THE CRITERION OF SCIENTIFIC TRUTH

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CASSELL AND COMPANY, LIMITED

LONDON, PARIS, NEW YORK & MELBOURNE

1902

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SYNOPSIS.

Section I. deals historically with the criterion of truth formulated, or implied in various systems of philosophy before the middle of the eighteenth century.

Section II. shows from the history of modern science that there is a single definite criterion which has been continuously employed for testing scientific truth. This criterion may be expressed as follows :— When a logical arrangement of a whole class of reminiscences affords a mental picture of relationship among them, clearer, more direct, and better adapted for complete mental comprehension than any arrangement previously suggested, then that arrangement is accepted as a basis of scientific truth, and the relationship involved therein is held to be a true nexus. Thereafter it is necessary, by the examination of fresh experiences, to make sure that the arrangement does not exclude any of the reminiscences which appear to belong to the class in question, but when this is done there is no further criterion to which

scientific truth can be subjected. Hence it follows that the truth which is attained by the method of science is in some sense mutable ; and that it cannot claim validity except in relation to the minds by which it is perceived.

Section III. is intended to strengthen the argument from history by showing that the scientific criterion is one which would naturally emerge in the process of animal evolution.

Section IV. exemplifies the use of the scientific criterion by showing its application to questions of evidence.

DEFINITION.

In the expression for the criterion of scientific truth mention has been made of the arrangement of reminiscences. This is no doubt very closely related to what Locke called the "association of ideas," but I believe that there is no single English word denoting the process. To supply this deficiency I have employed the term *syntaxis* in the sense of *a process of mental arrangement*, especially a logical arrangement of reminiscences ; and by way of distinction I have taken the shortened form *syntax* to denote the *sequence of reminiscences* as arranged by syntaxis. In English the word had been monopolised by the grammarians, but in Greek it denoted any orderly arrangement, physical or mental.

SECTION I.

In all experience reminiscence is so closely bound up with sensation that for ordinary purposes the distinction between the two is neglected. Shakespeare uses a grotesque instance of this in his *Henry VI.* (Part II., Act ii., Scene 1), where the impostor Simpcox, who pretends to have been born blind, is made to describe two colours, the one as being "black as jet," and the other "red as blood," indicating that he supposed the reminiscences of jet and of blood to be part of his present sensation. The discrimination between that which is given in sensation and that which is supplied from reminiscence requires an effort of analysis, the necessity of which was not appreciated by the earlier Greek philosophers; an oversight to which some of their difficulties may be traced. It was this which led them to consider the senses to be deceptive, and thus enabled them to adopt, in all good faith, opinions which could only be upheld so long as most of their sensations were disregarded.

The classical instance which was used by more than one school of philosophers to justify this disregard, was that of a straight stick dipped obliquely

into water. This affords a visual sensation of a kind which they could compare with that afforded by a bent stick as seen through air alone. By means of this inappropriate syntaxis the philosophers arrived at the conclusion that they *saw* the straight stick bent in the water, and, instead of seeking for a more suitable syntax, such as that which is suggested to our minds by the idea of refraction, they accused the senses of deception. Yet they knew that an object when observed from a distance subtends a smaller visual angle than the same object when near to the observer, and they do not appear to have found anything deceptive in this, because the appropriate syntax was familiar and occurred to them immediately.

The earliest Greek philosopher of whose system we have any very definite record was Xenophanes (born 620 B.C.), the founder of the Eleatic school. His central doctrine was the all-embracing unity and unchangeable nature of "The One;" a doctrine which is comparable to, though not identical with, the theological idea of an all-pervading Deity. Relying upon their deductions from this doctrine, the Eleatics asserted that there is no such thing as change, and that all apparent changes are only deceptions of the senses. Heraclitus (535–475 B.C.) on the other hand held that, although the senses are deceptive, yet only those who do not reason rightly will be deceived by them. He asserted, not only that everything changes, but that change is the sum of all existence;

the deception of the senses consists in their falsely suggesting ideas of permanence.

The test of truth used by each of these schools seems to have been that what they pleased to assert was true, and that all other opinion was mere ignorance. A similar test has often been used in later times when any particular school of thought has acquired a predominance, but the coexistence of the two rival schools in the fifth century B.C. called attention to the desirability of some less arbitrary criterion. Accordingly Protagoras (480–410 B.C.) returned to sensation as the test of truth, asserting dogmatically that there is a fixed relation between the changes of the outer world and the changes of sensation, and that in this way "Man is the measure of truth." He pushed this principle so far as to declare that what is perceived by man exists; what is perceived by no man does not exist. Socrates, again (470–399 B.C.), seems to have been of opinion that truth, which was uncertain, or confused, in the complexity of concrete instances, could always be clearly and certainly perceived when a proposition was put into the form of a generalisation. He applied his philosophy, however, only to ethics, paying little attention to other subjects.

Both Plato and Aristotle show the influence of the two last-named teachers, but in different degrees.

Plato (427–347 B.C.) sometimes shows a leaning

towards the ideas of Protagoras, but the doctrines which are most closely associated with his name flow from an extension of the Socratic teaching into speculations beyond the department of ethics. He found the test of truth in generalisations, which, according to some of his writings, are known to be true because the mind had become acquainted with them in a previous state of existence, and remembers them in the present state. In the Phœdo he describes his method of proof thus: "Laying down some general hypothesis which I considered to be the best, I accepted as truth whatever squared with it, respecting cause as well as other things"—and "If any one impugn the hypothesis I should . . . . . defend it, assuming some other hypothesis yet more general, such as appeared to me to be the best, until I came to something fully sufficient."

Aristotle (384–322 B.C.) in some of his writings appears to reject this entirely; in others his dissent is not very decided. The main current of his thought, however, is nearer to that of Protagoras; he held that truth can be attained by collecting the evidences of sensation, but he is not explicit as to the method (he calls it the Art) of discovering from these the underlying truths which he sought. He writes, for instance, "Experience furnishes the principles of every science. Thus, astronomy is founded on observation; for if we were properly to observe the celestial phenomena we might demonstrate the laws

which regulate them" (Analyt. Prior. I. 30). He seems to have been the first to teach clearly that the so-called "deceptions of the senses" are only false deductions from sense impressions, which in themselves are not deceptive (De Anima, III. 3, etc.), though the same thing seems to have been dimly perceived by some earlier thinkers.

