

# TULA *and* CHICHÉN ITZÁ: ARE WE ASKING *the* RIGHT QUESTIONS?

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**W**as Topiltzin Quetzalcoatl a real person? Did he, or perhaps some other Toltecs, move to Yucatan? Did Toltecs, or Itzas, conquer Chichén Itzá? Or did the influence go the other way? Who came first: the Puuc Mayas, the Itzas, or the Toltecs? Who were these Toltecs and Itzas anyway? Were they related to the Olmeca Xicalanca, the Xiu, or the Nonoalca? Was there a special relationship between Tula and Chichén? These are typical of the questions scholars have asked about the relationship between Tula and Chichén Itzá, from early debates between Desiré Charnay and Daniel Brinton through some of the chapters in this volume. But many of these are not useful questions. As phrased, most are unanswerable, and spending time pondering them will not advance our understanding of the Mesoamerican past. We need to abandon some of these questions and transform others into useful scholarly research topics.

Before we can understand the relationship between Tula and Chichén Itzá, advances are needed on a number of fronts. First, we need more basic information on these two urban centers. Neither can be considered well understood compared to other major Mesoamerican sites such as Tikal, Teotihuacan, or Monte Albán. Until their chronologies, layouts, activities, and institutions are far better known we cannot make much sense out of the relationship between Tula and Chichén Itzá. Second, scholars need to abandon their attempts to glean usable "history" about Tula and Chichén Itzá from mythological accounts like the books of Chilam Balam and the Aztec histories. Third, the concepts used to explain interaction between these cities—from migrations to conquests to world systems—need to be applied more systematically and rigorously. Finally, we need a better understanding of the wider context of Epiclassic/Early Postclassic Mesoamerica. It is impossible to evaluate how Tula and Chichén Itzá related to one another in isolation from wider Mesoamerican patterns. The essays in this volume make valuable contributions to these topics, but there is still much to be done.

## *The NATURE of the DATA*

One of the major obstacles to research on the relationship between Tula and Chichén Itzá has been a lack of basic information on the two sites. These sites need to be better documented and better understood on their own terms before we can address their possible interactions or relationships. A number of the chapters in this volume describe the results of recent fieldwork and analyses at the sites (Schmidt; Cobos; Healan; and Bey and Ringle), helping set the stage for a better understanding of their wider roles and their possible interactions. Nevertheless, serious problems remain with the data on Tula and Chichén Itzá, particularly the lack of publication of key information, uncertainties with archaeological chronologies, and the acceptance of mythical stories as historically valid chronicles.

### *Lack of Publication*

Unpublished dissertations, theses, and reports plague many areas and topics in ancient Mesoamerica, but they seem to be more prevalent for Tula and Chichén Itzá than for many sites. For nearly a half-century, H. B. Nicholson's (1957) unpublished Harvard dissertation was the major scholarly analysis of native historical accounts of Topiltzin Quetzalcoatl. This work was finally published in 2001 with little revision (Nicholson 2001). The archaeology of Tula is not very well published. Jorge Acosta, who conducted the major excavations of public architecture in Tula, wrote numerous journal articles but never synthesized his work nor published detailed plans, profiles, or other excavation data. Instead, we must rely on a series of recent summaries and syntheses of his work to put it into context (Healan 1989; Mastache et al. 2002; Sterpone 2000–2001). The artifacts from the Missouri Tula Project in the 1970s remain unpublished, other than the ceramics (Cobean 1990). George Bey's (1986) dissertation on Tula ceramics, from Dan Healan's excavations (see Healan, this volume), remains unpublished (although see Bey and Ringle, this volume). And now we are waiting for publication of the results of Osvaldo Sterpone's recent excavations (Sterpone 2000–2001), work that may contribute to a new interpretation of the chronology of Tula.

There are far more publications on Chichén Itzá than on Tula, but one looks in vain for rigorous data on the chronology or size of the site, or for explicit information on the overall activities and social context of Chichén Itzá as an urban center. Another unpublished Harvard dissertation (Lincoln n.d.) is widely cited (in this collection and elsewhere), but so long as it remains unpublished its data are less accessible to many in the scholarly community. The publication of these and other works would contribute greatly to our understanding of Tula and Chichén Itzá.

*Archaeological Chronologies. Long and Short*

To earlier generations of Mesoamericanists, Tula was considered a well-dated Early Postclassic site (ca. AD 950–1150) and Chichén Itzá was considered a poorly dated Early Postclassic site. Then opinions about the latter site changed, turning it into a poorly dated Epiclassic/Early Postclassic site (ca. AD 850–1150). This redating has been the primary “evidence” for rejecting Tozzer’s Toltec invasion hypothesis (e.g., Andrews 1990) (see Gillespie, this volume, and Kowalski, this volume). This argument suggests that the Toltecs from Tula could not have conquered (or migrated to, or otherwise greatly influenced) Chichén Itzá if the so-called “Toltec” traits were earlier there than at Tula. But now evidence is accumulating that the main construction and use of Tula Grande may extend back into the Epiclassic period. Can this be the case? How would it affect our reconstructions of possible interactions between the two sites?

I will discuss these issues using the concepts of short and long chronologies. In the “short chronology” the main intervals of construction and activity at Tula and Chichén Itzá are limited to the Early Postclassic period (ca. AD 900–1200); this is the traditional chronological model for the two sites. The “long chronology” describes revised sequences that move some or all of the buildings and activities of interest back in time into the Epiclassic period (ca. AD 700–900). The long chronology has become the new orthodoxy for Chichén Itzá, but at Tula the situation remains unclear.

The move from a short to a long chronology at Chichén Itzá has been reviewed by many authors (Kowalski and Kristan-Graham, this volume; Kowalski, this volume; P. K. Anderson 1998; Lincoln 1986), and I will not repeat the discussion here. What needs to be pointed out, however, is that the redating of Chichén Itzá has been done primarily on the basis of styles and inscriptions. These data are important, but without a good archaeological chronology they must be considered provisional and tentative evidence for dating the construction and activities at the site. Most archaeological chronologies in Mesoamerica consist of a sequence of ceramic phases. These phases are used to date occupations, activities, buildings, and other features. The first step in establishing such a sequence is to define a ceramic complex—a collection of ceramic types and modes that regularly occur together at a site or in a region. The second step is to arrange two or more ceramic complexes into a relative temporal sequence. The primary methods for achieving this (in areas like Mesoamerica) are stratigraphy, ceramic seriation, and certain relative dating techniques like obsidian hydration dating. The third step in building a ceramic chronology is to assign calendar dates to the phases. This is done primarily through radiocarbon dating and cross-ties to well-dated chronologies in other areas.

The ceramic chronology for Chichén Itzá appears to be at the second of the three stages outlined above. The relevant ceramic complex for the dominant occupation at the site is the Sotuta complex. Sotuta ceramics are relatively well published (Brainerd 1958; R. E. Smith 1971) and have been much discussed in recent literature (P. K. Anderson 1998; Andrews

et al. 2003; Bey et al. 1998; Cobos P. 2001; Pérez de Heredia Puente 1998). Although most archaeologists who work in Yucatan seem comfortable that this ceramic complex is associated with the major construction and occupation activities at Chichén Itzá (see Schmidt; Cobos; and Ringle and Bey, this volume), little of the evidence for elevating this ceramic chronology to the second level (relative sequence) or third level (true chronological sequence) has been made public. Where is the information from stratigraphy, seriation, radiocarbon dates, and other methods required to establish a standard archaeological chronology? Is there unpublished evidence? If so, how strong is it?

The best dates for Chichén Itzá are the inscriptions described by Krochok and Grube (this volume). These inscriptions may provide dates for a cluster of activities at the site between AD 864 and 889 (this is certainly the dominant view among Mayanists), if two types of assumptions are valid. First, one has to accept a series of assumptions about the meanings, contexts, and accuracy of this kind of calendrical dates, and second, one has to assume that the dates were contemporaneous with activity at the site (as opposed to referring to events in the future or the past). But until the ceramic chronology reaches the third level, it cannot be correlated with these independent dates. One of the most basic principles in comparing and correlating archaeological and historical chronologies is that each sequence must be developed independently before they are brought together (see Gillespie, this volume; K. L. Brown 1983; J. D. Evans 1974; Small 1995; M. E. Smith 1987, 1992). Otherwise, when prematurely hybrid chronologies are compared with individual dates (archaeological or historical) the result is circular reasoning that prevents an empirical evaluation of the degree of fit between the archaeological and historical chronologies. In summary, most archaeologists now seem to accept a long chronology for Chichén Itzá, and this model does fit with our knowledge of broader trends in the Yucatec past. But little of the evidence for the long chronology has been published, and as a result it is difficult to evaluate the strength of empirical support for the chronology or its implications.

