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Author(s): R. L. Raikes

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THE PHYSICAL EVIDENCE FOR NOAH'S FLOOD

By R. L. RAIKES

IN a recent issue of this journal Prof. Mallowan reviewed the historical evidence for the flood described in the biblical story of Noah.¹ In the table that he prepared three floods are noted at Kish. Two of them occurred in *c.* 2900 B.C. and rely for their identification on evidence of water-deposited material in the streets and damage to mud-brick walls: the third, the latest and most violent, dated to *c.* 2600 B.C. was again identified by evidence from the streets and deposited an average of 40 cm.² of water-borne material—that is about sixteen inches. The same table read in conjunction with the text (p. 80), indicated for Shurrupak (Fara) a deposit of clay and sand of about 60 cm. thickness over an “ordinary patch of charcoal and ashes” above the Jamdat Nasr level and below the Early Dynastic: it is dated to *c.* 2850 B.C. As these levels are dated *c.* 3000 B.C. and *c.* 2850 respectively, I suppose that the flood deposit could have occurred at any intervening date, but that specific evidence suggests 2850 B.C.

For Ur the table records flood deposits from 3·72 to 0·72 metres thick in *c.* 3500 B.C. at the end of the ‘Ubaid period (the text records greater depths) and deposits of unknown depth dated to *c.* 2700 B.C.

There is not very much to go on here and no means of correlating the absolute levels of those flood deposits that are approximately contemporary. However some interesting points emerge. The relatively thin flood deposits at Kish are capable of two interpretations. Either they represent a deposit of unknown depth all over a flood plain from which Kish already stood out as a *tell* or they were due to an abnormal flowing flood which was high enough just to affect the *tell* and its streets for a short period.

The second alternative would justify the adjective violent and would agree with the subsequent repairing of the walls flanking the street where the deposits were found. The first alternative would require such a depth and duration of flooding that the walls could hardly have survived as mud brick loses its strength when saturated.

At Shurrupak the deposit referred to by Schmidt as “yellow dirt, a mixture of sand and clay” could, from that rather imprecise description be almost anything but its thickness of (probably) 60 cm. is consistent with deposition under still water conditions. A flood stratum such as this of which the bottom part contained a known ware could have been deposited at any time during

¹ M. E. L. Mallowan: “Noah’s Flood Reconsidered”, *Iraq* XXVI (1964)

² The reference to *op. cit.* p. 78 given as ·40 cm. should read 40 cm.

or even after the currency of that ware. My concern is not with the dating, although I confess to the normal amateur's interest in it, and the correlation of flood data from different sites requires close dating.

The earlier flood at Ur, dated to *c.* 3500 B.C. deposited material identified by V. Malycheff³ as "Limons argileux fluviatiles d'inondation". The sentence on p. 72 of his paper in which Mallowan refers to the tests of this material is so relevant that I quote it in his words "he (M. Watelin) took away some of the silt and published an analysis of it made in the laboratory of the Physical Geography section of the Sorbonne which proves that it is fresh water silt *containing just those elements* which are to be expected from the waters of the Euphrates". The italics are mine. He goes on to say that the riverine origin of the material was confirmed by microscopic analysis carried out by the Petrographical Department of the Geological Survey in London.

If I interpret the italicised words correctly, as appears probable from my translation of Malycheff's description as "riverine clayey silt", they mean that the composition (minerals, particle-size distribution etc.) of the sample tested are what one finds on analysis of silt-laden Euphrates water. In other words all the sediments were deposited including the clays and this implies still water conditions. The great recorded depth of the deposits at Ur, over 3 metres, and at Shurruk, probably about 60 cms., are significant as they would require lagoon-like conditions for a fairly long time; not necessarily a great depth of water at any one time as some outflow, evaporation and infiltration could have balanced inflow. If the outflow were small in relation to the volume impounded the deposition of all sediments could be expected.

The two earlier of the three flood episodes at Kish, about which we know nothing except that their deposits were less than 40 cm. and that they also were observed mainly in streets, do not agree in date with those at Ur and Shurruk. If the evidence of flow has been correctly interpreted they do not agree in kind either.

