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THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE METHOD OF SCIENCE[1]

Science governs human life by determining the conditions of existence and by furnishing the means of civilization. Religion prescribes the motives, government formulates the customs of mankind, science fixes what we can do and how. If, at the present meeting, we appropriately emphasize the rôle of science, it does not imply that we belittle the ethical or social factors of civilized life, but answers the demand for a more just and general recognition of the actual importance of pure science.

We are so accustomed to the practical advantages that have followed from abstruse science, that we connect them with their source only by a distinct mental effort. The wonders of practical science have been recited so often, that their reiteration has become tedious, and we no longer feel strongly impelled to felicitate mankind on the parlor match, the telephone and the antitoxines, although we indulge at present in an unsubdued excited anticipation of wonders to come, especially in the domain of medicine. Are we not all on the watch for the announcement of the cure for cancer, and vaguely for other new and astounding reliefs from disease! Such concentration of interest upon novel practical results is not wholly favorable to science.

It is true that a large amount of investi-

[1] Vice-presidential address delivered before the Section of Physiology and Experimental Medicine of the American Association for the Advancement of Science, at Minneapolis, December 29, 1910.

gation is going on which aims to secure immediate practical results. In chemistry and medicine especially the activity in the work of applied science is very great. This condition gives a powerful fresh reason for defending pure abstruse science. Applied science always has been, is now, and probably always will be distinctly subsidiary to pure science. The final justification of all scientific research is undoubtedly the power it creates for the use of mankind, but the power must be created before it can be used. A little study of the history of science should suffice to convince any reasonable mind that the command we possess to-day over nature is due to the labors of men, who have almost invariably pursued knowledge with a pure devotion uncontaminated by any worship of usefulness. These devoted idealists have gathered the varied mighty harvests by which all men have profited, but the debt of gratitude to them is unpaid.

The pursuit of abstruse science needs to be encouraged. It is insufficiently esteemed. This doctrine ought to be emphasized on all suitable occasions, but especially before the section of experimental medicine. The people cry for relief from sickness and their demand for prompt useful discoveries is so urgent that there is danger in it, since it tempts medical investigators away from the fundamental enquiries, which, answered, will give great results, and seduces them to work exclusively at secondary problems, from the solution of which quicker, but smaller results may be expected. Pure science is broad; it embraces all. Applied science is a congery of fragments, of isolated problems, which lack cohesion and are without any necessary connection with one another. It is easy to understand why students of applied science have seldom made great discoveries.

In fact, scientific knowledge will not be compelled. We have to take what knowledge we can get, and by no means can we get always what knowledge we want. Pure science adapts its undertakings to these rigid conditions, and works where the opportunity is best—not so applied science.

Let us recall a few of the epoch-making discoveries. When Galileo turned his lean face up towards the swinging lamp in the Cathedral of Pisa and as he looked discovered the law of the moving pendulum, he was in quest of pure knowledge. We can not conceive such a man actuated by any lower motive. Even when we learn of his astonishing the Venetian merchants by enabling them to see their far-off vessels through his newly invented telescope, do we not feel that it was merely an episode to Galileo? Such a man does not ask "What use is it?" His demand for knowledge was insatiable. When Newton thought out the problem of gravity and his theory of planetary motion; when Malpighi explored the structure of animals with his crude microscope; when Lavoisier created modern chemistry; when Cuvier combined comparative anatomy and paleontology and made the combination yield new revelations; when Lyell proved geological history to be an evolution and not a succession of cataclysms; when von Baer against immense difficulties traced the development of the chick; when Schwann demonstrated the correspondence of cellular structure in animals with that of plants—was one of them actuated primarily by the wish to get practical results? We have only to read their works to convince ourselves that they were all in search of knowledge for knowledge's sake. Yet they are the giants of human history, who in importance are approached by few monarchs or statesmen. Compared with the growth of science the shiftings of governments are minor events. Until it is clearly realized that the gravest crime of the French revolution was not the execution of the king, but the execution of Lavoisier, there is no right measure of values, for Lavoisier was one of the three or four greatest men France has produced.

