



Jiroft and “Jiroft-Aratta”  
A Review Article of Yousef Madjidzadeh,  
*Jiroft: The Earliest Oriental Civilization*

O S C A R   W H I T E   M U S C A R E L L A

*Dedicated to my friend and colleague T. Cuyler Young, Jr.*

Rudyard Kipling's poem (from *The Elephant's Child*) has been with me for years—and I have previously cited it.<sup>1</sup> In researching the Jiroft story it resurfaced in my mind because the epistemological questions it poses are pertinent to the following discussion, and thus I (appropriately) cite it again here:

I keep six honest serving-men  
(They taught me all I knew);  
Their names are What and Why and When  
And How and Where and Who.  
I send them over land and sea,  
I send them east and west;  
. . . For they are hungry men,  
But different folk have different views . . .

Indeed, and it is the different views of the issues of asking or not asking, and not answering, the questions raised by the honest serving men that generates this review.

### 1. Introduction

Archaeologists have learned about a collection of remarkable unexcavated artifacts that were confiscated in Iran and accepted by many to have derived from the Jiroft area in south central Iran. They became aware of these objects from a number of sources: online Iranian news services beginning in 2001, Iranian Archaeological News Bulletin (IANB), Islamic Republic News Agency (IRNA), and the Persian Morning Daily; other on-

line news services; reports in two French archaeological magazines published in April (fig. 1) and October 2003 (2003a, 2003b), the latter fully dedicated to Jiroft with many photographs and articles by a number of scholars; three articles by A. Lawler (2003, 2004a, b); and in the United States public lectures have also commenced. In May of 2003 I began email discussions on Jiroft with some scholars in Iran and the U.S.

Madjidzadeh's book was published in Tehran in June 2003, after the French magazine report in 2003a, but before 2003b (I acquired it in April 2004).<sup>2</sup> Mentioned in some of the sources above but not here are brief discussions about excavations recently begun by Madjidzadeh in the Jiroft area (see 2003b, 65ff.) at Konar Sandal B; unnamed in Lawler 2004a, 40ff. Archaeological activity here, however, is just at the beginning stage of anticipated long-range excavation. Madjidzadeh 2003 is a casual, brief discussion in eight pages in English (pp. 5–12, with a French translation, pp. 13–19) about the confiscated material, accompanied by 163 plates of photographs (pp. 11–174) and a catalogue giving shape, material, and measurements of a *selection* of the confiscated corpus (pp. 175–218; see, for example, additional confiscated vessels in 2003b, 85); its paper jacket is superbly designed, most attractive. My evaluation and comments of the corpus follow in the text below, but it is pertinent to note up front that what we have is a hastily assembled catalogue-picture book, not an archaeological report. Vouchsafed are only a few brief,



Fig. 1. *Archéologia*, April 2003, cover.

inadequate, descriptions of some of the published objects; and less than a page dedicated to mentioning—but not evaluating—how the objects came into existence in the first place (other than to note that confiscation occurred). Utterly missing is awareness of the total absence of an *archaeological* background associated with the confiscated corpus and a perception of its limited value as data for archaeological and cultural

information. Unspoken, because assumed in the text, is that all the confiscated artifacts represent the ancient history of Jiroft (area)—a datum presented also in the scholarship offered in 2003a and 2003b. Indeed, although many of the objects have been published elsewhere (2003b) the photograph record here in one venue is valuable and important for continuous examination and research.

Up to the time of completing this review (August 2004) no excavated artifacts have been reported from Madjidzadeh's excavation (it is not a cemetery site) that relate or can be compared to the dissimilar, unexcavated objects published in his Catalogue. Therefore, only these latter objects *per se* are capable of being analyzed—and which objects, I argue, must in archaeological discourse be cited and labeled "Jiroft," not Jiroft, artifacts. An evaluation of information and assertions about "Jiroft" artifacts and the nature of their ancient culture explained in the 2003 Catalogue and the other published reports forces one to confront (again!) basic archaeological methodology, here concerning cultural and historical interpretations and evaluations involving unexcavated artifacts known solely from multiple confiscations, and with no records preserved of these acquisitions. Revealed is this, at present there is no knowledge of Jiroft (i.e., from excavations in the Jiroft area), and very little available of "Jiroft" except a number of objects. Between the two conceptions there is a profound epistemological and archaeological difference (but one would never know this from the "Jiroft" literature: alleged archaeological attempts at conclusion-forming procedures have altered very little in the last fifty years).

## 2. History of "Jiroft"

### *Date of the Plunder*

Most reports agree that flooding in the Jiroft area revealed ancient tombs leading to plunder at a number of cemeteries, but there is no consensus concerning precisely when the plundering first began: Madjidzadeh in 2003a, 37, gives no date except that of an incident in early 2002; in 2003b, 23, a specific date is mentioned for initial plundering "Au début de l'année 2001" a time repeated in the Catalogue (2003, 6) as February 2001; in IRNA online 7/29/03 the date is "mid 2001;" in Lawler 2004a, 46, the date is simply 2001, but in Lawler 2004b, 50, it was in 2000. Inasmuch as archaeologists and others eventually viewed the plundered areas, and photographs of destroyed tombs do exist (viz. Lawler 2004a, 44), it is a fact (one of the few in the whole story; see below) that mass plundering occurred at a number of sites in the southern Kerman province, in the area of Jiroft. A photograph of plunder-

ing in action is even published—but, unfortunately, without attribution or date (Madjidzadeh in 2003b, 22; Lawler 2004b, 50); and an Iranian scholar informed me in June 2003 that he had visited a cemetery site (but he did not say when) in the Jiroft area "while still a couple of hundred of people were still busy with illegal excavation . . ." In IRNA online 7/29/03, Abdolali Hessem Arefi stated that plundering was still in progress. For what purports to be a diachronic history of the initial plundering, the continuing plundering, and the difficulties of local and national police and archaeological authorities over two years, we have the article by Hamid-Reza Husseini in the Persian Morning Daily online, 9/8/02.

As if the same script writer were involved, the history of the discovery and confiscation of the "Jiroft" material uncannily parallels in all formal details, including local professional inaction, the discovery and partial confiscation of the plundered Kalmakarra cave material in western Iran: see Henkelman 2003, 214ff. To his credit, Henkelman also distinguishes between Kalmakarra and "Kalmakarra objects"; see below.

In the Bonham's antiquity sale of 9/22/88, at least nos. 171, and 172, a sculpted snake vessel and another vessel with cut-out rosette decoration are neatly paralleled in the Madjidzadeh Catalogue: pp. 109 and 115. This may indicate that plundering in the Jiroft area began much earlier than reported, at least sporadically, or that these objects have a wide geographical distribution.

### *Dates and Venues of Confiscations*

According to Madjidzadeh (in 2003a, 37), confiscations occurred in February 2002; in 2003b (p. 25) the date February 2002 is also mentioned as the time the police forces came into the area to protect it, and he gives the names of four towns and cities where confiscations occurred: Bardsir, Jiroft, Bandar Abbas, and Tehran; in the Catalogue (2003, 6), however, the date February 2001 is given as the time police arrested several smugglers and confiscated artifacts in Jiroft and Bardsir, and other cities. A 2001 date is supported by information reported in several online reports, although only one confiscation locus was mentioned, Jiroft. IANB 11/22/01 reports that 60 objects had been seized, in 12/13/01 that 120 objects had been seized; later, in IRNA 7/29/03,

50 objects are reported to have been confiscated in the previous three months. An Iranian colleague (Ali Vahdati) informs me that 33 objects were also recently (2004?) confiscated at Qum, south of Teheran. *Le Monde* online, 10/2/03, reports that in 2002 Madjidzadeh saw confiscated objects in the Kerman prison (their confiscation date is not mentioned), which suggests that sometime in 2002—at least a year after the time of the initial plundering—archaeological authorities first became involved. *Le Bien Publique* online (also 10/2/03) mentioned that customs agents had seized two trucks ("camions") filled with antiquities; the date and locus of this seizure were not mentioned, but the report stated that this seizure led to the discovery of "le site [sic] de Jiroft." Concerning a one-site identification, note that almost every archaeologist who discussed the "Jiroft" artifacts publicly (lectures) or in emails consistently mentioned them as if only one site were involved.

