

The Ghassulian Lacuna at Jericho

A communication for the Acta of this Conference was kindly requested from our Institute as initial sponsor of the Ghassul excavation in Jordan. My title was judged suitable to focus the major problem left unsolved even after further campaigns at the site by the British School. Our data on this subject, piled up over a long time, seemed likely to support and supplement Professor Hennessy's results as known from his only preliminary report¹. Meanwhile his more advanced oral report at this Conference showed that he now sees himself forced to move back the universally-accepted Ghassul dating by some two thousand years, well into the horizon of neolithic Jericho. We may nevertheless hope that this compilation of previous Jericho-Ghassul synchronisms will provide a still useful background to Hennessy's forthcoming final report and to the awaited lively reactions.

The absolute dates now for Ghassul were concretely numbered by Hennessy only as 4600–3600 BC. As he explained, these are based wholly on radiocarbon tests, uncalibrated, which even in that form seemed so implausible that for a long time he was tempted simply to overlook them. Eventually they became so cumulative that he was forced to re-interpret the pottery and other artifacts in this framework, and it all turned out to his satisfaction. However, he also made clear that to be utilized these uncalibrated dates must be put back roughly one thousand years earlier². Thus the lacuna of our title would assume gigantic proportions, at Ghassul as well as Jericho: nearly two thousand years are left unaccounted for in the overall chronology of both sides of the Jordan, or at the very least from 4600 to 3200.

It was pointed out by Jean Perrot at this Conference that the real issue involved is Egyptological. Along with Ghassulian the Egyptian Protodynastic dates would have to be pushed back in some way to fill that void of more than a thousand years. Perrot expressed himself ready to take this in his stride if the Egyptologists were too. But in his further

exchange with Hennessy, Perrot admitted candidly that we have here just another proof that radiocarbon dates are still too capricious to be granted a veto power over all other sources of archaeological information. While respecting this latter opinion of Perrot, we are by no means adopting it here. On the contrary, our presumption will be that radiocarbon dates, duly calibrated, are a real scientific datum; and like all other data must be fitted into an overall scholarly pattern, in which no one set of facts is unchallengeable but all must be constantly re-evaluated to eliminate apparent conflicts.

Earlier history of Ghassul digging

Since the start of excavation at Ghassul in 1929, the pottery and other artifacts were rightly recognized to *precede* the earliest definitely-known Bronze Age materials³. At that time the firmly-identified Bronze Age pottery types scarcely preceded what we now call MB, so that Mallon and his collaborators could hope to maintain a date in relation to Abraham about 2100. Between the Mari and Ebla discoveries the date of Abraham was unanimously brought down toward 1700⁴. Also, EB finds were gradually fitted into a stratification extending back to 3200 BC. Moreover at Megiddo and Beth-Shan an Esdraelon culture was recognized, also pre-

³ Alexis Mallon, Robert Koepfel, René Neuville, *Teleilat Ghassul I. Compte Rendu des Fouilles de l'Institut Biblique Pontifical 1929–1932* (Rome: P.I.B. 1934). The bronze itself from which 'Bronze Age' derives its name becomes normal only in Middle Bronze. On the other hand, it cannot be overlooked that Mallon's dating was somewhat influenced by his hope of finding at Ghassul the site of Sodom. See R. North, "'Ghassulian' in Palestine Chronological Nomenclature", *Biblica* 40 (1959) 543; 541–555 [= *Studia Biblica et Orientalia* I.VT: *Analecta Biblica* 10 (Rome P.I.B. 1959) 407–421].

⁴ Early efforts to relate Eblaite names to the Abraham narrative and thus push his date back to 2300 or even earlier [thus erstwhile Mardikh-Ebla epigrapher Giovanni Pettinato, as cited by David N. Freedman, 'The Real Story of the Ebla Tablets, Ebla and the Cities of the Plain (Gn 14)', *Biblical Archaeologist* 41 (1978) 143–164; 39 (1976) 44–54 by Pettinato; otherwise 94–113 by excavation director Paolo Matthiae, also in *Orientalia* 44 (1975) 337–360; 361–374 there by Pettinato] are noticeably left unmentioned in Pettinato's recent book, *Ebla, un impero inciso nell'argilla* (Saggi 126; Milano: A. Mondadori, 1979). On the date of Abraham around 1700, usually in some tacit relation to the discredited equation Amraphel = Hammurabi, see Roland de Vaux, *The Early History of Israel* (London: Darton, Longman & Todd, 1978) 1, 265, 216, and 'The Hebrew Patriarchs and History' (unabridged original lecture), *Theology Digest* 12 (1964) 227–240. On the three dates proposed for Hammurabi, all near 1700, see W. F. Albright, 'A Third Revision of the Early Chronology of Western Asia', *Bulletin of the American Schools of Oriental Research* 88 (1942) 28–33, correcting his 69 (1938) 18–21; 77 (1940) 20–22; 78 (1940) 23–30.

¹ J. B. Hennessy, 'Preliminary Report on a First Season of Excavations at Teleilat Ghassul [Jan. 16–Mar. 28, 1967]', *Levant* 1 (1969) 1–24; our summary in *Orientalia* 38 (1969) 131–2.

² So, for example, the table on p. 205 of David H. Thomas, *Archaeology* (New York: Holt, 1979); or Colin Renfrew, *Before Civilization: the Radiocarbon Revolution and Prehistoric Europe* (London: Cape, 1973) 77.

Bronze and akin to Ghassul except that it had shiny gray ware with beaded carination and round houses⁵. The influence of William F. Albright secured general acceptance for the view that the name 'chalcolithic' which he seems to have invented, was a good one for the whole pre-bronze millennium, in which Esdraelon-gray followed Ghassulian⁶. By a very casual approximation, really just a *hunch*, a date around 3500 was thought suitable for Ghassul, leaving some margin for its own duration as well as for the rise and fall of the gray-bead-carination culture before 3200.

