perspicacious and penetrating, enthusiastic and energetic, his faculty of seizing a vague idea, and shaping it for useful application, is inherent, and was by no means exhausted when he had accomplished the success of the electric enterprise. No man better than he can command the courage for a difficult undertaking, and carry it on while a point of perfectability remains unattained.

That the illiberal influence which has pertinaciously sought to deprive Mr. Cooke of the honours justly due to him may be overcome, must be the sincere wish of all lovers of truth.

I am, Sir, yours obediently,

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To the Editor of THE READER.

SIR,—Reference has been made in *The Reader* and also in the *Saturday Review* to the highly-meritorious plan of Mr. Ronalds, published in 1823, for a telegraph to work by frictional electricity. Permit me to mention that Mr. Cooke did justice to Mr. Ronalds in the concluding passages of his pamphlet of 1856, which I extract:—

"In what does the merit of the electric telegraph really consist? If the invention were to be described generally in a few words, how would you describe it? Might it not be called an application of a few known principles, by means of a few simple contrivances, to produce a practical result, which the experiments of scientific men, though their attention had been directed to the subject for a long series of years, had failed to produce? The merit of the invention must then consist, in a very great degree at least, in the *practical realization* of that which had been before an idea or an experiment. To the merit, such as it may be, of this practical realization, I have maintained, from first to last, one consistent claim. Eighteen years ago my unanswered letter of 1838 referred to it as understood and admitted. Fifteen years ago the Arbitrators solemnly awarded it to me; and it is not without cause, nor till after long forbearance, that I now expect a final confirmation of the same unpretending claim from the justice of the Public. 'There is no magic in terms,' as Mr. Wheatstone says; and it is not worth discussing whether the name of 'originator, projector, or any other title as sonorous and equivocal,' sufficiently expresses my right to a position, unambiguous in itself—which Mr. Ronalds, under the favouring circumstances in which I found myself, might probably have occupied—or a gentleman at Renfrew, whose anonymous suggestion, a century in advance of his time, has recently been made public.

"The philosopher's researches into the laws of nature are essentially distinct from the labours of the practical man who applies those laws to the purposes of daily life. I may therefore consistently yield to Professor Wheatstone a high rank among those scientific men, who in several countries, entertained theoretically the idea of an improved mode of transmitting intelligence."

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INTERNATIONAL COPYRIGHT.

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SIR,—I should be very sorry to involve you into printing a controversy on this vexed question, but will you kindly afford me space to correct one or two of "A. C. L.'s" misinterpretations of my text? In the first place, I distinctly stated that I have been myself annoyed by the attempts of other houses to interfere with the Lippincott editions of my novels. Indeed, it is at this moment a neck-to-neck race between Philadelphia and New York to secure the first issue of a forthcoming work of my own. Hence, "A. C. L." might have clearly gathered that I was in no way ignorant of, or desirous to ignore, similar cases to the one he cites regarding Mr. Tennyson. These are the exceptions which accompany every rule. I merely stated that American publishers can *usually* obtain a monopoly of a book by priority of advertisement. That they can do so is due to three things:—1st. That though there is no law to prevent twenty firms from issuing twenty editions of the same work, custom (a very weighty substitute for law all the world over), and trade interest combined, make each house see that, on the average, it will serve itself far best by not thus borrowing from its brethren. 2nd. If an American publisher gets the first supply of the market, he supplies it so largely that he does not care much who may follow him with the same wares. 3rd. If an American publisher interferes with another's commerce by bringing out a work already produced by that other, the one injured has so easy and so effective a retaliation in his hands through his power to make reprisals on any of his foe's publications that he pleases, that it is generally recognized how a mutual courtesy in this matter always proves also, in the long run, not only the safest but the most lucrative form of business. In answer to "A. C. 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The whole argument is, "Give us a copyright law, that we may insist on our hundreds and thousands being paid us, and be enabled to refuse our books if the hundreds and thousands offered are not numerous enough." Now, what can this motive possibly be except a mercenary one? It may be a very just, a very honest, a very sensible one. I do not gainsay it, but its root and its aim are both,—money. If what they desired were the circulation of their works, they have that already. But it is not this. They want the power to enforce high payments. Well, they have every atom as much right to this possibly as the fisherman has to be paid for his boat-load of herrings. But I insist that this being their motive they should frankly declare it; and not cloak their wish to swell their bank-balance under fine-English periods about international justice, intellectual rights, and all the rest of it. 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P.S.—Mr. Trollope, in his Harper grievances, always complained that the American publishing was miserable to an English author, because an American publisher, once having any of your works, always conceives that he has a sort of vested right in them all. "A. C. L.," on the contrary, bemoans the fact that an American publisher cannot claim any right, legal or moral, though you wish him to be your exclusive printer and producer. Now there is one way, simple enough, to avoid either of these annoyances. Make your agreement with your English publisher so stringently that he cannot enter into arrangements with a Transatlantic house for your book for his own benefit; and take care that you forward to the States your "early sheets" long enough before the appearance of your London edition for them to appear simultaneously. By this means you will have little to fear from American piracy, and need only take care that your English copies do not get over there in a cheaper form, to damage your United States sale. Of course, this rule will not serve with magazine serials, unless brought out also in American periodicals.

