OUR BOOKSHELF.

Die radioaktive Strahlung als Gegenstand wahrscheinlichkeitstheoretischer Untersuchungen. By Prof. L. v. Bortkiewicz. Pp. 84. (Berlin: Julius Springer, 1913.) Price 4 marks.

THIS mathematical work is a critical application of the theory of chance to the breaking down of radio-active atoms. Its discussion is mainly based on the experiments of Rutherford and Geiger. Scintillations were produced on a screen by polonium, and were counted over a succession of equal short intervals of time, and the intervals were classified by the number of them which showed either no scintillation or one or two or more. The experimenters found that their numbers agreed well with those predicted by the theory of pure chance, but they gave no criterion as to the closeness of agreement to be expected. The calculation of the "mean errors" is a simple matter, but in the comparison of such a series of numbers it is only likely that in a few of the cases the mean error should be considerably exceeded. Prof. Bortkiewicz therefore provides a single test for the whole experiment. He works out twelve cases, and concludes that the results are, on the whole, slightly closer to their most probable values than is predicted by theory. He suggests an experimental cause for this small discrepancy. He also discusses one of the experiments of Marsden and Barratt, who made their analysis by classifying the lengths of time between each two successive scintillations, and he concludes that the distribution is normal. In this case his test is not perfectly satisfactory, as it involves the use of quadrature and interpolation formulæ, processes which would seem to be very unsuitable for problems of chance. In both types of experiment distributions can be contrived which pass his tests, and yet are in reality very improbable, but no doubt there are great mathematical difficulties in the way of deriving the true probability test. From his work we may conclude that the search for regularity, other than the regularity of chance, in the disintegration of radio-active atoms is not a hopeful quest.

C. G. D.

A Pocket-Book for Miners and Metallurgists: Comprising Rules, Formulæ, Tables and Notes for use in Field and Office Work. Compiled by F. D. Power. Third edition, corrected. Pp. xiv + 371. (London: Crosby Lockwood and Son, 1914.) Price 6*s.* net.

MINING engineers are nowadays called upon for knowledge and powers in so many directions that to anticipate moderate success and escape serious blame, they must exhibit qualities for which Gilbert and Sullivan's heavy dragoon could not hope. To be ready to act at short notice as an explorer, a geologist, a civil and mechanical engineer, a chemist, a metallurgist, a doctor, and a lawyer, a man needs some little book in his pocket which he can consult as each new problem comes into view. Such a book Mr. Danvers Power set himself to construct many years ago, and the third edition, now issued, is not less successful than its forerunners. There is no trace of the amateur about the little volume. It is the work of a professional man who has set down the things he wanted to know himself. Like all pocket encyclopædias, it does not contain everything that could be wished for. There might have been included something about furnaces, refractory substances, and melting points, a few tips on mine-surveying problems, a little more about the strength of materials, and perhaps some information on first aid. But although there may be a few omissions, so much is included that the book deserves a trial by every prudent miner or metallurgist.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

The Constitution of the Interior of the Earth as Revealed by Earthquakes.

ON p. 45 of Dr. G. W. Walker's recently published book, "Modern Seismology," I find the following sentence:—"It has sometimes been asserted that S never reaches beyond a certain distance, and to explain this an impenetrable core of the earth has been assumed. We see that no such hypothesis is at all necessary to explain the observations." The reference here seems to be to a paper, by myself, "The Constitution of the Interior of the Earth as Revealed by Earthquakes," which was published in the Quarterly Journal of the Geological Society (vol. lxii., 1906), or, more probably, to the references to this paper contained in Prof. Wiechert's paper, "Ueber Erdbebenwellen," published in the *Nachrichten d. K. Gesellschaft d. Wissenschaften* (Göttingen, 1907), and as the summary dismissal of the subject indicates an imperfect appreciation of the problem, which is one of the important problems of the immediate future of seismological research, I trust you will afford me space to state the position.

In my paper, referred to above, I pointed out that the twofold character of the preliminary tremors, representing the arrival of two distinct forms of wave motion, can be traced continuously up to a distance of about 110° or 1200 km. from the origin, and that a comparison of the times of arrival of the waves at different distances shows a progressive and gradual increase of interval with distance, and affords no indication of any great change in the character of the material traversed by the wave paths. Beyond 12,000 km., however, the second phase can no longer be recognised with certainty, and has either entirely disappeared or is represented very feebly and with a considerable delay, as compared with the time of arrival which would be anticipated from the records of observations at lesser distances from the origin. From this I concluded that the wave paths to these more distant stations must have entered a central core of matter differing markedly in constitution from the outer portion of the earth, in that it was either quite incapable of transmitting the second-phase waves, or only transmitted them with a considerable diminution of energy and of rate of transmission.

Prof. Wiechert explains the facts in a different manner. From the laws of reflection, and assuming a tolerably homogeneous earth, he deduces the con-

clusion that waves incident on the earth's surface would suffer reflection accompanied by splitting up of the simple condensational or distortional waves into two sets, one of each kind, so that, at distant stations, the arrival of the direct waves would be complicated by the arrival of reflected waves, which had travelled one part of their course as condensational, and the other as distortional, waves. The critical point at which confusion from this cause would arise is at about 120° distant from the origin, and the disappearance of the second phase, as a recognisable feature in seismograms at greater distances, is attributed to this cause; the records, which I had accepted as possibly indicating a diminished and retarded appearance of this phase, being interpreted as the arrival of the reflected distortional waves.

