A table of contents for *Journal of the Transactions of the Victoria Institute* can be found here:

https://biblicalstudies.org.uk/articles_jtvi-01.php

JOURNAL OF

THE TRANSACTIONS

OF

The Victoria Institute,

OR,

Philosophical Society of Great Britain.

EDITED BY THE HONORARY SECRETARY.

VOL. X.

LONDON:

(Published for the Institute)

HARDWICKE & BOGUE, 192, PICCADILLY, W.

1877.

ORDINARY MEETING, March 15, 1875.

C. Brooke, Esq., F.R.S., V.P., in the Chair.

The Minutes of the last meeting were read and confirmed, and the following elections were announced :—

Members :—J. G. Gibbs, Esq. (Surgeon-Major Madras Medical Service), Rickmansworth; M. H. Habershon, Esq. (Hon. Master and Secretary of Rotheram College), West Hackney; Rev. C. F. Norman, M.A. (Cantab.), Mistley.

Associates :—Rev. E. J. Barrett, Cape Colony; Rev. W. S. Davis, Cape Colony; Rev. T. Eastwood, Cape Colony; Rev. P. Hargreaves, Cape Colony; Rev. W. Hunter, Cape Colony; Rev. James Morris, Cape Colony; Rev. Joseph Morris, Bristol; Rev. W. Park, A.M., Belfast; Rev. J. E. Parsonson, Cape Colony; Rev. T. Powell, F.L.S., Samoa, Pacific; Rev. W. H. Tucker, M.A., Brentwood; Rev. E. J. Warner, Cape Colony; Rev. C. White, Cape Colony; Colonel C. W. Hutchinson, R.E. (Inspector-General for Public Works Department, Bengal); W. Stephenson, Esq., Hull; A. Rivers Willson, Esq. (Chemist), Hammersmith.

Also the presentation of the following Works to the Library :—

"Proceedings of the Royal Society," Part 159.	*From the Society.*
"Proceedings of the Warwickshire Arch. and Nat. Hist. Soc. 1874."	*Rev. P. Brodie.*
"Atomism." By Rev. Professor Watts.	*The Author.*
"Materialism." By Rev. Dr. Hooppell.	*Ditto.*
"Man's Responsibility for his Belief." By Rev. J. Macnaughtan.	*Ditto.*
"Holland House." Vol. II.	*T. W. Masterman, Esq.*

The following paper was then read by the author :—

ON THE NATURE AND CHARACTER OF EVIDENCE FOR SCIENTIFIC PURPOSES. By the Rev. J. M'Cann, D.D., F.R.S.L., F.G.S.

SCIENCE is knowledge in the fullest and truest meaning of that word. We cannot be said to know any fact, unless we know its relation to other facts, the place it occupies in the economy of nature, and the laws by which it has been produced. I may see a flower before me, which I call a rose, and at first sight may learn something of its form, colour, perfume, &c.; but I do not know it in the scientific sense, till I have learned its affinities to other flowers, its uses in the world, and the modes by which it has been built from air and earth. There is, moreover, such a multitude of objects presenting themselves to our notice, such an infinite variety of apparently isolated facts, that the mind soon becomes overwhelmed by their numbers, and finds itself powerless to grasp them, even in their individual significance.

We can, therefore, only know as we classify, as we discover certain unities round which the varieties cluster, and by whose name they are designated. This is the special province of science, to search for similarities amid these diversities, and harmonies amid these apparent discords. The work to be done by the student is thus greatly reduced; instead of requiring to examine every separate individual, he need only examine one of that particular sort; the knowledge also of this one sort saves much study in the investigation of other individuals that resemble it in some points, while they differ from it in others. Even one point of true resemblance is useful, because it mostly happens that one point of likeness will be accompanied by others, not perhaps so patent to the senses, but still existing. It was something for the botanist to have found that he might group plants according to the structure of the embryo into three great classes; for this told him other particulars regarding the structure of the stem, and the character of the flowers and leaves. In like manner information about the buttercup will render the study of monkshood much simpler, because while there are specific and even generic differences between these two, there are many important similarities. The naturalist—and by naturalist I mean the student of any department of nature—thus gradually progresses from generalizations of less significance to those of greater, from unity to unity, till at last the whole field of observation is mapped out into a few great provinces or kingdoms, these having their minor divisions and subdivisions, so that we are able to take an intelligent, even if not detailed, survey of the whole, and feel ourselves competent, by the division of labour, to examine and relegate all phenomena to their appropriate departments.

It is, however, of the utmost importance that these unities should be real and not imaginary, the products of our investigations, and not the children of our wishes or our fancies. If the former, we gradually rise to the apprehension of that great unity called a law of nature; but if the latter, we inevitably sink to the pernicious occupation of constructing bubble theories, and add some more to the already too long list of the fallacies of philosophers.

A scientific is somewhat like a judicial court, where the purpose is to obtain a verdict; in other words, to procure information regarding the subject in dispute. The jury must see that they have sufficient evidence on which to base a verdict of *any* kind; secondly, that they have all the evidence before them which is procurable; and thirdly, that the verdict be according to the evidence. The naturalist also, before he can say he has discovered a fact or a law, must act in a similar manner. While the evidence is all on one side, the way is clear; but when it becomes conflicting, only the greatest care, strictest impartiality, and most thorough training can sift the false from the true, and decide the matter rightly; but even then it is not always possible.

There lies in this a strong temptation to concentrate our attention on those facts alone which favour the theory we wish to establish, excluding all others from our thoughts. This may be theory-manufacture, but it is not science. Let the confusion, or difficulty, be increased ever so much, the naturalist must search thoroughly, impartially, and critically, if he would have his science true, and his knowledge real.

Science, then, begins with facts obtained either by observation or experiment, passes on to inferences from these facts, which inferences, if conducted rightly, according to the laws of thought, will be as true as their premises; so that we end with facts as we began with them.

