

Paper No. 6178

Lake Nyasa and the River Shire†

by

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Discussion

Mr H. D. Morgan (Partner, Sir William Halcrow & Partners, Consulting Engineers, London) said that the natural phenomena with which the Paper was concerned were on a grand scale, but it might be said that as much, if not more, was now known about the Shire and its catchment as about many British rivers. About a year ago he had been surprised to find that it was impossible to obtain any recorded flood information about a large river in the Severn area, and to be told by the people concerned that they had never bothered to collect such information.

87. The Author had indicated that he did not accept that natural events had an entirely random behaviour. He had attempted, and Mr Morgan felt succeeded, to show that there was a remarkably close relation between the behaviour of sunspot activity, or its incidence, and climatic conditions. It was curious that there should be such an apparently close agreement.

88. If it was possible to predict solar behaviour, then clearly this would enable a short-term forecast to be made of the probable behaviour of rivers. In any event, even if one could forecast only the broad fluctuation of solar activity, to say whether it was increasing or diminishing at any time, a short-term forecast could still be made of probable river activity.

89. He had been in Rhodesia last February when there had been some excitement about the level to which Lake Nyasa would move. In the event, the agreement between the actual and predicted maximum levels had turned out to be much closer than anything he had hoped for at the time.

90. The Author's formula for evaporation was a little curious dimensionally! Nevertheless, it was based on extensive data, it seemed to fit the facts, and no artificial coefficients were introduced. No doubt more information would be forthcoming in the future to give a further check, although the agreement in Table 4 was remarkably good. Possibly members had been able to apply the formula to information of their own, and it would be interesting to know the results.

Sir Thomas Foy (Adviser to British Thompson Houston Co. (Canada) Ltd.) observed that the Author had stressed the delicate nature of the balance of the water account on Lake Nyasa, which was primarily attributable to the high evaporation from the lake relative to the rainfall and inflow. As the Author had stated, the average annual rainfall on the catchment down to Liwonde was about 49 in. The annual evaporation from the lake area was 60–70 in. It would readily be appreciated therefore that the annual water account of the lake area by itself was always in deficit. The free water was the amount contributed by the wet season run-off of the land catchment diminished by this deficit.

† Proc. Instn civ. Engrs, vol. 8, p. 363 (Dec. 1957)

92. The catchment area, including the lake, was 49,000 sq. miles while the actual area of the lake was 11,500 sq. miles. The ratio of land catchment to lake area was therefore only approximately 3.3 to 1, which was a very small figure. With high and fixed losses from the lake area, it was only to be expected that the effect on the free water in any year would be a magnification of the variation of the run-off of the land catchment.

93. With this delicate free-water balance the Author had very correctly attached great importance to seeing if there was any evidence of secular change in the rainfall, possibly of a cyclic nature, and to endeavouring to extend back the record by all available means.

94. The correlation which the Author had suggested between the 3 years' summation of the rate of change of sunspot numbers and the 3 years' summation of free water on Lake Nyasa was fascinating and appeared to be established by the records over the period 1920-55, as set out in Fig. 7.

95. Sir Thomas had made a detailed study of the data relating to Lake Nyasa assembled by the Author and had come to the conclusion that the amount of free water of the system showed a cyclic variation, with a half-period of about 33 years. This conclusion, while not incompatible with the Author's correlation, increased the desirability of examining the latter over a more extended period to determine with more precision the correlation factors and to evaluate the subsidiary causes which existed in a complex relation of this nature and which caused the rather large variations in individual years from the normal.

96. The most important practical conclusion deriving from the Author's studies was that if they were confined to the period 1915-56 the average annual utilizable yield would be 8,500 cusecs, whereas if the period 1847-1948 was studied the utilizable yield would reduce to 6,000 cusecs.

97. Having regard, first, to the enormous reserve afforded by the lake; secondly, to the present lake level being at its upper limit; thirdly, to the general advantages of operating the lake at a normal level well below the upper limits; and fourthly, to the feasibility at very small capital cost and annual charges of primary pumping from the lake into the upper river channel to utilize this enormous reserve, as set out in § 83, it appeared strongly advisable to base any project on the higher figure of utilization rather than the lower.

Mr A. N. M. Robertson (Sir William Halcrow & Partners, Consulting Engineers, London) said that the Paper was a striking example of the way in which civil engineers horizon of scientific knowledge had widened. He drew attention to inquiries which had been made for some years past which were related to the problems of the Shire Valley scheme—primarily to the reclamation and irrigation proposals for the project. It seemed that weather control might be of advantage to countries like Nyasaland and Ghana, each of which had a heavy and irregular seasonal rainfall followed by periods of dry weather. They both also had an uneven topography, and in such conditions the layout and operation of an irrigation system became expensive.

99. The inquiries had been addressed to organizations closely interested in this research and in the production of artificial rainfall, notably the physical research branch of the Meteorological Office at the Air Ministry, in the United Kingdom; the Radio-physics Division of the Commonwealth Scientific and Industrial Research Organization of Australia, in New South Wales; the East African Meteorological Department; and the Water Resources Development Corporation, at Denver, Colorado, U.S.A.; their ready response in every case was gratefully acknowledged. Valuable information had also been noted in the bulletins and technical notes of the World Meteorological Organization at Geneva and in the journals of the Royal Meteorological Society in Britain, as well as in other scientific and technical publications.

100. As an illustration of what was happening, Mr Robertson referred to first, the physical research branch of the Meteorological Office, which had been experimenting with artificial rainfall on Salisbury Plain for some time. It had been stated in March

1956 that more than 3 years would be necessary before published results could be expected from their programme of investigation. Secondly, there was a special advisory committee on weather control in the United States, due to report direct to the President of the United States. Their study had begun in July 1954 and its completion had been expected by June 1956, but they had found it necessary to submit an interim report and to ask for an extension of the time until June 1958—about 6 months ahead.

101. It therefore seemed that there was a pause in the advance of fundamental measures of weather control, but owing to the great efforts which were being made towards a solution of the problem it seemed reasonable to expect that methods of control would be adopted in many parts of the world to great advantage before many years had elapsed.

Dr F. Dixey (Director of Overseas Geological Surveys, Imperial Institute, London) commented on the variations in the level of the lake and on the relation they seemed to have to sunspot phenomena. It seemed to him that there was an appreciable relation between the lake levels and sunspot activity, as expressed through climatic variations, and this could be supported, as he would show, by independent mathematical analysis.

103. The Author had also rightly taken into account the recently available figures of outflow from the lake and had shown that there was a closer relation between the free water, which took into account lake level and outflow, and the rate of variation in sunspot numbers. The graphs seemed to indicate an unmistakable relation between sunspot and lake-level phenomena.

104. Fig. 10 was similar to one already shown by the Author; it showed only lake levels, with the sunspot variations in the bottom graph. In its relation with sunspot phenomena it might be regarded as superseded by the Author's more detailed, technical, and very useful investigation. Nevertheless, Dr Dixey wanted to refer to some features which were perhaps a little more than purely subjective. He realized the danger of looking at these things subjectively, but the graph had been submitted to mathematical analysis and the accord between lake-level maxima-minima and sunspot maxima-minima had occurred considerably more frequently than would be expected by mere chance. He would not go further than that.

105. Fig. 10 dealt with Lakes Nyasa, Tanganyika, and Victoria, and it would be noticed that there was a remarkable correlation in the period 1916-18, and other accordances could be shown. He wanted to refer mainly to the maxima mentioned in the Paper for lake levels—1937 and 1948—which in general were reasonably well in accord with the sunspot maxima. A point of interest at the time he had published this Figure, about 5 years ago, had been what would happen in the near future, because a number of these lakes had been under examination with a view to bringing into effect a number of engineering schemes concerning their level. He had then suggested that there was a reasonable probability on the basis of what was already known that near times of maximum sunspot activity there would be a relatively high lake level and in periods of minimum activity a relatively low level.

106. It was interesting to see what had happened in this connexion since then. About 1952 the level of Lake Nyasa had been somewhat low. There had been a rise in Lakes Tanganyika and Nyasa at that time, but afterwards the level fell for a year or two and since then, for the past 3 years, it had been rising. It seemed to him that whatever might be the relation, or however it might be expressed, between lake phenomena and sunspot phenomena, the lake level was rising towards a maximum about the time of the present sunspot maximum, which was about the present year. This was the International Geophysical Year, which was based on the sunspot activity maximum expected at this time.

107. In § 15 of the Paper it was stated that since 1948 there had tended to be a continuing decline in level, and he would be interested in the Author's comments on this.

108. He understood that the Liwonde bund had been built in 1955 and closed in September 1956, with a view partly to preventing a fall in the level of Lake Nyasa. It had been found necessary to breach the bund in August 1957, because 3 months

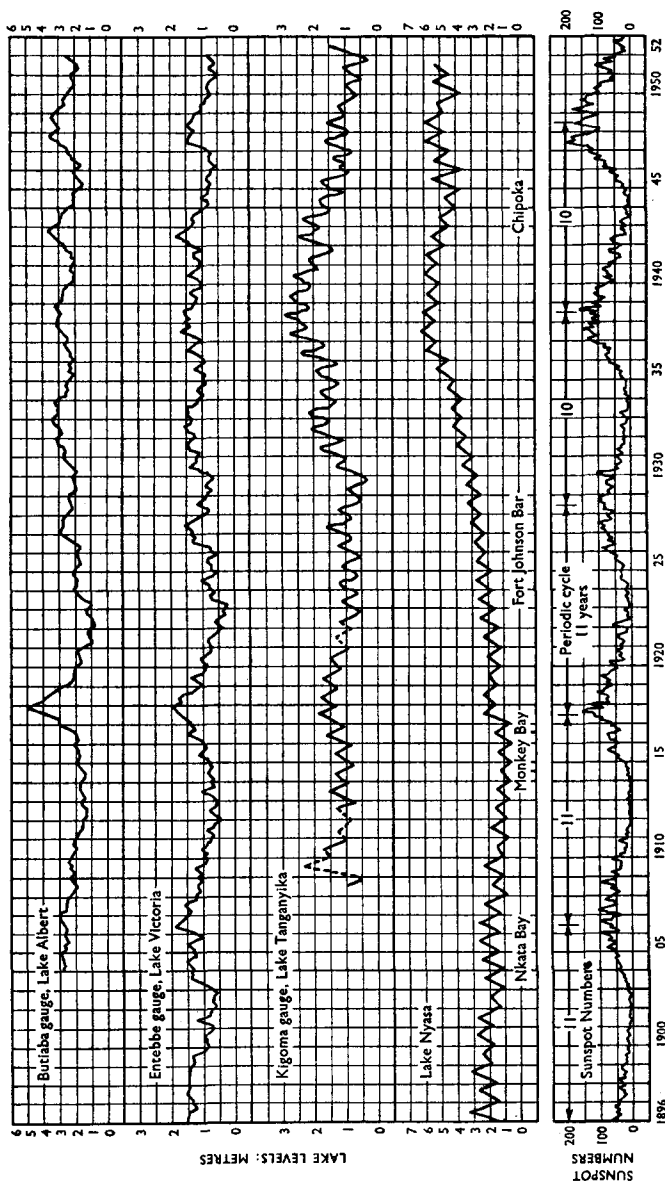


FIG. 10.—FLUCTUATIONS IN EAST AFRICAN LAKE LEVELS WITH CORRESPONDING SUNSPOT NUMBERS

before the lake level had in fact risen to practically the 1948 peak level and seemed likely to go still higher during the next season. Had the Author the latest figures for the level and the outflow of Lake Nyasa and had they indicated a rise to or above the 1948 and possibly the 1937 level rather than a continuing decline, as he had appeared to have implied when introducing the Paper.

109. He also understood that the lake had risen appreciably during the season when the bund had been closed, but no more than in the previous year when it was open, which meant that the lake level was rising also from natural causes. What effect had the bund had on the lake level during the time it was closed? It seemed to Dr Dixey that the available figures indicated an approach to a new maximum, and the probability that this maximum, like the two previous maxima in 1937 and 1948, would occur at or near a sunspot maximum. It also seemed to him more than a coincidence that at this period, near maximum sunspot activity, a wholly unprecedented rise of the upper and middle Zambezi should have occurred in the past season, when the Kariba coffer-dam had been submerged.

110. From direct observation of periods of falling lake level in times of no outflow, and of rising lake level in periods of continuous river flow, he had from time to time suggested that any dam to stabilize outflow from the lake would have to be a relatively high one, and he was interested to see that the Author had concluded, after his own extensive and detailed observations, that the simplest way of achieving stability would be to raise the lake level to 6 ft above the highest known level of 1937, when neither dredging of the river nor pumping water into it would be necessary in order to maintain river flow.

111. However, in view of what they now had learnt, might it not take many years to raise the lake to the required high level, especially if regular outflow was maintained? If the dam was built during one of the periodic declines in lake level it would presumably be difficult to accumulate much water at that time, and if by the next high level of the lake on the periodic cycle the dam was not filled, it would presumably involve another longish cycle before the high level could be attained. He noted that the Author had recorded that the control of the lake within narrow limits was an intractable problem to which there was no ideal solution.

112. The Author had stated that he had found no positive information about long-term climatic variations in the Southern hemisphere comparable with those in the Northern hemisphere since the last Ice Age. Some information was nonetheless available. Pre-historians of Africa, in their recent conferences, such as at Livingstone in 1955 and previously at Nairobi, had shown that climatic variations during the period covered by the Ice Age and since were very accurately known and that those variations could be matched by those determinable in Central Africa and South Africa. Dr Dixey thought that those could be held to apply to the Lake Nyasa region.

