Mr R. L. Raikes
Vine Tree Cottage
Llanvetherine
Abergavenny
Gwent NP7 8RD
UK

Robert L. Raikes

The Climate and Hydrological Background to the Post Glacial Introduction of Farming in the Middle East and its Subsequent Spread, With Examples from Jordan

Professor Baly's preliminary thoughts in his position paper make a useful, if perhaps sometimes rather unnecessarily philosophical, introduction to those who are comparatively new to the physical environments of Jordan. They should also alert those who, believing themselves already familiar, tend to see everything in terms of those of its physical environments which they have experienced (albeit often only briefly and seasonally); and to those many who tend to see these environments, or some of them, with the tunnel vision of their particular discipline.

For Jordan enjoys or suffers a large range of perceived environments and it is very tempting to believe that in this respect it is typical of the area vaguely known as the Middle-East. This area stretching from the Eastern Mediterranean to beyond the River Indus is too vast and varied for it to be possible to say that any part of it is typical. This is certainly true of Jordan where practically the only characters that it shares with the rest of the area is that it almost certainly houses some of the earliest attempts at settled farming and that it encompasses the tenuous frontier between the desert and the sown.

I am not sure that I follow Professor Baly's distinctions between actual environments and perceived ones as classifiable entities: naturally I would agree that for each observer the distinction exists; and I would further argue that the more scientific and objective the observer, the smaller the distinction should be. I say 'should be' advisedly because in practice, the scientific and objective observer tends to be observant only in his chosen field. This is why the history of archaeology is marked by so many statements—barely even questionable in the context of the pure discipline concerned— that turn out to be nonsense (in the sense of outrage of common sense) when made, often dogmatically, about other seemingly related matters. This is one reason why those archaeologists, botanists, climatologists and so on, who are capable of critical thought frequently find it impossible to agree with their specialist colleagues: the other possibly more frequent reason is that the specialists concerned often have very little, if any, idea of what their specialist colleagues are talking about.

Unfortunately, the educational philosophy of today with its tendency to value knowledge above understanding fails signally to produce polymaths—those intellectual equivalents of Decathlon athletes—while it also fails to give encouragement to those who are fortunate enough to be polymaths by nature. Not so long ago, the polymath, admittedly poorly served by really sound grounding in all he had to profess but nonetheless performing near miracles (Sir Aurel Stein leaps to mind), reigned supreme. In half a century his understanding has unhappily been submerged by myriads of specialists bulging with the latest in knowledge so that his place has perforce been taken by the equivalent of the Committee Chairman. Now I detect a trend towards the polymath once more which I hope will be realised. In this intervening period, catalogues of more or less relevant statistics have been unhappily stitched together by dig directors of varying breadths of vision and with varying success and no harm has been done because by the grace of God, Pitt-Rivers and Sir Mortimer Wheeler, the basic raw material has been properly and three-dimensionally recorded.

I started by thinking that I was applying some knowledge of the behaviour of water and climate to the solution of a small part of some archaeological problems. As I have grown older, I have realised that my contributions, mostly highly contentious, have owed progressively less to my specialised knowledge such as it is. They have owed at least as much to two important beliefs: first that a knowledge of water *per se* is pretty useless unless it is intimately connected with farming and land use (and misuse) which both widen its scope and give it purpose; second, that practical engineers tend to be fairly pragmatic, common sense sort of chaps and I had the big advantage of a lot of years of pragmatic engineering before I got interested in water. So I believe that what I have contributed owes as much to informed common sense as to anything else.

Professor Baly defines the perceived environment as, in effect, what is both perceived and interpreted by humans living within that environment. Unfortunately tunnel-visioned specialists in countries where few objective data exist tend to accept, *faute de mieux*, the interpretations expressed, often

dogmatically, as personal opinions by the more vociferous of the fallible and non-expert humans living within the environment. This is certainly true of my specialisations in climate and hydrology and not just in Jordan: it is, I think, equally true of the discipline of agronomy (a sort of hybrid of soils and botany) and zoology (including ornithology) and most of all perhaps of sociology where weird half-digested flights of imagination are often recorded as facts by specialists who neither speak arabic, or whatever be the language, nor understand life in the desert (or semi-desert or Savannah or whatever).

