NORTH AMERICAN REVIEW.

No. CCXXXVI.

JULY, 1872.

ART. I. — 1. *The Origin of Species, etc.* By CHARLES DARWIN. Sixth Edition. With Additions and Corrections. London: John Murray. 1872.

2. *Evolution and its Consequences:* A Reply to Professor Huxley. By ST. GEORGE MIVART. "Contemporary Review," January, 1872.

3. *Specific Genesis.* By ST. GEORGE MIVART. Communication to the "North American Review," April, 1872.

THE physical problem, proposed independently and almost simultaneously near the beginning of this century by three eminent men of genius, — by Goethe, Geoffroy St. Hilaire, and the elder Darwin, — the question *how* animals and plants came to have the structures and habits that characterize them as distinct species, which was proposed in place of the teleological question, *why* they were so produced, has now fairly become a direct question for scientific investigation. There is no longer any doubt that this effect was by some natural process, and was not by a formless creative fiat. Moreover, there scarcely remains any doubt that this natural process connects the living forms of the present with very different forms in the past; and that this connection is properly described in general terms as "descent with modification." The question has thus become narrowed down to the inquiry, What is the nature of

this modification, or what are the causes and the modes of action by which such modifications have been effected?

This is a great step in scientific progress. So long as a doubt remained about the fact that such modifications have been effected, and that present living forms are the results of them, the inquiry, how they were effected, belonged to the region of profitless speculation, — profitless except for this, that speculative minds, boldly laying aside doubts which perplex and impede others, and anticipating their solution, have often in the history of science, by preparing a way for further progress, greatly facilitated their actual solution. Difficulties and questions lying beyond such doubts — walls to scale after outworks and ditches are passed — do not inspire the cautious with courage. And so the scientific world waited, though prepared with ample force of evidence, and hesitated to take the step which would bring it face to face with the questions of the present and the future. Darwin's "Origin of Species," by marshalling and largely reinforcing the evidences of evolution, and by candidly estimating the opposing evidence, and still more by pointing out a way to the solution of the greatest difficulty, gave the signal and the word of encouragement which effected a movement that had long been impending.

The "that," the fact of evolution, may be regarded as established. The "how," the theory or explanation of it, is the problem immediately before us. Many years of patient investigation may be needed. Much discussion, which will doubtless be disturbed by acrimonious disputes, as well as helped by more generous rivalries, may be expected, more especially in the immediate future; while what may be called the dialectics of the subject are being developed, or while the bearings and the limits of views and questions, and while conceptions and definitions and kinds of arguments appropriate to the discussion, are the subjects on which it is necessary to come to a common understanding. It is highly desirable that this discussion should be as free as possible from mere personalities, and there is strong hope that it may be kept so through the experience which the history of modern science affords, or through the manners and methods of procedure which this experience has established. That it is impossible, however, to avoid errors of this sort alto-

gether, is evident from the provocations experienced and keenly felt by some of the noblest of modern students of science in their establishment of theories in modern astronomy, and of theories in geology, to which may now be added the theory of evolution. That the further discussion of rival hypotheses on the causes and modes of evolution will profit by these older examples may be hoped, since there have grown up general methods of investigation and discussion, which prescribe limits and precautions for hypothesis and inference; and, more than all, for the conduct of debate on scientific subjects, that have been of the greatest value to the progress of science, and will, if faithfully observed, doubtless direct the present discussion to a successful issue.

These methods are analogous in their purposes to the general rules in courts of law, and constitute the principles of method in experimental philosophy, or in philosophy founded on the sciences of observation. They serve to protect an investigation from prejudice, by demanding that it shall be allowed on certain pretty strict conditions (in the conduct of experiments and observations, and in the formation and verification of hypotheses) to proceed without hinderance from prejudice for any existing doctrine or opinion. An investigation may thus start from the simplest basis of experience, and, for this purpose, may waive, yet without denying, any presumption or conclusion held in existing theories or doctrines. Again, these rules protect an investigation from a one-sided criticism or *ex parte* judgment, since they demand of the criticism or judgment the same judicial attitude that is demanded of the investigation. Advocacy, and especially the sort that is of essential value in courts of law, where two advocates are set against each other, each with the duty of presenting only what can be said for his own side, and where the same judge and jury are bound to hear both, is singularly out of place in a scientific discussion, unless in oral debate before the tribunal of a scientific society. Moreover, there are no burdens of proof in science. Such advocacy in a published work claiming scientific consideration is almost an offence against the proprieties of such discussions. To collect together in one place all that can be said for an hypothesis, and in another all that can be

said against it, is at best a clumsy and inconvenient method of discussion, the natural results of which may best be seen in the present condition of theological and religious doctrines. These practical considerations are of the utmost importance for the attainment of the end of scientific pursuit; which is not to arrive at decisions or judgments that are probably true only on the whole and in the long run, but is the discovery of the real truths of nature, for which science can afford to wait, and for which suspended judgments are the soundest substitutes.

No work of science, ancient or modern, dealing with problematic views and doctrines, has more completely conformed to these principles or justified them by its success than the "Origin of Species." For its real or principal success was in convincing nearly all naturalists, a majority of whom, at least, were still unconvinced, of the truth of the theory of evolution; and this depended on its obvious fairness and spirit of caution almost as much as on the preponderance of the evidences for the theory when thus presented. But the very same qualities of spirit and method governed the leading and more strictly original design of the work, which cannot yet be said to be a complete success, namely, the *explanation* of evolution by natural selection. That Mr. Darwin himself is fully convinced of the truth of this explanation is sufficiently evident. By this, however, must be understood that he holds, as he has done from the first publication of his work, that natural selection is the principal or leading cause in determining the changes and diversities of species, though not the only cause of the development of their characters. Conspicuously at the close of the Introduction in the first edition of the work, and in all subsequent editions, occur these words: "I am convinced that Natural Selection has been the most important, but not the exclusive, means of modification." That the work is not a merely dialectical performance is clear; and it is equally clear that in proportion to the strength of the author's conviction is his solicitude to give full and just weight to all valid objections to it. In this respect the work stands in marked contrast to much that has been written on the subject and in reply to it.