113. Some information was available in the Lake Nyasa region in that certain mid-Pleistocene lake deposits were at a later stage laid bare and eroded, and overlain by sand laid down under arid conditions, indicating sharp changes of climate such as were known elsewhere. The Upper Zambezi Valley suffered aridity to such an extent that at least twice in Pleistocene times the Upper Zambezi ceased to flow and its bed was over-run by desert sands. Comparable arid periods had doubtless affected the Lake Nyasa region, too, and certainly within Pleistocene times the lake had been subject to variations of level far beyond the relatively small variations considered in the Paper.

Mr E. M. Gosschalk (Sir William Halcrow & Partners, Consulting Engineers, London) observed that in the past the state of the lake and river may have been subject to enduring transformations, but under present conditions a very remarkable degree of regulation of lake level was achieved by the natural processes of rainfall and evaporation, and by the capacity of the outlet channel which was governed by the level of the lake. This was perhaps an illustration of the innate dynamical stability of nature.

115. Under long-average conditions the lake would tend to rise and fall about 3 ft each year, between 1,548.5 and 1,551.5 ft above S.V.P. datum, the wet-season

inflow being balanced by evaporation and discharge over the year. When the lake rose above these levels more water was discharged down the Shire, tending to hold down the maximum level of the lake, and of course in dry years the reverse happened.

116. A probability study showed that, starting from the average levels, the highest recorded level of the lake, 1,556.4 ft above S.V.P. datum, might be exceeded by approximately 1 ft with a probability of one in a hundred as a result of three consecutive excessively wet years, whereas with the same probability after two very dry years the discharge might fall below 2,000 cusecs at the end of the dry season, and the spread of reeds might then further reduce the capacity of the channel. The Author had thought that these might not be random phenomena, and Mr Gosschalk did not suggest that the figures did more than indicate what might happen. The ever-changing state of the channel and the possibility of the excessively wet or dry years occurring when the lake was already swollen or depleted were not satisfactorily accounted for by this estimate.

117. A sidelight on this question was provided by numerous Africans who considered it reasonable to build on and inhabit the fringe of the lake below the highest recorded flood level. The density above that level was, however, a good deal greater.

118. The eventuality of raising the top water level of the lake mentioned by the Author in § 84, would liberate a control scheme to a great extent from the present limitations of the outlet channel capacity. It appeared that under average conditions it would take 7 years or so to raise the lake by the amount stated, even if the discharge was stopped entirely for the whole period, unless, as seemed desirable, the conservation of water could commence towards the peak of a cycle when the amount of free water had been greater than long average.

119. With reference to the lower river, Mr Gosschalk said it had been estimated that the maximum normal flood in the Ruvo above Chiromo, for comparison with the figures given in Table 3 and with the flood mentioned in § 46, was 155,000 cusecs.

120. The banks of the Shire in the lower marshes were overtopped on a wide front even when the river discharge was as low as 6,000–7,000 cusecs, and in addition there were perennial spill channels discharging into the swampland. As the result of the deposition of silt when the banks were overtopped, a ridge was often formed on which the river passed through the marshes. The suspended silt load carried by the Shire at Chiromo had varied from about 250,000 to 900,000 tons/year over the 3 years for which records were available, and the load carried by the Ruvo appeared to be somewhat greater.

121. Mr Gosschalk referred to the influence of the Zambezi, the gradient of which, about 1.5 ft/mile, was steeper than that of its tributary the Shire, given in Table 1. As a result, the water levels at the confluence of the Zambezi in flood enabled it to dominate the low ground to within about 5 miles of Port Herald, and the effect of backing up the Shire could extend even further upstream. The operation of hydro-electric schemes on the Zambezi would no doubt influence the regime in this reach and perhaps help to reduce the incidence of flooding.

Mr W. N. Allan (Irrigation Consultant to the Sudan Government) stressed the need for the continuance and extension of the collection of data. There had been reference to that need in Great Britain, and it was certainly as relevant, if not more relevant, in the case of Africa, since although certain catchments had already been well recorded, notably the Nile (with which he had been connected), a great many had not.

123. In § 56 the Author had said that Lake Nyasa had a rather indefinite relation with the behaviour of Lake Victoria, and it was therefore of interest to compare the hydraulic conditions of those two lakes more particularly. In both the proportion of land catchment to lake surface was of the order of three to one. On the land catchment of both the mean annual rainfall was about 49 in. The mean annual rainfall on the surface of Lake Victoria was considered to be about the same figure, as was the mean annual evaporation from that lake; in comparison, the Author's estimate of the mean annual evaporation from Lake Nyasa was appreciably higher—61 in.

124. Turning to the question of free water, the mean figure for Lake Victoria in a

period of 48 years was about +1 ft, compared with +0.83 ft in a period of 41 years for Lake Nyasa, as given in Table 2. In both these lakes the annual figure of free water was subject to wide variations. In Lake Victoria the maximum in a single year was +3 ft and the total of the three highest successive years was +6.7 ft. Corresponding figures for Lake Nyasa were +3.85 ft in a single year and +7.2 ft in 3 years—not very different. The largest negative figure in 1 year for Lake Victoria was -1 ft and for Lake Nyasa it was considerably greater—-2.5 ft. The standard deviations, in the same periods of 48 and 41 years, appeared to have been about 1 ft for Lake Victoria and about 1.3 ft for Lake Nyasa. In Lake Victoria, as in Lake Nyasa, the range of variation of the free water was much greater than that of the outflow, and regulation of the outflow could never entirely stabilize the contents and level, nor even ensure that the contents would not fall below any specified minimum figure.

125. Mr Allan had mentioned these figures to show that there was a considerable degree of similarity in the general pattern of the hydraulic conditions in the two lakes. It would be of great value and interest if comparative analyses of many such tropical catchments could be made, to see how far similar patterns of behaviour held good, or what points of resemblance between different patterns of behaviour could be found. Only in this way could satisfactory methods be devised for the regulation of such lakes by control of their outflow. These methods must be based on principles which were generally applicable, although adapted in their particular application to suit the individual conditions in each case.

126. With reference to the Author's comments in §§ 71-84 on the regulation of Lake Nyasa, Mr Allan thought that all engineers who had experience of large catchments would sympathize with the Author's conclusion that the problem was intractable and that there was no ideal solution. Just for that very reason, however, it was desirable and possible to devise methods of long-term regulation which would give a reasonable compromise between the conflicting interests and objectives which existed, such as navigation, irrigation, and power. For example, if for a lake such as Lake Nyasa there were specified maximum and minimum lake levels in the interests of navigation, and a minimum discharge down the river in the interests of irrigation and power, computations over the period of recorded data would test how far various proposed figures for these could have been maintained over that period.

127. In connexion with such test computations, a device which had been found of value in similar investigations on the Nile was the concept of target discharge and target content. Mr Allan was assuming in the case of Lake Nyasa that a control barrage existed and that its discharge was not limited significantly by the conditions of the river channel. In other words, he was assuming that the Author's proposition to raise the range of levels by about 6 ft was envisaged. The operation of the concept was as follows. For the target discharge out of the lake a figure was selected which might be the long-term mean of the recorded natural discharges, or something very near it. The figure for the target content of the lake might be somewhat below the mid-point of the proposed range of lake contents. For each time period, say a month, or perhaps a shorter period if found desirable, the discharge actually to be passed would be the target discharge plus or minus a small proportion of the amount by which the lake content at the end of the previous period was above or below the target figure.

128. If the discharge figure as so computed, or the resulting lake content at the end of the current period, were found to be outside the specified limits, the latter would over-rule. One should not let the discharge go below the specified minimum, but no maximum need be specified, because when the lake filled to maximum level the excess would merely be spilled. The magnitude of the small proportion of the divergence of contents from the target figure, and suitable values for the target figure themselves, were evolved by repeated trial and error. The method could be extended, if desirable, to take account of more than one criterion simultaneously.

129. It was true that target figures and parameters obtained in such a way were those suited to the conditions of the particular period of the recorded data, and there could be no guarantee that they would also exactly suit the conditions of some other

future period; indeed, this was highly unlikely. But even this could be taken care of by arranging that the figure for the target discharge itself should not be absolutely fixed but should be varied, say once yearly, by a small amount similarly assessed from the condition of lake content reached at the end of the previous year; in other words, from the cumulative effect of the regulated conditions to date. This was one possible way of making the scheme of regulation self-adjusting in order to conform with long-term trends of change in the hydrological conditions.

130. The use of any such trial-and-error or simulation method of testing schemes of control and ascertaining suitable values for targets and parameters, except in very simple cases involved large amounts of computation, and until recently this had limited its use. In the studies on the Nile to which Mr Allan had referred this difficulty had been overcome by the use of an electronic computer. Once a suitable programme for such a machine had been prepared it became possible, in a short time, to test a whole series of values for target figures and parameters and to ascertain which was best.

131. In §§ 83 and 84 the Author had pointed out that by raising the level by 6 ft it would be physically possible to pass discharges up to about 20,000 cusecs without dredging or pumping. The advantages of this were obvious; what were the chief objections to it? In particular, did the Author consider that the evaporation would be significantly increased by such a raising of the range of levels over which the lake would operate? Mr Allan did not see that if such a step was taken it would be essential to build up the lake level to its new maximum figure in any limited period. His own attitude towards such a problem would be to specify fairly low figures for the minimum and target discharges down the river, and then let the natural variations of the free water gradually build up the content until it began to range between the desired maximum and minimum. When that had happened the target discharge could be put up to a figure nearer the long-term mean figure.

Mr C. F. Lapworth (Partner, Herbert Lapworth and Partners, Consulting Engineers, London), referred to § 67 in which the Author had given a water budget and an estimate of the evaporation by a formula of his own. There was also a method of estimating evaporation from the lake surface by means of heat balance. The basic equation was:

$$H = E + K + S + C + M$$

where H denoted the net radiation derived from the sun and sky in heat units, E the evaporation from the lake in heat units, K the heat transfer between lake surface and atmosphere, S the quantity of heat storage in the lake itself (which varied with the volume and temperature of the lake), C the conducted heat between lake-bed and water, and M the heat advected by run-off or rainfall.

133. Information available from Lake Heffner in the United States and from Kempton near London, showed that K for annual periods was only 3 and 3½% of E respectively and could therefore be ignored. In both cases S over a period of a year was less than the equivalent of 1 in. of evaporation. He knew of no available information regarding the value of C but it was usually considered negligible. M could be shown to be negligible over a period of a year or more. The equation therefore reduced to $H = E$.

134. There was little meteorological information available at Nyasa from which H might usefully be calculated; there were, however, two stations near the lake, one at Kota Kota and another at Likoma Island, and readings were available for a period of 5 years. Based on these, Mr Lapworth had estimated that the short-wave radiation reaching the surface of the lake amounted to the equivalent of 97 in/year evaporation. That was to say, if all the heat which came from the sun and the sky was used for evaporation there would be a resulting evaporation of 97 in. However, some of that heat (about 5%) was reflected by the lake and had to be deducted in the calculation. The earth acted as a black body radiating heat at air temperature, and, making allowance for the quantity which was trapped by clouds and re-radiated to

earth, the back reflexion was estimated at 29 in. This made the total of evaporation 63 in., which compared with the water-budget evaporation of 61 in. given in the Paper and the estimated figure of 61 in. given by the Author's formula. This therefore confirmed the figures given in the Paper.

The following contributions were received in writing.

Mr H. F. Wilmot (Engineer, Sir Alexander Gibb & Partners, Consulting Engineers, London) observed that while the behaviour of lakes Nyasa and Victoria was very different, there were certain aspects which made a direct comparison between them of considerable interest. Table 5 demonstrated their physical and climatic similarities and differences.

136. The Author's attempt to link up sun-spot numbers with the quantity of free water available was interesting, in view not only of Dr Dixey's work referred to, but also of Dr Brook's work²⁸ on those lines relating to Lake Victoria levels. A very close correspondence indeed existed over the period 1896-1922 between the sunspot numbers and the levels of both Lakes Victoria and Albert (see Fig. 10, illustrating Dr Dixey's contribution). However, from 1923-49 this correspondence ceased.

137. It should be noted that Hurst and Phillips had disputed the existence of any other than accidental correlation at Lake Victoria on the rational ground that the rate of change of lake level, and not the lake level itself, depended on rainfall and evaporation.²⁹ It followed that if a relationship such as this held, it must equally hold for rainfall and for evaporation. Moreover, if the numbers were periodic, one would expect them to occur at a quarter of a period (about 3 years) in advance of the maximum lake level, whereas Dr Brook had found the maxima to coincide.

