BOOK REVIEW FORUM

Nicholas Thomas, *Out of Time: History and Evolution in Anthropological Discourse.* Cambridge Studies in Social Anthropology, no. 67. Cambridge: Cambridge University Press, 1989. Pp. x, 149, index. US$37.50/£22.50/A$55.00 cloth.

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This book is precisely that which is indicated by one meaning of the title, that is, "what it means for anthropological texts and comparative discussions to be out of time," in the sense of outside of time (p. 1). In the standard temporal sense, one may hope *Out of Time* has come in time. This is a timely book by a prolific author who has recently published a historical monograph on the Marquesas. It is a statement of a modernist and an objectivist historian of the Pacific, part of a concerted effort by a number of researchers to counter what they probably believe is a creeping culturalism and to establish another kind of historical anthropology of Oceania, both for the very long term stretching backward to prehistoric foundations and for the colonial and postcolonial periods of contact between indigenous societies and expanding Western hegemony.

The purported inspiration for this book is the desire to be able to "connect the sophistication of analysis with the actualities of political and economic crises, and in the mutual entanglement of observers and observed in colonial (or ostensibly 'post-colonial') inequalities" (p. 7). This is parallel to-- and perhaps springs from--the critique of ethnography that has emerged in recent years, an endeavor to lay bare the degree to which anthropological texts are monologues dependent upon a predefined objectivity of the anthropological observer and his or her

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ethnographic authority, which, in its turn, is based on the asymmetry of the colonial context within which ethnography developed. Now in its extreme form this argument is part of a more general postmodern disillusion with and dissolution of the very position of objective observer, of the *sujet supposé savoir* as the French would say, in a discourse where all knowledge is an immediate form of power. This is a hot issue nowadays, as the surging ethnification of the world is producing innumerable bodies of subaltern historical knowledge. But it is an issue not easily dismissed by entrenched academics defending their rights to speak objectively in contradistinction to everyone else. And this is surely a question of rights, all the more proof of the fact that objective knowledge is indeed founded on social authority. Thomas is sympathetic to this problem, but he is not terribly concerned to mess around with such issues. Instead, he proposes to analyze the ideological content of anthropological classifications, both functionalist and evolutionist, in terms of their social historical foundations. He is more explicit here than Clifford and others have been in his focus on the actual nature of historically situated academic discourse, and he seems to argue for a critical self-reflexivity reminiscent of Bourdieu. The latter, however, took a dangerous next step in assuming that "all objectivist knowledge encompasses a claim to legitimate domination" (Bourdieu 1980:49). Thomas's own objectivism is never in question here, and this might be seen as a lack of consistency in his exposition.

Thomas begins by arguing that somehow the fieldwork orientation of anthropology is itself to blame for the fixation on a society or culture extracted from historical context and objectified for the sake of relativistic comparison or even evolutionary ranking. One of his central assertions is that this ahistorical conception of social life generates an opposition between society as an internally coherent systemic field and a larger context that consists essentially of an unsystematized space of events, like a universal ether through which social bodies move. There is more than a sneaking suspicion that Thomas does not like fieldwork, although he certainly does not go so far as to argue for its elimination. He prefers to decenter it and allow the historical context its rightful place in our analyses. While it is true that the practice of fieldwork may tend to limit the field of analysis and to close it off to both larger spatial and temporal contexts, I would argue that fieldwork itself has been molded by the ideology of societal wholes as organisms, and by a similar ideology of culture as distinctive attributes of a given population. These are in turn variations on a superordinate cosmology of societal identity, implying a conflation of society, culture, and population. It is, I would

suggest, this cosmology that informs the similarities between British functionalism and American culturalism, as well as evolutionism. This problem, in any case, deserves more serious discussion in terms of historical context. This is all the more so since there is a systemic relation between the static nature of social typologies that informs functionalism and structural functionalism and the evolutionary schemes of the nineteenth century from which such typologies are by and large derived. Thomas is quite aware of this connection, but he appears to be more interested in accumulating debating points--which he does successfully. I am certainly in agreement with his general attitude toward the professional ethnographic mystique and his contention that the often hysterical attack on other sources (missionaries, travelers) of ethnographic data is and never has been founded upon anything other than the hocus-pocus of supposed anthropological competence.