Once to leave the vantage-ground of scientific method and adopt the advocate's *ex parte* mode of discussion almost neces-

sitates a continuance of the discussion under this most inconvenient form. Mr. Mivart's "Genesis of Species," which we examined in this Review last July, though a conspicuous example of such a one-sided treatment of a proper scientific question, was by a writer so distinguished for his attainments in science that his criticism could not well be passed by without notice; and, having also the character of a popular treatise, it came within a wider province of criticism than that of strictly scientific reviews. This notice was chiefly devoted to supplying something of what could be and had been said in favor of the theory thus criticised, both by way of defining and defending it. We also followed the author to some extent into the consideration of a subject, namely, the general philosophical and theological bearings of this theory, which does not, we endeavored to show, belong properly to the discussion, and ought to be kept in abeyance, in accordance with the laws of experimental philosophy, so long, at least, as these laws are properly observed in the conduct of the inquiry. One of the first questions asked in past times in regard to physical hypotheses, which have now become established theories or doctrines of science, was, whether they were orthodox, or at least theistic, or atheistical; and the adverse decision of this question by what was deemed competent authority determined temporarily and in a measure the fate of the hypothesis and the standing of those who held to it. It was to be hoped that, in the light of such a history, this discussion could be spared the question, at least till the hypothesis could be fairly tried, when, if it should be found wanting in scientific validity, its banishment to the limbo of exploded errors might, without much harm, be changed to a severer sentence; and, if it should withstand the tests of purely scientific criticism, the same means of reconciling it to orthodoxy would doubtless be found as in older physical hypotheses. Mr. Mivart himself claimed and argued a similar exemption for the general theory of evolution, or rather attempted the later office of reconciliation, or of proving its conformity to the most venerable and authoritative decisions of orthodoxy. But he appeared unwilling to allow either such an exemption or the possibility of an accordance with orthodoxy to the theory of natural selection, for he quoted and applied to

the discussion of this theory the saying and supposed opinions of an old heretical heathen philosopher, Democritus, in several passages of his book.

In his reply to our criticisms,* he wonders who could have so misled us as to make us suppose that his was a "theological education" and a "schooling against Democritus"; the fact being just the reverse of this, his education being in that philosophy of "nescience," out of the evils and fallacies of which he had at length struggled. Clearly we were misled by the author himself. Our error, slight except as a biographical one, would have been amended if we had referred the character of his criticism to his theological *studies*. This would have left the period in his life in which he acquired his mode of thought and discussion as undetermined, as it was unimportant to the point of our criticism; since, through the influence of these studies, or similar dialectical pursuits, his unquestionable abilities appeared to us to have been developed, and, as we believe, misapplied. It was the bringing in of "the fortuitous concourse of atoms," and "blind chance," "accidents," and "hap-hazard results," in a discussion with which they had no more to do, and no less, than they have to do with geology, meteorology, politics, philosophical history, or political economy;—it was this irrelevancy in his criticism which we regarded as oblivious of the age in which we live and for which he wrote,—the age of experimental philosophy. Mr. Mivart thinks he is clear of all blame for speaking of the theory of natural selection as liable "to lead men to regard the present organic world as formed, so to speak, *accidentally*, beautiful and wonderful as is confessedly the hap-hazard result," since he qualified the word "accidentally" by the phrase "so to speak." The real fault was in speaking so at all.

Accidents in the ordinary every-day sense are causes in every concrete course of events,—in the weather, in history, in politics, in the market,—and no theory of these events can leave them out. Explanation of the events consists in showing how they will result, or have resulted, through certain fixed principles or laws of action from the occasions or opportunities, which such accidents present. Given the state of the atmosphere over

* See the April number of this Review.

a large district in respect to temperature, moisture, pressure, and motion, — none of which could have been anticipated without similar data for a short time before, all in fact being accidents, — and the physical principles of meteorology might enable us to explain the weather that immediately follows. So with the events of history, etc. In no other sense are accidents supposed as causes in the theory of natural selection. Accidental variations and surrounding conditions of existence, and the previous condition of the organic world (none of which could have been anticipated from anything we actually know, all in fact being "accidents"), — these are the causes which present the occasions or opportunities through which principles of utility and advantage are brought to bear in changing structures and habits, and improving their adaptations. If this is like the philosophy of Democritus, or any other excommunicated philosopher of antiquity, and is, therefore, to be condemned for the heresy, then all the sciences with which we have compared it, and many others, the conquests of human intelligence, must share the condemnation.

We dwelt in our review, perhaps unnecessarily, on the fact that accidents in this sense, and in the theory of natural selection, as well as elsewhere, are relative to our knowledge of causes; that the same event, like an eclipse of the sun, might be an accident to one mind, and an anticipated event to another. We did so because we could not understand otherwise why our author should single out the theory of natural selection from analogous theories and sciences for a special criticism of this sort; or except on the idea that the accidents in natural selection were supposed by him to be exceptional, and of the type which Democritus is reputed to have put in the place of intelligent design, or on the throne of Nous. We did not, as Mr. Mivart imagines, think him "ignorant that the various phenomena which we observe in nature have their respective phenomenal antecedents," nor suppose that he "held the opinion that phenomena of variation, etc., are not determined by definite, invariable, physical antecedents." We only thought that, knowing better, — knowing that "natural selection," like every other physical theory, dealt with physical causes and their laws, — he was unjust and inconsistent in con-

demning the employment of it, as a leading or prominent cause, in explanation of the phenomena of the organic world, in the manner in which he did; except on the hypothesis, which we repudiated in behalf of experimental philosophy, but without positively attributing it to him, — the hypothesis of absolute accidents. It was inconsistency and irrelevancy which we meant to attribute to him.

That he supposed absolute accidents to be meant in the ancient atheistical philosophy appeared from a passage in his chapter on theology and evolution (p. 276), in which he speaks of the kind of action we might expect in physical nature from a theistic point of view, as an action "which is orderly, which *disaccords* with the action of blind chance and with the 'fortuitous concourse of atoms' of Democritus." But in his reply to us he repudiates the idea that this old philosophy held events to be accidental in the strict sense; and he further says of us that we "know very well that Democritus and Empedocles and their school no more held phenomena to be undetermined or unpreceded by other phenomena than do their successors at the present day." We are far from being so well informed, or willing to accept this as a statement of our views. For, in the first place, the terms "undetermined" and "unpreceded" are not quite synonymous. Moreover, so far as phenomena are determined, they are "orderly," "harmonize with man's reason" (p. 275), though in their complexity they may be quite beyond the power of any man's imagination to represent or disentangle; and, as our author has said, they are what we might expect "from a theistic point of view."