Among the Stoics who flourished in the third century B.C., Zeno, the founder of the school, seems to have been satisfied with the general statement that reason is the test of truth. His successor, Chrysippus, took as his criterion *τὴν καταληπτικὴν φαντασίαν*; which has been translated as "the sensuous apprehension." The phrase was adopted by the school, but different members appear to have understood it in rather different senses. It is, they said, "an impression on the mind," and Cleanthes compared it to the impression of a seal upon wax; while Chrysippus himself called the impression a modification of the mind comparable to the modification of the air by sound. This comparison would appear to the Greeks especially appropriate, because their whole language was based on the idea that the mind (or soul) was in its nature gaseous (conf. *πνεῦμα κ.τ.λ.*). The difficulty about the "sensuous apprehension" is that in order to give true results it must correspond to the real external object, otherwise hallucinations would give truth of the same kind as other sensations. The Stoics, however, held to their new criterion, and

tried to get over the difficulty by means of verbal subtleties.

The dogmatic assertions of these various schools naturally provoked a reaction, and several forms of the reaction are grouped together under the designation "Scepticism." The first sceptic who gave his name to a school was Pyrrho (360–270 B.C.), but as he confined himself to a denial of the possibility of any knowledge whatever, and held that no one assertion is more true than another (*οὐδὲν μᾶλλον*), he would not require notice here but for his connection with the later sceptics of the Academy. These philosophers asserted, with Pyrrho, that knowledge of truth is impossible, but they went on to admit that there are various degrees of probability. According to Carneades (213–129 B.C.) an idea is probable (*πιθανή*) when our impression of its truthfulness is derived simply from the idea itself. It is probable and undisputed (*πιθανή κάι ἀπερίσπαστος*) when the impression is confirmed by the agreement of related ideas. It is probable, undisputed and tested (*πιθανὴ και ἀπερίσπαστος κὰι περιωδευμένη*) when a careful investigation of all related ideas bears out the same conclusion. This substitution of probability for truth has been repeated in modern times; it appears, however, to be a rather clumsy device for emphasising that which is fully expressed by the simple statement of the relativity of knowledge.

There is no record of any further attempt to find

a rational basis of knowledge till comparatively recent times. The later Greeks were, in general, content to take as truth anything for which they could find authority in the accepted traditions of the particular school to which they had attached themselves. In the same spirit mediæval Christians referred every question to the authority of the Church ; a course which was more prudent because any deviation therefrom was visited by the most tremendous penalties. Even when the tyranny of this authority was at last breaking down, Copernicus (1473–1543) was obliged to introduce his heliocentric system of astronomy with an apology. The form which this took is memorable, because, hesitating as it was, it explicitly stated, for the first time, the principle upon which all science depends. He pleaded that, although the theory might not be true, yet it should be tolerated because it had the advantage of simplifying, and thus facilitating, astronomical calculation.

It will be remembered that in 1633, nearly a century later, Galileo was imprisoned at Rome and threatened with torture by the Inquisition for his advocacy of the Copernican theory.

Descartes (1596–1650) seems to stand at the parting of the ways ; for him there is a body of external truth dependent upon the will of the Deity, but known to man, presumably because it has been in some way revealed. "The metaphysical truths, styled eternal, have been established by God, and like the rest of

His creatures depend entirely upon Him." (Œuvres, VI. 109.) The test of truth is "the evident conception of a healthy and attentive mind, so clear and distinct that no doubt is left." (Œuvres, XI. 212.) Apparently, then, if on any question there be different opinions, that one is infallibly true which is the conception of a "healthy" mind; it will, moreover, be in accordance with "the truths styled eternal" which "have been established by God."

Descartes does, however, find an opening for a science which should depend to some extent upon observation, and he finds it nearly in the same way as Copernicus had done. In his "Principiæ Philosophiæ," III. 45, after premising that "undoubtedly the world was in the beginning created in all its perfection," he writes, "But yet, as it is best if we wish to understand the nature of plants, or of men, to consider how they may by degrees proceed from seeds, rather than how they were created by God in the beginning of the world; so, if we can excogitate some extremely simple and comprehensible principle out of which, as if they were seeds, we can prove that the stars and earth and all this visible scene could have originated, although we know full well that they never did originate in such a way, we shall in that way expound their nature far better than if we merely described them as they exist at present."

Many systems have, like that of Descartes, taken

the working of the mind itself as the test of the truth of the results of its work ; but Cartesianism differs from the others by reason of this attempt to include physical science within the scope of its inquiries. In spite of the great abilities of Descartes the attempt was a brilliant failure. Its most wide-reaching generalisation was the theory of vortices, purporting to explain the motions of the solar system ; an explanation which Newton showed to be utterly inconsistent with the observed appearances. On the physical side, therefore, the philosophy led to nothing, and its failure goes far to prove that the Cartesian test of truth is not valid when applied to science. Leaving the backwater of Cartesianism and returning to the main stream of thought, we go back to Bacon (1561–1626), who, writing somewhat earlier than Descartes, had put into literary form the new ideas about knowledge which were stirring men's minds at the time. That absolute truth could be ascertained, no serious thinker of the age seems to have doubted. Just as Descartes held that the metaphysical truths styled eternal have been established by God, so Bacon held that the relations among phenomena are fixed by divine decrees, and that it is the function of science to discover those decrees. In the preface to the "Instauratio Magna" we find the pious aspiration, "May He graciously grant us to write an apocalypse, or true vision, of the footsteps of the Creator imprinted on His creatures." The aim

of science being thus determined, it only remained for Bacon to discuss the method. He pointed out that traditional authority had proved an inadequate guide, and insisted again, as Aristotle had done, that observation must furnish the principles of every science; he went on to lay down a rule that no hypothesis should be framed until vast numbers of observations, unconnected by any syntax, should have been made and tabulated; and lastly he emphasised the necessity of testing every hypothesis by furthur observation, and, whenever possible, by experiment, that is to say, by observation under carefully arranged conditions. Although he nowhere states the grounds of his belief, Bacon evidently did believe that if his method were closely followed, the hypotheses which had stood the prescribed tests would correspond with the divine decrees for which he sought. He despised the Copernican astronomy which did not purport to reveal such a decree, and he wrote of Copernicus as "a man who thinks nothing of introducing fictions of any kind into nature, provided his calculations turn out well," a sentence which at once exposes Bacon's failure to appreciate the necessity that a scientific syntax should be the simplest and most direct whereby the mind can combine the given data.

A passage in "Paradise Lost," VIII., 76–84, seems to show that Milton, while he held the same opinion as Bacon regarding the aim of science, yet

expected that the divine decrees, when discovered, would approve themselves to the human mind as simple and intelligible.

