when in flood, or ·79 lb. to a cubic foot equal to 1/80th part in weight of the water in the tubes.

Both sea water and water saturated with ordinary salt were tried, the latter in the proportion of one pound of salt to a cubic foot. There was no appreciable difference between these.

The samples were placed in glass test-tubes 1 foot long and ½ inch in diameter, filled with clean water up to the ten inch mark.

The material was well shaken and incorporated with the water, and the time given for settling is that taken by the particles to settle through 10 inches and become visible in a solid form at the bottom of the tube, and when no more particles could be discerned as settling when the tube was held up to the light.

The column "water clear" is that in which the water in the tube had become sufficiently transparent for black marks on a white ground to be discerned through it.

Practically all solid matter had settled in the time given in the first column. The quantity deposited between the interval of "settling" and "clear" was almost inappreciable, but still sufficient to keep the water discoloured. With the specimens containing the coarser material the water became bright again in the time given in the second column, but with the very fine material intervals varying from two to three hours up to as many days elapsed before the water became as bright as it was before the solid matter was added, partly depending on the fineness of the material, but due more to the staining quality of some of the ingredients contained in the sample. Thus the material taken from Tilbury Dock Basin turned the water a black colour which took some time to clear. The salt water took much longer to become bright again than the fresh.

Samples were selected as fairly representing the material brought down in suspension by rivers, or eroded from the sea cliffs, and deposited either in the form of salt marshes or transported to the bed of the sea.

Thus numbers 1 and 2 represent the sand found on the foreshores of the sea coast and covered at every tide; 3 and 4, material derived from chalk cliffs; 5 and 6, the material in suspension in the rivers Ouse and Trent, of which the Warp lands bordering on those rivers are composed, 5 being the material first deposited and near the river, and 6 that further away where the water remains quiescent for some time; 7 and 8 represent the material of which salt marshes are composed, 7 being the silt deposited on the sand, and on which, when it rises to about neap tide level, 6·68 above ordnance datum, samphire begins to grow, 8 the finer warp deposited from about the level of mean high water to that of ordinary spring tides, or 10·21 to 13·34 above ordnance datum, on which salt water grass grows; 9 is alluvial matter chiefly derived from the erosion of clay banks, brought up by the tides and deposited in Boston Dock, whence it was dredged, elevated from the barges and discharged with a current of water on to low land, the sample being taken at the part furthest away from the place of deposit; 10 was taken from the "batches" on the banks of the river Parrett at about half-tide level of spring tides, or 13·67 above ordnance datum, where the finest part of the alluvium in the river settles and which is collected for making bath bricks; 11 was taken from Tilbury Dock Basin on the Thames when the water was being stirred up by the eroding pumps; 12 is from clay used for brick making; 30 per cent. of the particles of this material were from 1/800th to 1/10000th inch in diameter and the remainder smaller than this, the average size being 1/1600 of an inch.

W. H. WHEELER.

The Subjective Lowering of Pitch.

IF the subjective effect described by Mr. E. Hurren Harding (*ante*, p. 103) is of general occurrence, it is contrary to what one might expect from the observation of singers.

It is well known that persons with a good ear may sing flat, being unconscious of the defect, though they would notice it immediately in other singers. From this it seems that the singer's voice sounds *higher* to himself than to others, and yet it is *louder* to him than to any one else. Sharp-singing, on the other hand, is regarded as more indicative of a defective ear.

I have no large tuning-forks at hand, but with ordinary forks and the sound-board of a piano I find that, on bringing the ear close to the source of sound, the sense of pitch is not altered, though the elements of noise are added to the sound; and these elements consist mostly of vibrations of lower pitch, presumably the proper notes of parts of the auditory apparatus.

In connection with this subject it may be noted that, owing to the structure of the cochlea, vibrations of small amplitude affect chiefly the lower part of its spiral; and that as the amplitude increases (independently of pitch?) the concussion reaches further up the spiral, where the fibres of the basilar membrane are longer than in the lower part, and therefore more responsive to slower vibrations.

F. J. ALLEN.

Malvern, June 9.

WITH reference to Mr. Harding's letter (p. 103), it would be interesting to know whether the effect he has observed with the voice, with tuning-forks and with organ pipes can also be obtained from a siren.

