The Sound of Distant Gun fire.

WITH reference to the letter of Father Schaffers in *NATURE* of March 10, it is certainly a fact that sounds from moderate distances are heard most plainly when there is a wind reversal at a moderate height and when the upper wind comes from the same direction as the sound. At this place the sound of firing off the east end of the Isle of Wight is heard best when a south wind is blowing over a light wind from some other quarter. As regards conditions when the sound of gun-fire from the Front was heard in this country, I do not altogether agree with what Father Schaffers writes. He says that sound-waves are bent upwards "when temperatures are diminishing and the strength of a head wind is increasing with altitude. The former is at its maximum efficiency in summer, when there is a steep gradient over the surface of the earth; the other is nearly always a characteristic of air-flows, since, as a rule, friction against the soil retards the lower strata."

Father Schaffers goes on to say that temperature inversions at moderate heights are rare in summer, and that at all seasons a wind between south-west and north-west—that is, a head wind for sounds coming towards this country from Flanders—generally occupies the whole height of the troposphere. But with anticyclonic weather and with easterly surface winds these conditions are not always realised, and I am under the impression that it was chiefly in such weather that the sounds were best heard. There are certainly many occasions when the temperature gradient is very slight in clear, anticyclonic weather; and in an easterly wind there is often a sharp increase of velocity up to 1 km. or 2 km. before any decrease takes place. Moreover, it often happens in summer, and in other seasons, that no westerly wind is met with at any height up to the top of the troposphere.

There are, therefore, it seems to me, many occasions when a sound-wave might be refracted downwards by an easterly wind and reach the surface a considerable way to the west of the source. Sound-waves that went up at a fairly high angle might get through the strongest part of the easterly wind and never reach the surface, but those which went up at a less angle would be refracted and never get through the easterly wind. I am inclined to think that any cause which occurs to make sounds to be heard at great distances must operate fairly low down in the atmosphere; if the waves went to a great height before being bent down the sounds would seem to come from high up, whereas my experience was that they seemed to come from somewhere near the horizon. If this is the experience of others it should rule out the hydrogen-atmosphere theory; a sound-ray which went up to 100 km., say, and was thence refracted down to the surface at a distance of 200 km. from the source would come down at an angle of 45° , and such sounds would have been attributed by ordinary observers during the war to some aerial fighting.

The question of the propagation of sound-waves in the atmosphere has been very fully dealt with by Mr. S. Fujiwhara (*Bulletin of the Central Meteorological Observatory of Japan*, vol. ii., Nos. 1 and 4). Mr. Fujiwhara maintains that the abnormal propagation of sounds to great distances, silent regions, and regions of double audibility depend on the wind structure of the atmosphere, and that sound-waves may be reflected in certain conditions of a heterogeneous wind structure. He has taken certain cases of wind structure revealed by pilot-balloon ascents at Ditcham, and

has calculated theoretically the regions of audibility which should be found under the conditions existing at the time; he finds that these agree fairly well with the size and shape of the areas of audibility of explosions of the volcano of Asamayama. He also maintains that the wind structure of the atmosphere at the time of an explosion may be deduced from the areas of audibility.

C. J. P. CAVE.

Ditcham Park, Petersfield, March 21.

Sound Transmitted through Earth.

THE letters from Mr. C. Carus-Wilson and Dr. Charles Davison in *NATURE* of March 24 prompt me to give the following experience

In June, 1903, I was trekking towards the Victoria Falls. On the night before arrival we "outspanned" some twelve miles to the south, and on retiring to rest on the bare ground I became aware of a curious, rhythmic sound, quite distinct when my ear was pressed against the soil. I told my two brothers, who found they also could hear the pulsation, and one of them suggested that it must be due to the booming of the distant cataract.

To me the most interesting point is not that the sound was transmitted by the earth, but that it was transformed into rhythmic vibration—very different from the constant roar one hears when close to the Falls. Some process of interference would seem to occur and give rise to this result.

REGINALD G. DURRANT.

Rosetree, Marlborough, March 26.

X-rays and their Physiological Effects.

THE death of my brother, Dr. Ironside Bruce, from a hitherto unsuspected danger in the use of X-rays by medical men for purposes of treatment and diagnosis has an aspect other than its personal or medical one. I only write to *NATURE* because I feel impelled to address an appeal to workers on the purely physical research problems connected with X-rays. I suggest that there is a need for closer association between the latter and medical men practising radiology. The advance in medical knowledge which the X-ray has rendered possible has been immense, and it is becoming practically indispensable in the diagnosis of disease. But it is now clear that its use by practitioners may be curtailed unless some more effective measures of protection for radiologists can be devised.

On many occasions my late brother expressed to me his difficulty in obtaining precise physical knowledge bearing on the nature of the rays and their effects on human tissues. Not many days before his death he returned to this subject, and said that if he recovered he would devote his life to research on protective measures. If a layman might venture an opinion, it would be that medical men generally cannot be expected to conduct research on the methods of production of the rays, or on the exact nature of the various kinds of rays produced by different forms of apparatus. On the other hand, physicists are not ordinarily competent to investigate the purely biological effects of the rays. Hitherto medical men have been lulled into security by the belief that the only injury to be feared was dermatitis, which they believe is caused by rays of low "penetration," and are probably stopped even by ordinary clothing materials. Again, they believe that protective screens of lead glass afford full pro-

tection. We may now doubt whether they do, at any rate in some circumstances.

