

# History of Anthropology Newsletter



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# History of Anthropology Newsletter

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## TABLE OF CONTENTS

### FOOTNOTES FOR THE HISTORY OF ANTHROPOLOGY

"Dear Max": Victor Turner from the Field in 1951..... 3

### CLIO'S FANCY: DOCUMENTS TO PIQUE THE HISTORICAL IMAGINATION

Schneider on Kluckhohn, 1964: Myth and Memory, the Oral and the Written,  
Historical Retrospect and Self-Representation in the Historiography of Modern  
American and British Anthropology.....7

### SOURCES FOR THE HISTORY OF ANTHROPOLOGY

Archives of the Berlin Anthropological Society.....15

RESEARCH IN PROGRESS.....15

### BIBLIOGRAPHICA ARCANA

I. Recent Dissertations.....16  
II. Work by Subscribers.....16  
III. Suggested by Our Readers.....17

### GLEANINGS FROM ACADEMIC GATHERINGS

Teaching the History of Anthropological Theory.....19

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## FOOTNOTES FOR THE HISTORY OF ANTHROPOLOGY

### "Dear Max": Victor Turner from the field in 1951

Matthew Engelke (University of Virginia)

The following letter from Victor Turner to Max Gluckman is drawn from the newly established Turner Archives at the University of Virginia. This archive was organized during the 1995-96 academic year by Peter Metcalf, Sandra Bamford, and Edie Turner. Although the letter is dated 1951, it was found among Turner's papers from his second fieldtrip to the Mwinilunga District, 1953-54, and was presumably never sent to Gluckman. Turner's early impressions here of Kamahasanyi and Ihembi should be of particular interest, since both figured prominently in such works as "A Ndembu Doctor in Practice" and The Drums of Affliction. While Turner was never afraid to describe his informants in colorful terms, this letter does add something to a picture of 'the blithering pain in the neck' and 'old pal', respectively. The letter was typed and is presented here only slightly modified to fit the format of HAN. The original letter was edited by Turner, mostly for punctuation, but also notably once-for a change in pronouns. In the third paragraph, the third sentence initially read 'Next, I . . .', but was later changed to 'Next, we . . .' with a pencil mark. Turner often noted the collaborative nature of fieldwork with his wife, Edie. The spatial diagram and the kinship chart were drawn out with colored pencils, and I have tried to preserve their integrity by substituting marked lines. The letter is reproduced here with the permission of Edie Turner.

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Luinga River Camp  
Mwinilunga District  
N.R. [Northern Rhodesia]  
20th Nov. 1951

