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PREHISTORIC ARCHEOLOGY ALONG THE ZAGROS FLANKS

Edited by

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*In memory of
three Arabian gentlemen
without whose enthusiasm, goodwill, and cooperation
the various activities of the Iraq-Jarvis Project
would never have prospered*

DR. NAJI AL-ASIL
FUAD SAFAR
ABDULLAH SAID OSMAN AL-SUDANI

PREFACE

Another four years have gone by since the Introduction to this volume was written and the substantive editing of the various reports was completed. Our interested colleagues cannot feel more frustrated with the delay than we.

The Introduction, which follows, and the third chapter in our preliminary report of 1960, *Prehistoric Investigations in Iraqi Kurdistan* (SAOC 31), account for the general development of the Oriental Institute Prehistoric Project's activities in Iraq. We wish, however, once again to note the names of our field companions and also to thank those other individuals and institutions that aided our research efforts from the beginning. The Prehistoric Project of the Oriental Institute of the University of Chicago was first activated in early 1947.

From the original planning for our field campaigns onwards through the processing of the materials for publication we have had enthusiastic cooperation and support from the accession of directors of the Oriental Institute and from the chairmen of the Department of Anthropology of our university.

The field budget for the first season came primarily from the Oriental Institute, aided by a subvention from the Department of Anthropology for their graduate student grantee, and by Robert Braidwood's release from academic duties. In the second and third seasons there were also grants from the American Philosophical Society, the American Schools of Oriental Research, and the Wenner-Gren Foundation for Anthropological Research; and the Oriental Institute and the Department of Anthropology participated as before. In 1954-55 the Department of Anthropology received the first of a series of substantial grants from the National Science Foundation for our fieldwork. And, from the beginning, we have appreciated warm interest and financial support from special friends of the Project.

We were most fortunate, from our start in Iraq in 1947, in that three officials of the Directorate General of Antiquities knew us and about our research interests. We had helped Seton Lloyd and the late Fuad Safar get their Hassuna site report published; Lloyd had once been an Oriental Institute field staff architect, and Safar and Taha Bakir (an epigrapher in the Directorate) had both been Oriental Institute graduate students. Through these three officials we came to know and greatly respect the late Dr. Najfi al-Asil, the director general.

In the field we were also fortunate in having the late Abdullah Saïd Osman al Sudani aid us as general foreman and then, later, as field superintendent. Abdullah had worked for the Oriental Institute since the Syrian Expedition's first field season in the Amuq in 1932. There was never a better foreman or a better field companion.

In Kirkuk, the town that served as the supply base for our field camp, we came to greatly depend on the openhearted helpfulness of the late Reverend Jefferson C. Glessner and his wife, Helen Glessner. Various officials of the Iraq Petroleum Company also aided us in many ways.

It has been our great fortune, overall, to have had a sequence of highly competent, interested, and sensitive field companions. These we name by the field seasons during which they served, giving their identifications as of that time (unless otherwise indicated, affiliations were with the University of Chicago):

1947-48

Fuzj Basuschi (Iraq Directorate General of Antiquities representative);
Linda S. Braidwood (Oriental Institute);
Robert J. Braidwood (Oriental Institute and Department of Anthropology);
Charlotte M. Owen (Department of Anthropology grantee);

1950-51

Robert McC. Adams (Department of Anthropology grantee);
Fredrik Barth (Ethnographic Museum, Oslo, grantee);
Linda S. Braidwood (Oriental Institute);
Robert J. Braidwood (Oriental Institute and Department of Anthropology);
Vivian J. Brunson (Morales) (Oriental Institute grantee);
Bruce Howe (Peabody Museum, Harvard University);
Sabri Shukri (Iraq Directorate General of Antiquities representative);
Elizabeth West (Fitzhugh) (Oriental Institute volunteer);
Herbert E. Wright, Jr. (Department of Geology, University of Minnesota);
Cornelius Hillen and Gustavus Swift, Jr. (Oriental Institute graduate students) served as part-time volunteers. Eleanor Swift also served as a volunteer.

PREFACE

1954-55

Patty Jo Anderson [Watson] (Department of Anthropology grantee)
 Hussain Azam (Iraq Directorate General of Antiquities representative)
 Linda S. Braidwood (Oriental Institute)
 Robert J. Braidwood (Oriental Institute and Department of Anthropology)
 Vivian L. Bronson [Morales] (Oriental Institute grantee)
 Hans Helbæk (Danish National Museum)
 Yusuf Mansur (Iraq Natural History Museum, Baghdad, representative)
 Frederick R. Matson (Department of Anthropology, Pennsylvania State University)
 Margaret Mason (Oriental Institute volunteer)
 Charles A. Reed (Medical School faculty, University of Illinois)
 Sabri Shakeri (Iraq Directorate General of Antiquities representative)
 Mayo Schreiber (Oriental Institute volunteer)
 Beverly Schreiber (Oriental Institute volunteer)
 Herbert E. Wright, Jr. (Department of Geology, University of Minnesota)
 Rhea Wright (Oriental Institute volunteer)

For short periods, Mohammed Ali and Subhat Anwar also served as Iraq Directorate General of Antiquities representatives.

(The Braidwoods' daughter, Gretel, and son, Douglas, were in camp the first two seasons and Douglas also in the 1954-55 season. In the 1954-55 season the three Wright sons, Richard, Peter, and John, were also in the field camp.)

It is now over twenty-seven years since we left Jarmo for the last time, and our experience in completing these reports underlines the fact that the end of fieldwork is far from being the end of an archeological research project. The Introduction (page 1) mentions the names of authors in this volume who were not on any of the field staffs. One, Joseph R. Caldwell, an old student and warm friend (although, unfortunately, never our actual field companion), is no longer living. We list these additional contributors and their identification as of the time they made their reports (p.s. = professional staff; g.s. = graduate student):

James M. Adkwasie (Department of Anthropology, University of Pittsburgh, p.s.)
 Joseph R. Caldwell (Department of Anthropology, University of Georgia, p.s.)
 Margaret Dittmore (Department of Anthropology, University of Chicago, g.s.)
 Frank Hole (Department of Anthropology, University of Chicago, g.s.)
 Joanne P. Laffer (Department of Anthropology, University of Illinois, g.s.)
 Barbara Lawrence (Museum of Comparative Zoology, Harvard University, p.s.)
 Hattula Moholy-Nagy (Department of Anthropology, University of Chicago, g.s.)
 Hans R. Stampfli (Peabody Museum of Natural History, Yale University, postdoctoral fellow)
 Priscilla F. Turnbull (Department of Zoology, Field Museum of Natural History, Chicago, p.s.)

The truly onerous task of editing the twenty-six manuscripts, including two that were jointly authored, as well as most of the design and production work for the text and tables, was in the highly capable hands of Heather J. Taylor, and we are most grateful to her. She bore the brunt of our own exasperation over delays with fortitude, humor, and understanding. She was assisted at various times by Pamela Bruton and Mary Evans (both graduate students) and by Paul Hoffman.

Aside from Bronson Morales's fine pencil renderings of the clay figurines and objects, various illustrators were involved in preparing our drawings. Greta F. Blair made the excellent pen and ink drawings of the Karim Shahr flints and other artifacts. Valerie Clark and Nancy H. Flannery prepared many of the Jarmo illustrations. Shirley Jean Anderson, Mary Evans, and Paul Hoffman controlled the illustrative materials for publication.

We do hope that some day new archeological research may begin again in the Cherachemical valley. Our very best wishes to whoever undertakes it!

LINDA S. BRAIDWOOD
 ROBERT J. BRAIDWOOD
 BRUCE HOWE
 CHARLES A. REED
 PATTY JO WATSON

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INTRODUCTION

Linda S. Braidwood, Robert J. Braidwood, and Bruce Howe

It is now almost a quarter of a century since the Oriental Institute's Iraq-Jarmo Prehistoric Project left the Chermahal valley in northeastern Iraq. When we broke camp at Jarmo in June 1955, we did not realize then that we were leaving the valley for the last time.

There are a variety of reasons for the long-delayed appearance of these final reports, but to list the reasons as excuses would satisfy neither our readers nor ourselves. We did, at least, provide a reasonably full group of preliminary reports within four years after leaving the field. These preliminary reports were made by the various senior members of the field staff and appeared under the title of *Prehistoric Investigations in Iraqi Kurdistan* as volume 31 of the Oriental Institute's Studies in Ancient Oriental Civilization series, hereinafter referred to simply as SAOC 31.

