evidence for our conclusion can be given here. It lies, in part, in the great range of dimensions of the arm and leg lengths in the browns as compared with either whites or blacks alone as shown in Tables 43 to 46, 48, 49, 52 to 54, 66 to 68, 72. That despite the usual large range the standard deviation of the browns is not always the highest of the three racial groups is probably due to the fact that the browns are the most rigidly selected of all, about half being students at Mico College.

That species hybrids can form highly variable zygotes including those that die in embryo or at a later developmental stage is well known among plants[2] in both first and especially second hybrid generations. If species hybrids, often apparently through the presence of genes with conflicting tendency, die, there is nothing extraordinary in the finding that in a less extreme cross viable but not fully harmonious progeny may be produced in F_2. That disharmonious instincts appear is easily demonstrated in dogs and poultry. That morphological disharmonies appear is well illustrated by Bryn's findings on the easily dislocated hips of Norwegian-Lapp hybrids, and the still unpublished findings of Stockard with dog hybrids.

The capacity of the organism through self-adjustment to make good some gene deficiencies is no doubt very great. But it has its limits. I have myself seen one of twins who, at birth, lacked even bilateral symmetry of the legs.

Finally, a word of protest against some misrepresenting phrases that Castle (no doubt unwittingly) has introduced into the discussion. He says, "Perhaps it is leg length in relation to total stature that puts browns at a physical disadvantage in relation to whites and blacks." This certainly distorts our conclusion. We never stated that "browns" were at a physical disadvantage; we stated precisely "some of the hybrids." We certainly never drew the conclusion that the Negro-white cross is inferior to the Negro or the whites; but we did find some cases of browns that seemed to present greater extremes—and sometimes less well-adjusted extremes—than either of the parental races. Our conclusion is not as Castle suggests it is, that the browns "are a degradation of the white race." Our conclusion is given at p. 477: "While, on the average, the Browns are intermediate in proportions and mental capacities between Whites and Blacks, and although some of the Browns are equal to the best of the Blacks in one or more traits still among the Browns there appear to be an excessive per cent. over random expectation who seem not to be able to utilize their native endowment."

[2] See Renner, in "Handbuch der Vererbungswiss.," Bd. II.

In *Nature,* No. 3177, Professor Karl Pearson publishes a critical review of "Race Crossing in Jamaica." His main complaints are based on the fact that the samples are insufficient—the adults of the 6 groups running from 50 to 93 per group. There is nothing to be said on this point except that funds and time were limited and the numbers of adults cited are all that we could, under the circumstances, secure. Whether results based on such small numbers were worth publishing is a question on which different opinions may be held. The committee thought they were worth publishing. The uniform computation of probable errors was relied upon to indicate the limits of significance (or insignificance) of the numerical results.

In regard to another matter about which Pearson complains, *viz.,* homogeneity of material, this is discussed from page 5 to 19; also pages 20 and 22. We do not recall any work of this nature where the homogeneity of the material is discussed more fully. Certainly we had in mind the desirability of genetical purity of the blacks, and think that we have eliminated the hybrids nearly as successfully as we could have done it for the "West African Negroes" to whom Pearson refers, mixed as they have been from time immemorial with Arabians and Jewish traders. If 2 or 5 per cent. of errors were made it would not have changed, we think, the essential conclusions of the work. The whites certainly included no "pass-for-whites," as they were taken from very segregated white populations.

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INDUCED PARTHENOGENESIS AND HOMOZYGOSIS

THE report of the production of homozygotes through induced parthenogenesis in Nicotiana and Fragaria, by Dr. East,[1] may justify a short review of parthenogenesis in the grouse locusts (Tettigidae). During attempts, 1915–18, to cross males and females of *Paratettix texanus* with those, respectively, of *Apotettix eurycephalus,* it was observed that the comparatively few resulting offspring were homozygous for the dominant segregate, or cross-over color patterns of the females, were themselves females and showed no trace of the dominant characteristics of the males. It was at first thought and noted that perhaps the development of the eggs had been in some way induced, or initiated, by the males of the opposite species, or by their products. However, before publication[2] it had been ascertained that the parthenogenesis

[1] SCIENCE, August 8, 1930.

