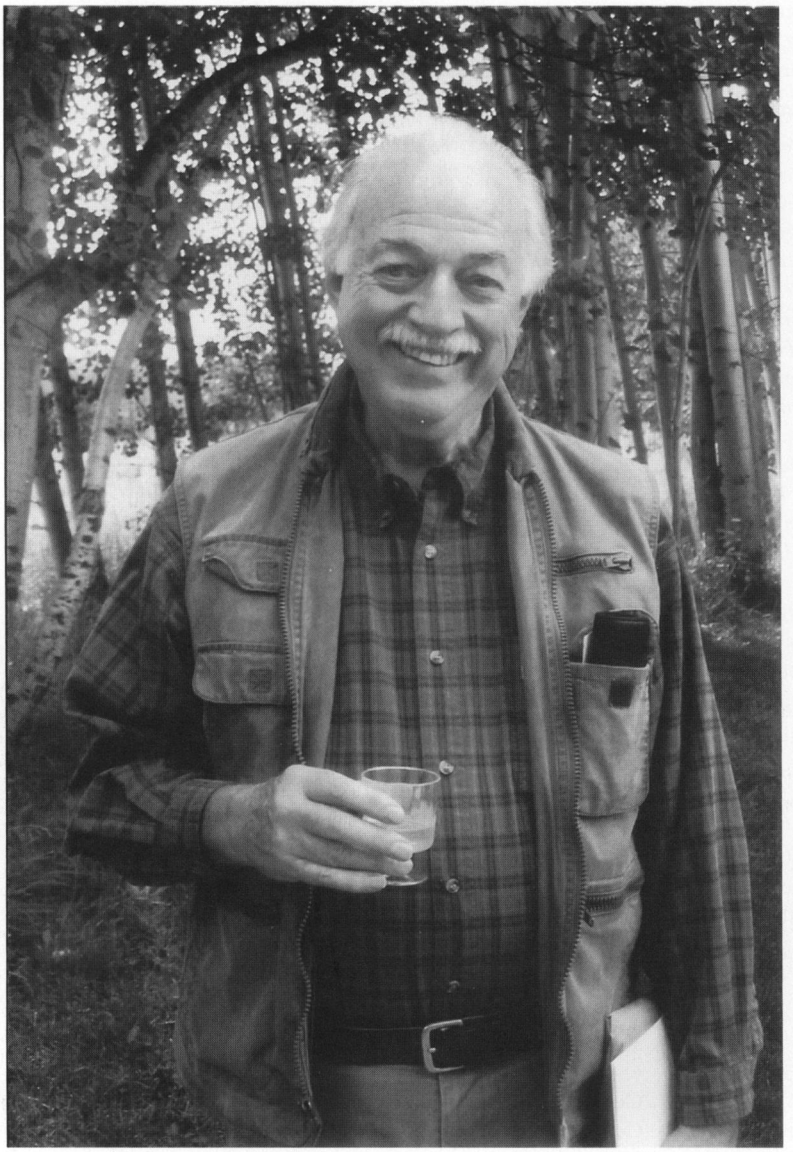

Ancient Mesopotamian Urbanism and Blurred Disciplinary Boundaries

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Abstract

Most would agree that anthropology needs a degree of consensus and structure and, arguably, of "identity" as well. But as a discipline, its boundaries are blurred, with ongoing negotiations along its changing peripheries. Frontiers with history and the humanities are examples. Other examples are in the biological sciences, other social sciences, and public and academic policy. This article follows the form of a 65-year contextualized semiautobiography juxtaposing difficulties and ambiguities that have long characterized archaeological preoccupation with building models out of recoverable, material evidence alongside philological fidelity to the testimony of early literate records. My substantive field is ancient southern Mesopotamia, where the earliest beginnings of both urbanism and literacy can be traced. The challenge is to move beyond little more than mere coexistence toward better articulating "text" and "context" to form a more truly interdisciplinary dialogue. This approach touches on other individualized choice and behavioral boundaries as well.

INTRODUCTION

The end of my training as a Navy radio technician about coincided with the end of World War II, and I was then shortly shipped out to newly accessible Shanghai to await reassignment. That settled into idly waiting aboard a troop-ship, eyeing a great and mysterious city that most of the time lay tantalizingly out of reach. Fortunately, temporary assignments on Navy shore patrols turned out to be an escape hatch to a welcome preview of an unknown corner of the postwar world to come.

But these encounters were a lot more than I had reckoned on. Minimally trying to keep sailors and marines from assaulting one another in bars and dives was the basic U.S. military order of the day, but Shanghai's larger ambience was itself deeply infected with violence, unrest, and decay: islands of opulence in a sea of angry impoverishment; raw political tensions; strikes, demonstrations, pervasive corruption; and no evident urban law of any kind. I was quickly jarred not only out of a voyeur's casual search for the exotic but also out of comfortable thoughts of soon returning to civilian status and ill-defined higher education, with no particular thoughts about a fulfilling later life. Past and prophetic images of China that I had read, including Andre Malraux's *Man's Fate* and Edgar Snow's *Red Star over China*, seemed newly salient again. At 19, was I narrowly conscious only of a city in distress, or perhaps already considering that it might be in the grips of uncertainties and painful choices that would presently face all of China and perhaps the rest of us?

I think it was in a continuing mixture of such uncertainties that I enrolled as a student at the University of Chicago under the G.I. Bill a year or so later. Cities were in themselves powerful generators and attractants. Perhaps they were arenas of change in which the postwar future would be hammered out, but brandishing a pen (let alone a hammer) to secure them was not high on my personal list of future assignments.

Universities, or at least great ones, are almost endless arrays of open doors. I spent considerable time looking into those in Chicago. Approaching cities as a generalized field of thought and involvement, the doors were operationally defined as distinct disciplines of academic departments. But I was not yet thinking of an academic career: If the subject was unitary, was such compartmentalization useful or diversionary? Would it not be better to identify salient historical or contemporary problems and address them with any and all bundles of relevant theory and data?

More or less reluctantly, students considering scholarly careers awaken to the limitations of this direct approach, largely through discovering the more intellectually compelling clarifications that come from rigorous analyses entered only through those narrower entry points. But for some, myself at the time included, the field of action and concern retained some of its original force, with the hold of individual disciplines thus left loosened. In that skeptical spirit, history was most inviting to me at first. William McNeill's (1963) sweeping *Rise of the West: A History of the Human Community* thesis, in particular, captured my interest, implanting a new sense of ascending scales of abstraction and cities as irregularly powerful nodes of change within them.

A geographical disciplinary input, also unsought, was outstandingly articulated in Chicago at that time by Gilbert White. As a human science, it grew out of the complex interactions of groups (or nations, for that matter) making rarely well-thought-out and often disastrous choices about what to do with resources, land, and water. But it had another, more empirical anchoring in real-world technologies and arrays of natural data that could be spun off into unimagined applications. Similarly I encountered powerful social theorists, among them Edward Shils, in a course he gave on social and political violence in early modern Europe. Its larger theme, the epochal transformations in thought carrying the Renaissance into the Enlightenment, did then and has ever since continued to resonate in my reading and imagination.

