during youth to acquiring special knowledge or skill in any line have had smaller families than those who have devoted less of their time in special preparation?

At the present time if a young man devotes his energies exclusively to preparation for future work until he has acquired a Doctor's degree from some institution of learning, he is not very likely to have reached that stage earlier than 25 years of age, and if he has been obliged to support himself or any others during that period, he cannot have become a specialist of note until a considerably greater age has been reached. This necessarily means that people who devote their time to preparation are not able to assume the responsibility of raising families of children. If such a man marries a woman approximately his own age, it is not at all likely that they will raise more than two or at most three children to maturity, and if any children are born to them who do not survive, the percentage must necessarily be somewhere about what is recorded in the report of Dr. Bush.

Obviously it is because the population at large, including those individuals who do not devote the major portion of their youth to special preparation, is able because of economic conditions to raise larger numbers of children to maturity, and to give them the proper heredity factors to enable some to become scientists and to provide the means for supporting them during the educational period, that the number of trained scientific people not only has been maintained but has been increased very greatly during the memory of the older persons now living. Admittedly, there are disadvantages to the postponement of the assumption of family duties which not only reduce the number of people able to acquire special training but also cause many who would like to acquire proficiency as scientific workers to fall by the wayside. It does not seem possible to recruit research workers chiefly from the descendants of other scientific research workers unless the means of support during the educational period is subsidized to such an extent as to permit students to assume family obligations of their own choosing. It is doubtful whether such a course would advance science materially, and it is not at all likely to be adopted by democratic communities.

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Anent "Blood Relationship"

The struggle to shape a proper terminology for use in social science is unending, and the difficulties to be surmounted rear greater obstacles than those confronting the physical scientist. Creation of a new terminology for a newly discovered phenomenon is a relatively easy matter for the physical scientist, who is penetrating new fields where no preconceived terminology has run ahead of him. He creates neologisms, usually from Latin or Greek roots, and more often than not with linguistically fantastic results. By those means he shapes a semantic tool that has a clear and sharp meaning for himself and his professional colleagues, and he little cares how it tortures the tongue of the layman or whether it has meaning to the uninitiate. Happily for the physical scientist, this merely increases his prestige and further awes the layman.

It is otherwise in the lot of the social scientist. His terminological struggles result from the attempt to bring clarity and precision into the language of human relationships—a body of phenomena with which men have been familiar for quite some period of time, if not scientifically informed thereon. Therefore, the layman looks with suspicion and sometimes with hostility toward the social scientist, who uses new terms to deal with what he, the layman, thinks are old and familiar facts. There must be something subversive in a movement that cloaks the "commonplace" in ununderstandable language. In this the layman is right, but not in the way he thinks he is. The purpose of the social scientist is to destroy the inaccuracies of thought which underlie inadequate popular conceptions in the field of social relations.

When the social scientist introduces a new term for an old, familiar, popular label, he does so because the popular term carries such a freight of error and cluttering emotional baggage that it is beyond easy salvage and should best be scuttled. "The meaning of a word lies in the action it produces."

During the recent war the American Red Cross felt it was necessary to segregate negro blood from white in "blood bank" collections and the processing of plasma. This was done, even though the Red Cross was itself aware of the fact that racial traits are carried in the germ plasm and not in the blood. It felt compelled to do so because of the popular belief that hereditary traits are "in the blood." This action of the Red Cross, as we all know, involved it in a tempest of controversy, aligned it with a false and undemocratic position, and created additional work in the extra handling and classification of negro blood and plasma. Fortunately, when it came to saving lives through the use of the plasma, the Army medical corpsmen (or at least some units of them in some areas of action) ignored the racial classifications which the Red Cross had so painstakingly maintained. They paid attention only to the blood-type classifications, thus limiting their selectivity to the needs of scientific fact and properly ignoring the consequences of popular error.

We scientists are in no position to criticize the action of the Red Cross, since we have consistently contributed to popular misconception and are continuing to do so at this very moment.

Like any thoughtless person, we speak of "blood relationship." Anthropologists, of all scientists, are most guilty! In all our discussions of kinship (a subject of consuming interest to anthropologists) we regularly use the term "blood relationship group." The terms "consanguine family," "consanguinity," "consanguine relatives," are all in constant use along with the synonym, "blood relatives." It is not necessary to cite cases. One can pick up any current book or monograph in

ethnology, or any issue of *The American Anthropologist*, and he is likely to find sufficient instances without painstaking research.