From the above information it seems then that just as with the time period of the plundering, the initial time of the confiscations is not remembered. Confiscations certainly began in 2001 and continued into 2002 and 2003—and later. But no information has been presented that indicates if the confiscations at the four loci mentioned occurred within days, weeks, or months of each other. And it seems that it was not until sometime in 2002 that archaeological authorities became involved in investigating the plundering—which had been in progress for a year or more.

### *Who Witnessed and Recorded the Confiscations?*

Nothing in the published sources vouchsafes information or interest concerning: how the multiple confiscations were accomplished (we have only one mention of trucks, but no geography)—i.e., whether by means of fortuitous or accidental information, via informants, voluntary surrender, etc.; which specific objects were confiscated at which of the towns and cities mentioned above; and what were the specific venues of the confiscations (aside from trucks). I raised these matters with colleagues monitoring the issues both in Iran and the U.S., stressing their importance and asking for answers. A scholar/administrator in Iran responded that it was "impossible" to answer the questions, that the confiscations were

accomplished by "different people belonging to various organizations." No records of the confiscation process or the nature of the venues were kept. An archaeologist involved in "Jiroft" issues wrote to me (email) vigorously ". . . you are not being objective or even rational . . . The information you demand will NOT be forthcoming! . . . villagers looted a [sic] site, local [sic] authorities confiscated the collection . . ."—it follows that archaeological decency demands that *all* the confiscated and published "Jiroft" artifacts be accepted as having been plundered from cemeteries near Jiroft: end of discussion.

The lack of basic data specifically indicates that no information exists that could enable archaeologists to comprehend what kinds of investigations and judgments were utilized by authorities in Teheran and Bandar Abbas (separated by over 1,000 km) that compelled the assignment of all the material confiscated as derived from the far-away "Jiroft" area. No records seem to exist that inform which specific objects were confiscated in the Jiroft area, i.e., adjacent to the plundered areas, and which in each of the two (three with Qum) distant cities—information that would reveal the corpus' modern cultural contexts, as well as allow analysis concerning whether objects or clusters of one particular style were confiscated in only one or in several of the loci. Confiscated objects are housed in a museum in the town of Jiroft (see Lawler 2004a, 48), but were they locally confiscated or were they transported there from Bandar Abbas or Teheran? In this context consider the following contradictory information—a leitmotiv of the problems discussed here: I was informed by a colleague in Teheran that there are no "Jiroft" objects housed in the Iran Bastan Museum and that all are curated in two venues in Jiroft and in the Kerman Cultural Heritage museum. An archaeologist colleague, however, informs me that while in Tehran he was told there were "at least three vessels in the Iran Bastan Museum," but he did not see them.

No archaeologist knows the venues where the confiscations occurred at each locus—i.e., whether in private homes, dealers' shops, bazaars, etc. Indeed, an object recovered in a dealer's shop in Teheran could have had (but not necessarily, see below) a different recent history than one recovered in the Jiroft area. Lacking such background provenance information confuses judgments regarding possible archaeological provenience and

possible authenticity of a confiscated "Jiroft" artifact.

To date, there exists in print *no* empirical evidence or information about a single "Jiroft" artifact, not even its confiscation history. This remains a crucial datum that has to be addressed up front in scholarly research on "Jiroft" and Jiroft. Hence, not a single one of the "said to have come from Jiroft" artifacts can archaeologically [*sic*] be identified as in fact plundered *solely* in the Jiroft region of Iran—not a minor issue here. This should be the starting point of any Jiroft/"Jiroft" discussion, but no scholar, including Madjidzadeh, has seen fit to confront it.

### *Quantity Plundered and Confiscated*

Madjidzadeh (in 2003a, 37) states that almost 1,000 objects were plundered, in 2003b (p. 25) that local authorities claim thousands of artifacts were plundered in the area, and later (in Lawler 2004a, 46) "We guess that 100,000 objects were looted." IRNA online 7/29/03 claimed "hundreds of thousands" of artifacts had been plundered. A fair statement on this matter is that no one knows how many objects have been plundered.

Speaking to the numbers of objects confiscated, Madjidzadeh (2003, 6) gave the figure as approximately 500 items, 300 of which were vessels; later (in 2003b, 25) he informed us that more than 2,000 objects had been confiscated. In his Catalogue (2003, 7) he is publishing "a large number of the objects recovered from the illicit excavations in the region of Jiroft," which comes to about 260 objects. Lawler (2003, 974) records that "many hundreds of vessels" were confiscated—how many were plain (a number of which were confiscated, see Madjidzadeh 2003, 159, 163), and how many decorated with motifs, is not revealed. We await an inventory of the confiscated objects.

### *Named Plundered Sites*

In Iranian Archaeological News Agency 11/22/01 and 12/13/01 online, objects were said to have been "unearthed in the old city of Jiroft . . ." Madjidzadeh 2003a, 37, also named Jiroft alone as the plundered site. And other scholars—viz. J. Perrot and S. Cleuziou (in 2003b) also refer

to only one site, Jiroft, as have scholars in the U.S. and Tehran in email messages, who mention "the site," "the cemetery," one cemetery—at Jiroft. Notwithstanding the one-site issue, Madjidzadeh (in 2003, 6 and 2003b, 25) names as the most important of the plundered sites, five situated from 29 to 53 km to the south of Jiroft (Jiroft itself is not mentioned here as a plundered site).

### *Excavated Tombs*

Tombs have been excavated by an archaeologist (H. Choubak) at Riganbar (one of the five named plundered sites). Although Madjidzadeh publishes a photograph of one with its burial goods intact (in 2003b, 25), nothing of its contents is mentioned. Ali Vahdati informs me from Teheran that this tomb was the only Bronze Age tomb excavated, and that the others are Islamic, and he confirmed my suggestion that no decorated vessels were recovered here. Worth noting is that Madjidzadeh is engaged in an excavation strategy of digging only a settlement site, not searching or excavating the plundered cemeteries (Lawler 2004a, 46). Such unilateral action ignores the precious work and model of the great Belgian archaeologist Louis Vanden Berghe, who spent fifteen years surveying cemeteries in Luristan that had been plundered for decades—and thereby recovered hundreds of intact burials. His model should be followed, not ignored—even one tomb excavated with Catalogue material would be a very significant archaeological discovery for Jiroft!

From information gathered from local plunderers regarding the number of objects recovered from the plundered tombs Madjidzadeh (2003, 6) says, "in many cases a single grave contained up to sixty objects." And from Lawler (2004a, 46) we are further informed from the same local sources that "each grave contained at least one stone vessel; the largest one contained 30." I think it not unfair to say that these inventory records are hearsay, and may not be cited as historical reality.

### *"Jiroft" Style*

There are several different styles, depictions, and a variety of iconographies represented in the confiscated corpus—especially articulated on bowls,

pedestal footed vessels, beakers, and "weights." Many of these styles and iconographies have not hitherto been encountered ("strange and different," having "many entirely new items" as one archaeologist—who defends the corpus' integrity—accurately reported to me). Perrot (in 2003b, 111) uses the phrase "style de Jiroft," but styles in the plural is a more accurate term, given that for many "entirely new items" parallels do not exist, except in "Jiroft." Madjidzadeh (in 2003b, 26) reported that clandestine diggers reported to him that relief decorated vessels were recovered, a claim again, which (here *parti pris*) has no archaeological value. And no one to my knowledge has reported finding a decorated vessel in surveys.

