

## REVIEWS

Zainab BAHRANI, *The Graven Image: Representation in Babylonia and Assyria*. Philadelphia: University of Pennsylvania Press, 2003. Pp. x + 242, 26 ills. ISBN 0-8122-3648-3.

Ancient Near Eastern studies originated as a byproduct of the colonial rule that European governments imposed upon the Middle East during the nineteenth and twentieth centuries, and interest in ancient monuments can already be traced in the writings of early European travelers who visited the Middle East. Monumental remains and uncanny inscriptions enhanced the curiosity to learn more about those peoples mentioned in the Bible and in Classical works. Scholars of ancient Mesopotamia are familiar with the romantic and adventurous origins of their discipline. Stories and anecdotes of early travelers and diggers usually appear in works dealing with the foundations of Assyriology.<sup>1</sup> The history of the field, however, is generally divorced from an evaluation of the influence that Western political, economic, intellectual, and religious history has had on the recovery of an ancient Mesopotamian ethos. Similarly, approaches to ancient artistic remains usually dissociate writing from visual representations. In *The Graven Image*, Zainab Bahrani studies the ways in which scholarly tradition rooted in Western intellectual practices has modeled a particular perception of Near Eastern art. She further provides an alternative way to understand ancient and modern representations of Assyrian and Babylonian art, and she challenges standard interpretations of art historians.

*The Graven Image* focuses on the concept of representation in the ancient Near East and on the Assyro-Babylonian practice of combining writing and visual representation for the production of images as a form of essential presence. The main thesis of the book maintains that the image takes the place of the real because the image carries an essential and conjuring presence. The study is also concerned with the practice of image making in academic discourse. This is an interdisciplinary approach encompassing theoretical knowledge from art history, anthropology, ethnography, historiography, semiology, cultural studies, post-structural literary theory, political practices, psychology, and philosophy. Bahrani incorporates Jacques Derrida's deconstruction and anti-Platonism, as well as the contributions of thinkers such as Edward Said, Timothy Mitchell, Michael Taussig, John and Jean Comaroff, Michel de Certeau, Hayden White, and Jacques Lacan, among others. Following the theoretical stance of post-processual archaeology, post-structuralist, deconstructivist, and postcolonial scholarship, the author asserts that ancient contexts do not simply emerge from the available data. Rather, they are forged by means of interpretative decisions and assumptions of modern scholars. Since the reconstruction of contexts makes culture an enunciative site, Bahrani regards the art historian as a translator of culture or ethnographer.

The introduction explains that the *Graven Image* was not written as a seamless narrative but as a series of essays confronting unexamined theoretical assumptions and proposing new readings of Assyro-Babylonian art. The first three chapters deal with the ways in which colonial and post-colonial discourses have influenced the study of ancient Near Eastern art, and have applied categories alien to Mesopotamian ontology. Chapters four through seven analyze specific examples of Assyro-Babylonian representation: cuneiform writing, the *salmu*, image abduction, and the altar of Tukulti-Ninurta. The first chapter refers to aesthetics, epistemics, and the notions of race, culture and antiquity, all of them related to the civilizing mission of imperialism. The discussion also includes Orientalism, Hellenism and barbarism, the idea of a "transcategorical primitive," and the natural history of art, all of which contributed to reinforce an alleged European superiority. The second chapter shows that Western characterizations of the Orient made of it an "extraterrestrial" entity, and a land governed by despots. Bahrani challenges the idea of Mesopotamia as a discursive formation by questioning the ontological concept of Mesopotamia. Chapter three examines two sets of oppositions:

the first is between the subject and object of study. The second is the division between the perceptual (sign) and the conceptual (symbol) representation, as equivalent to West/other. The analysis lays the theoretical bases to demonstrate that the divide between representation and the real does not apply to ancient Mesopotamia, which is the leitmotiv of the next four chapters.

The second part of the book aims to demonstrate that the Assyro-Babylonian tradition does not aspire to mimesis but “is conceived of as being part of the real” (p. 5). This section starts with a reexamination of certain interpretations concerning the cuneiform script to demonstrate that for the Assyrians and Babylonians the visual and the verbal were not clearly separated, but that they constituted one interdependent symbolic system. Chapter four characterizes cuneiform writing as a pluridimensional system narrowly connected with magic and divination. Bahrani defines cuneiform writing as an “image-text,” an interpretation based on the evolution from the Uruk IV pictograms to the later shape of the signs. Thus the author states that, although in later periods it is no longer possible to see the original pictogram, “the script always retained its pictographic origin within its logic as a system” (p. 106). Due to this characteristic, cuneiform had unlimited possibilities for signification because each sign could be used for “pictographic or phonetic value” (p. 114). Here the author seems to equate pictogram with logogram. This equation is difficult to sustain because a pictogram is a symbol intended to be a recognizable picture of what it means, while a logogram is a symbol representing a complete word. The inscriptions mentioned in the book, however, used a combination of syllabic and logographic writing. It seems highly unlikely to me that a first millennium scribe could easily have recognized the pictogram behind a given logogram, because by then cuneiform writing, already a couple of millennia old, bore little resemblance to hypothetically original pictograms. The characteristics of first millennium lexical lists and commentaries show the need to record (for pedagogic and utilitarian reasons) the shape, readings, and meanings of those signs used to write in Sumerian and Akkadian.<sup>2</sup> None of these lists includes, as far as I know, the evolution of signs as a modern scholar can trace it from looking at Labat’s handbook.<sup>3</sup>

Bahrani further illustrates the multiple and hermeneutic readings of cuneiform signs by referring to Bottéro’s study of the fifty names of the god Marduk listed in the last tablets of *Enūma elish*. This example also supports the idea that writing was considered an act of creation. It should be noted, however, that the section listing the fifty names of Marduk in the last part of the sixth and in the seventh tablet of *Enūma elish* is an unusual text, and it is hardly representative of the logic of other texts such as royal inscriptions or legal documents. As Michalowski suggested, the exaltation of Marduk in *Enūma Elish* belongs to what Machinist called *Kulturkampf*, that is, a political and literary contest between Assyrians and Babylonians in the Late Bronze Age.<sup>4</sup> The fifty names worked as a lock with a complex linguistic mechanism. The names were written in such a way so that the Assyrians could not appropriate *Enūma elish*. In principle it was impossible to unlock the names and change Marduk to the god Assur, a replacement that the Assyrians unsuccessfully tried. The potential hermeneutic reading of Marduk’s fifty names is not a suitable analogy to interpret the whole writing system because it is a late composition, and it does not account for a variety of texts beyond literature, magic, and divination.

The fifth chapter focuses on the concept of *šalmu*, an Akkadian word usually translated as “statue, relief, or monument.” Bahrani challenges the traditional rendering of the term, arguing that visual representation cannot be separated from the verbal system of the script. In that sense, then, Assyro-Babylonian manner of representation is better defined as a “pluridimensional chain of possible appearances,” similar to the writing system. *Šalmu*, therefore, should be considered “as a form of image that circulates within the real” (p. 127). The author proposes that rather than being an aesthetic concept, *šalmu* is an ontological category because through representation it substitutes for the real thing. This interpretation implies that the image of the king is not simply an image of the ruler, but that after the performance of rituals the statue becomes a substitute for the monarch himself. This conclusion is further

expanded in the next chapter, aimed at studying the abduction and mutilation of royal images in the Near East.

Chapter six deals with two sets of images that were assailed and robbed in antiquity. The first group comes from Nineveh and is now in the British Museum, and the second group comes from Elam and is in the Louvre. The analysis includes reliefs from the palace of Sennacherib, the Laws of Hammurabi, and the stele of Naram-Sin, among others. The author compares the abduction of royal statues with the capture of the statues of Marduk and his consort Šarpanitum by the Hittites in 1594 B.C., and by the Elamites in the twelfth century. Since the loss of the image of the god implied the loss of divine favor, and since the image of the king represented the king himself, the author concludes that the abduction of images was an act of political control. Thus, “having control of a person’s image was one more way of having control of that person” (p. 183). This interpretation, according to Bahrani, challenges the view of traditional scholarship that regards mutilation and looting of statues as barbaric acts undertaken for the material value of the object. Instead, she proposes that deportation of royal monuments should be considered as an act of magical and psychological warfare. This statement also questions the traditional interpretation of royal monuments as mere royal propaganda.

