

PENNY DRANSART. *Elemental meanings: symbolic expression in Inka miniature figurines*. (Institute of Latin American Studies Research Paper 40). vi+58 pages, 7 figures. 1995. London: Institute of Latin American Studies; 1-900039-00-1 paperback £4.50 & \$14.

FRANK KOLB. *Lykische Studien 1: Die Siedlungskammer von Kyaneai*. (Asia Minor Studien 9.) iv+207 pages, 31 plates, 83 figures. 1993. Bonn: Rudolf Habelt; 3-7749-2558-5 paperback DM.79.

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YASEMIN TUNA-NÖRLING. *Die Ausgrabungen von Alt-Smyrna und Pitane: Die attisch-schwarzfigurige*

Keramik und der attische Keramikexport nach Kleinasien. (Istanbuler Forschungen 41.) x+230 pages, 48 plates, 31 figures, 9 tables. 1995. Tübingen: Ernst Wasmuth Verlag 3-8030-1762-9 hardback DM.198.

JEAN-CLAUDE GARDIN, MARIA NOVELLA BORGHETTI & IVO MATTOZZI. *L'architettura dei testi storiografici*. 215 pages, 20 figures, 1 table. 1995. Bologna: Cooperativa Libreria Universitaria Editrice Bologna; 88-8091-153-8 paperback L27,000.

EDUARDO WILLIAMS & PHIL C. WEIGAND (ed.). *Arqueología del occidente y norte de Mexico*. 224 pages, 84 illustrations, 4 tables. 1995. Zamora: El Colegio de Michoacán; 968-6959-30-0 paperback.

P.A. MOUNTJOY. *Mycenaean Athens*. (Studies in Mediterranean Archaeology and Literature 127.) 160 pages, 88 illustrations. 1995. Jonsered: Paul Åströms Förlag; 91-7081-073-7 paperback SEK200.

ARVID ANDRÉN. *Tuscania: an Etruscan picture-book*. (Studies in Mediterranean Archaeology and Literature 133.) 64 pages, 27 plates. 1995. Jonsered: Paul Åströms Förlag; 91-7081-100-8 paperback SEK200.

Review articles

Irrigation, raised fields and state management: Wittfogel redux?

KARL W. BUTZER

VERNON L. SCARBOROUGH & BARRY L. ISAAC (ed.). *Economic aspects of water management in the prehispanic new world*. (Research in Economic Anthropology – Supplement 7). xii+471 pages, 125 illustrations, 12 tables. 1993. Greenwich (CT) & London: JAI Press; 1-55938-646-0 hardback £47

PATRICK VINTON KIRCH. *The wet and the dry: irrigation and agricultural intensification in Polynesia*. xxii+385 pages, 113 illustrations, 15 tables. 1994. Chicago (IL) & London: University of Chicago Press; 0-226-4379-3 hardback £39.95 & \$57.50.

The Wittfogel model, like Elvis, refuses to die. And like the impersonators of Elvis Presley who earn their keep by rocking around the clock, Karl Wittfogel's 'hydraulic hypothesis' (Wittfogel 1938; 1957) continues to be repackaged in a variety of guises that assign a unique causal role to irrigation in the development of socio-political complexity. In analogy to the Industrial Revolution, V. Gordon Childe long

ago propagated the concepts of Neolithic and Urban revolutions (see Harris 1994). These were debated, but more importantly, they served to stimulate both archaeological and ethno-historical research of substantial importance. Thus, studies of urbanism revealed that the processes of urban evolution not only were incremental, but that the very nature of urbanism was to some degree unique to particular historical, cultural and ecological contexts. While the term 'Urban Revolution' has not been used for quite some time, the impact of Childe in channeling fresh investigations of historical urbanism has been substantial. Similarly, Childe's Neolithic Revolution set in train broadly conceived empirical research, first into the 'origins', then into the processes of plant and animal domestication. Again there was no universal model, but that no longer is disappointing; it is precisely the variety of alternative pathways to domestication and agricultural subsistence, and the many different social and ecological con-

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texts of early agricultural transformation, that are interesting and informative. The ‘Neolithic Revolution’ is a term now only found in popularizing tracts. That does not discredit Childe, who was a major catalyst for archaeology and other disciplines interested in socio-economic evolution. Acknowledging the role of key players in the history of ideas does not—and should not—require reification of seminal hypotheses. Instead, it is the impact of new ideas in provoking fresh thinking, rather than derivative research, that matters.