Albright himself also very casually, much more than is generally noticed in the report, suggested that of the four levels which Mallon recognized in an extremely limited sounding, the top one could apparently be subdivided into two, and most of the efforts recorded in *Teleilat Ghassul II* tended in that direction⁷. Meanwhile his two closest and most prominent collaborators, Glueck and Wright, endeavoured varyingly to spread out the four (or five) levels through a good part of the fourth millennium; but really the only generally agreed Ghassulian dating remained around 3500⁸. The one significant addition to the Ghassul-related materials was the house-shaped terracotta ossuaries found at Hedera and 'Afūla, and eventually imposingly at Azor⁹.

This 'around 3500' consensus was briefly threatened by the discoveries of Perrot at Beersheba. His materials there were for the most part genuinely Ghassulian, with some interesting innovations like catacombs and ivory statuettes, but no gray-bead-carination. But he also published reasons for dating his Ghassulian well into the third millennium¹⁰. Just at that time Roland de Vaux was uncovering plenty of Ghassulian at (northern) Tell el-Far'ah near Esdraelon but without gray-ware; he called it Beersheba Ghassulian, and tended toward a later date, though not so late as Perrot¹¹. Seeing this, Maurice

Dunand felt that the time had come to publish a claim that his abundant Eneolithic (a coinage equivalent to chalcolithic) materials at Byblos really fitted the later horizon of Tell el Far'ah (N) Beersheba Ghassulian, and even hinted that maybe Mallon was right in the first place about his date nearer to 2200¹². Since all four of these experts were French, though of very different background, the horizon for a moment looked cloudy. But de Vaux and Perrot were both on excellent terms with English-language archaeologists, and I recall Wright telling me how co-operative they had both been to his compromise-formula, leaving Ghassul-Ghassulian more or less secure around 3500¹³.

The re-opening of excavation at Ghassul in 1959 was really not so much to establish a definitive stratum-dating, but rather to clarify the perplexing absence there of city wall and cemetery, as well as of those features unmistakably associated with Ghassulian elsewhere: round houses, gray ware, house-shaped ossuaries, catacombs. My published report shows the basis for a judgement that from top to bottom of the longest continuous occupation in any trench, no real variation of pottery-types was observable¹⁴. Moreover, my section-drawings show how difficult it was to trace any particular layer (homogeneous soil-colour with pertinent artifacts) *horizontally* for more than a few feet. Professor Hennessy's own section-drawings shown to this Conference seem comfortably similar to mine in this respect, as I pointed out publicly there to his apparent agreement (he had also discovered a *reason* for the 'churning-up' in ancient and modern earthquakes to be noticed below). He agreed further that if these fifteen layers (at most six metres from top to bottom) represented a thousand years duration, and *some* of the layers were no more than a couple of centimetres thick, then we would have the anomaly that a single jar or even sherd might lie within two or three layers, representing a median of a hundred years each. But for our present purpose the only testimony I draw from my own excavation of the site is that it left unchanged the consensus-view that Ghassul was occupied around 3500 with very little assurance as to how long before or after.

The Lacuna rears its ugly head

Meanwhile both Hennessy and I had enjoyed the privilege of working for Kathleen Kenyon during her first two seasons at Jericho. It is no embarrassment to me to acknowledge that the privilege was vastly greater and more efficacious in his case, because after several further seasons there with her he went

⁵ Geoffrey M. Shipton, 'The Earliest Pottery of Megiddo, Seasons 1937-8', *Journal of the Palestine Exploration Society* 18 (1938) 54; G. M. Fitzgerald, 'The Earliest Pottery of Beth Shan', (Philadelphia) *Museum Journal* 24 (1955) 5-22; on both, our 'Checklist of Allegedly Ghassulian Sites', in *Ghassul* 1960 (fn. 14 below) 57 and 43.

⁶ W. F. Albright, 'Recent Progress in the Late Prehistory of Palestine', *Bulletin of the American Schools of Oriental Research* 42 (1931) 13-15; also 48 (1932) 10-13, 'The Chalcolithic Age in Palestine'.

⁷ Chiefly J. W. Murphy and G. S. Mahan in R. Koeppl, H. Senès, *Teleilat Ghassul II: 1932-1936* (Rome: P.I.B. 1940); for the later campaign we have only Koeppl, 'Die achte Grabung in Ghassul (3. Jan.-1. März 1938): Vorläufiger Bericht', *Biblica* 19 (1938) 260-266.

⁸ G. Ernest Wright, *The Pottery of Palestine from the Earliest Times to the End of the Early Bronze Age* (New Haven: ASOR, 1937) 14-42 (but see n. 13 below); Nelson Glueck, *Explorations in Eastern Palestine IV = Annual of ASOR* 24 (1951) 327, and BASOR 122 (1951) 15.

⁹ E. Sukenik, 'A Chalcolithic Necropolis at Hedera' and 'Archaeological Investigations at 'Afūla', in *Journal of the Palestine Oriental Society* 17 (1937) 24; 21 (1948) 17; 'Late Chalcolithic Pottery from 'Afūleh', *Palestine Exploration Quarterly* 68 (1936) 150-154. [J. Perrot, Azor]; *Israel Exploration Journal* 8 (1958) 133; nothing further in 11 (1961) 171-175, on M. Dothan's Iron Age cemetery excavation there [or A. Ben-Tor, *Qedem* 1 (1975)].

¹⁰ Jean Perrot, 'The Excavations at Tell Abu Matar, near Beersheba', *Israel Exploration Journal* 5 (1955) 17-40, 73-84, 167-189; we further quoted him in *Biblica* 40 (1959) 552: 'he is on the whole disposed to admit for all the Ghassul material an indissolubly homogeneous relatively short occupation', even after his parley with Robert Braidwood, Wright, and others in Chicago 1958.

¹¹ R. de Vaux, 'La première [sixième] campagne de fouilles à Tell el-Far'ah près Naplouse', *Revue Biblique* 54 (1947) 399 [64 (1975) 555]; to be qualified by my oral report in *Biblica* 40 (1959) 551.

¹² Maurice Dunand, 'Date de l'installation IV de Teleilat Ghassoul', *Bulletin du Musée de Beyrouth* 9 (1950) 87.

¹³ G. Ernest Wright, 'The Problem of the Transition between the Chalcolithic and Bronze Ages', *Eretz-Israel* 5 (1978) *37-*45, relying heavily on de Vaux's N-Far'a to prove that Esdraelon culture, whose date Wright lowers to 3250, is contemporary with Wright's previous EBl.

