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THE RÔLE OF ANALYSIS IN SCIENTIFIC INVESTIGATION[1]

By Professor DOUGLAS JOHNSON

COLUMBIA UNIVERSITY

CUSTOM decrees that the chairman of your section should, at the interval of a year following his presidency, deliver before you an appropriate address. It has seemed to me that I could best command your interest in some field of discussion where every one of us, geographer and geologist alike, has had experience. So I have selected the broad field connoted by the highly inclusive term, "scientific investigation"; and I would direct your attention, not to any particular results of such investigation, but to a concrete problem of method which I suppose must concern every scientific worker. This problem can briefly be stated as follows: What is the precise rôle of analysis in a properly conceived and successfully executed scientific investigation?

It goes without saying that I am not competent to speak of methods of research in chemistry and physics, where experiment plays a far larger rôle than in geology and geography. So also the biologist, the astronomer and investigators in other fields must speak for themselves. For this reason the title of my address may seem unduly ambitious. Yet I prefer the broader, more inclusive term "scientific investigation" to the more restricted if more accurate "geologic and geographic investigation," because it seems to me probable that some of the principles here discussed may find application beyond the limits of my particular field.

It is a pleasure to express here my indebtedness to several of my Columbia colleagues, Robert S. Woodworth, professor of psychology; Adam Leroy Jones, associate professor of philosophy; Sam F. Trelease,

[1] Abbreviated form of address as retiring vice-president and chairman of Section E—American Association for the Advancement of Science, delivered at Atlantic City, on December 27, 1932. The address will be published in amplified form, with fuller illustration from shore line studies, in the *Bulletin of the Geological Society of America*.

professor of botany; and George B. Pegram, professor of physics; also to Professor William Morris Davis. All these were good enough to read the address in manuscript form and give me the benefit of helpful criticisms.

The dictionaries tell us that analysis is the process of separating a thing or a concept into its constituent parts, in order to arrive at the essential or ultimate elements, causes or principles; that it is the tracing of things back to their sources; and that it is designed to clarify and test knowledge. The chemist analyzes a complex substance to determine its precise composition. For the purposes of our discussion I would define scientific analysis as "the process of separating observations, arguments and conclusions into their constituent parts, tracing each part back to its source and testing its validity, for the purpose of clarifying and perfecting knowledge."

I am persuaded we can pursue our discussion most easily if I illustrate my observations, so far as feasible, by reference to some concrete example. For sake of simplicity I shall select a common phenomenon found on the rocky shores of many lands, and in tracing the part played by analysis at each stage of a comprehensive scientific investigation, shall illustrate my points freely by referring to the study of this phenomenon, the origin of which is still a subject of dispute. Those who follow this discussion should not attach undue importance to the particular example selected for illustrative purposes, nor to the statements made concerning it. I have purposely selected a debatable matter, the study of which is not yet completed. Our concern here is not with the origin of the phenomenon, but with the method of investigating its origin.

Along the rocky shores bordering the sea are found nearly horizontal platforms or benches from a few feet to a few hundred feet in width, cut in solid rock. At their seaward margins these benches terminate in relatively abrupt slopes which descend toward or even into the water. From their inner or landward margins rise steep slopes or rocky cliffs. In elevation the benches range from 1 or 2 feet above ordinary high tide indefinitely upward, often several hundred feet above the same datum plane.

The combination of bench and cliff bordering the sea might arouse no significant reaction in the mind of the ordinary observer. But such a combination instantly excites the brain of the geomorphologist to activity. Memory recalls text book diagrams, explanatory titles under photographs of similar features, discussions of the origin of such features in geological and geographical treatises, perhaps earlier field examination of such forms. Rapidly the mind compares the forms, the situations, the mutual relations of the assembled features in the various cases called before it by observation and memory, and before the observer is conscious of the process he has leaped to an inductive inference: that both bench and cliff were cut into the margins of the land by wave erosion. In this case the inductive inference involves an explanation. But it is only a partial explanation. If the bench is wave-carved, why is it exposed to view above the level of the sea? The mind rushes on: Perhaps the land has been raised since the bench was carved. Perhaps the sea level has dropped.