CHOLERA.

To the Editor of THE READER.

SIR,—I am really quite ashamed to take up your time and to be obliged to beg a place in your valuable paper for anything which can, in the least degree, have the appearance of a personal character. The last number of the *Reader* contains an extraordinary letter from Dr. Chapman, in which he intentionally makes two false quotations from my little book on Cholera, quotations which he is pleased to characterize as "amazing fictions." I should have passed over such an epithet with a smile, but I cannot allow these really "amazing fictions"—the creation of his own inventive faculties—to be saddled on me. Dr. Chapman makes me say, "no surface will secrete," and that the breath of cholera patients is "sensibly four or five degrees below that of the surrounding atmosphere *at all ordinary temperatures.*" It is marvellous to find such daring misrepresentations of a book which Dr. Chapman professes to have read. At page 8 he will find the following words:—"All the secretions are entirely suspended;" the same expression is repeated several times, in nearly the same words, in different parts of my essay. Such an expression as "no surface will secrete," never has been, and never could have been used by me. Dr. Chapman will kindly, then, acknowledge the authorship. At page 16, he will also find, "the expired air of cholera patients has the same chemical composition as that of the atmosphere, but it is four or five degrees below the surrounding temperature." Again, at page 91:—"The air expired by cholera patients . . . . is at 4° or 5° below the temperature of a warm room," which has a totally different signification from that given by his OBLIGING addition of "*at all ordinary temperatures.*"

It is most flattering to me to find that the only way by which my modest little essay can be attacked is by false quotations. Now, Dr. Chapman will permit me to quote CORRECTLY a few lines which I find at the commencement of his letter to the editor of the *Reader*: "One of the doctrines upon which my treatment of cholera is based is, that in all stages of this disease, before reaction sets in, the arteries throughout the body are in a state of spasmodic contraction, caused, proximately, by abnormally vehement stimulus from the sympathetic nervous centres." What authority has Dr. Chapman for stating that "the arteries throughout the body are in a state of spasmodic contraction, caused, proximatively, by abnormally vehement stimulus from the sympathetic nervous centres?" Such a vague assertion has no scientific meaning whatever, and consequently any theory constructed on such a BASIS can be nothing more than an "amazing fiction."

I regret to say that Dr. Chapman's treatment of cholera patients, although carefully applied by himself in the hospitals of Paris, proved a signal failure, leaving no other trace than that of regret that so much labour and so much talent should have been exhausted in the vain endeavour to support a fallacious theory.—I am, Sir, yours, greatly obliged,

CHARLES SHRIMPTON, M.D.

Rue d'Anjou, St. Honoré 17, Paris,
November 27, 1866.

SCIENCE.

ORIGIN OF SPECIES.

