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THE MOHENJO-DARO FLOODS RECONSIDERED

R. L. RAIKES AND G. F. DALES

VIA DEI MATTEI NO 3, 00030 COLONNA (ROMA) ITALY.
DEPARTMENT OF NEAR EASTERN STUDIES, 1229 DWINELLE HALL, UNIVERSITY OF CALIFORNIA, BERKELEY, CALIFORNIA, 94720.

INTRODUCTION

Both of the co-authors of this paper were invited separately to contribute to this volume. Very few new archaeological data having a direct bearing on Mohenjo-daro have become available but time, and the criticisms of others, have made necessary and possible a re-assessment of the "flood theory" generally attributed to us. We therefore decided to write a joint paper in which opinions on the possible effects of non-human agencies are the prime responsibility of the first-named author while archaeological opinions are that of the second-named. We hope that the need of each of us to make his disciplinary contribution comprehensible to the other will have helped to make the joint result more comprehensible to everyone. Unfortunately the accidents of geography (one author in Italy and the other in U.S.A.) and the very short time available for direct discussion have not helped.

The suggestion that geological factors could have caused, or contributed to, the end of the Indus Civilization in Sind was first put forward by M. R. Sahni (18) in 1956. It should be emphasised that he was concerned with that part of the Indus Civilization centred on Mohenjo-daro in Sind and his direct evidence came from much farther south in the delta area. The first paper by either of us on the subject appeared in April 1964(10): it was based on personal observations in Baluchistan (Makran and Las Bela) and in the Indus Valley, and on geological views and evidence published by, among others, M. R. Sahni (*op. cit*), C. G. Pendse (7), M. I. Siddiqi (19), Rodman B. Snead (21) and Asrar Ullah (22). This was followed by a paper and subsequent notes and comments (12), (13), (14) in *Antiquity* in 1965, all based on field work carried out by us at Mohenjo-daro. Other papers that have a close bearing on the problems are those by the first-named author on Kalibangan (15) and the Gabarbands of Baluchistan (11).

Only one new item of physical data and one related archaeological interpretation (as a result of NEDECO drilling at the site), both very pertinent, have become available : but, even without these, criticisms of the views published in 1965 would have led both authors to re-examine the 'flood' hypothesis. In propounding it eight to ten years ago it was too much taken for granted that readers already understood the mechanism of flood plain build-up. H. T. Lambrick did much in a paper in 1967 (16) to make good this deficiency and, while we do not agree with his conclusions, we acknowledge our indebtedness to him for his clear description of the mechanism.

All our papers dealing with Mohenjo-daro developed a hypothesis which attributes the end of the "Indus" cities in Sind to natural forces—in this case tectonic—that caused the engulfment of those cities in the silts deposited in a temporary lake. Very briefly, it was postulated that still water conditions at Mohenjo-daro resulted from the formation by tectonic activity of a natural barrier across the river near Sehwan. The tectonic activity could have been through faulting or by the intrusion of miocene clays (so-called volcanic muds) under the Indus plain sediments. The occurrence of a number of upward movements, with gradually increasing height of the natural barrier, was thought probable. Ultimately the space upstream of the barrier would have silted and later several thousand years of erosion would have rejuvenated the valley. The tentative nature of the hypothesis was frequently stressed and was clearly demonstrated by the way in which it had already developed between 1964 and 1967, from its original much over-simplified form. Proposals were made by us that would have enabled at least some aspects of it to be tested. It has not been possible to carry out these tests and so the hypothesis remains just that—one of many hypotheses that seek to solve one of the major problems of prehistory. From about 1967 until this year the hypothesis has come under attack, which is of course the proper treatment for any hypothesis. A noticeable thing about the criticisms is the degree of disunity among the critics. They include those still convinced that the end of the Indus Civilization came about at the hands of the Aryans ; those who postulate major Holocene climate change ; and those who persist in seeing exceptional floods of normal type or the meandering away of the river as the causes. The contrary or differing views taken into account in presenting this paper are those of Gregory L. Possehl (8), (9), Jean-Francois Jarrige (5), C. Ramaswamy (16), Gurdip Singh (20), S. R. Rao (17) and D. P. Agrawal (1).

Attempts in the past to make the flood hypothesis comprehensible involved not only cross-discipline gaps but also the fact that very few of those who read the various papers could have been able to visualise (and even fewer could have seen for themselves) such things as the vastness and flatness of the Indus flood plain and the fact that the hypothetical natural dam, eventually perhaps as much as 45 metres high, was never visualised by us as a narrow bank, but rather as an irregular and enormously wide (perhaps more than 10 km.) *swell* of ground. We doubt whether such a swell would have been noticeable from the ground and, at that sort of width, it certainly would not have been breachable by man.

