SCIENCE

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LIGHTNING-ROD PROTECTION.

What is the Problem?

In seeking a means of protection from lightning-discharges, we have in view two objects, —the one the prevention of damage to buildings, and the other the prevention of injury to life. In order to destroy a building in whole or in part, it is necessary that work should be done; that is, as physicists express it, energy is required. Just before the lightning-discharge takes place, the energy capable of doing the damage which we seek to prevent exists in the column of air extending from the cloud to the earth in some form that makes it capable of appearing as what we call electricity. We will therefore call it electrical energy. What this electrical energy is, it is not necessary for us to consider in this place; but that it exists there can be no doubt, as it manifests itself in the destruction of buildings. The problem that we have to deal with, therefore, is the conversion of this energy into some other form, and the accomplishment of this in such a way as shall result in the least injury to property and life.

Why have the Old Rods Failed?

When lightning-rods were first produced, the science of energetics was entirely undeveloped; that is to say, in the middle of the last century scientific men had not come to recognize the fact that the different forms of energy — heat, electricity, mechanical power, etc.— were convertible one into the other, and that each could produce just so much of each of the other forms, and no more. The doctrine of the conservation and correlation of energy was first clearly worked out in the early part of this century. There were, however, some facts known in regard to electricity a hundred and forty years ago; and among these were the attracting power of points for an electric spark, and the conducting power of metals. Lightning-rods were therefore introduced with the idea that the electricity existing in the lightning-discharge could be conveyed around the building which it was proposed to protect, and that the building would thus be saved.

The question as to dissipation of the energy involved was entirely ignored, naturally; and from that time to this, in spite of the best endeavors of those interested, lightning-rods constructed in accordance with Franklin's principle have not furnished satisfactory protection. The reason for this is apparent when it is considered that this electrical energy existing in the atmosphere before the discharge, or, more exactly, in the column of dielectric from the cloud to the earth, above referred to, reaches its maximum value on the surface of the conductors that chance to be within the column of dielectric; so that the greatest display of energy will be on the surface of the very lightning-rods that were meant to protect, and damage results, as so often proves to be the case.

It will be understood, of course, that this display of energy on the surface of the old lightning-rods is aided by their being more or less insulated from the earth, but in any event the very existence of such a mass of metal as an old lightning-rod can only tend to produce a disastrous dissipation of electrical energy upon its surface,— "to draw the lightning," as it is so commonly put.

Is there a Better Means of Protection?

Having cleared our minds, therefore, of any idea of conducting electricity, and keeping clearly in view the fact that in providing protection against lightning we must furnish some means by which the electrical energy may be harmlessly dissipated, the question arises, "Can an improved form be given to the rod, so that it shall aid in this dissipation?"

As the electrical energy involved manifests itself on the surface of conductors, the improved rod should be metallic; but, instead of making a large rod, suppose that we make it comparatively small in size, so that the total amount of metal running from the top of the house to some point a little below the foundations shall not exceed one pound. Suppose, again, that we introduce numerous insulating joints in this rod. We shall then have a rod that experience shows will be readily destroyed — will be readily dissipated — when a discharge takes place; and it will be evident, that, so far as the electrical energy is consumed in doing this, there will be the less to do other damage.

The only point that remains to be proved as to the utility of such a rod is to show that the dissipation of such a conductor does not tend to injure other bodies in its immediate vicinity. On this point I can only say that I have found no case where such a conductor (for instance, a small wire or gilding) has been dissipated, even if resting against a plastered wall, where there has been any material damage done to surrounding objects.

Of course, it is readily understood that such an explosion cannot take place in a confined space without the rupture of the walls (the wire cannot be boarded over); but in every case that I have found recorded this dissipation takes place just as gunpowder burns when spread out on a board. The objects against which the conductor rests may be stained, but they are not shattered.

I would therefore make clear this distinction between the action of electrical energy when dissipated on the surface of a large conductor and when dissipated on the surface of a comparatively small or easily dissipated conductor. When dissipated on the surface of a large conductor, — a conductor so strong as to resist the explosive effect, — damage results to objects around. When dissipated on the surface of a small conductor, the conductor goes, but the other objects around are saved.

A Typical Case of the Action of a Small Conductor.

Franklin, in a letter to Collinson read before the Royal Society, Dec. 18, 1755, describing the partial destruction by lightning of a church-tower at Newbury, Mass., wrote, "Near the bell was fixed an iron hammer to strike the hours; and from the tail of the hammer a wire went down through a small gimlet-hole in the floor that the bell stood upon, and through a second floor in like manner; then horizontally under and near the plastered ceiling of that second floor, till it came near a plastered wall; then down by the side of that wall to a clock, which stood about twenty feet below the bell. The wire was not bigger than a common knitting needle. The spire was split all to pieces by the lightning, and the parts flung in all directions over the square in which the church stood, so that nothing remained above the bell. The lightning passed between the hammer and the clock in the above mentioned wire, without hurting either of the floors, or having any effect upon them (except making the gimlet-holes, through which the wire passed, a little bigger), and without hurting the plastered wall, or any part of the building, so far as the aforesaid wire and the pendulum-wire of the clock extended; which latter wire was about the thickness of a goose-quill. From the end of the pendulum, down quite to the ground, the building was exceedingly rent and damaged. . . . No part of the aforementioned long, small wire, between the clock and the hammer, could be found, except about two inches that hung to the tail of the hammer, and about as much that was fastened to the clock; the rest being exploded, and its particles dissipated in smoke and air, as gunpowder is by common fire, and had only left a black smutty track on the plastering, three or four inches broad, darkest in the middle, and fainter towards the edges, all along the ceiling, under which it passed, and down the wall."

