cultures brought from Woods Hole; and in the original culture, No. 5, a smaller or larger number of conjugating pairs were observed almost daily up to October 15, 1927.

The question presents itself as to the causes of the failure of Woodruff's strain to conjugate, in comparison with the apparent readiness to conjugate on the part of the Woods Hole strains. The differences may be attributed either (1) to the difference in the original habitat, (2) to different culture methods in the laboratory or (3) to inherent racial differences, or some combination of these. In regard to the first possibility, it will be remembered that Woodruff's strain came from a fresh-water source while the Woods Hole strains have come from brakish water. Experimental tests demonstrated that these brakish-water strains would live in an apparently normal condition in fresh water and in various strengths of sea water up to pure sea water, provided the changes to the higher strengths were made gradually. The brakish-water habitat may therefore be considered a normal one.

In regard to the second possibility, it may be pointed out here that we have subjected Woodruff's strain to the same cultural conditions that we used for the Woods Hole strains, but have not as yet been able to induce conjugation in this strain. The evidence at present available rather favors the third possibility—that of inherent racial differences.

Segregated strains are being established from exconjugants and it is hoped to make an intensive study of the conditions which will induce conjugation as well as to investigate thoroughly the cytological details of the process.

D. H. WENRICH,
C. C. WANG

ZOOLOGICAL LABORATORY,
UNIVERSITY OF PENNSYLVANIA

CENTRIFUGING FILTERABLE VIRUSES

I READ with interest the note by M. S. Marshall, entitled "Centrifuging Filterable Viruses," which appeared on page 219 in SCIENCE of September 2, 1927. There seems to be little doubt as to the accuracy of Marshall's computations, and it seems likely that his conclusions are correct for a pure virus in water. However, it should be pointed out that his conclusions do not hold for virus which is in the plant extract. The writer's studies show that the virus of tobacco mosaic can be concentrated by means of the supercentrifuge. These investigations were published in *The Journal of Agricultural Research,* vol. 35, pp. 13–38, July 1, 1927. It should be pointed out that the supercentrifuge has been used in this and in other laboratories for concentrating bacteria and other micro-organisms. See the article by C. Juday, in the *Transactions* of the Wisconsin Academy of Science, Arts and Letters, vol. 22, pp. 299–314. 1926.

The writer's studies indicate that physical and chemical treatments which cause coagulation and precipitation to take place in plant extracts, also cause or assist the virus to settle out of the extract. However, the relative advantages of the various treatments, and the exact relations between the virus particles and other particles which are precipitated out of the extracts, are not fully known. Some treatments are less desirable than others because they are toxic to the virus in varying degrees. Some treatments produce only very finely divided coagula which do not settle out on long standing. Frequently these are heavily charged with virus, and they can be removed almost completely by means of the supercentrifuge.

It should be emphasized that centrifuge methods are of unquestionable value in studies on the virus of tobacco mosaic, and thus far the writer has found the supercentrifuge to be one of the most useful pieces of apparatus in the laboratory.

H. H. MCKINNEY

CEREAL VIRUS DISEASE INVESTIGATIONS,
BUREAU OF PLANT INDUSTRY

COLORIMETRIC METHODS IN BIOLOGY

PAST discussions of colorimetry, in the pages of SCIENCE, and particularly the recent appeal by Irwin G. Priest for bibliographic references and reprints bearing on this subject, have emboldened me to call attention in your columns to a paper of my own, published during the past year. I refer to an article entitled "Linear and Colorimetric Measurements of Small Mammals," which appeared in *The Journal of Mammalogy,* vol. 8, no. 3, August, 1927, pp. 177–206.

I hope that this unseemly bit of self-advertising on my part will be condoned for the following reasons. The scope of the journal in which the paper was published would doubtless tend to conceal it from the view of many biologists who are not especially interested in the Mammalia. On the other hand, the methods therein described are doubtless applicable to a wide range of biological and even of inorganic objects.