THE PROBLEM RESTATED

In view of the serious and often constructive nature of much of the criticism it is perhaps best to start afresh with an entirely new presentation of the problem that has to be solved. We start by presenting the observed facts, for no counter-theory that does not adequately explain these can be taken seriously. The facts are unfortunately few. They are listed, with comments where appropriate, as follows :—

... Drilling in the HR area at Mohenjo-daro by NEDECO and soundings made by us show occupation levels interspersed with, and saturated by, a clay material that could only be deposited under still water conditions extending down to between 7 and 11.3 metres below the present flood plain level. We described the material on the basis of granulometric analysis as clay-silt ; NEDECO described it as hard clay.

... Two of our soundings were taken down through ruins still surviving as mounds above the flood-plain ; the third was made in the low (flood-plain level) area between HR and the Citadel complex. One of NEDECO's boreholes was drilled through mound material (see later references to the Aryne/Khan report) at Mohenjo-daro itself : the other was drilled from flood-plain level 2 miles outside, and to the south of, the Mohenjo-daro ruins. The NEDECO boreholes were each associated with 11 observation holes.

... The borehole and observation holes drilled outside the ruins encountered no occupation material in the top 11 metres or so, but did show that what NEDECO call hard clay, and we call silt clay, extends there to depths that correspond almost exactly with what we have interpreted as still-water deposits at Mohenjo-daro itself.

... Beneath the occupation-plus-clay deposit at Mohenjo-daro there is fine silty sand with a relatively thin (2 metres) cover of clay-silt-sand.

... This clay or clay-silt-sand is *not* the same material—in either consistency or colour—as the clay-silt overlying it.

... Our third sounding and NEDECO's *New Site* borehole show that occupation deposits extend down to some 20 metres below plain level.

... Sir Claude Inglis (Indian Irrigation Dept.) (20) estimated that the alluvial rivers of former undivided India build up their flood plains at around 1.8 metres per 1000 years through natural sedimentation. This is not strictly speaking a fact : it is rather a reasonable estimate based on, presumably, careful observation.

... The Egyptian Irrigation Department estimates the corresponding figure for the Nile as 1.37 metres per 1000 years. Again, this is not strictly speaking a fact.

... A *fundamental change* occurred in the sedimentation regime of the Indus at Mohenjo-daro at whatever date is to be attributed to the occupation levels at a depth of 11 to 12 metres below the flood plain. The justification for regarding this as a fact is developed later when the whole problem is presented afresh in terms of sedimentation rates.

... The archaeological material recovered, from between 9.5 and 20 metres below the plain, from the NEDECO drilling at what is described as the New Site, 2 miles away from the area of mounds that has so far defined Mohenjo-daro, has been identified as pre-Harappan or formative Harappan. This implies that the identification of all material down to a depth of 11 to 12 metres below the flood plain at Mohenjo-daro as mature Harappan is probably correct. The horizon associated with the lowest level of this can, as a working hypothesis, be assigned a date of around 2400 BC. Much lower levels associated with the pre-Harappan or formative period and apparently separated from mature Harappan by sterile material do not have, so far as we are at present aware, anything to do with the gradual inundation.

NEW DATA FROM NEDECO DRILLING

The NEDECO drilling results which we have seen are those reproduced in a report by Sultan A. Aryne and Masood H. Khan (21), presented at the UNESCO/Pakistan Government International Symposium on Mohenjo-daro held in February, 1973.

The results are given as Figure 4 of that report, as lithologic bars, and clearly refer to only two wells described as being of 18 inches diameter. Each of the wells has eleven observation wells disposed, or so it can be inferred from the numbering, along lines north, east, south and west of the main wells. Presumably the observation wells were of the minimum practicable diameter. They were probably sited fairly close to the main wells since presumably their purpose was to define the cone of depression during pumping of the latter. The Aryne/Khan report identifies them simply as New Site and Old Site except that in the text, on page three, there is a reference to NEDECO drilling a tubewell just outside the periphery of the exposed monuments. Careful study of the text and of figure 6 (indicating the known area of ruins A and a possible, larger and isolated area B), figure 11 (the results of an electric analogue experiment presumably simulating observed behaviour as a result of test pumping at A and B) and figure 12, leaves little doubt that A is the Old Site or existing ruins and B the New Site. On figure 12 of the report one centrally situated tubewell not connected to the proposed canal system is probably the Old Site tubewell of which the lithologic bar is given in figure 4. The other, New Site, tubewell must be somewhere in the separate area B. Ground levels are not given for the latter but appear to be about +150′. The level of the flood plain is referred to by NEDECO as between 151 ft. and 154 ft. above m.s.l.

For the stratigraphy of the NEDECO wells to be of full use it is essential to know whether the drilling was carried out from plain level or from the surface of the complex system of mounds. Since the objective was to find out what was causing waterlogging in the ruins at Mohenjo-daro it is virtually certain that the latter was the case and this would explain the difference between the depths noted by NEDECO to the bottom of brick and sherd layers and those noted by us, in the case of the Old Site. It would also explain the great variation in depth to the bottom of brick-filled levels shown in the various observation holes. If we are right in locating the Old Site tubewell at the point indicated in figure 12 its situation is similar to that of our third sounding and indeed the results of it are very similar.

