

Now, regarding Mr. Harvey's letter, it should be pointed out that my Saccocirrus work, the only paper published on annelid oogenesis before Mr. Harvey's paper, was merely quoted by him in the bibliography. It is clear that Mr. Harvey had not taken the trouble to read my work before he published his criticisms. This was a pity.

Mr. Harvey points out that his paper was more in the nature of a critique of the methods of argument used by my school. Years ago, when I had already published several papers in the *Q.J.M.S.* under Sir Ray Lankester's editorship, that great worker wrote to me a good many letters of advice. In one of these he said, "Collect as many facts as you can, avoid discussion." If Mr. Harvey will take this piece of advice to heart, it will be all to the good.

Mr. Harvey complains that I have called into question his technique and powers of observation. This is true. Mr. Harvey quotes Dr. Cowdry. I know Dr. Cowdry's work: it is very fine, but it has no bearing on *Lumbricus*. Dr. Cowdry has worked on vertebrate mitochondria and chondriocontes. I am dealing with the eggs of the common earthworm.

Mr. Harvey says that "Prof. Gatenby and his school are content to accept the view that yolk is a general term covering anything in the cell which cannot otherwise be identified." On the contrary, Prof. Gatenby and his school are *not* prepared to accept everything and anything, as Mr. Harvey now realises clearly. Mr. Harvey goes on to say, "Is it not time that cytologists made some attempt to bring the observations on oogenesis to as orderly state as are those on spermatogenesis?" Such an attempt has already been made by Rodgers Brambell, Nath, and myself. Mr. Harvey should read these papers *carefully*, and then sit down quietly and try to help by adding facts himself.

I am grateful to the editor of NATURE for an opportunity of seeing the letters of Prof. Nath and Mr. Harvey. I have no wish to continue this controversy.

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November 6.

Chromosome Complements in Grasses.

THE hybridising and genetical study of grasses has reached a stage when information on their nuclear constitution is not only of general interest, but may also give guidance in elucidating problems of inheritance.

So far, we have had no recorded evidence of chromosome counts having been made in grasses, as apart from cereals.

I obtained, through the kindness of Mr. T. J. Jenkin, of the Welsh Plant Breeding Station, Aberystwyth, the use of grasses of known pedigree to carry out cytological investigations. This work was commenced in the summer of 1923 on *Lolium perenne* (Perennial ryegrass), *Lolium perenne* var. *multiflorum* (Italian ryegrass), and the hybrids derived from these two.

Anthers were selected as the organs most likely to yield the best results in the matter of chromosome counts. They were studied partly by means of microtome sections, and partly by means of the iron-acetocarmine (Belling's) method.

The whole series of events in sporogenesis was followed from the initiation of the meiotic divisions in the pollen mother-cell to the end of the homotype division.

Seven univalent chromosomes were so clearly distinguished in several phases of the reduction and homotype divisions that I can state with confidence that this number represents the haploid complement

in *Lolium perenne*, *Lolium perenne* var. *multiflorum* and their hybrid progeny. The diagrammatic disposition of the seven bivalents on the nuclear plate, forming a regular hexagon with a chromosome at each angle and one in the centre, is remarkable. It is of interest to mention that I found the divisions in the pollen mother-cells of the hybrids to be quite regular, except that 'lagging' chromosomes were occasionally found.

In studying mitoses in the root tips of *Festuca elatior* (probably var. *arundinacea*) (= Tall fescue), I discovered that the exact diploid number of chromosomes was difficult to determine, since the cell is comparatively small and the crowded chromosomes fill it to a great extent. However, from my counts the full complement appeared to be in the region of forty.

The anthers of this grass were investigated later, and the reduced number of chromosomes was determined, with a fair degree of certainty, to be twenty-one.

From a preliminary study of *Festuca elatior* var. *pratensis* (Meadow fescue) I have strong evidence that the haploid number in this case is seven.

The iron-acetocarmine method applied to the anthers of this grass was such a striking success that I was able to make a large number of counts in individual anthers.

It would thus seem that polyploidy obtains in genera of grasses, as in cereals.

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The Planet Mars.

IN NATURE of November 13, p. 709, I notice the following sentence: "An objection frequently brought by M. Antoniadi against the objective reality of the canals, based on their being drawn straight when far from the centre of the disc, is answered by a careful observation of the canal Amenthes-Thoth on October 19 and 20, 1924."

This passage scarcely represents the facts, for I have shown in numerous writings since the opposition of Mars in 1909 that, in the place of Schiaparelli's *canali*, the surface of the planet very often shows either (1) complex dusky streaks; or (2) jagged edges of half-tones; or (3) isolated, irregular dark spots. Amenthes-Thoth being a Schiaparellian marking, the probabilities, according to my views, are that, in its position, there exists an irregular streak. The Thoth I saw very broad, strong, and jagged in 1911, and insisted on its abnormal conspicuousness, considered it real, and discovered the fact that its changes are not seasonal—a fact that observation has fully confirmed since. (See my Mars Report for 1911-1912, of the British Astronomical Association, pp. 116-117.)

As the writer of the note on p. 709 refers to the 1924 appearance of the Amenthes-Thoth, and as I have represented that marking in 1924 as a winding, complex, dusky streak (*Bulletin of the Astronomical Society of France*, 1925, p. 82), it is only natural that it should obey perspective. The suggestion that the marking in question is illusive is thus completely answered.

Furthermore, Prof. Pickering did not mention me in connexion with his above observation, as the text on p. 709 seems to suggest.

My position is perfectly clear: there is no geometrical network on Mars, since I have shown (1) that the linear canals vanish in a large glass