

My discourse at the exhibition of the Physical Society is criticised and the statement made that it was "not the way to convince a sympathetic audience of experts." I was requested by Dr. Rankine, the secretary of the Society, to deliver a lecture suitable for a public audience interested in scientific matters generally, but not experts on television. The lecture was therefore of a semi-popular type and in no way intended for an "audience of experts."

While the writer of the article in *NATURE* appears to be dissatisfied, judging by the reception which the lecture was given and the appreciative letters which I have received, I am assured that opinion was not shared by the bulk of the audience.

I am further criticised for withholding technical details. The writer of the article is surely aware that my inventions are the property of a limited company. The disclosures by me of technical details likely to assist competing interests would therefore be a grave breach of trust to the shareholders. I may further add that we have demonstrated the invention to Government experts, and have received a letter from the Government requesting us to withhold publication of technical details.

The writer states further that: "There are at least three pioneers in the field who appear to be on the verge of a complete solution of the television problem," and mentioned Belin, Jenkins, and Alexanderson. The results which, according to press reports, they have demonstrated should be mentioned. Belin and Jenkins have succeeded in transmitting crude shadowgraphs; and Alexanderson, within the last few weeks, apparently claims to have achieved the same feat. This is a long way from television, and does not justify the statement that they appear to be on the verge of a complete solution of the television problem.

JOHN L. BAIRD.

Television Limited, Motograph House,  
Upper St. Martin's Lane, London, W.C.2,  
Jan. 19.

THE further information given above by Mr. Baird is precisely the kind which physicists were waiting for. In the absence of a clear description in technical language, many misconceptions are bound to arise, and it would be well for Mr. Baird to consider the advisability of making such a communication at the earliest opportunity consistent with his other obligations.—EDITOR, *NATURE*.

#### The Auroral Green Line 5577.

MESSRS. McLennan, McLeod and McQuarrie have published in *NATURE*, vol. 118, page 441 (Sept. 25, 1926) the results of some recent experiments on the origin of the auroral green line 5577. During July and August last, while a guest of the Nela Park Research Laboratory, at the kind invitation of Dr. W. E. Forsythe, I had the opportunity of investigating spectroscopically the electrical discharge in mixtures of (1) helium with oxygen, and (2) argon with oxygen. The relative proportions of the two gases in each mixture were varied as well as the current through the gases. In the case of helium with oxygen, traces of this green line were found on some plates, but its intensity was usually very feeble. In one experiment, however, with argon and oxygen, the line 5577 appeared very strongly, about one-half the intensity of the argon line 5559, the iron arc spectrum being used for comparison (Fig. 1). I am indebted to Prof. Lloyd for the enlargement).

The discharge tube was made of pyrex tubing 8 mm. in internal diameter, and was fitted with tungsten electrodes which had been 'outgassed' before intro-

ducing the mixture of argon and oxygen (argon pressure = 40 mm., oxygen pressure = 8 mm.). The tube was of H-form, and the discharge was viewed along the bar of the H with a Hilger quartz spectrograph of the D type. The current used was 80 milli-amp./cm.<sup>2</sup>, and the potential 800 volts.

When the tube had run for one and a half hours, a second photograph was taken on which the line 5577

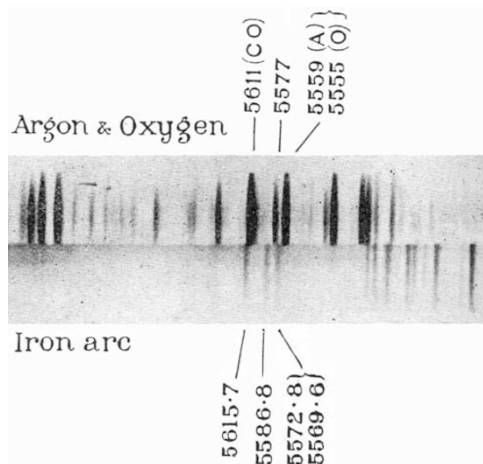


FIG. 1.

was entirely absent. The oxygen had combined with the tungsten, forming a bluish deposit of tungsten oxide on the glass near the electrodes. This result confirms the recent work of McLennan and his co-workers, and the absence of the line when the oxygen was removed is further evidence that the line is primarily due to the oxygen. DAVID A. KEYS.

McGill University,  
Montreal.

#### The Polishing of Surfaces.

MR. MACAULAY and Mr. Preston, in their letters in *NATURE* of Sept. 4, 1926, and Jan. 1, 1927, raise the interesting question of the mechanism by which the surface layer is produced in the process of polishing. This layer, so thoroughly studied by Beilby, resembles a supercooled liquid so nearly that it has been suggested that the surface layers are liquefied.

It does not seem necessary to suppose that actual liquefaction occurs. An amorphous layer indistinguishable from a supercooled liquid will be formed by any mechanism which rearranges the surface molecules at random. The polisher adheres to the surface, and especially with the high coefficient of friction mentioned by Mr. Preston, will tear away the surface particles of the glass when it is moved. Hardy found the surface of solids to be torn by the mere motion of a slider. Some of these particles will naturally be redeposited elsewhere at random, thus forming the amorphous layer. Some of the particles torn away must be of molecular dimensions for the final result to be a completely amorphous layer.

Surface tension in the ordinary sense does not enter into the problem, but since the flat, liquid-like surface is that of least potential energy possible in the circumstances, molecules are more likely to be deposited as part of such a surface, and less likely to be removed from it, than otherwise. The crystalline state, of still less potential energy, will not be formed, as sufficient time is not allowed in the redeposition of particles. This mechanism is actually more likely to