In the interests, therefore, not only of radiologists, but also of suffering humanity which any curtailment of the facilities for X-ray treatment will affect, I appeal for an organised effort on the part of physicists and biologists in *collaboration* to institute research into the effect of X-rays on living tissues. I have sufficient confidence in science to feel that, as a result, methods will be devised which, while preserving the usefulness of the rays for medical purposes, will guard the devoted band of practitioners against the tragic risk which now stands revealed. I feel that in making this appeal I am discharging a duty imposed upon me by my brother.

March 25.

Since the above was written I have learned that some months ago steps were taken by the Medical Research Council to organise research on the action of radio-active rays on living tissues. With this work prominent physicists will be associated. I am confident that this collaboration will be productive of good results, and I am glad to know that the appeal I ventured to make had already been answered.

A. B. BRUCE.

March 29.

Greenland in Europe.

DURING the present month a new light has been thrown upon the Aberdeen kayak (skin-canoe) referred to in *NATURE* of January 13, p. 648. Fresh information upon this subject is found in a diary of a tour through Scotland in 1760 by the Rev. Francis Gastrell (born 1707; M.A. Oxon. 1728), son of a Bishop of Chester, and owner—by purchase in 1753—of New Place, Stratford-on-Avon. His diary is now preserved in the Shakespeare Museum at Stratford. In a paper read on March 10 before the Edinburgh Bibliographical Society Mr. James Sinton quoted Gastrell's statement that when visiting King's College Chapel, Old Aberdeen, on October 12, 1760, he there saw "a Canoo about seven yards long by two feet wide wh[ic]h about thirty-two years since was driven into the Don with a man in it who was all over hairy & spoke a language wh[ic]h no person there could interpret; he lived but three days, tho all possible care was taken to recover him." This canoe is now in the anthropological museum at Marischal College, Aberdeen. Its exact length is 17 ft. 9 in., its greatest breadth being scarcely 18 in. and its weight 34 lb. Francis Douglas, who saw it in or about the year 1782, describes it as "a canoe taken at sea, with an Indian man in it, about the beginning of this century. He was brought alive to Aberdeen, but died soon after his arrival, and could give no account of himself."

These two statements do not coincide, but there can be little doubt that they relate to the same individual. The hairiness of which Gastrell speaks suggests a non-Mongolian type, but it might only mean an imperfect recollection of the fur hood, shirt, and breeches worn by kayak-men. A similar canoe, captured in Orkney waters, and preserved in Edinburgh in 1696, had with it "the shirt of the barbarous man that was in the boat." Dr. James Wallace (F.R.S. Lond.), writing in 1700, says that "there is another of their boats in the Church of Burra in Orkney." In the same year the Rev. John Brand states that such canoes and canoe-men were then frequently seen upon the coasts of Orkney, "as one about a year ago on Stronsay, and another within these few months on Westray—a gentleman with many others in the Isle

looking on him nigh to the shore,—but when any endeavour to apprehend them they flee away most swiftly."

DAVID MACRITCHIE.

4 Archibald Place, Edinburgh, March 21.

The Peltier Effect and Low-temperature Research.

WITH further reference to the suggestions of Mr. Campbell Swinton and Sir Oliver Lodge contained in *NATURE* of March 10 and 17 that the Peltier effect may disappear at a very low temperature, this appears very improbable from the fact that, as long ago pointed out by myself, there is a continuous transition between metals and non-metals, and this distinction between them does not vanish at low temperatures. Consequently, pairs of elements must always exist with electrothermic differences. The nearly "perfect" metal may become a "perfect" conductor of heat and electricity, and the nearly "perfect" non-metal may become a "perfect" non-conductor at low enough temperatures, but the intermediately graded elements would become neither perfect conductors nor absolute non-conductors, but would behave much like certain elements at ordinary temperatures. The periodic law would enable physicists to predict almost with certainty which elements would exhibit the desired effect best at low temperatures.

It may repay physicists who intend to study these effects to look up papers written by me many years ago, e.g. "Some Remarks on the Connection between Metals and Non-Metals," etc., which occur in the *Chemical News* during the years 1903, 1904, and 1905. Also my book "Researches on the Affinities of the Elements and on the Causes of the Chemical Similarity of Elements and Compounds" (1905). I have been hoping for the opportunity of revising the latter and bringing it up to date, but unfortunately have always been overwhelmed with technical work.

GEOFFREY MARTIN.

109 Corporation Street, Manchester,
March 22.

Relativity and the Velocity of Light.

THE great interest of Mr. Jeans's letter on this subject in *NATURE* of March 10 is, I think, sufficient justification for my letter by which it was evoked.

The argument used by Mr. Jeans to support the proposition that it can be shown that both on the outward and on the inward journey light travels with the same constant velocity is, to me, difficult to follow. Majorana's experiments deal respectively with a source and a reflecting mirror moving relatively to the observer, whereas in the Michelson-Morley experiment both are at rest with the observer. I cannot then see the bearing of Majorana's results upon the question whether β and α remain unchanged in the case given by Mr. Jeans.

I am sorry I misunderstood the words used by Mr. Jeans in his article in *NATURE* of February 17 to imply a belief in the possibility of measuring the velocity of light in a unidirectional course. It appears to me, however, that the truth of this proposition is involved in the affirmation of the proposition referred to in the paragraph above; for the mean velocity of light on its outward and return journeys after reflection from a mirror can be measured. If also its constancy outwardly and inwardly can be affirmed, does it not follow that the velocity on a unidirectional course becomes known, contrary to the principle of relativity?

C. O. BARTNUM.

32 Willoughby Road, Hampstead, March 15.