A major topic of *Out of Time* is evolutionary models as they have been applied to the supposed laboratory situation of the Pacific. Here again he offers strong arguments to the effect that the ranked social types of Sahlins, Goldman, and others are little more than abstracted, detemporalized social types aligned along an abstract scale of the degree of political hierarchy and driven by technological development or status competition. Such models have little to do with "real" history since they are based on rearranging the ethnographic--that is, "contemporary"--examples on a predefined scale. Thus, while structural functionalism and cultural anthropology openly practice the detemporalization of social reality, evolution--in spite of the illusion of change--is similarly grounded in the atemporal classifications while making use, at most, of an abstract and imaginary time scale. And the latter time scale is also part and parcel of nineteenth-century evolutionary ideology, the Victorian anthropology that arranged the extant and extinct societies-races of the world in a pseudo-order of progress toward civilization. An essential aspect of Thomas's argument consists in demonstrating the continuity between the classifications of the last century and the theoretical interests of the present.

Thomas does not simply dwell on the history of ideas; he provides examples of alternatives. In discussing Goldman's categorization of the Marquesas as "on the way" to stratification, he suggests from a broader historical foundation that the society was more likely "on the way" *from* stratification and that there were numerous transformational processes involved other than progressive development. I cannot but agree, seeing as I have argued for a similar kind of transformational process in general. But this leads me to Thomas's chapter 7.

Chapter 7 deals with the global systemic approach with which I have been associated. Thomas presents it, most flatteringly, as an alternative that has managed, for the most part, to free itself from atemporal evolutionary bias. But this is only for the most part since I, at least, am accused in the end of similarly using ethnographic types from the present in the construction of a historical model. The model that I proposed, essentially as a research program, suggested that an original prestige-goods system associated with the Lapita expansion--one whose systemically related features include asymmetrical marriage exchange (matrilateral), diarchy at all political levels, bilineality, and monopolies of varying degree over external exchange of goods necessary for the social reproduction of local groups (marriage, death, and all other essential payments)--stabilized to the point of being able to maintain historical continuity in western Polynesia. Eastern Polynesia, mostly settled after the terminal crisis of Lapita trade systems, never established such stable, long-distance trade in prestige goods because the extreme distances between island groups prevailed against the emergence of such trade. Rather, diarchy was violently collapsed into monarchy where the original two functions, fertility and politics-warfare, were absorbed in a single position, where the generalized exchange system linking ranks was reorganized in terms of a strategy of high-status endogamy, where production of prestige goods was replaced by increasing intensification of agricultural production for feasting and the support of a warrior aristocracy, where the sanctity of chiefs was increasingly sanctioned with violence in conditions where they had no evident monopoly of strategic goods, and where expansion was based on warfare and the redistribution of titles to lands (not property). I have referred to this as theocratic feudalism. In Melanesia, especially northern Melanesia, on the other hand, an increase in trade density led to the breakdown of exchange monopolies. This led to increasing competition among increasingly smaller political units in which production for feasting became increasingly dominant and resulted in cultural fragmentation and the emergence in the long run of big-man types of strategies.

