GLASS HOUSES UNDER THE ROCKS: A REPLY TO WATSON

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In late June 1986, I was enjoying a pint with several faculty and postgraduate students after presenting a seminar at the Cambridge Department of Archaeology. The seminar, entitled "The advent of food production in West Africa: reconsidering processes of culture change" was based on my recently completed doctoral dissertation which focused on what I then termed the Kintampo culture (contrary to Watson ***p. 4***, both ANQUANDAH [1993] and I [STAHL 1993] referred to this as the Kintampo *complex* by the time of the Southampton World Congress in 1986). During the course of conversation, someone asked why American scholars so often frame research in relation to "straw man" arguments. Today (having recently celebrated the half century mark) the details of the conversation have dimmed; however, the comment stuck with me over the years. At the time I thought I had been careful to avoid caricaturing the arguments of others to highlight the points I wished to make, but that Cambridge pub conversation convinced me to pay closer attention.

I was transported back to that pub as I read Derek Watson's "Under the Rocks: Reconsidering the Origin of the Kintampo Tradition and the Development of Food Production in the Savanna-Forest/Forest of West Africa", and find it not a little ironic that two decades later, the straw (wo)man seems to be me. A considerable portion of the paper is devoted to a critique of my research. Watson finds virtually all of my interpretations and many of my analytical approaches wanting.

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Let me be clear; a quarter century on, I would do a number of things differently than I did in 1982, particularly in dealing with questions of site formation and ceramic analysis. I welcome the opportunity to revisit some of these issues and am delighted that Derek Watson has located new sites and reopened discussion of the Kintampo complex. A number of his critiques are valid; yet in his zeal to forward a case for migration, Watson has ignored or underplayed aspects of the Kintampo literature, rejected insights based on reasonably sound evidence (some of which, including the notion of garden hunting, are not necessarily incompatible with his view of Kintampo peoples as agro-pastoralists), at the same time as he embraces other interpretations based on much slimmer evidence. In adversarial style, he caricaturizes earlier work (supported at times by disingenuously edited quotes) as a prelude to forwarding an alternative perspective that ironically dismisses all other alternatives. While one can appreciate the rigor of some of his reassessments, the same rigor is lacking in other dimensions of his argument. I focus here on what I see as several imbalances in interpretation/argument and conclude by commenting on several broader issues.

When I began doctoral research in 1982, the Kintampo complex was viewed as an intrusive phenomenon, though there were disagreements over its source. Oliver DAVIES, who first defined the Kintampo neolithic or culture (he used both terms) from surface materials, believed it to be "an intruder of Sudanic origin, overlying and borrowing from a local neolithic" (DAVIES 1962: 301). When in 1980 DAVIES published a description of his 1962-63 excavations at Ntereso near the White Volta River, he emphasized numerous parallels with sites in the Sudanic region, and more particularly with sites along the Niger. He argued that red-painted pottery, bifacial arrow points, and bone and ivory fishing equipment derived from the north (DAVIES 1980: 220-221), but suggested that other aspects of the material repertoire at Ntereso derived from the south, including "the coarse pottery and its commonest decoration [comb impression], the 'cigars', most of the stone axes, the shale bracelets, and apparently the quartz microliths" (Davies 1980: 220). Davies' comparisons were casual and sometimes drawn with northern sites that postdated Kintampo complex sites (*e.g.*, Kobadi, Davies 1980: 208-209). He generally linked the appearance of northern traits to a desiccating Sahara, but in his later publications was not specific about process, and seems to have entertained notions of migration alongside a diffusion of traits (Davies 1980: 220). Flight (1976) too saw the Kintampo culture as intrusive, and explicitly posited a migration scenario. His (and Rahtz's) excavations at several rockshelters in the 1960s provided the first stratified sequence of Kintampo materials in relation to an earlier ceramic late stone occupation (Punpun). Flight argued that marked discontinuities between the lower and upper strata of the rockshelter signaled population replacement. He observed, "I have no special liking for hypotheses of invasion, but in this case the differences are so pronounced in almost every respect that no other explanation seems possible" (Flight 1976: 216). Yet Flight did not see connections with the southern Sahara, suggesting instead that the Kintampo culture originated to the west, perhaps in Côte d'Ivoire (Flight 1976: 219-220). I returned to K6 rockshelter in 1982 in the hope of generating a stratified sequence of floral and faunal materials in order to assess the character of Kintampo subsistence (as an aside, the question of origins was not central to my original research proposal). Though our excavations at K6 were confined to a small area of intact deposits that remained after Flight's extensive excavations, the stratigraphic distribution of materials undermined the notion of a clear break between Punpun and Kintampo occupations of the rockshelter.

Stahl's arguments and Watson's objections

A brief summary of my arguments and Watson's critiques may help readers to understand the key issues in this debate. Based on my analysis of materials recovered during our 1982 excavations, I argued that: **1)** materials considered diagnostic of the Punpun phase and Kintampo complex (ceramics, struck quartz, shell beads, and organic remains) showed stratigraphic overlap, a pattern inconsistent with a model of population replacement (Stahl 1986: 533); Watson agrees that there is stratigraphic overlap, but argues that the overlap is the product of two distinct populations pursuing different subsistence strategies intermittently occupying the Kintampo inselbergs. I also argued based on stratigraphic distribution of materials at K6, that **2)** not all features considered diagnostic of the Kintampo culture appeared simultaneously. This was the basis of my claim that change across the range of material culture was not synchronous (Stahl 1985b: 144). Watson vehemently disagrees, citing evidence from his Boase excavations that all diagnostic elements are represented throughout the sequence, suggesting that Kintampo appeared 'full blown' at the site. This is a corollary of his migrationist argument, one that informs his insistence that Kintampo migrants came with their cultural arsenal intact. With respect to subsistence, I argued that **3)** because the bulk of the identifiable faunal remains from K6 derived from wild species, hunting was an important element of Kintampo subsistence, despite the presence of a domestic caprine and possible involvement in gardening. I interpreted this as a sign of continuity between Punpun and Kintampo occupations, though I suggested that the changing composition of wild fauna (toward species attracted to cleared areas) was consistent with adjustments in hunting strategies toward garden hunting (along with other adjustments in seasonal resource exploitation; Stahl 1985a: 241-242, 1985b: 143-144). Watson agrees that wild fauna continue to be associated with Kintampo materials, but does not see these as signaling continuity, arguing on the one hand that hunting is characteristic of many West African societies, and on the other, that we cannot sort out which of his two purported groups (hunter-gatherers or agro-pastoralists) generated these remains. A similar argument pertains to floral remains. Where I argue for continuity in exploitation of oil palm and *Celtis* across the so-called Punpun-Kintampo transition, he thinks not, for the same reasons. And **4)** I explored the implications of what I termed 'decreasing residential mobility' (Stahl 1985b: 144, 1993: 266) for subsistence production, food processing and storage, as well as social networks. Watson objects to this term, presumably because it implies a *process* relative to the greater mobility of populations that preceded the Kintampo complex, who he sees as separate populations pursuing a distinctive lifestyle. Yet Watson (***p. 20***) seems to mistake the argument for decreased residential mobility as synonymous with a claim for full sedentism. A variety of authors have made a sound case for *a degree* of sedentism based on evidence for durable structures in the form of stone foundations at some Kintampo sites (Dombrowski 1976; Anquandah 1993), burned daub at others (Flight 1976: 217; Stahl 1985b: 138), alongside the relative abundance of durable material culture, among other factors (Stahl 1993: 266-267). Watson criticizes the notion that