The Archangel is made to say that God—

> "His fabric of the heavens
> Hath left to their dispute, perhaps to move
> His laughter at their quaint opinions wide
> Hereafter; when they come to model heaven
> And calculate the stars, how they will wield
> The mighty frame; how build, unbuild, contrive
> To save appearances, how gird the sphere
> With centric and excentric scribbled o'er,
> Cycle and epicycle, orb in orb."

Newton went still further; he felt the necessity of simplicity, and would forego none of the advantages to be gained thereby; but, like others of his time, he was convinced that science must be a discovery of those divine decrees which underlay, and caused, phenomena. To reconcile and combine the two ideas he boldly declared that simplicity is pleasing to the Deity. At the beginning of the third book of his "Principia" he sets forth the "Rules of reasoning in Philosophy," the first of which is, "We are to admit no more causes of natural things than such as are both true and sufficient to explain the appearances," and commenting on this he writes, "To this purpose philosophers (*i.e.* Aristotle) say that 'Nature does nothing in vain'; and more is vain when less will serve. *For Nature is pleased with simplicity,* and affects not the pomp of superfluous causes."

From all this it appears that in those times philosophers, poets, and men of science alike, tacitly assumed the presence of a power in the mind which would enable it, under favourable conditions, to transcend experience and to discover under phenomena an absolute truth, an entity which existed prior to, and was the real cause of, the phenomena. Such truth was far from being merely an intelligible way of regarding reminiscences in connection with one another; it was somehow in existence, and was equally the truth whether a mind perceived it or no. Newton evidently felt confident that he was disclosing such truth to the world, and it may have been this feeling which prompted his celebrated assertion, "Hypotheses non fingo."

This assumption as to the nature of scientific truth would not bear examination. Hobbes (1588–1679) pointed out that all science rests upon sensation, and that sensation is a change in the sentient organism, not a reproduction of some quality inherent in the external object and independent of the organism. It follows at once from this that science cannot be a knowledge of the absolute relations of external objects *inter se*, but only a knowledge of relations among sensations, that is to say, relations among the consecutive changes of the sentient organism. From this, again, Hobbes was led to his theory of the association of ideas. Locke (1632–1704) afterwards worked independently in somewhat the same direc-

tion. It was he who noted that the kind of knowledge accesssible to science is that which is useful for the guidance of action, and he laid much stress on this, although it was of course impossible for him to see the importance of the fact in its bearing upon evolution. Hume (1711–1776) accepted the position of Hobbes and Locke that knowledge originates through sense impressions, as being the best which was available at the time; but he showed that no completely intelligible and consistent syntax of the genesis of knowledge was possible, even on this basis, so long as it was associated with the psychological theory then current. The theory was that each momentary sense impression should be regarded as an isolated unit having no organic connection with any previous impression; and Hume saw clearly that no mere collection of such isolated units could constitute knowledge. His predecessors had been more or less aware of this also, and had tried to supply what I may call metaphysical connections between the separate units; he examined these connections and found them unintelligible, or to use his own term "irrational." Owing to the current misconception of the nature of science, and to the rudimentary condition of physiology at the time it was not open to Hume to adopt a more consistent psychological theory; all he could do was to point out the difficulties, and to adopt an attitude of scepticism, or suspended judgment, till the means of overcoming them

should be within reach. It seems to me that most of these difficulties have at last been overcome, and that a consistent syntax of the empirical genesis of knowledge, that is to say of scientific truth, can now be formulated. But first it is necessary to inquire what is meant by the phrase "scientific truth."

SECTION II.

THE term scientific truth could not properly be applied to a mere personal statement of the immediate sensation of an individual. Thus, if a man feels hot he cannot doubt the fact of his sensation; it is a true fact for him, but it is not in itself science. Moreover, as Hume very clearly saw, no collection of such sensations could constitute scientific knowledge. Locke had already recognised that such knowledge must be the guide of action; but the immediate sensation can, of itself, give no further guidance than this—that action should be continued so long as sensation continues pleasant, and should be intermitted when sensation ceases to be pleasant. This elementary guidance appears to be sufficient only for the very simplest organisms; no animal which attempts to escape from its enemies by flight can be supposed to commence and continue that flight merely because the action is, in itself, pleasant.

The knowledge which affords effective guidance for action depends upon the combination of sensations; but what I have called the immediate sensation is in its nature transient, and cannot be combined with other sensations unless they be simultaneous; therefore the combinations of sensation which consti-

tute knowledge must be combinations of those secondary sensations which we call reminiscences. Hence it is necessary to have some definite idea of what is meant by reminiscence before we can form any satisfactory syntax with regard to knowledge in general, or scientific truth in particular. Now animals, as well as human beings, have reminiscences, and use them in some way as a guide to action, so that any complete theory of reminiscence must rest upon that which is common both to animals and to ourselves, namely, the nature and functions of the nervous system.

The primary function of a nerve in relation to muscular tissue may be compared to the chemical action of a fuse in a cartridge, the ignition of the outer end of the fuse corresponding with the stimulus which excites the nerve. The liberation of energy at one point disturbs the unstable equilibrium and sets free potential energy at successive points along the fuse in one case, along the nerve tissue in the other, and ultimately determines an explosion, or a muscular contraction, respectively. In less simple forms of nervous function the initial disturbance is transmitted, not directly to the muscle, but to the nervous centres, where it enters into the complex of the simultaneous nerve disturbances arising from other stimuli, and in this way the whole organism may become involved in the response to any stimulus. In all cases, however, the normal outcome of a nerve

disturbance is to be looked for in an alteration of the condition of some other tissue, most commonly muscular tissue, the contractions of which are excited and controlled by these disturbances.

When a fuse is ignited there is one path, and only one, along which the energy can be liberated; but in a complex nervous system the possible paths of discharge are innumerable, and the actual path depends on the particular state of the unstable equilibrium of the organism at the moment. It would appear that, whatever be the path taken, the discharge of energy along it makes a more or less permanent alteration in the condition of the nervous tissue along this path in such sense that it becomes easier for subsequent discharges to pass thereby than to take new directions. Thus it comes that whenever energy is liberated at any point along an established track, some part at least of that energy traverses the track, and more or less strongly reinstates the feeling which was the sensory aspect of the earlier discharge along the same track. When the reinstated feeling emerges into consciousness it is called a reminiscence; but evidently the same process may go on without this emergence, because the same repetition of disturbances along previous tracks produces habits which assert themselves without our consciousness. In view of this, that which I have called the immediate sensation can no longer be looked upon as an isolated unit; it is a disturbance of equilibrium, which at any

point of its course may initiate secondary disturbances, reinstating previous feelings, while these again may set up other disturbances ; so that every sensation is seen to be organically connected with an indefinite number of previous sensations. The difficulty, then, will be, not to make some connection, but to select out of the innumerable possible connections those which shall be most useful. This process of selecting and arranging reminiscences in usable connection one with another, is what I have termed syntaxis.

