definite in outline that their identity and number can be ascertained without question.

Jeffrey's divergent results from those of the large number of cytologists who have worked on *Drosophila melanogaster* must be ascribed to his inexperience in Drosophila technique. Judging from his figures, published in previous papers, he apparently mistakes certain cell inclusions which stain black with iron hematoxylin for chromosomes. This difficulty can be overcome when the Feulgen nuclear reaction, preceded by formol-alcohol-acetic acid fixation, is applied.

I wish to repeat that this account is not written in a controversial spirit, but is merely given as a point of information. I also fully realize that the geneticists could disprove Jeffrey's assumptions even better and more effectively than a cytologist, but I doubt whether any one of them would take the time to do so.

Since this article has been written, Guyénot and Naville have published a most thorough account of the spermatogenesis in *Drosophila melanogaster* in *La Cellule,* Vol. xxxix, No. 1, 1929. They also repeated my investigations on maturation divisions of the egg in which they agree entirely with my work. Their criticism of Jeffrey's work is almost a duplicate of mine given above.

ALFRED F. HUETTNER

DEPARTMENT OF BIOLOGY,
WASHINGTON SQUARE COLLEGE,
NEW YORK UNIVERSITY

THE "FERTILIZATION" MEMBRANE OF ECHINID OVA

IN SCIENCE[1] for October 11, 1929, Professor A. R. Moore contributes a note on "The Function of the Fertilization Membrane in the Development of the Larva of the Sea-urchin." Against his conclusion I wish to enter a protest.

I had supposed that even beginners in marine embryology realized that the "fertilization"—vitelline—membrane of echinid ova plays no rôle in development after its complete separation from the vitellus. Every such student knows that by centrifuging, pressure, and the like, uninseminated ova of the sea-urchin are easily deformed—can, for example, be pulled out into long strands—with a return to normal form. This is due to the elasticity of the closely adherent vitelline membrane which encloses the almost watery egg contents and which plays a rôle in the metabolism of the egg. After insemination not only does the membrane stand off from the egg; it becomes stiff, brittle and easily removable; it has changed chemically, as Harvey has shown, and it plays no part in the metabolism of the egg. Removal of the membrane (except by micro-dissection?) from the uninseminated egg is practically impossible. Its removal after insemination has been frequently accomplished and this without injury to the egg or impairment of development. Finally, every student of the living sea-urchin egg has doubtless observed its "hatching," *i.e.,* the escape of the swimming form through the ruptured membrane. What justification, then, has Moore for the conclusion in his note concerning the function of the vitelline membrane in development?

Perhaps Moore did not mean the vitelline ("fertilization") membrane. In that case he should have given his note a different title. If, on the other hand, he meant the hyaline plasma layer the statement in his conclusion is superfluous; here again, every student knows that the hyaline plasma layer is part of the developing egg.

In his experiments, Moore finds that after exposure to an isosmotic solution of urea (he does not give the pH of the solution) uninseminated eggs are capable of fertilization and development without the "formation" of either the "fertilization" or hyaline membrane. Obviously, this might mean simply that the preformed cortex which during and after membrane separation builds up the hyaline plasma layer is so injured by urea that the normal cortical changes underlying the separation of the vitelline membrane are abnormal. The result would then be not the failure to "form" hyaline plasma layer but the rapid disintegration of this layer after it "forms."

If Moore's interpretation of his experiment on the effect of urea—namely, that it inhibits formation of the hyaline plasma layer—be correct, then he has been most unfortunate both in the choice of his title and in the statement of his observations.

E. E. JUST

HOWARD UNIVERSITY

PEDOLOGY OR SOIL SCIENCE

IN reply to the comments of Dr. William A. Hamor in the January 17, 1930, issue of SCIENCE relative to the use of the term *pedology* to refer to soil science, attention should be called to the fact that pedology was first used by the Russian soil scientists in 1865, over thirty years before the child scientists adopted it. The latter, as Dr. Hamor notes, are using an incorrect spelling of the word. The term they should employ is *paedology* or *paidology.* In view of the prior use of pedology to refer to soil science and as the psychologists are using the word incorrectly and also because of the general acceptance and use of the term in Europe in place of soil science, the American Soil Survey Association at its annual meet-

[1] 70: 360.