

recognizable; will they not in the end become mere *nomina nuda*?

Identity being the fundamental basis of nomenclature, and intimately connected with the end of systematic work itself, it seems utterly absurd to ignore it or to give it but passing attention. Therefore immediate steps should be taken to insure it. Instead of having an international code of nomenclature recommended to zoologists, to be followed at their discretion, we have advanced far enough to have one which should be enforced by legislation of some such body as the International Zoological Congress, no systematist being recognized unless adhering rigidly to its rulings. At first thought this step may appear to be visionary, as we can not by law control such intangible or incorporeal things as the individual judgments of men concerning what is or is not a good description of a thing; nevertheless, we can prescribe, in cases of the kind considered, what shall or shall not be done in the future. Genera described without species can be rigidly barred; genera described without a description of the type species upon which they are based can be treated likewise. The authors of such genera could be reprimanded or discountenanced, in a sense proscribed. Further a date of departure for a new system of nomenclature based on the future should be designated, for the questions of the past should be studiously avoided in the future, and the new code should be conceived in the spirit of the future, that is to say, in the spirit of expansion, of progress. Such a code, for instance, could provide for the future cases coming under article 21 of the international code, which should be framed along lines tending to make descriptions infinite in detail. For example, an *indication* should not be allowed to hold for present-day or future descriptions and some provision should be made for the compulsory deposition of types in accredited museums. I have mentioned but one or two points which such a code should be expected to cover; for its development and adoption I can hope only; for these few suggestions, I beg the consideration due to the spirit in which they are offered.

The end should always be in mind; we must

broaden our view-point; let us look to the future, for properly the present belongs to it.

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SCIENTIFIC BOOKS

The Origin of the Vertebrata. By WALTER HOLBROOK GASKELL. Longmans, Green & Co. 1908.

Professor Gaskell during the past two decades has published an extended series of papers which have aimed to convert morphologists to the view that vertebrates are descended from arachnids. These papers, with additions and corrections, are now brought together in volume form. We suggest, however, the book's title "*The Origin of the Vertebrata*" is chosen inaptly. It should have read "*The Supposed Arachnid Origin of the Vertebrata*," or, better, "*A Plea for the Rejected Theory of the Origin of the Vertebrates from Arachnids*." For it is hardly fair that the purchaser of this book should believe that he has here a résumé of our knowledge of the ancestry of the vertebrates. He is given merely a one-sided view of the whole intricate problem.

It is just to say that Gaskell has devoted himself generously to the task which he has sought to accomplish. His work shows that he has been earnest and tireless, that his reading has covered a field much wider than that of the usual promoter of a lost cause—that he is not one of those whose effort is measured in terms of success, for he would himself admit that even his friends (and he has many sympathetic ones) in the wide zoological fraternity, do not subscribe (there is scarcely an exception) to a single tenet of his heretical morphology. If he had been trained as a morphologist instead of as a physiologist, perhaps he himself would never have developed his theory.

There has been of late years a tendency to ignore Gaskell's writings on the ground that his arguments, having been weighed carefully, have been found wanting. Then, too, we have lost zest for discussing his difficult theses, *e. g.*, that the arachnid gut and nervous cord fused

to establish the tubular vertebrate nervous system; that the vertebrate gut is a new structure formed by the fusion of the bases of arachnid appendages; that the notochord was later formed from this new gut; that the arachnid genital ducts were retained as the vertebrate thyroid; that the arachnid genital tissue and liver became converted into the arachnoidal fat which fills the brain-case and invades even the skeletal capsule of the vertebrate ear; that Kupffer's sensory plakodes are reminiscent of arthropod appendages; that coxal glands are the homologues of both the pronephros and thymus; that the wide discrepancy in the plan of the embryonic development of arthropod and vertebrate, as in the inversion of the dorso-ventral orientation, is a mere detail caused by the shifting of the mass of the yolk.

One hesitates at this day to reopen an indigestible discussion. And it would be profitless were it not that the volume has brought forward the theory in such a pleasantly written and well-published fashion, which will give it in all probability a wide circulation. The present review need comment on but a few of its teachings—those which touch fundamental conceptions in morphology.

I. Gaskell maintains the doctrine that evolution proceeds (genetically) from the dominant type of one geological horizon to the dominant type of the following geological horizon. This is a doctrine which at the best is intangible and unconvincing. Even the example cited by Gaskell does not support his case—that the vertebrates in their earliest occurrence superseded the sea-scorpions of the early Paleozoic. For at that critical time it was the cephalopods (*e. g.*, the huge *Orthoceratids*), not the sea-scorpions, which were the dominant race. However, for the sake of argument, granting that the cephalopods are lower than arachnids, they are obviously much higher (measured by the standard of the nervous system) than the lower crustaceans or the worms, hence by the doctrine of dominant types they should have taken an intermediate genealogical position between the crustacea and the arachnids, which even Gaskell would deny.