Although I agree with Bahrani that royal propaganda is not necessarily a manifestation of Oriental despotism, I would like to express my reserve with respect to her conclusions regarding image abduction. An important thing to consider is what happened to the presence embedded in the image once the king died. Did the statue become an empty shell? This question is relevant because some of the images that Elamite kings looted were of kings who had died many centuries before, as was the case with Naram-Sin and Hammurabi. Shutrak-Nahhunte’s scribes seem to have even mistaken the name of Hammurabi for that of Manishtushu, according to an Elamite inscription on Hammurabi’s stele. If indeed Shutrak-Nahhunte thought that by capturing images he was taking control of the king himself, then one would have to assume that the Elamite king did not know the monarch against whom he was fighting. The abduction of divine images is a different matter, because gods are immortal. But the stories about these statues are also problematic, for all the examples that have reached us are accounts from those kings who returned the statue and had texts written to praise their actions. As an example I shall mention the so-called “Marduk prophecy,” where the god himself predicts his own return to Babylonia thanks to Nebuchadnezzar I.<sup>5</sup>

The seventh chapter examines the altar of Tukulti-Ninurta, a monument well known to Near Eastern art historians. Bahrani offers a novel interpretation based on her theoretical premises. The author questions generic identifications of the altar as a political portrait of the king for public display, and her rereading of the piece emphasizes the integral visual and verbal character of the monument. Thus, the altar “is a representation about an episteme and ontology, about repetition and doubling, about representation itself” (p. 201). In chapter eight, Bahrani integrates her questionings of colonial discourse and traditional academic practices with her conclusions regarding Assyro-Babylonian ontology. She stresses the importance of being aware of the cultural project of imperialism in order to write a post-Orientalist history of Mesopotamia.

An evaluation of the influence that colonial and Western discourses have had on the writing of Mesopotamian art history is undoubtedly needed. Post-colonial scholars deal with discursive practices that shape otherness, the struggles between imperial powers and the oppressed, and resistance. By considering the conclusions of post-colonial and post-structuralist studies, the author has successfully questioned traditional assumptions of Near Eastern art historians. However, it seems to me that focusing almost exclusively on Assyro-Babylonian ontology when analyzing art monuments leaves aside questions of politics and power. For instance, the author seems to imply that the propagandistic effect of royal image was only tangential. That is the case when she affirms, “The system functioned through the ontological notion of the word-image-being entity. If this system served the ideology of

kingship, so much better, but it was not simple propagandistic assertion of absolute power” (p. 145). Even if we do not agree with the simplistic characterization of Oriental despotism, we should not forget that the surviving artistic works of the period originated in the ruling class: Assyrian and Babylonian kings were indeed the masters of people and land in the first millennium. Zainab Bahrani’s book is a thought-provoking work that undermines antiquarian approaches to ancient Near Eastern art and breaks the closed circle of the specialist by means of an interdisciplinary perspective.

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<sup>1</sup> See, for instance, Mogens T. Larsen, *The Conquest of Assyria. Excavations in an Antique Land 1840-1860* (London: Routledge, 1996).

<sup>2</sup> For the demise of cuneiform script see S. Huston, J. Baines, and J. Cooper, “Last Writing in Egypt, Mesopotamia, and Mesoamerica.” *Comparative Studies in Society and History* 45, no. 3 (2003): 430-479.

<sup>3</sup> R. Labat and F. Malbran-Labat, *Manuel d’Épigraphie Akkadienne (Signes, Syllabaire, Idéogrammes)*, 6th ed. (Paris: Librairie Orientaliste Paul Geuthner, 1995).

<sup>4</sup> P. Michalowski, “Presence at Creation,” in *Lingering over Words. Studies in Ancient Near Eastern Literature in Honor of William Moran*, eds. T. Abusch, et al. (Atlanta, Georgia: Scholars Press, 1990), 381-396. See also P. Machinist, “Literature as Politics: the Tukulti-Ninurta Epic and the Bible.” *The Catholic Biblical Quarterly* 35 (1976): 455-482.

<sup>5</sup> For the “Marduk Prophecy,” see B. Foster, *Before the Museums. An Anthology of Akkadian Literature*, vol. 1 (Bethesda: CDL Press, 1996), 302-305.

A.C.V.M. BONGENAAR (editor), *Interdependency of Institutions and Private Entrepreneurs. Proceedings of the 2nd MOS Symposium, Leiden, December 11-12, 1998*. (PIHANS, 87; MOS Studies, 2). Leiden: Nederlands Historisch-Archaeologisch Instituut te Istanbul, 2000. vii, 284 p. EUR 57.—. ISBN 90-6258-088-2

The book contains the proceedings of the second international symposium of the research project “The Economy of Ancient Mesopotamia.” In this second symposium, the issue of the institutional production and management was addressed. As Renger points out (p. 153), Mesopotamian institutions managed their assets through a combination of two different systems: the self-sufficient *oikos* system and the agency of entrepreneurs (whether private or not). These two systems may co-exist in one institution. In the symposium, the position of the entrepreneur was studied. Apparently, the original goal of the symposium was to address only agricultural entrepreneurship (Van Driel p. 5), but this scheme could not be maintained if Ur III, Old Assyrian, Nuzi and Ugarit material were to be included.

The point of departure for the symposium is the situation in the Neo-Babylonian period, because both institutional and private archives from different cities have been recovered (p. 1). This is also the case for the Old Babylonian period, when the edicts and the Code of Hammurabi provide more general information about the entrepreneurs. However, the Neo-Babylonian texts are much more informative concerning institutional-human relations (van Driel, p. 7).

The purpose of the symposium was threefold (p. 1):

1. to verify the thesis of the relation between the private entrepreneur (an outsider leasing institutional property or an insider providing commodities such as labor force) and the institutions (palace or temple) in the Neo-Babylonian period and to study this relationship in detail,
2. to investigate the importance of this connection for earlier periods (evident for OB, but other periods as well),
3. and to answer the question whether there are indications in the written sources for a sector of the economy which had no connections with the institutions.

Unfortunately, the book contains no conclusion in which these purposes are resumed. Thus, this book will be used mainly for the information contained by individual articles. It could have offered more, since the system of entrepreneurship has been studied for the individual archives and periods but not yet from a diachronic perspective.

Not all of the articles actually address one of the three objectives formulated in the introduction. Thus, Charpin studies an aspect of the *mīšarum* edicts which is not related to the connection between the private entrepreneur and the institutions, namely the reflection of the periodicity of a *mīšarum* promulgation in the archives. Just as the private archives from Ugarit, the Nuzi archives do not allow a study of the entrepreneurial activities. Therefore, Jas and van Soldt examine the nature of the archives in order to determine who kept an archive in Nuzi and in Ugarit respectively. Finally, the aim of Jongman's article is to give some new perspectives on the economic history of Mesopotamia and he does not touch upon entrepreneurial activities.

The contribution of the much-regretted van Driel, "Institutional and Non-institutional Economy in Ancient Mesopotamia," gives a diachronic perspective and touches upon the third purpose of the symposium, the question of a sector of the economy not connected to the institutions.

Before addressing the central issue of his paper, the degree of institutionalization of Babylonian agriculture, van Driel emphasizes the bias of the documentary evidence. Since practically all Mesopotamian archives are connected to an institution, the size of the non-institutional segment of agriculture is very difficult to assess. An important non-institutionalized part of society was the pastoral world. These tribal groups could be integrated in the Mesopotamian social fabric by incorporating them in the military structures. Only the Ur III period provides enough data to quantify the institutional agriculture, and allows Van Driel to conclude that enough agricultural land is left for non-institutional management. The size of the personnel of the Neo-Babylonian temple of Sippar points to the same proportions. The scarce documentary evidence concerning Babylonian villages, the actual loci of Mesopotamian agriculture, shows that they could be managed by institutions, by high ranking persons and by the military as maintenance land. Villages operating on subsistence level are of course not archivally documented.

In the first Neo-Babylonian contribution, Joannès ("Relations entre intérêts privés et biens des sanctuaires à l'époque néo-babylonienne") tries to situate the prebendary system and the Fermiers Généraux in a larger social context. After an overview of the possible transfer of goods between temples and private individuals, he examines the background of some well-known prebendary holders and the Fermiers Généraux of the Eanna. Prebends generally remained in the family, but the opposite is documented as well. Most of the prebendary holders were members of the local nobility. The first Fermier Général was sent and supported by the king. The later ones belonged to the nobility of Uruk.

The other articles concerning Neo-Babylonian material all illustrate the entrepreneurial activities documented in a(n) (group of) archive(s). Thus, Beaulieu ("A Finger in Every Pie: The Institutional Connections of a Family of Entrepreneurs in Neo-Babylonian Larsa") discusses the activities described in a small archive from Larsa. The texts illustrate diverse activities in the agricultural and commercial spheres. Apparently, the family, which was not part of the hereditary urban elite, profited from the rebuilding of Larsa by Nebukadnesar II to expand its economic assets and to gain a foothold in the temple economy. Eventually, one of the family members marries into a family with patronymic.

Bongenaar (“Private Archives in Neo-Babylonian Sippar and their Institutional Connections”) gives an overview of the private archives from Sippar. However, these were recovered in the temple complex, and thus, the families are related to the temple administration. Temple officers and prebend holders kept the archives. The documents concerning their private activities such as crediting were kept in the temple as well.

