THE METHODS OF RESEARCH[1]

I

BACK TO ARISTOTLE

SOME fifteen years or more ago I wrote to T. W. Richards at Harvard commenting on something that he had either done or left undone—it does not matter now which. He answered, almost by return mail: "There are nine and sixty ways of writing tribal lays, and every single one of them is right." That ended the discussion for the time being; but I have been thinking over the matter ever since. The great American public is firmly convinced that there can only be fifty-seven varieties of pickles, and pickle-making is a much more highly developed industry than that of writing tribal lays. We only need seven crystal systems in which to classify the innumerable crystal forms which are known. Robert Louis Stevenson says that there are only three ways of writing short stories, and some Frenchman, whose name I do not recall, has said that there are only three plots. Things are much simpler than one usually assumes. I am defending the thesis that there are only two methods of research and that one of these is bad. Unfortunately, most of us preach the false doctrine to our students.

The first of the two general methods of research is the

[1] A course of three lectures delivered by Professor Wilder D. Bancroft, Ph.D., D.Sc., of Cornell University, in the Chemistry Lecture Hall of the Rice Institute, April 9, 10, and 11, 1928.

deductive or Aristotelian method in which one first gets one's working hypothesis and then accumulates data to test it. The second general method is the inductive or Baconian method in which one accumulates data until the general theory underlying them becomes obvious.

Aristotle[1] says that "we acquire knowledge either by Induction or by Demonstration: and Demonstration is from universals, but Induction from particulars. It is impossible to have universal theoretical propositions except by Induction: and we cannot make inductions without sensations; for sensations have to do with particulars."

Bacon[2] is a bit more definite.

"There are and can be but two ways of investigating and discovering Truth. The one leaps from the senses and particulars to the most general axioms, and from these as first principles, and their unshaken truth, judges on and discovers medial axioms: and this way is in vogue. The other raises axioms from the senses and particulars, by ascending steadily, step by step, so that at last the most general may be reached; and this way is the true one but untried."

"Our method of discovering the Sciences is one which leaves not much to acumen and strength of wit, but nearly levels all wits and intellectuals."

"We must say of ourselves what one said jesting—especially as it hits the matter so well—that water drinkers and wine drinkers cannot think alike. For all men, hitherto, ancient or modern, have in learning drunk but a crude liquor like water, which has either flowed of its own accord from the Intellect, or has been drawn up as by wheels of a well, by means of Logic. But we drink and pledge others with a liquor made of many well-ripened and mature grapes, gath-

[1] Whewell: "Philosophy of Discovery," 19 (1860).

[2] "Novum Organum" (translated by G. W. Kitchin), 14, 32, 103 (1855).

ered and plucked from particular clusters; then pressed in the wine-press; and lastly cleansed and clarified in a vessel. And so no marvel that we and others do not agree."

Bacon's presentation of his case would certainly be ill-advised, and perhaps illegal, in these United States today. It is quite clear, however, that he believes that the general relations should be so obvious from the experiments, that even those of little wit will recognize the truth and necessity of the conclusions. Consequently, a general truth cannot have been reached by the Baconian method if it strikes a large number of people interested in the subject as being absurd and foolish. E. O. von Lippmann[1] says that if a conclusion is not obvious to other scientific men when stated it cannot have been reached by the Baconian method. The converse is of course not true, that a discovery must have been made by the Baconian method if it is accepted generally in a very short time. The way in which the discovery was presented to the world may have been especially good.

The objection is sometimes raised that one cannot make a working hypothesis without some facts on which to base it. This is true enough; but is not really relevant. One might discuss at length and unprofitably which came first, the hen or the egg. One must use common-sense even when doing research work. Of course one makes use of all the knowledge one has in formulating a working hypothesis. If there are no facts available, some must be got; but the difference between the two types of men is that one set gets its working hypothesis early in the game and the other late. It is probable that van't Hoff would never have deduced the theory of solutions from his inner consciousness; but it is surprising how few facts were necessary.

I do not sympathize at all with the point of view expressed

[1] "Zur Geschichte der Naturwissenschaften," 505 (1906).

by Henry Fairfield Osborn.[1] Fifty years as a teacher have afforded an original retrospect and prospect of the art of education, an art which has an unchanging element in the vicissitudes of our environment which we call civilization. Throughout this long half-century period I have been consistent with my oft-repeated advice to my students, namely, to think a subject out for oneself and then to read what others have thought about it. I have myself given an immense amount of personal thought to the methods of intellectual education, and I am free to confess that I have depended very little on reading of the works even of the great masters and innovators to whom I refer from time to time in this volume. My rule with myself and with my students has been, first, to try to master a subject or to thoroughly understand it; second to try to add something of my own to this subject—that is, to produce or create something new."

I have known men who made a point of not looking up what had been done before starting on a problem because they were afraid of getting into a rut and seeing things as their predecessors saw them. There is this danger, especially if one stands a little in awe of the printed page; but the remedy is to learn to read critically. The other way one is sure to waste a large amount of time in repeating the work of others. I have found it helpful to see what the flaws were in the reasoning and to try to think out an explanation which can be distorted into each one of the suggested hypotheses. What one should then do is to supplement the other man's work, with the smallest number of experiments needed to carry conviction. There is nothing that I delight in more than a problem in which ninety per cent of the work has already been done.

[1] Natural History, *27,* 309 (1927).

The difficulty with classifying research men as Baconians and Aristotelians is that these names have other connotations which confuse the issue. While the Greek method of attack was sound in principle, the Greeks were distinctly bad about doing the experimental work necessary to verify their working hypotheses. On the other hand Bacon did lay great emphasis on the importance of making experiments and one is apt to find that this is what the other man is talking about instead of about Bacon's research methods.

The words inductive and deductive would be useful if people agreed as to what they mean. Most of us would class Bacon as the exponent of inductive reasoning and Aristotle as the exponent of deductive reasoning; but Mellor[1] reverses this and says that the deductive method was favored by Francis Bacon and the inductive method by Isaac Newton. Mellor goes so far as to say that "the method of Aristotle was rediscovered and restated by Francis Bacon in his Novum Organum," a statement which would have surprised Bacon a good deal if he could have heard it.

It seems wiser therefore to invent entirely new terms which shall have only the connotations which I choose to give them. I am going to say that those who accumulate facts are accumulators and those who start early with working hypotheses are guessers. Even this apparently simple statement needs some explanation. I am using the word guess as it is used in New England and not as it is used in Central New York. I do not mean the result of pure chance, such as flipping a coin or deciding whether to finesse on the right or the left when the bidding has shown nothing. I mean by guessing making the shrewdest judgment one can from facts which are not entirely adequate. Guess is the

[1] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1*, 17 (1922).

word used by Newton in the same sense, so it has been in good use for several centuries.

It is still rather the fashion for people to have slogans and the slogan of the accumulators is: "First get your facts." I have heard that said to unfortunate students time and again. The slogan of the guessers is: "First get your working hypotheses." I use the plural advisedly because it is always better to have several working hypotheses if possible.

People do and justify what interests them. Accumulators must develop manipulative skill, and experiment becomes to them only too often an end in itself. Guessers are interested primarily in theory and do not care much about experiments *per se*. Consequently, they are often accused of not being able to do experiments. As a rule, that does not worry them. Sir Humphry Davy[1] said: "I thank God that I was not made a dexterous manipulator, for the most important of my discoveries have been suggested to me by failures." Sir William Hamilton,[2] the inventor of quaternions, said: "In physical sciences the discovery of new facts is open to every blockhead with patience, manual dexterity, and acute senses." Perhaps it would be fairer to quote now from Lord Kelvin.[3] "Accurate and minute measurement seems to the non-scientific imagination a less lofty and dignified work than looking for something new. Yet nearly all the grandest discoveries of science have been but the rewards of accurate measurement and patient, long-continued labour in the minute sifting of numerical results."

The distinction between the accumulator and the guesser has been put very tactfully by A. V. Hill when speaking of Martha and Mary in Science. "Martha represents the

[1] Mellor: "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1*, 569 (1922).

[2] "Discussions on Philosophy and Literature," 239 (1852).

[3] Gregory: "Discovery," 60 (1923).

scientist always busy in his laboratory collecting invaluable data. Mary takes the broad view, reflects and discusses, and listens to discussion. Science needs them both."

Bosanquet[1] has said the same thing in truly orthodox language. "In the gradual discrimination of the inquiries initiated confusedly by the omnivorous appetite for knowledge, two classes of truth come to be plainly distinguished—statements of particular facts, and statements of general connections. These constitute the branches of knowledge which fulfil less or more completely the ideal of science."

It will simplify discussion if I give a partial classification of some scientific men. Among the guessers, I put Galileo, Harvey, Kepler, Newton, Huygens, Young, Lavoisier, Dalton, Davy, Avogadro, Gauss, Franklin, Faraday, Joule, Darwin, Mendel, Pasteur, Lister, Helmholtz, Gibbs, van't Hoff, Arrhenius, J. J. Thomson, Rutherford, Einstein, and Bohr. Among the accumulators I place Tyco Brahe, Leeuwenhoek, Priestley, Stas, Becquerel, Kohlrausch, Newcomb, Raoult, Röntgen, Dewar, Fabre, Richards, Sabatier, Mme. Curie, and Millikan.

People will not be unanimous over this grouping, so it will be well to say a few words about some of the men, so as to justify the classification.

There can be no question about Galileo. His conclusion as to the motion of the earth was disputed by nearly everybody at the time, and even his experiment on falling bodies at the tower of Pisa did not convince many of his audience, conclusive though it may now seem to us.[2]

"Members of the University of Pisa, and other onlookers, are assembled in the space at the foot of the wonderful leaning tower of white marble in that city in the year 1591. A

[1] "Science and Philosophy," 23 (1927).