TABLE 5

Description	Unit	Lake Nyasa	Lake Victoria
1. Catchment area	sq. miles	48,850	103,200
2. Mean annual rainfall	inches	48.6 (1928-56)	43 (1899-1946)
3. Average run-off (estimated)	%	5.5	6.7
4. Lake area	sq. miles	11,430	26,500
5. Mean lake level (above M.S.L.)	feet	1,550	3,716 (U.S.D.)
6. Overall lake dimensions	miles	360 × (10 to 50)	250 × 156
7. Mean rainfall over lake	inches	49 (est.)	50 (est.)
8. Mean natural outflow from lake	cusecs	8,500	22,300
9. Evaporation from lake	inches	61 (est.)	?
10. Estimated average annual loss	%	94.5	93.3
11. Annual range in lake-surface temperature	°F	?	10
12. Mean air temp. at 12.00 hours	°F	?	80
13. Air temperature range	°F	76.3	40
14. Max. annual air temp. value	°F	?	88 to 95
15. Humidity	%	65% mean	20 to 50 by day 100 by night

138. It would seem that the yearly variation in sunspot numbers was very irregular (as evidenced over 1878-1956), but that their plottings over the period 1895-1948 happened to conform to well-defined cycles completed in 10 to 11 years, though with greatly varying origins (Fig. 10); and the spots of the cycles at the outset were centered in general in solar latitude 25° and at a maximum were most thickly clustered about

²⁸ References 28 *et seq.* are given on p. 245.

latitude 13° , being a maximum near the equator; the succeeding new-cycle spots then began to appear again in the high latitudes, and the whole cycle was repeated. If any terrestrial phenomena varied in like phase, this would point to a possible relation between them and the sunspots.

139. Dr Hurst in his recent book³⁰ had clearly proved that there was no causal correlation between lake levels and sunspots, but that there was between rainfall and annual changes in lake levels, as assumed by the Author. He had also demolished³¹ similar claims of correlation for the Columbia River, U.S.A. and had pointed out the danger of using a moving average, since it could obscure and even eliminate long-period effects, as in the above case where for 1887–1889 the discharge had fallen through nearly its total range, rising in 1889–94 from a minimum to a maximum, all within the period covered by a 7-year average. Dr Hurst's studies of Roda (Cairo) gauge records of the River Nile, covering about 1,080 years, had failed to disclose any marked periodicity beyond the general phasing of alternate long high and long low periods, and for the period 1870 and onwards had found no correlation whatsoever between the Nile discharges at Aswan and sunspots.

140. A critical examination of the 3-year grouping curve given in Fig. 7 showed close correspondence between peaks of sunspot rate-changes and the free-water inflow to lakes Nyasa and Victoria for the years available. The generalization connecting them with 3-year mean temperatures in central England did not seem so convincing. Development of these curves on a 1-year rather than a 3-year basis, while showing up annual irregularities, should prove more illuminating.

141. Fig. 9 showed more clearly the connexion between the low periods of the annual rate of change of sunspots, and critical deficits or low periods of water inflow in 8 years between 1915–1957 in Lake Nyasa; a lag of at most a year was exhibited. This lag could denote that it was rather minima sunspot values which were the causative effect, but it did not necessarily follow, as would be shown.

142. Since the effect of sunspot activity was not confined to any one part of the earth, one would expect all phenomena on which it supposedly had effect to exhibit similar characteristics, though not of the same intensity necessarily. Thus, if floods were caused by such activities, they should all occur more or less simultaneously, allowing appropriate lag intervals over the zones subjected to such influences. No such marked occurrences had in fact occurred, and indeed the contrary would seem to have happened. Thus, as a matter of interest, mass cumulative deviation curves for the Nile and the Thames, over similar periods (1870 to 1945 and 1883 to 1949 respectively) showed curves of closely *opposite* characteristics, with high peaks and low depressions occurring in 1898 and 1909 (see Fig. 11).

143. The difficulties of making generalizations were thus markedly apparent, demonstrating that all the factors which affected the results had not been taken into account.

144. An investigation of the main source of rainfall occurring in the eastern and central African plateaux might help to throw light on the problem of linking up the sunspot effect. It had been accepted in the main that this source was over the South Atlantic waters, where sunspot activity might well affect evaporation in a close relation. The amount of *additional* evaporation due to this cause solely did not necessarily occur at the peak of this activity, since it would depend on the movements of the moisture laden air in this area; at the peak it was possible that a partial stabilizing effect in the air currents took place, causing a slight blanketing effect over the water and so reducing evaporation, involving the maximum occurring, as claimed by the Author, over the period of maximum rate of change of sunspots.

145. Critical examination of the plot for each lake separately in Fig. 7, showed, on the whole, for Lake Victoria a close correlation between the peaks for the free inflow water and the sunspot rate of change, though a 2 years' premature rise had occurred in 1902, and in 1925, 1931, and 1941 the rises had been excessive, if solely related to the magnitude of the change rate; furthermore no corresponding rise had occurred in 1910 as would be expected, if the causative effect propounded held good.

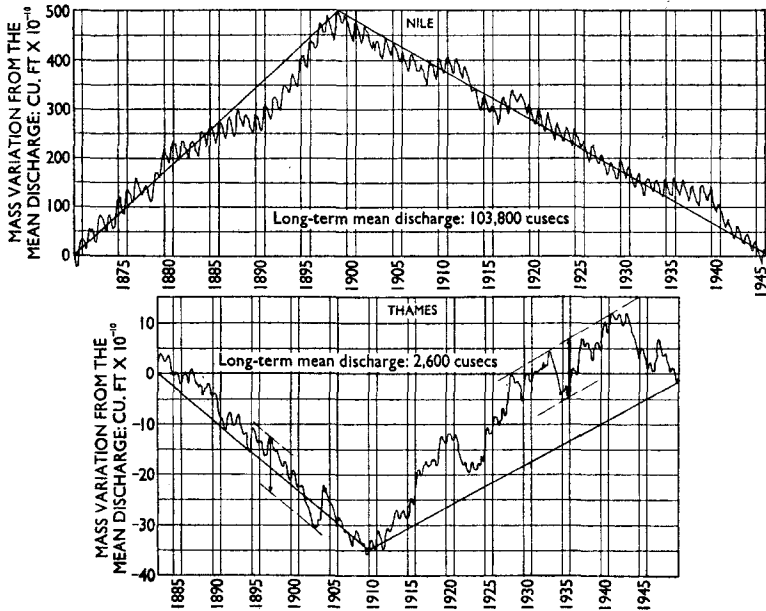


FIG. 11.—MASS CURVES FOR DISCHARGE OF NILE AND THAMES, SHOWING DEVIATION FROM THE MEAN

146. Part of the above criticism might be lessened by the possible explanation that an excess of the moist-air currents had been deposited over western and west-central Africa before reaching the central plateau, or as a possible additional cause that some or all of the excess had been borne past it and caused unduly heavy rainfall in Abyssinia. Thus the relationship tentatively established could probably only be rigorously confirmed by considering *total* rainfalls over a belt of Africa.

147. Turning to the data relating to Lake Nyasa, the correspondence in the peaks was nothing like so good, but again a certain correspondence existed, and a similar argument to that above held good.

148. Finally, if the results for both lakes were combined, the *relation in magnitude* to the sunspot variation for the short period available appeared to be very much closer, but the period was far too short to establish the validity of a law of causation within close limits.

Mr Harry F. Blaney (Vice-President, Hydrology Section, American Geophysical Union) referred to problems which he had had to solve in river basins in the western United States,^{32, 33} which were similar to those the Author had handled in connexion with Lake Nyasa and the River Shire.

150. He was especially interested in the unique way in which the Author had treated the hydrology section of the Paper. However, the use of water by phreatophytes³⁴ (water-loving vegetation) and other vegetation in the catchment basin above Lake Nyasa did not appear in the hydrological balance equation. This might be accounted for by the river-flow records. Data on evapo-transpiration (consumptive use) and changes in ground-water storage would add interest to the Paper if available.

151. The Author had developed an ingenious formula for calculating evaporation and had listed evaporation from several sites throughout the world in Table 4, which

were valuable additions to the limited data available on the subject. Mr Blaney had developed the following formula for computing monthly evaporation: ^{35, 36}

$$e = \frac{ktp}{100} = kf = \text{monthly evaporation in inches}$$

where k denoted monthly coefficient, t the mean monthly temperature in degrees Fahrenheit, p the monthly percentage of day-time hours during the year, and f the monthly use factor. The values of k could be calculated from the equation $k = \frac{e}{f}$ for areas where all the measurements were available. Coefficients so developed could be transposed to areas having similar climate for computing evaporation where only the mean monthly temperature and per cent of day-time hours were known. The latter could be calculated from sunrise and sunset tables at the latitude of the site.³⁷

Mr G. W. Latham (Senior Executive Engineer, Water Development Department, Nyasaland) referred to the statement in §46 that in April 1956 the flood in the River Ruo had reached a peak of 110,000 cusecs. This estimated value was, in Mr Latham's opinion, too low. The results of calculations made by the Water Development Department immediately after the flood, using slope/area method of computation based on very good site evidence of flood marks since gauges were overtopped, gave a value of 190,000 cusecs to this flood. These calculations employed a value of 0.030 for Cutters n in the Manning formula for the main part of the section, and 0.045 for flow over the berms.

153. This higher estimation had been arrived at hastily, but since the occurrence an analysis of measured discharges both before and afterwards, had been made to determine n -values and results had indicated that at the admittedly much higher stage of the flood (well beyond the point where stage/discharge curve extension could be applied) the value of n would tend toward the figure chosen for the main channel and no sensible reason had been found for discrediting the original estimate.

154. What method of calculation had the Author's firm used to arrive at the lower figure—presumably from the same limited data?

155. Referring to Fig. 1, Plate 1, Mr Latham would have expected to have seen the configuration of the lake more reflected in the rainfall pattern since it was popularly supposed that rainfall over the lake was markedly less than that over the neighbouring land. Was there any information of actual measurement of rainfall over similar large bodies of water in comparison to surrounding land values?

Mr W. H. R. Nimmo (Civil and Hydraulics Engineer, Brisbane, Australia) observed that lakes Victoria and Nyasa were large bodies of water in similar climatic regions and each occupied about one-quarter of its catchment area. They were both in approximate equilibrium with their topographic and meteorologic conditions and discharged a very small proportion of the rainfall. As the Author had shown, the apparently anomalous outflow from Lake Nyasa was due to the changes in the outlet channel.

157. Mr Nimmo had studied the variation in level of some lakes and the quantities of water discharged annually by some Australian rivers, and agreed with the Author that no marked relation between such phenomena and sunspots could be found to continue for a long period. There was, however, evidence of a rather irregular fluctuation in stream flow having an average period corresponding to that of sunspots and this was of importance in relation to the operating cycle of reservoirs.

158. Lake Victoria had a nearly constant area and, until the recent construction of the dam, a fixed outlet, and therefore variation of its level corresponded closely to the variation in the volume of free water. A plot of the level of the lake at December from 1898 to 1922 almost exactly followed that of the smoothed annual sunspot numbers, but thereafter it became irregular and showed two peaks to each peak of the sunspot curve up till 1955. Lake Albert exhibited a similar but somewhat more variable pattern. Such a pattern was not unusual, especially in higher latitudes. The Great Lake in Tasmania, situated about latitude 42°S. and at an elevation of 3,160 ft had a

catchment area of 153 sq. miles and a water area of 60 sq. miles. During the period 1917 to 1949—for which data were available to Mr Nimmo—the graph of free water from the lake showed not only the four peaks which occurred in the sunspot curve, but also intermediate, usually minor, peaks, at about the sunspot minima. Rainfall at Miena at the southern end of the Great Lake showed a generally similar pattern as far back as 1890. The occurrence of these intermediate peaks perhaps accounted for the Author finding better correspondence of the free-water curve with the rate of change of sunspot numbers than with the sunspot curve.

159. The Author's method of calculating free-water quantities from the rate of change of sunspot numbers was novel and interesting but a plot of the free-water levels, given in Table 2, bore less similarity to the plots of levels of Lake Victoria and Albert than might be expected.

160. For many catchments topography imposed a definite pattern of rainfall and, if that was the case for the Lake Nyasa area, it was suggested that there might be a sufficiently definite relation between the mean of the annual rainfalls at Zomba, Chinteché, and Karonga, and the average rainfall for the whole catchment derived from the isohyetal maps to permit of using the records of the three long-period stations to extend the trend lines of Fig. 2 backwards. From a study of records elsewhere, it was thought that the annual yield of the catchment during the period 1900 to 1903 might be less than that of any year subsequent to 1928–29.

Dr H. E. Hurst (Scientific Consultant to the Ministry of Public Works, Egypt) observed that the physical features and hydrology of Lake Nyasa had some resemblance to those of Lake Albert, particularly in the character of its effluent. The Shire and Bahr el Jebel were similar in longitudinal profile and in the possession of bordering swamps which would present similar problems in regard to reclamation works in both cases.

162. Part of the Paper was devoted to attempting to establish a connexion between the levels of lakes Nyasa and Victoria and sunspot numbers. Dr Hurst had discussed the supposed connexion of the levels of Lake Victoria with sunspot numbers³⁸ and had shown that there was no connexion between the rise or fall of level of Lake Victoria or the rainfall on the lake in a year and the mean sunspot number for the year. In the present Paper the question of a connexion was raised in a new form, namely that “free water” (i.e. increase of lake content plus outflow which was equal to supply from rainfall minus evaporation loss) was related to the rate of change of sunspot numbers whether increasing or decreasing. This supposed connexion with sunspots is illustrated in Fig. 7 for lakes Nyasa and Victoria and also for mean temperature in central England. The Figure showed 3-year overlapping sums for the variables. Objections could be raised to the use of moving averages when searching for periodicities or correlations³⁹ as being likely to obscure definite features and even to produce spurious relations. In the case of Lake Nyasa the connexion was not very striking and the short period of observations—28 years, less than three sunspot periods—would not be enough to establish a periodicity of the lake unless the period was very definite. Many supposed periodicities had proved to be transient.^{39, 40} It might be mentioned that taking rates of change of the variables, irrespective of sign, had the effect of producing two maxima and two minima per period of a periodic function, which would tend to increase the effect of accidental variations.

