

Paper No. 6270

The Jodrell Bank Radio Telescope †

by

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Discussion

The President, in proposing a vote of thanks to the Author, said he had no doubt that the Jodrell Bank instrument was only the first of many radio telescopes that were going to be built in the world, and therefore a knowledge of the problems involved would help members when a much bigger radio telescope was needed.

111. He added that although this was an engineering Paper, he would have liked to have heard some of the astronomical background to the radio telescope.

Mr M. F. Palmer (Chief Engineer and Director, Joseph Westwood and Company, Ltd) said that looking at the structure as a whole, it struck him that the bicycle wheel was not sufficiently used. Of course, the Author had explained at full length in the Paper that the bowl had to be inverted, and this must have had a considerable effect in adding to the cost of the structure. He could visualize that the bicycle wheel could have been used to turn the telescope in elevation fairly simply. Obviously, the condition had been laid down and the Author had had very little choice, but what would his alternative have been if the driving gear circles from the two battleships had not been available?

113. The moving of a mass of this size was always a difficult problem. It was easy to start a mass, but much more difficult to stop it in exactly the needed position. To some extent it seemed to be a comparable problem to a swing-bridge, which had to stop in an almost exact position. The use of the Ward-Leonard drives was very interesting, and the choice of these seemed a happy one. They were commonly used in heavy planing machines where the speed could be varied even in one stroke. The machine was slowed down when a cut had to be made, and it could be speeded up between the cuts. The Ward-Leonard drives were also very useful for reversing. Mr Palmer had been interested to note a coefficient of track friction of 0.003, which was, he believed, almost the friction of a roller-bearing itself; this showed that at the moment there must be an almost perfect track. Had this altered since the instrument had been put into use? It was too much to hope that in the future it would not increase.

114. With regard to the fabricating details, the welding of the 14-gauge membrane on the inside of the reflector could, he felt, have introduced problems. There was the contraction of the site welding over the whole of the inside of the reflector, which must result in some distortion. Had it deepened the bowl, and had checks been made on this from time to time?

115. Finally, regarding the site connexions, apparently fitted bolts had been used near the trunnions of the bowl structure. What bolts were used in the remainder of the reflector? Had they been black bolts, with just the main joints made with fitted bolts? If they were fitted bolts, were they reamerer on the site in position, or were large portions of the structure erected in the fabricator's shops and the majority of the reamering carried out there?

† Proc. Instn civ. Engrs, vol. 9, p. 65 (Jan. 1958).

Mr Christopher Scruton (Senior Principal Scientific Officer, National Physics Laboratory, Aerodynamics Division, Teddington) observed that his own interest in radio telescopes and in most engineering structures was centred mostly on the wind forces which might be encountered and the possibility of wind-excited oscillations rather than on the structural design, and he would confine his remarks to those specialized aspects of the problem.

117. With regard to the wind-excited oscillations which the Author had mentioned, there were several modes of oscillation which had to be considered, including the panel flutter of the membrane, but all had been deemed to be highly improbable other than that of the bowl oscillating as a rigid structure about the horizontal axis on its trunnions. The wind-tunnel tests had confirmed that this was a possibility, although they had not been sufficiently refined to give the Author any definite values of the damping required to prevent oscillations or even of the wind speed at which these oscillations might be encountered. Whether or not such oscillations occurred depended on the amount of structural damping and on the natural frequency. It had seemed hopeless to try to estimate the amount of structural damping which would be present in this novel structure, without, of course, the bicycle-wheel damper being present. However, frequencies were more amenable to calculation, and the Author had calculated the frequency for the bowl oscillation at $\frac{1}{3}$ c/s. Mr Scruton thought that this structure must have presented the Author with new problems in the estimation of its natural frequencies. Was it at all possible for him to measure the natural frequency on the completed structure so that it could be compared with his calculated value, and could he also measure the structural damping?

118. Perhaps it would be going too far to suggest to the Author that, for confirmation of the wind-tunnel tests, he should take the damper off and set the telescope at 45° in a high wind in order to give it the chance of oscillating!

119. Static tests were usually measured on small-scale models in the wind-tunnel (in the case of the radio telescope, the model had been one-hundredth of full scale) and there was often some doubt in scaling up the forces for design use. This uncertainty was due to the unknown influence of Reynolds number on the flow pattern. Was there any possibility of measurements of these forces on the structure to provide a check on the wind-tunnel predictions?