G. W. HEMMING.

YES; such effect can be obtained from a siren. If a siren be so rotated as to give a note approximating to middle C, the note appears flattened when the ear is placed close to the instrument, such flattening being estimated by different observers (at different times) as from a semitone to a whole tone.

Normal College, Bangor. E. HURREN HARDING.

THE NATIONAL ANTARCTIC EXPEDITION.

NO answer, so far as we are aware, has appeared to Prof. Poulton's letter to the Fellows of the Royal Society on Prof. Gregory's resignation of the leadership of the scientific staff of the Antarctic expedition (of which we published a copy on May 23). We are therefore forced to conclude that the representatives of this Society on the Joint Committee are content (to use our own words) to let judgment go by default, and admit Prof. Poulton's statements to be substantially correct. Since that date, according to a second letter which we published last week, rumours have been circulated that the real cause of Prof. Gregory's resignation was not that which had been publicly stated, but domestic considerations. The dates given to Prof. Poulton's statements and extracts from letters written by Prof. Gregory (which documents we have been allowed to examine) show these rumours to be baseless, and how they have arisen is no less a mystery than that alteration in the minutes of a resolution passed by the Joint Committee on February 14, 1900, mentioned in Prof. Poulton's former letter. Prof. Gregory's position has been consistent and definite throughout. He accepted the offer of the post on certain conditions, which he believed himself (not unreasonably, in our opinion) to have made clear. On returning to England last December he found the situation had been altered. Though not liking the changes he decided to accept them, and naturally supposed when he left England last February that the arrangement, concluded the day before he sailed, would be final. On receipt of a cable message that it had been further modified (by the acceptance, in substance, of Mr. Darwin's proposition), his first impulse, as he states, when the news arrived was to send a telegram announcing his resignation; but, after reflection, he thought it wiser to await the receipt of particulars by letter. Then came the refusal of the Council of the Royal Geographical Society to accept the instructions, thus modified, the appointment of an arbitration committee, as we may call it, and their decision, which virtually endorsed the action of that Society. When Prof. Gregory was informed by telegraph of the last step he at once cabled his resignation. We do not see how he could have done otherwise. There was now, to use his own words, "no guarantee to prevent the scientific work from being subordinated to naval adventure, an object admirable in itself, but not the one for which I understood this expedition to be organised."

Prof. Gregory, some experts have pleaded, is unreasonable in his expectations; the rules of the Service necessitate the complete autocracy of the naval officer in command. We content ourselves with the reply that if

this be so it is only one more instance of the deleterious effect of red tape in this country, of which we have just received, in the case of the War Office, so impressive, we may say so humiliating, a lesson. Others may ask: Why could not Prof. Gregory have shown a more trustful spirit and sailed in the *Discovery*, believing all things and hoping all things? There are limits even to faith. Had the commander of the expedition been a man distinctly his senior, already accustomed to scientific voyages, with some experience of polar exploration and those special problems which may be solved by the Antarctic expedition, Prof. Gregory might have ventured to dispense with securities and to feel confident that the interests of science would not be subordinated to the more showy work of adding new capes and islands to the map. But is this the case? The commander of the *Discovery*, we are informed, was, not many months ago, torpedo-lieutenant on a man-of-war, has had no experience in either Arctic or Antarctic seas, is no doubt well versed in those subjects of which a knowledge was demanded by his former post, may possibly be thoroughly competent to direct magnetic observations, but he has not as yet won the slightest reputation as a naturalist, a geologist, or an investigator of glacial problems. The last two qualifications are of exceptional importance in this expedition. They cannot be acquired on the voyage out, even by the help of a tutor; they demand, not only book learning, but also much practical experience. This Prof. Gregory possesses in an exceptional degree. He knows where a search for fossils will be the most hopeful and what will be of most value to palæontologists. He has mastered the literature of glacial questions, and he has studied glaciers themselves, in the Alps, Spitsbergen and elsewhere. He has travelled much, and on his notable expedition to Mount Kenya displayed powers of organisation, calmness in critical circumstances, physical endurance and moral courage which gave him at once a high place among explorers. He has a reputation to lose. Can he be expected to imperil that by absolute surrender to one who is probably his junior and is without experience in the branches of science of which he is a master? As Prof. Gregory truly remarks: "The position gives no power to secure a fair opportunity for work to the man who would have to bear the blame for scientific failure." It was not the position which he had originally consented to accept, it was not that which was agreed upon when he left England last February; so, perceiving that he was no longer supported by those whom he had regarded as representing the interests of science, he promptly withdrew from an untenable position.