[2] Gregory: "Discovery," 2 (1923).

young professor climbs the spiral staircase until he reaches the gallery surmounting the seventh tier of arches. The people below watch him as he balances two balls on the edge of the gallery, one weighing a hundred times more than the other. The balls are released at the same instant and are seen to keep together as they fall through the air until they are heard to strike the ground at the same moment. Nature has spoken with no uncertain sound, and has given an immediate answer to a question debated for two thousand years. 'This meddlesome man Galileo must be suppressed,' murmured the University fathers as they left the square. 'Does he think that by showing us that a heavy and a light ball fall to the ground together he can shake our belief in the philosophy which teaches that a ball weighing one hundred pounds will fall one hundred times as fast as one weighing a single pound. Such disregard of authority is dangerous and we will see that it goes no further.' So they returned to their books to explain away the evidence of their senses; and they hated the man who had disturbed their philosophic serenity."

In connection with this I wish to quote the delicious argument[1] from James Branch Cabell's "Something about Eve." Most of us do much the same thing to some extent.

"Ah, but I must tell you," said Tenjo, seeming yet more troubled, "that the man who looks into that mirror straightway finds himself transformed into two stones. For this reason it is hidden away in Peter's Tomb, and it is kept veiled, and of course no man has ever dared go near it."

"How, then, did this mirror ever manage to change anybody into two stones if nobody ever dared go near it?"

"Why, but the mirror was compelled to change them into two stones because that was the law. It was not at all the

[1] p. 116.

mirror's fault. . . . And the people kept away from the mirror because they knew about this law. Surely, that too was natural?"

"In a way, yes. But how could they be certain about this law?"

"How could they help it, how could anybody be ignorant of one of our very oldest and most famous laws, which comes down to us, indeed, from sources so august and venerable that they antedate all history?"

"Why, then, who enacted this law?"

"How should I know, when, as I was just telling you, this law is older than any recorded history? . . ."

"Yet, do you but answer me this very simple question? What if some intelligent, unsuperstitious person were to look in this mirror,—and were to come back not changed into stone, and not hurt in any way,—would that not prove to you the insanity of this law?"

"Of course it would not! That would only prove the man was a liar. The plain fact of his not being changed into two stones would be legal proof in any of our courts or in any law-respecting place anywhere that he had not ever looked into the Mirror of the two Truths."

Gregory[1] considers that "Harvey's work was an excellent example of the application of the inductive method of study laid down by Francis Bacon as the essential principle of scientific principle; but Harvey did not begin to teach the circulation of the blood until 1619, and as Bacon died seven years later he may be forgiven the omission of any reference to it in his writings, though he must have known of it."

Gregory is confusing, as so many people do, Bacon's emphasis on the importance of experiment and Bacon's method of inductive reasoning. We know that Harvey would have

[1] "Discovery," 139 (1923).

objected to his work being classed as an excellent example of Bacon's inductive method, because Harvey himself said of Bacon that "he writes philosophy like a Lord Chancellor."

In Harvey's own account of his work,[1] he gives "a description of the heart as seen in a living animal when the chest has been laid open and the pericardium removed. Three circumstances are noted:—

(*a*) The heart becomes erect, strikes the chest, and gives a beat;

(*b*) It is constricted in every direction;

(*c*) Grasped by the hand, it is felt to become harder during the contraction.

"From these circumstances it is inferred:—

(1) That the action of the heart is essentially of the same nature as that of voluntary muscles, which become hard and condensed when they act;

(2) That, as the effect of this, the capacity of the cavities is diminished, and the blood is expelled;

(3) That the intrinsic motion of the heart is the systole, and not the diastole, as previously imagined.

"The motions of the arteries are next shown to be dependent upon the action of the heart, because the arteries are distended by the wave of blood that is thrown into them, being filled like sacs or bladders, and not expanding like bellows. These conclusions are confirmed by the jerking way in which blood flows from a cut artery.

"In the heart itself two distinct motions are observed—first of the auricles and then of the ventricles. These alter-

[1] McRae: "Fathers of Biology," 88 (1910).

nate contractions and dilatations can have but one result, namely, to force the blood from the auricle to the ventricle, and from the ventricle, on the right side, by the pulmonary artery to the lungs, and on the left side by the aorta to the system."

These considerations suggest to the mind of Harvey the idea of the circulation. "I begin to think," he says, "whether there might not be a motion, as it were, in a circle." This is next established by proving the three following propositions:—

(1) The blood is incessantly transmitted by the action of the heart from the vena cava to the arteries in such quantity that it cannot be supplied from the ingesta, and in such wise that the whole mass must very quickly pass through the organ;

(2) The blood, under the influence of the arterial pulse, enters, and is impelled in a continuous, equable, and incessant stream through every part and member of the body, in much larger quantity than were sufficient for nutrition, or than the whole mass of fluids could supply;

(3) The veins in like manner return this blood incessantly to the heart from all parts and members of the body.

While there is no time-scale to these paragraphs, it is quite evident that Harvey started generalizing as soon as possible. He lectured on the subject for nine years before he published his views, which were received by most of his contemporaries with doubt and with scorn. Gregory[1] himself admits that Harvey did not and could not make out the whole course of the blood.

[1] "Discovery," 138 (1923).

"Harvey established absolutely the fact of the circulation of the blood, and the fact that the muscular action of the heart causes this movement. But he was unable, from his want of a microscope, to indicate the precise path along which the blood travels from the terminal arteries to the commencing veins. The large artery from the heart gives off branches to various parts of the body, and these branch off again into small arteries in different organs. Similarly, small veins carrying blood back to the heart unite to form large veins. How the blood passed from the small arteries to the small veins could only be conjectured by Harvey, and was not discovered until three years after his death. He concluded that the blood passes from the arteries to the veins mainly by percolation, as water, to use his own illustration, percolates the earth and produces springs and rivulets. No microscope in his time was powerful enough to enable him to see the meshwork of very minute tubes—the capillaries—which can now be observed easily."

Gregory[1] says in regard to Kepler: "In the process of discovery of the three fundamental laws known by his name, Kepler was led to make many fantastic hypotheses. But all through he was guided by the principle that God who made the world had established fixed laws throughout his works, laws that are often so definite as to be capable of expression in exact numerical terms. In accordance with these views he sought for numerical relations in the disposition of the planets and their arrangement, in respect to their number, their times of revolution, and their distances from one another. Each hypothesis he made, however fanciful, he tried by a vigorous test whenever possible, and, as soon as he found that the facts were not in accordance therewith, he abandoned it, and without hesitation proceeded to try

[1] "Discovery," 142 (1923).

others, which he submitted to the same severe ordeal, to share perhaps the same fate. He says, 'After many failures, I was comforted by observing that the motions in every case seemed to be connected with the distances; and that when there was a great gap between the orbits there was the same between the motions.' He was at length led to the discovery of his well-known 'Harmonic' law that the squares of the periodic times of revolution of the planets are as the cubes of their mean distances from the Sun."

Some people have claimed that Newton was not a guesser because he said that he did not make hypotheses; but all that this means is that he did not make speculative hypotheses which could not be tested. In a talk before the Research Club at Cornell, H. A. Lorentz said that Newton's questions were really hypotheses. E. O. von Lippmann[1] is equally definite. "While Newton said 'I do not make hypotheses,' he made them nevertheless in almost all his work. Nothing else could be expected, because, without hypotheses, one cannot formulate the laws which experiment confirms by questioning nature, in other words no induction. Not to have seen this is one of the greatest errors with which one must reproach Bacon."

Woodruff[2] says that "the particular form which the Newtonian method takes in science is to devise provisional generalisations called hypotheses or *working hypotheses* to explain facts and phenomena. The appeal is then made to observation and experiment in order to test the validity of the proposed generalisation."

Mellor[3] quotes Newton himself as saying that "no great discovery was ever made without a bold guess."

[1] "Zur Geschichte der Naturwissenschaften," 423 (1906).

[2] "Development of the Sciences," 134 (1923).

[3] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1*, 18 (1922).

The opposition of Newton was sufficient to keep the undulatory theory of light in the background for many years, so that it is quite evident that Huygens did not develop his theory by following the Baconian method. He was a guesser.[1] "Although Robert Hooke in 1668 and Ignace Pardies in 1672 had adopted a vibratory hypothesis of light, the conception was a mere floating possibility until Huygens provided it with a sure foundation. His powerful scientific imagination enables him to realize that all the points of a wave-front originate partial waves, the aggregate effect of which is to reconstitute the primary disturbances at the subsequent stages of its advance, thus accomplishing its propagation; so that each primary undulation is the envelope of an indefinite number of secondary undulations. This resolution of the original wave is the well-known 'Principle ot Huygens,' and by its means he was enabled to prove the fundamental laws of optics, and to assign the correct construction for the direction of the extraordinary ray in uniaxial crystals. These investigations, together with his discovery of the 'wonderful phenomenon' of polarisation, are recorded in his 'Traité de la lumière,' published at Leiden in 1690, but composed in 1678."

The work done by the "powerful scientific imagination" is of course what Newton meant by a bold guess. According to Gregory[2] the conclusive test of the undulatory theory was not given until 1850, nearly two centuries after the theory was developed.

"Newton supported with the weight of his great authority the theory that light is due to the emission of minute particles, at a high velocity by a luminous body. When these minute particles (corpuscles) impinge upon the retina of the

[1] "Encyclopædia Britannica," *14,* 21 (1910).

[2] "Discovery," 158 (1923).

eye, they produce the sensation of light. According to this view, light ought to travel more quickly in water than in air. Another theory, put forward by Huygens about Newton's time, and developed later, is that light is due to vibration in an imaginary medium called the ether, believed to prevail throughout all space. Luminous bodies set the ether in vibration, and when these undulations reach the eye they give us the sensation of light. On this view, light should travel more slowly in a substance like water than it does in air.

"To test the two theories, therefore, an experiment was required by which the relative velocities of light in air and water could be determined. By Newton's emission theory the velocity should be greater in water than in air; while it should be less according to Huygens' undulatory theory. Not until the middle of the nineteenth century was a means found of determining experimentally the velocities of light in water and air in a laboratory. The crucial experiment which would decide which theory was true was performed by a French physicist, Jean Léon Foucault, in 1850; and it showed that light, which travels at the rate of about 186,000 miles a second in air, travels throughout water at about three-quarters that velocity. The result of this experiment disposed finally of the emission theory, and re-established Huygens' theory that light is due to very rapid vibrations in a hypothetical ether pervading the universe."