One archaeologist believed at one time (email message to me) that there is a "virtual absence of the classic intercultural style . . ." but later shifted entirely, claiming that ". . . the vast [*sic*] majority are indeed of the IS!" Cleuziou (in 2003b, 116f., 122) notes differences between "Jiroft" and Tarut artifacts, but stresses "stylistic" parallels (*viz.* entwined snakes); and Madjidzadeh (2003, 7) places all the chlorite objects in the *série anciennes*, *i.e.*, early Intercultural Style (IS).<sup>3</sup> A good number of the confiscated objects are canonical members of the IS corpus, or readily relate to them: see Madjidzadeh 2003 for imbricate, whorl, spiral, palm tree, hut, and animal and scorpion patterns: pp. 44, 67–75, 110–11, 117–18, 125, 127–29, 142; also the vessels illustrated in Pittman (in 2003b, middle and bottom of p. 85; also Cleuziou 2003b, 117, fig. 4, and 122). For convenient excavated examples for these and other motifs in the IS corpus see of course Burkholder 1971, Kohl 1975 and 2001, Zarins 1978, and Lamberg-Karlovsky 1988.

According to Madjidzadeh the corpus reveals a "high quality of workmanship," (2003a, 37, also in 2003, 8), but he is also aware (2003, 10) that it "is not always equal," that (here no doubt reflecting the multiple styles and iconographies) the corpus was "produced in different workshops, and by different stone-cutters, having different levels of skill and talent. But, in comparison with the Mesopotamian reliefs in stone, they appear in general to be of superior skill, talent and capability." He does not explain or develop any of these important cultural and aesthetic issues further, give specific examples, or discuss whether the different workshops recognized could be corre-

lated with different confiscation loci. But no one can dispute his observations on the skill and workshop issues.

(In a discussion with Philip Kohl an idea occurred: stone analyses should be undertaken of both Yahya and "Jiroft" stone material to determine sources—we know there was one near Yahya. I see no evidence that one can claim that Yahya craftsmen manufactured the latter's stone artifacts.)

### *Chronology*

Since most archaeologists involved in "Jiroft" accept that the published corpus derived from (somewhere around) ancient Jiroft, chronological ranges may be estimated and have indeed been proposed. Madjidzadeh (2003a, 37, 44; 2003, pp. 7, 12) asserted an early date for chlorite vessel production, 3000 B.C., or late 4th to first half of the 3rd millennium B.C.; this chronology is accepted by Perrot—"il y a 5000 ans," 3100–2900 B.C. (in 2003b, 97, 111). Cleuziou (*ibid.*, 116) raises the question whether this early chronology is correct or a later date, Early Dynastic II–III, even into the Akkadian period, is better, but seems to favor the later dating; Pittman (*ibid.*, 81, 87) accepts a general 3rd millennium date; Lawler (2004b, 50) reports (from an anonymous source) an "around 2500 B.C." date. Recently P. Amiet (2002/2004, 95f.) rejected Madjidzadeh's early dating, arguing for the late 3rd millennium. He is correct; and one would expect that the genuine IS period artifacts in the corpus be invoked for chronological determinations, comparing them to excavated material from Mesopotamia and Yahya, and that recovered from Tarut.

Madjidzadeh, however, *knows* (and therefore need not explain) that the Mesopotamian artifacts post-date those from "Jiroft" and Yahya. But disinterested analysis indicates that his and Perrot's beginning and flourish chronology of "Jiroft" is fundamentally wrong, too high by more than a half millennium. Concerning at least the IS material, the second half of the 3rd millennium B.C., which includes the late Early Dynastic and at least part of the Akkadian period, is an accurate general chronology. Lamberg-Karlovsky (1988, 54) sees IS material continuing into post-Akkadian times, as does Kohl (1975, 30; *idem* 2001, 215, 220f., 222, 224, 226f.), where it is claimed

that at Yahya IS material was made late in the style, in Akkadian or even post-Akkadian times. One of the issues here is recovery of IS objects in post-Early Dynastic, Akkadian contexts: see Martin in Aruz and Wallenfels 2003, no. 233, an IS feline-snake combat scene in Berlin bearing an inscription signed by Rimush, Lugal Kish. This object, albeit not excavated, is quite important for establishing the chronological range for these scenes.

What must be faced unblinkingly here is that the early unanchored chronology is generated not by archaeological reasoning, but by a priori tendentious, self-serving conclusion formations about "Jiroft" and "civilization."

### "Jiroft" and Civilization

Quite soon after the appearance of "Jiroft" artifacts the pitch of interpretative language was set very high. Madjidzadeh's Iranian to Mesopotamian relative evaluation is but a minor ingredient in the "Jiroft" brew, but in the same publication, beginning on the first page (2003, 5f.), he raised the level of cultural evaluation higher. The hyperbole of the Catalogue's title of course warns us to anticipate the instructions: that the confiscated objects "compel us to review our current ideas about the origins of the Mesopotamian, and in particular the early Sumerian Civilization," and that the objects "clearly suggest that a considerable part of the Sumerian art may have originated in the southeastern Iran, in the region of . . . Kerman," and (ibid., 12; also in Lawler 2004a, 43f., 48) that Jiroft (the Kerman area) is to be recognized as the "Land of Aratta," that "mysterious civilization . . ." Not unexpectedly, no specific objects or parallels are presented (compare Cleuziou's 2003b, 116, 117, and 122, more rational approach regarding specific Mesopotamian and "Jiroft" parallels and differences). There is more. Madjidzadeh (2003, 11), mentioning but not supplying recognizable iconographic parallels, informs us that the Sumerian Etana myth originated in Iran and traveled west. Hyperboles easily spawn others. C. C. Lamberg-Karlovsky in *Le Monde* (online 10/2/03) asserts that Jiroft "calls into question our fundamental [*sic!*] concept of the origins [*sic!*] of the age of the Middle East;" and in Lawler 2003, 973, shares his belief that "From now on, we must speak of be-

fore and after Jiroft," a conceit echoed by Perrot (in 2003b, 111). This is heavy stuff indeed.

This rhetoric is presented to the archaeological and public communities as a given, a perceived manifest "fundamental" truth—notwithstanding it is not anchored in empirical archaeological arguments or chronological analyses, and it does not consider that the results of site excavations lie in the future. For, regardless of what the significance of the Jiroft area's culture and chronology may be, manifestly without the benefit of excavations there can be no justification for the present rhetoric and hyperbole broadcast.

Accepting as archaeologically reasonable that the genuine artifacts confiscated in the Jiroft area most probably were plundered there, H. Pittman's nuanced assessment is closer to a meaningful appraisal. She compares the "Jiroft" culture formally to that of Mesopotamia and the Indus, but notes that it is "smaller in scale and less complex" (in Lawler 2003, 974), and (in Lawler 2004a, 42) rejects Madjidzadeh's "civilization" claims. I too have argued that the ecstatic claims under review do not reflect reality, that even if all the "Jiroft" material were ancient artifacts, it "is not world-shaking" (in Lawler 2004a, 48—with my original phrase restored).

### 3. Ancient and Modern Jiroft

Are *all* "Jiroft" objects *manifestly* of ancient manufacture? When I first encountered the "Jiroft" material in a lecture by H. Pittman in April 2003 (I heard another in March 2004), aside from typical IS present, I was surprised by the variety of styles, iconographies, and forms other than what is known either in Mesopotamia or Iran, including the Kerman area. Locally, Yahya shares the typical geometric IS non-representative motifs—the snake-lion and snake-snake combat, whorl, and scorpion representations (see Kohl 1975 for patterns with locus map, p. 24, and p. 26, nos. 2, 3, 4; Kohl 2001, 222, 226, figs. 9.8, 9.13; Lamberg-Karlovsky 1988, 55ff., figs. 1, 3, 4, pls. IV–X; omit from both articles references to Azerbaijan and Palmyra as excavated sites yielding IS objects). My sense, however, was that a number of objects could not *automatically* be accepted as ancient productions merely because they had been confiscated in Iran (of which action little is known). These views were reinforced

after further reading, but I readily admit that with ongoing study over time I have changed my opinions several times regarding the ages of specific objects.<sup>4</sup>

Judging from the publications and personal discussions, raising the issue of possible forgeries in the "Jiroft" corpus is not a popular opinion. The fervor aroused in some who accept the whole corpus as genuine may be understood from email comments by an archaeologist with whom I exchanged views regarding ancient or modern. Initially believing many objects to be "impossible," my correspondent noted that this position was soon reversed: ". . . I accept this collection as authentic" and "What is in the catalogue [Madjidzadeh 2003] is genuine," the corpus in toto is embraced as productions of the 3rd millennium B.C. My more hesitant opinion on this matter was dismissed as "based on personal authority . . . absurd," as "a voice crying in the wilderness," indeed, as an opinion that "matters little" (in the archaeological community—the ultimate dismissal!). Added was this psychological insight: "Your emotions dictate your perceived reality . . . [your] objectivity has flown the coop. . . . You have made up your mind . . . This is your . . . desire." To further differentiate my views from reality, to objectively (but here not the flown from the coop variety) document the authenticity of all the "Jiroft" artifacts, a graduate student in Iran who believes that all the "Jiroft" artifacts are genuine was invoked as an authority; "another authority" invoked was an archaeologist who it was alleged accepts their authenticity "without hesitation."

