sterile, so that though I carried on the experiments for weeks, and infected the experimental cultivation with actively growing moulds derived from a butter cargo, I could only get the organisms to develop very slowly. I was therefore compelled to hold this method of firing as doubtful.

But bales 3 and 8 have proved beyond doubt the validity of this hypothesis. I have no doubt whatever that these would have fired if the heat had not rapidly escaped, and that the fungus would, as in the case of haystacks, have been the cause.

Another most interesting point is the fact that the oxidation of the grease by air and moisture (to which I attributed the fires on the s.s. "Gothic") is proved by these experiments. I have extracted the grease from Nos. 6 and 12, and have found it oxidized and rubber or varnish like in character. This is the more satisfactory, as the fired samples from the "Gothic" were too much injured for examination. The sample from which I extracted the grease was uninjured. The grease was consequently soft, and at ordinary temperatures took some months to pass into the condition of varnish.

Another important point determined by Mr. Burridge is connected with both of the two methods by which firing may occur. In the case of heating due to the growth of fungus, the plants during their life resolve the vegetable or animal matter on which they live into carbonic acid and water. There is consequently a considerable loss of weight due to these actions.

In the case of heating due to oil or grease oxidation there is, however, a very marked increase in weight, due to the oxidized oil retaining the oxygen it has absorbed.

Both these cases are illustrated, bales 1, 2, 3, 10 increasing, and 6, 8, 12 decreasing, and it may be inferred that in the former the fungus was the predominant factor and in the latter the oil oxidation.

Having thus thoroughly considered and discussed these experiments, I am of opinion—

1. That the effect of moisture in setting up heating in wool is amply demonstrated, and that better conditions only were required to cause actual firing, or, at any rate, carbonisation.

2. That the possibility of fungoid growths acting in this way, which was very obscure in the case of the "Gothic," is fully proved.

3. That the oxidation of the grease is proved by the discovery of oxidized grease actually now existing in bales 6 and 12.

4. That both causes may act concurrently, but that one or the other predominates, as is proved by some bales losing and some gaining weight.

5. That the observed thermometer readings do not represent actual temperatures reached in the bales; such temperatures were considerably higher.

6. That it is proved that wetted wool in any form is likely to fire, and that the placing of any such cargo in the hold of a ship or in a warehouse insufficiently ventilated should be absolutely prohibited.

7. That as a standard of moisture a maximum of 10 per cent. on the weight of the wool as total loss on drying at 212° Fahr. should on no account be exceeded, and that it is desirable to reduce this as much as is feasible. Possibly in the drier climate of New Zealand a maximum moisture of 8 per cent. could easily be secured.

I am, &c.,

The High Commissioner of New Zealand. R. J. FRISWELL.

DEAR SIR,— Sennen Cove, Land's End, 12th April, 1907.

I am away from home for a few days just now, and only received your letter last night.

The results of the tests quoted in your letter are, I think, very instructive. They seem to me to point to this: that the rise in temperature of the bales was due to some substance (probably an oil) capable of combining with oxygen at comparatively low temperature and so producing heat; that the amount of this substance is very small, since the process practically came to an end after about a fortnight, and that more or less of this substance was present in all the bales tested.

Had the bales been stored in greater bulk at Wellington they would probably have fired, just as certain qualities of coal do when stored in sufficient bulk, the flow of air through and over large masses of the material being insufficient to carry off the heat produced by the slow oxidation process.

I should think that the wool could be rendered safe for shipment if it were subjected to a preliminary warming for a few days in a warehouse, with air-spaces between the bales at a temperature of about 120°. The oxidizable material would in this way be safely destroyed, as appears to have been the case with the bales experimented on.

As regards further tests, I hardly think that anything is to be gained at present by analysis of the bales experimented on. Much more valuable results would be obtained by taking wool which had never been heated, and determining the rate of absorption of oxygen at various temperatures, varying percentages of moisture, and varying periods after heating had been started. In this way it would be possible to get at the root of the whole matter with a minimum of trouble and expense, and to suggest tests on a large scale as to the best means of securing safety from fire.

I am at present so much occupied with work for our Government, as well as with my ordinary work at Oxford, that I have no spare time to devote to this question, but should you contemplate having experiments made on the lines I have suggested, I think I could arrange to have them done in London at 66 Victoria Street (the laboratory of the Metropolitan Gas Referees) by our chemist, Mr. W. J. A. Butterfield, F.I.C., who is thoroughly familiar with the necessary methods, and whom I constantly see, and would gladly advise as to details.

Yours, &c.,

H. C. Cameron, Esq. J. S. HALDANE.