A rational review even of the data is extraordinarily difficult. For instance the absolute and relative levels of flood deposits at various sites are not known; the depths of deposits at Ur and Shurruk are not known relative to the level of the surrounding plain immediately before the floods. It is impossible therefore on present evidence to do more than guess that there may have been widespread flooding of long duration (much longer than forty days!) affecting several sites simultaneously.

An attempt that I made to reconstruct a flood stratigraphy for Ur itself from the excavation reports reveals a situation that may have been much complicated by subsequent rebuilding at various levels. Deposits occur in varying thickness from a depth of 2.5 metres below the present flood plain to a height of nearly 7.0 metres above it, all described in one or other of the ways which Woolley

³ V. Malycheff: "Analyse des Limons de Kish et d'Ur", *L'Anthropologie*, XLI, pp. 269-271.

used for his flood deposits. The descriptions vary from “clean clay” to “clean water-laid sandy clay”. I selected as “flood” deposits only those beds of which the description contained the word clay.

LEGEND :-

- "FLOOD" DEPOSITS - [diagonal hatching]
- OCCUPATION LEVELS - [horizontal hatching]
- TEMPORARY SURFACES. - [cross-hatching]
- POSSIBLE OUTWASH - [dotted pattern]

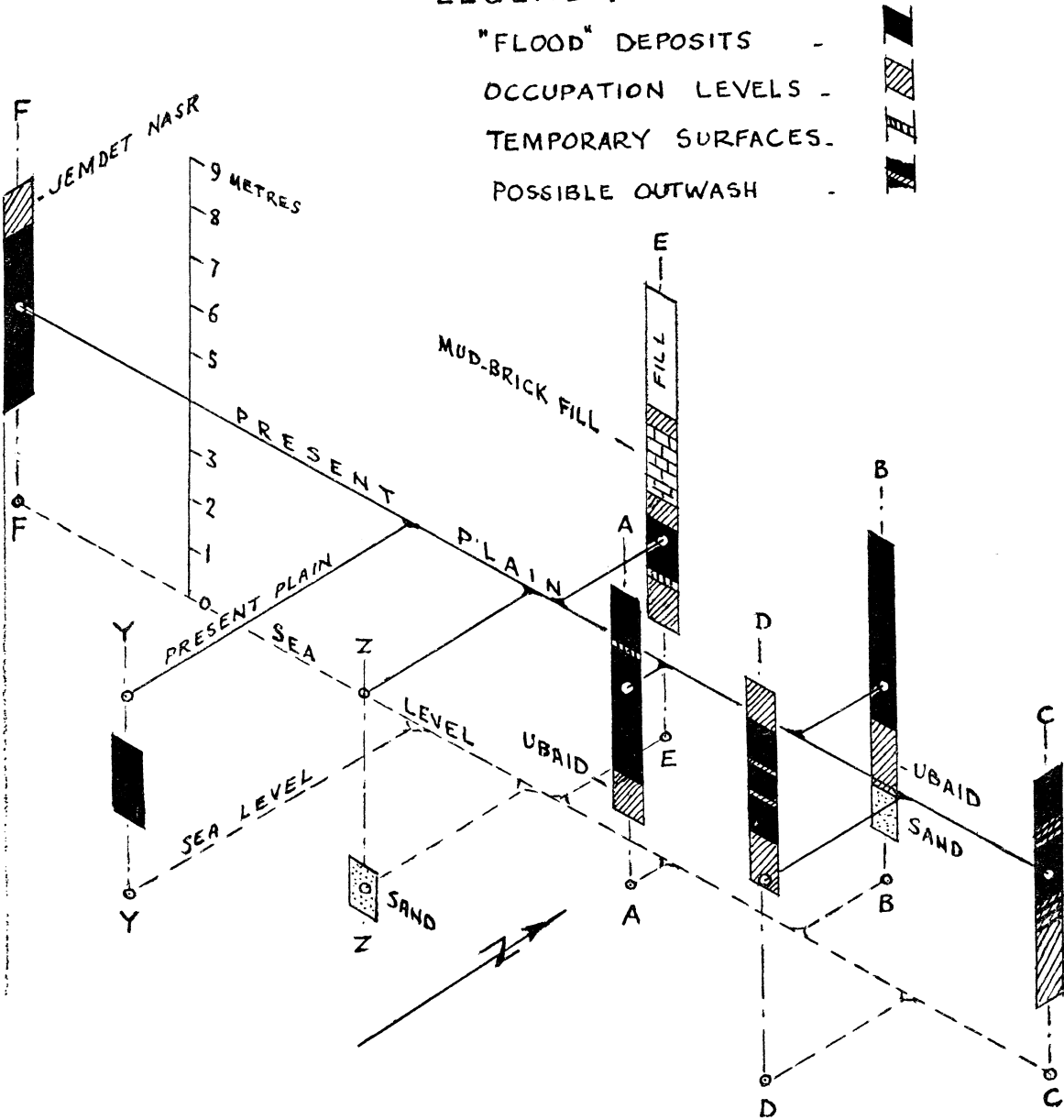


FIG. 1. Ur. Isometric view of Woolley's pits A to F, Y and Z. A, C and D indicate possible later flood(s).

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E

The resulting tentative flood stratigraphy shown in Fig. 1 *could* be consistent with the following sequence of events:

1. A normal somewhat irregular *tell* formed on a flood plain about 2.5 metres lower than that of today and reaching locally some unknown height above that flood-plain: occupied up to and including the end of 'Ubaid.
2. A flood that occurred at or after the end of the 'Ubaid occupation and buried the whole or part of the *tell* under flood deposits to a depth of at least 5.5 metres in relation to the flood plain of that time: total submersion would have involved abandonment; partial submersion would have permitted some continuous occupation.
3. Re-erosion with outwash at various levels.
4. Re-occupation of the exposed and eroded mound in the Jamdat Nasr period.

A longer period of abandonment may be indicated by the occurrence of the temporary surfaces referred to by Woolley at various levels but is not supported by other evidence. The existence of further flood material above and containing Jamdat Nasr material may indicate the repetition of the whole cycle.

After this article had already been prepared in its original form I was asked to include a summary of a recent article by Prof. H. J. Lenzen.⁴ This seems the appropriate place to consider it. His conclusions based on much of the evidence that I have already considered, are quite different from mine. Later I will refer to extremely important geomorphological evidence which is not quoted by Lenzen. The summary of his paper will be combined with my criticisms of it.