Finally, Wunsch (“Neubabylonische Geschäftsleute und ihre Beziehungen zu Palast- und Tempelverwaltungen”) investigates the institutional relations of the Egibi family. She gives an overview of the references to institutions in the texts from the different generations. Contrary to the families discussed in Beaulieu’s and Bongenaar’s articles, the Egibi’s were not dependent on the institutions. They took care to maintain excellent relations with the royal officials both on a private (through leases and loans) and on an official level. These contacts were very important for the business of the family. The military and the palace were the best market for products acquired through *harrānu* businesses. They also seem to have taken over some tax-farming activities from the Esagila.

The chronological overview of the connection between institution and entrepreneur starts with the Ur III period. Neumann (“Staatliche Verwaltung und Privates Handwerk in der Ur III Zeit: Die Auftragsstätigkeit der Schmiede von Girsu”) argues that, in spite of the impression one gets from the Ur III documentary evidence, craftsmen operated on an independent basis. Since the Ur III texts originated from the institutional bureaucracy, we are informed only rarely about their non-institutional orders. However, the state was by far the most important commissioner of the craftsmen.

Dercksen (“Institutional and Private in the Old Assyrian Period”) tries to locate the institutional connections of the Old Assyrian entrepreneurs. Since they were discovered in commercial outposts, the texts from this period describe the commercial activities from the point of view of the entrepreneurs, and not that of the institutions. The king of Assur appeared as an entrepreneur between the others. The city of Assur provided the conditions for the entrepreneurial activities (it regulated, monopolized and established the political network), but it did not seem to play an active role in the trade. The Assyrian traders functioned as intermediaries between the different Anatolian palaces.

Since the symposium departs from the Neo-Babylonian period, Renger’s contribution (“Das Palastgeschäft in der altbabylonische Zeit”) about Old Babylonian Palastgeschäfte is included in the chronological overview. However, it deserves a more prominent place since it examines the nature of the relations between the entrepreneur and the Old Babylonian palace. As Bongenaar observes (p. 3), the Neo-Babylonian entrepreneurship was organized along the same principles, only the temples seem to have taken over a large part of the role of the Old Babylonian palace. Renger reviews all of the Old Babylonian evidence concerning the theme and places it in its context. He arranges the evidence (institutional archives, letters, administrative documents, paragraphs of the edicts and of the Codex Hammurabi) thematically (relating to agrarian production and herding, to the management of natural resources and to the service sector). Thus, the article provides us with an excellent framework in which to situate the Palastgeschäfte.

The private entrepreneurs studied in Charpin’s contribution (“Les prêteurs et le palais: Les édits de *mīšarum* des rois de Babylone et leurs traces dans les archives privées”) are creditors issuing loans to private individuals. Since the loans could be subjected to a *mīšarum* edict, these creditors depended on the palace institution. Charpin illustrates how the promulgation of a *mīšarum* was preceded by an increase in the number of loan contracts found in the archives of the creditor. The creditor apparently did not throw away the canceled contracts, but kept them in a separate file.

The archives from Nuzi are not informative about entrepreneurial businesses. On the basis of scarce evidence, it can be concluded that the merchants were employed both by the palace and by private entrepreneurs. Jas (“Old and New Archives from Nuzi”) reconstructs some unstudied archives and situates them in a city quarter where military officers (several of them from Hanigalbat) lived.

Only a few private houses in Ugarit contained documentary evidence. In his contribution (“Private Archives at Ugarit”), van Soldt tries to discover the *raison d’être* of these archives. All of the administrative texts and letters can be related to the palace administration. Some juridical texts can be placed in a private context. The lexical and cultic texts allow us to determine whether we are dealing with a (teaching) scribe or a priest. All of the archive holders in Ugarit occupied a high position in the palace bureaucracy. Since some of the archive holders were definitely priests, the cultic organization(s) in Ugarit seem(s) to have been dependent on the palace as well.

As far as the textual evidence allows any conclusions, there was not much room for entrepreneurial activities in New Kingdom Egypt. High officials nominally supervised temples and managed royal lands, but agents, deputies or scribes represented them. Haring (“Outsiders in Charge of Institutional Property in New Kingdom Egypt”) thus situates the entrepreneurship on an official level.

The contribution of Jongman (“Hunger and Power: Theories, Models and Methods in Roman Economic History”) is not directly connected to entrepreneurship. It is included in the volume because the recent developments in the study of Roman economy provide new perspectives on the economic history of Mesopotamia. A correct use of economic theory, a thorough study of factors like demography, food supply, agricultural and transportation technology and the use of parametric data such as model life tables, may rejuvenate the debate concerning ancient economy.

Resuming the purpose of the symposium, we can state that entrepreneurship is a widespread phenomenon in Neo-Babylonian archives. Families owning an archive were tied to institutions through offices, prebends, because they managed institutional land or bought tax-farming rights. The management of the institutions depended for a large part on these entrepreneurs. Already in the documentary material from Ur III institutions, entrepreneurial activities can be detected. The system is well documented in the Old Babylonian period—though not as prevalent as during the Neo-Babylonian period. The documentary evidence from other periods (Old Assyrian and Middle Babylonian) does not give specific information on the nature of the relation between entrepreneurs and institutions, but archive holders from those periods often were involved in business relations with institutions. According to Van Driel, the pastoral world was the only sector of the economy which had no connections with the institutions mentioned in the written sources. Some villages may have operated independently on subsistence level but there are no traces in the documents of those segments of the economy.

The book contains some important contributions for students of Babylonian society, in particular for the Neo-Babylonianists. A concluding summary would have offered the opportunity to compare the situation of the entrepreneur in the different periods, especially between the Old and the Neo-Babylonian period.

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Horst KLENGEL with the assistance of Fiorella IMPARATI, Volkert HASS and Theo P.J. VAN DEN HOUT, *Geschichte des hethitischen Reiches*. Handbuch der Orientalistik 1/34, Leiden: Brill, 1999. xxvi + 430 + 63 illustrations.

The book under review here is written by one of the leading historians of the Hittite kingdom, and its neighbors, Horst Klengel. The introduction has sections discussing the geography where the history played out, the story of the rediscovery of the Hittites, and notes on the further development of Hittitology. The next five sections proceed chronologically. First there is a short discussion of Anatolia in the early Bronze Age. This is followed by a chapter

on Anatolia in the age of the Assyrian merchant colonies, during which period Hittites are first mentioned in written sources. Sections II, III and IV proceed chronologically with each king given a separate chapter. Unlike its English language rival, Trevor Bryce's *The Kingdom of the Hittites*, Klengel's work is not straightforward narrative history. Rather the section on each king begins with an introductory paragraph. Then every text datable to that king's reign is listed and given a number [A1], [A2], etc. This is followed by a list of every text from later reigns that refer back to this king's reign. These are given numbers [B1], [B2], etc. The entry for each text gives its text number, the name the text is known by today ("Annals of Muršili II") or its type ("fragment of a prayer"), its number in Laroche's catalogue, a sentence or two describing what of its content is relevant to the reign in question and where the text has been edited or translated. After this there is a narrative of the reign of the king. In the case of Muršili II, about whom the most is known, and for whom events can be easily placed chronologically, the chapter is divided into theaters of operations: north and northeast, west, and southeast, which are then discussed chronologically. The final section of political history deals with the fall of the Hittite empire and the general crisis at the end of the Bronze Age. It is followed by a section discussing the two portions of the empire that survived or may have survived the general catastrophe, Kargamiš and Tarḫuntaša.

There are in addition copious footnotes, many of which contain important details. Some are sufficiently important that one wonders why they are not up in the text, e.g. p. 22 n. 18 on the geographical suitability of Kaniš as a node point for the Old Assyrian trade, n. 19 on the archaeological chronology of Kültepe and p. 23 n. 28 that Piḫana, previously only known from Anitta's inscription is now attested in contemporary Old Assyrian texts.

This political history written by Klengel is followed by a section of seven chapters written by Imparati on the Organization of the Hittite State. Chapters discuss the texts, the royal family, the exercise of power, the administration of the kingdom, die kollegialen Gremien ("the collegial boards?"), the organization of work, the government of the empire and international relations.

Toward the end of the book are charts of synchronisms, a detailed bibliography divided up into twelve parts by subject covered, indices of personal names, place names and texts (according to Laroche catalogue numbers). Rounding out the book are 63 illustrations, including plans of Hittite cities and buildings so far excavated, photographs of Hittite tablets, drawings of Hittite seals, drawings and photographs of the Hittite kings who portrayed themselves on rock reliefs, and finally excerpts from Egyptian reliefs showing Hittites.

