2. SCOPE OF THE RESEARCH

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2.1 Environmental considerations

The modern geographical setting of Tell Mozan is, by all estimates, the same which conditioned the historical development of the site and its region in ancient times. Recent literature has paid special attention to the incidence of rainfall on agriculture, especially in contrast with the situation south of the Khabur triangle, where rainfall is insufficient for cultivation and farming is thus dependent on irrigation systems. Since both the lower Khabur and the Euphrates in its middle course have cut a deep trough in the steppe, the area which is actually accessible for irrigation agriculture in this region is quite limited; as a result, the contrast with the broad and fertile plains of the Khabur is even more striking. Van Liere pioneered this approach (see especially Van Liere and Lauffray 1954-55), and the book by Wirth (1971) has come to serve as a standard work of reference on modern conditions. H. Weiss has developed this theme with special reference to the third millennium and to Leilan in particular (1983, 1985a), and has devoted to it a symposium which he organized in Chicago for the Annual Meeting of the American Schools of Oriental Research (I have not had occasion to see the published report of the symposium).

An interesting perspective from which we may look at the question of the environment is that of the perception that the ancients themselves had of it. This has developed in various approaches to a study of the "landscape," as it is often called following French models, and the type of populations or social classes associated with it. For ancient Syria this approach has found an insightful proponent in M. Liverani, who has written particularly with reference to the second millennium (1975), while his student C. Zaccagnini (1979) has developed a similar approach for Northern Mesopotamia. While it is impossible to develop a full argument along these lines without the support of written sources, which are still few and indirect with regard to the region of Mozan, I would like to propose a few considerations which may help to place the work at Mozan in a wider perspective. The remarks that follow were first presented in a paper delivered at the University of Toronto in 1983, on which occasion I also presented for the first time our case for the special significance of Tell Mozan. I

have also discussed the nature of the landscape in the lower Khabur region in a paper delivered at the 196th meeting of the American Oriental Society in New Haven (1986). I will develop fully this perspective in a separate publication, while in this context I can only give a short summary, which is also reflected in the maps presented below as Figures 1 and 2.

Briefly put, the rural base of the developing urban civilization in the Khabur plains seems to have been particularly complex because of the interaction of three major rural populations.

(1) Given the nature of the local landscape, we may assume that there was a class of local farmers engaged in intensive farming. I am assuming that they were settled in small local communities where they lived on a year-round basis, and that they came readily under the control of the larger urban communities.

(2) At the same time, however, and for different reasons, there seem to have been other rural populations which came in direct contact with the cities of the Khabur plains. To the South of the Sinjar and Abd el-Aziz ranges there are wide expanses of pasture land which could begin to be exploited as such (i.e. as pasture lands) once it was discovered that wells could tap the water table and provide sufficient water for the animals if not for cultivation (except for larger springs, wells today are not even sufficient to support small orchards). These pasture lands represent a distinctive feature of the ancient landscape, since there was a word reserved for them (*nawû*). I assume that the wells had not been exploited systematically in prehistoric, but only in relatively recent times, presumably beginning in the third millennium, and possibly by the rural populations which were originally at home in the trough of the Euphrates (what is today called the *zôr* in the local dialect, as in the name of the provincial capital Der ez-*Zôr*). Rather than nomads converging on the cities, I would prefer to interpret these populations as rural groups progressively acquiring greater autonomy from the control of the cities in the *zôr*, but retaining at the same time fundamental rural characteristics, since no cities were ever established during the third and early second millennium in the *nawû*. Their autonomy was reflected in a number of traits by which these populations came to be known also outside their original homeland, summed up first of all in their own collective name: the Amorites. I am assuming that during the third millennium they already had lively and direct contacts with the urban centers in the Khabur plains, just beyond the Abd el-Aziz and Sinjar which form the northern boundary of the *nawû*.

(3) It would also appear that the Khabur cities were further in direct contact with rural populations to the north of the plains, in the mountains of the Tur-Abdin and beyond. It was from here that significant natural resources were flowing to the South, and we do not seem to have evidence for full fledged urban centers in these northern regions during the third millennium. Whether the cities of the plains extended their control to the mountains or whether they simply interacted with their populations we do not know.