The chronological situation at Tula is somewhat different. Fieldwork by Acosta in the 1940s and the Missouri Tula Project in the 1970s produced a generally accepted chronology with two Epiclassic phases (Prado, AD 650–750 and Corral, AD 750–900) and one Early Postclassic phase (Tollan, 900–1150), followed by two phases with Aztec ceramics (Fuego and Palacio). According to this chronology, Tula during the Epiclassic period was a small urban settlement with modest public architecture concentrated at the location known as Tula Chico. The Tollan phase then witnessed the construction of Tula Grande (the major urban epicenter), the expansion of the city's occupation to its maximal size, and the development of Tula's greatest range of macro-regional influence and interaction (see Bey and Ringle, Kristan-Graham, this volume; Cobean 1990; Davies 1977; Diehl 1983; Healan 1989; Mastache, et al. 2002). The ceramics of these phases are described in Cobean (1990); see also Bey and

Ringle (this volume). This is a short chronology in that the major period of urban development and long-distance interaction was limited to the Early Postclassic Tollan phase. The Mazapan phase in the Basin of Mexico is generally considered contemporaneous with the Tollan phase at Tula.

Most archaeologists working in central Mexico have found little reason to question the short chronology for Tula (e.g., S. T. Evans 2004: 316; Sanders et al. 1979; M. E. Smith 2003a). Nevertheless, a close look at the published sources cited above reveals a surprising shortage of archaeological support for this chronology. The stratigraphic distributions of ceramic frequencies have not been published and there are few radiocarbon dates. No ceramic seriations have been published, and only a few ceramic cross-ties are mentioned. The Epiclassic period is far better documented at sites other than Tula (Fournier and Bolaños, this volume; Fournier-García and Mondragón 2003), and there is still little chronological documentation for Early Postclassic occupations in other parts of central Mexico (Sanders 1996); one such site, Tlalpitzahuac, has been partially published (Pfannkuch Wachtel, et al. 1993; Tovalín Ahumada 1998). The weak level of empirical support for the short chronology at Tula does not mean that it is wrong, but it cannot be stated that this is a strongly supported archaeological chronology.

George L. Cowgill (1996: 327) first suggested that the Mazapan phase (and by extension, the Tollan phase at Tula) may have begun earlier than posited in the traditional long chronology; see also Cowgill (2000: 295–296); he suggests dates of 800–1000 AD, in place of the dates of the Tula project (900–1150). He bases this revision on two types of evidence. First, radiocarbon dates of Mazapan burials in the Cueva de las Varillas at Teotihuacan (Manzanilla, et al. 1996) suggest the earlier dates. Second, several types of ceramic cross-ties from the Mazapan and Tollan phases make more sense with the earlier dates, including imports of Tohil Plumbate ceramics at Tula, and stylistic linkages established by Scott (1993; 1998) between Mazapan figurines (and a ceramic sculpture) at Teotihuacan and figurines from the Xoo phase at Lambityeco in the Valley of Oaxaca. Although aspects of the definition of the Xoo phase (intended to replace the period Monte Albán IIIB/IV) remain controversial (Christina Elson and Joyce Marcus, personal communication 2003), members of the Proyecto Especial Monte Albán currently date this period to AD 500–800 (Martínez López et al. 2000).

Independently of this work, Osvaldo Sterpone suggests an earlier dating for the main urban architecture at Tula on the basis of his recent excavations. Sterpone directed a series of excavations in the north plaza and around Structure B at Tula Grande (Sterpone n.d., 2000–2001; Sterpone and Equihua Manrique 2000). As known from Acosta's earlier excavations (see Bey and Ringle, this volume), the architectural stratigraphy in this area is quite complex, and Sterpone's stratigraphic interpretations are detailed and multifaceted. Here I will only summarize some of the potential chronological implications of his work. In the short chronology, the construction

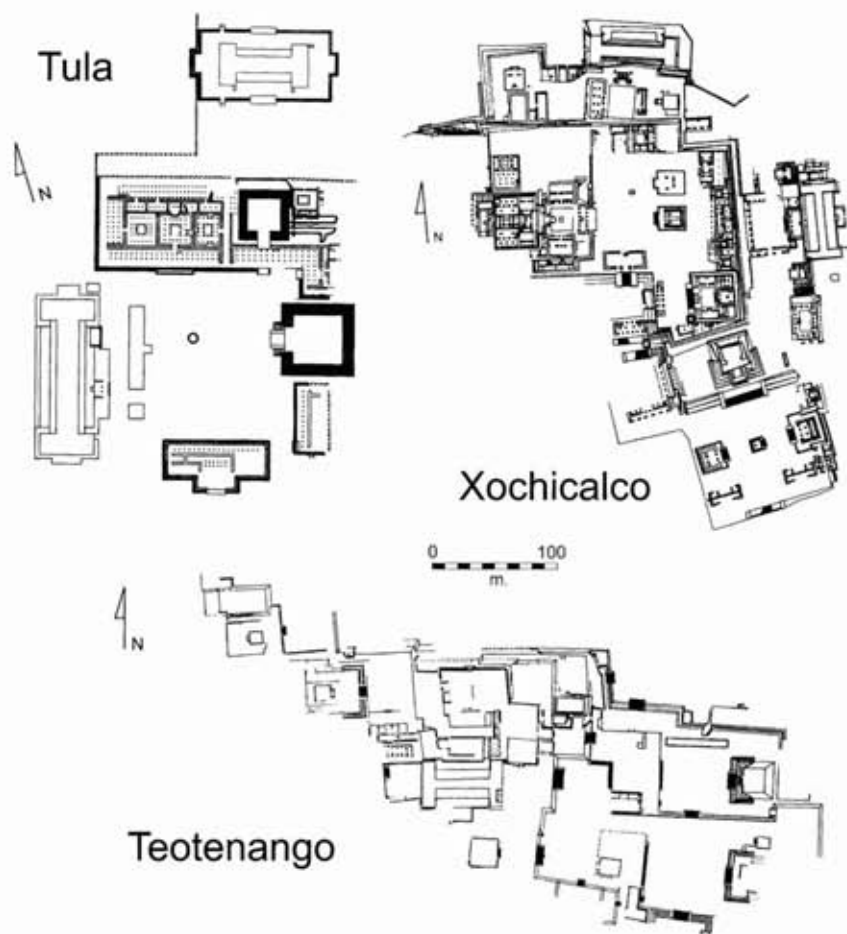
of Tula Grande is attributed to the Tollan phase because ceramic types from the Tollan ceramic complex (Cobean 1990) occur at the deepest and earliest excavation levels in most structures. Sterpone points out that the earliest deposits (associated with platforms that underlie Temple B and the Palacio Quemado) do not have the full range of types that compose the Tollan ceramic complex. The "Tollan complex" types from the earliest levels include Blanco Levantado, Mazapan, Macana, Soltura, Plumbate, and others. Sterpone suggests the creation of a new ceramic phase, called "Tula-Mazapa" (Sterpone n.d.a, 2000–2001). He argues that these types in fact began earlier in time—during the Epiclassic period—and then continued in use through the Tollan phase of the Early Postclassic period. Furthermore, he reports several radiocarbon dates for early construction in Tula Grande with an Epiclassic date range (ca. AD 700–950). If Sterpone is correct, some of the major construction of Tula Grande should be moved back in time, and the major urban architecture at Tula would have a long chronology, in line with Cowgill's suggestions.