In the case of the so-called New Site, where there are no mounds, the well and its observation wells were presumably drilled from plain level. The depth of hard clay in 9 out of 12 holes is 31′ and the bottom of this hard clay relative to an assumed plain level of 156′ in 37′ down. This compares with 37′ in the case of WAPDA well (described below) at the entrance to the site and 38′ in each of our first two soundings in HR area.

The main point of interest lies in the bricks and pottery found in both wells below what we have regarded as the lowest level of mature Harappan. Dr. Mughal has identified pre-Harappan material in the lowest levels of the NEDECO New Site holes : this material occurs between —37′ (relative to a flood plain level of +156′) and —60′. It is tempting to regard this as possible evidence of pre-Harappan tell formation in which case the sand, that occurs from the bottom of it to where the Harappan clay starts, could be evidence of an old bed of the Indus or of the coarse material deposited over a considerable period under flowing water by normal overspill flood or indeed of aeolian deposits during a period (between pre-Harappan and Harappan) when perhaps the main Indus flow followed the Nara channel.

We are still left with our 11.0 *metres* or so (feet have been used in the last few paragraphs because the reports use these units) of hard clay and occupational debris. We are also left with the same material outside the walls of Mohenjo-daro to the same depth below flood plain level.

What we have referred to as the WAPDA well is that described in the Aryne/Khan report which states "While drilling for a test well in January this year (i.e. 1973) ....when brick and pottery continues to be encountered to a depth of 37 ft.". The log of this borehole is shown as figure 5 of their report. The depth of brick and pottery agrees with that noted by us : being sited near the main entrance to the site the ground level is at approximately +156′.

ARCHAEOLOGICAL BACKGROUND

The archaeological background in so far as it is relevant to this paper is briefly as follows :—

The site of Mohenjo-daro has generally been regarded as culturally homogeneous in the sense that the remains belong to the mature stage of the Indus Civilization. Recent excavations, however, in Pakistan, and re-appraisals of earlier excavated materials, are demonstrating that the actual life history of the site is more complex. Material from great depth in NEDECO's New Site drilling confirms this view.

The answer to the difficult question of the origins of the Indus Civilization is much closer to realization as the result of these new researches. Whereas the archaeological evidence shows a remarkably close degree of cultural and economic inter-relationships with eastern Iran and Soviet Central Asia (Turkmenia) during the crucial formative period leading up to the mature Indus Civilization, there is now clear evidence that the major input into this formative process was provided by indigenous elements in the central and northern Indus valley itself. Recent discoveries at Kot Diji, Jalilpur, Sarai Khola, Gumla, and Kalibangan document this indigenous origin. They have also forced a re-appraisal of the history of Mohenjo-daro itself. Dr. Rafique Mughal, of the Pakistan Department of Archaeology, has been the leading scholar in the research.

Dr. Mughal has identified a cultural phase earlier than mature Harappan that can now be described as 'early Harappan'. This by itself not only makes necessary a reconsideration of the overall span of Mohenjo-daro as a continuously inhabited site but also of the enlarged time span within which 'normal' or catastrophic flood events would have affected the ecological setting. 'Normal' floods are here understood to be those, including extreme events, resulting from unimpeded run-off from the Indus catchment.

If we assume a beginning date of approximately 2400 B.C. for the mature phase of the civilization we must probably add on several centuries in order to arrive at the beginning of the important formative and/or 'early' phases of development.

As will be seen from our tentative reinterpretation of the soundings in general, in the section in which we discuss the NEDECO boreholes, the entirely new and potentially exciting possibility emerges of an earlier and distinct settlement. This is not only at a point about 2 miles away but is also stratigraphically separate from and lower than Mohenjo-daro. It is in fact separated by sterile sand material from clay which is in almost exact stratigraphic conformity with the mature Harappan material recovered from the HR soundings.

The ceramics recovered from the lowest parts of the tube-wells drilled by NEDECO in 1966 have been identified by Dr. Mughal as "early Harappan" : it is not completely clear whether this early material is limited to the New Site drillings, but it certainly seems to be so since the other holes do not have correspondingly low material. By early or pre-Harappan we understand cultural phases pre-dating Mature Harappan. All the material from our HR soundings in 1964-65, fragmentary as much of it was, is 'Mature Harappan' in this sense down to virgin soil.

DATING AND CHRONOLOGY

Much of the chronology and dating of the Indus Civilization has been pinned to a framework of radiocarbon dates. A reasonable compatibility was worked out over the years between these dates and the stratigraphic evidence obtained from new excavations. However, within the past five years an unexpected adjustment for *all* radiocarbon dates has been proposed. Correction factors, which increase the age of samples in the range of the Harappan period by 300 to 400 years, have been put forward by the University of Pennsylvania Museum Laboratory (called MASCA correction factors) and by others. They are derived from comparative analyses of radiocarbon and dendrochronological dates.

The authors accept the potential significance of these proposed correction factors but for the purpose of this paper the problem of calendar dates is not as crucial as that of relative dates and these remain substantially the same whether we accept the new "corrected" dates or not. All references to dates in this paper are to uncorrected dates and the use of specific dates such as 2350 B.C. should be regarded as "convenient" for the purposes of this paper and not as dogmatic statements.