On the basis of various considerations, some of which are summarized in the following section, we assume that the urban populations of the Khabur plains had a distinctive physiognomy epitomized by the term "Hurrian." Since this is in the first instance a linguistic term, its full significance can only be understood if and when sizable Hurrian archives can be found. The identification of a distinctive "Hurrian" civilization can not result from considerations pertaining to material culture alone (see especially Mellink 1972-75; Barrelet 1977, 1978; Hrouda 1985). But apart from what the appropriate (ethno-linguistic) label may be, the above considerations point to a rather unique constellation of factors in the network

of relationships among the urban settlements of the Khabur plain and the populations which formed their rural base. As a major one among such centers, Mozan must have played a significant role in this process.

We are thus approaching from a different angle the same question that had roused so much enthusiasm in scholars like Speiser (1930) and Moortgat (1932), and that received its most comprehensive and balanced synthesis in the work by Gelb (1944, 1956). The interest in the Hurrians is also reflected in recent publications and projects, such as the research project directed by M. T. Barrelet (1977), the 24th meeting of the Rencontre Assyriologique Internationale (*RAI* 1978), or the beginning of a new series devoted to the publication of the Hurrian corpus (*CHS* 1984-). While a review of Hurrian studies is beyond our present scope, a few remarks may be in place in that they help define some of the reasons underlying the choice of Mozan.

2.2 Historical considerations

The Hurrians represented a major cultural force in the ancient Near East and yet they remain so little known that much of our knowledge about them is derived from non-Hurrian sources. The question about their early history is clearly linked to another: Why is it that no single Hurrian city has been excavated as yet? The question is similar to what might have been asked about Western Syria before the discovery of Ebla: why was it that no Semitic city had ever been discovered? An answer to the Hurrian question can ultimately only come in the same way as it did for the question about a Semitic city. A Semitic city can be identified as such only if Semitic texts are found in it. Ebla turned out to be the source for such Semitic texts. Clearly, no Hurrian equivalent of Ebla has been found. And yet, we have perhaps more reasons to expect it than there were reasons to expect a city like Ebla: for we have evidence of a Hurrian scribal tradition in the third millennium, presumably at home in the Khabur plains, whereas we had no previous evidence of a scribal tradition, indigenous or otherwise, at Ebla or elsewhere in Western Syria.

Admittedly, such evidence is quantitatively extremely limited: the total epigraphic inventory attributable to a Hurrian scribal tradition in the third millennium amounts to no more than one document, extant in three parallel (and partial) versions:

(1) the tablet of Tishatal from the Louvre (Parrot and Nougayrol 1948);
(2) the plaque on the lion of Tishatal from the Louvre (*ibid.*);
(3) the plaque on the lion of Tishatal from the Metropolitan (see Chapter 9).

In addition, there are other texts that may have originated in the Hurrian area, such as:

(4) the tablet of Atalshen (Thureau-Dangin 1912); or
(5) the seal of Daguna (Nougayrol 1960),

but these do not have the same status as the text of Tishatal because they are written in either Akkadian (No. 4) or Sumerian (No. 5): if pertinent, these texts would indicate that Hurrian was not the only language written in the Hurrian cities. None of these texts comes from controlled excavations, and thus we have to rely almost exclusively on internal evidence;

arguments about provenience based on information from the dealers (which is the only clue in the case of the seal of Daguna, reported to have come from the same site as the lions of Urkish; see Nougayrol 1960, p. 213) have to be considered with extreme caution although they should not perhaps be discarded altogether (see the comments in the next section).

Yet, for all its limited size, this small Hurrian "corpus" of the third millennium has a significance which has not always been properly appreciated — so much so that when the archives of Ebla were first discovered, they were hailed as the *only* known third millennium texts from Syria. Now the text of Tishatal in particular raises momentous implications precisely when compared with the texts of Ebla. It is in fact written exclusively in syllabic Hurrian, whereas the vast majority of the Ebla texts have a low percentage of words written in syllabic Semitic. This implies the existence of a wholly indigenous scribal tradition in the service of Tishatal, sufficiently vigorous to develop and retain full graphemic autonomy from its southern Mesopotamian counterpart. It seems inescapable that texts like those of Tishatal should not be seen as an isolated experiment, but rather as the top of a veritable iceberg, still lurking beneath the waters of a cultural assemblage as yet very imperfectly known. Concretely, this makes it reasonable to expect not only more texts of the same type, but a concentration of the type that is found in an archive, a library, a scribal office or a school.