In the absence of any idea of an organic nexus between different sensations, philosophers had imagined metaphysical connections, constructed, as it were, artificially by an entity which they called The Mind. Hume examined the theory current in his time with regard to one of the principal of these metaphysical connections, namely, causation, and found it, as he said, "irrational." Now, the terms *cause* and *effect* are still used very loosely even by the most careful writers, but there is nothing irrational about the meaning of the words in their strict scientific sense. They denote an identity looked at under different aspects, or in different phases. Thus, when the elasticity of a stretched string is said to cause its return towards the position of equilibrium, the potential energy of its displacement is looked on as being identical with the kinetic energy of its return, but as exhibited in a different phase. When vibration is said to cause sound, the

meaning is that the same stimulus which acts on our visual and tactile senses as a tremor, acts on our auditory sense as a sound. In this scientific sense the terms cause and effect simply denote a particular mode of syntaxis. Similarly other forms of connection, such as time, space, matter, ether, and the like, will be found on examination to indicate other modes of syntaxis.

Every conceivable connection among reminiscences, simply because it is conceivable, is a possible syntax; but evidently any one syntax is not as good as any other; the whole problem of science turns on the distinctions between them. For centuries past the question has been discussed:—What criterion should be applied to distinguish a good syntax from a worthless one, or, as it is generally expressed, a true proposition from a false one? Instead of attempting a direct answer, what I propose to do here is to consider a somewhat different question, namely: What is the criterion which, as a matter of history, *has* been applied in science to distinguish truth from error?

In analysing the history of science with a view to isolating such a criterion, it is important to notice that the syntaxes of science have not been immutable, but that a syntax which has been accepted at one period as true, has often been rejected at a later period. If we find then, that all syntaxes are accepted when they possess a certain characteristic,

but are rejected when they possess it no longer, the inference will be that this characteristic is the one criterion by which the valuable syntax has been distinguished from the useless; the true from the erroneous.

My whole object here is to show that this criterion corresponds to the formula given in the heading of the paper, namely: When a logical arrangement of a whole class of reminiscences affords a mental picture of relationship among them, clearer, more direct and better adapted for complete mental comprehension than any arrangement previously suggested, then that arrangement is accepted as a basis of scientific truth, and the relationship involved therein is held to be a true nexus.

To prove the universal use of this criterion it would be necessary to go through the whole history of every branch of science, which is manifestly impossible; all that can be done here is to give a few illustrations of its use; and in doing this it will be best to take our examples from the older branches of science, because these have had a longer time for their development, have passed through more changes, and have produced results which are now more universally recognised than those of the more recent branches. Turning then to astronomy, the oldest of them all, we find that the earliest syntax relating to the motions of the heavenly bodies was animistic; that is to say, men, observing these motions, compared

them with the voluntary motions of their own bodies, and supposed that the celestial motions were also voluntary, until, in time, a certain regularity of these celestial motions suggested a mechanical instead of a volitional syntax. The early Greek astronomers held that the moon and sun, the planets and the stars were fixed upon hollow crystal orbs, or spheres, one outside the other, and that by the movement of these hollow orbs the visible heavenly bodies were carried round in orbits, of which the earth was the centre. This would, in a way, account for any movements which they were able to observe, but would not supply a convenient basis for calculation, since the movements of the hollow orbs could not be taken as uniform rotations about fixed axes through the earth's centre. Hipparchus, however, in the second century B.C., brought this mechanical syntax into close, and calculable, agreement with his lunar observations by supposing that the hollow orb carrying the moon revolves uniformly about an axis which does not pass exactly through the earth. Such a supposition introduces the idea of an epicycle; a mechanical syntax, the use of which was extended by Ptolemy, some two or three hundred years later, for the explanation of the motions of the planets. Concerning this, Mr. W. R. Ball writes, "The idea of eccentrics and epicycles on which the theories of Hipparchus and Ptolemy are based has been often ridiculed in modern times. No doubt, at a later

time, when more accurate observations had been made, the necessity of introducing epicycle on epicycle, in order to bring the theory into accordance with facts, made it very complicated. But De Morgan has acutely observed that in so far as the ancient astronomers supposed that it was necessary to resolve every celestial motion into a series of uniform circular motions they erred greatly; but that if the hypothesis be regarded as a convenient way of expressing known facts, it is not only legitimate but convenient. It was as good a theory as, with their instruments and knowledge, it was possible to frame, and, in fact, corresponds to the expression of a given function as a sum of sines or cosines, a method which is of frequent use in modern analysis." ("A Short Account of the History of Mathematics," p. 91.) No farther advance in the theory of astronomy was made until the time of Copernicus, whose book appeared in the year of his death, 1543. It is very noteworthy that Ptolemy, in the second book of his astronomy,* pointed out that the explanations would be much simplified if the earth were supposed to rotate on its own axis once a day; but he said that this was inconsistent with known facts, whereby he meant that it could not be brought into consistent relation with the

* This work is generally known as the "Almagest," which is said to be derived from an Arabic corruption of the Greek μεγίστη (μαθεματικὴ) σύνταξις.

dynamical theory then current. One of the chief difficulties was that if a stone be thrown straight up from the earth, it falls again to sensibly the same place from which it started, whereas, according to the dynamics of the time, the earth, if it had a motion of revolution, should have moved onwards during the time of flight, leaving the stone to fall behind its original position.

Copernicus insisted again on the astronomical simplification to be gained by supposing the earth to rotate, and on the farther simplification gained by supposing it and the planets to revolve round the sun. With fortunate audacity he disregarded the dynamical difficulty, but it was not till the publication in 1632 of Galileo's "Dialogues on the System of the World" that this difficulty was finally overcome by the introduction of a better dynamical theory. Then, and not till then, did the Copernican theory afford a mental picture of relationship among a whole class of reminiscences, clearer, more direct, and better adapted for complete comprehension than any previous syntax, and accordingly it was promptly accepted by all astronomers. The theory of astronomy had been stationary since the time of Hipparchus: the practice had progressed but little, and very slowly, since the time of Ptolemy; yet in little more than half a century from the publication of the simpler system of Copernicus, this had led to the discoveries of Kepler, and these again in

about another half century led up to Newton's "Principia," from which we trace all the developments of modern astronomical science.