The observer must now be on his guard against a dangerous tendency of the human intellect—the tendency to accept as valid a plausible explanation, and then to look for facts in support of that explanation. He must deliberately repress the tendency toward premature conclusions, and begin the task of gathering all the facts upon which alone can a satisfactory explanation be based. As already stated, we are not primarily concerned with the course of his particular investigation of these interesting forms, nor with the conclusions he may reach respecting their origin. But we shall draw freely upon this imaginary hypothetical study, to illustrate the uses of analysis in scientific research.

I. Analysis in the Stage of Observation

The initial stage in scientific investigation is normally that of observation. The first employment of this mental process may be made incidentally, perhaps almost passively. But once the investigator realizes that the observed facts present a problem, for the solution of which additional facts are desirable, he becomes an active inquirer, seeking to discover all facts bearing on the problem. He observes as widely and as accurately as possible, the observed facts being automatically recorded in his memory; but also, since the memory is notoriously fallible, deliberately recorded in his note book if he be a prudent investigator.

Since we are here concerned merely with the recording of external facts observed by the eye, it might appear that analysis has no rôle to play in this initial stage of an investigation. But let us dissect the matter and scrutinize its parts, first turning our attention to the material facts which are the object of observation. Can the analytical powers of the mind be brought to bear upon the facts themselves in such manner as to aid the investigator?

Let our hypothetical student of coastal benches answer. Having observed one such bench, and had his curiosity aroused to investigate, he begins his search for other examples. Very soon he is confronted by the necessity of discriminating coastal benches of the type described above from other forms similar in some respects yet different in others. He needs a name by which to designate the type forming the particular object of inquiry, and tentatively calls them elevated marine benches or simply "marine benches," because of their resemblance to submarine benches formed by wave abrasion and because of their apparent marine origin.

The question then arises: What constitutes a marine bench in the sense in which he is using the term? Is it any nearly level surface in the vicinity of the shore, terminated seaward by an abrupt descent, and landward by cliffs which rise steeply to higher levels? This combination of slopes appears repeatedly in nature, as the product of a variety of causes. The non-critical investigator, who welcomes every shelf and scarp facing the sea as one more link in the chain of facts he seeks to explain, is almost sure to accumulate, without knowing it, an assortment of unrelated observations, many of them irrelevant to his problem. His investigation is seriously compromised from its very beginning. How can he explain the origin and history of elevated marine benches if he unwittingly bases his reasoning on a confused mixture of wave-carved surfaces, benches due to differential weathering, rock terraces cut by a river before drowning brought in the sea, notches cut by a glacial stream flowing between waning continental ice and the sloping hillside, and terraces due to landslides, faulting or monoclinal warping. As all these features are found in the immediate vicinity of the shore in different places, and as all of them have repeatedly been ascribed to wave erosion, it is clear that the field data must in the first instance be subjected to rigid scrutiny.

The facts can not be lumped. They must be separated into their constituent parts, and each part tested as to its relevancy to the problem under investigation.

Thus far we have discussed the necessity of analyzing the things observed. But the investigator must probe more deeply than that. He must analyze the observational process itself. Does he really see what he thinks he sees? Not unless he is constantly on guard against the well known dangers to which observation is subject. Chief among these, perhaps, is the tendency to include inference with fact, to confuse theory with observation. The observer thinks he sees a bench cut in granite, when in fact all he really observes is a few scattered outcrops of granite protruding through the soil; from these he infers, and perhaps erroneously, that the vastly larger invisible mass is likewise granite. Another investigator may report that he saw a wave-carved bench which did not bevel the rock layers. All he really saw was a shelf or bench parallel to the rock layers. He inferred it was wave-carved, when in fact it may have been produced by differential weathering of weaker beds overlying a more resistant layer.

Another danger to which observation is subject is that of ocular deception. This danger looms large when one is looking for certain specific forms in the landscape. The power of suggestion is greater than many realize, and one looking for marine benches is in danger of finding them in faint undulations or irregularities of the terrain which under other circumstances would not impress him as significant. If he is looking for benches which he expects to be horizontal, the inclined position of something he mistakes for such a bench may quite escape his eye, even when the inclination is so marked as to be quite obvious to another who is without preconceptions as to what should be observed.

Incompleteness of observation is another common danger. The eye tends to pick from the landscape that which seems to it for the moment significant, and fails to note much that later stages of the study may show to be vitally important. Even where the initial observation is fairly complete, the benefits which should be derived from it may be lost through failure to record permanently in memory and in note book all the facts observed.

