be recorded the carotid pulse, the respiration, the time in seconds and the rate of muscular movements. Experiments made with this apparatus show that the curve of carbon dioxide excretion during work closely resembles that of the pulse, and that carbon dioxide is at least in part the cause of the secondary rise in the pulse rate observed by Bowen.

Dr. W. B. Pillsbury detailed some experiments on 'The Attention Wave as a Measure of Fatigue.' Not merely the daily rhythm of fatigue and practise of the typical morning and evening workers was reflected in the ratios of the period of visibility to the period of invisibility in the attention wave, but the degree of fatigue on days of severe work as compared with easy days had a corresponding variation in the fluctuation of attention. In the morning, practise shows itself in a continuous increase in efficiency through at least a considerable portion of the experiment; while in the evening there is a decreasing effectiveness almost from the beginning. As further substantiation of the theory that the attention wave is closely related to the Traube-Hering or Mayer vaso-motor waves, it was noted that both have the same daily rhythm of length.

Frederick C. Newcombe,
Secretary.

Discussion and Correspondence.

Morgan on Evolution and Adaptation.

To the Editor of Science: I have always supposed that what are generally called Lamarckian views of evolution were considered with less prejudice by biologists in the United States than in England or Europe, and that my own publications in support of such views were, therefore, likely to be known and read in America even if they were almost completely ignored by my own countrymen.

I find, however, that Dr. Thomas Hunt Morgan in his book 'Evolution and Adaptation,' which has just appeared, makes no mention whatever of my book 'Sexual Dimorphism in the Animal Kingdom, a Theory of the Origin of Secondary Sexual Characters,' which was published in London more than three years ago. Any biologist, American or other, has a perfect right to reject all my conclusions, but it seems to me that an author who devotes a great part of his book to the discussion of Darwin's theory of sexual selection and the evolution of secondary sexual characters, in entire ignorance of the facts and arguments which it cost me years of labor to collect and elaborate, lays himself open to the charge of writing without proper knowledge of the literature of his subject. I have published the results of experimental work apart from this, but the only reference Dr. Morgan makes to it is to a popular article in *Natural Science;* he has not apparently consulted the original memoirs.

Like other English writers it has been my ambition that my work should be known to the scientific public of the United States, which is not only very intelligent but free from prejudices which are stronger than reason in England. I am much disappointed to find that my chief contribution to the investigation of evolution is so little known to American evolutionists.

J. T. Cunningham.

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Mutation and Selection.

In reading Professor Morgan's very interesting and valuable book, 'Evolution and Adaptation,' it is surprising to find that he apparently regards the theory of evolution by selection and DeVries's mutation theory as being to a degree in conflict.

The evolution which observation shows us has taken place is chiefly characterized by the fact that it has brought organisms into favorable relation with their environmental conditions. That this could have been secured by mutation unaided by selection seems altogether unlikely.

In the case of the leaf butterflies of the genus *Kallima* the theory of evolution by mutation alone must assume that the remarkable resemblance arose all at once by a single mutation, or that there were a series of mutations which for some unaccountable reason were of such a character as to make the resemblance to a leaf gradually grow more perfect, though no selective action of the environment controlled this improvement in pattern.