There are a few references in print where "Jiroft" forgeries are mentioned. Pittman (in Lawler 2003, 974) accepts that the corpus derives "from graves" but doesn't "know if [fakes] were added"—to the corpus subsequent to plundering activities. In this same venue I stated a more sanguine view about whether the full corpus derived from the plundered graves, and suggested that we have to "start at square one," that is, examine each object on its own terms (see also Lawler 2004b, 49, 50). Amiet (2002, 96) indicts two objects, one as doubtful (Madjidzadeh 2003, 106) and one as an obvious forgery (*ibid.*, 147). And in the *Art News* January 2004, 9, an unnamed London dealer is quoted that he is "worried about the growing number of fake Jiroft vases now circulating on the market." Gasp!

What fakes could this man possibly be talking about? What does this dealer know that "authorities" do not know?

A concern for forgeries is relevant here given that some time had elapsed between the time of the initial plunder and confiscation (which I am informed by Ali Vahdati is still occurring), at different and distant loci, and times. Hence, a long recognized and enduring modern cultural activity demands consideration: forgers begin work immediately after significant archaeological or plundered or confiscated finds become known, viz. the forgeries that surfaced along with genuine material "said to come from" (by dealers, curators, and collectors—and yes, also by archaeologists) Ziwiye, Luristan, Hacilar, Marlik, etc. The most recent manifestation of this activity is the existence of forgeries associated with the Kalkmakarra cave objects that were also plundered from Iran (Muscarella 2000; see also Henkelman 2003, 214f. and n. 120). Henkelman reports that among the "Kalkmakarra" objects *confiscated* in Iran some "seemed suspicious," and these are in addition to those objects smuggled abroad that he correctly identifies as forgeries.

To this unending list we now add "Jiroft," another modern construction of archaeological scholarship. That forgeries could have been made in Iran, in the Jiroft region itself or elsewhere, is viable, possible, most certainly not "impossible." This would explain how "Jiroft" forgeries could have been collected at any of the confiscation loci in Iran together with genuine ancient material—the confiscation occurring before smuggling abroad commenced. "Jiroft" objects surfacing abroad could thus include both genuine and forgeries smuggled out together to present a "found together" corpus—(a topos beloved by the collecting world of dealers, museum curators, and private collectors). No dealer or smuggler would be stupid enough to smuggle out forgeries and genuine objects in separate shipments. (After this paper was essentially completed, Ali Vahdati informed me [June 2004] that a colleague working in the Jiroft area told him that the police caught a local smuggler of stone vessels. The prisoner vigorously denied plundering the vessels from a site—no, he was not a thief, he was an artist! From the plunderers he purchased plain vessels that were worth very little money, decorated them with motifs that were copied from "originals," and sold them at a good



price. Vahdati's source said the copies were quite good, "one could hardly distinguish [them] from the original ones." I am not surprised, but until one sees the alleged forgeries and "originals" together I refrain from comment.)

Starting "from square one" means that all the confiscated, aka unexcavated, objects receive a close reading, a stylistic examination, questions asked why are they ancient, and viable answers attempted: put another way, go back to Kipling's honest serving men's questions—I cannot state it better. All the more so when one focuses on the "Jiroft" style/iconography/workmanship issues, which are barely discussed in the literature: because inasmuch as all "Jiroft" objects are ancient, why waste time? But judging disinterestedly from known excavated artifacts (aka archaeological data), a number of the confiscated carved and unique representations of humans, animals, and flora appear to be crudely portrayed, stylized in execution, unskillfully and non-uniformly (even within one scene) executed, especially hands, beards, eyes, feet, noses, etc. Stylistic analyses joined to its modern acquisition history collectively suggest that it is impossible to declare that *every* object in the "Jiroft" published corpus is necessarily an ancient artifact—although a good number are indeed ancient, and are listed below (see "a," below).

I cannot assert that any given object in the corpus is *absolutely* a forgery: not because I doubt what my eyes and knowledge tell me, or that I underestimate the skill and knowledge of forgers (the best are guaranteed good pay and lifetime jobs), but because I cannot claim to know, to perceive all the possible scenarios in their ancient/modern histories. Hesitancy is formal, an attempt to keep all options open, one of which is the possible presence of forgeries. An archaeologist experienced in IS and Iranian scholarship shared this view with me: "I would argue that it would be almost impossible to decide whether most ["Jiroft"] objects were genuine or fakes." Which neatly defines the problem I am articulating here: absent a Jiroft against which to compare "Jiroft," one is compelled to focus on problematic orphaned objects. If we do not attempt to know which "Jiroft" objects are ancient and which modern, how can we begin to discuss Jiroft?

A number of "Jiroft" objects stand out from the IS corpus, suggesting (at least) that archaeologists not automatically accept and introduce

them into discourse on ancient artifacts; these are listed below as what at best are called probable forgeries (below, "b").

In addition, my eyes recognize a number of complex and ambitiously made pieces with which I continuously wrestle (see below, "c"). They may not be ancient—but I have changed my mind more than once with regard to the age of several of them. Given this unclear view—à mon avis of course—I propose that they be kept in abeyance, subject to physical and continuous stylistic analysis. And cited within parentheses, with a caveat. The objects themselves are not numbered and can be identified only by page references in Madjidzadeh's Catalogue (which I employ here). I have not autopsied a single object from the "Jiroft" corpus, but note that those who accepted the corpus objects originally did so also from photographs.

In the discussions I list some examples of possible "Jiroft" objects that have surfaced outside of Iran and appearing for sale in auction and dealer catalogues. If I am correct about attribution they should be considered as additions to the corpus. A number of objects offered for sale—both ancient and forgeries—that may have derived from "Jiroft" are not cited here, as I remain uncertain about their source.

#### a. Ancient "Jiroft" Artifacts

The following objects listed in Madjidzadeh's Catalogue are clearly ancient. A number are members of the IS group, with canonical motifs—whorls, imbricates, huts, palm trees, guilloches; triangles, mat weaves on IS vessels—all executed by ancient artisans (for IS parallels see Burkholder 1971—objects she herself witnessed as deriving from Tarut; de Miroschede 1973; Kohl 1975; 2001; Jarins 1978; Lamberg-Karlovsky 1988). Some objects are sophisticated works; others are relatively minor, plain and undecorated works. The following list identifies a corpus that (wherever confiscated and wherever actually plundered) forms the base from which one may eventually add more identified genuine artifacts: pp. (15–17?), 37–38, 39, 40–41, 44, 51–52, 53 (three objects), 57 (?), 60–61, 62–64 (?), 65–66, 67, 68, 69, 70, 71, 72, 73, 74, 75, 76–77, 80–81, 82–83, 86, 87–88 (see "c," below, no. 11), 89–90, 91, 92–94, 97, 98, 99–100, 103, 104, 107, 108, 109, 110, 111, (112–13?), 115, 116, 117,

118 (fig. 6, bottom), 119, 120 (some), 121, 122 (note the plain, undecorated pedestal base goblets here), 125, 127, 128, 129, 134, 137, 138, 140, (141? compare p. 120, top), 142 to 146, 148–65, 167–72 top, 173, 174 (?). For some of these objects see also Pittman in 2003b, 78–85.

