described by Gumbel occur in depths of over 2000 fathoms when near to land, while a Globigerina ooze or Pteropod ooze may occur in very shallow depths, in the tropics, far from land. These deposits of the *Drache*, being near the coast, it is found that quartz predominates. The fragments of plagioclase, orthose, hornblende, augite, bronzite, mica, garnet, tourmaline, diroite (is it not glaucophane?), magnetite, zircon, chlorite, all come from the disintegration of the ancient rocks which form the coast of Norway and Scotland. Gumbel also finds fragments of granitic rocks, dioritic rocks, &c. Fragments of modern volcanic rocks, such as lavas and pumice, are very rare when compared with the particles derived from ancient rocks. Glauconite was found in some of the specimens, and the author believes that these have been transported, which is quite unlikely, as large deposits of glauconite are now in process of formation along the coasts of the north of Scotland. The organisms—mollusks, echinoderms, foraminifera, and diatoms—are all the same as those usually found in partially inclosed seas like the North Sea, and do not present any peculiarities worthy of note.

The author supposes that there is a continuation under the North Sea of the ancient rock-masses of Scandinavia. This may be true, but the supposition can in no way have been suggested by the chemical, microscopic, and mineralogical examinations of the deposits of the North Sea. In conclusion, Gumbel states that the sediments of the North Sea prove that sandy deposits can be formed alongside of clayey and marly deposits, during the same time in the same sea. This conclusion has already been perfectly established, and this confirmation supports an interpretation generally received, which was one of the first results of the examination of the *Challenger* deposits.

The Hydrographic Office of the German Admiralty have done excellent service in taking up the scientific examination of the North Sea. It is a work that we would like to see continued and advanced by our own Hydrographic Office.

J. M.

OUR BOOK SHELF

Chemical Arithmetic. By Sydney Lupton, M.A., F.C.S., F.I.C. Second Edition. (London: Macmillan and Co., 1886.)

WE are pleased to note a new edition of this excellent work, in which several improvements have been made. The hundred pages of introductory matter in the first edition have been reduced by about one-half, much unnecessary pure arithmetic having been cut out. The 1200 examples with answers are, on the whole, well selected, though many of them can scarcely be called chemical. A greater number of typical examples might advantageously have been worked out at full length.

The book is especially to be commended for its clear and concise definitions, which are in many books very loosely expressed. The differences between density and specific gravity, atomic and molecular weights, for instance, are explained in a manner that any student of ordinary ability will readily understand. We feel sure that the book will be appreciated alike by students and teachers, but it will be especially valuable to teachers.

Experimental Chemistry. By C. W. Heaton, F.I.C., F.C.S. New Edition, Revised. (London: George Bell and Sons, 1886.)

ANOTHER edition of this work on experimental chemistry, adapted from the German of Dr. Stöckhardt, has

just been issued. To those students of limited means who desire to work at chemistry as well as to read it—and it is for those that the book is intended—it will be found useful. The introduction, however, is much too extensive and theoretical for beginners, and we fear that many would be disheartened before reaching the really experimental work. In our opinion, the book is not sufficiently practical, many experiments lacking detail. We would suggest that in future editions a few pages be devoted to instructions in the manipulation of apparatus and the working of glass.

Part IV., which is devoted to organic chemistry, is very clearly set out. The book is not sufficiently modernised for these days of competitive examinations, but the teacher who is desirous of encouraging his students to perform simple experiments in spare moments would get many valuable ideas from it.

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. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Sense of Smell

IN your issue of September 30 (vol. xxxiv. p. 521) your correspondent Dr. Arthur Mitchell is desirous of obtaining some data in regard to the sense of smell. In a paper presented at the Philadelphia meeting of the American Association for the Advancement of Science (1884) we have described a series of experiments designed to test the delicacy of this sense. These experiments, being of a preliminary character, have hitherto been withheld from publication, but the following brief statement of the results obtained may be of interest to Mr. Mitchell and to other readers of NATURE. We made use of the following substances:—(1) oil of cloves, (2) nitrite of amyl, (3) extract of garlic, (4) bromine, (5) cyanide of potassium. A series of solutions of each of these was prepared, such that each member was of half the strength of the preceding one. These series were extended by successive dilutions till it was impossible to detect the substances by smell. The order of the bottles containing these solutions was completely disarranged, and the test consisted in the attempt to properly classify them by the unaided sense of smell. The thirty-four observers who assisted in these experiments were of both sexes; the results are indicated in the following table (I.):—