Another illustration may be drawn from the two theories as to the phenomena of light. Hooke had suggested that the eye should be supposed to receive the stimulus of light from the luminous object through the modifications of an intervening medium; modifications which might be compared to undulations. Newton examined this theory and even proposed certain improvements in its details, but ultimately rejected it in favour of the theory that the stimulus is conveyed to the eye by means of material corpuscles emitted from the luminous body. Either theory provided an intelligible syntax of such phenomena as reflexion, refraction and the like; but in the seventeenth century no syntax had been found connecting undulations of light with the colours of thin plates; moreover, Newton was particularly impressed by the fact that waves of light had not been observed to lap round obstacles in the same way as waves of sound. At the time, therefore, it appeared that the observed phenomena could be co-ordinated in a clearer and more direct manner by the corpuscular than by the undulatory theory, and, although the latter was supported by Huygens, it was not generally accepted. At the beginning of the nineteenth century, however, Young and Fresnel turned their attention to the polarisation

of light, and to what are called the phenomena of interference; showing that these are much less intelligible on the corpuscular than on the undulatory theory. At the same time they suggested ways of surmounting many of the difficulties which had prevented the acceptance of the latter; hence it came, in its turn, to afford a clearer mental picture of relations among all the phenomena than the other. Nothing beyond this is required of the theory, which is accepted as being scientifically true in spite of the difficulty of imagining clearly any kind of medium which should be capable of transmitting undulations such as those required under the given conditions.

Take, lastly, the illustration offered by the two theories of heat. Bacon, in his "Novum Organum," had given his opinion that "the very essence of heat, or the substantial self of heat, is motion and nothing else." But he failed to show how any relation among the phenomena of heat could be clearly represented to the mind on these lines, so that his proposition remained a mere form of words expressing no usable syntax. In Newton's time and down to the end of the eighteenth century the clearest idea of relationship among the phenomena of heat was derived from the hypothesis of an imponderable fluid, termed "caloric," which entered or left ponderable bodies according as they were heated or cooled. Benjamin Thompson, better

known as Count Rumford, was the first to give scientific reason for rejecting the hypothesis of caloric and accepting the theory that heat is a mode of motion. Rumford had made experiments on the quantity of heat given off by bodies as the result of friction, and had not been able to find a limiting value of this quantity. His deduction therefrom is given in these words :—"Anything which any insulated body, or system of bodies, can continue to furnish without limitation cannot possibly be a material substance ; and it appears to be extremely difficult, if not impossible, to form any distinct idea of anything being excited and communicated in the manner that heat was excited and communicated, except it be motion." This is a definite statement by a competent authority that the grounds on which he adopted the dynamical theory of heat, and rejected the caloric theory, were the very grounds on which I am endeavouring to show that all scientific theories are respectively accepted or rejected. The direct arguments for the dynamical theory of heat are, to this day, based upon experiments similar in character to those of Rumford, and, as far as I am able to learn, no scientific writer has ever impugned the sufficiency of the argument which Rumford used.

Here we have three typical instances in each of which one scientific theory has been rejected and another accepted. In two of them we have the

express statement of the propounders of the theories that they were put forward for the sake of simplicity, or else because it was "difficult to form any distinct idea" of a different connection among the reminiscences involved; while in the remaining instance the grounds upon which the one theory was preferred at one period, and the alternative theory at a later period, are known from historical records to have been of the same kind. Moreover, throughout the whole history of science no instance will be found in which a syntax which is admitted to present the clearest and most direct picture of relationships among all the reminiscences involved therein has failed to gain acceptance as a scientific truth.

Now, if this be the single characteristic which determines the acceptance of a scientific theory—the sole criterion of its truth—there can be no ground for supposing that any such theory, after it has been accepted as true, acquires the validity of a pre-existing decree, or becomes anything else than what it was in its inception, namely, a clear, direct, and intelligible syntax or manner of arranging reminiscences. When a syntax is to be translated from thought into action it seems to be advantageous—perhaps it may be even a necessary part of the translation—that the reminiscences should be projected into that world of the not-self which is called by metaphysicians objective. But the reminiscences

are not thought of as isolated units ; they form parts of the syntax, and thus the syntax itself is also projected, and is looked on as part of an external world. My own idea is that this externalisation always occurs when a syntax passes into action without conscious hesitation ; the hesitation being a sign that the syntax has not yet been repeated so often as to form a fully established track among the nervous connections. Certainly the fact that externality is attributed to a particular syntax does not establish it as an unquestionable truth. The Ptolemaic astronomers of many centuries attributed external existence to the immovable earth which formed the centre of their system ; the Newtonians to the corpuscles which conveyed light from a luminous body to the eye ; men of science before Rumford, to the imponderable fluid which they called caloric : yet none of them are in any sense true for us.

SECTION III.

ANY given reminiscence may have a place in an indefinite number of trains of thought which we may picture to ourselves as intersecting one another in that reminiscence. Hence, by continual crossing and recrossing, our syntaxes form a sort of network, the different threads of which must fit one to another at each point of intersection if the whole structure is to be logically satisfactory when regarded as a single comprehensive syntax. For example, a seaman in sight of land may calculate his position in at least three different ways, namely, by astronomical observation, by dead-reckoning, and by the appearance of the coast. Here the different lines of thought converge towards the determination of the position of the ship. Evidently they cannot all be satisfactory unless they be compatible one with another in respect of their point of convergence, or intersection ; that is to say, unless they all indicate the same position. The establishment of such compatibility tends to increase confidence both in the correctness of the result, and in the validity of the various steps of the syntaxes by which this result was obtained. Again, we know that Ptolemy had appreciated some of the advantages of an

astronomical system in which the earth should be supposed to revolve on its own axis once in twenty-four hours, but that the acceptance of this system was delayed for more than a thousand years because it was not compatible with the dynamical theories which held the ground during that time. When a more consistent dynamical syntax was put forward by Galileo the objection disappeared, and the Copernican astronomy was at once accepted by all who were competent to judge of its value. It is to be noted that Galileo's dynamics would have been equally compatible with the Ptolemaic astronomy; they did not afford a proof of the Copernican theory, but, by providing a dynamical syntax with which this theory could be reconciled, they removed the most important obstacle to its acceptance.