The first step consequently in the procuring of adequate evidence for scientific purposes is the obtaining of facts, mostly by observation. This seems an easy matter to those who are unaccustomed to the task. "What simpler," they say, "than to look, and tell what you have seen?" It is, however, so difficult, that the well-known saying is unfortunately true, "that there are more false facts than false theories in the world."

The reason of this is that we confound our observations with our inferences, for observation is never a simple passive process of the senses, but is always accompanied by some active mental state. We think while we look. We consequently contribute to the observation something from ourselves, uniting the subjective and the objective into one. This mental addition very frequently is a prejudice; we are not content with trying to discover what is, but look out for what we imagine ought to be, or what we want to be. It would be very difficult, for example, for the creationist and the evolutionist to examine with equal care and fairness some phenomenon that would tell either strongly for, or strongly against the theory of development. Both might imagine they were honestly doing their best; but, unless their minds were of a high order, prejudice would warp one way or the other. This warping power is, however, often present when no such reason is to be found; it may spring from carelessness, want of training, or previous habit. It is a very common opinion, most difficult to shake, that the moon appears larger when on the horizon at certain times, than when her altitude is greater; measure her as you may, there is always the response, "But look at her; don't you see the greater size for yourself?" And seeing is held to be believing. "When," says Kant, "we have once heard a bad report of this or that person, we think that we read the rogue in his countenance." In such a case observation fails, and fancy completes the task. A parson and a lady having both heard that the moon was inhabited, believed it, and, telescope in hand, were attempting to make out the inhabitants. "If I am not mistaken," said the lady, who looked first, "I perceive two shadows; they bend towards each other, and, I have no doubt, are two happy lovers." "Lovers, Madam," said the divine, who looked second, "oh, fie! the two shadows you saw are the two steeples of a cathedral." It is no uncommon thing for naturalists of all ranks to turn shadows into lovers or steeples, as their prepossessions lead them. It reminds me much of an echo I once heard in a rocky chasm in Yorkshire. When I shouted "fracture" down the opening, the answer returned was "fracture"; but when I shouted "denudation," something like "denudation" came back to me. When I cried, "What are you?"—a surely fair question,—the startling one was asked of me, "What are *you?*" The rock was evidently of an accommodating nature, and determined to reflect my ideas, instead of its own facts. Something similar frequently occurs also where there is perfect honesty of purpose; but where the mind, running in old grooves, acting according to its latent modes, is not prepared to accept in their entirety new facts, which are more or less inconsistent with these previous experiences, as the following instance will illustrate. Shortly after Day had succeeded in decomposing the fixed alkalies, a portion of potassium, a substance light enough to swim on water, was placed in the hands of one of the most distinguished chemists, with a query as to its nature. The philosopher observing its aspect and splendour, did not hesitate in pronouncing it to be metallic, and, balancing it on his finger, he exclaimed, "It is certainly metallic, and very heavy." He united the idea of weight with that of metal, and the evidence of his senses having been insufficient to dissever ideas so inseparably associated in his mind, he mistook his judgment of the ponderosity of the substance for his sensation of it.* Of course, therefore, in the same degree as we mingle observation and inference in the record of what professes to be observation only, the evidence afforded is in the same degree invalidated. The first step then is to sever the one from the other, and see that our facts be true.

I do not mean, in what I have said, to imply that in the accumulation of evidence we ought, if possible, to keep our mental action wholly in abeyance, and observe indiscriminately all facts that come before us. It is most useful to have some suggestive hypothesis to guide our observations, in order that there may be method in our investigations, and to enable us to select for more careful scrutiny the more important circumstances. A certain amount of deductive reasoning must accompany the student from the first, if he would not accumulate his facts blindfold. I quite agree with what Hooke says in his work on Philosophical Method, that "the natural philosopher ought to be very well skilled in those several kinds of philosophy already known, to understand their several hypotheses, suppositions, observations, &c., their various ways of ratiocinations and proceedings, the several failings and defects, both in their way of raising, and in their way of managing their several theories, for by this means the mind will be somewhat more ready at guessing at the solution of many phenomena almost at first sight, and thereby be much more prompt at making queries, and at tracing the subtlety of nature, and in discovering and reaching into the true reason of things." What I may call the suggestively deductive method, accompanied by continuous observation—has accumulated more valuable and systematic evidence than any other, and has yielded most important results. The investigator in such a case uses "such facts as are in the first place known to him, in suggesting probable hypotheses; deducing other facts, which would happen if a particular hypothesis be true, he proceeds to test the truth of his notion by fresh observations or experiments. If any result prove different from what he expects, it leads him either to abandon or to modify his hypothesis; but every new fact may give some new suggestion as to the laws in action. Even if the

* This paper having been written away from books, I have not been able to tell always whom I am quoting, nor always to refer known quotations to the respective authors.

result in any case agrees with his anticipations, he does not regard it as finally confirmatory of his theory, but proceeds to test the truth of the theory by new deductions and new trials."* It is therefore of the utmost importance that the naturalist should have an almost instinctive aptitude in conceiving hypotheses, to be used, however, only as finger-posts directing him along a certain line of observation, and only to be used while they are useful, but to be discarded without hesitation when they would lead him into the quagmire of error. Hypotheses of this kind are only tentative, and must be regarded merely as the scaffolding to a more permanent erection, but must never be mistaken for the erection itself.

But before we begin to build we must see that we have sufficient materials with which to complete the structure, so that it may be well-proportioned and secure. In other words, it is of great importance that we should collect a considerable number of facts before we commence theorizing; if we have only a few, we have no range of vision, our power of comparison is limited, and, consequently, the evidence in favour of any explanation being insufficient, the explanation or hypothesis erected on it will be as a cone on its apex, in very unstable equilibrium, easily overturned, as many such have been. Several naturalists have fallen into the mistake of elaborating theories of the universe the instant they have discovered a few supposed facts, which seemed new to them, instead of patiently gathering more, or trying to verify those previously observed. The more abundant the evidence the more likely is the verdict to be true.