Expansiveness of scope, rather than disciplinary or methodological narrowness and

systematicity, was the direction toward which I gravitated—not text but context as I now characterize it. And presently there, too, not by prior design, was anthropology. Fred Eggan, whom I later came to regard as the central pillar of Chicago's department of anthropology, laid out an embracing, almost geometric framework within which human diversity fell into a limited number of forms of human association. More marginal and less encompassing but similarly global in scope was prehistorian Robert Braidwood's tracing of the conceptual contours of social evolution that led up to urban civilization. The promise of anthropology as a whole, I gradually came to believe, was not as a defensively bounded domain of its own but as a program of exploration, a hunting license as students were sometimes calling it.

I continue to see anthropology this way, although with increasing diffidence. In those early days, but I am afraid less so now, people in the field were passionately but flexibly experimenting with ideas and freely venturing into unknown conceptual and empirical territories. It was only some years later that I awoke to the need for the accompanying self-discipline of any form of serious scholarship that sees the need for some areas of consensus and hence negotiates constraints. Donald Campbell (1969) once wisely observed that the ideal abstraction of fish scales, a seamless web of individual overlaps in all directions, ignores problems of rigor and systematicity in thought and in how faculties can make judgments about the quality of those who will succeed them.

Braidwood held a joint appointment beyond anthropology, the only one with the Oriental Institute, whose museum and offices were not far away from the social science building. But it tended to be detached and somewhat insular, across a proverbial bridge-too-far (a characterization I take from the failed Allied airborne landing in Holland in World War II, beyond reach of reinforcements) for many colleagues at either end. Yet it was a world-renowned center for the humanistic study of ancient Near Eastern texts and the material and artistic remains that had been excavated along with them. For anthropology in particular, the narrowing of the direct link just to prehistory alone was troubling. The ancient Near East, especially southern Mesopotamia, was thought to be the earliest birthplace of urban civilization. So, simply ignoring the apparent disjunction between prevailing anthropological and humanistic approaches, I also soon enrolled in graduate-level courses in ancient Near Eastern history at the Oriental Institute. That little-studied intersection eventually turned out to be one to which I have devoted a major segment of my academic life. Working also on the University's student newspaper as managing editor tipped me at first toward journalism. (Editor David Broder, soon to become a major national journalist, was a stimulus.) But that was vaguely balanced against my disposition for left-wing student activism that had perhaps originated as early as the Spanish Civil War and was intensified by my experience in China. Dropping out for a year or so to look at heavy industry in that spirit, a Chicago steel mill's swing-shift scheduling conveniently offered little interference with Oriental Institute lectures and seminars. When a position then suddenly opened on Braidwood's pending Iraqi expedition in the fall of 1950 due to an illness and was offered to me, I jumped at it.

The project would continue with excavations at the early village site of Jarmo in the "hilly flanks" of Iraqi Kurdistan, where the introduction and production of pottery occurred within its several-century span of occupation around 7000 BC. Within weeks of leaving-off cutting I-beams on a giant saw, I was supervising the daily progress of skilled Arab pick-men and a local Kurdish workforce. Thus, things were a lot quicker and more informal in those very early stages of the postwar explosion of higher education. (Frederick Barth, before long needing no introduction in the world's anthropological leadership, joined me at Jarmo that year as a fellow graduate student. Transitioning from bio-anthropology into social anthropology, he undertook the study of a local village from which many of our Kurdish workmen came.)

Jarmo was at first regarded as a prime illustration of what V. Gordon Childe had dubbed the Neolithic Revolution covering the introduction of agriculture, although subsequently that site was better dated toward the end of what gradually turned out to be a multifaceted and rather slow-moving (8–10 millennia) process. Most prehistorians had tended to foreshorten this, thinking that they were dealing with an emergent pattern in a small, somehow exceptional region. Later it became clear that no narrow perimeter could be drawn around something so complex and nonlocalized. The most important feature about that season at Jarmo, however, was Braidwood's provision for active collaboration of natural science specialists—in paleo-climatology, paleo-zoology, and paleo-botany—in the field rather than only later studying collected specimens in their home laboratories. This was a hugely promising beginning for a practice that has become almost routine.

But Jarmo certainly was representative, indeed emblematic, of the central place of interdisciplinary science in studies of this kind. Having seen this first hand, however, I parted company with early villages after that single season in favor of a deepening concern with the dynamics of urbanism and its origins. This shift involved focusing on a relatively less-studied span of time from, say, the eighth through the fourth millennia BC, in southern Mesopotamia. For those interested in an overview of the highly productive sequel on agricultural and village origins, Zeder (2009) has written an admirable account of it.

Childe had prominently entered the agricultural origins picture a few decades earlier as a major synthesizer, and he played a no less significant role in focusing interest on the somewhat similar transformation to urban life that followed. (I met him only once, in London as he was disconsolately packing his books for retirement in Australia. Sad as the occasion may have been for him, it was a rich and promising one for me, on my way to Iraq in 1956 for my first season of field surveys of the Mesopotamian alluvial plains. An unparalleled earlier synthesizer of European prehistory and an accompanying believer that many of its stimuli lay in *ex oriente lux* diffusionism, he had turned his attention by the 1930s to the Near East as well. The Neolithic and Urban Revolutions were his overarching characterizations, and widely read works of his such as *Man Makes Himself* (1956) and *What Happened in History* (1964) had a formative role not only for me but indeed in what later came to be the somewhat freestanding subdiscipline of anthropological archaeology.

As both archaeologist and prehistorian, Childe sought to identify and understand temporal or other components of ancient change and their functional linkages in terms of tangible objects and the technological or social achievements they embodied, e.g., the wheel, the plow, palaces and temples, long-distance trade, and writing. *Post hoc ergo propter hoc* was, in effect, his provisional ordering principle for establishing causality. With material criteria and a Marxist outlook at the fore, mature hence unambiguously recognizable outcomes would have to substitute for what more often probably were nonlinear, irregular pathways leading to them. Radiocarbon dating became crucial in his later years and has advanced a long way since. But even today it still leaves us with very speculative linkages as well as irreducible temporal approximations and, hence, usually a long way from convincing causation.

Seldom to be found in isolated material traits are the subtleties of consciousness, the interpersonal synergies commonly at the root of many creative impulses, and more generally the accidents of human capacity, motivation, and circumstance responsible for great advances. Never coming directly to grips with written records, Childe's *What Happened in History* had a formative, boldly plausible role for me and many others. But on later reflection it often fell well short of certainty and sometimes even plausibility.

It is doubtful that Childe's approach a half-century or more ago can take us significantly further now. Advancing further in the (never fully attainable) direction of comprehensive causal understanding is a goal always to be

sought, but it requires frank recognition of the complexity of processes leading to sociocultural change. As we first came clearly to see in the 1950s through the publications of Braudel (1995) and the Annales school, these processes need in particular to be picked apart and analyzed into interacting longer- and shorter-term elements. Stone (1972) also soon sought to demonstrate on a narrower scale how these could be done in a widely noted article on the causes of the British Civil War.

