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.