163. Comparing yearly values of the net inflow (free water) into lakes Victoria and Nyasa for 36 years there was a coefficient of correlation of 0.43 ± 0.09 between them, and it was not surprising that two lakes in the same region of the tropics should have some similarity of rainfall and evaporation. But it might be mentioned that a correlation of 0.43 was not of much practical use, since if a phenomenon depended on two causes only, and there was a coefficient of 0.43 with the first, a coefficient of 0.9 was required with the second to account for the rest of the variation.

164. The record of levels of Lake Victoria went back to 1899, and since there was a definite rocky outlet at the Ripon Falls it had been possible to estimate net inflow with

fair accuracy during this time. Outflow from the lake was about one-fifth of rainfall on the lake, and roughly the same as inflow from tributaries, so that rainfall and evaporation on the lake were roughly equal on the average. If there was any relation between the net water supply of Lake Victoria and rate of change of sunspot numbers it ought to show itself when annual values of the two were compared. This had been done taking simultaneous values and also allowing a lag of one year of water supply on sunspots. Two cases had been considered, ignoring the sign of the change, following the Author's procedure, and also taking account of the sign. Some computations had also been made for Lake Nyasa. The results were given below:

	Coefficients of correlation	
	No lag	Lag 1 year
<i>Lake Victoria. 53 years</i>		
Ignoring sign of change	0.11 ± 0.09	0.30 ± 0.08
Considering sign of change	0.30 ± 0.08	0.34 ± 0.08
<i>Lake Nyasa. 36 years (Table 2)</i>		
Ignoring sign of change	-0.01	—
Considering sign of change	—	-0.1

These results did not support the idea that there was any definite relation between rate of change of sunspot numbers and the water supply of the equatorial lakes.

165. The water supply of the Central African lakes depended upon rainfall and evaporation and both of these depended directly on solar radiation, so that they had their well-marked annual periods of fixed lengths. It would be expected that if the less well-defined periods of sunspot activity exercised some influence on rainfall and evaporation the effect would be world-wide. To test this some of the large quantity of meteorological data analysed by Dr Hurst in connexion with over-year storage^{41, 42} had been examined. The material had been in the form of curves of accumulated yearly departures from the mean of all the observations. The general feature which had emerged from these was the existence of terms of years, of irregular lengths, during which on the whole the phenomenon was above or below its long-term average. As an example, Cape Town rainfall might be taken. From 1840 to 1869 rainfall on the whole had been below the average, from then to 1905 above, and from 1905 to 1940 below the average. Aswan discharge, which was a measure of the Ethiopian rainfall, had been above its 80-year average from 1870 to 1898 and below it from then to the present day. The minor perturbations on these general trends seemed to be quite irregular and showed no obvious periodicity. The curves for Cape Town rainfall and Aswan discharge were shown in Figs 14 and 5 of reference 42. Accumulated departures of the following tropical rainfalls had been examined: Aden, Bangalore, Batavia, Calcutta, Cherrapunji, Colombo, Darwin, Fortaleza, Freetown, Lagos, La Serena, Madras, Salisbury, Zanzibar. None of them had shown any resemblance to the curve of Lake Victoria, except Lagos, where a slight resemblance had been noted, nor had there been any appearance of periodicity. The ten accumulated departure curves of reference 42 gave a good general idea of those of more than 100 phenomena which had been examined.

166. Much work had been done in searching for relations between the Nile flood, the Indian monsoon rains, and weather all over the world, and a formula had been devised depending on three correlations whose coefficients averaged about 0.55.⁴³ The formula had been examined by Dr Hurst 20 years later, when the coefficients had been found to have decreased to about 0.4. In any case the results had not been certain enough to be of practical use.

167. It would seem strange if sunspots had influenced rainfall in one small area of the tropics only, without influencing tropical rainfall in general. Dr Hurst's conclusion from the above facts was that any definite influence of sunspots on rainfall had yet to be established, and that the relation suggested in the Paper was probably

accidental. That was a disappointing result but was not a reason why apparent correlations of natural phenomena should not be investigated.

Mr H. E. Whitehouse (Resident Engineer for Freeman, Fox & Partners, Auckland Harbour Bridge, New Zealand), referring to § 16, remarked that when Mr R. C. Wood, a keen archeologist, had contemplated building a house at Monkey Bay during the rising cycle of the lake before 1935, he had been anxious to site it above the maximum level that the lake was likely to reach and yet to be as near to the water as possible.

169. Mr Wood had found in the sand on the beach at Monkey Bay many bushman or bushmanoid artifacts which the experts estimated as being several hundred years old. Up to a certain level on the beach these artifacts were water-worn, above this level the small facets on the surface were clearly defined. This indicated that up to this level the lake had risen one or more times during the past few hundred years, but had not exceeded it. The level was about 5 ft higher than the maximum level reached in 1937.

Per Brask (Engineer, Public Works Department, Nyasaland) observed that in 1956, on the recommendation of the consulting engineers, a bund had been built across the Shire at Liwonde to conserve water because sunspot theory had predicted a series of dry years. The weather in fact had not followed the pattern predicted and after less than a year the bund had been breached. Had this occurrence affected the Author's belief in the relation between sunspots and free water?

171. The following paragraphs had been printed under the heading of "Forecast says dry spell possible" in the *Nyasaland Times* of 3 September, 1957:

"Nyasaland may have a dry spell this year according to deductions made by Sir William Halcrow and Partners, Consulting Engineers on the Shire Valley Project.

A spokesman said today that it was purely a deduction based on known past facts. The behaviour of the Lake over the past few years linked with the known sunspot activity bore some relation to the rainfall of those years. From these facts he said it was possible to forecast what future rainfall might be. As the present cycle now stands, indications point to poor rain in the coming season and we have suggested to Government that a dry spell is possible."

Did the Author believe this kind of weather forecast? The weather in Nyasaland had so far during the present year been wetter, not drier, than usual.

172. The free water in Lake Nyasa had been known with certainty only from 1915 to 1937, when there had been no outflow from the lake and from 1949 to 1957, when there had been flow measurements of the Shire. Did the Author think this short period enough to prove the relation he claims between free water and sunspots?

173. Did the Author think that his evaporation formula would be improved if hours of sunshine were introduced?

174. The upper part of the Shire from the lake to Liwonde could rightly be called the bottleneck, because the fall of the river was only 4 ft in 50 miles. The condition of this stretch of water would decide the outflow from the lake and therefore also the lake level. Did the Author think it possible that the reason for the lake depletion in the time before 1915 could have been a deep and wide channel in the bottleneck and not failing rain?

175. A barrage at Fort Johnston at the outlet of the lake would make it possible to dry out parts of the bottleneck in the dry season. Did the Author think that this would make it easier to explore and maintain this important part of the river? Did he agree that this would offer a chance to find out the reason for the closure of the bottleneck about 1915?

176. A barrage at Fort Johnston would not be founded on good foundations, but would have the following advantages:

a low barrage;
area of gates there smallest possible because there would be no uncontrolled tributaries above the barrage;
no danger of silt deposits; and
flush streams could be arranged to clean out the river.

177. The consultants' suggestion of the Matope Barrage and power station was remarkable because the water from the turbines flowed out above the Matope Falls. Did the Author agree that a more economical solution would be to have an intake without a major barrage above the falls and pipe the water down to a power station below the falls? The water could if necessary be piped down about 40 miles past the four other falls and a total head of 1,200 ft could be created. This, in connexion with the suggested barrage at Fort Johnston, would give considerable power potentiality.

178. It was estimated that only 6% of the total rainfall on the catchment ran out in the Shire; that was about one-third of the river flow to the lake. Could the Author give any reason for the fact, that the water of Lake Nyasa was fresh and not salty, when the nearby Lake Chilwa, which had no outlet, was definitely salty? Could it be that the prevailing south-east wind blew the fresh surface water towards the north and let the more salty bottom water come up in the south to flow out in the Shire?

Professor A. H. Naylor (Professor of Civil Engineering, The Queen's University of Belfast) expressed some diffidence in commenting on the Author's conclusions.

180. The complication of the regime of Lake Nyasa due to the erosion at times of high flood of the bars at the outlets presented a formidable obstacle to the analysis of past records and still more so to the prediction of future behaviour. With meagre data the Author had dealt with this very effectively and the agreement of recorded and calculated lake levels shown in Fig. 6 was impressive.

181. The seiche of 3 in. amplitude was an interesting phenomenon. A seiche of similar amplitude but shorter period had been observed in Loch Treig. In view of the enormous difference in size it was surprising that the amplitude in Lake Nyasa should be so low, but if it had a lunar cause it might well be because of the north-south orientation of the lake.

182. The correspondence of the rate of change of sunspot numbers with mean temperature in central England appeared very striking as depicted in Fig. 7. Nevertheless Professor Naylor could not help feeling that it had no significance and in any case the temperature differences involved were minute. Neither was he impressed by the resemblance of the free-water curves of lakes Nyasa and Victoria with the rate of change of sunspot numbers. It seemed to admit of no logical explanation.

183. The Author considered that it could be shown that solar energy tended to be higher when the rate of change of sunspots was high and *vice versa*. That seemed contrary to the generally accepted view of sunspots as a source of intense solar radiation. It would be logical to expect such radiation to have an influence on terrestrial phenomena with perhaps a small time lag, but it was difficult to conceive of an insubstantial concept like rate of change having direct effect on the earth's climate. If there was any substance in the coincidence it would be easier to accept it as a time lag in the earth after periods of maximum and minimum activity.

184. During the hydro-electric investigation of Uganda in 1935 Professor Naylor had himself felt convinced that the similarity between the sunspots curve and the curve of lake levels of Lake Victoria could be no mere coincidence. Admittedly the later records were not so striking, but having considered the curves of free water on Lake Victoria as brought up to date there did seem to be a certain correspondence with a time lag of 1-3 years of the peaks of the free-water curve. It had been pointed out to him by Dr H. E. Hurst that if there was any relation the maximum rate of change of free water should correspond to the peak of the sunspot activity so that there should be a phase lag of 3 years in free-water levels. However, as had been pointed out by both Hurst and Brunt, exhaustive examination of both climatic and lake records for periodicities had failed. Although there was a certain irregularity in sunspot

phenomena, the long-period cycle must be constant and would have shown up in those analyses. In the light of this Professor Naylor felt bound to accept the Author's findings as fortuitous.

185. The formula for evaporation in § 67 was admittedly empirical but one would have more confidence in it if it had been compounded of logical factors, such as for example those given by Penman.⁴⁴ The Author's formula for evaporation showed E_c proportional to $^3\sqrt{U+3}$ instead of about $U^{0.7}$. It was also given as proportional to $\frac{1}{H_r^{1.13}}$ instead of the dryness factor $(1-H_r)$. Professor Naylor would also expect the index of T to be somewhat higher than 1.2.

186. If the Author's formula could be amended on some such lines so as to fit existing data, it would, Professor Naylor thought, be more acceptable.

Mr F. L. Lawton (Chief Engineer, Power Department, Aluminium Laboratories Ltd, Montreal, Canada) observed that the difficult hydrological problems associated with Lake Nyasa and its outlet, the Shire River, had been rendered more difficult of solution by the complex topography and meteorology, but the Author had clearly recognized that the Shire flow regime was very intimately linked with the level of Lake Nyasa, itself a function of free water available for Shire River discharge and Lake Nyasa storage or depletion, amounting to only about 5½% of the average annual rainfall on the catchment. The significance of the "delicately balanced hydrological phenomena" had been clearly developed in the Paper.

188. The Author had made a very full use of the "fairly long-term records of lake levels, which go back accurately to 1916 but with somewhat less authority to 1896. The lake levels for the period between 1896 and 1915 are nevertheless of definite comparative value, although there may have been an error in transferring the datum of level from one gauge to another". Records of such duration would, in most areas, be considered reasonably long but, as the Author had clearly demonstrated, such was not the case in tropical areas.

189. The Author had utilized his "marked qualitative relation between the rate of change of sunspots and the free water available in the Nyasa-Shire for the 38 years for which he can set out hydrological data" to extend the hydrology of the Nyasa-Shire system back to 1947, with useful results.

190. It was rather surprising that the Author found such a paucity of relations between the behaviour of the Nyasa-Shire system and other great African river systems. It was suggested that the Author would materially enhance the value of the Paper by developing this point, at least to the extent of some quantitative benchmarks, in his reply. What, for instance, was the relation, if any, between the Shire and Zambezi discharges?

191. It was unfortunate that the Author referred, in § 52, to available records from evaporation pans, but then, in §§ 67, 68, and 69, neglected to give any correlation between the probable evaporation from a free-water surface (Lake Nyasa) as based on evaporation-pan records and as established from his formula, which itself appeared to be of considerable practical value.