The very next paragraph in Gregory's book establishes the case for Young as a guesser. "Fifty years before the crucial determination was made of the velocities of light in water and air, a genius of the first magnitude—Dr. Thomas Young—had shown that red light is produced by nearly 32,000 ether-waves to the inch, and that the number of such waves in a given length increases progressively in passing from red to violet, until at this end of the colour scale there are about 60,000 undulations to the inch. Young also proved that certain optical effects could be explained only by the principle of interference of ether-waves with one another; but his researches and interpretations, involving, as they did, the existence of an imponderable ether which, to use his words 'pervades the substance of all material bodies, with little or no resistance, as freely, perhaps, as the wind passes through a grove of trees,' met with ridicule from leaders in the literary world, and were not given serious attention by his scientific contemporaries. When, in 1815, a French investigator, Augustin Jean Fresnel, began experimental work in optics, and was also led to the discovery of interference in light, he knew nothing of the previous work done by Young in the same direction thirteen years earlier. The work of these two investigators revived the undulatory theory and opened a question which may be said not to have been settled decisively until Foucault's crucial experiment had been made."

Lavoisier[1] overthrew the phlogiston theory in the only way that a theory can be overthrown, by introducing a better theory, the oxygen theory. His attack on phlogiston was very severe. "Chemists have turned phlogiston into a vague principle which consequently adapts itself to all the explanations for which it may be required. Sometimes this principle has weight and sometimes it has not; sometimes it is free fire and sometimes it is fire combined with the earthy element; sometimes it passes through the pores of vessels, sometimes these are impervious to it: it explains both causticity and non-causticity; transparency and opacity, colours and their absence. It is a veritable Proteus, changing its form at each instant."

Priestley and Scheele, the two discoverers of oxygen, were

[1] Campbell Brown: "A History of Chemistry," 248 (1920).

phlogistonists.[1] "Priestley named the gas which would not burn, but which removed the combustible property from combustibles, and which was so far from phlogiston that it was purer than common air—'*dephlogisticated air.*' The name showed that he was even yet a phlogistonist, and in fact he remained so to the end of his life, although his own experiments had destroyed the theory. . . . Scheele argued that if the effect of combustion between phlogiston and air is to cause contraction, the remaining air ought to be heavier. On weighing it he found it to be *slightly lighter*. Hence, he inferred that part of the air must have disappeared, and that common air must consist of two gases, one of which has the property of uniting with phlogiston. In order to find out what had become of the air which had disappeared, he heated many metals, phosphorus, and other substances in air, and found that they behave in the same way as the iron and the sulphide. His inference was that the compound formed by the union of phlogiston with one of the constituents of air is the heat, or the fire, which escaped through the glass. He fancied he had proved the decomposition of the substance heat into *phlogiston and fire-air!* He decomposed heat chemically, he thought, by heating manganese dioxide with sulphuric acid, and again by heating the red calx of mercury. The calx combined with the phlogiston of the heat, and the other constituent of heat was fire-air, which he collected. This was oxygen."

Although Lavoisier was not the discoverer of oxygen, it was he who developed the oxygen theory of combustion. As these quotations have shown, the oxygen theory was far from obvious and met with considerable opposition. Consequently we must class Lavoisier as a guesser and not as a Baconian.

[1] Ibid., 268.

A quotation from Perrin[1] kills two birds, clearing up the case of Dalton, the chemist, and that of Boltzmann, the mathematical physicist. "To divine in this way the existence and properties of objects that still lie outside our ken, *to explain* the complications of the visible in terms of invisible simplicity, is the function of the intuitive intelligence which, thanks to men such as Dalton and Boltzmann, have given us the doctrine of atoms." Perrin does not lay special emphasis on the word "intuitive;" but it is the important one for us. Laue's discovery of the action of X-rays on crystals was certainly a triumph of intuitive reasoning, a bold guess, because there were absolutely no experiments along this line. Laue was searching for a phenomenon which nobody had ever seen; but which had been deduced from the fact that X-rays were short waves. No one can dispute the classification of Benjamin Franklin as a guesser.

In 1811 Avogadro published what is now known as his law, that equal volumes of gases at the same temperature and pressure contain equal numbers of molecules. This law is nowadays given to beginners in chemistry very early in the course; but it was neither understood nor appreciated by chemists until after the reform of Cannizzaro in 1858. We must therefore classify Avogadro as a guesser since his generalization does not conform in any respect to the criteria laid down by Bacon.

We are not in the habit of looking upon mathematicians as guessers; but that is apparently because most of us do not happen to know how the mind of a mathematician works. Huygens published his theorems on centrifugal force without proof. Gauss said: "I have the result, only I do not yet know how to get to it." Einstein said that "the really valuable factor is intuition." In a lecture at Cornell, Millikan

[1] "Atoms," VII (1923).

said that "Einstein's $h\nu$ formula was originally a mathematical hunch." Sommerfeld states that "the Bohr theory is not yet fully worked out mathematically; but is conceived intuitively." Gibbs was, of course, entirely deductive. Fermat's theorem was not proved.

I have been told several times that Faraday's discovery of the laws of electrolysis was a triumph for the Baconian method. This is based on an absolute misconception of what Faraday actually did. From experiments with a friction machine charging a battery of Leyden jars which were then discharged through a galvanometer, Faraday concluded that the deflecting force of an electric current is probably directly proportional to the absolute quantity of electricity passed, at whatever intensity that electricity may be. Faraday then proved that the deflection produced when the jars were charged with thirty revolutions of the friction machine was the same as that produced by specified wires of zinc and platinum a specified distance apart if immersed five-eighths of an inch in a specified sulphuric acid for eight beats of his watch—about 3.2 seconds. This work was reported[1] in January, 1833.

"373. The following arrangements and results are selected from many that were made and obtained relative to chemical action. A platina wire one-twelfth of an inch in diameter, weighing two hundred and sixty grains, had the extremity rendered plain so as to offer a definite surface equal to a circle of the same diameter as the wire; it was then connected in turn with the conductor of the machine, or with the voltaic apparatus (369), so as always to form the positive pole, and at the same time retain a perpendicular position, that it might rest with its whole weight, upon the test paper to be employed. The test paper itself was supported

[1] Faraday: "Experimental Researches in Electricity," *1*, 103 (1839).

upon a platina spatula, connected either with a discharging train (292), or with the negative wire of the voltaic apparatus, and it consisted of four thicknesses, moistened at all times to an equal degree in a standard solution of hydriodate of potassa (316).

"374. When the platina wire was connected with the prime conductor of the machine, and the spatula with the discharging train, ten turns of the machine had such decomposing power as to produce a pale round spot of iodine of the diameter of the wire; twenty turns made a much darker mark, and thirty turns made a dark brown spot penetrating to the second thickness of the paper. The difference in effect produced by two or three turns, more or less, could be distinguished with facility.

"375. The wire and the spatula were then connected with the voltaic apparatus (369) the galvanometer also being included in the arrangement, and, a stronger acid having been prepared, consisting of nitric acid and water, the voltaic apparatus was immersed so far as to give a permanent deflecttion of the needle to the 5 1/3 division (372), the fourfold moistened paper[1] intervening as before. Then by shifting the end of the wire from place to place upon the test paper, the effect of the current for five, six, seven, or any number of beats of the watch (369) was observed and compared with that of the machine. After alternating and repeating the experiments of comparison many times, it was constantly found that this standard current of voltaic electricity, continued for eight beats of the watch, was equal in chemical effect to thirty turns of the machine; twenty-eight revolutions of the machine were sensibly too few.

"376. Hence it results that both in *magnetic deflection*

[1] Of course the heightened power of the voltaic battery was necessary to compensate for the bad conductor now interposed.

(371) and in *chemical force,* the current of electricity of the standard voltaic battery for eight beats of the watch was equal to that of the machine evolved by thirty revolutions.

"377. It also follows that for this case of electro-chemical decomposition, and it is probable for all cases that the *chemical power, like the magnetic force* (366) *is in direct proportion to the absolute quantity of electricity* which passes."

Faraday's conclusion was right; but it does not follow necessarily from the experiments as described. He had proved, for this one case and with an experimental error of perhaps five per cent, that there was a definite relation between the quantity of electricity and the amount of chemical decomposition; but that would be equally true if the amount of chemical decomposition were proportional to some power of the quantity of electricity. The relation postulated by Faraday is the simplest one. It is possible that he overlooked the other relations; but I believe that he had guessed the result and that he was only concerned with proving the identity in chemical action of the electricity from the two sources.

Tyndall[1] lays stress continually on the intuitive and imaginative nature of Faraday's thinking. "Faraday has been called a purely inductive philosopher. A great deal of nonsense is, I fear, uttered in this land of England about induction and deduction. Some profess to befriend the one, some the other, while the real vocation of an investigator, like Faraday, consists in the incessant marriage of both. He was at this time full of the theory of Ampère, and it cannot be doubted that numbers of his experiments were executed merely to test his deductions from that theory."

"Faraday saw mentally the rotating disk, under the opera-

[1] "Faraday as a Discoverer," 23, 26, 28, 44, 73, 106, 121 (1868).

tion of the magnet, flooded with his induced currents, and from the known laws of interaction between current and magnets he hoped to deduce the motion observed by Arago. That hope he realised, showing by actual experiment that, when his disk rotated, currents passed through it, their position and direction being such as must, in accordance with the established laws of electro-magnetic action, produce the observed rotation."

"At the suggestion of a mind fruitful in suggestions of a profound and philosophical character—I mean that of Sir John Herschel—Mr. Barlow, of Woolwich, had experimented with a rotating iron shell. Mr. Christie had also performed an elaborate series of experiments on a rotating iron disk. Both of them had found that when in rotation the body exercised a peculiar action upon the magnetic needle, which was not observed during quiescence; but neither of them was aware at the time of the agent which produced this extraordinary deflection. They ascribed it to some change in the magnetism of the iron shell and disk.