The model was merely a sketch and Thomas rightfully criticizes some of its empirical shortcomings. He also offers certain more general criticisms. This kind of a model, he claims, while averting the assumptions of evolutionism, preserves the basic fallacy of using contemporary examples as building blocks. The charge would be true if the kinds of structures to which I refer were "societies," but such is not the case. Rather, my argument is a structural one in which the properties of dif-

ferent kinds of strategies and organizational tendencies are seen as transforms of one another. There is no abstract time dimension, nor a rank order of stratification. There is only the processual time of transformation itself, that is, a theoretical historical temporality, that depends on the properties of social reproduction and the conditions of reproduction of such structures. Prestige-goods systems can stabilize for long periods in the right conditions. They can also collapse and reappear in the right conditions, One might interpret certain archaeological material from Melanesia in terms of precisely such variation. Variation among the societies of the Kula Ring has been interestingly analyzed in terms of just such a historical model (Persson 1985). Thomas seems to conflate the structural model with empirically observable societies in a way strangely reminiscent of Radcliffe-Brown himself. Furthermore, to argue that all societies of the past must be qualitatively different than those we find in the present contains an assumption of continuous development in which all societies constantly transform themselves, yet another false evolutionism.

At the same time, a diametrically opposed argument is suggested as to the inadequacy of my schema. First, there is no reason why eastern Polynesia could not have developed regional systems of considerable scale. Second, there is some evidence that the western Polynesian regional system centered in Tonga emerged as late as the fifteenth century. Underlying this argument is an assumption that somehow conditions of operation or reproduction ought to be necessary and sufficient to account for particular social forms. It is true that it is said that the great geographical distances of eastern Polynesia prevented the establishment of trade systems, but I would add that the actual process of transformation is clearly more complex. I have always stressed that the existence of a particular structure can only be accounted for in terms of its morphogenesis. In such a framework, I would argue that the establishment of prestige-goods systems in western Polynesia was related to the extension of such systems into the area from further west, that is, the geographical expansion of an already existing trade system. This system may have experienced numerous crises, but the potential exchange network was always present so that variations in degree of hierarchy and control do not transform the basic properties of the system. This would account for the emergence and disappearance, and reemergence, of western Polynesian types of structures in Melanesia. In eastern Polynesia, on the other hand, it might be argued tentatively that settlement occurred largely on a different basis (with, perhaps, the exception of the Marquesas). It seems to have followed upon the collapse, crisis, or

decline of the earlier Lapita-based system, so that initial strategies combined with the absence of viable conditions for prestige-goods exchange might have channeled development in a novel direction. Further, it must be reiterated that these structures cannot be confused with societies or homogeneous populations defined by anthropologists. They are strategic properties of social processes of reproduction. As such, any population may make use of multiple strategies, even where they are in conflict with one another. Prestige-goods strategies may and have certainly emerged and reemerged in eastern Polynesia, even if they did not succeed in becoming dominant, that is, in colonizing all domains of social existence. Certainly, such goods were distributed by chiefs to their subordinate aristocrats (as in feudal Europe), and other evidence of such tendencies might be found in what exists of Marquesan dualism, in certain aspects of Tahitian social organization, and even quite late in precolonial Hawaii: Before his death, King Kamehameha withdrew to an increasingly sacred sphere, leaving the monopoly of external exchange and politics in the hands of his wife's family, that is, in the hands of his affines, thus tending to produce during a very brief period a dualism of functions replicated in an alliance relation.

With respect to my argument concerning the way in which different structures articulate with expanding Western hegemony, I suggested that if Europeans began to trade freely in the peripheries, eastern Polynesian strategies of expansion by warfare could lead, via monopoly of trade in weapons and military aid, to centralization and state formation, whereas prestige-goods systems that were previously centralized would break down due to loss of monopolistic control over trade. This is simply a statement of what I consider to be the logic of such relations. The degree to which they are manifested in the historical material was, I argued, exemplified by the divergent developments in Hawaii (and Tahiti) and Tonga in the initial period of contact. The model for the articulation of prestige-goods systems and European trade was developed on the basis of studies of the Kongo Kingdom (Ekholm 1972, 1977). The differences in some respects are great, especially the dimension of the Kongo Kingdom, well into the multimillion population class, as opposed to the Tongan "empire." But the rapidity of breakdown is just as strikingly rapid after trade becomes decentralized. It did not, as Thomas states, take "decades of systematic commercial exchange" (p. 98). I would add here, however, that it is very difficult to ascertain the way and the extent to which prestige goods functioned in Tonga, even if Thomas appears, very unlike his usual historian's style, to accept my assertion of the existence of such a system at face value. His own asser-

