ment of Vanderbilt Square, now occupied by the two Vanderbilt dormitories, is announced.

MR. G. H. KENRICK, Lord Mayor of Birmingham, has made a gift of £10,000 towards the funds of Birmingham University. This is his third contribution toward the development of the university, his total gifts amounting to a sum of £25,000.

THE Oakland *Tribune,* as quoted in the Boston *Transcript,* says that President Benjamin Ide Wheeler, of the University of California, is at Ann Arbor and may accept the presidency of the University of Michigan, to succeed Dr. Angell, who wishes to retire. Dr. Wheeler's ten-year contract with California will expire on January 1. His salary at Berkeley is $10,000, whereas it is said that Michigan has offered him $15,000.

BY vote of the corporation Harvard University will remit the regular tuition fees in all its departments for any students, not exceeding five in any one year, who shall be accredited by the Prussian ministry of education as students qualified to pursue advanced studies.

DR. ARTHUR L. DEAN has been appointed instructor in industrial chemistry in the Sheffield Scientific School, of Yale University.

DR. GEORGE DEAN, chief bacteriologist at the Lister Institute of Preventive Medicine, has been appointed to succeed Professor D. J. Hamilton in the chair of pathology in the University of Aberdeen.

DISCUSSION AND CORRESPONDENCE

A REPLY TO THE COMMUNICATION OF MESSRS. LOEB, MAXWELL, BURNETT AND ROBERTSON[1]

THE idea of using temperature coefficients for the analysis of living processes developed in two distinct stages. The first stage was the thought of employing the method whenever chemical reaction was supposed to be the primary cause of a living process. For, if the process were chemical then its velocity must follow changes of temperature as does the velocity of chemical reaction. This was the Cohen-Loeb portion of the idea, as was clearly stated in the present writer's doctor's dissertation on the subject, from which Professor Loeb himself quotes in his search for evidence on his side, and as has been reiterated by him in his "Dynamics of Living Matter" (1906).

The second stage was the thought that if primary chemical action can be detected by comparing temperature coefficients—why then primary physical (non-chemical) action can also be detected by comparing temperature coefficients. This part of the idea was original with the writer, to the best of his knowledge, and was communicated by him as such in a letter to Professor Loeb from Berlin during the winter of 1906–7—a letter to which no reply was ever received. The idea was later published and received its first clear and unmistakable enunciation in April, 1907, in the *Archiv für Anat. und Physiologie,* Physiol. Abt., p. 113, in a paper entitled "Der Temperaturkoefficient der Geschwindigkeit der Nervenleitung."

At no previous time did Professor Loeb or any of his colleagues ever so much as hint to Snyder that they had grasped, to say nothing of having contemplated or having begun work along, this extended line of thought. All the work proceeding from their laboratory up to October, 1907, was, so far as the writer knew, a constant and unswerving effort to obtain chemical reaction temperature coefficients.

However, in a paper, which the writer has never been able to see, until the present writing, it would appear that Professor Loeb did have an inkling of a thought concerning the further application of temperature coefficient determinations. This paper is entitled, "On Chemical Methods by which the Eggs of a Mollusc, *Lottia gigantea,* can be Caused to become Mature."[2] Here the author says that he wanted to find out whether NaOH, by which he succeeded in removing the chorion (ovarian-membrane ?) of the eggs, had a "physical or chemical action."

As the title of that paper implies, he decided it was chemical action, for, in a single case the velocity of maturation was 105 at 18° and 315 at 8°.[3] But from the whole tenor of

[1] SCIENCE, November 6, 1908.

[2] Univ. of Calif. Public., Physiology, Vol. III., p. 1, 1905.

[3] *Loc. cit.,* p. 4.

the article on *Lottia,* and from the fact that nowhere else in the publications from his laboratory—not in Burnett's paper on latent period of cross-striped muscle[4] nor in Robertson's on heart rate in *Daphnia,*[5] nor in the "Dynamics of Living Matter," 1906—from the fact that nowhere else, before the appearance of Snyder's April, 1907, paper, do we find another indication, even so slight as that in the *Lottia* paper, that Professor Loeb had thought of searching directly for *physical* temperature coefficients, it is quite evident that this part of the idea had not yet fully developed and crystallized in his mind.

However that may be, the writer wishes to repeat that, until the present writing he has never had access to the paper on *Lottia,* and was ignorant of the particulars of its contents. He could only quote it indirectly from Robertson's reference,[6] as he did in the *Amer. Jour. of Physiol.,* XVII., p. 350. Robertson, it is to be noted, expressly states that Loeb found the chemical temperature coefficient to hold good in the artificial maturation of *Lottia* eggs, and says nothing about a search for *physical* (non-chemical) coefficients.

On the other hand, the burden of Snyder's letter to Loeb in the winter of 1906–7, and of the entire introduction to his paper, "Der Temperaturkoefficient der Geschwindigkeit der Nervenleitung" (April, 1907), and of the "Comparative Study of Temperature Coefficients,"[7] *was in every case a deliberate and direct search for physical, and not chemical, temperature coefficients.*

This search was a distinct departure from the idea of demonstrating that the 2–3 rule of chemical reaction velocity holds good in living processes, such as is the one and only object in Cohen's "Physical Chemistry," 1901; in "The Dynamics of Living Matter," 1906; in Arrhenius's "Immunochemie," Leipzig, 1907, and in all the papers on temperature velocities which came from the California laboratory up to October, 1907.

