

THURSDAY, JUNE 13, 1901.

## HUXLEY.

*Life and Letters of Thomas Henry Huxley, F.R.S.* By Leonard Huxley. Vol. I., pp. viii + 503; Vol. II., pp. vi + 504. (London: Macmillan and Co., Ltd., 1900.) 30s. net.

THE real life of Huxley has still to be written. What is wanted is a critical study of the development of his striking personality and an estimate of the work of his life and the effect it has produced. I have nothing but praise for the two bulky volumes of the "Life and Letters," in which a filial duty has been accomplished with taste and judgment. But though they supply invaluable material they do not attempt to bring the facts of either career or performance to a clear focus.

Such a study in competent hands would be a fascinating undertaking. It would not merely give a picture of a very remarkable man, but would give also a chapter in the history of English science of supreme importance. I make no pretension to ability for the task myself, even if the columns of this Journal could afford the space. But I shall hazard the attempt to indicate the essential points which I should like to see more amply treated. I have gathered the material from a pretty close study of the "Life and Letters," and I have added the references of volume and page to quotations, which are not always easy to find, for any one who cares to verify them.

Nothing in tracing an eventful career is so attractive as speculation on the "might-have-been." It is probable, however, that within narrow limits "circumstance" counts for little beyond giving a dramatic touch to the story. But it played its part again and again in Huxley's life for what it was worth.

His family traces back to the north-west of England, where a certain fibrousness of character is commoner than in the south. His father was a master in Dr. Nicholson's school at Ealing, where Huxley was born in 1825. He describes himself as "a thread-paper of a boy" (ii. 35) with "a wild-cat element in me" (i. 5). For education in the ordinary sense:—

"I had two years of a pandemonium of a school (between 8 and 10), and after that neither help nor sympathy in any intellectual direction till I reached manhood" (ii. 145).

The school came to grief and Huxley's father moved to Coventry. Huxley was left to his own devices. What they were is almost incredible; but then he has told us that "a priori reasonings are mostly bosh" (ii. 212). At twelve he was sitting up in bed before dawn to read Hutton's "Geology" (i. 6). His great desire was to be a mechanical engineer; it ended in his devotion to "the mechanical engineering of living machines" (i. 7). His curiosity in this direction was nearly fatal; a *post mortem* he was taken to between thirteen and fourteen was followed by an illness which seems to have been the starting point of the ill-health which pursued him all through life. At fifteen he devoured Sir William Hamilton's "Logic." Twenty years later he says:—"From that time to this ontological speculation has been a folly with me" (i. 218).

At seventeen he came under the influence of Carlyle. Nearly fifty years later he wrote:—

"There is nothing of permanent value (putting aside a few human affections), nothing that satisfies quiet reflection—except the sense of having worked according to one's capacity and light, to make things clear and get rid of cant and shams of all sorts. This was the lesson I learnt from Carlyle's books when I was a boy, and it has stuck by me all my life" (ii. 268).

At the same age he began his regular medical studies at Charing Cross Hospital with his brother, to whom Newman (afterwards Cardinal), who had been educated at the Ealing school (i. 19), gave a testimonial. He attended Lindley's lectures at the Chelsea Botanic Garden and won one of the medals of the Apothecaries Society. At the Medical School he studied under Wharton Jones, a physiologist who never seems to have attained the reputation he deserved. Perhaps he got mixed up with "the other fellow," who, Huxley thought, had "mistaken his vocation" (i. 94), an opinion in which, from personal experience, I can quite agree. Of Wharton Jones, Huxley says:—

"I do not know that I ever felt so much respect for a teacher before or since" (i. 21).

At twenty he went up for his First M.B. examination at the University of London, winning the gold medal for anatomy and physiology. Ransom, of Nottingham, won the Exhibition. Here circumstance came in.

"If Ransom had not overworked himself . . . I should have obtained the Exhibition . . . and should have forsaken science for practice" (ii. 133).

Would he?