From what we know so far, it appears that the ancient city which is the most likely candidate to have served as the center for the development of such a scribal tradition was Urkish (see especially Pecorella and Salvini 1982, pp. 14-17). In spite of certain difficulties of both a philological and an archaeological nature (some of which are well described in the Appendix by Muscarella given below as Chapter 9), one may argue that Urkish was a city in the Khabur plains from which the foundation inscriptions of Tishatal come, and that Mozan is a possible candidate as the site correposnding to ancient Urkish. Let us review briefly the evidence — first from a philological point of view (in this section), and then from an archaeological point of view (in the next section).

The tablet of Tishatal is part of the foundation deposit of the temple of Pirig-gal, built by the "king" of Urkish (I am using both the standard translation "king" for *endan* and the standard readings Tishatal and Atalshen for the sake of convenience; on *endan* see Salvini in Pecorella and Salvini 1982, p. 15). It does not say that the temple was built in Urkish, nor is the geographical name of Urkish preceded by the logogram or determinative for city in the royal title of Tishatal.

The tablet of Atalshen is part of the foundation deposit of the temple of Nergal, "king of Hawilum," built by the king of Urkish and Nawar, presumably in Hawilum. Here again the geographical name Urkish is not preceded by the logogram or determinative for city.

The first issue then is whether Urkish refers in fact to a city, since it does not occur with a determinative for city in the third millennium attestations. I assume it to be so because on the one hand the second millennium evidence (see briefly below) clearly indicates Urkish to have been a city, and on the other the third millennium evidence admits of such a possibility. It should be noted in this regard that omission of the logogram for city is frequent in the third millennium, both in the royal titulary (e.g. LUGAL *Ma-ri*$_2$-KI, *RGTC* 1,117) and in other references to cities such as Mari or Ebla (*RGTC* 1,37 f.; 2,39; 2,128f.).

The second issue is whether Urkish, which is only mentioned in the *titulary* of king Tishatal, and not as a reference for the localization of the temple of Pirig-gal, may be further

assumed to be the city where the temple was located. Here too a positive answer seems plausible, since the royal inscriptions of Mesopotamia do not necessarily give the name of the locality where a given temple is built when this locality is the capital (see for instance for the Ur III period, *IRSA*, p. 138), whereas they do when the locality is a province (*IRSA* p. 142 for the Ur III period and p. 216f.).

A related issue is the identification of Nawar, which appears in the titulary of Atalshen, "king of Urkish and Nawar." Nawar is often assumed to be another Hurrian city, situated at the eastern extremity of a kingdom of which Urkish represented the western extremity (see for instance *IRSA*, p. 128; Weiss 1983, p. 49). If Nawar is not generally equated with Urkish in significance it is because it does not appear in later Hurrian mythology. In point of fact, we must in this case raise more serious doubts as to whether Nawar ought to be considered a city or a country. The evidence for the third millennium is similar to that available for Urkish, but is even more limited. In the inscription of Atalshen, Nawar appears, as already noted, in the titulary of the king, in second position after Urkish. In a text from the Ur III period (de Genouillac 1911, 83:3), Nawar is mentioned as the place of provenience of an individual whose proper name also contains the same toponym (Nawar-shen). As for the later periods, the evidence militates against the identification of Nawar with a city. The name Namar (which can properly be interpreted as a later phonological development of Nawar) is attested in a kudurru of the Kassite period, and it refers here clearly to a region (KUR *Namar, qaqqar* KUR *Namar: BBS* 6 i 47.48.51.55; ii 6.7.10.27.29.31.48).

In addition, it should be noted that there is little evidence from Mesopotamia in favor of a royal titulary comprising the names of two cities. On the contrary, it is a well attested pattern, especially in northern Mesopotamia and in western Syria, to include in the royal titulary the name of a city followed by the name of the territory, as in the well known example: LUGAL *Ma-rí*-KI *ù ma-at Ḫa-na* (see Buccellati 1967, pp. 140-46). Accordingly, the suggestion may be made that the title of Atalshen refers to a city and its territory rather than two cities: "Atalshen, king of the city of Urkish and of the land of Nawar." The correlation between this title and the title borne by the earlier king Tishatal may be compared to the correlation between two kings of Mari, as follows:

Tišatal, endan Urkiš	*Iplul-il, šar Mari*
Atalšen, šar Urkiš u Nawar	*Yaḫdun-Lim, šar Mari u māt Hana*

Whether in fact the land of Nawar extended all the way from the Khabur to the eastern regions of the Tigris (where the later Namar is traditionally located) remains to be seen. But if so, then the term Nawar would seem to correspond in its geographical import to the term Subartu (see Hallo 1978, esp. p. 71f.)

