made of the same individual sections, some of them still of great beauty, from which the original figures were drawn.

It is a pleasure to bear witness to the fine quality of these admirable preparations, to the exceptional precision with which they were described and figured and to the faithfulness of Werner and Winter's lithographic reproductions of the original drawings. The figures do not exaggerate the clearness of the preparations; they are not schematized; they represent accurately the facts as they were seen and in large measure may still be seen. No critical observer, I think, who closely studies these preparations could take seriously the naïve notion that the centrioles are merely random granules that happen to lie at or near the astral centers. The assumption that they are merely the coagulated central portions of the astral rays deserves more respectful consideration, but this too seems to me inadmissible in view of the fact that both in maturation and cleavage the centriole is double from the metaphase onwards, and that during the anaphases its halves are more or less widely separated at a time when the asters show no sign of duality, before they have begun to elongate at right angles to the spindle-axis and before the small daughter-asters of the telophase have appeared. Similar conditions are seen with equal clearness in the *Ascaris* spermatocytes, and in great numbers of cells.

In respect to the genetic relations of the central bodies, it must be plain to every observer that in these objects some kind of genetic continuity is maintained between the astral systems of successive divisions. From the first appearance of the polar asters in *Chaetopterus* down to their disappearance after the second polar division, and then again from the first appearance of the sperm-aster through all the operations of fertilization and the earlier cleavages, the new asters arise at each step within, or in close proximity to, the preceding ones. All points to the conclusion that, in these divisions at least, this relation is determined by the centrioles, which are handed on bodily from cell to cell and act as centers for the formation of new asters in each succeeding generation. In respect to all this, and much more, the *Chaetopterus* preparations show a remarkably close and detailed resemblance to the conditions figured and described by Coe in *Cerebratulus* and by Griffin in *Thalassema,* and they are in substantial agreement with the results of many other accurate observers of the same period, including Boveri, Meves, Heidenhain, Ballowitz, Kostanecki, MacFarland, Vejdovsky and others who contributed to the development of the classical view.

For the foregoing reasons I am convinced of the objective existence of the central bodies as normal components of the cell (I do not say of all cells) and of the correctness in principle of the conclusions concerning them drawn by Mead and his fellow workers in this field. Doubts concerning the centrioles have sometimes been caused by considerations relating to the technique of staining. It was long ago demonstrated by Boveri (1901) that so long as the centriole remains single its existence within the centrosome or aster is not susceptible of rigorous demonstration by the regressive methods of staining in iron-hematoxylin, for the apparent size of the centriole may be varied at will, down to seeming disappearance, by extracting the dye in successive degrees. But at certain stages, as above indicated, two centrioles are regularly found at a time when the centrosome or aster is still single—a condition explicable neither as a product of centripetal or concentric extraction (as Boveri also pointed out) nor of mere coagulation of the rays. As a third possibility, the centriole might be thought of as no more than a focus of centripetal condensation within the aster, having no sharply marked boundaries, yet capable of division as if it were an individualized body, and offering the aspect of such a body after centripetal extraction. Such a notion is hardly different in principle from the classical conception, but it introduces new and perhaps insurmountable difficulties, particularly in view of the fact (apparently well established) that in some cases the centriole may persist, as a double structure, in the absence of asters, during the whole interkinetic phase of the cell.

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CREDIT OR RESPONSIBILITY IN SCIENTIFIC PUBLICATION

MAY I venture a comment bearing on the question of credit for illustrations in connection with recent letters by Dr. Stiles and Dr. Mueller? Every one must acknowledge the justice of the criticisms made by both writers, but in neither case is there any practical method of securing real justice. Most illustrations of value enough to be frequently copied are the result of the combined effort of quite a number of individuals. A restoration of an extinct animal owes much of its value to the artist who drew it and likewise to the scientist who deduced from its skeleton the resemblances to and differences from existing animals that must serve the artist as his guide, who in many cases provided the artist with sketches or rough drawings to finish for publication. But no less it is the work of the skilled preparator who, guided by expert practical knowledge of osteology,

has spent months of labor in piecing and articulating the skeleton; and to the equally skilled field man who spent months in the bad-lands searching for it, and when found knew how to bring it all in in such condition that it could be reconstructed by the preparator. Who is to decide what meed of credit belongs in each instance to each man? And how about the great foundation or the generous donor whose money has paid for all this work? Most important contributions to science nowadays are composite work, of which the author's contribution is often only a minor part. Yet we continue to apply to them standards of professional ethics derived from a time when most research contributions were from start to finish the work of the author, usually on his spare time; and any minor aid from others was acknowledged.