As we have come to expect from Klengel, the book relies on facts and sound historical judgments and eschews trendy theory. Low probability suggestions and out and out errors are remarkably few.

p. 17, 3 lines from bottom, read STT 51, 78 + 166 (Sultantepe).

p. 18. The Old Assyrian tablet concerning the deeds of Sargon of Akkad has since been published in photo, copy, transliteration, Turkish translation and English summary by GÄnbatt in *Archivum Anatolicum* 3 (1997) [GsBilgic] 131-155.

pp. 33, 35, 43. The long established view that king Ḫattušili I moved the Hittite capital to newly refounded Ḫattuša will be challenged by R. Beal's contribution to the Festschrift for Harry A. Hoffner, Jr. Klengel's understanding (p. 37) that Labarna I was Ḫattušilli's aunt's husband and heir of Ḫattušili's grandfather by adoption is an excellent one.

p. 40 § [A 9]. This important text, which should be known as "Anecdotes" since it is not a "Chronicle," is edited with German translation by O. Soysal, in his dissertation *Muršili I Eine historische Studie* (Würzburg, 1989) and more recently with Italian translation by Paola Dardano, *L'aneddoto e il racconto in età antico-hittita* (Rome, 1997).

p. 53. I wonder why the Siege of Uršu text is declared a legend "und wohl nicht historische Ereignisse reflektiert." The text is certainly not "history" but appears rather to be "satire" and although differing in style it appears to fit in with the moralizing on the failings of royal officials seen in writings from the time of Ḫattušili I and Muršili I such as the "anecdotes." As the latter seem to have historical bases, so probably the Uršu siege.



p. 60 n. 123, read *AoF* 18 (1991).

p. 63. I do not see why “VS NF XII 2” (a.k.a. VS 28.2) is mentioned under Muršili I, since in this text “Muršili” occurs in a position after “Šuppiluliuma” it seems clear that Muršili II is meant.

pp. 64-65. Although Ḫantili I apparently tried to paint Muršili’s I raid on Babylon as sacrilegious to justify his own usurpation, it seems unlikely that it is due to this judgment that he is omitted from later genealogies. The fact that Telipinu paints Muršili as one of the good kings the sack of Babylon as a Hittite success, and the fact that there are later kings named Muršili points to the fact that Muršili was not seen by later generations as thoroughly disreputable. It seems most likely that he was not mentioned in later genealogies since he was in fact not a direct ancestor of any kings, as he was murdered before he managed to reproduce (Telipinu doesn’t mention the murder of any of Muršili’s children, unlike in the cases of Ḫantili I’s and Ammuna’s children).

p. 72, last paragraph, for Taurus read Amanus, since according to Telipinu Adana (on the far side of the Taurus, but near side of the Amanus) was lost by Zidanta I’s successor Ammuna.

p. 76 [B3]. Ḫuzziya in company with Ḫattušili, Labarna and Pimpirit is more likely to be Ḫattušili’s son Ḫuzziya of Ḫakpiš or even Ḫuzziya 0, distant ancestor of Ḫattušili I and dynastic founder, rather than king Ḫuzziya I. Similarly the Ḫuzziya of [B4] in the company of Papaḫdilmah probably dates to the time of Ḫattušili I.

p. 76. For a suggestion that Zuru the GAL *MEŠEDI* was Ammuna’s brother see Beal, *Theth* 20:329 w. n. 1257.

p. 80. I do not think that the plot that resulted in the murder of deposed king Ḫuzziya I and his brothers is properly understood. Those who instigated the plot are called “great ones” (*meggaeš*) by Telipinu, but saying they were “die höchsten” is overdoing it. They are UGULAs “supervisors” not GALs “chiefs” of departments. Even if we understand the military officer as a “overseer of 1000” this is still colonel level, subordinate to the GAL GĒSTIN and GAL *MEŠEDI*. What appears to have happened is that on Ammuna’s death his brother Zuru and Zuru’s sons Tanuwa, Taruḫšu and Taḫurwaili removed Ammuna’s legitimate sons and installed the illegitimate Ḫuzziya as their puppet. Telipinu, husband of a legitimate daughter, overthrew Ḫuzziya and took over and banished Ḫuzziya, his brothers and his supporters. Later while Telipinu was on a distant campaign with his way home conveniently blocked by a revolt in Lawazantiya, a group of magnates back home plotted a palace coup to depose Telipinu, should he manage to get home, and to replace him, not with the weakling Ḫuzziya, but with one of Zuru’s sons (who must first eliminate Ḫuzziya and his brothers). It should be noted that it is Tanuwa, Taruḫšu and Taḫurwaili who are singled out for particular opprobrium, so they were probably to be the beneficiaries of the plot. And, of course, eventually Zuru’s son Taḫurwaili did manage to seize the throne, temporarily interrupting the reigns of Telipinu’s descendants. (See p. 88.)

p. 95. For a suggestion that Zidanta II was the son of Ḫaššuilu, the GAL *MEŠEDI* and brother of his predecessor Ḫantili II see Beal, *Theth* 20:330.

p. 102f. For a suggestion that Kantuzili was son of Ḫuzziya II and father of Tudḫaliya II and perhaps king after the overthrow of the usurper Muwattalli I see Beal, *FsHoffner* (forthcoming).

pp. 103ff. Klengel, correctly in my opinion, posits only two kings named Tudḫaliya between Telipinu and Šuppiluliuma I. These have since virtually the beginning of Hittitology been known as Tudḫaliya II and III. When Tudḫaliya II was split into two kings called Tudḫaliya I and II and then subsequently reintegrated with himself, unfortunately it became the fashion, followed by Klengel, to call the king Tudḫaliya I. This means that his grandson who was previously always known as Tudḫaliya III is now called Tudḫaliya II (III) on p. 127. The next Tudḫaliya to certainly become king remains with his traditional number Tudḫaliya IV. Tudḫaliya the younger, who probably never was king, may or may not be

called Tudḫaliya III (p. 148 n. 27). Thus we have massive confusion. Originally the number I was given to a pre-Ḫattušili I prince named Tudḫaliya. While it is true that this ancestor may or may not have been king (see my discussion in FsHoffner [forthcoming]), it is better to reserve the number I for this obscure figure as 3/4 of a century of scholarship has done and return to the traditional number of all the later Tudḫaliyas, getting rid of parenthetical numbers and restoring missing numbers in what is a relatively clear sequence. I will continue to use the traditional numbering in this review and elsewhere.

p. 109f. Klengel is surely correct to point out that Tudḫaliya II did not found a new dynasty.

p. 113 l. 19 “Überführung” and p. 174 [A21] “Überstellung” and “Übersiedlung.” I once based a small part of an argument about political control on the evidence of the “transfer” of the Goddess of the Night from Kizzuwatna to Šamuḫa, an argument I’m happy to see Klengel follows. However, more recently a closer look at the verb *šarra-* which does not otherwise mean “transfer” has convinced me that the goddess’s divinity was “divided” (the usual meaning of the verb) so that she would be resident in both Kizzuwatna and Šamuḫa. (See R. Beal, in *Magic and Ritual in the Ancient World*, eds. Paul Mirecki and Marvin Meyer [Brill, 2002] 197-208.) The political implications are unchanged.

p. 116 [A1]. Kuwattalla is not a “Hierodule” (“a temple slave, especially the temple courtesans at Corinth and elsewhere” Liddell and Scott 821, *OED* s.v.). I thought that this traditional, but baseless, translation of SUḪUR.LAL had finally been discarded. There are no sacred prostitutes in Ḫatti (or Babylonia for that matter), and slaves of gods are not called SUŠUR.LALs. The original Sumerian meaning of the term appears to have been hairdresser. As Güterbock and others have pointed out (*JAOS* 103 [1983] 159 [“lady’s servant, attendant woman”], Beckman, *BiOr* 40 [1983] 113 [“maid”], Neu and Rüster, *HZL* [1989] no. 349 [“(Kammer-)Zofe, Dienerin”]) perhaps the best translation for the term at Ḫattuša is “lady’s maid.”

pp. 125-126. Concerning Ḫattušili II, the offering texts VS 28.2 i 10-13, KBo 39.86 ii 11-13, KBo 39.88 ii 7-9, KBo 39.89 iv 6-8, and KBo 39.91 ii 1-3 should have been brought up. In VS 28.2 offerings are made to the statues of Ḫattušili, then Tudḫaliya, then Šuppiluliuma, then Muršili. While these could be offerings to Ḫattušili’s greatest kings Ḫattušili I, Tudḫaliya II, Šuppiluliuma I and Muršili II, they could just as well be offerings to Muwattalli II’s immediate predecessors, Ḫattušili II, Tudḫaliya III, Šuppiluliuma I and Muršili II (with the ephemeral Arnuwanda II skipped). In favor of the latter, note van den Hout’s comment p. 252 n. 486.

p. 148. It has always bothered me that when Muršili mentions the incident of the murder of the heir Tudḫaliya the younger to the benefit of Muršili’s father Šuppiluliuma in the context of the gods being angry at him for his father’s crimes that he does not say “My father killed his own brother.”

p. 158. Press reports indicate that newly found letters from the chancellery at Qatna will further illuminate Šuppiluliuma’s conquests of Mittanni and Syria.