		<i>Amount detected</i>				
		Oil of cloves	Nitrite of amyl	Extract of garlic	Bromine	Cyanide of potassium
Average of 17 males	1 part in 88,218 of water	1 in 783,870	1 in 57,927	1 in 49,254	1 in 109,140	
Average of 17 females	1 part in 50,667 of water	1 in 311,330	1 in 43,900	1 in 16,244	1 in 9,002	

The same method of investigation has since been followed by one of us¹ in some experiments, the results of which are given in Table II.

in Table II.

<i>Amount detected</i>			
	Prussic acid	Oil of lemon	Oil of wintergreen
Average of 27 males	1 part in 112,000 of water	1 in 280,000	1 in 600,000
Average of 21 females	1 part in 18,000 of water	1 in 116,000	1 in 311,000

¹ "Some Special Tests in Regard to the Delicacy of the Sense of Smell," by E. H. S. Bailey and L. M. Powell (*Proc. Kansas Acad. of Science*, vol. ix.).

Many striking individual peculiarities were noticed in the course of the experiments, which these general averages fail to show. Three of the male observers were able to detect one part of prussic acid in about 2,000,000 parts of water. Two of these were persons engaged in occupations favouring the cultivation of this sense. Careful chemical tests failed to show the presence of prussic acid in several of the more dilute solutions, in which it could be detected by the sense of smell. We found some of both sexes who absolutely could not detect prussic acid even in solutions of almost overpowering strength. There were several instances of the same peculiarity as regards bromine. Again, our averages show that the sense of smell is in general much more delicate in the case of male than of female observers.

EDWARD L. NICHOLS
E. H. S. BAILEY

University of Kansas, November 4

Tidal Friction and the Evolution of a Satellite

ADVERTING to the correspondence in NATURE (vol. xxxiv. p. 286), I think that Mr. Darwin has not, so far, fully realised the results that would follow from the circumstance that the Martian satellite's period would be affected many hundred times more than that of the planet's rotation, as explained in the correspondence referred to. He argues that, the moon's mass being great, she should recede to an enormous distance before there will be a reversal of the direction of her tides on the earth; while the satellite of Mars, being very small, need only to recede a short distance before a similar tidal reversal ensues. No mention being made of any other supposed difference in the systems at the starting-point, it must be inferred that other things are supposed about equal. But, as a matter of fact, the present position of the Martian satellite is incompatible with an initial rotation of its planet anything like so great as that ascribed to the earth at a like stage. If Mars be supposed to rotate ten times while the satellite, at its present distance, makes nine revolutions, the satellite's period would still be affected or **lengthened much more** than would that of the planet's rotation. The difference between the periods of revolution of the planet and satellite would increase quickly at first, but more slowly as the satellite receded a certain distance, till at a certain time there would be no increase, after which there would be a decrease, and finally a reversal. When the satellite would have receded to a short distance, where she would revolve in the same period as Mars now rotates in, the planet would have lost but little of its original rapid rotation. Now, supposing the satellite tide to go round in the same time as the solar one, the period of the satellite would be affected about thirty times as much as that of the planet's rotation. Allowance being made for the comparative slowness of the satellite's tides, the satellite's period would still be **changed more than ten times** as much as that of the planet. It would be only when the little body got further out, and the planet's rotation slower than it now is, that there could be a reversal of the direction of the satellite's tides. Wherever started, the satellite must either go directly into the planet, or go out a short distance and back into the planet, before the rotation-period can have been much changed by solar tides; or else the satellite must go far out—as when it gets a fair start—and could not possibly turn back until the rotation of Mars be slower than now. Hence it seems that under no conditions could the rotation of Mars, at the birth of her moon, have been twice as rapid as now, and the evidence is very strong that the rotation-period could not have been changed more than a very few hours, if so much. Then, if the rotation of Mars was so slow in the beginning, and so little changed during the whole existence of the satellite, the circumstance does not support the view that the earth's rotation was very rapid in the beginning and so much changed during her past history, but rather inclines the other way.