"But Faraday at once saw that his induced current must come into play here, and he immediately obtained them from an iron disk. With a hollow brass ball, moreover, he produced the effects obtained by Mr. Barlow. Iron was in no way necessary: the only condition of success being that the rotating body should be of a character to admit of the formation of currents in its substance: it must, in other words, be a conductor of electricity. The higher the conducting power the more copious were the currents. He now passes from his little brass globe to the globe of the earth. He plays like a magician with the earth's magnetism. He sees the invisible lines along which its magnetic action is exerted, and sweeping his wand across these lines evokes this new power.

Placing a simple loop of wire round a magnetic needle he bends its upper portion to the west: the north pole of the needle immediately swerves to the east; he bends his loop to the east, and the north pole moves to the west. Suspending a common bar magnet in a vertical position, he causes it to spin round its own axis. Its pole being connected with one end of a galvanometer wire, and its equator with the other end, electricity rushes round the galvanometer from the rotating magnet. He remarks upon the '*singular independence*' of the magnetism and the body of the magnet which carries it. The steel behaves as if it were isolated from its own magnetism. And then his thoughts suddenly widen, and he asks himself whether the rotating earth does not generate induced currents as it turns round on its own axis from west to east."

"His mind rises from the minute to the vast, expanding involuntarily from the smallest laboratory fact till it embraces the largest and grandest natural phenomena. In reality, however, he is at this time only clearing his way, and he continues laboriously to clear it for some time afterwards. He is digging the shaft, guided by that instinct towards the mineral lode which was to him a rod of divination."

"In the researches now under review the ratio of speculation and reasoning to experiment is far higher than in any of Faraday's previous works. Amid much that is entangled and dark we have flashes of wondrous insight and utterances which seem less the product of reasoning than of revelation."

"With that admirable instinct which always guided him, Faraday had seen that it was possible, if not probable, that the diamagnetic force acts with different degrees of intensity in different directions through the mass of a crystal. In his studies on electricity, he had sought an experimental reply to the question whether crystalline bodies had not different specific inductive capacities in different directions, but he failed to establish any difference of the kind. His first attempt to establish differences of diamagnetic action in different directions through bismuth, was also a failure; but he must have felt this to be a point of cardinal importance, for he returned to the subject in 1850, and proved that bismuth was repelled with different degrees of force in different directions."

"The manner in which Faraday himself habitually deals with his hypotheses is revealed in this lecture. He incessantly employed them to gain experimental ends, but he incessantly took them down, as an architect removes the scaffolding when the edifice is complete. 'I cannot but doubt,' says he, 'that he who as a mere philosopher has most power of penetrating the secrets of nature, *and guessing by hypothesis* at her mode of working, will also be most careful for his own safe progress and that of others, to distinguish the knowledge which consists of assumption, by which I mean theory and hypothesis, from that which is the knowledge of facts and laws.' Faraday himself, in fact, was always 'guessing by hypothesis,' and making theoretic divination the stepping-stone to his experimental results."

One knows of Joule as the man who determined the mechanical equivalent of heat and there is consequently an expectation of Joule turning out to be an accumulator. This is quite untrue. Joule did much more than determine the mechanical equivalent of heat. He had to convince people that there was such a thing to determine.[1] "To the present student endeavouring to enter fully into the history of the discovery, difficulties are presented by his own familiarity with expressions and terms, now part of his language,

[1] Osborne Reynolds: "Memoir of James Prescott Joule," 17, 75, 109 (1892).

but which came into existence during the discovery, by his familiarity with those facts the discovery of which resulted in the generalization, and by his familiarity with facts almost innumerable, previously unknown, but which have been revealed as a consequence of the generalization.

"The terms 'energy' and 'work' did not exist in the language of science in their present significance. The *vis viva* of a body, the product of the square of its velocity multiplied by its mass, had since the time of Newton been recognized as a mechanical quantity, and the term 'energy' had been applied to half this quantity by Young. On the other hand, 'work'—motion against resistance—expressed as the product of the distance, multiplied by the mean resistance overcome, although it was known to express the half of the change in the *vis viva* which takes place in a body moving against resistance, had never been recognized in the schools of mechanical philosophy as a fundamental measure of mechanical action, either as 'work' or by any other name."

"The fact that the early papers of Joule were, at the time, apparently ignored by the many eminent physicists then living, though apt to inspire the present reader with a feeling of astonishment, if not of indignation, at the generation for their prejudice and neglect, was, in truth, the highest tribute that could be paid to the greatness of the advance in philosophy which he had made."

"Joule's paper at the Oxford meeting made a great sensation. Faraday was there and was much struck with it, but did not enter fully into the new views. It was many years after that, before any of the scientific chiefs began to give their adhesion. It was not long after when Stokes told me [William Thomson] he was inclined to be a Joulite. Miller or Graham, or both, were for many years quite incredulous as to Joule's results, because they all depended on fractions of a degree of temperature—sometimes very small fractions. His boldness in making such large conclusions from such very small observational effects, is almost as noteworthy and admirable as his skill in extorting accuracy from them. I remember distinctly at the Royal Society, I think it was either Graham or Miller saying simply he did not believe Joule because he had nothing but hundredths of a degree to prove his case by."

Joule was the first to give the now accepted explanation of shooting stars—that they are meteorites rendered hot by the friction they meet on encountering the atmosphere. Joule was led to this explanation by his experiments on the heat developed by the friction of fluids. This was certainly a case of getting one's working hypothesis early in the game and this would not have appealed to Bacon at all.

Some people class Darwin as an accumulator because he spent so many years collecting data before he published anything; but it is the spirit in which the data are collected which counts and not the time spent in collecting them. A more serious difficulty is to be found in a statement by Darwin himself. "By collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject. My first note-book was opened in July, 1837. I worked on true Baconian principles, and without any theory, collected facts on a wholesale scale, more especially with respect to domesticated productions, by printed inquiries, by conversation with skilful breeders and gardeners, and by extensive reading."

This would seem to be conclusive; but it contradicts other statements by Darwin and I believe that it is actually misleading. In the beginning there were no facts available and all that Darwin means to say is that he did not try to make a theory until he had collected a number of facts, which is perfectly proper. We get a more accurate conception of the way in which Darwin worked from Poulton's article in the Encyclopædia Britannica.[1]

"Soon after opening his note-book in July 1837, he began to collect facts bearing upon the formation of the breeds of domestic animals and plants and quickly saw 'that selection was the keystone of man's success. But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me.' Various ideas as to the causes of evolution occurred to him only to be successively abandoned. He had the idea of 'laws of change' which affected species and finally led to their extinction, to some extent analogous to the causes which bring about the development, maturity, and finally death of an individual. He also had the conception that species must give rise to other species or else die out, just as an individual dies unrepresented if it bears no offspring. These and other ideas, of which traces exist in his Diary, arose in his mind, together with perhaps some general conception of natural selection, during the fifteen months after the opening of his notebook.

In October, 1838, he read *Malthus on Population,* and his observations having long since convinced him of the struggle for existence, it at once struck him 'that under these circumstances favourable variations would tend to be preserved, and unfavourable ones to be destroyed. The result of this would be the formation of new species. Here then I had a theory by which to work.' In June 1842 he wrote out a sketch which two years later he expanded to an essay occupying 231 pages folio."

[1] *7,* 841 (1910).

In other words Darwin collected facts for fifteen months, turning over in his mind possible explanations for them. Then he found "a theory by which to work," and he proceeded to test it for twenty years more, when he was forced into what he considered premature publication. That is most emphatically, getting one's working hypothesis early in the game and I, therefore, class Darwin as a guesser. This is in accord with Poulton's statement that the essential causes of Darwin's success were "the creative genius ever inspired by existing knowledge to build hypotheses by whose aid further knowledge could be won, the calm unbiased mind, the transparent honesty and love of truth which enabled him to abandon or to modify his own creations when they ceased to be supported by observation. The even balance between these powers was as important as their remarkable development."

Mendel is one of the doubtful cases. One can easily claim that he is a brilliant example of the Baconian method, having collected his data until the results were fairly obvious. One can also claim that he was sure in advance that there would be some statistical ratio for hybrids and that he searched for it, which would make him a guesser. We know so little about him that it is difficult to make a definite decision. Bateson[1] gives a fair summary of what little we know about Mendel's starting point.

"His success is due to the clearness with which he thought out the problem. Being familiar with the works of Gaertner and the other experimental breeders, he surmised that their failure to reach definite and consistent conclusions was due to a want of precise and continued analysis. In order to obtain a clear result he saw that it was absolutely necessary to start with pure-breeding, homogeneous materials, to con-

[1] "Mendel's Principles of Heredity," 7 (1909).

sider each character separately, and on no account to confuse the different generations together. Lastly he realised that the progeny from distinct individuals must be separately recorded. All these ideas were entirely new in his day. When such precautions had been observed, he anticipated that a regular result would be attainable if the experiments were carried out on a sufficient scale."

Pasteur was a brilliant guesser and a great fighter. As he himself said: "Without theory, practice is but routine born of habit. Theory alone can bring forth and develop the spirit of invention." When he started on the question of alcoholic fermentation, he observed that the juice from the sick vats contained special rods. After trying several hypotheses which did not prove right, he finally decided that "the little rods in the juice of the sick vats are alive, and it is *they* that make the acid of sour milk—the rods fight with the yeasts perhaps, and get the upper hand. They are the ferment of sour-milk-acid, just as the yeasts must be the ferment of the alcohol."

In regard to the work on lactic acid fermentation Duclaux[1] says that "this memoir is full of suggestion and, strangely enough, all these propositions which were so new and so bold for the epoch were announced *de plano* almost carelessly, with the tranquil confidence of a man sure of his facts, and to whom, if one did not know him, one might even have attributed malicious intentions, he showed so much apathy. It is only at the end of his memoir that he admits that nothing of all this has been demonstrated."