On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., F.R.S., &c. Fourth Edition, with Additions and Corrections. 8vo, pp. xxi.—593. 14*s.* (Murray.)

THIS fourth edition of one of the most widely influential books of the century, is far from being a mere reprint. The additions and corrections are neither few nor unimportant. The author has tabulated most of them for the convenience of reference, so that the corroborations which the progress of science has brought to the theory of Natural Selection may be considered without trouble by every one who is at home in the last edition. Had Mr. Darwin but delayed the publication a little longer, he would have avoided quoting the existence of the *Eozoon Canadense* as an argument in his favour. The labours of Professors King and Rowney are conclusive on the point. However, with his usual caution, Mr. Darwin has not built much on what was till very recently looked upon as an undoubted fact by many distinguished naturalists, in whose company it is no disgrace to err. On the contrary, he frankly admits that to some extent it makes the difficulty caused by the absence beneath the Silurian formations of piles of strata rich in fossils greater than ever. Had the *Eozoon*, as Dr. Dawson thought, existed in countless numbers, it must have preyed on other minute organic beings, which must have been still more numerous. To support such a pyramid of animal life, plants must have existed, but of these no trace has been found. "The case at present must remain inexplicable, and may be truly urged as a valid argument against the view here entertained." Pre-Silurian life is, therefore, still a question. Amongst the recent observations which help Mr. Darwin's theory, those by De Candolle on the variability of the oak-genus are not the least interesting:—

He first gives in detail all the many points of structure which vary in the species, and estimates numerically the relative frequency of the variations. He specifies above a dozen characters which may be found varying even on the same branch, sometimes according to age or development, sometimes without any assignable reason. Such characters of course are not of specific value, but they are, as Asa Gray has remarked in commenting on this memoir, such as generally enter into specific definitions. De Candolle then goes on to say that he gives the rank of species to the forms that differ by characters never varying on the same tree, and never found connected by intermediate states. After this discussion, the result of so much labour, he emphatically remarks:—"They are mistaken, who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a feeble minority. This seemed to be true, so long as a genus was imperfectly known, and its species were founded upon a few specimens, that is to say, were provisional. Just as we come to know them better, intermediate forms flow in, and doubts as to specific limits augment." He also adds that it is the best known species which present the greatest number of spontaneous varieties and sub-varieties. Thus Quercus robur has twenty-eight varieties, all of which, excepting six, are clustered round three sub-species, namely, Q. pedunculata, sessiliflora, and pubescens. The forms which connect these three sub-species are comparatively rare; and, as Asa Gray remarks, if these connecting forms, which are now rare, were to become extinct, the three sub-species would hold exactly the same relation to each other, as do the four or five provisionally-admitted species which closely surround the typical Quercus robur. Finally, De Candolle admits that out of the 300 species, which will be enumerated in his Prodromus as belonging to the oak family, at least two-thirds are provisional species, that is, are not known strictly to fulfil the definition above given of a true species.

Perhaps no idea has been so much laughed at as the one that a mere sensitiveness to light produced the eye, instead of the eye being made to see. But is the notion so singular and unexampled after all?

To suppose that the eye, with all its inimitable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection, seems, I freely confess, absurd in the highest degree. When it was first said that the sun stood still and the world turned round, the common sense of mankind declared the doctrine false; but the old saying of *vox populi, vox Dei*, as every philosopher knows, cannot be trusted in science. Reason tells me, that if numerous gradations from a perfect and complex eye to one imperfect and simple, each grade being useful to its possessor, can be shown to exist; if further, the eye does vary ever so slightly, and the variations be inherited, which is certainly the case; and if any variation or modification in the organ be ever useful to an animal under changing conditions of life, then the difficulty of believing that a perfect and complex eye could have been formed by natural selection, though insuperable by our imagination, can hardly be considered real. How a nerve comes to be sensitive to light, hardly concerns us more than how life itself first originated; but I may remark that, as some of the lowest organisms, in which nerves cannot be detected, are known to be sensitive to light, it does not seem impossible that certain elements in their tissues or sarcode should have become aggregated and developed into nerves endowed with special sensibility to its action.