No one is wholly free from the dangers of defective observation. But one who acquires the habit of analyzing his observational powers and processes is less exposed to these dangers than are those who give no conscious attention to this initial step of a scientific investigation. In research, as in other things, to be forewarned is to be forearmed; and the investigator who is conscious of the dangers inevitably associated with the observational process will be on his guard against those dangers.

II. ANALYSIS IN THE STAGE OF CLASSIFICATION

Is there any need for employing analysis in the second stage of an investigation, the stage of classification? Let us note first that the analysis involved in stage one led to a sort of rudimentary classification, the separation of observed facts into those which were relevant and those of doubtful relevancy. We at once suspect that analysis must likewise be involved in any further effective classification of the facts of observation surviving the analysis of stage one.

The investigator is not content merely to exclude from consideration every doubtful bit of data. For example, our hypothetical student does not rest when he has excluded from his study every topographic form which does not certainly belong in the group of elevated marine benches. He raises the question as to whether all these benches need necessarily have had the same history. For he takes cognizance of the fact that if the benches were carved at different times, and some of them were affected by events, such as continental uplift, which did not affect the others, a serious difficulty may be introduced into the attempt to explain their history. The investigation, to be successful, must deal with a group of facts which are comparable. A simple hypothesis competent to explain benches five feet above high tide may conceivably be quite incompetent to explain benches fifteen feet above high tide.

III. Analysis in the Stage of Generalization

Classification paves the way to the third stage of investigation, in which generalization occurs. Thus in our hypothetical study of marine benches, it is only when classification has been effected that the investigator can make significant generalizations; such, for example, as that in one group the benches have a narrow range in altitude, are always close to the sea, present no evidence of weathering of the rock surfaces, and are free from debris; that in a second group the benches have higher altitudes but equally narrow range in altitude, present evidence of moderate weathering and have small quantities of debris near their inner margins; while in a third group the benches are still higher, vary extensively in altitude and have surfaces which are deeply weathered and prevailingly covered with debris.

We have now to inquire whether the analytical faculties of the investigator can serve any useful purpose in the stage of generalization. Let us first note that the empirical generalizations stated above, free from any explanatory element, had their roots in the careful analysis that made effective classification possible. But it is when inducing generalizations which involve more or less of explanation that the analytical process can render greatest service. Such generalizations, like those free from explanation, must be rooted in the facts and represent a normal outgrowth of them. They must be legitimate inferences induced from the facts themselves. As we shall see in a later section, the investigator may formulate conceptions involving explanation which are not an immediate normal outgrowth of the facts, but the products of invention. As these two types of explanation, the induced and the invented, have somewhat different standing before the court of the intellect, it is important that they be not confused. It is in making this discrimination that the investigator must again employ his analytical powers. The nature of the facts, the nature of their distribution, their relationships, and other pertinent elements must critically be examined, to the end that only legitimate inferences may be induced from them.

For example, the first two groups of classified facts described above, in both of which uniformity of altitude above sea level is a characteristic of the benches, may properly give rise to the generalization that after the benches were carved the sea level dropped. Uniformity of bench altitude suggests this as a legitimate inference involving explanation. But no such generalization may be based on the third group of facts, in which great variation in elevation is an outstanding characteristic of the benches. Such heterogeneity in elevation does not suggest either uniform drop of sea level or systematic differential uplift of the land. Perhaps the facts can be explained on the basis of one or the other of these explanations, or on both combined; but if so the explanations must be deliberately invented and applied to the facts. They are not normal outgrowths of the facts.

IV. Analysis in the Stage of Invention

In the fourth stage of our hypothetical investigation the inquirer takes the classified facts and the generalizations concerning their nature, and uses them as a basis for invention of as many explanations of the facts as may be possible. It is here that "the scientific use of the imagination," as Tyndall has happily phrased it, comes most prominently into play. The invention may be deliberate, the result of conscious effort. Often it springs unexpectedly into consciousness, the result of a mind well equipped with pertinent knowledge repeatedly "mulling over" the facts in variable combinations. If others have anticipated our inquirer in the inventive process, as is usually true in greater or less measure, he welcomes every idea of alien origin which offers a possible explanation of the facts, and accords it just as hospitable treatment as those born of his own intellect. If the generalizations of stage three involved explanation of the facts, the investigator seeks additional and independent explanations. Where generalization involved but partial explanation, or merely paved the way for an explanation, the investigator employs his inventive powers to complete the unfinished task; and then moves forward to the invention of alternative explanations.