¹⁴ R. North, *Ghassul 1960 Excavation Report* (Analecta Biblica 14; Rome: Pontifical Biblical Institute, 1961) 37 and Fig. 16 after p. 32.—Fuller data on the frescoes in 'A Unique New Palestine Art-Form', *Estudios Eclesiásticos* 34 (A. Fernández Festschrift, 1960) 381-390; on incised pottery, 'Ghassul's New-Found Jar Incision', *Annual of the Department of Antiquities of Jordan* 8f (1964) 68-74.

on to become the official representative of the Jerusalem British School which she headed, and it was in this capacity (according to repeated formulations in her latest book noted below, not just as head of archaeology operations for the University of Sydney) that he supervised the renewal of excavation at Ghassul. Here may be the most pertinent place to note a curious and chronologically important detail of our work at the two nearby sites. (Tell es-Sultân is 17 km. from Teleilat Ghassul in a straight line, 22 km. by the nearest bridge; but the nearness is realistically much greater in the sense that virtually no human settlement other than Greater Jericho has ever intervened between them.)

At Jericho in 1952 Hennessy's recognized achievement was to discover for Miss Kenyon that the brick-wall collapse which Garstang had attributed to an *earthquake* (described all unawares in Joshua 3, 16 and 6, 20) was really due to no cataclysm at all but just to the ordinary wear-and-tear of a mud-brick wall rebuilt 17 times before 2000¹⁵. But when Hennessy followed me at Ghassul, his equally sensational discovery was that here there *were* earthquake-destructions which no prior excavator had noticed. Though several of these affected the chalcolithic settlement, the worst of all was only a few years before Mallon's excavation in 1929. Participants at this Conference will recall that Hennessy proclaimed aloud explicitly (though with an inescapable wry humour), 'What's the use of wasting our time excavating at a site whose strata have been so mercilessly and irretrievably churned up? A lament not without chronological significance.'

As for Jericho Ghassulian, Hennessy summarized the prevailing consensus for this Conference with admirable accuracy: at Jericho-Sultân, despite one or other small object or remote resemblance, and the possibility of having overlooked larger deposits, there is an almost total lack of the Ghassulian or in general chalcolithic materials which dominated Palestine for nearly a millennium, and whose parent-site was so near across the river. This was at root the view of Jericho's latest excavator, Kathleen Kenyon. It may safely be added that for her as for anyone else this Ghassulian floruit was around 3500, certainly not later than 3200 when EB was generally held to begin, also hardly later than 3400 in order to allow a certain duration for the Esdraelon gray-ware culture; furthermore not specifiably *earlier* than 3500, though in a vague general way thought to occupy the whole fourth millennium.

Before tracing more in detail Dame Kenyon's purview on Ghassulian at Jericho, this might be the best place to insert the *only* published study by an expert who has excavated at both places. This study was made by Hennessy himself, though in a

chapter only preliminary to what was his essential Early Bronze focus¹⁶.

Ghassul's pre-bronze foreign relations

Hennessy there begins by apparently accepting a major breakthrough toward *lowering* the date of chalcolithic: Kenyon and Wright have recently demonstrated the contemporaneity of the Esdraelon gray-black ware and the painted and red burnished wares of Wright's former EB Ia. In a recent article, Wright has included the Esdraelon culture within the Early Bronze Age¹⁷. Hennessy questions only the existence of *subdivisions* which Wright claimed to find in this new unit 'Esdraelon = EB Ia'. More relevant here than the (non-)existence of such subdivisions is Hennessy's argument regarding them. It is drawn from Meşer, where the most significant connection is the [clear] contemporaneity of the gray-burnished Esdraelon ware with painted cornets, churns, cream ware and other elements of the Ghassul-Beersheba culture¹⁸. We note in passing that like the rest of the world, Hennessy here *presumes* a certain vague *homogeneity* of painted cornets, churns, and cream ware which are in fact found in *all* depths of *all* excavations at Ghassul¹⁹.

He adds that Meşer II has apse-buildings like Jericho Q-N-M (and Megiddo-Beth-Shan gray-ware chalcolithic) and its stratification makes sense of the assemblage of the upper phase of Megiddo XX, with both Ghassulian features and elements of the Late Chalcolithic of northern Palestine side by side²⁰. The excavations at Tell ('Gath') have again demonstrated the equation²¹. While thus denying subdivisions in the contemporaneous Ghassul-Beersheba-Esdraelon EB Ia, Hennessy then adds that there *are* subdivisions within the period which immediately precedes the Early Bronze Age, namely at Jericho Q-M, Meşer, and 'Gath'. Since these are precisely the sites which have just proved for him that EB Ia is indivisibly

¹⁶ J. B. Hennessy, *The Foreign Relations of Palestine during the Early Bronze Age* (Colt Archaeological Institute Publications; London: Quaritch, 1967) pp. 16–18, 9, and 47.

¹⁷ P. 15, citing Wright as in my n. 13 above, but with no reference to any Kenyon publication.

¹⁸ Moshé Dothan, 'Excavations at Meser, 1957', *Israel Exploration Journal* 9 (1959) 13–29.

¹⁹ It is this *homogeneity* (mentioned also by several other experts whom we quote in turn) which seemed the main problem involved in acceptance of the thousand year *span* of the radiocarbon dates (which is really a separate question from whether they began in 3600, 4600, or 5600) and this was my answer to our Jericho comrade G. R. H. (Mike) Wright, who asked me whether (apart from quite legitimate loyalty to an existing terminology) it would not be quite easy to agree on a sweepingly new framework for 'the real facts'. I certainly agreed *fully* with him that there is nothing at all sacrosanct about terms like 'chalcolithic' or 'Ghassulian' [already in 1961 I published as final word of my excavation report (p. 77), 'Our hope is that continuing researches into the Beersheba culture will reveal a new architectonic pattern of the whole Protoceramic Age, both Neolithic and (if the term be needed any more) Chalcolithic'.] Equally negotiable are the particular dates attached to the objects hitherto called Ghassulian, and their coalescence with what had hitherto been called Neolithic, provided they can be fitted into an overall framework with all the other Jordan-area excavation results. But the *homogeneity* (and therefore relatively brief duration) of the materials dug up at Ghassul seems to me and to others at least an apparent *fact*, until proof to the contrary is forthcoming.

