

1 DECEMBER, 1888.

philosophical and practical, enthusiastic and energetic, his faculty of seizing a vague idea, and shaping it for mental expression, is inferior and weak by an amount estimated when he had accomplished the success of the electric current. No man better than he has command of the language for a difficult undertaking, and carry it on with a spirit of perfect indifference unswayed either by praise or censure.

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

I am, Sir, yours obediently,

27th November, 1888. W. J. P.

#### To the Editor of THE READER.

Mr.—Reference has been made in *The Reader* and also in the Saturday Review to the highly-meritorious plan of Mr. Ronalds, published in 1865, for a telegraph to work by electrical electricity. Permit me to mention that Mr. Cooke directed to Mr. Ronalds in the concluding paragraph of his pamphlet of 1859, which I enclose—

"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 the few known principles, by means of a few simple electric currents, 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 those which had long been an object of desire. To the merit, such as it may be, of this practical realization, I have maintained, from first to last, one constant claim. Eighteen years ago my unanswered letter of 1865 referred to it as an experiment not achieved. Within three years ago the Atlantic telegraph was opened, or rather it is so not without cause, nor still after long hesitations, that I now expect a final confirmation of the same unrequited claim from the justice of the Public. "There is no magic in terms," as Mr. Whiston says, and it is not worth discussing whether the name of 'original inventor' or 'inventor' is so enormous and unpractical, as Mr. Whiston expresses my right to a position, unobtainable in itself—which Mr. Ronalds, under the foregoing circumstances in which I found myself, might probably have occupied—a gentleman at Bedford, whose recognition, as a gentleman, and whose assistance of his time, has recently been made public.

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

I am, Sir,

Your obedient servant, W.

Nov. 21, 1868.

#### INTERNATIONAL COPYRIGHT.

##### To the Editor of THE READER.

Mr.—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 unjustly accused by the attempt of other persons to interfere with the publication of my works. Indeed, it is at this moment a well-known fact 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 concerned at, or desirous to ignore, the position of the one in this case regarding Mr. Truogues. 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 arrangement. That they can do so is due to their things—fact. That though there is no law to prevent priority from being thereby obtained of the same work, custom (in a very weighty substance for law of the world over), and trade interests combined, make such hence so that, on the average, it will serve itself best by not being professed by that side. The one injured, but so easy and so effective a retaliation, can be made through his power to make republication on any

of his for's publications that he pleases, that it is generally recognized here a valid country in that matter. Always prove this in the long run, not only the safest but the most lucrative form of business. It is now to "A. C. L.'s" change of misrepresenting English literary feeling on the subject of copyright law, I am not aware that I anticipated my requirement if at all. I certainly did not consider that Mr. Truogues's letter stated the question fairly, and I merely went down a few lines as they occurred with regard, to prove that English writers are not invariably misquoted by Americans, as Mr. Truogues asserts. If I am to believe my own sense in reading such a statement and justice from my Transatlantic friends, I shall be fared to believe also that my books are unappreciated good treatment! Of course, I do not accept any such vain construction of it. I believe, on the contrary, that many English authors, if they could not, would corroborate my experience. If I misinterpreted them, because I say that their output against American publishers spring from necessary motives, how can I understand them? It may be very, very possible as the Liberator has to be paid for his loss—of his honor and his time are both—money. If what they desired were the elevation of their works they have that already. But it is not this. They want the power to enforce high payments. What they have every means of making such payments as the Liberator has to be paid for his loss—of his honor and his time are both—money. But I hold that this being their motive they should frankly declare it, and not check their wisdom with their lack—indeed such a high—of their articles about copyright law, international rights, and all the rest of it. That hapless quotation of the Liberator being worthy of his life has been stolen to death on this subject, but I confess, though the saying is perfectly just, I do not see that literature is benefited by its professors as Cambridge for their own sake's, not of service on a Saturday night. "A. C. L." with a common fault of many literary men, probably misquotes what has appeared here, by reading his own opinion. I never stated that an author's "due" is to be paid for his work." A "base robbery of all literary aims and desires to the opinion of A. C. L." I said that several persons had disputed that Hansard's phrasing, "as we feel you are here your y move our own, and it is not at all," suggests a nobler sense in pursuing a literary career than the governing one amongst Englishmen, who like to make their own money as best as they can, and therefore to turn out any popular read, or suitable fabrication of fact, that may be more to this purpose. There is hardly any writer of the present day (save one or two who get the half-hack therefore of themselves almost) who is inclined to pay what they know a truth, if they like their word, by challenge their proprietary gain in the market. This, however, would lead us out to a very wide subject, with which it is not for me to involve your columns. All I will remark is that a state of intellectual and artistic feeling in which poets, painters, and authors have a good job to make how diplomatically they have out worthless productions as those they graciously term "potholes," and how cheerily, and rather as a matter of compliment to their own standards, they will pay their "share" in for anything they do not pay a recognition of value, or at least some possibility of a higher motivating in the pursuit of both literature and art, are hardly to be expected, even if it be considered "black." With thanks,