Sterpone's research has not been published in detail yet, and so far only a few radiocarbon dates have been reported. Until this work appears in print and can be debated and discussed by experts in the stratigraphy, chronology, and architecture of Tula, the interpretations outlined above must be seen as provisional and hypothetical. Robert Cobean does not agree with Sterpone's revised chronology (Robert Cobean, personal communication 2004), and his current excavations at Tula should help clarify the issue. Nevertheless, Sterpone's chronological inferences do seem plausible, and they fit with the new data and interpretations Cowgill discussed (1996; 2000).

What if Sterpone is right and a long chronology were to prevail at Tula? The implications for the site, the region (Fournier and Bolaños, this volume), and all of Mesoamerica—particularly Chichén Itzá—are too numerous to discuss here, and it would be premature to do so. Some might think this would re-open the door for Tozzer's model, although that model is not very likely on a number of grounds (see below and Gillespie, this volume). I want to explore just one implication of a long chronology for Tula: our ideas about the history and large-scale context of central Mexican urbanism after Teotihuacan would have to change.

In the traditional model (based on the short chronology at Tula), the Epiclassic was a period of major urbanization throughout northern Mesoamerica. Powerful new political capitals arose at Xochicalco, Teotenango, Cacaxtla, Xochitecatl, El Tajín, and other centers, and Classic-period cities like Teotihuacan and Cholula (McCafferty, this volume) continued to play important roles. As described by Kristan-Graham (this volume), the Epiclassic period was a time not only of urbanization, but also of economic expansion, stylistic innovation, and heavy interaction among regions (Diehl and Berlo 1989; Ringle et al. 1998; M. E. Smith and Heath-Smith 1980). Tula, based upon the modest nature of its architectural and artifactual remains in the Prado and Coral phases, was only a marginal player in these networks. Most of these Epiclassic cities and interaction





networks came to a crashing halt around the tenth century, ushering in a period of ruralization and depopulation (M. E. Smith 1992). In central Mexico, Tula was the only major Early Postclassic city; the rest of the landscape was a rural backwater (except perhaps Cholula, whose size and influence in the Early Postclassic period are not clear).

If the long chronology were to become accepted for Tula, it would have contradictory implications for the macro-regional prominence of this city. On the one hand, Tula would be viewed as flourishing in a period of greater interaction and activity. This would alleviate some of the difficulties Kristan-Graham (this volume) identifies in her contrast of abundant Epiclassic activity with economic and urban declines in most areas in the Early Postclassic period. On the other hand, Tula's distinctiveness within central Mexico would be diminished. Instead of being the only major Early Postclassic political capital, it would become just another big Epiclassic city. Fig. 1 shows the epicenters of Tula, Xochicalco, and Teotenango

**FIG 1.** Plans of urban epicenters of Tula, Xochicalco, and Teotenango at a common scale. Sources: Tula: after Mastache et al. (2002: 92) and Mastache and Cobean (2000); Xochicalco: after Molina and Kowalski (1999: 143); Teotenango: after Piña Chán (2000: 41).

drawn at the same scale. Tula's zone of monumental architecture is not any larger than these other sites, although it does have larger individual structures and a far higher level of formal orthogonal planning. Its planning principles were later adopted by some Aztec city-state rulers to lay out their urban centers (M. E. Smith n.d., n.d.b).

Tula Grande could be inserted into our understanding of the large-scale dynamics of the Epiclassic period without much distortion, but where does this leave the Early Postclassic? Was Tula the only one of these Epiclassic metropoli to survive into Early Postclassic times? This is just one example of a number of issues that will have to be rethought if Tula and the Tollan phase turn out to have a long chronology. Right now, however, the above discussion should be seen as speculative in nature until additional fieldwork and analytical research produce a firmer and more widely agreed-upon chronology for Tula.

#### MYTH *and* NATIVE HISTORY

Conquest-period native historical accounts are unlikely to preserve reliable information about events from the Early Postclassic or Epiclassic periods. Surviving accounts of the Toltecs, the Itzas (prior to Mayapan), Topiltzin Quetzalcoatl, Tula, and Chichén Itzá all belong more to the realm of myth than history. Mesoamericanists are far too credulous in their acceptance of native historical sources; this is an example of what David Fischer (1970: 58–61) calls “the fallacy of misplaced literalism.” This lack of a critical perspective impedes our understanding of Tula and Chichén Itzá. Many scholars have posited artificially long native historical sequences in the vain hope of gleaning some historical tidbits from the legendary accounts of the Toltecs, Itzas, and other mythical groups. Instead, it is far more likely that the historical chronologies are much shorter, with a reliable time depth of not more than a century or two. Because many Mesoamericanists have chosen to ignore the historiographic problems with conquest-era native historical accounts, it may be useful to explore the topic briefly here.

The Aztec histories and the Chilam Balam accounts of Yucatan are the sources most relevant to Tula and Chichén Itzá. These Colonial-period oral traditions had Pre-Hispanic roots. Following the Spanish conquest, they were recorded in prose, and in central Mexico, in pictographic codices. The creation of an objective record of actual historical events with chronological accuracy was not a goal of the indigenous historical traditions nor their Colonial inscription. Rather, Pre-Hispanic native historical traditions served to legitimize polities, peoples, and dynasties, and to glorify the accomplishments of kings and ancestors (Boone 2000; Hassig 2001; Marcus 1992; Nicholson 1971). The written transcriptions of these traditions were produced to serve specific needs at the time of their production, particularly to legitimize polities, communities, and dynasties for the Spanish administration and to make sense of a colonial world turned upside down by the Spanish conquest (Gillespie 1989; Gunsenheimer 2003; Hassig 2001; Restall 1997; Townsend 2003; Wood 1998).



The oral nature of the conquest-era native historical traditions should be emphasized. The Chilam Balam texts were first written down between one and three centuries after the Spanish conquest, and then they were copied and recopied numerous times. Although clearly based on indigenous traditions, the surviving texts contain considerable Spanish influence (Farriss 1984: 247–249; Gunsenheimer n.d., 2003; Restall 1997: 276–282; Roys 1967). Many scholars consider it highly unlikely that these accounts preserve valid historical information about Chichén Itzá (Bricker 1981: 6–8; Edmonson 1982: xi–xx; Gunsenheimer 2003; Lincoln 1999).

We know considerably more about central Mexican native history both before and after the Spanish conquest. History was an oral genre that employed painted books—primarily continuous year-count annals (Boone 2000: 197–237)—as mnemonic devices to aid the historian or scribe in their recitation (Calnek 1978; Navarrete 2000b; Nicholson 1971). Although none of the painted history books that survive predate the Spanish conquest, it is very likely that pre-conquest polities did keep such historical records to verify the legitimacy of their kings (Boone 2000; Hassig 2001; Nicholson 1971).

During the early colonial period, local communities produced several types of painted history—the year-counts as well as maps with historical information (Boone 2000)—in order to prove their antiquity and legitimacy in Spanish courts (Leibsohn 1994; Wood 1998). The need for painted histories was so great that a “codex-on-demand” workshop was set up to provide Pre-Hispanic-looking painted maps for the ancient titles for central Mexican communities; many of these survive today as the so-called “Techialoyan codices” (Noguez 1999a, 1999b; Wood 1989). In conjunction with the production of painted histories, oral accounts and painted chronicles were transcribed in Spanish and Nahuatl prose. As Susan Gillespie’s (1989) research has shown, many of these “historical” accounts mixed up historical and mythical events and persons in order to make sense out of the colonial context of New Spain.