Ray K. Linsley (Professor of Civil Engineering, Stanford University, Stanford, California) observed that the Author had presented what appeared to be an excellent summary of the physical setting of Lake Nyasa and the River Shire, but his hydrologic evaluations did not seem to be as rigorous as the physical description. Would the Author give a specific explanation of the origin of the free-water figures of Table 2. To establish these he apparently had only the equation

$$\text{Free water} = \text{outflow} \pm \text{change in storage.}$$

For the period 1935 to 1950, outflow was unknown and apparently had been estimated to give a reasonably consistent relation with rainfall. Fig. 5 showed outflows between 1942 and 1948 which were in excess of any measured outflows from the lake. The estimated free water appeared to have then been plotted against rainfall and this relation

used, in turn, to estimate free water and lake levels shown in Fig. 6. If so the two computations were not mutually independent and Fig. 6 was not a true check on the reliability of Figs 2 and 5. It was significant that the worst errors in Fig. 6 occurred prior to 1935 and since 1950 when the outflow data were most reliable.

193. Much of the Paper was devoted to the consideration of the use of sunspots as a basis for estimating hydrologic variables. This subject could be discussed at length, but it was sufficient to say here that the Author had certainly failed to show a significant relation. Fig. 8 was offered as proof of success but was, in fact, no proof at all. The total free water estimated for a long period of years from sunspots (or any other random variable) would approximate to the observed total and the mass curves would terminate at nearly the same level. The significant feature of a mass curve was its slope, and the concurrent slopes of the two shown in Fig. 8 were quite different. A more direct test was to compute the coefficient of correlation between rate of change of sunspot numbers (Fig. 9) and free water (Table 2). That turned out to be about 0.4. Since the coefficient of determination was the square of the coefficient of correlation, it might be said that only about one-sixth (0.4²) of the variation in free water seemed to be related to changes in sunspot number. That was hardly an adequate basis for extrapolating estimates of free water.

194. In § 67 the Author introduced another empirical evaporation formula, despite the existence of several theoretically sound methods of computing lake evaporation. Reference 16 did not really explain how this formula was derived. If the six examples cited in Table 4 were the data on which the formula was based, the close check was not surprising. The formula as written contained eight constants or exponents which had to be established from six equations. That left considerable latitude for curve fitting. The source of the figure for Lake Hefner evaporation (Table 4) is not given. The published report⁴⁵ on this experiment gave data for 16 consecutive months. The totals for the possible 12-month periods ranged from 53.2 to 55.3 in. None were as low as the 52 in. given in Table 4.

195. It was difficult to understand why the Author had felt it necessary to propose a formula which did not remotely resemble the form suggested by sound theory. Many formulae required only wind speed and vapour pressures (easily determined from temperature and relative humidity) and were no more complex in form than that of the Author. Admittedly empirical methods were sometimes necessary in engineering in the absence of good theoretical background. However, to ignore established and well documented scientific facts in favour of purely empirical approach reduced engineering from applied science to a handbook-based craft.

196. Many lakes such as Lake Nyasa having a precarious water balance were found to be saline. It was interesting that Lake Nyasa was apparently not saline and it might be possible that an analysis of the salt balance of the lake would give a clue to the probable mean inflow over a long period of time. There had also been studies reported recently in the United States which suggested that it might be possible to get an indication of the long-period hydrologic balance of a lake or ground-water basin through an analysis of the tritium (a natural isotope of hydrogen) content of the water.

Mr W. G. Owen (Geographer, Uganda Water Development Department) remarked that the equation connecting free water, out-flow, and change in storage, described by the Author in § 53, was clearly fundamental in the argument of that paragraph. The equation was seen to have been used in the period 1915–34 when, with no outflow from the lake, free water was represented simply by change in storage. In view of this it was difficult to understand how the Author produced such a poor relation between calculated and recorded lake levels in Fig. 6 for the period 1928–34, when by reversing his procedure he could return precisely to lake levels.

198. When outflow began, it would be possible with known change in storage to determine free water by estimating outflow, or alternatively to determine outflow by estimating free water. In either case, full use would then be made of the one quantity in the equation which was known.

199. Neither of these courses had apparently been taken. The Author stated that he first produced the rating shown in Fig. 5. The three measurements of discharge between 1935 and 1939, and the measurements from 1950 would to some extent substantiate the ratings in those periods. In the intervening period 1940-49, the Author's rating was drawn in an apparently quite arbitrary manner, and a variation from that rating of as much as 25% was not inconceivable. There were possibly available, in the period, estimates of discharge of some degree of reliability, but if so they were not mentioned.

200. Figures of free water were then calculated from a consideration of rainfall, and the calculated lake levels thus available and shown in Fig. 6 might be considered to compare well with recorded levels when the inherent uncertainties of two of the derivatives of the calculated levels, independently determined, were appreciated.

201. The calculation of free water from rainfall, the explanation of which was unfortunately not given in detail in the Paper, must necessarily have included a consideration of losses from rainfall, and in this respect it was to be noted that the figures of rainfall and calculated free water in Table 2 related to different "years". These varying years would be compatible only if the effect of rainfall on the lake made itself felt with a 6-month lag. There was no indication of storage in the form of snow, or of delayed ground-water flow. With regard to evaporation, it would be seen from § 69 to vary from year to year. In the 5 driest months, June to October, it varied between +25% to -20% from the mean of the period 1929-53. It would therefore appear possible that the imperfect relation shown in Fig. 2a, between rainfall and free water, was due in part to the variation in the years to which they were related.

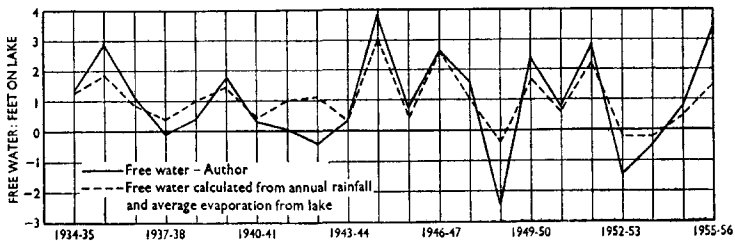


FIG. 12

202. An alternative derivation of annual free water would be as followed. An examination of Fig. 1 had shown average annual rainfall on the lake area to be 12% higher, and on the land area 4% lower than the average for the whole lake catchment. Through the period 1928-34, during which the Author's calculation of free water was understood, the balance of free water available was +91.8 in. The total rainfall of the period gave 337.7 in. on the lake and 289.4 in. on the land. Accepting from § 68 the figure of 61 in/year evaporation loss, which was by calculation an average estimate, losses from the lake would have been 366 in., giving a period deficit of -28.3 in. The sum of period gain (free water) and deficit is 120.1 in., which represented the land area contribution and was 12.7% of land area rainfall. Applying the same lake evaporation loss and 12.7% run-off from the land area to the total rainfall 1935-56, an overall period free water gain of 22.30 ft was obtained, which agreed exactly with the Author's figure (Table 6). Application of the procedure to individual years showed a general qualitative relation with the Author's figures (see Fig. 12). A quantitative check could be made only from a knowledge of actual evaporation and run-off each year.

Mr I. D. Pillar (Postgraduate Student of Hydrology, Imperial College) found the

Author's statements in § 5 difficult to accept. First, it was puzzling that he should suggest that he found no usable data concerning climatic changes in the Southern Hemisphere. Second, to say that changes of climate, over the past 10,000 years had been relatively limited, gave a false impression, especially since he admitted that even a small change could have a marked influence upon vegetation and upon the hydrological balance.

204. Any standard reference on climatic change would have supplied information as to the work done in the Southern Hemisphere. For example, in Chapter 22 of Shapley's book⁴⁶ Deeley summarizes most of the work that has to be done and makes reference to the original Papers. Admittedly, the data⁴⁷ are few compared with the wealth of material for the Northern Hemisphere, yet these are sufficient to indicate a parallel sequence of events, with a "climatic optimum" *circa* 5000 B.C., and an oscillating recession of temperature⁴⁸ since that time. It was surprising that the Author made no reference to the work that had been done in East Africa by Leakey,^{49, 50, 51}

TABLE 6.—DERIVATION OF FREE WATER FROM ANNUAL RAINFALL AND MEAN ANNUAL LAKE EVAPORATION

(1) Year	(2) Rainfall	(3) Rainfall on lake	(4) Rainfall on land	(5) 12.7% rainfall on land applied to lake area	(6) Mean evaporation from lake area	(7) 3-6	(8) Free water (5)+(7)		(9) Author's free water
							Inches	Feet	
1934-56	1059.5	1187	1017	422.7	1342	-155	+267.7	22.30	22.30
1934-35	49.8	55.8	47.8	19.9	61	-5.2	+14.7	+1.20	+1.30
1935-36	55.1	61.7	52.9	22.0	61	+0.7	+22.7	+1.90	+2.90
1936-37	46.9	52.5	45.0	18.7	61	-8.5	+10.2	+0.85	+1.10
1937-38	43.1	48.3	41.4	17.2	61	-12.7	+4.5	+0.35	-0.10
1938-39	48.4	54.2	46.5	19.3	61	-6.8	+12.5	+1.05	+0.40
1939-40	51.4	57.6	49.3	20.5	61	-3.4	+17.1	+1.40	+1.80
1940-41	43.7	48.9	42.0	17.5	61	-12.1	+5.4	+0.45	+0.30
1941-42	48.1	53.9	46.2	19.2	61	-7.1	+12.1	+1.00	+0.05
1942-43	48.6	54.4	46.7	19.4	61	-6.6	+12.8	+1.05	-0.40
1943-44	42.3	47.4	40.6	16.9	61	-13.6	+3.3	+0.25	+0.35
1944-45	64.6	72.4	62.0	25.8	61	+11.4	+37.2	+3.10	+3.85
1945-46	43.1	48.3	41.4	17.2	61	-12.7	+4.5	+0.35	+0.75
1946-47	61.1	68.4	58.7	24.4	61	+7.4	+31.8	+2.65	+2.60
1947-48	48.0	53.8	46.1	19.2	61	-7.2	+12.0	+1.00	+1.60
1948-49	37.2	41.7	35.7	14.8	61	-19.3	-4.5	-0.35	-2.50
1949-50	53.7	60.1	51.6	21.4	61	-0.9	+20.5	+1.70	+2.40
1950-51	44.9	50.3	43.1	17.9	61	-10.7	+7.2	+0.60	+0.80
1951-52	55.7	62.4	53.5	25.9	61	+1.4	+27.3	+2.25	+2.80
1952-53	39.2	43.9	37.6	15.6	61	-17.1	-1.5	-0.10	-1.45
1953-54	38.6	43.2	37.1	15.4	61	-17.8	-2.4	-0.20	-0.50
1954-55	43.9	49.2	42.1	17.5	61	-11.8	+5.7	+0.45	+0.80
1955-56	52.1	58.4	50.0	20.8	61	-2.6	+18.2	+1.50	+3.45

Nilsson,⁵² and others.⁵³ From data obtained in the Olduvai Gorge, it was seen to have extended Lake Naivasha and, by archeological dating, a sequence of pluvial periods and dry periods had been established for an area no more than 750 miles from Lake Nyasa. This pattern had been related to the sequence in both north and south Africa and in Europe.

205. Strictly to consider the last 10,000 years would be to include the oscillation from the "Highland Readvance" (see Fig. 1 in reference 3) to the "Climatic Optimum" and that could scarcely be considered "limited". Admittedly, the first- and second-order fluctuations since the climatic optimum had been small compared with changes during the glacial periods, but even so they had been of the order of 1 to 2°F temperature. The critical point was defined by whether such variations would be adequate to cause changes in the vegetation and in the hydrological balance. Most authorities accepted that only slight changes in the energy balance could lead to marked changes in the pattern of the atmospheric circulation and, in particular, in precipitation.⁵⁴ Furthermore, work done upon vegetational changes in Africa since the glacial period suggested that, in critical areas, such as desert margins, quite marked changes had occurred. For example, in Nigeria fossil ergs may be traced from aerial photographs up to 350 miles south of the present desert. Other archeological, biological, and geomorphological data⁵⁵ could be co-ordinated to elaborate the pattern of climatic and vegetational changes in tropical Africa.

206. Clearly, the conclusion to be drawn from this was that any study of the critical hydrological balance in the Nyasa area must be seen against this background of fluctuations of climate. Any attempts to determine the pattern of present first-order fluctuations should then be integrated into this general background. The references quoted should be sufficient to demonstrate that the subject of climatic change was deserving of more attention than the brief dismissal given in the Paper.

Mr M. F. Clarke (Postgraduate student of Hydrology at Imperial College) observed that the Author postulated a quantitative relation between rate of change of sunspots and certain hydrological changes. The correlation indicated in Fig. 8 was particularly interesting and suggested an inquiry by statistical analysis into the correlation of the data used to build up the mass curves.

208. Annual quantities of free water were plotted in Fig. 13, the true* free-water figures having been taken from Table 2 and the estimated ones as accurately as possible from Fig. 8. The distribution of the points shown had a correlation coefficient of $r=0.48$. This was not a high degree of correlation, and it might be asked how significant this value of r was. Accordingly the regression curves (assumed rectilinear) and their standard errors of estimate (S_x , S_y) had been evaluated, and the regression curves and the lines representing S_y and $2S_y$ plotted on Fig. 13.

209. Obviously, under ideal circumstances $r=1$ and the regression curves coincided, passing through the point (O, O) with a gradient of unity. That was, in fact, not the case; ideal circumstances were not even approximated. The S_y and $2S_y$ curves assisted the understanding of the correlation. Since the correlation would be a measure of the success of predicting free-water values, the regression curve of Y on X was used in the example. The limits $2S_y$ bounded the range of true free-water values, referred to the X -axis, which would be likely to cover 95% of all cases of estimated free water. For example, an estimated value of 2 ft of free water would, in 19 cases out of 20, fall within the range of ± 2.2 ft about a mean value of approximately 2 ft of true free water. In a similar manner it could be shown that the limits S_y gave a true free-water value to ± 1.1 ft of the mean for 68% of all cases.