Something had to be done to get a livelihood, and at the suggestion of a fellow student, now Sir Joseph Fayrer, he applied for an appointment in the Navy. Circumstance again, he came under Sir John Richardson, himself no mean naturalist, and through his influence was attached to the *Rattlesnake*. One of the oddest things about Huxley's career is the fact that almost every one he had to do with turned out sooner or later to be somebody notable. Through his Captain, Owen Stanley, "a thorough scientific enthusiast" (i. 25), he was introduced to Owen, Gray and Forbes, the first and last of whom had a good deal to say to his future career. The voyage of the *Rattlesnake* occupied four years. Huxley was twenty-five on his return. Few scientific men ever used their opportunities with keener sagacity. He spent no time in mere collecting. But, with an instinct which appears to me altogether extraordinary in one who was little more than a youth fresh from a medical school, he seized upon everything that was important and with regard to which new ground was to be broken; and, characteristically, he steadily kept their physiological interest to the front. The rest may be passed over rapidly; he had, in a scientific sense, his reward. His paper on the structure of the Medusæ had been published during his absence in the *Philosophical Transactions*. In this paper he laid down the fundamental character of the "ectoderm and endoderm." As Allman justly remarks, "this discovery stands at the very basis of a philosophic zoology" (i. 40). It is not too much to say that it is the foundation of modern zoological theory, and had Huxley never done anything

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else he would still have retained a classical place in its history.

At twenty-six he was elected a Fellow of the Royal Society. At twenty-seven he not merely received the Royal medal, but was placed on the Council. Certainly, half a century ago, our venerable Society showed no want of alacrity in recognising rising merit. And if any one wants to suggest that it has become less active in that respect he may be reminded that it was equally prompt in the case of Hertz.

It is certainly a notable circumstance that three men who were contemporaries and ultimately close friends, Darwin, Hooker and Huxley, each began his scientific career on board one of Her Majesty's ships. The consequence in each case was momentous to science: Darwin gave us the "Origin," Hooker a rational theory of geographical distribution, Huxley a reformed zoology. The odd thing is that while the two former returned confirmed naturalists the latter came back as impenitent as ever, and never was of a better mind till quite the end of his life.

At the age, then, of twenty-seven Huxley had placed himself with absolutely no aid in the very front rank of English scientific men. "What makes," he says, "the bigwigs so marvellously zealous on my behalf I know not. I have sought none of them and flattered none of them" (i. 78). Yet he did not emerge from the struggle altogether unscathed. Writing to Kingsley some eight years later he for once allows the cry of the wounded heart to escape him:—

"Kicked into the world, a boy without guide or training, or with worse than none, I confess to my shame that few men have drank deeper of all kinds of sin than I have" (i. 220).

Frankly, I do not believe a word of it. My experience of life does not lead me to think that any one who begins with a rank crop of weeds is very likely to ever reap a more substantial harvest. The plain fact is that the mood of confession is a perilous one, and from St. Augustine onwards most men who have yielded to it have found a sort of painful satisfaction in painting their past in the blackest colours. But I am entirely unable to find any point in Huxley's youth at which I can fix that outburst of the natural man. In a rather serious conversation I once had with him he spoke of a period in his life when he *might* have taken to evil courses; but he did not give me the smallest reason to suppose that in the retrospect he saw more than the existence of a possible *crevasse* in his path into which he might have fallen.

If Huxley's scientific reputation was established, his material position was still unassured. "Nothing," he says, "but what is absolutely practical will go down in England. A man of science may earn great distinction, but not bread" (i. 66). The struggle, however, in his case, if sharp, was less prolonged than it has been in the case of many other men. Owen got him his first temporary appointment (i. 95). Edward Forbes, "a regular brick"—an opinion I never heard any one gain-say—backed him "through thick and thin" (i. 107). He refrained, therefore, happily, from abandoning "all his special pursuits and take up chemistry, for prac-

tical purposes" (i. 86). He had tried to get "crystalised carbon" at fifteen (i. 10)!

Huxley was now thirty, and at last happily married. He might have succeeded Forbes in the Edinburgh chair, but "preferred to live in London on a bare sufficiency" (i. 120). He settled down at the School of Mines; his ship had come into port; what was the cargo he brought with him? As a boy he conceived a profound distrust of metaphysical speculation; at fifteen he writes in his note-book, hammered out from "Novalis," "Philosophy bakes no bread" (i. 9); that he stuck to to the end. From Carlyle he learnt his empiricism, a determination to see things as they are. From Wharton Jones he acquired an exact method, and from the *Rattlesnake* voyage confidence in his own powers of observation and courage to criticise the word of others. And here I must interpose the remark that it is difficult to estimate the services which biological science in this country owes to our medical schools. Up to the present time without them it would possibly not have existed amongst us at all. Huxley later on was more willing than I am to kick away the ladder:—

"Our side has been too apt to look upon medical schools as feeders for science. They have been so, but to their detriment as medical schools. And now that so many opportunities for purely scientific training are afforded, there is no reason that they should remain so" (ii. 310).

For my own part, owing much to medical training, I entirely dissent. The foundation of medical studies on a scientific basis, far from being detrimental, has in my opinion been of incalculable benefit to them. If Huxley really contemplated a division between medicine and science it was the worst cause he ever advocated.