The present volume includes the final reports on the artifactual and nonartifactual remains recovered from all of our sites save those within the paleolithic time range; these will be dealt with in a subsequent volume. What we shall treat here is the evidence acquired from the regular excavations at the sites of Karim Shahi and Jarmo in the Chermahal valley (figs. 1, 2b) and from our brief soundings at the sites of M'lefaat, Ali Agha, al-Khan, and Banahilk, farther toward the northeast (figs. 1, 2a). The purpose of our reports on these sites is primarily to provide—insofar as is possible—an objective description of the materials exposed and recovered. These reports have been completed at various times over the last twenty years. This should be an advantage in that they were not done in a rush toward publication, but it has also inevitably resulted in an unevenness in presentation, especially in the currency of other work referred to and the degree to which interpretation has been attempted. Among other things, this situation reflects each author's individual interests, idiosyncrasies, and priorities, and we have not attempted to homogenize these unevennesses editorially.

Furthermore, it will also be apparent that the authors of some of the following sections are not always people who were part of the field staff. Unfortunately, American universities seldom, if ever, have a corps of home-based professionally trained technicians or analysts available to aid in the analysis and presentation of field-acquired materials. Also, senior field personnel, once back home, are usually inundated with academic responsibilities. Our own solution to the problem has been to allow well-advanced and promising graduate students to undertake the analysis and presentation of some of the categories of artifacts or of small site soundings. The authors of certain sections of this volume—Adams, Brennan Morales, and Watson—were junior members of the field staff, but Dittmar, Hole, Loffer, Moholy Nagy, Lawrence, and Turnbull unfortunately never saw the sites their materials came from, nor did Caldwell and Stampfli, both already at the postdoctoral level at the time they wrote.

In this introduction, we shall first include a few remarks on our field procedures and on the recording and processing of the materials recovered. Next, we turn to a very brief summary of how we viewed certain matters bearing on the appearance of an effective village farming community way of life in southwestern Asia when SAOC 31 was written in 1959. To this summary, we add a few remarks on several of our subsequent papers indicating how our ideas gradually changed since SAOC 31 appeared. Reed and Wright do the same in sections of their own and Watson has updated Helbaek's ideas through direct correspondence with him. Next comes our brief assessment of what we take to be the more important points in the following reports and our remarks on what the evidence we recovered may suggest about the ways of life of the people who once lived on the sites we excavated. In conclusion, we offer a few modest observations on current theories concerning the beginnings of village-farming communities in southwestern Asia.

REMARKS ON PROCEDURES

We do not hesitate to suggest that our normal field procedures during the three field seasons, 1948-55, were well advanced over what RJE saw during his first Iraqi field season in 1930. In the same sense, we now realize the many inadequacies of our procedures during the earlier 1950s. It is not unreasonable to claim, however, that what is taken to

The manuscript for this chapter was essentially completed by 1978. Very few additions and only minor corrections have been made since that date.

be current excellence in field procedure is usually what is practiced at sites within the boundaries of the excavator's home country.¹ In such cases, much of the actual earth moving and even all of the field processing are likely to be done by students or intelligent amateurs who have at least some comprehension of the goals of culture-historical research and with whom the senior staff has no barrier either as to language or ethos. This is a type of situation which excavations by foreign archeologists in southwestern Asia do not normally achieve.

In our own case, we employed up to forty local workmen (when the excavations at both Jarmo and Karim Shahir were in work together) along with six to eight Shergati pickmen. The Shergatis are that guild of now third- and fourth-generation highly skilled archeological workmen who come from the town of Qalar Shergat (ancient Assur), where their forefathers were first trained by that site's German excavators in the period before World War I. Like our Egyptian field superintendent, Abdullah, the Shergatis were of course native Arabic speakers, but the locals were Kurds who had learned Arabic as army draftees. Their Arabic—and our own—was "basic" at best and we had no Kurdish whatsoever. They had little comprehension of our culture-historical goals in digging, and in the matter of Weltanschauung or ethos there was little common understanding.

The Shergatis' skill sometimes led to complications. Highly competent but also very proud, the Shergatis bitterly resented the sieves with which we began our excavation routine. For this reason, we tried restricting the use of sieves to situations where exceptionally rich floor debris was encountered, making frequent unannounced spot checks of our dumps, and assessing stiff fines on any careless pickman. Indeed, an advantage of the baksheesh system of rewards is that a fine (*jaiza*), if imposed, is also taken in good spirit. The number of individual kernels of carbonized grain, along with the number of minute microliths recovered, makes us firmly believe we lost little evidence which sieves could have saved. Further, the restriction of their use contributed much to the good morale we maintained with both the Shergatis and the locals. In addition, considering the relative toughness of the matrix we usually encountered, the use of sieves would have seriously slowed down our already slow pace of work (given the number of workmen our budgets would allow). Also, there would have been more general breakage in routinely forcing tough chunks of matrix through sieves.

We did not begin work with what is now known as an intensive surface survey. We did stake out our areas for exposure with respect to marked surface scatters of artifacts, but we also took topography, convenience for dumpage, and (in the case of Ali Agha) the presence of a cemetery into account.

No attempt was made to keep our exposures strictly rectilinear when interesting architectural features appeared. Because of the previous experience which Abdullah, the Shergatis, and we ourselves had had with clearing sun-dried mud-brick walling, we had no particular problem with the *tauf* (simple loaded mud) walls of Jarmo, once we were below the line of marked rainfall penetration (p. 156).

We worked without balks but took frequent level readings. Our section drawings were developed with the aid of these and the trench faces as we made the drawings of each level with a plane table and telescopic alidade.

Of the various new field procedures which have come into vogue since we dug, flotation for the reclamation of botanical materials (and of minute bones and even artifacts) fascinates us most. Nevertheless, we feel bound to wonder what would have happened to much of the lightly baked or sun-dried clay categories, for example, unless flotation had been very sparingly and cautiously used.

For purposes of assessing the proportion in the original universe from which a sampling of archeological material has been reclaimed, we believe there is value in listing the number of square meters of area exposed and the cubage of matrix removed (cf. Braidwood and Braidwood 1960, table I). While we naturally reject the purely mechanistic use of such figures (since accidental encounters of particularly rich or particularly poor yields can never be anticipated), areas and volumes exposed do hint at the relative values of generalizations from sites with very restricted exposures as against those with broader exposures.

For the Karim Shahir hilltop, the area of surface scatter totals about 6000 m². How much of this includes the original settlement and how much of the settlement was eroded away by the wadi are of course unknown. BH's exposures totaled almost 600 m², or about 10% of the hilltop. The area of the remaining settlement core exposed along the scarp edge itself (operation I) was about 400 m²; the amount of matrix removed was 520 m³. Certain other exposures were made (see p. 28), but these appear to have been outside the core area of occupation.

As to Jarmo, if we follow Wright's estimate (p. 155) that about one third of the original site had been eroded away by the wadi, we would estimate the area of the site as we first found it to be ca. 13,000 m². Our rechecking indicates that of this total, ca. 1,372 m², or 10%, were exposed in the near-surface levels. Only 1.2% exposure was made at the level of virgin soil, however. If the area presumably lost by erosion were taken into account, our upper level exposures would total only 7% of the original site. The amount of matrix removed in all our exposures totals ca. 2,290 m³. Since there seems to have been some confusion over these data from Jarmo (see Renfrew, Dixon, and Cunn 1966, p. 52), we provide the following tabulation:

INTRODUCTION

Operation and level*	Approximate area and depth exposed		
	Area (m ²)	Depth (m)†	Volume (m ³)
J-I			
1 & 2	86	2.80	196
3	105	0.60	63
4 & 5	105	0.70	74
6 & 6a	105	0.40	42
6b-d	98	0.05	5
7	98	0.70	69
8 & 9	144‡	0.65	132
Total, J-I	--	5.60	573
J-II (incl. Lt213 where pertinent)			
1	995‡	0.90	118
2	895	0.65	255
2II	995	0.25	98
3	895	0.90	118
4	201	0.45	90
5	201	0.65	131
6	95	1.05	100
Total, J-II	--	3.65	910
J-III	25†	1.75	44
J-A (see fig. 57)	50†	6.90	110
J-B, C, & D	96†	1.00	96
HLJ-116	96†	1.90	96
PQ11	18†	3.50	63
180 2 × 2 m squares	604‡	average 0.75	453
Grand total	1,572		2,246

*For an explanation of operation/level designations, see pp. 163ff.

†Reference to the sections, e.g., figs. 37-38, 46-49, will show how the measurements between different floor levels vary, depending on where they are taken on any given vertical line.

‡These are the figures used in arriving at the area totals.