Here, also, is a fresh passage, which touches upon the highest problems of creation:—

With respect to the view that organic beings have been created beautiful for the delight of man, —a view which it has lately been pronounced may safely be accepted as true, and as subversive of my whole theory,—I may first remark that the idea of the beauty of any particular object obviously depends on the mind of man, irrespective of any real quality in the admired object; and that the idea is not an innate and unalterable element in the mind. We see this in men of different races admiring an entirely different standard of beauty in their women; neither the Negro nor the Chinese admires the Caucasian beau-ideal. The idea also of beauty in natural scenery has arisen only within modern times. On the view of beautiful objects having been created for man's gratification, it ought to be shown that there was less beauty on the face of the earth before man appeared than since he came on the stage. Were the beautiful volute and cone shells of the Eocene epoch, and the gracefully-sculptured ammonites of the Secondary period, created that man might ages afterwards admire them in his cabinet? Few objects are more beautiful than the minute siliceous cases of the diatomaceæ: were these created that they might be examined and admired under the higher powers of the microscope? The beauty in this latter case, and in many others, is apparently wholly due to symmetry of growth. Flowers rank amongst the most beautiful productions of nature; and they have become through natural selection beautiful, or rather conspicuous in contrast with the greenness of the leaves, that they might be easily observed and visited by insects, so that their fertilisation might be favoured. I have come to this conclusion from finding it an invariable rule that when a flower is fertilised by the wind it never has a gaily-coloured corolla. Again, several plants habitually produce two kinds of flowers; one kind opened and coloured so as to attract insects; the other closed and not coloured, destitute of nectar, and never visited by insects. We may safely conclude that, if insects had never existed on the face of the earth, the vegetation would not have been decked with beautiful flowers, but would have produced only such poor flowers as are now borne by our firs, oaks, nut, and ash trees, by the grasses, by spinach, docks, and nettles.

One of the most popular tales of natural history, which has afforded a text for moralists of every age, is rudely assailed at page 271:—"I hear from Professor Wyman, who has made numerous careful measurements, that the accuracy of the workmanship of the bee has been greatly exaggerated; so much so, that, as he adds, whatever the typical form of the cell may be, it is rarely, if ever, realised." Of course, much of the new matter has been adopted from scientific papers already in print; but it has been carefully digested, and worked up with the original composition in such a form that no one would detect the juncture. The experiments of Sir John Lubbock on Chlöon dimidiatum, reports of which have at various times appeared in The Reader, are commented upon at great length; in fact, they have compelled the whole chapter on "Embryology and Development" to be entirely re-written. "Fritz Müller, who has recently discussed this whole subject with much ability, goes so far as to believe that the progenitor of all insects probably resembled an adult insect, and that the caterpillar or maggot, and cocoon or pupal stages, have subsequently been acquired; but from this view many naturalists—for instance, Sir J. Lubbock, who has likewise recently discussed this subject—would, it is probable, dissent." But Mr. Darwin's ideas as to the probable nature of our own common ancestor, derive fresh strength from the conjectures of Müller on another point. "It is probable, from what we know of the embryos of mammals, birds, fishes, and reptiles, that all the members in these four great classes are the modified descendants of some one ancient progenitor, which was furnished in its adult state with branchiæ, had a swim-bladder, four simple limbs, and a long tail fitted for an aquatic life." A picture, or, at all events, an outline of this imaginary animal, would be very attractive.

It is to be understood that we have only touched on a few of the additions to the Darwinian arguments. The remarks on the Australian cuckoo, and the various discoveries of Mr. Wallace, we omit with reluctance. Such a work as this will never be complete in the sense of being perfect. No one is better aware of this than Mr. Darwin himself. He has altered none of the passages which describe it as an "Abstract," and we gather that much is only a selection from the rich stores he is accumulating by way of proof and exposition. Let us hope they will themselves be given to the public before long, and by no inferior hand.