Since pure science has been preeminent in the past not only in furnishing useful knowledge, but also as a chief foundation of human progress, and is likely to long continue equally preeminent, it is well worth while to study the general principles by which original research is guided. No previous definite study of these principles is known to me, although I have searched not a little to find one. All that I have been able to discover are treatises on logic, the reading of which, most active investigators would, I fear, find tedious and unprofitable rather than helpful and inspiring. We have too many real difficulties to quite enjoy wading through the artificial morass of pedantries, in which logicians by profession embed their significant truths. The stricture is severe, but not too severe even for so sound and valuable a work as Jevons's "Principles of Science." It must be doubted very seriously whether the study of logic is really essential for the right training of an investigator. While it goes without saying that logical thinking is indispensable in science, neither may it be overlooked that thinking is a complicated physiological function, which is brought to efficiency by practise, and that training by actual use is the one indispensable means of disciplining and developing the function. Playing the violin is a complicated physiological function, but it is not thought necessary that the violinist should study the anatomy of the muscles and nerves of the hand and arm. He perfects himself by practise. Anatomical knowledge might enable him to understand why he can make certain motions and can not make others. Our analogy limps perhaps, but is a real analogy, for practise in right thinking creates the necessary habit of being logical, and ability to describe the mental processes in the language of logicians is an accomplishment which few even of the greater scientific discoverers possess.

It is my belief that the logical work of scientific men is usually well done, and is the part of their work which is least faulty. The difficulties and the majority of failures are due, it seems to me, to two chief causes, the first inadequate determination of the premises, the second exaggerated confidence in the conclusions. If I am right, the method of science is the result of the effort to get rid of these two causes of error.

We must recognize in starting that the expression "the method of science" means more than "logic," being far more comprehensive when rightly defined. We can not alter the fundamental conditions of knowledge, for we are still unable to add new senses or improve the brain—although eugenics dreams of a future with such possibilities—nor can we change the nature of the phenomena. The same fundamental resources are available for daily life and for science. We must be clear in our minds on this point, in order to comprehend that the fundamental distinction of the scientific method is its accuracy. As I have said on another occasion "there is nothing to distinguish the scientific method from the methods of every-day life except its precision. It is not a difference in kind or quality, but a quantitative difference, which marks the work of the true scientist and gives its validity." Such being the case, a broad examination of the method of science reduces itself to the study of the general principles of securing accuracy.

If you will examine frankly your own opinions and those of your acquaintance you will, it may be presumed, quickly acknowledge that many, perhaps most, of the opinions are not of scientific accuracy. On the contrary, they are, to a large extent, mental habits and the result of the summation and averaging of impressions. I, for example, know a generous man, but can give very little of the evidence on which my opinion is based. I know a seacoast on which fog occurs in summer quite frequently, yet I can not state how often the fog occurs nor just when I have observed it. At sundry times I have received an impression, in one case of the man's generosity, in the other of fog. The exact data can not be recalled, but the impression on my mind has been fixed by repetition. The evidence is lost, but the conclusions persist and are accepted by me as correct. For my practical needs they are sufficient. We get along in ordinary life satisfactorily enough with opinions thus formed by summation. Most human opinions, even when they are merely imitative, originate in this way, and are correspondingly unreliable. If we seek to explain the fallibility of ordinary opinions and testimony must we not attribute it to the absence of the detailed evidence and the consequent impossibility of verifying the testimony?

We are thus led to recognize the preservation of the evidence as the fundamental characteristic of scientific work, by which it differs radically from the practise of ordinary life. I venture accordingly to define the method of science as the art of making durable trustworthy records of natural phenomena. The definition may seem at first narrow and insufficient, but I hope to convince you that it is so comprehensive as to be not only adequate, but also almost complete.

All science is constructed out of the personal knowledge of individual men. Science is merely the collated record of what single individuals have discovered. Accordingly, we must consider, first, the way in which the individual knowledges are recorded and collated. The process begins, of course, with the publications of the special scientific memoir in which the investigator records his original observations and makes known his conclusions. Permit me to quote from Oldenburg's preface to the first volume of the *Philosophical Transactions of the Royal Society*. The date is 1665.