I beg to remain, Sir,

Faithfully yours,

CHAS.

P.S.—Mr. Truogues, in his Harper's review, always complained that the American publishing was favorable to an English author, because an American publisher, once having any of your works, always considers that he has a sort of vested right in them all. "A. C. L." on the contrary, assumes the fact that an American publisher assumes any right, legal or moral, though you will like to be sure to make his own money. Now there is one way, simple enough, to avoid either of these conceptions. Make your agreement with your English publisher so stringently that he cannot enter into arrangements with a Transatlantic publisher for your book for his own benefit; and take care that you forever to your own "only about" law, though before the appearance

of your London edition for them to appear simultaneously. By this means you will have (in fact) the American publisher send you no table cover for 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 articles, unless brought out also in American periodicals.

#### CHAS.

##### To the Editor of THE READER.

Mr.—I am really very glad to be able to take your time and to be obliged to get 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 Chlores, quotations which he is pleased to characterize as "amazing fictions." I should have passed over such an outlier with a smile, but I cannot allow these really "amazing fictions"—the creation of his own inventive faculties—to be added on me. Dr. Chapman makes me say, "so surface will merge," and that the breath of chlores patients is "usually four or five degrees below that of the atmosphere, at whatever the ordinary temperature." It is impossible to find such daring misstatements in a book which Dr. Chapman professes to have read. At page 8 he will find the following words—"All the nervous centres are equally supplied," the mis-statement is repeated several times, by merely the same words, in different parts of my essay. Such an expression as "so surface will merge," never has been, and never could have been used by me. Dr. Chapman will kindly, then, acknowledge the authority. At page 16, he will also find, "the expired air of chlores patients is usually at least one-third of the amount of that of the atmosphere, but it is four or five degrees below the surrounding temperature." Again, at page 17, "The air expired by chlores patients . . . is at 4° or 5° below the temperature of a warm room," which has a totally different significance from that given by his careless addition of "at all ordinary temperatures."

It is most deplorable 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 he has taken as comment of his letter to the editor of the *Reader*: "One of the doctrines upon which my treatment of chlores 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 abnormal stimulation 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, proximately, by abnormal stimulation from the sympathetic nervous centres"? Such a vague statement 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 chlores patients, although carefully applied by himself to the hospital of Paris, proved a signal failure, leaving no other issue 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, respectfully,

CHARLES BRIDGEMAN, M.D.

Ess of Angles, St. Eloi's 11, Paris.

November 17, 1888.

#### 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. Two, pp. xxi.—282. 1s. (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 conclusions which the progress of science has brought to the theory of Natural Selection may be obtained without trouble by one who is at home in the last edition. Mr. Darwin had delayed the publication a little

larger, he would have credited getting the existence of the Eocene Chamber in an argument in his favour. The labours of Professors King and Huxley are conclusive on the point. However, with his usual candour, Mr. Darwin has not built much on what was till very recently looked upon as an unshaken fact by many distinguished naturalists, in whose company it is no diagram to err. On the contrary, he frankly admits that to some extent it makes the difficulty caused by the absence beneath the Silurian formation of fishes of teeth rich in fossils greater than ever. Hence the Eocene, as Dr. Darwin thought, existed in constant numbers, it must have preyed on other marine 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 regarded as a valid argument against the view here entertained." Pre-Silurian life is, therefore, still a question. Although the recent observations which help Mr. Darwin's theory, those by De Candolle on the variability of the organisms are not the least interesting:—