THE CHALLENGE OF COMPLEXITY

Perhaps most succinctly put forward in physics by Nobelist Phillip Anderson's (1972) demonstration that "more is different" was the guiding principle that greater complexity is a decisively transformative consideration. Today it finds its increasingly rewarding content and expression in the interdisciplinary programs of "complexity science" pioneered by another physics Nobelist, Murray Gell-Mann, and at the Santa Fe Institute and also calls on social scientists' model-building propensities and computational skills. But needed with them are other, complementary, approaches to the vast textual resources of mixed qualitative and quantitative data of which (for the present, at least) only the humanities are masters. I return to beat that drum again, but Childe will permanently hold a necessary and honored place along the way hither.

Returning to Chicago from Jarmo in 1951 as a graduate student, I was now committed to a study of early Mesopotamian cities in both anthropology and the Oriental Institute. Archaeology was an important avenue of investigation, but the substantive findings of the cuneiform texts—with limited attention to their decipherment—soon became of fundamental importance for me. This first brought me into rewarding contact with Thorkild Jacobsen, a renowned Sumerologist who had participated in an extended program of the Oriental Institute's 1930s excavations at the city-state of Eshnunna and other, somewhat smaller urban centers in the basin of the Diyala River immediately east of Baghdad. My interest then, at first following his, focused primarily on early third millennium BC textual sources, while of course embedding them in their wider historical context.

Illuminating as this study was, in the end I judged it another bridge-too-far because of limitations on the breadth and informality of Jacobsen's and my communications with one another. He tended, I felt, to be brusquely dismissive of contrary opinions on issues I was becoming aware were in controversy. His gestalt tended to lead him firmly into reconstruction of the divine order as attested in literary texts, which he then took to be accurate, continuing embodiments of an older, preliterate sociopolitical milieu. Coming from both anthropological involvement with evolutionary ideas and the salutary earlier experience of wide-ranging interdisciplinary informality at Jarmo, I was gradually left dissatisfied and uncomfortable with this didactic style.

Early on, I once made the informal suggestion that perhaps I should formally become a student of Sumerology under Jacobsen's mentorship. He discouraged this—wisely, as I later recognized. He was in profound sympathy with the world view his scribal protagonists (most of them anonymous) reflected, projecting what might be simultaneously admired and criticized as a mythopoeic outlook. I was (and remain) decidedly more skeptically detached, inclined to pounce on royal and scribal "myopia" (Adams 2004, 2006b), and less willing, as he once put it, to suspend disbelief. Similarly, he readily assumed that dynastic pronouncements could be interpreted as representative of the prevailing culture and outlook of their time. I was more ready to take consciously modernist positions and see them as self-interested proclamations verging on propaganda (Larsen 1979).

Almost uncannily sensitive to the nuances of ancient words and ideas as Jacobsen was widely acknowledged to be, this combination of deeply intuitive and contrarian traits became regarded by many colleagues collaborating on the early stages of pursuing what ultimately became the

Chicago Assyrian Dictionary (CAD) as a delaying barrier to its progress. Ultimately, Leo Oppenheim, the Editor-in-Charge, received the firm backing of both the CAD's staff and its large and distinguished group of collaborating Assyriologists at other institutions in the United States and Western Europe, and Jacobsen then departed for his final years at Harvard.

I had also come to know Oppenheim well before receiving my degree in 1956. His "Assyriology—Why and How" (1960) was, in my judgment, the most sweepingly perceptive and venturesomely original reconnaissance across that bridge-too-far leading to anthropology and the social sciences. An absorbing, if also deeply involved, view of this painful but crucial turning point was later published by Reiner (2002), Oppenheim's colleague who succeeded him. Now, many years later still, there is a truly comprehensive account by Roth (2010), its final editor, of both what the CAD is and how it came to be, published coincident with the publication of its final volume. It is in no respects controversial to describe the CAD today as an unparalleled humanistic achievement, something not far short of an encyclopedia covering three millennia of greater Mesopotamian history and culture within the greatly relaxed frame of what its users are still content to think of as a dictionary.

In 1962, through a process which I then found almost wholly obscure, I received tenure and became director of the Oriental Institute. Publications of mine, most of them listed in the concluding bibliography of this article, soon began to come out reflecting the mixed influences of active participation in both anthropology and ancient Near Eastern studies. My 1956 anthropology dissertation, "Level and Trend in Early Sumerian Civilization," as its title reflects, juxtaposed holistic characterizations more frequently encountered in older anthropological studies of nonliterate societies than in Assyriological accounts of Mesopotamian city-states. By the early to mid third millennium, the latter were known primarily from their forbiddingly numerous, somewhat stiffly categorized textual records. The oddly inspirational title of a volume that I coedited with then-director of the Oriental Institute, Carl H. Kraeling, *City Invincible: A Symposium of Urbanization and Cultural Development in the Ancient Near East* (Kraeling & Adams 1960) broadly suggested its humanistic affiliations. Yet my contributions (Adams 1960a,b), in it and elsewhere, used terms and pursued goals that tended to place me uncomfortably further in a loosely environmentalist direction than I would not write today in a like setting: "ecological and socio-economic features," "subsistence and environment," and "the evolutionary process."

Having married Ruth Salzman in 1953, by 1955 I had joined Braidwood in holding a joint appointment (as an incoming instructor) along the tenuous link between anthropology and the Oriental Institute. En route, that had been anticipated by my extensive reading of New World archaeological sources, in preparation for giving comparative courses balancing pre-Hispanic New World civilizations against older Old World ones. Another step in preparation had been an opportunity to participate in the 1953 field season of the outstanding team of Carnegie Institution of Washington excavators (the brilliant Tatiana Proskouriakoff among them) at the late Postclassic Mayan city of Mayapan, a walled late pre-Hispanic center for which, in its remarkable preservation, there may be no Mesoamerican counterpart.

For more than a decade I continued to nurse the unrealistic hope of being able to manage not only writing on the onset of civilization in both hemispheres but even carrying on fieldwork in both. A number of such papers did indeed appear, and I finally undertook a book, *The Evolution of Urban Society: Early Mesopotamia and Prehispanic Mexico* (1966). In addition, I carried on settlement-pattern surveys in highland Chiapas, Mexico, within the Department of Anthropology's larger project there until 1961. But soon thereafter, having assumed the burdens not only of the Oriental Institute directorship but also of having become heavily engaged in an evolving program of Mesopotamian fieldwork, that duality became out of the question. Comparative approaches to ancient civilization

became and remain a vigorously pursued subfield within anthropological archaeology. No longer following the subject in detail, I still cannot leave the subject here without noting my admiration for a much more recent, comprehensive exemplar of it by the late Bruce Trigger, *Understanding Early Civilizations: A Comparative Study* (2003; see also Adams 2004).

ARE PACE AND COHERENCE DEFINING FEATURES OF A REVOLUTION?