> Whereas there is nothing more necessary for promoting the improvements of Philosophical Matters, than the communicating to such, as apply their Studies and Endeavours that way, such things as are discovered or put in practice by others; it is therefore thought fit to employ the *Press*, as the most proper way to gratifie those, whose engagement in such Studies, and delight in the advancement of Learning and profitable Discoveries, doth entitle them to the knowledge of what this Kingdom, or other parts of the World, do, from time to time, afford as well of the progress of the Studies, Labours and attempts of the Curious and Learned in things of this kind, as of their compleat Discoveries and performances.

All that he says is true to-day, although our taste has changed in favor of shorter sentences.

It is interesting to note that our present standards for original memoirs have developed gradually. In Harvey's essay on the circulation of the blood, published in 1628, there are no precise data as to his observations. The author does not think it necessary to specify how he has laid bare the heart or how often he has repeated his observations. His descriptions of the beating heart are vividly realistic. He writes with conviction and authority. The reader is compelled to believe him. Harvey, however, does not provide information to facilitate repetition of his work—he offers little aid towards the verification of his results. Francesco Redi, the founder of experimental biology, published his "Generation of Insects" in 1660. His experiments proved that insects are not spontaneously generated in putrifying meat. His conclusion[2] is sound, but he does not give more than a general account of the actual experiments. A century later Spallanzani established the modern standard, and in his works we find the details as to his evidence put down with scrupulous care, for example in his paper on the circulation (1773) the single experiments are exactly described. But Spallanzani in this, as in other respects, was far in advance of his time.

In a cotemporary article we expect a presentation of all the data necessary to render subsequent verification by other observers possible. We further expect clear information as to the amount of material on which the observations were made, or the number of experiments on which the work is based. In other words, a modern investigator will hardly receive consideration for his researches unless he furnishes every aid he can to facilitate criticizing and testing his results. This severe standard has been only gradually evolved, but is now stringently enforced in all departments of science and is the response in our practise to our need of eliminating the purely personal factor. It would be advantageous if scientific authors generally viewed the obligation of providing for verification as an even more serious duty than it is esteemed at present. It might, indeed, be a wholesome practise to demand that every scientific article should contain a special section or paragraph on the means of verifying the result, for verification by *Fachgenossen* is second in importance only to discovery in the progress of science.

The conditions of scientific progress have changed greatly though very gradually. Two hundred years ago the number of active investigators was small. This year there are at least ten thousand men of substantial ability carrying on original researches, consequently each theme is being worked at by several men, and the final outcome is the consequence of collaboration, which is none the less actual and effectual because it is unorganized, and is usually not formally designated as collaboration. For example, our present knowledge of the complex and very varied processes of cell-division has been constructed not merely by successive accumulations, but also by incessant debate and repeated mutual criticism. If we examine a paper on mitosis we find not merely the author's own observations, but also references to other related investigations, to specify which there is often a formidable bibliography. Within a generation the modern science of bacteriology has been created. Within a few years radiology, the wonders of which still thrill us, has suddenly come into existence. Both great achievements are the results of both the original observations and also the constant mutual discussions of a number of scientific men.

These conditions have rendered great men somewhat less important than formerly. Science grows by the accretion of ideas. Now, a great man has, let us say, twelve new ideas, where a man of ability has one. If science gets twelve new ideas it matters little whether they come from one man or from twelve. To a certain

[2] At vero ubi loco ita clauso illud (stercorem bovis) dentinui, ut intrare muscae & culices, et ova sua ponere non possent, nihil omnino natum vidi.

extent numbers make a substitute for genius—but nothing probably will ever replace that type of great genius, to which we owe most, the man who has a great thought, which no one has ever conceived before.

The nineteenth century in response to the new conditions, which have arisen in its course, has added another new standard for scientific memoirs—they must include a conscientious consideration of recent and cotemporary related work. Now the second step in science-making, after recording the new original observations, so as to make them accessible to others, is the collation of these same observations into broad general results. The aim is to eliminate the personal factor and to impart the character of impersonal absolute validity to the conclusions.

