any sense be called foggy, the other eight months being, either absolutely or nearly, fogless ("Scientific Results of *Fram* Expedition," 1893–96, vol. 6, p. 572). But these fog statistics which Prof. Hobbs tries to turn in his favour have really little bearing on the argument at all, inasmuch as fog can exist at temperatures far below freezing; and, in any case, if fog-laden air moved outwards from the polar basin into warmer latitudes the moisture would soon be dissipated and the air become in perfect condition to form discontinuities with warm humid equatorial air along the polar front.

Thirdly, Prof. Hobbs is incorrect in underestimating the cold of the North Pole in winter, as Mohn's isothermal charts should convince him. The Arctic Ocean in its outer portions is milder than the inner polar basin, and the fact referred to by Prof. Hobbs, that along the Arctic shores of Siberia and Canada southerly winds in winter are colder than northerly, is in accordance with the principle lately formulated by Dr. C. H. Pollog of Munich, that where there is a sudden transition from land-ice to sea-ice, the air over the latter is warmer because heat is conducted upwards through the ice from the unfrozen water beneath (*Mitt. Geog. Gesell. München*, vol. 27, No. 2, 1924).

L. C. W. BONACINA.

27 Tanza Road,
Hampstead, N.W.3,
February 19.

Parasitism of the Dodder.

THE paragraph on Dr. Thomson's interesting work (1925) in NATURE of February 6, p. 210, contains a reference to the results of "earlier observers" which I cannot pass without comment.

Thomson found no phloem in the haustorium of Cuscuta, either in the shaft or in the brush of hyphæ; and no special connexion to the phloem of the host, corresponding with the xylem connexions. But Peirce (1893) described and figured both xylem and phloem in the shaft of the haustorium. This phloem I also found (1911), and Zender, who is not here, as the writer of the note states, in complete agreement with Thomson, also refers to phloem that is "only found in the primary portion" of the sucker (*Inst. de Bot. Genève*, 1924, p. 9). This phloem is small in amount; the sieve tubes are rather short, but quite typical and like those in the main stem of Cuscuta. Peirce and I cannot have differed in our descriptions of the position of the phloem in the shaft, as I did not attempt to describe the anatomical distribution of the tissues either of the main stem or of the shaft of the haustorium.

My work concerned mainly not the above typical phloem in the shaft but the strings of short sieve tubes in the distal part of the haustorium of Cuscuta. Here there are no elongated sieve tubes with well-bored-out sieve plates. This part of the haustorium is formed from the brush of hairs or 'hyphæ' which originate from the tip and sometimes from the sides of the haustorium proper. Thomson and others before him have described the formation of strings of short xylem elements from some of these invading hyphæ. I find similar strings of short phloem elements (perhaps better called sieve elements than sieve tubes) developed from other of the invading hyphæ, nearer the periphery of the brush. There is no possibility of regarding these elements as other than phloem, as there are well-defined sieve areas in their walls; one cannot mistake a typical sieve area properly stained by the safranin and London blue method, each deep red string surrounded by its blue callus rod. The method cannot be used with material which has been in alcohol, and histological work of this kind requires, of course, very high magnifications and critical illumination.

Just as, where a 'hypha' becomes attached to a xylem element of the host, the cell walls of the hypha become lignified, so in a hypha which is attached to a host sieve tube the division walls of the hypha develop sieve areas. These attachments do not appear to be so numerous as those in the xylem, but I found them quite as definite in the species I studied as did Zender. The wall of the hyphal tip disappears and the naked protoplasm is applied to the host sieve plate. The further very elaborate behaviour of the protoplasm inside the cavity of the host sieve tube, which is described by Zender, has not been seen by me.

I have explained that the statement that Peirce and I are "not in agreement as to the position of the phloem" is based on a misapprehension. I hope I have made it clear that the evidence of independent observers renders the assertion that "the earlier observers . . . were in error in reporting its presence" somewhat hasty, especially as it is not possible to judge of Dr. Thomson's methods.

M. G. THODAY(SYKES).

University College, Bangor,
February 20.

Ozone and the Upper Atmosphere.

A BENIGHTED chemist, I listened with awe and interest to the discussion by the physicists on the electrical state of the upper atmosphere, on March 4, at the Royal Society. Apparently they were not quite at home above cloudland. The blessed word "ionisation" played a great part but did not seem to mean more than "conducting." Ozone was referred to with respect. Perhaps a chemist may be allowed to suggest a way in which it might serve as an "accumulator."

Those who attended the late Sir James Dewar's wonderful demonstrations at the Royal Institution may have witnessed the production of ozone, at the surface of liquid and even of solid oxygen, under the influence of ultra-violet radiation and will often have seen the glow of its decadence (to oxygen). Presumably, ozone is not the direct or immediate product of the action of such radiation upon mere oxygen but is formed *reversibly, in a complex electrolytic system*, in which oxygen is associated not only with a determinant ($\epsilon\delta$) but also with a catalyst (κ)—as the outcome of a series of interactions. I may refer to my article on "Catalysis and Oxidation," in NATURE of August 22, 1925, p. 294, where I have discussed the process.

The necessary conditions may well prevail in the upper atmosphere—they must, in fact, if it be formed at all. Let it be assumed then, that the condition be such, that the change of oxygen into ozone is not greatly encouraged—that it is difficult, on the whole, a large quantity being produced, during daylight, only because of the intensity of the solar effect. Then it equally follows, that the reverse action will take place only slowly and the accumulated energy be let down (electrically) only slowly, so covering the period of darkness, perhaps.

It may be dangerous for the mere chemist to put foot down where the angels tread but lightly: still, these are matters to which we have paid some attention and our intervention may not be altogether foolish—the more if it lead some day to better appreciation of the philosophy of dirt, at present unnoticed by the physicist.

HENRY E. ARMSTRONG.