

labour-saving device for the dairyman it has been ranked with the separator.

Certain statements will want correction in a future edition. We are told, for instance, that "the covering of land in summer prevents the temperature from rising so high as to destroy the organisms of the soil."

E. J. R.

OUR BOOK SHELF.

Astronomischer Jahresbericht, Vol. viii. Die Literatur des Jahres 1906. By A. Berberich. Pp. xxxvi+671. (Berlin: Georg Reimer, 1907.) Price 21 marks.

ASTRONOMERS are fortunate in the matter of having their literature catalogued, for, in addition to the volume published by the Royal Society for the International Council, we have this very excellent annual, instituted by the late Dr. Walter F. Wislicenus, which has now reached its eighth volume.

The contents of the present issue deal with the literature of the year 1906, and it only requires a cursory glance to indicate how important it is that such a catalogue is in existence, considering the great mass of work that is being turned out every year and published, not only in all sorts of journals, but in various languages.

The very arduous task of collating and cataloguing is now annually being successfully accomplished by Dr. Berberich and his co-workers, and an important feature about the publication is its early issue.

In the present volume, which contains no less than 1961 separate brief abstracts of published papers, accompanied by a complete name-index, some minor changes have been made.

Thus all references to publications with regard to minor planets are brought together under one section number, and the tabular statement of their observation is here omitted, as it appears in full in the Berlin *Astronomical Year-book*.

The literature relating to comets is now divided between two sections, while one section includes the whole of meteor-astronomy.

In spite of the above and other alterations, the volume is not reduced in size, for longer abstracts are given of works of greater importance.

The value of this publication to astronomers cannot be overestimated, and it behoves everyone interested in this science to support it, so that the continuation of future issues may be assured.

Lehrbuch der theoretischen Elektrochemie auf thermodynamischer Grundlage. By J. J. van Laar. Pp. xii+307. (Leipzig: W. Engelmann; Amsterdam: S. L. van Looy, 1907.) Price 6 marks.

THE present volume differs greatly in character from those to which we are accustomed from the pen of Dr. van Laar. His "Thermodynamik in der Chemie" and his "Lehrbuch der mathematischen Chemie" are so formal in their nature, so mathematical in their dress, and so slightly connected with the facts of observation, that the majority of chemists can have derived little benefit from them, excellent though they may be of their kind. Here the author adopts a different method; the mathematical deductions have the clearness and conciseness which might be expected, but everywhere the experimental data are brought into the foreground, so that the electrochemist with a modest mathematical equipment may hope to gain a clear view of the thermodynamical theory of his science.

The book is divided into twelve chapters, of which the first deals with electrical units, chapters ii.-iv.

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with conductivity, chapter v. with diffusion, chapters vi.-x. with electromotive force, chapter xi. with polarisation, and chapter xii. with capillary electric phenomena.

A good account of the work of Kohlrausch is given in chapter iii., and the recent researches of Walden and others on the conductivity of non-aqueous solutions, and of Lorenz on fused electrolytes, are well summarised in chapter iv. Chapter vii., on the partition equilibrium of electrolytes, contains much that is novel.

Altogether it may be said that the book is readable, original, and suggestive.

Coal. By James Tonge. Pp. vii+275. (London: Archibald Constable and Co., Ltd., 1907.) Price 6s. net.

THE author recently published an excellent little work on coal-mining for the use of students. It is disappointing, therefore, to find that in writing a book on coal for the general reader he has been less successful. The work appears to have been hastily compiled, and the proofs carelessly revised. For example, the Ruhr appears as "Rurh," Courrières as "Courrieries," Anzin as "Auzin," Resicza as "Kesicza," and Karwin as "Kirwin."

There is a useful chapter on the preparation of coal for the market; and the chapter on the botany of the Coal-measure plants is excellent, though somewhat technical for the general reader. Both these chapters are admirably illustrated. The chapters on the British and foreign coalfields, on the valuation and uses of coal, on the production of heat from coal, and on the waste of coal, contain, however, little that is not better set forth in the report of the Royal Commission on Coal Supplies, in the valuable digest of that report published by the *Colliery Guardian*, in Prof. Flux's revised edition of Jevons's work, or in other works dealing with coal. Of such works many have recently been published, for we cannot agree with the author that "it is now many years since a work on coal was presented to the public."