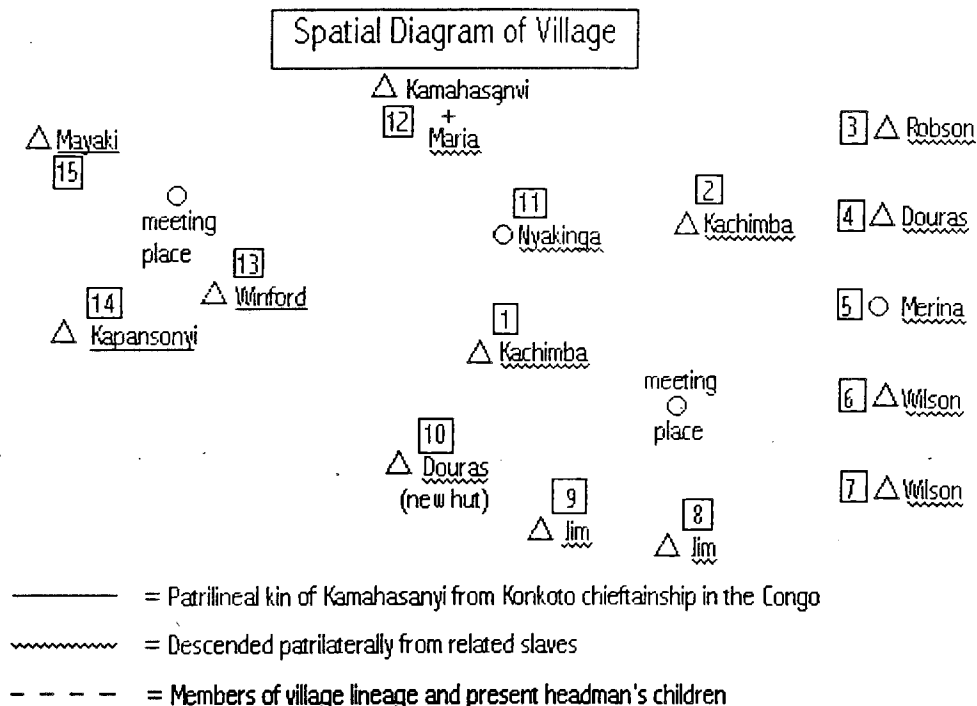
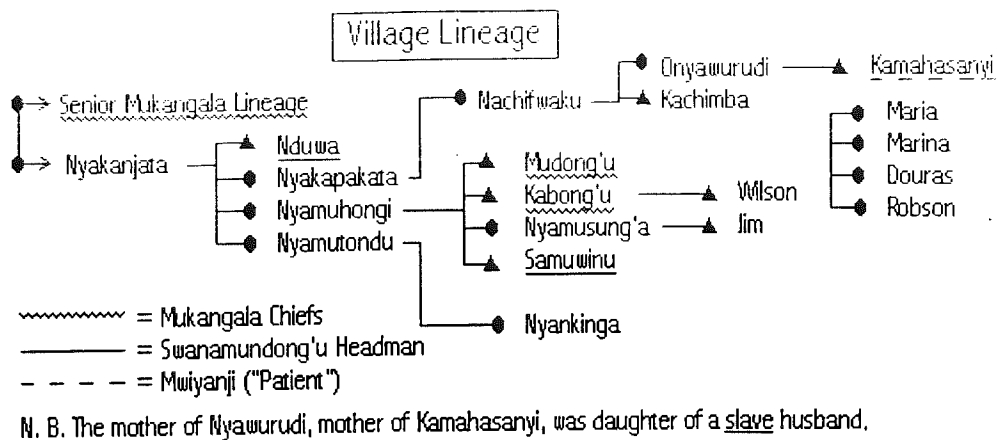
Dear Max,  
[...]

I took your advice and moved away from Chief Ikelenge's capital. My relationship to this charming, unscrupulous and ambitious young man was beginning to lose me friends among the villagers. We built a pole-and-dagga [brick-like mud compound] affair in the middle of a cluster of villagers, at a point where three sub-chieftainships intersect. We have built up friendly relations with the people by dispensing medicines, taking bad cases of illness to the Mission Hospital and writing to the Game Department for guards to shoot hyenas and wild pigs that have been destroying small stock and cassava gardens.

When I first entered the area I embarked on a 'naupliar or free-swimming phase', traveling fairly extensively over the two Paramount chiefdoms and collecting census and genealogical data. During this period I collected structural genealogies for 57 villages, 12 chiefly genealogies and census data of varying quality (depending on the ability of the clerks employed) on about 600 people. Next, we (Edie did the bulk of the work in this) took annual budgets for over 600 individuals, grouped into 37 families and a few odds and ends. But all this was merely scratching the surface. I resolved to dig my toes in and really get to know a small 'area of common life'. Edie and I took pains to strike up personal friendships. We frequently went to the gardens and sometimes lent a hand with a hoe or axe. After a while we began to strike into the rich rift of ritual (not with hoe or axe!). We were invited to ceremonies of all types, exorcism of spirits, rites de passage, divining (illegal) and ritual directed against sorcerers. I cultivated two old ayimbuki ('witch-doctors'[,] but this isn't the proper term--the 'great' doctors are more like 'hierophants' or 'priests' in some mukishi ceremonies[,]) and certain ceremonies such as Chihamba and Mungoni almost amount to 'Mysteries' with degrees of initiation into knowledge of the esoteric elements) for some time (they are both wusensi 'joking partners' of mine) and they have shown me several complete mukishi ceremonies and allowed Edie to take a photograph sequence of the performance in two cases. The 'Archimedean point' of Lunda culture is the ritual system, just as among the Lozi it is the economic system. The dominant motif of Lunda ritual is the ridding of an individual by a group, led by a hierophant and consisting of an inner circle of initiated persons and