Mathematical Theory.

There is stored up in each cubic centimetre of the column of dielectric from the cloud to the earth, just before the lightning-discharge, an amount of electrical energy given by the expression $\frac{1}{8\pi} KE^2$, where K is the specific inductive capacity of the dielectric air, and E the electro-motive intensity, both in electrostatic units. This expression is given on p. 156, Vol. I., second edition, of Maxwell's "Treatise on Electricity and Magnetism." Substituting the values of K and E (remembering, of course, that they are in electrostatic units), and reducing, we find that the amount of energy involved amounts very nearly to one foot-pound for each cubic foot of air involved. If we consider that the dissipation of this electrical energy takes place throughout the whole length of the column of dielectric from the cloud to the earth, we shall see that all the energy that we have to care for in our lightning-rod is that existing in the section of the column contained between two horizontal planes passing through the top and foundation of our house respectively. This may not, of course, be strictly true, but it must be essentially.

No reason can be assigned why the electrical energy should disappear at the top, or at the bottom, or at the centre, of the column of dielectric in which it exists, so that it is reasonable to maintain that what we call a lightning-flash is simply a line of air in which the electrical energy is being dissipated as heat. The energy, therefore, is transmitted, not from the cloud to the earth or from the earth to the cloud, but horizontally from all portions of the dielectric to some central core where it appears as heat, and where the phenomenon we call a lightning-flash is manifested.

One result of this consideration is, that, in order to produce the amount of energy which is known to exist in lightning-discharges, the radius of the column of dielectric at the surface of the earth must be very considerable, in order that there shall be a sufficient mass of air to furnish, at the rate of one foot-pound per cubic foot, enough energy to produce the well-known results. N. D. C. HODGES.

ARISTOTLE AS A NATURALIST.[1]

HAVING had occasion of late years to make myself acquainted with the observations and ideas of ancient writers upon matters connected with natural history, and having been thus more than ever impressed by the unique position which in this respect is held by Aristotle, it appears to me that a short essay upon the subject may prove of interest to readers of various kinds. Therefore, as far as space permits, I will render the results of my own inquiries in this direction; but, as it is far from an easy task to estimate with justice the scientific claims of so pre-scientific a writer, I shall be greatly obliged to more professed students of Aristotle if they will indicate, either publicly or privately, any errors of fact or of judgment into which it may appear that I have fallen.

Aristotle died B.C. 322, in the sixty-third year of his age. As a personal friend and devoted pupil of Plato, — who, in turn, was a friend and pupil of Socrates, — his mind was at an early age brought under the immediate influence of the best thinking of antiquity. Nevertheless, although entertaining a profound veneration for his master, like a true devotee of truth, he did not allow his mind to become unduly dominated even by the authority of so august a tutor; and in after-life he expressly broke away from the more mystical principles of Platonic method. While still a young man, he was invested with the magnificent office of educating Alexander the Great. He held this position for a period of four years, and then the young prince, at the age of eighteen, became regent. It is interesting to note that the relations which subsisted between this greatest philosopher and this greatest general in the world's history were throughout relations of warmest friendship. Indeed, had it not been for the munificent aid which was afterwards given by Alexander, it would have been impossible for Aristotle to have prosecuted the work which he accomplished.

Questions have been raised, not only as to the authenticity of this work, but also as to the originality of much that is undoubtedly authentic. Into these questions, however, I need not go. Whether or not Aristotle borrowed from other writers without acknowledgment, it is certain that in his writings alone are preserved the records of early biological thought and observation, which would otherwise have been lost; and the preservation of these records is of more importance for our present purpose than is the question to whom such thought and observation were in every case due.

Whether we look to its width or to its depth, we must alike conclude that the range of Aristotle's work is wholly without a parallel in the history of mankind. Indeed, it may be said that there is scarcely any one department of intellectual activity where the mind of this intellectual giant has not exerted more or less influence, in some cases by way of creation, in others by way of direction. The following is a list of the subjects on which Aristotle wrote: physics, astronomy, meteorology, zoölogy, comparative anatomy, physiology, and psychology; poetry, ethics, rhetoric, logic, politics, and metaphysics. Of these subjects he was most successful in his treatment of the second series as I have arranged them, or of the more abstract and least rigidly scientific. In his "Politics" he gave the outlines of two hundred and twenty-five constitutions, and, although but a fragment of his whole work in this direction has come down to us, it is still regarded as one of the best treatises that has ever been written on the subject. His "Ethics," "Rhetoric," and "Logic," also, still present much more than a merely historical interest, for he may be said to have correctly laid down the fundamental principles of these sciences, his analysis of the syllogism, in particular, having left but comparatively little for subsequent logicians to complete; and, lastly, his "Metaphysics" alone would have been sufficient to have placed him among the greatest thinkers of antiquity.

That his labors in the field of more exact science should not now present a comparable degree of value, is, of course, inevitable. At the time when he wrote, the very methods of exact science were unknown; and I think it constitutes the strongest of all his many claims to our intellectual veneration that he was able to perceive so largely as he did the superior value of the objective over the subjective methods in matters pertaining to natural sci-

[1] From The Contemporary Review.

ence. When we remember how inveterate and how universal is the bondage of all early thought to the subjective methods; when we remember, that, for the best part of twenty centuries after the birth of Aristotle, the intellect of Europe was still held fast in the chains of that bondage; and when we remember that even at the present time, with all the advantages of a long and painful experience, we find it so extremely difficult to escape it, — when we remember these things, we can only marvel at the scientific instinct of this man who, although nurtured in the school of Plato, was able to see — darkly, it may be, and, as it were, in the glass of future things, but still was able to see — that the true method of science is the method of observation and experiment. "Men who desire to learn," he said, "must first learn to doubt, for science is only the solution of doubts;" and it is not possible more concisely to state the intellectual duty of scepticism, or the paramount necessity of proof, which thousands of years of wasted toil have now enabled all intelligent men more or less to realize.