p. 163 n. 103. I do not see how Liverani can claim that the name <sup>m</sup>Zannanza, borne by Šuppiluliuma’s son sent to become king of Egypt, was an Egyptian epithet for “prince” and not a Hittite name. While a Hittite might mistake an Egyptian title of an Egyptian for that person’s personal name, it is hard to see how Muršili (the author of the text) would not have known his own brother’s name from an Egyptian title.

p. 180. It is good to see emphasized Muršili II’s constant need to keep the lid on the close-in Kaška, before he could lead campaigns further afield, against places that we (and probably the Hittites) consider more interesting.

p. 193 l. 15, for “Onkel” read “Vetter.” Ḫudupianza is the son of Ziti the GAL *MEŠEDI*, Šuppiluliuma I’s brother, and Muršili II’s uncle and thus Ḫudupianza is Muršili II’s cousin.

p. 208 l. 26, for GAL *GESTIN* read GAL *MEŠEDI*. This is correct on p. 255.

p. 211 l. 24, for SU-an read ŠU-an.

p. 240 n. 451. It is distressing to see from this footnote total agreement on the location of Ura at the mouth of the Kalycadnus (Gök Irmak), agreement created by ignoring my article arguing against this location and in favor of Kelenderis (Gilindere), *AnSt* 42 (1992) 65-73.

p. 249 [A36], to the text KUB 21.20 (+) KBo 16.36 add + HHT 82 + KUB 48.87, ed. Alp, *Belleten* 41/164 (1977) 644-646 repeated in *HBM* 32-35.

p. 274 n. 561, add here R. Beal, *THeth* 20 (1992) 387 n. 1466.

p. 286. For a newer suggestion concerning what had been know as “Thronnamen” and “Prinzenname” see Beal, *FsImparati* (Eothen 11) 55-71.

p. 294 n. 633 l. 9, for StBoT 18 read StBoT 38.

p. 300. Merneptah??? why not Siptah/Tawasret?

p. 314. “Der Weg zu Mittelmeer via Tarḫuntašša war(en) offenbar für das Überleben Ḫatti immer wichtiger geworden,” implies that unrest in Tarḫuntašša would cut off grain shipments from Syria to relieve famine in Ḫatti. This ignores the fact that the best road from the Mediterranean coast to the Anatolia plateau is the Cilician Gates which runs through Kizzuwatna not Tarḫuntašša.

p. 314. Modern historical periodization here forces disparate events into a straightjacket. Although Greece, Anatolia, Syria and Palestine all suffer destruction of civilization and Egypt goes into steep decline after about 1180 B.C., the scholarly export of a simultaneous “End of the Bronze Age” to Mesopotamia does not work. The Kassite Dynasty lasts another 25 years and its end comes not from the west but from the Elamites to the east. It is replaced by the 2nd Isin dynasty, including the powerful Nebuchadrezzar I (1125-1104), a dynasty unaccountably ignored in *GHR*. It is not until 1026 that this dynasty dissolves into the obscurity of the 2nd Sealands Dynasty. In Assyria, too, this periodization gets in the way of understanding. It is true that the assassination of Tukulti-Ninurta I (some twenty to thirty years before the fall of Ḫatti!) ends a period of Assyrian expansionism, and Assyria’s brief rule of Babylonia is ended, but there was no “Machtrückgang” due to massive Aramean invasions for another hundred years, until the Middle Assyrian kingdom’s arms had reached their farthest extent yet and then suddenly collapsed due to over expansion and Aramean incursions. It is quite clear from excavations at Dur-Katlimmu that Assyrian rule over Ḫanigalbat (the former Mittanni) continued for another century after Tukulti-Ninurta’s death.

p. 323. *Imparati* follows the excellent suggestion of Beckman that the title “My Sunî for the king is more likely to have come from North Syria than Egypt. However, *Imparati*’s further suggestion that the link was with the Sungoddess of Arinna, wife of the Stormgod, chief deities of the pantheon, seems unlikely. It was the Hittite queen that was linked to the Sungoddess according to the iconography: at Fraktin, Queen Puduḫepa is shown dressed the same as the Sungoddess. However, as is well known, at Yazılıkaya the Sungod, dresses differently from all of the other gods and wears the same style clothing as the Hittite king, portrayed there and elsewhere, often does. This fits perfectly with Mesopotamian concepts where the Sungod is also god of justice and so also of kingship (but is not the chief god of the country).

p. 335 w. n. 55. It is disheartening to see an old translation ( $LÚ\dot{I}\dot{S}/KU\dot{S}_7$  = “Wagenlenker/Knappe”) that one has spent considerable effort to correct (actually  $LÚ\dot{S}\dot{U}\dot{S}/KU\dot{S}_7$  = “chariot fighter”) not only continue to be used unchanged but to see oneself cited favoring this old translation.

p. 337. That following Starke “Wie zuvor die Großen, hätten sie (the  $LÚ.ME\dot{S}SAG$ ) nun mehr eine politische Kraft dargestellt, die gemeinsam mit dem König regierte” seems off base. The Hittite king appears to have been the active and unchallenged ruler of the kingdom, obviously needing the help of grandees to rule, but Hatti certainly does not appear to be a limited monarchy. The opinion of many that the  $LÚ.ME\dot{S}SAG$  were eunuchs should have been taken into consideration.

p. 342 n. 83. How do we know that Šaḫurunuwa bore the three titles he is given in the edict dividing his estate at the same time. In my study of military titles, I suggested they were borne consecutively (*THeth* 20.383-385, 387 n. 1466).

p. 344. It is hard to believe that with communications what they were that “Dennoch kontrollierte und bestimmte die Zentralverwaltung auch die administrative Strukturen von Dorfgemeinden und ließ ihnen nur wenig Autonomie.” It is certainly clear that the defenses and water supplies of small walled towns were strictly and minutely watched over by the governor during his periodic visits, and that the government clearly took an interest in getting vacant land back into taxpaying production. However, there are no instructions concerning villages, nor are there building codes for houses. Statements such as “wherever they execute let them execute him, wherever they exile, let them exile him” argue for differences of law and lack of the centralizing standardization that only really becomes government theory with the European “enlightened” despots and subsequent revolutionaries of the 18th and following centuries A.D. It seems clear from the instructions for the governor that even in provincial towns legal cases would be expected to be settled by the local elders (who *Imparati* discusses earlier) and notables and only a difficult case would be passed on to governor or to the king. He is not instructed to examine settled cases to see that the settlements correspond to some central government policy.

p. 344. “Eine ganze Reihe von Dokumenten reflektieren eine königliche Politik, die auf eine gleichmäßige Verteilung des Grundbesitzes abzielte.” I don’t know what those documents are. Sure, land was granted, land was confiscated from rebels or fell to the crown in default of direct heirs, and regranted, and the king might confirm a complicated division of an estate among heirs, but I do not see anything that aims at an equal division of land.

pp. 349f. *Imparati* follows Diakonoff in seeing beside temple and palace agriculture, “community agriculture” (Gemeinde-Wirtschaft). I would agree it was the case that much of Ḫatti was farmed outside the temple and palace sectors at a more or less subsistence level. It is also true that there was a certain amount of village corporate behavior (village elders [§ 71], the men of the village shall [temporarily] work the empty land [§ 40], collective village responsibility if no individual could be penalized for a murder [§ IV]). However, the texts do not refer to a village short a person, but a plot of land that is empty. They envision land sales (§§ 39, 169), an individual taking the initiative in farming an empty field (§ 40, 41), division of land as part of marriage settlements (§ 46), individual ownership of animals (§ 71, XXXV), wage labor at harvest time (§ 158). It thus appears that most land was held as private property and that the villages had leadership, who could act for the village vis-à-vis the central government if necessary. There is no evidence here for primitive Communism. It is clearly Orientalist slander (in the sense of the word invented by Edward Said) to state that “Die Nahrungsproduzenten waren gehalten, einen großen Teil ihrer Erzeugnisse der zentralen Autorität zu überliefern.” The statement would probably hold if the word “Erzeugnisse” (“yields”) were changed to “Überschüsse” (“surpluses”) or the word “großen” were removed. The Neo-Assyrian government took 10% of the harvest (N. Postgate, *Taxation & Conscriptio* 176) (which is considerably less than the US government takes from my income). The Islamic land tax (*kharaj*) was according to the jurists also 10%, but the Imam could raise or lower this percentage based on the quality of the individual cultivator’s land and the individual’s ability to pay (A. Lambton, *Encyclopaedia of Islam*<sup>2</sup> 4 [1978] 1037). There is no evidence for the Hittite rate, but it was probably not that different.

p. 351 w. n. 115. *Imparati* is to be commended for recognizing (unfortunately only in a footnote) that in addition to the palace and temple sectors of the artisanal economy there was a private sector that is largely absent from our palace and temple centered documents, few of which in any case are administrative.

p. 353. “Wie in den anderen Königreichen des alten Nahen Ostens war auch in Ḫatti der Arzt dem Palastbereich verbunden und widmete seine Bemühungen vor allem der Elite” makes no sense and cannot be supported by any facts. It is quite clear that the *ašipu*, who was both the doctor and the exorcist of Mesopotamia (see J. Scurlock, in *Mesopotamian Magic*, eds. K. van der Toorn and T. Abusch [Styx, 1999] 69-79) was attached to temples and was thus in a position to provide services for free (except for drugs and equipment) even to the lower echelons of society (note comments “if he is poor, substitute the following”).