In another passage,[2] Duclaux quotes Pasteur's own words. "I take the liberty of recalling to my confrère, M. Blanchard, that the illusions of an experimenter form a great part

[1] "Pasteur, the History of a Mind," 72 (1920).
[2] p. 280.

of his power. These are the preconceived ideas which serve to guide him. Many of them vanish in the long path which he must travel, but one fine day he discovers and proves that some of these are adequate to the truth. Then he finds himself master of new facts and new principles, the application of which, sooner or later, bestows their benefits."

The case of anthrax is similar in principle to that of the acid fermentation, though very much more striking so far as the public was concerned.[1]

"The confusion of ideas on the origin of contagious and epidemic diseases was about to be suddenly enlightened; Pasteur had now taken up the study of the disease known as charbon or splenic fever. This disease was ruining agriculture; the French provinces of Beauce, Brie, Burgundy, Nivernais, Berry, Champagne, Dauphiné, and Auvergne paid a formidable yearly tribute to this mysterious scourge. In the Beauce, for instance, twenty sheep out of every hundred died in one flock; in some parts of Auvergne the proportion was ten or fifteen percent, some times even twenty-five, or fifty percent. At Provins, at Meaux, at Fontainebleau, some farms were called *charbon farms;* elsewhere, certain fields or hills were looked upon as accursed and an evil spell seemed to be thrown over flocks bold enough to enter those fields or ascend those hills. Animals stricken with this disease almost always died in a few hours; sheep were seen to lag behind the flock, with drooping head, shaking limbs and gasping breath; after a rigor and some sanguinolent evacuations, occurring also through the mouth and nostrils, death supervened, often before the shepherd had had time to notice the attack. The carcase rapidly became distended, and the least rent in the skin gave issue to a flow of black, thick and viscid blood, hence the name

[1] R. Vallery-Radot: "The Life of Pasteur," 257, 259 (1923).

Anthrax given to the disease. It was also called splenic fever, because necropsy showed that the spleen had assumed enormous dimensions. If that were opened, it presented a black and liquid pulp. In some places the disease assumed a character of extreme virulence; in the one district of Novgorod, in Russia, 56,000 head of cattle died of splenic infection between 1867 and 1870. Horses, oxen, cows, sheep, everything succumbed, as did also 528 persons, attacked by the contagion under divers forms. A pin prick or a scratch is sufficient to inoculate shepherds, butchers, knackers, or farmers with the malignant pustule."

"Pasteur tackled the subject. A little drop of the blood of an animal which had died of anthrax—a microscopic drop—was laid, sown, after the usual precautions to ensure purity, in a sterilized balloon which contained neutral or slightly alkaline urine. The culture medium might equally be common household broth, or beer-yeast-water, either of them neutralized by potash. After a few hours, a sort of flake was floating in the liquid; the bacteridia could be seen, not under the shape of short broken rods, but with the appearance of filaments, tangled like a skein; the culture medium being highly favourable, they were growing longer. A drop of that liquid abstracted from the first vessel, was sown into a second vessel, of which one drop was again placed into a third, and so on, until the fortieth flask; the seed of each successive culture came from a tiny drop of the preceding one. If a drop from one of those flasks was introduced under the skin of a rabbit or a guinea-pig, splenic fever and death immediately ensued, with the same symptoms and characteristics as if the original drop of blood had been inoculated.

In the presence of the results from those successive cultures, what became of the hypothesis of an inanimate substance contained in the first drop of blood? It was now diluted in a proportion impossible to imagine. It would therefore be absurd, thought Pasteur, to imagine that the last virulence owed its power to a virulent agent existing in the original drop of blood; it was to the bacteridium, multiplied in each culture, and to the bacteridium alone, that this power was due; the life of the bacteridium had made the virulence. "Anthrax is therefore," Pasteur declared, "the disease of the bacteridium, as trichinosis is the disease of the trichina, as itch is the disease of its special acarus, with this circumstance, however, that in anthrax, the parasite can only be seen through a microscope, and very much enlarged." After the bacteridium had presented those long filaments, within a few hours, two days at the most, another spectacle followed; amidst those filaments, appeared the oval shapes, the germs, spores, or seeds, pointed out by Dr. Koch. Those spores, sown in broth, reproduced in their turn the little packets of tangled filaments, the bacteria. Pasteur reported that one single germ of bacteridium in the drop which is sown multiplies during the following hours and ends by filling the whole liquid with such a thickness of bacteridia that, to the naked eye, it seems that carded cotton has been mixed with the broth."

Lister's work was entirely deductive. His original idea was that oxygen caused suppuration of wounds, because the presence of air was apparently necessary. He was quick to perceive the importance of Pasteur's work on microorganisms in the air and he followed out this line of attack.[1]

"Lister did for the craft of surgery what John Hunter had done for its science. When he first began his work, operations were few owing to the danger of putrefaction

[1] Sir Berkeley Moynihan: Nature, *119,* 572 (1927).

in the wound, followed in almost all cases by death. Even the simplest operation was a great anxiety to the surgeon, from the ever-present fear of suppuration developing. Lister's discovery was very gradual. His earliest surgical inquiries dealt with inflammation and the coagulation of the blood; but his chief interest lay always in the problem of the healing of wounds. He had arrived at the conclusion that the essential cause of suppuration in wounds was decomposition brought about by the atmosphere acting upon blood and serum retained in them, or upon portions of destroyed tissues; but, since oxygen was considered to be the agent causing this putrefaction, it appeared hopeless to devise a method by which suppuration might be prevented. But when Pasteur had shown that putrefaction was caused by minute organisms suspended in the air, a method of prevention at once came to his mind, to apply to the wound some substance which would destroy the micro-organisms without injuring the body tissues. Still later he developed a method by which the organisms might be destroyed before they had even entered the wound. Around every step of his advance, fierce controversy raged; the scepticism of early contemporaries was stupid, unimaginative, and petty. But the history of science frequently discloses this bitter opposition to new truths, as in the case of Harvey, Pasteur, and other famous men. Lister's answer was unfaltering continuance in inquiry and experiment, with demonstration of his results. The heavily infected wounds seen during the War have enabled us to realise much more acutely the problems which confronted Lister at the beginning of his work, and have increased our admiration for the way he overcame them. Although Lister sought to destroy the organisms which might enter a wound, yet he was not blind to the natural resistance of the body's cells to infection, so that a natural step was the development of aseptic surgery in which organisms are prevented from entering a wound so far as possible and any that do can then be dealt with by the body's own bactericidal forces. There is no real clash between 'antiseptic' and 'aseptic' methods, for no surgeon ever practised with success a method which omitted the use of agents for the destruction of organisms. The consequences of Lister's work were many and far-reaching: when the few operations, which were practised in those days, became safe, it was obvious that others might be attempted, and thus has grown up the science and art of modern surgery. Ovariotomy was one of the first operations to be made safe; and once it was found that the abdomen could be safely opened, a vast field of usefulness was before the surgeon. The cranial and thoracic cavities then became accessible to surgical methods of treatment, so that nowadays almost all parts of the body can be safely submitted to surgical operation. Not the least of the debts we owe to Lister is the curability of cancer if complete surgical removal is practised in the early stages of the disease. We may almost claim that the full effect of Lister's work is now accomplished. The art of surgery is far in advance of the sciences on which its future progress depends. The great search must be for methods of applying new discoveries in other sciences to the study of disease.

No one would question the propriety of putting Helmholtz, van't Hoff, Arrhenius, J. J. Thomson, and Rutherford among the guessers. It is interesting to recall that Helmholtz's paper on the "Conservation of Energy"[1] was called unintelligible speculations by Poggendorff.

Dewar is a striking example of an accumulator who was very versatile. When he studied low-temperature problems, for instance, he covered the ground very thoroughly. It was

[1] (1847).

not merely a question of developing improved methods of liquefying gases and of determining melting-points, boiling-points, and densities. Dewar studied specific heats, latent heats of vaporization, diffusion, adsorption by charcoal, optical and magnetic properties, color, photochemical reactions, the effects of low temperatures on bacteria and on electrical resistance, etc., etc. It is a real pleasure to note how many sides Dewar saw to a problem. While one cannot fail to be impressed by the brilliancy and versatility of Dewar's work and by the remarkable manipulative skill that he showed, there is surprisingly little in the way of theory. Dewar was an accumulator and not a guesser. He was intensely interested in experimentation; but he did not care at all for the theoretical bearing of his experiments. He did a great deal of work on the adsorption of gases; but he was not interested in the laws of adsorption. This characteristic appears very strongly in his work on soap films. The results are fascinating and are veritable triumphs of experimental ingenuity. Bubbles were blown four feet in diameter which lasted several hours; one bubble, 46 cm. in diameter, lasted sixty-three days; and a horizontal black film, 20 cm. in diameter, lasted for a year. Dewar gives all details; but he draws no theoretical conclusions from them and makes no effort to do so.

No classification of this sort gives a sharp dividing line. One can find all sorts of gradations between the two extremes and each one of us can pick out men whom he finds difficult to put definitely in one or the other of the two groups. For me Ramsay is on the line. Some days I decide to classify him as a guesser and on other days he seems certainly an accumulator. Perhaps it will save trouble to call him a transition personage.

While it is true that the overwhelming majority of the important theoretical advances have been made by guessers and not by accumulators, it would be ridiculous to claim that no important discoveries stand to the credit of the accumulators. Bumstead[1] says that "accurate measurements, however, do sometimes produce brilliant discoveries—when they fall into the right hands. A classical example of this is the discovery of argon by Lord Rayleigh, as the result of a quite prosaic undertaking to redetermine with great accuracy the density of nitrogen." The hydrogenation of oils and much of the catalytic work of today depends on the experiments of Sabatier, who is a typical accumulator. Millikan's work on cosmic rays, which has interested the public so much, must be credited to the accumulators. There are doubtless other important discoveries, with which I am not familiar and which belong in the same class; but the total number in this group is relatively small. On the other hand the scientific public has been so encouraged to believe in the tremendous value of exact measurements that a really first-class accumulator is practically certain of being awarded a Nobel prize. For instance, Millikan says that "progress in physics follows invention of new measuring apparatus and the proof from exact measurements that formula is not adequate. Instances are isotopes and the fact that 4H is not exactly equal to He in its bearing on the source of the sun's radiation."