The excavations made at Ali Agha, Banahilk, al-Khan, and M'lefaat were only soundings, and thus—in conformance with the Iraqi government's antiquities law prevailing at that time—were very short-term affairs. Given the very limited time we had for the examination of each of these sites, our estimates of their original areas of occupation may be open to question. We list here the *apparent* site size, the approximate areas of our exposures, and the approximate amounts of matrix excavated:

Site	Total apparent area (m ²)	Total area exposed (m ²)	Percentage of apparent whole	Volume excavated (m ³)
Ali Agha	7,000?	(A & B) 55	0.5	34
Banahilk	13,000?	108	0.8	154
al-Khan	7,000?	35	0.2	23
M'lefaat	6,500?	II & III: 38	0.4	40

At the time we worked, the Iraqi Directorate General of Antiquities required that a field register book of the artifactual yield be kept in duplicate, with one copy to go to the Iraq Museum. There was also at that time a division of the finds: first, unique objects were automatically registered and set aside for the Iraq Museum, then a half-and-half division of the remaining registered pieces was made between the Museum and the excavator. At the end of each of our three seasons, we felt we had received very fair treatment. The nonartifactual materials were directly assigned to us for laboratory study abroad. We were most fortunate, too, in that the Directorate General allowed all of the figurines of the 1954-55 season to be returned to Chicago for Bruman Morales's study and postponed its selection of unique examples and the subsequent division of that season's material until after she had returned the whole collection to Baghdad.

Furthermore, as regards registration, the Directorate General, acting on behalf of the Iraq Museum, was at that time not interested in acquiring a full half share of the bulk categories of artifacts such as potsherds, flint or obsidian implements, and coarse ground-stone pieces. The Directorate was quite willing for us to make up small representative samples from these bulk categories for the Museum. Since all registered pieces were automatically required by the

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antiquities law to be treated within the division, we were not encouraged to register the surfaces of the bulk categories and hence we did not do so. Instead, these artifacts of the bulk categories and, in addition, most of the very fragmentary pieces in some other categories were simply marked with their findspot designations. Therefore, in order to aid colleagues, either in Baghdad or Chicago, who may in the future work with the collections of the materials we recovered, we offer the following aid:

Karim Shahir. Registered objects were marked K- followed by a number in sequence (K-1, K-2, through K-52). Unregistered objects were marked in a code, in order to simplify the task of labeling in the bulk categories, thus:

Area	Depth (cm)	Code mark	Area	Depth (cm)	Code mark
Surface (sf)		K	Grid F	0-20	—
Operation I				20-30	—
The step	0-20	K10	Grids A, B, F, E extension cleanup	30-30	I
	0-40	K11		0-20	II
	40-50	K10	Grid G	20-30	III
	(outer edge)			30-40	IV
	40-50	K12		40-50	V
	(interior block)			50-60	VI
	60-120	K13		60-80	VII
	120-160	K14		80-100	VIII
	160-180	K15		100-110	GI
	(chic pit)	K1P		110-120	G2
Grid A	0-20	□		120-140	G3
	20-30	□	The trench (tr)	0-30	J
Grid B	0-20	K1B1	Operation II	0-20	II
	20-30	△	Operation III	0-20	C
Grid C	0-20	○		20-30	D
	20-30	○		40-60	N
	30-40	○		60-80	M
	40-50	○		80-100	P
	50-60	○		100-300	III
	60-80	O1	Operation IV	0-20	R
	80-90	O2		20-30	T
	90-100	O3		40-60	Y
	100-110	C1		60-90	Z
	110-120	C2		80-280	IV
	120-140	C3	Operation V	0-20	■
	140-160	C4		20-25	◆
	160-180	C5	Operation VI	0-20	V1
	180-380	C6	Operation VII	0-20	VII
The extension (ext.)	0-20	└	Operation VIII	0-20	VIII

Jarab. Registered objects were marked J- for the 1948 season, J2- for the 1950-51 season, and J3- for the 1955 season, in each case followed by sequential numbers (e.g., J2-134). Unregistered objects were marked either directly as to findspot or by a code. For the findspot designations:

In the 1948 season, operation I was marked simply "J" with a level below, thus—

J
3

For the test squares A, B, C, and D, their letter with either the level or the depth (in cm) below it, thus—

J-B
2

or

J-B
25

In the 1950-51 season, the actual findspot was marked with the operation number above (with or without a preceding "J") and the level below, thus—

I
6b

or

J-I
6b

II
3

or

J-II
3

III
1

or

J-III
1

A
IV-1

or

J-A
IV-1

or a code (see p. 5) was used.

In the 1955 season, the letters and figures of the 4 × 5 m coordinate grid system were used for each of the 2 × 2 m test squares, with the level or depth (in cm) marked below, thus—

K8
3K21
175

For groups of the 2 × 2 m squares, where the depths were still shallow, we often labeled the groups together as a general area, thus—

HJ.1116
50

which should be read "squares in the area bounded by H14 to H16, L14 to L16 at depths of ca. 50 cm."

The Jarmo code for simplified marking, according to operation, is:

<i>J-I</i> (or simply "J")		<i>J-II</i>		<i>J-III</i>		<i>J-IV</i>	
<i>Level</i>	<i>Code</i>	<i>Level</i>	<i>Code</i>	<i>Level</i>	<i>Code</i>	<i>Level</i>	<i>Code</i>
sf or dmp	≡	sf or dmp	X	sf or dmp	V	sf or dmp	≡
1	none	1	Y	sf-1	△	II-1	A1
2	none	1 East	E	1	J	II-485	A2
3	△	2	S			II-687	A3
4	none	2L	I			III	A4
5	none	3	□			III-1	A5
6	none	4	4			IV	A6
6a	A	5	W			IV-1	A7
6b	B	6	+			IV-V	A8
6c	C					V	A9
7	Z						
7a	z1						
8	O						
8a	01 or 9						

NOTE: The symbols and abbreviations of the Jarmo code can be used to identify artifacts that came from the operations and levels indicated in the above tabulation. However, other excavation units exist that were *not* coded. The uncoded units indicated by "none" in the second column above, as well as other levels not referred to in this tabulation, can be directly located by their findspot designations.

Code symbols used generally by us since the early seasons in the 'Armag are:

Surface ≡ also *sf*
 Surface to first clear level ≡-1 also *sf-1*
 Dump or matrix with lost context □ also *dmp*
 Floor ⊥ also *fl*
 Pit or *sho* ⊕ also *pit*

The same general principles in labeling were used in marking the materials from the various soundings, thus:

Ali Agha = AA, plus the operation, plus the level
 Banahilk = BH, plus the operation, plus the level
 al-Khan = EK, plus the operation, plus the level
 M'lefaat = ML, plus the operation, plus the level.

The materials of the bulk categories were all marked individually, in the manner indicated above, then the whole yield from a unit findspot in such categories as heavy ground stone and pottery was put into a stout cloth sack. This was tied with a good-quality string, to which was attached a stout baggage label also marked with the findspot. For more fragile materials were packaged in labeled boxes.

We cannot claim that the artifacts within each of the different categories received the same degree of exhaustive study when being processed for publication. Naturally, the finer ground stone and the figurines attracted our early attention. At the same time, however, these were the categories that we were required to divide with the Iraq Museum and they were thus not all available for study in the Chicago laboratory for a long period, unlike most of the bulk material. Perhaps this tends to equalize the matter. We are, however, conscious that certain groups of artifacts, such as the heavy coarse stone mortar and queen group, should have received much more attention than we gave them.

We are also conscious that our petrographic analyses are still inadequate and that certain smaller collections such as the marine shells remain unstudied. It is also hoped the time may come when reliable identification of the source areas of many minerals is possible.

In 1952, with impressions of our first two field seasons very fresh in her mind, LSB wrote a popular account of the expedition's work (L.S. Braidwood 1953), which gives a general picture of our field procedures.

RECAPITULATION OF PRELIMINARY WRITINGS

COMMENTS UPON REREADING SAOC 31

We see little point in a detailed repetition of the Prehistoric Project's general history through the time of the Iraq field seasons or of our original conception of the Project's research goals. SAOC 31 does, we believe, adequate account for most of these, and its bibliography also cites our other papers which appeared up to 1960, although the SAOC 31 manuscripts were completed in 1959.