The process of invention is not fully understood. It resembles induction to the extent that the mind starts with concrete facts, and from them passes to conceptions of broad application which take the form of tentative explanations of the facts. But the mind has far greater liberty in the stage of invention than in the preceding stage of generalization. The relation to facts is here less close. A proper generalization, being an outgrowth of the facts, must have its roots well grounded in them. An invention may spring from "thin air," the rarefied atmosphere of more abstract reasoning. The stimulus to invention comes from the facts, and the thing invented must not palpably be contradicted by the facts; but it need not be a normal outgrowth of the facts. Thus, the conception of a uniform drop of sea level was, as we have seen, wholly improper as a generalization based on the third group of facts, relating to marine benches of widely varying altitude, since nothing in this group of facts could rightly give birth to such a conception. But the same conception may properly be invented, "out of thin air," and offered as a possible explanation of the facts in that group, since none of them necessarily contradicts the explanation.

We have noted the greater liberty enjoyed by the mind in the stage of invention. But it is not wholly free. Since the explanations induced in stage three and those invented in stage four are to serve as working hypotheses in the deductive operations of stage five, they must conform to certain fundamental requirements governing the formulation of hypotheses. In the first place, it is essential that the tentative explanations should be so precisely molded that specific deductions may be derived from them. If an explanation is so vague in its inherent nature, or so unskilfully molded in its formulation, that specific deductions subject to empirical verification or refutation can not be based upon it, then it can never serve as a working hypothesis. A hypothesis with which one can not work is not a working hypothesis.

Again, the explanation as formulated must be possible, for explanations clearly contradicted by well-established natural laws waste time and energy without advancing the investigator toward the true goal of his researches. On the other hand, it is manifestly dangerous to exclude explanations which merely appear incredible or absurd because they run counter to established opinions. Many a door to truth has thus prematurely been closed and long remained closed. The hypothesis of continental glaciation seemed incredible in 1840, as did the hypothesis of evolution in 1860.

How then shall the inventive mind properly mold its hypotheses and distinguish between explanations which appear plausible but are really unsound and those which immediately provoke distrust yet merit hospitable consideration? It seems to me that here the analytical process offers us an instrument of incalculable value. Let us dissect each tentative explanation into its component elements, trace each element back to the assumptions and inferences which lie hidden behind it and test the reasonableness of the whole by testing the validity of each part.

V. ANALYSIS IN THE STAGE OF VERIFICATION AND ELIMINATION

On entering the fifth stage of the investigation our hypothetical inquirer possesses, let us suppose, some half dozen tentative explanations which have survived the analytical processes applied in the preceding stage. He now treats each of these in turn as a working hypothesis, and reversing the mental processes employed in the third stage, endeavors by deductive reasoning to determine as precisely as possible just what features should characterize the shore platforms in case the particular hypothesis for the moment under scrutiny be the true explanation of these forms. His object is to verify, so far as possible, the competence of some hypotheses and to eliminate others which critical tests show to be manifestly incompetent.

He secures the required tests by an appeal to facts. After deducing the reasonable consequences of a working hypothesis, he confronts these by such facts as are already in his possession, drawing upon memory, upon his field notes and upon the published observations of others. If the consequences logically deduced from the hypothesis are in accord with the facts, he accepts this as verification of the competence of the hypothesis to explain the facts already in hand. Where deduced consequences find no counterpart in observed facts, he feels justified in eliminating the hypothesis from further consideration.

To what extent, if any, can analysis serve the investigator in this stage of verification and elimination? Since effective use of the deductive process depends upon one's ability to make logically correct deductions, it is expedient that the investigator should subject his own mental processes to careful scrutiny. There are fallacies in reasoning which must be avoided. These are treated in works on logic and need not be discussed here. But some study of logic will direct conscious attention to one's mental habits, and help to secure the precision and completeness requisite for careful deduction.

The investigator must realize that if his deductions are false, it matters little whether observed facts correspond to them or not. The conclusions are in any case invalidated, and he may either reject a perfectly valid hypothesis or embrace one which is invalid, on the basis of false reasoning. Saftey lies only in analysis. Deductions, like inductions, must be separated into their component parts, and each part scrutinized and its validity tested. Let us illustrate what this involves by turning once again to the problem of the shore platforms.