I have so far spoken only of the evidence obtained by direct observation; when, however, we can by experiment repeat the phenomenon at will, and so verify or correct it, our confidence in the results we have obtained is greatly increased. But to speak of the absolute necessity for varied and accurate experiment in the procuring of scientific evidence would be here a mere waste of time; there is, however, one great result aocomplished by it which I would not wholly overlook, and that is, the deciding some of several supposed causes to be the actual one in the production of the observed effect. The corpuscular and undulatory theories of light seemed each of them for a time to satisfactorily account for the phenomena; but when it was proved by experiment that light moved more slowly in glass than in air, the undulatory theory which predicted this, was known as more likely to be true than the corpuscular,

* Jevons' *Principles of Science*, vol. ii. p. 137.

which required it to move rapidly. Again, when it was discovered that an acid and an alkali were produced at the poles, together with oxygen and hydrogen, when water was decomposed by electricity, it was supposed by some that electricity had the power of generating acids and alkalies; but Davy supposed that this might be the result of the circumstances in which the experiments had been performed; he therefore varied those circumstances, until he performed the experiment without any acid or alkali having been detected.

Having now, by the methods indicated, obtained a certain body of tolerably trustworthy facts or materials for science, the next step is to give them cohesion, or convert them into science—to bind them into as few unities as possible. We have now to pass from facts to inferences, from the senses to the intellect; to bring into play that unifying power of the mind by which we detect the one in the many, and discover the special law, of which various facts are illustrations. This is done by what I may perhaps be permitted to call an inductive guess.

The mind that is trained to close and cautious inference, and at the same time possesses a special aptitude for generalization, will almost instinctively see the hypothesis that supplies the needed explanation. As in the case of Pascal, who, rejecting the previous idea of nature having an abhorrence of a vacuum, conceived that air had weight; or in that of Roger Bacon, explaining by refraction the bending power of a convex lens towards the perpendicular, while his predecessors thought it to be the result of the material of the substance through which the light passed, the form having been supposed to be of no importance. All persons, however, have not been of this accurate character. Most discoverers have tried many suppositions before they have hit upon the right one; numbers have passed in review before their judgment has selected any as probable; and even of those so selected, not one may have survived the test of experiment. The weakest analogies, the most whimsical notions, the most apparently absurd theories, may pass through the teeming brain, and no record may remain of more than the hundredth part. Kepler, for example, imagined and discarded no fewer than nineteen hypotheses before he established the actual fact regarding the motion of Mars, and then applied to it the correct term "elliptic."

But although a guess or hypothesis may be erroneous, it does not follow that it is useless; it may be a means of collecting and binding together evidence for a certain purpose, which, although eventually useless in the proving of that for which it was collected, may eventually prove most serviceable in the establishment of some other doctrine. This was the case with the false hypothesis of epicycles; which, however, proved of great service to a truer astronomy, by giving a mass of observations, which represented the velocities and places of the planets in a way not far from true, and also by giving knowledge sufficient to predict eclipses and construct astronomical tables.

Such conjectures as those of the hypotheses of spontaneous generation, conservation of energy, or evolution, however they may by future observation be demonstrated as erroneous, will yet prove exceedingly useful by the most important facts they are accumulating in such large numbers; they are, as it were, cutting from the quarry of nature a great quantity of building-material, which some future architect may erect into a noble and permanent building. But while fertile and intelligent conjecture is so advantageous to science, a bigoted adherence to these conjectures, when all evidence is against them, is just as pernicious to its interests, and arrestive of its progress. The character of the true naturalist is indicated by the words of Leslie, who said: "In the course of investigation I have found myself compelled to relinquish some preconceived notions; but I have not abandoned them hastily, nor till after a warm and obstinate defence, I was driven from every post.". He, of course, held on while he could; but when he could no longer honestly hold his post, he abandoned it; an example much needed by some modern theorists. "The candid and simple love of truth," Whewell well remarks, "which makes the discoverer willing to suppress the most favourite production of his own ingenuity as soon as it appears to be at variance with realities, constitutes the first characteristic of this temper. He must neither have the blindness which cannot, nor the obstinacy which will not, perceive the discrepancy of his fancies and his facts. He must allow no indolence, or partial views, or self-complacency, or delight in seeming demonstration, to make him tenacious of the schemes which he devises, any further than they are confirmed by their accordance with nature. The framing of hypotheses is, for the inquirer after truth, not the end, but the beginning of his work." Having then framed an hypothesis, the next step is to test it by contact with fact, to verify the correctness of our inferences by further observation or experiment; to examine by an appeal to nature whether the conclusion at which we have arrived is in harmony with the evidence at our disposal. In other words, we must now proceed deductively from the intellect to the senses, from an imagined law to its consequences. By induction we have bounded to the top of the stair by one leap, but we must now descend deductively, steadily, and methodically, trying each step, in order that we may establish the solidity of our footing. The deductive reference of any theory to every detail of the evidence from which it was supposed to spring cannot be too strongly enforced. If our law be a correct one, certain consequences ought to follow; experiment or observation must search and see whether these consequences actually do follow; if they do, our confidence is strengthened; if not, it is in the same degree weakened. Newton, when meditating on the subject of gravity, thought it might extend as far as the moon, and at last guessed that she was retained in her orbit by it; but if so, certain results must follow. One was that the moon must be deflected from the tangent every minute through a space of more than 15 feet; but his calculations made, so as to determine the truth of this, gave a deflection of only 13 feet. Here then was discrepancy between theory and fact; he had, proceeding deductively, apparently proved himself wrong, by a small quantity indeed, but yet sufficient to induce him to give it up at once. But when he found he had been basing his calculations on a wrong magnitude of the earth, he commenced afresh, and now found that theory and fact agreed with remarkable exactness. Here then was an inference verified by evidence of the most satisfactory kind, and he was warranted in looking upon the universal prevalence of gravity as a good hypothesis. Because a good hypothesis is one that foretells or allows of deductive reasoning; that is, it must anticipate the results of new combinations of series of facts, prophesying the, as yet, unknown consequences. Another generalization was that the gravity of every material body is in the direct proportion of its mass; but if this be true, all objects, when opposing obstacles are removed, will fall with equal velocity. This was verified in the familiar experiment of the guinea and feather.

