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The JPEC

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In their Report for the year ending 1970 August 31 the Council of the Royal Society announced a decision to dissolve the Joint Permanent Eclipse Committee.

> 'This committee, a joint committee with representatives of the Royal Society and the Royal Astronomical Society, was set up in 1894 to supervise expeditions to observe solar eclipses. Among the reasons for considering the committee no longer necessary were the fact that few problems seem urgently in need of eclipse observations, other techniques could now provide some information needed, and recently the Science Research Council has financed most astronomical research, including that based on expeditions.'

What is written here is merely an outline of JPEC activities; it is not intended to be a detailed history, still less is it any kind of history of eclipse observing in general, or of the growth of ideas about solar physics. The UK contributions in these fields have been substantial, but it would be wrong to suggest that they outweigh the contributions of many others, especially of the Americans and Germans.

In 1894 the first glimmerings of modern physics were beginning to appear. Radioactivity had been discovered but the implications were far from being worked out or appreciated. The electron had been isolated but virtually nothing was known of atomic structure, although a great amount of progress had been made in practical spectroscopy. Planck's idea that energy exists only in quanta was still five or six years in the future: Einstein was 15 years old, Niels Bohr was 9, and Heisenberg and many other physicists not yet born. Hertz had demonstrated the existence of long-wavelength electromagnetic radiation, but its use in communication was yet to come and radio astronomy was still half a century away.

The chromosphere had been recognized as a distinctive part of the solar atmosphere about 25 years earlier, and thanks to many eclipse observers, among whom Lockyer was prominent, its spectrum was not unfamiliar, including the fact that different elements extend to very different heights. The corona was an almost complete mystery. Its appearance in white light was beginning to be known, and observers realized that there were changes of form during the sunspot cycle. Lockyer had measured the wavelengths of some of the bright lines in its spectrum, but of course no one had any good idea as to their origin; indeed the elements responsible were not identified until 45 years later.

The history of the JPEC is somewhat obscured by the fact that no Minute Book prior to about 1918 has survived, although some early correspondence is in the possession of the Royal Society. The Secretary for many years was E.H.Hills (later Grove-Hills, RAS Treasurer 1905–13, 1921–22, President 1913–15, and benefactor of the Society) who had formerly been an active eclipse observer himself. Among the Committee's 22 members in 1900 were Abney, Christie (Astronomer Royal), Common, Huggins, Lockyer, Schuster, for the R.S., and Dyson, Evershed, A.Fowler, Hills, Newall and Turner for the R.A.S. It appears that the meetings were not always uneventful, for the surviving letters give a tantalizing glimpse of a row between Lockyer and the rest, which seems to have gone on for several years. One may quote from a long letter written by Lockyer in 1896: ' . . . pressing a button or employing any equivalent procedure to obtain a photograph and measuring and reducing it in strict relation to all prior work both in laboratory and observatory, on all classes of celestial bodies, are two different things. . . .'; and from a 1900 memorandum by Fowler: 'The general feeling of the Committee was that Sir Norman Lockyer's application to the Royal Society implied reflections on the procedure of the Committee and that such reflections could not be substantiated'.

In some senses the Committee reached a peak at the turn of the century, for it supported successful observations of the total eclipses of 1898 (India), 1900 (Portugal, Spain, Algeria), and 1905 (Spain, Algeria, Egypt), as well as a partially successful eclipse of 1901 (Sumatra, Mauritius). Looking back at the programmes which were undertaken one at first feels that there was a lack of originality, and a lack subsequently of physical discussion of the results, but we have to remember that there was little or no solar atmosphere theory, almost no spectroscopic theory, and not much encouragement from the physics of the day. Many were content to take photographs of the white light corona as a pictorial record, the most that some did otherwise being to argue about the relative merits of long focus cameras with fast plates, or short focus with slow plates of fine grain. The existence of arched structures over prominences was known, and efforts were made to find how fast the coronal structure changes, by observing near the ends of a long belt of totality. It had been conjectured following Langley's observations of 1878 that the structure was the result of electromagnetic forces. What would nowadays be attributed to lack of competence or imagination was more a reflection of the primitive state of solar physics at that time; the shortcomings were not peculiar to eclipse expeditions of around 1900, nor indeed to British astronomers.

