

SCIENCE

FRIDAY, SEPTEMBER 15, 1911

THE BRITISH ASSOCIATION FOR THE
ADVANCEMENT OF SCIENCE

THE CHARACTERISTICS OF THE
OBSERVATIONAL SCIENCES¹

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It will doubtless startle my audience to hear that this section has only once in its history been addressed by an astronomical president upon an astronomical topic. I hasten to admit that I am not using the term astronomical in its widest sense. Huxley once declared that there were only two sciences, astronomy and biology, and it is recorded that "the company" (which happened to be that of the Royal Astronomical Society Club) "agreed with him." One may agree with the company in assenting to the proposition in the sense in which it is obviously intended without losing the right to use the name astronomy in a more restricted sense when necessary; and at present I use it in its classical sense. At Brighton, in 1872, Dr. De La Rue addressed Section A on "Astronomical Photography" in words which are still worthy of attention, though they are all but forty years old; and this is the only instance I can find in the annals of the section. There have, of course, been occasional astronomical presidents such as Airy, Lord Rosse and Dr. Robinson, but these presided in early days before the address existed, or when it was brief and formal; and the only allusions to astronomical matters were the statements, by Robinson and Airy, of what the association had done in subsidizing the reduction of Lalande's observations and the Greenwich lunar observations. In 1887 Sir Robert Ball occupied this chair, but he

¹Address of the president to the Mathematical and Physical Section. Portsmouth, 1911.

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selected from his ample scientific wardrobe the costume of a geometer, and left his astronomical dress at home. A great man whose death was announced almost as I was writing these words, Dr. Johnstone Stoney, spoke (in 1879 at Sheffield) of the valuable training afforded by the study of mechanics and of chemistry, with that keen insight which made him so valuable a member of our section. Other presidents whom we have been glad to welcome as astronomers at certain times and seasons did not choose the occasion of their presidency for any very definite manifestation of astronomical sympathy.

The addresses of Sir George Darwin (in 1886) and of Professor Love (in 1907) on the past history of our earth certainly have an astronomical bearing, but if we distinguish between the classical astronomy and its modern expansions they would be assigned to the latter rather than to the former; and so do the few astronomical allusions in Professor Schuster's address at Edinburgh in 1892. Even if we include, instead of excluding, all doubtful cases, there will still appear a curious neglect of astronomy by Section A in the last half century, all the more curious when it is remarked that the neglect does not extend to the association itself, seeing that there have been three astronomical presidents of the association who had not been previously chosen to fill this chair. The neglect is not confined to astronomy, but extends, as some of us recently pointed out, to the other sciences of observation; and we thought that, as a corollary, it would be better for the section to divide, in order that these sciences might not continue the struggle for existence in an atmosphere to which they were apparently ill-suited. But the section decided against the suggestion, and I have no intention of appealing against the decision. This explicit statement will, I trust, suffice

to prevent misunderstanding if I proceed to examine the possible causes of neglect—for I can not but regard the record as significant of some cause which it will be well to recognize even if we can not remove it. Personally I think the cause is not far to seek, and my hope is to make it manifest; but as the statement of it involves something in the nature of an accusation, I will beg leave to make it as gently as possible by using the words of others, especially of those against whom the mild accusation is to be made.

Let me begin by quoting from the admirable address—none the less admirable because it was only one quarter of the length to which we have become accustomed—delivered by my late Oxford colleague, the Rev. Bartholomew Price, at Oxford in 1860, wherein he referred to the constitution of this section as follows:

The area of scientific research which this section covers is very large, larger perhaps than that of any other; and its subjects vary so much that while to some of those who frequent this room certain papers may appear dull, yet to others they will be full of interest. Some of them possess, probably in the highest degree attainable by the human intellect, the characteristics of perfect and necessary science; while others are at present little more than a conglomeration of observations, made indeed with infinite skill and perseverance, and of the greatest value: capable probably in time of greater perfection, nay, perhaps of the most perfect forms, but as yet in their infancy, scarcely indicating the process by which that maturity will be arrived at and containing hardly the barest outline of their ultimate laws.

A little later in the address Professor Price made it quite clear which were the sciences "in their infancy."

And finally we come to the facts of meteorology and its kindred subjects, many of which are scarcely yet brought within any law at all.

There is here much that will command ready and universal assent; but is there not also a rather unnecessary social scale? The

science of planetary movement had not yet been "brought within any law at all" (as we now use the term) in Tycho Brahé's time; but was the astronomy of Tycho Brahé socially inferior to that of Kepler? It is difficult to fix the eye on such a question without its being caught by the splendor of Newton towering so near; and the idea of a scale descending from that great height is almost irresistibly suggested. But in spite of this grave difficulty, I ask whether there is of necessity any drop whatever from the plane of Kepler, who realized the laws, to that of Tycho, who never reached any suspicion of the true laws, but had, nevertheless, such faith in their existence that he cheerfully devoted his life to labors of which he never reaped the fruits? Is it not a dangerous doctrine that the work done previous to the formulation of a law is in any way inferior? Take the case of a man like Stephen Groombridge, who made thousands of accurate observations of stars in the early part of last century. Fifty years later something of the value of his work began to emerge from a comparison with later observations which showed what stars had moved and how; but it was not until nearly a century had elapsed that something about the laws of stellar movement was extracted from his patient work, combined with a repetition of similar works at Greenwich. Then, with the skilful assistance of Mr. Dyson and Mr. Eddington, Groombridge at last came into the fruits of his labors; but had he been asked during his lifetime for credentials in the shape of laws, on pain of being classed as an inferior in the social scientific scale, he would have been lamentably unprepared. Or consider the case of M. Teisserenc de Bort, when he began sending up his balloons. "Show me your laws," cries the mathematician. "But they are just what I hope to find," replies M. de

Bort. "Yes, but surely you have formulated some law you wish to test?" pursues the invigilator. "How am I to give you proper scientific rank unless you can produce at least a tentative law?" "On the other hand, I wish to keep a perfectly open mind," maintains M. de Bort. "Then I fear I can not admit you to our class at present; you must join the infants' class, and I can only give you my best wishes that you may reach maturity some day." Unperturbed, M. de Bort continues to send up his balloons, and almost immediately discovers the great fact about the isothermal region which will be a permanent factor in the meteorology of the future. The mathematician is now ready to admit him, as a worthy person who has found a law about the constitution of the atmosphere. But was not the merit in sending up the balloons whatever came of it? Is it not sometimes more courageous to take risks of failure? The mathematician, safe in his stronghold which possesses "probably in the highest degree attainable by the human intellect the characteristics of perfect and necessary science" is like a man who has inherited a good old-established business, and he has a distaste for the methods of those who have to try new ventures. No doubt many who make such trials fail; but, on the other hand, great fortunes have been made in that way.