Returning to the thinly filled conceptual and substantive gap between Childe's Neolithic and Urban Revolutions, this slice of time became my first major concern upon becoming a young faculty member. What were the dynamics behind the seemingly rapid rise of cities on the Mesopotamian plain? Was this process self-generating, cohesive, and bounded in time enough to be usefully described as revolutionary? Can one translate fluid processes into distinguishable, succeeding episodes (I avoid the word stage) or decisively contributing processes leading to the heightened complexity associated with urban, literate civilization? Frankly, I was then and am still uncertain about whether making such judgments is a matter of science or of alchemy.

I encountered particular difficulties in approaching these questions on the Mesopotamian alluvium with traditional archaeological methods alone. Because this area was a land surface built up irregularly by the combinatory processes of irrigation, plow cultivation, periodic flooding, and ongoing aeolian erosion and deposition, most of the relevant evidence was likely to be both obscure and deeply buried. A few early sites dating back into the fourth millennium and earlier had been discovered and excavated. Certainly more would be found, but how many, and how well would they be represented by their surface manifestations? Many of the most promising were likely to be antecedents of later towns and cities that had already been discovered and partly excavated. But apart from being prohibitively slow and costly, relying on merely digging deeper into the latter would lead to the destruction of later urban remains that were certainly of no less importance in themselves. That viewpoint argued strongly for a comprehensive surface reconnaissance—lengthy and quite uncertain as to outcome.

At around this time, to be sure, the same challenge was simultaneously being faced and overcome in other parts of the world, most impressively, indeed paradigmatically, by Gordon R. Willey (1953) in the Viru Valley on the north coast of Peru. Although on a very small scale in comparison with the Mesopotamian alluvial plain, his report, fortuitously for me published in 1953, was a detailed demonstration of the efficacy of systematic surface reconnaissance not only for the basic elucidation of regional settlement patterns but also for their prospective relevance to many other features of society, economy, and political power.

Meanwhile, elsewhere and especially in the United States and Western Europe, rapid and important refinements were under way in new formal models of patterned spatial regularities of archaeological sites. No less significant were changes soon to be under way in technologies of remote sensing and computer-based data management. These were very slow to find their way into Iraqi practice, which was at least partly because of a disposition of foreign archaeologists, feeling that surveys themselves were new and under scrutiny, to keep novelties of practice "under the radar." But my own locational and recording instruments never exceeded the nineteenth-century level of a prismatic compass, three-arm protractor, and pocket telescope.

Also becoming accepted at the time was the mantra of "full coverage" survey, stressing high standards of intensity of coverage with the objective of greatly refining estimates of periods of occupation at newly discovered sites from surface pick-up of potsherds and other index fossils. Very high multiples of (relatively) trained human efforts were an essential and unattainably costly requirement for this practice in Iraq at the time, rapidly reducing the overall spatial

extent of survey coverage per unit of available field time. (It also quickly came into direct conflict with Iraqi restrictions on the retention of samples for later study.) But in any case, my own contextualizing outlook placed higher emphasis on widening the geographic frame of the study. Enlargement of the area covered made both discovery of replication and variation in locational patterning clearer and more significant. The trade-off was for the greater certainty and precision of regional patterning that significantly widened coverage alone could then provide. So I opted for greater inclusivity.

The first season's work in 1956 naturally began in the offices of the Directorate General of Antiquities in Baghdad. In those years, the city was in a phase of amorphous, chaotic growth. There was as yet very little attention to urban infrastructure, nor to amenities that would later receive increasing (and still later, increasingly grotesque) attention as the capital gained unprecedented dominance over every aspect of Iraqi life. In every direction, the outskirts of Baghdad were crowded with squalid, temporary housing of mud brick and even reed matting. Plans to follow the Willey approach depended heavily on finding local cartographic coverage, but that possibility quickly turned out to be disappointing, indeed virtually unusable in most rural areas. Aerial photomosaic coverage had also not yet become available, although foreign advisers were beginning to arrange for its introduction as ambitious programs of planned agricultural development were coming into focus. The northern end of the alluvium around Baghdad, while best represented in available maps, was also most densely populated, and its land surface was badly interrupted by enormous, crisscrossing medieval or later canal levees, which impeded systematic search.

Also missing was vital knowledge of long, accurately dated sequences of ceramic index fossils that would be useful for domestic surface remains. Existing knowledge was spotty at best and most nearly adequate for the fourth through mid-second millennia BC. For what turned out to be the much denser and more extensive remains of Parthian, Sasanian, and Islamic settlement and canal systems several millennia later, it was largely unusable. At another level, there was basic ignorance about the prospective effectiveness and productivity of ground reconnaissance on an alluvial plain known to have been repeatedly disturbed and overlaid with deep deposits of silts. At least in immediate retrospect that first year's results thus seemed disappointingly limited, somewhat delaying publication (Adams 1972), and leaving still in doubt the longer-term productivity of ground reconnaissance of the Mesopotamian alluvium.

Bear in mind that this work nearly six decades ago is by now almost a piece of ancient history itself. There would be still several decades left for significant improvements and corrections before the pace of archaeological research slowed and came to a halt in the harsh later years of Saddam Hussein. This disturbed area south of Baghdad was much more effectively dealt with during that following period, receiving detailed, technically well equipped, and methodologically sophisticated studies by very able Belgian teams under the leadership of Hermann Gasche. Extensively published results now include accurate determination of ancient watercourse sources and their bed levels with borings, forecasting an informative direction that future work will surely take again. A brief, readily accessible introduction to their important findings is available in *Annales Histoire, Sciences sociales* (Gasche et al. 2002).

The Diyala plain east of Baghdad, the object of the second season of survey in 1957–1958, brought major new insights as well as a sense of modern relevance to the entire project (Adams 1965). Under the direct sponsorship of the Iraq Development Board, it was addressed to a convergence of policy-oriented and historical questions. With growing agrarian unrest becoming a major concern, the reopening of its long-abandoned lands through giant new irrigation projects had come to be viewed as a politically palliative measure. Large-scale, capably engineered development plans based on air photo coverage had been prepared, making this important new resource available for

government-sponsored archaeological surveys. The modern dynamic of improved irrigation was once again leading to rapid salinization (Jacobsen & Adams 1958), which now required large systems of deep drains in order to be kept barely in check (or not, if unmaintained and then quickly blocked by dunes). How was it possible that these had no known ancient counterpart? In fact, how productive and stable had the ancient systems been? Jacobsen (1982), in overall charge, undertook to assemble and weigh the ancient textual evidence.

The survey of the Diyala basin was my responsibility (Adams 1965). Much of it had been very lightly occupied since the fall of the Abbasid Caliphate but now was adjacent to volatile and rapidly growing Baghdad. In Sasanian and early Islamic times it was recorded to be a major breadbasket, watered by the great Nahrwan Canal. How populous, productive, and stable had that system and the surrounding region really been? Both texts and surveys shed useful light on these questions. But for the longer-term objectives of the survey, having to confront a major ancient irrigation system, and having vastly improved, air-photo-based maps with which to do so, opened an entirely new chapter of survey possibilities. That potential was even more valuable in itself. Such irrigation networks turned out to have been highly complex and unstable. They were treasuries of new information on demography, the mismatched articulation of local and large-scale bureaucratic management, the exercise of power, and the vital issue of sustainability. The integrative role of levees as arteries of major economic transport unexpectedly turned out at times to have been as important as irrigation agriculture surely was as a *sine qua non* for settled life itself.

