

APART from the fact that the conclusion that *Calluna* is a "xeromorphic mesophyte" should be attributed to Stocker, there is nothing in Dr. Delf's remarks with which I find myself in disagreement, except with regard to their relevance to the point at issue.

Certain features of the structure of moorland plants, inrolled leaves, sunk stomata, thick cuticle, etc., have been explained teleologically as designed to reduce transpiration. Stocker and Montfort show experimentally that the plants transpire very freely in spite of these features. Dr. Delf does not help the teleological argument by pointing out that, even though they lose water freely, the plants may still thrive because of other xerophytic characteristics. Nor are Crump's results relevant to the problem of moorland plants growing in regions of excessive rainfall. The contemplation of these same "xerophytic" structural features in such swampy moorland regions had led to the suggestion that the water might not be "physiologically" available, but Montfort's experimental results show that plants absorb it freely.

It interested me very much to learn, from the presidential address of Prof. F. E. Weiss to the Ecological Society on January 10 last, that these same structural features provide a similar puzzle to the palaeobotanist. On one hand, it is argued from such leaf characters that the coal measure forests grew under xerophytic conditions; on the other, from root systems and other features, it is argued that they grew in vast swamps.

J. H. PRIESTLEY.

### The Need for a Universal Language.

NOBODY, not even Prof. Kent or Mr. Heron-Allen, really wants Latin as a universal language. Latin is dead; its natural development has ceased and could not possibly meet the growing needs of international thought. Every one recognises that the classical tongue, if it is to satisfy modern needs, must have its grammar simplified and its vocabulary, especially of abstract terms, enlarged. But the features which are to be changed are those which give Latin its peculiar savour and the educational value that some would claim for it. "Modernised Latin" is not Latin at all; it is a hybrid jargon as artificial as Volapuk or Esperanto, as devoid of literary tradition, as incapable of artistic expression, as subject to national and individual vagaries. Of this last defect, of which he accuses Esperanto, Mr. Allen gives a crushing example. Few but English speakers would understand *statio* for railway-station; few but blundering schoolboys would use *quae* as a substantive for *quis*.

The Latinists are crying for the moon. They want as a medium for international communication a "natural" language with at least the possibility of literature. The thing is impossible. To fulfil its object the medium must mean the same thing to all men, however diverse their mentality or experience. The allusiveness that makes literature possible is the fruit of a common life and history; it is the fruit of that nationalist sentiment which it is one of the main purposes of the language to remove. Universal intelligibility and artistic expressiveness can never be combined in a single medium; for art is the trick of meaning rather more than you say. I hope nobody will reply that, if a universal language must always be divorced from literature, we are better without it. That was the fallacy of Ruskin and the cause of most of the sordidness of the Victorian age. An art which seeks to limit utility is doomed; and if literature is set in opposition to scientifically impersonal expression, literature will wither.

Of course, the view is tenable that modernised 'Latin,' confessedly artificial and developed from the language of the Romans by some conventional scheme, would be better than its rivals even according to their standards. But it is much more important to have some international language than to have the best conceivable. Esperanto is within the bounds of practical possibility; it has made definite progress; it is supported by an enthusiastic organisation; it is actually being broadcast. For the great mass of the unlearned, anything called Latin is eternally damned by its associations. To some it suggests examinations and school punishments; to others pedantry and obscurantism. It is not a practical possibility, and those who press its claims are merely hampering the cause they profess to serve.

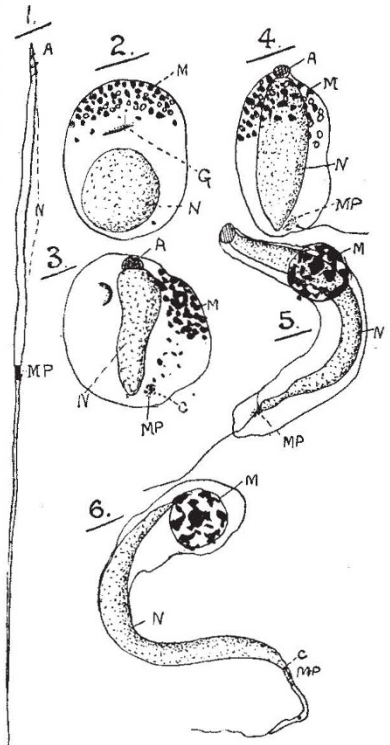
NORMAN R. CAMPBELL.

### Spermatogenesis of *Peripatus*.

THIRTEEN years ago Montgomery, an American cytologist, published a paper in the *Biological Bulletin*, claiming that in *Peripatus* there was complete rejection of the mitochondria during spermateleosis. This work has been widely quoted as strong evidence against the Meves' mitochondrial idioplasm theory; but Montgomery's investigation has never since been confirmed or denied.

Owing to the good offices of Prof. Gilchrist, of Cape Town, South Africa, and of Canon Forrester, Chaplain of Trinity College, Dublin, six small specimens of the *Cape Peripatus* arrived alive in Dublin, and I was able to procure some fairly good sections showing spermatogenesis.

At first I believed that Montgomery was correct, but latterly I have modified my views. In Fig. 1, it will be seen that the middle-piece (MP) is very small—probably proportionately smaller than that of any other animal; consequently the problem of its manner of formation is not easy to solve. Figs. 2, 3, 4, and 5 show the mitochondria at M, finally forming a ball, as claimed by Montgomery. This ball is certainly extruded (Fig. 6) as explained by Montgomery, but if one examines a large number of well fixed cells at the stages depicted in Figs. 3, 4, and 5, one will nearly always find a number of fine granules (MP) which eventually form the middle-piece of the tail. I think that these granules are of the nature of mitochondria, because their reactions are similar; and I have got the impression that the middle-piece of the *Peripatus* sperm is formed from a few of the finer



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