All this discussion of credit omits the really important and essential reason why an author's name is attached to a published contribution or illustration. He is the party responsible for the statements, results and conclusions therein. That is the point with which the reader of the article is chiefly concerned. Scientific publications are printed and distributed primarily for the benefit of the reader, not to enhance the reputation of the writer. We are all apt to forget that, being all egoists in varying degree, but it is nevertheless true and should be kept in mind in discussing professional ethics. What concerns me when I am reading a scientific paper is how far I should accept its statements and conclusions, whose authority lies back of its illustrations—who is responsible. I don't care who made the drawings, so that they are certified as accurate by a reliable authority. Nor do I care who collected or prepared the specimens, so that my authority is responsible for stratigraphic and collecting data and reconstruction. As a side issue and a matter of personal acquaintance with the men concerned, these points may be of interest. But my prime concern is to get hold of as much authoritative, well-expressed information as possible in the field covered, and to size up just how far I can trust in its accuracy and thoroughness.

This I take it is the practical reason why an article or illustration in a scientific journal should be, as it usually is, credited to author A, even though most of the work was done by assistants B and C, artist D, preparator E and collectors F, G and so on. In an art magazine the artist would naturally be cited as author of the illustrations, as the reader would care very little about the scientific soundness. In a popular book or magazine they would usually be credited to the collaborator who had the biggest newspaper reputation. The late Mr. Carnegie is presenting a Diplodocus to Mexico. That is the aspect of the transaction that interests the public. But the Mexican scientist who studies that reconstruction regards Dr. Holland as his authority for its accuracy. I see no more reason why Professor Berry in copying a restoration of *Diatryma* should credit it to the artist instead of the authors than for the text-book in which it appears to be called McGraw-Hill's "Paleontology" instead of Berry's.

As to the "customary acknowledgments" in a scientific article, they may relieve the conscience of the author, but does any one really suppose that a line or two in the introduction which nobody reads does justice to the part played by a collaborator who often has contributed much more work than the author and sometimes more brains? His real profit lies in the fact that he has got his work and his ideas on record, accessible to all who are interested. That is what we all profess to be working for in scientific research. If personal credit is all that concerns us, let us drop the hypocrisy of pretending otherwise, or claiming that the world owes any special recognition or reward to our "unselfish efforts."

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GENETICS

I HAVE just received the third edition of Walter's "Genetics," of which it may be said that it is well written, comprehensive and essentially up to date. But it seems to me to start out with an extraordinary confusion of ideas. A diagram represents "the triangle of life," the three sides being heritage, environment and response. It is said that when this idea is applied to man, "there are theoretically at least a minimum of twenty-seven possible kinds of human beings, as shown in Fig. 2." Fig. 2 shows "The scale of success. A stands for high grade; M for mediocrity; Z for low grade." These grades are assumed to exist in respect to each side of the triangle, and to vary independently. Thus No. 4 has first-class heredity and environment, but exhibits Z response or conduct. No. 25 has Z heredity and environment but A conduct. Now it is certainly true that the springs of human personality are far too subtle to be completely "explained" in scientific terms, but from any point of view, the exhibit in Fig. 2 is contrary to reason and experience. Further confusion of ideas is shown in the definition: "Response, on the other hand, represents what the individual *does* with his heritage and environment. It is what may be described as the training or educational factor, most clearly demonstrable in the higher animal forms."

The book also reminds us of a serious dilemma which confronts teachers of genetics, or indeed of scientific subjects in general. The subject-matter has