SOCIAL AND NATURAL SYSTEMS IN DYNAMIC INTERACTION

As a result of the season in the Diyala (terminated on the eve of the overthrow of the monarchy on July 14, 1958), the layout of larger networks of canals and watercourses became thenceforth a central object of study. And in recognition of these important increments of new knowledge, the decision was easy after this second season to go ahead with further seasons of survey as long as the always fragile opportunity remained open.

Justifying that optimism, progress went ahead even as I became aware that the political situation was radically deteriorating. A major factor was that as one moved further southward into the rapidly widening alluvium there was a corresponding reduction in the burden of overlying silt. And with the Diyala season having demonstrated the promise and effectiveness of alluvial survey, presently other colleagues joined in making roughly similar, independent contributions to the effort. Two early and important additions chose to focus on early urban centers other than Uruk. Wright (1981) undertook a study of a southern lobe of the larger plain, a basin dominated by ancient Ur and also a locus of earlier Eridu, with his 1966 dissertation fieldwork appearing as an appendix in my concluding volume, *Heartland of Cities: Surveys of Ancient Settlement and Land Use on the Central Floodplain of the Euphrates* (Adams 1981). My Oriental Institute colleague McGuire Gibson (1972), with a specific interest in ancient Kish, importantly supplemented my early work in this district east of Babylon (at the same time accommodating my own appendix on the first season's findings). The multiseason work of Hermann Gasche and his colleagues, discussed above, would better be considered as a later, entirely reconceptualized study of the greater environs of Sippar and the dynamics of its twin rivers, in effect initiating an entirely new phase no longer of surface surveys but of combined, multilevel studies.

The surveys as I originally visualized them thus have slipped into the relatively inert compendium of older data selectively drawn on for the advancing frontiers of new scientific approaches. Detailed commentary here on these later improvements and transformations in which I had no direct part would be inappropriate, but I warmly recommend two detailed recent and critical discussions that

accurately place the original survey process in a larger, later context (Pollock 2001, Ur 2012). Both were able to take advantage of another quantum jump in technical capability, based on the gradual emergence (without security restrictions) of satellite imagery beginning in the 1990s, with much higher resolution than aerial photographs.

Very briefly, however, let me mention four major conceptual advances growing out of what has been a wide spectrum of important technological improvements, of which satellite imagery is probably the most central: (*a*) Assyriologist Piotr Steinkeller (2001) undertook to construct from cuneiform sources a mapped system of busy commercial waterways with suggested names of ports and distributaries; (*b*) Tony Wilkinson (2003) offered a powerful, original reminder that our common field of scholarly concern was never merely the loci of archaeological sites but was instead the embracing totality of worked and inhabited landscapes; (*c*) Elizabeth Stone (Stone & Zimansky 2004, Stone 2007b), working currently with advanced DigitalGlobe satellite imagery and very detailed, dated climatic data, is radically expanding the surface areas of scores of older surveyed sites with previously undetected architectural information; and (*d*) Jennifer Pournelle (2007), recognizing the superficiality of the older form of surface identification, has highlighted the virtual certainty that not merely linear, artificial canalization patterns but clearly delineated, natural river courses in the lower alluvium emerged out of marshlands in conjunction with urbanization itself during the entire course of the fourth millennium. All these approaches point the way to the likely renaissance of future landscape themes when archaeological fieldwork in Iraq again becomes possible.

After these many preliminaries, retrospectively and to what extent did those original surveys elucidate an urban revolution as a highly creative segment in a long and unique sequence of human occupation? The following overview naturally tends to proceed somewhat within the scope and design of the fieldwork and its findings. But I hope it also helps to bring forward in a contemporary, interdisciplinary context new research agendas still awaiting us.

Very numerous remains of even quite small settlements that were occupied only during the early fourth millennium BC were found in the southern but not northern alluvial plain from roughly below the latitude of ancient Nippur. It is not unreasonable to assume that this is probably a reflection of a greater proportion being still buried and undiscovered beneath the heavier burden of overlying silt on the upper alluvium.

Some evidence indicates an apparent concentration of still earlier, fifth-millennium Ubaid, sites even further to the south, close to the probable shoreline of the Persian Gulf at that time. This may be a further gradation of the declining silt overburden, but recent discoveries extending far to the south along the western shoreline of the Gulf make it more likely that it reflects a substantially maritime, marshland, and shoreline subsistence pattern of the earliest settlers of the region. That very early pattern does not continue into the fourth millennium but interestingly confirms the existence of fairly sophisticated knowledge of boat building (Carter 2010).

The fourth millennium was the decisive interval during which most of the defining features of early urbanism became manifest. Quite rapidly during its early centuries, a large number—well over 200—of new mostly small settlements made their appearance with a distinctive inventory of Uruk pottery and other artifacts. This probably represented an infilling of the lightly populated alluvial zone from a considerable part of the entire surrounding region rather than a more coherent migration, with the new arrivals having to adapt in as yet undefined ways to different subsistence resources or modes of exploiting them. At least a few towns had already grown to ~50 hectares in size during the Early Uruk period (~4000–3750 BC), but much more common were hamlets and small villages. Spatial aggregations into geometric models based on size were not readily apparent at the time, nor were linear arrays suggestive of more than strictly local

systems of canalization, but may emerge with later surveys.

Ancient Uruk, modern Warka, appears to have been the earliest, and was certainly the largest, early southern Mesopotamian city. Around Uruk, there was a pronounced increase in settlement density that paralleled the very large and sustained growth of the city of Uruk itself, which by late in the fourth millennium reached 200 hectares (Finkbeiner 1991). This growth would imply that a substantial inflow of population was under way not only into Uruk but into its immediate hinterlands. And smaller settlements remained viable in substantial numbers, to judge from textual as well as archaeological sources, for more than another millennium (Adams 2006a,b, pp. 139–42; Steinkeller 2007). Settlements in general could gradually be discerned to lie along networks of interconnected, somewhat canalized watercourses that later began to serve as strategic and economic pathways of intercourse interrupted by intense rivalries and shifts in political control.

Although textual documentation gradually began only in the final phase of this millennium-long transition, and is significantly informative for only the last part of that time, it is an illuminating, indispensable complement to the survey and archaeological data. In the 1970s, Hans Nissen, my colleague and partner on the most important, Uruk-based component of the survey (Adams & Nissen 1972), assembled a very gifted group of linguistic collaborators in Berlin and mounted a major attack on several thousand "ArchäischeTexte" excavated earlier in Uruk itself. In an outstanding tour de force, this group fully traced the transition from pictographic to cuneiform writing. Comprehensive translation only slowly became possible toward the end of this developmental process, but, progressively clarified along the way, began to provide qualitative and quantitative details of subject matter, organization, and patterns of social and especially economic relations and activities (Nissen et al. 1993, Englund 1998). Further reflective of growing complexity, if we look beyond the evidence of writing, were carved stone seals, which appeared earlier but later became integrated with writing in the process of keeping permanent records.

