

LETTERS TO THE EDITOR

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Liquid Hydrogen.

IN his letter published in your issue of the 9th inst., replying to mine published on May 26, Prof. Dewar does not question the accuracy of the following statements, which form the most important part of my letter: (1) That the combination which I described in that letter as constituting the self-intensive method of refrigeration was proposed by me at the Royal Institution to his chief assistant, Mr. R. N. Lennox, in November 1894; (2) that this combination had not been previously employed; (3) that it formed the chief novelty of Prof. Dewar's paper and experiments of December 1895; (4) that it is essential to the apparatus which has made the step from liquid air to liquid hydrogen. These facts make a sound claim on my part to the invention of the process and to recognition in historical or explanatory accounts of work which involves the use of the process. Prof. Dewar says: "My results would have been attained had Dr. Hampson never existed, just as they have been developed." On the other hand, at the Society of Arts (see *Journal*, March 11, 1898, p. 382), in speaking of Dr. Linde's process, which is admittedly to be substantially the same as mine, Prof. Dewar said that "after some fourteen years' work he ought to know something about low temperatures, but he must confess that the practicability of such a mode of working had never struck him." In illustrating the paper of December 1895, after showing an apparatus in which my process is embodied, and which has since been manufactured and sold by a firm of which his assistant, Mr. Lennox, is a member, Prof. Dewar said in my hearing that the chief credit for persevering with the development of that apparatus to a successful issue was due to Mr. Lennox. In his account (published in your issue of May 19) of the hydrogen apparatus, which also employs my process, Prof. Dewar says that it was constructed by Mr. Lennox's firm, and afterwards, in recognising "the invaluable aid of Mr. Robert Lennox," says "it is not too much to say that but for his engineering skill, manipulative ability, and loyal perseverance, the present successful issue might have been indefinitely delayed." I must allow that it is unfortunate for Prof. Dewar that an assistant so very useful and helpful should have kept the source of his inspiration on the vitally important features of the new development from the knowledge of his chief, who, in discussing my paper of May 2 before the Society of Chemical Industry, stated that he had been quite unaware of my communication of plans and drawings to Mr. Lennox. He ought however, when he did learn the facts, to have done me justice; whereas he says in his letter of the 9th inst.: "My assistant has explained his position in the matter in letters addressed to *Engineering* within the last few weeks." I earnestly hope that all who care for the credit of science will read for themselves the series of letters to *Engineering* by "Arenel," Mr. Lennox, and myself, from March 25 to May 13, in which it will be difficult to find a satisfactory explanation of Mr. Lennox's position. As I fear, however, that few people will exert themselves to look up these letters, I shall be pleased to send a copy of the series to any one who writes for it to No. 20 Gower Place, W.C.

Prof. Dewar criticises my statement that I was the first in this country to liquefy air and oxygen without employing other refrigerants, on the ground that it had previously been done in experiments at the Royal Institution. Now Mr. Lennox has been given very great credit for the work in these experiments; and I do not admit that experiments by my method, developed in collaboration with a gentleman to whom I had explained the method embodied in them, and who had confessed that this method was a novelty to him, and had promised to help me to the appliances required to work it, can be quoted as anticipations of my own work; but to make my statement more correct literally, I will say that my method (as compared with that of Dr. Linde, which differs from it in details) was the first in this country to liquefy air and oxygen without employing other refrigerants.

I may add that I mentioned my introduction to Mr. Lennox, not as an "excuse" for calling on him, a course which

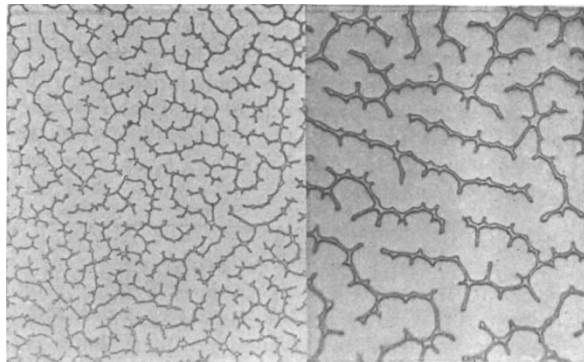
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obviously needs neither excuse nor justification—but to show that I made my visit deliberately for a definite purpose, knowing that I was in possession of an invention of great value for work such as he was practically engaged in. W. HAMPSON,
June 11.

Dendritic Patterns caused by Evaporation.

I HAVE been much interested in Miss Raisin's Royal Society paper "On certain structures formed in the drying of a fluid with particles in suspension," of which an account appears in NATURE for June 2. In connection with this subject it may be worth while placing on record the fact that the presence of suspended particles is not essential for the production of dendritic forms.

Many years ago, when dabbling in microscopy, I mounted a number of objects in glycerine jelly, and was much troubled by the production of bubbles starting from the object and spreading in all directions, leaving a highly elaborate network of ramifications caused, no doubt, by the evaporation of water and consequent shrinkage of the jelly. Having called attention to this defect in a box of slides circulated by the Postal Microscopical Society, Mr. J. J. Wilkinson, of Skipton, very kindly sent me the two accompanying photographs taken with magnifications of 25 and 50 diameters respectively. An additional interest attaches to these from the fact that the slide from which they were taken belonged to the collection of the late Mr. Tuffen West. Needless to say, this slide was mounted for an entirely



x 25

x 50.

different object, and the specimen it contained was rendered worthless by the subsequent formation of these beautiful but troublesome vacuoles. It should be explained that it is the thin branches which are formed of the remains of the jelly, the air filling the broader spaces between them. G. H. BRYAN.
Bangor, June 10.

Iridescent Surf at Cromer.

CAN any of your readers account for what seems to me to be a singular phenomenon, as, although familiar with the beautiful sea-coast and clear green waves of many lands, I have never seen anything of the sort elsewhere.

The cliffs here, though fine when seen from a distance, are only composed of sand and earth, large quantities of which have been washed down by the recent rains, so that the sea is very dirty, each turning wave being dark with mud. This mud has apparently some curious property, which causes a very moderate surf to deposit long lines of foam all along the shore. Of this foam (which is in no hurry to disperse) each bubble is brilliantly iridescent, even on the dullest day of cold sea-fog, when there is not one gleam of sunshine to produce prismatic effects.

The inhabitants take this so entirely as a matter of course, that a lady whose attention I called to it, said that having always seen it, she had supposed it to be the natural condition of all sea-foam.

Beautiful in themselves as are these myriad rainbows of the shore, I am glad they are not universal, if they are only to be seen as compensation for a discoloured sea!

It would be interesting to learn what is the ingredient in the mud which, when combined with salt waves, produces such tints.

CONSTANCE F. GORDON CUMMING.

Cromer, Norfolk, June 15.