120. Concerning the wind-speed measurements at heights of 10 and 100 ft which had been made on the site, had it been possible to correlate the speeds so obtained with any of the various formulae which purported to give the variation of wind speed with height? Had those measurements any bearing on the choice of the particular formula which the Author had quoted in the Paper?

Mr M. H. Jeffery (Assistant Engineer, Freeman, Fox and Partners) referred to another radio telescope, not quite so large, but more than 200 ft in diameter, to be built in Australia, near Sydney. The Author of the Paper under discussion had described in more detail than usual, not just the completed work, but the basic philosophy and the steps in the thinking that led up to the final design. Those present who were engaged on design studies for the Australian telescope had followed a somewhat different path; could the Author therefore give some more explanation of some of the steps that he had taken?

122. Someone usually asked an Author of a Paper whether, if he had to do a job over again, he would do it any differently. Mr Jeffery felt that the present Author would say "Yes" in this case, if only because of the rapid rate of progress in radio-astronomy techniques, and in the availability of greatly improved non-structural elements, such as much more accurate servo-devices, and "maser" radio receivers. These receivers had an extremely low internal noise level, but their basic components operated only a few degrees above absolute zero temperature, so that the structural engineer might now be asked to provide for a load of a few hundredweight of liquid helium bottles, as well as radio equipment, all to be located as near as possible to the aerials at the focus of the bowl. Back in 1954, however, would the Author still have

designed the Jodrell Bank telescope as it was if he had not already obtained two full-circle racks; in other words, would he still have arranged for the bowl to be inverted for access to the aerial? It seemed that when the bowl was inverted the aerial array was still more than 100 ft above the ground and had to be lowered on a cable by a winch for any alterations to be made. Surely this was not the easiest form of access? Did it not, incidentally, preclude the use of rigid waveguides for carrying radar transmissions up to the focus?

123. A more serious disadvantage of arranging to invert the bowl seemed to be that it must then be pivoted right out at the rim on tall trunnion towers 170 ft high. The Author had said in § 15 that a supporting ring girder had first been considered, with a diameter about two-thirds of the bowl diameter. Mr Jeffery and his colleagues on the Australian design had found that these proportions provided the stiffest bowl structure, but they had also found that although the bowl deflexions did not increase much if the ring-girder diameter was decreased, the bowl deflexions increased quite sharply to a maximum when the ring girder was moved out to the rim of the bowl.

124. The Author mentioned, in connexion with the possibility of wind flutter, that the natural frequency of the bowl was only about $\frac{1}{4}$ c/s, and had quoted, in § 52, the equation for its natural frequency, which showed that it was inversely proportional to the square root of its inertia. Thus, to increase the frequency, the inertia must be reduced, and supporting the bowl centrally would help to do this.

125. It was now generally considered that the error of the reflector surface should be not more than 10% of the minimum operating wavelength, i.e. about 2 cm for a 21-cm wavelength. The Jodrell Bank telescope seemed to achieve this, but the pointing and tracking error should also be small if the high degree of resolution obtainable with a large-diameter reflector was to be fully utilized. The client's specification for the Australian telescope allowed a pointing error of only one minute of arc in a 10-m.p.h. breeze or 2 minutes in 20 m.p.h. One minute of arc was about 10% of the beam width at 21-cm wavelength.

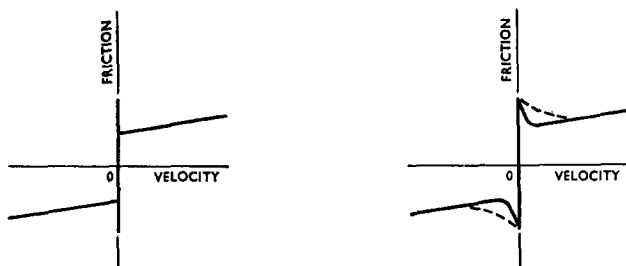
126. The Author had stated with truth that the structural, mechanical, and electrical problems could seldom be examined separately. Now, if this very small tracking error of one minute of arc was to be achieved, the requirements of the servo-control experts also must be heeded at an early stage. In connexion with the studies to which Mr Jeffery was referring, the servo studies had shown that, for the case of a 230-ft-dia. bowl on an alt-azimuth mount, they could not achieve a stable control and drive system if the speed of response of the system was too rapid. In fact, the servo-control specialists had wished to restrict the natural frequency of the drive system to below 2 c/s, while he and his colleagues, to avoid resonant oscillations of the drive with the structure, had to keep the natural frequency of the structure above 2 c/s. This had proved to be a very important factor in deciding the best basic form of their design, and they could achieve it only by mounting the bowl centrally on a rigid hub, pivoted on a small compact trunnion structure only 30 ft high. Of course, it was not possible to invert the bowl with this layout.

