the nerves issuing from the brain, which require careful reexamination at leisure, and passing over as not worthy of comment the *lapsus calami* on page 235 to which he calls attention, and the importance of which he magnifies, as it is abundantly corrected elsewhere, I wish to protest against the misrepresentations contained in his article, founded upon a total failure to understand my meaning and attitude.

Dr. Hay labors to make it appear that I suggest that *Diplodocus* was an animal "with three pairs of nostrils." I never suggested such a thought and no fair interpretation of my words could lead to it. In my paper I simply called attention to the obvious fact that two openings in the bones of the skull apparently leading into the narial cavity occur on either side in advance of the large posterior opening, which Professor Marsh interpreted as the nasal aperture. *They are there!* Dr. Hay is at liberty to amuse himself at his leisure in endeavoring to explain them as he pleases. I did say of the foramina, which I named "the mesial foramina of the maxilla," that they might possibly have had "a function supplementary to the function of the true narial opening." This does not necessarily imply that they were nostrils opening from the nasal cavity into the outer air. Whether they were nares, or were covered by tegument in life, I did not venture to say. It is quite conceivable that the large opening which Professor Marsh has interpreted as the true narial opening may have been covered by tegument and that one or the other of the smaller pairs of anterior openings may have been the true functional nares, as was long ago suggested, if I remember correctly, by the late Dr. Baur. In attempting to make me to have suggested that *Diplodocus* had three pairs of functional nares Dr. Hay is traveling quite beyond my text, and this "fanciful proposition," as he is pleased to call it, is the creature of his own imagination. I object to have him thus misinterpret me.

Dr. Hay devotes a paragraph to a foot-note on page 245 which he does not quote, but which he garbles. This foot-note is as follows: "*Sphenodon* has no external ear, agreeing in this respect with many other recent reptilia and ophidia. It is possible that *Diplodocus* had no external ear." I might have omitted the word "many," and have written the word "probable" instead of the word "possible," but then I do not claim omniscience. Omniscience is not one of my fads; and besides I know, as Dr. Hay should also know, that we have in some of the batrachia and lacertilia folds of skin partially covering the tympanum, suggesting and apparently to a certain extent functioning as rudimentary outer ears, and that in the crocodilia there is provided an opercular flap, which distinctly functions as an outer ear. I think my temperate statement may stand as I left it. It does not imply, as Dr. Hay tries to twist it into implying, that I held the ridiculous opinion that there exist reptilia with outer ears fully developed, as for instance, in the mammalia. Dr. Hay in his article is evidently making an attempt to be "funny." He ought first to be sure that he understands what he is talking about.

W. J. HOLLAND

CARNEGIE MUSEUM,
PITTSBURG, PA.,
October 20, 1908

ON THE ENCOURAGEMENT OF MR. CHARLES D. SNYDER

IN a recent paper[1] Charles D. Snyder has published the following statement:

> If we believe that any given physiological activity is due to some particular physical change, we need only to determine at which velocities the action proceeds under various temperatures and then compare these results with the velocities of (probable) physical processes under similar changes of temperature in order to test for ourselves the correctness of our view.

He here refers to a foot-note which reads as follows:

> See the author's *original* communication, *Archiv für Anatomie und Physiologie, Physiol. Abh.*, 1907, p. 113. In this paper the idea of comparing temperature coefficients for possible physical causes underlying physiological actions, as outlined above, was clearly expressed. It was

[1] Snyder, Chas. D., *Am. Jour. Physiol.*, Vol. 22, No. 3, p. 309, August, 1908.

clearly stated in the abstract of the present paper, as published first at the Congress in Heidelberg, August, 1907, and later in the proceedings of the same which appeared in the various journals and archives of physiology during the fall of the same year. Since that time it is encouraging to note that J. Loeb (*Journal of Biological Chemistry*, October, 1907) and J. Bernstein (*Pflüger's Archiv*, 1908, CXXII., p. 129) have both thought well enough of the idea to use it as a basis for investigations in their own laboratories.

The italics in the above quotation are inserted by the writers of this note, since the word italicized renders the sentence in which it occurs a deliberate misstatement. Mr. Snyder's original communication, to which, through some curious oversight, he omits to refer either in this paper or in his paper in the *Zentralblatt für Physiologie* (22: 1908, s236), appeared in the University of California publications in 1905.

We think it advisable to draw attention to the fact, doubtless forgotten by Mr. Snyder in the stress of scientific production, because the statements quoted above would lead the reader, unacquainted with the history of this phase of physiological investigation, to suppose that members of this laboratory had utilized Mr. Snyder's results and views without giving him the full credit which he deserves. The true situation is, however, exactly the reverse, and it is for the purpose of removing the stigma which has thus been placed by Mr. Snyder upon the workers in this laboratory, that this short note is written.