It may seem, however, that too much is being deduced from a single quoted opinion, which may easily have been personal and not representative. Let me, therefore, take another which presents a different aspect of the same matter. I take the opening words of Sir G. H. Darwin's address to this section at Birmingham in 1886.

A mere catalogue of facts, however well arranged, has never led to any important scientific generalization. For in any subjects the facts are so numerous and many-sided that they only lead

us to a conclusion when they are marshaled by the light of some leading idea. A theory is then a necessity for the advance of science, and we may regard it as the branch of a living tree, of which facts are the nourishment.

Those who have read the letters of Charles Darwin will recognize that this opinion was also held by the father, and may have been adopted by the son. It is no part of my purpose to raise any question of originality: I mention the point merely to take the opportunity it gives me of showing that I do not approach lightly an opinion held by two such men. With the utmost respect I wish to question whether the criterion indicated goes deep enough. Often have we had ocular demonstration of the value of a theory in stimulating the advance of science, but is advance wholly dependent on the existence of a theory? I have tried to indicate already a deeper motive power by such instances as the work of Tycho, who had no theory, but who perceived the need of observation. And I will now definitely formulate the view that the perception of the need for observations, the faith that something will come of them, and the skill and energy to act on that faith—that these qualities, all of which are possessed by any observer worthy the name, have at least as much to do with the advance of science as the formulation of a theory, even of a correct theory. The work of the observer is often forgotten—it lies at the root of the plant; it is easier to notice the theories which blossom and ultimately produce the fruit. But without the patient work of the observer underground there would be neither blossom nor fruit. It is also easy to fix attention on the mechanical nature of much observation; but this is not the principal feature of observing any more than is numerical computation of mathematics. There are men like Adams who perform gigantic numerical computa-

tions faultlessly, but there are others who would take equal rank as mathematicians who can not do three additions correctly; and again others who could compute well and quickly but prefer to hand over that part of their work to some one else. Similarly some great observers themselves look through the telescope, and some merely direct others how to do so; the spark of divine fire is not dependent on this detail, but on the possession of the qualities above mentioned—perception, faith, skill and energy.

By way of bringing out more fully the nature of the assertion made by Sir George Darwin, let me beg your attention to a striking incident in recent astronomical history. We all know how the great astronomer we lost last year, Sir William Huggins (one of those already mentioned as having occupied the presidential chair of the association without having filled that of Section A), initiated the determination of velocities of the heavenly bodies in the line of sight by means of the spectroscope. We know further how the accuracy of these determinations was improved by the application of photography, so that it has recently become possible to measure the velocity of the earth in its orbit (as it alternately approaches and recedes from a given star) with a precision which matches that of other known methods. Now Mr. W. W. Campbell, on his appointment as director of the Lick Observatory in 1900, perceived the desirability of observing the line of sight velocities of as many stars as possible, believed that that outcome would be in some way for the advancement of science, and resolutely acted on that belief, so that for many years the resources of his great establishment have been devoted to this work. He has not turned aside from it even to publish provisional results, and has thereby incurred some adverse criticism.

But, having now accumulated a large mass of observation, he is proceeding to let them tell their own tale, and a wonderful story it is. We have, unfortunately, not time to listen to more than a fraction of it at the moment; but that fraction is well worthy of our attention. When the stars are grouped in classes according to their spectral type, their average velocities differ; and if the spectral types are arranged in that particular order which for quite independent reasons we believe to be that of development of the stars, there is a steady increase in the velocities. To put the matter in a nutshell, the older a star is the quicker it moves. There are, no doubt, several assumptions made in reducing the matter to this simple statement, but I venture to think that they do not affect the point I now wish to make, which is as follows. There is no doubt whatever that the catalogue of facts accumulated by Mr. Campbell, when arranged in an obvious order, has led to a most important scientific generalization—a direct negative at this date of Sir George Darwin's opening sentence, however true it may have been when he wrote it. If we read on, his next sentence doubtless entitles him to say that it was the marshaling of the facts which led to the conclusion. It is not altogether clear to me in what way this marshaling differs from the permitted "arrangement" of the catalogue; but the third sentence seems to imply that the distinction lies in the existence of a theory. But certainly Mr. Campbell had no theory; so far is he from having had a theory that he finds it extremely difficult, if not at present actually impossible, to formulate one, which will satisfactorily account for the extraordinary fact brought to light by the simple arrangement of his catalogue.

Witness his words in Lick Observatory "Bulletin," No. 196, dated April 20 last:

The correct interpretation of the observed facts referred to in this "Bulletin" seems not easy of accomplishment, and the brief comments which follow make no pretensions to the status of a solution.

That stellar velocities should be functions of spectral types is one of the surprising results of recent studies in stellar motions, for we naturally think of all matter as equally old gravitationally. Why should not the materials composing a nebula or a Class B star have been acted upon as long and as effectively as the materials in a Class M star? . . . The established fact of increasing stellar velocities with increasing ages suggests the questions: Are stellar materials in the ante-stellar state subject to Newton's law of gravitation? Do these materials exist in forms so finely divided that repulsion under radiation pressure more or less closely balances gravitational attraction? Does gravity become effective only after the processes of combination are well under way?