Most hydrologists would, I am sure, be able to quote examples of statements made by peasant cultivators (and even sometimes educated farmers) that there has been no rain for such and such a period. The period tends to be years in the case of desert and semi-desert countries and as little as months in northern Europe. There were many such statements made about England in the summer of 1976 and they are the norm in all dry countries: they are of course complete nonsense and are invariably contradicted by factual observation. But here again, it must be emphasised that observations can be far from factual and that no series of data should be trusted unless it is obvious at a glance that a highly variable record at one place is closely matched by several other adjacent yearly series; or unless one of the various quality control checks has been applied.

When I first came to Jordan in 1961 to help set up or reset up the hydrological survey, I made it one of my first tasks to apply a double mass plot (not itself infallible) quality control to rainfall data (using initially only stations with continuous records of 20 years). The plots revealed some fairly startling things. At Irbid for instance there were two rain gauges, one at the primary school and the other at the secondary school. The plot for the former showed a nice straight line while that of the latter went all over the place and the record had to be discarded for practical purposes. I have always believed that the headmaster of the primary school, with very young pupils, made the observations himself (and with reasonable accuracy) while the other headmaster either succumbed to the temptation of having boys old enough to make the readings for him or the boys were just tall enough and mischievous enough to urinate in the gauge. This of course does not happen only in Jordan!

Hydrology has, even in the relatively few years since I first came to Jordan, become a very highly sophisticated and complicated affair. But, despite many advances and many approaches that contain some degree of built-in checks and balances, and despite a growing (if not already universal) tendency to regard all hydrological interpretations as at best more or less accurate assessments of probability: despite, I insist, all this, even the assessment of probabilities cannot be done in vacuo. Some, as much as possible, data are essential and a great big dose of commonsense. Jordan has been very fortunate in this and probably had until 1967 one of the best Hydrological surveys in the Middle East. What is left is, I'm told, still very good.

I have spent rather a long time on this because it must be obvious that talk about constancy or change of climate is absolutely meaningless unless the present climate is reasonably well known. If we say, and I do not, that the climate has changed, we must know what it has changed from: if we say, and I do, that the climate with all its vagaries and fluctuations has not changed, we must know what its present norms, variations and fluctuations are.

And this leads me to the central point of my paper. I am one of those (now perhaps rather more than half of those concerned with climate change, whereas I used to be almost a lone voice) who believe that the general characteristics of our global, or certainly northern hemispherical, climate were already recognisably those of today (with all that this implies in the way of short-term fluctuations) as soon as the areal extent of permanent glaciation had shrunk to about its present limits. I believe that this occurred at about 8,000 calendar years BC. It was probably several millennia later, when ice-melt had achieved the present eustatic balance between ocean and ice storage, that all the minor anomalies disappeared. The anomaly that has most affected European holocene climatic thinking was that caused by the conversion of the North Sea from a land plain with a relatively continental climate to a sea with a most emphatic maritime climate. Most continental shelf areas whose depth is now less than around 100 metres would have shown similar effects and climate anomalies. I do not believe that any of this had any significant effect on the Middle East; not even the hypothesis that the Caspian Sea (and Volga) probably drained across to the Black Sea and the Mediterranean.

There are many arguments in favour of overall constancy of holocene climate over the fertile crescent from 8000 BC onwards. This is not primarily a lecture on climate change so for detailed arguments I can only refer the audience to the bibliography with one proviso: where I previously estimated 7000 BC, I now put the date about a millennium earlier. However, a brief digression about the climate may save searching for difficult references.

Since some of you may not be familiar with the climate change controversy, I will try briefly to describe how I see the current state of play. To some extent the controversy is a semantic one depending on the meaning that should be accorded to these two words. The original proponents of Holocene changes posited changes of the duration of several hundred years or even as much as several thousand years. They gave a series of names: Pre-Boreal, Boreal, Atlantic, Sub-Boreal, Sub-Atlantic with the corresponding dates being—pre 7900 BC; 7900 BC to 6200 BC; 6200 BC to 3500 BC; 3500 BC to 500 BC; and 500 BC to the present. The climate divisions and the names given to them were first propounded for areas around what is now the North Sea (later apparent parallels were found in areas of North America and, I believe, elsewhere but almost invariably in coastal areas where coast lines have undoubtedly changed, often by several hundred miles, owing to eustatic sea-level