Respecting the statement that two heavenly bodies cannot revolve about their centre of inertia as parts of a rigid body with their surfaces nearly in contact, unless one be smaller and denser than the other by a certain amount, I can only say, at the present time, that such was the conclusion at which I arrived when investigating the results of the tidal effects of two bodies on one another at close quarters. Without going far into the question, it can be seen that if the rule holds when the two bodies are of the same size and density, it will hold throughout. There will be no difficulty in seeing that the rule holds so far that when the difference in size between the bodies is as great as

between any of the satellites and its primary, the small body must be invariably the denser. Now the argument that was supposed to apply in general would at least apply in the case of the solar system. That argument, as explained in my pamphlet, was that, if a rapidly-rotating body were to separate into two, the small body given off must be denser than the other to withstand the tidal disturbance, and that it would be impossible for the small body to be denser than the primary, since the secondary body must be formed from the surface and therefore lightest part of the other body.

JAMES NOLAN

Dergholm, Victoria, October 5

Seismometry in Japan

I HAVE read, with no small surprise, a paragraph in NATURE of November 11 (p. 36), giving a summary of a letter from Prof. John Milne, with reference to an article by me on the seismographs now manufactured by the Cambridge Scientific Instrument Company. Prof. Milne is represented as saying that, "with the exception of one or two which have been modified, a set of instruments like those recommended by Prof. Ewing are, so far as Japan is concerned, quite obsolete." His letter is not published, and it is possible that the paragraph inadvertently does him an injustice in making him assert what has absolutely no foundation in fact.

In any case the statement cannot be allowed to pass without contradiction. My seismographs have been in regular use at the University of Tokio since they were invented; they are now used for systematic observations by the Japanese Meteorological Bureau; they were sent last year by the Japanese Government to the Inventions Exhibition in London, where they were awarded the highest diploma among Government exhibits: one of them, the comparatively cheap and simple duplex pendulum seismograph, is employed by many private observers in Japan. In a letter received only a few weeks ago, my friend and former assistant, Mr. Sekiya, now Professor of Seismology in the University, says:—

"We are going to start a journal called the *Journal of the Science College of the Imperial University, Japan*. In the first number I will give a paper on 'Comparison of Earthquake Diagrams simultaneously obtained at the same station by two instruments involving the same principle, and thereby proving the trustworthiness of these instruments.' Of course I treat those diagrams recently obtained by two of your seismographs."

Other letters from Prof. Sekiya are full of accounts of the excellent work he is doing with these instruments, and of their continued and extended usefulness in his very able hands. A paper lately received from him describes a rough but effective form of the duplex pendulum, cheaply made in order to bring it within the reach of private observers, and with reference to this the *Japan Mail* of February 2, 1886, says:—

"The duplex pendulum seismograph designed by Prof. J. A. Ewing, has been employed for earthquake observations in the Tokio Daigaku by Mr. S. K. Sekiya, who has improved many of its details during his long use of the instrument. On account of the simplicity and scientific nature of its construction, and its easy management, it has found its way into the hands of many observers."

The *Mail* goes on to mention the name of a native firm by whom the instrument is made and sold. In March last Mr. Sekiya writes:—"The duplex pendulum sells well; some fifteen or twenty of them have been sold."

So much for the duplex pendulum seismograph, which is one of those described in my article, and now made with the utmost refinement of construction by the Cambridge Company. The other is a three-component instrument, of which the principal part is the horizontal pendulum seismograph—consisting of a pair of horizontal pendulums for recording separately two rectangular components of the horizontal motion of the ground on a moving surface driven by clockwork. This method of recording earthquakes was introduced by me in 1880 (*Trans. Seis. Soc. Jap.*, 1880; *Proc. Roy. Soc.*, No. 210), and has been in regular use ever since. The instruments made to my designs by native workmen are still doing good service in Prof. Sekiya's hands. Those now made by the Cambridge Company have the advantage of better workmanship and an improved arrangement of parts. As Prof. Sekiya has recently written to me with regard to the purchase of a set of them by the Japanese Government, it is probable that Mr. Milne will before long have