Applying these illustrations to my present subject: I have, I hope, shown in the previous section that throughout the history of modern science the criterion of truth in actual use has been the one which I have formulated above. I cannot offer more in the way of direct proof, but perhaps objections may be obviated and the proposition may be made more acceptable if it can be shown to be consistent with the theory of evolution that such a criterion should naturally come into use in the course of nervous, that is to say mental, development.

In the simplest forms of animal organism nerves

are not yet differentiated, but every part of the body reacts directly to stimulus. At a rather later stage of evolution we find that nerves have been developed, and that these run in definite directions; from which it seems natural, on any theory of evolution, to suppose that even before the specialisation of tissue to the performance of nervous functions, the liberation of energy by stimulus had usually followed these paths, and that the repeated occurrence of the changes involved in such liberation had left traces in the permanent modification of structure; modifications which may be looked on as constituting an elementary form of memory. According to the selective theory of evolution, the factor which determines the perpetuation and progress of any modification of structure is the advantage which that modification gives to the organism. Now the advantage given by nerve specialisation must have been mainly the better guidance of action. It is difficult to suppose that, in the earlier stages of evolution, this guidance went much farther than to ensure the persistence of activity so long as sensation remained pleasant, and the intermission of activity when sensation became unpleasant. The first step towards the development of anything that could be called a mental function would probably be taken in respect to the obtaining of food. When organisms had developed in such a

way that their nervous energy discharged itself along definite paths, and in so doing reinstated previous sensations, thus affording a basis of reminiscence, it would follow that the animal which became capable of combining and arranging its reminiscences so as to connect certain sensations with the presence of food within its reach, would have a great advantage over animals which could only seize upon any conveniently shaped substance near them, endeavour to absorb it, and thus try whether it could, or could not, be assimilated. This advantageous power of arrangement, or syntaxis, having been once developed, would be perpetuated and increased in the usual course of natural selection, but might conceivably take many different forms. That form of syntaxis, however, would have the best chance of survival which was the simplest and most direct; both because it could be used by organisms which were incapable of any other, and also because it could be most rapidly translated into action; promptitude in seizing food being important where the competition for it is keen. Another early application of the power of syntaxis must have been to the escape from enemies. Now this may be effected either by combat, by flight, or by concealment; but whichever course be adopted, quickness of decision is most important, so that in this respect also the animal would have the better chance of survival

in proportion to the simplicity and directness of the syntaxis employed. Among all the possible forms, therefore, which syntaxis might assume, the simplest and most direct would have the advantages (i.) that it could be used by a larger number of organisms than any other, (ii.) that these organisms would more easily obtain food in times of scarcity, and (iii.) that they would be more likely to escape from their enemies; so that the criterion which we have seen in use throughout the history of science, has its foundation in the conditions which were necessary for the survival of nervous organisms long ages before the evolution of the earliest vertebrate. With regard to this theory that our present mental functions are determined by heredity working through the advantage gained by the guidance of action, I may note here, by way of indirect confirmation, that it gives equally intelligible results when applied to other aspects of the problem of syntaxis. For instance, the question arises, how do we come to accept our reminiscences as being in any degree reinstatements of former sensations? That we do accept them as such is indisputable, yet we have no guarantee that they are reinstatements, beyond the fact that we feel as if they must be so, and the feeling is frequently found to be misleading, because different reminiscences are so contradictory that if they be reinstatements at all they must be extremely confused and

inaccurate. Certainly, if it be once granted that there is some correspondence between reminiscence and previous sensation, then it becomes possible by means of records, to test the closeness of that correspondence, but before such records can be used it is necessary to have some reminiscence connecting the record with the previous sensation, and we are thus brought back to the original question, how do we come to accept any reminiscence whatever as being a reinstatement rather than an entirely new experience? Now it is evident that organisms which did not, in practice, take their reminiscences as representing previous sensation could not use them for the guidance of action, and failing such guidance they would not succeed in the struggle for existence. Thus our feeling that reminiscence is a reinstatement of previous sensation must be regarded as an inheritance from innumerable generations of ancestors, and on this feeling we necessarily act, in spite of our knowledge that the reinstatement is often extremely imperfect, Take again the much discussed question, who do we come to attribute externality to stimulus? —or in other words, seeing that our consciousness is concerned only with changes of sensation in ourselves, how comes it that we so universally attribute the origin of these changes to an external world of the not self—a world of whose existence sensation cannot inform us? This also would follow as a direct result of heredity. I have referred above to

organisms in a stage of evolution so primitive that we may suppose immediate pleasantness of sensation to have been their sole incentive to action. To such organisms it might be of no importance whether a stimulus were, or were not, external; but beyond this stage it seems evident that no animal could maintain itself in life if it did not act upon the vast majority of its sensations as though they were due to external stimuli. Those organisms whose nervous systems reacted in this way most readily, would have a decisive advantage, and this is fully sufficient to account for their transmitting, to their descendants, nervous systems which react, normally, in the same manner. It will, I think, be found that when action is to be taken, a mental reference to an external world can hardly be avoided; though in the case of dreams, hallucinations and like, where no action is involved, we may be able to dispense with the idea of the externality of the stimulus.

Recurring once more to the main subject; the simple and direct syntax has been shown to constitute the basis of all that has been accepted as true in science, and reasons have been given for supposing that it represents the line of development of nervous function which was most conducive to survival, at least among animals; but I do not suggest that it is the only form of syntax prevalent among men, or even that, for other than scientific purposes, it is at all universally regarded as the

best. On the contrary, there is a large, though decreasing, class of men who admire intricacy and even confusion of thought, while they think meanly of syntaxis which is merely simple and direct; hence they either contemn science altogether, as did our ancestors in the Middle Ages, or else they adapt it to their taste by substituting fanciful obscurities in place of those clear syntaxes by which alone science is differentiated from quackery. Among men of this type the prevailing idea seems to be that a syntax involving the unintelligible is a mystery, or a special kind of knowledge, higher than any which can be attained by the understanding; an idea which may or may not be well founded, but which is in any case beyond the reach of argument; for this at the best can only demonstrate that, of the syntaxes under consideration, one is clearer and more direct than another; if the clearest syntax be then deliberately rejected, argument fails.