rise). The climate phases were based on such matter as varve counts, particularly in Scandinavia, on pollen analysis and on macro-botanic evidence and so had an ecological basis. At the most they were applied to minute areas of the earth's surface but regrettably were sometimes extrapolated world-wide (or at least hemisphere-wide). This process can be reasonably compared to that used by early archaeological restorers of the murals at Knossos. At Knossos a whole large panel was 'reconstructed' on the basis of a tiny percentage of original mural with results that have now been rightly rejected by serious archaeologists but of which the damage, in the form of totally false impressions of the Minoan scene, lives on in popular imagination.

Several fundamental points were overlooked by the proponents of world wide holocene climate phases or indeed of even local changes. To start off with, pollen analysis can by no stretch of the imagination be called a precise instrument: vegetation does not depend mainly or entirely on rainfall and even less on fairly minor long-term temperature fluctuations; it does depend on soil moisture which in turn depends on drainage, on the soil pH (which again can be enormously affected by drainage and by gradual leaching of certain minerals) on erosion, fire clearance by man in aid of the chase (and later of agriculture or the creation of grazing areas). Drainage can be affected by purely chance occurrences such as accumulation of fallen trunks and even by the work of beavers: it can be even more fundamentally affected by changes in drainage datum caused by changing sea-level, which was in operation from about 8000 BC to around 4000 BC; to accompanying isostatic land level changes not to mention tectonic changes associated with plate-tectonics. With all this plethora of possible causes, the matter for marvel is that on the whole the original connection between pollen analysis and climate (which in most cases meant rainfall and temperature) gave approximately correct results in those limited areas where it was first used. But to argue, from observed changes around the North Sea which, from being a vast dry plain, gradually became the sea we know (with inevitable local changes from relatively continental conditions to very definitely maritime ones) that this minute local effect was reflected over areas thousands of kilometres distant, is not valid.

This local evidence is not of course always taken by itself and indeed 'correlations' of a kind have been found (with date discrepancies of several thousand years) in the North Africa deserts. Here there is quite good evidence of former shallow lakes now buried under drifting sand. The easiest explanation may appear to be climate change but if this were the explanation, two requirements have to be satisfactorily fulfilled: first, that these 'wet' periods of alleged climatic origin, part of a world-wide climatic continuum, were reflected in all other northern hemisphere desert areas; and second, that no other local explanation can be found. The first condition is not fulfilled. The second, other local causes, allows several feasible alternatives of which the one I prefer is gradual lowering of a zonal phreatic water-body through lack of sustained recharge, through continual evaporation, through local over-use and through disastrous erosion around lake margins producing dust and sand that have obliterated practically all evidence of the lakes. This view has the support of a leading hydrogeologist[1].

With evidence for long duration climate change doubtful, to say the least of it, what is the evidence for lack of such change? And here I must return to semantics. To me, climate change involves such magnitude of changes in climatic parameters—rainfall, temperatures, lengths of seasons, wind direction and strength—persisting for so long that a significant change in world climate circulation must be involved. This again presupposes major and long-lasting changes in incoming energy from the sun and the sort of lasting cosmic change that could account for it: there seems to be little, if any, support from astonomers for this.

I prefer to distinguish between short-term climatic fluctuations (maximum duration of any one episode of the order of 30–50 years), which are demonstrably with us all over the world, and climate change. It is important to remember that in sensitive areas such as the arid and semi-arid zones, the effect of such fluctuations can be so disastrous as to simulate thousands of years of change.

Probably the strongest argument for fluctuation, within ecologically tolerable limits, for the sort of things we are interested in, such as cereals, is that the same cereals have continued to grow in the same limited areas. Cereals are heavy grains and even before husking they are not easily windborne large distances up steep slopes: conversely they are fairly easily washed down. Those that somehow get blown or carried uphill, may establish themselves in competition with other grasses and herbs, better adapted than they to more soil moisture (until the next heavier than usual rain washes their fruits back to where they came from). Those that get washed downhill find themselves in too dry conditions. Some up and down movement there must have been, but never of such wholesale proportions as to eliminate them from this particular niche.