Lenzen's paper reviews the evidence for a flood or floods at Ur in relation to evidence from a number of other sites in the same area. These sites are Kish, Eridu, Warka (Uruk), the Qal'a of Sheikh Juabi Haji Mohamed, and one un-named near Warka.

He starts off with quotations from the Epic of Gilgamesh and from Genesis and the effort of translating fortunately made me look again at the Bible story with which I was clearly not as familiar as I had thought I was. A possibly important point had escaped me even after reading Mallowan's article to which I will refer later.

In going on to claim, as most people do, some common origin, probably in Sumerian times, for both accounts he states that there exist accounts of the flood in almost all parts of the world. Surely this is not so? I do not believe that, despite the evidence of really disastrous prehistoric floods in the Indus valley, there is a parallel legend there.

He sums up the problem by putting a question that must be answered: were flood deposits found in other excavations?

⁴ H. J. Lenzen: "Zur Flutschicht in Ur", *Baghdader Mitteilungen* 3 (1964).

In the case of Kish he rejects that the physical evidence, which the excavators "have not hesitated to describe as flood deposits", could have had anything to do with the Gilgamesh epic because of the discovery of seals representing that hero below the flood deposits. It is for archaeologists who have studied the seals and their depth below the flood deposits to judge whether this reasoning is convincing. Altogether four flood levels were identified by the excavators at Kish and Lenzen quotes S. Langdon⁵ as considering that only the oldest (one of the two evidenced by comparatively deep deposits) could possibly be the origin of the epic. The use that can be made of data from Kish in a regional review is hampered by lack of information about absolute levels there.

In considering Ur, Lenzen points out that only seven of Woolley's fourteen soundings contained flood evidence.⁶ In passing it should be noted that his basis of definition for flood-deposited material clearly differs from mine. He considers that soundings W, A, B, C, X, Y and Z have flood strata and indicates their levels and thickness while D and E are specifically described as containing no flood deposits. I was not impressed by the claims of W or Z (although I have indicated in my drawing its sand deposit at sea level) and do detect, from Woolley's description, possible flood deposits in both D and E. Clearly so much depends on the way that one interprets Woolley's unscientific soils terminology that a detailed comparison of Lenzen's and my tentative identifications of flood deposits would be a waste of time. He does not dispute that what he identifies as alluvial deposits were in fact laid down under water but he does not seem fully to have appreciated the importance of the state of motion of the water. On the whole therefore he accepts the existence of the evidence but interprets it quite differently.

