people, throw a grim and sinister side light on Arctic exploration as carried on by Americans. In the *Yale Alumni Weekly* of October 8, p. 56, for instance, is a highly interesting human document from the pen of Mr. George Borup, a young man who accompanied Lieutenant Peary on his recent expedition. He recites some of his experiences as follows:

> . . . Here [at Cape Morris Jesup, past Lockwood's furthest of 83° 24′] we stayed two weeks. . . . Here we lived high, killing 47 musk oxen in four hunts, and dogs and men had sirloin and tenderloin all the time. As none of us had had any fresh meat in three months it was more than good. I got mixed up in one herd of sixteen and took some good photos of them. Then we killed them all by gun. I beat all records, Duffy's included, when I got within ten feet of a big bull, held at bay by two dogs, to take his photo, and he charged the dogs, which happened to be on a line between us. I only hit the high spots for a hundred yards or so. Coming back . . . went off on a hunting trip. Killed four musk oxen, 100 miles away, and brought back a calf on the sledge alive to the boat, only to have it die the next day. When we got down to Eskimo land we put in about four days walrus hunting. In all about 72 were secured.

It thus appears that the indiscriminate killing of the musk oxen has been the common diversion of Americans in the north. Other explorers have been more thoughtful, notably the Swedes, who made an unsuccessful attempt to domesticate these splendid animals in Lapland. It is well enough known that the musk oxen once spread southward to the Hudson Bay country, and westward to the Mackenzie River, and that they are now on the verge of extinction, but a few hundred being perchance all that are left. In short, Mr. Borup does not appear to have been aware of the deadly results following in the train of his hunting.

But it is difficult to understand such a recital, such an exhibition of the "*mord lust*" by any human being, leaving out of account the unwitting confession that this slaughter took place just about the calving time, or a little before in the case of the larger herds, it might seem. And did this finally compassionate hunter expect to suckle the last musk ox he saw, the little calf he took back to the boat, "only to have it die"!

In the main, however, we hold Lieutenant Peary directly responsible. He should have issued orders to protect these animals, and if our Arctic exploration had been carried on on a higher and more scientific plane this would have been done. Nor do I hesitate to say that in my judgment it was of more importance to avoid the slaughter of these musk oxen and walrus than it has proven to march across the ice, only to bring back the records of a scanty performance. The man who is broad minded and thoughtful and merciful, and careful of his temper, and who describes with the needed care a single new beetle or brachiopod, deserves better than these notoriety-seeking types of scientists. Indeed it is time that the halo they wear should be more carefully examined. For my part, I have never seen the day when I did not find it easier to work in the field than in the laboratory, and I believe it is so with most scientific workers.

There is, with the great increase in comfort within recent years, relatively no more risk in the glorious holiday that an Arctic exploration can with ordinary forethought be made to mean, than there is in more serious scientific work indoors.

In view of such pertinent facts it is greatly to be hoped that future Arctic exploration will be carried on in a more humane and scientific spirit.

G. R. WIELAND

SCIENTIFIC BOOKS

The Science and Philosophy of the Organism. The Gifford Lectures before the University of Aberdeen. By HANS DRIESCH, Ph.D., Heidelberg. Vol. I., 1907, pp. xiii + 329; Vol. II., 1908, pp. xvi + 381. London, A. and C. Black.

Das Kausalitätsproblem der Biologie. Von Dr. med. FRIEDRICH STRECKER, Privatdozent an der Universität Breslau. Pp. viii + 153. Leipzig, Engelmann. 1907.

Driesch's Gifford Lectures give the English reader his first introduction to an interesting and important movement in recent German thought—the *rapprochement* between biology and philosophy that has been taking place

