Among his publications are memoirs on 'Omaha Sociology,' 'Osage Traditions,' 'a study of Siouan cults,' 'Omaha dwellings, furniture and implements,' printed in the annual reports of the Bureau of American Ethnology; 'Omaha and Ponca letters,' a bulletin of the same bureau; and the 'Dhegiha language,' forming Volume VI. of the Contributions to North American Ethnology. In addition he edited a Dakota-English dictionary, and a volume on Dakota grammar, texts and ethnography, by the late Rev. S. R. Riggs, published in two volumes of the last named series. Numerous minor articles were published in different anthropologic journals. Mr. Dorsey was Vice-President of Section H of the A. A. A. S. in 1893, and at the time of his death was Vice-President of the American Folk-lore Society. In the absence of the President of this Society he presided over the annual meeting in Washington during the Christmas holidays, this being his last public work in science. W J M

DISCUSSION.

ON INDISCRIMINATE 'TAKING.'

IN many of the text-books which have of late appeared, and even in articles by some of the most renowned chemists, the verb 'to take' is frequently used in a way that is very annoying to teachers who are endeavoring to train students in brevity and exactness of expression. Pages could be filled with examples of bad style and verbosity that ill-accord with the clearness and brevity that are desirable, and that are supposed to characterize scientific literature. A few quotations from recent text-books will suffice to illustrate this particular case—that of indiscriminate 'taking.'

"Take a cylindrical porous jar, such as is used in a galvanic battery, close the open end, etc."

It were better to say, "close the end of a cylindrical porous jar, such as is used, etc."

Another example: "Take two flasks and connect them."

Better—"Connect two flasks," etc.

Another: "The method of experimenting adopted by Graham was to take a bottle or jar with a neck contracted somewhat and fill it to within half an inch of the top with the solution of the salt to be investigated."

Better—"The method . . . was to fill a bottle or jar with a somewhat contracted neck to within half an inch," etc.

Another: "If we take an iron tube closed at one end and connected at the other with a Sprengel pump and exhaust it completely."

This awkward form of diction often excites mirth in the class-room, as it gives unusual opportunities for double meanings.

"Take a pound of sugar and an equal weight of sulfuric acid." This would be a severe dose, even for a trained scientist.

The following is from a recent text-book: "Take a lump of chalk or sandstone, some very dry sand, a glass of water and a glass of treacle."

This might do for a bill of fare in a Chinese restaurant, but it is out of place in a scientific book.

"Take some white arsenic."—"Take a sedlitz powder,"—are the singular directions which preface two experiments in a book recently published by the Society for Promotion of Christian Knowledge in London.

If editors and teachers will pay more attention to this awkward use of the word 'take' they will incur the gratitude of a patiently suffering public.

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SCIENTIFIC LITERATURE.

The Life of Richard Owen. By his grandson, the REV. RICHARD OWEN, M. A. With the scientific portions revised by C. DAVIES SHERBORN. Also an essay on Owen's position in anatomical science.

By the Right Hon. T. H. Huxley, F. R. S. Portraits and illustrations. In two volumes. New York, D. Appleton & Co. 1894. Pp. 409, 393. $7.50.

The life of the great English comparative anatomist as told in these volumes was in many respects an ideal one. It is the old story of a self-made man, who, without the advantages of good preparatory schools, or of the university life at Cambridge or Oxford, by his own native ability and industry, as well as by his kindly disposition and social tact, rose to the highest scientific position in Great Britain, came to be the friend of some of England's leading statesmen, of her greatest poets and novelists; the recipient of marked favors from the Queen; living to see the completion of the magnificent natural history museum at South Kensington planned by himself, and dying at the great age of eighty-eight years, during sixty of which he published the long series of monographs and general works which form his most enduring monument.

This biography, as prepared by his grandson largely from Owen's letters and diary and those of his wife, even if it includes what may be thought to be many trivial details, gives what seems to us to be a most attractive and life-like sketch of the man. We see Owen, not only in his study at the College of Surgeons and afterwards at the British Museum, but also at his home in the little rambling thatched cottage in Richmond Park, presented him by the Queen. We also catch glimpses of his club life, of his success as an administrator, as a lecturer, as a literateur; we are given evidences of his fondness for art and music and the drama, as well as poetry, and accounts of his journeys over the continent and up the Nile.

