent elucidation of the nature and function of these islets required the patient labor of a long series of morphologists, physiologists, pathologists, biochemists and clinicians. According to tradition, one important step (the experimental production of diabetes by extirpation of the pancreas) was due partly to a German laboratory *Diener,* who happened to observe a sweet taste in some crystals formed by the evaporation of urine from the test animals. Accident thus plays a part in scientific research, though, as Pasteur remarked, "chance favors the prepared mind."

But even with the discovery of insulin, the subject was by no means exhausted. Literally thousands of workers, both laboratory scientists and clinicians, are now actively engaged in a further study of insulin, its chemical structure, physiological significance, standardization for clinical use, methods and limitations in various stages and complications of diabetes and in numerous other related problems. The utilization of insulin in country practice, in contrast with its use where laboratory and hospital facilities are available, raises a series of questions which can be answered only by the test of experience. It is believed that further research upon insulin will shed light on other obscurities of metabolism, with results which eventually may prove even more important than the conquest of diabetes mellitus. At any rate, this problem has enlisted the efforts of a multitude of workers interested in the various phases of what is really a huge, loosely coöperative investigation to perfect and extend our knowledge of insulin.

The recent advances in the field of nutrition, as related to the deficiency diseases, likewise represent the results of a large number of workers in various countries. The history of the discovery and application of the diphtheria antitoxin (on which work still continues), and the problems of scarlet fever and of goiter are other examples from the many which could easily be cited.

I would like to emphasize the fact that even the ordinary practitioner in the smallest village, far from hospital, laboratory or library, can, if he will, make *some* contribution to medical progress. It is unnecessary to remind this audience of Crawford W. Long, the rural doctor who first used ether anesthesia in a surgical operation. Another Georgia physician, Robert Battey, was the first to remove the ovaries for the relief of intolerable dysmenorrhea. I commend to you, members of the graduating class, these examples of your distinguished fellow-citizens as demonstrating what may be accomplished, even under very unfavorable conditions. While you may not be so fortunate as to make great discoveries, it is your privilege and duty to do what you can in the promotion of medical research. Each one of you should strive to repay at least slight interest on your great indebtedness for the common social heritage of our present medical science. If you will cultivate the scientific spirit, if you will make your observations accurately and record them carefully, if you will later study critically your own data (or make them available for the use of other workers), and if you will honestly test your conclusions, you can not fail to promote in some degree the advancement of medicine.

There are, indeed, certain important phases of medicine for the study of which the opportunities are in some respects most favorable in small communities. For example, various hereditary aspects of disease can be studied most readily in rural districts, where the population undergoes relatively little migration. Here likewise the effects of racial intermixture upon predisposition and immunity can be most easily followed. Or, as a more practical problem, it would be interesting and instructive to compare the complications met in a large series of obstetrical cases in country practice with those found in the city or under hospital conditions. Every physician of an inquiring mind, wherever he may be located, can readily find all about him numerous medical problems of importance, which he can help to solve. Whether his ability and opportunity be great or small, it is his duty to make the most of them. Aristotle long ago noted that: "The search for Truth is in one way hard and in another easy; for it is evident that no one can either master it fully or miss it wholly. But each adds a little to our knowledge of Nature, and from all the facts assembled there arises grandeur."

The recognition that mediocrity must share with genius in the advancement of science is a matter of practical importance. Therein lies the answer to those who urge that progress would be facilitated by concentrating the support for scientific research into grants or prizes limited to the few investigators of the first rank. These gifted individuals should, of course, be rewarded and honored. The most ample facilities should be placed at their disposal. But let us not make the serious mistake of failing to recognize also the value of the contributions from the lesser lights. Their work is likewise indispensable, and each deserves encouragement and support in proportion to his merits. Our efforts to promote progress through scientific research should therefore be directed not merely to the training and support of a few talented investigators, but to securing the maximum result by aiding wisely the activities of all. While many agencies may cooperate in the prosecution of research, we naturally look to the schools as the most potent factor in the promotion of science.

