



Rejoinder to Henry Munson, Jr., "On the Irrelevance of the Segmentary Lineage Model in the Moroccan Rif"

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American Indians After 1492: Reply to Jeffrey M. Mitchem

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I have no quarrel with Mitchem's statements about the date of the European artifacts recovered at Hontoon Island. The European artifacts, with one exception, were all recovered from Zone II (see Figure 1). The average of eight dates received for Zone II is ca. A.D. 1535. The range to two sigmas (95%) of this date, however, is incredibly broad. The dramatic changes we documented at Hontoon Island did not occur in the same stratum as the appearance of the European artifacts but in a lower stratum (Zone III) in which we have now recorded modifications in all seven major categories of material items. The death knell at Hontoon Island was already ringing when the European artifacts were deposited. The stimuli for change had occurred earlier.

As for what Mitchem considers my neglect of ethnographic sources, in the 22 years I have been in Florida I have read every one of the accounts he cites, and many more. I did not cite them because they did not seem applicable to the situation at Hontoon, especially the reference to the missions. I am convinced that Hontoon Island was not occupied during the mission period. Perhaps the last few of Hontoon's inhabitants were forced to move to a nearby mission as soon as it was constructed. In 1520, when de Ayllón was on his way back to Hispaniola from the present-day Carolinas, he encountered three Indians at sea. To make his escape from Santo Domingo, one Indian had cut down the trunk of a tree, worked it into a beam, provided himself with maize and gourds containing water, and set sail with his wife and a friend. The Spaniards returned the Indians to Hispaniola (Kerrigan 1951:5), but others may have been more successful in fleeing a devastating situation.

Mitchem mentions that citations were missing from *References Cited* in my article. The two that were not included were both personal communication. His comments about the source of the rectilinear guilloche design are well taken. The problem of the origin of the design is still a mystery. I have now seen it on

specimens from West Africa, Copán in Honduras, and Costa Rica, all dating to the Age of Exploration!

Mitchem asks whether or not the Southern Cult is a prehistoric or early historic manifestation in the Southeast. Frankly, I do not know. But then I do not believe that anyone else really knows either. He says my references are outdated and that people no longer refer to the Mississippian Period ceremonialism as the Southern Cult; on this matter, please see Fagan (1989) and Jennings (1989). Mitchem mentions a 1976 "recent" reference that shows conclusively that the designs are late prehistoric. In an article of mine that will appear in a forthcoming issue of *Southeastern Archaeology*, I address the problems of archeological dates that fall within the last five or six hundred years. When radiocarbon ages are calibrated to dendrochronology, normalized to C₁₃; when the ranges to two sigmas are taken into consideration; and when combinations of errors due to humans, instruments, or nature are calculated, it is difficult to determine if the time period is prehistoric, protohistoric, or historic.

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Rejoinder to Henry Munson, Jr., "On the Irrelevance of the Segmentary Lineage Model in the Moroccan Rif"

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The fact that Henry Munson's article, "On the Irrelevance of the Segmentary Lineage Model in the Moroccan Rif" (*AA* 91:386-400, 1989), has been brought to our attention forces us to give him a part-answer, if not a true rejoinder,¹ one that by its very nature must be couched in an "either/or" form:

Either (1) a segmentary lineage system and ideology *did* exist in the precolonial Rif, de-