In retrospect, our general views as set forth in SAOC 31 probably can be taken as a summation of the ideas on the problem of the early appearance of the village-farming community way of life—at least in the trans-Euphrates portion of southwestern Asia—as these ideas had developed up to about 1959. With the exception of a brief note on Shimshar and the Soleckis' preliminary accounts of their earlier seasons at Sharidar and Zawi Chemi Shanidar, nothing, in fact, was in hand that related to the critical time range except the Prehistoric Project's materials. While it seemed clear to us (SAOC 31, p. 3) that there were other instances of the independent achievement of food production in other parts of the world, we certainly took the Near Eastern instance to have chronological precedence. Regarding the transition to food production and village-farming communities, we wrote (SAOC 31, p. 1) that

on the upswing to the transition we note (1) a growing ability (and perhaps some motivation) for intensifying and localizing the exploitation of a given piece of terrain by food-collectors. . . .²

When SAOC 31 appeared, our earlier fascination with the "hilly flanks of the Fertile Crescent" as an environmental "nuclear habitat zone" within which the domestication of plants and animals appeared had become only slightly tempered. We wrote (SAOC 31, p. 13) that

there is little doubt in our minds that the nuclear area for the appearance of the village-farming community in the Near and Middle East can—as a reasonable working hypothesis—already be taken to coincide with this zone. . . . Obviously, a major feature of the testing of an environment will be to discover the degree to which the present observable situation resembles, or is different from, the situation which obtained some eight or ten thousand years ago. Did the zone then, as it does now, naturally contain potentially domesticable plants and animals? We shall see that the available evidence suggests far greater similarity than difference.

However, in the field in the same year that SAOC 31 appeared, Wright was to begin his palynological work in Iraq which brought new evidence suggesting that at least the climatic regime in our hilly flanks at the end of the Pleistocene was not that of the present!

Throughout SAOC 31, we noted that there were many regions from which evidence was then lacking:

. . . we know as yet practically nothing of the earlier culture history of the zone of foothills and intermontane valleys in the Iranian, Syrian, and Turkish portions of the arc [p. 13].

And although Stuart Harris's surface finds at Urwell and Rasdien (SAOC 31, p. 49) had prepared us for what might appear on the lower piedmont at Ali Kosh and Tamarshan, we were clearly not even imagining what was yet to be found along the middle Euphrates at Mureybit and Abu Hureyra or in the Syrian desert at Kowm.

As to more specific points covered in SAOC 31 regarding the sites we excavated and their yields, we are still inclined to believe that Karim Shahr represents the remains of a short-term encampment (SAOC 31, p. 28). Had we returned to the Chemchemal valley in the later 1950s, we would not have resumed work at Karim Shahr; its very shallow deposit did not augur well for comprehensive architectural remains or for the recovery of contextually trustworthy nonutilitarian or dating evidence. As already suggested in SAOC 31 (pp. 182-83), such sites as Turkaka and Qara Chawat would have had our attention.

SAOC 31 (e.g., p. 39) also makes clear that when we left Jarmo in 1955, we realized that it had not been "adequately tested" and that a layer-cake simplicity of stratification was certainly not the case. Our earlier generalizations, based on the more restricted 1948 and 1950-51 exposures, were that "pottery appeared in the upper third of the deposit," that is, it seemed as if there might be pottery anywhere we dug within the uppermost levels and no pottery below that. The proposition for an absence of pottery in the lower levels quite clearly must still hold throughout Jarmo. However, by the end of the 1955 season we had acquired evidence that if we had made our exposures in certain other areas of the mound, we would have found no pottery even in the upper levels, although SAOC 31 (esp. p. 39 and fig. 6) then made the point only tentatively. We now know that the "upper third" generalization is vastly overgeneralized (to coin a phrase) and that the presence of pottery as a facile indicator of a late or "ceramic subphase" (or even a full "phase") as against an early or "preceramic subphase" (or even a full "phase") at Jarmo cannot—on present evidence—be maintained. There was, of course, a sequence through time in the occupation of Jarmo; we undoubtedly have a strong sampling for either end of this continuum but lack adequate evidence of its middle. For example, Adams's chart 2 (chap. 3) illustrates this particularly well with respect to certain stone vessel shapes.

In chapters 12, 13, and 15, Watson, Wright, and Reed give their own present reflections on the treatment given to the botanical, climatic, and faunal evidence in SAOC 31.

SUMMARY OF IDEA CHANGES SINCE SAOC 31

Our surveys in Braidwood and Willey, eds., 1962² (esp. pp. 152-46, 530-59) and in Braidwood 1962 (pp. 115-26) were written after our 1959-60 work in the Kermanshah valley in Iran. In both of these surveys we seem to have felt increasingly unable to specify either the approximate beginning date for—or the duration of—our suggested range of incipience, seeing it “perhaps as the culmination of a longer range of unconscious manipulations of the potential domesticates” (see RJB and BH in Braidwood and Willey, eds., 1962², p. 144). In that same volume (p. 537) we are very clear in that while incipience “was attained within a natural-habitat zone . . . we do not yet know the exact boundaries of this zone,” and we noted also the very probable importance of physiographic and environmental diversity within the natural-habitat zone as the “settling-in” process proceeded.

By the time a short paper titled “Current Thoughts on the Beginnings of Food-Production in Southwestern Asia” (Braidwood and Braidwood 1969, pp. 149-55; appeared in the *Dunand festschrift*, we ourselves had already had two seasons at Çayönü (Çambel) and Braidwood 1970). In addition, Suberde had been opened in Turkey; Ganj Dareh, Gurun, Choga Mami, and Ali Kosh along the Zagros flanks were known; work had begun at Mureybit on the middle Euphrates; and the sites of the French colleagues and Beidha were being excavated in the Syro-Palestinian province. Furthermore, the implications of the palynological evidence acquired by van Zeist and Wright (1963) in the Iranian Zagros were beginning to appear. Jean Perrot’s (1962) proposal that the Natufian was part of a long-continued level of intensified food collection in Palestine was taken into account, and it seemed equally clear to us that “villages without food production” had existed in the Near East. Further, we wrote (Braidwood and Braidwood 1969, p. 153) that

any distinction between a level of intensified food collecting and of incipient plant and animal domestication may be more a matter of semantics than of reality.

In the earlier 1970s, RJB wrote three retrospective summary papers (Braidwood 1972, 1973, 1974)—the third almost a brief autobiography of the Prehistoric Project and of himself. Each of these papers reflects discussions which the three of us had about the implications of the considerably increased archaeological activity—we even said “fashion”—concerning the problem of the origins of food production in southwestern Asia. Each paper also shows, among other things, our attempts to consider the full implications of the Wright-van Zeist palynological evidence (Wright 1968, 1970), but without declaring ourselves to be complete environmental determinists. We did not then and still do not think the sum total of available evidence warrants this position. At the same time, our fascination persisted with the problem of whether it was meaningful to think of an inchoate or incipient “level,” *per se*, in the appearance of food production. RJB’s (1972, pp. 317-18) formulation of our position was that

the conceptualization of a level of incipient cultivation and domestication—as distinct from that of a terminal specialized hunting and collecting level—has always remained difficult, and the field identification of the incipient level has been even more difficult. As a heuristic device, the idea of a level of incipience may have convenience for us, but would have meant little to the people who were living in it, doubtless still by predominantly very specialized patterns of hunting and collecting.

In his 1973 paper, RJB wonders, in effect, whether sites such as Asikli, basal Ganj Dareh, and Suberde may represent either a very late phase of the general level of incipience or a markedly pre-Jarmo phase of the general level of effective village-farming communities.

In the most recent of our (far too many!) summary papers before the final reports appearing in this volume, RJB (1976, pp. 41-49) again attempted to make our general ideas current. The paper maintains a generally agnostic point of view (Braidwood 1976, p. 64):

Exactly how domestication itself came about, I frankly do not know, nor have the several recent “explanations” seemed to me to make adequate use of the available evidence or to have respect for the lack of it. Wright, himself, perhaps quite naturally, has swung towards a degree of environmental determinism. My own (perhaps overly cautious?) tendency, given the very restricted number of sites and of exposures on them, for the incipient phase, is to patiently await more evidence.

Thus, since 1959 when the SAOC 31 manuscripts were completed, our main theoretical concerns appear to have been with the conceptualization of meanings for the still very restricted evidence of incipience or inchoateness—in other words, the actual transition from the end of the terminal hunting-collecting era to that of the primary village-farming communities. The Braidwoods and Reed recall a memorable luncheon with V. Gordon Childs in London in 1955, when we were enroute home from the last Jarmo season. Childs, with a sparkle in his eye, remarked that it would be exactly with the interpretation of the evidence for the transition that we would have our greatest difficulty.

It has always seemed to us much less difficult to conceptualize what was going on, when thinking about the evidence from an inventory such as Jarmo’s. Perhaps we are wrong in this feeling. Perhaps it comes from our having lived during many field seasons either in or closely adjacent to simple present-day villages of the Near Eastern peasantry in the Zagros-Taurus hill country, and therefore perhaps we make too facile an assumption that life in a site such as Jarmo was very similar. Certainly, however, we have no illusions that we have ever observed a way of life which can conceivably be close to the one reflected by the Karim Shahir inventory. In any case, while inventories and

exposures of the Jarmo type of sediment are by no means yet adequate for really broad-scale culture-historical interpretations, they do say more to us than the far too few excavations on sites of the Karim Shahir type. In the west, of course, the increasing number of Natufian and even Kebaran exposures makes the situation there somewhat clearer, but the processes of change there may also have been somewhat different.