With regard to pp. 114–15, six vessels decorated with rows of scorpions: all examples are very ambitious in execution, and perhaps not what a forger might undertake. They are crowded with scorpions, and none on each vessel is executed in the same way with regard to claws, tails, "wings," nor are these units juxtaposed in the same manner. The conical vessel on the right of p. 114 and the bowl on the right of 115 are the best made here, and seem to be authentic; they relate to the scorpions carved on the weight, p. 127, which is ancient; and also to an example excavated at Yahya—note the tail pattern (Kohl 1975, 26, no. 3; Kohl 2001, 224, fig. 9.11; Lamberg-Karlovsky 1988, pl. VIII). Are the other vessels here also ancient? Probably yes. A scorpion-decorated vessel was excavated at Nippur (Kohl 1979, fig. 3). And a number of similarly decorated vessels exist in various collections; they are not uncommon.

I suggest that a vessel in the Ishiguro collection is not ancient (Muscarella 2000a, 171, no. 18-a), but as reported in n. 3 here, a vessel bearing a complex decoration that includes scorpions (*ibid.*, no. 18-e) about which I was originally cautious I now realize is most probably ancient.

For the three raptor plaques, pp. 130, 131, 132, the only iconographical and formal parallels—almost exact, even to the use and position of inlays—are the 8th century AD Visi- and Ostrogothic brooches, which naturally generate doubts (I anticipate articles on the proto-Sumerian Ostrogoths migrating [slowly] from their homeland, "Jiroft/Aratta." The first two plaques are quite close, but all three derive from different hands. The raptors' heads and beard positions of the first two are close to those of pp. 92–94 (a masterpiece: perhaps the same motive occurs on a fragment of a vessel from Uruk where two snakes are attacked by two, not one as the authors state, raptors, Lindenmeyer and Martin 1993, 161 and pl. 68, no. 1102). These plaques are probably ancient, unique, artifacts (otherwise we have a really first-rate forger's work before us): but for the record, I shifted them from here to problem pieces, section "c," below, and back, several times. Again, the "Jiroft" problem.

### b. Probable Forgeries

1. bowl, pp. 45–46 (fig. 2), Hero mastering bulls: I suggest that the very poor execution and inconsistencies of the bulls' heads—eyes, noses, beards, bodies, and the very insecurely and badly planned and executed wavy water lines over the bulls (compare pp. 53, 125), the position of the leg tufts jutting from the hooves themselves, and the man's head condemn this piece. Compare all the details of the bulls' physical forms, and especially the flowing water lines above the heads, with those on a conical vessel recovered from Tarut (recorded there by G. Burkholder; eventually it was sold to a collector), and an unexcavated but genuine vessel in the British Museum (Aruz in Aruz and Wallenfels 2003, nos. 226, 227); the Tarut vessel must have been the model for the "Jiroft" object (see also below, no. 3).

See also the bull depicted in Bonhams 11/7/02, no. 196, a conical vessel most likely from "Jiroft." The vessel shape is the same as that of pp. 54–56 (see below, no. 3), and which P. Kohl informs me is not a classic IS form.

2. pyxis, pp. 47–48 (fig. 3): mermen (not intended) holding something in each hand apparently feeding bulls—for which compare those represented on pp. 53 and 125. Also, the uniform gross workmanship, style, and iconography force us to reject this piece as an ancient artifact.

3. bowl, pp. 54–56 (fig. 4): an ambitious iconography—a human beardless head projects above the rims of four pithoi—perhaps meant to depict individuals bathing during the summer's heat. But the crude and unskilled, as well as inconsistent, execution of the heads and misunderstanding the tied-up chignon, as well as their different placements on the vessels' rims; the different neck forms of the depicted vessels; the uneven body decorations; and the snakes' body decoration and closed-mouthed heads collectively suggest a modern creation. Compare the bowl shape to that of pp. 45–46 above, no. 1.

The IS fragment recorded from Tarut (Zarins 1978, pl. 70, no. 49; Burkholder 1971, pl. VII, no. 21; Muscarella in Aruz and Wallenfels et al. 2003, no. 224-e) is the model against which the present example must be compared, detail for detail: the human head with chignon, the open-mouthed snake behind. The differences are quite manifest.

One could posit that "Jiroft" was locally copied from Tarut (but when?). On the Tarut example there is a vertical straight line below the human head, as opposed to the curved one at "Jiroft." It is not certain this line depicts a vessel—but one cannot exclude the possibility. A stone fragment from Uruk depicts a figure identified as a male with the same chignon form situated to the right of an unidentifiable motif—not a snake (as Lindenmeyer and Martin 1993, 128, pl. 61, no. 690). These two examples document the IS chignon hairstyle, but was the chignon gendered for men or women? (Lindenmeyer and Martin's alleged male parallels are not parallels.)

The "Jiroft" bowl has a neat motif-mate; it was purchased (of course "in good faith") by the Louvre (A. Benoit, *Revue du Louvre*, October 2003, 13ff., fig. 1) after being offered for sale in Hotel Drouot, Sept 30, 2002, no. 212 (see also Cultural Heritage News online, 10/29/03). Depicted are two human heads facing each other within a centrally placed vase that is framed by snakes. The motif is precisely that of pp. 54–56; both remain unique. The Louvre human heads have feminine-appearing faces and a rear chignon, all of which features seem to be correctly executed—far better, than that of pp. 54–56. Is the latter ancient?—uncertain, not impossible; it is much better executed than the bowl.

The Louvre curator Benoit (*ibid.*, 14) does cite the parallel with pp. 54–56, as does Perrot (in Lawler 2003, 975), but in the latter publication A. Caubet (Benoit's senior curatorial colleague) is (transparently) indignant, "dismayed by the accusation" that her purchased vessel derived from the "Jiroft" plunder—even though aware that the only known parallel derives from that corpus. In fluent museum-speak both curators dissimulate, disingenuously disassociating the (if genuine, then manifestly plundered) piece they purchased from the vulgar plundering of Jiroft. Benoit notes a different color of stone, which is not relevant to stone composition; and Cleuziou correctly observes (2003b, 120) that its shape does not occur at "Jiroft."<sup>5</sup>

Perhaps we will also be instructed that other "Jiroft"-like vessels recently purchased (in good faith) by the Louvre most absolutely did not derive from the "Jiroft" (or of course, as any curator knows, from any other) plundering activity (see A. Benoit, *Revue du Louvre*, June 2003, 86f., figs. 1, 2; Hotel Drouot 2/26/03, no 221); rather—as

everyone in the Louvre knows—they were found by poor peasants when tilling their gardens.

4. p. 106 (fig. 5): vase with entwined snakes: ambitious but very crowded and not good workmanship or execution of the heads and ears, and especially the uncanonical presence together of both round and oval body markings. Certainly it remains at least suspicious (as Amiet 2000, 96). Perhaps p. 103, which has a similar scene, is ancient as here the snake heads are more securely executed.

5. p. 118 (fig. 6): top two conical beakers: both decorated with a whorl pattern that displays incompetent workmanship—a lack of understanding of the pattern, and insecure execution of each whorl. Such qualities I do not find among the ancient corpus (viz. de Miroschedi 1977, pl. II-a; Zarins 1978, nos. 114, 308; Lamberg-Karlovsky 1988, 78, pl. X-b). P. 117 is a vessel with a superbly executed whorl pattern, one matched exactly except in size by a vessel offered for sale by the Safani Gallery (*Ancient Form*, 2004, 11). Two other vessels decorated with whorl patterns are in the "Jiroft" corpus: Pittman in 2003b, 85, lower left and right. Both are not made by the same hand; I am more comfortable with the latter example.

Some other vessels with overall whorl patterns were offered for sale at Christies, London, 5/15/02, no. 254, and the same venue, 5/13/03, no. 13. The Louvre purchased a very obvious, stupid forgery of this motif (P. Amiet in *Revue du Louvre* 1, 1987, 16, no. 7; Muscarella 2000a, 170, no. 9).

