

Kansas State Agricultural College and Experiment Station, has resigned to become state entomologist of New Jersey, succeeding the late John B. Smith. In Dr. Headlee's place at the Kansas College and Experiment Station, Geo. A. Dean, M.S., has been placed in charge of entomology and Robert K. Nabours, Ph.D. (Chicago), in charge of zoology. Further promotions and additions in the department have been as follows: John W. Scott, Ph.D. (Chicago), has been promoted from instructor to assistant professor of zoology; Maurice C. Tanquary, Ph.D. (Illinois), has been appointed instructor in entomology, and Mary T. Harmon, Ph.D. (Indiana), in zoology and J. W. McColloch has been appointed assistant entomologist.

DR. C. J. STEINMETZ, formerly managing editor of *Country Life in America*, has been appointed assistant professor of landscape horticulture at the University of Illinois, and Ralph Rodney Root, of Harvard University, has been appointed instructor. A number of prominent specialists in landscape gardening will lecture before the students this year; Mr. Charles Mulford Robinson, a specialist in city planning, will lecture for two weeks beginning on November 8. There are thirty students in the four-year course in landscape gardening and one hundred and fifty in the elementary course.

THE vacancy in the staff of the mechanical engineering department of Lehigh University, due to the death of Assistant Professor E. L. Jones, has been filled by the appointment of R. L. Spencer, B.S. Mr. Spencer is a graduate of the Iowa State College, where he has taught for three years.

BARTGIS McGLONE, Ph.D. (Hopkins, '07), has been appointed associate in physiology and embryology at the College of Physicians and Surgeons, Baltimore.

AMONG the committees appointed by the board of overseers of Harvard University for the year 1912-13 are the following:

The Medical and Dental Schools—J. Collins Warren, George B. Shattuck, Charles W. Eliot, Alexander Cochrane, William Sturgis Bigelow,

Henry H. Sprague, Henry Saltonstall Howe, William L. Richardson, Charles P. Briggs, James C. White, Charles H. Tweed.

The Bussey Institution—Carroll Dunham, Walter C. Baylies, J. Arthur Beebe, John Lowell, Nathaniel T. Kidder, Augustin H. Parker, William H. Ruddick, Isaac S. Whiting, Simon Flexner, Daniel W. Field, Warren A. Reed.

The Observatory—Joel H. Metcalf, George I. Alden, Mrs. Henry Draper, Edwin Ginn, George R. Agassiz, Elihu Thomson, Erasmus D. Leavitt, Charles F. Choate, Jr., Charles R. Cross.

The Museum of Comparative Zoology—J. Collins Warren, George P. Gardner, Dudley L. Pickman, Rodolphe L. Agassiz, John C. Phillips, J. B. Henderson, Jr., Louis J. de Milhau.

The Peabody Museum—George D. Markham, Charles P. Bowditch, Augustus Hemenway, Jesse W. Fewkes, Clarence J. Blake, Clarence B. Moore, Elliot C. Lee, Louis J. de Milhau, John C. Phillips, Thomas Barbour, Robert G. Fuller.

The Jefferson Physical Laboratory and Department of Physics—Howard Elliott, Elihu Thomson, Erasmus D. Leavitt, Elliot C. Lee, Samuel Hill, Hammond Vinton Hayes.

The Chemical Laboratory—J. Collins Warren, Clifford Richardson, Elihu Thomson, Charles H. W. Foster, John D. Pennock, Alexander Forbes.

On Geology, Mineralogy and Petrography—George B. Leighton, Rodolphe L. Agassiz, George P. Gardner, William E. C. Eustis, Raphael Pumpelly, William Sturgis Bigelow.

On Zoology—William L. Richardson, Augustus Hemenway, William Brewster, Alexander Forbes, John E. Thayer, Dudley L. Pickman, Francis N. Balch, John C. Phillips.

On Botany—Nathaniel C. Nash, George G. Kennedy, Walter Deane, Edward L. Rand.

On Mathematics—William Lowell Putnam, George E. Roosevelt, George V. Leverett, Philip Stockton.

DISCUSSION AND CORRESPONDENCE

THE MEANING OF DRIESCH AND THE MEANING OF VITALISM

PROFESSOR JENNINGS's letter in *SCIENCE* of October 4, 1912, contains some comments on an article by the present writer, published in *SCIENCE*, July 21, 1911. These appear to manifest some misapprehension, confirmed by some inadvertent misquotation, of the article in

question; and to convey, accordingly, an erroneous impression both as to what was said, and as to what is the fact, concerning Professor Driesch's view of the relation of vitalism to indeterminism.