Nearly all scholars agree that the historical reliability of Aztec native historical accounts declines as one moves farther back into the past (Boone 2000; Davies 1977, 1980; Nicholson 1971). The historical traditions tend to begin with the Toltecs and Tollan, then move on to migrations from Aztlan, which are followed with specific dynastic histories of individual polities (most abundantly, the Mexica of Tenochtitlan). Florescano points out that the native sources:

are examples of historical narrations in which real human events are mixed in with mythic and legendary stories. Thus, the farther back in time the stories go, the less visible are human actions and the more overwhelming is the presence of myth: Teotihuacan is a sacred city; Tula, a mythic kingdom; Topiltzin-Quetzalcoatl, a legendary being with archetypal qualities—he is the model of the priest, cultural hero, and wise ruler. (Florescano 1994: 48)

Among scholars, four positions on the historical usefulness of Aztec native historical accounts can be identified: 1) A highly credulous attitude that assumes most of the native historical records do indeed record accurate information if we can just find the correct interpretations (Carrasco 1971; Jiménez Moreno 1954–55; Kirchoff 1961); 2) The application of explicit historiographic methods coupled with key assumptions leading to the view that the Aztec histories do preserve some valid information on Tula, Tollan, and Topiltzin Quetzalcoatl (Davies 1977; Nicholson 2001, 2002; H. J. Prem 1999); 3) The application of explicit historiographic methods and a more critical attitude, leading to the view that events of the Early Postclassic period (and certainly the Epiclassic period) are so far removed from the time of production of the surviving accounts that they are outside of the realm of credible historical reconstruction (Gillespie, this volume; see also Gillespie 1991; Graulich 1988; Olivier 2003; M. E. Smith 1984, 1992, 2003a: 30–31); 4) The assertion that no usable historical information exists in the native histories (Price 1980). The first and fourth positions are fringe views that need not concern us further; the important issue is the distinction between the second and third positions.<sup>1</sup>

Comparative cases of oral political history indicate that such accounts rarely have great time depth, and this finding supports the third position. Joseph Miller (1980) shows that many African oral historical traditions have two chronological stages. The earliest events, those that long predate the telling or recording of the history, he classifies as “the absent past.” In African traditions the absent past typically includes creation myths, followed by origin myths, and then “transferral myths” (accounts of migrations to a group’s homeland). The Toltec (creation myth) and Aztlan stories (origin myth and transferral myth) fit into this category. More recent events are included under “the present past”; these include a wide variety of events and episodes more closely associated with the place of origin of the historical account.

In a discussion of the comparative role of social memory, Fentress and Wickham (1992) make the following observations about oral history:

It reveals what the group’s feelings and beliefs are, rather than what the past itself was. Ignoring this distinction can lead to disastrous results (p. 78). . . . As a rule, oral tradition combines mythology, genealogy, and narrative history rather than holding them apart. This means that king lists are often spliced onto stories of mythological ancestors, and clan origin myths are embedded in stories of tribal movements. (Fentress and Wickham 1992: 82)

1 A recent exchange between H. B. Nicholson and Michel Graulich in the *Nahua Newsletter* provides a succinct account of some of the conceptual, methodological, and empirical differences between these two positions (*Nahua Newsletter*, vols. 33 and 34, 2002). I subsequently contributed a summary of some of my comparative research on oral history (M. E. Smith 2003d), and that paper was vigorously attacked by Offner (2004), who branded me a “nihilist.” Africanist David Henige then jumped into the fray (Henige 2004), supporting some of my views of historiography. Gillespie (this volume) reviews scholarly approaches to the use of native historical accounts of Tula and the Toltecs. Other good treatments of this topic include Jones (1995: 307–324) and Olivier (2003: 126–135), who provides a useful discussion in terms of the opposing “historicist” and “mythological” views of the topic.

David Henige's (1974) book, *The Chronology of Oral Tradition* (see also Henige 1971) is the standard work on the historiography of the chronological status of oral political history. He compares accounts—particularly king lists—from around the Old World to derive principles of interpretation. In some traditions, there is a “telescoping” of events such that long sequences are compressed into a short time frame. Far more common than this, however, is the “lengthening” of traditions by a variety of processes. A number of these clearly apply to the Aztec native traditions, including euhemerism (interpreting myths as historical accounts; Topiltzin Quetzalcoatl fits here), outright fabrication (likely in the aftermath of Itzcoatl's infamous burning of the history books), and genealogical parasitism—the attachment of recent dynasties to ancient dynasties in order to increase their prestige (the alleged Toltec origins of Aztec and other Postclassic Mesoamerica dynasties). Joyce Marcus (1992: 143–152, 261–302) discusses examples of these practices in Aztec native histories, and Prem (1984) points out additional chronological difficulties in the Aztec accounts. Throughout Africa, and in many other areas, the arrival of colonial rule with the accompanying loss of local sovereignty resulted in the rapid creation of lengthy historical records to help establish local legitimacy—for benefit of the conquerors—through reference to great antiquity. In numerous respects, the Aztec native histories fit right into the patterns identified by Henige (1971; 1974; 1982), Miller (1979; 1980), and others (e.g., Hemmingsen n.d.; Vansina 1985; Goody 2000: 47–62).

Henige (1974: 190–191) concludes that in most cases, oral political history does not preserve reliable chronological information for more than a century prior to the transcription of the oral tradition. The political nature of dynastic oral histories is the force most responsible for this situation. Oral traditions “are primarily seen and used as political symbols, and like the whole array of political symbolism, they serve specific purposes at particular times—primarily purposes of legitimation. In these circumstances the content of oral traditions continually underwent modification as necessity required” (Henige 1974: 6). Examples are known from early political historical traditions in all parts of the world. Historian Lars Hemmingsen, for example, states that “Danish legendary history was made up in the 12th and early 13th centuries from a mixture of oral traditions and written sources in order to satisfy a demand among Danish nobles” (Hemmingsen n.d.: 57); this quotation is from Carl E. Anderson (n.d.: 113). Anderson (n.d.), however, takes issue with many of Hemmingsen's conclusions and uses the same sources to make inferences about the nature of society and culture in early Scandinavia. In New Zealand, detailed historiographic research by D. R. Simmons (1976) has shown that traditional Maori oral traditions have been accepted uncritically to produce a historical sequence with far greater time depth than is warranted.

In Mesopotamia, the Sumerian king list provides an instructive parallel. Early scholarship focused on working out the chronological and thematic details. Historians assumed that this was a relatively direct record

of actual kings and practices, in spite of the fact that it was recorded many centuries after the events described (Jacobsen 1939). Subsequent scholars, however, pointed out numerous discrepancies and unlikely events, and identified the specific ideological role of this tradition for later rulers (Finkelstein 1979: 59–63; Michalowski 1983); see also Van de Mierop (1999). Much of the material recorded by the Greek historian Herodotus was from oral history (Murray 2002, first published in 1987), and it has been argued that his reliance upon king lists to date and discuss early heroic events such as the Trojan War produced numerous inconsistencies and inaccuracies (Burkert 1995).

Anthropologist Donald Brown (1988) has published the most thorough comparative study of ancient historical traditions. One of his basic conclusions is that societies with more open systems of social stratification tend to produce historical traditions that are empirically reliable, whereas societies with more closed stratification systems almost always produce mythologized official historical accounts. He develops his argument through detailed cross-cultural comparisons (e.g., China vs. India) and cross-temporal comparisons (e.g., Renaissance vs. Medieval Europe, and Classical vs. Homeric Greece), coupled with a theoretical examination of the nature of historical consciousness and its relationship with class structure, government, ideology, and other social institutions and practices. Within Brown's comparative scheme, Mesoamerican societies are clearly examples of closed stratification systems (Carrasco and Broda 1976; Chase and Chase 1992). Therefore it is no surprise that on the continuum of empirical reliability of historical traditions—from mythological to empirically reliable accounts—Mesoamerican native histories fall closest to the mythological pole.

Given what we know about the context and production of native histories in Yucatan and central Mexico, and the results of comparative research by Henige and others, it simply is not tenable to maintain that these traditions can provide historical information on Tula or Chichén Itzá. Although few of the authors in this volume use these accounts uncritically, many Mesoamericanists continue to apply historical sequences from the Chilam Balam accounts to Chichén Itzá (Ball 1986; Boot 1997; Coe and Koontz 2002: 154; Milbrath and Peraza Lope 2003: 33–40; Miram 1999; Schele and Mathews 1998: 258–260; Villa Rojas 1984) and Aztec native histories to Tula (Beekman and Christensen 2003; Coe and Koontz 2002: 154; Mastache et al. 2002: 74–75, 104, 303; Nicholson 2002; H. J. Prem 1999; Ringle 1994: 210–211). The presence of Itzas at Chichén Itzá, and Topiltzin Quetzalcoatl and his Toltecs at Tollan, make far more sense as myth than as history (Gillespie 1989; Graulich 1988, 2002).