There is nothing especially new in this criticism of the Baconian method. People have been doing it from the time that Bacon put forward his views; but it has had surprisingly little effect. Harvey said that "the Lord Chancellor writes of science—like a Lord Chancellor."

Whewell[2] is strong against the Baconian method and an

[1] Woodruff: "The Development of the Sciences," 56 (1923).

[2] "Novum Organum Renovatum," 63, 78 (1858).

enthusiastic supporter of the doctrine of guessing. "When mere position, and number, and resemblance, will no longer answer the purpose of enabling us to connect the facts, we call in other Ideas, in such cases more efficacious, though less obvious.

"But how are we, in these cases, to discover such Ideas, and to judge which will be efficacious, in leading to a scientific combination of our experimental data? To this question, we must in the first place answer, that the first and great instrument by which facts so observed with a view to the formation of exact knowledge, are combined into important and permanent truths, is that peculiar Sagacity which belongs to the genius of a Discoverer; and which, while it supplies those distinct and appropriate Conceptions which lead to its success, cannot be limited by rules, or expressed in definitions. It would be difficult or impossible to describe in words the habits of thought which led Archimedes to refer the conditions of equilibrium on the Lever to the Conception of pressure, while Aristotle could not see in them anything more than the results of the strangeness of the properties of the circle:—or which impelled Pascal to explain by means of the Conception of the *weight of air,* the facts which his predecessors had connected by the notion of nature's horrour of a vacuum; or which caused Vitello and Roger Bacon to refer the magnifying power of a convex lens to the bending of the rays of light towards the perpendicular by refraction, while others conceived the effect to result from the matter of medium, with no consideration of its form. These are what are commonly spoken of as felicitous and inexplicable strokes of inventive talent; and such, no doubt, they are. No rules can ensure to us similar success in new cases; or can enable men who do not possess similar endowments, to make like advances in knowledge.

"Yet still, we may do something in tracing the process by which such discoveries are made; and this it is here our business to do. We may observe that these, and the like discoveries, are not improperly described as happy *Guesses;* and that Guesses, in these as in other instances, imply various suppositions made, of which some one turns out to be the right one. We may, in such cases, conceive the discoverer as inventing and trying many conjectures, till he finds one which answers the purpose of combining the scattered facts into a single rule. The discovery of general truths from special facts is performed, commonly at least, and more commonly than at first appears, by the use of a series of suppositions, or *Hypotheses,* which are looked at in quick succession, and of which the one which really leads to truth is rapidly detected, and when caught sight of, firmly held, verified, and followed to its consequences. In the minds of most discoverers, this process of invention, trial, and acceptance or rejection of the hypothesis, goes on so rapidly that we cannot trace it in its successive steps. But in some instances, we can do so; and we can also see that the other examples of discovery do not differ essentially from these. The same intellectual operations take place in other cases, although this often happens so instantaneously that we lose the trace of the progression. In the discoveries made by Kepler we have a curious and memorable exhibition of this process in its details. Thanks to his communicative disposition, we know that he made nineteen hypotheses with regard to the motion of Mars, and calculated the results of each, before he established the true doctrine, that the planet's path is an ellipse. We know, in like manner, that Galileo made wrong suppositions respecting the laws of falling bodies, and Mariotte, concerning the motion of water in a siphon, before they hit upon the correct view of these cases.

"But it has very often happened in the history of science that the erroneous hypotheses which preceded the discovery of the truth have been made, not by the discoverer himself, but by his precursors; to whom he thus owed the service, often an important one in such cases, of exhausting the most tempting forms of errour. Thus the various fruitful suppositions by which Kepler endeavoured to discover the law of refraction, led the way to its real detection by Snell; Kepler's numerous imaginations concerning the forces by which the celestial motions are produced—his 'physical reasonings' as he termed them,—were a natural prelude to the truer physical reasonings of Newton. The various hypotheses by which the suspension of vapour in air had been explained, and their failure, left the field open for Dalton with his doctrine of the mechanical mixture of gases. In most cases, if we could truly analyze the operation of the thoughts of those who make, or who endeavour to make discoveries in science, we should find that many more suppositions pass through their minds than those which are expressed in words; many a possible combination of conceptions is formed and soon rejected. There is a constant invention and activity, a perpetual creating and selecting power at work, of which the last results only are exhibited to us. Trains of hypotheses are called up and pass rapidly in review; and the judgment makes its choice from the varied group.

"It would, however, be a great mistake to suppose that the hypotheses, among which our choice thus lies, are constructed by an enumeration of obvious cases, or by a wanton alteration of relations which occur in some first hypothesis.

It may, indeed, sometimes happen that the proposition which is finally established is such as may be formed, by some slight alteration, from those which are justly rejected. Thus Kepler's elliptical theory of Mars's motions involved relations of lines and angles much of the same nature as his previous false suppositions: and the true law of refraction so much resembles those erroneous ones which Kepler tried, that we cannot help wondering how he chanced to miss it. But it more frequently happens that new truths are brought into view by the application of new Ideas, not by new modifications of old ones. The cause of the properties of the Lever was learnt, not by introducing any new *geometrical* combination of lines and circles, but by referring the properties to genuine *mechanical* Conceptions. When the Motions of the Planets were to be explained, this was done, not by merely improving the previous notions, of cycles of time, but by introducing the new conception of *epicycles* in space. The doctrine of the Four Simple Elements was expelled, not by forming any new scheme of elements which should impart according to new rules, their sensible qualities to their compounds, but by considering the elements of bodies as *neutralizing* each other. The Fringes of Shadows could not be explained by ascribing new properties to the single rays of light, but were reduced to law by referring them to the *interference* of several rays.

"Since the true supposition is thus very frequently something altogether diverse from all the obvious conjectures and combinations, we see here how far we are from being able to reduce discovery to rule, or to give any precepts by which the want of real invention and sagacity shall be supplied. We may warn and encourage these faculties when they exist, but we cannot create them, or make great discoveries when they are absent. . . .

"To discover a Conception of the mind which will justly represent a train of observed facts is, in some measure, a process of conjecture, as I have stated already; and, as I then observed, the business of conjecture is commonly conducted by calling up before our minds several suppositions, and selecting that one which most agrees with what we know of the observed facts. Hence he who has to discover the laws of nature may have to invent many suppositions before he hits upon the right one; and among the endowments which lead to his success, we must reckon that fertility of invention which ministers to him such imaginary schemes, till at last he finds the one which conforms to the true order of nature. A facility in devising hypotheses, therefore, is so far from being a fault in the intellectual character of a discoverer, that it is, in truth, a faculty indispensable to his task. It is, for his purposes, much better that he should be too ready in contriving, too eager in pursuing systems which promise to introduce law and order among a mass of unarranged facts, than that he should be barren of such inventions and hopeless of such success. Accordingly, as we have already noticed, great discoverers have often invented hypotheses which would not answer to all the facts, as well as those which would; and have fancied themselves to have discovered laws, which a more careful examination of the facts overturned.

"The tendencies of our speculative nature, carrying us onwards in pursuit of symmetry and rule, and thus producing all true theories, perpetually show their vigour by overshooting the mark. They obtain something by aiming at much more. They detect the order and connexion which exist, by conceiving imaginary relations of order and connexion which have no existence. Real discoveries are thus mixed with baseless assumptions; profound sagacity is combined with fanciful conjecture; not rarely, or in peculiar cases, but commonly, and in most cases; probably in all, if we could read the thoughts of discoverers as we read the books of Kepler. To try wrong guesses is, with most persons, the only way to hit upon right ones. The character of the true philosopher is, not that he never conjectures hazardously, but that his conjectures are clearly conceived, and brought into rigid contact with facts. He sees and compares distinctly the Ideas and Things;—the relations of his notions to each other and to phenomena. Under these conditions, it is not only excusable, but necessary for him, to snatch at every semblance of general rule,—to try all promising forms of simplicity and symmetry.

"Hence advances in knowledge are not commonly made without the previous exercise of some boldness and licence in guessing. The discovery of new truths requires, undoubtedly, minds careful and scrupulous in examining what is suggested; but it requires, no less, such as are quick and fertile in suggesting. What is Invention, except the talent of rapidly calling before us the many possibilities, and selecting the appropriate one? It is true that, when we have rejected all the inadmissible suppositions, they are often quickly forgotten; and few think it necessary to dwell on these discarded hypotheses and on the processes by which they were condemned. But all who discover truths, must have reasoned upon many errors to obtain each truth; every accepted doctrine must have been one chosen out of many candidates. If many of the guesses of philosophers of bygone times now appear fanciful and absurd, because time and observation have refuted them; others, which were at the time equally gratuitous, have been confirmed in a manner which makes them appear marvellously sagacious. To form hypotheses and then to employ much labour and skill in refuting them, if they do not succeed in establishing them, is a part of the usual process of inventive minds. Such a proceeding belongs to the *rule* of the genius of discovery, rather than (as has often been thought in modern times) to the exception."

In a later book Whewell[1] says: "Scientific discovery must ever depend upon some happy thought, of which we cannot trace the origin:—some fortunate cast of intellect rising above all rules. No maxims can be given which will inevitably lead to discovery. No precepts will elevate a man of ordinary endowments to the level of a man of genius."

Brodie[2] wrote: "Lastly, physical investigations, more than anything besides, help to teach us the actual value and right use of the Imagination, of that wondrous faculty, which, left to ramble uncontrolled, leads us astray into a wilderness of perplexities and errors, a land of mists and shadows; but which, properly controlled by experience and reflection, becomes the noblest attribute of man; the source of poetic genius; the instrument of discovery in Science, without the aid of which Newton would never have invented fluxions, nor Davy have decomposed the earths and alkalies, nor would Columbus have found another Continent."

Tyndall[3] takes practically the same view. "Mathematics and physics have long been accustomed to coalesce. For, no matter how subtle a natural phenomenon may be, whether we observe it in the region of sense, or follow it into that of imagination, it is in the long run reducible to mechanical laws. But the mechanical data once guessed or given, mathematics becomes all-powerful as an instrument of deduction.