Altogether, we can see here an accelerating social transformation coming to a head at about the end of the Uruk period (~3000 BC). That it can reasonably be viewed as a veritable revolution seems undeniable. "Urban Revolution" is thus surely a suitable name, but was impressive demographic and physical growth its determinant, independently driving characteristic? There had to be major innovations in the amassing and distribution of subsistence and other resources, and presumably also in managing the accompanying large population increase. We will do better (H.J. Nissen, forthcoming manuscript, "Urbanization and Techniques of Communication: The Case of the Rise of the Southern Mesopotamian City of Uruk During the 4th Millennium B.C.") to look for key enablements in these areas, spurred by progress in administration and communications ultimately dependent on advances in the direction of writing, rather than associating causality with simply the most physically massive features we can discover. Somewhere along the line it clearly becomes reasonable, although not necessarily any more explanatory, to characterize this process of development as the rise of what may be the earliest exemplar of the state.

It is regrettable that the archaeological preoccupation with the ceremonial precincts of Uruk itself, amply justified as it seemed by the extraordinary importance of the finds there, was unaccompanied by any parallel investigation of surrounding areas of the rapidly growing city. This disproportion of study leaves us with a picture wholly dominated by accumulating treasures and largely theocratic imagery and with very little hinting at the character of the social pyramid or structures of governance. And in the succeeding Early Dynastic I period there is a still further massive expansion to the northeast of the later city wall of Uruk, adding perhaps another quarter to the settled area (Finkbeiner 1991).

For this later time, however, at the transition to the third millennium, we are also able to begin to draw on the beginnings of cuneiform

writing. It is also a time when there were monumental buildings constituting ceremonial precincts that were both richly appointed and relatively well preserved. And among the earlier still partly pictographic texts were important, so-called lexical lists. Most of these may have been simple records of economic transactions, but others enumerate lists of trees, cities, and sometimes also hierarchically organized lists of individuals headed by an enlarged "master of the club," which may be the prototype for a much later Akkadian term, *sharru* = king. Also in such lists are tripartite divisions of groups suggestive of a ranking of skilled individuals such as craftsmen. Missing in such lists, it may be noted, is anything suggestive of cultic offices (Nissen et al. 1993).

Warfare and bound captives are shown in contemporary, late-fourth-millennium seals and sealings, as are dominant figures associated with both. But distinctively political or religious leading figures are not so identified in the earliest writing. The latter depicts an urban society, 400+ hectares, easily 50,000 or more people, with a highly developed division of labor. Female slaves, in particular, are referred to in a large number of separate instances (not necessarily translatable into a high aggregate number of individuals, although possibly reflecting some elevation in the number of female slaves imported). And as writing begins to come into view as a tool of management, a picture unfolds of an impressive array of specialized sources of subsistence and hence of attendant patterns of activity for care, utilization, harvesting, and distribution of barley, wheat, date palms, and other field and garden crops all dependent on irrigation, a fishing industry, sheep, goats, cattle, donkeys, and pigs, all as fully domesticated animals.

BUT WERE CHIEFDOMS AN INEVITABLE INTERMEDIATE STEP?

This remarkable complexity, closely tied in substance and concurrent with the rapid and precocious origins of writing in the Late Uruk period, is a distinguishing feature of the emergence of urbanism in southern Mesopotamia. Developments in ancient Egypt are now viewed as more or less contemporary and rather similar in content (Woods 2010, pp. 19–23), but there are no other known parallels. As I have earlier proposed in accordance with Anderson's emphasis on complexity as the decisive variable, this leaves me suspecting it is misleading to think of the originative transition to urbanism and civilization as following a unilineal model through a chiefdom as a single, intermediate step.

The presence of frequently contending city-states in the early to mid third millennium should not be construed as indicating a way of life wholly given over to militarized urbanism. Nor does our knowledge of irrigation regimes at the time suggest vast latifundia on the Roman model of primarily slave-conducted monoculture. Population densities are essentially impossible to estimate with any accuracy from survey data alone, but textual sources fully confirm survey findings of widely scattered smaller settlements as well as large areas within formally defined provincial boundaries given over to unmaintained pasturage and marshland, with diverse natural flora and fauna that were informally exploited outside of state control (Adams 2007, 2008; Steinkeller 2007). A selectively closer approach to that other pattern, with large, wholly state-initiated projects, did make an unambiguous appearance only some 3,000 years later in the Parthian, Sasanian, and early Islamic periods, under circumstances suggesting it may have become a strategic choice of investment made by larger, interregional empires of later periods (Adams 1978, 2006a,b).

The progress of the surveys was shadowed by our increasing recognition that political developments were threatening the stability and even survivability of the monarchical regime in Iraq. Even in remote teahouses where we often stayed, Radio Cairo could commonly be heard in the mid- to late 1950s fiercely attacking it. How long was permission likely to be readily forthcoming for foreign investigators to engage in such work? Yet I should note as a mark of the breadth and depth of the internal changes

that have transpired since then that in those under way at the time of which I was aware the strongest forms of organizational protest were seemingly communist affiliated and displayed almost nothing of the overwhelmingly Islamist features that are now present throughout the region a half-century later.

A first, abrupt and unexplained suspension of Iraqi permission was imposed in 1969, a year after completion of the Uruk-Warka phase of our surveys. In the absence of any prior planning, irregularities in the existing survey coverage meant that no comprehensive form of publication could be undertaken at that point. Nor was there any way of knowing if or when stabilization of political conditions might at least allow a more orderly tidying up of loose ends. That led me, reluctantly but with no real alternative, to the choice of a contextualizing, interdisciplinary alternative to Iraqi fieldwork that fortunately was just then made available. I accepted the deanship of the University of Chicago's Division of the Social Sciences.

WIDER RELEVANCE AND CONTEXT

This is as good a point as any to interrupt the primary concerns of this account with my experience within anthropology and on Iraq by taking note of my heavy involvement in responsibilities outside these domains of reference. The term of deanship unleashed a sense of permanent responsibility extending to a cognate field of action that included no narrow disciplinary reference point and then has left that stance in place for another four decades, long into my "retirement."

Details are somewhat superfluous in this setting, some involving board memberships and others more active responsibilities. The list probably should begin with Chicago's affiliated National Opinion Research Center and its Committee on Public Policy Studies, as well as a long-term trusteeship at the Russell Sage Foundation. Reports undertaken on behalf of the American Academy of Arts and Sciences later contributed in one case to long-term federal financing at the initiative of Senator Patrick Moynihan of the Council of American Overseas Research Centers (Adams & Schelling 1979) and in another case to a report associated with the German American Academic Council (Adams 2002b).