In addition to the inscriptions of Tishatal and Atalshen, two other possible rulers are mentioned in Sumerian texts from the Ur III period. One of them remains unnamed, while we have the name of his messenger:

É-ni-da-gú? *lú-kin-gi*$_4$*-a Ur-kiš*-KI-*sè* (Nakahara 15, Rv 3)

Another is mentioned twice, qualified simply as *lú Ur-kiš*-KI, which might serve simply

as a gentilic or might identify the individual as a ruler of Urkish (possibly a governor under the Ur III kings):

An-na-tal lú Ur-kiš-KI (Langdon 1913-23, 240: 14 Rv.1)
An-na-tal lú Ur-kiš-KI u_4 *Ur-kiš*-KI-*ta ì-im-gen-na-a* (*TCL* 2 556; 2f.).

If the title *lú* refers in both cases to a ruler, then we would have in the epigraphic record the reference to three (or more) rulers of Urkish in the later third millennium, as follows:

Tišatal endan Urkiš
Atalšen LUGAL *Urkiš u Nawar*
An-atal, lú Urkiš
(unnamed) *lú Urkiš* (whose messenger is Enidagu).

It would appear then from the limited evidence at our disposal that Urkish was one of the more important, if not the most important, Hurrian city of the late third millennium, since we can associate with it both the isolated but significant evidence of an autonomous Hurrian scribal tradition and the names of two and possibly more rulers.

Its significance had waned by the second millennium, to judge from the number and type of references in which the city is mentioned. It seems nevertheless to retain some degree of autonomy, since there is mention of a *king* of Urkish, and on one occasion Zimri-Lim makes a personal effort to pacify the city.

(1) A certain "Te-ir-ru, *roi* d'Ur-gi-iš^{KI}" is mentioned in a tablet of Mari of which only a brief excerpt has been published without any other pertinent information (Jean 1938, p. 132): this is the only indication of a "king" of Urkish in the second millennium, and, in the light of what else we know about Urkish in this period, may well refer to a minor vassal ruler. The same spelling (*Ur-gi-iš*-KI) is found in all the remaining references from the Old Babylonian period.

(2) Zimri-Lim writes to Shibtu that, having just installed a governor in the city of Shenah, he intends to go to Urkish where he will "thoroughly pacify" the city ([*š*]*ullumu*[*m ušallam*], *ARM*, 10 121: 9, 10,13); note that where the name of Urkish occurs as the object of the verb *ušallam* it is preceded by the determinative for city (URU). He further writes that he will go from Urkish to Shuna (Hamidi?, see Hallo 1964, p. 74), and repeats his assurance that he will thoroughly pacify "these cities."

(3) A letter addressed by Ibal-El to Zimri-Lim relates a fragmentary message which had been addressed "to Urkish" by the men of Hurraya and Shinah, and reports that a special (public?) announcement (of the message?) has been made in Urkish (*ARM* 2 38: 6, 16,18).

(4) A letter from Ishme-Dagan informs Yasmah-Addu that Urkish and Shinah are posing some resistance (? *uḫḫuru*), but that he will take hold of them (*leqû*; *ARM* 4 40: 14).

(5) A tablet from Chagar Bazar (located some 25 km. south of Mozan) refers to Urkish twice, as the destination of some commodities (Gadd 1940, A994: Obv. 29, Rev. 8, Pl. 4 and p. 59); the other known sites mentioned in the text are Kahat (some 25 km. southeast of Chagar Bazar) and Shubat-Enlil (some 65 km. northeast of Chagar Bazar, accepting the identification of Leilan with Shubat-Enlil).

(6) Finally, Urkish is mentioned in the itinerary published by Goetze (1953, p. 53f; 22, with the spelling *Ur-ge-eš*; see Hallo 1964, pp. 72, 83). Here it appears as a detour station used only on the return trip: one may deduce from this that Urkish was of secondary importance (since it is omitted on the outgoing trip), but the converse could also be true, namely that even though it was not on the main course of a direct road from Assur to Emar it was nevertheless worth a detour at least on one of the legs of the trip. The arguments for the localization of Urkish in the area of Amuda have been strengthened by the arguments which have been convincingly adduced recently to support the identification of Leilan with Shubat-Enlil (Charpin 1986; Whiting 1986).