With regard to this explanation, I may say that the reality of the reflected waves, though accepted by many seismologists, and practically universally by the Germans and the whole school dominated by the influence of their work, still seems to me far from being established. The theory is based on the assumption of a globe of uniform constitution bounded by a reflecting surface, but this does not exist in nature, for the outer crust of the earth is composed of material which was long ago shown, I believe first by Prof. Rudzki in 1899, to be composed of material which cannot transmit simple condensational and distortional waves, but transforms them into more complex forms of wave motion. Nor have we reason to suppose that the lower surface of this outer crust presents a definite surface of contact between two media of different character, from which reflection could take place; rather it is to be expected that the transition is gradual and that the simple forms of wave motion, which can be propagated through the central portion of the earth, would be gradually converted into more complex forms, and become extinguished, in the surface layers. On the observational side, too, the case is not conclusive, for though the presence of reflected waves in the record has been claimed, more particularly in the case of earthquakes originating in the Malay Archipelago and beyond, their presence does not seem to be constant, nor by any means so conclusively established as the reality and distinctness of the first- and second-phase waves.

Accepting, however, the reality of reflected waves and the interpretation, offered by Prof. Wiechert, of the records accepted by me, with considerable hesitation, as possibly representing the arrival of the second-phase waves, it does not afford a sufficient explanation of the absence of the record of the arrival of the second-phase waves, travelling along the direct course from the origin, in seismograms from stations at and beyond 12,000 km. from the origin. This phase is well represented, and usually conspicuous, especially in the records of horizontal pendula with a moderate rate of travel of the recording surface, and up to the limiting distance, at which it disappears, forms a feature in the seismogram which should be recognisable even if superimposed on the record of reflected waves; for, apart from the hypothesis of a central core of material less capable of transmitting these waves, there is no reason for anticipating a diminution in the amplitude of the record at greater distances, but rather the reverse.

The length of wave path of the waves emerging at the antipodes of the origin is certainly greater, about 12,750 km., as against about 9500 km. for waves emerging at 10,000 km. from the origin, but, on the other hand, two wave paths starting directly downwards with a divergence of 1° will issue on the surface at a distance of about 222 km. apart, and two wave paths starting at an inclination of about 70° to 74° downwards from the horizontal and a divergence of 1° will reach the surface at some 10,000 to 12,000 km. from the origin, and at a distance of about 500 km. apart. Setting these two against each other, we have, on one side, the increased energy due to a more than twofold concentration of wave paths, and, on the other, the greater absorption due to about 30 per cent. greater length of wave path, the former of which should more than counterbalance the latter, so that the record of the direct waves ought to be more conspicuous at greater distances than between 10,000 and 12,000 km., up to which it is easily recognisable. I have examined most of the records obtained at greater distances previous to 1906, and some of later date, but have failed to discover the second phase, and it seems reasonable to suppose that this may be explained by the wave paths to these greater distances having encountered a different form of matter which is less capable of transmitting the second-phase waves from that traversed by the wave paths which do not descend so deeply into the interior of the earth.

Though this letter has run to a considerable length, I hope you will allow me space to refer to another passage in Dr. Walker's book, on p. x. of the introduction, where he refers to a paper by me (published in *Phil Trans.*, 1900) as the first application of the well-known theory of longitudinal and transversal waves to Milne seismograms. Had Dr. Walker verified his reference he would have found that the paper has nothing to do with Milne seismograms, and that it was the first published demonstration of the *three-fold* character of the wave motion recorded at a distance from the origin, and incidentally an explanation of the failure of earlier attempts to interpret the records in terms of the two forms only, of longitudinal and transversal waves.

R. D. OLDHAM.

The Evidence for Spontaneous Generation.

IN reference to the letter of Profs. Farmer and Blackman in NATURE of February 12, it seems needful to state that only two of my tubes were opened in their presence. One of them showed, as I had predicted, bodies very closely resembling Torulæ, in large numbers. They were not, however, typical Torulæ, such as are represented in Figs. 1, 3, and 5, of my communication published in NATURE of January 22, and I am prepared to admit some doubt as to their nature. The other tube showed, as others of the same series had done, peculiar spores, which when shown together with their mycelium (as in Fig. 2) to an eminent fungologist, were said by him to belong to a mould allied to the genus Oospora. He had no doubt as to its nature; and I am certain that these moulds must have grown within the tubes after their sterilisation, in one case to the extent of producing, after sixteen months, two tufts plainly visible to the naked eye.

I am glad to learn that one of the colleagues of Profs. Farmer and Blackman is repeating my experiments, and trust he will, after a time, be able to solve their doubts.

H. CHARLTON BASTIAN.

The Athenæum, February 13.

The Wearing of Birds' Plumage—A Woman's Protest.

IT is very gratifying to find how earnestly the best papers are now taking up the cause of the various beautiful birds hitherto so cruelly and callously slaughtered for the sake of their plumage. The dealers in feathers seem to think that because they have embarked in that particular trade it must never