LETTERS TO THE EDITORS

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J. B. Hannay and the Artificial Production of Diamonds

REFERRING to Dr. C. H. Desch's article on this subject in NATURE of August 7, p. 148, I wish to put certain recollections of my own on record.

During the years 1885–96, my father, the late Lord Rayleigh, was one of the secretaries of the Royal Society, and not infrequently mentioned in the family circle incidents which had occurred in connexion with the Society's business, though of course he never gave the names of referees. In particular he spoke a good deal about J. B. Hannay's claim to have produced diamonds, though he never expressed any personal judgment on the matter. It would not have been in his character to do so, without personal investigation.

Hannay sent to the Royal Society a further communication on the artificial production of diamonds at a much later date than that of his published paper on the subject, certainly later than 1890—perhaps about 1894. It may be in the Society's archives, which are at present inaccessible. My father, as secretary, had to deal with it. He made it clear that among the members of the committee of papers (I believe this committee was in fact the same body as the Society's Council) there was distrust of Hannay's good faith. Rayleigh personally found him plausible. Hannay, he said, wrote that the non-publication of his paper seemed to imply disbelief in his good faith, and that as he had offered to demonstrate the matter he thought that this was rather hard. Rayleigh was inclined to agree that it was hard. In Hannay's earlier work, only very occasional success was claimed, and under these conditions a demonstration would scarcely be practicable. I do not know whether at the later period he claimed uniform success. However, the reason why his offer to demonstrate was not accepted was that it was not thought that any demonstration carried out by Hannay could satisfy the sceptics, and no one seems to have undertaken to repeat the work independently.

Hannay wrote a paper "On the Microrheometer" (*Phil. Trans.*, **170**, 275; 1879) which deals with viscosity. This is commented on by R. E. Barnett (*Proc. Roy. Soc.*, **56**, 259–261; 1894), in a paper inspired by T. E. Thorpe, and communicated by him. Barnett gives a very unfavourable analysis of Hannay's work. Since Barnett's paper is not at present easily accessible, owing to libraries having put the volumes of periodicals away for safety, I think some extracts from it may fittingly be quoted.

Hannay gives the rate of flow of various liquids through a capillary tube of stated dimensions. These measurements are not reduced by him to give absolute values of viscosity and thus to admit of easy comparison with the measurement of others, but Barnett points out that all the necessary data are given for such a reduction, which he proceeds to make. He says: "On comparing these results with the values given by Poiseuille, Slotte, Sprung, and Thorpe and Rodger as tabulated below (II), it will be seen that Mr. Hannay's observations yield discordant, and indeed, utterly absurd, values for the viscosity of water. At 0°, for example, the viscosity would appear to be below that of any known liquid, and at 6° it becomes *nil*.

"As a matter of fact, it is physically impossible to pass a volume of water such as Mr. Hannay employs under a pressure of 1 m. of water through a capillary of the dimensions given in the time recorded. At 20°, for instance, the time of flow required under these conditions would be about 4600 seconds instead of 131·3 seconds as stated.

"In the light of these results, it would seem to be premature to discuss Mr. Hannay's observations on saline solutions, or to criticise the generalisations he deduces from them."

I have not been able to discover that Hannay made any attempt to reply to this apparently very damaging criticism.

He also wrote a paper on the metallurgy of lead which was read before the Royal Society on April 15, 1893 (see the *Proceedings*), but not published. This paper was, I believe, judged not to have been written in good faith. It is almost certainly still in the Society's archives.

I think that attention should be directed to these facts which, though perhaps not decisive of anything relating to the diamond problem, may nevertheless help in forming a provisional judgment. If Hannay was after all right, they will at least be a contribution to scientific history. The incident about viscosity seems to show clearly enough that his critics had something more than prejudice to go on.

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Oct. 23.

Single-Fibre Response from an Intact Animal

IT is rather difficult to obtain electrical records of the activity of a single nerve fibre, because there are usually present some hundreds of other nerve fibres the contribution of which must be eliminated, and because the shunting of the active fibre by inert tissues or tissue fluid reduces the available action potential to vanishing proportions. It is therefore essential in most cases that the recorded fibre be fairly well isolated both physiologically and electrically, and we are not aware that any records have hitherto been published showing single fibre responses obtained from an animal which has not been dissected nor operated upon in any way.

The median giant fibre in the nerve cord of the common earthworm, *Lumbricus terrestris* L., is especially favourable for such a record, for it may be physiologically isolated from other structures and it has such a large cross-section area that a fair amount of shunting may be tolerated[1].

If a normal lively worm is used the nerve activity is accompanied by muscular action which complicates the record, but if the worm lies in a glass tube just large enough to contain it, it remains quiescent and the record is fairly simple. The nerve is stimulated by a shock applied to the anterior end of the worm, and the record is obtained from leads passing through the glass tube and making contact with the worm's body in the middle region. These leads connect through a balanced amplifier to a cathode ray tube with time base synchronized with the nerve stimulus.

The first record shows the effect of a just threshold stimulus; the least diminution of stimulus gives the