In the latter part of the second millennium, Urkish appears as a major point of reference in the realm of mythology, where it is mentioned as the seat of Kumarbi:

> Kumarbi took his staff in his hand, put swift shoes on his feet. He set forth from Urkis, his city, and betook himself to the... [Translation by A. Goetze in *ANET*, p. 121; see Otten 1950, and Guterbock 1952.]
>
> [...] the father of the city of Urkiš [...]
>
> [...] he is in the city of Urkiš [...]
>
> [...] he arrived in Urkiš, but [...] did not find him in his house [...] [Otten 1950, pp. 27-29:9,10,20.]

In the last text, "Ishtar, queen of Nineveh" (see Wegner 1981, pp. 11-12) is apparently addressed as the "sister" of Kumarbi, a fact which is interesting in terms of the geographical spread of the kingdom of Urkish, postulated on the basis of the titulary of Atalshen, king of Urkish and Nawar, and on the basis of the possible identification of Tishatal king of Urkish with Tishatal "man" of Nineveh (Whiting 1976). It is also interesting to note that, since his travels take Kumarbi to the sea on the one hand and to the mountains on the other, a piedmont location for Urkish (such as that of the area of Mozan) is well suited for the place of origin of Kumarbi. Finally it should be noted, with regard to the remarks made above concerning the identification of Urkish as a city, that in these later texts Urkish is clearly understood as a city, and that the corresponding determinative is used regularly.

The proposed identification of first millennium Urakka with Urkish (see especially Kessler 1980, pp. 221-26) conflicts with an interpretation of Mozan as Urkish, since no first millennium remains are found in Mozan. It is conceivable that the *name* had survived (in a slightly altered form) but had been transferred to the nearest city — which may then well be Tell Shermola (in the modern town of Amuda, for which see below, 8.2). This would mean that, if Shermola's Middle Assyrian name had been Kulishhinas (see Aynard and Durand 1980; Machinist 1982), a change had occurred which had brought back to life the name of the most significant site of the area in earlier periods.

2.3 Archaeological considerations

Subsequent to the publication of the tablet of Tishatal (Parrot and Nougayrol 1948), Van Liere reported that from information obtained accidentally the provenience of the objects was "Tell Amuda." Here is the exact wording of Van Liere:

> Le point de départ pour Moortgat est la provenance des deux lions en bronze et une tablette de fondation du temple de Kummarbi. Des informations fortuites, que j'ai obtenues recemment, indiquent que ces lions ont été excavés de Tell Amouda. Le Tell Amouda se trouve juste au sud de la ville! c'est un des rares tells abimés de la Jezireh. [Van Liere 1957, p. 12.]

More has been made in the literature of this statement than was warranted, and doubts if not criticisms have been voiced only rarely (Hrouda 1958; 1985; and see especially the Appendix by Muscarella below). Here I will review the issue in some detail, and indicate what in my opinion may be retained of Van Liere's statement.

In the first place it should be noted that the reference to the temple of Kumarbi is inaccurate, since the tablet that goes with the lion of the Louvre mentions a temple of Pirig-gal, which there is no reason to identify with Kumarbi.

But what is most significant about Van Liere's statement is the new information about the provenience of the lions, information which he labels as "fortuitous." With this wording, Van Liere omits the source of his information, not so much because he does not mention names, but because he does not even qualify the nature of his source. By inference we may assume that it was a local dealer. First, Van Liere had been active in the area and was likely to have come in contact with local individuals interested in antiquities. Second, it is difficult to imagine any other source that he would have wanted to protect with silence: if, for instance, he had been told by the local villagers he would have had no compunction in letting it be known.

What is most puzzling about his statement, however, is that the tell he describes in his article is *not* Tell Amuda — it is rather Tell Shermola. This is the name that local inhabitants give without hesitation when referring to the "tell abimé" which is "juste au sud de la ville." They also say without hesitation that Tell Amuda is on the other side of the border, in Turkey, where it has been renamed Tell Kemaliya. Several of the people I questioned in Amuda are old enough to have known the situation of 1957 (the year when Van Liere published his article) as well as they know it today, so that the possibility of a change of name seems ruled out. If Van Liere was wrong about the name of the tell (as he apparently was with regard to the name Mozan as well, see above), how reliable is the rest of his information?

