argon. If that is the case there seems, as far as I know, no *à priori* reason why sparking with oxygen should necessarily remove the carbon. The ratio of specific heats must take care of itself. It is a matter of the greatest interest to pursue the subject; for the origin of the spectrum, whatever it may turn out to be, will probably throw much light on the source of the spectra of comets and of carbon stars.

ARTHUR SCHUSTER.

Liquid Hydrogen.

MR. HAMPSON seems insatiable of contradictions. He has produced a vast quantity of irrelevancies with which I have no concern. But I have denied the accusations he brings against me, and every single statement of his that is relevant. Yet he still complains that I do not deny enough. It is absolutely false to say that I appropriated or profited by any plan, idea, or statement of Mr. Hampson's, either directly or indirectly. I was never informed of his visit, far less of any of the plans he brought to the Royal Institution, nor would anything have induced me to look at them. I have been long enough in this "temple of science" not to know what that might involve. Mr. Hampson got at my assistant behind my back, and persuaded him to look at the plans. I infer from the public correspondence, that he saw that they would not work, and he told Mr. Hampson why they were unworkable.

Even with this assistance it took Mr. Hampson another year to perfect a provisional specification of his invention, which is totally devoid of any plan or drawing of a workable apparatus.

In the meantime Linde had completed his invention, and the Royal Institution went on working on its own lines, just as it did before Mr. Hampson was heard of, and as it would have done had he never been heard of at all.

Like the rest of us, Mr. Hampson was using ideas and principles established by other men, and was trying to apply them and combine them so as to reach a given result. He has no property either in the principles or in the idea of combining them, or in anything except the particular combination to which he himself may give concrete form.

Other men besides myself have successfully combined these principles without any help from Mr. Hampson.

Long before Mr. Hampson's patent was published, I said at the Chemical Society in 1895: "It is a mistake to attribute to Linde the idea of using the cumulative withdrawal of heat for the first time in his apparatus, but he has succeeded in making a workable industrial machine, and that is a very important step."

In the Society of Arts Journal I said: "Both Onnes and myself used or economised the temperatures of the expanding gas in order to cool the gas coming forward, but Linde was entitled to every credit for elaborating a machine in which this was done as perfectly as possible."

Further, in the *Chemical Industry Journal* the following passage makes my position clear: "He (Prof. Dewar) was willing to give all credit to Dr. Hampson, Dr. Linde, and any one who effected improvements in these investigations. All he asked was that they should not exaggerate their claims, and seek to block the way to other people who were working in the same direction. Dr. Hampson did not appear to realise that anybody else could be working in the same path and utilising the same ideas. It was quite clear, however, from the facts before them, that that was precisely the state of affairs in the present case."

Such extracts show that I have recognised to the full the merits of the true inventor within the limits of his just claims.

JAMES DEWAR.

Summer and Winter in Relation to the Sunspot Cycle.

THE quality of a winter season may be fairly estimated from the number of days on which the minimum temperature has gone below a given limit; and the quality of a summer season, from the number of days on which the maximum temperature has gone above a given limit. Two tables issued from Greenwich are here convenient for use; one giving frost days (since 1841), the other days on which the temperature reached or exceeded 70°. There are more of the latter than of the former; seventy-seven on an average, as against fifty-five frost days (in September to May).

We may roughly call a winter season *severe* or *mild*, according as it has more or less than the average number of frost days; and a summer season *hot* or *cool*, according as it has more or less than the average number of hot days (in the sense specified).

Confining attention to the groups of five consecutive years having a sunspot maximum, or minimum, year third (or central), I propose to inquire whether there is anything in the winters and summers of these groups pointing to sunspot influence.

(The sunspot maximum years are 1848, 1860, 1870, 1884, 1894; and the minima, 1843, 1856, 1867, 1879, 1890. Winters may, for brevity, be designated by the year in which they end; thus, 1842 means 1841-42. In the tables exceptions are marked *e*).

The following statements regarding winter may be verified:—

(1) *In five-year groups having each a sunspot maximum year third (or central), there are generally more mild winters than severe.*

(2) *In five-year groups having each a sunspot minimum year third (or central), there are generally more severe winters than mild.*

The proof is in these tables:—

A.				B.			
Max. groups.	Severe.	Mild.		Min. groups.	Severe.	Mild.	Av.
1846-50	2	3		1841-45	3	2	
1858-62	2	3		1854-58	5	0	
1868-72	3	2 ^e		1865-69	2	2	1
1882-86	1	4		1877-81	3	2	
1892-96	2	3		1888-92	4	1	
	10	15			17	7	1

The first table shows one exception to the rule (maximum of 1870). The five years' total, however, it may be stated, is *under*, not *over*, the average.¹ In table B the winter 1840-1 has been included, though the record properly begins with 1841. It was a severe winter. The table has one group with an average winter, so that this group may be considered neutral.

Coming now to the summer season, proof is offered of the following:—

(3) *In five-year groups having each a sunspot maximum year third (or central), there are generally more hot summers than cool.*

The corresponding and opposite statement for minima seems hardly warranted by the present data. The tables are these:—

C.				D.			
Max. groups.	Cool.	Hot.		Min. groups.	Cool.	Hot.	
1846-50	2	3		1841-45	4	1	
1858-62	2	3		1854-58	0	5 ^e	
1868-72	1	4		1865-69	2	3 ^e	
1882-86	3	2 ^e		1877-81	4	1	
1892-96	2	3		1888-92	4	1	
	10	15			14	11	

Here we have one exception to our third rule, the group for min. 1884 showing two hot summers and three cool ones. The table D has three cases pointing one way, and two the other.²

In view of these facts, I have sought light from a different quarter, taking the mean temperature of the four months May to August, and dealing with the series (from 1841) in the same way.

We thus obtain the following tables for summer:—

E.				F.			
Max. groups.	Cool.	Hot.		Min. groups.	Cool.	Hot.	
1846-50	1	4		1841-45	4	1	
1858-62	2	3		1854-58	3	2	
1868-72	2	3		1865-69	3	2	
1882-86	4	1 ^e		1877-81	3	2	
1892-96	2	3		1888-92	4	1	
	11	14			17	8	

Comparing E and F with C and D, we find general agreement of the two former (E and C), the exceptional group, 1882-86, remaining,³ while table F gets rid of the exceptions of D. In fact, while the summer seasons 1854, 1855, 1856, and 1867 had more than the average number of hot days, the mean temperature of May to August was, in each year, *under* the average.

¹ It is right to say that this criterion would make the neutral case in a Ban exception.

² In the curve of hot days, there is evidently a long wave of variation, which may complicate matters.

³ The sunspot maximum of 1884, I may point out, was abnormally low.