This brings up the second aspect of the research question. While the resultant increase of knowledge would fully justify all our efforts to promote scientific research, there is another reason equally good, though often overlooked or unappreciated. It is the educational principle that all instruction is most effective when imbued with the research spirit, inculcating the scientific method.

Let us first examine this question in its relation to general education, with which medical education is inseparably connected. We may assume that the primary purpose of education is to afford training which will aid in solving the problems of life. Broadly speaking, the methods of education, past and present, may be classified under two headings: the dogmatic and the scientific. Until quite recent times, education has been almost exclusively by the dogmatic method. The underlying principle of this method is to provide a system of rules for the guidance of conduct, or, in other words, a set of ready-made solutions for the problems of life. The teacher "lays down the law"; the student accepts it passively. Learning is chiefly through memorizing from lecture or text-book. This, the method of authority, has been a favorite throughout the ages, and is still predominant.

Doubtless many of you can recall personal experience with this system, especially in the elementary schools. In arithmetic, where there was perhaps the best opportunity for independent thinking, you noted that many students, through laziness or inability, relied entirely upon their classmates to solve the set problems. The more able and industrious faithfully worked out these problems, following the rules and "examples" as given in the text-book, but with little or no comprehension. Occasionally an ingenious pupil might actually dare to propose an original solution, to the astonishment of the class and (often) to the perplexity of the teacher. In the other branches of study, the methods were similar. Memorization was everywhere at a premium, and originality discouraged.

While conditions have undoubtedly improved, there is ample evidence that the cultivation of independent thinking in our schools and colleges, though recognized in theory, is still too rare in practice. The dogmatic method stubbornly persists, in spite of the efforts of numerous educational reformers, who, from time to time, have tried to introduce the scientific method as the basic principle of pedagogy. The pioneer in this attempt was Comenius, who nearly three centuries ago urged that:

> Men must, as far as possible, be taught to become wise by studying the heavens, the earth, oaks and beeches, but not by studying books; that is to say, they must learn to know and investigate the things themselves, and not the observations that other men have made about the things. We shall thus tread in the footsteps of the wise men of old, if each one of us obtain his knowledge from the originals, from the things themselves, and from no other source.

Rousseau likewise insisted that our first teachers are our hands and eyes. "To substitute books for them does not teach us to reason, it teaches us to use the reason of others rather than our own; it teaches us to believe much and know little." Rousseau expressed what is now termed the heuristic method as follows: "Let him know nothing because you have told him, but because he has learnt it for himself. Let him not be taught science, let him discover it." The same plea has been echoed in vain by subsequent reformers, down to the present day. Even in the teaching of the sciences, the dogmatic method, rather than the scientific or heuristic, has generally prevailed.

This heuristic plan of teaching seems to be in especial accordance with the nature of childhood, where curiosity is so prominent. Every normal child is a living question mark and an eager investigator. Soon, however, this native curiosity seems largely to disappear. Is it merely outgrown, along with other primitive characteristics of the childish mind? Is it stifled by our artificial mode of education? Or does it still persist, though diverted into other less obvious channels? The answer to these questions is important for our present theme, because the research spirit depends to a large degree upon the persistence of curiosity as a mental trait. While society encourages scientific progress for utilitarian purposes, the strongest motive for research in the individual is the desire to know. An unquenchable thirst for the truth is the chief stimulus of scientific investigation.

While this primitive spirit of inquiry persists in every one to some extent, in most individuals it apparently weakens. Conservatism notably increases with age. Human nature prefers to follow the lines of least resistance, and drops readily into the well-worn channels of custom. Innovations are disturbing and are therefore subconsciously resented. This perhaps may help to explain why authority and book-worship remain so strongly intrenched, and why the scientific attitude makes slow headway in our schools.

Another drawback is the greater difficulty and expense of teaching by the scientific method. It requires more able teachers and more extensive equipment. As a result only feeble and sporadic attempts have yet been made to train students in the systematic application of this method to the problems of life. It is true that the pedagogic doctrine of formal discipline has been abandoned, and it is realized that no specific form of training gives ability to cope with other problems quite different in character. Nevertheless, in school work mental and physical habits are formed, and these do carry over and affect behavior in new and different situations. This possibility gives science unique significance in education, and justifies great hopes for the future.