[1] "Philosophy of Discovery," 44 (1860).

[2] Proc. Roy. Soc., *10,* 165 (1860).

[3] "Fragments of Science," 410, 426, 546 (1884).

The command of Geometry over the relations of space, and the far-reaching power which Analysis confers, are potent both as means of physical discovery, and of reaping the entire fruits of discovery. Indeed, without mathematics, expressed or implied, our knowledge of physical science would be both friable and incomplete.

"Side by side with the mathematical method we have the method of experiment. Here from a starting-point furnished by his own researches or those of others, the investigator proceeds by combining intuition and verification. He ponders the knowledge he possesses, and tries to push it further; he guesses and checks his guesses; he conjectures, and confirms or explodes his conjectures. These guesses and conjectures are by no means leaps in the dark; for knowledge once gained casts a faint light beyond its own immediate boundaries. There is no discovery so limited as not to illuminate something beyond itself. The force of intellectual penetration into this penumbral region which surrounds actual knowledge is not, as some seem to think, dependent upon method, but upon the genius of the investigator. The profoundest minds know best that Nature's ways are not at all times their ways, and that the brightest flashes in the world of thought are incomplete until they have been proved to have their counterpart in the world of fact. Thus the vocation of the true experimentalist may be defined as the continued exercise of spiritual insight and its incessant correction and realization. His experiments constitute a body of which his purified insight is, as it were, the soul. . . .

"There are Tories even in science who regard Imagination as a faculty to be feared and avoided rather than employed. They have observed its action in weak vessels and are unduly impressed by its disasters. But they might with equal justice point to exploded boilers as an argument against the use of steam. Nourished by knowledge patiently won; bounded and conditioned by co-operant Reason; imagination becomes the prime mover of the physical discoverer. Newton's passage from a falling apple to a falling moon was, at the outset, a leap of prepared imagination. In Faraday the exercise of this faculty preceded all his experiments, and its function has been impressively set forth by Brodie. When William Thomson [Lord Kelvin] tries to place the ultimate particles of matter between his compass points and to apply to them a scale of millimetres, it is an act of the scientific imagination. And in much that has recently been said about protoplasm and life we have the outgoings of this faculty guided and controlled by the known analogies of science. In fact, without this power, our knowledge of nature would be a mere tabulation of co-existences and sequences. . . .

"First of all, I am blamed for crossing the boundary of experimental evidence. This, I reply, is the habitual action of the scientific mind—at least of that portion of it which applies itself to physical investigation. Our theories of light, heat, magnetism, and electricity, all imply the crossing of this boundary. My paper on the 'Scientific Use of the Imagination' and my 'Lectures on Light,' illustrate this point in the amplest manner; and, in the brief discourse which follows this Address, I have sought incidentally to make clear that in physics the experiential incessantly leads to the extra-experiential; that out of experience there always grows something finer than mere experience; and that in their different powers of extension consists, for the most part, the difference between the great and the mediocre investigator. The kingdom of science, then, cometh not by observation and experiment alone, but is completed by fixing the roots of observation and experiment in a region inaccessible to both, and in dealing with which we are forced to fall back upon the picturing power of the mind."

Woodruff[1] quotes Huxley as saying that "it is a favorite popular delusion that the scientific enquirer is under a sort of moral obligation to abstain from going beyond that generalization of observed facts which is absurdly called 'Baconian' induction. But any one who is practically acquainted with scientific work is aware that those who refuse to go beyond fact, rarely get as far; and any one who has studied the history of science knows that almost every step therein has been made by the 'Anticipation of Nature,' that is, by the invention of hypotheses, which, though verifiable, often had very little foundation to start with; and not infrequently, in spite of a long career of usefulness, turned out to be wholly erroneous in the long run."

G. K. Gilbert[2] says: "It is the province of research to discover the antecedents of phenomena. This is done by the aid of hypothesis. A phenomenon having been observed, or a group of phenomena having been established by empiric classification, the investigator invents an hypothesis in explanation. He then devises and applies a test of the validity of the hypothesis. If it does not stand the test, he discards it and invents a new one. If it survives the test, he proceeds at once to devise a second test. And he thus continues until he finds an hypothesis that remains unscathed after all the tests his imagination can suggest.

"This, however, is not his universal course, for he is not restricted to the employment of one hypothesis at a time. There is indeed an advantage in entertaining several at once, for then it is possible to discover their mutual an-

[1] "The Development of the Sciences," 217 (1925).

[2] Am. J. Sci., (3) *31*, 286, 287 (1886).

tagonisms and inconsistencies, and to devise crucial tests,—tests which will necessarily debar some of the hypotheses from further consideration. The process of testing is then a process of elimination, at least until all but one of the hypotheses have been disproved.

"In the testing of hypotheses lies the prime difference between the investigator and the theorist. The one seeks diligently for the facts which may overthrow his tentative theory, the other closes his eyes to these and searches only for those which will sustain it.

"Evidently, if the investigator is to succeed in the discovery of veritable explanations of phenomena, he must be fertile in the invention of hypotheses and ingenious in the application of tests. The practical questions for the teacher are, whether it is possible by training to improve the guessing faculty, and if so, how it is to be done. To answer these, we must give attention to the nature of the scientific guess considered as a mental process. Like other mental processes, the framing of hypotheses is usually unconscious, but by attention it can be brought into consciousness and analyzed."

"The great investigator is primarily and preëminently the man who is rich in hypotheses. In the plenitude of his wealth he can spare the weaklings without regret; and having many from whom to select, his mind maintains a judicial attitude. The man who can produce but one, cherishes and champions that one as his own, and is blind to its faults. With such men, the testing of alternative hypotheses is accomplished only through controversy. Crucial observations are warped by prejudice, and the triumph of the truth is delayed."

Gregory[1] says that "Bacon drew up the rules by which he considered Nature should be studied, but he treated

[1] "Discovery," 134 (1923).

almost with contempt all progress accomplished without the use of his prescription, and he persistently rejected the Copernican theory, though it formed the best possible example of the application of his own system of collecting observations and arriving at conclusions from them. Few natural philosophers who came after him took heed of his artificial process of discovery; and there is little evidence that the method assisted in the advance of science in any way. Newton never mentioned Bacon or his system, though he was born and educated after its publication; and a study of the progress of science fails to furnish sufficient reason for believing that Bacon's 'Novum Organum' has been either a powerful source of inspiration or has provided the formula by which natural knowledge has been increased. It is, indeed, a mistake to suppose that all scientific investigation must proceed from the general to the particular according to a prescribed formula, or be determined by any like hard-and-fast principle. Devotion to such doctrines has often led men astray and is always an undesirable obsession."

Mellor[1] discusses what he calls Newton's inductive [?] method. "Here the attempt is made to infer the hidden generalization from the consequences of the assumption (hypothesis) what that generalization is. The process is sometimes called *a posteriori* reasoning. This method of investigation was extensively employed with glorious results by Isaac Newton, although it had been advocated by Aristotle two thousand years earlier. Francis Bacon, indeed, before Newton's time, protested against anticipating nature by hypotheses; but the greatest triumphs of modern science have been won by the application of the Newtonian method, while the Baconian method has been singularly unfruitful.

[1] "A Comprehensive Treatise on Inorganic and Theoretical Chemistry," *1*, 18 (1922).

Francis Bacon's failure in the practice of his own method was complete."

Mellor quotes De Morgan as saying: "According to Francis Bacon, facts are made to make theories *from,* and according to Isaac Newton, to try ready-made theories by." Woodruff[1] also says a good word for the Aristotelian method. "The climax of the scientific Renaissance involved a turning away from the authority of Aristotle and an adoption of the Aristotelian method of observation and induction."

Two generations ago, Claude Bernard,[2] quoting de Maistre, agreed "that they who make the most discoveries in science know Bacon least, while those who read and ponder him, like Bacon himself, have poor success."

Wright[3] emphasizes very properly the stress that Bacon laid on experiments; but he is, equally properly, not enthusiastic over the Baconian method itself. "Bacon may indeed have had an idea of the way the human mind works, but his chief preoccupation seems to have been with the way he thought it ought to work. Yet his own inductive method did much, a very great deal indeed subsequently to guide the natural mind of man into more profitable ways of working, to broaden the basis on which it must work; but without deductions, theories, hypotheses, it will not work at all and this Aristotle well understood. Apparently neither Bacon nor Newton did. [This is an error as regards Newton.] They may have thought so, but there was nothing wrong with Aristotle except the Aristotelians who preceded Bacon. Bacon as well as the Baconians were largely in error, not

[1] "The Development of the Sciences," 221 (1923).

[2] "An Introduction to the Study of Experimental Medicine," translated by Henry Copley Greene.

[3] Scientific Monthly, *26,* 38 (1928).

only as to Aristotle, but as to the natural workings of the human mind."

This mass of evidence might lead one to infer that it is flogging a dead horse to belabor the Baconian method. Unfortunately, this is not the case. So far as I can judge, the belief in the Baconian method is just as strong today and just as wide-spread as it has ever been.

Although Whewell has expressed his disbelief in the Baconian method, he begins his "Novum Organum Renovatum" with the following quotation from Herschel:—"It is to our immortal countryman, Bacon, that we owe the broad announcement of this grand and fertile principle; and the development of the idea, that the whole of natural philosophy consists entirely of a series of inductive generalizations, commencing with the most circumstantially stated particulars, and carried up to universal laws, or axioms, which comprehend in their statements every subordinate degree of generality; and of a corresponding series of inverted reasoning from generals to particulars, by which these axioms are traced back into their remotest consequence, and all particular problems deduced from them; as well as those by whose immediate considerations we rose to their discovery, as those of which we had no previous knowledge."

Only a few years ago Sir William Pope[1] said, in an address, that at the end of the sixteenth century Francis Bacon "accentuated, though he did not himself discover, the principles of the modern method of scientific enquiry. Although Bacon himself was a philosopher and not an experimenter, and although he made the most ineffective use of the tools which he did so much to perfect, his authority as Lord Chancellor of England and his eloquence as a writer gave a tremendous impetus to the experimental method of scientific

[1] Chemistry and Industry, *42*, 53 (1923).

inquiry, and may be said, for all practical purposes, to mark him as the founder of modern science."

