method of receiving at B the illumination of the little squares at A."

Even this plan appears to have been anticipated in one sense two years ago, by Mr. J. E. H. Gordon of London, who says:

"I used an electromagnet consisting of an iron bar 2 feet 4 inches long and $2\frac{1}{4}$ inches in diameter, surrounded by 70 lbs. of wire, and excited by ten Grove cells.

The total *double* rotation produced, not by slightly altering the resistance, but by reversing the current, was never more than 26′ (twenty-six minutes of arc).

To see this at all with a very delicate Jellett analyzer, it was necessary for the observer to increase the sensitiveness of his eye by sitting in total darkness for some ten minutes before each observation.

Your readers can judge what chance of obtaining visible changes of illumination there would be with 'little' magnets and mere variations in a current not powerful enough to fuse a selenium resistance."

Lastly we may offer an apparatus arranged by Mr. Middleton of Cambridge, England, who gives the following account of it:—

"A lens is used to throw on a plane or suitably curved receiving plate (inclosed in a camera) the image of any object. The receiving plate of the camera is composed of thermopile elements, ground to a smooth surface, and having their posterior faces put in electrical communication by a system of wires, with a somewhat similarly constructed plate. The heating, &c., effect of the image on the first plate generates currents of electricity, which flow through the wire system, and on reaching the second thermopile plate are reconverted into heat, &c., according to the law discovered by Peltier, the amount of heat, &c., being directly proportional to the amount of electricity.

Moreover, according to the manner in which the elements of the plate are arranged with respect to each other, we can get a 'positive' or 'negative' (to use the ordinary phraseology of photography) picture on the second receiving plate, since the Peltier effect here holds, and the copy of a picture depends solely on establishing a constant ratio in the radiant heat and light which corresponding points of the picture and copy send to the eye.

Furthermore, these images can be either viewed directly or by reflected light (after the fashion of the the Japanese mirrors and projection on a screen), or by suitable apparatus they can be retained as a photograph, a thermograph, or chemicograph, the details of which will be found in the paper alluded to, and of which an abstract will, I believe, soon appear in the Proceedings of the Cambridge Philosophical Society. Also, I touched upon the method of attacking the problem of photographing in colors, and in conclusion pointed out a striking anology between the camera of the instrument and that of the human eye; the thermo-electric elements of the instrument and the rods and the cones of the eye; the conducting system of insulated wires emanating from the plate of the instrument and the optic nerve (or bundle of conducting fibres of the eye)—supposing that as the electric currents in the instruments effected a registration on the sensitive paper, so in the eye the nerve currents of the optic nerve probably leave some brain trace on the mind."

It will thus be seen that while "seeing by telegraph" is not by any means a new invention, the principle involved is one full of interest, and as yet but partially developed; in this field of research ample scope will be found for those working in this direction and valuable results may be anticipated.

THE COMING OF AGE OF THE ORIGIN OF SPECIES.[1]

Many of you will be familiar with the aspect of this small green-covered book. It is a copy of the first edition of the "Origin of Species," and bears the date of its production—the first of October, 1859. Only a few months, therefore, are needed to complete the full tale of twenty-one years since its birthday.

Those whose memories carry them back to this time will remember that the infant was remarkably lively, and that a great number of excellent persons mistook its manifestations of a vigorous individuality for mere naughtiness; in fact there was a very pretty turmoil about its cradle. My recollections of the period are particularly vivid; for having conceived a tender affection for a child of what appeared to me to be such remarkable promise, I acted for some time in the capacity of a sort of under-nurse, and thus came in for my share of the storms which threatened even the very life of the young creature. For some years it was undoubtedly warm work, but considering how exceedingly unpleasant the apparition of the new-comer must have been to those who did not fall in love with him at first sight, I think it is to the credit of our age that the war was not fiercer, and that the more bitter and unscrupulous forms of opposition died away as soon as they did.

I speak of this period as of something past and gone, possessing merely a historical, I had almost said an antiquarian, interest. For, during the second decade of the existence of the "Origin of Species," opposition, though by no means dead, assumed a different aspect. On the part of all those who had any reason to respect themselves, it assumed a thoroughly respectful character. By this time the dullest began to perceive that the child was not likely to perish of any congenital weakness or infantile disorder, but was growing into a stalwart personage, upon whom mere goody scoldings and threatenings with the birch-rod were quite thrown away.