Mr. Campbell is far from being helpless in the situation he has created; he is ready with suggestions, though he modestly puts them as questions; but they are obviously consequent, and not antecedent, to the advance which he has made. Even if the like has never happened before, *this* scientific advance is at any rate due to little more than the accumulation of facts which arranged themselves, as Bacon hoped would naturally happen. But does it detract from the merits of this fine piece of observational work that it was suggested by no leading theory? And I will ask even further: Would its merits have been less if no such immediate induction had presented itself? To this second question I can scarcely expect a general answer in the affirmative; it is so natural to judge by results, and so difficult to look beyond them to the merits of the work itself that I shall not easily carry others with me in claiming that the merits of the observer shall be assessed independently of his results. And yet I affirm unhesitatingly that until this attitude is reached we can not do justice to the observer. I believe it will be reached

in the future, and I shall endeavor to give reasons for this forecast; but I admit frankly that our habit of judging by results will be hard to break. It extends even to the observer himself, and leads to the withholding of his observations from publication, so that he may himself extract the results from them. In the pure interests of the advance of knowledge, it would be far better to publish the material, so that many brains rather than one might work upon it. But the observer knows that by this course he risks losing almost the whole value of his patient work, which would pass as unearned increment to the particular person who was lucky enough to make the induction. Hence arise quarrels such as those between Flamsteed and Newton; the former refusing to publish his observations until he had himself had an opportunity of discussing them, while Newton and Halley exerted their powerful influence in the contrary sense. This situation by no means belongs to a bygone age; it may and does arise to-day, and will continue to arise so long as the recognition of the observer's work is inadequate. It was mentioned a few minutes ago that Mr. Campbell had incurred adverse criticism by accumulating a considerable mass of unpublished observations. Let me be careful not to suggest that his primary motive was the desire to have the first use of them, for I happen to know that there was at least one other good and sufficient reason for his action in the difficulty of finding funds for publication, a difficulty with which observers are only too familiar. But whatever the reason, there were those who regretted the delay in publication as hindering the advance of science. The whole question is a delicate one, and might have been better left unraised at the moment but for a most curious sequel, which puts clearly in evidence the importance of the observer and the desira-

bility of allowing him to discuss his own work. To make this clear a small digression is necessary.

During the last half-dozen years astronomers have been startled on several occasions by pieces of news of a particular kind, indicating the association of large, widely scattered groups of stars in a common movement. The discussion of these movements is to occupy the special attention of this section at one of our meetings, which is an additional reason for brevity in the present allusion. Possibly also most members of the section have already heard of Professor Kapteyn's division of the great mass of bright stars into two distinct groups flying one through the other; and again of the discovery by Professor Boss of a special cluster of stars in the constellation Taurus, moving in parallel lines like a flock of migrating birds. The fascination of this latter discovery, and of one or two others like it, is that when the information supplied by the spectroscope is combined with that furnished by the long watching of patient observers, we can determine the distance of the cluster and its shape and dimensions. We realize, for instance, that there is a large flat cluster migrating just over our heads, so that one member of it (Sirius) is close to our sun—that is to say, only three or four light-years from him. "Close" is a relative term; and the distance traveled by light in three years is from some standpoints by no means despicable. But it is small in comparison with the dimensions of the cluster, which is about one hundred light-years from end to end. The study of these clusters will doubtless occupy our close attention in the immediate future; and it is very natural than the discovery of one should lead to the search for others. Accordingly, we heard last autumn with the deepest interest, but with modified surprise, the an-

nouncement of common movement in a class of stars of a particular spectral type. The announcement rested to some extent on the work done at the Lick Observatory, much of which has been published in an abbreviated form. But Mr. Campbell, in the Lick Observatory "Bulletin" already quoted, gives reasons why he can not accept the conclusion, which is vitiated, in his opinion, by the existence of a systematic error in the observations. Now on such a point as this the observer himself is at any rate entitled to a hearing, and is often the best judge. To take proper precautions against systematic errors is the business of the observer, and his efficiency may very well be estimated by his success in this direction—this would be a far safer guide than to judge by results. But sometimes such errors, which are very elusive, do not suggest themselves until the observations have been completed, and must be detected from the observations themselves. This, again, is rightly the business of the observer, and the desire to free his observations from such error is a perfectly sound and scientific reason for withholding publication. In the present instance the error is a peculiarly insidious one; and, indeed, we are not even certain that it is an error. It is a possible alternative interpretation of the facts that the stars with Class B spectrum are in general moving outwards from the sun, and the additional fact that there is a comparatively large volume of space round the sun at present empty of B stars would seem to favor this alternative. But, as already mentioned, the observer himself prefers rather to credit his observations with systematic error which gives a spurious velocity of 5 km. per second to stars of this type. Now it will readily be understood how an error of this kind may appear doubled: two vehicles traveling in opposite directions approach or recede from one

another with double the speed of either; and if one were erroneously supposed to be at rest, the other would be judged to travel twice as fast. In this way the B stars in a particular portion of the sky were judged to be traveling with a common motion of 10 km. per second, which would have been a discovery of far-reaching importance if true, but which the observer relegates to the category of systematic errors.

The illustration will suffice to remind us that the work of the observer is far from being merely mechanical: it demands also skill and judgment—skill in defeating systematic error, and a fine judgment, born of experience, of the success attained. All this is independent of the generalizations which may or may not be arrived at. Bradley's skill as an observer enabled him to discover the aberration of light and the nutation of the earth's axis; it was enhanced rather than lessened when he went on to make further observations which, had he lived, would have conducted him to the discovery of the variation of latitude. After his death the world waited more than a century for this discovery to be made, but Mr. Chandler, who played a leading part in it, has declared that Bradley was almost certainly on its track. It would almost seem that an observer is only properly appreciated by another observer. There are doubtless many who, assisted by the knowledge that Bradley's skill had twice previously conducted him to a discovery, would be ready to admit the value of his later work, although he did not live to crown it; but how many of these could properly appreciate Bradley without such assistance?

I venture to think that the great brilliance of Newton has dazzled our vision so that we do not see some things quite clearly.

Had it not been for Newton [writes De Morgan in his "Budget of Paradoxes," p. 56] the whole dynasty of Greenwich astronomers, from Flamsteed of happy memory, to Airy, whom Heaven preserve, might have worked away at nightly observation and daily reduction without any remarkable result: looking forward, as to a millennium, to the time when any man of moderate intelligence was to see the whole explanation. What are large collections of facts for? To make theories *from*, says Bacon; to try ready-made theories *by*, says the history of discovery; it's all the same, says the idolater; nonsense, say we!