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 Speed of Racing Animals.

IN NATURE, March 14, 1907, p. 463, there is an article giving the results obtained by Prof. Kennelly, of Harvard, from an examination of racing records. There is no harm in again directing the attention of your readers to these results. Prof. Kennelly's paper was sent July 6, 1906, to the American Academy of Arts and Sciences, and published in the Proceedings in December, 1906. It is entitled "An Approximate Law of Fatigue in the Speeds of Racing Animals." His general result is given in a question set by me in an examination in practical mathematics, January, 1907. Here is the question:—

If t seconds is the record time of a race of y yards; the law $t = cy^n$ seems to be wonderfully true for all record races of men and animals excepting men on bicycles; n is the same number in all cases. c has a special value in each case, men walking, running, skating, swimming, or rowing; horses trotting or galloping or pacing.

(1) For any particular kind of race it is found that when y is increased by 100 per cent., t is increased by 118 per cent.; find n .

(2) For men running, when $y = 600$, t is 71; find c in the above formula. Express s , the average speed of each race, in terms of y .

(3) Assume that an animal has a certain amount of endurance E which is exhausted at a uniform rate during

the race, and that $E = E_0 + kt$, where E_0 and k are constants. Calling E/t the rate of fatigue f , express this in terms of s .

Assuming that an animal going at s_0 miles per hour feels no fatigue, or when $s = s_0$, $f = 0$; find f in terms of s .

(1) Contains the general result; the law is that t is proportional to $y^{3/8}$ or $y^{1.125}$. It may be stated in various other ways; for example, that the average speed in each race is inversely proportional to the eighth root of y , or a race 256 times as far is done at half the average speed.

In short races there is increase of speed at the beginning and almost always increase near the end; and it may be that there is continuous change of speed during all record races. We have only average speeds recorded, unfortunately, but still I must consider this wonderful general law to be worthy of the attention of biologists.

(3) Contains a poor speculation of my own, good enough for such an examination; the answer to it is that f is equal to $E_0 c^8 (s^9 - s_0^9)$. There is nothing extraordinary in the fact that record bicycle races do not follow the law; they have been run on machines of varying quality.

The values of c found by Prof. Kennelly are, his distances y being in metres:—trotting horse, 0.0295; pacing horse, 0.0291; running horse, 0.0236; man running, 0.0588; man walking, 0.0861; man skating, 0.0385; man swimming, 0.381. Men rowing, four oars, 0.0628; two oars, 0.0768; singles, 0.0824.

JOHN PERRY.

Royal College of Science, S.W.

The Isothermal Layer of the Atmosphere.

THE investigation of the upper air which has been in progress during recent years has revealed conditions for which it is very hard to find an explanation. When Mr. Rotch first inaugurated observations on temperature and humidity by means of kites, it was hoped that the results obtained would solve many problems connected with meteorology, and this hope was strengthened when M. Teisserenc de Bort greatly extended the height to which observations could be made by his system of *ballons sondes*. It has not, however, been realised, and we seem to be as far as ever from knowing the cause and mechanism of the cyclonic storms that are so common in the oceanic parts of the temperate latitudes.

Since last June some forty balloons have been sent up in Great Britain, carrying with them a small instrument which draws automatically a pressure temperature diagram, and of these more than thirty have been recovered. The results confirm those previously obtained on the Continent, and no doubt can now remain about the existence of the curious isothermal layer in the atmosphere.

Briefly, the more important phenomena are these. As we ascend the temperature of the air decreases, at first often irregularly, with breaks and inversions, but after the first 10,000 feet (3 kilometres) have been passed with fair regularity, the usual decrease being about 3°·3 F. per 1000 feet (6° C. per kilometre). This continues to a height that varies, as a rule, from 30,000 feet to 40,000 feet. Nearly always at somewhere about this height the decrease suddenly ceases. Above this point the air in most cases gets a little warmer; occasionally, however, it continues to get cooler, but at a totally different rate, and we may take the remaining air to be at one practically uniform temperature in so far as change of height is concerned. This isothermal layer, as it has been called, has been reached in England more than thirty times. On the average its height is about 35,000 feet (10·7 kilometres), but the extreme values found were 25,500 feet and 49,000 feet. As a rule, it is higher than the mean when the barometer is high, and conversely. Its mean temperature was found to be -53°·6 F. (-47° C.), and the extremes were -22° F., at Ditcham Park, Hants, on July 24; -24° F., at Crinan, on July 26; -78° F., at Pyrton Hill, Oxfordshire, on September 12, 1907, and also on February 5, 1908; and -74° F., at Manchester, on November 7, 1907.