ANTHROPOLOGY.

Memoirs Read before the Anthropological Society of London: 1865-6. Volume II. (Trübner and Co.)

THE recent publication of the Second Volume of the Anthropological Society's Memoirs will be hailed with satisfaction by all who had not the opportunity of hearing these papers read and discussed at the ordinary meetings, or at the last Congress of the British Association. We cannot but congratulate the Society on the position which it now deservedly occupies, and from which it had been so long unjustly debarred. But no more of this. The volume before us tasks all our powers. Thirty Papers contain, as may be imagined, a very large amount of information, and for our convenience we will arrange them under the several heads of General, Descriptive, Archaic, and Historical Anthropology—a classification for which we are indebted to the President. We cannot pretend to give in this article anything like an analysis of the Memoirs, and must be content to direct attention to those most worthy of notice. The last in the series, by Dr. Mitchell, on "*Blood Relationship in Marriage*," is decidedly not the least in importance. The author has applied himself to the question in a cautious, candid, philosophical spirit, untrammelled by any bias that would render his facts or his deductions less trustworthy; therefore he leaves us the more open to conviction. His enquiries are confined to Scotland, where his duties as a Deputy-Commissioner of Lunacy have induced him to investigate the influences of consanguinity, more especially in reference to the production of insanity and cerebral disease; but his observations extend to the existence of deaf-mutism, consumption, scrofula, and other morbid conditions, as the possible results of the same pathogenetic cause. Some of the cases appear to be striking proofs of the evil effects of intermarriage between blood-relations; but a wider field of enquiry has convinced him that these were exceptional cases, and that effects equally deplorable may be found to proceed from unions which have no kinship at all about them. Still it would appear that although the direct issue of an allied union may be healthy, the evil effects may show themselves in the third or fourth generation. It must be a nice and difficult point to determine, whether a particular disease be the result of hereditary transmission, or whether it has originated in the union of blood. There can, however, be no doubt that if consanguinity does not of itself produce disease in the offspring, it strengthens and intensifies those morbid proclivities in the parents' constitutions which tend to the production of disease in their children. Therefore the risk should be avoided. The conclusions arrived at by the author he formulates thus:—

1. That consanguinity in parentage tends to injure the offspring. That this injury assumes various forms: "as diminished viability; feeble constitution; bodily defects; impairment of the senses; disturbance of the nervous system; sterility."

2. That the injury may shew itself in the grandchildren: "so that there may be given to the offspring by the kinship of the parents a *potential defect* which may become *actual* in their

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CHOLERA.

To the Editor of THE READER.

Sir,—I am really quite ashamed to take up your time and to be obliged to beg a place in your valuable paper for anything which can, in the least degree, have the appearance of a personal character. The last number of the *Reader* contains an extraordinary letter from Dr. Chapman, in which he intentionally makes two false quotations from my little book on Cholera, quotations which he is pleased to characterize as "amazing fictions." I should have passed over such an epithet with a smile, but I cannot allow these really "amazing fictions"—the creation of his own inventive faculties—to be saddled on me. Dr. Chapman makes me say, "no surface will secrete," and that the breath of cholera patients is "sensibly four or five degrees below that of the surrounding atmosphere *at all ordinary temperatures.*" It is marvellous to find such daring misrepresentations of a book which Dr. Chapman professes to have read. At page 8 he will find the following words:—"All the secretions are entirely suspended;" the same expression is repeated several times, in nearly the same words, in different parts of my essay. Such an expression as "no surface will secrete," never has been, and never could have been used by me. Dr. Chapman will kindly, then, acknowledge the authorship. At page 16, he will also find, "the expired air of cholera patients has the same chemical composition as that of the atmosphere, but it is four or five degrees below the surrounding temperature." Again, at page 91:—"The air expired by cholera patients . . . . is at 4° or 5° below the temperature of a warm room," which has a totally different signification from that given by his OBLIGING addition of "*at all ordinary temperatures.*"