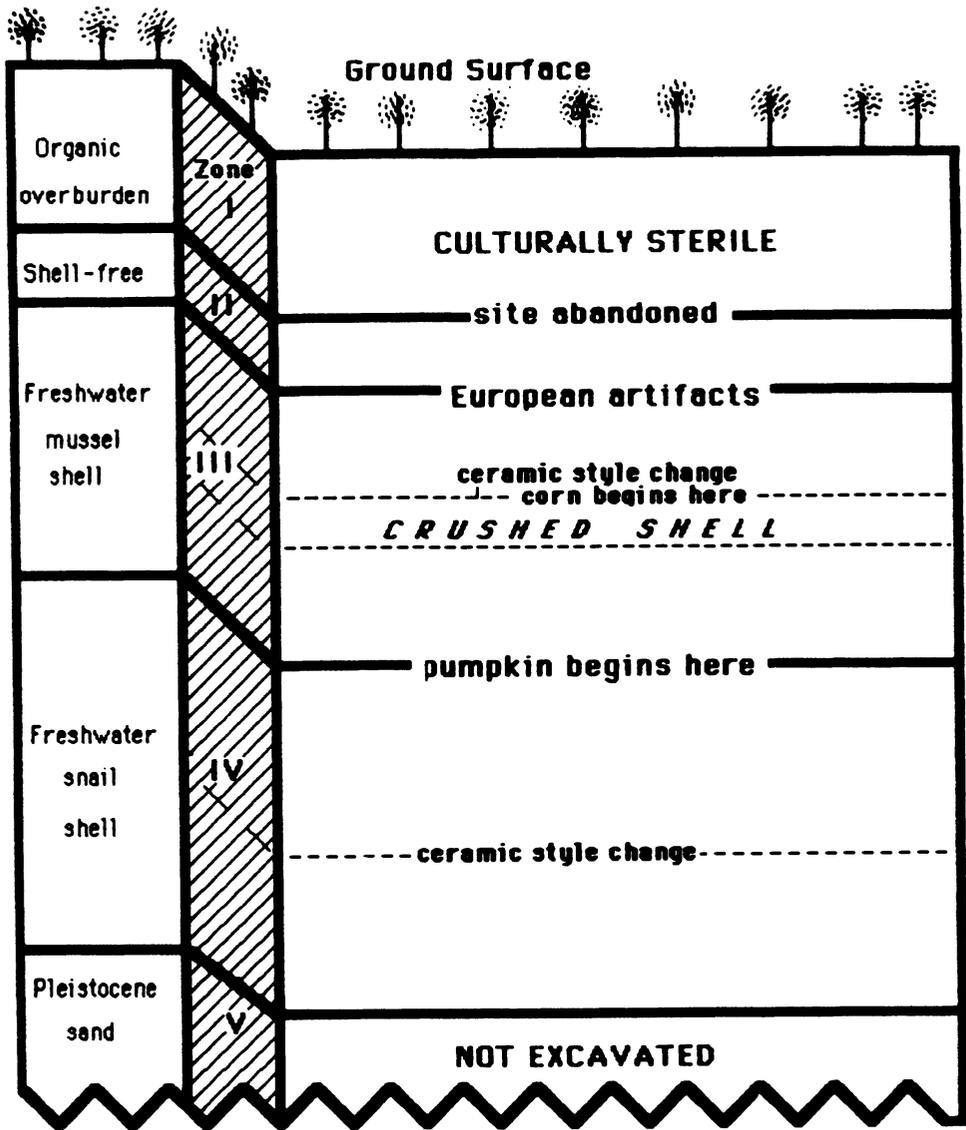


Figure 1
Stratigraphy at Hontoon Island, 1988.

spite Munson's asseverations, and operated there at very considerable variance with the territorial, *liff* alliance and *haqq* fine systems, exactly as all of these are described in *The Aith Waryaghar of the Moroccan Rif* (Hart 1976). This is the case simply because the lowest, or least, common denominator of the society was neither the agnatic lineage nor even the nuclear family, but the individual—who nonetheless existed and lived out his or her life within the context of that same family and lineage.

Or (2) a segmentary lineage system did *not* exist in the Rif—as Munson attempts to show through using our own materials—and was in fact “irrelevant,” as he claims, despite the segmentary ideology prevailing in the region.