Mesoamericanists should heed the call of Peter R. Schmidt for archaeologists to “set aside literal and facile treatments of oral tradition” (Schmidt 1990: 270). Writing about African history and archaeology, Schmidt shows how research and scholarly understanding on several topics have been held back by the application of simplistic and incorrect correlations between archaeology and oral history. Just as scholars now accept the revisionist

interpretation of documents like the Sumerian king list (Kuhrt 1995: 30–31), it is time for Mesoamericanists to acknowledge the mythological nature of colonial-period native history, at least for the earliest periods. The continued use of these flawed accounts as “historical” narratives is highly detrimental to the advancement of knowledge of Tula, Chichén Itzá, and their possible relationships (Henige 2004).

## CONCEPTS and MODELS

### *Migration and Ethnicity*

Accounts of Tula and Chichén Itzá, starting with Charnay and continuing through a number of the authors in this volume, tend to employ the concepts of migration and ethnicity in an uncritical manner. These are difficult processes to study with archaeological and art-historical data, and it is my contention that most interpretations of migration and ethnicity in relation to these two sites do not stand up to empirical scrutiny. The first problem is chronological; most accounts of migrations and ethnic groups at these sites are from native history and these sites were occupied far too early for such accounts to be reliable. Migration and ethnicity are in fact prominent themes in the origin myths of peoples all over the world (Henige 1982: 90–96), and this casts doubt on their historical veracity in the Mesoamerican examples under discussion here. I will focus first on migration.

Prior to the mid-twentieth century, migration and diffusion were the main concepts used to interpret the archaeological record. Changes resulted from groups of people moving here and there around the landscape, or from vague processes of diffusion of ideas or traits (Trigger 1989: 148–206). The influential “New Archaeology” movement of the 1960s and 1970s, on the other hand, emphasized local processes of adaptation; migrations and other long-distance processes were played down as explanatory concepts. It is only since the mid-1980s that archaeologists have returned to migration as a respectable topic of study (Anthony 1990; Beekman and Christensen 2003; Rouse 1986). Archaeologists are now developing new rigorous methods to document and analyze ancient migrations (Anthony 1990; Beekman and Christensen 2003; Burmeister 2000; Chapman and Hammerow 1997), putting the subject on a much firmer empirical foundation than previously.

Within Mesoamerican studies there has been a dichotomy in approaches to migration. Most archaeologists were influenced strongly by the ideas of the New Archaeology. For several decades, migration was simply not a very respectable topic of study. Although this “stay at home mentality” (McCafferty, this volume) hindered scholarship on migrations, it had the useful effect of purging Mesoamerican archaeology of the simplistic early-twentieth-century habit of invoking migrations uncritically to explain the past. Most ethnohistorians and art historians, on the

other hand, were unaffected by the New Archaeology and many failed to develop a critical attitude toward models of ancient migrations in Mesoamerica. Unlike archaeologists, these scholars did not have to invent migrations to explain ancient changes; Mesoamerican native historical traditions are full of migration accounts.

Migration stories are present in virtually all Mesoamerican native historical traditions. In most of these traditions, migration from elsewhere was a fundamental component of ethnic identity (and sometimes political legitimacy) in the sixteenth century, both before and after the Spanish conquest (Boone 2000; Braswell 2001; Christensen 1997; M. E. Smith 1984). These beliefs are almost universal in early historical traditions around the world. Henige (1982: 96) reviews cases from Africa to Scandinavia, from South America to Thailand, and from the Holy Land to central Mexico, and observes that, "it is clear enough that the notion of coming from somewhere else, whether it is somewhere best suited to the preconceptions of outsiders or to members of the society, is one that is uncannily attractive." He goes on to point out that in most cases where such foundational migrations—"transferal myths" in the terms of Miller (1980)—can be investigated apart from their role in oral history, they turn out to have little empirical validity. One cross-culturally common mytho-historical theme is the "stranger king" who arrived from distant parts to take over a kingdom (Feeley-Harnik 1985; Sahllins 1985: 73–103). When an appreciation for the mythological nature of early migration stories is combined with the chronological problems of Mesoamerican native history, it becomes untenable to discuss Toltec or Itza migrations as anything other than mythological accounts.

This is not a popular position. In spite of historiographic critiques (Gillespie 1989; Gunsenheimer n.d., n.d.a), numerous scholars accept stories of migrating Itzas and Toltecs as historical fact, questioning only the specific dates at which the migrations supposedly happened (Andrews et al. 2003; Asselbergs 2001; Boot 1997; Coe and Koontz 2002: 154–155; Davoust 2002; Jiménez Moreno 1954–55; Pugh 2003; Reyes García 1977; Schele and Mathews 1998: 198–204; Thompson 1970: 10–25; Tozzer 1957). In this volume, McCafferty takes this position. It seems to me that such credulity with respect to dubious historical sources owes more to a strong desire to recover historical information than to a critical historiographic approach to the sources. Parallel to the case with Toltec and Itza migrations, I give no credence whatsoever to accounts of migrations by the "Olmeca-Xicalanca" or the "Nonoalca" (Asselbergs 2001; Chadwick 1966; Reyes García 1977); in fact I question the empirical existence of groups of people with these labels outside of the ideology of conquest-era elites.

The burden of proof is on scholars who believe that the obviously fragmentary, propagandistic and mythological native historical migration accounts can be analyzed to yield reliable historical



information.<sup>2</sup> I am not the only skeptic about migration legends in this volume (Fournier and Bolaños, this volume). Regardless of how much accuracy one is willing to attribute to native historical migration accounts, however, the fact remains that no one has applied rigorous archaeological migration models (Anthony 1990; Chapman and Hammerow 1997) to data from the Epiclassic or Early Postclassic periods, although Beekman and Christensen (2003) do make an important start here; see also Arnauld and Michelet (1991). I am not claiming that migrations did not take place in ancient Mesoamerica; the distribution of languages provides clear evidence for numerous major migrations (Justeson and Broadwell 1996). Instead, I am calling for a more skeptical attitude toward native historical accounts of migrations, and the application of more rigorous methods in documenting and analyzing possible cases of migration.

The concepts of migration and ethnicity are closely associated, both in Mesoamerican native historical traditions and in modern Mesoamericanist scholarship. Ethnicity may be even more difficult to document archaeologically than migrations. Leaving aside theoretical debates between primordialist and instrumentalist theories of ethnicity (Bentley 1987; S. Jones 1997), there is a fundamental problem in studying ethnicity with archaeological evidence. Some ethnic groups, in some situations, can be identified through their material remains (such material markers of ethnicity can result from either self-conscious public ethnic expression or from more private, traditional ethnic practices). Other ethnic groups, or ethnic groups in other situations, do not express themselves in material terms, even though their ethnicity may be of fundamental social importance in their daily lives and their wider social contexts. At our current state of knowledge, it is simply impossible to determine, *a priori*, the conditions that determine whether past ethnicity was expressed in material culture or not. When archaeologists recover patterned variation in material culture (whether in pottery, jewelry, architecture, iconography, or any other trait), it can be difficult, if not impossible, to determine whether that variation reflects ethnicity, some other kind of identity (gender, status, occupation), or a different sort of

2 I am not disputing all native historical accounts of migrating ethnic groups. I have argued for the historical reality of the Aztlán migrations based on several lines of evidence (M. E. Smith 1984, 1992, 2003a: 30–31): 1) these are dated to a more recent time span (twelfth to thirteenth centuries) than many other Mesoamerican migration stories; 2) a large number of independent historical sources, from many parts of central Mexico, converge in their depictions of the Aztlán migrations; 3) there is archaeological evidence supporting the arrival of immigrants at about this time; and 4) this migration model seemed to find support in historical linguistic reconstructions of the northern origin of Nahuatl and its relatively late arrival in central Mexico. Although some scholars suggest that the Aztlán migrations belong more to the realm of myth than history (Florescano 1990; Graulich 1995: 22–26), the current historiographic consensus is that the pictorial and text accounts are more or less accurate in outline, and that the ancestors of the Aztec peoples did indeed migrate into central Mexico from the north (Castañeda de la Paz 2002a, b; Navarrete 1999, 2000a). In my view, much of Aztec native history *after* the Aztlán episode can be used for historical reconstruction, whereas none of the *Pre-Aztlán* material is useful for this purpose. Recent linguistic research that pushes the initial arrival of Nahuatl in the Valley of Mexico back to ca. AD 500 (Kaufman 2001) will force some revision of my earlier model. Another example of a well-documented Postclassic Mesoamerican migration is the case of the Nahuatl-speaking Pipil groups in Central America. As analyzed by Fowler (1989a, 1989b), there is a convergence of multiple lines of evidence—historical linguistics, toponyms, native history, and archaeology—that provide a very strong case for the migration of the Pipil to Central America in Postclassic times.

social factor not relating to identity at all. Do murals at Chichén Itzá show different ethnic groups, as Tozzer claimed but others dispute (see discussion in Kowalski, this volume)? There is no reliable way to tell.