THE NATURE OF THE EVIDENCE FOR INUNDATION

We have already referred to as facts (among those listed earlier) what we have described as still-water deposits at various levels of Mohenjo-daro but we admit that it is reasonable for anyone who cannot himself visit Mohenjo-daro and see the evidence for himself to ask for more proof of this. Proof, in the form of grain size analyses of the whole silt load of the Indus, based on samples taken over several years and at every stage (i.e. level) of discharge undoubtedly exists but is not available to us. G. L. Possehl [*op. cit.* (14)] wrote that the investigations carried out by us "tell us pitifully little about them" (the silt levels at Mohenjo-daro). He went on to write quoting us, with his emphasis added, that "about 150 occurrences of silty clay were selected that *seemed* to correspond with 'flood' levels (nearly all above the present flood plain level). Many (and he adds, 'how many?) of these were of deep sterile material that showed evidence of deposition under still water". It does not really matter how many such occurrences there were ; in fact all those which were of silty clay only, not cluttered with brick debris, showed this evidence. After all there was nothing particularly new in identifying flood levels since these had been referred to in the literature virtually from the first reports on Mohenjo-daro onwards. In fact, of course, he tacitly admits this by writing that the "real question here is whether all or *any* of the silts must be derived from *standing* water". This time the second emphasis is added by us. We agree with him.

So, let us for the moment leave aside the vexed question of these flood levels and silt levels and consider the facts and reasonable assumptions that we have already listed. Let us go even further and not even feel tied by the reasonable assumptions.

THE INUNDATION HYPOTHESIS RE-EXAMINED

We start from a "virgin" flood plain level at about +36.0 metres, some 11 or 12 metres below the flood plain level of today which is at +47.5 metres above m.s.l.

Let us assume that nothing between 2350 B.C. and 1975 A.D. (today) has occurred to interfere with the long term sedimentation regime so that, despite the estimates of the Indian Irrigation Department, we have to reckon with a rise of flood plain level of about 11.5 metres in 4325 years, or c. 3.7 m per 1000 years. This rate of rise cannot be considered impossible since the estimates made long ago by Sir Claude Inglis were either extrapolated from a short time base or were dependent on archaeological dates which were then even more open to doubt than they are now. But, accepting this rate of build up and following the logic of it the level of the flood plain at the end of the Indus Civilieation in 1750 B.C. would have been (after 600 years that is) +38.2 metres, about 2.2 metres above the virgin level or some 10 metres below the flood plain level of today. Allowing for some degree of increase in the sedimentation rate around the second/third millenium B.C. and even for a date for the end of the civilization of 1500 B.C. (with which few are likely to agree) the depth of siltation could conceivably have reached to 3.2 metres above the original virgin soil level (i.e. about 9 metres below the flood plain of to day or +39.2 m above m.s.l.). Let us go even further and assume that, without any extraneous factors the 'normal' rate of sedimentation had already built up to this level of +39.0 metres above m.s.l. by 1750 B.C., a more probable date for the end of the city and the civilization. This would involve a rate of build up of about 5 metres per 1000 years. So what do we have ? We have a flood plain at about +39.0 metres and a city whose highest parts (excluding the citadel) reach to +56.3 metres. Within this (raised) city archaeologists have identified from five to seven inundation levels with associated silts of which the only characteristic about which almost everyone agrees is that they were laid down horizontally by flood water. Nearly all are above +39.0 m.

Most archaeologists and most of the critics of the gradual inundation hypothesis have continued to talk of floods and their attendant silt deposits in the normal overspill sense. In other words they have visualised floods that have exceeded the capacity of the Indus bed and its natural levees and have consequently flowed over large areas (or indeed the whole) of the flood plain.

It cannot be emphasised too strongly that the general depth reached by flood water and the general depth of whatever silt is deposited by it are different by more than an order of magnitude. As a generalization the proportion of sediments deposited increases as the velocity of flow decreases and for any given gradient the mean velocity increases with depth. In fact it would increase with depth to such an extent as to reach fairly soon (probably within three to five metres mean depth) a general equilibrium between sediment deposit and scour ; and at greater depths (and velocities) scour would occur. At any really great depth—say eight metres or more—net sediment deposit would become highly improbable since scour would predominate. As a result deposition of silt occurs mainly in relatively small amounts and at shallow depths of flow, including the decreasing depth toward the end of a flood recession. It is almost impossible to imagine circumstances in which the *mean* depth of sedimentation could exceed ten centimetres (100 mm) or so and if it were possible most of this would occur in an irregular manner during the flood recession. Such a depth would be, in the nature of things, a rare event when one considers that the mean annual rate of deposition is about 2 mm (or, as we have assumed just now, about 3.7 mm).