Huxley's official duties, much against the grain, brought him face to face with palæontological problems. This not merely led to some of his most brilliant work, but put a weapon in his hand which he used afterwards with irresistible effect.

Half a century ago Owen was the dominant, and I think it must be admitted an evil, influence in the English biological world. He was saturated with the "naturphilosophie" and the teaching of Oken. Huxley was bound to come into collision with this. The Croonian Lecture in 1858, "On the theory of the vertebrate skull," demolished Oken's theory, and with it "fell the superstructure raised by its chief supporter, Owen, 'archetype' and all" (i. 141). Owen had already felt that his throne was tottering and, having borrowed the lecture-room in Jermyn Street for a course of lectures, boldly assumed, without the smallest warrant, the title of "Professor of Palæontology at the School of Mines" (i. 142). For this and many subsequent proceedings of a like nature the only plausible explanation that I can see is lunacy.

Here again Huxley laid one of the foundation stones of modern biological science. In his paper on the *Medusæ* he supplied the key which has unlocked the secrets of embryology; his Croonian lecture, followed by the work of Gegenbaur, has placed vertebrate morphology on a scientific basis.

This was his first conflict with scientific idealism, but

it was a mere affair of outposts compared with the campaign that was to follow. He tells us :—

"I was not brought into serious contact with the 'species' question until after 1850. At that time I had long done with the Pentateuchal cosmogony . . . from which it had cost me many a struggle to get free" (i. 167).

Later on he calls "the hypothesis of special creation . . . a mere specious mark for our ignorance" (ii. 302). What was to be put in its place? Herbert Spencer, whose acquaintance he made in 1852, was unable to convert him to evolution (i. 168). He could not bring himself to acceptance of the theory—owing, no doubt, to his rooted dislike to *a priori* reasoning—without a mechanical conception of its mode of operation. Like Darwin, he derived no comfort from either Lamarck or the "Vestiges" (i. 168). For the former, nevertheless, he always entertained the most profound respect, and thought he would run Darwin "hard both in genius and fertility" (ii. 39). His review of the latter was the only one he ever had "qualms of conscience about on the grounds of needless savagery" (i. 168).

His attitude to evolution continued to remain altogether sceptical and stand-off. In his first interview with Darwin, which seems to have been about 1852, he expressed his belief "in the sharpness of the lines of demarcation between natural groups," and was received with a "humorous smile" (i. 169). Hooker, on the other hand, he thought "*capable de tout* in the way of advocating evolution" (i. 170); but then Hooker was in the secret.

Before continuing the story I think it will be helpful to state in simple terms the problem that Darwin attempted to solve, and to which he got his first clue in the Galapagos. Take a number of organisms at random and proceed to sort them according to their resemblances. When this has been done it will be found that they have fallen into groups larger or smaller, as the case may be. The members of the groups will closely agree in all essential particulars; they are *individuals*. Yet no two are exactly alike; this is *variation*. Yet within the group there will be nothing to oppose the view that each may pass into the other; the variation is *continuous*. This will not be the case in comparing groups themselves; the variation is more marked and *discontinuous*. The discontinuity can be expressed in technical terms, and these give us an abstract definition of the *species* or the distinctive marks common to the individuals forming the group. Treating species in the same way we arrive at a series of discontinuous groups of a higher order; these are *genera*. Continuing the process we obtain *families*. Proceeding onwards in the scale we find ourselves face to face with two, perhaps the most difficult of all to define—the Vegetable and Animal kingdoms.

Now Darwin, of course, saw with every one else that if the mode of origin of groups of the first order could be explained, all the rest followed. What was wanted was the discovery of some intelligible agency which could effect the passage of one organic form to another. As Huxley put it :—

"That which we were looking for, and could not find, was a hypothesis respecting the origin of known organic forms which assumed the operation of no causes but such as could be proved to be actually at work" (i. 170).

Darwin assumed continuous variation as an empirical fact and "natural selection" as the agency which had directed the course of organic evolution. This was a generalised form of the "artificial selection" which the cultivator and the breeder use every day in moulding organic nature pretty much as they will. As Huxley says :—

"My reflection when I first made myself master of the central idea of the 'Origin' was, 'How extremely stupid not to have thought of that'" (i. 170).