during the past fifteen years. The movement is significant chiefly because of the side from which most of the approaching has proceeded. It is a case of biologists turning to philosophy because of problems arising in their own inquiries, not the more usual case of philosophers utilizing or criticizing the results of biology. To the student of philosophy the discussions thus arising have something refreshing about them because they bring philosophical issues into apparent connection with concrete and contingent matters of fact; whereas the idealistic or the "double entry" systems of the *Kathederphilosophen* have, for the most part, long since been divorced from any such relation to specific items of reality, and have represented philosophic truth as a fixed, all-inclusive, ornamental frame for the universe, into which any particular that chances empirically to turn up will fit as harmoniously as any other. The disputations of the newer biological philosophies may often be philosophically naïve, but they lend to metaphysics an unwonted and pleasing appearance of life and pertinency, through their constant movement back and forth between philosophic principle and empirical fact. The occasion for this awakening of interest in ulterior problems on the part of biologists has been the appearance, among their own ranks, of a large and aggressive school of vitalists. In the case of those who, perhaps, best deserve to be called "neo-vitalists"—Driesch and G. Wolff—this tendency has been an unexpected and somewhat paradoxical outcome of the impulsion given by Roux to experimental research in morphology and physiology, to the science of *Entwicklungsmechanik;* "all the new facts," says Driesch, "in support of the doctrine have been found in this field of inquiry." But vitalism has found vigorous spokesmen among specialists of high standing in nearly all branches of biological science: Bunge representing physiological chemistry, Hertwig representing morphology and embryology, Pauly representing phyletic zoology, Reinke representing botany, and, in France, Bergson representing psychology. Besides these leaders, a considerable army of writers less well known have produced an imposing array of vitalistic books and pamphlets; and one of the wings into which the school is divided has since 1907 had a special periodical organ for the diffusion of its opinions, Francé's "Zeitschrift für den Ausbau der Entwicklungslehre." So great and so rapid has been the progress of the movement that the philosophic thinker who has most influenced it, E. von Hartmann, declared confidently before his death (in his "Das Problem des Lebens," 1906) that we are justified "in looking forward to a complete triumph of vitalism in the course of the twentieth century."

However that may turn out, the doctrine has thus far shown itself to be a sort of biological protestantism. Its adherents are united in their negations—they are at one in declaring that vital phenomena can not be described or "explained" in "merely mechanistic terms." But when it becomes a question of new theoretical construction, they split into warring sects. The ground of quarrel concerns the nature of the non-mechanical factor or factors to be recognized by biology, after the inadequacy of mechanistic causation has been admitted. The two principal views held upon this point may be called respectively psychological vitalism (or biological animism) and non-psychological vitalism; the latter, unsympathetic interpreters have sometimes been tempted into calling mythological vitalism. The psychological vitalists are those who find in the phenomena of consciousness—and especially in the immediately felt inner nature of simple awareness and desire and aversion—some clue to the sort of causal process which must be assumed to account for the peculiar unity, the definiteness of form, and the adaptiveness, of living things and their functioning. If science finds mere mechanism at one end of the scale of being—and, as is generally assumed, at one end of the evolutionary series—it just as surely, or more surely, finds sensation, feeling, memory and volition at the other end. And to the psychological vitalists it seems nothing less than an axiom of scientific method that our "immediate" knowledge of psychic causation should be used to inter-

pret the causal processes at work in those living beings below us in the scale of complexity and behind us in the evolutionary order. Bunge says:

> The essence of vitalism consists in this, that we follow the one correct road to knowledge, that which starts from the known of the inner world in order to arrive at the unknown of the outer world. Mechanistic theories follow the opposite and misleading course—they set out from the unknown in order to arrive at the known. Johannes Müller in his doctoral dissertation defended the thesis *psychologus nemo nisi physiologus*. The time will come when the contrary thesis, *physiologus nemo nisi psychologus*, will need no defender.

Of this type of vitalism Strecker's book is representative; it urges the necessity of reversing the customary logical procedure of biology, of "beginning with man, in whom the causation of development is now going on, and reading *rückwarts nach dem Primitiven* that which we find in man, namely, the processes of inner life." Similar views have been elaborated by Pauly, in his "Darwinismus und Lamarckismus." The oddest outcome of this tendency is the production by Francé of a system of "plant-psychology," expressed most fully in his "Das Leben der Pflanze," 1905, 1907, and in papers in his magazine. Though this certainly has a queer sound, it is not quite so grotesque as may be supposed. The doctrine of Pauly and Francé, at least, is merely a kind of Lamarckism, with the potential animistic elements in the Lamarckian conception of "needs" very much emphasized.