In fact, those who have watched the progress of science within the last ten years will bear me out to the full when I assert that there is no field of biological inquiry in which the influences of the "Origin of Species" is not traceable; the foremost men of science in every country are either avowed champions of its leading doctrines, or at any rate abstain from opposing them; a host of young and ardent investigators seek for and find inspiration and guidance in Mr. Darwin's great work; and the general doctrine of Evolution, to one side of which it gives expression, finds in the phenomena of biology a firm base of operations whence it may conduct its conquest of the whole realm of nature.

History warns us, however, that it is the customary fate of new truths to begin as heresies and to end as superstitions; and, as matters now stand, it is hardly rash to anticipate that, in another twenty years, the new generation, educated under the influences of the present day, will be in danger of accepting the main doctrines of the Origin of Species with as little reflection, and it may be with as little justification, as so many of our contemporaries, twenty years ago, rejected them.

Against any such a consummation let us all devoutly pray; for the scientific spirit is of more value than its products, and irrationally-held truths may be more harmful than reasoned errors. Now the essence of the scientific spirit is criticism. It tells us that to whatever doctrine claiming our assent, we should reply, take it if you can compel it. The struggle for existence holds as much in the

1 A Lecture delivered at the Royal Institute, Friday, March 19,

intellectual as in the physical world. A theory is a species of thinking, and its right to exist is coextensive with its power of resisting extinction by its rivals.

From this point of view it appears to me that it would be but a poor way of celebrating the Coming of Age of the Origin of Species were I merely to dwell upon the facts, undoubted and remarkable as they are, of its far-reaching influence and of the great following of ardent disciples who are occupied in spreading and developing its doctrines. Mere insanities and inanities have before now swollen to portentous size in the course of twenty years. Let us rather ask this prodigious change in opinion to justify itself; let us inquire whether anything has happened since 1859 which will explain, on rational grounds, why so many are worshipping that which they burned, and burning that which they worshipped. It is only in this way that we shall acquire the means of judging whether the movement we have witnessed is a mere eddy of fashion, or truly one with the irreversible current of intellectual progress, and, like it, safe from retrogressive reaction.

Every belief is the product of two factors: the first is the state of the mind to which the evidence in favor of that belief is presented; and the second is the logical cogency of the evidence itself. In both these respects the history of biological science during the last twenty years appears to me to afford an ample explanation of the change which has taken place; and a brief consideration of the salient events of that history will enable us to understand why, if the "Origin of Species" appeared now, it would meet with a very different reception from that which greeted it in 1859.

One-and-twenty years ago, in spite of the work commenced by Hutton, and continued with rare skill and patience by Lyell, the dominant view of the past history of the earth was catastrophic. Great and sudden physical revolutions, wholesale creations and extinctions of living beings, were the ordinary machinery of the geological epoch brought in fashion by the misapplied genius of Cuvier. It was gravely maintained and taught that the end of every geological epoch was signalized by a cataclysm, by which every living being on the globe was swept away, to be replaced by a brand-new creation when the world returned to quiescence. A scheme of nature which appeared to be modelled on the likeness of a succession of rubbers of whist, at the end of each of which the players upset the table and called for a new pack, did not seem to shock anybody.

I may be wrong, but I doubt if at the present time there is a single responsible representative of these opinions left. The progress of scientific geology has elevated the fundamental principle of uniformitarianism, that the explanation of the past is to be sought in the study of the present, into the position of an axiom; and the wild speculations of the catastrophists, to which we all listened with respect a quarter of a century ago, would hardly find a single patient hearer at the present day. No physical geologist now dreams of seeking outside the ranges of known natural causes for the explanation of anything that happened millions of years ago, any more than he would be guilty of the like absurdity in regard to current events.

The effect of this change of opinion upon biological speculation is obvious. For, if there have been no periodical general physical catastrophes, what brought about the assumed general extinctions and re-creations of life which are the corresponding biological catastrophes? And if no such interruptions of the ordinary course of nature have taken place in the organic, any more than in the inorganic world, what alternative is there to the admission of Evolution?

The doctrine of Evolution in Biology is the necessary result of the logical application of the principles of uniformitarianism to the phenomena of life. Darwin is the natural successor of Hutton and Lyell, and the "Origin of Species" the natural sequence of the "Principles of Geology."