But a moment's reflection shows us how ridiculous and false this terminology is. The clan, for instance, is not founded on any blood tie, nor is any other relationship of family or kin. The relationship may be genuine or fictive, but it rests on a *genetic* principle and nothing else.

The error is even more agregious in physical anthropology when we commonly speak of racial ancestry in terms of blood. It is not unusual, for example, to hear a contemporary anthropologist say that the composite Polynesian race is predominantly Caucasian with a definite Mongolian admixture and a minority element of Negro *blood*. We still speak of half-blood and full-blood Indians.

The error of the anthropologist is shared by his fellow scientists. We unthinkingly carry and perpetuate the timeworn popular fallacy of hereditary continuity through blood. *In the interests of scientific accuracy it is incumbent upon us to stop it.* The imprint of accurate word usage based upon scientific fact must be stamped upon the lexicon of all men by scientists. It is our responsibility not to permit ourselves to be bound by popular word usage based on age-old error, especially when the error serves to generate and perpetuate untold mischief in the affairs of men.

A simple correction can easily be made. For "blood relationship" and "consanguine" substitute "genetic relationship" and "genetic group." For Negro, Caucasian, and Mongolian "blood" substitute "ancestry" or "hereditary of genetic component" unless it *is* blood you are talking about and not racial heredity.

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Book Reviews

Animal cytology and evolution. **M. J. D. White. London: Cambridge Univ. Press, 1945. Pp. viii + 375. (Illustrated.) $7.50.**

This timely and, in many respects, excellent review of the present status of chromosome morphology in relation to genetics hides its subject nature under a misleading title. The author may have intended originally to write a general cytology text; but he narrows the scope of the book in the Introduction, announcing that "by cytology we mean nuclear cytology, since the evolution of cytoplasmic constituents of the cell is an entirely different subject." In fact, even the term nuclear cytology seems too inclusive, considering that the book deals almost entirely with the chromosomes as the carriers of heredity. The comparative lack of purely cytological interest is reflected in the illustrations of the book, which consist of diagrams and simple line drawings.

The author makes a deliberate effort to relate his material with "neo-Darwinian" views, but in many instances, these attempts are unconvincing. By excluding from discussion all protozoans, plants, and lower organismic forms, the largest source of material which might serve as a basis for phylogenetic speculations remains unused. Consequently, the chapter on "The Evolution of Meiosis and the Chromosome Cycle" does not deal with the origin but merely with some modifications of meiosis, mostly as observed in aberrant insect groups. One may justly apply to this and other chapters of the book the commentary which the author attaches to his review of Goldschmidt's Lymantria work: "Its significance from the evolutionary point of view is, however, by no means clear."

The chapter on "The Evolution of the Sex-determining Mechanism" probably comes closest to the proclaimed aim of the book. The obvious fact that several times within the animal kingdom the change from hermaphroditism to gonochorism is followed by an evolution of sex chromosome mechanisms provides a tempting field for theory and speculation. White follows traditional ways in suggesting that sex-determining mechanisms evolve from monogenic differences (at a single chromosome locus), expand through the acquisition of differential regions, and finally become visible under the microscope in the shapes of X and Y chromosomes. He offers an excellent discussion of the relationships between pairing segments and differential segments of the sex chromosomes and their bearing on chiasma formation, crossing-over frequency, and reductional or equational distribution of the sex-chromatin in the first meiotic division. The scarcity or absence of sex-linked mutants in many of the more advanced-type sex chromosomes is connected interestingly with the progress of heteropycnosis. Gradually, the chromosomes seem to lose all original genetic functions except for their role in sex determination. The concluding paragraphs of the book contain the surprising statement that "monogenic sex-determining mechanisms which we observe at the present day, were almost certainly evolved as reversions. . . ." Accordingly, amphibians and teleosts should no longer be considered as links between the primitive hermaphrodite and the advanced sex chromosome types. But such a conclusion receives little support from recent investigations which reveal that, among lower vertebrates, rudimentary hermaphroditism is a relatively frequent occurrence and genetical sex determination is usually in a very labile condition. Furthermore, comparative chromosome studies furnish no indication that, once present,