Given our still rudimentary understanding of the nature and processes of ethnicity at the time of Spanish conquest, it is very unlikely that we can document specific ethnic groups and ethnic processes many centuries earlier.<sup>3</sup> Most archaeological accounts of ancient ethnicity rely upon a series of questionable assumptions. Jones (1996, 1997) has identified three such assumptions in the work of Old World archaeologists: 1) boundedness: ethnic groups are tightly integrated social groups with clear boundaries; 2) homogeneity: all members of an ethnic group use the same styles and types of material objects; and 3) continuity: ethnic groups, their practices, and their material culture remain constant over long periods of time. Any familiarity with work on ancient Mesoamerican ethnicity (including several chapters in this volume) will recognize these assumptions. Jones (1997, 1999) discusses the inadequacies of these assumptions in the light of ethnographic and historical data on ethnicity (see also Barth 1969; Emberling 1997; Terrell 2001; Wells 2001).

The most credible archaeological studies of ethnicity focus on possible ethnic enclaves. These are situations where a group of people deliberately settled among people of a foreign ethnic identity for specific economic or political reasons (Spence 1996). The best Mesoamerican candidate is the so-called "Zapotec barrio" at Teotihuacan, where the available evidence strongly suggests the presence of Zapotecs from Oaxaca (Ratray 2001; Spence 1992, 2005). Weaker cases of possible ethnic enclaves have been proposed by Santley et al. (1987), Ratray (1990), Hernández Reyes (1990), and Smith and Lind (2005). It is significant that no one has made a credible suggestion (or even a likely hypothetical suggestion) for a Toltec enclave at Chichén Itzá or a Maya enclave at Tula.

If one accepts the unrealistic nature of the three assumptions listed above, then serious doubt is cast upon much of the scholarship on ancient Mesoamerican ethnicity. In the words of Eric Wolf (1982: 6): "By endowing nations, societies or cultures [or ethnic groups] with the qualities of internally homogeneous and externally distinctive and bounded objects, we create a model of the world as a global pool hall in which entities spin off each other like so many hard and round billiard balls." These conceptual and empirical problems with traditional approaches to ancient Mesoamerican ethnicity are another reason I do not accept the validity of the Postclassic Itzas (prior to Mayapan), the Toltecs, or the Olmeca-Xicalancas as historically valid ethnic groups. Scholarly accounts of migrating ethnic groups to or from Tula or Chichén Itzá provide some of the most glaring examples of asking the wrong questions.

3 I am not including groups of people speaking the same language under the category of ethnic group here. Historical linguists can reconstruct many aspects of the history of languages and language groups, including migrations and contacts with speakers of other languages (Kaufman 2001; Nichols 1992; Ringe 2000). The correlation of historical linguistics with the archaeological record is still in its infancy in Mesoamerica. This is too bad, since many reconstructed languages and inferred groups of speakers have more empirical validity than many proposed ancient ethnic groups.

### *Shared Forms, Styles, and Symbols*

What does it mean when a distinctive form of building or architectural feature—a colonnaded hall, a platform decorated with skulls, or a sunken patio—occurs at distant cities? What about shared sculptural styles or similar images—for example, feathered serpents or warrior's costumes? How can scholars use evidence for long-distance similarities in buildings, styles, and symbols to reconstruct processes of interaction in the past? These questions lie at the heart of much of the debate over the relationship between Tula and Chichén Itzá. Unfortunately, there is a real lack of systematic models for evaluating and interpreting this kind of evidence. This is a more nebulous terrain than migration or ethnicity; the latter are social phenomena that we know something about from modern times, and the problem is to document and understand them for past societies. Shared forms, styles, and symbols are empirical observations about ancient material culture, and it is not immediately clear how scholars should approach their analysis. Could the similarities arise from processes of economics, politics, religion, ancient tourism, psychic unity, scholarly bias, or simple chance? Without agreed-upon models or approaches, scholars have been free to make up stories about interaction with no way to evaluate their empirical strength or plausibility.

One way to avoid speculative accounts is to start not with the forms or styles, but with an explicit model of interaction (trade, migration, conquest, and the like) and evaluate the material similarities against the model. This is the approach Lisa Montiel and I applied to studies of central Mexican empires (M. E. Smith and Montiel 2001). Cross-culturally, imperial expansion tends to be associated with particular patterns of material remains, from craft production to trade to iconography (see also Parker 2003). We evaluated the archaeological data for Teotihuacan, Tenochtitlan, and Tula (and their near and distant hinterlands) in terms of cross-cultural patterns and concluded that the first two cities were almost certainly capitals of empires, whereas Tula probably did not rule an empire. This kind of explicit testing of models of interaction (e.g., Schortman and Urban 1992; Stark 1990) is a fruitful approach to dealing with shared styles and forms; chapters by Kepecs, Healan, Bey and Ringle, and Kristan-Graham provide useful studies along these lines. Before turning to some of the models that have been proposed for interaction between Tula and Chichén Itzá, I will offer some comments on the general process of analyzing shared styles and symbols. These comments reflect my personal archaeological perspective on the topic. Scholars of art history have also developed concepts and methods for analyzing images that occur at distant locations (Baxandall 1985; Graham 1998; Kubler 1962; Panofsky 1955; Pasztory 1989), but these are beyond the scope of the present chapter.

The first step in addressing shared forms, styles, and symbols is to evaluate the degree of similarity. This can be a real problem. To some observers, Tula and Chichén Itzá "are practically mirror images of one another" (McCafferty, this volume), whereas others see the main architectural groups at the sites as very different (Cobos, this volume; Lincoln

1986). Which is it? Are they similar or different? I am more struck by differences than similarities, but the reality is more complex (which is one reason we need better models). What about the iconography at the two sites: similar or different (Stocker 2001; Taube 1999)? Until scholars develop some kind of objective criteria for comparison, we will remain mired in unverifiable subjective interpretations. This problem plagues a number of topics in Mesoamerican architectural and urban studies, including the possible role of cosmology in Maya city planning (M. E. Smith 2003b). Kristan-Graham's (this volume) review of varying interpretations of colonnaded halls and sunken patios in northern Mesoamerica is a good discussion of these issues. Everyone has a different view, so how are we going to choose among them?

Once we have decided to someone's satisfaction that two distant cities share a common building form, style, or symbol, a second step is to reconstruct the use or social context of the elements as fully as possible. Similarities in temple form have different implications than similarities in palace form. Similarities in public religious art arise from different processes than similarities in restricted imperial tribute records. A third step is to compare the temporal and geographical distributions of the similar elements. Recent research, much of it reported or synthesized in chapters in this volume, is providing a much better understanding of the geographical patterning of key features from Tula and Chichén Itzá. But as I discuss above, the chronology issue remains unresolved.

A fourth and final step is to place the comparisons within the wider context of independent evidence about the sites, their organization, and the wider cultural system. Architectural or stylistic comparisons made in isolation cannot be interpreted properly. For example, the Aztec and Inka empires were both characterized by architectural similarities in public buildings between their capitals and their provinces. But when a broader array of evidence is considered, it becomes clear that these similarities had radically different origins. In the Inka case, similarities in key building types (e.g., kallankas, kanchas, and temples) arose from deliberately imposed imperial construction programs (D'Altroy 2002; Gasparini and Margolies 1980; Morris 1972). In the Aztec case, similarities in the double-stair pyramid, for example, predated the formation of the empire by centuries, and can best be attributed to the basic cultural uniformity of central Mexican Aztec peoples in the Early Aztec period (M. E. Smith 2003a: 37–42, n.d.b).