Deposits of sediments of appreciable depth could only occur within the perimeter of Mohenjo-daro, at levels above that of the flood plain, if flood waters with their silt load entered into, and were trapped within, the higher parts of the city. In such a case for example a 2 metre depth of flooding (the top 2 metres of a much deeper flood) within a confined space could maybe deposit 20 mm of silt. This silt would not be matched by similar deposits outside the city walls and indeed would only be uniform over any area if the latter were effectively 'watertight'. Imagine for a moment the sort of flood required to deposit even 20 millimetres at a level (of trapped and deposited silt) about 10 metres above the then flood plain. The water level would have had to be at least 10+2 metres (=12) above the plain and the average depth of the flood over the 40 to 50 kilometres width of the flood plain could have been some 15 metres. The mean velocity of such a flood could have been of the order of 3 m/sec. and at that velocity scour would surely predominate.

There are three very strong arguments against there having been overspill floods of this magnitude (or those larger ones needed to account for the highest silt levels). The first is that a flood peak of this magnitude would imply a total discharge representing at least several tens of metres of *run-off* from a catchment that probably has an average precipitation of not more than 2 metres. Run-off, expressed thus, is the proportion of precipitation which drains away as surface flow after satisfying the demands of evapotranspiration and infiltration. It can never in the long term exceed, and seldom even approach, unity. The second is that there is no evidence of such floods at Harappa, whereas by far the greater part of the flood in the Indus and its Punjab tributaries derives from the Himalaya far upstream of Harappa. The third is that repeated floods of exceptional magnitude could not credibly be confined to the brief period between 2400 and 1750 B.C. : one vast flood event caused for example by release of water held behind a landslide in the mountains could just be credible but not a succession of such floods in that brief period. Even if it were conceded that floodings of unrealistic magnitude were already occurring before 2400 B.C. it is inconceivable that a people able to draw on the experience of Kotdijians and Amrians could have been foolish enough to build and re-build Mohenjo-daro (and the other Sind settlements) in the Indus flood plain.

Singh (*op. cit.*) postulates an extraordinary, and to us highly improbable, brief period of high rainfall in the Rajasthan desert. He even estimates the mean rainfall as having been 500 mm more than the present rainfall (which he gives as 250 mm). His argument applies only to the Rajasthan desert (and possibly Punjab ?) and cannot be extended automatically to Sind. The effect of Singh's postulated rainfall, of which the run-off would not drain to the Indus, (except for a small part draining to it via the Ghaggar) would not have increased the total Indus discharge by more than about one percent via the Ghaggar, and possibly by another ten percent via the mountain catchments. It must be emphasised that floods in the Indus and its Punjab tributaries are due in major part to the melting of snow which in turn is derived in large measure from non-monsoon sources. The view of one of the authors that the Ghagg captured the Yamuna (5) during the Harappan period would account for a further ten percent or so increase in total Indus discharge *and* a corresponding increase in flood peaks.

The various excavators of Mohenjo-daro have agreed unanimously on one point at least, and that is the conscious and determined efforts of the ancient inhabitants to protect their buildings against the destructive forces of, or inundation by, flood waters. These protective measures took two main forms : first, the construction of massive solid mud brick platforms within the city to raise artificially the living level above flood water. Secondly, there is evidence of the use of fired brick facings of the mud platforms as protection against the risk of erosional damage of structures by flood waters. Such platforms and facings have been recognised throughout the entire depth of excavated remains at the site. Of course if the view is taken that these "flood" deposits are not due to floods we must find another explanation not only for them but for the confidently identified flood protection works. If they are flood deposits, then their very thickness and the levels at which they stand in relation to the then flood plain call for some mechanism that must have caused deposition under still water. This statement depends on no assumption regarding the granulometry, but simply on the sheer depth of the deposits.

If a gradual engulfment model, involving some kind of damming of the flow, is accepted the relatively rapid build up of the whole flood plain does away with the necessity of floods of great magnitude and depth.

There are a lot of things however that we do not like about the impounding theory. We agree for instance that an uplift episode accompanied by earthquake shocks, as in the case of Allah Band, is difficult to fit in with the lack of evidence of earthquake damage at Mohenjo-daro : it could be, however, that the postulated uplift, if due to intrusive neogene muds, was gradual on the model of the steady mud extrusion along the Hala and Haro mountains to the west of Bela. We do not like very much the need to postulate a group of episodes isolated in time : in fact, however, the same kind of thing may well have happened both in the remoter past and during the very long period after 1750 BC. when the valley seems to have been abandoned. We are, as will be seen, uneasy about water balance considerations in the absence of knowledge about the pre-flooding valley geometry, the permeability of the dam, the effect of decreasing permeability, with increasing sedimentation and the area exposed to evapotranspiration. We share misgivings about the possibility of the people coming to terms with a periodically inundated country side but at the same time have equal misgivings about their ability to cope with floods of the magnitude we have been discussing, mainly because we believe that just one such flood would have destroyed both the city and its landscape. Most of these misgivings would be largely set at rest if it could be established that the Indus flow was divided above Sukkur, either naturally or with the aid of man-made structures, between the Indus proper and the Nara-Luni channel. Prior to the (natural) cutting of the Sukkur channel in the 12th century of our era the main flow followed the Nara and only flood overflows followed the Indus. With normal build up of sediments the tendency must surely have been for use of these channels to alternate, with the river following one for possibly several hundred years until raising of the bed in use made the other unused (or partly used) channel lower. It is possible therefore that, in Harappan times, either naturally (because the Nara bed and flood plain were lower) or artificially by means of some man-made diversion or cut, or by a combination of both, only a relatively small proportion of the total Indus flow followed the present flood plain. The idea has much to recommend it and we will revert to it later. All would have been well until the occurrence of whatever it was that dammed the Indus (overflow) channel or flattened its gradient and even when this occurred the amount of water to be disposed of by leakage, evapotranspiration and overflow would have been so much less as to ensure that there was seldom more than a very small depth of water (and that only seasonally) above the deposited silts. Of course the rate of build up of silts would have been slower.