Let us suppose that our investigator has accepted as one working hypothesis the tentative explanation of storm-wave erosion under present conditions of land and sea level. From this hypothesis he deduces certain expectable consequences, among which let us consider only one. If the platforms are the product of storm waves operating at the present time, he reasons, then there should be found upon their surfaces rock debris constituting the cutting tools with which waves accomplish their erosive work. Turning to his record of field observations, he finds that many of the platforms are remarkably free from debris. Indeed, their outer edges commonly drop abruptly into moderately deep water, so that it is difficult to see how much debris could be cast upon the platforms or maintained there. The facts in hand conspicuously fail to match the expectations deduced from the hypothesis, so our investigator rejects the hypothesis of storm-wave erosion as incompetent. In doing so, he rejects a perfectly valid hypothesis solely because he failed to examine his deductions critically before using them as tests of the competence of the hypothesis.

Let us suppose, however, that our investigator was so fully convinced of the value of analysis as an instrument of research that he would neither accept nor reject a hypothesis until he had subjected each deduction derived from it, and each test of its correspondence with the facts, to such rigid scrutiny as he was able to make. Under these circumstances he would first have examined the deduction relating to the expected presence of wave-cutting tools on the platforms. He would have asked: Upon what does the deduction rest? Evidently upon the assumption that waves of water unarmed with debris can not effectively erode the coast. And on what, in turn, does this assumption rest? Current geologic opinion may run this way, and geologic text books may assert that waves without cutting tools are feeble. But current opinion and the text books have often proved fallible guides. So our investigator would have gone back to the sources on which both opinion and text-book should be based; the actual recorded evidence as to the nature and causes of damage accomplished by waves. Here he would have found much information concerning havoc wrought by waves under conditions which seem clearly to preclude the effective intervention of debris. The terrific impact of the water itself, the force of the currents it generates, the direct pressures exerted upon air and water imprisoned in crevices in the rock, the sudden expansion of air in crevices and pore spaces when the rapid retreat of a wave creates a partial vacuum outside, all these are described as effective causes of damage by waves, on the basis of concrete and seemingly reliable evidence. Our investigator must then have concluded that while shore debris is presumably a highly important factor in wave erosion, there is no reason to exclude the possibility that waves relatively free from debris, and striking with a force varying from hundreds to thousands of pounds per square foot, may accomplish much erosive work. He would therefore have rejected, not the hypothesis of storm-wave erosion, but his tentative deduction respecting the necessity of finding debris on the platforms; and would have looked for other deductions from the same hypothesis which he could use as a more reliable basis for testing its validity.

VI. Analysis in the Stage of Confirmation and Revision

From the fifth stage of the investigation the inquirer should emerge with a much depleted stock of working hypotheses. In some cases only one will remain as apparently competent to explain all the recorded facts. In other cases two or more may survive thus far. If but one, the cautious investigator will seek confirmation of its validity before utilizing it in his ultimate interpretation of the facts. If more than one, he pushes his studies further to discover which one represents the true explanation of the facts or whether they are jointly responsible for a complex result.

Whether one or several hypotheses survive the tests applied in the preceding stage, the survival is almost always in an "unfinished" state. The tests remain incomplete, and therefore inconclusive, because not all the desired facts are available, and perhaps not all the desirable deductions have been made. The investigator, let us imagine, has deduced five reasonable expectations from a certain hypothesis, of which three only are matched by facts in his possession. On the other hand, he has five categories of facts, of which three only correspond to expectations thus far deduced. He has two deductions having no counterpart in observed facts, and two groups of facts which are disturbing because not related to anything thus far deduced from the hypothesis. Just what these discrepancies mean, he does not know. They may mean that observation was incomplete and that new facts remain to be discovered. They may mean that deduction was incomplete or imperfect. In either of these two cases the hypothesis may be valid as it stands. But there are other possibilities. The discrepancies may mean that the hypothesis is invalid and must ultimately be rejected; or they may mean that it only requires a certain amount of revision to bring it into harmony with all the facts.