He then considers Warka 80 km. away where he agrees with Woolley that there is no evidence of a flood but differs as to the reason. Woolley did not expect flood evidence from the centre of the mound while Lenzen points out that some of the trenches were put down in places that were not in the central area before the IIIrd millenium and that therefore there could well have been evidence of a flood if a flood had occurred. Here he makes a point of the relative levels which are known.

In the case of Eridu, which in Lenzen's paper is considered before Warka, the evidence or lack of it gains in importance because of the closeness of the two sites, only 11.25 km. apart. However no level lines link the two sites and there is no means of telling what the effect would have been at Eridu of a certain degree of flooding at Ur. Lenzen points out that the oldest cultural level reached at Eridu, that of the Haji Mohamed phase there, is earlier than

⁵ Watelin-Langdon: *Excavations at Kish*. On the problem of the seals, see Mallowan, *op. cit.* pp. 72-79 and notes 50-51.

⁶ Sir Leonard Woolley: *Ur Excavations*, Vol. IV, London, 1939.

the 'Ubaid levels at Ur and that no flood evidence has been found. The interpretation of this in relation to Ur must await the establishment of their relative levels and examination of the possibility that the Eridu excavations might have been done at places where no flood evidence had ever been or had survived. For instance they may all be in the main part of a mound that was only liable to flood damage on its exposed flanks: or flood deposits could have been eroded away.

In reviewing the whole of the data Lenzen makes certain assumptions that cannot possibly be verified and which are very likely to be incorrect. For instance writing of the flood plain below Mughair he notes a break in the flood plain gradient and assumes that the general conformation of the plain has remained virtually unchanged for 5000 years. His description of the process by which the plain level is gradually built up by annual flooding and siltation to some extent contradicts this for the rate of build up must have always been greater in the flatter parts and the major shift of the Euphrates from just south-west of Ur to a line several kilometres away to the north-east argues against any rash assumption about the plain.

He refers to an un-named mound north-east of Warka, now buried under the general present surface of the plain, that contains only late Jamdat Nasr material between 7.0 and 9.0 m. It is not clear whether these represent depths from the surface. In the absence of known ground levels the relevance of this mound is difficult to assess. If they represent absolute levels they are above the level of the highest "flood deposits" at Ur; and of course it is quite a long way from Ur.

Lenzen describes how Woolley explained the mechanics of siltation at Ur. He rightly points out that with the flood stage of the river "from 50% to 100% above the deposited sand" (in parenthesis what do these percentages relate to? The thickness of the deposited material? Surely not, for this varies enormously), a water level of + 7.0 m. to + 11.0 m. would be required which would have implied flooding of the whole of Mesopotamia from the desert to the Persian Mountains. As Lenzen himself points out earlier in his paper this concept is absurd. The full absurdity of it is not however revealed until one considers where all this immense amount of water could have come from and tries to picture it pouring into the Persian Gulf.

Drawing parallels from what happens each year in the Tigris and what happened to a flooded trench at Warka after heavy rain, Lenzen is of the opinion that the flood deposits at Ur represent siltation caused by two distinct great floods of the normal overflow type. The second flood, held at bay by peripheral midden accumulation during the years succeeding the first one, left its deposits in different places. The idea is ingenious and must be considered with other ideas, but I do not find it convincing. Flowing floods do not, as Lenzen himself implies elsewhere, deposit huge thicknesses of silt. The physical evidence of widespread deep deposition is quite unlike that of a rain-filled

trench in which practically all the solid material must be derived from the walls of the trench, aided perhaps by some erosion of the surrounding surfaces, as there is nowhere else it can come from. The Tigris evidence has no direct relevance, for the flood regimes and velocities of the Tigris and Euphrates are quite different. Apart from the fact that the range of level (stage) from lowest minimum to highest maximum is nearly 7.5 m. at Baghdad on the Tigris and only a little over 5.0 m. at Ramadi on the Euphrates the seasonal distributions of flood discharge differ.⁷

Lenzen concluded his article with the words "it is absolutely certain that a catastrophic overflow of a great size formed the basis of the Sumerian legend of the Flood. However, despite the conclusions reached by Sir L. Woolley, it is not possible to find evidence of a catastrophic flood in which—just as would happen today under similar circumstances—towns and villages, men and animals were destroyed. Actually we cannot say or write that the Universal Flood has been proved.