It is a record not of a scientific recluse, but of one who had many outside interests, and who lived in touch with the best minds and the best thought of his time.

Richard Owen was born in 1804 at Lancaster, the son of a merchant. After leaving the grammar school, he was when sixteen apprenticed to a surgeon, and when twenty matriculated at Edinburgh University as a medical student. Six years after he became prosector to Dr. Abernethy in London and assistant curator of the Hunterian Collection at the College of Surgeons, and in 1856 was appointed superintendent of the Natural History collections of the British Museum, a position created for him and which he held until shortly before his death.

His first paper was published in 1830, and two years later his famous memoir on the pearly nautilus. This at once gave him a national and continental reputation as a comparative anatomist of the first rank. Huxley makes the generous claim, in referring to the work, that there is nothing better in Cuvier's 'Mémoires sur les Mollusques,' and he adds: "Certainly in the sixty years that have elapsed since the publication of this remarkable monograph, it has not been excelled, and that is a good deal to say with Müller's 'Myxinoid Fishes' for a competitor." Owen's last work (the list of the entire series of articles, monographs and general works embracing 647 titles) appeared in 1889. What a record! Sixty years of almost uninterrupted health, of unexampled productiveness, of accurate, painstaking, honest labor.

Owen's place in biological science, a science which has widened and deepened so immeasurably since the date of publication of his first great work in 1832, is not altogether easy to determine, but the task is much lightened by the appreciative and magnanimous essay by Professor Huxley on Owen's position in Anatomical Science, placed at the end of the biography.

Owen was called by some of his contemporaries 'the British Cuvier,' and this fairly well expresses his position. He may be said to have lived in the interregnum between

the age of Oken, St. Hilaire and Cuvier, and the age of the modern school of morphologists. He made no special contributions to comparative embryology; he was guiltless of histology and of microscopic technique. His ideas and lines of thought and work were a fusion of Okenism and of the doctrine of correlation of organs taught by Cuvier, with perhaps a slight infusion of the transformationist school of France. Like some of the fossil forms which he restored with masterly skill and philosophic insight, he was in a sense a synthetic or prophetic type of naturalist. For example, he declined when asked to attack the 'Vestiges of Creation', rather sympathizing with the views put forth in that book; but also objected to become a loyal disciple of his friend, Darwin. He partially accepted the general doctrine of evolution; but though his views were vague and unformed, like many others perhaps in the period between 1850 and 1870, he probably felt that Natural Selection was not a sole, efficient cause, though believing in the orderly evolution of life by secondary law.

We find in this life no statement from Owen's own letters or journals regarding his attitude to the doctrine of natural selection. Either he was late in life somewhat indifferent, or he was guarded in speaking or writing of the matter. Certainly there are no grounds for the statement sometimes made that he showed outright 'hostility' to Darwinism, unless his Athenæum article be regarded as such. In Owen's evidence before Mr. Gregory's committee regarding the removal of the Natural History Collection to South Kensington his biographer tells us: "Owen made some interesting remarks concerning Darwin's work on the 'Origin of Species,' just published, which helps to strengthen the impression that he was at first much taken with the new views, and felt the same friendliness toward them as he had previously shown to the views expressed in the 'Vestiges of Creation.'" Owen remarks concerning the arrangement of the new museum: "With regard to birds, I must say that not only would I exhibit every species, but I see clearly, in the present plan of natural history philosophy, that we shall be compelled to exhibit varieties also. . . . . As to showing you the varieties of those species, or any of those phenomena that would aid one in getting at the mystery of mysteries, the origin of species, our space does not permit;" and again he replies to a question of the chairman: "I must say that the number of intellectual individuals interested in the great question which is mooted in Mr. Darwin's book is far beyond the small class expressly concerned in scientific research."