Nevertheless, as I have said, the vision of scientific method which Aristotle had was a vision of that which is only seen in part: the image of the great truth which he perceived was largely distorted by passing through the medium of pre-existing thought. Consequently, of late years a great deal of discussion has taken place on the subject of Aristotle's method. On the one hand, it is maintained that he is entitled to the place which is usually assigned to Bacon as the father of the inductive methods; while, on the other hand, it is maintained that in respect of method he did not make any considerable advance upon his predecessors. In my opinion, a just estimate lies between these two extremes. Take, for example, the following passages from his writings: —

"We must not accept a general principle from logic only, but must prove its application to each fact, for it is in facts that we must seek general principles, and these must always accord with facts."

"The reason why men do not sufficiently attend to the facts is their want of experience. Hence those accustomed to physical inquiry are more competent to lay down the principles which have an extensive application; whereas others who have been accustomed to many assumptions without the apposition of reality, easily lay down principles because they take few things into consideration. It is not difficult to distinguish between those who argue from facts and those who argue from notions."

Many similar passages to the same effect might be quoted, and it is evident that the true method of inductive research could not well have its leading principles more clearly enunciated; and to say this much is in itself enough to place Aristotle in the foremost rank among the scientific intellects of the world. But it would be unreasonable to expect that this great herald of scientific method should have been able, with any powers of intellect, to have entirely emancipated himself from the whole system of previous thought; or in the course of a single lifetime to have fully learned the great lesson of method which has only been taught by the best experience of more than twenty centuries after his death. Accordingly, we find that, although he clearly divined the true principles of research, he not unfrequently fell short in his application of those principles to practice. In particular, he had no adequate idea of the importance of verifying each step of a research, or each statement of an exposition; and therefore it is painfully often that his own words just quoted admit of being turned against himself, — "It is easy to distinguish between those who argue from facts and those who argue from notions." To give only a single example, he says that if a woman who has scarlet-fever looks at herself in a mirror, the mirror will become suffused with a bloody mist, which, if the mirror be new, can only be rubbed off with difficulty. Now, instead of proceeding to verify this old wife's tale, he attempts to explain the alleged fact by a rambling assemblage of absurd "notions." And numerous other instances might be given to the same effect. Nevertheless, upon the whole, or as a general rule, in his thought and language, in his mode of conceiving and grappling with problems of a scientific kind, in the importance which he assigns to the smallest facts, and in the general cast of reasoning which he employs, Aristotle resembles, much more closely than any other philosopher of like antiquity, a scientific investigator of the present day.

Thus, in seeking to form a just estimate of Aristotle's work in natural history, we must be careful, on the one hand, to avoid the extravagant praise which has been lavished upon him, even by such authorities as Cuvier, De Blainville, Isidore St. Hilaire, etc.; and, on the other hand, we must no less carefully avoid the unfairness of contrasting his working methods with those which have now become habitual.

In proceeding to consider the extraordinary labors of this extraordinary man, in so far as they were concerned with natural history, I may begin by enumerating, but without waiting to name, the species of animals with which we know that he was acquainted. From his works on natural history, then, we find that he mentions at least 70 species of mammals, 150 of birds, 20 of reptiles, 116 of fish, 84 of articulata, and about 40 of lower forms, making close upon 500 species in all. That he was accustomed from his earliest boyhood to the anatomical study of animal forms, we may infer from the fact of his father having been a physician of eminence, and an Asclepiad; for, according to Galen, it was the custom of the Asclepiads to constitute dissection part of the education of their children. Therefore, as Aristotle's boyhood was passed upon the seacoast, it is probable that from a very early age his studies were directed to the anatomy and physiology of marine animals. But, of course, it must not be concluded from this that the dissections then practised were comparable with what we understand by dissections at the present time. We find abundant evidence in the writings of Aristotle himself that the only kind of anatomy then studied was anatomy of the grosser kind, or such as might be prosecuted with a carving-knife as distinguished from a scalpel.

We generally hear it said that as a naturalist Aristotle was a teleologist, or a believer in the doctrine of design as manifested in living things: therefore I should like to begin by making it clear how far this statement is true; for, unquestionably, when such an intellect as that of Aristotle is at work upon this important question, it behooves us to consider exactly what it was that he concluded.

Now, I do not dispute — indeed, it would be quite impossible to do so — that Aristotle was a teleologist, in the sense of being in every case antecedently convinced that organic structures are adapted to the performance of definite functions, and that the organism as a whole is adapted to the conditions of its existence. Thus, for example, he very clearly says, "As every instrument subserves some particular end, that is to say, some special function, so the whole body must be destined to minister to some plenary sphere of action; just as the saw is made for sawing, — this being its function, — and not sawing for the saw."