Patrick Kirch’s monograph is a good prototype of what I mean by innovative research. He examines two case-studies in detail, to demonstrate divergent socio-economic evolution from similar socio-cultural roots, in different ecological contexts. Futuna and Alofi are relatively isolated islands in western Polynesia, where Kirch combined an ‘ethnography of agricultural practice’ with archaeological investigation to study agricultural change over the long term. He focused not just on irrigation but on the full range of agricultural pursuits, including short-fallow dry-farming and arboriculture. He traces these components and their evolution, in contrasting wet and dry micro-ecologies, to draw them into the dynamics of competitive social relationships as the islanders attempted to cope with finite resources and growing populations, by some groups acquiring hegemonic control over a more diversified economy. Intensification is the critical process, and it is intensification that pulls agricultural production above the level of demographic and caloric necessity, to meet the requirements of social exchange or irrigated taro and pigs for quality yams at feasts.

The archaeology shows that pond-field irrigation developed fairly late, in conjunction with demographic growth, upland degradation and innovation. The lack of Proto-Oceanic or Proto-Polynesian words for irrigation features supports a post-dispersal and local development, despite some linguistic cognates between adjacent island groups. In dry-farming areas, field walls and alignments suggest short-fallow cropping and concern for permanent demarcation of plot boundaries, while fortifications and architectural symbols of status and power argue for internal competition. The dry-farming and irrigation chiefdoms responded very differently to the pressures of population increase and of the social demand for surplus—to supply the cycles of competitive exchange and feasting. These divergent pathways to intensification served to accentuate the differences between socio-political structures, this process emerging as a particularistic and ecologically-grounded means to an end.

In his concluding chapters, Kirch compares alternative pathways and environmental histories to intensification in protohistoric Hawaii, and on Mangaia and Tikopia (based on his own research),

with comparative notes on other island groups. He remarks that the evidence ‘invites us to turn the “hydraulic” hypothesis on its head’ (p. 320). On islands where irrigation was practised, the direction of socio-political evolution was towards a fluid political dynamic, rather than increasing social stratification. In fact, the impetus to political elaboration and hegemonic expansion arose in those areas where irrigation was absent or insignificant. Instead of a tight, causal feedback loop between irrigation and socio-political complexity in the various Polynesian island groups, it was intensified dry-farming that provided the dynamic tensions between environmental constraints and the social relations of production that ultimately spawned the most complex societies of the Pacific Ocean domain.

The key to Kirch’s effectiveness lies in over 25 years of comparative experience, his training in biology and allied aspects of natural history, his close collaboration with specialists and his proclivity to focus on agro-ecological systems as a whole. That leads to sophisticated evaluations of complex interrelations, in contrast to the naïve introduction of irrigation as an afterthought to a standard, uni-disciplinary excavation project.

It would miss the point to construe Kirch’s oceanic findings as a model for intensification *versus* social complexity in other world regions—with very different cultural repertoires, time depths, ecological opportunities/constraints and spatial parameters or 100 (no covers) investigative mode that should provide a model for better research, that will yield genuine understanding.

Vernon Scarborough’s & Barry Isaac’s edited volume on *Water management in the prehispanic New World* has different goals in that it presents eight different examples, each chapter testing its own hypotheses. Scarborough’s neo-Wittfogelian leanings aside, the collection breaks new ground in that only two deal exclusively with canal irrigation, the others elucidating ‘still-water’ systems, i.e. reservoirs, lake margins and reclamation of wetlands. Canalization can be reviewed first.

The great Hohokam canal network on the Salt River (near Phoenix, Arizona) is systematically re-examined by Jerry Howard, as to the internal chronology of its components, and the labour efforts represented by canal rebuilding or expansion over time, c. AD 600–1450. As a result of channel deepening, lower mean flow and repeated high-magnitude events after 1200 AD, labour investment almost doubled, with only modest increases in irrigated land, until destruction of the irrigation works by a catastrophic flood in 1358 probably meant rapidly declining canal discharge and led to widespread local abandonment. Indeed, I personally have identified flood deposits closing up one of the key canals. There are no hard data to prove the point but the ecologi-

cal crisis may have eventually required a centrally-organized, large-scale mobilization of labour, i.e. a higher level of political control and integration, at least according to the current model of socio-political complexity.