II. Gaskell seems to have little conception

of parallelism, and he is probably, therefore, unaware of the mighty literature dealing with his theme. This is the more regrettable, since it is this principle which bears so directly upon phylogenetic studies. For it can now be demonstrated beyond peradventure that animals, *e. g.*, of different orders, may develop similar structures to such a degree that they are sometimes mistaken for members of the same family or even genus. And if this be true, how can we believe that certain specified resemblances of king-crab to vertebrate can be accepted as tests of genetic kinship? If such a form as a *Litoptern* can develop many essentials of a horse, yet be not included within the great group of Ungulates, how can we accept Gaskell's elaborate details when he compares forms as widely apart as a vertebrate and an arachnid? An arachnid has no tubular nervous system, no gill slits, no notochord; it has widely different appendages, skeleton, skin, urogenital system, sense organs—how therefore do we venture to compare its details with such vertebrate structures as the abducens, or the various branches of the fifth nerve, or the jaw muscles? We might, in fact, make a comparison of this kind as convincingly, or unconvincingly, by selecting the structures of a cephalopod. In a word, it is this kind of comparison which makes the distressed reader cast down Gaskell's book even when assured by the author that the evidence of these "homologies amounts almost to a certainty."

III. Gaskell fails to take into account a fundamental rule in descent study, that *series* of forms whose structures are the most closely connected should be used for comparisons. This rule he violates constantly when he compares the Paleozoic *Cephalaspis* and the recent lamprey-larva, *Ammocetes*, for these forms are by no means the nearest links in a possible chain. As a matter of fact, we now know that *Cephalaspis* is but one example of a great group of Paleozoic creatures, that these creatures show the greatest range in forms and structures, and that many of them are unlike arachnids even in superficial regard, in fact some of them are soft-bodied and covered with shagreen like a

shark. It is, accordingly, by no means to be accepted that these creatures are nearly akin to *Limulus*, even if cases of superficial resemblance be pointed out. For the general outward shape of *Limulus* may be acquired independently by creatures of very different groups, even to a certain degree among vertebrates by rays and siluroids. On the other hand, it is clear that ammocetes should be compared at first not with a Paleozoic form of dubious kinship, but with other cyclostomes, especially with the hag-fishes, which Gaskell rarely mentions. The fact is that after comparison with the latter forms, we are less inclined to regard the ammocete as a primitive and unmodified creature. For we find that the hag-fishes have no metamorphosis, and we may, therefore, more easily harbor the suspicion that the exceptional sand-living life habit of the larval lamprey has been responsible for many of its curious features, and that these have no wider phylogenetic bearings than have, for example, the peculiar larvalisms developed by many teleosts. But let us not go into details. The momentous problem of vertebrate beginnings is still "on the knees of the gods." We gravely doubt whether Gaskell's book will be of great value in dislodging it.

BASHFORD DEAN

Modern Thought and the Crisis in Belief.

The Baldwin Lectures, 1909. By R. M. WENLEY. New York, Macmillan. 1909. Pp. ix + 364.

This volume results from the nomination of Professor Wenley by the Protestant Episcopal Bishop of Michigan to give a series of lectures in an endowed course "for the Establishment and Defence of Christian Truth." The circumstance will, perhaps, not especially commend the book to the interest of some readers of this journal. Few ways of spending money seem to some modern minds less desirable, or more productive of ethically awkward situations, than the creation of permanent foundations for scholarly inquiries or discussions, whose results are predetermined by the terms of the endowment supporting them; this is true whether the predetermined result be the truth of Christianity or the truth of socialism.

With old foundations of this sort we must do the best we can; but it is a somewhat regrettable anachronism that new ones should appear in recent years, and in connection with American universities. One can hardly suppose that the Christian truths which Professor Wenley establishes and defends would have been recognized as such by the episcopal founder of the lectureship, no longer ago than 1885. The book is almost equally divided into a destructive criticism of religious beliefs still current, and philosophical reconstruction; but one apprehends more clearly what it is that is destroyed than what it is that is constructed. The best, and the longest, division of the book deals with a topic that does not call for discussion here: the religious consequences of historical criticism; the outcome is a frank abandonment of the historical character and content of Christianity, and the transfer of interest from a historic teacher to a "metahistorical Christ." The precise ontological status of this entity, and its relation to the historic Jesus, remain obscure to the present reviewer. The other main division of the book concerns the religious bearings and the philosophic validity of the "natural science view of the world"—the doctrine unfortunately labeled by Ward "naturalism," by which appears to be meant a mechanistic cosmology, biology and psychology taken as equivalent to a complete account of the nature of reality. With this, Professor Wenley vigorously argues, religious thought must now have a definite reckoning; for while historical criticism can destroy nothing essential to religion—since nothing historical is essential to religion—naturalism is the "executioner of the ideal life." Since the refutation of naturalism is presented as the main task, not only of this book, but of the present age, one is disappointed to find Professor Wenley devoting expressly to it only some forty pages—one ninth of his space. It should be said, however, that the author regards the task as one for the most part already accomplished, by Ward's "Naturalism and Agnosticism," which he here, so to say, reënacts. His own argument rests chiefly upon two points: (1) Every science begins by deliberately abstracting certain as-