The obvious defects in our present system of general education naturally limit the possibilities of later medical instruction. Medical students begin their work handicapped by inadequate earlier training. They have already passed the youthful age of greatest mental plasticity, when desirable habits and attitudes are most easily established. Medical teachers often complain that the incoming students memorize readily but are notoriously deficient in powers of independent observation and reasoning. Especially in the earlier years of the medical course, it is necessary to devote much time and energy to training in scientific method, if this is to be made the basis of medical education. Great emphasis is therefore laid upon scientific research, even during undergraduate work, because it is assumed that the same principles and methods are required for efficiency later in the practice of medicine.

But is this a valid assumption? Some writers have lately challenged this principle, asserting that while scientific research is desirable for the advancement of knowledge, it bears no relation whatever to efficiency in practice. Thus a recent critic (in "Our Medicine Men, by One of Them") states that "there is no such thing as a science of medicine and that the study of disease is a matter distinctly apart from the art of healing"; that clinical practice "is entirely antithetical to the spirit of science"; and that in medical education "what is necessary is a sharp delineation between the practical sheep and the scientific goats. This should take place at the very beginning of the medical course."

An editorial in a recent number of *Surgery, Gynecology and Obstetrics,* criticizing undergraduate medical teaching, likewise asserts that, "If perchance we should occasionally turn out a scientist or an embryo discoverer, so much the better, but it is a fatal mistake to try to make all our students research workers in the hope that one or two of each class may ultimately turn out to be a real scientist."

A prominent physician, in a recent article on medical education, similarly maintains that:

> There is no place in the actual practice of medicine for sustained medical research. It is condemned by law, and, according to all moral standards, is a reprehensible practice. No man employs a physician to advance science at his expense and risk. No man would knowingly tolerate it. If a student is taught to do research work on his patients in the university hospital and is complimented on something great or small in connection with it, he might become convinced that that was a proper or permissible course—convinced, in a word, that the patient is only a sort of advanced laboratory animal. Could he then reasonably be expected to disgorge his mind of all these impressions and become an honest servant of his patrons on graduation? Would he not feel justified in trying out his new ideas on them?

The obvious reply is that all depends on the definition of research. That every case presents a new scientific problem has often been pointed out and is generally admitted. Every patient is not only willing but anxious to be the subject of scientific research, if he understands that to mean the most careful attempt to discover the exact nature of his affliction, and to find the most appropriate and efficient remedy. The scientific spirit is entirely consistent with the highest humanitarian motives, which make the welfare of the patient the foremost consideration.

It would indeed be hard to find a better example of scientific methods than the ordinary procedure in medical practice. The diagnosis represents the first step in the analysis of the problem. For the discovery of the existing facts, the case history is taken. This is followed by a systematic and careful examination to reveal the present state of the patient, the observer using both the unaided senses and the various instruments of precision. In seeking the cause of the disorder, provisional hypotheses must be verified or corrected by comparison with the observed data, and by further tests for purposes of differential diagnosis. The prognosis really represents a scientific prediction, the validity of which is tested by the outcome. The choice of treatment is a deduction from the diagnosis, under all the ascertainable conditions affecting the individual case. The results of treatment further confirm or disprove the previous line of reasoning. Rational principles likewise determine the method by which the recurrence of the disease is most likely to be prevented. Thus medicine exemplifies the typical scientific methods of observation, hypothesis, deduction and experimental verification.

As Barker puts it:

The study of a single patient by modern methods includes the making of a very large number of experiments, that is, of test procedures adopted on the chance of their yielding to observation under especially controlled conditions definite information that is not obtainable by simple non-experimental observation. There is no other science in which the technic of accumulating facts is as extensive as in clinical medicine, for its methods of examination are based on and include the technical methods of all the preliminary natural sciences and of all the intermediate, simpler preclinical sciences.

