those, the vast proportion, who will have no subsequent regular instruction in the subject, the method and mode of treatment of the author may be heartily commended. It is, we learn, the outcome of actual class work, modified by experience and the mutual play of the minds of teacher and taught. The author has a belief in the especial value of "outdoor chemistry" as appealing to the interests of the learner, and there is an excellent chapter towards the end on plant respiration and nutrition.

One notices a few important omissions and errors. The use of the spectroscopy in chemistry is nowhere alluded to. The mention of argon and its companions in the air should be amplified or omitted. As it is, it contains one of the few mistakes, in the statement that the density of argon is forty times that of hydrogen. Helium is not even referred to by name, surely a remarkable omission for an author addicted to "outdoor chemistry." We read, "The exact specific gravity of oxygen . . . is 15.88 ($H=1$). This makes the atomic weight of oxygen 15.88. . . ." But these few blemishes in no way detract from the general accuracy of the treatment.

It is a pleasure to notice a book of this description, for it indicates the serious and important place chemistry is taking in the school curriculum. It deserves a high place, not only in the school, but generally as an excellent introductory first course, understanding by this term not a mere smattering of the kind deemed sufficient only a few years ago, but a course in keeping with the true position of the science as a serious and profitable part of a good modern education.

Altitude Tables. Computed for Intervals of Four Minutes between the Parallels of Latitude 31° and 60° and Parallels of Declination of 0° and 24° , designed for the Determination of the Position Line at all Hour Angles without Logarithmic Computation. By F. Ball. Pp. xxxii+241. (London: J. D. Potter, 1907.) Price 15s. net.

THE main purpose of these tables is to facilitate the determination of the position line from an observation of any heavenly body and to eliminate, practically, the chance of errors of computation in the result. When the idea occurred to the Rev. F. Ball he consulted the Astronomer Royal, who consented to the employment of several of the Greenwich computers on the work, under the direction of Mr. Crommelin; the accuracy of the tables is therefore beyond suspicion. The tables are computed for intervals of every four minutes between latitudes 31° and 60° and parallels of declination 0° and 24° , and they enable the observer to determine the position line at all hour angles without having to solve any spherical triangle. This does away with the necessity for logarithmic computations, and so the probability of errors is eliminated. For altitudes less than 70° it is expected that the tables will give results accurate within $12''$; for greater altitudes their use is not recommended. The author hopes soon to publish a companion volume for latitudes 0° to 30° .

Problems in Strength of Materials. By Dr. William Kent Shephard. Pp. vii+70. (London: Ginn and Co., n.d.) Price 6s.

Whittaker's Arithmetic of Electrical Engineering for Technical Students and Engineers. Pp. vii+159. (London: Whittaker and Co., n.d.) Price 1s. net.

To set students of applied science to work numerical problems involving thought in their subject is a good test as to whether they understand the principles involved. It is customary in many classes to associate the laboratory and lecture work with practice in

solving such problems, and teachers will find in these volumes many examples suitable for the purpose. The first volume, in addition to 568 exercises, contains some useful tables, but little in the way of worked-out examples to guide the student is given and no answers are provided. The second book, on the other hand, contains seventy-two typical problems fully solved, and a set of answers.

An Essay upon Disease: its Cause and Prevention.

By Dr. G. E. Richmond. Pp. 96. (London: H. K. Lewis, 1907.) Price 2s. net.

THE main object of Dr. Richmond's little book is to point out the large number of diseases either spread by food or directly due to impurities in food or articles in common use. It is surprising to find a confession in the preface to the effect that "the essay has been written rather hurriedly," and unfortunate that no index is provided.

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.]

Radium and the Earth's Heat.

IT has been shown by the Hon. R. J. Strutt and other investigators that the materials composing the surface of the earth contain on the average about 10^{-12} gram of radium per gram, while about one-twentieth of this amount is sufficient to account for the heat lost by the interior of the earth by conduction. Mr. Strutt has therefore suggested that the interior of the earth contains less radium per gram than the surface. It is interesting to calculate what would happen if the whole earth contained 10^{-12} gram of radium per gram. If the specific heat of the interior of the earth is taken to be 0.1, and 1 gram of radium is supposed to generate 100 calories per hour, then it is easy to show that the temperature of the interior of the earth would rise by 10^{-5} degree C. per year if all the heat generated by the radium were used up in raising the temperature.

If the temperature of the interior rose 100° C., it is probable that the rise near the surface would be too small to be detected, so that observations extending over at least ten million years would probably be necessary to detect the effect of the heat generated by radium even if the whole earth contained 10^{-12} gram of radium per gram and all the heat generated went to raise the temperature.

I understand that geologists are inclined to think that the temperature near the surface of the earth has not altered much for many million years, but this is not inconsistent with a rise of 10^{-5} degree C. per year in the interior. It is, of course, quite possible that the specific heat of the earth is considerably greater than 0.1. The high pressure in the interior probably makes the specific heat larger than at the surface.

HAROLD A. WILSON.

I MENTIONED the possibility which Prof. Wilson discusses in my original communication on radium in the earth's crust in the Royal Society's Proceedings; but he certainly shows that it is more worthy of attention than I then thought it.

On this theory it becomes necessary to suppose that the primary stock of radio-active material in the earth—uranium—has not been in existence for a longer time than is required for the attainment of thermal equilibrium by conduction, for we know that the uranium is wasting away, and unless the supply is replenished it is clear that the gradient of temperature must diminish, instead of increasing as Prof. Wilson supposes. We cannot at present form any notion as to how the uranium could come into existence, so that any further development of the idea

would have too little contact with ascertained fact to be of much value.