A companion piece is the case of the Moche and Chicama valleys of coastal Peru, studied by C.R. Ortloff. Here repeated coastal uplift progressively led to channel entrenchment, so that irrigation canals had to draw water from higher-lying 'heads', located ever further back in the foothills. Canals also had to be protected from El Niño floods, although rainfall trended downward *c.* 1000–1300 AD. Eventually the Intervalley Canal was devised as a last resort to draw water from one valley across to the next *via* a 74-km canal. That would have required massive labour investments *c.* AD 1100–1200, in addition to great engineering sophistication. The project was never fully completed. Ortloff offers a circumstantial argument that the large-scale planning in anticipation of changing circumstances, as well as the labour mobilization required, reflect a creative and effective bureaucracy that managed water policy across several centuries with a good grasp of complex environmental and engineering problems. Yet with the exception of the Intervalley Canal, that may well represent state intervention, the role of the state may otherwise have been limited to providing labour or capital (Netherly 1984). Further, the juxtaposition of the state and labour-intensive irrigation works does not demonstrate causality (Schaedel 1987). In ancient Egypt, massive labour conscription is verified in the building of royal monuments, and the pharaoh symbolically guaranteed the annual flood cycle; but an adequate written record going back almost 5000 years (and confirmed by 19th-century ethnohistorical sources) shows that there simply was no managerial bureaucracy controlling water resources (Butzer 1976).

Two chapters address the undulating karstic plains of the southern Maya Lowlands, in Guatemala and Belize, where irregular and interconnected concavities (*bajos*) harbour seasonal or perennial wetlands. Scarborough's paper on a variety of monumental sites (*c.* 300 BC–AD 900) in the region documents various drainage (rather than irrigation) canals, reservoirs and raised causeways that are hardly surprising within settlement or ceremonial precincts. I have, however, walked along various 'canals' at Edzna and, with one possible exception, they are a result of karstic solution along major joints (large-scale *grikes*), with irregular and often reversed gradients. Canal irrigation has, in my mind, not been demonstrated in this region, where wetland manipulation is the central issue. Peter Harrison attempts an overview of the clustered patterns of ditched fields but does little to clarify either the several functional types of narrow 'ditches' or broad 'canals' implied, or the inter-

pretation of sediments within them, or at and below the 'raised fields' between them. Wetland manipulation is not generally in doubt, but divergent observations or interpretations must be resolved on the ground, rather than by academic critiques aimed at the mainly unpublished, research reports of 'the soil scientists' since 1990. The fine geo-archaeological study of Jacob (1995) provides a model for what is really needed.

Deborah Nichols & Charles Frederick are on more solid ground in identifying similar *chinampas* in former Lake Xaltocan, in the northern Basin of Mexico. Two exploratory trenches suggest that the very shallow 'canals' (60 cm) perhaps were wider than the raised, planting surfaces. The latter were less than 40 cm thick, resting on a weak palaeosol (formed in lake beds that are saline at depth), and include a lower, coarser base under an organic, clayey horizon. The planting soil is texturally similar to both the organic 'canal' fill and the less organic palaeosol, but there also is admixture with the immediate substrate. To me, this suggests a particularistic solution to very local conditions: the 'canals' may have been excavated to considerable width simply to provide sufficient sediment for a planting surface; their shallowness may have been dictated by salt accumulation at greater depth; the sandier substrate of the planting surface would have facilitated lateral percolation to the root zone, although sandy loams are not present locally and would have had to be transported from some distance. The palaeosol and the base of the 'canal' fill are both dated about 2200 b.p., but the oldest sherds in the fill are Early Aztec (perhaps AD 1400). This tantalizing example should caution the generalizers that the micro-environment, planning goals, function, maintenance and evolutionary change of 'raised fields' are highly likely to differ from one location to another. With the modesty and small scale of the Xaltocan *chinampas* (100–200 ha), it is also apparent that wetland reclamation can indeed be carried out by local communities, with the benefit of new, diffusing information.