MODIFIED HYPOTHESIS

In view of our misgivings which reflect some of the criticisms of the originally proposed hypothesis it is necessary to consider what modifications of the model are required. But first, and with great diffidence, we attempt to quantify the problem. First let us restate the problems involved in the siltation anomaly and then go on to quantify the water balance problem very approximately.

THE SILTATION ANOMALY

We can stretch credibility to breaking point and assume that the whole of the flood deposits, right up to the highest observed, occurred by normal siltation during the 600 years occupation of the city. It would involve a total build up at the rate of 33 metres per 1000 years —roughly sixteen times the rate estimated by the Indian Irrigation Department. It would be necessary to assume a very flat gradient: how flat would depend on the flood regime and the vegetational condition of the flood plain from the rock control at Sukkur downstream. Since velocity, if other factors remain unchanged, varies as the square root of the slope it would be necessary to assume about one quarter of the present gradient to halve the velocity. It is unlikely that this would result in sixteen times the rate of sedimentation, but let us assume for the moment that it would. A valley gradient of about 1 : 30,000 would be involved which, projected south of Mohenjo-daro and Sehwan would push the delta of the Indus something like 1000 km. out into the Arabian Sea. Since this obviously has not been the case it would be necessary to introduce some natural cataract-forming structure between Mohenjo-daro and the sea : a cataract involving a drop of some 40—50 metres. Or, alternatively, it would be necessary to postulate that the rock control at Sukkur was then about 50 metres lower, and has since risen by that amount. Neither the cataract nor uplift at Sukkur are any more inherently improbable than the postulated uplift at Sehwan. Any lower level 4000 years ago at Sukkur would have had a profound effect on the whole Indus system upstream, of which the effects would surely be visible at Harappa. However it is not only necessary to postulate a cataract but also, in order to arrive at the flood plain level of today, the subsequent elimination of it by erosional processes. This would involve a long period of headward erosion basically similar to that which would have eliminated the postulated natural dam at Sehwan.

There may be other possible explanations for a massive build up of silt during a few hundred years—such as differential uplift and subsidence on a regional scale, i.e. affecting virtually the whole of the Indus Valley. All explanations must involve a fundamental difference between the siltation regime of that period and of the periods preceding and following.

TENTATIVE WATER BALANCE CONSIDERATIONS

On the basis of the present day mean annual flow of the Indus—about 160×10^6 Acre feet $=2\times10^{11}$ cubic metres—it is possible to make a very rough calculation of how long would be required for complete siltation of the volume impounded behind the dam if we assume that this was about 45 metres high, 80 kms in transverse length and that the 'lake' extended upstream at its maximum to near Khairpur about 150 km. The original capacity of such a dam would have been in the order of 2.0×10^{11} cubic metres : it could have been anything between about 1×10^{11} and 3.0×10^{11}, depending on the original flood plain geometry. The mean silt load of the Indus in its original state, uncontrolled by barrages and the like, could have been in the order of 0.6%, giving a mean annual volume of 1.2×10^9 cubic metres. If it is assumed that virtually all suspended silt were intercepted by the dam the period required to fill it with silt would be between 80 and 200 years.

What is immediately evident from the above figures is that the volume estimated for the capacity of the 'dam' is approximately that of mean annual discharge. Losses by evaporation can be estimated to the same degree of accuracy or inaccuracy as the capacity of the dam, since they depend on much of the same geometry. At the maximum extent of the lake the evaporation losses would have been about 2.5×10^{10} cubic metres in a year, or about one eighth of the mean inflow. Leakage losses cannot be estimated without knowledge both of the post-"dam" aquifer geometry downstream of the dam and that of the "dam" itself ; and of course of the permeabilities, piezometric surfaces and so on. None of these can be known or discovered by research today except that perhaps a permeability $K=0.5\times10^{-3}$ metres per sec., that estimated for the underlying sand today, can be assumed.

On the face of it, it seems improbable that leakage could account, with evaporation, for the whole of the annual flow. For leakage to be effective enough to avoid deep accumulation of water, all assumptions would have to be maximised.

We have to try to find a mechanism that will account for interception of silt without any great seasonal accumulation of water over the silt. While it is possible that the uplifted sand acted as a rapid filter it cannot be assumed that it did.