Whether Democritus believed in absolute accidents or not we do not know. Little is really known of his opinions in this respect. The question has been disputed, but not decided. All his works are lost, except a few quoted sentences and maxims. He is in a peculiarly exposed condition for an attack from any one disposed to be his opponent. His teachings, probably already sufficiently garbled, are unprotected by contexts, or by the scruples an opponent might feel about them in assigning to him his place in the history of speculation. It is very likely that he did not hold to such accidents as occurring in

the course of nature; though it is very doubtful whether he was so thoroughly convinced as his "successors of the present day" are of the universality of the "law of causation," or that *every* event must have determinant antecedents. The conception of cause, as based by experimental science on the elementary invariable orders of phenomenal successions, is altogether too precise and abstract for the apprehension of a mind untrained by scientific studies, even at the present day. How much more so must it have been when among the old Ionian philosophers the first crude conceptions of science were being fashioned by attempts at discovering the physical bond of union and the inchoate form of nature, regarded as a universe. It is an anachronism to speak of these philosophers as materialists and atheists, since the distinctions and questions which could make such a classification intelligible had not yet been proposed. And it is equally an anachronism to attribute even to later thinkers like Democritus such a conception of physical causation as only the latest and maturest products of scientific thought have rendered definite.

There can be no antithesis in the problem of the beginning of the world between accident and law, or accident and the orderly movements which imply determinant antecedents. The real antithesis is between accident and miracle, or accident and the extraordinary action of pre-existent designing intelligence; and in this relation Accident can only have an absolute meaning, equivalent in fact to Destiny or Fate, when unintelligible. Unintelligible Destiny or "blind chance" is directly opposed to the intelligible Destiny which is the principle of "law" in nature; though these have often been confounded as equally fatalistic and atheistical. Our author, however, does not confound them; for he has said that the latter is what we might expect from a theistic point of view. It is altogether likely, however, that the Democritus to whom the former meaning could be attributed as a characteristic one is not the real thinker, but is a myth; or is rather the orthodox lay-figure of atheism of the theological studio.

The reputation for atheism which the real Democritus doubtless had, may have come from a cause which has often produced it in the history of physical science. He invented a

theory of atoms, with which he attempted physical explanations quite in advance of previous speculations. And the invention of physical hypotheses has often been regarded as an invasion of the province and jurisdiction of divine power and a first cause. For men rarely allow the explanation of any important effect in nature to remain an open question. If observed or carefully inferred physical causes do not suffice, invisible or even spiritual ones are invented; and thus the ground is preoccupied and closed against the inquiries of the physical philosopher. It is probably the general direction or tendency of these inquiries, rather than any positive positions or results at which they may arrive, which puts the physical philosopher in an apparently irreligious attitude. For in following out the consequences of physical hypotheses into the details of natural phenomena, reasoning from supposed causes to their effects, his interests and his modes of thought are the reverse of those of mankind in general, and of the religious mind. He appears to turn his back on divinity, and though seeking to approach nearer the first cause, or the total order of nature, his aspect of looking downward from a proximate principle through a natural order appears to the popular view to be darkened by a sombre shadow. The theory of universal gravitation was condemned on this account for impiety by even so liberal and enlightened a thinker as Leibnitz. This seems very strange to us now, since the law of gravitation is almost as familiar as fire, or even gravity itself. When in ancient times any one had burnt his fingers, or been bruised by a fall, one did not, except perhaps in early childhood, attribute the harm to a person, a spirit, or a god, but to the qualities of fire or gravity; yet the sounds of the thunder were still referred by him directly to Zeus. We all remember how in the "Clouds" of Aristophanes the comic poet puts impiety in the mouth of Socrates, or the doctrine that Zeus does not exist, and that it is ethereal Vortex, reigning in his stead, which drives the clouds and makes them rain and thunder. Such a view of physical inquiries is not confined to comic poets or their audiences. The meteorological sophists of that day were in very much the same position as the Darwinian evolutionists of the present time.

However important it may be to bear these considerations in mind, there is, as we have said, no more occasion for it with reference to the theory of natural selection than with reference to many other analogous theories, not only in physical science, like those of meteorology and geology (including the theory of evolution), but also in sociological science, like theories of political economy, and those theories of history which explain the growth of institutions, governments, and national characteristics. The comparison of the continuous order in time, and the total aspect of the organic world at any period, to the progressive changes and the particular aspect at any time of the weather, will, doubtless, strike many minds as inapt, since the latter phenomena are the type to us of indetermination and chance, while the former present to us the most conspicuous evidences of orderly determination and design. This contrast, though conspicuous, is, nevertheless, incidental to the point of view, and is not essential to the contrasted orders themselves. The movements in one are almost infinitely slower than in the other. We see a single phase and certain orderly details in one. We see only confused and rapid combinations and successions in the other. One is seen in fine, the other in gross form. Looked at from the same point of view, regarding each as an *ensemble* of details in time and space, they are equally without definite order or intelligible plan; "beautiful and wonderful as is," according to our author, "the hap-hazard result." It is in the intimate and comparatively minute parts of the organic world, in individual structures or organisms, that the beautiful and wonderful order is seen. When we look at great groups, like the floras and faunas of various regions, or past geological groupings, — the shifting clouds, as it were, of organic life, — this order disappears or is hidden for the most part. There remains enough of apparent order to indicate continuity in time and space, but hardly anything more. Perfectly as the individual organism may exhibit adaptations or the applications of principles of utility, there is no definite clew in it to the cause of the particular combination of uses which it embodies, or the existence of it in a particular region, or at a particular period in the history of the world, or its coexistence with many other quite

independent particular forms. But in precise analogy with what is conspicuously regular and indicative of simple laws in the organic world, correspond the intimate elementary changes of the atmosphere, some of which, like the fall and even the formation of rain and snow, the development and disappearance of clouds, are almost as precisely simple exhibitions of natural laws as experiments in the laboratory. What, even in the laboratory, can exceed the beauty, simplicity, and completeness of that exemplification of definite physical laws which the fall of dew on clear, calm nights demonstrates? Moreover, there are in the successions of changes in the weather sufficient traces of order to indicate a continuity in space and time corresponding to the geographical distributions and geological successions of the organic world. The elementary orders, which exhibit ultimate physical laws in simple isolation, are, in their aggregate and complex combination, the causes of these successions of changes in the weather and the source of whatever traces of order appear in them, and are thus analogous to what the theory of natural selection supposes in the organic world, namely, that the adaptations, or the exhibitions of simple principles of utility in structures, are in their aggregate and complex combinations the causes of successive and continuous changes in forms of life.