It can not fail to strike an observant reader of Mr. Snyder's paper as very curious that he does not in his "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time" refer to the paper by S. S. Maxwell entitled "Is the Conduction of the Nerve Impulse a Chemical or a Physical Process?" published in the *Journal of Biological Chemistry*, October, 1907, and this fact will appear the more strange when the reader observes that in the foot-note quoted above, on page 310 of Snyder's paper, this very publication is alluded to as one of the investigations from the laboratory of J. Loeb, which "it is encouraging to note" has been based upon the ideas of Mr. Snyder. From the wording of this footnote it would appear that Maxwell overlooked Snyder's publications and, in particular, omitted to refer to his publication upon the transmission of the nerve impulse published in April, 1907. So far is this from being the case that a considerable proportion of Maxwell's publication was devoted to a criticism of this publication of Snyder's. The most damaging criticism was, however, omitted from Maxwell's paper out of regard for Mr. Snyder. Should Mr. Snyder desire it, however, that criticism can still be published.

The actual succession of events which led to the appearance of this series of publications was, as far as this laboratory is concerned, as follows:

In 1903 Martin H. Fischer, at that time a member of this department, translated Cohen's "Physical Chemistry." In the fourth chapter of this work Cohen reviewed the work of Arrhenius and van't Hoff upon the temperature coefficients of chemical reactions, and showed that the temperature coefficient of various life-processes was that of a chemical reaction-velocity. He also suggested that the same principle might be found to apply in many other physiological processes.

The ideas thus expressed by Cohen led Loeb to consider it advisable that a series of investigations upon the temperature-coefficients of life-processes should be undertaken, with the view of ascertaining whether these processes are determined primarily by physical or chemical agencies.[2] For this reason he assigned to Mr. Snyder, at that time a candidate for the degree of Ph.D., the problem of ascertaining the temperature-coefficient of the heart-beat, and urged him to present his results as his thesis for the degree. This origin of his investigation is very clearly stated by Mr. Snyder in his own words, as follows:[3]

> In his book on "Brain Physiology," Loeb mentioned the possibility that the heart beat might be caused by a fermentative process which is going

[2] Cf. Loeb, *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 3.

[3] *Univ. of Calif. Publ., Physiol.*, 2, 1905, p. 126.

on constantly in the heart. He suggested to me that this idea could be tested by exact quantitative determinations of the influence of temperature upon the rate of the heart beat. We know through Arrhenius how the velocity of a chemical reaction varies with the temperature. If the heart beat were, as supposed, a function of a reaction velocity, then the law of Arrhenius should also hold for the influence of temperature upon the rate of the heart beat.

While Mr. Snyder was preparing his thesis for the degree of Ph.D., Loeb suggested to Maxwell that he should undertake the investigation of the influence of temperature upon the velocity of the nervous impulse: Maxwell, however, started work upon another problem, and consequently Loeb asked Burnett to undertake this investigation. Burnett's investigation had not proceeded far when he found it advantageous to study the influence of temperature upon the latent period of striated muscle. The result of this investigation was published early in 1906.[4] Afterwards, on account of pressure of other work, Burnett was unable to continue the investigation, and, at Loeb's request, Maxwell carried it through, and the results were embodied in the paper to which Mr. Snyder refers, in the foot-note quoted above, as an encouraging example of the extent to which his ideas have been appropriated by members of this laboratory. Meanwhile Loeb had published his observations upon the temperature coefficient of artificial maturation in *Lottia*,[5] and of the production of artificial parthenogenesis by the action of hypertonic sea water,[6] and Robertson had published his observations upon the temperature coefficient of the heart beat in the crustacean *Daphnia*.[7] In his "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time," however, Mr. Snyder does not refer to any of these papers, and, with remarkable consistency, also fails to refer to all papers upon the temperature coefficients of life processes which contain extensive references to these publications, as, for example, the paper on the influence of temperature upon the refractory period in the sartorius muscle of the frog, published by Bazett in the *Journal of Physiology*, February, 1908. Mr. Snyder is, however, undoubtedly familiar with some of these publications, inasmuch as he has inserted references to that of Loeb upon the temperature coefficient of artificial maturation and to that of Robertson upon the temperature coefficient of the heart beat in *Daphnia*, in a previous publication of his own.[8]

Since Mr. Snyder's "conspectus of the temperature coefficients of the velocities of all the physiological actions determined up to the present time" is singularly defective in other respects, we here insert a list of the literature on this subject which, to the best of our knowledge is complete up to the present date. Save those containing results originally utilized by Cohen in calculating the temperature coefficients of life processes published in his "Physical Chemistry," all references to papers not actually regarding the influence of temperature upon life phenomena from the point of view of its influence upon chemical reaction velocity, are omitted. We publish this list in the hope, not only that it may be of use to the student of this department of physiological research, but also that it may serve to further encourage Mr. Snyder by demonstrating to him how widely his ideas have been adopted, how many laboratories have thought well enough of them to use them as a basis for investigations and in how many cases these ideas were, with remarkable prescience, utilized before Mr. Snyder had ever published his "original communication,"[9] in which they were so "clearly expressed":

Clausen: *Landwirtsch. Jahrb.*, 19, 1890, p. 892.