In addition to the original memoirs science profits by a large number of publications, almost all of which are of modern, often of very recent, creation. Broadly speaking, their aim is to promote that collation, which is begun in the original memoirs. Germany is the home of most of these undertakings, which are familiar to us under the names of "Jahresberichte," "Centralblätter" and "Ergebnisse." So far as I have learned, Jacob Berzelius's "Jahresberichte" for the physical sciences, which Gmelin translated into German, issuing the first volume at Tübingen in 1822, was the first ancestor, the Adam, of this modern biblic race, which therefore can not yet celebrate its first centenary. As concerns those branches of biology known as the medical sciences, the summarizing publications under consideration have become important only since 1870, although they began earlier. For biology 1834 may be taken as the starting point, for it was the initial year of Schmidt's "Jahrbücher der gesammten Medicin" and of Johannes Müller's first Jahresbericht. Meckel had just died and Müller assumed the editorship of the *Archiv für Anatomie und Physiologie,* which he conducted for so long that it is still often known simply as Müller's *Archiv,* although the *Archiv* since his death has had several distinguished editors. Müller wrote for the *Archiv* the first Jahresbericht entirely himself. His report is interlarded with many keen criticisms and even with references to unpublished observations of his own. Later he engaged others to assist in the yearly reports, which were kept up until 1857. Their place was taken by Henle's Jahresberichte, which were continued until 1871, when they in turn were replaced by the *Jahresberichte der Anatomie und Physiologie* founded by Franz Hoffmann and Gustav Schwalbe in 1872. The growth of anatomical science is indicated by the fact that in round numbers 400 pages sufficed for the abstracts of anatomical papers in 1872, but 1,500 were necessary in 1908. Similar increases have occurred in the output of the other medical sciences, hence it has become more and more difficult to bring out the Jahresberichte promptly—a delay of two or three years is common. To meet this growing difficulty the various Centralblätter have been started—those with which we are here concerned are periodicals issuing small numbers (Hefte) at short intervals and filled with brief abstracts of recently published researches.[3] They have proved of limited utility and their completed volumes are so inconvenient to consult that one

[3] The dates when some of the Centralblätter started are as follows: für medizinische Wissenschaften, 1863; für Physiologie, 1887; für Bakteriologie, 1887; für allgemeine Pathologie, 1890; für allgemeine Biologie, 1910. Although the number of German "Centralblätter" is very large, yet in other countries corresponding magazines are viewed with limited favor.

habitually avoids them. They are useful, perhaps, at the moment of publication, but the back volumes encumber rather than enrich our libraries. Fortunately the last decade of the nineteenth century brought us a new and very valuable form of report, the avowed purpose of which is the systematic collation of results. I refer to the "Ergebnisse." The earliest of them known to me was founded in 1892 by Merkel and Bonnet to cover anatomy and embryology. The annual volumes contain essays on various topics which really collate recent discoveries; they differ fundamentally and advantageously in method from the Jahresberichte and Centralblätter, by presenting a combined picture rather than abstracts of single papers. They are substantial contributions to science because they systematize and coordinate the new information. The enterprise of Merkel and Bonnet deserves our most grateful appreciation. Its value is witnessed to by the foundation of similar "Ergebnisse" for other sciences. The series for pathology began in 1896, for physiology in 1902, for zoology in 1909. In the admirable *Revue d'Histologie* (1906) they found a French follower. The "Ergebnisse" are likely to prove of increasing importance and as the number of new investigations mounts higher and higher their comprehensive essays will become even more indispensable than at present.

Although logically more remote from the original sources than the annual and special collations just reviewed, yet handbooks are historically older. Formerly one man could master completely his whole science and keep up with all the new discoveries. In 1834 Johannes Müller wrote the whole annual report upon anatomy, comparative anatomy and physiology, and did it well. A hundred years ago a single author could write a thorough manual. To-day such a feat is impossible. The difficulty has been met with commendable success by cooperation. A science is divided into chapters; each chapter is undertaken by a specialist, and so the task is done, but with consequences easily anticipated, for every one of us knows some of these huge modern composite hand-books.

We recognize in the present methods of recording and collating scientific discoveries many adaptations which are due, it seems to me, essentially to the mere increase in the number of workers. But though the methods are modified the essential steps are the same: *first,* the record of the individual personal knowledge; *second,* the conversion of the personal knowledge by verification and collation into valid impersonal knowledge; *third,* the systematic coordination and condensation of the conclusions.

