Biographical Byways.[1]

By Sir ARTHUR SCHUSTER, F.R.S.

9. GEORGE GABRIEL STOKES (1819–1903).

THE collection of Sir George Stokes's published papers, together with the "Memoirs" and scientific correspondence edited by Sir Joseph Larmor, contain such an excellent account of Stokes's activities and personality that nothing remains but to confirm, illustrate, or emphasise what is already on record.

Stokes was elected as one of the secretaries of the Royal Society six years after the constitution of the Society had been altered by the limitation of the number of fellows elected annually. It was a critical time, and though there was no sudden change in the policy of the Society, new traditions had to be established. The range of his knowledge, the width of his sympathies, and his almost infallible judgment peculiarly fitted Stokes for a position which offered so many opportunities of advising striving and sometimes stumbling men, and guiding their work into profitable directions.

My own experience was similar to that of many others. In the account I gave of Osborne Reynolds, I mentioned a certain experiment which I had performed demonstrating that the motion of the radiometer was due to internal stresses. The paper describing the experiment was sent to the Royal Society and I received, in due course, a communication from Stokes forwarding some suggestions made by the referee. I complied to the best of my ability, and in informing me that the paper had been ordered to be printed, Stokes added, that in his judgment the paper was not improved by the changes I had made in deference to the referee. He further made a significant remark, which is worth remembering by those charged with the difficult and responsible task of reporting on papers. It was to the effect that, in his opinion, it was best to allow the authors of papers to express what they had to say in their own words, even when improvements might be effected. When I quoted this remark to Maxwell a year or two later he told me that he had been the referee, but I believe he agreed with the general principle. The suggestions which Stokes himself so frequently made to the authors dealt with matters of principle rather than with the manner of expression.

I have in my possession five letters written by Stokes during March and April 1885, and dealing with a subject on which there has been, and still is, a good deal of misapprehension. The question at issue is referred to in the correspondence of July and August 1899, reprinted in the "Memoirs," vol. ii. pp. 123-125.

Stokes writes to Rayleigh in July 1899:

> "Some years ago Thomson or Kelvin (I forget which he was then), you, and I were together at the Royal Society, and Kelvin asked me what I thought of a result you had arrived at that the appearance of bands of interference in a spectrum did not prove regularity in the light, but only high definition in the spectroscope. If this meant what it appeared to mean I utterly disbelieved it, it seemed so manifestly untrue."

In his reply Rayleigh writes:

> "I am afraid that I shall stand condemned, for I do think that 'a vast succession of independent impulses following one another casually' would show interference, of course with the aid of a spectroscope."

My correspondence with Stokes, which took place fourteen years earlier, deals with a design for an experimental arrangement suggested by him, which it was hoped would give "a large retardation of one of two interfering streams of light relatively to the other, and yet having the bands in one part of the spectrum so broad as to be easily observed, unless that should be prevented by the irregularity of the vibrations of the incident light."

The method depended on introducing into one of the interfering streams of light a dispersive medium, having a length adjusted so as to make the difference in path measured in wave-lengths in the two streams equal to each other within a certain range of the spectrum, in a manner suggested by that adopted in achromatising lenses. I do not now remember what ultimately prevented the investigation from being carried out. When I examined the question some years later (*Phil. Mag.*, June 1894) in the light of Gouy's and Rayleigh's discussion of the subject, I was fully converted to their opinion, but I do not believe that Stokes was ever convinced. In my judgment, the effect anticipated by Stokes in his arrangement would have been observed with sufficiently great resolving power, but it would have taught us nothing on the regularity of the incident light, because the observed regularity would have been introduced by the resolving power.

During my stay at Cambridge, many tales were current with regard to Stokes's taciturnity. My own experience is in the other direction. On several occasions I sat next to him at College dinners, but never had any difficulty in finding a subject of conversation on which he would enter with pleasure and sometimes with animation. He had several interesting tales of his intercourse with Brewster, who never could be made to abandon the corpuscular theory of light. Even when Foucault had proved that light was transmitted more slowly through water than through air, Brewster refused to give in.

Stokes was an old man when he died, but his scientific outlook always remained young. New ideas pleased him, and he delighted in hearing of experiments that did not fit in with any of the accepted theories. His peculiar form of wit is referred to in the "Memoirs," and I recollect one instance of it. At an excursion, during the celebration of the Kelvin Jubilee at Glasgow, Röntgen's discovery of the X-rays was referred to in the presence of some of the foreign delegates. Quincke stood up for the claims of Lenard, whose work according to him had to some extent anticipated Röntgen. Stokes replied: "Lenard may have had the rays in his brain but Röntgen got them into other people's bones." Whenever I afterwards met Quincke he never failed to repeat this remark with enjoyment.

The strong religious opinions held by Stokes are well known. I am told, on trustworthy authority, that he voted against the extension of university privileges to

[1] Continued from p. 271.

non-conformists, but this should not be taken as an indication of any want of religious tolerance. His whole life would contradict such interpretation. He could only have acted under a strong sense of personal responsibility.