Huxley's attitude to Darwinism deserves careful study. Some have thought that in his last public appearance at Oxford in 1894 he hinted his willingness to make a present of Darwin's theory to Lord Salisbury, as organic evolution could be established without it. And no doubt that is a view which can be maintained. Lord Salisbury had ridiculed the idea of the advantageous male in pursuit of the advantageous mate. This only showed that he could have studied Darwin to very little purpose. I am not one of those who think that the discontinuous "sport," advantageous or not, has played much part in evolution. But in any case its appropriate pairing is not essential, as it is now known that sports are frequently prepotent and their influence not easily swamped. The unmatched advantageous male is not so easily dismissed as Lord Salisbury seemed to think.

Huxley found in Darwin what he had failed to find in Lamarck, an intelligible hypothesis good enough as a working basis. But with the transparent candour which was characteristic of him he never to the end of his life concealed the fact that he thought it wanting in rigorous proof.

Now Darwin was a naturalist, and the "Origin" is emphatically the production of a naturalist. Huxley has repeatedly told us, what is perfectly true, that he was not one himself. "His love of nature had never run to collecting either plants or animals" (ii. 443). For him as for others Lyell "was the chief agent in smoothing the road to Darwin" (i. 168), for evolution is implied in uniformitarianism. Huxley was an anatomist, and the distinctions of the higher groups with which he chiefly occupied himself are anatomical. The discontinuity of those groups no longer troubled him now that he knew what lay behind Darwin's "humorous smile." But with "species" or primary groups he still found difficulties which I think he would not have found if he had had a naturalist's experience. At Edinburgh :—

"In common fairness he warned his audience of the one missing link in the chain of evidence—the fact that selective breeding has not yet produced species sterile to one another" (i. 193).

He states the point more precisely in a letter to Kingsley :—

"He (Darwin) has shown that selective breeding is a *vera causa* for morphological species; he has not yet shown that it is a *vera causa* for physiological species" (i. 239).

Now it seems to me that, to use one of his own favourite expressions, this is a shadow of the mind's own throwing. The species which Darwin undertook to account for are morphological. No other category conveys any meaning. There is a physiological difference between the sweet and bitter almond, because one is harmless and



the other will kill; but it is unaccompanied by the smallest morphological distinction. Nägeli pointed out the importance of recognising this in bacteriology. What Huxley really meant by physiological species are species which are mutually sterile, and in this both he and Romanes seem to me to have rather begged the question.

Darwin, who was more aware of the weak points of his theory than any of his critics, took immense pains to show that sterility does not run parallel with taxonomic order. It is well known that it is *not* a criterion of species, as Huxley seemed to think—it does not seem to be even a criterion of genera. I can only suppose that some hint of Huxley's furnished the foundation of Romanes's heroic attempt to establish "physiological selection." If so, Huxley seems to have been little impressed with the result:—

"It (the 'Origin') is one of the hardest books to understand thoroughly that I know of, and I suppose that is the reason that even people like Romanes get so hopelessly wrong" (ii. 192).

But then Romanes was not a naturalist either.

Another difficulty was the principle that "*Natura non facit saltum*" (i. 176), and I think from the same cause. Bateson, of course, receives a benediction:—

"I always took the same view, much to Mr. Darwin's disgust" (ii. 372).

That "considerable 'saltus'" may occur is not improbable; but there can be little doubt that a species passes from one configuration to another, as Darwin supposed, by minute changes; and, as he has himself pointed out, we are not justified in assuming that the rate of variation has always been uniform.

Huxley, however, felt that he had at last a secure grip of evolution, and was soon on the war path; he warns Darwin:—

"I will stop at no point as long as clear reasoning will carry me further" (i. 172).

Nor did he. The history of "the great 'Sammy' fight" has often been told. It is interesting to know that it was Chambers, the author of the "Vestiges," who was responsible for it (i. 188). Its importance has been somewhat exaggerated. Evolution has made its way by a process of slow permeation. It has done so because, in the words of Helmholtz, it contains "an essentially new creative thought" (i. 364). But it was a brilliant dialectic victory for Huxley, and Oxford loves dialectic: "The black coats . . . offered their congratulations" (i. 189). "The Bishop . . . bore no malice, but was always courtesy itself" (i. 188). Huxley was, however, less forgiving, and put him in his pet little Inferno (ii. 341). Personally I entertain more than a sneaking admiration for him. He "cleaned up" the diocese of Oxford with a vigour worthy of Huxley himself.

One incident in the discussion is of some theoretical interest. The permanence or, as I prefer to say, stability of species seem to have been adduced as an argument against Darwin's theory. Lord Avebury:—

"instanced some wheat which was said to have come off an Egyptian mummy, and was sent to him to prove that wheat had not changed since the time of the Pharaohs,

but which proved to be made of French chocolate" (i. 187).

