Musical Sands.

MAY I record the existence of musical sands along the shore at the Sandbanks, Poole Harbour?

Some years ago the Poole authorities erected a series of box groynes along this coast between Poole Head and the Haven, and these have considerably increased the natural accumulations of sand, so that it is "making" everywhere, and the growth of the marram grass on the dunes is in many places (independently of that recently planted) rapidly extending seawards.

The beach now, between each groyne, consists of wide and flat deposits of sand, shells, and flint pebbles, but about midway between the dunes and the sea, where the sand is comparatively free from these, musical zones are of frequent occurrence.

In walking along the shore in a westerly direction, starting from the first groyne, the sounding qualities of the sand notably increase. Thus between the first and second groynes there are no musical patches, between the second and third the sounds are very faint, and between each of the other groynes, until one reaches the last at the Haven Point, the intensity of the sound increases. In a small cove at the Point, formed by the last groyne (constructed of barrels of concrete and an old ship), the sand is remarkably musical.

The increase of sound observed when walking in a westerly direction is due, I think, to the fact that the prevailing westerly winds, and the littoral drift, separate the finer particles from the sand and carry them eastwards, and a microscopic examination of samples obtained from distances about a mile apart on this shore confirms this.

This musical sand is of the Studland Bay type, and near the Haven gives even better results than any I have found there. The occurrence of musical sands along this particular shore through the conserving influence of the groynes is an interesting fact, for their existence there previously was very unusual, being only once noted in very small quantity during the last twenty years.

Parkstone-on-Sea, July 4. CECIL CARUS-WILSON.

The Commutative Law of Addition, and Infinity.

REFERRING to the review of Hilbert's "Grundlagen der Geometrie," on p. 394 of No. 2066 of NATURE (June 3), may I point out that the commutative law of addition can be proved without the help of any axioms at all, other than those of general logic? The method, indeed, used by Peano in 1889 ("Arithmetices Principia . . .," Turin, 1889, p. 4), which is only based on axioms of a general nature (such as the principle of mathematical induction), and not on such special laws as the distributive ones, appears in so far superior to Hilbert's; and, since all Peano's axioms were proved in Mr. Russell's "Principles of Mathematics" of 1903, Hilbert's proof seems quite superseded. Further, the difficulties arising out of Dedekind's proof of the existence of infinite systems can be avoided without the introduction of "metaphysical" arguments about time and consciousness (see Russell, *Hibbert Journal,* July, 1904, pp. 809–12), as, indeed, your reviewer seems to think possible. But the connection of the fact that the existence of an infinity of thoughts (which must be in time) with Hamilton's idea that algebra was interpretable especially in the time-manifold, just as geometry is in the space-manifold, is not obvious. PHILIP E. B. JOURDAIN.

The Manor House, Broadwindsor, Beaminster, Dorset, July 2.

NEITHER Dr. Hilbert nor the reviewer make any suggestion that the commutative law of addition is best proved as a deduction from the laws of multiplication. But the laws of multiplication are so often treated as deductions from those of addition that it is interesting to have a case of the converse procedure. The fact that both these operations and their laws have been treated independently and in a strictly logical manner by Dedekind, Peano, and others is of course, perfectly well known to all who have paid any attention to this part of mathematics. Whether Dedekind's critics have really avoided metaphysical arguments without at the same time making metaphysical assumptions is a question on which a difference of opinion is permissible.

G. B. M.

THE THEORY OF CROOKES'S RADIOMETER.

I HAVE noticed that the theory of this instrument is usually shirked in elementary books, even the best of them confining themselves to an account, and not attempting an explanation.[1] Indeed, if it were necessary to follow Maxwell's and O. Reynolds's calculations, such restraint could easily be understood. In their mathematical work the authors named start from the case of ordinary gas in complete temperature equilibrium, and endeavour to determine the first effects of a small departure from that condition. So far as regards the internal condition of the gas, their efforts may be considered to be, in the main, successful, although (I believe) discrepancies are still outstanding. When they come to include the influence of solid bodies which communicate heat to the gas and the reaction of the gas upon the solids, the difficulties thicken. A critical examination of these memoirs, and a re-discussion of the whole question, would be a useful piece of work, and one that may be commended to our younger mathematical physicists.