Another important test is that there be nothing contradictory in the hypothesis to the known laws of nature, as ascertained in other departments of investigation. "Mere difficulties of conception must not discredit a theory which otherwise agrees with facts, and we must only reject hypotheses which are inconceivable in the sense of breaking distinctly the primary laws of thought and nature" (Jevons).

Then confidence in our inference is very much strengthened when it explains to us the meaning of evidence wholly different, apparently, in kind from that on which the inference is based. Thus the theory of the universality of gravitation, based on the evidence of the perturbations of the planets, was corroborated

E 2

by the fact that it accounted for the dissimilar fact of the precession of the equinoxes. This indirect evidence is of more value than the direct, because in the case of the direct there is often a danger of our observations being somewhat warped by the prejudice of a wished-for result, but the indirect must be altogether honest.

It sometimes happens that the result of experiment may approximate very closely, but not exactly, to that required by the hypothesis; the divergence having been caused by some residual fact, which, when examined, strikingly confirms the hypothesis instead of weakening it. The law of the development of heat in elastic fluids by compression affords an illustration in its relation to the propagation of sound through the air. Newton calculated that sound ought to travel at the rate of 968 feet per second; experiment however, at that time, showed it to travel at the rate of 1,142 feet. Here, then, was a residual velocity which Newton and others made many ineffectual attempts to explain. Laplace, however, suggested that it might arise from the heat produced by the condensation taking place at every vibration, increasing the elasticity of the air. In 1816 he published the theorem on which the connection depends. On applying it, the calculated velocity of sound agreed very closely with the best antecedent experiments, and thus this residual velocity strengthened the foregoing law of the development of heat by compression. There are many other characteristics of true evidence, and tests of the hypotheses inferred from it; there is much more that might be said regarding the evolving of science by the threefold process of observation, hypothesis, and verification; but time will not permit. Indeed, the subject is so extensive, that I could only detach a small portion of the fringe; and as this hasty paper has not been written for those who understand the subject far better than I do, but for those who may not have given much attention to this special aspect of science, I hope I shall be pardoned for the superficial manner in which I have treated it. Before proceeding to apply these principles to cases of present theories, I must give you the character of the true naturalist as drawn by Professor Jevons and by Faraday. Jevons says, "It would seem as if the mind of the great discoverer must combine almost contradictory attributes. He must be fertile in theories and hypotheses, and yet full of facts and precise results of experience. He must entertain the feeblest analogies, and the merest guesses at truth, and yet he must hold them as worthless till they are verified in experiment. When there are any grounds of probability, he must hold tenaciously to an old opinion, and yet he must be prepared at any moment to relinquish it when a single clearly contradictory fact is encountered." "The philosopher," says Faraday, "should be a man willing to listen to every suggestion, but determined to judge for himself. He should not be biassed by appearances; have no favourite hypothesis; be of no school; and in doctrine have no master. He should not be a respecter of persons, but of things. Truth should be his primary object. If to these qualities be added industry, he may indeed hope to walk within the veil of the temple of nature." He may indeed, and when there we should have from him fewer crude speculations when facts are absent; fewer fallacious reasonings when logic can nowhere be found; less talk about that which is inherently impossible, contradictions between the science of God's creation and that of creation's God. We may hope, however, that the establishment of schools for original investigation and mental discipline will eventually produce students competent to see facts truly, describe them accurately, and infer from them reasonably; qualities very much needed in the present day.

I shall select my first illustrations from the beautiful discoveries by spectrum analysis. The stars, we know, resemble the sun in being sources of light and heat, not mere reflectors, as are the planets. It was therefore inferred that whatever might be discovered regarding the physical constitution of the sun, would be in great degree true of them also. The telescope however could not afford us much information here, because to it they are but points of light. However, the spectroscope decided the question, and confirmed the supposition by showing that their spectra were similar in kind to that of the sun. But a still more striking confirmation of a cautious deduction, one regarding the motions of the stars, has been yielded by it. Giordano Bruno was, I think, the first to suggest that as the planets moved round the sun, the stars also had planets revolving round them; and not only so, but they also themselves moved in space. This guess, since proved by direct astronomical observation, has received additional confirmation by the fact that the spectroscope can distinctly detect such motion in the change of the hydrogen line, caused by the different effect produced on the retina by light when the luminous body is stationary, from that produced when it is in motion. There is, however, a difference in the rate of motion as yielded by spectroscopic and by telescopic observation; that given by the spectroscope being about 29 miles per second for the star Sirius; while that given by the parallax of M. Abbe is 43;

but the parallax of Henderson gives only 24, which approaches very closely to that given by the spectrum.

I now proceed to select a few illustrations from the Belfast Address of Professor Tyndall, but, with the exception of the first, of a kind to show how hypotheses are built upon insufficient evidence, and consequently are not scientific. I begin with his opening sentence: "An impulse inherent in primeval man turned his thoughts and questionings betimes towards the sources of natural phenomena. The same impulse, inherited and intensified, is the spur of scientific action to-day. Determined by it, by a process of abstraction from experience, we form physical theories which lie beyond the pale of experience, but which satisfy the desire of the mind to see every natural occurrence resting upon a cause."