Owen's controversial papers, as well as his statements of scientific belief, were at times vague and a grain oracular, and were presented in a labored style, quite different from that of his letters and popular lectures, or even his work on Archetypes, the style of which has been characterized as 'clear and forcible.' Darwin in the well known reference to Owen's views in the Historical Sketch prefacing the sixth edition of the Origin of Species was, he says, 'completely deceived' by such expressions as 'the continuous operation of creative power,' and he was apparently unable to determine what his real opinions were, and was evidently piqued and disappointed that the great anatomist, his old scientific friend of many years, did not accept the doctrine of natural selection. On p. 91 his biographer states: "If not 'dead against' the theory of natural selection, Owen at first looked askance at it, preferring the idea of the great scheme of Nature which he had himself advanced. He was of the opinion that the operation of external influences and the resulting 'contest of existence' lead to certain species becoming extinct. Thus it came about, he supposed, that, like the dodo in recent times,

the dinornis and other gigantic birds had disappeared. But he never, so far as can be ascertained, expressed a definite opinion on Darwinism."

It is well enough at this day, when the scientific world is of one mind as regards the truth of the evolution theory, to ascribe indifference and even 'hostility' to Owen, but we fail to see that this is quite just. For Owen, so far from attacking or minimizing the new plan of evolution invoked by Darwin, was even said by the editor of the 'London Review,' as Darwin tells us, in his own words, to have 'promulgated the theory of natural selection before I had done so.'

So strong a Darwinian as the acute and clear-headed Gray states, more fully and satisfactorily perhaps than Darwin, the position of Owen. In his 'Darwiniana' Dr. Asa Gray, who, writing in 1860, frankly confesses: "We are not disposed nor prepared to take sides for or against the new hypothesis," and yet who by his own studies and mental tendencies was 'not wholly unprepared for it,' thus humorously refers to Owen's views, published before the appearance of Darwin's book, "Now and then we encountered a sentence, like Prof. Owen's 'axiom of the continuous operation of the ordained becoming of living things,' which haunted us like an apparition. For, dim as our conception must needs be as to what such oracular and grandiloquent phrases might really mean, we feel confident that they presaged no good to old beliefs" (p. 88). Further on he writes: "Owen himself is apparently in travail with some transmutation theory of his own conceiving, which may yet see the light, although Darwin's came first to the birth. . . . . Indeed to turn the point of a pungent simile directed against Darwin—the difference between the Darwinian and the Owenian hypotheses may, after all, be only that between homœopathic and heroic doses of the same drug" (p. 102). Again, in 1873, he writes: "Owen still earlier signified his adhesion to the doctrine of derivation in some form, but apparently upon general, speculative grounds; for he repudiated natural selection, and offered no other natural solution of the mystery of the orderly incoming of cognate forms."

Finally we may quote from a letter of Darwin's (Life ii. p. 388), written in 1862 to Sir Charles Lyell: "I was assured that Owen in his lectures this spring advanced as a new idea that wingless birds had lost their wings by disuse, also that magpies stole spoons, &c., from a remnant of some instinct like that of the Bower bird, which ornaments its playing passage with pretty feathers. Indeed, I am told that he hinted plainly that all birds are descended from one."

From all that has been said it would seem to follow, from a perusal of these scattered fragments, that Owen was an evolutionist somewhat of the Lamarckian school; that he was not a Darwinian as such, not being fully persuaded of the adequacy of natural selection as the sole cause of all evolution. To this class certainly belong some naturalists and philosophers of the present day. But it should be added that Owen, in the latter part of his life, did not use the hypothesis or theory as a working one, as did some of the elder naturalists of his own period, as Lyell, Wyman, Leidy, etc. He was fifty-five years old when the 'Origin of Species' appeared, and either was not then prone to speculation, or had little leisure for it.

It must be granted that Owen, clear-headed and sagacious as he was, did not rise to the plane of that high quality of genius which opens up new lines of investigation. His was not an epoch-making mind of the quality of Lamarck or Darwin, in the field of evolution, nor of Müller, Von Baer, Rathke, and Huxley, the founders of modern morphology; nor of Koelliker or

Leydig, the founders of modern histology. He was a closet naturalist, made no collections with his own hands, was not a field paleontologist; and his travels were rather for health and recreation than for study or exploration. The vast collections which poured in upon him from South America, Australia and New Zealand, as well as from his own land, occupied his working hours and energies decade after decade, until the passing years left him stranded on the shores of a world of ideas and modes of cooking now subsiding beneath the incoming flood of modern methods and theories.

And yet, his philosophic grasp and suggestive mind exhibited in his treatment of the subject of parthenogenesis, in his essay on the subject which appeared in 1849, and in which he has, as Huxley states, anticipated the theory of germ-plasm of Weismann, are qualities of genius, and prove what he might have produced, had he received any training along the lines of embryology and cell-doctrines.