ASSESSMENT OF THE PRESENT REPORTS

KARIM SHAHIR

The assemblage found in the single prehistoric occupation of Karim Shahir points to an open-air hilltop encampment tending to be centered near the edge of a scarp but probably associated with small, shifting mullers. Here, at an unknown date, an undetermined number of individuals appear to have lived for some period of time, or come and gone over shorter periods, and carried on the routines of daily existence, which included the making of tools, constructing simple makeshift shelters, and preparing, cooking, and eating wild animals they had hunted and wild plants they had collected. Their sojourns here were probably seasonal or at least only periodical. The possibility of a single uninterrupted occupation of limited or longer duration cannot be excluded, but the apparent absence of well-developed more permanent sorts of dwelling structures argues against this interpretation. While there most likely were shelters of some sort here, they are not really demonstrable, and the only evidence for possible living emplacements and installations of the more temporary sort is the following:

1. An extensive and vaguely defined bed of field stones and stream pebbles at the edge of the scarp. These were somewhat closely set in one limited location and more widely scattered elsewhere, and all had been specially brought up to the otherwise silty hilltop site, suggesting the preparation of the area for some sort of shelters there.
2. Eleven small pits of varying size and depth and at least two shallow, broader depressions. The pits are presumed from their contents to have been fire or cooking pits. The depressions may have provided focal points or rudimentary circumscribed refuges or protection a little below the surrounding ground level for some sort of passing activity needing a shelter, although it is also possible that they were completely natural.
3. A deep pit that had a coating of red ochre over its bottom and yielded two small clay figurines suggesting stylized human or animal forms. The association of these elements points to some special function of a distinctly ceremonial nature, which might have been linked to structure or shelter.
4. Two lumps of heat-hardened ruddy red clay, one with impressions of vegetable matter including a deep, straight groove (20 mm wide) evidently left by a stick or branch. These lumps hint at the existence of dwelling structures—perhaps wattle and daub—but their actual significance in this regard is very debatable (and perhaps should be entirely dismissed), coming as they did from a near-surface context in an eroding scarp-edge zone of the single prehistoric occupation horizon.

The ubiquitous remnants of a plentiful chipped stone industry, evidently manufactured on the spot, were found crammed in among the rocks—in particular large quantities of blade cores, blades and blade tools (both normal-sized and microlithic), with double-backed drills and numerous microlithic backed blades being the most carefully formed and outstanding types. There were moderate numbers of indifferent burins and of side, steep, and rounded scrapers, and a handful of poorly defined special forms such as denticulated, pointed, or obliquely truncated blades. Twenty-four unworked pieces, mostly blades, bore faint and limited traces of silica edge sheen. By far the most numerous single formal tool category of all was the one that comprised, for the most part, poorly made dented end scrapers on blades or flakes, both normal and microlithic. Most of these end scrapers appear to have been created by use and wear rather than by means of any deliberate retouch. There were also enormous numbers of ribbled, notched, or variously used blades and lesser quantities of undistinguished similarly marked flake tools, all displaying a mingled assortment of edge wear—several kinds on an edge—and thus doubtless multiple-use implements. Some of these pieces may have been scarred only by natural attrition in the ground. The sheer quantity of all these chipped tools and of the additional unworked and presumably featureless chipping debris found indicated a major set of activities at the site. It is assumed that these flint artifacts all belong to categories associated with various routine activities having to do with clothing, equipment, and the preparation and eating of wild foods. The backed and angled microlithic bladelets may perhaps be seen as possible components of composite forms hided in wood or bone to serve as weapons.

Ground stone tools and ornaments constitute a numerically very minor but significant and characteristic element of the Karim Shahir assemblage. The principal types that make up the main part of this working kit of ground, pecked, and polished tools include numerous chipped and polished celts of both adze/axe and axe forms, as well as two more fully ground celts of small, narrow chisel form; the group of fragmentary milling stones comprising a very few grinding stones (querns), boulder mortars, some used pebble pestles, extremely rare shaped and ground pestles, and a very limited number of hand rubbing stones (mullers); quite a few distinctive small grooved rubbing stones, seen as possible shaft straighteners or perhaps smoothers for soft stone ornaments; and numerous hammerstones. Completing the body of artifactual material that probably reflects, directly or indirectly, a considerable proportion of

the activities and functions carried on at the encampment are a fragmentary decorated palette; a few striated and pecked pebbles perhaps used as awls, whetstones, workstones, or polishers; some fragments of pierced pebbles possibly once attached to sticks as dibles or maceheads; and an assortment of bits of red and yellow ochre, scanty and mostly unsatisfactory evidence of bitumen, and some obviously worn lumps of a distinctive heavy, iron-filled, cherry black rock, or lodestone.

The adze/hoe and axe forms imply soil working and woodworking or perhaps skin dressing. The blades with latent edge sheen and the milling stones imply the manipulation of plants (presumed to be wild in the absence of any contrary evidence), with both these categories rare and seemingly incipient here. On the other hand, the types and quantities of chipped stone tools suggest a strong continuing tradition of collecting and preparing materials according to long-standing practices and with long-familiar tools used in earlier horizons of a hunting and food-gathering existence. At the same time the limited typological range and debased technical aspects of a number of these artifacts, when compared to the technotypological variety and elegance of the preceding late upper paleolithic period, definitely imply partial abandonment of old forms and a shift toward newer usages. The chipped and polished celts, the fine double-barked drills, and the backed blades/lancelets, together with the milling stones and pieces with edge sheen, perhaps signify the new more vital trends.

The ornaments found at Karim Shahir, primarily of ground stone, were perhaps made on the spot like the tools. They are present in very small numbers and are largely fragmentary. Included in this category are marble rings and so-called bracelets; a stone rod; simple, largely unshaped natural pebble pendants; well-shaped discoid, oval, or angular plaques and pendants made of marble or limestone, and pierced; some simple cylindrical tubular or barrel-shaped stone beads; and an outstanding form of biconical planoconvex stone bead with doubly pierced ends, possibly a plaque or button to be appliquéd. Present in extremely limited quantity are some small rectangular pierced shell plaques, some natural and virtually unshaped tiny shell beads, and a few badly preserved tubular beads of bird bone, as well as a small number of fragmentary tools suggesting awls and pins.

All these more finely finished ornaments and implements of stone, shell, and bone reflect some kind of a preoccupation with apparel and appearance and hint at the preparation and decoration of clothing or other equipment. Two tiny clay figurines and a fragmentary clay rod or stalklike object surely represent some manipulation and light baking of clay and when considered in conjunction with the pit faced with red ochre hint at some unknown ideas and ceremonial activity.

The animal bones recovered indicate a mode of life centered on the hunting of wild animals, with no evidence for domestication. Basing his opinion on a sampling of materials that was very small and poorly preserved, Stenopff reports that sheep and goat evidently made up virtually half of the kill. Boar, deer, gazelle, wild cattle, fox, wolf, marten, hare, and bird were each present in small quantities. All are types not unknown in the general region in historic times and are considered indigenous there in prehistoric times. It is not clear whether the predominance of sheep and goat represents only easy availability or else selective or seasonal hunting or even some further manipulation. The small sample permits no speculation about the possibility of age selection; only adult specimens of sheep and goat and of all the other species were represented. Restricted numbers of land-snail shells and rare fragments of freshwater clam shells, as well as some turtle bone (but no fish bones), attest to further possible items of diet from indigenous forms that are still extant. The widespread littering of the site with these fragmentary faunal remains, together with the presumed cooking and fire pits (in some instances containing obviously burned rocks and bones), indicates that the preparation, cooking, and eating of these foods took place repeatedly there. Examination of the scarce charcoal remains recovered showed the presence, probably nearby, of *Zizyphus*, a tree of the elm family, and, perhaps more distant, of *Tamarix*, a tree of stream valleys in semiarid regions, and possibly *Prosopis*, a mesquite-like dry grassland form. Doubtless because of the shallow deposit at Karim Shahir it was not possible to obtain any evidence of nuts, seeds, grains, and the like, although the rare grindstones, mortars, hand rubbing stones and pestles, and the pieces with edge sheen indicate the likely presence and preparation of such foodstuffs in a form one must assume was wild.

Karim Shahir was, then, a sizeable, active, and in all probability seasonal—or at least periodic—encampment of migratory hunters and collectors, of unknown number and time, who lived off wild animals (primarily sheep and goat, but also some boar, deer, gazelle, cattle, and smaller forms such as fox, wolf, marten, hare, turtle, birds, and molluscs) and wild plants (no evidence of specific edible plant materials was found, but the variety of milling stones and the trace of silica sheen on the edges of a very small handful of blades and flakes surely permit this assumption). These animals and plants were obtained in the immediate district and possibly further afield as well; the animals range from the gazelle, an open land species, to deer from forest areas, to sheep and goat from higher terrain. Even the severely restricted sample of tree and plant forms gives evidence of disparate and contrasting habitats—for example, dry grassland, semiarid stream valley, and perhaps parkland or forest. Some of these different areas may very likely have been a considerable distance from the Karim Shahir encampment.