He first speaks of a scientific *impulse*, of a *determination* in a certain direction. Is there any evidence of this impulse? Yes. abundant evidence in our own conciousness. We know that when we see a change we *cannot help* believing in a cause for the change, and when more actively intelligent, we are *impelled* to search for that cause. From this we infer that if such search be an inherent impulse, it will often, if not always, act without reference to expediency or profit. This deduction is fully verified in the fact that numbers are enthusiasts in this search who never hope to receive any equivalent in the way of prudential recompense. But we have also the affirmation that the impulse is inherent in primeval man; that is, not derived from inheritance, or obtained by experience. The evidence for this is that there is no trace whatever in our supposed ancestors, the monkeys, of turning their thoughts towards the sources of natural phenomena; being found in the first men, it could not be inherited, so must be inherent. So far I think the Professor is thoroughly scientific, though his first proposition directly negatives nearly the entire remainder of his address. But I regret that I cannot long coincide with him, for in his second sentence he speaks of this impulse as being inherited by us. This is surely a flaw, for if it was not inherited by the first man, what reason have we for inferring that it was inherited by any of his descendants? If it were inherent in him, why should we not say that it is inherent in ourselves? We now proceed to the propositions of Democritus, which are, all but one, accepted by Tyndall in these words: "The first five propositions are a fair general statement of the atomic philosophy as now held. One statement in that philosophy is that 'nothing that exists can be destroyed.'" The only evidence for this being, that however we may change the form of any compound, we do not destroy the materials. This is sufficient evidence that man has not destroyed any substantial existence, and a very important generalization it is in some respects; but there is not one tittle of evidence for the wider proposition of Democritus, either in observation or the laws of thought. Another statement is, "every occurrence has its cause, from which it follows by necessity." I agree with this, but not in the sense of Democritus. I believe there is a necessity, but that it flows from the will of a Creator, whose will is law; but Democritus held that the necessity was inherent. That this is not evidentially proved, is shown by the fact that many of his own school reject this necessity altogether, and use the word antecedence instead. Bain says, "To express causation, we need only name one thing, the antecedent, or cause, and another thing, the effect." Huxley writes, "The notion of necessity is something illegitimately thrust into the perfectly legitimate conception of law." The invaluable evidence of the fundamental laws of thought, and the testimony of consciousness is ignored by those naturalists who maintain that the only bond of union between successive happenings is that of time and regularity, and that by these two terms they give an adequate explanation of causation.

Indeed this whole atomic hypothesis, while a most valuable one for working purposes, and very useful to the chemist, is not sufficiently verified to be assumed as a fact, or made the basis of a theory of the universe. Professor Cooke, of Harvard University, who says he has been called a blind partisan of the atomic theory, writes regarding it, "I wish to declare my belief that the atomic theory, beautiful and consistent as it appears, is only a temporary expedient for representing the facts of chemistry to the mind; although in the present state of science it gives absolutely essential aid both to investigation and study; I have the conviction that it is a temporary scaffolding around the imperfect building, which will be removed as soon as its usefulness is passed."* This is consistent and scientific, but Tyndall's mode of treating the molecules seems neither one nor other. He first adopts the idea that "the varieties of all things depend upon the varieties of their atoms in number, size, and aggregation," and states distinctly that Maxwell's logic was not legitimate when he took the step from the atoms to their Maker, that we must abandon all conception of creative acts. Here then is a distinct

---

* *The New Chemistry*, p. 103.

hypothesis, the atomic or molecular, to account for the phenomena of nature, to explain the facts of observation and experience. We are pointed to the atom as the one unity, or resting-place for thought. But the very man who does this says, that molecular motions and groupings not only do not explain everything, but in reality they explain nothing. But he does not end here, for he goes on to say that if the materialist cannot explain these things or tell the "why" of phenomena, no one else, "priest or philosopher," can.

Here, then, we have evidence of two things,—that the science of material phenomena cannot solve what he rightly calls the "problem of problems." This is beyond its province, and ought not to be expected of it. But we have evidence also of a baseless assumption, an unwarranted generalization in the statement that if that science cannot solve it, no other can, that solution is impossible. It is seen, however, that we have the authority of Tyndall for saying that not to the naturalist must the man go who believes in the reality of awe, reverence, wonder, religion, &c., for he can do nothing for him; if there *be* hope anywhere, it must be found in the priest, not the philosopher.

We are also introduced, of course, to the subject of evolution, which means an indefinite or continuous change of structure, from the simple upwards to the more complex, from the monad up to man. The only direct evidence he adduces of such a fact is, that varieties are continually being produced, "no chick and no child is in all respects and particulars the counterpart of its brother and sister; in such differences we have variety incipient." I object here to the word "incipient," which I take to mean a beginning. From the hypothesis of evolution we would deduce the expectation of finding the varieties continuous. But in this case they have remained incipient ever since man has been known; how long that is, I prefer, in this case, leaving our opponents to determine. Now a variation that is always beginning, and at the same time always ending, is not a verification, but a refutation of an hypothesis, from which we deduce a variation always beginning and never ending. Again, the theory is that these variations are produced in the struggle for existence, by the preservation and accumulation of small inherited modifications, each profitable to the preserved being. If so, we are warranted in expecting that these preserved varieties must be in the first place actually beneficial; but Tyndall says they are "differential," that is, indefinitely small; but a differential advantage not only could not preserve the life of its possessor, which is the reason assigned by the theory for its transmission, but could not possibly be of any advantage at all.