The action of those representatives (or rather the majority of them) is inexplicable. They have worked, we hear it said, in the interests of peace. But there are occasions when even peace may be too dearly bought. "There must be give and take," one of them pleaded at a notable crisis. Certainly, but it has been all give on the one side and all take on the other. "The Council of the Royal Geographical Society," it was urged, "were acting within their rights when they rejected the instructions, as modified by their secretary." Certainly, so is one nation doing when it breaks off diplomatic relations with another, and in this way their action should have been regarded by the Council of the Royal Society. That body, or its representatives, seem to have adopted, at least during the present year, "a peace at any price" policy. In consenting to the appointment of a committee of arbitration they meekly accepted a snub, and in designating its members they exposed themselves to defeat. We have the highest respect for their nominees individually, but not one of them is a recognised expert in those branches of science the interests of which were most at stake. The other half of the court consisted of geographers—that is, of men who were really counsel for that side—and yet this court called no scientific experts to plead before it, though this had been virtually promised, but promptly gave its decision. Time would be saved if this practice were imitated in our law courts, but whether justice would be promoted is another question.

It has, however, been asserted that the Royal Geographical Society ought to be allowed a preponderant influence in the organisation of this expedition, because it had provided, directly or indirectly, most of the funds. This difficulty, however, is so obvious that it should have been foreseen at the outset, and the Royal Society have been careful to protect itself from being forced into a false position by inequality of contribution.

But we may go on to ask, does the Royal Geographical Society flatter itself that the Government would have made that grant of 45,000*l.* if its application had not been so energetically and heartily supported by the Royal Society? We venture to be sceptical on this point, and so to affirm that it was the duty of the latter body, at any rate after the rejection of the amended instructions by the Council of the Royal Geographical Society, to have announced that, while wishing all success to the Antarctic expedition, it could no longer be responsible for the guidance of its scientific work or the expenditure of public money. Now it must be content to follow whither the geographers lead. It will receive little honour for any successes, but will have to bear much of the blame if the scientific results are of small value. Its representatives have not afforded, as Prof. Poulton complained, to "the claims of the scientific chief in an expedition undertaken to do scientific work . . . that unflinching, undivided and resolute support" which not only he, but also those who set science above even geographical exploration, had expected and desired.

THE TELEGRAPHONE.

A DESCRIPTION of the telegraphone—the remarkable recording telephone invented by Herr Poulsen—was given in these columns in August last (vol. lxii. p. 371). At that time the instrument was on view at the Paris Exhibition, and though we were able the explain the principle upon which it was designed we could give no detailed description of the actual instrument, nor had we ourselves been able to test its powers. Since then it has been brought to England and has been exhibited at the Royal Society and at other places, where it has deservedly attracted a very large amount of attention. A further description may, therefore, in the circumstances be acceptable.

Herr Poulsen's invention fully deserves to be called one of the most astonishing that have been made of late years. That the delicate vibrations of the human voice could be changed into variations of an electric current and thus be transmitted over a distance and reproduced at the far end came as a surprise to men of science a quarter of a century ago. With no less surprise do we learn today that these telephonic currents, small though they are, can yet be used to create permanent magnetic fields in a steel wire, which will thus be made to serve as a tablet on which to write one's speech. It is not to be wondered at that when first Herr Poulsen's discovery was announced many were incredulous as to its genuineness; the invention is precisely of the kind that one does not believe could be practical until one has actually seen or heard it in operation. That it will have the effect of putting the phonograph on an entirely new basis no one who has heard it can doubt. The speech reproduced by the telegraphone is almost as much superior to that reproduced by the wax cylinder phonograph as are the living pictures of the kinematograph to those of the zoetrope. There is none of the very unpleasant twang inseparable from the