It is most flattering to me to find that the only way by which my modest little essay can be attacked is by false quotations. Now, Dr. Chapman will permit me to quote CORRECTLY a few lines which I find at the commencement of his letter to the editor of the *Reader*: "*One* of the doctrines upon which my treatment of cholera is based is, that in all stages of this disease, before reaction sets in, the arteries throughout the body are in a state of spasmodic contraction, caused, proximately, by abnormally vehement stimulus from the sympathetic nervous centres." What authority has Dr. Chapman for stating that "the arteries throughout the body are in a state of spasmodic contraction, caused, proximatively, by abnormally vehement stimulus from the sympathetic nervous centres?" Such a vague assertion has no scientific meaning whatever, and consequently any theory constructed on such a BASIS can be nothing more than an "amazing fiction."

I regret to say that Dr. Chapman's treatment of cholera patients, although carefully applied by himself in the hospitals of Paris, proved a signal failure, leaving no other trace than that of regret that so much labour and so much talent should have been exhausted in the vain endeavour to support a fallacious theory.—I am, Sir, yours, greatly obliged,

CHARLES SHRIMPTON, M.D.

Rue d'Anjou, St. Honoré 17, Paris,
November 27, 1866.

SCIENCE.

ORIGIN OF SPECIES.

On the Origin of Species by Means of Natural Selection, or the Preservation of Favoured Races in the Struggle for Life. By Charles Darwin, M.A., F.R.S., &c. Fourth Edition, with Additions and Corrections. 8vo, pp. xxi.—593. 14s. (Murray.)

THIS fourth edition of one of the most widely influential books of the century, is far from being a mere reprint. The additions and corrections are neither few nor unimportant. The author has tabulated most of them for the convenience of reference, so that the corroborations which the progress of science has brought to the theory of Natural Selection may be considered without trouble by every one who is at home in the last edition. Had Mr. Darwin but delayed the publication a little longer, he would have avoided quoting the existence of the *Eozoon Canadense* as an argument in his favour. The labours of Professors King and Rowney are conclusive on the point. However, with his usual caution, Mr. Darwin has not built much on what was till very recently looked upon as an undoubted fact by many distinguished naturalists, in whose company it is no disgrace to err. On the contrary, he frankly admits that to some extent it makes the difficulty caused by the absence beneath the Silurian formations of piles of strata rich in fossils greater than ever. Had the *Eozoon*, as Dr. Dawson thought, existed in countless numbers, it must have preyed on other minute organic beings, which must have been still more numerous. To support such a pyramid of animal life, plants must have existed, but of these no trace has been found. "The case at present must remain inexplicable, and may be truly urged as a valid argument against the view here entertained." Pre-Silurian life is, therefore, still a question. Amongst the recent observations which help Mr. Darwin's theory, those by De Candolle on the variability of the oak-genus are not the least interesting :—

He first gives in detail all the many points of structure which vary in the species, and estimates numerically the relative frequency of the variations. He specifies above a dozen characters which may be found varying even on the same branch, sometimes according to age or development, sometimes without any assignable reason. Such characters of course are not of specific value, but they are, as Asa Gray has remarked in commenting on this memoir, such as generally enter into specific definitions. De Candolle then goes on to say that he gives the rank of species to the forms that differ by characters never varying on the same tree, and never found connected by intermediate states. After this discussion, the result of so much labour, he emphatically remarks :—"They are mistaken, who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a feeble minority. This seemed to be true, so long as a genus was imperfectly known, and its species were founded upon a few specimens, that is to say, were provisional. Just as we come to know them better, intermediate forms flow in, and doubts as to specific limits augment." He also adds that it is the best known species which present the greatest number of spontaneous varieties and sub-varieties. Thus Quercus robur has twenty-eight varieties, all of which, excepting six, are clustered round three sub-species, namely, Q. pedunculata, sessiliflora, and pubescens. The forms which connect these three sub-species are comparatively rare; and, as Asa Gray remarks, if these connecting forms, which are now rare, were to become extinct, the three sub-species would hold exactly the same relation to each other, as do the four or five provisionally-admitted species which closely surround the typical Quercus robur. Finally, De Candolle admits that out of the 300 species, which will be enumerated in his Prodromus as belonging to the oak family, at least two-thirds are provisional species, that is, are not known strictly to fulfil the definition above given of a true species.