If, again, the theory be sound, we have a right to anticipate that where an experiment has extended over at least 6,000 years—some would say 60,000—where the struggle for existence has been severe, and favourable variations have often occurred, some definite advance would have been produced. Such a case is that of man; no one can say he has had no struggle for existence. Take the case of the labourer, where development of muscle is so advantageous, and where use does develop certain muscles in a high degree. Now here is a distinctly useful modification; but are his children born with a more fully developed muscle than their father? Is the race of such men steadily growing more muscnlar? The reverse seems nearer the truth. Once more, therefore, the theory lacks the evidence needed for verification. But Tyndall says, and rightly, that "the function of the experimental philosopher is to combine the conditions of nature and produce her results"; but, he adds, "this was the method of Darwin." Here I differ from him, because I consider Darwin's experiments on pigeons, to which Tyndall refers, as being quite distinct from the methods of nature. He selected a variety that struck his fancy, and with his eye directed to the particular appearance which he wished to exaggerate, he selected it as it reappeared in sueessive broods, and thus added increment to increment, until, as he says, an astonishing amount of divergence from the parent type was effected. Here, then, we have wish, observation, intelligence, and voluntary selection, every one of which is a conscious state, and every one of which is wanting in nature. Am I justified from the evidence, that a conscious intelligence, having an end in view, can produce some slight useless variations, for such are those of pigeons, in inferring that nature without consciousness, without intelligence, and without a purpose, can produce endless beneficial variations? Am I warranted in inferring that, because a compositor can, by selecting the particular type he requires, arrange them into a connected statement; therefore, if you fling them on the floor, they will arrange themselves into a more difficult and longer statement? If I be, then I strangely misapprehend the nature of evidence; but if I am not, Darwin's experiments are of no evidential value whatever as to nature's method; and his hypothesis is not a good one, because in this case at least it is not in agreement with fact, does not allow of deductive iuference, and conflicts with known laws of nature.

He also instances Darwin's investigations into the cell-making instinct of the hive-bee as an instance of his analytic and synthetic skill, and in confirmation of evolution. That Darwin's experiments were most interesting, and afforded additional illustrations of the wondrous instinct of the hive-bee I gladly acknowledge, but that they afford evidence of this power having been acquired by natural selection I cannot admit. The experiments were made with hive-bees; that is, with bees already possessing this economical instinct, and could not, therefore, show how they acquired it. The hypothesis is that humble-bees have gradually evolved themselves into hive-bees; to prove this by experiment, he must collect a number of humble-bees together, see if they will swarm, and then, supposing them to swarm, watch whether they make any progress towards cell-building. When he has taken some steps in this direction with success, he will have commenced experiments affording important evidence, but not before. Another flaw in this explanation seems to be that the bees "transmit by inheritance their newly-acquired economical instincts to new swarms." Is this a fact? The bees that make the cells have no descendants, and the bees that have the descendants, the drones, do not make the cells; how then can they have the instincts without doing the work? Darwin has shown how it is useful for communities to have working insects which are neuters; but I cannot find where he attempts to show that non-constructing insects can transmit a constrncting instinct. The next important point to which attention is called, is the important doctrine of teleology. Tyndall says, "It is the mind thus stored with the choicest materials of the teleologist that rejects teleology, seeking to refer these wonders to natural causes. They illustrate, according to him, the method of nature, not the 'technic' of a man-like artificer." On this point Huxley speaks still more decidedly. "The teleology which supposes that the eye, such as we see it in man or one of the higher *vertebrata*, was made with the precise structure which it exhibits, for the purpose of enabling the animal to see, has undoubtedly received its death-blow." Nevertheless, it is necessary to remember that there is a wider teleology, which is not touched by the doctrine of evolution, but is actually based upon the fundamental proposition of evolution. That proposition is, that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed. If this be true, it is no less certain that the existing world lay, potentially, in the cosmic vapour; and that a sufficient intelligence could, from a knowledge of the properties of the molecules of that vapour, have predicted, say the Fauna of Britain in 1869, with as much certainty as one can say what will happen to the vapour of the breath in a cold winter's day. Why limit the prediction to the fauna, if we be, as he says we are, machines as much as the fauna; why not have been able to predict this paper this evening, and also the criticisms on it, if it be thought worthy of any? Why not predict the state of every man's mind and life at any particular moment? The one ought, by his hypothesis, to be as possible as the other. But as regards teleology, are all the phenomena of natnre to teach this, that by merest accident, according to Darwin, or by some unconscious force possessed by primitive nebulosity, according to Huxley, the eye for example just happens to be as it is, but that all the structure, every detail of which is so admirably adapted for seeing, had in its combinations no reference whatever to sight. That the fact that we are able to see with the eye and hear with the ear are only accidents, in accordance, indeed, with law, as all accidents are, but not the purposes of either; in fact, that they have no purpose; for if they have a purpose or end of any kind, that is teleological. Are we also to infer that those cases of—adaptation I was going to say, but may not, as adaptation, Huxley says, has received its "death-blow"—those cases where flowers and insects are mutually suitable, and which Tyndall himself quotes, are mere coincident suitabilities, the one having no designed relation to the other? All this may by its disciples be called inductive philosophy. Perhaps it is presumptuous in me, but I would call it by another name, as I cannot discover the inductions, still less the philosophy. It is wholly unnecessary for me, in this Society, to point out the overwhelming and accurate evidence in favour of teleology, which has superabundantly every test of a true theory. There is another doctrine coming prominently to the front now, which was only alluded to in the Belfast address, but which formed the subject of a masterly lecture by Huxley: I allude to automatism. There is difficulty in dealing with this subject, because the word has not yet been satisfactorily defined in its scientific application; one thing, however, is clear, that by animal automata are meant conscious machines. Huxley says "that consciousness is a spectator not an actor, that we are in fact conscious machines." The facts from which he infers this show a certain amount of involuntary, or what he calls automatic action; but they do not warrant the further inference that, because some actions are automatic, all are; that because our circulation, &c., is involuntary, our choice of evil rather than good is involuntary also. This is contradictory of consciousness, which testifies that volition is not a farce; that we can compare and select one action rather than another; that we can, if we will, choose the right and reject the wrong. If we be only machines, all terms of praise or blame are fallacious; there can be neither right nor wrong, virtue nor vice. But our whole moral consciousness testifies to the existence of these things; it is a fundamental law of our nature that we should approve or disapprove in certain cases; and consequently, whatever hypothesis contradicts this, must be so far unsound. The surest evidence we can have testifies that we are voluntary agents, and not involuntary machines.