But nothing of this will fit in with what we know of Bradley's work; he discovered aberration, not by any help from Newton, but by accumulating a mass of observations. He had no ready-made hypothesis, or rather he had a wrong one, viz., that the stars would show displacement due to parallax; and after this was proved wrong, as it was at the very outset, he had nothing in the way of a theory to guide him, and found great difficulty in devising one *after* he had collected his facts, which spoke for themselves so far as to reveal plainly the essential features of the phenomenon in question.

Modern discoveries (on the preceding page of the "B. of P.") have not been made by large collections of facts, with subsequent discussion, separation, and resulting deduction of a truth thus rendered perceptible.

To this I venture to oppose not only such work as that of Bradley, but much in the recent history of astronomy; the discoveries about systematic proper motions, about moving clusters, about the growth of velocity with life history, and so forth.

There is an attempt at induction going on, which has yielded little or no fruit, the observations made in the meteorological observatories. The attempt is carried on in a manner which would have caused Bacon to dance for joy. . . . And what has come of it? Nothing, says M. Biot, and nothing will ever come of it: the veteran mathematician and experimental philosopher declares, as does Mr. Ellis, that no single branch of

science has ever been fruitfully explored in this way.

De Morgan was a mathematician, and I have noticed that mathematicians are apt to be crisp in their statements: but he is a bold man who says "nothing will ever come of it." Perhaps an equally crisp statement on the other side may be pardoned. I adventure the remark that if nothing has hitherto come of such observations, it is because observers have been misled by the very teaching of De Morgan and others who share his views: they have been told that they will do no good without a theory until they have come to believe it; whereas the truth probably lies in a quite different direction. To present my reasons for this proposition I must ask you first to consider in some detail the method of discussing meteorological observations suggested some years ago by Professor Schuster. He gave an account of it to the Department of Cosmical Physics, over which he presided in 1902, so that I must face some repetition of what he said; but the matter is so important that I trust this may be pardoned.

Let us compare the records produced on a gramophone disk by the playing of a single instrument and by that of an orchestra. The first will be comparatively simple, and when suitably magnified will show a series of waves which in certain parts of the record form sequences of great regularity. These represent occasions when the single instrument played a long-sustained note, the pitch of which is indicated by the frequency of the wave. If the instrument plays more loudly, while still keeping to the same note, the heights of the waves will increase, though their frequency will not be altered. The exact shape of each wave will represent the quality of tone which characterizes the instrument: and if another instrument were to

play the same note it would be different. But so long as we keep to the same instrument, whenever the same note recurred we should find, generally speaking, the same shape of wave: and we could resolve it into its constituents, one being the main wave and others harmonics of different intensities. The analysis of such a record would thus be a comparatively simple matter, on which we need scarcely dwell further. Very different is the case of the orchestral record. There are numerous instruments, playing notes of different pitch, intensity and character, each of which, if playing alone, would produce its own peculiar record. But when they play together the records are all combined into one. The needle can only make one record, but it is a true sum of all the individuals; for when the instrument is set to reproduce the playing of the orchestra, a trained ear can perceive the playing of the separate instruments—when the strings are playing alone, and when the wind joins them: when the horn comes in and whether there are two players or only one: nay, even that one of the second violins is playing somewhat flat! This could not happen unless the individual performances were essentially and truly existent in the combined record; and yet this consists of only one single wavy line. The waves are, however, now of great complexity, and it seems at first sight hopeless to analyze them. The mathematician knows, however, that such analysis is possible, and is quite simple in conception, though it may be laborious in execution. Selecting a note of any given pitch, a simple calculation devised by Fourier will reveal when and how loudly that particular note was being played. This being so, it is only necessary to repeat the process for notes of different pitch. But though this can be stated so simply, the carrying out in practise may involve immense labor, by

reason of the number of separate notes to be investigated. It is not merely that these will extend from low growls by the double bass to high squeaks by the fiddles, but that their variety within these wide limits will be so great. The series is really infinite. We might indeed prescribe a certain scale of finite intervals for the main notes, as in a piano: but the harmonics of the main tones would refuse to obey this artificial arrangement and would form intermediate pitches which must be properly investigated if our analysis is to be complete. Moreover the orchestral instruments will not keep to any such prescribed intervals, but will insist on departing from them more or less, according to the skill of the performer. There is a story told of an accompanist who vainly tried to adjust the key of his accompaniment to the erratic voice of a singer. At length in exasperation he addressed him as follows: "Sir, I have tried you on the white notes, and I have tried you on the black notes, and I have tried you on white and black mixed: you are singing on the cracks!" Some instruments will almost certainly "sing on the cracks" so that we shall not easily escape from the examination of a very large number of possibilities indeed—we may well call them *all* the possibilities within the limits of audibility. The illustration is already sufficiently developed for provisional use. My suggestion is that science has only dealt so far with the easy records and that the genuine hard work is to come. If we can imagine a number of deaf persons turned loose among a miscellaneous collection of gramophone records, with instructions to make what they could of them, we can readily imagine that they would pick out those of single instruments first. We must make the researchers deaf so that they may not use the beautiful mechanism of the human ear which has as

yet no analogue in scientific work. Possibly something corresponding to this wonderful and still mysterious mechanism may ultimately be devised, and then the course of scientific research may be fundamentally altered: but for the present we must regard ourselves as deaf, and as condemned to work by patient analysis of the records. It is perfectly natural, and even desirable, to begin with the easy ones, and the finding of an easy one would no doubt in our hypothetical case be a sensational event, reflecting credit on the lucky discoverer, who would be hailed as having detected a new law, *i. e.*, a new simple case. But sooner or later these will be used up and we must attack the more complex orchestral records in earnest. Shall we find that the best music is still to come, as our illustration suggests?