In contrast with this concept of medicine as a science, we have what may be styled the empirical method, which disdains science as impractical, and attempts to deal directly with the phenomena on the basis of experience. The empiric generally treats merely the symptoms, or the symptom-groups (syndromes), which are designated as the various diseases. He scorns the scientific concept as theoretical and visionary. Yet often he is unconsciously himself a slave to the most absurd theories, which lead him far astray. Empiricism sometimes achieves brilliant success, but more frequently results in failure. As Minot once remarked, the difference between the so-called practical doctor and the truly scientific is that the patients of the former are more likely to die. The scientific practitioner looks beyond the symptoms and persistently seeks to remove the real cause of the disorder, by more difficult but more rational methods. He knows and frankly admits his own limitations, which is the beginning of scientific wisdom. Success in practice demands scientific judgment and practical skill, both of which are dependent upon the character of the previous training, though perfected by further experience.

We should, therefore, protest against the custom of designating the fundamental branches of medicine as "scientific," in contrast with the "clinical" subjects, which by implication are thereby considered unscientific. The methods used are, or should be, essentially similar in all parts of the field of medicine. The primary aim of the practitioner is to interpret and control the clinical phenomena. The purpose of medical education is to give the training which makes this possible, through a working knowledge of the human organism under both normal and abnormal conditions. Mastery of the basic data and their utilization at the bedside alike require the persistent application of the same scientific methods by which all knowledge is discovered. While medical instruction can not possibly foresee all the specific problems which will arise in practice, it can and should provide the methods of procedure by which the future practitioner will be able to meet and solve these problems in the most effective manner. The more thoroughly medical practice becomes imbued with the spirit of scientific research, the more successful will it be.

The scientific method should, therefore, be made the guiding principle throughout the medical curriculum. In all medical instruction, it should be kept constantly in mind. Every teacher should be an active investigator. Moreover, I firmly believe that every medical student, at some time during his course, should have the opportunity to undertake on his own account a modest bit of original research in the more formal sense. Although the resultant contribution to the common stock of knowledge in most cases would be slight, the experience would be invaluable in giving him a better grasp of the scientific method, its uses and its limitations. The limited elective system in the medical curriculum, now growing in favor, gives this needed opportunity. If adopted in all first-class medical schools, this system would provide, or at least make possible, a good substitute for the medical thesis, an ancient requirement still maintained in the best medical schools abroad. This encouragement of productive scholarship helps to emphasize the scientific character of medicine.

The application of scientific research to the problems of medicine will doubtless result in the improvement of medical practice, not only along the conventional lines, but also in opening up new modes of treatment. For example, the study of psychology in its relation to medicine presents, in my opinion, one of the most promising and fruitful fields of the future. I refer not merely to psychiatry, but to the psychic aspects of medicine in general. Psychotherapy has been sadly neglected by the medical profession, though largely utilized in empirical fashion by various cults and quack systems. Medical science should be open-minded and devoid of prejudice, ever ready to recognize the truth, whatever its source. Great possibilities are in store for the future, perhaps in directions entirely unexpected.

In our efforts to promote medical progress we must be not only tolerant but patient. We must not expect results too quickly. In medicine, as in other fields, we should realize that "Science moves, but slowly, slowly, creeping on from point to point." The achievements of the past, however, encourage us to look hopefully to the future. We are on the right path and moving in the right direction. And with the more general recognition and support of the scientific method in medical education and practice, we may confidently expect that improvement will become increasingly rapid and certain.

In conclusion, let me repeat that in medicine as elsewhere the rôle of scientific research is twofold. In the first place, it presents the indispensable means for the continued growth of our common body of knowledge, a process in which plodding talent must cooperate with creative genius. In the second place, it provides also the methods by which this knowledge can be most efficiently applied in solving the daily problems of life. We must, therefore, recognize the fundamental importance of the research spirit, for education in general as well as for medical training and practice.

