

thirdly, because useless publications increase the burden of librarians and scholars. We are far from wishing to discourage genuine attempts to reproduce Fermat's line of thought. In view of the state of mathematical knowledge 250 years ago, Fermat's proof, assuming it to be correct—a point on which expert opinions differ—is as likely to be discovered by a clever schoolboy of seventeen as by a more highly trained mathematician.

Mr. Cashmore, in the tract before us, presents three distinct "proofs," all erroneous. In I. (p. 14) he states that when

$$ax^2 + by^2 = w^n, \text{ then } w = au^2 + bv^2,$$

the letters denoting ordinary integers. A numerical example is enough to show that this is erroneous; thus

$$2^2 + 5 \cdot 1^2 = 3^2, \quad 11 \cdot 2^2 + 9^2 = 5^3;$$

but there are no integral solutions of

$$x^2 + 5y^2 = 3, \quad 11x^2 + y^2 = 5.$$

The first of several fallacies in II. occurs on p. 26, and in III. (p. 43) Mr. Cashmore states that when $(p^n - q^n)y^{n-1}$ is divisible by pq , then y is divisible by pq , it being assumed that p and q are integers with no common factor. It is seen that this deduction is erroneous by taking

$$p=9, q=4, y=6, n=3.$$

W. E. H. B.

Secrets of Animal Life. By Prof. J. Arthur Thomson. Pp. viii + 324. (London: Andrew Melrose, Ltd., 1919.) Price 7s. 6d. net.

THIS is a collection of forty essays, contributed during recent years by Prof. Thomson to the *New Statesman*, and now collected in a handy and attractive volume. In his own clear and charming style the author seeks "to interest thoughtful readers in the multitudinous problems of animal life," and he wisely enforces the lesson that, in many cases, the solutions of these problems are "secrets" still. Such familiar subjects as the habits of rooks and cuckoos or the "Fall of the Year" are mixed with review-summaries of noteworthy recent zoological literature of general interest such as Watson and Lashley's observations on the "homing" of terns, Emery's researches on the habits of Amazon ants, or Petersen's surveys of the *Zostera*-beds off the coasts of Denmark. The problems of inheritance and evolution are prominent, as might be expected, and from such papers as "With Darwin Forwards" and "The Mendelian Clue," the "thoughtful reader" may gain a clear introductory view of the fields of biological inquiry, as well as guidance in the way of deeper study. Prof. Thomson never misleads those for whom he writes by implying that after reading him they have no more to learn; his treatment of "The Problem of Cave Blindness," for example, affords a needed corrective to widespread dogmatism on a subject that has appealed to popular imagination since the early days of evolutionary biology.

G. H. C.

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LETTERS TO THE EDITOR.

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International Relations in Science.

I DO not agree with the proposals made by Prof. D'Arcy Thompson in NATURE of October 23. I think that the less our academies and societies move in this matter the better. For my own part I objected altogether to the proposal made during the war to strike off our records the names of distinguished men of enemy nationality who had been elected "foreign members" before the war. They had not been admitted to any power or rights in consequence of that election, and it was, in my judgment, futile and petty to endeavour to obliterate the record of the honour which had been justly accorded to them.

As to making overtures to, and the reception of overtures from, the academies of those hostile nationalities with which peace is not yet ratified, it seems to me that our own societies and academies should at present neither offer nor accept any such overtures. They are mere formalities and demonstrations without any real significance or value, and must be, and are often designed to be, misleading. On the other hand, I think every individual should act according to his own feeling and judgment, and not according to mass sentiment, in regard to entering into friendly relations with German men of science. At present I personally could not accept such relations. I wish to reserve all action in the matter until my memory of many things has faded. But I will never wittingly treat even those whom I most dislike with less than justice tempered by generosity.

E. RAY LANKESTER.

The Response of Plants to Wireless Stimulation.

A GROWING plant bends towards light; this is true, not only of the main stem, but also of its branches and attached leaves and leaflets. This movement in response is described as the tropic effect of light. Growth itself is modified by the action of light: two different effects depending on the intensity are produced; strong stimulus of light causes a diminution of rate of growth, but very feeble stimulus induces an acceleration of growth. The tropic effect is very strong in the ultra-violet region of the spectrum with its extremely short wave-length of light; but the effect declines practically to zero as we move towards the less refrangible rays, the yellow and the red, with their comparatively long wave-length. As we proceed further in the infra-red region we come across the vast range of electric radiation, the wave-lengths of which vary from the shortest wave I have been able to produce (0.6 cm.) to others which may be miles in length. There thus arises the very interesting question whether plants perceive and respond to the long æther-waves, including those employed in signalling through space.

At first sight this would appear to be very unlikely, for the most effective rays are in the ultra-violet region with wave-length as short as 20×10^{-8} cm.; but with electric waves used in wireless signalling we have to deal with waves 50,000,000 times as long. The perceptive power of our retina is confined within the very narrow range of a single octave, the wave-lengths of which lie between 70×10^{-8} cm. and 35×10^{-8} cm. It is difficult to imagine that plants could perceive radiations so widely separated from each other as the visible light and the invisible electric waves.

But the subject assumes a different aspect when we