"Owen, in fact," says Huxley, "got no further towards the solution of this wonderful and difficult problem than Morren and others had done before him. But it is an interesting circumstance that the leading idea of 'Parthenogenesis,' namely, that sexless proliferation is, in some way, dependent upon the presence in the prolifying region, of relatively unaltered descendants of the primary impregnated embryo cell (A + B) is at the bottom of most of the attempts which have recently been made to deal with the question. The theory of the continuity of germ-plasm of Weismann, for example, is practically the same as Owen's, if we omit from the latter the notion that the endowment with 'spermatic force' is the indispensable condition of proliferation."

Owen's greatest works, those of most lasting value, in vertebrate zoölogy were, as pointed out by Huxley, besides his memoir on the anatomy of Nautilus, his work on Odontography, his papers on the anthropoid apes, on the aye-aye, on Monotremes, and on Marsupials, as well as on Apteryx, the great auk, the Dodo, and Dinornis, as well as Lepidosiren, while chief among his essays on fossil mammals were those on Mylodon, Megatherium, Glyptodon, etc. He also proposed the orders of Theriodonta (Anomodontia), Dinosauria, and Pterosauria, and as early as 1839, as Zittel states, "he began his long series of fundamental works which continued to appear for half a century, and which laid the foundation for all later researches on fossil reptiles." He also revised the classification of the Ungulates, his divisions of odd and even-toed Ungulates being well founded and generally accepted.

Unlike Cuvier and others, Owen labored without the aid of trained assistants; he did his own work unassisted. And here arises the question how far he was indebted to Cuvier for his methods of work. It is generally supposed and stated that Owen studied in Paris under Cuvier, and that "Cuvier and his collections made a great impression on Owen, and gave a direction to his after studies of fossil remains." But his biographer explicitly states that he only made a brief visit to Cuvier in July, 1831, and gives us the following account of his intercourse with the great French anatomist: "His rough diary, which he kept during his stay at Paris, seldom mentions the fossil vertebrate collection, and shows that his interviews with Baron Cuvier were for the most part of a purely social character. It notes, for example, that he attended pretty regularly Cuvier's soirées, held on Saturday evenings, and that he enjoyed the music. With the diary agree his letters. Both devote page after page to the sights and amusements of Paris. Owen, in fact, seems to have regarded this stay at Paris as an exceedingly pleasant and entertaining holiday. At the same time it is impossible to form a just estimate of Owen's work without tak-

ing the labors of Cuvier into account. Although Owen stands on ground wholly his own, he was ever willing to acknowledge the debt which he owed to Cuvier."

The name of Owen will ever be associated with those of Oken, Goethe, Spix, and Carus, or the school of transcendental anatomy. The discussion by Huxley of Owen's work on the archetype of the vertebrate skeleton is handled in his peculiarly trenchant and clear-minded way, and yet his criticisms are genial, just and broad. It should be remembered that Owen's work 'On the Archetype and Homologies of the Vertebrate Skeleton' appeared in 1848, over ten years before the appearance of the 'Origin of Species,' and at a period when many minds in the scientific world were tinged more or less deeply with the spirit of the German and French transcendental school of anatomy. As Huxley eloquently expresses it, "The ablest of us is a child of his time, profiting by one set of its influences, limited by another. It was Owen's limitation that he occupied himself with speculations about the 'Archetype' some time before the work of the embryologists began to be appreciated in this country. It had not yet come to be understood that, after the publication of the investigations of Rathke, Reichert, Remak, Vogt and others, the *venue* of the great cause of the morpology of the skeleton was removed from the court of comparative anatomy to that of embryology." He then adds: "It would be a great mistake, however, to conclude that Owen's labours in the field of morphology were lost, because they have yielded little fruit of the kind he looked for. On the contrary, they not only did a great deal of good by awakening attention to the higher problems of morphology in this country; but they were of much service in clarifying and improving anatomical nomenclature, especially in respect of the vertebral region."

As regards the vertebrate theory of the skull, perhaps the last word has not been said, if traces of vertebræ still, as is alleged, appear in certain of the sharks.