an outer circle of uninitiated (to supply 'generalized social power' perhaps you would say), by an elaborate sequence of ritual events and with the aid of numerous plant and animal medicines collected in a strict irreversible order, of a disease or affliction which has been troubling the mwiyanyi (Lunda do not distinguish between a 'patient' who is physically sick or a person who has been unlucky at, say, hunting--both are afflicted by a mukishi). The disease or affliction may be caused by the spirit of a deceased relative or by the medicine of a living sorcerer or sorceress. The diviner is the diagnostician. In the case which I am going to describe, both the living and the dead contrived to make the poor chap's life miserable. But the mukishi cannot properly be termed an 'ancestor'. Akishi in every case I have recorded are the spirits of the recently dead, never going further back than the 2nd gen. asc. [2nd generation ascending]. The term for ancestor, nkakulula, never refers to a spirit but to the name of the mother or sister of the grandparent generation. Shrines (of various types which I haven't time to describe here) are only raised or planted to a deceased person if the latter has afflicted a living descendant and been exorcised. It is a symbol of reconciliation or perhaps placation. There is thus no regular ancestor cult, no regular worship or pouring out of libations. Spirits return to afflict their relative because the latter was their enemy during life or because he or she has affronted them by their recent conduct. One must also distinguish between mukishi and mufu; a mukishi is the spirit of either a matrilineal or patrilineal deceased relative and usually acts of its own volition. A mufu (lit. 'dead person') is most frequently the spirit of a dead person under the control of a sorceress, of a living person. It can be changed into any shape its possessor may desire, lion, buffalo, hyena etc. or may be made to manifest itself as a kahwehwa [or] a 'zombie' in which form it must prepare the 'meat' of a dead person for a concourse of sorceresses to devour on the morning of the interment. They eat the 'real body' of the dead, a spurious 'double' only being interred by the burying party. Only a woman may use nyifu in sorcery. They are obtained by getting a chimbuki ('witchdoctor' again; doctors work for good or evil purposes according to their financial reward) to kill a close relative such as a son or father--nyifu are always male, but they can kill other women. Incidentally, women can kill any relative by wuloji; men cannot kill their own children or their wives. Chiefs and hunters (those with a hunter-name taken after a special mukishi ceremony--one acquires ritual status through having been afflicted oneself and cleansed by the appropriate ritual) are the exception; they can kill wives but not children. It is said that sorcerers can kill non-relatives, but I recorded instances where this is said to have happened only in the case of chiefs.

I want to describe a connected series of rituals I witnessed which bring out more clearly Lunda structural and ritual principles than any general account. First of all I must give an outline of the spatial and structural situation in the village of the mwiyeji (the same person in all the rituals). Swanamundong'u Village belongs to Mukang'ala sub-chieftdom formerly important historically as it was given to the son of the first Ndembu Paramount Chief as a reward for driving out the ambuella who occupied the area before the Ndembu Lunda came from Mwantiyanvwa. This meant that his descendants could no longer be considered for the Paramountcy but must succeed to the Mukangala chair only. Now, Govt. in 1947 abolished the chair on the grounds that few villages were attached to it and closed the mission out-school at the capital. Thus the area labours under a grievance and owing to the loss of their 'voice' have no one to speak for them when wild pigs etc. raid their gardens. It is a depressed and backward area. The Swanamundong'u Village lineage supplied the last two Mukangala chiefs but the present 'chief' comes from the senior house of the royal matrilineage. When he succeeded[,] Samuwinu, Headman of Swanamundong'u Village and full brother of the late chief, expected to succeed. When Govt. and the meeting of HM [headmen] appointed the present chief he fled into Ikelenge area, fearing the jealousy and sorcery of the latter. Swanamundong'u Village were thus left without a proper HM and with no one of the proper seniority (about 55 plus) to deputise. The chieftdom's and village's misfortunes, both interlocked, and the poverty of the gardens, pressed hard on the village. Further, intra-village tensions were set up recently which require diagrammatic explanation; [here Turner inserted the following figure]