But we must return to Professor Schuster's suggested plan of work. It is closely similar to that already sketched for dealing with a complex gramophone record. Let us consider the record of any meteorological element such as temperature or rainfall. When these records are put in the form of a diagram in the familiar way we get a wavy line, which has much in common with that traced by a gramophone needle on a smaller scale. The sight of the complexities is almost paralyzing, especially when those who would otherwise attack the problem are deterred by the emphatic assertion that it is useless to do so without the equipment of some guiding hypothesis. Most of the obvious hypotheses have of course already been tried, and the majority of them have failed. It is to Professor Schuster that we owe the vitally important advice to disregard hypotheses and make a complete analysis of the record. Of course the labor is great, but the genuine observer is not afraid of labor: he has a right to ask, of course, that it shall not be

interminable: and when we are told that we must examine an almost infinite series of possibilities there would seem to be some danger of this. But in practise the work always resolves itself into a series of finite steps, owing to the finite extent of the observations. A definite illustration will make this clear. Suppose we have ninety years of rainfall and we test the record for a frequency of nine years, which would run through its period ten times: we must certainly test independently for a frequency of ten years, which would only run through its period nine times, and thus lose one whole period on the former wave: and so also for a possible frequency of nine years and a half, and of nine years and a quarter. But a frequency of nine years and one day would not be distinguishable from that of nine years, for the phase would only change 1° in the whole available period of observation. Indeed the same might be said of all frequencies between nine years and nine years and one month: for the extreme difference of phase would not exceed 40° . But in course of time when the series of ninety years' observations become 900 years, the differences of phase will approach or exceed a complete cycle, and we must accordingly narrow the intervals between frequencies chosen for examination.

The length of the series of observations is thus an important factor in our procedure, for which Professor Schuster has indicated a beautiful analogy. Our illustrations hitherto have been provided by the science of sound, but we may also gather them from that of optics. Testing a series of rainfall observations for a periodicity is like examining a source of light for a definite bright line. The process of computation indicated by Fourier gives us what corresponds to the measured brilliance of the bright line; and the complete process

of analysis corresponds to the determination of the complete spectrum of the source of light, which may consist of bright lines superimposed on a continuous spectrum. And the length of the series of observations corresponds simply to the resolving power of the optical apparatus. The only point in which the analogy breaks down is unfortunately that of ease and simplicity. In the optical analogy, an optical instrument performs for us with completeness and despatch the analysis, which in its counterpart must be performed by ourselves with much numerical labor.

Let us consider how we should most conveniently proceed to the complete delineation of a spectrum. We should ultimately need an apparatus of the greatest possible resolving power, but it might not be advisable to begin with it: on the contrary, a small instrument which enabled us to glance through the whole spectrum might save much time. Suppose, for instance, that there was a bright line in the yellow; our small instrument might suffice to show us that it was due either to sodium or helium, but no more: the decision between these alternatives must be reserved for the larger instrument. On the other hand, if no line is seen in the yellow at all, we have ruled out both possibilities at once, and so economized labor. Hence it is natural to use first an instrument of low resolving power and afterwards one of higher.

Now in the work for which this serves as an analogy this procedure is actually imposed upon us by the march of events. It has been pointed out that the resolving power of the optical apparatus corresponds exactly to the length of our series of observations. Hence our resolving power is continually increasing. Quite naturally we begin with a short series of observations, which shows us our lines blurred and confused: to define and resolve them we have

but one resource—"wait and see"; wait and accumulate more observations, to lengthen the series. But the lengthening must be in geometrical progression: we must double our series to increase the resolving power in a definite ratio; and double it again. We begin to get a glimpse of the important part to be played by the observer in the future, and of his increase in numbers.

Let us glance at a few illustrations of the use of this method. Professor Schuster has applied it, for instance, to the observations of sunspots. Now it may fairly be said that the general law of sunspots was thought to be known: the variation in a cycle of about $11\frac{1}{2}$ years has long been considered to represent the facts: it catches the eye at once in a diagram, and though there are also obvious anomalies, they had not been deemed worthy of any particular attention (with one exception presently to be mentioned), until Professor Schuster undertook his analysis. To his surprise, when he calculated the periodogram of sunspots, he found two entirely new facts: (1) that there were other distinct periodicities, notably of about four, eight and fourteen years; (2) that the eleven-year cycle had not been continuously in action, but that during the eighteenth century it had been much less marked than the eight-year and fourteen-year cycles.

A further most interesting fact seems to emerge, viz.: that several of the periodicities are harmonics of a major period of some thirty-three years or more, and it seems just possible that a connection may ultimately be established with the Leonid meteor-swarm, which revolves in this period. But it would take us too far from our main point to follow these most interesting corollaries: the point well worthy of our special attention is this, that we have here an undoubted advance in knowledge

resulting, not from observations made with regard to any particular theory, but from the simple collection of facts and the arrangement of them in all possible ways, the very method which has been despised and condemned. Let us contrast with this the method hitherto adopted, which has been to hunt for some particular possible cause which will give the eleven-year period. Thus Professor E. W. Brown suggested² in 1900 that the eleven-year cycle was due to the tidal action of Jupiter, altered periodically by two causes:

	Period	Mag. of Force
By Jupiter's eccentricity	11.86 years	0.33
By the motion of Saturn	9.93 years	0.11

and he suggests his contention by an ingenious and striking diagram, which seems to explain not only the main cycle, but its anomalies. (This paper is, in fact, the exception above referred to.) But if his contention is correct the periodogram should show bright lines at 11.86 and 9.93 years, which it does not. This is worth noting, since it is sometimes said that there is nothing new in Professor Schuster's method, which is true enough in one sense, since it is simply the analysis of Fourier. The novelty consists (1) in calling attention to the necessity of applying the analysis in all cases, a necessity which I venture to think was overlooked in this instance by so able a mathematician as Professor Brown; and (2) in the insistence on the examination of *all* periods, irrespective of any particular theory or preconception. And in this second character the method seems to me to cut at the root of the canons of procedure which have found favor hitherto.

As a second instance I present with much more diffidence a few results which seem to emerge from a very laborious analysis of the rainfall at three or four sta-

tions, for which Professor Schuster and myself are jointly responsible. There is some evidence for a cycle of 600 days in the Greenwich rainfall to which a further cycle in the quarter period (150 days) lends support. On analyzing the Padua records it is found that these cycles do not exist, but it seems quite possible that there are cycles of rather shorter period, viz., 594 days and 148½ days: the relation of four to one being maintained. The separate links in this chain are none of them very strong, but they seem to hang together, and there is certainly a case for further investigation. But would this case have been likely to present itself in any other way than by the examination of the whole periodogram? I find it very difficult to think, even now, the periods are suggested, of any theoretical cause: to let the facts speak for themselves took much time and labor, but I venture to think that we might have waited far longer, and cudgeled our brains much more, before we got the clue by formulating hypotheses of causation.