A defect—perhaps the most serious defect of our education—arises from our failure to make our students appreciate vividly the fundamental fact that science is based on personal knowledge. Our students are allowed to graduate from college, for the most part without any comprehension of this great truth. The best of them start forth with a high reverence for the library, the place of records, but quite unaware that a still higher reverence is due to those who, by being the first to observe unknown things, have founded the knowledge, the records of which the library keeps.

The divergence between philosophy and science shows itself most conspicuously in the personal mental attitude, which philosophy cherishes and science seeks to overcome. Philosophers still discuss philosophers and their systems, scientific men pursue impersonal knowledge with such ardor that they are apt to know little of the history of science.

May I venture to divert your attention to two matters, which suggest themselves in connection with our main theme? The first is the question of style in original scientific articles, for we probably all are ready to admit that the care bestowed on the presentation in print and picture of original discoveries is often insufficient. Do we not all know articles which are bungled in form and weakened by prolixity? Surely the heads of laboratories should insist by example and precept that all the workers under their influence should write clearly and briefly—for if an author fails to show respect for his own scientific work, how can he expect others to respect it? Yet there are few matters so important as intensifying the world's respect for science. For us, whose language is English, the standard should be the highest. Rivarol in his famous prize essay said "ce que n'est pas clair, n'est pas Français"—but we might say what is not true, is not English. By its wealth of synonyms and its logical construction the English language is preeminently adapted to the exact statement of scientific truth. We should not misuse so fine an instrument, which if well employed is sure to win for Anglo-Saxon science the wide influence it deserves. Good thinking is the blastema of good style, therefore our learning will never appear good if our learned articles are written badly.

The second matter for digression is a suggestion concerning bibliography. Almost every important memoir is accompanied by a bibliography. Custom prescribes it. The literature is indicated by the titles in full, and when the list is well made the volume, page and plates are all given. Other memoirs on the same subject give similar bibliographies. We know from experience that these selected bibliographies are very helpful to those who follow—but is there not a needless waste through frequent repetition? There would be a great economy if we had a complete international catalogue of the scientific literature of each year, in which all the publications of each author were entered with serial numbers. It would then suffice to quote an author's name, the year and the serial number, as, for example "John Doe, 1910, 1," to give a complete reference, for it is to be presumed that the catalogue would be found in at least all the principal scientific centers of the world. This system has been utilized privately already, and experience with it has demonstrated its eminent practicability and simplicity in use. The International Catalogue of the Royal Society, which is at present not only imperfect but excessively inconvenient and really of little use, might be transformed by the plan suggested into an invaluable aid to science. The plan could be still more easily applied to the cards of the Concilium Bibliographicum of Zürich. It is deplorable that the Royal Society neither cooperates with, nor adopts the system of, the Concilium. As matters are the International Catalogue remains merely a respectable failure.

To return: The records, which we have considered thus far are those which serve to make the discoveries of individuals available for others. As soon as the discoveries are properly collated and sufficiently verified they become permanent parts of science. Many definitions of science have been given, and did time permit it might be profitable to quote some of them—but is it not sufficient to define science as *knowledge which has acquired impersonal validity?*

We must now attempt a general examination of the records, which are used primarily to help the original investigator, though often preserved to assist his successors. The simplest form of record is the preservation of the actual specimen. Scientific museums are essentially storehouses for such records. Most of them to be sure maintain public exhibitions, which interest, stimulate and possibly instruct the public, but the precious part of their collections comprises the objects possessed, which have served for some original discovery. Scientific museums are very modern, nearly all those in America have been started within a few years. The Philadelphia Academy of Natural Sciences was founded in 1812, the Boston Society of Natural History in 1831, Agassiz's Museum in 1859, the National Museum in Washington in 1876,[4] and the Field Columbian Museum in 1893. A history of museums, dealing especially with the progress of the art of caring for collections would be cheering to read, for it would picture a remarkable growth of the appreciation of the value of objects as original records. This may be illustrated by the change of opinion as to "type" specimens of plants and animals. The systematic zoologists and botanists constantly lament that the earlier authors did not preserve the actual specimens from which they described new species and they consider no pains too great to ensure the preservation of "types" of new species, which any cotemporary worker describes. In the Laboratory of Comparative Anatomy at Harvard we have felt the influence of the example of museums and have established a permanent embryological research collection, a sign of the times and an acknowledgment of the new insistence upon the preservation of the original proofs of discoveries.