The fundamental doctrine of the "Origin of Species," as of all forms of the theory of Evolution applied to biology, is "that the innumerable species, genera, and families of organic beings with which the world is peopled have all descended, each within its own class or group, from common parents, and have all been modified in the course of descent." [1]

1 "Origin of Species," ed. 1, p. 457.

And, in view of the facts of geology, it follows that all living animals and plants "are the lineal descendants of those which lived long before the Silurian epoch." [1]

It is an obvious consequence of this theory of Descent with Modification, as it is sometimes called, that all plants and animals, however different they may now be, must, at one time or other, have been connected by direct or indirect intermediate gradations, and that the appearance of isolation presented by various groups of organic beings must be unreal.

No part of Mr. Darwin's work ran more directly counter to the prepossessions of naturalists twenty years ago than this. And such prepossessions were very excusable, for there was undoubtedly a great deal to be said, at that time, in favor of the fixity of species and of the existence of great breaks, which there was no obvious or probable means of filling up, between various groups of organic beings.

For various reasons, scientific and unscientific, much had been made of the hiatus between man and the rest of the higher mammalia, and it is no wonder that issue was first joined on this part of the controversy. I have no wish to revive past and happily forgotten controversies, but I must state the simple fact that the distinctions in cerebral and other characters, which were so hotly affirmed to separate man from all other animals in 1860, have all been demonstrated to be non-existent, and that the contrary doctrine is now universally accepted and taught.

But there were other cases in which the wide structual gaps asserted to exist between one group of animals and another were by no means fictitious; and, when such structual breaks were real, Mr. Darwin could account for them only by supposing that the intermediate forms which once existed had become extinct. In a remarkable passage he says:—

"We may thus account even for the distinctness of whole classes from each other—for instance of birds from all other vertebrate animals—by the belief that many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vetebrate classes." [2]

Adverse criticism made merry over such suggestions as these. Of course it was easy to get out of the difficulty by supposing extinction; but where was the slightest evidence that such intermediate forms between birds and reptiles as the hypothesis required ever existed? And then probably followed a tirade upon this terrible forsaking of the paths of "Baconian induction."

But the progress of knowledge has justified Mr. Darwin to an extent which could hardly have been anticipated. In 1862, the specimen of *Archæopteryx*, which until the last two or three years has remained unique, was discovered; and it is an animal which, in its feathers and the greater part of its organization, is a veritable bird, while, in other parts, it is as distinctly reptilian.

In 1868, I had the honour of bringing under your notice, in this theatre, the results of investigations made, up to that time, into the anatomical characters of certain ancient reptiles, which showed the nature of the modifications in virtue of which the type of the quadrupedal reptile passed into that of the bipedal bird; and abundant confirmatory evidence of the justice of the conclusions which I then laid before you has since come to light.

In 1875, the discovery of the toothed birds of the cretaceous formation in North America, by Prof. Marsh, completed the series of transitional forms between birds and reptiles, and removed Mr. Darwin's proposition that "many animal forms of life have been utterly lost, through which the early progenitors of birds were formerly connected with the early progenitors of the other vertebrate classes," from the region of hypothesis to that of demonstrable fact.

In 1859, there appeared to be a very sharp and clear hiatus between vertebrated and invertebrated animals, not only in their structure, but, what was more important, in their development. I do not think that we even yet know the precise links of connection between the two; but the investigations of Kowalewsky and others upon the develop-

1 "Origin of Species," ed. 1, p. 458.
2 "Origin of Species," ed. 1, p. 431.

ment of *Amphioxus* and of the *Tunicata* prove beyond a doubt that the differences which were supposed to constitute a barrier between the two are non-existent. There is no longer any difficulty in understanding how the vertebrate type may have arisen from the invertebrate, though the full proof of the manner in which the transition was actually effected may still be lacking.

Again, in 1859, there appeared to be a no less sharp separation between the two great groups of flowering and flowerless plants. It is only subsequently that the series of remarkable investigations inaugurated by Hofmeister has brought to light the extraordinary and altogether unexpected modifications of the reproductive apparatus in the *Lycopodiaceæ*, the *Rhizocarpeæ*, and the *Gymnospermeæ*, by which the ferns and the mosses are gradually connected with the Phanerogamic division of the vegetable world.