The spatial diagram shows that there are two groups in the village, linked only by Kamahasanyi. K. himself is an odd character. He is sterile--married four times without issues, always changing his hair style as women do, and a v[ery] great snob (even more than most Lunda[,]) who are always mentioning even the most threadbare connection with a chief). He is nervous and self-pitying. He was previously married to his patrilineal cross-cousin in the Congo, in Kanema, the village of which his father was HM. When she died he accused her mother and sisters of procuring her death by sorcery in order 'to eat a big mpepi (payment made by a widow or widower to a spouse's relatives to finally rid themselves of the latter's spirit) and left his F[ather]'s (now dead) village for his m[other]'s village, Swanamundong'u. He brought with him Makayi and his son Winford and old Kapamsonyi, who had taken his side in Kanema. Last year he married Maria the acting HM Kachimna's daughter and his own matrilineal cross-cousin. Under the famine conditions of the village and the depression of Mukangala's fall from Govt. favour, mutual recriminations flew between the Konkoto group and the Swanamundong'u group. And Kamahasanyi was the link between them, and psychologically a weak link. He began to dream of spirits threatening him. When his traps proved always empty of duiker, he attributed this to spirits driving them away. What prevented an open breach between the two groups was the HM's need to have as big a following as he could and the fact that Kamahasanyi was both his nephew (and

therefore possible successor) and also son-in-law. After a time K. went to a diviner who told him that his 'GF' [grandfather] the late chief Kabong'u was afflicting him, not because of any personal animus but because the village did not have its 'true HM Samuwinu'. I asked K. why Kachimba the acting HM. was not himself afflicted. K. said that it would have then been considered 'personal matter' between the mukishi and Kachimba--if you want to get at a HM qua HM[,] get at his people so that they will reconsider the question of the right man for the job or else run away to less haunted territory. The diviner made ng'ombu; ie. consulted the objects in his divining basket, and prescribed that since Kabong'u was a hunter, three hunter's rituals should be performed to 'send him away', Mukala, Ntambu and Ihamba (not Chihamba, a very diff[eren]t complex ritual). Mukala is a manifestation of a spirit in which the dead drives game away from traps by whistling from the top of an anthill at them. Ntambu like a lion chases game from spear or bow. Ihamba is a much more concrete manifestation being nothing less than the middle upper incisor of the dead man deeply embedded in the sufferer's flesh! I won't describe the rich cultural content of these rituals in this letter (I'm trying to collect data for a thorough analysis of Lunda ritual and medicine from this aspect). The original diviner turned up to perform Ihamba, but it was felt he 'didn't know it properly'. So I brought my old pal Ihembi, the arch-wizard of Ikelenge area and Number One Ihamba man to Swanamundong'u by car. Half-way through the ceremony he took over, and in his genial way (he has a perfect 'bedside manner') 'extracted' the tooth by means of cupping horns from Kamahasanyi's quivering back. We photographed the rite. I brought old Samuwinu the true HM. along by popular request, for it was thought that his older brother's spirit would listen to him when he prayed on behalf of his grandson and because for a while the village 'would be as it ought to be'. There was great relief in the village afterwards, everyone shaking hands with everyone else. K. happily paid up 10s to each chimbuki. Alas, K. began to dream again, this time of his own father. He hopped off to Angola to consult a big diviner (divining is not illegal there) and this bloke really gave him his money's worth (K. is the kind of man who would spend a fortune on patent medicines in our society--he fancies all sorts of pains and headaches, yet looks thoroughly healthy). Ng'ombu said that there was yet another Ihamba in his body, that of his father, who was offended with him for leaving his village and cursing his wife's kin--deceased's own matrikin it must be remembered. But before it could be removed the malignant influence of one of the villagers, Wilson (see genealogy and diagram) must be removed by the anti-sorcery ritual Kaneng'a. Wilson had always opposed the entry of the Konkoto group into the village and disliked K. personally. Ihembi told me this privately and I confirmed it by eavesdropping. A chimbuki who could perform both Kanenga and Ihamba was wanted and who better than Ihembi? K. asked me again if I would motor the old man and his assistant to Swanamundong'u. I agreed and got Ihembi to take me into the bush to collect medicines before both rituals and show me the tricks of the trade. I have some very full notes on them I can show you sometime. The implications of each would occupy a chapter on their own. Main points are that Wilson was made to take a leading role in the purificatory aspect of Ihamba to give him a 'clean liver' before Kamahasanyi, that K. during the ritual confessed that there was a bitterness in his heart that his mother's brothers Kachimba and Jim did not offer to go to Angola and get a diviner for him, but seemed oblivious to his sufferings. I think most men in the village regard K. as a blithering pain in the neck. Ihembi looks on him as a chimbuki's dream, the perfect type of the 'sucker'. But he is in a sense the scapegoat. Animosities are drawn off through him as by a poultice. The latent aggressions in the village are manifested as hunters' teeth and hauled out of him. Once he had confessed his resentment the Ihamba 'came out'; it had previously remained in his neck; the four cupping horns had drawn an immense amount of blood out of him during the day.