²⁰ Gordon Loud, *Megiddo II* (Chicago: Oriental Institute, 1948) PL. 1, 2, 92–95, cited as in M. Dothan, 'Some Problems of the Stratigraphy of Megiddo XX', *Eretz-Israel* 5 (1958) 38–40, Eng. p. *85; from which I add 'Most Megiddo XX is Ghassulian [cornet!]; some is pre-Ghassulian'.

²¹ Shmuel Yeivin, *First Preliminary Report on the Excavation at Tel 'Gat'* (Jerusalem 1961): no specifics cited.

¹⁵ K. Kenyon, 'Excavations at Jericho 1952, Interim Report', *Palestine Exploration Quarterly* 84 (1952) 64; 62–82; R. North, 'The 1952 Jericho-Sultân Excavation', *Biblica* 34 (1953) 1–12; 'Fosoj de Jeriho kai ekzegezo de la Biblio', *Biblia Revuo* 9 (1973) 223; 215–232; 'Les murs de Jéricho', *Bible et Terre Sainte* (Oct. 1958) 10–17 ['Le camp de Josué autour de Jéricho' 52 (Jan. 1963) 6–20] reprinted in our *Fouilles dans la région de Jéricho* (Rome: P.I.B. 1967) 63–72 [3–22].—In the same Sultân trench, earthquake traces were indeed found by Hennessy, but at a much lower level, some thousands of years before either Joshua or the mud-brick wall.

contemporary with Beersheba-Ghassulian and Esdraelon chalcolithic, one must conclude that at that time he regarded the so-called EBIa as really *pre*-Bronze Age, though he does not specify whether he accepts the corollary that the commonly received *outset* of the Bronze Age must be lowered nearer to 3000 (3100 in Miss Kenyon's view to be noted below), or whether on the contrary he had some ace up his sleeve to show that the EB-outset can be kept at 3200 or pushed even earlier. As noted above²² Wright claimed to be lowering the date of Esdraelon culture to 3250, although its whole duration had generally been set prior to 3200. But significantly Albright in a 1965 team-survey firmly dates the Late Chalcolithic as 3400–3000²².

While skirting this absolute-dating problem, *Foreign Relations* adds further strengthening of the parallels between the painted wares of Proto-Urban B [i.e. Jericho tombs akin to Q-M] and Ghassul iv, from which he cites no less than nine close but not direct parallels of pottery, which concern us in detail²³. He adds that neolithic Ghrubba too shows with the Ghassulian culture, parallels which are certainly legitimate but again not direct for equating Jericho Proto-Urban B with Chalcolithic Ghassul²⁴. Is this meant perhaps to distinguish from the above Ghassul iv = Beersheba = Esdraelon? What follows leaves this unclear: 'The important suggestion is the contemporaneity of the Proto-Urban A, B, C cultures, the Late Chalcolithic of Beersheba, Meşer and Gath and Ghassul iv.' Page 9 had suggested that Kenyon's [Jericho Neolithic] Proto-Urban A and B are contemporaneous, and p. 47 will extend this explicitly also to C. We thus hear Ghassul iv is coextensive with Jericho [Neolithic] Proto-Urban A, B, and C. He adds some radiocarbon dates, of which Ghassul iv is perceptibly the earliest but all are in the range of 3300 BC; though he qualifies this radiocarbon operation as recent he does not hint whether it is calibrated. From p. 47 we learn further that the *beginnings* of Jericho ('Neolithic') Proto-

Urban (not, however, A rather than C) and Ghassul iv are possibly contemporary with the rise of Gerzean in Egypt; and the *end* of Jericho Proto-Urban (A, B, C; also Ghassul iv) overlaps the beginning of the First Dynasty in Egypt. Moreover, Proto-Urban in all of Palestine is in general contemporary with Byblos Eneolithic B and possibly with Uruk ware (chart p. 48).

We see that thus what Kathleen Kenyon long called Neolithic at Jericho is brought down not merely *into* the Ghassulian Chalcolithic period but *to its very end*. Why do we not rather say that 'the *end* of Ghassulian is pushed *back* to the time of Jericho Neolithic'? Only because *Foreign Relations* has given no hint of what will fill the gap between EBI (now Ib) and what is called neolithic at Jericho. Before approaching this gap, we must make sure that we have well understood just what the excavators of Jericho-Sulţān have really said on the subject.

Sulţān Diggers' own view of Ghassulian

The German theologian Ernst Sellin and his architect Carl Watzinger knew nothing, of course, about 'Ghassulian' or the term 'chalcolithic' later popularized by Albright. Though they had already discovered (and interpreted conflictingly) all the walls which loomed so large later in relation to Joshua, their only published pre-Bronze materials are all lumped together as 'Canaanite', and indeed the most Ghassulian-looking samples (rope moulding) are described as 'Late-Canaanite'²⁵.

British Mandate antiquities director, John Garstang, in earlier reports of his Sulţān excavation maintained uncompromisingly 'the characteristic Ghassulian pottery is fully represented in the chalcolithic level at Jericho, i.e. in the deposits found (in the 7th metre) between the Upper Neolithic and the Earliest Bronze'²⁶. Especially striking, and valid today, is the parallel between Jericho and Ghassul mat-base pottery²⁷. He gave this statement in 1936: 'a series of chalcolithic floors (later than the neolithic floors found a year ago) have been uncovered'²⁸. (Our own efforts to compare Byblos and Jericho neolithic floors illustrate much relevant material but with no new judgment on the chalcolithic question²⁹). Garstang suggests that Ghassulian would be even more noticeable at Sulţān if it had been published more adequately at Ghassul.

²² W. F. Albright, 'Palestine before 1500', in Robert W. Ehrich, *Chronologies in Old World Archaeology* (Chicago U., 1965) 47–60; the chart p. 49 puts before Ghassulian, 'Jericho VIII etc. chalcolithic 4000–3600', and then 'Ghassulian 3700–3300', thus with an overlap of one hundred years on both sides. Ehrich's volume is really the updating of a 1954 *Relative Chronologies*: p. v notes that the 'relative' can be dropped because [of the 'extraordinary precision', Albright p. 47] of radiocarbon!