A new method is not adopted widely all at once. Professor Whittaker has, I am glad to say, begun to apply the method to variable star observations, and is already hopeful of having obtained valuable information in the case of the star *SS Cygni*. Possibly we may hear something from him at this meeting. Meanwhile I take the opportunity to remark that the history of variable star observation affords us many lessons as to the desirability of simply accumulating observations and letting them speak for themselves instead of being guided by a theory on hypothesis. Let me give an instance. One of the fathers of variable star-observing, the late N. R. Pogson, made a series of excellent observations of the star *R Ursæ Majoris* in the years 1853 to 1860. He then seems to have

² *Monthly Notices R. A. S.*, LX., p. 600.

formulated a particularly unfortunate hypothesis, viz., that he knew all about the variation; and he accordingly only made sporadic observations in succeeding years. Now this star, along with many others, varies in a manner which may be illustrated from the occurrence of sunrise. The average interval between two sunrises is exactly twenty-four hours: but this is only the average. In March the sun is rising two minutes earlier every day, and the interval is therefore two minutes short of twenty-four hours; as the year advances the daily gain slackens, and at midsummer the interval is exactly twenty-four hours: then the sun begins to rise *later* each day, and the interval exceeds twenty-four hours and so on: so that there is a regular yearly swing backwards and forwards through a mean value: and as in the case of all such swings there is a sensible halt at the extreme values. Now when Pogson made his observations of *R Ursæ Majoris* in 1853-60 it was time of halt at an extreme: the period remained stationary and the variation repeated itself eleven times in closely similar fashion, so that Pogson concluded it would continue in the same way. How many instances suffice for an induction? Many inductions have been based on fewer than eleven. Unfortunately the period was just beginning to change sensibly, and we lost much valuable information, for no one else repaired Pogson's neglect adequately: and the whole swing of period occupies about forty years, so that the opportunity of studying the changes he missed has only quite recently returned. We are thus reminded how disastrous may be a break in the record. It should be one of the articles of faith with an observer that the record is sacred and must not be broken. Most of them indeed act on that principle already, but there are heretics, and it pained us to find even Professor

Schuster himself tinged with heresy. On the very occasion when he did so much for the observer by presenting his beautiful method, he suggested that it might even be advisable to drop observing for a time in order to apply the method to accumulated observations. He may possibly be right, but the observer had better believe him wrong. There ought to be an "observer's promise" like the promise of the boy scout; and one part of it should be not to interrupt the record, and another should be to publish the observations regularly, and never to let them accumulate beyond five years.

The method of Professor Schuster is not the only one that has been recently proposed for dealing with large masses of observations. We have also the methods of Professor Karl Pearson. These have been far more widely adopted for use than the periodogram, and they have also been more adversely criticized. As regards criticism, I think it is fair to say that it has chiefly been directed towards the nature of the material on which Professor Pearson has used his process than on the process itself, and at present we need not be concerned with it. The processes themselves are sound enough; one of them, for instance, is much the same as the old method of least squares in a simple form. But if the same criticism is made as has been made on the method of the periodogram—viz., that it is not new, we can reply in almost the same words in the two cases: the mathematical calculus may not be new, the novelty is the insistence on the application of it, and the application to all possible cases. Professor Pearson ceases to look for one principal factor only, and examines all possible factors, just as Professor Schuster examines all possible frequencies. Let us recur for a moment to the words of Sir George Darwin previously quoted.

A mere catalogue of facts, however well arranged, has never led to any important scientific generalization. For in any subject the facts are so numerous and many-sided that they only lead us to a conclusion when they are marshaled by the light of some leading idea.

Let us take, for instance, a catalogue of variable stars such as those of Mr. Chandler. Particulars for each star are given in separate columns, exclusive of the name and number. We might wait long for a leading idea to guide us in marshaling the facts, and so far as I know we have waited till now without any such idea occurring to any one. But Professor Pearson insists on the plain duty of determining the correlation between each and every pair of these columns, and any others we may be able to add. Anybody could have made the suggestion, and there was plenty of elementary mathematical machinery in existence for carrying it out; but so far as I know nobody did, any more than the critics of Columbus suggested how to stand up an egg. But the suggestion having been made by Professor Pearson, it was so clearly sound that I did what lay in my power to follow it up: with the result that certain correlations were at once indicated which at least pave the way for further inquiry. If we can not say more than this it is simply because the catalogue of facts was not large enough. So far from the observers having wasted their energies by observing without any theory to guide them, more work of the same kind would have been welcome, for it would have reduced the probable error of the correlations indicated. As an example I may quote the following. It has already been mentioned that a variable-star maximum though it may recur after a more or less definite period on the average, is subject to a swing to and fro like the time of sunrise. Let us call the average interval *the day* of the star and the period of swing

the year, without implying anything more by these names than appears in the analogy. Then I found³ that the day and the year were correlated, the value of the coefficient being

$$r = 0.56 \pm 0.08.$$

Having obtained this clue, it was interesting to use it for the elucidation of individual problems. The *days* of many stars are by this time pretty well known, but their *years* are very uncertain. In nine or ten cases the assessment of the vaguely known *year* was under revision, and in all, without exception, the revised assessment tended in the direction of the formula. In one case (*S Serpentis*) the formula suggested the solution of a long-standing puzzle.⁴ Finally the inquiry is suggested whether our own sun may be treated as a variable star with a period or *day* of eleven years, in which case its time of swing a *year* should be about seventy-five years, if the formula is strictly linear. There are found to be indications of a swing of this order of magnitude, though the time given by the periodogram method is fifty-four years.⁵ If the relation between *year* and *day* is not strictly linear these figures could easily be reconciled for a case lying so far outside the limits within which the formula was deduced. But the ultimate successful establishment of the connection is of less importance for our present purpose than to notice the fruitfulness of the method of suggestion, which is as mechanical as Bacon himself could have wished.