The writer is far from wishing to pose as an expert on colorimetry, either practical or theoretical, but he has been dealing for many years with color differences in certain species of rodents, and has been obliged to treat these differences quantitatively. Since no recognized technique was available for the purpose, it was necessary to work this out through protracted experimentation. A type of instrument (the Ives Tint Photometer) was finally adopted, which was already in use for industrial purposes. Some further equipment was necessary, however, and

the details of the procedure were not finally standardized until the lapse of several years. I have been fortunate enough to have had the advice of several persons who are far better informed in this field than I am.

These remarks are offered in the hope that they may be of service to some of those who wish to treat the colors of biological objects in a quantitative way. It is not claimed that the procedure adopted by me records absolute values, or ones which are accurate enough for the exacting requirements of physics. But it is, so far as I know, the only practicable method yet offered for measuring individual and racial differences of color and shade in the pelages of mammals, and it renders possible the treatment of these characters according to familiar biometric procedure.

F. B. SUMNER

SCRIPPS INSTITUTION OF OCEANOGRAPHY

SPECIALIZATION AND COOPERATION IN SCIENTIFIC RESEARCH

PROFESSOR COMPTON'S interesting address delivered at Lehigh University and printed in SCIENCE of November 11, 1927, brings up pointedly the question as to whether the conception of mass production is to dominate the teaching of advanced physical science. Two remarkable statements on this subject have recently come, one from the Bishop of Ripon who pleaded for a ten-year holiday in research at the recent meeting of the British Association, and the other statement from Sir Ernest Rutherford, the president of the Royal Society, which was made at the opening of a new physics laboratory at the University of Bristol. "When I look back over the thirty years or more of my connection with research," says Sir Ernest, "I am conscious that I have always been looking for a breathing space when, for a few years, no advances of consequence would be made; when I should gain an opportunity for studying in more detail, at my leisure, the ground already won. Alas, that breathing space has never come, and I am sure will never come in my time."

A plea for limitation of the intensive methods of research which are at present advocated and practised in our universities is unpopular. But the intensive methods are bringing on very peculiar results. The quantity of "research" output has grown to be immense. It is not unlike the production of motor cars. The aim is not the very best, but something new and "salable." Anything that is good enough to hold its own is good enough to be produced as a piece of "research." An enormous amount of duplication has followed in the wake of this mass research. This is very noticeable in connection with the issue of electrical patents many of which are granted on devices which have been clearly described ten and twenty years previous to the date of the applications. Some very extraordinary examples might be cited in this connection besides the one referred to by Professor Compton.

The thesis work is growing more intricate and complex. In the majority of cases it must be difficult to secure proof of the accuracy of much of the work done. But it is new, or it appears new, and that suffices. Now, in the historical development of science we have the example of Darwin holding his manuscript of the Origin of Species for twenty years, checking and re-checking before publishing. It would not be difficult to name other examples though less noted. If this so-called research work were more carefully done, its rate of increase would slow down of its own accord. If scientific teaching was set into the proper historical relief, it would be more thoroughly done and there would be less of it. When we plead for the teaching of fundamentals it is this which we mean. It is not clear whether Professor Compton pleads for teaching the "easiest way" by following altogether the inclinations of the students. The kind of research work he refers to is a little like athletics and much of it can be done without a long systematic training but such training is the essence of science. If the test is to be utilitarian, science as we like to see it taught will depart. If the criterion is to be that taught and urged by such teachers as Huxley and Helmholtz, then it is the "truth" which is the standard. Constant care as to whether new work is worth publishing, whether previous workers have received the credit which their work deserves, whether experiments are accurately made, whether the theory is simplified to the point where it is something other than a cloud of symbols, such considerations would, of their very nature, slow down this high speed production and permit, to some extent, the realization of the hours of philosophic contemplation of the work already done for which Sir Ernest Rutherford makes his plea.

Mass production in science may prove to become as fatal as it is likely to become in industry, where it bids fair to be master instead of servant.

B. A. BEHREND

WELLESLEY HILLS, MASS.

A FUNCTION OF REGIONAL SCIENTIFIC SOCIETIES

ONE thing of prime importance to be stressed by regional groups of scientific workers which seems to have been entirely forgotten by the various secretaries of State Academies of Science as reported by Professor Segerblom, SCIENCE, October 16, is the incentive for young researchers which comes from being per-