²³ J. B. Hennessy, *The Foreign Relations of Palestine during the Early Bronze Age*, London, 1967 p. 17: (1) Small spouted bowls with painted decoration: TC II (my n. 7 above) pl. 94: 11, 12, as J. Marquet-Krause, *Les fouilles d'Ai (et-Tell)* [Paris 1949] pl. 67; 72. (2) Deep straight or curved bowls, painted on the interior: TG I (n. 3 above) pl. 42: 9, as *Jericho I* (n. 42 below) fig. 22: 4; *Jericho II* (n. 41 below) fig. 7: 1, 4; *Fouilles d'Ai* pl. 67; 72: 18, 828. (3) Shallow bowls with simple, vertical, linear decoration: TG I pl. 65: 9; TG II pl. 89: 20, as *Jericho I* fig. 11: 18; 22: 9; *Jericho II* fig. 4: 3. (4) Shallow bowls with interior painted decoration of circle and radiating lines: TG I pl. 42: 8 as *Jericho II* 4: 7; 7: 14. (5) Wavy line decoration: TG I pl. 54: 111; frontispiece fresco; TG II pl. 80: 15, 19; 90: 9, 10, 17, 19 [no parallels noted from Jericho or elsewhere]. (6) The use of a light to pinkish white slip and red paint: TG I pl. 65 [no parallels]. (7) Bottles with or without small pierced lug handles: TG I fig. 50: 3; pl. 43: 3. (8) Use of applied plastic ornament; thumb impressed jar and bowl rims [no parallels cited; these are both admittedly very numerous at Ghassul, and perhaps more than anything else would have lent themselves to being chronologically subdivided and interrelated]. (9) Serrated ledge handles [no examples, and like others here cited I doubt whether the ledge handles of Ghassul are sufficiently proved].

²⁴ J. B. Hennessy, *The Foreign Relations of Palestine during the Early Bronze Age*, London, 1967 p. 18; James Mellaart, 'The Neolithic Site of Ghrubba' [10 km. N of Ghassul], *Annual of the Department of Antiquities of Jordan* 3 (1956) 24: 'a small beaker (no. 30) of Ghassulian type'.

²⁵ E. Sellin and C. Watzinger, *Jericho, die Ergebnisse der Ausgrabungen* (Wissenschaftliche Veröffentlichungen der Deutschen Orient-Gesellschaft, 22; Leipzig: Hinrichs, 1930) 109 fig. 97; 108 fig. 94.

²⁶ J. Garstang, 'Jericho Fifth Report', (Liverpool) *Annals of Archaeology and Anthropology* 22 (1935) 222; he compares his illustrations there 41: 1–5 with TG I fig. 38; his 41: 7, 8 with TG I fig. 40; his 41: 28 with TG I fig. 62; his 35: 17 and 41: 6 with TG I fig. 40; his 43: 6 with TG I fig. 41. On p. 223 Garstang holds that Mallon's Level iv at Ghassul is really EB [4], as Mallon had always claimed, with the unwavering support (despite other strong oppositions) of L.-H. Vincent, *Revue Biblique* 44 (1935) 87–100, never later qualified.

²⁷ J. Garstang, *Annals of Archaeology and Anthropology* 22 (1935) pl. 55a, as TG I pl. 39: 1–2; on which, see A. Jirku in *Biblica* 29 (1948) 270 n2.

²⁸ *Palestine Post* Feb. 3, 1936; cited by Albright, BASOR 61 (1936) 24; he continues the quotation from Garstang, 'the top [floor] yielded two Egyptian objects, similar to those of the last predynastic culture in Egypt': for Albright this would date the floor rather in the EB 1 of 'Ai and Megiddo.

²⁹ R. North, 'Byblos and Jericho Neolithic Floors', *Fifth World Congress of Jewish Studies* (1969; Jerusalem 1972) 2, 35–49.

The Sulṭān pottery analysed by Droop is firmly called chalcolithic³⁰. In criticism, the Beth-Shan excavator, Fitzgerald, insisted that the alleged 'ledge-handles' of Ghassul are really not what is meant by that name at Jericho and elsewhere³¹. Perhaps because of this criticism, or similar ones communicated to him orally, Garstang performed a rather bold about face. His ceramicist in 1936 was Immanuel Ben-Dor, and he handed to Garstang his report with the title 'Pottery of the Neolithic and Chalcolithic Periods'³². Garstang, attestedly without securing the consent of the expert whose name was signed to the article, changed its title to 'Pottery of the Middle and Late Neolithic Periods', as it now stands³³. Several of the world's top experts have stated supportively, Ben-Dor has declared that he cannot endorse the transformation, and desires that an 'oral tradition' should restore uniformly the expression of his reasoned conviction³⁴.

Albright too endorsed Ben-Dor's position³⁵. Yet in this case as in others the emphatic views of himself and his school undergo changes. His claim that Garstang's Sulṭān VIII is 'pre-Ghassul' must mean 'pre-Ghassul-IV', since that is the only stratum discussed in the book under review³⁶. Anyway, he there admits that his earlier view dating Ghassul IV after Sulṭān VIII has now been shown erroneous by Joan Crowfoot³⁷. The view that Ghassul III is prior to IV (and thus equated with Sulṭān VIII) is presumably reflected in Glueck's statement which Albright published³⁸. Glueck, however, denies the assertion of Ben-Dor that the 'neolithic culture of Jericho had a long duration and its latest phase existed contemporaneously with Ghassulian chalcolithic'³⁹. Yet Wright's long-definitive chronology of early pottery finds Garstang's pottery with Ghassulian affinities at Sulṭān to be the absolute proof of Albright's position regarding the pre-Bronze date of Ghassul⁴⁰. Miss Kenyon at Jericho showed an understandable hesitancy about publishing during Garstang's last years her discoveries discrediting his Joshua wall chronology; later changes in her terminology suggest that she might have felt a similar initial hesitancy to proclaim that Ben-Dor was right after all in calling 'chalcolithic' what Garstang changed to 'neolithic'.