I will now recapitulate the main points of the three sections. Looking first to the history of Philosophy: many of the early Greeks appreciated the importance of the proposition that scientific truths must necessarily be clear and intelligible, but were misled by the fallacy that the evidence of the senses is in itself untrustworthy. Aristotle, and perhaps one or two others, held the opinion that the senses are the principal sources of knowledge, but they did not found any consistent system of philosophy upon

this opinion. When the question of the origin of knowledge was next discussed from an independent philosophical standpoint, Hobbes and Locke were able to show that no basis of scientific knowledge other than sensation is possible, and Locke pointed out moreover the practical nature of such knowledge, which is essentially of the kind suited for the guidance of action. Hume took up the question where Locke had left it, and found that although knowledge had been shown to rest, in some way or another, on a basis of sensation, yet an intelligible theory was required as to the mental process by which the two were connected. He did not himself propose any such theory; he indicated very clearly the nature of the problem, but was obliged to leave it unsolved because at that period the data necessary for a solution were not available. Hume was about sixteen when Newton died, so that science, in the modern sense of the term, was still of very recent growth and its nature was not yet thoroughly understood. We have seen that Newton and his contemporaries imagined it to be a disclosure of pre-existing decrees whereby phenomena are caused and governed; a discovery which obviously could not be attained through the senses. It has been shown, however, from the subsequent history of science, that in the course of its development many hypotheses have been changed; that in every case the change has been made because

the later hypothesis affords a mental picture of relations among all the reminiscences involved, clearer and more direct than that given by the earlier one; and that in no instance has there been any attempt to prove scientifically that either hypothesis enunciates a pre-existing decree, or that one represents an external reality any more than the other. This seems to show that science consists in the orderly and intelligible syntaxis of our own reminiscences; a process which may very well be part of the function of the nervous organism. I have further endeavoured, in this third section, to show that, from a very early stage of evolution, the power of syntaxis would be advantageous to all nervous organisms in respect of the guidance of action, and that the advantage thus obtained would be sufficient to determine the lines of the development of nervous function in our progenitors, and ultimately in ourselves.

SECTION IV.

If it be admitted that a scientific syntax, in order to be satisfactory, must include all the reminiscences which can be brought together, relating to the matter in hand, it is evident that a place must be found among the rest for reminiscences of information obtained from the evidence of others; hence it will not be inappropriate to illustrate the use of the scientific criterion by showing its application to the question of the truth of evidence. We are sometimes told that evidence should be accepted or rejected, according to its probability; but in the absence of any criterion of probability, the phrase affords no guidance, since the estimation of the probability of the evidence is nothing else than the formation of an opinion as to its truth; the two expressions merely describe the same process in different words. The question whether a particular piece of evidence is trustworthy, and if so, to what extent—that is to say, the question of the weight of that evidence—is in its own way a minor scientific problem, and the only reasonable method of dealing with it is to use the same criterion which I have shown to be applicable to other problems. Just as the criterion has long been practically applied to these, so it has also been applied to questions of

evidence, but hitherto without any explicit recognition of the nature of the procedure. Let us begin by inquiring what it is to which the test applies. Evidence at first hand is sometimes supposed to be simply a record of the sensations of the witness; but this never is so in practice, for we have not words to describe many simple sensations, and if we had the words, the use of them would still involve the syntaxis of reminiscences of previous sensations with which to compare those described. Thus, nothing could appear to be more directly a record of sensation than the statements of a patient to the doctor at his bedside; yet the simplest of these involves a syntax which may be misleading even when the statement is made in all good faith. Many a patient has complained of pain in the toes of a foot which the doctor knows to have been amputated—one more proof, the ancient Greeks might have said, how little truth there is in the "lying witness of the senses." A clearer arrangement of ideas is obtained by imagining a separation between the sensation and the syntax with which it is associated; the sensation itself is not misleading, but it is connected in the nervous organism of the patient with reminiscences of former sensations which he had learnt to refer to his toes; later, when he has acquired a fresh set of reminiscences, if the same pain be felt it will be referred with equal confidence to the stump of the amputated

limb. The doctor makes the suggested distinction in a practical way; accepting the evidence that the patient feels pain, a d dismissing the syntax that the pain is in the toes, he turns his attention at once to the dressings of the wound. Another illustration of the dependence of evidence upon syntaxis has been given at the beginning of Section I.

As a general rule, then, no evidence of any scientific value consists entirely of records of the direct sensations of the witness: and farther than this, it will be found in practice that direct sensations, though always implied, are seldom explicitly mentioned, except in reference to disease. When evidence does contain a statement as to direct sensation, the question arises whether this is the best description of the actual sensation which the particular witness can offer. It is generally recognised that the basis for an answer to this question is to be obtained by noting the looks and actions of the witness as well as his words; the criterion is to be applied by forming the simplest and most direct syntax which combines these elements with reminiscences of previous experience relating to the case; reminiscences, for instance, in medical practice of the usual symptoms of any disease from which the witness might be suffering. Thus the statements of begging impostors and malingerers are often to be rejected as being wholly false, while the exaggerated statements made by patients suffering from hysteria, or

some other nervous disorder, may be the best which can be offered by witnesses in their condition, and may be of real assistance in diagnosis. "Swearing that black is white" is a proverbial example of unscrupulous lying, but a witness giving evidence in perfect good faith might swear that red was green, if he were colour-blind. Seeing, however, that all statements involve syntaxis, and that comparatively few contain explicit reference to anything else, it is of much more general importance to consider the truth of evidence in relation to this. The ideal evidence would be a record of the witness's whole series of mental operations, so far as they had any relation to the matter in hand. This is unattainable; but evidence is not of the best kind unless it contain sufficient indications of the main syntaxes, or processes of thought, through which the witness passed in coming to the conclusions which he states. I hold farther, that evidence is not given with perfect openness if the witness intentionally suppress these indications in regard to syntaxes which he remembers to have considered important, whether they be compatible with his main conclusions or not. But witnesses, however willing, are seldom able to give much information of this kind; to the witness himself the syntaxes are of less importance than the conclusions, and he will remember the latter vividly when he has only vague and confused reminiscences of the former. It is one of the obstacles to a scientific study of

history that, in old times, men hardly ever went out of their way to indicate the line of thought which led them to make any particular historical statement; we can sometimes supply the omission by conjecture, but the value of the historical evidence in this case is much less than it would have been if the syntax had been plainly indicated by the original writer. Again, men seem especially apt to forget any syntaxes which they have not been able to bring into harmony with their main conclusions, and it is this forgetfulness which constitutes most of the bias often recognisable in evidence otherwise trustworthy.