Perhaps no idea has been so much laughed at as the one that a mere sensitiveness to light produced the eye, instead of the eye being made to see. But is the notion so singular and unexampled after all?

To suppose that the eye, with all its inimitable contrivances for adjusting the focus to different distances, for admitting different amounts of light, and for the correction of spherical and chromatic aberration, could have been formed by natural selection, seems, I freely confess, absurd in the highest degree. When it was first said that the sun stood still and the world turned round, the common sense of mankind declared the doctrine false; but the old saying of *vox populi, vox Dei*, as every philosopher knows, cannot be trusted in science. Reason tells me, that if numerous gradations from a perfect and complex eye to one imperfect and simple, each grade being useful to its possessor, can be shown to exist; if further, the eye does vary ever so slightly, and the variations be inherited, which is certainly the case; and if any variation or modification in the organ be ever useful to an animal under changing conditions of life, then the difficulty of believing that a perfect and complex eye could have been formed by natural selection, though insuperable by our imagination, can hardly be considered real. How a nerve comes to be sensitive to light, hardly concerns us more than how life itself first originated; but I may remark that, as some of the lowest organisms, in which nerves cannot be detected, are known to be sensitive to light, it does not seem impossible that certain elements in their tissues or sarcode should have become aggregated and developed into nerves endowed with special sensibility to its action.

Here, also, is a fresh passage, which touches upon the highest problems of creation :—

With respect to the view that organic beings have been created beautiful for the delight of man, —a view which it has lately been pronounced may safely be accepted as true, and as subversive of my whole theory,—I may first remark that the idea of the beauty of any particular object obviously depends on the mind of man, irrespective of any real quality in the admired object; and that the idea is not an innate and unalterable element in the mind. We see this in men of different races admiring an entirely different standard of beauty in their women; neither the Negro nor the Chinese admires the Caucasian beau-ideal. The idea also of beauty in natural scenery has arisen only within modern times. On the view of beautiful objects having been created for man's gratification, it ought to be shown that there was less beauty on the face of the earth before man appeared than since he came on the stage. Were the beautiful volute and cone shells of the Eocene epoch, and the gracefully-sculptured ammonites of the Secondary period, created that man might ages afterwards admire them in his cabinet? Few objects are more beautiful than the minute siliceous cases of the diatomaceæ: were these created that they might be examined and admired under the higher powers of the microscope? The beauty in this latter case, and in many others, is apparently wholly due to symmetry of growth. Flowers rank amongst the most beautiful productions of nature; and they have become through natural selection beautiful, or rather conspicuous in contrast with the greenness of the leaves, that they might be easily observed and visited by insects, so that their fertilisation might be favoured. I have come to this conclusion from finding it an invariable rule that when a flower is fertilised by the wind it never has a gaily-coloured corolla. Again, several plants habitually produce two kinds of flowers; one kind opened and coloured so as to attract insects; the other closed and not coloured, destitute of nectar, and never visited by insects. We may safely conclude that, if insects had never existed on the face of the earth, the vegetation would not have been decked with beautiful flowers, but would have produced only such poor flowers as are now borne by our firs, oaks, nut, and ash trees, by the grasses, by spinach, docks, and nettles.