What Miss Kenyon's only definitive report actually published about Ghassulian or any chalcolithic at her own Sulṭān excavations, concerns the *tombs*. Even this is not really definitive, insofar as there is no synthesis not only with the tell-material, but even between the two groups of tombs excavated in 1952–4 and 1955–8. For this latter group K2 has a Phase I which is like Tell el-Far'ah (N) Late Chalcolithic Proto-Urban C or Esdraelon ware⁴¹. In the earlier volume tomb A94 is taken as the basis (p. 5) for a Proto-Urban period in the second half of the Fourth Millennium, with groups A, B, and C. A and C groups were hitherto (by Garstang? or by herself?) called Late Chalcolithic, and B group EB 1a. Wright in EI 5 now recognizes that the three classes of pottery are contemporary. He would prefer to call them EB. Kenyon does not agree; they are too different. Her own inclination is to label all these groups Late Chalcolithic (p. 9). The absolute dates 3400–3100 are suggested on p. 19, presumably *not* Ghassulian, and not necessarily corresponding to any occupation level on Tell Sulṭān itself⁴². Since already in an earlier publication she had noted for these tombs a *radiocarbon date* of 3260 ± 110 (precisely the received date of post-Ghassulian Esdraelon chalcolithic), one must wonder whether the artifacts were dated by this norm rather than by any typological resemblance to excavated chalcolithic wares.

Kenyon's pre-Bronze Tell materials

During Miss Kenyon's first two seasons at Jericho, my work with her team was in the capacity of apprentice-archaeologist from that Institute which had sponsored Ghassul and had hopes of further excavation work there or elsewhere. During those two seasons she sometimes mentioned the amazing absence of Ghassulian material at a site so near and so rich in materials from just before and after. I beg pardon for describing here a generally unnoticed detail which my own 'family interest' in the Ghassul results had enabled me to discover for the first time at Jericho. I had been assigned to watch over the dismantling of some possibly cultic rooms in an area which both Garstang and Kenyon firmly called neolithic. I noticed quite by chance that the workman's pick in tearing off the wall-plaster laid bare a long mud brick which had finger-impressions to hold the mud-cement, exactly the same type as Mallon had published from his Ghassul discoveries⁴³. The holes were in two rows running along parallel, made by two fingers prodding at successive

³⁰ J. Droop, 'Jericho Fifth Report: the Pottery from the Chalcolithic and Neolithic Levels', AAA 22 (1935) 169–173.

³¹ G. M. Fitzgerald, review of *TG I* in *Journal of the Palestine Oriental Society* 16 (1936) 46–48; similarly J. Perrot in IEJ 5 (1955) 180.

³² Cited in this form with the note 'titre corrigé par l'auteur' by Claude F. M. Schaeffer, *Ugaritica I* (Paris 1939) 8 n4.

³³ I. Ben-Dor, 'Jericho Sixth Report: Pottery of the Middle and Late Neolithic Periods', AAA 23 (1936) 77.

³⁴ L.-H. Vincent, *Revue Biblique* 47 (1938) 579; Schaeffer, n. 32 above.

³⁵ W. F. Albright, BASOR 86 (1942) 10.

³⁶ Albright's review of *TG II* in *Biblica* 28 (1947) 309.

³⁷ Joan Crowfoot, 'Notes on the Flint Implements of Jericho 1936', *Annals of Archaeology and Anthropology* 24 (1937) 39–46.

³⁸ N. Glueck on Sa'idiyya, AASOR 25 (1951) 327; 'Go, View the Land', BASOR 122 (1951) 15.

³⁹ Ben-Dor AAA 23 (1936) 90; Glueck BASOR 97 (1945) 17.

⁴⁰ G. E. Wright, *Pottery* (fn. 8 above) 16.

⁴¹ Kathleen M. Kenyon, *Excavations at Jericho II. The Tombs Excavated in 1955–8* (London: British School in Jerusalem, 1965) 13–14. Note that all *her* tombs are in an extensive cemetery *north* of the tell (under the refugee village) and *not* in the tomb area west of the tell excavated by Garstang and controverted by Vincent.

⁴² Kenyon, *Excavations at Jericho I. The Tombs Excavated in 1952–4* (London 1960) 4–51.

⁴³ R. North, 'El nuevo "Santuario" de Jericó', *Estudios Eclesiásticos* 27 (1953) 336 [325–337 = *Fouilles dans la région de Jéricho* (Rome: P.I.B. 1967) 47–62] and fig. 10 [Biblica 34 (1953) PL. 2] comparing *TG I* 34 and PL. 14: 1. Miss Kenyon's 'Early Jericho', *Antiquity* 26 (1952) 116–122 says on p. 120 that the Ghassul markings 'in no way resemble' the 'cigar-shaped' bricks of Jericho; but the fact remains that what little resemblance there was, furnished the first occasion for recognizing the existence of such bricks at Jericho.

intervals⁴⁴. At Jericho-Sultân we eventually found many such complete bricks, which I would describe as loaf-shaped, but Miss Kenyon called at first cigar-shaped and later hog-back. She duly publicized this discovery, but neither she nor I apparently thought to wonder why a culture-feature already solidly recorded as chalcolithic should turn up at neolithic Jericho.

Indeed, during the whole of those two seasons there was hardly even any hesitancy expressed at the established fact that the Sultân mound contained no chalcolithic at all. Much less was there any effort to set up precarious parallels of the Sultân pieces called neolithic or EB with chalcolithic types, or even make soundings expressly in the hope of discovering chalcolithic (as we were incessantly doing for Joshua-era LB). The archeological consensus which Hennessy quite correctly described for this Conference, without attributing it explicitly to Miss Kenyon, must have emerged chiefly during the later seasons, when a) it became clear that the Sultân chalcolithic lacuna was a frightening and implausible datum; b) it might or indeed must be that chalcolithic deposits could yet be encountered; c) *some* of the Neolithic or EB pieces at Jericho upon careful examination show remote chalcolithic traits; d) there were negligibly few real Ghassulian pieces really found at Sultân after all.

Taking for what it is worth this haphazard record of my own direct experience of the Sultân excavation, let us get on to the real question of what Kenyon herself has seen fit to put into the published record.