Over against these stand such vitalists as Driesch, Reinke, K. C. Schneider, who find the doctrines just mentioned reprehensibly anthropomorphic. But in their flight from the hobgoblin of anthropomorphism, some of these theorists fall into the arms of what—to the average biologist unaccustomed to them—will seem monsters of still more frightful mien. Driesch, for instance, having, as he believes, proven that such phenomena as morphogenesis, restitution and purposive behavior, have a teleological character not to be explained by the operation of any of the forces or entities ordinarily recognized by science, feels obliged to assume the existence of certain non-physical, but also non-conscious, agents for these effects. By culling diligently in scattered places in Driesch's second volume, one may gather the following list of the attributes of these agents, generally called "entelechies" (or, collectively, "entelechy"): (1) They are not in space; (2) they are not quantitative; (3) they are, therefore, not forms of energy; (4) they have "in no case anything of a psychical nature," though metaphorically or "by analogy" they may be said to know and will—a figurative mode of speech to which, it should be noted, their discoverer is himself much addicted; (5) they seem to be capable of self-multiplication—for "the primordial entelechy of the egg creates derived entelechies"; (6) their function is not to cause in the organism chemical reactions which otherwise would not occur, but only "to suspend for as long a time as (they) want any one or all of the reactions which are possible with such compounds as are present, and which would happen" if the entelechy did not intervene; (7) this function they probably perform, not by acting directly upon ordinary processes of chemical union, but by "activating" certain catalytic agents, which are the physically perceptible instruments of their teleological control. Such are entelechies in general; they are divided into two principal classes with distinctive habits; namely, morphogenetic agents and behavior-directing agents, the latter specifically known as "psychoids." Even these last are not conscious; they only accompany consciousness. For Driesch has a curious psycho-psychoidal parallelism of his own, quite distinct from the usual psycho-physical parallelism. It is not clear just how Driesch's entelechies differ from Reinke's "dominants," except that the dominants seem to be more numerous. Both form hierarchies somewhat like that of the German army. Neither sort of agent either creates or nullifies energy; both Reinke and Driesch are very solicitous to avoid conflict with the first principle of energetics, though they show less deference to other physical principles that appear empirically to be equally well grounded.

The teleological factors merely give direction and correlation and *tempo* to transfers of energy.

All this midsummer-night's dream land of biology, peopled with strange, elflike creatures, is, assuredly, the negligible part of contemporary vitalism. It is not in these premature and over-ingenious efforts of the constructive imagination that the new tendency has anything of present value to contribute to science or philosophy. But in the common and fundamental negative creed of all vitalists—their denial of the possibility of "reducing" all laws of organic action to consequences of the laws of mechanical action—there lies a significant and debatable issue. Yet even this fundamental issue is often ill formulated. One result of the whole discussion thus far has been to involve in a good deal of obscurity and confusion the meaning of the primary notions concerned—the notions of mechanism and vitalism. It has become difficult, in the absence of clear and generally accepted definitions, to make sure who should be called vitalists and who should not. Thus the writer of a recent historical sketch (Bräunig) classifies Nägeli, Eimer and Haacke as vitalists in spite of themselves. Driesch insists that Strecker is a real, though misguided, vitalist, while Strecker prefers to regard his doctrine as a "third standpoint," essentially different from both vitalism and mechanism. Certain of the school of *Energetiker* who recognize a specific vital form of energy call themselves vitalists, others appear to regard themselves as champions of mechanism; Driesch regards the latter classification as correct, because even "vital energy" is a quantitative conception. Le Dantec affirms, while Driesch denies, the chemical distinctiveness of living matter; but Le Dantec passes for an extreme opponent of vitalism. Through their diverse affiliations with current theories on cognate problems, the two parties exhibit almost all possible varieties of doctrinal hybrids. Le Dantec appears to be a mechanistic neo-Lamarckian; Pauly, a vitalistic neo-Lamarckian; Wolff, a vitalistic anti-Lamarckian. The result of all this confusion has been a great deal of arguing at cross-purposes, of which both sides have been guilty. I cite only one illustration. Zur Strassen, in a "refutation" of Driesch[1] contends that before any such phenomenon as restitution can serve as a pertinent argument for vitalism, each instance of it must be shown "to have no utility for the organism in which it occurs, and therefore to be a power that can not have been produced through natural selection." For causation through selection is mechanical causation. In so arguing, Zur Strassen is jumbling together two distinct senses of "mechanical." We speak, indeed, of the Darwinian explanation of the adaptive characters of organisms as a mechanistic explanation. But we mean thereby only that it is not teleological, that it represents the given effects as resulting from the pressure of external circumstances, through accidental conformity to which certain variations get selected. We do *not* mean that Darwinism traces the laws of species-forming back to the laws of molecular mechanics. Yet it is only in the latter sense that "mechanism" need be antithetic to vitalism. Darwinism takes the incipient useful variation for granted. But the argument of the vitalist is, or may be, drawn solely from the nature of the variation, not from the fact of its selection and survival as a character of a species. Its survival may be due to its usefulness; but it may itself, from the outset, constitute a mode of behavior of matter which is not reducible to mechanical law nor explicable as the result solely of the spatial ordering of material particles. It is for this latter conclusion that Driesch contends. Zur Strassen's objection to the argument from restitutions is, therefore, quite beside the mark.