By far the largest component involved the National Academy of Sciences (NAS) and its operating arm, the National Research Council. These included National Academy of Sciences Council membership, chairmanship of the Commission on Behavioral and Social Sciences and Education in the National Research Council, its operating arm, and in the NAS Committee on Science, Engineering, and Public Policy, cochair of the (Joint American and Russian Academies of Science) Committee on Conflict and Reconstruction in Multi-Ethnic Societies, and many other committee assignments. My essential point is that all such affiliations tend to be multiple, blurred, and inconstant and can operate successfully only by ignoring the customary boundaries of disciplines.

Blurring of another kind went on also at some indefinite time during my years in Chicago in the 1970s. My ties with comparative civilizations of the Old and New Worlds had gradually grown frail and out of date, and Anthropology at Chicago found better ways to meet its instructional needs there. In fact, I largely dropped out of an identification with archaeology altogether, although I can no longer recall how this was rationalized or accomplished. One key step grew out of conversations with anthropologist-historian George Stocking whose existing course on "Europeans and Others" evolved first by mutual consent into a dialogue between us and then into two independent but related offerings. His subject remained centered in ethnography. Mine, reflecting the important influence of Eric Wolf (see especially Wolf 1982), gradually shifted emphasis toward paths to European hegemony.

Closer to archaeology in at least its origins was an interest I developed in the history of technology. I wrote (Adams 2000) as well as taught on that subject, earlier publishing *Paths of Fire: An Anthropologist's Inquiry into Western*

Technology (1996) after looking into the subject at intervals for a decade while heading the Smithsonian Institution.

The window of opportunity for Iraqi surveys abruptly reopened for a final, very useful season in 1975. There were many indirect indications that this would not be continued, but it allowed for solid gains in coverage as well as for tidying up the ragged perimeters of surveyed areas and hand-drawn maps. This was a very tense time of numerous unreported assassinations vaguely attributed to the security apparatus that had become a source of wide popular concern. It became evident that senior members of the Directorate of Antiquities (especially Fuad Safar, its research director) had fought hard at some personal risk for my permission for this last season, so that leave-taking was also a warm acknowledgment of what had become a kind of collaborative enterprise. I did return sporadically for brief later meetings, the last not long before the Iraqi invasion of Kuwait but not since.

An immediate effect was that two major prospective survey objectives had to be abandoned. The larger one lay to the southeast and included the important province around the ancient Mesopotamian cities of Lagash and Girsu. Both are very well represented in looted collections of cuneiform texts. But the internal layout of this largest of provinces, apart from those two cities, and of all the accompanying watercourses of the time, remains essentially a cipher.

A further effect was the erasing of any hope of completing the survey of the entire alluvium, involving the delineation of the entire courses of the ancient Euphrates and Tigris Rivers across it. One of the Tigris branches, perhaps the most significant one for its involvement in ancient Mesopotamian political and economic history, is extensively documented in Steinkeller's (2001) major article on "New Light on the Hydrology and Topography of Southern Babylonia in the Third Millennium." But in particular, a broad, essentially unsurveyed path of the Tigris extending from Baghdad to the Persian Gulf cannot yet be clarified before about Islamic times. Intensely occupied and cultivated today, and until recently perennially subject to dangerous, destructive flooding, this will be a serious challenge when surveys again become possible.

East of Girsu-Lagash province, across the present course of the lower Tigris, lay the ancient land of Elam. It was watered not by the Tigris but by the Kharkeh and Karun Rivers (see Adams 1962, an initiatory foray in this direction). Hence no attempt has been made here to take account of it, although it involved extensive and technically sophisticated anthropo-archaeological surveys and excavations over many years beginning in the 1960s. Its publication program still continues under the sponsorship of the University of Michigan and the leadership of Henry Wright.

THE SMITHSONIAN: A GREAT INSTITUTIONAL STANDPOINT

Thus Iraq drifted out of my immediate field of view after the mid 1970s. For eight years it became aggressively involved in a savage, mutually destructive war with Iran amid mutual posturing over claims of responsibility. Left behind were uncleared minefields, which are likely to foreclose almost permanently the possibility of resuming surveys of a large border region. But meanwhile, having become provost of the University of Chicago under President Hanna H. Gray in 1982, I was fully engaged in a rewarding look at the complexities of disciplines and their scholarly protagonists from further up the hill of academic administration. Then in 1984, two years into that absorbing term, I was invited by its Regents to succeed S. Dillon Ripley as the Secretary of the Smithsonian Institution in Washington.

This unanticipated invitation was at first painful even to think about. Ruth Salzman Adams and I, already very happily married for more than three decades at that time, had each been independently involved in overseas work and travel, and thus had learned to reconcile ourselves to periods of separation. But this would be for an indefinitely longer period, with heavily absorbing and completely separate responsibilities. Moreover, she had recently

moved from the editorship of the *Bulletin of the Atomic Scientists* to the John D. and Catherine T. MacArthur Foundation, also located in Chicago, becoming director there of a challenging new program on peace and international cooperation, which would mostly keep her in Chicago or on international travel. I could not really imagine what being the Secretary of the Smithsonian would entail, except that everything would be new and nothing routine.

In the end I went to the Smithsonian, and Ruth found more times and good reasons to join me in Washington than we had ever thought possible. Under its wonderfully broad, nineteenth-century charter, the Smithsonian was simply dedicated at its origin to the increase and diffusion of knowledge among men. Dropping the last two words long ago, who could ask for a public institution encouraging a greater latitude of view? The public identification of the Smithsonian is largely of great museums along both sides of the National Mall, but more broadly they are not merely repositories of vast collections of unique cultural and scientific amenities but also centers both devoted to research and engaged in a dialogue with a vast, differentiated public on the dynamic nature and value of knowledge itself. Inevitably the number of Smithsonian museums, collections, and laboratories continues to grow as its subject matter is not merely added to but endlessly transformed. It was a source of particular pleasure to try to represent this diversity and the ferment of fresh ideas and wider understandings coming out of it in my monthly *Smithsonian Magazine* columns. This was my first exposure to how in such open-ended processes of mutual discovery and intercourse these aspirations can flourish within the necessary checks and balances of democratic governance.

The Smithsonian's own governing body, its Board of Regents, is unique, chaired by the Chief Justice (with whom it was a pleasure to lunch privately from time to time in his chambers), and its membership includes the Vice President and a composite of members of Congress and citizens. Every aspect of its program is subject to review, of course, not only by the Regents but by members of the federal bureaucracy with related claims of expertise or discretion, by Congressional committees with budgetary oversight responsibilities, and, by no means least, the general public in all its microdivisions of affiliation and interest. There is a ponderous but necessarily deliberate majesty with which new ideas are debated and sifted before they are implemented.

In interaction with many Native American tribes and their leadership, with key Congressional movers and shakers, and with a number of key private donors, I had much to do with the fact that there is now a new National Museum of the American Indian. Among many other involvements less in public view, I had not a little to do with the fact that the Smithsonian's interests spread from being preponderantly in whole, living (or fossil) organisms and eco-systems to gain a foothold in the scientific revolution of our times in suborganismic biology. A sprinkling of new Bureaus and their architectural expressions have continued to take their appearance when funding can be found. Also having continued since my time is the role of the (nonstatutory) Smithsonian National Board as a responsible public viewpoint and avenue of communications with expounders of public interests and potential private funders. In a flow of continuous, sometimes crucial and more often seemingly isolated and fairly minor decisions, necessarily bureaucratized democracy must also operate under the strains of growing polarization and complexity.