But we have absolute evidence from tombs that Egyptian plants have not appreciably changed for 4000 years. And it is now known that the fact, instead of being an argument against, is rather one for the Darwinian theory.

Owen made a last desperate attempt to save the situation by asserting for man, on anatomical grounds, a completely isolated position in the animal kingdom. Huxley, in 1862, "showed that the differences between man and the higher apes were no greater than those between the higher and the lower apes" (i. 192). The case for the evolution theory was now complete.

Carlyle did not forgive the publication of "Man's Place in Nature," though it only carried the veracity of "Sartor Resartus" a step further. However, master and disciple both received together an honorary degree at Edinburgh, and I think there must have actually been some sort of reconciliation. For I have a distinct remembrance of hearing, I think from Huxley himself, that Carlyle expressed to him unbounded admiration for "Administrative Nihilism," coupling it with a by no means flattering estimate of another eminent philosopher.

Here I must leave Huxley's scientific work. He was now only thirty-seven. He found zoology in this country enchaind in fantastic metaphysical conceptions; he extricated it almost single-handed. Writing to Leuckart in 1859 he says:

"Ten years ago I do not believe there were half-a-dozen of my countrymen who had the slightest comprehension of morphology. . . . I have done my best, both by precept and practice, to inaugurate better methods. . . . I confidently hope that a new epoch for zoology is dawning amongst us" (i. 163).

The hope has been amply realised. And if a quickening spirit has been breathed into every branch of biological teaching in this country, it was Huxley it came from. It is much to be wished that some one would record some recollections of the memorable courses of instruction at South Kensington which Huxley commenced in 1871, in which teachers and taught were alike inspired by an enthusiasm the tension of which almost reached breaking point, and in Huxley's own case, in fact, speedily did so.

Notwithstanding ill health his mental activity, constantly stimulated by a certain innate combativeness, kept him to the end immersed in public work of the most varied description and in the controversy that he loved. "Under the circumstances of the time," he says, "warfare has been my business and duty" (ii. 213). All this it is needless for me to touch upon. But no picture of Huxley would be complete which left out of sight the speculations which more and more absorbed him as his life drew to a close. In this Journal these can be only treated from a purely scientific point of view.

It is necessary to remember that Huxley's grasp of the principle of organic evolution was only arrived at by the process of reasoned and by no means hasty conviction. He satisfied himself that man could not be excluded from it. He was naturally therefore drawn to discuss human phenomena in relation to evolution.

The first was the problem of ethics. He summed up his conclusions in the Romanes lecture delivered at Oxford in 1893. This was his second speech delivered there; the first was in the "great Sammy fight," thirty-three years before. He might well say that "Oxford always represents English opinion in all its extremes" (ii. 441). He nearly succeeded in producing as much hubbub as on the first occasion. It is amusing, if not very edifying, to read the anxious preliminary negotiations. Huxley wrote, "Of course I will keep clear of theology" (ii. 350), and Romanes naturally writes back "in great alarm" (ii. 354). The pith of the whole thing was, "the cosmic order is not a moral order."

Morals are part of the cosmic order, but not identical with it. Seriously regarded, this is a very simple statement of pure fact, which is indeed the basis of one of Dr. Watts's most familiar "Sacred Songs," the orthodoxy of which no one has ever impeached. The order of nature is self-regarding, and, as that familiar writer implies, society "would be dissolved by a return to the state of simple warfare among individuals" (ii. 352). The contrary view, embodied in the phrase "ethics of evolution," Huxley traces to the ambiguity of the word "fittest." That "which survives in the struggle for existence may be, and often is, the ethically worst" (ii. 303).

"The actions we call sinful are part and parcel of the struggle for existence . . . and have become sins because man alone seeks a higher life in voluntary association" (ii. 282).

So far this is a utilitarian theory of morals, and, as far as it goes, accounts for the phenomena. But, as Huxley saw, it leaves unexplained the fact that probably every ethical system aims at a higher standard than is ordinarily reached or is perhaps even necessary in practice. This apparently he would explain by "an innate sense of moral beauty and ugliness (how originated need not be discussed)" (ii. 305). I confess I am sorry for that parenthesis. But the principle itself is comparable to Matthew Arnold's "Something not ourselves which makes for righteousness." At any rate, short work is to be made of those who do not possess it.

"Some are moral cripples and idiots, and can be kept straight not even by punishment. For these people there is nothing but shutting up or extirpation" (ii. 306).

I hope it is not irreverent to say that "Injuns is pisin" seems to be a natural corollary. Huxley meant to look up Nietzsche (ii. 360), but probably never did. Had he done so the result would have been edifying.