So, again, it is only since 1859 that we have acquired that wealth of knowledge of the lowest forms of life which demonstrates the futility of any attempt to separate the lowest plants from the lowest animals, and shows that the two kingdoms of living nature have a common borderland which belongs to both or to neither.

Thus it will be observed that the whole tendency of biological investigation since 1859 has been in the direction of removing the difficulties which the apparent breaks in the series created at that time; and the recognition of gradation is the first step towards the acceptance of evolution.

As another great factor in bringing about the change of opinion which has taken place among naturalists, I count the astonishing progress which has been made in the study of embryology. Twenty years ago, not only were we devoid of any accurate knowledge of the mode of development of many groups of animals and plants, but the methods of investigation were rude and imperfect. At the present time there is no important group of organic beings the development of which has not been carefully studied, and the modern methods of hardening and section-making enable the embryologist to determine the nature of the process in each case, with a degree of minuteness and accuracy which is truly astonishing to those whose memories carry them back to the beginnings of modern histology. And the results of these embryological investigations are in complete harmony with the requirements of the doctrine of evolution. The first beginnings of all the higher forms of animal life are similar, and however diverse their adult conditions, they start from a common foundation. Moreover the process of development of the animal or the plant from its primary egg or germ is a true process of evolution—a process from almost formless to more or less highly organized matter, in virtue of the properties inherent in that matter.

To those who are familiar with the process of development all *à priori* objections to the doctrine of biological evolution appear childish. Any one who has watched the gradual formation of a complicated animal from the protoplasmic mass which constitutes the essential element of a frog's or a hen's egg has had under his eyes sufficient evidence that a similar evolution of the animal world from the like foundation is, at any rate, possible.

Yet another product of investigation has largely contributed to the removal of the objections to the doctrine of Evolution current in 1859. It is the proof afforded by successive discoveries that Mr. Darwin did not overestimate the imperfection of the geological record. No more striking illustration of this is needed than a comparison of our knowledge of the mammalian fauna of the Tertiary epoch in 1859 with its present condition. M. Gaudry's researches on the fossils of Pikermi were published in 1868, those of Messrs. Leidy, Marsh, and Cope on the fossils of the Western Territories of America, have appeared almost wholly since 1870; those of M. Filhol, on the phosphorites of Quercy, in 1878. The general effect of these investigations has been to introduce us to a multitude of extinct animals, the existence of which was previously hardly suspected; just as if zoologists were to become aquainted with a country, hitherto unknown, as rich in novel forms of life, as Brazil or South Africa once were to Europeans. Indeed the fossil fauna of the Western Territories of America bids fair to exceed in interest and importance all other known Tertiary deposits put together; and yet, with the exception of the case of the AmericanTertiaries, these investigations have extended over very limited areas, and at Pikermi were confined to an extremely small space.

Such appear to me to be the chief events in the history of the progress of knowledge, during the last twenty years, which account for the changed feeling with which the doctrine of Evolution is at present regarded by those who have followed the advance of biological science in respect of those problems which bear indirectly upon that doctrine.

But all this remains mere secondary evidence. It may remove dissent, but it does not compel assent. Primary and direct evidence in favor of Evolution can be furnished only by palæontology. The geological record, so soon as it approaches completeness, must, when properly questioned, yield either an affirmative or negative answer; if evolution has taken place, there will its mark be left; if it has not taken place, there will lie its refutation.

What was the state of matters in 1859? Let us hear Mr. Darwin, who may be trusted always to state the case against himself as strongly as possible.

"On this doctrine of the extermination of an infinitude of connecting links between the living and extinct inhabitants of the world, and at each successive period between the extinct and still older species, why is not every geological formation charged with such links? Why does not every collection of fossil remains afford plain evidence of the gradation and mutation of the forms of life? We meet with no such evidence, and this is the most obvious and plausible of the many objections which may be urged against my theory."[1]

Nothing could have been more useful to the opposition than this characteristically candid avowal, twisted as it immediately was into an admission that the writer's views were contradicted by the facts of palæontogy. But, in fact, Mr. Darwin made no such admission. What he says in effect is, not that palæontological evidence is against him, but that it is not distinctly in his favor; and without attempting to attenuate the fact, he accounts for it by the scantiness and the imperfection of that evidence.