The other strong argument for overall constancy is the sub-tropical circulation which transfers unstable warm humid tropical air upward and poleward while hot dry desert air (or, at certain seasons, humid monsoon air) is drawn in to take its place. The hot dry air is in turn replaced by a mixture of sub-tropical air—The North East Trades in our hemisphere—and a re-descent of that tropical humid air. This, by the time it reaches the ground, has undergone more or less adiabatic warming and so the cycle continues. It is difficult, if not downright impossible, to imagine circumstances which can create more than minor dents in this vast system: sometimes cold polar air (polar outbreaks) reaches as far as the tropics and may give rise to intense frontal or cyclonic activity but the system goes on. The zone along which ascending unstable air

[1] Mr W. Barber: personal communication.

draws in hot dry trade wind air from the north, and low level humid (monsoon) air from the south is known as the Intertropical Convergence Zone (ITCZ). It is a continuous band around the world, moving south and south to follow the apparent movement of the sun: it is a remarkably constant entity and even more remarkably variable in the range of its movement.

It does not really concern us here in Jordan since the nearest place which it reaches regularly is about Medina. But we are concerned with the equally permanent, integrally related and equally varied circumpolar circulation—part of the same global system—which brings winter rain and summer drought to the Middle East.

In two—perhaps more—areas the extreme southward deflection of circumpolar depressions, and their associated fronts, overlaps the extreme northward deflection of the ITCZ. They are in Saudi/Yemen and in Baluchistan. The latter is important because the winter rain would make it—with very little increase—a potential cereal cultigen habitat. It now seems possible that, perhaps as late as 7000 BC, lingering ice accumulation on the Tibetan plateau formed a blocking high pressure area that could have deflected Mediterranean depression paths farther south. This could have eliminated the bi-modal rain which has helped the flowering of Neolithic and Chalcolithic cultures there: with winter-only rain the hills of Baluchistan could have formed an eastward extension of the wild cereal habitat. Archaeological evidence is now coming to light supporting this view.

Jordan, in this and any other context, cannot be reconsidered in isolation. A glance at the FAO/UNESCO vegetational map of the Mediterranean brings this out very clearly indeed. In fact, Jordan and Palestine are at the extreme westerly or southwesterly end of what has reasonably been called the Fertile Crescent. The first steps in farming, for reasons which I hope to make clear, were taken at various points along the fertile crescent and in similar areas (e.g. the Anatolian plateau) adjacent to it. The area having potential for primitive dry farming, that of the cultigens of cereals and pulses, becomes very narrow indeed in Jordan and it is not surprising that most of the evidence for first farming steps here tends to be circumstantial while little of the direct evidence has been tested by the spade.

But I don't think this really matters: it doesn't matter whether Çatal Huyuk led the way or Jarmo or Karim Shahr or Beidha or Jericho. What I think may emerge from the disparities of dates, which may be more apparent than real, and from the physical lacunae between suitable sites, is that centres of incipient farming were initially independent although all within a unique matrix of shifting nomadic peoples of whom they were indeed an integral part.

The physical lacunae existed and exist because of the extraordinarily fragmented nature of the vegetational zones all around the fertile crescent. I do not know of and have not been able to find, any information pointing to which of the zones shown on the FAO/UNESCO map were those where dense harvestable stands of cereal cultigens would be most likely to be found. The cereals are supposed to thrive on relatively steep, stony hillsides at around 800 m.a.m.s.l where summers are markedly dry and winters cool to cold. These probably fall within the zones indicated as having 'climats froids a étés secs et tempérés' not very felicitously translated as 'Dry summers cold and temperate climates': they may fall within certain of those zones described as 'Mediterranean and attenuated sub-desert climates'. Many of the sub-divisions in both general classes come under the general descriptions of short-grass steppe, shrub and tree pseudo steppes and woodland, grass steppes or pistacchio, almond or juniper tree steppes, merging into mediterranean evergreen oak formations. Several of these, particularly those with pistacchio, juniper and short grass woodland would seem likely candidates, other things such as the rainfall/soil combination and altitude, rainfall and soil moisture, they are scattered in very were to assume that all these sub-divisions were suitable for cereal and other cultigens and satisfied the requirements of altitude and rainfall soil moistures, they are scattered in very small areas and I feel sure that the areas really worth regular annual visits around harvest time would have been few and far between. And do not forget that not only man the hunter–gatherer was interested: competition with birds and grazing animals would have been fierce. Some areas may have been so rewarding that their exploiters each May or June would surely have rapidly come to consider them as their family group, or tribal, property; and as such requiring protection against competition—perhaps nothing more than a very simple primitive encampment.