But in any other sense than this of recognizing adaptation in Nature, I do not think there is evidence of Aristotle having been a teleologist. In his "Metaphysics" he asks the question whether the principle of order and excellence in Nature is a self-existing principle inherent from all eternity in Nature herself; or whether it is like the discipline of an army, apparently inherent, but really due to a general in the background. Aristotle, I say, asks this question; but he gives no answer. Similarly, in his "Natural History," he simply takes the facts of order and adaptation as facts of observation: and therefore in biology I do not think that Aristotle can be justly credited with teleology in any other sense than a modern Darwinist can be so credited; that is to say, he is a believer in adaptation, or final end, but leaves in abeyance the question of design, or final cause. The only respect in which he differs from a modern Darwinist, although even here the school of Wallace and Weismann agree with him, is in holding that adaptation must be present in all cases, even where the adaptation is not apparent. In the case of rudimentary organs, he is puzzled to account for structures apparently aimless, and therefore he invents what we may term an imaginary aim by saying that Nature has supplied these structures as "tokens," whereby to sustain her unity of plan. This idea was prominently revived in modern pre-Darwinian times; but in the present connection it is enough to observe that here, as elsewhere, Aristotle personifies Nature as a designing or contriving agency, having the attainment of order and harmony as the final end or aim of all her work. He appears, however, clearly to have recognized, that, so far at least as science is concerned, such personification is, as it were, allegorical; for he expressly says that if he were asked whether Nature works out her designs with any such conscious deliberation, or intentional adjustment of means to ends, as is the case with a builder or a shipwright, he would not be able to answer. All, therefore, that the teleology of Aristotle amounted to was this: he found that the hypothesis of purpose was a useful working hypothesis in his biological researches. There is nothing to show that he would have followed the natural theologians of modern times, who seek to rear upon this working hypothesis a constructive argument in favor of design. On the other hand, it is certain that he would have differed from these theologians in one important particular; for he everywhere regards the purposes of Nature as operating under limitations imposed by what he calls absolute necessity. Monsters, for example, he says are not the intentional work of Nature herself, but instances of the victory of matter over Nature; that is to say, they are instances where Nature has failed to satisfy those conditions of necessity under which she acts. Thus, even if there be a disposing mind which is the author of Nature, according to Aristotle it is not the mind of a creator, but rather that of an architect, who does the best he can with the materials supplied to him, and under the conditions imposed by necessity.

Turning, now, to the actual work which Aristotle accomplished in the domain of biology, I will first enumerate his more important discoveries upon matters of fact, and then proceed to mention his more important achievements in the way of generalization.

He correctly viewed the blood as the medium of general nutrition, and knew that for this purpose it moved through the blood-vessels from the heart to all parts of the body, although he did not know that it returned again to the heart, and thus was ignorant of what we now call the circulation. But he was the first to find that the heart is related to the blood-vascular system; and this he did by proving, in the way of dissection, that its cavities are continuous with those of the large veins and arteries. Nor did he end here. He traced the course of these large veins and arteries, giving an accurate account of their branchings and distribution. He knew perfectly well that arteries contain blood; and this is a matter of some importance, because it has been the habit of historians of physiology to affirm that all the ancients supposed arteries to contain air. In speaking of the cavities of the heart, he appears to have fallen into the unaccountably foolish blunder of saying that no animal has more than three, and that some animals have as few as one. But, although this apparent error has been harped upon by his critics, it is clearly no error at all. Professor Huxley has shown that what Aristotle here did was to regard the right auricle as a venous sinus, or as a part of the great vein, and not of the heart. The only mistake of any importance that he made in all his researches upon the anatomy of the heart and blood-vessels, was in supposing that the number of cavities of the heart is in some measure determined by the size of the animal. Here he undoubtedly lays himself open to the charge of basing a general and erroneous statement on a preconceived idea, without taking the trouble to test it by observation. But we may forgive him this little exhibition of negligence when we find that it was committed by the same observer, who correctly informs us that the heart of the chick is first observable as a pulsating point on the third day of incubation, or who graphically tells us that just as irrigating trenches in gardens are constructed to distribute water from one single source through numerous channels, which divide and subdivide so as to convey it to all parts, and thus to nourish the garden-plants which grow at the expense of the water, so the blood-vessels start from the heart in a ramifying system, in order to conduct the nutritive fluid to all regions of the body. Lastly, Aristotle experimented on coagulation of the blood, and obtained accurate results as to the comparative rates with which the process takes place in the blood of different animals. He also correctly described the phenomenon as due to the formation of a meshwork of fibres, but he appears to have erroneously supposed that these fibres exist in the blood before it is drawn from the body.

So much, then, for his views upon the heart, the blood, and the blood-vessels. He was less fortunate in his teaching about the bladder, kidneys, liver, spleen, and so forth, because he had no sufficient physiological data to go upon. Still, one would think he might have avoided the error of attributing the formation of urine to the bladder, seeing that he had gone so far as to perceive that the kidneys separate out the urine, which, as he correctly says, then flows into the bladder. His chapters on the digestive tract display a surprisingly extensive and detailed investigation of the alimentary systems of many animals, and the observations made are for the most part accurate. In particular, his descriptions of the teeth, œsophagus, epiglottis, and the mechanism of deglutition, display so surprising an amount of careful and detailed observation throughout the vertebrated series, that they read much like a modern treatise upon these branches of comparative anatomy. The same remark applies to his disquisition on horns. Where inaccurate, his mistakes here are mostly due to his ignorance of exotic forms.

Adipose tissue he correctly viewed as excess of nutritive matter extracted from the blood, and he noted that fatness is inimical to propagation. Marrow he likewise correctly regarded as having to do with the nutrition of bones, and observed that in the embryo it consists of a vascular pulp.