Now, this “either/or” factor depends almost entirely on the ethnographer's point of view. “Classical” segmentary lineage theory and the pervasive issue of the corporateness of agnatic lineages as a *sine qua non* have little to do with the matter at hand. We quite agree

with Munson's implied criticism that we should have seen and been made aware of this issue earlier, before writing our book, and that we should not have been blinded by segmentary lineage theory in the Rifian case. But in our own defense we would also call Munson's and the general reader's attention to at least four citations in our book where we show that there are basic behavior patterns in the Rif that get very much in the way of anything resembling a "totally segmentary" explanation of precolonial sociopolitical organization in that region, as follows:

1. "Each clan is segmented into subclans; each of these is segmented into a number of large lineages, which again themselves segment into smaller lineages—and again and again, right down to the level of father, mother and unmarried children" (Hart 1976:10). We must note here that we would today no longer translate *rba*^c as "clan" or "subclan" but as "section" or "subsection," while rejecting Munson's alternative translation of it as "quarter," which, while certainly accurate enough (especially as it suggests the notion of "fourth" inherent in the Arabic root), nonetheless has urban overtones that do not mesh with the rural situation that we observed in the Rif.

2. "Another proverb" (with reference to one cited earlier, *r-^cadhu nj-^cadhu-inu dh-imdukkar-inu*, "the enemies of my enemies are my friends,"), "usually attributed to Arabs, to the effect that 'Nobody hates like brothers', is evidently not known in Waryagharland . . . but this is exactly what happens in fact, as opposed to theory, more often than the Aith Waryaghar themselves would care to think. Yet while they are generally rueful and ashamed about the fact that it occurs, they do not seek to conceal it. This is precisely what happened in the rivalry within the Aith Turirth (subsection) that broke out initially between the lineages of the Dharwa nj-Hajj Am-^cawsh of Bulma and the Imjjat (or Dharwa Ufqir Azzugwagh) of l-^cAss. In this particular case, the shift in alliance was absolutely crucial to the new direction that the feud then began to take, for this new direction was a base-level violation of first segmentary lineage principles [i.e., the rivalry shifted in focus to turn inward and explode in an intestinal, visceral and violent intra-Imjjat struggle]. . . . It was by no means an isolated case" (Hart 1976:322).

3. We discuss in outline six other such cases (Hart 1976:324–325) in which violence arose between brothers of close agnates either over partible inheritance or over women, quite apart from our analysis in depth of the Imjjat

feud (Hart 1976:329–338). All these cases underscore dramatically the fragility of even the most minimal agnatic lineage; while more recently, in the article from which this rejoinder is abstracted, we have calculated, on the basis of previously unpublished field notes dated 1953–55, that during the 40-year period between 1880 and 1920 there were no less than 193 different blood feuds taking place among the Rifians of the Aith Waryaghar. Of these, we have categorized 122 (or 63%) as "internal," that is, as occurring not only within a given local community or *dshar*, but also most probably within a given lineage group or *dhar-fiqth* (we do not accept Munson's rendition of this term as "extended family") within that local community and between two or more component *ijujga* (sing. *jajgu*) or branches of that *dharfiqth*, whose leading members, very often half or even full brothers, became heads of the two opposing factions or *lfuf* (sing. *liff*) involved in the struggle. Each of these factions acquired outside allies, often through affinal connections. The other 71 feuds (or 37%) we have characterized as "external," or extra-community, in that they involved members of two or more distinct *dshur* or local communities.

4. "The living members of the Imjjat lineage say . . . that if their ancestors had stayed together as good agnates should do, they would have become the strongest single lineage in all of Waryagharland. . . . Their claim may be debatable, but it nonetheless illustrates the fact that they were fully aware . . . that their agnatic unity (the agnatic unity so touted by so many social anthropologists) became a hollow mockery. Although they self-reproachfully think their case to have been . . . an isolated one, we have already seen that this was not so at all. *It was almost certainly, in fact, the norm rather than the exception.* Vengeance killings within agnatic lineage groups may be considered utterly deplorable by surviving members of these groups . . . but the data from the Aith Waryaghar show that such events occurred far too often to be dismissed as mere exceptions to the rule" (Hart 1976:337, emphasis added).