"As already indicated at the beginning, neither the short flood of Gilgamesh, nor the Biblical Flood, whose story probably derives from it, can possibly be held to be true. Why then look for proof when there is nothing to be proved?"

This conclusion seems rather contradictory, so I fear some nuance has been missed in the translation. I have given the latter in full so that it can be compared with the German original. The conclusion also seems premature in the light of available knowledge. There is every reason for further investigation though it should be objective and not directed to proving assumptions.

I mentioned earlier the possibly relevant point arising out of re-reading the Biblical story. With every reservation and all allowances for poetic language it may be more pertinent than Mallowan concedes that this account contains the words "... were all the fountains of the great deep broken up". Unless this is pure fantasy along with the rest of the story, it surely could imply that the flood had other than meteoric causes. If a sudden or even fairly sudden subsidence occurred with accumulation of water that would otherwise have drained away is not this just the sort of way in which it could have been described?

A proper consideration of Lenzen's article has been made difficult by the absence of a map of the area and the difficulty of distinguishing sometimes between his depths, and heights above sea-level. A map with spot levels and a few schematic sections would have made the argument much clearer. At the risk of seeming carping I would like to suggest that the use of the mathematical symbol \pm , which in mathematics has an unequivocal meaning, should be used only where that meaning is intended. The use as a substitute for "more or less" can be very misleading. For instance I am still not absolutely certain that where Lenzen writes ± 4.0 m. above sea level he means more or less

⁷ M. G. Ionides: *The Regime of the Rivers Euphrates and Tigris*, E. & F. N. Spon, London 1937, p. 3.

4.0 m.; for it could mean within the range $+ 4.0$ m. and $- 4.0$ m. above sea level. The second use is perfectly correct, as a minus quantity above sea level means mathematically the same quantity below sea level.

An uniquely valuable and authoritative contribution to understanding of the problem was made by Lees and Falcon⁸ in 1952. They examined the problem from the standpoint of geology. Macfadyen, quoted by Mallowan,⁹ emphasises the complicated nature of the problem and how whatever the origin or origins of the main deposits—marine, estuarine or lacustrine—river-borne or aeolian sediments were continually being laid down. The inference is that these latter sediments could be and probably are, either intercalated with the main deposits or intimately mixed with them. However this opinion does not necessarily conflict with a riverine origin. Woolley's interpretation of the Sorbonne analysis may have been sanguine in isolation, but surely not in the light of the microscopic analysis carried out in London.

The opinions of Lees and Falcon certainly do not conflict with those of Macfadyen already referred to. Rather do they give at least a partial and cautious explanation of those opinions. They refer for instance to alternating advances and retreats of the head of the Persian (or Arabian) Gulf throughout historic and prehistoric times and do not connect all the retreats with delta formation. In this connection they refer to the observed fact that the waters of the Shatt el Arab, below the confluence of the Euphrates and Tigris, are practically silt-free, but I suppose there may have been periods during which the conditions (referred to later) that now render the water silt-free were absent. During such periods delta formation could have occurred. Lees and Falcon also refer to the fact that floods in Mesopotamia are endemic rather than exceptional. They are normally seasonal at that. The point that they bring out which may be of the greatest relevance is the peculiar geological situation of Mesopotamia. They explain that "the plains of Iraq and the Persian Gulf occupy a zone in which gradual subsidence has been taking place during the concluding episode of the mountain building movements of Iraq and south-west Persia". The picture that emerges is of tangential thrust causing folding of strata (originally occupying a syncline) and resulting in the south-west Persian mountain belt being uplifted with a complementary zone of subsidence. This zone of subsidence is in Mesopotamia where the level at any time is the result of the opposing influences of sedimentation and subsidence. The lower Mesopotamia lakes and marshes appear in general to be, at least temporarily, stable as to level. Continuing settlement affords continuing sedimentation space and the result is the silt-free Shatt el Arab. However elsewhere in their paper they refer to *episodic* movements and their description of Dar-i-Khazinah confirms this.