tion that weapons were used primarily as prestige goods in eastern Polynesia is true in one sense (pp. 98-99), but the history of Kamehameha's conquest of the Hawaiian Islands makes clear that monopoly of just such British imports was the key to centralization. To argue, contrary to my own approach, that "obvious military purposes were less important than prestigious display, and, most crucially, exchange value" (p. 98) is a misunderstanding of the social form of military strategy. No weapon is merely a weapon, of course. It is an expression of cosmic force and its mere possession or display may be enough to vanquish an enemy. Its exchange or prestige value resides in its *mana,* so to speak. But this does not detract from its place in a strategy of conquest, as is clearly evident in the historical texts.

My own discussion was limited to initial contact and I would argue that the emergent political structures of the nineteenth century are increasingly dependent on the intervention of Western forms of organization in the political life of a region successively integrated into the "world system." The fact that Tonga emerges as an autonomous "constitutional" monarchy just as the Hawaiian "constitutional" monarchy becomes totally dominated by a white settler class cannot be accounted for in terms of the initial articulations to which I refer.' When a local society becomes integrated into a larger system in such a way that the latter penetrates and disarticulates the former, then we cannot simply speak of a relation between or even confrontation between two structures. We have instead a new social field--for example, colonial society --that must be understood in its own terms.

Thus far, Thomas's argument would seem to run from the proposition that an original ahistorical--even antihistorical--bias in the foundation of anthropology via ethnography is carried over into evolutionary anthropology and to some residual extent into global systemic anthropology, even if the latter is based on an explicit critique of evolutionary thinking. Sahlins is last on Thomas's list as a former evolutionist who is today concerned with how culture produces history via a dialectic of structure and practice, sense and interest, sense and reference. Thomas argues, correctly I think, that while Sahlins's approach is well suited to confrontations--to the "encompassment of events by a cultural order" (p. 106)--" the process of transformation seems to have got lost hereabouts" (p. 107). It might be more accurate to say that the model does, theoretically, at least offer an explanation for changes in structures of meaning, of semantic categories, but not of social process in a more holistic sense. An elementary structure of kinship as Lévi-Strauss uses the concept refers to the exchange properties of kinship rather than to

the semantic, that is, culturally specific, categories that might be used to designate relevant kin by a given society. In Sahlins's discussion only the latter are relevant. On this point Thomas, in my view, does not go far enough in his critique. The problem with this kind of structuralist history is that it continuously eliminates essential social processes, those properties of the latter that do not belong and cannot be deduced from anyone's cultural code. The opposition between sense and reference and sense and interest is organized as an opposition between that which is structured and encompassing and that which lies, momentarily, beyond structure and encompassment, that is, the world of happenings. This might have easily been brought back to the earlier discussion of structural functionalism and its implicit opposition between society as a structured whole and history as a mere externality. Structuralist history is an extension of this premise in which there is now a dynamic interaction between the two terms of the opposition, but where the latter are preserved intact.

The chapter ends with a consideration of several factors that ought to play a role in the construction of a historical anthropology. Thomas stresses the role of agency, of active structural creativity, as opposed to models of acculturation that are passive or that assume historical process is only about categories in transformation. He also points out that not everything changes or disappears in processes such as colonialism and modernization, to be replaced by the culture of the conquerors. Notions of personhood and agency among dominated peoples may have "little to do with those of the dominant culture" (p. 113). The latter, of course, may inform in various ways local strategies of survival, of cultural production and interpretation, and of the formation of local movements. All of this is nice to know, but it is difficult to see how it is related to the general argument. One gets glimpses of an attempt to grasp the multiplex nature of a world that has too long been categorized as Other but which in reality has been very much a part of our own world. In this sense, there is a continuity from the attack on static notions of primitive coherence to the statement of the need to study colonial and postcolonial historical worlds in their total presence and coevalness.