With respect to the article upon which he animadverts, Jennings declares or plainly implies: (1) that it purports to be an account of Driesch's personal views concerning the relation of vitalism to "experimental indeterminism," but that what it gives "is in reality an exposition of the conclusions which Lovejoy himself might draw from Driesch's data, assuming these to be the conclusions which Driesch draws"; (2) that in consequence of this confusion the article erroneously maintained that Driesch is not an "experimental indeterminist." Both these assertions require correction.

1. The article expressly distinguished between Driesch's actual views as a whole, and the conclusions which I regard as properly inferrible from a single one—though the most emphasized and most characteristic one—of his arguments. For the exposition of the former I disclaimed responsibility, remarking that I did "not wish to complicate the discussion with exegetical inquiries into the precise meaning of a rather difficult writer." My discussion was explicitly limited to the morphogenetic data brought together in "The Science and Philosophy of the Organism," to the exclusion of the arguments from animal behavior, which are more markedly indeterministic in their tendency. I endeavored to point out the real "conclusions suggested by Driesch's analysis of what is implied by the totipotency of parts," etc., to show "all that it logically *need* imply"; and the reader was definitely informed that these logically necessary implications of Driesch's premises fall short of the conclusions which he at times deems himself entitled to draw.

I do not say that Driesch himself clearly and consistently adheres to this assumption [*i. e.*, that his entelechies, supposing them to exist, act in a uniform manner and in correlation with specific physico-chemical complexes]; but in so far as he

departs from it and gives color to the charge of indeterminism, he introduces a foreign element into his conception of a "harmonious equipotential system," and confounds the second sort of vitalism with yet a third essentially distinct one [*i. e.*, with experimental indeterminism]. And this is one of the confusions which it is needful to guard against in the discussion (p. 78).

The reader of Jennings's recent letter would certainly gather that I had failed to make this distinction, and would never guess that the article under discussion contained such a passage as that just cited. Jennings, in fact, takes from the article sentences referring to what I urged were the only proper inferences from Driesch's premises, divorces these sentences from their context, and cites them as evidences of my misconception of the actual and total position personally held by Driesch. He quotes, for example, the phrase "a closer scrutiny of the doctrine's implications," etc.; the "doctrine" here referred to is *not*, as he assumes, Driesch's entire system of vitalism, but a more limited doctrine, formally defined in the preceding paragraph.¹ In two other cases Jennings cites disconnected sentences and assigns the demonstrative pronouns in them to antecedents other than those intended.

2. It is, however, true that two passages in the article referred directly to Driesch's actual position. One of these, already quoted, consisted in the admission that Driesch in fact, though without warrant from his premises, at times construes his vitalism as equivalent to experimental indeterminism. The other was an *obiter dictum*: "though I think Jennings misconceives Driesch's position in ascribing to him a wholesale 'experimental indeterminism,' I do not wish," etc. Against this Professor Jennings now quotes letters from Professor Driesch in which the latter frankly calls himself an experimental indeterminist. Since I had elsewhere in the article noted that he

¹ It was to this kind of vitalism, as defined in my earlier paper—"the second kind of vitalism distinguished by Lovejoy"—as well as to Driesch's personal doctrine, that Jennings in his previous article imputed indeterministic implications (SCIENCE, June 16, 1911, pp. 927-28).

was such in some sense and to some degree, I should have supposed that Professor Jennings would have given consideration, in reading this phrase, to the qualifying adjective "wholesale." By a "wholesale indeterminism" I intended to designate precisely that extreme doctrine which Jennings in his paper had apparently ascribed to the author of "The Science and Philosophy of the Organism." That doctrine Jennings had formulated as follows (*italics mine*):

All living things are complexes of great numbers of chemicals so that *the conditions under which entelechy comes into play are always realized*. We may therefore *expect its action at every step in our work*; we must be prepared *at all times to find the same physical configuration giving rise now to one result and now to another*. (SCIENCE, June 16, 1911, p. 932.)

Such a view would mean that, in organisms, not merely behavior but also all morphogenetic and psychological processes would be absolutely variable and unpredictable, that no amount of past experience of vital phenomena would justify even the slightest anticipation of any uniformity in their future sequences. This doctrine, if accepted, would, as Jennings rightly points out, make biology as a science impossible and compel us to regard biological investigators as engaged in a "hopeless task" (*ibid.*). If Driesch adheres to this "wholesale experimental indeterminism," and takes this extreme view of the impossibility of generalization and prediction in biology, I must frankly confess that I had *not* gathered the fact from his Gifford lectures. And I must add that I even yet remain unconvinced that he does so. If he does, he ought in consistency to lead a movement for the suppression of physiological laboratories. I am strengthened in my disbelief that Driesch cherishes any such fell designs against the happiness of experimental investigators in biology by the fact that another letter of his to Professor Jennings—which the latter does not quote, but which he has kindly permitted me to see—contains the following words:

Practically, we may say that complete knowledge of the physico-chemical constitution of a

given egg in a given state and of the behavior following this constitution in one case, implies the same knowledge for other cases (in the same species) with very great probability. But this is a probability *in principle* and can never be more. It would not even be a probability, in the case that we did not know the origin (or history) of a given egg in a given state, viz., that the egg is the egg of, say, an ascidian. But to know this history or origin *is, of course, already more than simply to know "the physico-chemical constitution"* and its consequences in one case (what suffices in the realm of the unorganic). It may be that the eggs of fishes, echinides and birds are the same in all *essentials* of the physico-chemical constitution.² There happens something very different in the different cases on account of the different "entelechies." In spite of this, we know what will happen with great probability from one case if we know that this egg "comes from a bird" and that the other "comes from an echinid." . . . Therefore, *practically*, "experimental indeterminism" is not a great danger for science. [*Italics in the original.*]

This appears to me to be a tolerably pertinent passage, which might well have been included among Jennings's selections from his correspondence with Driesch. It seems equivalent to a statement that the sort of indeterminism which Driesch professes is virtually negligible, so far as the every-day, practical purposes of the experimentalist are concerned. If Jennings had considered this passage in connection with the others which he quotes, he would not, I am sure, have contended that "Dr. Driesch's statements of the matter are fully as strong" as his own: they obviously fall very far short of his own. The experimental indeterminism in them is not at all of the "wholesale" sort.³ Possibly Jennings holds

² The reader will observe that this particular proposition Driesch gives as merely possibly true. It has, in fact, no sort of logical connection with his arguments from morphogenesis and restitution. Not only do those arguments not prove this conclusion, they do not even suggest it.

³ In published writings Driesch uses language which seems to express a yet more definite repudiation of wholesale experimental indeterminism. Thus in *Die Biologie als selbständige Grundwis-*

that one who admits that there is any "experimental" indeterminateness in any organic process can not consistently stop short of the extreme view he has himself defined. But he has scarcely proven this; and in any case, if he imputes the acceptance of this view to Driesch, he is identifying the conclusions which he himself might draw from certain of Driesch's positions (if he held them) with the conclusions which Driesch draws.

I am afraid the foregoing shows that Professor Jennings has, after all, succeeded in luring me into "exegetical inquiries into the precise meaning of a rather difficult writer." However interesting these may be, there are other questions in which, I confess, my interest is more acute—as, no doubt, Professor Jennings's really is also. Among these is the question: What do the data chiefly emphasized by Driesch *really* tend to prove about organisms? On this, which was the principal theme of my previous communication on the subject in SCIENCE, Professor Jennings's recent letter has little to say. Yet I think that his letter leaves the matter in a not wholly satisfactory logical condition; and that there is a good deal more which might with advantage be said, in the interest of a full clearing up of this genuinely significant issue. But that undertaking, to which I hope before long to attempt to contribute elsewhere, would call for a lengthier disquisition than would be suitable for publication in this journal.

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October 15, 1912

WINTER WEATHER IN FLORIDA

UNDER the above caption in SCIENCE for May 31, 1912, Mr. Andrew H. Palmer submitted some observations on Florida weather. The winter of 1911-12, in Florida, was by no means severe, but the temperature averaged low during January and February, as compared with the normal, the monthly departures during the winter months being: December, $+5^{\circ}.1$; January, $-0^{\circ}.6$, and February, $-4^{\circ}.6$.

Mr. Palmer's statement that "Florida's climate did not receive careful attention until large numbers of settlers were attracted by the recent land-boom," is rather gratuitous. For forty years the weather bureau records of Florida have been consulted by people of broad intelligence in their search for truth, regarding the climatology of the state. With regard to the statement: "In all but eight of the last seventy years freezing temperatures have occurred in Jacksonville," a few supplementary facts are essential to a correct understanding. Mr. Palmer's figures were correctly copied from "Climatology of the U. S.," but included in that report were miscellaneous records that antedate those of the weather bureau, and, though given official cognizance to the extent of publication, yet, the official life of local weather bureau data begins with the establishment of a station in Jacksonville in 1871. The records previous to 1871 were mostly by voluntary observers, and they are not recognized as coordinate in importance with those compiled under official supervision during subsequent years; hence, to a certain extent, they are taken *cum grano salis*. A freezing temperature in Jacksonville is not followed, necessarily, by similar conditions in the citrus belt for Jacksonville sustains, approximately, the same relation to the rest of the state as Sacramento, California, does to the San Diego section.

The above qualifications are pertinent also in the matter of snowfall in Florida. During the severe blizzard of February, 1899, snow fell over the extreme northern portion of the State to the depth of several inches; that is, over an area of slightly more than 1° in latitude. This was the heaviest snow fall in Florida of which there is authentic record, and it is believed to be an expression of maximum intensity along that line. Certainly it was not exceeded during the century.