127. Could the Author say what the measured pointing error of the Jodrell Bank telescope had proved to be? Did tests show that the specified maximum values of ± 12 minutes of arc in elevation and azimuth had been bettered? Were these values obtained when scanning, as well as when tracking? How near to the zenith was accurate operation possible? Could the Author say roughly how much of the total error resulted from each of the main components, i.e. from the structural, mechanical, and electrical elements and from the control system?

128. Had the Author obtained any further test data, since the Paper was written, on the behaviour of the drive system at very low speed? At declination angles around 90° , the tracking velocities in both azimuth and elevation dropped down to zero, so that estimates of "stiction", as well as rolling friction, were necessary. Mr Jeffery had also experienced difficulty in obtaining data on friction values. It had been found, however, that if the friction characteristic was of the usual idealized form (Fig. 15a), the drive system was stable at creep speeds, even if the stiction coefficient was as high as 0.02.

But if there was a negative-slope portion (Fig. 15b), there was a danger of small-amplitude oscillations when the slope became fairly flat. They had been unable to discover any published data on this point. They intended to initiate tests as soon as they could; had the Author come across any relevant information during the course of his design?

129. Concerning the control system, did the Author contemplate inserting automatic corrections to the elevation motion at small angles of elevation to compensate for the gravity sag of the structure, and possibly also for atmospheric refraction of the radio waves? It should be relatively easy to do this, since these quantities were fairly simple functions of elevation angle only. Thus the corrections could be applied to the elevation drive at the load end of the control system, without in any way affecting the method of computing the alt-azimuth driving co-ordinates from the right-ascension and declination co-ordinates.



(a) Usual idealized friction-velocity characteristic.

(b) Probable actual characteristic with negative slope at very low velocities.

FIG. 15.—FRICTION-CHARACTERISTIC CURVES.

130. Since it never froze at Sydney, they had been able to adopt a mesh surface for the reflector; they thought that this considerably reduced the wind drag and temperature distortion. Mesh, although up to 99% efficient as a reflector of radio waves, was a very poor reflector of light and heat. It had occurred to them that a difficult decision might need to be made when using a solid reflector surface. Did one paint it black, so that the bowl absorbed the maximum heat and distorted the maximum amount, or did one paint it white, so that the bowl kept cool, but efficiently focused the heat so that the aerial was burnt up if the telescope was pointed at the sun? Possibly a coating of the "Scotchlite" type of substance that one saw on strips of tape on car bumpers was an answer. It reflected nearly 90% of the light back at the source, regardless of the inclination of the reflecting surface to the source. Had the Author found any trouble from temperature distortion with his solid reflector surface?

Mr Kenneth Paterson (Director, United Steel Structural Company, Scunthorpe) said that his only regret in reading the Paper was that the drawings and photographs gave little idea of the size and impressive appearance of the instrument. In that respect, he thought that the lantern slides which the Author had shown in his introduction were very helpful. It really was a staggering thing to see, especially when it was in motion; the movement in both planes was remarkably smooth, at whatever pace it travelled.

132. Mr. Paterson hoped that the Author would permit him to make two small corrections. The gabbard derricks mentioned in § 44 were in fact capable of only 15-ton lifts, and not 20-ton lifts. It was, of course, conceivable that they might have lifted slightly more. The clear height to which the hooks could be raised was 230 ft and not 250 ft. They were extremely conscious of that difference of 20 ft; they could have done with it on several occasions, as was always the case with these high cranes!

133. An enormous amount of credit was due to the steel erectors and the people who had worked on the erection of the telescope. At times it had been a nasty erection job, especially when the elements in the bowl were being put together. The men on top had been in an extremely exposed position, and the absence of serious accidents was very gratifying.