Hertwig, O.: *Arch. f. Mikrosk. Anat.*, 51, 1898, p. 319.

Cohen, Ernst: "Physical Chemistry," trans. by Fischer, New York, 1903, pp. 50–67.

[4] *Journal of Biological Chemistry*, 2, 1906, p. 195.

[5] *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 1.

[6] *Loc. cit.*, p. 40.

[7] *Biol. Bulletin*, 10, 1906, p. 242.

[8] *Amer. Jour. Phys.*, 17, 1906, p. 350.

[9] *Arch. für Anat. und Physiol., Physiol. Abh.*, 1907, p. 113.

Aberson: *Rec. d. traver. chim. d. Pays-Bas et de la Belg.*, 22, 1903, p. 78.
Herzog: *Zeit. f. Physiol. Chem.*, 37, 1903, pp. 149 and 396; *Zeit. f. Electrochemie*, 11, 1905, p. 820.
Matthaei, G.: *Phil. Trans. Roy. Soc. London*, 197 B, 1904, p. 47.
Snyder, C. D.: *Univ. of Calif. Publ. Physiol.*, 2, 1905, p. 125; *Am. Jour. Physiol.*, 17, 1906, p. 350; *Arch. f. Physiol.* (Engelmann), 1907, p. 113; 1907, p. 118; *Am. Jour. Physiol.*, 22, 1908, p. 179; 22, 1908, p. 309; *Zentralblatt f. Physiol.*, 22, 1908, p. 236.
Peter, Karl: *Arch. f. Entwicklungsmechanik*, 20, 1905, p. 130.
Abegg: *Zeit. f. Electrochemie*, 11, 1905, pp. 528 and 823.
Euler: *Zeit. f. Physiol. Chem.*, 44, 1905, p. 53.
Senter: *Jour. Physical Chem.*, 9, 1905, p. 311.
Loeb, J.: *Univ. of Calif. Publ., Physiol.*, 3, 1905, p. 1; 3, 1906, p. 39; "Dynamics of Living Matter," New York, 1906, lecture 6; *Pflüger's Arch.*, 124, 1908, p. 411.
Kanitz, A.: *Zeit. f. Electrochemie*, 11, 1905, heft 42; 13, 1907, p. 707; *Pflüger's Arch.*, 118, 1907, p. 604; *Biol. Centralblatt*, 27, 1907, p. 11.
Robertson, T. Brailsford: *Biol. Bull.*, 10, 1906, p. 242; *Arch. Internat. de Physiol.*, 6, 1908, p. 388.
Taylor, A. E.: *Jour. Biol. Chem.*, 2, 1906, p. 87.
Jost: *Biol. Centralblatt*, 26, 1906, p. 225.
Burnett, T. C.: *Jour. Biol. Chem.*, 2, 1906, p. 195.
Madsen and Nyman: *Zeit. f. Hygiene und Infektionskrank.*, 57, 1907, p. 388.
Maxwell, S. S.: *Jour. Biol. Chem.*, 3, 1907, p. 359.
Arrhenius, S.: "Immunochemistry," New York, 1907, chap. 3 and 4.
Bazett, H. C.: *Jour. Physiol.*, 36, 1908, p. 414.
Lucas, Keith: *Jour. Physiol.*, 37, 1908, p. 112.
W. J. Woolley: *Jour. Physiol.*, 37, 1908, p. 122.
Bernstein, J.: *Pflüger's Arch.*, 122, 1908, p. 129.

Jacques Loeb, T. Brailsford Robertson, S. S. Maxwell, Theo. C. Burnett
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QUOTATIONS

PUBLICATIONS OF THE WISTAR INSTITUTE

The publication of five important biological journals under the direct control of the Wistar Institute of Anatomy of the University of Pennsylvania has attracted widespread attention among anatomists and zoologists of the country. The step is significant, not so much because the institute has acquired five well-established biological journals, but because it marks an important advance in the cooperation, so much talked about recently, among institutions which consider it their duty to devote some of their best energies to the advancement of human knowledge.

The Wistar Institute began its career as a publishing institution by the distribution of Bulletin No. 1, which was a three-page leaflet setting forth some of the plans the institute proposed to follow in promoting anatomical science. This was in 1905. At the close of 1908 the institute is publishing five journals, with a combined yearly output of about 3,000 pages. Several of these journals are self-supporting, while others incur a considerable yearly deficit. The institute has been able to assume the entire financial responsibility of these publications, without encroaching upon its regular income devoted to its museum and research work, through the efforts and enthusiasm of Dr. Horace Jayne. Doctor Jayne, who is in charge of the newly created department of publication, has done much to improve the journals, increase the subscription lists and has been untiring in his efforts to put the department upon a successful working basis. No similar combination of biological journals has ever been attempted in this country, and the Wistar Institute is to be congratulated upon the success attained in this new venture.

The first journal acquired by the Wistar Institute was the *Journal of Morphology*. Founded in 1887 by Professor C. O. Whitman,