This mean value is considerably higher than the Continental one for previous years, but as thirty observations do not suffice to give a true mean, this may be accidental. Balloons have been sent up from five stations on the same day, and the temperatures over the stations have been found to differ widely. Thus on November 11 Mr.

Cave's balloon from Ditcham Park reached the isothermal layer at 36,000 feet, and its temperature was -42° F. Over Oxfordshire the height was 38,500 feet, and the temperature -58° F., while at the same time, which was a little after sunset, Mr. Petavel, at Manchester, found it at 37,000 feet, with a temperature of -74° F. This is not an isolated instance, and although the heights given may be uncertain to an extent of perhaps 5 per cent. or even more, it is very unlikely that the error in the temperature can exceed three or four degrees F. It may be accidental, but the temperature over Ditcham Park, which is near the sea, shows a tendency to exceed that over the Midlands. The balloons mostly drift to the eastward, the centre of their falling points being thirty-four miles E., 23° N., of the starting point. Doubtless several of the unfound balloons fell in the North Sea, as some have been returned from France and Holland.

Various suggestions have been made to account for these results. There is, of course, no difficulty about the general decrease of temperature with height, but why should the fall suddenly cease when from one-third to one-fourth of the mass of the atmosphere remains above? In general, the transition point is perfectly sharp and distinct. It is said that the vertical circulation ceases at this point, and no doubt the statement is true, but why should it cease? There is a further difficulty. The absence of vertical motion implies a condition of equilibrium, but how can there be equilibrium with such large horizontal differences of temperature? At the height of 40,000 feet the pressure is small, and therefore trifling changes of pressure produce large changes of volume and temperature; hence large changes of temperature might be expected if we could assume some horizontal force, comparable with gravity, and capable of producing changes of pressure without producing vertical motion. The horizontal acceleration due to centrifugal force in a curvilinear path and that produced on a moving body by the earth's rotation are too small; also it seems to me that these forces, being due to motion of the air, must produce some vertical circulation, which apparently does not exist.

The problem is one of the most interesting presented at the present time to physical science, and it is not unlikely that its solution might clear up many other puzzling questions.

W. H. DINES.

The Inheritance of "Acquired" Characters.

MR. ARCHDALL REID in his previous letter said (p. 293) "innate characters arise inevitably as the child develops, whereas some acquirements are more or less rare, but this is *only* because the stimulus of nutriment is inevitably received, whereas the stimulus of a particular use or injury may not be received. If, however, the latter be received, the acquirement arises just as inevitably as the innate characters." This latter statement seemed to me particularly incorrect for the reasons which I stated; and now Mr. Reid practically admits (p. 342) that it cannot be accepted as it stands, in face of the fact that the inherent tendencies of the germs of different human beings vary so much, and must therefore react variously to the stimulus of use.

My chief objection, however, to Mr. Reid's view was that contained in his statement that "in man the main difference between the infant and the adult is due to use acquirements made by the latter during development." His whole case in the discussion hinges upon this statement, by which he seeks to establish a purely arbitrary distinction between the causes leading to the development of the human being anterior to birth and subsequent to birth—the stimulus in the one case, he says, being nutriment, and in the other use. As an illustration of his position, Mr. Reid said (p. 293):—"Thus, if the limb of an infant be paralysed it grows comparatively little, and the muscles atrophy." But this is by no means to be taken as a simple illustration of the fact that the muscles will not develop except under the stimulus of use, because the illustration ignores the fact that what happens in the way of retarded growth of the paralysed limb, together with actual atrophy of muscles, is, quite apart from the lack of use, largely due to a severance of the normal relations between the limb and the great nerve centres, and the