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RECENT ACHIEVEMENTS IN PALEOBOTANY

UNTIL within the last few years a sort of forbidding lonesomeness has seemed to attend the student of fossil plants. Paleobotany is in fact the last of the paleobiologic trio to reach accuracy and finality in the methods of research employed. Above and beyond the discovery of new materials, the great need is men. There are far too few workers in definitely recognized paleobotanic positions which make active contribution possible—in all the world a few over twenty. In Germany there is Gothan, Kräusel and R. Potonié; in Sweden, Halle; in France, Carpentier and Paul Bertrand; in Austria, Kubart; in Holland, Jongmans; in Great Britain, Kidston, Lang, Scott, Seward, Benson, Weiss, Stopes, Oliver; in India, Sahni; in the western world, Knowlton, Berry, Noé, White, Jeffrey, Wieland, Goldring, Torrey, Chaney.

Where the workers are so few, contributions uniformly fail of needed criticism, and such comment as does rise to the surface may not have a sufficiently constructive effect. Only in subjects where elaborators are active and their fields somewhat overlap, does criticism become an organic, functioning thing—a force such as can carry the study of ancient plant life to the goal. There is pith in the remark of Dr. Marie Stopes that "paleobotany requires a serene civilization."

Turning to actual discoveries the initial dates may serve. Taking the past thirty-five years, that is, the close of the last century and the opening of the present, the outstanding discoveries and elaborations in the larger fields are: Seed cone of Bennettites Morierei, Lignier, 1894; finding of new localities of American cycads, Macbride, Ward, '93–94; volumes on the status of the Mesozoic floras with initial description of cycadeoids by Ward, '99–'05; cycadeoid collection and discovery of fructification and foliage, Wieland, 1898–99; early Williamsonians, Nathorst, 1902; great development of "coal ball" study by the British group of paleobotanists leading to the publication of great text-books on fossil plants and especially to the determination of the seed ferns by Oliver and Scott, 1903; taxonomy and distribution of American Carboniferous floras by White with codiscovery of seed ferns; the microspore-bearing disk Codonotheca, Sellards, 1903; chemical methods and demonstration of Williamsonians by Nathorst, 1907; sustained investigations of American dicotyledonous floras by Knowlton and Berry; brilliant study of the Kreischerville lignites by new methods, Hollick and Jeffrey, 1909; many Mexican Williamsonians with the latest of the typical Cordaite floras, Wieland, 1916; Rhynia of the lower Old Red of Scotland, the most primitive of vascular plants, Kidston and Lang, 1917; extension of the Hollick and Jeffrey methods to wider studies of lignitic gymnosperms of Cretaceous time, Torrey, 1923; collection and initiation of microscopic study of an immense series of American "coal balls," Noé, 1923; the Gilboa forest plants of the Upper Devonian of New York, Goldring, 1924.

While it is unlikely that any of these events, discoveries and accomplishments will seem to diminish in importance as the years pass by, others not mentioned may be found significant. Other names stand out, such as Zeiller, Grand-Eury, Bertrand, Gothan, Pelourdé. Although when the present century draws to a close some reviewer will surely throw into broader and fuller light as an outstanding event of these earlier years the discovery of the Pennsylvanian "coal balls." There doubtless are calcified parts of coals of extensive occurrence in other horizons than the Carboniferous. But it already appears that if any pre-Cretaceous forests may ever be fully and thoroughly reconstituted from a cosmopolitan record they must be those of the coal swamps. It is improbable that vegetal life of other periods can ever be so extendedly visualized from precise structure. The coal itself, both bituminous and anthracite, has been recently found from polished surfaces etched by flame to have the structures of lignites and of the "coal balls." Only in the latter (petrified parts of coal seams) has nature done the staining and imbedding for the investigator, who merely requires application of the simpler but well-carried-out arts of the lapidary and thin sectioner of rocks.

Just 69 years have passed since Joseph Dalton Hooker and Edward William Binney turned their attention to the seeds of the coal balls, and it appears incredible that Americans should have let all these years pass away without noting or reporting a single coal ball or cutting a single thin section from such, if a few sections of English material cut at Yale be excepted. However, the American coal balls are all at once here from Illinois, Indiana, Kentucky, Texas; and they are the key to a record of imposing extent and importance. What limits may be set to the data these great accessions may be made to yield no one now living may say. As we stand at the threshold of this new era in the study of ancient plants, the question of questions is, What were the ancestors of the Jurassic and lower Cretaceous dicotyls? Are the