What is more, even a hurried visit to Tell Shermola indicates that it can hardly qualify as the site from where the lions could have come. While the tell did have third millennium pottery on the surface, it was hardly in quantities that would indicate a major third millennium occupation; and traces of architecture in the visible section point to a date

in the latter second millennium (for more detail see the report by G. Bunnens and A. Roobaert, below in Chapter 8). Thus the middle Assyrian texts which are also reputed to have come from Tell "Amuda" (Aynard and Durand 1980; Machinist 1982) fit the remains of Tell Shermola very well.

On the strength of such remarks it appears that Muscarella's strong arguments against accepting dealers' information as to the identification of sites acquire even greater impact — and I certainly agree in any case with his premises and his conclusions (Muscarella 1977; 1979; and the Appendix in Chapter 9 below). If in spite of this I do not dismiss out of hand the information provided by Van Liere, but only aim at modifying its import, it is because of an observation which I was able to make as I searched into the background of Van Liere's report. While we could not locate anyone who might have been aware of Van Liere's source, we did meet local individuals who appeared knowledgeable about antiquities (a few of whom even turned over objects to be given to the Museum). Since we were known as legitimate archaeologists acting openly through the intermediary of the official representative of the Directorate, and since we made no pretenses whatever as to any alleged commercial interest in antiquities, we were very unlikely candidates for any confidence as to possible provenience of antiquities. Yet one interesting fact emerged from the casual conversation with these individuals. While there was obviously an unwillingness to associate any given artifact with a specific site, they were not at all reticent to share their knowledge about sites in general. They were clearly aware of the sites in the general vicinity of Amuda, and could give descriptions of artifactual evidence from them which matched to some extent our own observations derived from surface reconnaissance.

This fairly specific knowledge of the archaeological landscape, however, did not extend very far; certainly not, for instance, to any site south of Hasseke, nor to sites much east of the line Hasseke-Qamishli (Tell Farfara was the only noticeable exception). I see no reason why these individuals would have deliberately tried to make us believe that they were knowledgeable about the local archaeological horizon, and ignorant instead of sites farther afield. The conclusion I draw from this is that the local awareness of ancient sites as exhibited by these fairly knowledgeable individuals may be a gauge for the possible acquisition range of antiquities. If so, Van Liere's information might retain some value as an indication of provenience for the lions — not with respect to Amuda, but possibly with respect to its immediate region.

These considerations about Van Liere's information have been advanced here in some detail because so much has been made of it in the literature and because the Urkish lions are in fact of such unique significance that any possible clues as to their provenience should be assessed for what they may be worth. My main conclusions are as follows. It is likely that Van Liere had some specific information connecting the lions with Amuda. While the association with Tell "Amuda" (i.e. Tell Shermola) has to be excluded, it seems very likely that if the source of information was in Amuda, then their original acquisition also took place in this town, and that the lions themselves most likely came from a site nearby.

Since Mozan is the largest tell of the third millennium in close proximity to Amuda, it appears as a likely candidate. This is of course supported by all the other arguments which have been adduced to maintain that Tell "Amuda" (i.e. Tell Shermola) could well correspond to ancient Urkish. Its location fits in very well with the indication of the itineraries (Goetze 1953; Hallo 1964), and its position just below the wide pass of Mardin places it in an ideal

situation to serve as the hub in the communication network which linked the Khabur with the valley of Diyarbakir beyond the Tur-Abdin, where the great mines of Ergani and Maden are located (see the map, Fig. 2).

The recent possible discovery of tin in the Taurus (Yener 1986) adds even greater significance to the role that these trade routes would have played. The main road south from Diyarbakir comes through the pass of Mardin, which dominates the landscape of Mozan, and stands almost as a visual symbol of an opening to the northern highlands. An interesting speculation along these lines is suggested by a potential etymology for the name Urkish, which has been proposed by Alexis Martin (personal communication; the evidence will be presented by Martin in a larger work on Hurro-Caucasic linguistics). He suggests that the suffix *-is* (already isolated by Gelb 1944, pp. 41, 56-58, 114) may be linked with a Caucasic word for "mountain," and the base *urki* may be related to a Caucasic word for a *selliform cradle*: if so, the name might be a reflection of the saddle-pass of Mardin, one of the most noticeable aspects of the local landscape.

In conclusion, the following points may be made. (1) In spite of the paucity of our evidence, the existence of an autonomous Hurrian scribal tradition in the late third millennium is potentially of great consequence, especially when one compares this situation with that presupposed by the discoveries of Ebla. (2) Urkish appears to be the most significant center of such Hurrian tradition: it is a city which played a key political role in the third millennium, dwindled to the status of a secondary road station in the early second millennium, and remained present in later mythology as the seat of the chief god of the Hurrian pantheon. (3) The circumstantial evidence concerning the provenience of the lions and the tablet of Urkish, plus the more positive evidence derived from the study of the Old Babylonian itineraries, suggests that the location of Urkish was in the area of Amuda.