Several other illustrations from Tyndall's address, as well as from evolution in general, might be selected to show that many of its inferences are from insufficient or untrustworthy evidence; that it often violates what we know to be laws of nature; that its deductions are but seldom verified; but what I have selected are sufficient for my present purpose. It must not for a moment be supposed that because evidence is sifted and explanations tested, the fullest investigation of nature is objected to; yet this is what our opponents often insinuate, or openly state. For example, Professor Roscoe says, in the conclusion of his lecture at Manchester on the atomic theory, "In order to flourish and produce fruit, science must be free—free to experiment and observe, without let or hindrance; free to draw the conclusions which may flow from such experiments or observations; free, above all, to speculate and theorize into regions removed far beyond the reach of our senses." To all this I am convinced every theologian will give a hearty assent: it is not knowledge, but ignorance we have to fear, either in our own department of thought or any other. What we do object to are conclusions that do not flow from observation or experiment, speculations that are not only beyond the reach of sense, but also of reason; the wandering, fancy free, in regions where the logician can find no solid ground for his foot, and consequently cannot follow. We object to the freedom which is untrammelled by the laws of observation, of inference, and of verification. And we object to these things more in the interest of science than of theology, because while science may be seriously hindered by the blundering of injudicious friends, or irrational votaries; the fundamental bases of theology are too firmly seated in the consciousness of humanity ever to be overturned by any amount of illogical reasoning on the part of its friends, or any amount of illogical rancour on the part of its foes.

The CHAIRMAN (the Rev. Prebendary Row)—having conveyed the thanks of the Institute to Dr. M'Cann for his paper,—observed, that he had carefully studied the general laws of evidence, but that he had given less attention to those which regulate the inductions of physical science than to any other branch of the question. No doubt the principles of the paper were capable of a far wider application than to this special subject, and the application of the principles contained in the latter part of it were of much value. That portion of the paper which dealt with the subject of transmitted instincts seemed worthy of great consideration, as the question was becoming one of grave importance in reference to the controversies of the day; but before any general theory could be laid down upon this subject, it would be necessary to collect a much greater number of facts respecting it than those already in our possession. He far from wished to dispute that instincts were in some way or other transmissible; but it was quite clear that we were not in a position to determine the law which regulated their transmission. The fact that the father of the working bee was a drone who never gathered honey or performed any labour in the hive, and the mother one whose exclusive business was to breed, afforded a conclusive proof that the instinct of the working bee was not a mere accumulation of instincts gradually acquired through a long succession of fathers and mothers. He made this remark because there were not wanting persons occupying a high standing in the ranks of physical science, who affirmed that the moral nature of man was merely the result of a mass of accumulated instincts gradually acquired in the course of an indefinite (nay, almost infinite) number of generations. No less unknown, he might almost say capricious, was the law which regulated the transmission of likeness, whether it were mental or bodily, passing over one or two generations, and reappearing in another; but the transmission of likeness in some way or other was unquestionably a fact. In the same manner there could be no doubt that many of our actions, and even of the operations of our intellects, were automatic. Many of his own mental operations were carried on in a manner that he was utterly unable to analyze the process by which they were performed. What was designated "cerebration" might account for some of these phenomena, but he did not think that it could account for all of them. Again, with respect to adaptation, more popularly designated design; any one who examined the structure of living organisms, and yet who denied that they testified to the existence of an Intelligence, seemed to him to maintain a most astonishing paradox. He was glad to find that the late Mr. J. S. Mill, in his posthumous essays, admitted the validity of this argument. He (Mr. Row) admitted that the argument from design had been unduly pressed in some cases; but it was manifest that the innumerable adaptations in nature could only be accounted for on the supposition that they originated in intelligence. What was the only substitute that scientific men who denied its existence could find for it? An infinite chain of happy coincidences and concurrences of events during the eternity of the past. Let us take one out of the innumerable instances of adaptation—the skeleton of a serpent in the British Museum, with perhaps not less than 300 joints, admirably fitted to each other, and to the whole; if these marvellous adaptations were to be accounted for by nothing but the principle of natural selection and survival of the fittest, it would require an eternity for the production of that serpent alone: what then should we say of the adaptations in nature which existed in numbers that surpassed all comprehension? One could hardly conceive how it was possible that men of high intelligence should have propounded such doctrines.

Mr. J. E. HOWARD, F.R.S., while expressing a strong general approval of the paper, did not think the description given of evolution was altogether correct; nor did he think that the account Professor Tyndall gave of the atomic theory was adapted to anything else but to mislead. The atomic theory of the old Greeks had about as much relation to the theories of modern science as Tenterden Steeple had to Goodwin Sands (according to Kentish traditions): there might, indeed, have been a connection in some way, but it was exceedingly remote and difficult to appreciate. It was equally misleading to speak of "the" doctrine of evolution, for the doctrine of evolution propounded by Tyndall was as different from the doctrine of Lucretius as it was possible to be.