The pompous title of 'large-scale hydraulic works' in western Mexico by Phil Weigand masks the fact that this author's key examples are dubious. His 800 ha of *chinampas* in the Laguna de Magdalena form an absolutely rectangular grid that, on the basis of my own observations could only have been excavated by modern machinery — to provide a 'drainage field' for the local sewage plant. Surface sherds of Classic affiliation do not prove *chinampas*. The 'Teuchitlan–Ahualulco–Tala hydraulic system' is little more than a grandiose theoretical construct. The area was not noted for its marshiness by Colonial era observers, but the great dam impoundment does create such an impression today. There are, however, small tracts of extant *chinampas* (of irregular shape), while

clear-cut 'canal' profiles that suggest *chinampa* blocks have been verified on higher ground. If anything, this article should summon archaeologists to a more responsible, geo-archaeological programme to study *chinampas* and other forms of wetland reclamation in various settings. The exploratory observations on such features in the Basin of Mexico in 1945 or in the Bolivian Amazon in 1970 were innovative and commendable. But far more intensive research is essential today, especially if there is to be a foundation for high population arguments and inferences about the organization of labour.

Fortunately, the 'raised fields' discussed from the Lake Titicaca Basin (Peru) by Clark Erickson are quite concrete, based on empirical or experimental data, while the author's cogent social inferences allow a return from taxonomy to process and theory. Convergent lines of evidence identify two phases of construction and use, the first (with small-wavelength fields, geometric but irregular) dating *c.* 1000 BC–AD 300, the second (with larger fields) *c.* AD 1000–1475. These periods do not coincide with population stress or state presence; on the contrary, the tracts of raised fields were partly abandoned during the two eras of higher political organization (Tiwanaku and Inca, AD 600–1000 and 1476–1534 respectively). Individual bundles of fields are roughly the size that a single household of five today can construct and manage in a year, and that provide the necessary caloric productivity (in potatoes) to support such a household. Further, intraregional diversity in ancient field forms may represent local community expressions of style. Finally, Mesoamerican (and Andean) rulers saw their role as custodians of the cosmic balance and as agents in the politics of dynasties and polities, rather than as rationalizing managers of local production. Today, the intrusion of the state in traditional local systems is disruptive for water distribution, and creates tensions and local resistance by favouring local élites. Indeed, the basis for water management among Andean communities is based on ritual-symbolic traditions of water, earth and mountain worship, reciprocal labour exchange, rotating offices of water officials and local organizations (p. 376). On the other hand, 'raised field' systems can function successfully with local or regional co-ordination, unlike medium or large-scale irrigation systems, and they are less labour intensive.

The concluding chapter by Barry Isaac begins with a lengthy discourse of Marx and later Marxian ideas on the 'Asiatic mode of production', and concludes by ridiculing Wittfogel as a bigoted and shoddy scholar and an evil Cold War warrior. That calls for a rejoinder. During his academically formative years, 'Westerners' (still politically incorrect) wondered why the great civilizations of Asia had not developed 'progressive', capitalistic and industrial societies,

with the exception of Japan. Without belabouring the dichotomy between industrialized and developing nations that persists in academic discourse today, it was not unreasonable for Wittfogel to seek to understand the divergent evolution of 'East' and 'West' (terms appearing in quotes in his German writings). He was very much a product of his times, and a respected sinologist, not a fusion of Marx and Kipling. Unlike Childe, Wittfogel did suffer for his beliefs and principles, spending hellish months in a Nazi concentration camp in 1933–34, and articulating his deep distress at human suffering in a haunting essay on 'thirty women standing at the concentration camp gate' (see Ulmen 1978 bibliography under '1935'). That may help to understand his outrage at the Stalinist *gulags*, and perhaps place his outrageous, victimizing testimony on Owen Lattimore to the McCarran Committee into some sort of perspective.