6. p. 126, "weight" (fig. 7): an ambitious work depicting raptor and snakes on one side, floating scorpion men on the other. No such decorated weights have been excavated, but the raptor motif is of course well known. Here I see poor workmanship and inconsistencies and a lack of uniformity in the execution and positioning of all details of the depicted figures, which makes me reluctant to accept it without question. Note the scorpion-men's thumbs, fingers and arms, ears, eye positions; the whorl constructions and positions, the tails; also the raptor's body, talons, beak, and beard (compare also pp. 15–17, 112–13, nos. 3 and 15 in section "c," below). For excavated raptors flanked by snakes see the examples from Nippur and Tarut (Aruz in Aruz and



Fig. 2. Catalogue, pp. 45-46.

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Fig. 3. Catalogue, pp. 47-48.

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Fig. 4. Catalogue, pp. 54-56.

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Fig. 5. Catalogue, p. 106.

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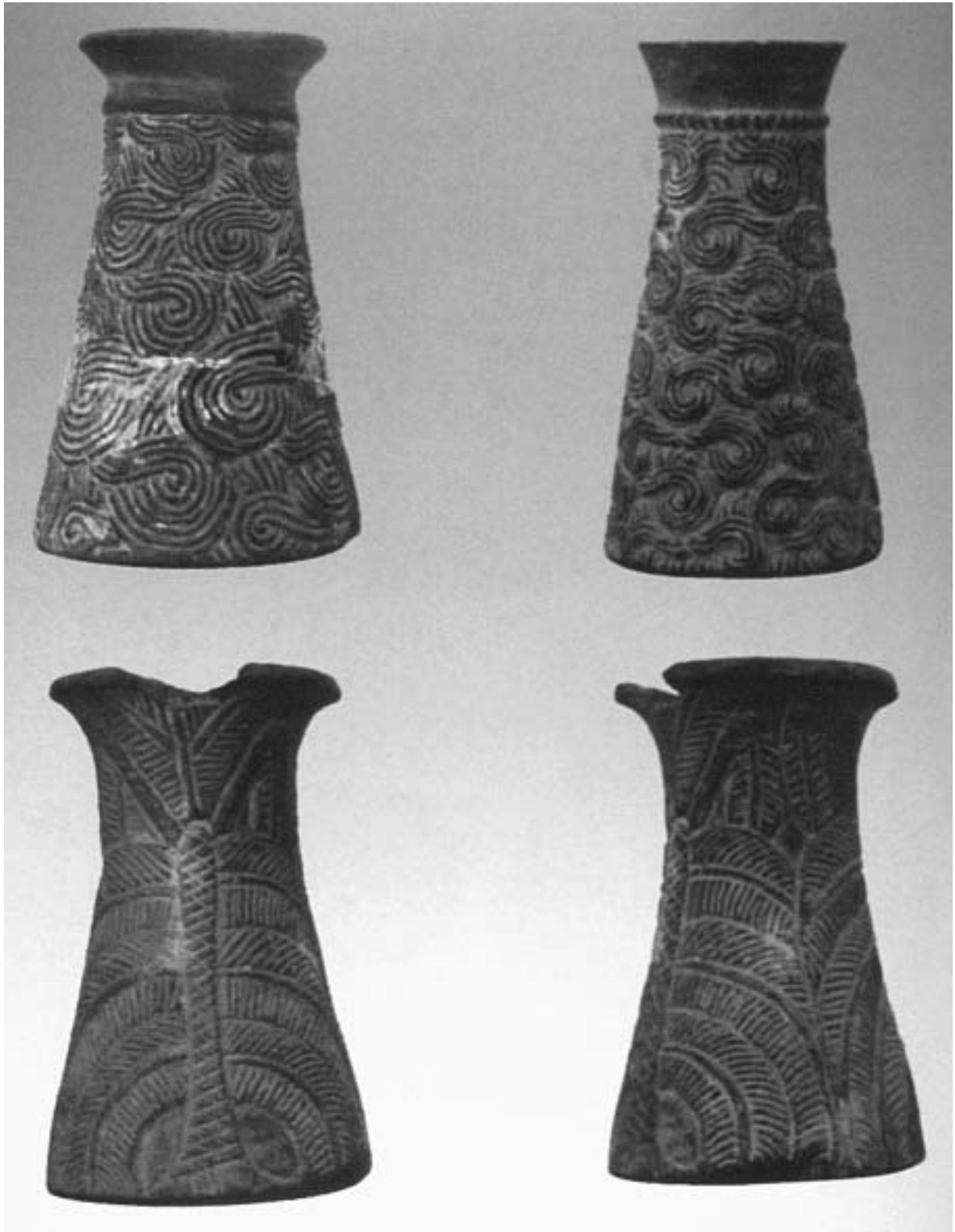


Fig. 6. Catalogue, p. 118.



Fig. 7. Catalogue, p. 126.

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Fig. 8. Catalogue, p. 139.

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Wallenfels 2003, no. 234; Kohl 2001, 223, fig. 9.9; Zarins 1978, pl. 68, no. 159), as well as “Jiroft” (Madjidzadeh 2003, 92–94). For the snakes’ ear forms, noses, mouths, compare the British Museum vessel (Aruz in Aruz and Wallenfels 2003, no. 227), and again the Nippur and “Jiroft” vessels. See also Muscarella 1993, 144, no. 1, fig. 5-a.

Plain, undecorated weights were also confiscated and included in the “Jiroft” corpus; they are not published in Madjidzadeh’s Catalogue (but see Islamic Republic News Agency 7/29/03, 3). Further, the Yahya team has revealed the presence and availability of undecorated weights in their area (Potts 2001, 115, 142f., figs. 4.41–4.43). Such weights could (would) have attracted forgers (local artists) who embellished them. For other examples of probable weight forgeries or problem pieces with the raptor and snake motif see:

a. a weight offered for sale by Christies, London 12/7/1994, no. 181 (Muscarella 2000a, 169, no. 5; see also n. 58). That it is not ancient is manifest by the snakes’ teeth—in what looks actually like felines’ heads; the legs and talons of the raptor are misrepresented; and the execution of the snakes’ incised body oval decoration are not those of an ancient craftsman.

b. for the crude, inexperienced modern artisan’s weight on sale at Bonhams 4/13/2000, no. 300, and again in Bonhams 11/8/01, no. 201, no comment is necessary if one looks at it for one second.

c. the weight decorated on both sides offered for sale in Hotel Drouot 2/27/03, no. 30, probably from “Jiroft,” and decorated on both sides—a Hero with a tail mastering snakes, on the other a raptor-snake scene; in both cases the snakes’ bodies continue onto the handle itself. This is a complex and very skillfully made work, better than that of p. 126 and the Barakat example (“d,” below)—but the snake’s body crossing over the raptor’s body puzzles me, and the Hero has an oval inlay in his hair. Nevertheless, a fragment of a weight handle from Yahya seems to preserve the body of a snake (it has oval markings), but not in the same manner as the present example (Lamberg-Karlovsky 1988, fig. 3-F, pl. IV). It may be genuine, but warrants more study.

d. the weight for sale in the Barakat Gallery catalogue vol. 32 is surely from the “Jiroft” corpus, primarily because of the lion-raptor combat motifs. It has very complex and amazingly ambitious decorations on both sides, lion-snake combats and central figures. But a close reading of all the details reveals many poor and inconsistent carvings: on the obverse, the central double-headed monster’s hands, shoulders, ears, necks, feet, and his standing on air; the flanking snakes’ awkward bodies and positioning, nose markings, mouths, tail, vertical body markings; and the lions’ crudely executed not-uniform claw structures, mouths, feet, tail terminals—one is sculpted as a unit of the animal’s back: compare Madjidzadeh’s pp. 87–90—models for this weight? The same execution problems exist for the other side: the lions’ feet, head, and eyes, and tails; the squatting man holds water flowing from the addorsed bulls’ heads; his face, body, his leg lacking a foot, and his kilt should be compared to an example from Tarut (Muscarella in Aruz and Wallenfels 2003, no. 224-d), one in the British Museum (Aruz in Aruz and Wallenfels 2003, no. 227, fig. 85), one in Japan (*ibid.*, no. 235); and to section “c,” below, no. 2. This ambitious piece generates reservations, and at present cannot automatically be accepted as ancient.

e. the weight offered for sale in Hotel Drouot, 2/23/02, no. 393, seems also to be a member of the “Jiroft” corpus, but whether from this area or elsewhere I suggest it is not an ancient artifact: examine the raptor’s body decoration and beak, and the single animal in one talon; the mountain motif (compare Aruz and Wallenfels 2003, no. 226), the isolated and upside-down moon crescent (compare *ibid.*, fig. 85). (In the same auction catalogue, again whether from “Jiroft” or not, nos. 397 and 398 are also not ancient.)

f, g, h: three weights in private collections are probably forgeries; they are listed in Muscarella 1993, 146, no. 3, p. 149, nos. 7, 10, figs. 7-b, 11, 13; *idem* 2000a, 169, nos. 2, 3, 4, also pp. 484, 485.