Well, Max, I must catch the once-weekly post, although there's a hell of a lot I wanted to expand upon . [ . . . ] We're all pretty fit but I'm feeling a little stale just now after a non-stop year in the field. How's the Department this session? And how is the book on Lozi Law proceeding? My own data on law are very sketchy. Hope to catch up next tour. Edie sends best wishes to all.

Yours, Vic.

## CLIO'S FANCY: DOCUMENTS TO PIQUE THE HISTORICAL IMAGINATION

### Schneider on Kluckhohn, 1964: Myth and Memory, the Oral and the Written, Historical Retrospect and Self-Representation in the Historiography of Modern American and British Anthropology

Looking for material to fill a scant number, I came across a file given to me in 1985 by David Schneider ("Dear George, it's yours to do with as you like"), containing a carbon copy of a letter Schneider had written in 1964 to Talcott Parsons, in response to a manuscript on "Clyde Kluckhohn and the Integration of Social Sciences." Having recently read the volume Richard Handler reconstructed from interviews with Schneider, each of them focussed on a series of "moral tales," "autobiographical stories" and "stories-within-stories" (Handler 1995:13) I was interested, both substantively and historiographically, in the way in which some of the same or related stories might have been represented in a more spontaneous (but very mixed) mode three decades closer to the issues and events that they treated. However, given limitations of space, there will be no attempt here to explore the historiographical or other issues that the document raises—including what Schneider himself referred to as matters of "fact"; for the present, I will leave that task to the historical imaginations of readers of HAN. Except for the bracketed addition of words or punctuation marks to clarify passages that seemed obscure, and several bracketed bibliographical cross-references, the letter is reproduced exactly as typed, including misspellings—with one exception, the deletion of an anecdote about Malinowski's personal behavior, four decades after its alleged occurrence, which had no direct link to Schneider's representation of Kluckhohn.

Dear Talcott:

December 16, 1964

Many thanks for sending me the ditto copy of your paper on Clyde and the Integration of the Social Sciences. I found it very interesting, of course, and I have a couple of comments on it. Some of these comments have to do with Clyde, some with your view of the history of the Social Sciences, and some of the comments may be irrelevant to both of these points.

On page 14 you recognize the very great importance that "language" and linguistics assumed both in general and for Clyde in particular; on page 15 you say, "it is interesting to speculate on the basis for the greater British emphasis on the 'social' aspects of Anthropology and for the greater American emphasis on its 'cultural' aspects." On page 16, "Kluckhohn was certainly one of his generation of Americans who minimized the central significance of social organization rather much and were rather cold to the important contributions of a number of British structuralists in such fields as kinship." Then on page 19 in your footnote you say "I find it somewhat difficult to understand why Kluckhohn's attitude toward Malinowski's contribution was so persistently negative."

I think that some of these different points can be brought together very easily. First, Malinowski. If you recall Clyde's obituary of Malinowski (published in the *Journal of American Folklore*), you will remember that Clyde in effect called Malinowski a "charlatan." The history of Malinowski's and A. R. Radcliffe-Brown's relations with American Anthropology is a particularly dismal one. At one extreme [here follows the Malinowski anecdote]. At the other end there is a famous anecdote which tells of the great triumph which Linton announced when he was able to completely block the allocation of \$2,000,000 of research funds which the Rockefeller Foundation had offered because Radcliffe-Brown would have had something to do with that money (the details of this anecdote are not entirely clear; Fred Eggan has documentation on it; whether the total amount was \$2,000,000 I am not certain, but it was at that time a very large sum which the Rockefeller Foundation had offered to Anthropology because it had been deeply moved by the belief that if we did not all go out and study primitive tribes right now there would be none left to study within 50 years) [cf. Stocking 1995: 391-406]. I remember often hearing Clyde remark quite caustically that the only reason that Radcliffe-Brown was read was that he (Radcliffe-Brown) could read French and most other