Nor is there any evidence that all pharmacists (*asû*) worked for palaces. Kings may have sent their favorite practitioners to other kings, but since there are no private records for anything in Hatti it is pure cynical speculation that there was no medicine outside the palace.

p. 357. Concerning trade, the fact noted by Imparati that we have almost no private records of trade or anything else for that matter should not lead to her conclusion that trade was dependent on the palace and merchants were royal functionaries, and that private trade was but a trifling. There are, after all virtually no documents concerning trade from the palace, which we at least know was producing documents on other subjects. That the king of Ugarit calls the merchants of Ura “merchants of Your Majesty” may mean they worked for the Hittite king (see also pp. 261f. w. n. 508), but it seems more likely that the term simply means “merchants (who are subjects) of Your Majesty.” The whole problem between the merchants of Ura and the king of Ugarit concerns the merchants buying up or foreclosing on property in Ugarit. This sounds like something merchants buying and selling to make a profit would do with their profits and not something government buying agents would do. In any case, Ugarit already belonged to the Hittite king as overlord, so why would his government buying agents be buying up its real estate? If the Hittite king had wanted to build something for himself in Ugarit, eminent domain would have been so much easier. Beside the king of Ugarit does not complain to the Hittite king that the Hittite king is buying up all of the land, but that the merchants are.

p. 365. What, I wonder, leads Imparati to say that the tributary king could possess no fortified cities? One of his main jobs was to repel his and thus the Hittites’ enemies. How ever was he to do that without fortified cities on his borders?

This is an important book that generally provides a thorough and well founded survey of Hittite history and society.

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Naguib KANAWATI, *Conspiracies in the Egyptian Palace. Unis to Pepy I*. London: Routledge, 2003. IX, 208 pp. £55.00 (cloth).

Was king Teti really assassinated? And did his confidants and dignitaries take part in this plot? Professor Naguib Kanawati chose for his latest book an extremely interesting and thought-provoking subject from the history of the late Old Kingdom and tried to find the answer to a question that is crucial for our understanding of the culture and history of the ancient Egyptians—were they able to murder their king, the only living god on earth? Professor Kanawati is mainly connected with the excavations, clearance works, and publications of the tombs located in the Teti Pyramid Cemetery at Saqqara. The archaeological material originating from this restricted necropolis forms the basis for the book under review.

The author conceived the whole book as a sort of “historical investigation.” The core of the book concerns the suspicious circumstances surrounding the death of the founder of Dynasty 6, King Teti, who according to Manetho was murdered by his own bodyguards. With this in mind, Kanawati named the three main chapters of the book “Assassination claim,” “The Suspects: Case Studies,” and “The Investigation.” An introductory chapter, conclusions, notes and a detailed bibliography complement the main part of the study.

The author pays particular attention to the archaeological and historic evidence of the three “palace conspiracies” that occurred during the reigns of Teti (his assassination) and his son Pepy I (the queen trial and the plot of vizier Rawer). His examination is based on material from the period between the reigns of Unas and Pepy I. He analyzes the reasons behind Teti’s enthronement, the course of his reign, and the occasion of his death. In his overview

of the reign of Pepy I, Kanawati focuses mainly on the punishments handed out to the conspirators.

In the introduction, the author claims that it was not his intention to write a history of Dynasty 6, but rather to focus on some specific events that occurred within this unstable period. He then briefly describes the history of the Old Kingdom in general, with special attention to the royal succession. However, his emphasis on the secrecy of the royal palace and the description of the royal court as a “fertile land for intrigues” is somewhat exaggerated—perhaps done to suit the taste of the general public. Although the genealogical connections among the Old Kingdom kings are sometimes not easily determined, according to the preserved written evidence, the mother of the king Snofru was queen Nimaathap and not Meresankh, the queen Khentkaus I from Giza was rather the daughter of Menkaure and not of Djedefhor, and king Neferirkare was the husband (and not the son) of queen Khentkaus II from Abusir, who bore him two sons—the succeeding kings Neferefre and Niuserre.

The first chapter deals in detail with two subjects—the authenticity of Manetho’s statement about the assassination of Teti by his bodyguards, and the related problem of defining those “bodyguards” in the preserved written material from the Old Kingdom. The author argues that the Egyptian equivalent of the term “guard” is the title *hntjw-s*, predominantly translated as “(palace) attendant” and he describes this term in detail. The holders of this title were qualified by the reference to a palace (*pr-ʿ3*) or to a mortuary temple of a king. There is no doubt that the bearers of this title performed personal services for the king in the palace for they hold other titles referring to feeding, bathing and clothing the king. This personal aspect of the title contrasts with the tasks they performed in the royal mortuary cult, such as transporting food, and dressing and feeding the cult images of the deceased king. At the dawn of Dynasty 6, the number of “guards” rapidly increased and the nature of this title underwent distinct changes. During the previous dynasty, the tomb owners formed a rather independent social class of middle rank. In Dynasty 6 this title occurs among titles held by officials with two different positions within the social structure of the Egyptian society. For one, it was incorporated into the extensive title strings of the most powerful dignitaries at the court, perhaps marking the initial stage of their careers. On the other hand, among the titleholders were also still middle class men, sometimes with no other responsibilities. It remains hard to say to which extent these officials guarded the king and his security.

The second chapter represents the most important part of the book. Here the author deals with the available archaeological evidence that originates mainly from two parts of the Saqqara necropolis—the cemeteries lying to the north of the Unas causeway and his mortuary temple and those to the north of Teti’s pyramid. Altogether, evidence of 47 tombs, whose owners bear the title “guard” and which date to the period between the reigns of Unas and Pepy I, is taken into account. The description of each tomb is structured following a similar pattern. First, Kanawati gives a brief overview of the career of the tomb owner and his titles, then he discusses the architecture and position of the tomb within the cemetery, and finally he suggests a date for the construction of the tomb. The dating is of special importance because it sometimes differs from the dates indicated in the archaeological publications of the separate tombs. This is not without importance when considering the time/spatial development of the cemetery as a whole. Each entry ends with an overview of later treatments done to the tomb architecture, and the epigraphic and decorative program.

Although the author gathered a great amount of reliable information, there seem to be some omissions. Not all recorded tomb owners bear the title “guard.” For at least eighteen of them the title is not recorded on the preserved epigraphic material—Akhethetep/Hemi (No. 1), Ihy (No. 3), Geref (No. 13), Hefi (No. 14), Inumin (No. 17), Iries (No. 20), Kaaper (No. 22), Kagemni (No. 23), Memi (No. 27), Mereruka (No. 30), Nedjetempet (No. 32), Nikausisi (No. 35), Rawer (No. 36), Shepsipuptah (No. 38), Tetiankh and Hesy (No. 40), Tjetji (No. 42), name lost (No. 45), and Sabu (No. 47).

There is no doubt that further archaeological excavations will reveal more material about this unstable and lesser known period of Egyptian history. Tombs dated to this period were

also discovered in other parts of the Memphite necropolis, e.g. by the team of the Czech Institute of Egyptology in the area of Abusir South—the tombs of the vizier Qar and his family members.

One of the advantages of this book lies in the possibility to read chapters 2 and 3 independently from one another. The last chapter was written as a self-contained part of the book for those readers who are only interested in the historic analysis, free from the archaeological descriptions contained in chapter 2.

The investigation develops chronologically and the chapter is further subdivided into smaller entities dealing with different aspects of the period. A study of the development of the Teti Pyramid Cemetery, with special attention to the decision of Teti to allocate this specific place at Saqqara for the cemetery, opens the chapter. Based on an analysis of the titles of the dignitaries and the archaeological evidence, the author reconstructs the events that may have led to the enthronement and death of Teti, the turmoil concerning the ephemeral reign of Userkare, the seizing of power by Pepy I, and the punishment of disloyal officials by the king. Kanawati also mentions the difficult years of the reign of Pepy I, in particular the trial of the anonymous queen and the conspiracy led by the vizier Rower around the twenty-first occasion of account of Pepy I. The results of the analyses of the archaeological evidence are gathered in two sets of charts—one for the reign of Teti and the other for Pepy I's—according to the extent of the damage executed on the tombs.

In the concluding chapter, the author has gathered a group of kings with several aspects in common (the location of their mortuary complexes, exclusion of Re from the royal name and perhaps a similar attitude towards the priesthood of the god Re), namely Unas, Djoser, Userkaf, Teti and perhaps Menkauhor (if his pyramid is also located in this area).