In the sketch of her father's life, Mrs. Humphrey writes (vol. i. p. 6): "As a little boy he was subject to violent though transient fits of rage . . ." I was interested in this remark, but not altogether surprised, because I once saw an almost ferocious look on Stokes's face. It was at a meeting of the British Association when he thought that some one was taking a liberty with him. But this look was quickly replaced by his usual smile, as he turned round and saw that it was only Lord Kelvin patting him on the back.

Stokes lived a long and useful life, alert and vigorous almost to the end. There are few men who have secured the esteem and love of their fellow workers to the same extent. I shall always remember Lord Kelvin, as he stood at the open grave, almost overcome by his emotion, saying in a low voice: "Stokes is gone and I shall never return to Cambridge again."

The Vision of Nocturnal Animals.

By Prof. S. Russ.

IN some experiments, conducted with Dr. J. C. Mottram during the War, upon the best conditions for night vision, the question arose as to what part the transparency of the media of the eye plays in determining acuity of vision in dim lights. It is known that individuals vary between wide limits in their night vision, some appearing almost blind in a night of average darkness. Further, it is known that many wild animals have very little difficulty in making their way about or in finding their prey in dim lights, though the sense of smell may not, in the latter category, be neglected. In the night owls this sense is, I understand, excluded, for they find their prey solely by the sense of sight.

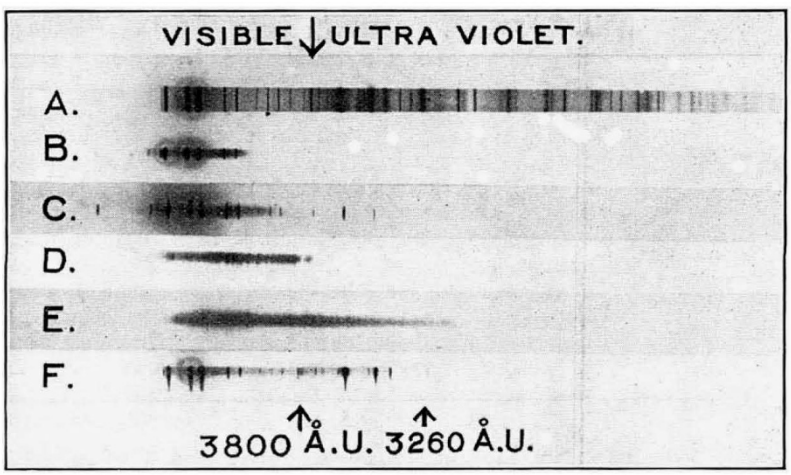

FIG. 1.

Experiments of a direct character were made upon the enucleated eye by cutting away a small portion of the retina at the back of the eye and cementing over this aperture a thin quartz plate. It was generally possible to do this without loss of fluid or deformation of the eye; this was now mounted in front of the slit of a quartz spectrometer, and the extent to which the radiation from an arc (cadmium or tungsten) was transmitted through the eye was measured photographically. It was soon evident that all of the eyes tested exerted, as was expected, a considerable degree of absorption for the major portion of ultra-violet radiation from the arc, but that what appear to be significant differences were shown by the eyes of different animals.

In Fig. 1 are collected the results of a number of such experiments: The strip A is a photograph of the ordinary arc spectrum of cadmium; the remaining photographs are with the experimentally mounted eyes interposed between the arc and the slit of the spectrometer. B shows the transmission by a human eye; and the other strips transmission by eyes of a lioness, C; a bear, D; great eagle owl, E; and a tiger, F. Others tested were the eye of an ox, which showed about the same degree of absorption as the human eye, and a cat, which closely resembled that of the tiger. Three human eyes were tested, and they showed no important differences from one another. How far the different degrees of transparency shown are dependent upon post-mortem changes, it is scarcely possible to say; it may be mentioned, however, that the human specimens were probably fresher than any of the others, yet they showed the greatest opacity to the short wave-lengths.

On testing in the same way the various parts of the eye, it was found that the lens was a more absorptive element than the cornea, the humors of the eye being more transparent. It is interesting to note that, if a person is examined in pure ultra-violet radiation, the lens is seen to fluoresce vividly, the cornea less so. The sensation when one is subject to this invisible radiation is that of the eye being filled with a pale blue light.

At a meeting of the Ophthalmological Section of the Royal Society of Medicine on January 9, these observations were discussed in their relation to night vision. If it be a fact that the eye of the nocturnal animal or bird is transparent to a certain range of ultra-violet radiation, and that this reaches the retina in an appreciable quantity, it may be that it is a valuable aid to vision. In the discussion, Sir John Parsons questioned the value of such a radiation to an eye chromatically adjusted to the visible region, but little is known about the range of radiation for which the optical parts of these eyes are most efficiently adapted. Prof. Hobday mentioned that, during the War, Australian horses were used in Palestine for night work, because they did not suffer from night-blindness.