The first question to be investigated regarding the syntaxes involved in evidence is whether they are the best which that particular witness could make, which is equivalent to an inquiry whether the evidence was given in good faith. It is recognised that the things to be considered here are—any circumstances which might have induced a desire to mislead, any indications of the character of the witness, and any examples of the kind of evidence given by him in other matters, especially in similar matters. Besides these, account should be taken of the kind of syntaxis which is usual among men in an environment similar to that of the witness. Here, again, the scientific criterion is applied by forming the clearest and most direct syntax which can be made to include all the available reminiscences bearing upon these points.

But when the question as to the good faith of the witness has been decided there remains another most important question to be considered, entirely distinct from the former, namely, are the syntaxes involved in the evidence the clearest and most direct which are possible for us? The mere statement of the question in this form implies the scientific criterion; but of course the usual form of statement is quite different. I think that the customary inquiries concerning evidence may fairly be put into the typical forms:—"Is the witness telling the truth?" And, "If he tells us what is not true, is this done intentionally, or by mistake?" Now if truth be supposed to represent some correspondence, hitherto inexplicable, between ideas and an external reality independent of our senses, and if it be farther supposed that men are somehow able, normally, to discover such truth; then what is true for one man is true for all men at all times, and any one whose opinion diverges from this truth may be said to make a mistake, for he falls away from a fixed normal standard according to which there should be no mistake. Upon such a theory, that form of question regarding evidence which is the most usual would also be the simplest and most direct. If, however, truth, in matters of science, be nothing else than the clearest available syntax of all the reminiscences which can be found to relate to the matter in hand, then that which

is true for one man may not be true for another, or even for the same man at a different time. When this view is taken, the ordinary form of inquiry appears inadequate, since it fails to distinguish between the different aspects of truth under the different conditions. Moreover the nervous system discharges its functions normally by arranging reminiscences in the syntax most satisfactory to the individual; some men will use the most direct syntax which they can find; others seem more apt to use syntaxes wherein the connections are unintelligible even to themselves; but the results of either kind of syntax may be recorded in perfectly good faith. If in reviewing the evidence we are able to form therewith a syntax clearer and more direct than that of the witness, our syntax represents what in the scientific sense is true for us; we may consider the witness to have been ill-informed, or grossly ignorant; his syntaxis may have been unscientific, or utterly confused; but it does not seem appropriate to apply to the normal and deliberate action of his nervous functions the term mistaken.

As a concrete example of some aspects of the question of evidence, the history of witchcraft is interesting. The written records of all nations, European or Asiatic, indicate a belief in this, which was practically universal. The Romans seem always to have considered it criminal, and their legislation

against it is as old as the laws of the Twelve Tables, while one title, or book, of the Code of Justinian is entirely devoted to it. We find laws against witchcraft in the English codes before the Conquest, and there were frequent proceedings against it in the ecclesiastical courts for some five hundred years after that epoch. Fresh laws were made on the subject by Henry VIII., Elizabeth, and James I., and proceedings under these laws were so numerous that in the three years 1645–6–7 between two and three hundred people are said to have been indicted for witchcraft in Suffolk and Essex alone; more than half of whom were convicted. In England trials for witchcraft continued till 1712, the last being that of Jane Wenham, who was convicted but not executed. The mass of evidence offered in proof of witchcraft at all these trials must have been enormous, and much of it would be certainly be given in good faith; it was sufficient to obtain the condemnation and death of thousands of prisoners, yet no one now imagines that it is true for us. Some of those who were accused of witchcraft doubtless believed themselves to be gifted with magic powers, and there would occasionally be evidence of the incantations whereby such prisoners had frightened their neighbours; but this was not deemed necessary for the conviction of the accused; all that was required was evidence of the effects which they were supposed to have caused by other than natural means.

Apparently, no syntax which was simple, direct, and obvious, had any chance of acceptance at these trials; when the idea of witchcraft had once been suggested all evidence was mentally arranged in syntaxes which should involve supernatural causation. Now Hume, writing some years after the date of the last trial for witchcraft, showed that all the ideas concerning cause and effect, which up to his time had been current, were "irrational," that is to say, confused and unintelligible; still more confused must have been the ideas of those who imagined that they could distinguish between natural and supernatural causes. This indicates clearly enough why their proofs of witchcraft cannot be accepted as true for us. A complete demonstration of the truth of these general statements could be obtained only by examining a large number of trials; a single specimen must suffice here, but it is fair to suppose that a state trial, presided over by the eminent judge Sir Matthew Hale, would not be an unfavourable sample of the bulk, especially as its date was 1665, long after the grosser darkness of the Middle Ages had passed away, and only about fifty years before trials for witchcraft became obsolete.

Two women were accused of having bewitched young children, and the main points of the evidence were as follows:—There had been a quarrel between the prisoners and the parents of the children, in consequence of which the former had uttered threats.

Thereafter the children had fits, and during these cried out the name of the prisoners; they also vomited crooked pins. They could not pronounce the words "Lord," "Jesus," or "Christ" in reading, but when they came to "Satan" or "Devil," they cried, "That bites, but makes me speak it right well." One of the children was suckled by one of the accused; the child fell into a swoon, and out of its blanket came a great toad, which exploded in the fire like gunpowder; immediately afterwards the accused was seen sitting at home maimed and scorched. By way of experiment the children were made to touch the prisoners and others in court, and their behaviour was such that many in the court openly declared that they thought the children were impostors. The medical expert witness (Sir Thomas Browne) said that the fits were natural, "though heightened to great excess by the sublety of the devil co-operating with the witches." Here one would say the direct line of thought was obvious: a village quarrel, arousing a desire to injure the prisoners; spiteful parents teaching their children to act; children liable to real fits at some times, and able to simulate them at others; crooked pins introduced among that which the children had vomited; and, lastly, the toad incident invented, or rehearsed, when one of the prisoners was known to have injured herself. Yet the judge and jury preferred another train of ideas, for no apparent reason except that it involved the

supernatural. A farmer also swore that when his cart had on one occasion touched the house of one of the prisoners it overturned continually and they could not get it home; a statement which may have been made in good faith, but which, to us, irresistibly suggests a drunken driver. The Court apparently put this also down to witchcraft. The other evidence was that the "witches' mark" had been found on the women; that they had the reputation of being witches, and that some of their relatives had been condemned as such. On this evidence the women were found guilty, and the record states that as soon as they were condemned the children recovered immediately. Even this did not avail to save the prisoners, who were duly executed. From all which it would seem that although the scientific criterion has not been explicitly recognised, yet its practical application has greatly increased, even in courts of law, during the last two centuries.

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