134. He thought that the erection sequence given in § 62 was not quite correct. The sequence had been as follows: the falsework tower was erected at the centre first and the hub put on top of it and accurately positioned; then the two pieces of the circumferential girder had been erected against the trunnions and the four radial girders spanned across. He would say in passing that the only erection of steelwork that had been actually completed in the shops and checked was that of the centre hub. The radial girders had been built on the ground at the site and very carefully checked before they were erected, but none of those things had been assembled in the shops.

135. An interesting point to note in the falsework was that these towers had been erected as required. They could not all be put up at one time because they got in the way of the derricks. There had been a considerable degree of settlement—more than he had expected—in the falsework towers. That was no doubt attributable to the taking up of a good deal of minor slack in a large number of joints, compression in the tubes, and some settlement in the timber grillages on which the towers were standing. It had also been very difficult to keep the weight equally distributed on top of the towers. It had proved essential to have small screw jacks on top of the falsework towers to allow adjustments to be made, and the maximum amount of movement on one of those 130-ft towers had been of the order of 3 in.

136. Regarding § 21, which described the method of surveying the interior of the completed bowl under varying conditions, Mr Paterson said that he presumed allowance had been made for the sag in the stretched wires because the span was very great and considerable accuracy was necessary. He was not quite clear about the exact method, but it appeared to him that the wires had been stretched across the top of the bowl, and he wondered how the vertical spans from the reflecting surface were measured up to the wires. It sounded a very difficult operation.

137. In Appendix I there was an analysis of the expenditure to August 1957, which was the time when the instrument had been commissioned. The presumption, therefore, was that it was substantially complete, financially as well as otherwise. He had seen it stated that the total cost of the instrument was of the order of £750,000—i.e. at least £150,000 higher than the figure which was stated in Appendix I. If that was true, how was the difference made up in the different elements of the construction?

Mr R. J. M. Sutherland (Chief Assistant Engineer, Messrs A. T. & T. D. Harris) asked about the foundations, on which the Author had been reticent, and requested some details of the soil and of any pile tests that had been done. Had any attempt been made to assess settlement? Had any measurements been made since, and, most important of all, did they agree? He asked this because he thought the telescope was an exceptional structure, and probably the foundations had been given very much more consideration than usual; he felt that a lot could be learned from them.

139. Finally, there was the question of parking. Had the Author considered the problem of parking the telescope, when not in use, always in one position, say, to conform with the direction of the prevailing wind, and had he then wondered if he would get differential settlement owing to the dead load being in one position for too long?

The Author, in reply to the discussion, commented on the President's remarks about the details of structural design. No exceptional calculations had been necessary in connexion with the towers, the chord girders, and the diametral girder. In addition to dead loads and wind loads, allowances had been made for stresses due to accelerations and retardations. In the case of the bowl structure it was necessary to calculate the stresses under a great number of varying conditions of wind and in a number of different