Another way of approaching the problem is to select the case at the opposite extreme, regarding the gas as so attenuated as to lie entirely outside the field of the ordinary gaseous laws. Some suggestions tending in this direction are to be found in O. Reynolds's memoir, but the idea does not appear to have been consistently followed out. It is true that in making this supposition we may be transcending the conditions of experiment, but the object is to propose the problem in its simplest form, and thus to obtain an easy and unambiguous solution—such as may suffice for the purposes of elementary exposition, although the physicist will naturally wish to go further. We suppose, then, that the gas is so rare that the mutual encounters of the molecules in their passage from the vanes to the envelope, or from one part of the envelope to another part, may be neglected, and, further, that the vanes are so small that a molecule, after impact with a vane, will strike the envelope a large number of times before hitting the vane again.

Under ordinary conditions, if the vanes and the envelope be all at one temperature, the included gas will tend to assume the same temperature, and when equilibrium is attained the forces of bombardment on the front and back faces of a vane balance one another. If, as we suppose, the gas is very rare, the idea of temperature does not fully apply, but at any rate the gas tends to a definite condition which includes the balance of the forces of bombardment. If the temperature be raised throughout, the velocities of the molecules are increased, but the balance, of course, persists. The question we have to consider is what happens when one vane only, or, rather, one face of one vane, acquires a raised temperature.

The molecules arriving at the heated face have, at any rate in the first instance, the frequencies and the velocities appropriate to the original temperature. As the result of the collision, the velocities are increased. We cannot say that they are increased to the values appropriate to the raised temperature of the surface from which they rebound. To effect this fully would probably require numerous collisions. Any general increase in the velocity of rebound is sufficient to cause an unbalanced force tending to drive the heated surface back, as O. Reynolds first indicated. If we follow the course of the molecules after collision with the heated surface, we see that, in accordance with our suppositions, they will return by repeated collisions with the envelope to the original lower scale of velocities before there is any question of another collision with the heated face. On the whole, then,

[1] See for example Poynting and Thomson's "Heat," p. 150.

the heated face tends to retreat with a force proportional both to the density of the gas and to the area of the surface.

A calculation of the absolute value of the excess of pressure cannot be made without further hypothesis. If we were to suppose that the molecules, after collision with the heated face, rebound with the same velocities $(v+dv)$ as they would have were the temperature raised throughout, the pressure would be increased in the ratio $v+(v+dv):2v$ or $1+dv/2v:1$. On the other hand, if the temperature were actually raised throughout, the pressure, according to the usual gaseous laws, would be increased in the ratio $(v+dv)^2:v^2$ or $1+2dv/v:1$. On this hypothesis, therefore, the unbalanced increment of pressure on the heated face is one-quarter of the increment that would be caused by a general rise of temperature to the same amount. This estimate is necessarily in excess of the truth, but it is probably of the right order of magnitude.

The supposition upon which our reasoning has been based, viz. that the mean free path of a molecule is large in comparison with the linear dimension of the vessel, has been made for the sake of simplicity, and is certainly a very extreme one. It is not difficult to recognise that in the extreme form it may be dispensed with. All that is really necessary to justify our conclusions is that the mean free path should be very large in comparison with the *vane*. The magnitude and distribution of the velocities with which the molecules impinge will then be independent of the fact that the face of the vane is heated, and this is all that the argument requires. The repulsion by heat of a silk fibre suspended in a moderately rare gas was, it will be remembered, verified by O. Reynolds.

RAYLEIGH.