He first goes 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, and never found connected by intermediate stages. After this discussion, the result of so much labour, he emphatically remarks:—"They are mistakes, who repeat that the greater part of our species are clearly limited, and that the doubtful species are in a little minority. This seemed to be true, so long as a genus was imperceptibly known, and its species were founded upon a few specimens, that in any case were provided, that as we come to know these better, intermediate forms flow in, and doubt as to specific limits ceases." 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, according to us, are discovered under three sub-species, namely, *Q. pedunculata*, *Q. agrifolia*, and *Q. pubescens*. The species which contain these three sub-species are comparatively rare, and, as Jan Gray remarks, if these connecting forms, which are not new, were to become extinct, the three sub-species would hold exactly the same relation to each other, as do the four or five pre-eminently-distinguished species which closely surround the typical Quercus robur. Finally, De Candolle admits that all of the 500 species which will be considered in his Prolegomena as belonging to the oak family, at least two-thirds are provisional species, that is, we do not know entirely to hold the definition above given of a true species.

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

To suppose that the eye, with all its inimitable contrivances for adjusting the focus in 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. Yet it is not as if we said that the eye was made to see, and that the eye, from the common sense of mankind declared the distance, like, in the old saying of our people, *non sibi, sed utrum philosophus loqueretur*, cannot be treated in science. Reason tells us, that if numerous productions 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, even at slightly, and the variations be inherited, which is 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. Now a nerve comes to be sensitive to light, hardly concerns us more than how life itself has originated; but I

may remark that, as some of the lowest organisms, in which nerves cannot be located, are known to be sensitive to light, it does not seem impossible that certain elements in their tissues or secretions should have become aggregated and developed into nerves endowed with special sensibility to its action.

Hence, 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 hardly been pronounced as yet safe to accept, or to treat as an instance of that which I think theory.—I may first remark that the idea of the beauty of any particular object obviously depends on the mind of man, inasmuch as any real quality in the selected object; and that the idea is not an innate and unalterable element in the mind. We see this in cases of different men admiring an entirely different standard of beauty in women; neither the Negro nor the Chinese admires the Caucasian form.—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 might be to be shown that there was less beauty on the face of the earth before man appeared than there has been on the stage. Were the beautiful voice and comely shape of the Eocene epoch, and the graciously-constructed antennae of the Secondary period, created that man might gaze afterwards admire them in his cabinet? Few objects are more beautiful than the minute structures of the diatomaceae; 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 rendered more strikingly beautiful by means of their fertilization might be favoured. I have come to this conclusion from finding it an inevitable rule that when a flower is fertilized by the wind it never has a pale-colored corolla. Again, several plants habitually produce two kinds of flowers, one that opened and coloured so as to attract insects, the other closed and not coloured, destitute of corolla, and never visited by insects, so that 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 fern, oak, 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 marvels of every age, is rarely assailed at page 371:—"I hear from Professor Wynman, 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 obtained from scientific papers already in print; but it has been carefully digested, and worked up with the original composition in such a form, that an error would be almost impossible. The experiments of Sir John Lubbock on Children's diatomites, 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 resided in an adult insect, and that the caterpillar or maggot, and cocoon or pupal stages, have subsequently been acquired; but from the 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 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 branchia, 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. "I have altered more of the passages which I describe in an 'Abstract,'" and we gather that man is not a subjective from the rich store he accumulating by way of proof and exposition. Let us hope they will themselves be given to the public before long, and by a better hand.

## ANTHROPOLOGY.

Memoirs Read before the Anthropological Society of London, 1882-3. Volume II. (Trotter and Co.)

THE recent publication of the Second Volume II. of the Anthropological Society's Memoirs will be hailed with satisfaction by all who had not the opportunity of leaving 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 occupies, and from which it has had no long unjustly departed. But no more of this. The volume before us takes 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, Archæological, and Historical Anthropology—a classification for which we are indebted to the President. We cannot proceed 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 "Race and Relationship in Man," is doubtless 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 to leave us the more open to conviction. His conclusions are confined to England, 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 delirium, consumption, profligacy, and other morbid conditions, as the possible results of the same pathogenic 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 there 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 predispositions in the parent constitutions which tend to the production of disease in their children. Therefore the risk should be avoided. The conclusions arrived at by the author be forewarned thus:—

1. That consanguinity in marriage tends to injure the offspring. That this injury amounts without forms "to diminished vitality; feeble constitution; bodily defects; impairment of the senses; disturbance of the nervous system; wasting."

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