At this period, Newall, who significantly was trained as a physicist, perhaps came to closer grips with the corona problem than did most of his British contemporaries, but as in his pioneering astrophysical research in other fields he was very near an important advance and yet somehow missed clinching the matter. Schuster had pointed out in 1879 the importance of the polarization of the corona, and Newall realized in his attempts to measure it that the matter is complicated by the polarization of the sky background. Presumably he understood what others failed to grasp, then and for many years later, that most of the light in the sky during totality does not come from the corona, but is scattered in from outside the shadow cone, so that the sky never gets much darker than it is quite soon after ordinary sunset, and the background polarization changes rapidly. Newall was one of the first to attempt to calibrate his photographic plates, so as to make a surface photometry of the corona. He also tried more than once to measure the rotation of the corona spectroscopically, but was defeated by the technical difficulties.

The other favourite routine was photography of the chromospheric spectrum with an objective prism, and Fowler in particular obtained a remarkably good series of spectrograms in 1898. But although Fowler was one of the best spectroscopists then working there was no technique of photographic spectrophotometry, and no theory of the chromosphere to guide him, so that the results were mainly line wavelengths and identifications. However, he succeeded also in showing that the chromospheric spectrum is not an exact reversal of the Fraunhofer spectrum, but 'more enhanced'.

The series of good accessible eclipses was broken after 1905, until August 1914, when the JPEC organized several observing parties, mainly to go to Russia. These efforts were mostly frustrated, partly by clouds and partly by the outbreak of war. Some of the apparatus had to be left in the Crimea, where it survived a great war and a great revolution and was returned to England in 1922, almost intact. Indeed, the rumour later was that most of the small amount of loss and damage had occurred in the British Customs!

Meanwhile, however, there had been the eclipse of May 1919, perhaps the most famous of all. Dyson was by now Astronomer Royal, and he and Eddington during the closing months of the war had come to realize that on this occasion the Sun would be in the richest field of moderately bright stars that is to be found anywhere on the ecliptic, so that there would be an almost unique opportunity for measuring the deflection of light by the Sun. Despite the then difficult circumstances, two parties were organized with JPEC support, one to West Africa and one to Brazil, Eddington and C.R.Davidson forming the backbone. They triumphantly succeeded in demonstrating that the deflection agreed with Einstein's prediction from relativity theory within about 10 per cent: relativity theory being then new, very revolutionary, and still viewed with suspicion by some. Later and more elaborate attempts, made without the benefit of such a good star field, have confirmed but added little to this result.

In 1926 Davidson scored another success, this time in Sumatra with Stratton, where among other results they obtained an excellent ultraviolet slit spectrogram of the chromosphere. This is probably the first eclipse spectrogram ever to be examined with a microphotometer, but even in 1926, although spectroscopy had greatly developed as a result of Bohr's theories, good spectrophotometric techniques had not become a routine, and a sufficient background of chromospheric theory was still lacking.

Stratton had become JPEC Secretary in the early 1920s, and continued in that capacity for 32 years. During this period all those in the UK who were interested in eclipses, as well as many overseas, owed much to his unfailing and enthusiastic support. As an observer himself he was notorious for his bad luck; wars robbed him of two eclipses, clouds of three entirely, and of yet another almost entirely. However, on this last occasion, in 1936 (Japan), his party started the use of slit spectrographs of larger resolving power, with complete photometric calibration of the photographic emulsion, aiming at studying the detailed structure of the chromospheric emission lines, or of the Fraunhofer lines near the extreme limb, as well as the transition between absorption and emission spectra. Work continued in this direction, still under JPEC auspices, at the 1940 (South Africa) and 1952 (Sudan) eclipses, with significant results which showed the complexity of chromospheric line structure, and were useful in eliminating oversimplified ideas in chromospheric theory. Studies of this kind are by no means finished to-day, but much of the material can now be obtained without an eclipse, by careful exploitation of good sites and modern instruments.