That Aristotle should have had no glimmering notion either of the nervous system or of its functions, is, of course, not surprising; but to me it is surprising that so acute an observer should have failed to perceive the physiological meaning of muscles. Although he knew that they are attached to bones, that they occur in greatest bulk where most strength of movement is required, — such as in the arms and legs of man, the breasts of birds, and so forth, — and although he must have observed that the muscles swell and harden when the limbs move, yet it never occurred to him to connect muscles with the phenomena of movement. He regarded them only as padding, having also in some way to do with the phenomena of sensation. Thus we appear to have one of those curious instances of feeble observation with which every now and then he takes us by surprise. To give parenthetically a still more strange example of what I mean, one would think that there is nothing in the economy of a star-fish or an echinus more conspicuous, or more calculated to arrest attention, than the ambulacral system of tube feet; yet Aristotle, while describing many other parts of those animals, is quite silent about this ambulacral system. I think this fact can only be explained by supposing that he confined his observations to dead specimens; but, as he was not an inland naturalist, even this explanation does not acquit him of a charge of negligence, which, when contrasted with his customary diligence, appears to me extraordinary.

His ignorance of the nervous system led him to a variety of speculative errors. In particular, he was induced to regard the heart as the seat of mind, and the brain as a bloodless organ, whose function it was to cool the heart, which he supposed to be not only the organ of mind, but also an apparatus for cooking the blood, and by it the food. The respiratory system was also conceived by him as a supplementary apparatus for the purpose of keeping the body cool, — a curious illustration of early philosophical thought arriving at a conclusion which, to use his own terminology, was directly opposed to the truth. Nevertheless, the reasoning which landed him in this erroneous conclusion was not only perfectly sound, but also based upon a large induction from facts, the observation of which is highly creditable. The reason why he supposed the office of respiration to be that of cooling the body was because nearly all animals which respire by means of lungs exhibit a high temperature; and, imagining that temperature or "vital heat" was a property of the living soul, his inference was inevitable that the function of the lungs was that of keeping down the temperature of warm-blooded animals. Here, then, his error was due to deficiency of information, and the same has to be said of the great majority of his other errors. For instance, with regard to the one already mentioned about the heart being the seat of mind, this is usually said by commentators to have been due merely to the accident of the heart occupying a central position; and no doubt such was partly his reason, for he considered that position the noblest, and repeatedly argues that on this account it must be the seat of mind. But over and above this mystical, not to say childish, reason, I think he must have had another: for, seeing that the error is a very general one in early philosophical thought,— we find it running through the Psalms, and it is still conventionally retained by all poetic writers,— I think we must look for some more evident reason than that of mere position to account for it; and this reason I take to be the perceptible influence on the heart-beat which is caused by emotions of various kinds. Furthermore, Aristotle expressly assigns the following as another of his reasons: "In the embryo the heart appears in motion before all other parts, as if it were a living animal, and as if it were the beginning of all animals that have blood."

Turning, now, for a moment to Aristotle's still more detailed discoveries in comparative anatomy and physiology, his most remarkable researches are, I think, those on the *Cetacea*, *Crustacea*, and *Cephalopoda*. Here the amount of minute and accurate observation which he displayed is truly astonishing, and in some cases his statements on important matters of fact have only been verified in our own century; such, for instance, as the peculiar mode of propagation which has now been re-discovered in some of the *Cephalopoda*.[1] He also knew the anomalous fact that in these animals the vitellus is joined to the mouth of the embryo; that in certain species of cartilaginous fish the embryo is attached to its parent by the intervention of a placenta-like structure; and, in short, detailed so many anatomical discoveries, both as regards the vertebrata and invertebrata, that a separate article would be required to make them intelligible to a general reader. In this connection, therefore, I will only again insist upon the enormous difference between Aristotle and the great majority of his illustrious countrymen in respect of method. Unless it can be shown that an ancient writer has been led to anticipate the results of modern discovery by the legitimate use of inductive methods, he deserves no more credit for his guesses when they happen to have been right than he does when they happen to have been wrong. This, however, is a consideration which we are apt to neglect. When we find that an old philosopher has made a statement which science has afterwards shown to be true, we are apt to regard the fact as proof of remarkable scientific insight; whereas, when we investigate the reasonings which led him to propound the statement, we usually find that they are of a puerile nature, and only happened to hit the truth, as it were, by accident. Among a number of guesses made at random and in ignorance, a certain percentage may well prove right; but, under these circumstances, the man who happens to make a correct guess deserves no more credit than he who happens to have made an erroneous one. Indeed, he may deserve even less credit. For instance: when the Pythagoreans, on a basis of various mystical and erroneous speculations, propounded a kind of dim adumbration of the heliocentric theory, far from deserving any credit for superior sagacity at the hands of modern science, they merit condemnation for their extravagant theorizing and unguarded belief. In their time, whatever evidence there was lay on the side of the then prevalent view that the sun moves round the earth: therefore, when, without adducing any counter-evidence of a scientific kind, they affirmed that the earth moved round the sun, they were merely displaying the spirit of what the Yankees call "pure cussedness;" that is to say, they were shutting their eyes to the only evidence which was available, and showing their own obstinacy by propounding a directly opposite view. The sound maxim in science is, that he discovers who proves; and this is a maxim which many classical scholars would do well to remember when writing about the scientific speculations of the early Greeks.

Now, I have made these remarks in order again to emphasize the almost unique position which Aristotle holds among his contemporaries in this respect. Instead of giving his fancy free rein upon "the high *priori* road," he patiently plods the way of detailed research; and, when he proceeds to generalize, he does so as far as possible upon the basis of his inductive experience.

[1] Lewes, however, denies that the evidence is sufficient to show that Aristotle knew this.