She took the responsibility of publishing, with her 1954 report, her geochronologist Zeuner's detailed proof of a ± 300 -year lacuna on the Tell between the Neolithic and Early Bronze ages⁴⁵. Whatever others may think, I find that this allotting of only 300 or so years to the duration of *both* Ghassulian and the subsequent Esdraelon(-?Beersheba) chalcolithic has not been contradicted either typologically or stratigraphically by the materials (chiefly pottery) excavated at Ghassul. Naturally, I have had no chance to study the Ghassulian pottery-sequence which Hennessy has been able to divide to his satisfaction into separate periods extending over a full thousand years. His success in this enterprise may well remain unshaken, independent of the separate question of whether those thousand years were 4000–3000 or 5600–4600. But the point of greatest interest will be not merely the development alleged for the various *forms* already well known at Ghassul, but much more the proof of their discovery in a *stratified sequence* corresponding *both* to the asserted form-development *and* (eventually) to the radio-carbon dates of the *respective* pieces.

Despite Zeuner's stand, however, Miss Kenyon herself in publishing the 1955 campaign actually assigns a subtitle to

the Chalcolithic Period. Here is her justification of that subtitle. 'A study of the pottery from these [tell] pits shows that it can be clearly distinguished from a considerable quantity of other pottery, certainly ante-dating the Early Bronze Age, which has now been found, which equates with the pottery assigned by Professor Garstang to his Level VIII (none illustrated). It is to be presumed that when the removal of Early Bronze Age levels . . . has been completed, levels of the Chalcolithic phase will be reached. The occupation was perhaps on a smaller scale than that of the two Neolithic periods, and was [now not 'perhaps' or 'presumably'] concentrated toward the spring on the east side [where no materials whatever survive the highway-building operations before Sellin's time], leaving the west side of the tell unoccupied. . . . The culture represented by Professor Garstang's Level VIII does not immediately precede the Early Bronze Age'⁴⁶.

This already subtle position is further nuanced in her 1957 popularizing volume: 'the archaeological evidence of a gap is that at Jericho there are no traces of a culture, usually accepted as Chalcolithic, which has now been found widespread over Palestine (i.e. Ghassulian) characterized by very individual types of pottery and flints, and these are not found at Jericho. . . . Ghassul is only some nine miles from Jericho, and it seems very improbable that there would be no traces of contact if Jericho had been occupied at the time, particularly as there is a small Ghassulian site only about a mile away [?Dûq] . . . The Ghassulian culture is the most striking element which has to be fitted into this gap. . . . It is clear that it is the product of an intrusive group, of which the origins have not yet been satisfactorily established. It is likely that remnants of some indigenous culture or at least one with older roots in the country, are to be found, and these are probably represented by the earliest levels found at sites like Tell el-Far'ah (N) (=Tirzah) and Beisan. At neither of these is there any trace of the Ghassulian culture, but there were remains which were earlier [than EB or post-Ghassulian chalcolithic], and which have some links with the Pottery Neolithic B period at Jericho. But at Jericho [-Sultân] itself, this culture too is apparently lacking'⁴⁷.

This about covers what she has published directly on the Jericho excavation itself. She also did relevant chapters for the Cambridge Ancient History, but only on the Late Bronze chronology; the two chapters on Neolithic-Chalcolithic and EB were by Père de Vaux, who well indicates that despite the confusion of terminology her work at Jericho showed no Ghassulian occupation⁴⁸: (p. 520) 'Great confusion prevails . . . Material from Jericho VIII, called by J. Garstang Chalcolithic in 1935, and Late Neolithic in 1936, became Pottery Neolithic B with K. M. Kenyon . . . Wright classified Jericho VIII as Middle Chalcolithic in 1938; as Early Chalcolithic,

⁴⁴ Note that an extremely different type of indentation, with all five fingers of one hand stuck deep into the gooey-mud surface, was found in my Ghassul 1960 excavation (n. 14 above) p. 14 and PL. 8 lower left.

⁴⁵ F. E. Zeuner, 'The Neolithic-Bronze Age Gap on the Tell at Jericho', *Palestine Exploration Quarterly* 86 (1954) 64–68.

⁴⁶ Kenyon, 'Excavations at Jericho 1955', *PEQ* 87 (1955) 113.

⁴⁷ Kenyon, *Digging Up Jericho* (London 1957) 94.

⁴⁸ Roland de Vaux, 'Palestine [in the Early Bronze Age 1/2 (1971) 208–237] during the Neolithic and Chalcolithic Periods', *Cambridge Ancient History* 3/1 (1970) 499–538.

together with Yarmukian, in 1962 [further Kaplan, Amiran, Anati; p. 521:] . . . uniformity of this culture, which is called Ghassulian; p. 529, Subterranean dwellings at Beersheba may have been contemporary with Ghassul I-II; p. 534, Kenyon's A and C groups, contemporary but of different origins, she held to be intermingled at Tell el-Far'ah (N); p. 536 (despite Kenyon and others) Ghassul was later than Jericho VIII and Pottery Neolithic; . . . Jericho was abandoned during the Ghassul occupation⁴⁹.

But we have yet to examine some even more definitive and recent statements of Miss Kenyon in successive editions of her survey of the whole of Palestinian archaeology. The first edition in 1960 had said: 'Ghassul pottery has some affinities with that of Jericho . . . but many highly individual forms found at Ghassul are not found at Jericho . . . It may be that Ghassul belongs mainly to a period when there was a gap in occupation at Jericho . . . The present weight of evidence does suggest that the Ghassulians were newcomers from outside Palestine, and the fact that near-by Jericho shows only slight evidence of connection with them may support the idea of a gap there covering most of the period of occupation at Ghassul'⁴⁹.