Sometimes the author tends to read more in the evidence than there actually is. For example, the usage of the name of Djoser is attested for the first time only in texts dated to the Middle Kingdom. Moreover, nothing can be guessed from the absence of the name of Re in his royal name (Netjerykhet) because it was formed according to other rules than later Old Kingdom royal names. There is no doubt that the mortuary complex of Djoser, who the ancient Egyptians considered to be the founder of the Old Kingdom, played a central role in the development of the royal necropolis at Saqqara.

Perhaps the weakest point of the book is the stress the author puts on the rather spurious role of the priesthood of Re and the ambiguous attitude of kings under discussion towards it. The author considers the priesthood of Re to have been a mighty power, which sometimes forced the kings to make compromises and even appeasements, e.g. Userkaf (the enlargements of his sun temple) and Pepy I (the change of the throne name from Nefersahor to Meryre). However, there is no written evidence that could support the theory of a struggle between the royal and divine power.

The reviewed book is a very important contribution to a better understanding of the highly problematic history of the later part of the Old Kingdom. More importantly, the study reveals to the eyes of a non-professional reader the method, fascinating in itself, of extracting historical information from a variety of archaeological material. As for the professional reader, the main asset of the book lies in the extensive collection of relevant archaeological and textual material—sometimes not yet published—, the historical analyses of this evidence, and the ingenious personal interpretation of the sources by the author.

The author is known for his great scholarly erudition, yet his style of writing is vivid and the language elegant. We are presented here with a study filled with material sources, arranged in a concise and accessible manner to the benefit of our reading pleasure.

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Beshara DOUMANI (editor), *Family History in the Middle East, Household, Property and Gender*, Albany, SUNY Press, 2003. 340 pp. \$86.50 (cloth), \$29.95 (paper). ISBN 0-7914-5680-3.

This book is one of a growing number of works devoted to family in the Middle East. It contains twelve articles that cover a regional focus on Egypt and Syria in addition to an article on Iran. The last article in the book, written by Akram F. Khater, is the only one which studies a region outside of the Middle East; notably it explores Lebanese migrants in the United States, tracing some of the tensions they faced as they settled in their new homes and the painful conditions of their interaction with middle class America.

The articles focus on the period from the mid- to late nineteenth century until the contemporary period, with the exception of two articles, by Beshara Doumani and Heather Ferguson, which deal respectively with Greater Syria in the period 1700-1860 and with Tarablus al-Sham in the seventeenth century. More importantly, they explore some little known regions and localities, rural, provincial and tribal families, material that is not often available to the historian. For instance, Erika Friedl's study of the villagers of Deh Koh in Iran between 1880-1990 explores a subject that few people know about, notably the inhabitants of a village in the Zagros Mountains, and tracing the long-term development of marital customs and relations as well as the expectations of the young spouses in this crucial period. The book, moreover, brings together research from several disciplines. The contributors are roughly evenly divided between historians on the one hand, and social scientists and demographers, on the other.

The first two articles, by Phillippe Fargue and Tomoki Okawara, based on population census, identify demographic trends in nineteenth century Cairo and Damascus. The Cairo census of 1848 that Philippe Fargue analyses shows some surprising results. It provides some important data on little known subjects like children, showing that many of them were separated from their parents at an early age, either by the death of their parents or by living outside their family residences. Fargue also indicates that the majority of households, some 70%, were structured along nuclear lines. Unlike Cairo, the dominant pattern in Damascus was a large household with a mean size of over eleven persons. Taken together with Duben's work on Istanbul households, Fargue's and Okawara's articles clearly show the great diversity in family structure in different regions of the Middle East and consequently the inadequacies of grouping together 'Arab families' and 'Muslim families' as clearly identifiable entities.

The issue of gender, the methods that women used to maintain some level of control over wealth and property, are explored by Annelies Moors, Martha Mundy and Richard Saumarez Smith. They show the spaces within which women negotiated their way within predominantly male-oriented societies. By exploring the ways in which wives tried to control possessions in gold, Moors' study of a village near Nablus shows the tensions between Islamic law which privileged male descendants and inheritance practice in relation to gold. Likewise, Mundy and Smith explore the strategies used by women in the village of Kufr Awan in northern Jordan, in relation to the wife's bridal payment or *mahr* which sometimes consisted of property rather than money.

The larger issue of writing family history on a non-linear non-Eurocentric approach is somewhat more complex. This has long been an objective for historians of non-European societies whose histories still bear the stamp of the trends born of colonialism. The difficulties and complexities of this issue is that many decades after it was formulated, it still remains with us and the solutions offered remain partial. This book is not an exception.

The transition to the modern family is a major concern in a number of articles. Fargue links this modernity to the fact that the focus of the 1848 population census was based on the individual as the statistical unit rather than a collectivity. For instance, it focussed on a person's economic activity, his employment and whether or not he was a student. Most of



the other articles dealing with the issue of modernity link it to the emergence of the nuclear family and as the move from a polygamous to a monogamous family. There is certainly much truth in this view, but the issue nevertheless remains problematic on a number of levels. One of these relates to the absence of references to the period prior to the nineteenth century. The presumption that multiple households or extended families were the only pattern or the dominant pattern in the seventeenth or eighteenth centuries has yet to be proven. As a matter of fact, studies on Cairene housing seem to indicate that the nuclear household existed and was perhaps even fairly common. One need only look at the living quarters of the *rab'*, made up of small apartments which sometimes consisted of a single room, to suggest that the issue needs further consideration. My own book *Habiter au Caire: les maisons moyennes et leurs habitants aux XVII<sup>e</sup> et XVIII<sup>e</sup> siècles* (Cairo, 1991) explored various housing types used by the middle class very likely to have been inhabited by a nuclear family. In more ways than one, it would have been interesting to see how this phenomenon can be linked to the developments of the nineteenth century. Such links could also constitute one of the ways in which historians can move away from a non-Eurocentric approach.

Likewise, the presumption that 'nuclear' was evidence of 'westernization' echoed in various articles in the book, can occasionally be misleading if it presupposes a single model of nuclear families. In reality, families (or couples) could be nuclear in their residential patterns while maintaining very close relations, financial or other, with their 'extended' families or with their parents. They could be bearing financial responsibility for their parents, or for their respective siblings, or on the contrary be financially dependent on them. Likewise, 'nuclear' families might live in an independent lodging while at the same time they resided in the same locality or district as their extended families, presuming frequent interaction. Finally, the expansion of a nuclear family structure, in the sense of the monogamous marriage replacing the polygamous one and the disappearance of the tradition of having concubines also needs to be rethought. Both Kenneth Cuno and Mary Ann Fay, who studied royal and elite families in late nineteenth century Cairo, were interested in an exploration of the move from polygamy to monogamy. Both have insisted on the internal forces that could have brought this change about, rather than simply attributing it to western influence. One nevertheless wonders if any connection could be made between this trend towards the monogamous marriage and the radical expansion of prostitution which we know to have occurred at about that same time. Did the expansion of prostitution impact the nuclear household, was it one of the factors that allowed it to flourish? These are as yet unanswered questions, and the connection between the two trends may be impossible to establish. Yet to avoid a positivist approach, the darker sides of the nineteenth century should be evoked.

As announced in the introduction, the book combines different methodologies. Historical demography based on population census was already mentioned. A number of articles by anthropologists are essentially based on field work and interviews. With their emphasis on material conditions, they develop an entirely different approach than analysis of the 'Islamic' family based on an exploration of religious texts. The emphasis on practice rather than on legal provisions also permeates the articles based on court records. These three articles, by Beshara Doumani, Iris Agmon and Heather Ferguson, approach court records by an in-depth study of one or two cases. We thus get a close view of family disputes and of the way that family members, on the one hand, and the legal system, on the other hand, dealt with such disputes. We also see that the bulk of such disputes revolved around financial matters, like the wife's claim to maintenance from her husband; disputes between various family members about an inheritance, about illegally withholding waqf revenues from the legal beneficiaries, or about misuse of funds, the allegations and counter-allegations.

Admittedly, the balance between the individual case and the broader society, between the little known village or locality and the region as a whole, is not an easy one. The reader may occasionally find himself lost in the minutie of a family dispute narrated in great length, and ask what relevance the wealth of details of a single case recorded in a court register or of

an interview with a family member can have for the larger picture and in what way it advances research towards a particular issue or debate of interest or controversy.

The book contains some sloppy footnoting. In spite of its shortcomings, this collection of articles is an important addition to a growing literature on the family which scholars in the field hope with time will help provide an understanding not only of the family but of the broader society.

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Tony DAY, *Fluid Iron: State Formation in Southeast Asia*. Honolulu: University of Hawaii Press, 2002. Pp. xii, 339. References, Index. \$56.00 (cloth); \$21.95 (paper).