As to the short-comings in the "Comparative Study," as already indicated, that paper was ready to print in March, 1907. It was only received back from the Warren prize committee in the fall of 1907; was sent then to the *American Journal of Physiology* for publication, but was refused on account of the great number of tables (20–30 pages). It was likewise returned from the *Journal of Physiology,* Professor Langley saying that its historical part was unnecessarily long and its text, being a compilation of known experiments, rather than description of new, the paper did not come within the scope of his journal.

For that reason the historical part was curtailed as well as nearly all the tables of observations. In that form the paper was received for publication by the *American Journal of Physiology* and appeared there a year and a half after having been written. There was no attempt to bring the literature up to the date of publication. The results of Burnett, Robertson and Loeb were not incorporated because those papers were not accessible to the writer at the time of compiling the "Conspectus." The only attempt to mention later work was in that hasty and unfortunate foot-note, "It is encouraging to note," etc.

That reference was written with the feeling of having been complimented by one whom the writer had always thought of as friend and master—complimented in knowing that whatever his master meant by that paper of October, 1907, yet he too, thought well enough of his pupil's work to repeat it, and verify it.

But, unfortunately, Professor Loeb chose to give that foot-note, which the wording permitted, another interpretation.

And so it happens that the communication in Science for November 6, last, as the reader will readily see, hinges upon the old question as to the relation between teacher and student.

Had Snyder had a generous master that article would never have been written.

And now since the question is not a question of science at all, but one of ethics be-

[4] *Jour. of Biol. Chem.,* 1906, II., p. 195.

[5] *Biol. Bull.,* 1906, X., p. 242.

[6] *Loc. cit.,* p. 242.

[7] Warren Triennial Prize Contest, April, 1907; International Congress of Physiologists, Heidelberg, August, 1907; *Amer. Jour. of Physiology,* August 1, 1908.

tween physiologists and their students, the writer wishes to suggest that this dispute can best be settled by a competent committee of physiologists.

If there are enough persons really interested in the matter, such a committee can easily be appointed, say, by the American Association of Physiologists. The writer for his part would be quite willing to place his case in their hands and abide by their judgment.

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November 10, 1908

AN ECONOMICAL INSECT BOX

THE price of cork-lined insect boxes has always seemed to me to be unnecessarily high. As listed in the dealers' catalogues these glass-covered boxes cost from one to three dollars each, according to the size and finish.

It may be of interest to some of the readers of SCIENCE to know that a very satisfactory box may be obtained at about one third of the above price.

While corresponding, recently, with the Jesse Jones Paper Box Co., of Philadelphia, in regard to cardboard museum trays, I asked if they could furnish me with insect cases. The box that they finally made at my suggestion is extremely neat, and will, I believe, prove quite as durable as the wooden boxes that are now sold by the regular dealers in entomological supplies.

The boxes could, of course, be made of any size, but the ones I have are of the larger size, $14 \times 22 \times 2\frac{1}{2}$ inches. They are made of heavy "stock board," which is a very stiff pasteboard about one eighth of an inch in thickness. They are covered outside with black book-cloth, which has a very attractive appearance and does not scratch as does a polished wooden surface. The lid is hinged, and is of glass; it fits closely over a three-fourth inch shoulder. The inside of the box is lined with white glazed paper.

Instead of the expensive sheet cork, the bottom of the box is lined with corrugated paper, such as is used for wrapping glassware. This corrugated paper receives the pins almost as well as does cork, and costs nothing. It may either be put into the box before the glazed paper lining is introduced, probably the better way, or it may be covered with the glazed paper and fastened in with pins or glue, so that it may be removed and recovered when it becomes filled with pin holes. Being covered with white paper, this corrugated paper bottom looks as well as any other.

The only defect of the box is that the lid is not pest-proof. This defect may easily be remedied, when the box is filled, by sealing the lid with a strip of black passe-partout cloth, which will just match the book-cloth covering; the box will thus be made absolutely pest-proof, and there will be no danger of open cracks such as sometimes appear in wooden boxes on drying.

If it be necessary to keep the box unsealed, a moth-ball in each corner will keep out the few pests that might work their way under the lid.

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LIGHTS ATTRACTING INSECTS

TO THE EDITOR OF SCIENCE: I should like to inquire through the columns of SCIENCE whether any of your readers have had an opportunity of observing the relative efficiency of mercury vapor lights, flaming arc lights using sodium carbons, and ordinary arc lights in attracting insects, especially moths. My experience has led me to believe that an ordinary arc light is a very much stronger attraction to moths than an incandescent light with a carbon filament, even allowing for differences in candle power. It therefore occurred to me that it might be the rays in the blue end of the spectrum which attracted them most. Inside a room moths will always leave a sixteen-candle power incandescent light or a series of them to go to the window as soon as there is any daylight. In the evening they will go to the windows at the approach of twilight and will not leave them for incandescent lights in the room until it is