I retired from the Secretaryship in 1994 after a highly instructive decade of discovery of these coupled constraints and opportunities in Washington and all the Smithsonian's outlying theaters of operation. Following a year at the Wissenschaftskolleg in Berlin—our three daughters were widely dispersed with homes and careers of their own—Ruth and I settled permanently in La Jolla (apart from an initially small but growing home in western Colorado), where I became an adjunct professor of anthropology in the University of California, San Diego. Ruth's death in early 2005 after

a brief illness brought to an end more than a half-century during which we had complemented and drawn strength from one another.

CONTEXTS AND TEXTS BELONG TOGETHER!

That was a time of quiet introspection. Because I have already been inclined for years to think of myself informally as a kind of contextualist concentrating on larger views of the interdependence of fields of change with associated phenomena of life, it seemed fitting to return more directly to the still-neglected disciplinary boundary with Assyriology. Fresh discoveries from Iraqi field surveys as well as excavations had been foreclosed for many years by an international invasion and endemic violence. But meanwhile wonderful new remote-sensing and archaeological survey approaches with (to me initially unfamiliar) satellite imagery were well under way in Syria and Turkey by fine teams of archaeological investigators. Most urgent was the prospect of more productive interdisciplinary dialogue between anthropology and Assyriology, which had first brought me into the field of ancient Mesopotamian studies five decades or so earlier.

Leaving behind the fourth-millennium Urban Revolution with the beginnings of literacy only at its end has involved my moving on a millennium and more, into historical rather than prehistoric time into the Third Dynasty of Ur and Old Babylonian periods (~2100–1500 BC). Massively recovered, written records that are more or less fully representative of oral speech having become available (most of them unfortunately looted rather than scientifically excavated over the past century and a half or so) hugely overshadow archaeology as the primary resource and, accordingly, have increasingly engaged my attention for the past decade or two. Yet this work has continued to be consistently and explicitly performed as something of a "contextualist" and generalist (Adams 1988a,b, 2001, 2002a, 2004, 2006a,b, 2007, 2008, 2009, 2010). Perhaps it can best be described as an implicit effort to invite a kind of hypothetical dialogue with others of a more "textualist" persuasion whose primary sense of responsibility is to fully and accurately represent the cuneiform corpus itself.

That, of course, is not the same as claiming that there actually exists a collectivity of card-carrying Assyriologists ready to focus more directly on both textual and archaeological evidence for relatively materialistic realms of human action, association, and behavior rather than concentrating on administrative archives and closed systems of authoritarian control. But among the larger issues that can be raised in this way are the following.

- To what extent were the rising and falling military fortunes of successively dominant dynasties implicated in and responsible for underlying more localized, lower-level changes? Without necessarily (or at least directly) challenging formal dynastic manifestations of power and control, to what extent did local, nonstate governance structures and corporate or extended-family groupings increasingly referred to loosely as households become complementary and sometimes even decisive on procedures of dispute resolution, imposition of exchange rates, effective control of land, and manipulation of markets?
- Did the waxing and waning of a dynast's external political and military powers induce, or instead even sometimes result from and reflect, a shifting internal balance of power with these local, formally subordinate groupings? Although not a dominant trend of thought, at least a scattering of more recent work seems to suggest growing interest in this latter alternative (e.g., Diakonoff 1985, pp. 50–51; Garfinkle 2004, Widell 2008). To what extent, in short, may we have failed to take full cognizance of the ambiguity and fluidity of intermediate bodies? Continuing to weigh against recognition of this, of course, is the firmly assured, authoritarian control that seems to be affirmed by the

very large proportion of both our present textual and archaeological evidence that derives from our own concentration on state installations and archives.

- Why and how did the originally temple- or state-imposed system of corvée labor and allotted rations gradually give way in practice to wage labor supplementing or partly replacing land allotments and rations, in spite of relatively little replacement of terminology?
- Why has it been so difficult to distinguish substantively and terminologically among behaviors toward and by slaves? Clearly, a great deal of variability and ambiguity was tolerated, not least because "there were more different 'ancient' forms of exploitation than just slavery" (Diakonoff 1982, p. 100). But while actual manumission of slaves was apparently rare, the frequently expressed traditional position that slavery was of comparatively minor importance probably needs closer scrutiny. With ongoing multivolume publication of cuneiform archives from Garshana in particular (unfortunately looted rather than properly excavated, and its location still in dispute), we finally have a unique, quantitative record of a large-scale, multiyear construction project, which helps significantly in making this analysis possible (Heimpel 2009, Adams 2010). [As this review is being written, I have in hand a newly published sixth volume on the Garshana archives (Owen 2011) testifying to an unfolding of new horizons yet to be discovered. A penetrating analysis of Akkado-Sumerian bilingualism in written cuneiform (hello, anthropology!) is one of them (Sallaberger 2011), as are also discussions of Garshana's brewery-kitchen-mill complex and its processing, household garrison, 28 identifiable species of trees (some of them in their thousands), crews, cargoes and the loading of boats, craft production and its organization, and much more.]
- These new sources also disclose many details of context and behavior that argue for a reassessment of the roles and responsibilities of women in the labor force (see Adams 2010), in this case the production of leather goods and textiles as well as heavy-duty carrying. Specifying in detail their roles and management responsibilities, these studies implicitly call into question the somewhat simplistic characterization of the larger society itself as patriarchal.
- Why were scribally trained individuals generally considered only as artisans and remunerated as such in formal rankings of professions (Charpin 2010, pp. 22, 97)? Could this apparent lack of unqualified support for scribes merely reflect a lack of scribal self-assertion in a dominating hierarchical structure, or possibly also fear by higher authorities that scribal control of records could be dangerous?
- At Garshana, for example, shabra/scribe Adad-tillati's comprehensive responsibilities in multiple spheres of activity were, in practice, anything but obscure (Garfinkle 2010, pp. 134, 139). They almost seem to define an entirely new, plenipotentiary role, suggestive of his having become something like a four-millennium lineal antecedent of a modern CEO, but there is very little to suggest any comparable augmentation in his status.
- More directly, to what extent could scribal training ever produce either effective critics or active protagonists of a dynastic regime, or simply passive bookkeepers under autocratic surveillance?
- And how much of the terrain, resources, and population within the perimeters of kingdoms was in fact actively managed and accounted for on its elaborate bureaucratic rolls?

These are representative of interesting issues, anomalies, and areas of ignorance that even huge numbers of ordinary state archival texts essentially never address directly. They need to be better understood by asking

different questions from different perspectives. A general feature of the new research horizons along such lines that I would like to envision is not simply that they are interdisciplinary but that boundaries dissolve and joint efforts begin to look for new and unexpected answers. That, in a word, is the main goal of what this and other anthropologists taking the part of contextualists among textualists should be all about.

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