Far more important, however, than such analogies in the doctrine of evolution is the clear understanding of what the theory of natural selection undertakes to explain, and what is the precise and essential nature of its supposed action. There appears to be much confusion on this subject, arising probably from the influence of preconceived opinions concerning the nature, both of the matters explained and the mode of explanation, or on the nature of the changes which take place in species and the relations of them to this cause. These would seem, at first sight, very simple matters for conception, and difficult only in the evidences and the adequacy of the explanation. Such appeared, and still appears, to be the opinion of our author. Perhaps the best way to make a difficult theory plain is the negative one of correcting the misconceptions of it as they arise. This is what we attempted in our review with reference to the character of the variations from which nature normally and for the most part selects. But new difficulties

have emerged in Mr. Mivart's later writings which deserve consideration. In his answer to Professor Huxley, in the January number of the "Contemporary Review" (p. 170), he says of the theory of natural selection, "That the benefit of the individual in the struggle for life was announced as the one determining agent, fixing slight beneficial variations into enduring characters," for which he thinks it quite incompetent. And again, in reply to us (p. 453), he speaks of "*The* origin, not, of course, of slight variations, but of the fixing of these in definite lines and grooves"; and this origin, he believes, cannot be natural selection. And we believe that his conclusions are right! That is, if the more obvious meaning of these expressions are their real ones. They appear to mean that natural selection will not account for the unvarying continuance in succeeding generations of *simple* changes made accidentally in individual structures (whether the change be large or small), or will not account for the direct conversion of a *simple* change in a parent into a permanent alteration of its offspring. Such is the apparent meaning of these expressions, but they might possibly be taken as loose expressions of the opinion that this cause will not account for permanent changes in the *average* characters, or mid-points, about which variations oscillate; and, in this case, we believe that he is wrong. This permanency must not be understood, however, as meaning that changes cease, but only that they are not reversed. The same cause, natural selection, prevents such reversion, on the whole, and except for the individual cases, which it exterminates.

The first and obviously intended meaning of these expressions has let in light upon the author's own theory and his general difficulty about the theory of natural selection, which we did not have before. They show how fundamentally the matter has been misconceived, either by him or by us. That we did not more fully perceive this fundamental difference doubtless arose from a tacit assumption in his earlier criticisms of the principle of "specific stability," which was explicitly treated of in a later chapter and as a subordinate topic. This, as we shall find, is undoubtedly the source of the most serious misunderstanding. We were not aware that it was anywhere supposed that particular variations ever became *fixed* and heritable

changes in the characters of organisms by the direct agency of natural selection, or, indeed, by any other known cause. The proper effect of this cause is not to fix variations, though it must *determine their averages and limit their range*, and must act indirectly to increase the useful ones and diminish the injurious; or rather to permit the one and forbid the other, and when these are directly opposed to each other, it must act to shift the average or normal character, instead of fixing it. Variation as a constant and normal phenomenon of organization, exhibited chiefly in the ranges of individual differences, is, as it were, the agitation or irregular oscillation that keeps the characters of species from getting too closely *fixed* in "definite lines and grooves," through the too rigid inheritance of ancestral traits; or it is a principle of alertness that keeps them ever ready for movement and change in conformity to changing conditions of existence. What fixes species (when they are fixed) is the continuance of the same advantages in their structures and habits, or the same conditions for the action of selection, together with the force of long-continued inheritance.

This, though almost trite from frequent repetition, appears a very difficult conception for many minds, probably on account of their retaining the old stand-point. It would appear that our author is really speaking of the *fixed* species of the old and still prevalent philosophy, or about *real* species, as they are commonly called. Natural selection cannot, of course, account for these figments. Their true explanation is in the fact that naturalists formerly assumed, without proper evidence, that a change too slow for them to perceive directly could not exist, and that characters widely prevalent and so far advanced as to become permanently adapted to very general and unchanging conditions of existence, like vertebral and articulate structures, the numbers and positions of the organs of locomotion in various animals, the whorl and the spiral arrangement of leaves in plants, and similar homological resemblances, could never have been vacillating and uncertain ones. It was not many years ago that a distinguished writer in criticising the views of Lamarck affirmed that "the majority of naturalists agree with Linnæus in supposing that all the individuals propagated from one stock have certain distinguishing characters in common, which never

vary, and which have remained the same since the creation of each species." The influence of this opinion still remains, even with naturalists who would hesitate to assert categorically the opinion itself. This comes, doubtless, from the fact that long-prevalent doctrines often get stamped into the very meanings of words, and thus acquire the character of axioms. The word "species" became synonymous with *real* or *fixed* species, or these adjectives became pleonastic. And this was from the mere force of repetition, and without valid foundation, in fact, or confirmation from proper inductive evidence.

Natural selection does not, of course, account for a fixity that does not exist, but only for the adaptations and the diversities in species, which may or may not be changing at any time. They are fixed only as the "fixed" stars are fixed, of which very many are now known to be slowly moving. Their fixity, when they are fixed, is temporary and through the accident of unchanging external conditions. Such is at least the assumption of the theory of natural selection. Mr. Mivart's theory seems to assume, on the other hand, that unless a species or a character is tied to something it will run away; that there is a necessity for some internal bond to hold it, at least temporarily, or so long as it remains the *same* species. He is entitled, it is true, to challenge the theory of natural selection for proofs of its assumption, that "fixity" is not an essential feature of natural species; for, in fact, so far as direct evidence is concerned, this is an open question. Its decision must depend chiefly on the preponderance of indirect and probable evidences in the interpretation of the "geological record," a subject to which much space is devoted, in accordance with its importance, in the "Origin of Species." Technical questions on the classification and description of species afford other evidences, and it is asserted by naturalists that a very large number of specimens, say ten thousand, is sufficient, in some departments of natural history, to break down any definition or discrimination even of living species. Other evidences are afforded by the phenomena of variation under domestication. Mr. Mivart had the right, and may still have it, to resist all this evidence, as not conclusive; but he is not entitled to call upon the theory of natural selection for an explanation of a feature in organic structures

which the theory denies in its very elements, the *fixity* of species. This is what he has done, — implicitly, as it now appears, in his book, and explicitly in his later writings.