If it did not rapidly dispose of the water, leaving silt behind, we would be left with a situation—in the case of a single uplift event—of very deep flooding under water, which is neither in accordance with the archaeological evidence nor remotely tenable as a hypothesis since the city would have been destroyed beyond any possibility of reconstruction by the first flood.

In the rough calculations made above, it has been assumed that the whole storage capacity of the dam, up to a level that would account for the highest levels of silt at Mohenjo-daro, was available in one episode. If instead there were several episodes of which the more recent in time corresponded with the postulated reconstructions of Mohenjo-daro (earlier events in the series having caused flooding that did not reach upstream as far as Mohenjo-daro) we have to allow for some considerable amount of spill flow over, or around the end of, the dam in order to achieve a balance between inflow and outflow. This would have had as a consequence a reduction in the amount of silt load intercepted and the process of siltation would therefore have required a somewhat longer time. How long could only at best be very roughly estimated. It requires a calculation of the routing (storage) effect on Indus flow which is not even worth carrying out as an academic exercise since the ranges of all the unknowns—rate of outflow, rate of leakage, evaporation losses, rate and timing of inflow, depth/area/capacity relationships of the dam, and the tolerable limits in time and depth of flooding—are so great individually and so interdependent that only a computer programme could handle the vast amount of trial and error involved. Let it suffice to say cautiously that the rate of outflow required could, in certain combinations of circumstances, still have permitted total siltation in something more than 200 years of which a great part would have affected Mohenjo-daro itself. The necessary circumstances would be a more or less regular and continuous uplift interspersed with episodic movements of greater extent perhaps corresponding with the archaeologically recorded periods of reconstruction and platform building.

But for this to be feasible it is necessary to consider what might have been the conditions of overspill. In the case of Allah Bund the sudden rise of a few metres affected a width of several kilometres, giving rise to an increased downstream slope : in that case the downstream slope was described as *abrupt* and the upstream slope practically *undiscernible*. Thus abrupt downstream slope undoubtedly contributed to the rapid breaching of the bund. If the abrupt slope had been on the upstream side the downstream slope could have been in the order of 1 in 3000 and the breaching of the bund would have taken much longer. In the postulated events at or near Sehwan there could have been a similar increase of downstream slope from about 1 : 3500 to 1 : 1750. This is still a gradient within the range of those of existing alluvial rivers (e.g. Tigris) so that outflow by a single or multiple channel (more likely the latter) would not have had to give rise to a vast erosive rush of water. In other words, it would be possible to have annual outflow without significant headward erosion during each period of flow. Some headward erosion would undoubtedly have occurred. It is worth noting that the velocity in a single outlet channel at this gradient and (say) 5 metres depth of water would have been in the order of two metres per second, which is quite high but not necessarily disastrous on what would have been a sandy bed : the 5 metres depth and relatively high associated velocity would have been of, at most, a few months duration in each year.

This solution fits the archaeological data, including the absence of earthquake evidence, and requires only annual inundation for a short period but it does require the equivalent of one very large spill channel to evacuate the balance not lost by evapotranspiration and leakage. It has to be admitted that this solution depends on several nicely synchronised assumptions to be readily acceptable.

Again in the rough calculations made so far it has been assumed that the annual discharge of the Indus then was the same as that of today. Apart from the fact that this does not take account of a roughly 10 percent increase that would have derived from the Ghaggar, it assumes that the whole Indus flow followed the present Indus channel. We have already mentioned that before about the 12th century of our era the main flow of the Indus was down the Nara channel. There seems to be no inherently good reason to suppose that the river did not divide—either naturally or through the intervention of the Harappans—between Indus and Nara channels. If only flood surpluses flowed in Harappan times down the present Indus channel or vice versa the whole question of the balance of inflow and outflow is much simplified.

The end of this group of Sind settlements would have occurred when the river diverted entirely to the Indus, despite whatever efforts to stop it doing so were within the Harappan's capacity. The process of rejuvenation of the Indus flood plain would have started at this time.

Three hypothetical models have been examined in this paper.

... A "dam" caused by uplift, probably in episodic movements, capable of maintaining an approximate balance between annual inflow, and outflow as evapotranspiration and leakage. This requires very high leakage.

... A "dam" caused by gradual uplift interspersed with episodic upward movements of greater magnitude with whatever river outflow was necessary to maintain the inflow/outflow balance. This probably involves too long a period for siltation and requires too many coincidences.

... A "dam" on either the first or second model across the present Indus channel which at that time had to accommodate only a part of the discharge, the rest following the Nara branch. This is undoubtedly the preferred hypothesis.

We therefore continue to favour the model involving gradual inundation behind a transverse area of uplift located somewhere near Sehwan. What we are attempting to do is to explain the great depths of anomalous clay. There seems to be little dissent about floods and much dissent about the nature and causes of them.