It is the task of the investigator in the stage of confirmation and revision to find out just what the discrepancies do mean. He searches for the missing facts which, if found, will constitute logical proof of the validity of the generalization involved in the hypothesis. Here the extraordinary value of the deductive processes of stage five becomes apparent. The observation of stage one was a wandering, unguided observation. But the renewed observation of stage six is skilfully directed observation. He now knows what to look for, and hence how and where to look. If, like the geologist or geographer, he is dealing with facts to be found in the field, directed observation will materially shorten his field work and greatly increase its fruitfulness. He goes directly to the places where the new facts should be found. If, like the physicist, chemist or modern student of biology, he must discover new facts through experimentation, skilfully directed experiment will replace the vague gropings of a less advanced stage of research. The deductive process has added new interest to the quest for truth, and has enormously increased the probabilities of finding it.

Directed observation has another great value. It is apt to lead to the discovery of facts wholly unanticipated by the deductions which direct the search. As Davis has well said, the investigator's outsight on the facts is sharpened by his insight into the nature of his problem. Things which formerly made no impression on eye or mind now instantly arrest his attention, some because they are expected, but others for the very reason that they are quite unexpected. The discovery of facts that are wholly unanticipated, but which none the less find full explanation in a hypothesis under investigation, has exceptionally high value as confirmation of the hypothesis.

Where none of the deductions of stage five fully account for certain of the facts already in hand; or where newly discovered facts fail to find any counterpart in previous deductions; or, again, where some of the facts which should occur, according to a hypothesis otherwise well substantiated, can not be found, revision of the hypothesis is strongly indicated. Lack of completeness in deduction is especially apt to cause difficulty, and proper revision to include some new consequences not previously foreseen may bring complete harmony out of partial discord.

The process of securing the requisite confirmation, revision or rejection of hypotheses makes new and heavy demands upon the critical faculties. Especially is this true of the process of revision and rejection, which carries the investigator back over previous stages of the investigation in a search for some weak link in his chain of reasoning, some factor overlooked, or some item improperly included. If, as we have seen, analysis had a rôle to play in each of these preceding stages, how much more important is it that the analytical faculties should be on the alert in a review of one's reasoning in search for error. If previous work has been carefully performed, the error is not apt to be obvious. More likely it is obscurely involved in some apparently sound procedure, in which case it will continue to escape detection unless each bit of evidence, each argument and each conclusion are separated into their component parts and subjected to critical scrutiny. It is such analysis as this which must, in the circumstances detailed above, determine whether certain apparently incongruous facts should be rejected as irrelevant to the problem or included as fully pertinent; and in the latter case, whether a given hypothesis must be rejected or merely revised, before deduction and observation are brought into harmony.

VII. ANALYSIS IN THE STAGE OF INTERPRETATION

Let us suppose that the investigator has reached the seventh and final stage of his labors, in which he proceeds to formulate for publication his interpretation of the origin of shore platforms. Can he at last dispense with analysis, the instrument which has served him so well at every previous step in his labors?

Not wholly, I think. In any event there must remain with him a caution inspired by his full understanding of the nature of the proofs with which he is dealing, an understanding which comes only from critical analysis of both evidence and arguments. Rarely can geologist or geographer offer mathematical demonstration of the truth of his conclusions. He can, as a rule, go no further than to show that a given proposition has a high degree of probability. Certain facts carefully observed and properly classified prompt the invention of various hypotheses. The deduced consequences of one of these hypotheses alone is matched by all the facts, including some which are newly discovered and highly peculiar. The investigator therefore has much confidence that an interpretation based on this hypothesis will be valid. But he realizes there is still opportunity for error. He may not have thought of all possible hypotheses. He may not have deduced all the reasonable consequences of those hypotheses considered. He may not have discovered all pertinent facts. Some future contribution to knowledge may throw unexpected light on his problem and radically affect his conclusions. He therefore wisely regards his present interpretation as highly probable theory, rather than as demonstrated fact.

In previous stages an error led only one investigator astray, and that one could retrace his steps and repair damage by embarking on some new line of reasoning. But in this final stage an error committed to the printed page may lead many astray and may show wholly unexpected vitality and power for evil. The printed statements must go far enough; but they must not go farther than the evidence and fully tested reasoning warrant. A given interpretation must not be applied to all shore platforms before it has been completely demonstrated that there are no such platforms for which it is not the most satisfactory explanation. It must not even be asserted as the sole cause of any platforms, unless and until it has been clearly proven that no contributing cause has played a part in their formation.