⁸ G. M. Lees and M. L. Falcon: "The Geographical History of the Mesopotamian Plains", *Geog. Jour.* CXVIII, Part I (1952).

⁹ *Op. cit.* 1.

The evidence from that site is consistent with a single catastrophic flood episode (or a closely spaced series) in about 3000 B.C. The situation of the site to the east of the plain towards the mountains and the picture of almost concertina-like folding that comes from Lees and Falcon's paper suggests the possibility of a more or less rapid uplift episode downstream and west of the site with a complementary more or less rapid subsidence further west again out on the plain. If there were a rapid and fairly localised subsidence flooding would be the inevitable consequence.

It is essential to distinguish between the two kinds of flood of which different deposits seem to be evidence. The thick deposition of clayey material dated to 2850 B.C. or 3500 B.C. could only have been laid down under conditions of long sustained and deep flooding—those that could have resulted from subsidence. The thinner deposits on the other hand appear from the evidence to have resulted from abnormal flowing floods.

Both the legend of the Noachian flood and the Gilgamesh epic connect the flood with rain. As noted earlier, part of the Biblical story could be interpreted cautiously in another way but the reference in it to prolonged rain is entirely specific. The reference to a tempest from the south in the epic is equally specific.

Such scanty meteorological data as are available to me can only be interpreted with extreme caution. They indicate that the most likely months for rain are, in decreasing order of importance, January, November or February, and March. Rainfall later in the spring is known and may, as elsewhere in the same climatic belt, be characterised by high intensity storms of short duration. It is conceivable that heavy rain could occur continuously for seven days and records show more than seven days consecutively on each of which some rain fell. Rainfall is so enormously variable, particularly in areas of low mean rainfall, that it would be a rash person who would discuss as impossible even forty days' heavy rain. But rain of high intensity is almost invariably associated with isolated storms of convective type that are of extremely limited duration so that it would not be too rash to say that forty days of intensive rain are virtually impossible.

But let us imagine the chance and extremely memorable combination, and deduce the effects, of (a) ten times the present mean annual rainfall, (b) all falling within a period of forty days (or seven days, for it makes little difference in the circumstances provided by the next factor) and (c) the flood plain already covered by an entirely exceptional overflow flood from the river or rivers. However apparently flat the flood plain, it does have sufficient slope to maintain flow in the rivers and ultimately to drain itself, and the deeper the overflow flood the more rapidly the flood-waters would flow to the sea. This increased velocity would incidentally inhibit the deposition of silt. On top of floodwater brought in from outside the lower flood plain area, there would be added a further metre or so of rain over the period of rainfall all of which, if one ignores

evaporation losses, would run off. It would tend to increase the depth of flooding, but the increase would be limited owing to the greater velocity of flow. With the outlet to the Persian Gulf unaffected, for this reconstruction does not admit any non-hydrological factor such as uplift or subsidence, a flood of the kind that could deposit very great depths of silt would be impossible. It is worth noting too that the ark or any primitive raft on which the legend of the ark was based would have been swept into the Persian Gulf. The description of Noah's ark contains no reference to sails: that of Gilgamesh's ship states that the mast was carried away. So even with the south wind either ship would have been completely at the mercy of the current. In case anyone suggests the use of oars, let them consider who could have wielded them!

I am not saying that the combination of conditions could not happen although the records of Euphrates floods¹⁰ shows that the annual highest discharge occurs in April or May, well outside the normal period of highest rainfall. These spring floods are not generated in Mesopotamia but hundreds of kilometres away in Anatolia. The meteorological records show no consistent correlation between rainfall and a wind from the south (from any point between east and west). They merely indicate that such a combination is possible.

The argument has been used, and has considerable weight, that a flood of such size that it became part of the legend of Mesopotamia must have had entirely exceptional causes. I entirely agree with the principle, but as I have tried to show, meteorological and hydrological factors alone could not conceivably have created widespread and deep flooding by still water. So it is reasonable to look for some other equally unusual factor of a different nature. That people, to whom regular annual flooding was one of the inescapable facts of life, should have singled out one event (or possibly more) for description to their children, seems, even allowing for the natural tendency for oral tradition to exaggerate, to point to a flood event of a different type as the origin of the legends.