LIFE IN AN OASIS.[1]

ALTHOUGH the oases of the Libyan Desert have been frequently visited by travellers—Poncet in the seventeenth century, Browne in the eighteenth century, and Cailliaud, Drovetti, Edmonstone, Hoskins, Rohlfs, Zittel, Schweinfurth, Brugsch, and others in the nineteenth century—yet none of these authors enjoyed anything like the opportunities for the study of these remarkable districts which have fallen to the lot of the writer of the work before us. For nine years Mr. Beadnell, as a member of that active body the Egyptian Geological Survey, was engaged in the study of the Libyan Desert—including the four oases of Baharia, Farafra, Dakhla, and Kharga—while during the last three years, as director of the operations of a development company, he has resided in the last mentioned, and has carried out important observations and experiments in connection with the questions of water supply, the effects of moving masses of sand in increasing the fertility of some areas, while overwhelming and destroying others, as well as of many other problems of great historical and antiquarian interest.

Now that the opening of a railway from Qena, a little north of Luxor, to the village of Kharga has been completed, the long and tedious camel-journey of four or five days along very rough caravan routes is avoided, and excursions from the Nile valley to this typical oasis will doubtless become much more frequent. The appearance of the present work is, therefore, very opportune. The detailed topographical and geological survey of the Libyan Desert with its oases was undertaken in 1897–8. Mr. Beadnell carried out the mapping of the Farafra and Dakhla oases, while Dr. Ball was engaged in surveying that of Kharga, the work in the Baharia Oasis being shared between the two investigators. Dr. Ball's map of the Kharga Oasis, with the accompanying official report, is a work of great geological value and interest, and Mr. Beadnell's residence in the district has enabled him to add not a few important scientific details to the admirable sketch given by his colleague.

The whole Libyan Desert forms a plateau, having an elevation which, at its maximum, is but little less than 2000 feet above sea-level, yet with a fairly general slope towards the north. In this great expanse of rough limestone and flint-covered flats, with hillocks and troughs of drifting sand, the oases are deep depressions, the bottoms of which vary from 100 to 300 feet above sea-level, surrounded, for the most part, by steep escarpments, through which only a few passes can be found which are capable of being used as camel-tracks. The whole of the deserts are underlain by great beds of sandstone (the Nubian series), forming two divisions, the "surface-water sandstones," never more than 160 feet thick, separated by 250 feet of impervious grey shales, from a much thicker series of sandstones below, the "artesian-water sandstone," which has been penetrated by borings to the depth of 400 feet.

It is by the removal, through denudation, of great masses of Eocene and Upper Cretaceous limestones and shales that the "surface-water sandstones" have been exposed on the floors of the oases. These beds are the source of springs, and, since the districts have been occupied by human beings, a great part of the area of the Kharga Oasis was covered by shallow lakes, probably formed by the outflow from these springs. But these great lakes have been gradually dried up, and the constant drain on the limited supplies of water afforded by the "surface-water sandstones" has greatly reduced its importance as a means of irrigation. The accounts of the various deposits laid down in these old lakes, with their interesting contents of worked flint-flakes and pottery, are among the most novel and interesting portions of Mr. Beadnell's book.

Far more important, however, than the surface-water sandstones, as a source of irrigation water, are the "artesian-water sandstones," which, by means of borings, have been drawn upon from the earliest times, and constitute even now a practically inexhaustible means for promoting the cultivation of the oases. On all questions connected with the nature and amount of the yield of the different kinds of wells, the author of this book writes as an authority, and he is able to give the results of numerous ingenious experiments, carried on, in some instances, for many months. That the enormous quantities of water contained in the thick sandstones of the Nubian system have their source, in part in the highlands of Abyssinia, in part in the Sudan, and to some extent in the upper waters of the Nile, where it flows over these pervious sandstones, there can be little doubt, though as to the proportional parts played by these several factors of the supply there is still much room for doubt—a doubt which can only be removed by prolonged observations.

The manner in which the ancient wells have been made, kept open, and from time to time repaired, has engaged the author's attentive study. It is surprising to learn how much has been accomplished with the aid of very simple appliances; and the long subterranean aqueducts—tunnels driven for miles into the sandstones for the purpose of increasing the flow of water—with numerous manholes up to the surface

[1] "An Egyptian Oasis: an Account of the Oasis of Kharga in the Libyan Desert, with special reference to its History, Physical Geography, and Water Supply." By H. J. Llewellyn Beadnell. Pp. x+248; with 28 plates and 4 maps and sections. (London: John Murray, 1909.) Price 10s. 6d. net.