Coming, now, to his generalizations, it was a true philosophical insight which enabled Aristotle to perceive in organic nature an ascending complexity of organization from the vegetable kingdom up to man. Instead of the three kingdoms of Nature, which were afterwards formulated by the alchemists, and which in general parlance we still continue to preserve, namely, the mineral, vegetable, and animal — instead of these three kingdoms, Aristotle adopted the much more philosophical classification of Nature into two divisions, the organic and the inorganic, or the living and the not-living. Nevertheless he fell into the error — which was, indeed, almost unavoidable in his time — of supposing that there is a natural and a daily passage of the one into the other. However, he again shows his philosophical insight where he points out the leading distinctions between plants and animals, the former manifesting life in the phenomena of nutrition alone, including germination, growth, repair, and reproduction; while the latter, besides these, exhibit also the phenomena of sensation, volition, and spontaneous movement. He was not so fortunate in his attempts at drawing the boundary-lines between plants and animals: for while he correctly guessed, from erroneous observation, that sponges should be classified as animals, he decided in favor of placing the hydroid polyps among the plants; and he appears to have classified certain testaceous mollusks in the same category. Man, of course, he places at the head of the animal kingdom, and shows a profound penetration in drawing the true psychological distinction between him and the lower animals; namely, that animals only know particular truths, never generalize, or form abstract ideas.

His conception of life was more in accordance with that of modern science than that of any of the other conceptions which have been formed of it either in ancient times or the middle ages, for he seems clearly to have perceived the error of regarding the "vital principle" otherwise than as an abstraction of our own making. Life and mind, in his view, were abstractions pertaining to organisms, just in the same way as weight and heat are abstractions pertaining to inanimate objects. For convenience of expression, or even for purposes of research, it may be desirable to speak of weight and heat as independent entities: but we know that they cannot exist apart from material objects; that they are what we term qualities, and not themselves objects. And so with life and mind: they are regarded by Aristotle as qualities — or, as we should now say, functions — of organisms. And here we must remember that the whole course of previous speculation on such matters proceeded on the assumption that the vital principle was an independent entity superadded to organisms, serving to animate them as long as it was united to them, leaving them to death and decay as soon as it was withdrawn from them, and even then being itself able to survive as a disembodied spirit, enjoying its conscious existence apart from all material conditions. Thus it was that the creations of early thought peopled the world with ghosts and spirits more numerously than Nature had supplied it with living organisms. Now, Aristotle boldly broke away from this fundamental assumption of the vital principle as an independent and superadded entity. In the phenomena of life and mind he saw merely the functions of organism: he assigned to them both a physical basis, and clearly perceived that for any fruitful study of either we must have recourse to the methods of physiology.

The scientific genius which could have enabled a man in those days thus to have anticipated the temper of modern thought, appears to me entitled to our highest veneration. Here, perhaps more than anywhere else, he showed his instinctive appreciation of the objective methods; and here it is that the longest time has been taken for mankind to awaken to the truth of his appreciation.

In subsequent centuries, when European thought drifted away from science into theology, the question was long and warmly debated whether or not Aristotle believed in the immortality of the soul. The truth of the matter is that his deliverances upon this question are more scarce than clear. The following brief passage, however, appears to show that he regarded the thinking principle, as distinguished from the animal soul, to be virtually independent of the corporeal organization: "Only the intellect enters from without. It alone is god-like. Its actuality has nothing in common with the corporeal actuality."

Aristotle appears to have been the first philosopher who at all appreciated the importance of heredity as a principle, not only in natural history, but also in psychology; for he distinctly affirms that the children of civilized communities are capable of a higher degree of intellectual cultivation than are children of savages.

Among his other more noteworthy enunciations of general truths, we may notice the following :—

"The advantage of physiological division of labor was first set forth," says Milne-Edwards, "by myself in 1827." Yet Aristotle had said repeatedly that it is preferable, when possible, to have a separate organ for a separate office; and that Nature never, if she can help it, makes one organ answer two purposes, as a cheap artist makes "spit and candlestick in one."

Again, that the complexity of life varies with the complexity of organization; that the structural differences of the alimentary organs are correlated with differences of the animal's alimentation; that no animal without lungs has a voice, and that no animal is endowed with more than one adequate means of defence; that there is an inverse relation between the development of horns and of teeth, as also between growth and generation; that no dipterous insect has a sting; that the embryo is evolved by a succession of gradual changes from a homogeneous mass into a complete organism; that the development of an organism is a progress from a general to a special form,—these and numerous others are instances of generalization made by Aristotle, which have lasted, with but slight modifications of his terms, to the present day.[1]

Of these generalizations the most remarkable is the last which I have mentioned; for one of the greatest and most momentous controversies which the history of science has afforded is that which took place nearly 2000 years after the time of Aristotle, with regard to so-called evolution *versus* epigenesis. The question was whether the germ or egg of any organism contained the future or young organism already formed in miniature, and only requiring to be expanded in order to appear as the perfect organism, or whether the process of development consisted in a progress from the indefinite to the definite, from the simple to the complex, from what we call undifferentiated protoplasm to the fully differentiated animal. During the seventeenth and eighteenth centuries, when this subject was most warmly debated, the balance of scientific opinion inclined to what is now known to be the erroneous view, that germ is merely the adult organism in miniature. It therefore speaks greatly in favor of Aristotle's sagacity that he clearly and repeatedly expressed the opinion which is now known to be right; viz., that the organism develops out of its germ by a series of differentiations. And not only with reference to this doctrine of epigenesis, but likewise throughout the whole course of his elaborate treatise on generation, he displays such wonderful powers, both of patient observation and accurate scientific reasoning, that this treatise deserves to be regarded as the most remarkable of all his remarkable works pertaining to biology. The subject-matter of it is not, however, suited to any detailed consideration within the limits imposed by an article; and therefore I will merely back the general opinion which I have just given by quoting that of the most severe and exacting of all Aristotle's critics from the side of science,—severe and exacting, indeed, to a degree which is frequently unjust. I mean the late George Henry Lewes. This is what he says of the treatise on generation :—

"It is an extraordinary production. No ancient and few modern works equal it in comprehensiveness of detail and profound speculative insight. We there find some of the obscurest problems of biology treated with a mastery which, when we consider the condition of science at that day, is truly astonishing. . . . I know no better eulogy to pass on Aristotle than to compare his work with the 'Exercitations concerning Generation' of our immortal Harvey. The founder of modern physiology was a man of keen insight, of patient research, of eminently scientific mind. His work is superior to that of Aristotle in some few anatomical details; but it is so inferior to it in philosophy, that at the present day it is much more antiquated, much less accordant with our views."