This is a fascinating book, full of lively ideas synthesized from far-ranging and systematic reading, rich in comparative suggestion, and offering critiques of some more-or-less received ideas about the subject at hand. It is hard to imagine a Southeast Asianist with more than a passing interest in culture, politics, or history who won't find this thought-provoking, and many scholars outside that field should also see in it a great deal that is worthwhile noting. At the same time, however, the book possesses a dark side: it is full of conceptual contradiction and hesitation, is deeply conflicted about the nature of history as a discipline, and in the end it careens into precisely the postmodern cul-de-sac it appears to have hoped to avoid. More than most, this is a study both to learn from and argue about.

*Fluid Iron* is primarily a sophisticated exercise in reconsidering how scholars have thought about the (more-or-less) Weberian "state" in Southeast Asia, and in discussing certain elements which the author believes may be important to its formation (or, it might be argued, non-formation) and persistence. Despite the implication of its subtitle, the book offers neither a history of state formation in the region nor an argument explaining how states came into being and developed there. Questions about when a state may or may not be said to exist, or when a state may be described as "localized" or not, are raised but not answered. Tony Day's chief aims are, instead, to put culture (or elements of culture such as kinship and violence) and cultural analysis "back into" the study of the Southeast Asian state, past and present; to suggest "ways of thinking across and beyond the reigning dichotomies that separate 'traditional' from 'modern' Southeast Asia. . . . [and] one part of Southeast from another" (p. 37); to "raise doubts about 'change' . . . that great fetish-concept of the historian" (p. 290). These goals are pursued sensitively and with politesse, but what Day seems to be after is not merely an adjustment in what and how we consider history, but a tectonic shift.

The book consists of an introductory chapter reviewing the (mostly theoretical) literature on Southeast Asian states (again, not so much their historical formation as their nature), followed by four chapters discussing the topics of kinship, knowledge, bureaucracy, and violence, and their relationship to state structure and behavior. A brief conclusion does more than summarize the whole. The opening chapter will be of widest appeal, and has already found its way into reading lists for advanced students. It offers deft commentary on a number of ideas about states and state formation from Weber, Marx, Foucault, and Tilly, as well as from Furnivall, Wolters, Anderson *frères*, Geertz, Aung-Thwin, Leiberman, and Robinson, among others. We are reminded, for example, of the heavy legacy of dichotomous social science thinking about states—modern/traditional, Western/non-Western, rational/cultural, state/society, and so on—which Day characterizes as "account[ing] for virtually everything written about the state in Southeast Asia" (p. 7). An important concomitant of this binarism, Day

believes, is that “culture”—a term which he does not define—has been identified, where it is considered at all, with whatever is not modern, and is neglected as a factor in studies of the state, particularly comparative ones. This is what he strives to escape.

The argument driving the topical chapters is that the historical discipline’s propensity to view the trajectory between past and present in linear and essentialized or reductionist terms prevents us from appreciating both the ambiguities of culture and continuities across time. Day uses literary, social science, and historical studies to attempt to alter the balance, and many of the insights arising from this will resonate with the contemporary Southeast Asian scholar. He suggests, for example, that Southeast Asian societies are best viewed as the product of constant tension between opposites; that “Western causes alone are not responsible for ‘re-feudalizing’ and ‘centralizing’ indigenous Southeast Asia social and political practices” (p. 89); that Southeast Asian societies have a greater tolerance for ambiguity than many others (p. 260ff); that coercion and resistance are interrelated, as are security and freedom; the West and Southeast Asia are (in many respects) not so different after all (p. 224). These and other ideas are skillfully drawn or constructed from parts provided by other authors, a welcome process of synthesis all too few Southeast Asian specialists have been willing to undertake. There are a few lapses, such as when an already rather rococo conceptualization of Thongchai Winichakul’s is further ornamented to conclude nothing more startling than that the great nineteenth century Thai ruler Mongkut (Rama IV) was a transitional figure (p. 99), but on the whole the result is both helpful and plausible. What Indonesia watcher is not grateful for, even seduced by, the proposal that the New Order may be seen as Majapahit, or that Taman Mini is akin to Angkor Thom?

But how useful are such plausible “insights” as history? Problems arise when Day insists upon advancing what are not—or not yet, anyway—more than what the French call correspondances (roughly, connections or harmonies) to the level of (soft?) fact, and using them as principles with which to deconstruct fundamentals of the historian’s craft. He seems determined, for example, to argue that there is no dichotomy—and therefore, one must infer, no appreciable or analytically useful distinction—between the traditional and the modern. Such distinctions, he says, are mere artifacts of “historicism,” a troublesome term which he uses (not entirely defensibly, I think) to mean the practice of essentializing by theorizing back from the present to portray (and to measure and evaluate) the past. Day wants to “dehistoricize our understanding of the past” (p. 290).

Here Day wavers. In several instances he takes a moderate approach, suggesting for example that Indonesia’s New Order was the “predictable outcome” of centuries of “developing repertoires” (p. 222), or that nineteenth century bureaucracy on Java owed something to Majapahit and also to seventeenth century Holland (p. 217). We are provided with no details, but we do get a sense of a gradual, time-sensitive or evolutionary process at work. In other cases, however, Day seeks to convince us that there is in fact no such process, that culture is culture then as now; it floats above or outside of Time. A film about Flor Contemplacion is thus fundamentally the same as an ancient ceremony mobilizing departed kin (p. 53); contemporary states are fundamentally nothing more than the expanded family networks of ancient “states.” In order to make the argument work, Day emphasizes a postmodern insubstantiality of things: bureaucracies are simply “repertoires,” and states mere “effects”; they don’t possess any real content that we can compare or analyze further, certainly not linearly, and they are what they are. He also leans heavily on a concept of hybridization, by which he appears to mean a kind of change which involves no long-term, evolutionary alteration, at least not of essences, but only dead-end, *sui generis* forms. In short, Day wants to say that change takes place, but at the same time to see it as non-temporal in nature and to insist that it cannot be real change.

This is hardly a satisfactory way of solving the riddle posed by the tension between change and continuity in our representation of past and present, a riddle particularly important to Southeast Asian studies between the 1950s and 1970s. Most historians have long since come to terms with this tension and found ways of dealing with it. They understand that if history is about anything, it is about charting, or precisely describing, or “measuring” (in any num-

ber of ways that may be chosen) change over time. They know as well that hybridization is not only about mules and ligers, which are sterile; in other (probably more frequent) cases hybridization is part of a continuing chain of evolutionary change, which sometimes produces new species. And they also know that “culture,” which they recognize as a vague formulation, both changes and stays the same over time, like DNA strung over generations. Yet they expect (hope?) that both what changes and what does not change can be identified, and that this identification may result in a better understanding of both the process and the outcome. The concept of a linear—not predetermined and not at all necessarily “straight,” but linear in the sense of “traceable”—trail of causation, is key to making things understandable. Without it, past and present collapse into one another, reduced to nothing more than an indispensable fog; this too is a form of essentializing, and a particularly obscurantist one at that. To argue, as *Fluid Iron* does, that historians often underestimate and fail to appreciate the power of human agency (a.k.a. “culture”), is fair enough, a sensible warning. But to argue as well, as *Fluid Iron* frequently appears to do, that the solution is to abandon linearity altogether and see change as a mere fiction, goes much too far.

A notable curiosity in *Fluid Iron* is the author’s conflicted approach to Southeast Asia as a region. This is a hoary issue over which generations of scholars have labored and argued, for often rather obscure reasons. Once again, Day wants to have it both ways: on the one hand Southeast Asia is not a region, certainly not in the supposedly self-evident sense of, say, Europe or East Asia; nothing there is “authentic,” and whatever civilization or civilizations may be said to exist there are “polyvalent” and “conflictual” rather than “pure” (p. 293). At the same time it is a region; borrowing from de Certeau, its cultural interactions define it and distinguish it from other regions. It is even unique (implicitly, among regions): its states, for example, are “like no other historical formations on earth” (p. 291). At the very end of the book, Day struggles to describe Southeast Asia, rejecting several possibilities raised in recent works, ending by declaring that Southeast Asia is “incongruous,” though it is not entirely clear whether this means it is unique or not, a region or not. (Are other world regions or, for that matter, nations not incongruous? Where in this case does the incongruity shade off into congruity? South Asia? China? How? Surely there are strong arguments against placing much stock in the characterization.) Very early in the book, Day seeks to head off the problem by pleading that he is merely using the concept of Southeast Asia as a “heuristic frame” (p. 37). (Presumably in a postmodern “borderless” world it is politically incorrect to speak seriously about regions just as it is about nations.) On the one hand, this is stating the obvious: of course the idea of regions is a device, a tool of fairly high-level generalization well-recognized as such, no more no less. On the other hand, using such a device to shape a book’s worth of argument only to attempt to disavow its value in the end seems to me perverse. Throughout *Fluid Iron* Day in fact provides us with ample and vivid evidence that across the region comparisons and connections and harmonies can indeed be found that give us more reason than ever to see Southeast Asia as a region whose parts have something to do with each other and may be usefully and responsibly generalized about. Waffling over or throwing up one’s theoretical hands at inauthenticity and incongruity is unwarranted, self-defeating and—given the material presented here—quite unnecessary.

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