would superintend the examination if Airy supplied him with an assistant from Greenwich for the purpose. He concluded by saying, "The time for the said examination is approaching near".

When Challis informed Airy that he would undertake the search, Airy drew up as a guidance for Challis his "Suggestions for the examination of a portion of the Heavens in search of the external planet which is presumed to exist and to produce disturbances in the motion of Uranus" (dated July 12, 1846). In sending this paper to Challis he wrote, "I only add at present that, in my opinion, the importance of this inquiry exceeds that of any current work, which is of such a nature as not to be totally lost by delay". Airy could not have done more to further the search and to impress upon Challis its urgency. There is little doubt that if the search had been carried out by an assistant from Greenwich, the planet would have been found, for it was an essential part of Airy's system that reduction of observations proceeded *pari passu* with the observations themselves.

As regards the actual researches of Adams and Le Verrier, full abstracts of Le Verrier's investigations had been published in the *Comptes rendus*, but neither Airy nor Challis had received anything from Adams beyond the bare summary of his results; they knew nothing of the methods he had employed.

After the discovery of the planet by Galle at Berlin, Airy wrote to Le Verrier and informed him that collateral researches, which had led to the same result as his own, had been made in England, and that they had been known to him earlier than those of Le Verrier. His "Account of some circumstances historically connected with the discovery of the planet exterior to Uranus" presented to the Royal Astronomical Society on November 13, 1846, left no doubt about the priority of the researches of Adams. In a letter of later date to Biot, Airy wrote, "I believe I have done more than any other person to place Adams in his proper position".

Prof. Smart agrees that the contemporary criticism of Airy, made in ignorance of many of the facts, was on some points unfair and unjustifiable. In my opinion, his verdict that Airy's treatment of Adams was unbecoming is equally unjustifiable.

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THE Astronomer Royal does not see eye to eye with me in my judgment of Airy, in connexion with the Neptune controversy, as expressed in my article in *Nature* for November 9. This article, which was written in response to an editorial request, was a summary of the two addresses—dealing with different aspects of the discovery of Neptune—which I gave at the centenary commemoration on October 8; these addresses were themselves a summary of a fairly long 'essay' (if I may call it so) written at the invitation of the Council of the Royal Astronomical Society and accepted, as I understand, by the Council for eventual distribution to the fellows in one of the Society's publications. The 'essay' is a historical study of events of a century ago, and I was very conscious throughout its preparation that I must follow the methods of the historian as efficiently as I knew how. The job of the historian, as I see it, is to elicit facts, to present these in proper form, and to paint as accurate and complete a picture as possible. The 'essay' was accordingly built up on a very large amount of historical documents—I explain in the 'essay' how many of these became available, for the first time, for a study of the Neptune controversy, in which Sir Harold's great predecessor was in many ways the dominant figure.

All this, it seems to me, must be said before one turns to the criticism of the Astronomer Royal. Sir Harold's arguments, when documentary evidence is invoked, are based on Airy's letters alone. Most of his quotations will also be found in my 'essay', if—in one or two instances—not as direct quotations then as transcriptions of them. There is no suggestion in my article or 'essay' that Airy was to blame for Adams's failure to see the former on the occasion of his abortive visit to the Royal Observatory in October 1945—it was far otherwise—and as to the famous query about the 'radius vector', Adams never failed to reproach himself for not replying to Airy, although he was convinced that the matter was 'trivial', an opinion shared at the time by Challis.

The main questions are: Why did Airy claim to know the whole history of the business? Why did he declare unambiguously that Le Verrier must be regarded as the real 'predicter' of the planet? Why did he affirm that there was no one (in England) in competition, as regards scientific insight, with Le Verrier, etc.?

It is to be remarked that Airy's correspondence with Le Verrier was understood by him to be 'private', and he was exceedingly indignant—and justly so—when his letters were published in the French press without his sanction being even asked. Later, Airy described Adams as his 'oracle' in all matters relating to lunar and planetary theory; but this has nothing to do with the Neptune controversy as a historical episode. Airy was unjustly criticized on many points, as the Sedgwick correspondence makes abundantly clear, and as I hope my article and 'essay' demonstrate.

Any judgment on Airy's actions must be based, not on his letters alone, but on the whole corpus of contemporary documents. I do not claim that my 'essay' is the last word on the subject, but I do claim that, whatever its faults may be, it was written as a purely historical study with all the implications that this description suggests.

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Elastic Constants of Ice

EXPERIMENTS on the thermal scattering of X-rays by ice crystals, made by Dr. K. Lonsdale, have revealed an interesting pattern consisting of strong diffuse bands which extend along the boundary of the second and third Brillouin zone, and to a lesser degree between the fourth and fifth zone. An explanation of this behaviour in terms of atomic vibrations seems scarcely possible. Another feature of ice difficult to explain with the help of vibrations is the Raman effect. A figure representing the Raman scattering of ice according to Cross, Burnham and Leighton[1] is reproduced herewith. Other experiments made by Hibben[2] agree with these in all essential features. One sees that there are two small peaks at about 200 and 600 cm.$^{-1}$, and an enormous hump between 3,000 and 3,600 cm.$^{-1}$. Cross, Burnham and Leighton try to explain this hump as a superposition