Clothing, equipment, and shelter are barely hinted at—and, even so, only indirectly—by the scanty traces of tools such as awls and pins or needles, and ornaments such as beads, plaques, pendants, bracelets, and rings. Similarly, we have only the various pits, depressions, and the rocky scatter to indicate focal points for possible habitation. The tool kit was made up preponderantly of chipped stone implements—a blade industry marked by a limited variety of tool types—and a small number of chipped and polished celts suggesting hoeing, adzing, axing, or perhaps even skin-

working enterprises. The entire artifactual component was evidently made on the spot out of locally available stone, bone, shell, and presumably wood, as well as other vegetable and animal materials now lost. There was not a trace of obsidian or any true and acceptable geometric microliths worthy of the name to be associated with the entire horizon. Evidence for the use of bitumen in the hafting of adze/hoes and perhaps certain other chipped stone tools lay in traces of this substance on some specimens and in suspiciously similar-looking dark crusty matter on certain pebbles. The use of bitumen here may imply familiarity with sparsely scattered natural seeps not only in the nearby region but also further afield to south and west (chap. 16). Finally, the lightly baked clay pieces (e.g., the figurines), the pit lined with red ochre, and a few ochre-smeared tools may reflect ritual procedures woven into the life at this campground.

JARMO

In the years since 1959, when the preliminary reports of SAOC 31 were actually finished, we have noted a variety of points and problems of importance which—had excavation ever been resumed on the site—would have needed further attention. Certainly more survey, with respect to both ancient sites and the present situation in the same area, could improve on the bare essentials of population estimates and settlement patterns made by Braidwood and Reed (1957). That paper noted the presence of at least two Jarmo-like sites within less than 5 km of Jarmo itself; it also proposed that Jarmo might have had as many as twenty-five households, averaging about six people each. Thus a village population estimate of about 150 inhabitants was made. However, a more intensive study of the present situation in the Chemuchemal valley, both as to the location of ancient sites and modern villages (assuming of course that the situation is not greatly changed from what it was in 1955!), would undoubtedly be rewarding. A caveat about any conclusions that might be drawn would be the very heavy degree of erosion that has taken place in the Chemuchemal valley since Jarmo was occupied; many neighboring sites may have been completely eroded away.

Not only do we lack substantial evidence for the regional settlement pattern of the time of Jarmo (and of Karim Shahir) but also our attempt to recover the settlement plan of the village itself was not successful (pp. 158, 164). We did recover more or less comprehensible indications of some Jarmo house plans, and we appear to have at least one instance of separate houses built contiguously and without the use of party walls (p. 161). This might, of course, mean that there were many more than twenty-five households within the area we reckon to have been occupied. One cautionary note here follows from observance of the usual situation in modern villages: there are usually some unoccupied houses already slumping into ruin while other houses flourish nearby. In dealing with the remains of a long-extinct village, exquisitely detailed and broad exposures would be required in order to prove the exact simultaneity (or lack of it) of all the houses on one "level." Thus a simple count of all the house remains on any one "level" might lead to a very exaggerated population estimate.

We have no really good evidence that might help us specify the use to which the various rooms in a given house were put. Considering what has been observed in other recent exposures at sites such as Çayönü and Ulmür Dağahiyah, the small size of many rooms in the Jarmo houses was not unusual; in fact, some small "rooms" may have been little more than storage bins.

Other outstanding architectural problems of detail remain. What, for example, is the meaning of the tough and blocky orange buff silty clay zone (p. 155) and also of the gray to black ashy zone (p. 164)? The thick tauf wall in PQ11 (p. 165) certainly needs more attention, and a long trench to yield the stratigraphic profile between operations J-I and J-II would be most instructive; among other things, it might help to establish what went on in the now largely missing "middle" levels of the site.

An overall (not only architectural) concern is with what facilities the Jarmo people made for grain storage and the enfolding of animals. We did not encounter evidence of grain storage save for the appearance of three pits in J I. Furthermore, since the inner surfaces of these J-I pits appeared to be untreated, their use for grain storage is questionable. The real significance of the fairly numerous Jarmo ovoidlike features (p. 157) also still eludes us.

The meaning of the discontinuity in the appearance of pottery in different areas of the site (p. 155 and fig. 24) remains unclear, and we lack an explanation for Adams's "midden" (p. 215) in the western portion of J-II and westwards. It would be good, also, to know more of the uses to which the various stone and pottery vessels were put, and—in this connection as well as among others—we were unfortunate in not having encountered any very significant caches or "activity clusters" of artifacts.

The problem of how the use of pottery was first introduced to the Jarmo villagers remains open, and, although we reasonably suppose it to have been initially an introduction from without, was the later coarse pottery developed on the site itself? Adams is properly concerned with the "high esthetic achievement" which the stone vessels show and with the degree of specialization implied by the time-consuming craft by which such great numbers of stone vessels were produced.

The abundant chipped stone industry at Jarmo—both flint and obsidian—is predominantly a blade industry. Since all chipped stone work tends to be conservative and traditional, it is not surprising that many of the tool types in the Jarmo kit, types both obvious and not so obvious, are a repetition of those found in previous eras in the general region; hoppers, various scrapers on both blades and flakes, notched blades, fabricators, burins (rare at Jarmo and poor examples), backed blades (rare), geometric microliths but only triangles and trapezes (upper Jarmo levels), and,

above all and in overwhelming numbers, blades and bladelets and their segmented fragments, used "as is." Undoubtedly, most of these last items served as all-purpose tools.

An outstanding feature of the Jarmo industry is its microlithic character. There are large flint tools throughout the levels and a limited number of obsidian blade fragments that are relatively small but still broader than 50 mm (on cutoff width for "microlithicness"). The flint and obsidian microliths *together*, however, make up at least 70% of the chipped stone tools in the J-I as well as the J-II levels (and blades and blade fragments are included in this estimate but not flakes unless retouched or heavily used). For the most part, it is the overwhelmingly microlithic character of the obsidian artifacts—from the earliest levels and from all the upper levels—and their abundance that insure the industry's microlithic bias. If one ignores the obsidian component in the chipped stone category, taking only flint into consideration, the microlithic element of the industry decreases radically from the overall 70% to proportions that vary from 55% in J-I,8 to 33% in J-II,3, with an average of 51.1% in J-I,1 to 8, and 38% in J-II,1 to 6. However, one might speculate from the evidence in J-I,8—where the obsidian examples are the sparsest but the flint microliths are proportionately most abundant—that the Jarmo inhabitants had a desire and need for microliths and that this microlithic balance in the industry might have obtained even had obsidian not been available.

In addition to the traditional aspects of the tool kit that are mentioned above, there are also some new forms. Neat parallel-sided flint blade segments—for the most part unretouched—bearing "sickle sheen" are present in substantial numbers throughout all levels. (LSB feels disinclined to make much of the few blades and flakes with sheen found at Karim Shahir.) Obsidian, a material that had made an occasional rare appearance in earlier eras, was definitely appreciated by the Jarmo inhabitants, who used it for tools from the very earliest levels. A few new forms confined to obsidian appear in the earliest levels: "truncated obsidian blades and flakes," "pressure-flaked obsidian fabricators," and "thin sections" (side-blow blade-flakes). As Hole notes, the "truncated obsidian blades and flakes" are no longer in use by the time of the occupancies of the various levels in J-II. The "pressure-flaked obsidian fabricator," most common in the earliest levels, does seem to continue in use, though sparingly, in the later levels. "Thin sections" (side-blow blade-flakes) are present in minute quantities from the first. The idea is there, but the early examples are all extremely tiny and not retouched except for two or three dubious examples. Given our present exposures, it is only in the upper levels, from J-II,3 onwards, that one gets nicely retouched examples of this puzzling artifact, with the largest, most handsome, carefully retouched ones in J-II,3 and J-II,2.

As matters now stand, a new variation on the drill or borer shape was found only in the levels of J-II. This is the somewhat fiddle-shaped drill with a long narrow shaft (fig. 11:3-1.0).

Perhaps the most incomprehensible fact about the Jarmo chipped stone industry still remains the appearance and flourish of microlithic trapezes and triangles in both flint and obsidian. Present in the Paleogawra Zarzian, they are missing at Karim Shahir and essentially absent from all but the very uppermost levels of Jarmo.