One of the most popular tales of natural history, which has afforded a text for moralists of every age, is rudely assailed at page 271 :— "I hear from Professor Wyman, who has made numerous careful measurements, that the accuracy of the workmanship of the bee has been greatly exaggerated; so much so, that, as he adds, whatever the typical form of the cell may be, it is rarely, if ever, realised." Of course, much of the new matter has been adopted from scientific papers already in print; but it has been carefully digested, and worked up with the original composition in such a form that no one would detect the juncture. The experiments of Sir John Lubbock on Chlöon dimidiatum, reports of which have at various times appeared in THE READER, are commented upon at great length; in fact, they have compelled the whole chapter on "Embryology and Development" to be entirely re-written. "Fritz Müller, who has recently discussed this whole subject with much ability, goes so far as to believe that the progenitor of all insects probably resembled an adult insect, and that the caterpillar or maggot, and cocoon or pupal stages, have subsequently been acquired; but from this view many naturalists—for instance, Sir J. Lubbock, who has likewise recently discussed this subject—would, it is probable, dissent." But Mr. Darwin's ideas as to the probable nature of our own common ancestor, derive fresh strength from the conjectures of Müller on another point. "It is probable, from what we know of the embryos of mammals, birds, fishes, and reptiles, that all the members in these four great classes are the modified descendants of some one ancient progenitor, which was furnished in its adult state with branchiæ, had a swim-bladder, four simple limbs, and a long tail fitted for an aquatic life." A picture, or, at all events, an outline, of this imaginary animal, would be very attractive.

It is to be understood that we have only touched on a few of the additions to the Darwinian arguments. The remarks on the Australian cuckoo, and the various discoveries of Mr. Wallace, we omit with reluctance. Such a work as this will never be complete in the sense of being perfect. No one is better aware of this than Mr. Darwin himself. He has altered none of the passages which describe it as an "Abstract," and we gather that much is only a selection from the rich stores he is accumulating by way of proof and exposition. Let us hope they will themselves be given to the public before long, and by no inferior hand.

ANTHROPOLOGY.

Memoirs Read before the Anthropological Society of London: 1865-6. Volume II. (Trübner and Co.)

THE recent publication of the Second Volume of the Anthropological Society's Memoirs will be hailed with satisfaction by all who had not the opportunity of hearing these papers read and discussed at the ordinary meetings, or at the last Congress of the British Association. We cannot but congratulate the Society on the position which it now deservedly occupies, and from which it had been so long unjustly debarred. But no more of this. The volume before us tasks all our powers. Thirty Papers contain, as may be imagined, a very large amount of information, and for our convenience we will arrange them under the several heads of General, Descriptive, Archaic, and Historical Anthropology—a classification for which we are indebted to the President. We cannot pretend to give in this article anything like an analysis of the Memoirs, and must be content to direct attention to those most worthy of notice. The last in the series, by Dr. Mitchell, on "*Blood Relationship in Marriage*," is decidedly not the least in importance. The author has applied himself to the question in a cautious, candid, philosophical spirit, untrammelled by any bias that would render his facts or his deductions less trustworthy; therefore he leaves us the more open to conviction. His enquiries are confined to Scotland, where his duties as a Deputy-Commissioner of Lunacy have induced him to investigate the influences of consanguinity, more especially in reference to the production of insanity and cerebral disease; but his observations extend to the existence of deaf-mutism, consumption, scrofula, and other morbid conditions, as the possible results of the same pathogenetic cause. Some of the cases appear to be striking proofs of the evil effects of intermarriage between blood-relations; but a wider field of enquiry has convinced him that these were exceptional cases, and that effects equally deplorable may be found to proceed from unions which have no kinship at all about them. Still it would appear that although the direct issue of an allied union may be healthy, the evil effects may show themselves in the third or fourth generation. It must be a nice and difficult point to determine, whether a particular disease be the result of hereditary transmission, or whether it has originated in the union of blood. There can, however, be no doubt that if consanguinity does not of itself produce disease in the offspring, it strengthens and intensifies those morbid proclivities in the parents' constitutions which tend to the production of disease in their children. Therefore the risk should be avoided. The conclusions arrived at by the author he formulates thus :—

1. That consanguinity in parentage tends to injure the offspring. That this injury assumes various forms: "as diminished viability; feeble constitution; bodily defects; impairment of the senses; disturbance of the nervous system; sterility."

2. That the injury may shew itself in the grandchildren: "so that there may be given to the offspring by the kinship of the parents a *potential defect* which may become *actual* in their