Let us admit frankly that there is an appearance of brutality about such methods. Is our method of search to be merely the old and prosaic one of leaving no stone unturned? We have been led to believe that there should be more of inspiration in it;

³ *Monthly Notices R. A. S.*, LXVIII., p. 544.

⁴ *Monthly Notices R. A. S.*, LXVIII., p. 561.

⁵ *Ibid.*, p. 659.

that a true man of science should have some of the qualities of that fascinating hero of fiction, Mr. Sherlock Holmes, who picks up his clue and follows it unerringly to the triumphant conclusion. Such qualities will do the man of science no possible harm: indeed they will be of the utmost value to him. The point to which I am now calling attention is the change in nature of the opportunities for using them, which are becoming every day more confused. Dr. Conan Doyle, in the exercise of his art, keeps our attention fixed on a single trail: he conceals from us by mere omission the numerous trails which cross it. We admire the skill of the Indian who pursues an enemy through the trackless forest: but his success depends on the simplicity brought by this very tracklessness, and would be imperilled if there were numerous tracks. It may be remarked, however, that there is a still higher sagacity—that of the hound who even among a number of tracks can pick out the right one by scent. Let us imagine for a moment that the scientific man can be endowed in the future, by training or by some new invention, with a faculty of this kind, so that he may unerringly pursue a single trail even when it is crossed and recrossed by others. Then in the terms of this metaphor I draw attention to the fact that he has still to determine which is the right trail; and that in general he can only do so by pursuing each in turn to the end. To take an example from a recent scientific anecdote: I relate the story as I was told it, and even if incorrect in detail it will serve its purpose as a parable. The Röntgen rays were discovered originally by their photographic action, but afterwards it was found that they would render a screen of calcium tungstate phosphorescent. I was told that this discovery had been made in this wise: Mr. Edison had a large collection of different

chemicals, and a number of assistants: he set his assistants busily to work to try each substance in turn until the right one was found. Now this is not only a genuine scientific process, but it is *the fundamental process*. Let it be frankly admitted that our instincts are against it. We should much prefer to hear that some *hypothesis* had pointed the way, even a false hypothesis such as actually led to the discovery of the possibility of achromatism in lenses. Or if *memory* had played a part: The other day Professor Fowler identified the spectrum of a comet's tail with one taken in his laboratory; of which he had some recollection, and our human sympathies fasten at once on this idea of recollection as a praiseworthy element in the discovery. Nay, even mere *accident* appeals to us more than brutal industry: if Mr. Edison had wandered into his laboratory, picked up a bottle at random, and found it answer his purpose, I venture to say that we should have instinctively awarded him more merit: there would have been just a chance that he was inspired. Let us by all means welcome hypothesis, memory, inspiration and accident whenever and wherever they will help us: but they may fail, and then our only resource is to help ourselves by the unfailing method of examining all possibilities. The aid of the others is adventitious and comes, like that of the gods, most readily to those who help themselves.

The maxim of "leaving no stone unturned" was enunciated from a rather different point of view some dozen years ago by an American geologist, Professor T. C. Chamberlin, of Chicago, in a short paper for students entitled "The Method of Multiple Working Hypotheses."⁶ After recalling how much the march of science in early days was retarded by the tyranny

⁶ University of Chicago Press, 1897.

of a theory formulated too hastily, and how in later times attempts have been made to remedy this evil by holding the theory, provisionally only, as a working hypothesis, Professor Chamberlin points out that even the working hypothesis has serious disadvantages:

Instinctively there is a special searching-out of phenomena that support it, for the mind is led by its desires. . . . From an unduly favored child it readily grows to be a master and leads its author whithersoever it will. . . . Unless the theory happens perchance to be the true one, all hope of the best results is gone. To be sure, truth may be brought forth by an investigator dominated by a false ruling idea. His very errors may indeed stimulate investigation on the part of others. But the condition is scarcely the less unfortunate.

To avoid this grave danger the method of multiple working hypotheses is urged. It differs from the simple working hypothesis in that it distributes the effort and divides the affections. . . . In developing the multiple hypotheses, the effort is to bring up into view every rational explanation of the phenomenon in hand and to develop every tenable hypothesis as to its nature, cause or origin, and to give all of these as impartially as possible a working form and a due place in the investigation. The investigator thus becomes the parent of a family of hypotheses: and by his parental relations to all is morally forbidden to fasten his affections unduly upon any one. In the very nature of the case, the chief danger that springs from affection is counteracted.

For the further elucidation of Professor Chamberlin's proposals I must refer my audience to his original paper, which is well worthy of careful attention. He does not shirk consideration of the drawbacks—"No good thing is without its drawbacks," he writes. And it may be added that no good thing is entirely new, or entirely old. Perhaps it is better to say that it is generally both new and old. The method of multiple hypotheses is new because it is still necessary to remind scientific workers of all kinds that so long as they restrict themselves to the examination

of one hypothesis only they can never reach complete logical proof: they can only attain a high measure of probability. What is often called verification⁷ is not complete proof, but only increase in probability: for complete proof it is necessary to show that no other hypothesis will suit the facts equally well, and thus we are bound to consider other possible hypotheses even in the direct establishment of one.

But the method is also old in that it has long been adopted in practise, however partially and unconsciously by scientific workers of all kinds. When as a boy at school I began to make physical measurements under Mr. J. G. McGregor (now professor of physics at Edinburgh) I learned from him one golden rule: "Reverse everything that can be reversed." The crisp form of the rule may be new to many who have long used it in their work: and its use is simply that of "multiple hypotheses." For when the current in a wire is reversed, the hypothesis is tacitly

⁷ To show that the facts agree with the consequences of our hypothesis is not to prove it true. To show that is often called *verification*: and to mistake verification for proof is to commit the fallacy of the consequent, the fallacy of thinking that because, if the hypothesis were true, certain facts would follow, therefore, since those facts are found, the hypothesis is true. . . . A theory whose consequences conflict with the facts can not be true; but so long as there may be more than one giving the same consequences, the agreement of the facts with one of them furnishes no ground for choosing between it and the others. Nevertheless, in practise we often have to be content with verification; or to take our inability to find any other equally satisfactory theory as equivalent to there being none other. In such matters we must consider what is called the weight of the evidence for a theory which is not rigorously proved. But no one has shown how weight of evidence can be mechanically estimated; the wisest men, and best acquainted with the matter in hand, are oftenest right.—"An Introduction to Logic," by H. W. B. Joseph, fellow and tutor of New College, Oxford, Clarendon Press, 1906, p. 486.