The sheer number of artifacts in the ground stone category at Jarmo is most impressive, especially as regards the smaller and more finely finished groups. We have, however, the uncomfortable feeling that we did not give enough attention to the fragmentary pieces in the coarser stone groups, such as mullers, querns, mortars, pestles, and hammerstones; broken pieces of these types were often found reused in stone foundations and were thus left in place until the architectural drawings were made, and hence not collected as part of the regular daily routine. The ground stone catalogue (pp. 302ff.) suggests that there were far fewer examples in these coarse groups in J-II than there were in J-I. The reason for this is not clear to us. Neither can we guess why there were more examples in the groups of smaller and more finely textured objects, such as the so-called bracelets, the rings, beads, and pendants, in J-II than in J-I. Given the restricted exposures and sample size on which judgment can be based, it would not be very meaningful to say that the deeper J-II levels represent "richer" households than do the deeper J-I levels. Further, we have no provable idea of the functions of many of the smaller, finer stone objects (chap. 5), such as the so-called bracelets, the small grooved stones, the small borers and/or pestles, the ground stone balls, and the so-called phallic objects. As in the case of the stone vessels, we may wonder about the degree of specialization of craftsmanship which the quantity in some of these finer ground stone object groups implies, and in this connection it would doubtless be useful if we knew much more than we do of the sources of the minerals involved.

The worked bone category (chap. 6) is well represented in the Jarmo yield, although most of the types are not extraordinary for this time range. The appearance of the phalangeal hafts is noted. We might wonder whether the function of the so-called spoons was exactly that of a spoon in our own sense. The shaft hole type of bone "point" or gouge is, to us, a new form, while the deer canine head or pendent recalls Natufian examples.

The category of figurines and other clay objects makes up one of the most remarkable but indeed tantalizing groups of artifacts from Jarmo. It is particularly fortunate that Broman Morales had the two latter field seasons at the site, that she controlled the whole category from start to finish, and that she had the ability to make excellent pencil drawings of the pieces, which—along with a selection of photographs—aided much to their clear presentation here. She was also able to study all of the pieces from the 1948 season in the Baghdad Museum and had full control in the field of the study and drawing of the 1950-51 season's figurines. As mentioned above (p. 5), she was allowed to bring all of the figurines from the shorter 1955 season back to Chicago for a short period of study, after which they were all returned to Baghdad for selection and division. Broman Morales handled all of the well over 5,000 pieces several times and the

more comprehensible examples many times. She has also—with her understanding of how some of the many fragmentary parts fitted together and with her remarkable ability to test her ideas effectively with modeling clay—provided an exhaustive study of the whole category.

Naturally, it is her concern for the original meanings and use of the figurines that makes Broman Morales's task so difficult. She writes, for example (p. 392), that "for the cultural level exhibited at Jarmo, clay figurines can give almost the only clue to the intellectual life of the inhabitants, above and beyond the material life to be seen in their tools and architecture." Nevertheless, she adds, rather ruefully (p. 384), that "it is indeed remarkable how few definite conclusions can be drawn from the study of the human figurine category." The evidence of both their manufacture and their completely scattered occurrence throughout our available exposures suggests that their makers were not interested in the permanence of the figurines; it appears to have been the act of making them that was important. Broman Morales rejects the usual more generalized idea of fertility for the animals and in discussing the human females does not once speak of a "mother goddess."

The two groups called double-wing bases and stalks are also of particular interest, making up as they do a very early instance of the artistic attempt to delineate both human and animal forms in an abstract and, to us, incomplete way. The human-headed double-wing bases, which seem to appear only in the upper levels, cannot be identified as to the sex they represent.

A sufficient number of the figurine forms continue from the lowest to the uppermost levels in our various exposures to underline the homogeneity of the whole category, but there seems to be a proliferation of new forms in the upper levels.

There are a large number of small clay balls and some other geometric forms, some miniature vessel-like pieces, and a few other miscellaneous forms in clay. On some of the clay balls there are incisions or fabric impressions, and the latter have been given special attention by Adovasio (chap. 7, Appendix).

Unfortunately, our Jarmo exposures yielded very little evidence of the physical types of the original inhabitants or of their burial practices. We can only suppose that the dead were normally buried in a cemetery outside the village.

Stampfli's study of the bulk of the animal bones indicates that the percentage of wild animals was higher than was our first estimate and that the percentage of juvenile animals was not high.

So far, the bones of domesticated pigs have appeared only in the broader upper level exposures; domesticated goats and probably domesticated sheep were already indicated in the lower level exposures and persisted throughout the sequence. Also, Lawrence and Reed found convincing evidence for large domesticated dogs at Jarmo. The *Bos* and *Equus* bones were identified as wild cattle and onager respectively, and Turnbull's study accounts for a few small and some additional larger mammalian forms. Many bones of really large animals may well have been left at the place of a kill, while the whole carcasses of smaller animals may have been brought back into the village itself from an adjoining but ecologically different niche. The matter of traditional choice and Reed's (Braidwood and Reed 1957, p. 23, n. 12) notion of the "culture filter" also enter. Thus the impression given by the direct counting of bones from the site might well not represent the full environmental reality. It is interesting that from Palegawa through Jarmo times there is relatively little difference in the animal species represented by the bones.

In her recapitulating and updating of Helback's original study of the Jarmo plant material, Watson benefited by direct correspondence with Helback. She was also helped by new evidence from a variety of excavations subsequent to ours. Both wild and domesticated Einkorn and emmer wheat were identified at Jarmo by Helback, and barley was indicated by wild two-row and apparently cultivated two-row forms. He also identified the field pea, the lentil, and the blue vetchling.

Wright's "revisitation" of his SAOC 31 paper reflects, of course, the results of the palynological investigations which he, van Zeist, and other workers undertook subsequent to our Iraqi field seasons (see Wright's references, chap. 18). This new research indicated that there was a cold, dry climate in southwestern Asia during the last glacial period. Obviously, the implications of this finding call for some rethinking of our original proposition that the "hilly flanks of the Fertile Crescent" were the "natural habitat zone." Our assumption had been that the wild cereals were available in the general region throughout the late Pleistocene (see also Wright 1960, p. 97). Wright's interpretation of the palynological evidence now suggests that the wild cereals could only have become established (east of the Euphrates, at least) after 14,000 B.P. or even later. Such a circumstance, which must have involved a relatively rapid appearance of new plant forms within the new climatic regime, must of course be taken into consideration when we theorize on the beginnings of cultivation in this area. We feel bound to take the implications of the Wright-van Zeist interpretations very seriously, although we sense some lack of palynological confirmation from the more southerly portions of the Levant. Most of all, we feel again the crucial lack of archaeological evidence for the end phases of the upper paleolithic and the need for more and fuller inventories from the phases of the incipient or incipient range of time.

In Reed's (chap. 14) retrospective summary of his earlier sections in SAOC 31, he concentrates mainly on the new techniques for the analyses of evidence which have appeared since 1960 and on the types of changes in interpretations which these analyses allow. With both his own "model" for speculations on early domestication and his conclusions based on the impressive group of symposium papers in the *Origins of Agriculture* (Reed 1977, pp. 545-67, 879-958) now in hand, Reed has not attempted to repeat many of the details of his position which are available in that volume.

In our section on chronology, RJB has retained the opinion, as expressed in SAOC 31, that Jarmo flourished for several hundred years at about 6750 B.C. (Libby) and that the primary village-farming community era or level generally spanned the time range from ca. 9250 to 7750 B.P. (that is, ca. 7300-5800 B.C., Libby). Exasperatingly, the use made of radiocarbon age determinations increasingly appears to depend on the personal opinion of whoever is presenting a chronology. We all have our whimsies. Thus Hole (1977, table 2) labels each of the Jarmo determinations he selects as either "too old" or "too young," marks various of his Ali Kosh determinations as "OK" which we would consider too old, and also presents four Ganj Dareh determinations as of the later tenth millennium B.C. and one of the eleventh. Smith (1976b, p. 18) wisely writes concerning the Ganj Dareh situation that

our early sondage in 1965 yielded several radiocarbon determinations that suggested that the basal level (E) dated to ca. 8500 B.C. and the latest levels to roughly 7000 B.C. This seemed reasonable in relation to other early village sites known in the Zagros of Iraq and Iran and in western Asia in general. This dating seemed further supported by radiocarbon determinations resulting from the 1967 season, when several charcoal samples from Levels B to D gave dates ranging between 7300-7500 B.C. However, four new samples collected from fire-pits in Level E in 1971 produced determinations averaging about 6600 B.C.—later than those in the stratigraphically younger levels above! It is clear that, just as in many other Neolithic sites in western Asia, some complicating factor which is still imperfectly understood is responsible for these contradictions.