On the basis of the information presented in the following chapters, it appears that Mozan is a likely candidate as the site of ancient Urkish. It is a large urban center in the third millennium, it shows more limited evidence of occupation for the early second millennium, and is abandoned thereafter. Given its close proximity to Amuda (some 5 km. to the east), all the arguments which have been adduced in the past in favor of the identification of Amuda (i.e. Tell Shermola) with Urkish apply equally as well to Mozan.

It goes without saying that such a suggested identification remains highly tentative, and that the significance of Mozan is not to be tied down to an ultimate verification of such identification. The reason for dwelling at some length on the evidence pertaining to Urkish has been primarily to correct the generally accepted opinion that Urkish is to be sought in Amuda (i.e. Tell Shermola), and that its recovery is accordingly impossible given the bad state of preservation of that tell. Since what limited evidence we have for Urkish indicates that its recovery would yield immeasurable information about the history of the Hurrians, of ancient Syria and of the ancient Near East as a whole, and since we may expect one of the sites in the region of Amuda to correspond to the ancient city, it is a worthwhile endeavor to develop a systematic search for it. A preliminary phase of this search, based on a survey of the area of Amuda and on preliminary soundings at Mozan, has yielded enough evidence to suggest to us that Mozan is the site that best meets the current requirements for Urkish. And obviously any site that fits such a profile is well worth excavating, regardless of what the ultimate outcome of its possible identification with Urkish might be.

2.4 Methodological considerations

Our methodological aims are best exemplified by the publications which are planned to be issued as a result of our excavations. Our standard reports will appear in the series which is inaugurated with this volume. Named simply after the site, *Mozan* will include both preliminary and final reports. "Final" reports are devoted essentially to either stratigraphic aggregates or typological assemblages which are self-contained in scope and complete in terms of recovery. "Preliminary" reports, on the other hand, include essentially information on work in progress, although at times such information may be presented with such detail that it will not in fact be duplicated in additional, final reports.

Next to the traditional reports of the series *Mozan*, we also intend to inaugurate a separate series, the *Mozan Record*, which will be new both in orientation and in form. A goal of this series is to make available the total record of the excavation in electronic format. Based on a thorough revision of the IIMAS encoding manual (Buccellati and Kelly-Buccellati 1978), and fully oriented toward electronic data-processing, the *Record* will make available the complete range of primary information that has been gathered during the excavation. We expect to publish both the new manual and the first volume of the *Record* at a later date, and while the full implications of the approach will be outlined there, it may be well to mention here briefly what the rationale is for such new departure. Conceptually, I consider such a publication to be an answer to the need for greater objectivity in the presentation of excavated materials: in this case objectivity derives from the effort to limit as much as possible the degrees of selectivity which affect the excavated material from the moment when a research design is drawn up to the moment when its results are published. Theoretically, the systematization of the recording process, especially in its stratigraphic aspect, should help develop patterns of regularity after the model of a grammar: such regularity should enhance the possibility of both a structural understanding of the stratigraphic record in itself and a fuller realization of its distributional complexities, within and across site boundaries. Organizationally, the use of electronic data-processing makes it possible, on the one hand, to compact vast amounts of data in a format that is easily distributed and updated at almost zero cost; but what is more significant, this medium allows us to optimize the relationship between a capillary documentation of the data on the one hand, and the most highly generalized synthetic overview on the other.

Finally, a well integrated use of data processing allows us to retain at all levels a more consistent degree of precision. Characteristically, even when precise measurements are taken in the field, they are often lost after they are transferred analogically in the form of a drawing, be it a floor plan or a section. Electronic data-processing makes it possible to retain such precision without causing the user to drown in a mass of unstructured information. In this way, a true centimetric grid can be retained at all moments and in all areas of the excavation. Our special concern for such a degree of precision has found the most congenial type of collaboration and support on the part of two professional surveyors who have worked closely with us — Stephen M. Hughey who produced the map of the high mound (see his remarks below in Section 3.5) and helped me develop the conceptual approach to surveying; and Gabriel V. Pesce, who has given our staff formal training in the use of the instruments. Through a fuller application of graphic plotting programs (after the prototype published in Buccellati and Rouault 1983), we have tried to increase the degree of precision even at the

level of the individual supervisor without an undue increase of cost in terms of the corresponding accuracy.