The question of zoölogical philosophy, "Whether species have a real existence in nature," in the decision of which naturalists have so generally agreed with Linnæus, refers directly and explicitly to this question of the fixity of essential characters, and to the assumption that species must remain unaltered in these respects so long as they continue to exist, or until they give birth to new species; or, as was formerly believed, give place in perishing to new independent creations. The distinction involved in this question should not be confounded, as it might easily be, with the distinction in Logic of "real kinds" from other class-names. Logic recognizes a principal division in class-names, according as these are the names of objects which agree with each other and differ from other objects in a very large and indefinite number of particulars or attributes, or are the names of objects which agree only in a few and a definite number of attributes. The former are the names of "real kinds," and include the names of natural species, as man, horse, etc., and of natural genera, as whale, oak, etc. These classes are "real kinds," not because the innumerable particulars in which the individual members of them agree with each other and differ from the members of other classes, are themselves fixed or invariable in time, but because this sort of agreement and difference is fixed or continues to appear. An individual hipparion resembled its immediate parents and the other offspring of them as closely as, or, at least, in the same intimate manner in which one horse resembles another, namely, in innumerable details. But this is not opposed to the conception that the horse is descended from the hipparion by insensible steps of gradation or continuously. For examples of names that are not the names of "real kinds," we may instance such objects as those that are an inch in length, or in breadth, or are colored black, or are square, or (combining these particulars) such objects as black square inches. These may be made of paper, or wood, or ivory, or differ in all other respects except the enumerated and definite particulars. They are not "real" or natural "kinds," but factitious ones.

The confusion which, as we have said, might arise between the "real kinds" of Logic, and the *real* species of biological speculation, would depend on a vagueness in the significance of the word "real," which in common usage combines in uncertain proportions two elementary and more precise ideas, that of fixedness and that of breadth of relationship. Both these marks of reality are applied habitually as tests of it. Thus if an object attests its existence to several of my senses, is seen, heard, touched, and varied in its relations to these senses, and moreover is similarly related to the senses of another person, as evinced by his testimony, then I know that the object is real, and not a mere hallucination or invention of my fantasy; though it may disappear immediately afterwards in an unexplained manner, or be removed by some unknown but supposable agency. Here the judgment of reality depends on breadth of relationship to my experience and sources of knowledge. Or again I may only *see* the object, and consult no other eyes than my own; but seeing it often, day after day, in the same place, I shall judge it a real object, provided its existence is conformable to the general possibilities of experience, or to the test of "breadth." Here the test of reality is "fixity" or continuance in time. That natural species are real in one of these senses, or that individuals of a species are alike in an indefinite number of particulars, or resemble each other intimately, is unquestionable as a fact, and is not an invention of the understanding or classifying faculty, and is moreover the direct natural consequence of the principles of inheritance. In this sense species are equivalent to large natural stocks or races existing for a limited but indeterminate number of generations. That they are real in the other sense, or fixed in time absolutely in respect to any of the particulars of their resemblance, whether these are essential (that is, useful for discrimination and classification) or are not, is far from being the axiom it has seemed to be. It is, on the contrary, highly improbable, though tacitly assumed, as we have seen, in criticisms of the theory of natural selection; and in that significance often attached to the word "species" in which the notions of fixedness and distinctiveness have coalesced. It is true that without this significance in the word

"species" the names and descriptions of organic forms could not be permanently applicable. No system of classification, however natural or real, could be final. Classification would, indeed, be wholly inadequate as a representation of the organic world on the whole, or as a sketch of the "plan of creation," and would be falsely conceived as revealing the categories and thoughts of creative intelligence, — a consequence by no means welcome to the devout naturalist, since it seems to degrade the value of his work. But this may be because he has misconceived its true value, and dedicated to the science of divinity what is really the rightful inheritence of natural or physical science.

If instead of implicitly assuming the principle of specific stability in the criticisms of the earlier chapters of his book, and deferring the explicit consideration of it to a later chapter and as a special topic, our author had undertaken the establishment of it as the essential basis of his theory (as indeed it really is), he would have attacked the theory of natural selection in a most vital point; and if he had succeeded, all further criticism of the theory would have been superfluous. But without success in establishing this essential basis, he leaves his own theory and his general difficulties on the theory of natural selection without adequate foundation. The importance of natural selection in the evolution of organic species (its predominent influence) depends entirely on the truth of the opposite assumption, the *instability* of species. The evidences for and against this position are various, and are not adequately considered in the author's chapter on this subject. Moreover, some of the evidences may be expected to be greatly affected by what will doubtless be the discoveries of the immediate future. Already the difficulties of discrimination and classification in dealing with large collections have become very great in some departments of natural history, and even in paleontology the gradations of fossil forms are becoming finer and finer with almost every new discovery; and this in spite of the fact that nothing at all approaching to evidence of continuity can rationally be expected anywhere from the fragmentary geological record. To this evidence must be added the phenomena of variation under domestication. The ap-

parent limits of the changes which can be effected by artificial selection are not, as they have been thought, proofs of the doctrine of "specific stability," or of the opinion of Linnæus, but only indications of the dependence of variation on physiological causes, and on laws of inheritance; and also of the fact that the laws of variation and the action of natural selection are not suspended by domestication, but may oppose the aims and efforts of artificial selection. The real point of the proof afforded by these phenomena is that permanent changes may be effected in species by insensible degrees. They are permanent, however, only in the sense that no tendency to reversion will restore the original form, except by the action of similar causes.

Against the conclusions of such inductive evidences the vague analogies of the organic to the inorganic world would avail little or nothing, even if they were true. They avail little or nothing, consequently, in confirmation of them in being proved false; as we showed one analogy to be in the illustration given by our author, namely, the supposed analogy of specific characters in crystals to those of organisms; and his inference of abrupt changes in organic species, corresponding by this analogy to changes in the mode or species of crystallization, which the same substance undergoes in some cases with a change of surrounding conditions, such as certain other substances may introduce by their presence. A complete illustration of the chemical phenomena is afforded by the crystals of sulphur. Crystals produced in the wet way, or from solution in the bisulphide of carbon, are of a species entirely distinct from those formed in the dry way, or from the fused mineral; and there are many other cases of these phenomena of *dimorphism* and *polymorphism*, as they are called. We recur to this topic, not on account of its importance to the discussion, but because Mr. Mivart accuses us of changing a quotation from Mr. J. J. Murphy, so that he "is unlucky enough to be blamed for what he never said, or apparently thought of saying." We have looked with true solicitude for the evidences of the truth of this charge, and find them to be as follows: We transcribed from Mr. Mivart's book these sentences, as quoted by him (p. 185), from Mr. Murphy: "It needs no proof that in the