Since little that is profitable can be said upon the subject until these confusions are cleared up, it is worth while to attempt to distinguish the possible meanings of the denial that vital phenomena can be "mechanistically" explained. The denial may refer primarily to the way in which the matter in the organism behaves, while, in a given living individual, a given physiological process is

[1] Roux's *Archiv*, 1908, p. 158.

going on; or it may refer to the antecedent processes by which organisms came to have that adaptation of form and function to the requirements of their external environment, which has somehow been brought about in the course of evolution. Taken (A) with the former reference, the vitalist's negation might mean any one of three propositions. (1) It might mean merely that vital phenomena are irreducibly unique, in such wise that the laws of their occurrence could never be deduced from even a complete knowledge of the laws of the behavior of matter under all *other* conditions than those under which these phenomena occur. In this sense, vitalism would be a species of "logical pluralism," of the general doctrine of "the heterogeneity and discontinuity of phenomena," which M. J. H. Boex-Borel has just set forth at length in his "Le Pluralisme." Such a doctrine would not be incompatible with a "chemical interpretation" of vital phenomena, so long as they were regarded as the unique modes of action of a unique chemical compound under certain physical conditions—modes of action which no acquaintance with the components separately, nor with other compounds, would have enabled one to predict. In this meaning, which seems to me the most convenient general meaning for the term, many biologists not commonly so called might be classified as vitalists. (2) The vitalistic negation may go farther, and declare that certain peculiarities of the behavior of matter in organisms can not be regarded as functions of even a unique chemical compound with unique modes of action. This might be proved if it could be shown that the peculiarities in question and chemical composition are independent variables. Driesch contends that such proof is possible, through an examination of the facts of morphology. "Specificity of form as such does not go hand in hand with specificity of chemical composition." (3) Chemistry itself, however, is by no means a truly mechanistic science; for it has never succeeded in interpreting all its qualitatively diverse phenomena as mere quantitative multiples of the separate properties or modes of action of the individual atoms entering into chemical relations. Vitalism may, however, passing by the question whether vital specificities and chemical specificities are correlative, attack the notion of mechanism as such; it may, namely, deny that the properties or activities of an organism can be functions of the presence, in a specific spatial grouping, of a determinate number of physically interacting units of matter or energy. It is to the defense of this view that Driesch chiefly devotes himself. He finds his arguments for it in such facts as the totipotency of isolated blastomeres, and the development of excised portions of the branchial apparatus of *Clavellina* into small but complete organisms. These facts show that in certain cases part of an organism can do the work of the whole—*i. e.*, produce the typical form ordinarily produced by the interaction of that part with all the other parts. Here, unquestionably, is a perfect refutation of mechanism *in the sense just defined;* the facts mentioned constitute a virtually tautological proof that, in these cases, morphogenetic processes are not functions of the *absolute number* of material units present, nor of any single scheme of relative spatial positions of a determinate number of units. Not the single cell, but the whole organism, is the morphological individual; for when the normal number of other cells are present in interaction with a given cell, the cell behaves in one manner; when the other cells are removed or transposed it behaves in another manner; but the resultant morphogenesis of the entire organism remains the same, in spite of these diversities of behavior of the single cells composing that organism. In bringing this out so plainly as he has done, Driesch has made a contribution of the first importance to our knowledge of the distinguishing peculiarities of living material systems. But it does not follow (as he supposes) from these facts that the specific morphogenetic action of, say, an *Echinus* egg, may not be a function of *some* (as yet undetermined) specificity of composition or structure or physical relations of the original material complex constituting the egg at the moment of fertilization. In other words, Driesch's arguments from morphogen-