With a view toward a wider dissemination of research results, the text portion of *Mozan 1* is also made available in electronic format as a disk published within the series *Cybernetica Mesopotamica Volumes*. These disks are available at cost from the publisher and may be copied at will with only nominal limitations.

As a companion publication to *Mozan 1* we are also issuing a set of color slides, published as the first in a series of *Photographic Data Sets*. This series replaces the series *Audio-Visual Modules*, of which three units have been published as companions to the *Terqa Preliminary Reports*. The *Audio-Visual Modules* have been discontinued partly because the narrative aspect which they had been meant to provide is now better served by videotapes, and partly because the highly structured nature of the Modules made them ultimately too expensive for the documentary function which they were meant primarily to serve. The *Photographic Data Sets* have neither a narrative structure nor an audio component, so that they are more flexible in structure and more accessible in cost. As a result, we hope that they may be more effective in serving the documentary need they are addressing. References to the first set (*PDS*-1) are given in this volume wherever pertinent.

Mozan 1 presents the most important substantive data excavated during the first two seasons. We have provided ample documentary illustrations and the essential factual information about the most important items, with greater detail than is usual for preliminary reports. Coupled with the global record, which is planned to cover the first three or four seasons, this will provide very rapidly an exhaustive data base of the material recovered. We intend of course to come back to different aspects of the data for a fuller treatment of the stratigraphic setting, of the typological description and of the cultural implications of our own finds; but we wish to build such a long term crystallization of our interpretation on a substantive and objective record which, in its basic details and its fundamental outline, can be laid bare from the very beginning as the essential starting point for subsequent research.

Such a deliberate effort at providing a rapid dissemination of the facts is not to be viewed as resulting from a distrust in the value of broader syntheses or from a disinterest in establishing the full comparative framework within which, we fully believe, the data must ultimately be understood. Our concern should rather be understood as a commitment to provide a solid base on which such syntheses may be more securely built. While this approach involves the risk of exposing certain rough edges, it has, we believe, an important theoretical consequence which in some ways runs counter to accepted conceptual approaches to field archaeology. Such a basic and exhaustive presentation of the data is less influenced by an overall interpretive conceptual scheme, precisely because the data have not yet been fully studied in terms of their cultural implications. This may, on the surface, appear to detract from the integrity of a research design. If we believe this to be true on the surface only, it is because in fact, upstream of any specific and well articulated topical orientation, the fundamental research design of an excavation *qua* excavation ought to be the recovery of the data in as pristine a manner as possible. In the first instance, therefore, we must be led by the data more than by a topic, especially inasmuch as we excavate data which are irretrievable in their contextual associations. In other words, even though we are led in our research by very a specific problem orientation, such as I have articulated in part above, we have a responsibility for global documentation which must be fulfilled regardless of how the data recovered fit into the research

strategy: for, however cogent and significant a research design may be, once we wield the tools and begin to disassemble the deposition we owe greater allegiance to the data than to the theory.

Certain aspects of our work which form an essential part of our ultimate goals are not represented as fully in this publication as will be the case in subsequent ones. In particular, I refer to the analysis of faunal and botanical remains on the one hand and of metals on the other, both of which have been collected systematically but require long term study (for a preliminary note on botanical remains by K. F. Galvin see below, 7.1). In addition, we also are developing a program for the study of human remains, if the indications of the presence of burials and possibly cemeteries in the outer city are verified by future research.

While I am leaving a presentation of the details of the electronic system to the forthcoming publication of the new encoding manual (which I intend to publish under the title *A Grammar of the Archaeological Record*) and of the first volume of the *Mozan Record*, I will add here a few words about a much less sophisticated instrument which for all its simplicity has contributed its share to making data gathering more effective. It is the triangulation rod illustrated in Figure 17. Since its practical operation should be readily apparent from the sketch, I will not describe it here. Suffice it to say that the rod is used to measure ties from fixed control points (set with the transit or other surveying instrument), and that a single person can easily operate it. The rod can readily be moved to different spots within a range of 5 to 7 meters from the control points, and within such range it could consistently reproduce measurements with an error factor within acceptable limits. It is very inexpensive to build, so that there may be as many available on the excavation as there are supervisors who take measurements and write notes.