The value of continuing tradition is perhaps still better exemplified in coronal studies made during the past 20 years with JPEC help. In 1952 (Sudan) von Klüber made a well-planned attack on the polarization problem, taking the sky fully into account and without assuming that the polarization is radial (although not unexpectedly this is what was found), and thereby completed a task which Newall had begun to explore without the help of a really adequate technique half a century earlier. On the same occasion Blackwell made infra-red measurements of the corona (there had been attempts with a bolometer by Abbot as early as 1900), which led him on to an adventurous series of expeditions. In 1954, with Hignell, he made the first effective use of a high-flying plane, taking photographs through an open door for quantitative photometry of the outer corona. In 1955 he attempted a repetition over the Pacific, from a flying boat. Taking off in a very rough sea led to plane trouble and lost him the eclipse, but other flights gave photometric measurements of the zodiacal light closer to the Sun than ever before. The task was subsequently completed in two (non-JPEC) zodiacal light expeditions to the high Andes. By this time adequate theory was available; after many years of speculation the corona and zodiacal light were demonstrated beyond question to be parts of the same entity, the contributions of electrons and dust could be reasonably well separated, and reliable estimates could for the first time be made of the electron density distribution from solar surface to earth. As a good first approximation this ground-based work still stands, but rockets and space vehicles have now taken over, and have shown us that the interplanetary medium is both variable and much complicated by magnetic fields.

In the meantime, among other work, the JPEC had supported von Klüber and Jarrett in 1954 (Sweden) and again in 1958 (Atafu, Tokelau Islands). By using better coatings for interferometer plates and better interference filters they mastered an experimental problem which had defied other observers for more than 30 years. They photographed the corona through a Fabry–Perot interferometer, first in the light of the green line and later in the red line, measured the spectrum line widths from the fringes distributed over the image, and hence derived kinetic temperatures.

One has already remarked on an apparent lack of originality and well-considered scientific purpose among the earlier eclipse workers, and one suspects that at a time when travel was a rarer luxury than now most of them were motivated as much by desire for adventure as for science. One imagines that a common sequence of thought was: (1) there's a wonderful eclipse coming soon in the heart of darkest Africa (or wherever); (2) let's go; (3) what can we do to justify an expedition? The irrepressible author of the Oxford Notebook in the Observatory Magazine once advised: 'On these expeditions the best time for sight-seeing is before and not after the great event...' Of the existence of an urge to exploration and excitement there has been no doubt; even Stratton, the most upright and conscientious of men, admitted that he liked gambling, 'and the best gamble I know is an eclipse'. But a quick review shows clearly how the nature of eclipse work has been wholly conditioned by the state of the physics and astronomy of the day. The eminent gentlemen who sat on the

JPEC during the late 1890s, trying to contain the impatience of Lockyer, had no means of knowing about quantum theory, radiative equilibrium, electron scattering, non-LTE, etc. They had no electronic devices whatever, and their spectroscopic equipment was crude by our standards. They would have been greatly interested to hear that with care the corona can be observed in broad daylight; they would have found great difficulty in understanding the use of Hertzian waves, again in broad daylight, to follow disturbances travelling out through the corona; they would have been dumbfounded had they been told that the corona has a temperature of 1 or 2 million degrees; and, with X-rays only just discovered, they would have needed a great deal of explanation before understanding that within the lifetime of the JPEC the outer parts of the Sun would be observed without eclipses, by apparatus using wavelengths down to below 1Å and carried on artificial satellites.

The JPEC survived for three quarters of a century, through the two largest wars in history and through unprecedented revolutions in every branch of human thought: scientific, technical, political, social. It served well the purposes for which it was brought into being and in its heyday was quoted as an example which others might do well to follow. It was one of the first Committees in this country to administer funds regularly for projects in scientific research, and its activities helped give rise to the system of Government Grants for research, administered by the Royal Society for many years and still in existence. It is perhaps worth mentioning that at all times expenditure by the JPEC was very modest. A rather long, and probably profitless, investigation would be needed now to estimate what effectively the Committee spent altogether during its life, one difficulty being that the value of the pound sterling has fallen irregularly by an order of magnitude since 1894. If one might hazard a guess the total is less than £100,000 at present-day prices.

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