210. In the circumstances described, $r=0.48$ was associated with so large an error of estimate that quantitative correlation was unwarranted. Fig. 8 should, therefore, be held to represent a possible, but not a certain, qualitative correlation between the rate of change of sunspots and the free water on Lake Nyasa—hardly the "perceptible quantitative relation" suggested by the Author in § 60.

211. Mr Clarke thought that Fig. 8 was misleading in that had the rate of change of sunspots been quite different from what it was with respect to amplitude or cyclical

* The "true" mass curve, it should be noted, was in itself partly hypothetical, but Mr Clarke accepted it as a good approximation.

pattern, an estimated free-water mass curve could be drawn similar to the true mass curve and purporting to show a close fit. Either a graphical representation like Fig. 13, or a plot of the two free-water curves, without summation, would have illustrated their relation far more significantly.

212. Statistical correlation had also been applied to Fig. 2a (from which, incidentally, three pairs of figures were missing, those for 1953-56; a fourth pair would lie outside the range covered by the graph), using the data in Table 2. The correlation coefficient was $r=0.86$, associated with regression curves—assumed to be linear, although there was no absolute justification for the assumption—and standard errors of estimate S_x , $2S_x$, as shown in Fig. 14. There, the relatively high value of r in conjunction with regression curves of similar gradients did indicate a significant relationship between free water and rainfall (as one would expect). However, S_x and $2S_x$ on the curve of X on Y (to be used if free water was to be estimated from rainfall), were quite considerable. 95% of all cases estimated would have errors of approximately ± 1.5 ft or less, and 68% of ± 0.75 ft or less.

213. Even at S_x the possible error was nearly twice the mean annual free water, so that a graph such as Fig. 2a would be unsatisfactory for prediction. The magnitude of S_x indicated that there was another significant factor (presumably temperature, or—more directly—evaporation) affecting free-water accumulation. Fig. 2a should, therefore, be taken to show a good qualitative, but only a fair quantitative relation between free water and rainfall. To improve the picture, recourse would have to be had to multiple correlation.

Mr Mohamed Yousaf (Postgraduate student of Engineering Hydrology at Imperial College) referred to the part of Fig. 2 which related annual rainfall to the annual free water on Lake Nyasa, presumably taken from Table 2. The author had explained that from 1915 to 1935 the outflow from the lake had been negligible and the known fluctuations in lake levels were, therefore, equal to the fluctuations in free water on the lake. From 1935 to 1950 the outflow from the lake had been measured only at three known dates. Could the Author explain in detail how he had calculated the free water for the remaining years between 1935 and 1950?

215. According to the information given in the Paper, only 17 of the 41 values of free water in the last column of Table 2 were the result of measurement, and if these 17 values were identified on Fig. 2 it was found that these points did not lie any closer to the straight line (which, presumably, had been drawn by eye) than the remainder. If the dates were entered against the points on Fig. 2, it was found that successive groups would lie above or below the straight line. This suggested either that there might have been systematic errors in the data given to the Author, or that some factor which was of importance in the hydrological analysis and which varied only slowly had been omitted.

216. Starting from the beginning, and denoting the free water on the lake by F , the outflow from the lake by O , the increase in storage, on the lake by $(S_2 - S_1)_L$, the precipitation on the lake by P_L , that on the catchment by P_C , the evaporation from the lake by E_L , the evaporation and other losses from the catchment by E_C , and the increase in storage on the catchment by $(S_2 - S_1)_C$, the following relation was obtained:

$$F = O + (S_2 - S_1)_L = P_L + P_C - E_L - E_C - (S_2 - S_1)_C$$

Even if the last term was negligible, the free water on the lake, F , was seen to be a function of four variables each of which, in turn, was dependent on a number of factors. Even if he had no quantitative information on this large number of factors influencing the correlation represented by the straight line in Fig. 2, could the Author not have improved it by some qualitative estimates?

Mr N. K. Bala Sundar (Postgraduate student of Hydrology at Imperial College) observed that as a geologist he was very interested in ground-water, and he asked whether ground-water flow into and out of the lake, and underground storage in the catchment area of the lake, might not be important in the hydrological balance sheet

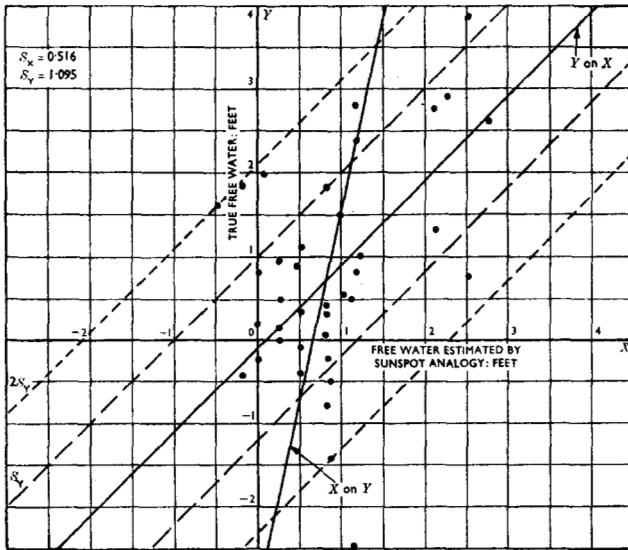


FIG. 13.—CORRELATION OF TRUE ANNUAL FREE WATER AND ANNUAL FREE WATER ESTIMATED BY SUNSPOT ANALOGY

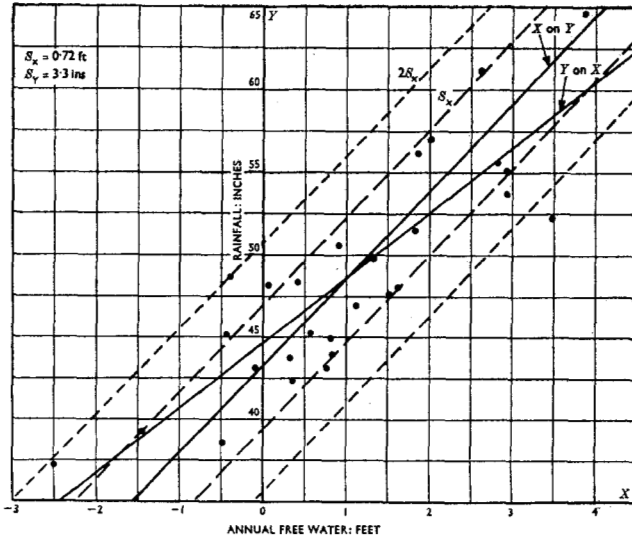


FIG. 14.—CORRELATION OF ANNUAL RAINFALL AND ANNUAL FREE WATER

for the area. It appeared that the valley of the River Shire just south of Lake Nyasa was covered in alluvial deposits. He had assumed that the outflow from the lake into these deposits would increase with a rising lake level and had attempted to estimate the discharges involved, but lack of geological data had made it impossible for him to arrive at figures in which he would have had confidence. If information on depths and extent of strata, and on their porosities and permeabilities, was available, he hoped the Author would give it. If not, he thought that field tests would yield information of direct help in the numerical analysis of the water resources of the area.

218. In § 14 there were some figures of water depths in the lake, and Mr Bala Sundar asked if depth contours (and consequently volumes) were available. If volumes were combined with records of water temperatures and with continuous records of chemical composition, the results would assist in providing a check of any estimates of evaporation loss and of total inflows and outflows.

Mr P. O. Wolf (Reader in Hydrology, Department of Civil Engineering, Imperial College) observed that the Author had been faced with the task which so frequently confronted hydrologists of explaining the flows and levels in a lake and a river system on the basis of records which were certainly inadequate in number and probably deficient in quality. The uncertainties in every equation derived must, therefore, be great. The accuracy of the forecast described by the Author when introducing the Paper was so good that Mr Wolf asked him to explain the successive steps of the method which had enabled him to eliminate the various errors with such success.

220. The Author had made two further general points to which engineers might profitably give more thought. The first was his dislike of "the variable constant" or coefficient. No engineer or scientist would dispute the advantages of formulae in which every term was numerically constant, but the use of coefficients whose numerical values varied with the circumstances was often convenient and scientifically respectable. To give an example of convenience, the Manning formula for the velocity of flow included a roughness coefficient which was well known to vary with the material forming the bed or walls containing the flow, yet the formula was used in preference to the Colebrook-White formula in which there was no need for estimates of the values of coefficients nor for a profound knowledge of the effect of Reynolds Number variations on the values of Manning's (or Kutter's) coefficient. The case for convenience—with which the Author would no doubt largely agree—had been more fully stated elsewhere.⁵⁶

221. With regard to the scientific aspect, the Author would surely recognize the great value of such variable coefficients as the Reynolds, Froude, or Mach Numbers. A fuller understanding of the natural processes affecting the work of the civil engineer was often brought about by the development of formulae from first principles, by way of physical or dimensional reasoning. Allowance for the individual circumstances of each case might then be made by means of coefficients the variations in which could be related to the causes of variation by experiment or by observation in nature. The Author's evaporation formula mentioned in § 67 might well prove as useful to the hydrologist as the Manning formula proved to the hydraulic engineer, but the errors in the results and the limits of validity of such convenient formulae could be determined with confidence only by comparison with equations which allowed for every part of a physical process (and were dimensionally consistent) and the search for which should, therefore, receive encouragement rather than ridicule.

222. The Author's second general point was that he mistrusted statistical theory and preferred his own, mostly graphical, methods. Mr Wolf thought that there was no fundamental difference between graphical and numerical methods in statistics and that a combination of both techniques often led to the greatest economy in effort. The advantage of the graphical presentation, as was apparent in the Paper, was that trends could readily be recognized. Arithmetical analysis, on the other hand, led to numerical definitions of significance which could not be deduced from such diagrams as Figs 7 or 9, and certainly not from the mass curve of Fig. 8 which appeared to have given the Author more confidence than the data in the Paper justified. A good

knowledge of statistical theory would enable the hydrologist, when faced with inadequate information, to recognize trends (which included the recognition of distributions other than "normal" or "log-normal" and of the disturbances in sequence which were common in hydrology) and to make a rational estimate of the value of his conclusions. The further advantage of independence of individual judgement which, for example, entered into curve-fitting by eye was important in cases such as the present one, where free water represented a marginal quantity.

The Author in reply said that Mr Morgan was aware that as late as 1950 there were less than about 5 miles of tarmac road in Nyasaland, and the present fairly satisfactory position with regard to the Shire had been largely due to the efforts of a large survey party under Mr E. V. Richards and to those of the Nyasaland Water Development Department. Dr Kanthack had done earlier valuable work also.

224. The earth's atmosphere prevented observation of the sun's behaviour in anything but a rather primitive manner. With all their scientific crudity sunspots were the only available long-term continuous solar data and one of the few varieties that could be observed unquestionably to change in cycles. The Author was not inclined to speculate too widely on what sunspots were and he had judged it prudent to examine climatic phenomena in temperate latitudes also, where the substantial seasonal changes might be feared to mask long-term secular relations like those observable in the low latitudes.

225. With this in mind he had, when introducing the Paper, shown slides illustrating the correlation between the rate of change of sunspots and the mean annual temperature at Berlin, Severn River flow, and cloudiness at Oxford. Together with the three in the Paper, these six correlations, observable over many decades, were widely representative with regard to area and analogous data. His approach to natural data was that while they could sometimes be rendered apparently less intractable by statistical appreciations in which random behaviour was the fundamental postulate, that did not prove that the phenomena were random. It merely meant they were intractable, and the natural mechanism obscure or unknown.

226. From the record, the free-water gain in Lake Nyasa had been anything from 15 to 78 in. in the wet season. The residual yearly free water had been anything from -30 to +46 in. A prediction based on the Author's hypothesis had been given in writing to the Nyasaland Government in January 1957 to the effect that the 1957 wet-season free-water increase should be about $56\frac{1}{2}$ in.; it had in fact been 60 in. The residual free water for that year should have been about $27\frac{1}{2}$ in.; it had in fact been 28 in. Although that was accurate the Author knew the weaknesses of the method and did not want to claim more than a qualitative success.

227. The Author's evaporation formula was based on an exhaustive analysis of evaporation-pan data from seventy-seven sites, distributed as widely over the world as possible, plus four of the water budgets quoted in the Paper. The results of three more budgets at Salton Sea, Kempton Park, and Lake Nyasa had later been procured at intervals and the Author's deductions had been borne out in those situations also.

228. Sir Thomas Foy, in supporting the Author's attempt to extend the climatic record for the Shire, had adopted the realistic view that the higher value for the free water could be taken because of the enormous reserve of water in the lake, which could if necessary be pumped out. The Author hoped that some day enterprise matching the exceptional potentialities for development of both lake and valley might come forward.

229. Mr Robertson and the Author both had a great interest in ways of controlling natural phenomena, such as evaporation and rainfall. They had had disappointments but hoped that there were exciting things to come. In particular, Australian research had shown a remarkable technical vitality since the 1939-45 war.

230. Dr Dixey had been Director of Geological Services in Nyasaland and had worked with extraordinary virtuosity and perception. He had probably unequalled knowledge of the great Central African lakes.