esis and from restitutions do not appear to compel us to go beyond vitalism in sense (1) —a sense which he would evidently regard as tantamount to mechanism.

The vitalistic negation may, however, (B) refer to the large processes of phyletic evolution, or to the adaptations which have been realized in the course of that evolution, rather than to the peculiarities of the behavior of the material elements in individual living bodies. Some vitalists (Bergson and Pauly, for example) make much of considerations of this type. But this is only a superficially distinct form of the negative side of vitalism. For all these large aspects or consequences of evolution must be due primarily to processes of form-building taking place in the development of individual organisms. The vitalist must, then, in any case, maintain that these separate processes in the individual are not capable of "mechanistic" explanation; and his doctrine will, therefore, in the last analysis reduce to one of the three negations mentioned in the preceding paragraph. It remains possible that important evidence for one or another of these contentions may be found by the examination of the lines of direction and the broad results of racial evolution—of such phenomena, for example, as orthogenesis.

This review seemed most likely to be useful if it were made a species of historical and systematic introduction to the vitalistic controversy. In the discharge of the usual duties of a reviewer, however, it should be added that Driesch's book, though an important and valuable contribution to the discussion over vitalism, is not very successful as a work of popularization. It is ill planned and awkwardly executed, diffuse, involved, and written in a tongue far removed from idiomatic English. If designed to appeal to biologists and philosophers, on the other hand, the book would have been more effective if the author could have brought himself to let the entelechies alone, to omit many of his excursions into Kantian epistemology, and to content himself with expounding and interpreting (as he is eminently qualified to do) all those distinctive peculiarities and "discontinuities" in the action of living matter, which have been definitely established by the past twenty years' progress in the study of *Entwicklungsmechanik*.

A. O. Lovejoy
The University of Missouri

A Treatise on Zoology. By Sir Ray Lankester. *Crustacea,* W. T. Calman. Part VII. Appendiculata. Third fascicle. London, Adam & Charles Black. 1909. Price, twelve shillings and sixpence, net.

This is an excellent account of the class of the Crustacea from a purely zoological standpoint. The most important and striking feature of the book is the systematic arrangement of this highly diversified group of creatures, and the zoologist who is acquainted with the older systems of the crustaceans will be astonished, at the first glance, that certain systematic groups, which are familiar, have entirely disappeared. So, for instance, there are no "Entomostraca," no "Edriophthalma" and "Podophthalma," no "Schizopoda." Yet the new system used by Calman is entirely founded upon the most recent investigations, to which he himself has contributed a good deal.

In the present book, the class of Crustacea is divided into five subclasses: Branchiopoda, Ostracoda, Copepoda, Cirripedia, Malacostraca. The latter subclass consists of two "series": Leptostraca (order: Nebaliacea) and Eumalacostraca, with four "divisions": Syncarida, Peracarida, Eucarida and Hoplocarida. The Syncarida consist of the order Anaspidacea (the remarkable, recently discovered genera *Anaspides, Paranaspides, Koonunga* and possibly *Bathynella,* the affinities of which have been worked out chiefly by Calman himself); the Peracarida contain the orders Mysidacea, Cumacea, Tanaidacea, Isopoda, Amphipoda; the Eucarida possess the orders Euphausiacea and Decapoda, and the Hoplocarida the order Stomatopoda. This arrangement surely represents the natural affinities better than any of the older systems. Of course, it is impossible, in a review, to give a full account of the morphological facts, which substantiate the views of the author, but these facts are prop-