[1] Dr. W. Ogle, in his admirable work on Aristotle, has already alluded to these and some of the other points previously noticed.

I have now said enough to convey a general idea of the enormous range of Aristotle's work within the four corners of biology, his amazing instincts of scientific method, and his immense power of grasping generalizations. While doing this, I have selected instances of his accuracy rather than of his inaccuracy, not only because it is in the former that he stands in most conspicuous contrast with all preceding and with most succeeding philosophers of antiquity, but also because it is here that we may be most sure of according justice. Where we meet with statements of fact which are accurate, we may be satisfied that we are in immediate contact with the mind of Aristotle himself; but when we meet with inaccurate statements, we must not be so sure of this. Not only is it probable that in the great majority of these cases he has been misled by erroneous information supplied to him by travellers, fishermen, and others, but there is good reason to suppose that in some places his manuscripts may have been tampered with. These were hidden underground for the better part of two centuries; and when they were eventually brought to light, Apellicon, into whose hands they fell, "felt no scruples in correcting what had been worm-eaten, and supplying what was defective or illegible."[1]

Thus, to quote Dr. Ogle, who suggests the view here taken : "Is it possible to believe that the same eye that has distinguished the cetacea from the fishes, that had detected their hidden mammæ, discovered their lungs, and recognized the distinct character of their bones, should have been so blind as to fancy that the mouth of these animals was on the under surface of the body ?" And so on with other cases.

Inaccuracies of observation, however, there must have been; and there must have been inaccuracies of reasoning. Looking to the enormous range of his work in biology alone, remembering that in this work he had had no predecessors, considering that at the same time he was thus a single-handed collector of facts and a single-minded thinker upon their import, it becomes evident that Aristotle would have been something more than human if either his observations or his reasonings could everywhere be justly compared with those of scientific genius when more favorably circumstanced. But it is the glory of Aristotle that both his observations and his reasonings can stand such comparison as well as they do : for when on the one hand we remember the immensity of his achievement, and on the other hand reflect that he was worse than destitute of any ancestral experience of method, born into a world of mysticism, nurtured in the school of Plato, therefore compelled himself to forge the intellectual instruments of research, himself to create the very conception of scientific inquiry,—when we thus remember and thus reflect, it appears to me there can be no question that Aristotle stands forth, not only as the greatest figure of antiquity, but as the greatest intellect that has ever appeared upon the face of this earth.

The overmastering power with which this intellect swayed the course of subsequent thought was in one respect highly beneficial to the interests of science, but in another respect it was no less deleterious. It was beneficial in so far as it revealed to mankind the true method of science as objective, and not subjective : it was deleterious, inasmuch as the very magnitude of its force reduced the intellect of Europe for centuries afterwards to a condition of abject slavery. Nothing is more deleterious to the interests of science than undue regard to authority. Before all else the spirit of Science must be free : it must be unfettered by the chains of prejudice, whether these be forged by our own minds or manufactured for us by the minds of others. Her only allegiance is that which she owes to Nature, to man she owes nothing; and here, as elsewhere, it is impossible to serve two masters. Therefore, the only use of authority in science is to furnish men of less ability with suggestions which, as suggestions, may properly be considered more worthy of testing by the objective methods on account of their parentage in the mind of genius. But it is an evil day for science when such parentage is taken as in itself a sufficient warrant for the truth of the ideas which have been born of it, for then it is that authority is allowed to usurp the place of

[1] See Grote's Aristotle, i. 51.

verification. Instead of her true motto, "Prove all things," Science thus adopts its very opposite, "Only believe."

Now, the whole history of Science has been more or less blotted by this baleful influence of authority, which, even in our own days, is far from having been wholly expunged. But in no part of her history has this influence been exerted in any degree at all comparable with that which was thrown over her, like a shadow, by Aristotle. Partly owing to the magnitude of his genius, but still more, I think, to the predominance of the spirit in the dark ages which regarded submission to authority as an intellectual virtue, through all these ages stood to science the name of Aristotle in very much the same relation as stood to religion the name of God. His writings on purely scientific subjects were regarded as well-nigh equivalent to a revelation, and therefore the study of Nature became a mere study of Aristotle. There was almost a total absence of any independent inquiry in any one department of science; and even in cases where the utterances of Aristotle were obscure, the men of intellect who disputed over his meanings never thought of appealing to Nature herself for a solution. They could only view Nature through the glasses which had been given them by Aristotle, and therefore the only questions with which they troubled themselves were those as to the exact meaning of their oracle.