This fourth step—contextualization—is where much of the earlier work on Tula and Chichén Itzá faltered; by focusing exclusively or primarily on architecture at these two sites, scholars virtually guaranteed that they could not produce a credible explanation. A number of chapters in this volume are noteworthy for contributing to a new understanding of the wider Mesoamerican context of these two cities (particularly chapters by Kepecs, Kowalski, Freidel, Healan, Bey and Ringle, Kristan-Graham, and Fournier and Bolaños). Eventually this line of research will lead to a much better understanding of the situation.

*Interaction Models: Empires, Trade Networks,  
World Systems, and World Religions*

Montiel and I have argued that in all probability Tula did not rule an empire (M. E. Smith and Montiel 2001). We concluded that Tula may have controlled a regional state that extended to the Bajío (this is relevant to Kristan-Graham's discussion), but not to the Basin of Mexico or other parts of central Mexico, and certainly not to Yucatan. Imperial expansion from Tula to Chichén Itzá is extremely unlikely for two reasons: 1) our archaeological criteria for imperial expansion are not met for Chichén Itzá or Yucatan; and 2) given the demographic, technological, and economic constraints on Mesoamerican warfare (Hassig 1992), it would have been almost impossible for any Pre-Aztec central Mexican polity to launch a successful invasion of Yucatan. Even the Aztec empire never managed to conquer the Maya-speaking areas of Mesoamerica.<sup>4</sup> Conversely, it is also very unlikely that a Maya polity could have invaded central Mexico and conquered Tula, Cacaxtla, Xochicalco, or any other Classic or Postclassic city.

Trade is a common theme in discussing interaction between Tula and Chichén Itzá. As discussed here by Healan (this volume), Tula was involved in the trade networks that brought central Mexican obsidian to Chichén Itzá, Isla Cerritos, and other sites in Yucatan. The green obsidian from the Pachuca source area almost certainly came through Tula, and Tula may also have played some kind of a role in the movement of Ucareo obsidian to Yucatan (Healan, this volume, Healan 1997). Kepecs (this volume) suggests that the high-quality northern Yucatecan salt was traded to Tula; this is a reasonable hypothesis, but one that is impossible to verify right now. It seems clear that there was trade of some sort between Tula and Chichén Itzá (Bey and Ringle, this volume; McVicker and Palka 2001). While this establishes a definite connection, the simple existence of imported goods does not specify very much about the nature, the context, or the significance of that connection.

This is where the world-systems approach comes into play. World-systems models focus on the social and political implications of exchange systems among states. Most Mesoamericanists who have written about world systems are following what Peregrine (1996) calls the world-systems perspective—a loose collection of concepts based around the study of long-distance trade that transcends polity borders and has important social impacts—rather than “world systems theory,” a particular model of the modern capitalist world economy (Wallerstein 1974). In an earlier article, Montiel and I argued for the usefulness of the world-systems perspective in understanding the international prominence of Tula within Early

<sup>4</sup> The willingness of some Mesoamericanists to accept a Toltec conquest of Yucatan is exceeded by a willingness to accept a possible Teotihuacan conquest of Tikal and other Classic Maya cities (e.g., Fash and Fash 2000; Stuart 2000). For arguments against this position, see Braswell (2003) or Smith and Montiel (2001).



Postclassic Mesoamerica (M. E. Smith and Montiel 2001: 268–269). We argued that whereas Tula was most likely not the capital of an empire, the central Mexican city nevertheless played an important role in Mesoamerica with influence far beyond its local hinterland. One advantage of the world-systems perspective is that it can help account for the international prominence of a city without requiring that the city be an imperial capital.

Kepecs (this volume) is the only author in this collection who applies explicit world-systems concepts to Tula or Chichén Itzá; several other authors mention the phrase “world systems” without actually carrying out any kind of world systems analysis. Kepecs has previously applied a world-systems perspective to Chichén Itzá (Kepecs 2003; Kepecs et al. 1994). From this viewpoint, the Epiclassic/Early Postclassic trade in ceramics, obsidian, salt, and probably other goods between Yucatan and other parts of Mesoamerica was of crucial importance for the social and economic dynamics of Chichén Itzá.

In a parallel argument about symbolism and rulership, Ringle (2004) suggests that foreign symbols provide important clues to the nature of political organization at the city. Scholars like Ringle and the other contributors to this volume are now turning away from outmoded arguments about diffusion and influence to interpret such foreign imagery as tools employed deliberately by local rulers and elites for their own purposes. Again, these data fit very well within a world systems perspective. Whereas older world-systems models focused almost exclusively on economic and political forces, more recent works have incorporated style, symbolism, writing, and visual culture as major world systems processes (Boone and Smith 2003).

The world-systems perspective provides a tool kit of concepts that describe dynamic international social systems (Abu-Lughod 1989; Algaze 1993; Blanton and Feinman 1984; Chase-Dunn and Hall 1997). These concepts are most useful in situations where distant independent polities were linked together through processes of economic interaction and information exchange. The key requirement is that these processes of international interaction had important impacts on the participating societies. This clearly describes the situation of Epiclassic–Early Postclassic Mesoamerica, and I think that the world-systems perspective has much to offer scholars in our continuing efforts to understanding Tula and Chichén Itzá.

Another useful concept that might find applicability for Tula and Chichén Itzá is the merchant diaspora. Although Gil J. Stein (1999, 2002) promotes this concept as an alternative to world systems theory, it actually fits quite well into the world systems perspective. In a merchant diaspora, groups of merchants move from a core zone to foreign areas, where they set up colonies or enclaves to help organize long-distance commerce. Such colonies may last for generations or even centuries. Merchant diasporas are a common institution in human history (Curtin 1984; Stein 2002; Zenner 1991), but they have been little explored in Mesoamerica. When Mesoamericanists have looked for models for long-distance trading systems, they have tended

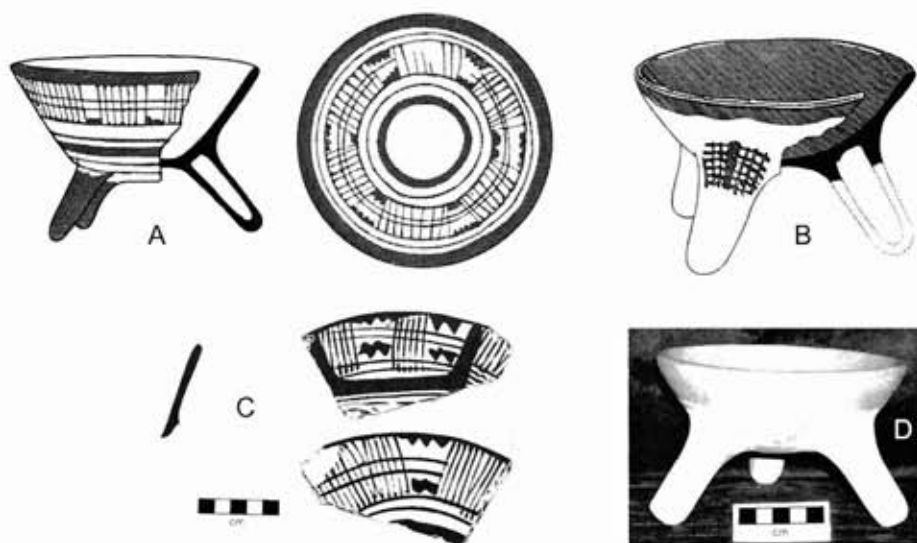


to focus on the Aztec *pochteca*, who were organized very differently from merchant diasporas. This emphasis on the *pochteca* may have prevented consideration of the merchant diaspora model. The “Oaxaca barrio” at Teotihuacan, however, looks very much like an enclave of merchants from Oaxaca (Spence 1992, 2005; Winter 1998), and perhaps other examples can be identified. I must admit that the data from Tula and Chichén Itzá do not seem to fit this model, but my point is that Mesoamericanists need to draw from a wider range of concepts and approaches in order to understand long-distance exchange and interaction.