The importance of Thomas's overall statement is both that it criticizes the way in which the categories of anthropological analysis have been abstracted by ideological lenses from the concrete context of historical documents and ethnographic realities and that it prods us to always and everywhere take this concrete context into consideration in our work. Many will experience Thomas's modestly polemical gesture as a serious

threat to the anthropological mystique. I myself would applaud it and would claim that it could have been even more polemical in the sense of systematically stating a position. This is absolutely necessary when one considers the enormous number of cultures of the so-and-so that are paraded as the result of a research method that consists of locating the pieces of other people's essences in disparate texts and statements strewn over hundreds of years, all legitimated by the criminally insane notion that such facts are "held together" by some cultural totality. [2]

If I sense an essential contradiction in the argument, it is best revealed in the summary statement at the end of the book.

> A refocussed anthropological vision would often take a greater interest in archaeological evidence about longer-term social change. It would also deal much more extensively with historical events and their consequences: this evidence would assume the same sort of importance that observed ethnographic minutiae and informants' statements now carry. (P. 122)

Although very much in sympathy with such a plan, I find it loaded with problems that need systematic exploration. The archaeological record cannot be read, nor has it ever been read, like a series of events. Its interpretive categories have usually come from the static typological schemes produced by anthropology, schemes that necessarily deal with macroscopic changes that are about as far from evenemential history as one can get. A scenario for the actual emergence of a state, a neolithic transition is perhaps what we are waiting for, but its categories are bound to be saturated with the categories of the present unless we find a new way of doing things; and no new way has been suggested in *Out of Time.* At the other end of the scale the concrete historiographical method that Thomas suggests ought to be applied to ethnography would take the form of the history of what happened when the fieldworker was there to record it. How to get to structure, to locate it in the flow of events at the local level, is not addressed. And the ultimate question remains open of how to arrive at the "systemic process" (p. 121) that ought to be the new object. But the question is a good one, and that is most important. One may hope that we're not all out of time.

NOTES

1. Here again, comparison with the Kongo Kingdom is incorrectly invoked. Thomas argues that both Tonga and Samoa maintained variants of traditional hierarchical rela-

tions until very late while the Kongo-- which is, apparently, the sole basis of my model-- "vanished into a mass of localized, egalitarian societies" (p. 99). This is quite mistaken. The Kongo region produced a large number of hierarchical societies throughout the several hundred years following the collapse of the first contacted kingdom and the final colonial onslaught. Even today, the area exhibits, as a dominant feature, transformed versions of such relations, i.e., just as found in Tonga and Samoa.

2. I recall having a conversation with a very well known specialist on central Africa who was convinced beyond all repair that the Kongo had a system of patrilateral cross-cousin marriage as a matter of essence, so that they could absolutely not have ever practiced matrilateral marriage in the past. But the list is long of those who have built models of "societies" based on the data of cannibalism, witchcraft epidemics, and egalitarian reciprocity that were the results of disturbances, *crises,* and catastrophes produced by colonial penetration (MacGaffey 1986; Sahlins 1985; Geertz 1980).

REFERENCES

Bourdieu, P.
1980 *Leçon sur la leçon.* Paris: Minuit.

Ekholm, K.
1972 *Power and Prestige: The Rise and Fall of the Kongo Kingdom.* Uppsala: Skriv-service.
1977 "External Exchange and the Transformation of Central African Social Systems." In *The Evolution of Social Systems,* ed. J. Friedman and M. J. Rowlands. London: Duckworth.

Geertz, C.
1980 *Negara: The Theater State in Nineteenth Century Bali.* Princeton, N. J.: Princeton University Press.

MacGaffey, W.
1986 *Religion and Society in Central Africa.* Chicago: University of Chicago Press.

Persson, J.
1985 "Cyclical Change and Circular Exchange: A Re-examination of the Kula Ring." *Oceania* 54:32-47.

Sahlins, M.
1985 *Islands of History.* Chicago: University of Chicago Press.