Perhaps it is time that we admit that for southwestern Asiatic prehistory at present, the assessment of chronology on the grounds of radiocarbon age determinations alone does have more of whimsy than of science. The degree of comparability of the inventories available from a variety of sites within a general region (for which a chronological scheme is being attempted) must be given due respect. Such a "relative archaeological chronology" can then serve to test the probable validity of "dates" within a wide scatter of radiocarbon age determinations.

Finally, as regards Jarmo, it is to our embarrassment that our section on miscellaneous analyses is so brief. At least there is a respectable selection of artifacts in the major categories in the Chicago sample at the Oriental Institute that is available for future analyses.

In sum, Jarmo appears to us to contain the remains of an early village-farming community of some 150 or more people—a permanent settlement of a size very well beyond that of the typical hunting-collecting band. BH takes the remains of Karim Shahir to suggest possibly only periodic or seasonal settlement.⁴ At Jarmo we see no reason to doubt year-round settlement, probably for the duration of several centuries. Although at least two other sites (Kari Sur and Khora Namik, SAOC 31, p. 27) with surface indications of a Jarmo-type inventory are less than 10 km from Jarmo, no evidence for the general settlement pattern or of any "spatial hierarchy" of sites of the Chermahal valley in the earlier seventh millennium B.C. is yet available. Although a thick *tayf* wall was encountered in a narrow search trench (p. 165), it still seems unlikely that the Zagros villages of this time were fortified in any way. Insofar as we have architectural evidence, it points exclusively to the proposition that the Jarmo people lived in rectilinear single-family units made up of several rooms.

There is at Jarmo not only secondary evidence of food production (artifacts such as sickle blades and milling stones may well imply it) but also substantial primary evidence: both the bones of domesticated animals and traces of domesticated plants were recovered. Houses do contain ovenlike structures and also hearths undoubtedly associated in some way with food preparation and perhaps also with warming the households. Given Wright's evidence that the climatic trend in the area from cold, dry steppe conditions toward the present-day situation was not yet complete and also given the spells of bitter cold winter weather we ourselves experienced at Jarmo, the need for heat does call for serious consideration. Unfortunately, we have as yet no firm evidence of how grain was stored in bulk or of how the herds were sheltered. Certainly not all of the community's food was the result of farming; the bones of wild animals were more common than we had at first believed (p. 496), pistachio nuts and acorns were used, and Helback seems never to have specified that all of the identified legumes (p. 502) were necessarily domesticated. The very considerable bulk of land-snail shells was perhaps the most impressive reminder of the amount of food collection still being done by the Jarmo people.

There is no question but that there are some deposits within the exposures we made at Jarmo which do include the sherds of portable pottery vessels and some which do not. As stressed above, however, our first concern is that the matter not be oversimplified. While we ourselves originally thought that the upper third of the site had pottery and the lower two thirds did not, the results of the 1955 season showed the situation to be more complicated. Exactly what the solution to this problem may be must await further clearance at the site.

The second matter of interest regarding pottery is that where it was indeed encountered, the examples in the earlier pottery-bearing levels, although far fewer in number than those in the uppermost levels, were both technically and artistically of better workmanship (or preservation?) than those that were recovered from our near surface exposures. This must bear on the assumption that the invention of pottery vessels did certainly not take place at Jarmo.

Once the use of pottery vessels did become established at Jarmo, however, the number of units produced, as well as the variety of sizes and of profile types, increased markedly. Pottery vessels did not, however, lead to the displacement of stone vessels within the duration of Jarmo's occupation. Unfortunately, the generally friable nature of the pottery and

the fact that we encountered no recognizable "activity clusters" or vessels that still had traces of their original contents leaves us with little basis upon which to suggest the particular purposes for which any particular profile type was made. Again with regard to the friability of the later Jarmo pottery, it is worth remarking that almost no potsherds were seen on the surface, a fact which doubtless prompted the first surveyors from the Directorate General of Antiquities to label the site as "mesolithic."

When it comes to the chipped stone industry—traditionally conservative—and especially wonders about its antecedents at Jarmo. It would be reasonable to expect that so old a tradition as the knapping of flint blade tools had a smoothly paced evolution. If this is correct, it underlines the discontinuities between the Paleogawra, Karim Shahir, Jarmo, and Matarrah industries. The presently available materials particularly emphasize the fact that there are gaps in the record. It may possibly be that when more is known of Kauri Sur and Khora Namik (see p. 13) or other comparable—but as yet not located—sites in the general area, the information from one or more of them may help solve this problem.

Aside from the types held in common with various earlier industries in the area, the few hints in the Jarmo industry that point in the direction of the Zargian at Paleogawra are the geometric microliths—specifically trapezes and triangles—found only in the uppermost levels at Jarmo and the neatly retouched and shaped end scrapers on blades. As regards Karim Shahir, perhaps the two types of Jarmo chipped stone artifacts that are most reminiscent of the Karim Shahir industry in particular are the rare, poorly made, ill-defined burins and the steep scrapers on core fragments (cf. figs. 17:1-2 and 120:7-9).

We are in a somewhat similar situation when we attempt to trace the Jarmo industry down through time. The undistinguished Ali Agha, Hassuna, and Matarrah chipped stone industries have little in common with the Jarmo industry. The only significant link between the chipped stone industries of Jarmo, Matarrah, and Ali Agha is the curious obsidian "thin section" (the side-blow blade-flake). On the other hand, the overall impression one gets from the small sampling of early material from Tell Shimshara (Mortensen 1970) is that it shares a tradition with Jarmo of neatly made blades, bladelets, and other tools (including well-retouched end scrapers on blades and a few trapezes). The Shimshara residents seem to have had more access to obsidian than the Jarmosans and were more generous in its use. Surprisingly, the side-blow blade-flake was apparently not a part of the Shimshara industry; however, another equally curious (its use or uses also unknown) obsidian artifact that is characteristic of the site is also characteristic of Çayönü. This is a type of backed blade, usually with both edges carefully backed by excellent pressure flaking (Mortenson 1970, figs. 23, 29, 36-37). A distinctive trait of most of the examples at Çayönü (Reidman 1979, fig. 5:6-8) and also at Shimshara (Mortensen, pers. comm.) is the striations at and parallel to the edge on the surface below the retouch. Many of the Jarmo "pressure-flaked obsidian fabricators" would seem to be of this same type. This is perhaps not clearly evident, because these Jarmo examples are much smaller and more fragmentary and worn (probably due to reuse) than the Shimshara and Çayönü examples. In most of the Jarmo examples, however, where the surfaces are not too worn, the accompanying striations are in evidence.

These peculiar obsidian types seem to be missing so far in the Iranian Zagros; at present we can only say that these Iranian sites share with Jarmo the general tradition of neatly made blades and bladelets, as well as some of the better known blade tool categories.

It is currently fashionable to picture the hunter-collectors as having been "the original affluent society" (e.g., Sablin 1972) and to visualize the early food producers in a sort of full-time biblical "in the sweat of thy face shalt thou eat bread" image. Whatever the case may have been at Jarmo, the site's inhabitants certainly committed an impressive number of man-hours to activities other than simply producing food. It is perhaps in the stone vessels and in the other small ground-stone object categories that we see this best and in connection with them that the question of a degree of craft specialization must at least be posed. The number of figurines produced must also imply available fire and so must the activities (whatever they actually were in detail) by which such materials as bitumen, obsidian, and ochre arrived at the site. One may also ponder over the amount of time doubtless spent in producing matting, basketry, and cloth and consider the probability of whole artifact classes in such perishable materials as hides and wood. The latter is implied at least by the stone tools which must have had hafts, including tools involved in the carpentry which went into the framing of roofs and doors and other details of house construction.

While it is clear that there are still serious gaps in the evidence for a general space-time continuum of cultural-historical development in the Zagros Flanks country, we know enough of the Jarmo inventory to see where it must fit in the general scheme. It did not immediately succeed the phase represented by Karim Shahir—the only other excavated site in the Chermahal valley itself. Both the presence of obsidian with its implications of outside contact and Jarmo's various artifactual analogies with sites in higher intermontane valleys (Sarab, Guran) and sites along the piedmont (Tamarshan, Ali Koshi) provide us with good reason to reject any suggestion that Jarmo lay in a cultural backwater.

Indeed, in a very broad sense, the Jarmo inventory appears to be part of the generalized assemblage which has appeared from as far south as the Khuzistan plain and thence to the northwest along the Zagros intermontane valleys and piedmont. At the moment, its northwesternmost available manifestation might be seen at Ali Agha if that site's inventory can be taken as the assemblage's latest aspect. Nor only the degree of artifactual analogies within this generalized assemblage but also the instances of the use of such items as bitumen, marine shells, and obsidian underline

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