The Rev. J. SINCLAIR said Dr. McCann had maintained that inherent and inherited qualities could not be the same, as they were incompatible; but as a matter of fact there was no incompatibility between the two. A quality might be inherited, and yet might be inherent, as being an essential part of a man's nature and constitution. The origin of that quality might be hereditary or otherwise; but if it were an essential part of the being, it was inherent. With regard to the evidence, he (Mr. Sinclair) doubted whether there was any difference between scientific and any other kind of evidence; or, in other words, whether there was any other than scientific evidence. With reference to teleology, he felt that something more than was contained in the present paper was necessary to refute the theory of Darwin and Tyndall. That theory was a perfectiy consistent one—that the instincts of an animal combined with the circumstances were sufficient to produce certain effects, or to increase, strengthen, or develop existing faculties of which the germ might already exist. There might thus rise up a perfect harmony between the faculties of a being and the circumstances in which it existed; the only question was as to the facts; as to hereditary transmission, there could be no doubt that qualities were so transmitted, and often from ancestors more remote than the immediate parents. Dr. McCann had referred to navvies and others whose work developed the muscular system, and pointed out that their children were not more muscular when they were born than were the children of other people; but there might be other causes to account for that; such as insufficient food or bad sanitary conditions, which would counteract the effect of the exercise of the muscles in the employment of the father. Some races were distinguished for their muscularity or for other qualities inherited through successive generations, from the exceptional employments of their ancestors. As instances of this, he referred to the hippopotamus-hunters mentioned in *Livingstone's Journal*, and to the New-haven fishwives near Edinburgh, who were distinguished for their great muscularity and strength.

Mr. M. H. HABERSHON pointed out (as bearing upon the question whether the development of muscle might be referred to the individual alone, or in a measure also to the transmission of quality), that the iron-workers of Staffordshire and Sheffield were examples of great muscular development, which seemed to indicate that persistence for a long series of years in a certain trade occupation had a marked effect on the physique of the people of the district. It was said, at the time of the Chartist riots, that a much greater number of troops would be required in the neighbourhood of Sheffield than among an agricultural population, on account of the greater muscularity of a race of men whose arms had great power from the daily use of the hammer. The sons of a race of blacksmiths would make stronger-armed blacksmiths than the sons of a race of printers or weavers. Among animals it was unquestionable that certain qualities developed by use were transmitted from generation to generation, and it would be easier to train a dog whose progenitors had been trained than one whose progenitors had not.

Mr. ROW asked, in reference to the peculiar qualities of pointers and setters, whether any dog was ever known to point or set at game without instruction, and simply through the transmission of qualities from one generation to another.

Dr. MCCANN said dogs had been known to point and set without instruction, but only very slightly.

The Rev. G. CURREY, D.D., remarked that in weighing scientific evidence care must be taken not hastily to conclude, because certain facts militated against any hypothesis as originally stated, that the hypothesis therefore was fundamentally wrong. It was possible that the hypothesis might have been too broadly stated, and so might need modification, and yet be in the main correct; or, on the other hand, it might contain a partial truth, which ought not to be overlooked, although the main hypothesis might not be sustained. This seemed to be the case in regard to the theory of Evolution. Careful investigation seemed to discredit the hypothesis that the whole of creation was governed by evolution as one universal law, and yet the same investigation left little doubt that evolution took place within certain limits. To assign these limits, was a work well deserving the attention of men of science; and if Mr. Darwin had been too hasty in his assumption of a general law, we were not to pass over the facts which he had observed, or to imagine ourselves concerned to deny all evolution under the general name of Darwinism.

Dr. E. HAUGHTON agreed that a scientific theory ought to be based upon facts; but before we were asked to believe that all living creatures came from one little monad or molecule, the facts in support of such a belief ought to be very startling indeed. He complained that the facts given in support of the doctrine of evolution were wholly insufficient to sustain it, and protested that there was no reason to believe that man had descended from a monkey because there were certain breeds of pigeons or of horses which differed from one another.

Mr. I. B. NICHOLSON complained that Dr. McCann's paper was not of a sufficiently elementary character for those who really required instruction: it assumed too large an amount of knowledge among those who heard it read. He asked that some definition of the meaning of teleology should be given.

Dr. McCANN briefly replied. Having thanked the audience for the kindness with which his paper had been received, he said that he did not think there was any action on the part of a human being which was altogether automatic, but the great difficulty in dealing with such questions was the absence of definitions. The word automatic had never received any adequate definition, and the result was that different people speaking of automata meant something quite different from one another. There was no analogy between a watch as an automaton and any conscious being; but in mental action there were certain moods in which the mind became to some extent mechanical in following out a line of thought. There was a latent mental mode in which the mind, although it acted voluntarily, yet acted almost unconsciously, but not quite, or we should not remember afterwards what we had thought about. In threading our way through groups of people in the streets, we voluntarily turned to the left or right, as circumstances might render necessary, but we were almost unconscious of any mental operation at the time. With regard to inherited and inherent qualities, whatever was essential or necessary for a being was inherent, and could not well be described as inherited. Inherited qualities were clearly something in addition to those which were inherent—they were not essential, but acquired. The inherent habit he had referred to in his paper was that of the bee, which, in making its cell, was carrying on an operation which had never been performed by either of its parents, for the working bees were the neuters which had no descendants. With reference to the muscularity of Sheffield workmen, he could only say that he had seen a good many Sheffield babies, and they were not a bit heavier, stronger, or more muscular than others. As a matter of fact, however, these children began from their earliest years to develop their muscles, because they were put to work at as early an age as possible. He quite agreed with Dr. Currey that it was not right to reject a whole theory because of one failure of verification; but it must be remembered that, in proportion to the value of the fact upset, was the theory weakened. As to the definition of Teleology, it simply meant purpose in the arrangement or contrivance of anything. If he had a distinct end in view in the construction of anything, that was so far a teleological act.

The CHAIRMAIN in closing the discussion said, it appeared to him that there had been some misapprehension in the minds of some of those present as to the distinction between evolution and natural selection. Darwin's theory was evolution by natural selection; but the theory of Lucretius was pure and simple evolution, without any reference to natural selection.

The meeting was then adjourned.