The flood or rather the gradual inundation hypothesis has been criticised on the grounds that flooding of the kind postulated would have made it impossible to dispose of sewage and that malaria and water-borne diseases would have been rife. The argument would be a good one if it were not that it misses the essential point that there were indeed floods. Everyone agrees that there were floods, most believe that they played their part in weakening the civilization : there is not even much disagreement about the catastrophic (and therefore in a flood plain situation enormously wide-spread) nature of the floods. There is plenty of disagreement about what caused the floods and it seems pertinent to point out that, whatever the cause, floods that made necessary the frequent rebuilding and raising of the city would have created conditions in which disposal of domestic wastes would have been always difficult and seasonally impossible : conditions too, in which malaria on the one hand and water-borne diseases on the other would have flourished.

The following final notes on the archaeological context for our updated views on the Mohenjo-daro are mainly contributed by G. F. Dales.

LATE HARAPPAN OR POST-HARAPPAN

The hypothesis concerning the role of geological factors in the decline and demise of Mohenjo-daro is not merely of scientific importance and interest. It must be linked with the much more important question of what was responsible for the decline of the *entire* Indus Civilization and the virtual disappearance of the first attempt at urbanized civilization east of Mesopotamia.

The most widely accepted theory in the past few years has been that invaders speaking an Indo-European language (Aryans) were responsible for the destruction of the Harappan capital cities and ultimately of the Indus Civilization itself. More recent work, such as our own at Mohenjo-daro and along the Makran coast, has injected a new element—that of geological factors—that provides an alternative set of explanations for this phenomenon. The two explanations are not of course mutually exclusive. Even if these geological factors are accepted, there remains the cultural question of what really happened to the institutions, traditions and stylistic preferences that characterised the mature Harappan period. Very recent excavations by Dr. Mughal, of the Pakistan Department of Archaeology, at Jhukar near Mohenjo-daro have provided evidence to substantiate the theory that the so-called Jhukar was, in fact, representative of the *late*, decadent Harappan civilization. This in itself, whether accepted or not, *is* related to the subject of this paper to the degree to which it demonstrates a rapid or gradual decline at the end of the Harappan period. Certainly there was a late "squatter" type of occupation of at least part of Mohenjo-daro to judge by the architectural remains, but it remains to be explained whether "late and decadent" Harappans or other as yet unidentified peoples were involved. It is relevant to this argument to stress that the rejuvenation of the valley, which would have resulted from the whole Indus flow following the present Indus Valley, would have made impossible while it lasted an irrigation-based economy. During the whole rejuvenation period there could have been no gallery forest so that burnt bricks would have become an expensive luxury.

KULLI-NAL-HARAPPAN

The relative chronology and the degree of economic cultural inter-relationships among the various groups of the early Indus Valley and Baluchistan has been a point of interest and debate for decades. The most recent evidence from the southern part of the Indus Valley and Southern Baluchistan clarifies part of the problem—even if it does not explain it in cultural terms. First, the so-called Kulli culture, with its typological and stylistic affinities extending to the Persian Gulf, was contemporaneous with, and closely related to, the mature phase of the Indus Civilization. The French Excavations at Nindowari and the current American excavations at Balakot provide supporting evidence for this interpretation.

Second, the relative chronological position of the so-called Nal culture of Southern Baluchistan has been fixed by the Balakot excavations. Typical Nal polychrome pottery dominates the more than 9 metres of deposit beneath the mature Harappan occupation levels at Balakot that include Kulli material. The precise time range represented by this thick accumulation is as yet uncertain, but preliminary radiocarbon dates from the site suggest that this Nal-related occupation may date to as early as 3,000 BC. Continued excavations at Balakot should clarify this problem. In the context of this paper the relevance of this is in the indirect support it gives to separating a pre-Harappan or formative phase from the mature phase. It was, we believe, only this mature phase which suffered gradual engulfment in mud.

Balakot, some 55 miles north of Karachi, is the only Pakistani coastal site being subjected to extensive excavation. In 1973 the University of California, Berkeley, began annual excavations at the site. During the 1975 season the focus will be on geographical and geological questions relating to the present position of the site and its probable ancient position in relation to the ocean coast. It is hoped that information from Balakot will help to throw light on geological phenomena that may have some bearing on the question of the regional role of natural forces in the life and times of the Harappan Civilization.

As a final footnote it is worth considering the actual day-to-day or year-to-year effect on the inhabitants of Mohenjo-daro of our preferred hypothesis : that is, gradual uplift near Sehwan affecting a flood channel of the Indus with the depth of flooding kept generally within tolerable limits by evaporation, infiltration and overflow. The inhabitants would have become aware of the beginning of lake formation through travellers' tales from down-stream. They would have had many years to accustom themselves to the gradual upstream spread of this lake until it reached their city. When this happened they would have had annual inundation differing only from the Indus annual flood in being of still water with very heavy silt deposition. The deposited silts would not have precluded cultivation though they would have made a mess of irrigation canals and would have involved considerable modification of irrigation practices. But during the life of one farmer the land might build up only 2 metres so, while our theory has been described as cataclysmic, it would not necessarily have seemed so to an inhabitant.

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