I believe that where this happened, a surplus would be created in most years and with this would be born the idea of storage—in baskets or skins first and soon in rough pottery. Not all these sources of abundant wild grain would have had a convenient source of water but this would not have imposed a serious constraint. Even today, and in areas of higher rainfall, the inhabitants are prepared to go long distances for reliable drinking water—15–20 km. is not considered excessive.

Now a water supply in the form of a small perennial stream does not become something to guard until population pressures are considerable, so I would expect the temporary encampment to be situated right where the wild food grew abundantly, with drinking water fetched as necessary mainly during the winter growing season; and likewise, the primitive storage would be right there.

I do not believe primitive man was unaware of the facts of vegetational reproduction, he saw it going on around him all the time, so I can see the first steps in sowing as having taken place around the relatively cleared periphery of the camp by those who stayed to guard the possessions. Obviously, some trial and error as regards time of sowing and an empirical choice of soils and probably quite a long period of false starts (premature germination before sufficient rain to sustain the crop). This is still a recurring hazard. The cultigens would run

the same risk, but the family or individual with the sense and strength of character to hold off sowing until rain seemed assured were the first proper farmers.

Each group probably had a different range of cereal and pulse cultigens to start off with so I would expect some difference right from the outset. However, word could barely fail to get around that certain groups had started proper dry-farming and the idea would spread by purchase, barter, theft and so on until most ground suitable for dry-farming was in fact farmed. Since man does not seem to have been particularly numerous, there would have been plenty of land along the right isohyetal strip. One can only guess what happened when soil fertility became exhausted or reduced. It would probably not have happened to the cultigens on their stony slopes constantly replenished by fresh soil eroded down slope, and may not have happened on adjacent flatter areas benefitting from some annual on-wash of colluvial material. The picture I am trying to build is however not concerned at all with the techniques of farming but very much as to whether it originated in one or more places. The multiple origin one appeals to me.

Against the foregoing general description, it is obvious that in Jordan only a limited number of known sites can qualify as farming nuclei. Tell Ghrubba, which I have not seen and of which the greater part appears to be buried under silt from a river draining to the Ghor, almost certainly qualifies. The tiny site which I found in Wadi Kufrein[2] almost certainly does. But Jericho and Beidha do not, and represent instead a very early advance, through the successful growing of grain away from its natural habitat. In the case of Jericho, the original step was probably taken in the Judaean hills some 15 km. away and the grains found at the large settlement are almost certainly evidence of very early primitive irrigation either from ʿAin es Sultan or along the Zhor. In the case of Beidha[3], the most likely site of the original domestication must be sought on the slope between Sobat and Beidha's source of drinking water.

For millennia the matrix of hunter–gatherers seems to have done little beyond act as a vector of the idea of cultivation. In the case of the recently discovered site of Mehr Garh[4] in Baluchistan, it is difficult to believe that this vector worked over such enormous and inhospitable distances. A local increase (or rather delayed decrease) in Baluchistan's rainfall in Jhalawan, explicable in terms of lingering perennial ice on the Tibetan plateau, could have provided a habitat for the cultigens in Jhalawan where I personally have found three tiny settlement mounds with an entirely microlithic industry. Baluchistan differs from much of the Middle East in having lowlands utterly unsuitable for wheat or barley cultivation except in winter and then only with full irrigation. There is a growing case for the belief that early transhumance (mainly for livestock) enabled cereals to be grown under irrigation at Mehr Garh in around 7000 BC. At Mehr Garh there is an unbroken sequence through to the chalcolithic with the development of extraordinarily fine and beautiful ceramics free of Iranian influences until probably about fifth millennium BC.

But basically, what happened there probably happened (and awaits proof through discovery of missing link-sites in the chain) at Jericho, Beidha, Çatal Huyuk and the Jarmo group. Each of them produced the same transhumance response (over very varying distances) a first stimulus to irrigation from a spring or river and, one gets the impression, distinctive ceramics: Haçilar ware ex Çatal Huyuk was destined to be found later—not very much modified—in Greece and Italy; early Jordan ceramics ex Jericho; the distinctive Samarran ware perhaps from the Zagros foothills.