[2] *Amer. Nat.,* 53: 131–142, 1919.

was in no way consequent upon the activities or products of the males. Still later[3] more than 5,000 females and 13 males of *A. eurycephalus* had been produced parthenogenetically, mostly from females which had not been exposed to males of any kind, and some of them had been carried seven consecutive generations without exposure to males.

Comparable results were obtained from the parthenogenetic breeding of *P. texanus*[4] and *Telmatettix aztecus*.[5]

All the partheno-produced individuals, including two males which were tested by further breeding, with one or two possible exceptions, proved to be homozygous for the several characteristics noted. The two tested males and several of the females, taken at random, were examined cytologically by W. R. B. Robertson. He has ascertained[6] that the homozygous pairs, respectively, of the chromosomes of the soma and oogonia lie together in early cell divisions, and not far apart, each from the other, in later cell divisions, in such position as to suggest that the second polar body division had been inhibited.[4] The second polocyte division in the grouse locusts, as in other forms, is probably normally consequent upon the entrance of the sperm, another case of "a later stage of maturation being overlapped by an early stage of fertilization."[7] In the absence of the fertilizing sperm and the resultant complete or partial inhibition of the last polocyte division, the diploidal condition is retained or restored, and if the specific or complementary genes responsible for the parthenogenetic processes are present, a chemical situation arises which conditions the initiation of development. Since it appears that any egg of these species is capable of being fertilized (those without the genes responsible for parthenogenesis require it), such educement of development may, perhaps, be considered *induced* or *artificial* parthenogenesis.[4]

It should be noted that Peacock and Harrison (1925–6)[8] advanced the very interesting hypothesis *that parthenogenesis was consequent upon hybridity,* using as a basis their work with hybrid moths from the crossing of *Tephrosia bistortata* males with *T. crepuscularia* females, and finding support from the materials used and results of the parthenogenetic breeding of the grouse locusts (*loc. cit.*). This hypothesis is probably valid, but it should be provided, in addition, that the process of hybridism may bring together specific, complementary or climaxing genes which are responsible for, or cause, as a kind of hybrid emergence,[9] the development of the unfertilized egg.

The list of instances of tychoparthenogenesis among such organisms as the grouse locusts, moths and, apparently, the plants used in Dr. East's experiments is constantly augmenting, and it is probably as yet far from complete. If a mutation, or stable, hybrid emergence, of sufficient transcendence should occur among the females of such, and since males do occasionally occur, certainly among the parthenogenetic grouse locusts, at least one long recognized major difficulty besetting the supposition of species transmutation might be considered as partially obviated.

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[3] *Kansas Tech. Bull.*, 17, 1925.
[4] *Biol. Bull.*, 66 (2): 129–155, 1929, and *Bibliographia Genetica*, 5: 27–104, 1929.
[5] *Genetics*, 13: 126–132, 1928.
[6] *Jour. Morph.*, 1930.
[7] E. B. Wilson, "The Cell," 1925.
[8] See literature cited, *Biol. Bull.*, 66: 155.

MUSICAL PITCH AND PHYSICAL PITCH

THE suggestion made by W. B. White in SCIENCE of September 19, that men of science entirely abandon the pitch standardization based on making middle C equal to 256 cycles, is a healthy one. But his positive suggestion of substituting for it a standard A of 440 cycles could serve well only those acousticians who are concerned with problems of music, that is, the esthetic-artistic side of acoustics. On the other hand, telephone engineers and psychologists interested in sensation units or in hardness of hearing and like problems which have no direct relation to music ought to use 1,000 cycles as their primary pitch standard and multiples and fractions of 1,000 as their auxiliary standards, such as 50, 100, 250, 500, 1,000, 2,000, 4,000, 5,000 cycles.

The remark by Mr. White that the discrepancy between any artificial scales used by physicists and musicians becomes particularly serious in the higher regions is meaningless to musicians, since mistuning in all regions is to them a purely relative matter. Physicists, however, will no longer suffer if they wholeheartedly, instead of half-heartedly as in their middle C custom rightly criticized, emancipate themselves from the orchestra leaders. The present writer happens to be equally interested in mere hearing and in music; and he has for almost a lifetime found a complete divorce of the two methods of standardizing extremely satisfactory for both purposes. By all means let every man of science for all purposes abandon the middle C of 256 cycles. But audiometers and similar instruments ought to be standardized on a decimal scale of cycles having 1,000 as its center.

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[9] SCIENCE, April 11, 1930.