[4] The genesis of this museum dates back to Smithson's bequest, 1826, and was in part due to accumulations of materials from various government expeditions before 1876.

The progress of science is marked by the advance in the art of making research records. We all admit, in other words, that the progress of science depends partly on the perfecting of old methods, but chiefly on the invention of new ones. Despite the enormous variety in their nature and aims, all our technical methods have this in common that their real purpose is to yield us records. Our microscopes, spectroscopes, measuring instruments and many another apparatus have indeed their primary scope in rendering possible observations, which are impossible with our unaided senses. They enlarge our field of enquiry and put precision within our reach. Yet their usefulness is conditioned upon their enabling us to make records which else would remain beyond our power. On the other hand, there is a still larger class of apparatus which are obviously designed to make records. What has been said concerning apparatus might be repeated concerning methods.

It is remarkable that the vast majority of methods and apparatus are contrived to furnish a visible result. Sight has long been acknowledged by science as the supreme sense. Perhaps the philosopher was right who asserted that nothing is really known until it is presented in a visible form. We biologists can not deplore too frequently or too emphatically the great mathematical delusion by which men often of very great, if limited, ability have been misled into becoming advocates of an erroneous conception of accuracy. Although I have expressed myself on the subject before its importance justifies recurring to it. The delusion is that no science is accurate until its results can be expressed mathematically. The error comes from the assumption that mathematics can express complex relations. Unfortunately, mathematics have a very limited scope and are based upon a few extremely rudimentary experiences, which we make as very little children and of which probably no adult has any recollection. The fact that from this basis men of genius have evolved wonderful methods of dealing with numerical relations should not blind us to another fact, namely, that the observational basis of mathematics is, psychologically speaking, very minute compared with the observational basis of even a single minor branch of biology. Moreover, mathematics can at the utmost deal with only a very few factors and can not give any comprehensive expression of the complex relations with which the biologist has to deal. While, therefore, here and there the mathematical methods may aid us, we need a kind and degree of accuracy of which mathematics is absolutely incapable. For our accuracy it is necessary often to have a number of data in their correct mutual relations presented to our consciousness at the same time, and this we accomplish by the visual image, which is far more efficient for this service than any other means of which we dispose. When we wish to understand a group of complex related details, such as an anatomical structure, we must see them, and if we can not see them no accurate conception of the group can be formed. With human minds constituted as they actually are, we can not anticipate that there will ever be a mathematical expression for any organ or even a single cell, although formulæ will continue to be useful for dealing now and then with isolated details. Moreover, biologists have to do with variable relations, some of which of course can be put into mathematical form, but we find that even the simplest variations become clearer to us when presented graphically. The value to every student of science of the graphic method has been immense. Biologists can work to advantage with quantitative methods, we welcome the increasing use of measurements in biology, we welcome the English journal *Biometrika,* the organ of the measuring biologists—but none the less we refuse to accept the mathematical delusion that the goal of biology is to express its results in grams, meters and seconds. Measurements furnish us with so-called "exact" records, but the aim of science goes beyond the accumulation of exact records to the attainment of accurate knowledge, and the accuracy of our knowledge depends chiefly on what we see. The practise of science conforms to this principle, the definite affirmation of which may prove of continuing advantage.

No class of records illustrates the value of sight in science more impressively than those made by instruments for registering the time factor. The kymographion invented by Carl Ludwig is the prototype of many apparatus. In them all a succession of events, like heart beats for example, together with marks showing the time are so registered that they can be seen simultaneously and thus readily compared. If no such apparatus were available much of our most important scientific knowledge would not exist. To deprive mankind of microscopes or telescopes would be hardly a more serious blow to science. We do not of course depend on our eyes for the notion of time—for the congenitally blind perceive time—but as soon as we wish to know accurately the relation of changing events to time intervals we depend upon having them recorded in a visible form. It is the practical acknowledgment of the superiority of the eye as an agent to make clear the correlation of data.