The fourth, entirely rewritten, edition of this work was published after her death in 1979, under the supervision of T. A. Holland, who singles out for comment in his preface, 'Recent excavations at Teleilat Ghassul provide pottery links with the final stages of Pottery Neolithic Jericho'⁵⁰. Her own first relevant statement is that Byblos Eneolithic A and B is slightly like Sultân neolithic (p. 48). She thereupon (p. 50) regrets that 'the difficulty in deciding where to place the transition from the Neolithic to the Chalcolithic is reflected in the existing confusion in nomenclature'. But the terminology which she herself is creating and following is not clearly set forth at any one place in this volume either, and so we must dig it out of statements like this under *Halafian/Protoliterate*:

(p. 51) the first discovery of a group which seemed to belong to this period was at Teleilat Ghassul, which has given its name to the Ghassulian culture. The pottery on this site has some affinities with that of Jericho Pottery Neolithic B [no mention of the thumb-indented bricks] . . . Some fragments of cornets found at Jericho [no reference to any published source] . . . But many highly individual forms found at Ghassul are not found at Jericho . . . It may be that Ghassul belongs mainly to a period when there was a gap in occupation at Jericho . . . (p. 52) that Ghassulians were newcomers from outside Palestine. (p. 54) The household goods, tools, and weapons of the inhabitants of Ghassul differ greatly from those of any of the Jericho stages.

In this 1979 volume the Jericho Neolithic B stage is only obscurely linked with an A and C stage. The Megiddo lowest

levels, though hitherto not properly analysed, have material allied to both Jericho Pottery Neolithic A and B and Chalcolithic sherds 'with some contact with Ghassulian' (p. 77). The phrase enclosed in separate quotes cannot simply be taken to mean Ghassulian; but neither does it seem to be implied that she has criteria for making these materials more undoubtedly 'chalcolithic' than 'Ghassulian'. In the following statement about the B stage, the second sentence seems to apply rather to a C stage: (p. 80) as a date for B 'that of the third stage of Jericho and Ghassul, the first half of the fourth millennium has been tentatively suggested. There is no doubt that the period shortly precedes and eventually merges into the Early Bronze Age and at that point Palestinian stages can be linked with Egyptian chronology'.

As for changes in Sultân-Ghassul relationship brought about by Hennessy's excavation (which in the *text* she always refers to as that of the 'Jerusalem British School', presumably thus under a kind of responsibility of her own) we have two statements: (p. 57, at Ghassul) 'at least ten successive building periods were identified, with a uniform architectural style, though developments in the pottery style can be suggested [I call attention to the words *uniform* and *suggested*] . . . but it remains certain that the main occupation of Ghassul is subsequent to the end of the Pottery Neolithic stage at Jericho'; and in the footnote mentioning Hennessy's name and mine at Ghassul (p. 328) 'At the base there was a level linking with Pottery Neolithic at Jericho, with above it a long and homogeneous succession of the Chalcolithic Ghassulian'. But really the most significant sentence in the whole book is the one by which she faces up to an already existing problem of radiocarbon dating much more conservative than Hennessy's, which nevertheless (p. 65) 'leaves us in Palestine with a nasty long period to fill'.

Conclusions

There has never yet been a generally accepted chronology spreading the *whole* of chalcolithic over a period more specific than from 3500 to 3200 BC, allowing time also for an Esdraelon and/or Beersheba culture *after* Ghassulian. There has also never been any definitive evidence from Jericho to exclude, during this whole relatively mild 300-year term, that it had no *real* Ghassulian or other chalcolithic (apart from 'indirect' similarities, which basically means '*not* chronologically-probative *parallels*'). This was the lacuna with which my paper was to have been concerned.

It is obvious that much of my compilation consists in resurrecting long-forgotten description of evidence, inadequately interpreted at the time of its discovery. Even if it had been reported *adequately*, scholarship does not make progress by idolatrous exegesis of what bygone or current experts have pronounced, but rather by constant re-examination of the *facts* on which they claimed to be basing themselves; and jointly by the search for *new* facts. We venture to hope that in re-tracing the history of the older claims, a few of the facts themselves will have been thrown into a more usable light.

⁴⁹ Kenyon, *Archaeology in the Holy Land* (London: Benn, 1960) 70.

⁵⁰ Kenyon, *Archaeology in the Holy Land*⁴ (London: Benn, 1979) vii.

- 1) Relative *homogeneity* of the Ghassul material is a more generally-accepted factual datum than its subdivision into five successive strata or even typological developments.
- 2) This homogeneity would of itself seem to postulate a fairly short duration, such as would make a *total absence* of simultaneous occupation at Jericho-Sultân less implausible.
- 3) From the very start, however, certain remote or rare similarities to Ghassul in the pre-bronze Sultân materials have been noticed, always leaving open the chance that Ghassul could be a contemporaneous *local* variation due to immigrants.
- 4) All subdivisions of chalcolithic customarily taken to be later than Ghassul (Esdraelon, Tell el-Far'ah-N, Beersheba), as well as the alleged *earlier* chalcolithic (pre-Ghassulian or Ghassul I-III; Jericho VIII) have recently and plausibly been shown to be in some sense identical with the minutely-analysed Pottery Neolithic of Jericho.
- 5) This whole batch of materials is also now generally admitted identical with what had hitherto been called Early Bronze Ia, whether it now be subsumed into the Bronze Age or, as seems preferable, kept in a prior epoch.
- 6) The Ghassulian which, at least in part, is now admitted simultaneous to some EB (Ia) and also to some Neolithic could just as well be called Pottery Neolithic (hitherto held prior to 4000 BC) except for the even more alarming gap which this would create in existing archaeological frameworks (4000-3000 or even worse).
- 7) Radiocarbon dates around 3300 hitherto furnished in support of a date for chalcolithic materials outside Ghassul have perhaps been taken uncritically as the only real basis for alleged *typological* or *stratigraphic* dating of those materials (chiefly pottery, but including, of course, flints).
- 8) A radiocarbon date, like an inscription or any other solid evidence, cannot automatically disqualify the norms, *either* typological or stratigraphic, which are meanwhile justly being taken as the basis of overall excavation chronology. Radiocarbon can only suggest and demand that the pottery-criteria be *re-examined* and if necessary *revised* on their own terms.
- 9) Even a long series of radiocarbon dates does not automatically assure either the typological or the stratigraphic sequence of the materials from which the samples were drawn. As specifically as possible, each separate sample must be *independently* shown to have a) a *form* showing technically and artistically a stage of development proving (or at least compatible with) the date suggested by radiocarbon; b) a *location* in the ground amid materials demonstrably *preceded* by those which radiocarbon declares earlier, and *followed* by those it declares later.
- 10) In any case the Ghassul-Jericho comparison confronts us with an *enigma* still unsolved despite persistent efforts: in face of which there is need of bold innovating scientific hypotheses.