What is the state of the case now, when, as we have seen, the amount of our knowledge respecting the mammalia of the Tertiary epoch is increased fifty-fold, and in some directions even approaches completeness?

Simply this, that if the doctrine of Evolution has not existed palæontologists must have invented it, so irresistibly is it forced upon the mind by the study of the remains of the Tertiary mammalia which have been brought to light since 1859.

Among the fossils of Pikermi, Gaudry found the successive stages by which the ancient civets passed into the more modern hyænas; through the Tertiary deposits of Western America, Marsh tracked the successive forms by which the ancient stock of the horse has passed into its present form; and innumerable less complete indications of the mode of evolution of other groups of the higher mammalia have been obtained.

In the remarkable memoir on the Phosphorites of Quercy, to which I have referred, M. Filhol describes no fewer than seventeen varieties of the genus *Cynodictis*, which fill up all the interval between the viverine animals and the bear-like dog *Amphicyon;* nor do I know any solid ground of objection to the supposition that in this *Cynodictis-Amphicyon* group we have the stock whence all the Viveridæ, Felidæ, Hyænidæ, Canidæ, and perhaps the Procyonidæ and Ursidæ, of the present fauna have been evolved. On the contrary, there is a great deal to be said in its favor.

In the course of summing up his results, M. Filhol observes[2]:—

"During the epoch of the phosphorites, great changes took place in animal forms, and almost the same types as those which now exist became defined from one another.

Under the influence of natural conditions of which we have no exact knowledge, though traces of them are discoverable, species have been modified in a thousand ways; races have arisen which, becoming fixed, have

(*Concluded on page* 20.)

1 "Origin of Species, ed. 1. p. 463.
2 This passage was omitted in the delivery of the lecture.

thus produced a corresponding number of secondary species."

In 1859, language of which this is an unintentional paraphrase, occurring in the "Origin of Species," was scouted as wild speculation; at present, it is a sober statement of the conclusions to which an acute and critically-minded investigator is led by large and patient study of the facts of palæontology. I venture to repeat what I have said before, that, so far as the animal world is concerned, Evolution is no longer a speculation, but a statement of historical fact. It takes its place alongside of those accepted truths which must be taken into account by philosophers of all schools.

Thus when, on the first day of October next, the "Origin of Species" comes of age, the promise of its youth will be amply fulfilled; and we shall be prepared to congratulate the venerated author of the book, not only that the greatness of his achievement and its enduring influence upon the progress of knowledge have won him a place beside our Harvey; but, still more, that, like Harvey, he has lived long enough to outlast detraction and opposition, and to see the stone that the builders rejected become the head-stone of the corner.

T. H. HUXLEY.

WATER ANALYSIS.

The recent publication of Dr. Frankland's convenient little volume on this subject;* the important memoir by Dr. Tidy read and discussed last year before the Chemical Society, and published in its *Journal*;† and the volume published as long ago as 1868, and now appearing in a fifth edition, by Professor Wanklyn and Mr Chapman,‡ in which Dr. Frankland's title was anticipated, contains the literature of a subject which has of late years assumed extreme importance, but concerning which there is a haze of mystery and obscurity that assuredly ought not to exist. It is certainly very much to be regretted that there is no common and recognized method of procedure in this department of chemistry, but it is almost discreditable that not only are the results of analyses given in discordant chemical expressions, but even the figures are not in the same terms, so that a comparison of results is impossible without performing an arithmetical operaton. So long as one chemist expresses his results in 100-1000th parts, another in grains per gallon, and a third in milligrammes, or parts in a million; while one estimates ammonia as a total, another separates free from organic ammonia, and the third regards the quantity of what is called albumenoid ammonia as of vital importance; while one adopts the combustion or evaporation method to determine the actual quantity of organic contents of water, and another accepts the permanganate method to discover the quantity of oxygen required to oxidize the organic matter present; it is evident that the comparison of analyses affected by the pupils of the various schools cannot be satisfactory or conclusive, because they cannot be compared. Surely the time has come when methods of analysis giving the quantity of organic carbon and nitrogen and its condition or history in some intelligible form, the quantity of nitrogen as nitrates, the quantity of chlorine, and the hardness, in similar terms, should be so far agreed upon that results can be compared, and those who are not chemists will then be able to form some opinion as to facts. We believe all our most distinguished chemists would agree that this is possible. It only needs that each should give way in some matters that are not essential, but rather belong to the individual. Dr. Tidy well and properly observes in his paper (*Journal of the Chemical Society*, Jan., 1879): "I am afraid the public *have* taken note and *are* taking note of chemists' differences, and distrust our work accordingly. Nor indeed is their distrust to be wondered at, deeply as it is to be lamented."