case of spheres and crystals, the forms and *the* structures are the effect, and not the cause, of the formative principles. Attraction, whether gravitative or capillary, produces the spherical form; the spherical form does not produce attraction. And crystalline polarities produce crystalline structure and form; crystalline structure and form do not produce *crystalline* polarities." The superfluous letter and words, which we have put in italics, were omitted in the printing, we do not know how, but it looks like an unwarrantable attempt in a final revision of proofs to improve the English of the quotation. Certainly the changes were of no advantage to our criticism, especially as they only have the effect to render the antithesis, which was the object of the criticism, slightly weaker. Even less advantage, we believe, will come to the author of such an accusation, made without specifications or proofs. It is quite impossible to see how these changes have exposed Mr. Murphy to undeserved censure. We blamed him and our author, not for the use of abstractions as causes, — a use which, as our author says, we make ourselves whenever it is convenient, — but for asserting the antithesis of cause and effect between abstractions, both of which are descriptive of effects, namely, the character of the attractions, gravitative and capillary, which produce spherical forms *vs.* the spherical form itself; and the polar character of the forces that produce crystals *vs.* the crystalline form and structure. Each of these effects (both in the case of the sphere and of the crystal) is doubtless a concause or condition that goes to the determination of the other. The spherical form arranges and determines the resultants of the elementary forces, and thus indirectly determines itself, or determines that action of the elementary forces thus combined, which results in the maintenance or stable equilibrium of the spherical form. Again, in crystallization the already formed bodies, with the particular directions of their faces and axes, determine in part how the resultants of elementary polar forces will act in the further growth of the crystal, or in the repair of a broken one; and the elementary forces, thus determined and combined, result in the crystalline form and structure. Both of the effects which are put in the antithesis of cause and effect in the above quotation are also partial agents.

They act and react on each other in the production of actual crystals.

But this point was of importance to the discussion only as exhibiting a kind of "realism" by which scientific discussion is very liable to be confused. In this case, the wordy profundity was not quite so bald and conspicuous as the ordinary putting of a single-worded abstract description of an effect for its cause, since it consisted in putting one of two such abstractions as the cause of the other. More important, as affecting the truth of the supposed analogy of *species* in crystals to those of organisms, was the statement which our author confesses is utterly beyond him, and as he certainly has misinterpreted it, we may be pardoned for repeating and explaining it. We said, "Moreover, in the case of crystals, neither these forces [the elementary] nor the abstract law of their action in producing definite *angles* resides in the finished bodies, but in the properties of the surrounding media, portions of whose constituents are changed into crystals, according to these properties and other conditioning circumstances." Our author has made us say "crystals" where we said "angles," though the unintelligible character of the sentence ought to have made him the more cautious in copying it. We said "angles," because these are prominent marks of the *species* of the crystal; and this species we referred to the nature of the fluid material out of which the crystal is formed, and to the modifying influences of the presence of other substances, when the crystallization takes place from solutions, or in the wet way. The fact that the determination of the *species* of a crystal is not in any germ or nucleus or anything belonging in a special way to the particular crystal itself, but is in the molecular forces of the fluid solution, makes the analogy of species in crystals to those of organisms not only vague but false. What is really effected by the introduction of a foreign substance, acid or alkali, in the solution, is a change, not in such accidents as the surrounding conditions are to an organism, but in the essential forces, which *ought* to change the character or species of the crystal suddenly, *per saltum*, or discontinuously; and it has not, therefore, the remotest likeness to such suppositions as that a duck might be hatched from a goose's egg, or a goose from a duck's; or that a horse might have been the foal of an hipparion.

Notwithstanding that our statement was "utterly beyond" our author, he has ventured the following confident comments (p. 460): "If this is so," he says, "then when a broken crystal completes itself, the determining forces reside exclusively in the media, and not at all in the crystal with its broken surface! The first atoms of a crystal deposited arrange themselves entirely according to the forces of the surrounding media, and their own properties are utterly without influence or effect in the result!" The marks of exclamation appended to these statements ought to have been ours, since nothing in the statements themselves has the remotest dependence on anything we said; but on the contrary these statements are directly opposed to the objections we made to Mr. Murphy's antitheses. They might be deducible, perhaps, from our proposition, in the form to which it was altered through the substitution of the word "crystals" for "angles," by supposing the concrete actual crystals to be referred to, instead of their *species*, of which these angles are prominent marks. But we had insisted that neither the resulting form, nor the resultants of elementary forces, are exclusively effects, or exclusively causes in the formation or in the mending of actual crystals; yet the *species* of the crystal is fully determined by what is outside of it, or by causes that may be abruptly changed by a change in the medium. Hence the phenomena of *dimorphism* and *polymorphism*, and similar chemical phenomena, have nothing in common with the hypothesis of "specific genesis."

Several similar misunderstandings of more special criticisms in our review tempt us (chiefly from personal considerations) to undertake their rectification; but our object in this article is only to further the discussion, so far as it can be done under the inconvenient form of polemical discussion, by removing, as far as we are able, confusions and misunderstandings in essential matters. Hence we shall not dwell upon the discussion of what may be called hypotheses of the second degree, or the discussion of hypothetical illustrations of the action of natural selection. It was a part of Mr. Mivart's plan, in attacking the hypothesis of the predominant agency of natural selection in the origination of species, to discredit a number of such subordinate hypotheses, as well as challenge the theory to offer any

adequate ones for the explanation of certain extraordinary structures. We considered in detail several objections of this sort, though we might have been content with simply pointing out a sufficient answer in the logical weakness of such a mode of attack. The illustrations of the theory which have been proposed have in general not at all the force of arguments, or except where the utility of a structure is simple and obvious and can be shown by direct evidence to be effective in developing it out of accidental beginnings, and even in perfecting it, as in cases of the mimicry of certain insects by others for a protection, which is thus really acquired. In general, such illustrations serve only to show the mode of action supposed in the theory, without pretending to reconstruct the past history of an animal, even by the roughest sketch; or to determine all the uses, or the relative importance of them, in any structure. To discredit these particular secondary hypotheses has no more weight as an argument against the theory than the hypotheses themselves have in confirmation of it. To be convinced on general grounds that such a structure as that of the giraffe's neck was developed by insensible steps from a more common form of the neck in ungulates, through the oscillations of individual differences, and by the special utilities of the variations which have made the neck longer in some individuals than in others, or through the utilities of these to the animals under the special conditions of their past existence, is very different from believing that this or that particular use in the structure was *the* utility (to adopt our author's favorite form of definiteness) which governed the selection or determined the survival of the fittest. *The* use which may be presumed in general to govern selection is a combination, with various degrees of importance, of all the actual uses in a structure. There can be no more propriety in demanding of the theory of natural selection that it should define this use, or trace out the history hypothetically of any particular structure in its relations to past conditions of existence, than there would be in demanding of political economy that it should justify the correctness of its general principles by success in explaining the record of past prices in detail, or accounting in particular for a given financial anomaly. In either case, the proper evi-

dence is wanting. Any instance of a structure which could be conclusively shown (a very difficult kind of proof) to exist, or to be developed in any way, without reference in the process of development to any utility whatever, past or present, or to any past forms of the structure, would be an instance in point, and would go far towards qualifying the evidence, otherwise mostly affirmative, of the predominant agency of natural selection.