When we refer to the history of modern medical science we begin with the anatomist Vesalius, because he reintroduced reliance on seeing in place of reliance on the reading of old authorities.

To dilate longer before this section of the American Association upon the value of seeing is superfluous. We have all been trained by dissection and by looking through the microscope, and we will not deny our training, which many of us are engaged in perpetuating.

Scientific records have a far wider scope than ordinary business records, which merely put down details that can not be carried in the memory. Science strives constantly after new ways of recording and demonstrating facts, which would otherwise be imperfectly known, or not known at all, and at the same time of eliminating the personal factor, by getting the data into a form to assist others in the work of verification.

Scientific men base their work upon a series of assumptions: first, that there is absolute truth, which includes everything we know or shall know; second, that we ourselves are included in this absolute truth; third, that objective existence is real; fourth, that our sensory perception of the objective is different from the reality. These conceptions constitute our fundamental maxims, and even when not definitely put in words they guide all sound scientific research. Metaphysicians find such maxims interestingly debatable, but science applies them unhesitatingly and is satisfied because their application succeeds. Philosophy, ever a laggard and a follower after her swifter sister, has lately and somewhat suddenly termed the scientific habit of work pragmatism and has taken up the discussion of it with delightful liveliness. Let us acknowledge the belated compliment and continue on our way.

The practical result of the four maxims has been that we further assume that all errors are of individual human origin and that there are no objective errors. We make *all* the mistakes, nature makes none. To render the pursuit of new knowledge successful our basic task is to eliminate error, or in other words to decide when we have sufficient proof. The elimination of error depends primarily upon insight into the sources of error, which, since methods of all sorts are employed, involves an intimate technical acquaintance with the methods, with just what they can show, with what they can not show and with the misleading results they may produce. In the laboratory training of a young scientific man, one chief endeavor must always be to familiarize him with the good and the bad of the special methods of his branch of science. Not until he thoroughly understands the character and extent of both the probable and the possible errors is he qualified to begin independent work. His understanding must comprise the three sources of observational error, namely, the variation of the phenomena, the imperfections of the methods and the inaccuracy of the observer. The personal equation always exists, although it can be quantitatively stated only in a small minority of cases.

The history of science at large, the history of each branch of science and the personal experience of every active investigator all equally demonstrate that the greatest source of error is in our interpretations of the observations, and this difficulty depends, it seems to me, more than upon any other one factor, upon our unconquerable tendency to let our conclusions exceed the supporting power of the evidence. Since generalization is the ultimate goal, we are too easily inveigled into assuming probabilities to be certainties, and into treating theories and even hypotheses as definite conclusions. Each generation of investigators in its turn spends much time killing off and burying older erroneous interpretations. The business is seldom accomplished by direct attack, for error perishes only in the light of truth, as microorganisms are said to perish suddenly when struck by ultra-violet rays. Owing to the load of false theories, we work like a mental chain-gang and are never unfettered. The handicap imposed by wrong hypotheses has always impeded the growth of science. Allusion to a few celebrated instances will suffice. Phlogiston long prevented chemistry from becoming the peer of other sciences. It was a notion which remained alive and dominant until Lavoisier rendered it a mere historical curiosity, by discovering the true principle of combustion. The corpuscular theory of light, upheld by Newton, long retarded physics. It was got rid of, not by proving it false, but by proving the undulatory theory true. The doctrine of the special creation and fixity of species was universally accepted, although utterly without justification. It vanished from science when the true doctrine of evolution was convincingly established. The hypothesis that great epidemics are due to diseases spread by smell, although only the bad guess of ignorance, lasted until modern bacteriology showed us the real causes of infection.