It is, of course, only fair to add that Aristotle himself was in no way responsible for this evil effect of his work. The spirit in which his work was thus received was quite alien to that in which it had been accomplished, and alike by precept and example he was himself the most noble opponent of the former that the world has ever produced; and therefore I doubt not, that, if Aristotle could have been brought back to life during the middle ages, he would have made short work of the Aristotelians by himself becoming their bitterest foe: for listen to his voice, which upon this, as upon so many other matters, speaks with the spirit of truest philosophy — speaks, moreover, with the honesty of a great and beautiful nature — let us listen to what this master mind has told us of its own labors, and with a veneration more worthy than that of the Aristotelians let us bow before the man who said these words: —

"I found no basis prepared, no models to copy. . . . Mine is the first step, and therefore a small one, though worked out with much thought and hard labor. It must be looked at as a first step, and judged with indulgence. You, my readers or hearers of my lectures, if you think I have done as much as can fairly be required for an initiatory start, as compared with more advanced departments of theory, will acknowledge what I have achieved, and pardon what I have left for others to accomplish."

GEORGE J. ROMANES.

NOTES AND NEWS.

"IT is my belief," said a representative of the Scott Stamp and Coin Company of New York to *The Illustrated American*, recently, "that there never was any 1804 dollar. That dies were cut in that year, similar in all respects, save the date, to the dies of 1803, is certain. It is also certain that these dies were destroyed in 1869. But no dollars or half-dollars were issued in that year, nor were they issued at any time by governmental authority.

— The Bureau of the International Congress of Geologists has decided that its fifth session shall be held at Washington, and the date of the session has been fixed for the last Wednesday (26th) of August, 1891. The annual meeting of the American Association for the Advancement of Science and the summer meeting of the Geological Society of America will be held in the same city during the preceding week. The committee of organization will endeavor to obtain from the ocean steamship lines the most favorable terms for the transportation of foreign members to and from the United States, and to arrange with the respective railroad companies for reduced rates for the geological excursions. To accomplish this satisfactorily, it is important that they should know beforehand the approximate number of members who propose to attend the meeting, and that they should have an expression of opinion from these members in order to arrange in advance a series of excursions to places that will be of interest to the greatest number. Owing to the great number of points of geological interest, and to the great distances to be traversed, it would be impossible for the committee to arrange these excursions so that their expense should fall within reasonable limits, without some such previous information. Any geologist who may be desirous of taking part in the congress, or of receiving its publications, which will probably include many valuable geological papers, who will send his name to the secretary, S. F. Emmons, 1330 F Street, Washington, D.C., will be put upon the list and receive the invitation to become a member of the congress. The small fee for membership ($2.50) is for this congress only, and intended to defray the cost of printing and other necessary expenses. It is customary for geologists of the country where the congress is held to subscribe, even if they cannot be present at the congress.

— The Audubon Monument Committee of the New York Academy of Sciences acknowledge the following subscriptions to the Audubon Monument fund : previously acknowledged, $1,298.50; Morris K. Jesup, A. R. Eno, Andrew G. Carnegie, Thomas A. Edison, James Constable, William E. Dodge, William Schermerhorn, Charles Stewart Smith, C. G. Gunther's Sons, W. W. Astor, J. Pierpont Morgan, C. P. Huntington, Robert Hoe, and Charles Lanier, each $100; Parke Godwin, $25; Coleman Drayton, $5; R. H. Derby, 5, — total $2,733.50. It thus appears that the result of four years of hard labor on the part of the committee has not been quite $3,000. There is certainly a lack of interest in raising money for this object which calls for an explanation.

— At a meeting of the Royal Meteorological Society, London, on Feb. 18, Mr. C. Harding read a paper entitled "The Great Frost of 1890–91." This paper dealt with the whole period of the frost from Nov. 25 to Jan. 22; and it was shown that over nearly the whole of the south-east of England the mean temperature for the fifty-nine days was more than 2° below the freezing-point, while at seaside stations on the coast of Kent, Sussex, and Hampshire, the mean was only 32°. In the extreme north of Scotland, as well as in the west of Ireland, the mean was 10° warmer than in the south-east of England. In the southern midlands and in parts of the south of England the mean temperature for the fifty-nine days was more than 10° below the average; but in the north of England the deficiency did not amount to 5°, and in the extreme north of Scotland it was less than 1°. The lowest authentic reading in the screen was 0.6° at Stokesay, in Shropshire, but almost equally low temperatures occurred at other periods of the frost. At many places in the south and south-west of England, as well as in parts of Scotland and Ireland, the greatest cold throughout the period occurred at the end of November; and at Waddon, in Surrey, the thermometer in the screen fell to 1°,—a reading quite unprecedented at the close of the autumn. At Addington Hills, near Croydon, the shade thermometer was below the freezing-point each night, with one exception, and there were only two exceptions at Cambridge and Reading; while in the Shetlands there were only nine nights with frost, although at Biarritz frost occurred on thirty-one nights, and at Rome on six nights. At many places in England the frost was continuous night and day for twenty-five days, but at coast stations in the north of Scotland it in no case lasted throughout the twenty-four hours. On the coast of Sussex the temperature of the sea was 14° warmer than the air throughout December, but on the Yorkshire coast it was only 6° warmer, and in the Shetlands and on parts of the Irish coast it was only 3° warmer. The Thames water off Deptford, at two feet below the surface, was continuously below 34° from Dec. 23 to Jan. 23, — a period of thirty-two days,—while the river was blocked with ice during the greater part of this time. In Regent's Park, where skating continued uninterruptedly for forty-three days, the ice attained the thickness of over nine inches. The frost did not penetrate to the depth of two feet below the surface of the ground in any part of England; but in many places, especially in the south and east, the ground was frozen for several days at the depth of one foot, and at six inches it was frozen for upwards of a month. In the neighborhood of London the cold was more prolonged than in any previous frost during the last hundred years, the next longest spell being fifty-two days in the winter of 1794–95, while in 1838 frost lasted for fifty days, and in 1788–89 for forty-nine days.