I do not think that many will agree with Prof. Lenzen's attitude to the floods of Genesis and the Gilgamesh epic. They will continue I hope to arouse much controversy. Let me summarise the contribution that my review of the evidence can make.

1. Any idea of a vastly expanded version of the annual Euphrates flood can be dismissed as a prime reason for the legend, for its results would be entirely different in every conceivable way from those described in the legend.
2. The evidence of deep depositions of silt which, in Lenzen's postulated second overflow flood, occur outside a zone of peripheral debris is not consistent with flowing water.

¹⁰ *Op. cit.* 7.

3. The analyses made of the deposited material are consistent and show what one would expect if *all* the sediment load of the Euphrates, including the clay fraction, were dropped: and this indicates still water conditions.
4. Still water conditions would only be achieved by impounding of the water through the operation of some hitherto unidentified phenomenon. This could have taken the form of either local uplift or local subsidence.
5. Uplift alone could only have deposited the great depths of silt observed if it had been of many tens of metres in vertical extent: otherwise overflow and subsequent cutting through the barrier would have provided neither the time nor the depth to account for the great depths deposited.
6. Subsidence alone could have accounted for all the depth of deposit without necessarily having caused abandonment of the higher parts of the then-existing mound but would not explain subsequent re-erosion. To account for this there would have to be some subsequent change in gradient or uplift. Neither the subsidence nor subsequent uplift conflict with Lees and Falcon's picture of the underlying geology, or with their hypothesis of slow subsidence interspersed possibly with episodic fast movements.

I do not put forward the possibility of subsidence as a hypothesis, for the data are quite insufficient for anything as daring as that. I am putting it forward only as one of possibly many explanations. All one can say is, that it is not inconsistent with the information available and that it should be one of the possibilities to keep in mind when and if the problem is studied. I nearly wrote studied anew, but this would be to miss the point. So far the flood has not, outside the texts, been studied as a problem in its own right. It has been a side issue of some interest for various digs, and so long as it is no more than this, it will not be solved, simply because its study cannot be limited to what can be found at an archaeological site or sites.

Could it not be considered as an archaeological problem in its own right and tackled in a quite different way? A combined operation would be required in which many disciplines would be directly or indirectly involved. Macfadyen has already suggested something on these lines in the already quoted letter that he wrote to Mallowan. The specialist study of foraminifera and micro-mollusca in flood deposits that he suggests is essential, but I would go rather further than him in not limiting it only to the relevant flood deposits at sites. Much could be learned by detailed study of flood plain profiles *away* from ancient sites, where no risk of confusion of natural deposits and mud-brick can lead to errors, provided that the levels of the various horizons were carefully and systematically measured and correlated. It would not need a dense survey: probably auger holes on a grid of about 40 kilometres each way would yield sufficient information. The re-excavation suggested is also essential, but should not be limited to the few sites where flood deposits have been already deduced

and should be carried out according to accepted archaeological techniques. At all sites the new soundings would be dug by archaeologists in areas likely to be of flood interest, without any consideration for their normal archaeological interest. The resulting stratification should be tied in, by precise levelling, with the soil horizons, and all should be referred to mean sea level. It can probably be safely assumed that any soils in the plains not positively identifiable as of aeolian origin have been laid down under water in one way or another, and visual evidence of stratification is not therefore essential for them. The deposits at sites should however be visually identified by a specialist, as in such cases it is essential to distinguish between disintegrated mud-brick or artificial fill and naturally laid deposits.

The whole body of data, including such dating correlations as might result from limited soundings, should then be studied with a view to determining whether there is evidence of widespread impounding of water, or conversely of normal steady sedimentation. Air photos at this stage might be helpful in identifying old buried courses of the rivers and canals: this has already been done in some areas. Air photos might also indicate at an earlier stage desirable sites for a few additional auger holes. If there is evidence of impounding of water the geological causes of it should be studied by a geologist and the flood, sedimentation, and evaporation regimes should be referred for study to a specialist in that field.

In some such way we should not only be able to solve the problem of the flood but at the same time learn a tremendous amount about the whole environmental background of prehistoric Mesopotamia.

Let us hope that someone may be persuaded that it is worthwhile.