If Huxley by his destructive criticism has destroyed, or seemed to have destroyed this theory, the ghost is apparently not wholly laid. The more ideal constructive, German minds, as Gegenbaur and others, claim that the adult skull is in a degree segmented, as evinced by the serial arrangement of the nerves, as well as of the branchial arches. Though Wiedersheim states* that "the attempt to explain the adult skull as a series of vertebræ fails completely," adding, "it is a case of protovertebræ only," he says in a foot-note that Rosenberg has, however, shown that in a shark (*Carcharias glaucus*), "the portion of the cranium lying between the exit of the vagus and the vertebral column is clearly composed of three vertebræ." Gadow finds four vertebræ in embryos of Carcharias, while Sagemehl has found a somewhat similar modification in Ganoids. It would seem that the segmentation of the head observed in the embryo of vertebrates, and probably inherited from their vermian ancestors, has been obliterated in the adults by adaptation, but that traces may have survived in certain sharks and Ganoids.

Finally, it must be conceded that though it is the fashion of the younger men to characterize Owen as a comparative anatomist of the old school, and now quite overshadowed by the scientific leaders of the present generation, the kindly and discriminating judgment of the great English anatomist and essayist we have just quoted, will undoubtedly be sustained by many coming generations. Owen's place in natural science, in many respects an unique one, will be among the greatest anatomists of the first half of our century. His name will bridge over the gap between Cuvier, and the embryologists and morphologists

* Elements of the Comparative Anatomy of Vertebrates, p. 56.

of the second half of the nineteenth century.

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Heat; Light; Elementary Text-Books, Theoretical and Practical for Schools and Colleges: By R. T. GLAZEBROOK. 12 mo., about 220 pages each. New York, Macmillan & Co. Price $1.00.

These are recent volumes in the series of Cambridge Natural Science Manuals.

All American physicists are familiar with the previous excellent products of Mr. Glazebrook's pen in the line of text-books for laboratory and class-room, and will be interested in this new series which is intended to fill a place quite different from that for which his previous works were prepared. They are less extensive and more elementary. According to the author, they represent what has for some time constituted a practical course for medical students in the Cavendish laboratory. There has been much discussion, and there will continue to be much discussion for some time to come, as to the proper sequence of laboratory, text-book and lecture instruction in elementary physics. In the Cavendish laboratory the system adopted for this course, at least, seems to be that the instructor first presents a portion of the subject in the form of a lecture in which he illustrates, by the use of simple apparatus, and explains the theory of the experiments, deriving principles and numerical results, as far as possible, from the results of experiments actually performed. The members of the class then make the experiments, singly or in pairs, or occasionally in large groups, using the same, or similar, apparatus. The volumes contain descriptions of experiments and also theoretical principles and deductions, so that they constitute at once text-book and laboratory hand-book. At intervals throughout the work there will be found well selected collections of problems and examples, and a good set of examination questions at the end. The apparatus described is usually simple, and most of it could be made with simple materials by one having some technical skill of the right sort.

It is hardly necessary to say that the theoretical discussions and presentation of principles are, for the most part, clear and clean as far as they go.

In the 'Heat,' the first chapter has to do with its nature, and its relation to work or energy is concisely but clearly stated. In the second chapter the treatment of temperature and its measurement is unusually satisfactory, considering the limitations to which the whole work is subjected. It is to be regretted, however, that there is no mention of the hydrogen scale, since so many of the most important temperature measurements now depend upon it. Calorimetry is discussed quite thoroughly, with many practical illustrations, and in the chapters devoted to expansion several neat suggestions as to methods will be found. In the reference to the necessity for 'compensating' the effect of temperature on the balance wheel of a watch, it is erroneously implied that the principal reason for this grows out of the change in the dimensions, and consequently moment of inertia of the wheel due to change in temperature, while, as a matter of fact, it is the temperature change of the modulus of elasticity of the 'hair' or balance spring which makes nearly all the trouble. The volume ends with a brief but good chapter on the mechanical equivalent of heat.

In the volume on 'Light,' the geometrical treatment is used exclusively. There is a single brief reference to the physical nature of light, which is so thoroughly discussed in the author's volume on 'Physical Optics' published some years ago, but in the book under consideration the rectilinear propagation of a 'ray' is assumed and made the basis of the whole discussion. The chapters