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.

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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.

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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