In the light of the above evidence, it seems clear that if we are to opt for the existence of a segmentary lineage system in the precolonial Rif, or of any lineage system there, for that matter, we must do so by stressing that corporate lineage unity was nonexistent and indeed totally irrelevant (which is precisely Munson's central point), and that the lineage could be broken down into its individual and constituent though agnatically related members with astonishing ease. Here, however, it

must be recalled that the very first line of another Arab proverb, one not cited in our book because the Aith Waryaghar do not seem to know it, runs "*I against my brothers,*" and only then continues, "my brothers and I against our cousins; my brothers, cousins and I against the other patrilineages in our clan; our clan against the other clans of our tribe; our tribe against other neighboring tribes," and so on—all expressed in an upward-moving, ever more inclusive and segmentarily organized progression, with the appropriate balance and opposition between all segments of the same order of segmentation, as decreed by Evans-Pritchard and subsequent purveyors of the orthodox segmentary line (among which, until very recently, this author was also included). What could show more clearly than this example the potential stress and strain (whether or not it actually reached the breaking point in given cases) in the so-called "corporate unity" of any agnatic lineage system in the Arab-Islamic Middle Eastern context? The lineage may have been unified in a *de jure* sense, but the potential for its rupture was always present.

Now, if we (1) follow Munson's line and dismiss segmentary lineage theory (or any other lineage theory) as "irrelevant," as well as agree with him about the primacy of *liff* alliances, at whatever level² along factional lines that normally split Aith Waryaghar *dharfiqin* (pl. of *dharfiqith*) into two blocs of unequal size; and if we (2) also admit that this murderous variety of fission, itself largely the result of partible inheritance of property (which, as Munson notes, could be bought and sold by individuals), was reflected in the division of *haqq* fines for murder among the *imgharen* (sing. *amghar*) or "big men" of the local markets, then two further points must emerge: when a conflict manifestly begins within a lineage that is already latently split, do we refer to it as a "feud" or as a "vendetta"? Furthermore, do we refer to the behavior and actions of those involved in the conflict as merely "unsegmentary" or "nonsegmentary" or, more pointedly, do we refer to them as "*anti*-segmentary" or "*counter*-segmentary"? Munson, opting for "vendetta" in the first instance, may be partially right, but in opting for "unsegmentary" in the second, we believe him to be definitely wrong.

First, even though "vendetta" has been referred to as a conflict, usually short in duration, between individuals rather than between groups (Peters 1975:xiii-xiv), its often protracted nature and the multiple killings it generally involves seem to us to equate it with "feud" for all practical purposes (Boehm

1984:198)—although of course the protracted Imjjat-Dharwa nj-Hajj Am^oawsh feud turned into the equally protracted intra-Imjjat vendetta, if we have to split hairs over the matter. Second, we would hold that the difference between what Munson labels as "unsegmentary" and what we would now label as "anti-segmentary" is not merely terminological: the first refers to anything that falls outside the segmentary sphere, while the second implies a full and explicit knowledge and ideology of segmentary principles, on the part of the actors concerned, precisely with a view to violating or combating them.

We submit that this last is exactly what happened in most of the Aith Waryaghar cases referred to earlier in this rejoinder and discussed in our book (Hart 1976:324-325), with pride of place given to the intra-Imjjat feud or vendetta (Hart 1976:329-338): the actors were fully aware of the principles involved but chose to ignore them. Anti-segmentary or counter-segmentary behavior on the ground (for shooting one's brother, or one's father's brother, or one's father's brother's son is a violently anti-segmentary act, one that indeed inverts the principle of "segmentary sociability"!), coupled with a highly segmentary ideology in the abstract, are thus among the major factors characterizing precolonial Rifian society, marked as it was by what we may now conveniently label the "Four F" syndrome: fines (*r-haqq*, for murder in the markets), factions (*liff*, or its plural *lfuf*), fratricide (i.e., *nghith w-uma-s*, "he kills his brother," or *Aith Fran minghan jirasen*, "the Aith So-and-so fought against each other"), and feud (or vendetta, *r-adhawth*). All of these traits flourished in the atmosphere of "institutionalized dissidence" that characterized the Rif before the advent of bin ^oAbd al-Krim, especially during the "*Ripublik*" period between 1898 and 1921, at a time when the hand of central government control, present from a distance through lucrative and coveted emoluments to local "big men," in earlier times, was so weak as to be virtually absent.