231. The Author thought that Fig. 6 showed the considerable downward trend of

the troughs of lake level since 1937 which had been interrupted by recurrent short-term upheavals, which he had attempted to explain in Fig. 7. The latest sunspot peak, in 1957, had been phenomenally high, but since 1937 the lake level had several times been higher than in 1957.

232. The Author and his colleagues had not been able to convey the distinction between the long-term trend and the short-term upheavals, and so had been instructed to breach the bund during one of the short-term upheavals. The Author had predicted accurately the exceptional lake behaviour of 1957 and had indicated that drier conditions should follow; in fact, the 1958 rains had started slowly and by 8 March, 1958, famine stocks were being distributed in the Southern Province. There had, however, been erratic and heavy rain as well.

233. The timing of the construction of a lake-control barrage depended fundamentally on the state of the lake. It would have been difficult to persuade a layman in 1948, when the lake level was very high, that the level was to become uncomfortably low within only 5 years and yet high again in a further 4 years.

234. The Papers of the 1955 Prehistory Conference had not been available when the Author had written the Paper under discussion. He would, however, have needed something at least as good as the peat, pollen, radio-carbon, and prehistory correlations which were being agreed in Europe. Information of a more general nature was not of much immediate value to him.

235. The Author agreed with Mr Gosschalk that the natural control already exercised by the lake on the complex was remarkably good and it would be difficult to improve on it. He did not know when the Zambezi had captured the Shire but it was an interesting example of the main river being steeper than its tributary. The lower Shire marshes could not fail to be lessened in extent if the Zambezi floods were substantially controlled.

236. Mr Allan could be assured that hydrological data were collected in Nyasaland by an effective department set up for that purpose. The Author did not see any anomaly between an evaporation of 61 in/year from Lake Nyasa and 49 in/year from Lake Victoria. On the basis of rather inadequate meteorological data he had deduced a figure in the low 50s for Victoria by his own formula.

237. He had some time ago been privileged to see the computer which Mr Allan and his colleagues were using for behaviour trials of the development of the Nile Basin, a far-sighted analysis and a very formidable problem. He assumed that the operation of the Nile controls could be a little more effective if some sort of prediction of events was possible. The Author thought the trends of behaviour of Lake Victoria were to some extent predictable.

238. A relatively insignificant increase in area would follow a raising of Lake Nyasa but it was shared by Tanganyika, Nyasaland, and Portuguese East Africa, and there were several small ports.

239. Mr Lapworth had provided the Author with the Kempton Park evaporation data. He and his committee of water engineers were doing invaluable field work on this problem.

240. The Author had been relieved to observe that most of the terms in Mr Lapworth's heat-balance formula had proved insignificant. He suspected, however, that the effect of wind on the whole mechanism was often much more important than Mr Lapworth inferred. The Author had been rather soured about such approaches by the gross anomalies presently existing between reality as measured at Kempton Park or Staines and the published results of an authoritative radiation-balance assessment of evaporation in Great Britain.⁵⁷ He wished Mr Lapworth good fortune in his own researches and his conclusions were eagerly awaited.

241. The Author could not accept some of the opinions quoted by Mr Wilmot from other literature. He had not worked in terms of lake levels, since these were merely incidental and it would be seen that, if the Victoria lake-level correspondence ceased in 1923, the Victoria free-water correlation was very striking indeed after this date (Fig. 7).

The Author suggested that there was a logical dilemma when using a statistical technique devised to deal with events and not with averages. It might be that a re-examination of some of the data discarded by others might prove fruitful if different increments of time or different techniques were used. (See §§ 252 to 254.)

242. The Author did not agree that the Thames and Nile should necessarily have the same, and not opposite, long-term trends. He had made no generalizations of that kind because he was acutely aware of the great complexity of the climatic balance. He had merely stated that he had observed several natural phenomena to act in a non-random cyclic manner and he would expect to have to treat each on its own merits. (See §§ 225 to 260.) It was perhaps not prudent to presume the effect from the cause too specifically.

243. With regard to sunspots as such, the Author referred Mr Wilmot to §§ 224, 225. He had done the best he could with these peculiarly intractable phenomena of somewhat variable periodicity, from 7 to 17 years at peaks. They could be seen and measured; speculations could be made about them, but little was really known about what they were or represented. If their turbulence or rate of change was not some crude indication of changes in radiated solar energy then of course the Author's hypothesis was obscured. The Author pointed out two matters he was not certain Mr Wilmot had not overlooked; he did not mean "inflow" by free water and there were rate-of-change minima at both sunspot minima and maxima.

244. Mr Blaney, who made real measurements while others speculated, had supplied the Author with much data, including those on Salton Sea and any contribution on evaporation from him was an honour.

245. The Author had not included an allowance for marsh plants because, except at the southern outlet, Lake Malombe, there were few marshes around the lake which were fed by it and were below the gauging points on the streams flowing into the lake. Clearly there must be an adjustment eventually but its magnitude should be insignificant. The boundaries of the lake were nearly all at least shelving and often precipitous. The great Shire marshes were below the lake and were the subject of much thought.

246. Mr Latham could be assured that the Author did not feel dogmatic about the Ruo flood of April 1956. He and his colleagues had made three approaches and none was really adequate. There were the flood marks on the Ruo, rainfall data, and floods on the tributaries to the Ruo. He had always found it difficult to deal with non-parallel flow in a varying river section in extremes and he had not arrived at quite so high a value as Mr Latham. The other two methods, which were probably just as substantial, indicated much lower values. He felt that the reality would be nearer to 110,000 than to 190,000 cusecs.

247. Mr Latham's point about rainfall over the lake was very germane because there was a great gap in the Author's knowledge in this respect. The Author had neither heard of nor seen any sudden change in the fall of rain from a cloud advancing from the land to Lake Nyasa or *vice versa*. He had therefore continued the isohyets in what he thought was a reasonable pattern in such a continental climate. He thought the problem would be much more difficult in, say, Great Britain, where there were marked changes to be observed at times over the sea and over land. The Author had been aware of this and other problems and had specifically described the yearly rainfall maps as all being prepared by the same hand. They should perhaps be considered merely as "Cochrane's maps" and probably another interpreter would arrive at slightly different results.

248. The Author reminded Mr Nimmo that he was not trying to explain why his hypothesis about rate of change of sunspots differed from the common one about sunspots; he was postulating that sunspots or their absence were not significant in this context, but that their rate of change or turbulence appeared to be. He suggested that it was not possible to say if a sun with a nominal 150 spots on it produced any more or any less total energy than one with no spots at all. It appeared desirable to divorce the mind from any conception of magnitude by association with the actual numbers.

249. The Author had tried to use the long-term rainfall data mentioned to extend

the record without very satisfactory results, perhaps because there was such a striking range of levels and climates in the catchment.

250. Dr Hurst's use of the word "showed" aroused the Author to make a minor protest. Presumably it was intended to convey only that he had reached certain conclusions, for Dr Dixey and others had reached different conclusions about the same thing, the levels of Lake Victoria.

251. The Author also pointed out that the period of observation for Lake Nyasa on Fig. 7 was 38, not 28 years.

252. He had not taken the warnings of others about moving averages as more than warnings. Their benefits were considerable and potential dangers both recognizable and avoidable. Averages were not events and it must also be recollected that the time increment in all natural averages was arbitrary and convenient only. Many natural data which led to useful conclusions on, say, a yearly basis were quite intractable on a daily or hourly basis, yielding no statistically significant correlations. The Author knew of no *a priori* reason for using the year as the increment of time and he had in fact used 3-yearly means.

253. It would be accepted by Dr Hurst that, since his judgement in this matter relied on a mathematical conclusion, his conclusions could be only as valid as his method, for which he appeared to have ignored sequence and had arbitrarily chosen the year as the increment of time. If a mathematical correlation was to be used to reach a general conclusion it ought to be at least consistent numerically and it was of interest to look again at some of Dr Hurst's figures and compare them with the coefficients of correlation using different time increments, e.g. 3-year groups, "triades".

TABLE 7.—FREE WATER, LAKE NYASA, AND RATE OF CHANGE OF SUNSPOTS, IGNORING SIGN

Time increment	Coefficient of correlation	Source
1 year, 1916 <i>et seq.</i>	-0.01	Hurst
" " "	0.4	Linsley
Triades, 1918, 1921 <i>et seq.</i>	0.36	Author
" 1919, 1922 "	0.57	Author
" 1920, 1923 "	0.58	Author

254. Different arbitrary time increments might be chosen and it would be found that the coefficient of correlation varied and so, logically, must the statisticians' conclusions. Table 7 did not, however, prove that either the Author or Dr Hurst was right or wrong; it merely illustrated that the method had a limited range of application and had been misapplied here since the data were averages in sequence and not individual events.

255. The Author suggested that Dr Hurst's statistical methods were logically inapplicable in this context and that his correlation coefficients could not in any event serve as conclusions since they had no absolute values. His attention was drawn to § 272.

256. With regard to the occurrence of "rate of change" maxima and minima, the 1917 sunspot peak was of great significance since it had been so sharp that the rate of change peak had been coincident with it. There had been only one peak in the Lake Victoria free-water curve also at that time and not the two which Dr Hurst postulated.

257. Dr Hurst had mentioned that many natural periodicities had proved transient and the Author agreed but suggested that the fact they had happened at all should be a stimulus to further research. He felt that the modern tendency was to invent rare manifestations of apparent fortuitous or random behaviour and to use the word transient to explain away what was simply not yet understood.

258. The Author did not see any fundamental reason why these solar-terrestrial climatic relations should necessarily be numerically continuous *ad infinitum*. All climatic phenomena were interrelated in a very complex manner and it was not possible to say exactly what the effect of, say, a slow progressive increase in solar energy would be at any one place. The temperature might first rise, then evaporation and cloudiness increase, leading to an increase in rainfall and a fall in temperature. Numerically this would appear to be a transient correlation whereas it would in fact be a very progressive one.

259. It appeared to the Author that it was strictly only necessary for a sequential correlation to be observed for some phenomena at some places in the world at any one time for his hypothesis to be proved. It appeared to be scientifically irrelevant if the sequential relation ceased to be clear cut, or reversed, or completely obscured at any one place, so long as it was apparent at others.

260. The Author suggested therefore that, since appreciation of the precise mechanism of climate was so indeterminate, he might logically postulate a climatic relation with the turbulence or rate of change of sunspots, so long as he could observe any climatic elements behaving in this sequence in the world and irrespective of whether any particular element started to correlate, conformed well, changed its mode of correlated behaviour, or ceased to evince any observable correlation at all. For the purpose of this Paper and introduction, data of seven phenomena of a diverse nature, widely representative, and widely distributed had been adduced. They could be clearly seen to vary in an irregular fashion and all at about the same time over many decades. They were:—

- (1) Rate of change of sunspots.
- (2) Temperature of Central England.
- (3) Free water Lake Nyasa/Shire complex.
- (4) Free water Lake Victoria/Nile complex.
- (5) River Severn flow.
- (6) Temperature of Berlin.
- (7) Cloudiness at Oxford.

261. He was unable to comment specifically on Dr Hurst's own researches in this field but suggested that while one correlation continuing in sequence over 1 or 2 years might possibly be accidental, one correlation continuing over several years was unlikely to be accidental, and six continuing in sequence over many decades was an unacceptable attribution to accidents of fate.

262. The Author was delighted to hear of the foresight of Mr Whitehouse's friend at Monkey Bay. He himself had not been able to track down anything so interesting as the water-worn artifacts. He had believed for a long time that the 1937 level could not be the highest in recent centuries. Unfortunately the Arab slavers who sailed the lake before Livingstone's day had not left any records of lake level so far as he could ascertain and the 1937 level, being the highest on record, had attained almost mystical significance to the layman.

263. From Mr Brask's contribution it was clear that it was not only to laymen that it had been impossible to convey the distinction between long-term trends and short-term cycles. In Fig. 6 the generally downward trend of the troughs could be seen to be interrupted by the short-term upheavals shown in Fig. 7. It was of course conceivable that history would not repeat itself.

264. Mr Brask seemed unaware that in January 1957 the Author had made a formal prediction to the Nyasaland Administration as described in § 226 and, in the event, it had been reasonably accurate. He had not made the prediction quoted against him from the Nyasaland Times nor had he ever made any prediction about rainfall there. He had, however, observed that he expected less severe conditions to follow 1957 with regard to free water in the Nyasa/Shire complex, which included a cyclic evaporation factor also.¹⁴ He referred Mr Brask to § 232 and suggested that he had confused local

intensity of rain with overall quantity. Apparently the lake had risen only about 2 ft by 8 March, 1958.

265. *The Author did not believe he could make weather forecasts and had not made any.* He worked in terms of trends if the data permitted.

266. With regard to the length of the Shire record, the Author could not prove anything; he could only exhibit the data for the Nyasa/Shire and for other phenomena from other places with a longer record. He tried not to interpret the behaviour of any natural phenomenon in isolation, but as part of a bigger family.

267. His evaporation formula had been devised to use the only three kinds of data likely to have been widely recorded. Even if sunshine hours were available they were not very tractable, if only because of different degrees of haziness in the sky.

268. The Author did not think there had been a deep channel in the upper Shire before 1915 which depleted the lake and the Consultants' proposals for a Matope barrage and power station were not remarkable if it was realized that it was the cheapest and most practical, not the biggest, development they had to devise because of the poverty of the country. Nevertheless the Author had examined several major developments.