I will take this opportunity of mentioning an alternative theory, which has the advantage of being amenable to experimental test. If we suppose that the rate of transformation of uranium is much diminished by increase of temperature, the quantity of radium and of all the other products will be diminished too, and with it the general rate of heat production inside the earth.

The effect of heat on radium and its products has no direct bearing on the problem. Everything is governed by the primary slow transformation—that of uranium.

There is no experimental evidence on this question so far as I am aware. It could probably be best attacked by comparing the ratio of formation of uranium X at various temperatures. The amount of uranium X which had grown in the course of a few days could be determined by β -ray measurements, which might be made after cooling.

R. J. STRUTT.

Sunnyside, Cambridge, February 13.

Ground Ice.

I SEE in your issue of January 30, p. 295, a letter from the Rev. John J. Hampson asking some questions on the subject of ground ice. I should like to say that my father, the late Prof. James Thomson, read a paper on this subject at the Natural History and Philosophical Society of Belfast on May 7, 1862, and I think his paper answers most of the questions. Thus he writes, after reviewing and setting aside several older theories:—"My own view is that the crystals of ice are frozen from the water at any part of the depth of the stream: whether the top, the middle, or the bottom, where cold may be introduced, either by contact or radiation; and that they may also be supplied in part by snow or otherwise: and that they are whirled about in currents and eddies until they come in contact with some fixed objects to which they can adhere, and which may perhaps be rocks or stones or may be pieces of ice accidentally jammed in crevices of the rocks or stones: or may be ground ice already grown from such a beginning."

"That pieces of ice under water have the property of adhering to one another with a continually increasing firmness, and this even when the surrounding water is above the freezing temperature, has been shown in a set of very interesting experiments by Prof. Faraday. I think too that the ready adhesion to the bottom, or to ice already anchored there, may possibly be increased by the effects of radiation, but I am confident that the anchor ice is not formed by crystallisation at the place where it is found adhering."

This paper has never been printed *in extenso*, but I hope soon to bring it out in a volume of collected papers written by my father.

JAMES THOMSON.

22 Wentworth Place, Newcastle-on-Tyne,
February 11.

The Stresses in Masonry Dams.

MR. MARTIN at first asserted that my reasoning was wrong on some general principle which I failed to grasp, whereas he has now fallen back on the order of the approximation, and appeals to what he terms an axiom of practical mathematics, which he illustrates by the statement that between 0 and π a parabola can be found "differing but little from $\sin x$." If by the method of least squares a parabola be fitted to $\sin x$, it will be found to differ by more than 30 per cent. from the ordinate of $\sin x$ when $x=5^\circ$; whether that difference is material or not depends entirely on what use is to be served by the correspondence.

In the memoir which has led to this controversy I showed that the equation for the stress function V , i.e. $\nabla^2 \nabla^2 V = 0$, was the same for a thin slab and an actual dam. Since writing the paper I noticed that the third equation for the stresses was *apparently* not the same. I now see that this is only in appearance, for the terms

¹ Only thick plates can be properly used in dam experiments, for thin plates buckle and require a side support which destroys accuracy of experimental result. Even Messrs. Wilson and Gore's plates were at the toe as thick as they were broad.

which have a coefficient involving different functions of Poisson's ratio for the two cases are

$$\left(\frac{d^2}{dx^2} + \frac{d^2}{dz^2} \right) (xx + zz)$$

and I find that this vanishes by means of the differential equation for V . Hence, as I stated in my memoir, thin plates can be used to find experimentally the stresses. Mr. Martin is therefore quite correct in his views on this point, although I cannot still agree with his demonstration of the principle.

There are, however, far more vital criticisms to be made of the memoirs recently read before the Institution of Civil Engineers than the mere question of whether the stresses in a slab and an indefinitely long dam differ by 10 per cent. or 20 per cent. A very little experimenting will suffice to show that dams when they collapse go by *stretching*, and partly at points where there may be no tension at all. The strains measured by Messrs. Gore and Wilson are not those in a real dam at all, and if we now accept the view that the stresses are the same, then we must ask Mr. Martin to allow that their stretches differ by 30 per cent. from those in an actual dam.

It was this point which I endeavoured to bring out in the criticism of the paper to which Mr. Martin has referred. If their strains correspond to those of a real dam, then their stresses differ widely; if their stresses are correct, then their strains, upon which ultimately rupture depends, will be very different from those of the actual dam. I must leave Mr. Martin to choose his own horn of the dilemma.

Again, there is another point which is physically very obvious. If a dam, reservoir empty, were split up by a series of vertical divisions parallel to its length, each plate would be of different height, and compressed under its own weight would be subjected to a different squeeze at the base of the dam. To bring these vertical sheets into contact at the correct points it is needful to suppose shear over the vertical planes at the base of the dam. In other words, there must be a distribution of shear over the base of the dam due solely to its own weight. Since the total shear over the base is zero, this distribution of shear, if the extremity of the toes be vertical, must take some such form as is shown in the diagram. Our experi-

ments at University College showed that this base shear due to the weight of the dam only was as important as, and probably more important than, the distribution of shear due to the water pressure.

There is no evidence at all that I can see in Sir John W. Otterley and Dr. Brightmore's recent paper that they have paid attention to this point. They speak of the "original vertical lines on the model," and of measuring the displacement of these lines from "vertical lines on the glass." They speak of the return of the vertical lines on the model to the vertical lines on the glass on the removal of the water pressure. It would appear, therefore, that they have only measured the slide due to water pressure. But to deduce the stresses in the dam they must have the total shear, that due to the weight as well as that due to water pressure. I can find no evidence in their paper of any determination of the shear due to the weight of the dam. They say that the shear along the base is uniformly distributed. This, as Mr. Pollard and I showed in our memoir of last July, is *roughly*, but only *roughly*,