We must admit, honestly if grudgingly, that Munson is almost certainly fundamentally correct in his assessment of our analysis of Aith Waryaghar society (though not of their ethnography): the *liff* or faction certainly took precedence over the *dharfiqith* or patrilineage—which, unlike the "classical" definition, was not corporate or even truly segmentary—as the major building block in the precolonial Rif; and the distribution and payment of *haqq* fines collected as "perks" by the *imgharen* of each section concerned, for having conducted its *Realpolitik*, were invariably organized in ac-

cordance with it, at whatever level. But we would also stress that the “either/or” factor as expounded above and as a part-answer to Munson depends very largely upon the point of view of the observer: for, irrevocably, the lowest common denominator of either the agnatic lineage or, as Munson would prefer to read it, of the patrilocal extended family is the individual.

Notes

¹This rejoinder is to a large extent excerpted from the relevant sections of a longer article, “Rifian Feud or Rifian Vendetta, and Segmentation or Anti-Segmentation? Additional Materials from the Aith Waryaghar and a Part-Answer to Henry Munson,” to appear in *The Aith Waryaghar and Their Rifian Neighbors* (Menas Press, in press). This paper, as indicated by its title, is in part an answer to an earlier version of Munson’s article. A certain amount of additional commentary, however, is included here in our critique of his final, published version.

²We must also agree that our earlier categorizations of *liffs* as “upper level” and “lower level,” as well as “permanent” and “temporary,” merely complicate the issue.

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To Disguise and to Unveil: Comment on the Issue of the Anonymous Community

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In a recent article on “the anonymous community,” that is, the practice of giving an

anonymous or fictitious name to a community, Robert Reed and Jay Szklut (*AA* 90:689–692, 1988) propose “to open this issue for public discussion.” Commentary and reviews raise another dimension: the issue of whether to disguise the disguised, whether other researchers or their editors do or do not follow the precedent of anonymity. In a prominent anthropological journal, one researcher recently reported on his study of a public folk festival in a community that he called by a fictitious name (Errington 1987). It was an interesting contribution, but as is frequently the case, other researchers felt that there were important critiques to make (Heidenreich and Heidenreich 1988). The critical comments were accepted by the journal, but with some changes.

In the original manuscript, the commentators asked why a readily identifiable town was disguised. They questioned this especially because the disguise was thin: a simple change of the name of the town from its actual name to the name of the major creek that runs through it. They noted that in the references three sources had been altered in citation to help conceal the identity of the town, though the county in which the town exists is named in some references. These comments were marked for elimination with a letter requesting that the length of the commentary be cut to fit within the space requirements of the particular issue of the journal in which it was to appear. On the letter accepting the revised comments—in which the real name of the community was used—was a yellow Post-It note stating: “Since [the original researcher] did not give the real name of ‘[X Community],’ we have followed this convention in your article.” The final printed commentary still calls attention to the issue: “The fictitious title ‘[X Community]’ does not disguise the identity of the town, which is the only community in [the state] to hold the prominent Festival” (p. 566). But key concerns—about why the community should be anonymous and whether public discussion of one analysis of it merited knowing just what community and festival were being discussed—were muted.

This episode shows that there was concern about the issue by the journal involved, and some ambivalence about the amount of emphasis it should receive. Certainly, timely comment on several substantive issues was more important than the single issue of community identity or editorial policy. This particular incident is in itself somewhat trivial because few people familiar with the state involved would mistake X Community for any other. But it raises significant issues. One is