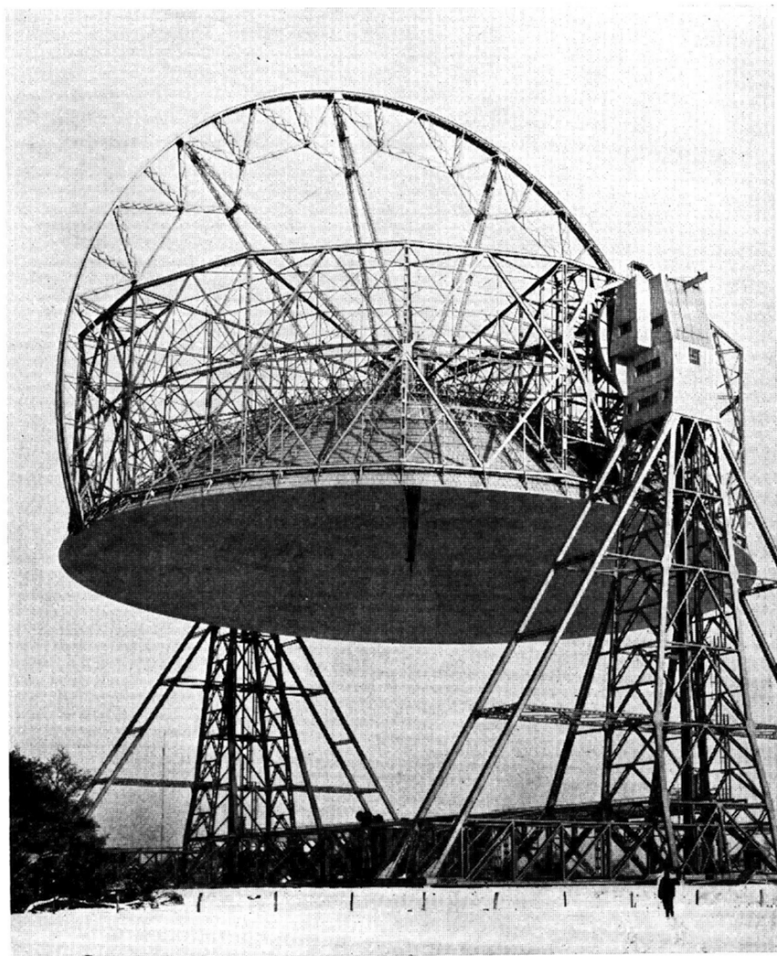


FIG. 16.—THE JODRELL BANK TELESCOPE INVERTED

positions. This had been very tedious work, but the fact that the whole structure was supported on two trunnions, and these trunnions revolved in journals carried in lubricated ball-and-socket mountings, had simplified the treatment of the main reactions. The bowl structure had to be entirely self-supporting without any assistance from the bicycle wheel, which had been added to the design for the purpose of damping dynamic oscillations.

141. Replying to Mr Palmer, who had suggested that insufficient use had been made of the bicycle wheel, the Author said that, from the very start of the project, the clients had appreciated the great advantage of inverting the bowl so that the aerial assembly could be readily lowered to the ground, this being a fairly frequent operational procedure. For this reason it was not possible to use a big wheel behind the bowl either as a method of drive in elevation or for assisting greatly in the support of the bowl. When the bowl was completely inverted the trunnions were able to carry the whole weight, including the weight of the bicycle wheel (Fig. 16).

142. As an alternative to lowering the aerial assembly to the ground the first detailed design of the Jodrell Bank telescope had included a travelling tower which was to have moved out on tracks carried by the diametral girder, so that it would be immediately over the central pivot when the bowl was inverted. The apparatus at the focus could then be reached from the platform at the top of the tower, which would have been equipped with a hoist. This arrangement had been abandoned because there were so many possible dangers to personnel. The present system of lowering the aerials to the ground worked quite easily.

143. In designs which the Author had developed for other large radio telescopes he had avoided the complete inversion of the reflector bowl. Future large radio telescopes were likely to be designed with a longer focal length in relation to the diameter of the reflector. It would then be convenient to suspend the aerial assembly from a tripod or tetrapod anchored to the periphery of the bowl structure. The aerials could then be lowered either to the bowl, when this is facing the zenith, or to ground level through a suitable aperture in the bowl. An important advantage of being able to invert the bowl was the great assistance thereby given to repainting operations.

144. Other designs which he had prepared for future large radio telescopes had used a series of wheels behind the bowl, bearing on balanced systems of rollers, which obviated the necessity of working to very close limits of dimension. At no time were structural tolerances of less than $\pm \frac{1}{16}$ in. likely to be required in large telescope structures because differential temperature effects would produce considerably greater errors than this. If very large diameter racks were used behind the bowl, for the elevation drive, comparatively simple methods could be adopted for ensuring constant mesh of the driving pinions.

145. As regards Mr Palmer's comparison with a swing bridge, it should be remembered that a swing bridge had only to stop in one or two positions, and in these it could usually come up against some sort of buffer. The telescope had to be able to stop and start at any point of the azimuth circle. If the large-diameter gun-turret racks had not been available it had been intended to use heavy chain drives for the bowl, with tensioning devices.

146. Referring to the question of track friction, the Author had been pleasantly surprised when the telescope commenced to rotate the first time a small current was fed to the azimuth driving motors. Although the telescope had been standing in one position on the track for 2 years, using protective lead pads, it had moved off without any trouble, using only a small fraction of the available power. There was no difficulty with regard to wheel slip because of the very heavy loads carried by the driving rollers, and the track could be lubricated. The coefficient of track friction quoted in the paper had been measured with the track dry. The track friction, as measured by the power input to the azimuth motors, had steadily decreased as the turn-table became more and more polished with use. The Author did not consider that there would be difficulty in maintaining this condition.

