raised several of these (uncastrated) males each year and they were remarkably uniform in horn development. Compare again Wallace's figures, which are accurate, being photographs. Arkell and Davenport's statement concerning the "variability of the horned condition in the males of the Merinos" leads me to think they must refer to *grade* Merinos, certainly not to the pure bred ones.

To my mind the evidence is clear that in pure Merino sheep castration does prevent development of the horns, and I have no doubt that in other breeds also castration has similar though perhaps less conspicuous effects. In breeds which are horned in both sexes the males regularly have *better developed* horns than the females, and castrated males have smaller horns than uncastrated ones. See figures in Wallace.

If castration has the effect stated, the assumed nuclear *inhibiting* factor of Arkell and Davenport is quite superfluous. Their experimental results are fully in accord with those of Wood and are fully covered by the simple statement of Bateson that the horned character is "*dominant in males and recessive in females.*" Why this is so I have attempted to point out. Presence of the testicle is necessary for full horn development, in some breeds it is necessary for *any* horn development. Reasoning from the experimental results obtained in poultry it seems probable that injections of testicle extract into the female would cause increased horn development similar to that of the male. It would be interesting to know whether the testicle of all breeds would behave alike in this experiment. Whether the female sex gland acts as an inhibitor of horn development would be a wholly different question, yet one capable also of experimental solution.

To assume, as Arkell and Davenport do, that inhibiting factors present in X-chromosomes affect the horn development seems to me unwarranted, for the simple reason that neither inhibitors nor X-chromosomes are known to exist in sheep. That Guyer has recognized the existence of an X-chromosome in man has no very direct bearing on the question, but even Guyer's result is unconfirmed by Gutherz, who has reinvestigated the spermatogenesis of man upon exceptionally favorable material.

Arkell and Davenport reason thus:

> The results of the table [of crosses] accord very closely with expectation, so that we are justified in concluding that an explanation of the results like that we offer is the correct one.

But Bateson's explanation accords also; wherein lies the superiority of the new one offered? To establish the probable correctness of a hypothesis it must be shown that no other hypothesis accords with observed facts equally well. Has this been shown in the case before us?

Consider how one unproved hypothesis has been added to another. First it is assumed that in hornless animals a gene for horns has either been lost or is inhibited. It is equally probable that no gene has been lost and that nothing is inhibited. Secondly, it is assumed that one inhibitor is inferior to one horn-gene in power, but that two inhibitors surpass one horn-gene, yet two inhibitors are themselves overpowered by two horn-genes; without all three of these ungrounded assumptions of the relative valency of imaginary genes the explanation fails altogether. Further, it is assumed that the female is capable of carrying two inhibitors, but the male only one. And finally when this colossal structure of hypothesis encounters one well-known physiological fact, the result of castration, that fact is calmly brushed aside. Is this a desirable extension of Mendelian interpretation?

W. E. CASTLE

March 13, 1912

THE MOTH OF THE COTTON WORM (ALABAMA ARGILLACEA HUBN.)

TO THE EDITOR OF SCIENCE: In connection with the notices appearing in SCIENCE (October 16, November 10 and December 29), concerning the moth of the cotton worm and the destructive work of the caterpillar on cotton, a note from Missouri may be of interest.

During the fall this moth was present in great numbers in various parts of the state.

The northward migration seems to have been quite general. They made their first appearance in the vicinity of Kansas City early in September, and by September 26–30 were present in countless swarms at Kansas City and Holden. They reached Macon in north Missouri September 24, and were very abundant and troublesome until frost. Here at Columbia they were especially abundant the last two weeks of September and early October; while later in October the moth of the army worm (*Leucania unipunctata*) was far more abundant, collecting about cider mills and injured and decaying fruit. With conditions favorable next year, we may expect considerable injury from the army worm in this state.

One point with reference to the moth of the cotton worm which the other notes have not brought out, is the injury which they do to ripening fruit in the orchard and where fruit is exposed in the market. This has been especially emphasized in all the letters received at this office this fall. As is well known, this moth has rudimentary mandibles by means of which it can break the skin of fruit and then with its proboscis it sucks out the juices. Late peaches, especially Heath Clings, are reported as having been severely injured this year. In some cases a dozen or more at a time collect on a single peach and eventually all the juice is consumed, leaving only the skin, pulp and pit. Grapes and even bananas in the market are attacked. In the orchard, after the peaches were picked, the moths turned to the apples. Their attack on the apple is similar to that on the peach except that the juice is drawn out in patches which turn brown and become mellow like bruises. The affected patches vary from the size of a pea to that of a dime or a quarter. The most of the fruit so attacked decays.

The strange northward migration of this moth which has always been of considerable interest to the entomologist has proved to be of special interest to many of the Missouri fruit growers this year.

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CRYSTALLOGRAPHIC TABLES

To the Editor of Science: As a teacher of crystallography I have found that students rarely appreciate the full significance of the fundamental laws of the science until, by actual measurement and calculation, they have found concrete evidence. With large crystals of quartz, calcite, tourmaline, zircon, rutile, barite, staurolite and a number of others, satisfactory results may be obtained by use of the Penfield hand goniometer. The advantage of such crystal measurement is twofold; it illustrates the laws which govern the arrangement of crystal planes, and it teaches the value of the science as a means of mineral determination.

Regarding the latter phase of the study, students are taught the methods of measurement and calculation necessary in each system for the determination of axial ratios. To bring out clearly the real value of such calculations as a means of practical mineral determination some sort of reference table of axial ratios seems desirable. Such tables have been compiled in a somewhat imperfect form, and it is to these that attention is directed.

The axial ratios of common tetragonal and hexagonal minerals are arranged in ascending values of *c*, the mineral names being placed in a parallel column. In practise the chart is placed before a class with the mineral names covered. After careful measurement and calculation the student refers to the column of ratios, and the fact that he can, in many cases, determine the mineral properly by this means alone, makes it a most illuminating and interesting exercise.

The orthorhombic system presents considerable difficulty in compiling a table of ratios since there are three possible ways in which the values of *a*, *b* and *c* may be arranged and still be in accord with the convention that *b* must be greater than *a*. Having determined the axial ratios, one is in doubt as to the proper arrangement. The three possible values of *a*, which may occur, *b* being unity, are the value of *a* when (1) *c* is greater than *a* and also greater than *b*; (2) *c* is greater than *a* and less than *b*; (3) *c* is less than *a*