In my synopsis, I wrote that farming spread downhill (which in the whole Middle East means into areas of less rainfall) where it developed using gradually more sophisticated techniques of irrigation culminating in the vast 'hydraulic' civilisations of protohistory (Sumer, Nile, Indus etc). In fact I believe that the idea of irrigation was sometimes born practically at the same moment as the idea of cultivation. I also wrote that farming spread uphill, using various adaptive measures, to wetter (rainier) conditions. In practice, the opportunities for doing this were very limited and the stone tools necessary for clearing heavy vegetation, including trees, were inadequate and uneconomical. There may have been some slash and burn (bush fallow) but I would suspect that absence of demographic pressure on the land and the survival of the chase, not to mention the need for timber and for feeding hogs and the like, discouraged an upward spread. It seems much more likely that the original dry-farming cum irrigation was still practised by those who, under some degree of population pressure, ventured across the Bosphorus, or island-hopped through the Aegean, to found the first neolithic farming sites in Europe. It is surely no coincidence that Nea Nicomedia in Greece is in a semi-arid area while the earliest neolithic sites in Italy (dating to the beginning of the 6th millennium BC) are found around Taranto, the Tavoliere and the Ofanto valley, the only areas of Italy where the climate approaches that of the fertile crescent. By the time that northern Greece was colonised, the Chalcolithic (= stone plus copper) level of tool technology was about to blossom into the extraordinary development of bronze which enabled agriculture to move north into the Danube valley and thence across Europe.

This represented not merely a tool technology break through. Almost more importantly effective means of storing grain, dry and proof against premature germination, made it possible to choose sowing times such that, even in climates with mean rainfall far higher than that required by the original cultigens (and by now, their cultivars) the grain (or pulse) was only in the ground long enough to make use of

[2] Sites in Wadi Shu'eib and Kufrein; Jordan: Robert Raikes, *Palestine Exploration Quarterly*, July–December 1965.

[3] *Beidha, Prehistoric Climate and Water Supply* (Appendix C), a preliminary report by R. Raikes (Appendix to main Report by D. Kirkbride).

[4] Preliminary Report by Jean-François Jarrige; *mission archéoloqique au Pakistan*, about 1981.

roughly its gross 'crop water requirement'—around 400 mm.

Research stations did not exist in prehistory—or at least there is no evidence of them—but farmers quickly selected strains and hybrids that would stand up to the new range of pests—insects, fungi (e.g. rusts) and so on—to which the first-introduced cultigens and cultivars must have been happy hunting grounds for whole pest populations struggling to survive on largely immune or resistant hosts.

Practically, all our Western European staples and certainly all those that pre-dated the discovery of the New World have come, via patient adaptation and selection, from ancestors that grew wild and were first cultivated in Jordan and its neighbouring countries.

To end on a rather sombre note, it could be said that the whole idea of possession of land, culminating as it has in wars of every kind, marked both man's first step in civilisation and the beginnings of all our territorial problems.

Suggested Reading:

Note: Many of the following Titles contradict my view wholly or in part, they are recommended in order to give the reader a chance of a balanced view.

Arid Lands in Perspective: Ed. McGinnies and Goldman for American Association for Advancement of Science. University of Arizona.

Dating the Past: F. E. Zeuner, 4th edition 1964, Methuen.

Environment and Archaeology: K. Butzer, 1964, Methuen.

Changes of Climate: proceedings of Rome Symposium 1963, UNESCO/WMO.

Climate: Past, Present and future, Vol. I; H. H. Lamb 1972, Methuen.

Courses towards Urban Life: Ed. Braidwood and Willey, 1962. University of Edinburgh.

History of the British Flora: H. Godwin, 1956, Cambridge.

Plants and Archaeology: E. W. Dimbleby, 1967. Baker.

The Domestication and Exploitation of Plants and Animals Ed. Ucko and Dimbleby, 1969, Duckworth.

Water, Weather and Prehistory: Robert Raikes, 1967, Baker.

World Climate from 8000 to 0 BC: proceedings of International Symposium, Imperial College, 1966. Royal Meteorological Society.