We may remark by the way that Mr. Mivart's definite thesis, "that natural selection is not *the* origin of species," is really not *the* question. No more was ever claimed for it than that it is the most influential of the agencies through which species have been modified. Lamarck's principle of the direct effect of habit, or actual use and disuse, has never been abandoned by later evolutionists; and Mr. Darwin has given much more space to its proof and illustration in his work on "Variation under Domestication" than any other writer. Moreover the physiological causes which produce reversions and correlations of growth, and which, so far as they are known, are quite independent of natural selection, are also assigned as causes of change. But all these are subordinated in the theory to the advantage and consequent survival of the fittest in the struggle for life, or to natural selection. Upon this point we must refer our readers to the "additions and corrections" in the lately published sixth edition of the "Origin of Species"; in which also all the objections brought forward by Mr. Mivart, which had not previously been examined in the work, are fully considered; and, we need hardly add, far more thoroughly and adequately than could be possible for us, or in the pages of this Review.

We will, nevertheless, give in sheer self-defence the correction of one perversion of our criticism. Our author had argued in his book that the use of the giraffe's long neck for browsing on the foliage of trees, and the advantage of it in times of drought, could not be the cause of its gradual increase by selection; since this advantage, if a real one, would be equally an advantage to all ungulates inhabiting the country of the giraffe, or similar regions; and that the other ungulates, at least in such regions, ought to have been similarly modified.

We allowed that there was force in the objection, but we were mistaken. The very conditions of the selection must have been a competition which would have soon put a large majority of the competitors out of the lists, and have narrowed the contest to a few races, and finally to the individuals of a single race. All the rest must have early given up the struggle for life in this direction; since a slight increase in the length of the neck could have been of no advantage if the reach of it still fell far short of the unconsumed foliage. The success of the survivors among them must have been won in some other direction, like the power of rapid and wide ranging, or organs better adapted to close grazing. For a fuller development and illustration of this reply we must refer to Chapter VII. in the new edition of the "Origin of Species," in which most of Mr. Mivart's objections are considered. We attempted a reply to this objection in a direction in which his own remarks led us. Granting that the advantage of a long neck would have been equally an advantage to all ungulates in South Africa; that there was no alternative or substitute for it; and that the use of the neck for high-reaching in times of drought could not *therefore* have been *the* efficient cause of its preservation and increase through selection; still there were other and very important uses in such a neck, to which these objections do not apply, and through which there would be advantages in the struggle for life, that would determine competition only among the individuals of a single race; while those of other races would compete with each other on other grounds. Our author admitted that there might be several lines of advantage in means of *protection* or *defence;* and cited instances from Mr. Wallace, showing, for example, that a dull color, useful for concealing an animal, would not be an advantage to those animals which are otherwise sufficiently protected, and do not need concealment. The use of the giraffe's neck, then, as a means of defence and offence, for which there was ample evidence, its use as a watch-tower and as a weapon of offence, would be raised by the author's objection to greater prominence, and might be the principal ground of advantage and competition between giraffe and giraffe, or one herd of them and another, with reference to protection from the larger beasts

of prey; an advantage which would be incessant instead of occasional, like the high-reaching advantage in times of drought. *The* use, as we have said, means, with reference to the advantage in the struggle for life, the combination of all the uses that are of importance to the preservation of life. Accordingly we demanded whether our author, having made a special objection to the *importance* of *one* use, as affording advantages and grounds for selection (an objection which we allowed, though unwarrantably), we demanded whether he could possibly suppose that this exhausted the matter, or that the supposed small importance of this use precluded the existence of uses more important which *would* afford grounds of advantage and competition in the struggle for life.

As with one having the true "philosophical habit of mind," and distinguished from the "scientific," our author's notice was attracted to the *form* in which we made this inquiry, rather than to the material import of it, and "as we might *a priori* expect to be the case," he showed "that breadth of view, freedom of handling, and flexibility of mind" which he believes to characterize the true philosopher, as contrasted with the mere physicist; but in a manner which appears to us to characterize rather the mere dialectician. With great fertility of invention he attempts the interpretation of our inquiry (which we grant was not sufficiently explicit for the "philosophical habit of mind"). The first interpretation is playful, and too delicate a jest to be transplanted to our pages. The next is, on the other hand, altogether too serious. He asks in return (p. 463), whether we can suppose "that he ever dreamed that the structures of animals are not useful to them, or that his position is an altogether anti-teleological one." No, we certainly do not. We only suppose that his position is not sufficiently teleological to interest him in the inquiry, and that he has overlooked many uses in the structures of animals, to which his special objections do not apply, and has vainly imagined, that by making those he felt called upon to examine as few and as faint as possible (except for the purpose of inspiring the agreeable emotion of admiration), he has reduced them to mere luxuries, having little or no value as grounds of advantage in the actual, incessant, and severe strug-

gle to which all life is subject. "Nothing is easier than to admit in words the truth of the universal struggle for life, or more difficult" — even Mr. Darwin finds it so — "than constantly to bear this conclusion in mind. Yet unless it be thoroughly engrained in the mind, the whole economy of nature, with every fact on distribution, rarity, abundance, extinction, and variation, will be dimly seen or quite misunderstood."