In Dr. Frankland's book we find stated, in a compact and convenient form, the requirements of a water analysis, and in an appendix examples of typical analyses. He begins by pointing out the fact that complete and ultimate analyses are by no means called for in ordinary cases. He points out the unimportance, in a sanitary sense, of the dissolved gases, which vary but little in waters of very different kinds, and of which the presence of a smaller or larger quantity does not affect the goodness of the water; the difference, in fact, lying chiefly in the quantity of carbonic acid. The separate estimation of the quantity of each of the saline matters and of each organic constituent of the suspended matters, may in like manner, and for the same reason, be omitted. The processes adopted to determine the quantities of inorganic solids, the ammonia, the chlorine, the nature of the hardening ingredients, and the presence of poisonous metals, if any, are those which are really important, and a knowledge of them and of the amount of nitrates, and lastly, but of chief importance, the means of estimating approximately the proportion of the organic elements in a sample of water, are the objects of which attention is really required, and to the elucidation to which the volume is dedicated. Professor Frankland considers that there is no process, short of the actual combustion of the organic matter present in water, which affords thoroughly trustworthy evidence of the organic carbon and nitrogen, and of the fitness or otherwise of the sample for dietetic purposes. The "ignition" and "albumenoid ammonia" he merely mentions, and evidently disregards. The former is described by Dr. Tidy in his memoir, and he considers it is not satisfactory, as failing to show that, in carrying out the process—(1) no organic matter is lost, (2) that all the organic matter is burnt off, and (3) that no organic matter is added. Notwithstanding this, he adopts it in the analysis of sewerage, and thinks that in some respects it may be indicative, and suggestive in other cases. The ammonia process, described by Mr. Wanklyn as "a sort of combustion process, with ammonia for the ultimate product," has for its object the comparative determination of the nitrogenous organic matter by the quantity of ammonia yielded by the destruction of the organic matter, this quantity being called "albumenoid ammonia." (Wanklyn's "Water Analysis," 5th Ed., p. 31.) Dr. Tidy has considered in detail the advantages and disadvantages of this method and has given some remarkable illustrations of its failure in important cases. He points out the very important fact that the quantity of albumenoid ammonia in peaty water is very large, although it has never been proved that such water is in any sense injurious; and, on the other hand, that in waters regarded by Mr. Wanklyn as exceedingly bad, the albumenoid ammonia is almost *nil*. For these reasons apparently, as he quotes Dr. Tidy's paper and gives no other reference, Dr. Frankland rejects them.

In the commencement of this work, following Mr. Wanklyn in this, Dr. Frankland describes the preliminary considerations in water sampling, the quantity required, and the tests that should be applied to determine the presence of mineral poisons, the nature of refuse from manufactures, the action on soft lead, and the cause of turbidity. Having thus opened the subject, he proceeds to show in what way the total solids in solution can be best determined. To determine the organic contents, he prefers the combustion method. He describes the precautions required in evaporation, and believes that "the proportion of solid residue left on evaporation affords an approximate, though somewhat rough indication of the comparative purity of water." This, no doubt, is true in a certain sense, although it must not be concluded that waters showing a large residue are necessarily bad. It is with water as with many other things, we must be content with the best we can obtain under existing circumstances, and absolute purity is practically unobtainable. A tolerably good river water at hand is often better than deep well water or lake water from a distance, though theoretically superior.

* "Water Analysis for Sanitary Purposes, with Hints for the Interpretation of Results." By E. Frankland, Ph.D., F. R. S., &c. London: Van Voorst. 1880.

† "The Processes for Determining the Organic Purity of Potable Waters." By C. Meymott Tidy, M.B. *Journal of the Chemical Society*, Jan., 1879.

‡ "Water Analysis: a Practical Treatise on the Examination of Potable Water." By J. Alfred Wanklyn, M.R.C.S., and Ernest Theophron Chapman. London: Thrübner & Co. Fifth Edition. 1879.