The multitude of such experiences, great and small, has gradually created among scientific men a special highly characteristic mental attitude. They regard the majority of the accumulated data and many of the inductions of science as correct. This is their estimate of the great body of information which, though personal in its origin, has been in the course of time, so tested and verified that it is looked upon as established and secure. When Asellus in 1622 discovered the lymphatics or so-called lacteals of the mesentery and demonstrated that they convey products of digestion from the intestine, his knowledge was his own, and at first his only. Since then the observations have been so repeatedly verified and of course extended that all uncertainty has vanished from our minds. Similarly in innumerable other cases reasonable impersonal certainty has been attained. Yet the investigator lives in an atmosphere of concentrated uncertainty, for he is convinced that at any time new data may turn up, and that all generalizations are likely to require modification. We might well adopt as our cry—Incredulity towards the known; open credulity towards the unknown.

We think of science as a vast series of approximations and our task is constantly to render our approximations closer to absolute truth, the existence of which we take for granted. We use our approximations as best we may, treating them in large part and at least for the time being as if they were accurately true, yet meanwhile we remain alert to better them. This has long been the standard of scientific thought. It is the pragmatic attitude of mind, but its new name has not rendered it a novelty.

The pivot of all research is adequate proof. It would certainly aid science if some competent philosopher should make a study of the practise of investigators in the various branches of science sufficient to render clear the general principles, by which investigators decide when a new observation or a new induction is sufficiently proven. If we follow the advance of research in any particular direction we soon realize that there is a more or less definite standard of proof, which, though never clearly formulated, is none the less insisted upon, so that any paper which does not come up to this standard is subject to unfavorable criticism. Two elements of this standard we know, the first the elimination of the recognized sources of error, second the repetition of the observations so that the constancy of the phenomenon is assured. We can not do more than allude to this theme, which I must leave to the future and to a more competent mind to analyze and develop.

To sum up: The method of science is not special or peculiar to it, but only a perfected application of our human resources of observation and reflection—to use the words of von Baer, the greatest embryologist. To secure reliability the method of science is *first,* to record everything with which it deals, the phenomena themselves and the inferences of the individual investigators, and to record both truly; *second,* to verify and correlate the personal knowledges until they acquire impersonal validity, which means in other words that the conclusions approximate so closely to the absolute truth that we can be safely and profitably guided by them. The method of science is no mystic process. On the contrary, it is as easily comprehended as it is infinitely difficult to use perfectly and at its best the method supplies merely available approximations to the absolute.

We set science upon the throne of imagination, but we have crowned her with modesty, for she is at once the reality of human power and the personification of human fallibility.

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THE FORMATION OF CARBOHYDRATES IN THE VEGETABLE KINGDOM[1]

THE classical discovery of Woehler in 1828 first revealed to chemists the possibility of the synthetic production of those compounds which occur naturally in the members of the animal and vegetable kingdoms. Woehler himself evidently realized the importance of his discovery. Thus, in a letter to his old teacher, Berzelius, he wrote:[2]

> You may remember how, while I was with you, when trying to make ammonia combine with cyanic acid, I always obtained a crystalline body which gave the reactions of neither the one body nor the other. I have just made this crystalline body the subject of a little investigation, preparing it by the action of ammonia on lead cyanate and have discovered it to be nothing less than urea.

Then he significantly adds, "This may be taken as an artificial production from inorganic substance."

The idea, however, that such compounds could be formed only through the agency of the vital forces of the living organism was one of such long standing and was so deeply established in the popular belief that even the chemists contemporaneous with Woehler were slow to grasp the full significance of the discovery. Berzelius himself was evidently not convinced, since in his text-book published in 1837, nine years after Woehler's discovery, he expressed doubt as to the possibility of being able to discover the differences between the causes of reactions in the living organism and those in the inorganic realm. Likewise Gerhardt[3] wrote seven years later (1842) as follows: "I have shown that the chemist works in a way altogether opposite from living nature. The one burns, destroys, operates by analysis. Vital force alone operates by synthesis and reconstructs the edifice torn down by chemical forces."

Other discoveries, however, of a nature

[1] Address of the vice-president and chairman of Section C—Chemistry—at the Minneapolis meeting of the American Association for the Advancement of Science.

[2] "Berzelius-Woehler Briefwechsel," I., p. 206; Armitage, "A History of Chemistry," p. 143.

[3] *Compt. rend.*, 15, p. 498. Bunge, "Text-book of Organic Chemistry," p. 1.