made, the effect observed may be due to the direction of the current: and when a measured spectrum photograph is turned round and remeasured, it is an admission of the hypothesis that the direction of measurement may be partly responsible for the observed displacements of the spectrum lines. By the various reversals we endeavor, in Professor Chamberlin's words, "to bring up into view every rational explanation of the phenomenon in hand" which can be brought up into view in this way. But truly "no good thing is without its drawbacks," and one drawback to the recognition of this principle is that, by a process of mental confusion, it seems sometimes to be regarded as a distinct merit in a piece of apparatus that it can be reversed in a large number of ways. It must be remembered that the hypotheses thus examined and ruled out are chiefly instrumental ones superadded to those of nature: and the latter are already sufficiently numerous, without our ingenious additions.

The view which I have endeavored to put before you of the inevitable course of scientific work is that it will depend more and more on the patient process of "leaving no stone unturned." It may not be an inspiring view, but it should be at least encouraging, for it follows that no good honest work is thrown away. And it is just this encouragement of which the observer, as opposed to the worker in the laboratory and the mathematician, stands sometimes in sore need. The worker in the laboratory can often clear away his hypotheses on the spot: he can reverse his current then and there: but this is often impossible for the observer, who can and does reverse his spectrum plate for measurement, but to reverse the motion of the earth which affected the lines must wait six months: and to reverse also the motion of the star may have to wait six years, or sixty, or sixty

thousand. In many cases he must leave the reversal to others, and thus not only can he not test all his hypotheses, but he may not even be able to formulate them. His aim can not, therefore, be to establish within his lifetime some new law, and his work is not, therefore, to be appreciated or condemned by his success or failure in this respect. There are truer aims and surer methods of judgment. Something is inevitably lost when we endeavor to express these aims in the concrete; but for the sake of illustration we may say that the true observer is always endeavoring to reach the next decimal place, and is ever on the alert for some new event. Of the pursuit of the next decimal place it is needless to say more: the aim is as familiar in the laboratory as in the observatory. But I often think that the recognition of new events is scarcely given its proper place in the annals of science, if we have due regard to the consequences. I have protested that in much of his work the observer can not be judged by the fruits of his labor, though there is an instinctive tendency to judge in this way: but here is a case where he might well be content to be so judged, and yet the consistent award is withheld. Think for a moment of the very considerable additions to our knowledge which have accrued from the discovery by Professor W. H. Pickering of an eighth satellite to Saturn. The discovery led directly to the recognition of the retrograde motion; and to explain this we were led to revise completely our views of the past history of the solar system. Incidentally it stimulated the search for other new satellites, resulting in the discovery of a curious pair to Jupiter and next of the extraordinary eighth satellite; while it was the investigation of the orbit of this curiosity which suggested an eminently successful method of work on cometary orbits. If we judge scientific work

by its results we must take into account all this subsequent history in our appreciation of Professor Pickering's achievement. But whether we do so or not is probably a matter of indifference to him, for the true observer is above all things an amateur, using the word in that splendid sense to which Professor Hale recently introduced us. There have been many attempts to define an amateur. One was given by Professor Schuster in his eloquent address to this section at Edinburgh in 1892:

We may perhaps best define an amateur as one who learns his science as he wants it and when he wants it. I should call Faraday an amateur.

We need not quarrel with his definition and certainly not with the noble instance with which he points it. But after all I prefer the definition of Professor Hale:⁸

According to my view, the amateur is the man who works in astronomy because he can not help it, because he would rather do such work than anything else in the world, and who therefore cares little for hampering traditions or for difficulties of any kind.

The wholly satisfactory nature of this view is that it provides not only a definition, but an ambition, and a criterion. We feel at once the ambition to become amateurs, for I deny stoutly that the distinction is conferred at birth: it comes with work of the right kind. And we may know what is work of the right kind by this if by nothing else: that by diligently performing it we shall become amateurs who find it impossible to stop: "who work in astronomy because we can not help it." Before an army of such men even the vast hordes of dusky possibilities of which we are beginning to catch glimpses must yield. The fight may seem, and no doubt is, without end; and the opportunities for glorious deeds by which outlying whole troops of the enemy are demolished at once are be-

coming rarer. We are confronted with the necessity of attacking each possibility singly, which threatens the stopping of the conflict through sheer weariness. Clearly the army of amateurs is the right one for the work: weariness can not touch them: they will go on fighting automatically because "they can not help it."

H. H. TURNER

SAMUEL HUBBARD SCUDDER

SAMUEL HUBBARD SCUDDER was born at Boston, April 13, 1837, and died at 156 Brattle Street, Cambridge, May 17, 1911, at the age of seventy-four years. He was, perhaps, the greatest American entomologist of his time. Whether we regard the mere mass of his work or its excellence or the breadth of view shown, we who belong to this later generation must stand amazed and humbled. Which of us can even imagine himself girding his loins for such a task as the "Nomenclator Zoologicus" or the great volumes on the "Butterflies of the Eastern United States"? Such things may now be undertaken cooperatively, or with much expert and clerical assistance; but Scudder was both architect and builder of his great works, responsible for everything, very rarely seeking collaboration, except for the purpose of gathering materials. I corresponded actively with him for many years, and have before me a pile of old letters and postal cards in the familiar handwriting. As I look them over I think of two especially prominent characteristics, his *enthusiasm* and his *kindness*. Herein he ranks with another famous entomologist, W. H. Edwards, who at one time wrote me almost daily concerning the progress and welfare of an interesting caterpillar I had sent him. It was not enough for Scudder to discover new facts or perceive new relationships; he must at once communicate them to those likely to be interested; and the charm of his letters, without the reserve natural to the printed page, must have warmed the heart and increased the zeal of many a younger man. May we, who now are obliged in such manner as we can to fill in the vacated ranks,

⁸ *Monthly Notices R. A. S.*, LXVIII., p. 64.