269. All the water entering Lake Chilwa (or Shirwa), which was small in area in relation to its catchment, was eventually evaporated and there was no outlet. Possibly there was sufficient mixing of the water in transit through Lake Nyasa, which was large in relation to its catchment, to maintain its freshness. There had been some quite substantial mixing phenomena reported by travellers on Nyasa.

270. The Author was interested in the seiche in Loch Treig mentioned by Professor Naylor. He did not think the Nyasa seiche could be dominantly lunar because its periodicity persisted at about 6 and not $6\frac{1}{2}$ hours.

271. The temperature differences shown on Fig. 7 were small but they were dimensionally comparable to those which were believed to have caused widespread changes in climate since the last Ice Age in Europe. Professor Gordon Manley had made an interesting summary of the evidence.³

272. The Author considered that all climatic elements were involved in a delicately balanced and uneasy equilibrium, which was the terrestrial manifestation of the sun's energy or at least of those parts of the energy spectrum which affected climate. He did not believe it was generally accepted that sunspots were sources of intense heat or that a sun with a hundred spots was hotter or colder than one with no spots. Little was known about them and it seemed essential to eradicate conditioned and irrational preconceptions about the significance of their numbers as such. The Author's hypothesis was that, so far as some aspects of climate were concerned, it was probably the turbulence (which he had to express crudely as rate of change) of sunspots which was significant. There was a considerable irregularity of from 7 to 17 years in the sunspot cycle and a greater irregularity still in the rate of change curve. Yet several kinds of phenomena followed this irregularity over many decades. Professor Naylor appeared to rely very heavily on chance in this context. The Author suggested that Professor Naylor might have asked himself the question "What are the chances against the Author's data correlating in this particular irregular sequence for so long?" He felt he had been judged on the failures and conclusions of others postulating quite different theses.

273. The three factors used in the Author's evaporation formula were the only ones likely to be on record in the primitive parts of the world with which he was most concerned. There was very little else available in many civilized countries. Professor Naylor's attention was drawn to §§ 280, 281, and to § 240, in which certain difficulties in respect of existing radiation balance formulae were mentioned. The Author thought it was difficult to associate whatever were considered to be the "logical" factors in a way which dealt comprehensively with the matter. The different factors affecting in-going and out-going radiation and heat, the changes in the water surface, the boundary layer and its temperature, and the speed at which the vapour diffused into the surrounding atmosphere or was carried away by wind had all to be considered. He

had not found a number of diverse and thoughtful theoretical approaches to be in reality much less empirical than his own version, which had not yet gone wrong so far as he knew.

274. In reply to Mr Lawton, who had exceptional knowledge of world-wide hydrology and its problems, the Author agreed that he had not made very much of the Nyasaland evaporation-pan data. The pans were similar to the U.S. Class A pan but he believed they were all lightly screened with wire mesh. The pan factor appeared to be about 0.7 at Kota Kota, about half-way along the lake. This appeared to be in keeping with American experience.

275. One of the difficulties in adducing analogies with other African rivers was the fact that the Shire was an equable perennial river, well regulated by Lake Nyasa. Except for those leaving the other Rift Valley lakes, most other African rivers were highly variable from wet season to dry season. Although the Nyasa/Shire complex could show true annual deficits, as in Table 2, the seasonal rivers never did this because the run-off was not stored to be evaporated later. At the now well known Kariba Gorge the average annual flow in the Zambezi was about $4\frac{1}{2}$ times the average annual free water in the Nyasa/Shire and about the same as the Volta in Ghana, which Mr Lawton knew well, though the Zambezi had normally floods of lesser magnitude. Where it reached the confluence with the Shire, the Zambezi was a very formidable feature in the wet season.

276. The great difficulty in African hydrology was the scarcity of long-term data on rainfall and river flow. Nyasaland had been early settled by Europeans and that was why there was so much information available there compared with other situations.

277. As Professor Linsley had pointed out, Fig. 6 was not an absolute check on Figs 2 and 5 and no one could be certain of what was not in fact known between 1935 and 1950. The Author appreciated that he had done no more than play two reasonably deduced factors against a third known one and observed the anomalies if any. On the other hand he had done no less than this and the fact that in Fig. 6 the deduced and actual lake levels agreed reasonably well for every one of 15 successive years in that period led him to reiterate that he was probably nearly correct in his deductions.

278. Professor Linsley appeared to see more significance in Fig. 8 than did the Author. The latter merely observed that, in a total accumulation of about 30 ft of water extending over a period of 38 years, the maximum accumulated anomaly between reality and estimation was only about 3 ft. This led him to believe, and still did, that he could usefully extrapolate the water balance back to say the middle of the 19th century. He agreed that he could be quite wrong in some individual years and had not quoted any yearly data for this period before 1915. Nevertheless the Author remained of the opinion that the long-term free water was probably of the order of 6,000 cusecs and not the 8,500 cusecs suggested by the data available since 1915.

279. Had Professor Linsley been able to deal with the problem of sequence in his statistical appreciation?

280. The Author was aware of a number of thoughtful and authoritative appreciations of evaporation in scientific literature but he had not believed it prudent to overlook their often diverse approaches and results. Presumably only one method could actually be "theoretically sound" but Professor Linsley did not state which approach and which physicist he supported. It was presumed from his contribution to another Paper⁵⁸ that he favoured a radiation-balance method. The Author agreed that this type of analysis was most attractive, although it had recently been described as no more than plausible.⁵⁹

281. Mr W. N. Allen, who was vitally interested in Lake Victoria, had quoted its annual evaporation as 49 in. (§ 123). Some years ago the Author had arrived at a value a few inches higher by using his own formula but with rather inadequate meteorological data. Hurst and Phillips⁶⁰ had elsewhere suggested a value of 44 in/year. Engineering opinion appeared therefore to agree moderately well. Nevertheless in December 1956 an authoritative radiation-balance study⁶¹ of Lake Victoria had been set out; it concluded with a value of 62 in/year, a surprising value if the published meteorological

data were typical of the area. This illustrated the problem for the practising engineer who might have to make a painful choice between an apparently "theoretically sound" method known to give unexplained and gross anomalies at times and a less elegant empirical approach not yet known to have given an incorrect answer.

282. The Author entirely agreed that established and well documented scientific facts should not be ignored and assured him that a handbook-based engineer would have a short life as a Consultant, though probably not so short as that of one who overlooked *quasi*-theoretical anomalies.

283. The Author's evaporation formula was not based on 6 equations but on 81 as described in § 227. Until something demonstrably more accurate, using widely recorded parameters, became available, the Author felt he could not afford not to use his own formula as a yardstick.

284. The Lake Heffner report⁴⁵ contained various sections and some of the Authors had "adjusted" some of the data. That was not a universally acceptable practice and Professor Linsley's attention was drawn to the section entitled "The water budget control" by Harbeck and Kennan where the least modified data were set out in their Table 1. However, the figure in Table 4 should be 52.2 in. and not the overlooked 52.0 in.

285. The Author reminded Mr Owen that the full line in Fig. 6 was clearly described as being calculated from rainfall. On the relevant Fig. 2 the observed points were scattered about the chosen curve and so it was impossible to obtain complete coincidence between calculation and reality on Fig. 6, which was used only as a test of the Author's deductions in the undefined period 1936-1950. With regard to the rating curve in Fig. 5 between 1939 and 1950, although there had been no river gaugings then, there had been much casual recollection of the stages of the river at Liwonde vehicle ferry, at Mzimba, and at Matope. The value of these was enhanced since the Shire was an exceptionally well regulated substantial river in the dry season when most of the water came from the lake and so Fig. 5 was not entirely arbitrary. He hoped its numerical accuracy would be within $\pm 15\%$ in the early and $\pm 7\frac{1}{2}\%$ in the late 1940s. With regard to the "years" in Table 2, they did in fact include almost the same water and had been chosen for that purpose.

286. Mr Owen's calculation of the water balance was actually an encouraging confirmation of the Author's estimated 61 in. of evaporation. Would Mr Owen have chosen an unvarying 61 in. if he had not known of the Author's water budget evaluation¹⁴?

287. Mr Pillar could be assured that the Author was interested in climatic variation and agreed that it was a subject worthy of more space than could be devoted to it in the Paper. Attention was drawn to § 234.

288. The Author was well aware of the climatic changes in say archaeological time but he was not planning for the next millennium, only for a hundred years or so, and he had wanted something fairly positive for several hundred years backwards. He knew very well that the Nile had remained perennial for several millennia and he also felt that 2°F was a small numerical change comparable to no more than the change say from one month to the next.

289. It was of interest that one of Mr Pillar's references⁵⁴ has postulated sudden secular reductions in tropical rainfall in the past 100 years of the order of 20%. If this had happened a great proportion of habitable Africa would have suddenly been depopulated through famine. A catastrophe on such a scale would hardly have passed unnoticed. Some discrimination in interpretation of general opinions seemed desirable.

290. Mr Clarke's attention was drawn to § 278. The Author did not really feel he could advise anyone that an answer was about 2 ft between the range of -0.2 and +4.2 ft. In spite of Mr Clarke's fears there had been a maximum accumulative anomaly of only about 3 ft in Fig. 8 in a total accumulation of 30 ft spread over a 38-year period.

291. In the Author's experience Fig. 2a indicated an exceptionally limited scattering

of points for a large tropical complex and if Mr Clarke was hesitant about it quantitatively he might find himself hampered in practice for there was much worse to come.

292. Mr Yousaf's attention was drawn to §§ 280, 281, and 285. From 1928-35 and since 1950 rainfall and free water were known, hence a tentative relation could be drawn. From 1936-1950 rainfall was known and free water could be deduced. Outflow was also known at some dates and had been deduced at others. Lake levels were always known. Hence deduction was played against actuality and the anomalies observed. In Table 2 all free waters from 1915-35, and since 1950 were known by measurement. Three years between 1936-1950 were known fairly closely by measurement and the rest were deduced as described above.

293. The Author had not relied much on the relation suggested by Mr Yousaf. The difficulty was not in speculating in that way but in restraining oneself from doing so. Evaporation was all important and least definite. He did not know of any situation where there was data to allow the equation to be evaluated for 4 years, let alone 40 years. He suggested it would be better if the equation remained symbolic and a focus of the attention.

294. In reply to Mr Bala Sundar, the Author observed that at Liwonde where river-flow measurements were made, decomposed rock came to the surface and he considered that unmeasured leakage was not dimensionally significant. There obviously must be some storage in the catchment, which was one of the reasons why the Author generally worked in 3-year means and not in individual years if he could avoid it. The storage could not be great because of the geology, and many of the streams dried up in the dry season. Dr Dixey and his colleagues had published much geological information on Nyasaland.

295. Some deep soundings in Lake Nyasa were available, as were temperature observations for a few years. The Author agreed that the examinations suggested by Mr Bala Sundar might be useful in the future.

296. The Author believed he could assure Mr Wolf that the lake levels and river gaugings were of a high order of accuracy by any practical standards. Many of the rain gaugings were of similar calibre but it would be noted that no catchment rainfall data had been quoted before 1928 although many were available.

297. With regard to the 1957 prediction, the first requirement in the prediction of the behaviour of natural phenomena was at least an average allocation of good fortune and perception. These alone were not enough and the Author also worked in terms of 3-year running means. It was necessary to predict the sunspot number 1 or 2 years ahead, which was not impracticable though it was difficult. Thence from the predicted rate of change and the Author's correlation, the 3-year water mean was deduced. This included 2 known years and the unknown one was obtained by subtraction. In spite of this recent success the Author was not really very confident about predictions of one-year events. He was more interested in trends.

298. Mr Wolf's reproof about "variable constants" was accepted but it might be pointed out that those which were now respectable were for most practical purposes no longer variable but actually more or less "fixed" in standardized situations. It was the abuse of the convenience which was insidious.

299. The Author had not said that he mistrusted statistical theory. On the contrary he had observed that the professional statisticians were very discriminating in their use of the technique. Engineers and others, including himself, had been less inhibited in their interpretations and the numerical results usually conveyed a strong sense of security or fear, often quite groundless. His own use of the techniques had become more restricted but it might be taken that his responsibilities did not permit him to discard any technical tool through mere prejudice.

300. He had been impressed how a number of contributors had, by statistical methods which apparently ignored sequence, or by allusion to other people's statistical methods, reduced the Author's visual sequential correlations to apparent insignificance with fluency and precision, and yet the relations remained for all to see.

301. The Author's perturbation was also reduced by the knowledge that there

appeared to be no statistically significant relation between the hourly incidence of clouds at his home in Sussex and the hourly incidence of rainfall there. In fact no statistician dared logically conclude from such data that there was a significant positive relation between the rain and the clouds, although there was a fundamental one.

302. Without doubt there were many fascinating intellectual dilemmas in the application of statistical methods to natural phenomena which were not true events but averages and there were so many curiously persistent recurrences of statistically improbable "events" that the solution of the dilemma was unlikely to be easy. Nevertheless statistical analysis had so much to commend it that the Author hoped the circumventing of the very real perplexities of sequential behaviour in nature did not lead hydrologists to become the Baconians of engineering.

303. With regard to his own hypothesis the Author hoped he might echo Bertrand Russell's statement⁶²: "I do not pretend that the above theory can be proved. What I contend is that, like the theories of physics, it cannot be disproved and gives an answer to many problems which older theorists have found puzzling. I do not think that any prudent person will claim more than this for any theory."

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