Another model that has been applied to Tula and Chichén Itzá is the spread of a major international cult focused on the feathered serpent (López Austin and López Luján 1999, 2000; Ringle 2004; Ringle et al. 1998); see Bey and Ringle, this volume. I find this model attractive for its geographical and thematic breadth, and it seems very likely that some kind of a new cult or religion did flourish in many areas in Epiclassic and Early Postclassic times. I hesitate to embrace this model more strongly, however, because it has not been established firmly enough in theoretical and comparative terms. It would be useful to see the presentation of a material culture model for the spread of international religions—something parallel to the model Montiel and I describe for imperialism (M. E. Smith and Montiel 2001), or the models Stark (1990) describes for various types of political and economic interactions. What exactly are the material expressions of this kind of process as documented historically from other regions and time periods? How closely do the Mesoamerican data fit the general comparative model? How can this process be differentiated from other similar interaction processes in terms of the archaeological record? Bey and Ringle have not presented this kind of comparative context for their model apart from a few superficial references to early Christianity and early Islam; consequently their model remains speculative and hypothetical. Nevertheless, I think it has great potential for illuminating many aspects of religious artifacts, temple architecture, and iconography in the Epiclassic and Early Postclassic periods, including some components of the relationship between Tula and Chichén Itzá.<sup>5</sup>

One of the major virtues of the models outlined above (empires, trade, merchant diasporas, world systems, and international cults) is that they involve social processes that operate over broad areas. Tula and Chichén Itzá were part of the larger world of Epiclassic/Early Postclassic Mesoamerica. Diehl (1993) provides one of the best analyses using this framework to advance our understanding of interactions at this time (see also Bey and Ringle, this volume). It is instructive to note that in recent analyses of foreign iconographic traits in the public art of Chichén Itzá,

5 The world religion model can also fit easily into the world-systems perspective. Although most world systems research has focused on economic exchanges, recent research has begun to incorporate religious, iconographic, and stylistic interaction. Several chapters in *The Postclassic Mesoamerican World* (M. E. Smith and Berdan 2003) focus on iconography and styles, integrating these phenomena into broader patterns of long-distance interaction in Late Postclassic Mesoamerica.



**FIG 2.** Painted ceramic vessels from Yucatan and similar vessels from the Toluca Valley of central Mexico: a) vessel in the Regil collection in Merida (Brainerd 1958: fig. 75n); b) partial vessel from the Monjas complex of Chichén Itzá (Brainerd 1958: fig. 75o); c) Type D4 vessel from Tonicico (Arana Alvarez 1990: lám. 13); d) Type E1 vessel (vessel no. TV6-206) from Calixtlahuaca. Photograph by Michael E. Smith.

most of the central Mexican examples are not from Tula at all. For example, in Taube's (1999) widely cited paper, nine of his central Mexican examples (used to compare with art at Chichén Itzá) are from Aztec sculptures, fifteen are from Aztec and Borgia-group codices, and only two are from reliefs at Tula. A similar pattern is present in Ringle's (2004) more recent discussion of iconographic parallels between Chichén Itzá and central Mexico. The implications of these patterns are clear; scholars need to consider a wider universe, not just Tula and Chichén Itzá, in order to understand developments at that time.

Tula interacted with many areas in addition to Yucatan (Mastache et al. 2002), just as Chichén Itzá had international connections beyond Tula. The Maya city appears to have been the more cosmopolitan of the two, with greater international connections. Imported obsidian came not just from Tula, but from western Mexico and highland Maya sources as well (Healan, this volume; Braswell and Glascock 2002). "Foreign" symbols and styles at Chichén Itzá originated not just in Tula, but in a broader part of central Mexico (see discussion above). It is likely that continuing research will provide examples of distant connections with still other areas.

I can mention one possible example of exchange between Chichén Itzá and another area, the Toluca Valley of central Mexico. Brainerd (1958: fig. 75n, o) published two foreign painted tripod bowls, one from the Regil collection in Merida (fig. 2a) and the other from excavations at the Monjas complex at Chichén Itzá (fig. 2b). The

Merida example is nearly identical to a partial vessel (fig. 2c) recovered in a survey near Tonicaco, in the state of Mexico south of the Toluca Valley (Arana Alvarez 1990). This red-on-white ceramic is an example of type D4 in my classification of Postclassic Toluca ceramics (M. E. Smith n.d.a), a type common in the southern portion of the Toluca Valley. Brainerd relates the second vessel (fig. 2b) to Toltec ceramics, and it resembles the Tula type Macana red-on-brown (Cobean 1990: 289–312). Nevertheless, if the red slip covers the entire interior surface, as Brainerd's illustration suggests, then this vessel may resemble more closely the Toluca Valley ceramic type E1 (M. E. Smith n.d.a), an example of which is shown in figure 2d (the crisscross decoration on the support of Brainerd's example does occur in the Toluca Valley). Unfortunately, the chronology of the Toluca Valley types has not been worked out yet beyond a general Postclassic time frame (M. E. Smith 2003c; M. E. Smith et al. 2003).<sup>6</sup> Although these examples are far from definitive, they do suggest that continuing research on external connections at both Tula and Chichén Itzá will help expand our frame of reference beyond the two-site polarity that has characterized much past work.

#### WHERE DO WE GO FROM HERE?

In the introduction to this chapter I suggest four areas where advances are needed to better understand the relationship between Tula and Chichén Itzá. The papers in this volume present new data and interpretations that help move scholarship forward in all four areas.

1) We need better data on the two sites. I have emphasized the need for more refined and more firmly supported archaeological chronologies for the two sites. Additional architectural excavations and analyses in the centers of the sites would be welcome, but scholars should also consider an often-overlooked source of information: the excavation of commoner residences. Urban residents in Late Postclassic Mesoamerica participated in processes of long-distance exchange of goods, styles, and information, and their domestic remains provide crucial data for reconstructing economic, social, political, and religious patterns (M. E. Smith and Berdan 2003). Their ancestors in earlier periods were probably no different in this respect. There has been more residential excavation at Tula than Chichén Itzá (Cobean and Mastache 1999; Healan 1989; Paredes Gudiño 1990), but both sites could benefit from larger doses of household archaeology.

2) Scholars should abandon treating native historical accounts of Tula and Chichén Itzá as if they contained reliable historical information. Two kinds of changes will benefit scholarship on this issue: a more critical atti-

6 It is entirely possible that the red-on-white bowl from the Regil collection is from a Yucatec context later than Chichén Itzá (perhaps Mayapán); in fact such an interpretation would accord better with my admittedly provisional understanding of the chronology of the parallel type D4 in the Toluca area. It is also possible that the bowl in question was obtained in central Mexico and was never part of the archaeological record in Yucatan. These are only some of the problems that arise in relation to unprovenanced objects in private collections.

tude on the part of scholars, and the development of innovative historiographic methods for extracting useful information from native historical accounts. My suggestion that these accounts do not contain reliable facts on politics and society at Tula and Chichén Itzá does not imply that these traditions are without value. The problem is to extract useful information without succumbing to an overly literalist reading of the sources. Alan Covey's (2003) innovative analysis of Inka king lists shows the potential of this kind of approach, and his methods could easily be applied to Mesoamerican native historical sources.

3) Scholars need to be more rigorous and systematic in applying social concepts to these sites and to their processes of interaction. I have focused on several of the relevant concepts, including migration, ethnicity, trade, imperialism, and world systems. A key development here is a growing awareness of the importance of explicit models and methods—in both archaeology and art history—in our interpretations of the past.

4) The wider Mesoamerican context of Tula and Chichén Itzá is a crucial factor in any attempt to understand these cities and their interactions. Numerous chapters here make important contributions to this topic, but much more work is needed. It could be argued that the Epiclassic and Early Postclassic periods are among the most poorly understood epochs in the entire Pre-Hispanic Mesoamerican past. Chronologies are problematic, excavated sites are rare, ceramics are not well published, and interpretations are controversial. It's not as if we have a good understanding of this time period elsewhere in Mesoamerica and only need to figure out Tula and Chichén Itzá. We need to work to understand all of Mesoamerica during this key transitional period, and advances in other regions—from highland Guatemala to Michoacán—will help illuminate the two cities of focus here. These may have been the largest and most powerful Mesoamerican cities of their day, but they cannot be understood in isolation.

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# TWIN TOLLANS



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