7. p. 139, small idol (fig. 8): is there any reason that allows one to assert it is ancient? Note the mouth open (caught, in surprise?), the unique hairstyle (“Jiroft” style), and the presence of scorpions as the figure’s arms.



8. disc, p. 172-b: a seemingly meaningless, apparently failed attempt to depict something, a human face?

### c. *Problem Pieces*

As noted, there may be more forgeries in "Jiroft" than listed above. A number of objects presented in this section may in reality belong in section "b," above—or indeed, perhaps in "a." This category exists because I find it difficult to react positively to unparalleled or inconsistently executed styles and iconographies (merely because they were confiscated), and think more stylistic and technical analysis is required; here too I have changed my mind on several objects. What is on trial here—the jury is still out—are the very objects that in fact define the "Jiroft" Style.

1. pp. 11–12, pedestal goblet (fig. 1, lower left): two Heroes master two upside-down lions: I see the human head positions, their faces, eyes, ears, hair to be frozen, not alive; equally so the legs, feet, clothing. I am uncomfortable with the articulation of the felines' rear leg positions, and they stand on their front legs; also, the scorpion uncharacteristically lacks its sting. I do not know any specific parallels.

2. pp. 13–14, beaker (fig. 1, right): motif of a Hero with bulls, while another is in the mountainous sky holding water or a rainbow, or a jumping rope? All is seemingly satisfactory until we compare the men's heads, faces, the nose mouth curves, hair, clothing, different belts, the articulation of their shoulders, forearms and fingers, and the leg and feet of the seated Hero—with excavated male figures in the same kneeling position: Zarins 1978, pls. 70, 72, no. 546; also Muscarella in Aruz and Wallenfels 2003, no. 224-d; Aruz in *ibid.*, no. 227. The stylistic and detail differences are apparent, and probably instructive. The iconography of the lower bulls below a mountain motif is closely paralleled to a genuine vessel recovered at Tarut (see also above, "b," no. 1), here lacking the male figure (Aruz in Aruz and Wallenfels 2003, no. 226, for mountain motif and bulls), but the execution skills are quite different—when was the one modeled from the other? I also think the star and crescent are

poorly executed (compare Burkholder 1971, pl. IV, no. 11). And contrast the bulls' heads, double peaks at the top, water pattern, and hump to p. 53 in the Catalogue.

Cleuziou (2003b, 122) accepts the authenticity of this vessel, comparing it favorably to the Tarut vessel (above). I tend toward the negative, but remain indecisive—which is again an example of the "Jiroft" problem!

3. pp. 15–17, bowl (fig. 9): Hero mastering scorpion-men; for the latter compare p. 126 (no. 6 in section "b," above), and no. 15, below. I find the human and scorpion-men's heads difficult to judge—but I am more than not compelled to accept it as probably ancient.

4. pp. 18 through 33, a difficult group to work with given their unexcavated status and stylized elements: six pedestal goblets with grazing animals and stylized trees. Some of these trees I find too stylized, with outlined leaf borders: pp. 21–23 (fig. 10), 27–28 (fig. 1, upper left), 30–31 (fig. 11)—although the trees of pp. 18–20 (fig. 12), 24–26, perhaps 32–33, do seem more natural with their isolated leaves. Compare the similar trees from Mari (Wilson in Aruz and Wallenfels 2003, no. 231), and the ancient trees of the Catalogue's pp. 40–41. Pp. 18–20 has at least four separate animals, with their young, in no regular order or position, depicted eating at trees. Pp. 24–26 has two separate levels of different animal groups; the trees are different. Pp. 32–33 seem well executed. These three vessels warrant more attention; they should be examined and compared to the others I list here. They seem easier to accept as ancient, more confidently executed and the motifs seem natural—was one or more the model for the others? Note that a number of plain pedestal vessels were also confiscated (p. 122): how many of the original were plain and later embellished with scenes—copied from which vessel?

There are similarly formed excavated tree representations in naturalistic depictions from Mari (Aruz in Aruz and Wallenfels 2003, no. 231); see also naturally executed palm trees on pp. 37–30, 118, 127, 128 of the Catalogue; also Muscarella in Aruz and Wallenfels 2003, nos. 225-a, b.

The Barakat Gallery has offered for sale a conical vessel with the same basic animal and tree decoration; see also Hotel Drouot 9/30, 2002, no. 213.



Fig. 9. Catalogue, pp. 15–17.

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Fig. 10. Catalogue, pp. 21–23.

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Fig. 11. Catalogue, pp. 30-31.

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Fig. 12. Catalogue, pp. 18-20.

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Fig. 13. Catalogue, pp. 34-35.

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Fig. 14. Catalogue, pp. 58-59.

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Fig. 15. Catalogue, pp. 65-66.

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Fig. 17. Catalogue, pp. 95-96.

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Fig. 16. Catalogue, pp. 78-79.

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Fig. 18. Catalogue, p. 105.

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Fig. 19. Catalogue, pp. 112-13.

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Fig. 20. (above left) Catalogue, p. 123.

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Fig. 21. (above right) Catalogue, p. 133.

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Fig. 22. (bottom left) Catalogue, p. 136.

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5. pp. 34–35, pyxis (fig. 13): the Christmas tree ball effect—compare a similar motif on pp. 24–26, the positions and executions of the raptors—one trampled by a caprid, others hitting the ground—and three different trees, collectively bother me. But from what I can see of the execution, it looks good: the work is possibly ancient.

6. p. 36 top, conical vessel, with vertical, one-horned animals. Its crudeness bothers me, also the two methods of depicting the unicorns' heads, as well as the straight lines on the horn (a vessel of this form also with a unicorn is in the California Museum of Ancient Art); but the execution of the tree's leaves seem correctly executed.

7. pp. 42–43, pyxis: two lions each with a totally unique head and body-form design. Were they added to a damaged ancient vessel to increase value? Two conical vessels on pp. 37–39 seem also unique, but their execution looks fine and I am not compelled to indict them. Compare the executions of the trees on all these vessels, also the date tree to pp. 110 and 111. A problem piece.

8. pp. 49–50, pedestal goblet, recumbent ibexes below trees: the ibexes' eyes, tails, horn tips, and the trees are poorly executed. A companion decorated goblet made by a different craftsman is in the Barbier-Mueller Museum (Amiet in *Arts and Culture*, fig. 15).

9. pp. 58–59 (fig. 14): back-to-back lions gored by bulls, a young bushy-tailed animal and a raptor rests on the back the bulls, another is between them. The inconsistency of the drawing and execution of the lions' underbelly and their claws, and the raptors wings creates doubts.