A critical study of Huxley's theological views, especially in the light afforded by the "Life and Letters," would be extremely interesting. This is not the place to attempt anything of the sort. But some brief account is necessary. The starting point is to be found in a letter to Kingsley:—

"Sartor Resartus' led me to know that a deep sense of religion was compatible with the entire absence of theology" (i. 220).

Now this suggests two remarks which are both justified, I think, by my own personal knowledge. In the first place I am firmly persuaded that he, if any one, was a deeply religious man. I am equally persuaded that he had a perfect passion for technical theology. He often

thought himself, at least so he told me, that he might have been a successful lawyer. I do not doubt it. But the cerebral equipment which might have found employment in that direction got turned on to theology. This, I think, throws light on his shortcomings in this field. Dogma may be treated, and I think should be, in a scientific spirit; Huxley too often indicted it as if he were in a police court. There is no doubt that he adopted this attitude deliberately.

"My object has been to stir up my countrymen to think about these things; and the only use of controversy is that it appeals to their love of fighting and secures their attention" (ii. 291).

"I must," he says, "have a strong vein of Puritan blood in me somewhere" (ii. 91), and I think it cannot be doubted that he was right. His point of view was that of an extreme nonconformist. I need not say that this implies no disrespect, for nonconformity has been one of the roots of the English character.

In one aspect the religious sentiment is a response to the craving for a supernatural sanction to rules of conduct. Its varied but practically universal manifestation amongst mankind has got to be accounted for by evolution just as much as the possession of a vertebral column. It is not practically helpful to dismiss it as irrational.

Huxley, like others of a Puritan temperament, had more liking for the Old Testament than the New: "the only religion that appeals to me is prophetic Judaism" (ii. 339). But Calvinism, I think, contained much with which he most nearly sympathised. "Science," he wrote to Kingsley, "seems to me to teach, in the highest and strongest manner, the great truth which is embodied in the Christian conception of entire surrender to the will of God" (i. 219). "I have the firmest belief," he continues, "that the Divine Government . . . is wholly just." There is a very interesting passage, too long to quote (ii. 303), in which he points out that "the best theological teachers . . . substantially recognise these realities of things, however strange the forms in which they clothe their conceptions." For my own part, I wish he had applied the principle which is implied here in some of his controversial essays. Writings thousands of years old would have been unintelligible if they had not been expressed not merely in the language but in terms of the ideas current at the time. The demonology of the first century was scarcely worth the powder and shot bestowed upon it. If it had cost Huxley himself "many a struggle to get free" from the Pentateuchal cosmogony (i. 167), he lived to see Canon Driver give up its "physical truth . . . altogether" (ii. 218); the process of attrition of what is superfluous will go on.

Huxley, however, in his episcopophagous mood was a grievous disappointment to extremists when it came to practical business. It is difficult, I think, to exaggerate the importance of the work he did on the London School Board and at a terrible cost to his health. He expressed "his belief that true education was impossible without 'religion,' of which he declared that all that is unchangeable in it is constituted by the love of some ethical ideal to govern and guide conduct" (ii. 340), and he unhesitatingly adopted the words of Mr. Forster in 1870:—

"I have the fullest confidence that in the reading and

explaining of the Bible what the children will be taught will be the great truths of Christian life and conduct, which all of us desire they should know" (ii. 344).

He fought, therefore, "for the retention of the Bible, to the great scandal of some of my Liberal friends," and "never had the slightest sympathy with those who, as the Germans say, would 'throw the child away along with the bath'" (ii. 9).

Years after he remained of the same mind:—

"I do believe that the human race is not yet, possibly may never be, in a position to dispense with it" (ii. 300).

Ethical and religious problems occupied so large a place towards the end of Huxley's life that it was impossible to leave them out of sight. But a sharp distinction is, I think, to be drawn between what he accomplished in this field and what he did for knowledge. The latter was eminently constructive: he reconstituted biological science in this country from the foundations upwards. The former was only critical and, as he did not deny, mainly negative. His defence was that his part had been to clear "the ground for the builders to come after him" (ii. 301). Meanwhile he had nothing but respect for those who honestly held opposite views. But he would have nothing to do with the "half-and-half school," with whom he had less sympathy than "with thorough-going orthodoxy" (i. 471). For Magee, Bishop of Peterborough, he had "a great liking and respect" (ii. 244). I wish I felt at liberty to amplify what is said (ii. 205) as to the admiration he conceived for Father Steffens.