Even worse is the statement by Westaway.[1] "There is one fundamental difference between English and German scholars. Taught by Bacon, English scholars seem to have acquired an instinctive desire to accumulate all possible facts before attempting to frame anything of the nature of a general law. But German thinkers tend to generalize before the accumulated facts afford the necessary justification. Their curious love of abstraction and their desire to deduce a whole universe from a few general propositions, constantly lead to their illegitimate use of deductive reasoning; they seem to be unsuspicious of the dangers of loosely establishing generalities."

This is about as wrong as a paragraph can be. Such great Englishmen as Harvey, Newton, Young, Dalton, Davy, Faraday, Joule, Darwin, Lister, J. J. Thomson, and Rutherford were guessers, and generalized long before the experimental data really justified it. On the other hand the German physicists and chemists have not been, as a rule, great generalizers. Their most striking characteristics have been the ability to recognize the importance of a generalization when it had been made by somebody else, and to develop the consequences and applications of the generalization in all its details.

As late as the end of April Peltier[2] said that "while many of Aristotle's observations are sound, his deductive reasoning on natural phenomena probably more than any other factor held in abeyance the adoption of the experimental or inductive methods in science." The mistake here is the common one of making experimental synonymous with inductive.

[1] "Science and Theology," 26, 151 (1920).

[2] Science, (2) *68*, 192 (1928).

The Character Education Institute of Chevy Chase, Maryland, has sent out an enormous number of yellow slips dealing with what it calls the scientific methods. This Institute lists twenty-five "intellectual immoralities," number four being "generalizing beyond one's data." It was also stated that letters of advice are asked from all interested. Being still ignorant enough to believe that people sometimes mean what they say, I wrote to the unknown Institute saying that I thought that "Intellectual Immorality No. 4" should read "Not generalizing beyond one's data." I got back a letter saying: "Don't you think that hard and fast rules are a mistake," or words to that effect. I quite agree; but it would have been well if Mr. Milton Fairchild had thought of that before publishing his twenty-five "intellectual immoralities."

I have shown why I believe it to be better for a scientific man to try to be a guesser rather than an accumulator—if he can. No method of research is fool-proof and the obvious danger about the Aristotelian method is that one may become infatuated with a given working hypothesis and warp the facts to fit the hypothesis. This is a real danger but not so serious a one as many people think. The best protection against this source of error is to have several working hypotheses and to shift from one to the other with great readiness. Michael Faraday said once: "The world little knows how many of the thoughts and theories which have passed through the mind of a scientific investigator have been crushed in silence and secrecy by his own severe criticism and adverse examinations; that in the most successful instances not a tenth of the suggestions, the hopes, the wishes, the preliminary conclusions have been realized."

If a man like Faraday was pleased if he guessed right once out of ten times, most of us can rejoice if we make one successful guess in a hundred. If one really believes that, there is not much danger of clinging unfairly to any working hypothesis.

A more real danger, because most people do not know that it exists, is the belief that a working hypothesis must be right if it describes the facts accurately. This is not sufficient. One is safe only if one knows that no other hypothesis will disturb the facts equally well. There have been a number of cases in the last fifty years in which two entirely different hypotheses led to the same result. Three typical examples may be cited. Goodwin[1] made some admirable calculations on the electromotive forces of concentration cells, starting with the explicit assumption that the formula of mercurous chloride is HgCl. While I have mislaid the reference, I am quite certain that Ostwald stated somewhere that Goodwin had proved the formula of mercurous chloride to be HgCl as completely as anything can be proved in this world. Later, it developed that the formula for mercurous nitrate is $Hg_2(NO_3)_2$ and not $HgNO_3$, from which it follows that the formula for mercurous chloride should be written Hg_2Cl_2.

This was very disconcerting until it was found that Goodwin's equations came out the same way no matter what formula was assumed for mercurous chloride. Of course, somebody should have seen that in the beginning; but we are dealing with things as they are, and nobody did.

Some years ago Michelson[2] studied the colors of iridescent insects and of iridescent feathers, such as the tail feathers of the peacock, the throat feathers of the humming-bird, etc. He made such a difficult series of tests that nobody has yet had the courage to repeat them. These tests agreed

[1] Z. physik. Chem., *13*, 577 (1894).
[2] Phil. Mag. (6) *21*, 554 (1911).

with the working hypothesis that the colors were due to solid pigments showing selective reflection, analogous to magenta, for instance, and Michelson consequently concluded that the iridescent colors were due to solid pigments giving selective reflection. It has proved impossible, however, to extract any bright-colored pigment from any of the iridescent portions of insects and feathers. It has also been shown conclusively that these colors are due to thin films and not to solid pigments. Consequently, it follows that Michelson's tests did not enable him to differentiate between solid pigments showing selective reflection and colors due to thin films. This has not yet been shown, apparently because some of the physicists are afraid to question the accuracy of Michelson's work and because the others are not especially interested one way or the other.

When hydrochloric acid or caustic soda is added to gelatine and water, there is a disappearance of hydrogen ions in the first case and of hydroxyl ions in the second case. There is no dispute about the facts, but only about the interpretation of them. This disappearance of hydrogen or hydroxyl ions might be due to the formation of a salt, with amphoteric gelatine, gelatine hydrochloride or sodium gelatinate as the case might be. It might also be due to strong adsorption of hydrogen or hydroxyl ions by gelatine with no formation of stoichiometric compounds. The two conceptions are diametrically opposed. The first postulates the formation of definite chemical compounds with stoichiometric relations; the second postulates the absence of definite chemical compounds with stoichiometric relations. Making use of the Donnan equilibrium, as it is called, Procter,[1] Wilson and Wilson,[2] and Jacques Loeb[3] have been very successful in cal-

[1] J. Chem. Soc., *105,* 313 (1914).

[2] J. Am. Chem. Soc., *40,* 886 (1918).

[3] "Proteins and the Theory of Colloidal Behavior."

culating the swelling of gelatine in acids on the explicit assumption that gelatine is an elastic jelly, insoluble in but completely permeable to water, which forms a definite compound with acids, gelatine hydrochloride, for instance.

Gelatine hydrochloride is also assumed to be soluble in water and completely dissociated; but the gelatine ion will not diffuse through gelatine and consequently the latter acts as a semipermeable membrane to gelatine hydrochloride, though not to chlorine ion. By combining the formula for the Donnan equilibrium with Hooke's law, Wilson gets a formula which reproduces admirably Procter's data on the swelling of gelatine, and he considers this a conclusive proof that gelatine and hydrochloric acid form a definite chemical compound, gelatine hydrochloride.

This is true only in case the formulas obtained by Loeb and Wilson cannot be deduced on some other assumption. Donnan[1] has pointed out that "an adsorption of hydrogen ions by colloidal aggregates or micelles (constituting the units of the 'molecular' network) would lead to the same general equations as the ionization of the amphoteric protein molecules assumed by Procter."

A. V. Hill[2] is equally definite. "It is contended by Loeb that the Donnan Membrane Equilibrium involving the presence of an indiffusible ion is the basis of the colloidal properties of a protein solution. While the possibility of this conclusion is admitted, it is pointed out that one of the chief arguments employed in its favour by Loeb is incorrect. Loeb shows that the potential difference observed experimentally between a protein and a non-protein solution separated by a membrane agrees very exactly with that 'calcu-

[1] Chem. Reviews, *1*, 87 (1924).

[2] Proc. Roy. Soc., *102A*, 705 (1923).

lated' from the difference in hydrogen-ion concentrations also observed experimentally, and concluded that this supports his theory. As a matter of fact, this equality is a necessary and inevitable consequence of the manner in which his observations were made and of general thermodynamical reasoning, and its proof is independent of any theory of the mechanism by which the potential difference is produced."

I do not advocate trying to turn all students either into guessers or into accumulators. A man who is of the accumulator type cannot be converted successfully into the guesser type and it may be a waste of good material to try to do so. It is hard to believe that anything would have kept T. W. Richards, for instance, from making exact measurements, and it would have been a great loss to science if that had happened, because he was the best chemist at exact measurements that has ever lived. When he tried to do something else, as in his work on compressible atoms, the theoretical side was depressing.

There is a passage in Wallace's Russia which has always interested me. "Of all the foreign colonists the Germans are by far the most numerous. The object of the Government in inviting them to settle in the country was that they should till the unoccupied land and thereby increase the national wealth, and that they should at the same time exercise a civilising influence on the Russian peasantry in their vicinity. In this latter respect they have totally failed to fulfil their mission. A Russian village, situated in the midst of German colonies, shows generally, so far as I could observe, no signs of German influence. Each nationality lives *more majorum,* and holds as little communication as possible with the other. The *muzhik* observes carefully—for he is very curious—the mode of life of his more advanced neighbours, but he never thinks of adopting it. He looks upon

Germans almost as being of a different world—as a wonderfully cunning and ingenious people, who have been endowed by Providence with peculiar qualities not possessed by ordinary Orthodox humanity. To him it seems in the nature of things that Germans should live in large, clean, well-built houses, in the same way as it is in the nature of things that birds should build nests; and as it has probably never occurred to a human being to build a nest for himself and his family, so it never occurs to a Russian peasant to build a house on the German model. Germans are Germans, and Russians are Russians—and there is nothing more to be said on the subject."

I do not object at all to people, who should be accumulators, becoming accumulators. When a graduate student tells me that he wishes to make exact measurements, I do not try to show him the error of his ways; I advise him to work with somebody else. I do object however to a student, who might have become a guesser, being forced into the ranks of the accumulators because he does not know that there is another and better type of research, and because he does not appreciate the futility of the slogan "First get your facts."

Only recently I heard of a boy who had made an excellent showing in chemistry as an undergraduate and as a graduate. His nose was kept, however, to the grindstone of exact measurements and that bored him because he was evidently not intended by nature to be an accumulator. He got out of chemistry and we shall never know whether he would have remained a chemist if this lecture had been published earlier.