10. pp. 65–66, conical vessel with a bull-leaping scene (fig. 15): I do not call attention to this piece here because I doubt that this scene could appear in central Iran, for the motif existed from the Aegean to the Indus Valley (see Aruz in Aruz and Wallenfels 2003, 409 and fig. 100-c). But again—and of course at the very core of the "Jiroft" problem—I am held by the crudeness of the execution and pose of the figure standing on a bull while holding a tree at the same time, his slightly raised right foot, and his bald, speckled head. I find, on the other hand, that the leaping figure situated between the bull's horns with his foot wrapped around the tree not difficult to accept. The bull is tied to the tree, an original and intriguing scene, worthy of further research. If the

scene is anciently executed—not impossible—then the fact that the human's contact with the trees has cultural significance.

11. pp. 78–90, and Pittman 2003b, 85, upper left: there are eight bowls each with feline-snake encounters. I do not find it difficult to accept as ancient the conical vessel on pp. 76–77, but am less certain with the execution on two other bowls: pp. 78–79 (fig. 16), 84–85, viz. the felines' heads, their aberrant claw constructions, the snakes' heads and ears, the form of the oval body decorations: compare these with the same forms or scenes on excavated pieces from Yahya, Mari, Nippur, and Tarut (viz. Kohl 1975, 26, no. 2, idem 2001, 222, fig. 9.8; Lamberg-Karlovsky 1988, 78, fig. 3-G, pl. IV; Muscarella in Aruz and Wallenfels 2003, nos. 224-a, 224-e, 234; Aruz in *ibid.*, no. 232 and fig. 87; Godarzi in *ibid.*, no. 242-a; Zarins 1978, nos. 58, 135, 157, 542, 545; Kohl 1979, fig. 5). Compare also the superb snake and lion combat scene on a fragment in Berlin, (Martin in Aruz and Wallenfels 2003, no. 233), bearing an Akkadian inscription on its rear—Rimush Lugal Kish (see above). Some of the excavated examples depict snake combats or snake and lion combats; on the latter examples note especially the lions' claw construction.

The six vessels on pp. 80–81, 82–83, 86, 87–88, 89–90, and Pittman, above, appear to be ancient (see section "a," above; and compare the entwined snake heads of pp. 89–90, 93–94, 99–100 to the Khafajeh vessel cited by Madjidzadeh (2003, p. 10, n. 8; see in addition Kohl 2001, 215, fig. 9.5). The fragment on p. 91 is most probably ancient (are the claws unfinished?).

Representations of the same scene and style occur in the antiquities market: Hotel Drouot 2/13/02 nos. 399, and 405—purchased by the Barbier-Mueller Museum (Amiet in *Arts and Culture*, fig. 1) but here the lion bites into the snake, which to my knowledge is unique (?). A very bad forgery of the scene is in a Japanese collection (Ishiguro?): *The Ancient Orient Museum*, Tokyo 1978, no. 48, Muscarella 2000a, 171, no. 17.

12. pp. 95–96, bowl: raptor and snakes (fig. 17); the placement of the serpents across the raptor's wings is I believe unique, but not impossible; and we should expect to see a beard on the raptor's chin, and is his beak too elongated. But not insignificant, the snake has round, not oval, body



patterns, not present on excavated snakes, which feature catches our attention.

13. pp. 101–2, bowl: what is the scene? Note the floating snakes' heads and an unidentifiable unit—a head, fire? Perhaps genuine, but to be kept in abeyance.

14. p. 105, pedestal goblet (fig. 18): the bodies of the entwined combating snakes seem to get lost in the entwining; other examples in the corpus are better executed—an example from Tarut (Burkholder 1971, pl. VII, no. 20). Also how do we explain the line that divides the decoration on each snake's body? A problem piece to be further investigated.

15. pp. 112–13: bowl with double row of swimming-floating scorpion-men (fig. 19): it is possible this is an ancient creation. It seems too ambitious even for a sophisticated forger to make—but there is no consistency in the execution of the heads, faces, and tails. Compare pp. 15–17 and 126, b, no. 7, and no. 3 above. A bowl in the Barbier-Mueller Museum and another offered for sale (Amiet in *Arts and Culture*, fig. 11; Christie's, London, 5/15/02, no. 265) have the very same swimming scorpion-man motif, but all were made by different craftsmen.

16. p. 120, lower right, vase: the upper row of attempted guilloches is very badly executed in form and spacing, and differ in each case, and is not paralleled from the excavated corpus.

17. pp. 123 (fig. 20), 124: two weights in the form of openwork entwined snakes. Both are entirely different in shape, body decoration, and sculptural symmetry; no. 124 seems less finished. Models for the motif exist: a weight from Soch in Uzbekistan, and an example in the Louvre (Muscarella 1993, 144, fig. 4, 149, no. 9; Kohl 2001, 227, fig. 9.14). Are both the "Jiroft" weights ancient? Or is one ancient and the other a modern copy?

These two snake weights call our attention to a handle offered for sale in Hotel Drouot 6/26/03, no. 113, a handle sculpted in open work that depicts a male figure sculpted in the round from his kilt to his head. No feet are depicted, but below the kilt there is an unparalleled unit of three triangles at the front and a unit of four curved forms at the rear, so he cannot be said to be kneeling conventionally; he masters two snakes, one of which is connected to him by a strut. Of interest is that the male's eyes seem to depict a

blind person—consciously, or unintended? Perhaps this weight doesn't belong to the "Jiroft" corpus. It is a complex, unique, well-made object—but its uniqueness demands more investigation before secure acceptance.

18. p. 133, double-headed raptor plaque (fig. 21): aside from not first-rate workmanship and five and six claws terminating in different lengths, it is an unparalleled work—why accept it unconditionally?

19. p. 135, scorpion plaque: doubts raise themselves with regard to this plaque unparalleled elsewhere; and the excellent execution of the whole, especially the face, confuses me. Why are its "wings," body and tail forms and their decoration different than the other scorpion plaque on p. 136 (fig. 22)? I am not so secure with this scorpion plaque either but cannot outright condemn either one.

20. p. 147, two odd, seemingly unfinished stone figurines, one a human, one an animal head (?). Amiet (2002, 96) considered these to be forgeries. He may be correct as they are quite formless, but who knows. In any event, they can have no archaeological value.

21. p. 166, two resting felines, both said to be made from lapis lazuli. They are close but not the same and quite simple, and it is difficult to form a conclusion about their ages. Note the intentionally scarred right eye of one lion, and the different front feet constructions, one open, the other closed. The figures on p. 167, however, do not cause concern.

## Conclusion

It is argued here that if Madjidzadeh and other scholars had approached the issues while attentive to Kipling's honest serving men's questions, all appropriate to archaeological discourse, this review might have been of a different nature.

## Notes

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1. See *Bronze and Iron* (New York: Metropolitan Museum of Art, 1988), p. 9.

2. On the book spine and its cover the author's name is spelt Madjidzadeh, but in the three pre-text publisher pages it is spelt Majidzadeh. I use the former spelling because libraries will use it. Fred Hiebert kindly sent me copies of photographs previous to my acquiring the volume—as a gift from from Ali Vahdati in Tehran.

3. For a defense of the term Intercultural Style, see Kohl 2001, 209, 215f.

4. I experienced similar views in the past regarding then strange iconography's surfacing on the antiquities market that related them to IS objects (viz. snake-lion confrontations), and raised the issue of "whether they are ancient or not" (Muscarella 2000a, 171). In some instances an abeyant cautious view was recommended *ibid.*, 171, nos. 16, 18a–f. For the record, of the complex no. 16, I am now more at ease; I also believe in the antiquity of no. 18-e—primarily because of the execution of the guilloche/whorls; for no. 18-c, p. 488, the iconography is not an issue, and aside from the snake's ears and body line, cannot condemn it; pp. 169, 486, no. 7, still puzzles me but I cannot condemn it outright. *Nota bene*, that changing one's mind over time is a correct and necessary activity, it results from what I call the gift of the bazaar, the chaos caused by the antiquities market.

5. If evidence exists, as Benoit and Caubet claim in the Louvre and Lawler articles, that it was in a private European collection since 1968 (which I go on record as doubting: it was not mentioned in the Hotel Drouot sales catalogue), it should be presented to archaeologists (who do not work for the Louvre) for documentation examination.

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