147. Care had been taken in the design to ensure good geometry, the main rollers were tapered, the tracks were arranged to conform with the taper and the roller shafts were truly radially centred on the centre pivot of the telescope. To date it had not been necessary to make any adjustment to the turn-table track, although this was arranged with anchor bolts and packings to facilitate readjustment. No settlement of the turn-table had been observed even where the telescope had been standing in one position during its construction period.

148. Dealing with the question on the welding of the membrane, a certain amount of trouble had been encountered. It was not intended to make any final adjustment of the slightly distorted parts of the membrane until the telescope had been in use for a considerable period in order to give the shape of the whole bowl structure a chance of settling down. It should be appreciated that the structural strength of the reflecting membrane was small compared with the comparatively rigid framework behind it, so that it did not exert very great forces on the framework. For that reason they had not been able to make as much structural use of the membrane as they would have liked. The steel membrane itself had cost more than £20,000. If, say, £50,000 could have been spent on it, a thicker membrane could have been used as a stressed skin, possibly with a considerable advantage.

149. With regard to site connexions, the structure was originally designed generally with black-bolted connexions, but when the use of the telescope on shorter wavelengths was contemplated, with the corresponding smaller deflexion tolerances, it was decided to use riveting practically throughout the framework, except for some of the large shop-welded units used in the tops of the towers. Fitted bolts were used for certain major site connexions. These were generally assembled and reamed in the fabricating shops before being sent to the site.

150. Replying to Mr Scruton, the Author said that nothing would give him more pleasure than to use the telescope as a most interesting piece of structural research apparatus. One could certainly do all the things that both he and Mr Scruton would like to do in the way of full-scale investigations on wind oscillations, and there should be no particular difficulty in measuring natural frequencies of the structure. The damping devices acting on the bicycle wheel could quite safely be uncoupled, at the same time arranging a break in the gear trains driving the elevation racks. Large wind strain gauges had been designed using high-tensile steel bars and the jacks which were used for pre-stressing these in concrete construction could be used as load-measuring devices. These bars could be coupled tangentially to the bicycle wheel. At present there was a programme to use the telescope 24 hours a day for radio research, and quite clearly a piece of apparatus costing £750,000 must be used to the best advantage and as often as possible. Some time it might be possible to carry out large-scale wind measurements.

151. People had remarked on the small size of the model that had been used in the wind-tunnel tests. The 1/100-scale model had been adopted on Mr Scruton's advice because if a bigger model had been used it might have tended to choke the throat of the wind-tunnel, causing complicated and indeterminate phenomena.

152. The wind-speed measurements at various heights which had been obtained so far from the wind-tower at Jodrell Bank did not agree very well with the accepted formulae; it would be useful at some future date to analyse these over a considerable period.

153. The Author referred to Mr Jeffery's interesting contribution. The steps taken in the design of the Jodrell Bank telescope were different from the procedure in the case of the more recent designs the Author had prepared for the Americans, after considerable practical experience had been gained at Jodrell Bank.

154. With regard to the influence of servo-device techniques, the Author was strongly of the opinion that experts in the development and use of these should be dealt with very cautiously, on both sides of the Atlantic. If one allowed the theories of servo-control to dictate the criteria for the structural design there was a grave danger of spending far more money on the structural and mechanical engineering than was really

necessary. Specialists in servo-devices appeared to the Author to be pessimistic. In the case of the Jodrell Bank telescope a comparatively large supply of driving power had to be available in order to control the telescope in high winds. The driving motors were, therefore, very many times on top of their job during the normal tracking or scanning operations and developed useful power at extremely low speeds. It had been extremely gratifying to see how the 2,000-ton Jodrell Bank telescope moved, started, stopped, accelerated, and behaved generally as it should. Very useful information could be obtained from the ammeters and volt meters on the direct-current circuits of the driving motors. After 6 months' use it had been found that the telescope was gradually improving in its behaviour. Experience showed that the friction velocity curves were similar to the second curve sketched by Mr Jeffery, but the actual differences between stiction and friction and the troubles which the servo people had prophesied had not been serious.