Wittfogel's thesis can best be overviewed in an early summation (Wittfogel 1938), that is less complicated by tangential arguments and his anguish with the terrors of totalitarianism. He saw himself as an inductive student of social process, rather than a dogmatic, Marxist historian; he condemned ontogeny (the later Model-T of Mesoamerican archaeology), because the organismic view of history neglected the specifically human character of the problems of human society. Instead, he proposed a multilinear theory of cultural evolution, grounded in Marxist concepts, but elucidated by the empirical scholarship of his day. A 'western' trajectory had led to industrial capitalism, primarily because authority there was traditionally decentralized. Another, 'eastern' trajectory saw the emergence of highly centralized societies, controlled by an autocratic sovereign and a powerful priestly-bureaucratic caste, that employed astronomy to predict nature's cycles. For the purposes of flood protection and a good water supply, they mobilized massive labour forces to build monumental dykes and canals, that assured high productivity as well as managerial control over their populations. As the empowered sought to perpetuate the economic and technological order that supported them (hypercoherence!), such societies tended to economic stagnation rather than innovation. For 1938, these were prescient and innovative ideas, if we are not sidetracked by the Marxist framework.

Given the intellectual poverty of evolutionary-theory-as-typology during the 1960s, some social scientists pounced on the notion of irrigation as the 'engine' of history, resulting in a welcome spate of ethnographic and archaeological field studies. A long list of cross-cultural studies revealed that, despite some level of co-ordination, locally-based socio-political organizations managed water-related problems in an un-centralized and non-hierarchical fashion (Hunt 1988). Furthermore, differences in

managerial practices reflect particular cultural heritages at least as much as they do particular environmental contexts (Kraus 1992). The hang-up seems to be the tenacious assumption that early forms of intensification were a result of socio-hierarchical demands (Steward 1955, reflecting Wittfogel's influence), rather than cumulative, small-scale, local decision-making. Obsessed with the vertical dimensions of socio-political complexity, we have neglected to give horizontal structures their proper due.

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Yes, Wittfogel's method and his positions are dead, and they should be buried. But as in the case of Childe, we must acknowledge him as an innovative thinker in his day. He stimulated a suite of excellent studies on irrigation that eventually rendered his hypothesis obsolete. Both volumes discussed here imply that the underlying issues need reformulation, so that empirical research can be more sharply focused. If and when we can turn that corner, and stop rehashing Wittfogel, flowers will indeed be appropriate.

Apes and ancestors

STEPHANIE MOSER*

R. CORBEY & B. THEUNISSEN (ed.). *Ape, man, apeman: changing views since 1600*. 408 pages, 42 figures. 1995. Leiden: Department of Prehistory, Leiden University; 90-73368-05-7.

Corbey & Theunissen's edited volume marks an important development in the study of human ancestry. While human-origins research has long boasted of having an interdisciplinary approach, this volume represents a different kind of interdisciplinarianism. It marks the move into the realm of history of science, which is an area where few palaeoanthropologists have ventured (*cf.* Hammond 1982; Landau 1991; Spencer & Langham 1990). *Ape, man, apeman* not only provides an interdisciplinary approach to questions concerning the historical study of apes and hominid ancestors, it firmly locates the study of human origins in traditions of historical understanding and scholarship. The book is the result of the coming together of a wide range of scholars with diverse interests regarding the interpretation of apes and ancestors. If there is one unifying theme, it is the vexed and age-old question of the status and relation of apes to humans, and the obsession with

the idea of a missing link. One of the most significant achievements of the volume is that it does not start with, or only deal with, the history of academic study regarding apes and ancestors; it demonstrates something we have been avoiding for far too long, which is that contemporary scientific research on primates and hominids draws heavily on interpretative frameworks developed since classical times.

The volume begins with the acknowledgement that apes and monkeys, more than any other animals, had a profound impact on the understanding of human status. As Corbey notes, 'Nowhere in the animal, or not-so-animal, world, it would seem is the emotional involvement of our species, studying its own ancestry and next of kin, so great' (p. 3). The book is then structured according to four major sections. The first, 'Interpreting Apes', deals with the ways in which apes have been understood, described and studied since classical times. Beyond clarifying the terminological confusion regarding non-human primates, the authors provide important insights into the major frameworks through which the status of the apes has been perceived prior to their scientific understanding. Starting with percep-

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