Looking back on the whole story as I have attempted to tell it, I am struck with the character of inevitableness about Huxley's career. I do not call to mind any other in which a controlling purpose so definitely manifests itself. "My sole motive," he said in 1891, "is to get at the truth in all things. I do not care one straw about fame, present or posthumous" (ii. 281), and certainly, so far as it is given to any one to be successful, he obtained a large measure of success.

Much has been said of the odium and obloquy he encountered in the process. He was certainly supremely indifferent to both, and probably rather enjoyed them. But Englishmen will concede anything to honesty, and Huxley was transparently honest. And obloquy is perhaps not intolerable which is accompanied by the repeated offer of a professorship at Oxford, followed by that of the headship of a college, by the presidency of the Royal Society, and by admission to the Privy Council.

But it was not merely as a man of science and of affairs that Huxley achieved success. He was possessed of an extraordinary literary gift. "I have," he writes, "a great love and respect for my native tongue, and take great pains to use it properly" (ii. 291). It is much to be wished that scientific men generally would follow his example. He could always, says Sir Spencer Walpole, "put his finger on a wrong word, and he always instinctively chose the right one" (ii. 25). But this, like everything else that he ever did, was not accomplished without labour. It was from the literature of the eighteenth century that

young Englishmen "would learn to know good English when they see or hear it" (ii. 285). In his own case it helped to make him, as Mr. Arthur Balfour said, a great master of English prose; perhaps even, as Sir Spencer Walpole thinks, "the greatest master of prose of his time" (ii. 25).

Nor less sedulously did he cultivate the art of oral exposition and of public speaking, or with less success. Lord Salisbury exclaimed, "What a beautiful speaker he is" (ii. 25). Apart from eloquence as it is ordinarily understood, or rhetorical effect, I myself have never heard any one who in method or manner could compare with him. It is quite consistent with this that he should say, "I funk horribly, though I never get the least credit for it" (i. 311). Before one of his greatest performances he asked me to take his hand: it was stone-cold. "It is always like that," he said. Yet he held an enormous audience enchained while he unfolded, using no notes, but with faultless choice of words, an intricate and technical argument.

Nor was he less captivating in conversation. He rises to my mind's eye, drawing down his mouth when he was serious, as if to give momentum to the propulsion of the thought. In a moment, as some humorous aspect of the matter struck him, it would relax into a smile, and then, if one tried too audaciously to attack his arguments, his head would go back with a leonine sweep, as much as to say, "young man, be careful." But it was what Mr. Skelton admirably calls "the Shakespearian gaiety of touch" (ii. 16) that made converse with him so unforgettable. Darwin had something of it, but attuned to a gentler key. With Huxley it was irrepressible. "I suppose," he says, "I shall chaff some one on my death-bed" (ii. 76).

But, in truth, through these two volumes there runs a tragi-comedy, often moving to mirth and not seldom to tears, and sometimes almost Meredithian in intensity. The demon of dyspepsia broods over the drama, as it unfolds, like fate. The wonder is that a man who fought such a life-long battle with ill-health could oppose such a courageous and uncomplaining front to the outside world. He carried the fox gnawing at his vitals with a Spartan fortitude.

And to ill-health there was added, for no small portion of his life, the no less uncomplaining struggle with poverty. To keep his brother's widow he was even compelled to part with his Royal medal (i. 248). When he retired from the public service it was the desire of the Education Department that he should do so on a full pension. This the Treasury were unable to grant. But it is to be counted to the credit of a Tory Government that the amount was eventually made up from the Civil List.

A few words and I have done. In these volumes the reader has the privilege of being brought into as frank an intimacy with Huxley as was enjoyed by even his closest friends. I am wholly mistaken if there does not emerge from their perusal a personality of singular fascination behind which lay an intellectual and moral force, second perhaps to none in its influence on his countrymen during the latter half of the century which has closed.

As Lord Hobhouse has said, "he fought the battle of intellectual freedom" (ii. 407), and his success was due



to the integrity of purpose and dauntless courage which never failed him. Sir Spencer Walpole says justly,

"Of all the men I have ever known, his ideas and his standard were—on the whole—the highest" (ii. 27).

He proceeds—

"He recognised the fact that his religious views imposed on him the duty of living the most upright of lives."

A very unfair use has, I think, been made of this opinion, which I am persuaded is based on a profound misconception. However derived, it is in an innate sense of moral beauty that I prefer to find the true secret of Huxley's life. W. T. THISELTON-DYER.

#### TERRESTRIAL MAGNETISM AND ATMOSPHERIC ELECTRICITY.

*The Norwegian North Polar Expedition, 1893-96. Scientific Results.* Edited by Fridtjof Nansen. Vol. ii. (London: Longmans, Green and Co., 1901.)