155. Mr Jeffery raised the matter of the accessibility of the aerial. Having adopted the principle of completely inverting the bowl the easiest form of access was to lower the aerial assembly to a platform near the ground. The use of rigid wave guides could easily be arranged to the focus. With regard to Mr Jeffery's criticism of the supporting ring girder being so near to the periphery of the bowl, the Author was well aware that this was a compromise, influenced by the desire to invert the bowl, but other factors, apart from limiting deflexion with the utmost saving in weight, had to be considered. Amongst other things it was essential that large sections of the bowl could be erected as conveniently and safely as possible. The method of tying the spokes to an outer rim had, he thought, been a safer method of erection than some others which might have been chosen. The stress considerations which had given the greatest trouble were those concentrated round the trunnions, transmitting the large wind loads and large dead loads from the bowl framework into the bearings. The large-diameter ring girder had certain advantages in dealing with the wind torques, which were the most worrying of all the considerations in the design of a reflector of this magnitude.

156. The whole of the control system, including the convertor for changing astronomical co-ordinates into elevation and azimuth, was not the type that the Author would use today. It was based on standard apparatus that could be obtained in 1950, and the general principles were similar to those developed for gunnery control. A lot of work had been necessary to bring this system to reasonable perfection, but now that this had been done it was achieving a following accuracy of about 1 minute of arc, which was 12 times better than had originally been thought possible.

157. On a new design the Author would probably use a system based on an analogue, that is a system representing a small-scale alt-azimuth telescope coupled to a polar axis telescope, so that when one moved the other moved with it. Such a mechanical system, using optical as well as electro-magnetic aids, would not completely obviate difficulties in achieving precise following accuracy near the zenith, but in practice this did not appear to be a serious disadvantage—the Author thought the main thing was to concentrate on sound structural and mechanical engineering with a good reserve of power. Experience at Jodrell Bank showed that the servo-control difficulties could be overcome. The question of inserting automatic corrections to compensate for gravity sag had been considered. For all practical purposes a simple servo-controlled movement of the focus would probably fulfill any operational requirements. So far it had not been considered necessary to add such refinements to the Jodrell Bank telescope. Very naturally the Jodrell Bank design had taken into account questions of heat reflexion and absorption by the reflecting membrane. A comprehensive series of experiments showed that a matt white finish on the inside of the bowl and normal painting on the back of the membrane prevented any exceptional temperature effects or dangerous concentrations of heat at the focus due to specular reflexion.

158. Replying to Mr Sutherland, the Author said it had been appreciated from the start that the whole telescope would be completely useless if appreciable settlement occurred. The site had been explored by boring and a large number of piles had been

used, comparatively lightly loaded, going down to a fairly hard red marl at a depth of about 60 to 80 ft. The turn-table was being checked every 3 months as a routine matter but no settlement whatever had been observed. The cost of the piling had been a small proportion of the total cost of the telescope.

159. With regard to "parking" the telescope, the normal instruction for leaving the telescope in high winds was to have the bowl facing the zenith and the two towers roughly in line with the direction of the wind. The telescope control room is constantly supplied with meteorological data and there was always someone on duty, and power available, to look after the safety of the telescope in the event of a serious gale.

160. The Author thanked Mr Paterson for adding to the description of the erection. He agreed with everything Mr Paterson had said. A physical survey of the bowl was being carried out when weather conditions and operational use of the telescope permitted. Tensioned wires were used in order to make a series of triangulation surveys in planes passing through the major axis of the paraboloid on a number of diameters. One could easily make direct measurements along the surface of the bowl and by recording the variations in length of the tensioned wires, after allowing for sag, a survey of the bowl deflexions can be made by solving the triangles. Weights were arranged to keep the wires in constant tension as the bowl tilted in elevation. All the radial measuring points were arranged at the top of the aerial tower near the focus. The focus could also be determined by radio means during observations on radio stars.

161. Replying to Mr Paterson's question on expenditure; the Author emphasized it had been made clear in the paper that the cost figures excluded acquisition of site, radio receiving and transmitting equipment, and professional fees. The total cost of the project was, therefore, considerably greater than the net constructional cost given in the paper.