Supposing us possessed by some such idea as that his "position is an altogether anti-teleological one," Mr. Mivart observes that we proceed "to exhibit the giraffe's neck in the character of a 'watch-tower.' But," he adds, "this leaves the question just where it was before. Of course I concede most readily and fully that it *is* a most admirable watch-tower, as it also *is* a most admirable high-reaching organ, but this tells us nothing of its *origin*. In both cases the long neck is most useful *when you have got it;* but the question is how it *arose*, and in this species *alone*. And similar and as convincing arguments could be brought against the watch-tower theory of origin as against the high-reaching theory, and not only this, but also against every other theory which could possibly be adduced." It appears that our author is prepared, *a priori*, to meet any number of foes of this sort that may present themselves singly. But *the* use, that is, all the essential uses of a structure, do not thus present themselves to our consideration and criticism. To deal adequately with the problem, we need the power to conceive how closely the uses lie to the actual necessities of life; how, while we may be admiring in imagination the almost superfluous bounties of nature, this admirable watch-tower and high-reaching organ may just be failing to save the poor animal, so highly endowed, from a miserable death. A lion whose stealthy approach it would have detected, if a few inches more in the length of its neck, or in those of its companions, had enabled it, or them, to see a few rods further, or over some intervening obstacle, has meantime sprung upon the wretched beast, and is drawing its life-blood. This, if we were aware of it, would be the proper occasion to turn our admiration upon the fine endowments of the lion. Or, continuing our contemplation of the giraffe, it may be that its admirable high-reaching organ has just failed to reach the few remaining leaves

near the tops of trees, which might have served to keep up its strength against the attacks of its enemies, or enabled it to deal more effective blows with its short horns, so admirably placed as weapons of offence; or might have served to sustain it through the famine and drought, till the returning rains would have given it more cause for gratitude (and us more occasion for admiration) for a few additional inches of its neck than for all the rest. Meantime for the lack of these inches our giraffe may have sickened and perished miserably, failing in the competition and struggle for life. This need not stagger the optimist. The bounty of nature is not exhausted in giraffes. We can still admire the providential structure of the tree, which by its high-reaching branches has preserved some of its foliage from destruction by these beasts, and perhaps thereby saved not only its own life, but that of its kind. The occasions of destruction, even in the best guarded, most highly endowed lives, are all of the nature of accidents, and are generally as slight as the individual advantages are, for which so much influence is claimed in the theory of natural selection. Even death from old age is not a termination preordained in the original powers of any life, but is the effect of accumulated causes of this sort. Much of the destruction to which life is subject * is *strictly fortuitous so far as* either the general powers or individual advantages in structures and habits are concerned; and is, therefore, quite independent of the effects of these advantages. Hence these effects are not

* The fortuity or chance is here, as in all other cases, a relative fact. The *strictest* use of the word applies to events which could not be anticipated except by Omniscience. To speak, therefore, of an event as *strictly accidental* is not equivalent to regarding it as undetermined, but only as determined in a manner which cannot be anticipated by a finite intelligence (see Mr. Mivart's reply, p. 458). There are degrees in the intelligibility of things, according to human means and standards. Events like eclipses, which are the most normal and predictable of all events to the astronomer, are to the savage pure accidents; and with still lower forms of intelligence events are unforeseen which are familiar anticipations in the intelligence of the savage. To believe events to be *designed* or not, according as they are or are not predictable by us, is to assume for ourselves a complete and absolute knowledge of nature which we do not possess. Hence faith in a *designing* intelligence, supreme in nature, is not the result of any capacity in our own intelligence to comprehend the design, and is quite independent of any distinctions we may make, relative to our own powers of prediction, between orderly and accidental events.

thereby limited; for though a form of life presses, and is pressed upon, in all directions, yet it presses forward no less in the directions of its advantages.

The "philosophical habit of mind," which our author admires for its "breadth of view, freedom of handling, and flexibility of mind," is sometimes optimistic, sometimes pessimistic in its views of providence in nature, according as this flexible mind has its attention bent by a genial or morose disposition to a bright or dark aspect in things. But, whichever it is, it is generally extreme or quite absolute in its judgments. The "scientific" mind, which our author contrasts with it, and believes to be characterized by "a certain rigidity and narrowness," is held *rigidly* to the truth of things, whether good or bad, agreeable or disagreeable, admirable or despicable, and is *narrowed* to the closest, most uncompromising study of facts, and to a training which enables it to render in imagination the truest account of nature as it actually exists. The "scientific" imagination is fashioned by physical studies after the patterns of nature itself. The "philosophical habit of mind," trained in the school of human life, is the habit of viewing and interpreting nature according to its own dispositions, and defending its interpretations and attacking others with the skill and weapons of forensic and dialectical discussion. The earlier physical philosophers, the "physicists" of the ancient schools, were "philosophers" in our author's sense of the term. They had not the "scientific" mind, since to them nature was a chaos hardly less confused than human affairs, and was studied with the same "breadth of view, freedom of handling, and flexibility of mind" which are fitted for and disciplined by such affairs. They were wise rather than well-informed. Their observation was guided by tact and subtlety, or fine powers of discrimination, instead of the machinery of knowledge and the arts which now fashions and guides the "scientific" mind. Thus the theory of atoms of Democritus has little resemblance to the chemical theory of atoms, since "the modern theory is the law of definite proportions; the ancient theory is merely the affirmation of indefinite combinations." Indefinite, or at least inexplicable, combinations meet the modern student of science, both physical and social, at every step of his researches; and in all

the sciences with which we have compared the theory of natural selection. He does not stop to lay hold upon these *a priori*, with the loose though flexible grasp of the "philosophical habit of mind," but studies the intimate and elementary orders in them, and presumes them to be made up of such orders, though woven in infinite and inexplicable complexity of pattern.

The division which our author makes in kinds of intellectual ability, the "philosophical" and "scientific," and regards as a more real distinction than the threefold division we proposed, is really determined by a broad distinction in the object-matter of thought and study, and is not in any way inconsistent with what we still regard as an equally real but more elementary one, which is equivalent in fact to the logical division of "hypothesis," "simple induction," and "deduction." These are not, indeed, co-ordinate as logical elements, since induction and deduction exhaust the simple elements of understanding when unaided by trained powers of perception and imagination. But practically, as habits of thought and disciplined skill in the study of nature and human affairs, they are distinct and divergent modes of investigation, partly determined by the character of the problem, — whether it be to explain, to properly name and classify, or to prove a fact from assumed or admitted premises. Skill in the formation and verification of hypotheses, dependent on a power of imagination, which physical studies discipline peculiarly, belongs peculiarly to the student of physical science; and though, perhaps, "a poor monster," as our author says, when without an adequate basis in more strictly inductive studies, yet in that division of labors and abilities, on which the economy and efficiency of scientific investigation so largely depend, there is no propriety in so regarding it, so long as co-operation in the pursuit of truth mends the monster with its counterpart and produces a symmetrical whole in that solid progress of science which such co-operation promotes.

CHAUNCEY WRIGHT.