*Report on Observations in Terrestrial Magnetism and Atmospheric Electricity made at the Central Meteorological Observatory of Japan for the Year 1897.* Pp. 60. (Tokio: Central Meteorological Observatory.)

THE first of the above volumes consists of three memoirs, numbered VI., VII. and VIII., written respectively by Prof. H. Geelmuyden, Mr. Aksel S. Steen and Prof. O. E. Schjøtz. In a brief preface Dr. Nansen states that the great majority of the observations dealt with were made by Captain Sigurd Scott-Hansen.

VI. *Astronomical Observations.*—In a preface, pp. vii. to lx., Prof. Geelmuyden describes the astronomical instruments and the circumstances of their use. His principal object is to determine the drift of the *Fram* and the track of Nansen and Johansen after leaving the ship. The results are embodied in two large scale charts (in a pocket at the end of the volume). A second object is to determine the azimuth in connection with the observations of magnetic declination.

The latitude and local time were found by altitude observations, the sun alone being available during part of the year. For the determination of longitude, and of the chronometer rates, a variety of data were accumulated. There were observations during two eclipses, a few lunar distances and a number of observations of eclipses of Jupiter's satellites. In connection with these last data there is an enumeration of corresponding observations at various observatories, and a discussion of the theory and of various sources of uncertainty. The differences between the chronometers in use from 1893 to 1896 are recorded and discussed. The difficulties met with in reducing the astronomical observations are considerable. Most referred to a station in motion, while many were taken at extremely low temperatures, under conditions when ordinary astronomical formulæ for refraction, &c., are open to question. The differences between the chronometers are not always easy to explain, and the data as to their temperature corrections are somewhat uncertain. As to the data obtained by Nansen and Johansen in their journey, in Prof. Geelmuyden's words,

"the observations during this expedition, where the  
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principal work of the travellers was very often a struggle for life, and where the instruments had to be handled in temperatures down to  $-40^{\circ}$  C., with no other source of heat than the observer's own body, could not attain any high degree of accuracy" (p. lvii.).

The fact that the observations were made at all is the strongest possible evidence that scientific zeal is compatible with the possession of remarkable physical courage and resolution.

After Geelmuyden's preface follow tables, pp. 1-136, giving full details of all the astronomical observations, with a few explanatory notes.

VII. *Terrestrial Magnetism.*—In his introduction, pp. 1-9, Steen describes the instruments. Acknowledgment is made of the assistance rendered by Dr. Neumayer, of Hamburg, who selected the apparatus and had some of it made under his own eye. The great majority of the observations were taken on the ice, inside a tent or a house of snow or ice. "As a defence against bears . . . a weapon was always at hand, generally a revolver." The position of this useful but embarrassing auxiliary and its influence, or absence of influence, on the magnets is a frequent item in the observational records. The different magnetic elements are discussed separately. The declination observations occupy pp. 10-61. The majority were taken with a "Neumayer Declinatorium," of which the principal feature is that its magnet consists of "two laminæ, between which the mirror was fixed"; the magnet rested on a pivot, but could be inverted so as to determine or eliminate the collimation error. Declination results are also deduced from the deflection experiments, intended primarily for the determination of the horizontal force. There were in all about 130 days on which declinations were obtained. The changes observed during each of these days are shown graphically, occupying seventeen plates. The observations seldom extended over more than two or three hours on any one day, and in no case was there a continuous day's record. On November 24, 1894, in the course of fifteen minutes, the declination changed fully  $26^{\circ}$ . On no other occasion did the observed range exceed a quarter of this; but changes of  $2^{\circ}$  or  $3^{\circ}$  in the course of an hour or two were not uncommon.

The discussion of the horizontal force observations occupies pp. 62-126, the results being summarised on pp. 119-126. The apparatus was by Zschau. Observations of vibration and deflection were made much in the usual way. The moments of inertia of the two magnets used had been determined, but only approximately, and instead of employing the values so calculated use is made for each magnet of a "constant," involving the moment of inertia, which was determined by observations made at Hamburg and Wilhelmshaven. In some instances the horizontal force is deduced from a deflection experiment alone, by means of a second "constant" involving the magnetic moment of the deflecting magnet. The times of vibration were taken without a telescope, and no observations were made on the torsion of the silk suspension. Mr. Steen also experienced some trouble in connection with the temperature coefficients, which had not been determined at Arctic temperatures.

The inclination observations are discussed on pp. 127-165. The instrument used was a Fox circle, as