In these and other ways the investigator must be on his guard against overstatement, understatement, erroneous statement of his results. Analysis is the weapon with which he defends himself against too broad generalization and other errors which are wont to intrude themselves into the final stage of an investigation. Each sentence he scrutinizes, each conclusion he dissects, checking here and verifying there, to the end that the formulation of his interpretation shall contain no less, and particularly no more, than critically observed facts and carefully tested reasoning may justify. An interpretation thus cautiously reached and conservatively formulated will surely command the serious consideration of scientific men.

CONCLUSION

Have we, in the method of multiple working hypotheses, applied with the aid of rigorous analysis something which will guide us unfailingly to the discovery of truth? We are compelled to answer this question in the negative. No device, however perfect, can wholly deprive the human intellect of its capacity for making mistakes. De Leon searched in vain for the fountain of youth. Can we hope for a magical fountain of truth?

The most for which we may reasonably hope is by correct methods of research to reduce the chances of error to a minimum and to raise to its maximum the probability of discovering the real causes and relations of things. This we have done, so far as lies within our power, when we are accurate in observing facts, careful in classifying them, cautious in generalizing from them, fertile in inventing hypotheses, ingenious and impartial in testing their validity, skilful in securing their confirmation or revision and judicial in formulating ultimate interpretations.

Multiple working hypotheses as a method, employed in connection with critical analysis as an instrument of precision, offer us, in my opinion, the best guarantee of success in scientific research.

SCIENTIFIC EVENTS

UNIFICATION OF RADIO RESEARCH FACILITIES IN GREAT BRITAIN

THE facilities for radio research carried out by the British Department of Scientific and Industrial Research on the advice of their Radio Research Board have been improved by the unification of the Wireless Division of the National Physical Laboratory and the department's Radio Research Station at Slough into a new radio department of the National Physical Laboratory. Mr. R. A. Watson Watt, hitherto the superintendent of the Radio Research Station, is the superintendent of the new department.

Under the new arrangements the Radio Research Board continues to be appointed as at present by the lord president of the council, and its constitution and functions remain unchanged except that the opportunity has been taken to remove the anomaly by which the general work on the maintenance of radio standards at the National Physical Laboratory was a responsibility of the board. In the future the executive committee of the National Physical Laboratory will assume direct responsibility for these standards in the same way as it assumes responsibility for other national standards.

On the formation of the Radio Research Board in 1920, the National Physical Laboratory was entrusted with all work which required a laboratory equipped with instruments of the highest precision. Such work included the development of radio frequency standards, the study of problems of selectivity, aerial arrays and the generation of extremely short waves, as well as methods for the measurement of fundamental quantities necessary in accurate circuit design. Owing, however, to its situation and the proximity of other electrical work, the National Physical Laboratory was considered unsuitable for the conduct of radio research work requiring measurements in the field or on isolated sites. For work of this character facilities were provided by the department at the Radio Research Station erected on land adjoining the Admiralty Compass Observatory at Ditton Park, near Slough. Practically the whole work carried out on behalf of the Radio Research Board was thus divided between the Radio Research Station, Slough, and the National Physical Laboratory.

Although the Radio Research Station and the National Physical Laboratory have closely cooperated in the past, the unification which has taken place gives the Radio Research Board much greater freedom in planning its program as a single unit. The amalgamation of the staff of the two sections under a single direction is in the interests of efficiency and economy, and enables the increasing number of inquiries from industry to be made to one single establishment.

THE ANNUAL MEETING OF THE ROYAL INSTITUTION

THE annual meeting of the members of the Royal Institution was held on May 1, the president, Lord Eustace Percy, being in the chair. According to the report in *Nature* the annual report of visitors for the year ended December 31, 1932, testified to a year of considerable activity. The membership had been well maintained. The privilege of free attendance at the afternoon lectures by *bona-fide* students in London had been much appreciated and used. The report on the progress of the researches in the Davy Faraday Laboratory gave a good indication of the considerable extent of the research organization which is at work under the direction of Sir William Bragg. Some sixteen or eighteen workers are engaged, the majority on problems related to the x-ray determination of structure. Mention was made of Dr. J. M. Robertson's determination of the structure of anthracene, of Dr. A. Müller's work on the long-chain compounds, and of the growth in accuracy and capacity of the