Anthropologists could not. Clyde made the same sort of remark against Malinowski. Clyde told me a couple of times how much both had plagiarized from Durkheim without ever acknowledging it. Clyde felt, I believe, that Malinowski's whole notion of reciprocity had come straight from Durkheim and Mauss without a single acknowledgment while he had taken from Freud while publically denying any relevance to what Freud had written.

In certain respects at least most of Clyde's feelings against Malinowski, of a personal sort, were well grounded and not without foundation intellectually. The fact is that Malinowski borrowed, stole, and distorted material from Durkheim, Freud, and others and did in fact offer it on the intellectual market as his own. Your comment in the footnote on page 19, "It is interesting to note that Malinowski was a member of the Yale group in his last years and attempted to ground his theory of culture with the learning theory bridge. Though I personally do not really think this was really successful." As you may remember, I spent my first years of graduate study at Yale during the year that Malinowski was there. Your very straight sentences, therefore, seem particularly hollow to me. In Clyde's view, Malinowski was an adroit intellectual as well as personal parasite. I share this view.

In this connection [I offer] a single tangential note with reference to your page 19, the part before the footnote.

The significance of Yale, for Clyde, did not lie alone in the fact that his frie[n]d John Dollard was there. Nor that it also represented in the Institute for Human Relations a new and on the whole important trend with which Clyde himself was associated. It lay also, first, in the fact that Edward Sapir ran a very, very important seminar at Yale which Clyde attended and which also included Ruth Benedict, Margaret Mead, and a number of others. Over and over his common participation in this seminar (I do not remember the dates; I could perhaps locate them if it would be important to you) second, was a very special relationship which he had with Edward Sapir. Occasionally when Clyde had had a drink or two we students would sometimes accuse him of being "Judeaphilic" [--] that is to say, he shared a non-rational belief that there was something about Jews that made them superior to all other people. And in his cups Clyde would confess that this was so at least so far as Edward Sapir was concerned. Clyde had the deepest, warmest, and gravest affection for Sapir personally and the most profound admiration for Sapir's genius. Still another element in his relationship to Sapir (third) and therefore to Yale was the fact that Sapir had made some of the most important linguistic advances through the use of Navajo. Clyde spoke Navajo and the common interest which Clyde and Sapir had in Navajo, in linguistic structure, in problems of psychology and language, in problems of culture and personality all went far to binding them in many different ways. Let me add one more final note on this. Again, occasionally with Clyde in his cups, some of us heard him very bitterly assail the Yale Department of Anthropology and Yale in general for having, and I quote from memory, "killed Edward Sapir." Again you will note the peculiar Jewish note here. Clyde felt, I think, that Edward Sapir's fatal heart attack had been brought on by the fact that the Yale Faculty Club had explicitly denied membership in the Club on the ground that he was Jewish [cf. Darnell 1990:401]. You recall, of course, that Edward Sapir was at this time the Sterling Professor of Anthropology at Yale University [,] having come from the University of Chicago to assume this highest honor that Yale had to offer him. Here again, you will see the very special place which linguistics occupied in Clyde's life.

But to go back now to some of the problems of English Anthropology. As I say, the relationship between Radcliffe-Brown, Malinowski, and American Anthropology was on the whole extremely poor. Certain notable exceptions occurred, for instance here—Radcliffe-Brown and Fred Eggan were very close[,] but the situation at Chicago, insofar as I can understand it now, did not differ radically from the situation at Yale when I was a graduate student there and Malinowski had just come from England to assume the Sterling Professorship.

There is now a belief among Anthropologists that all Sterling Professors die early in office—Sapir held the Chair and died in it, Malinowski held the Chair and died in it, and Linton assumed that Chair and died in it—it has not been occupied by an Anthropologist since.

Murdock was Chairman of the Department at that time, having a three-cornered feud with Osgood and Spier (Spier went to New Mexico). Malinowski had the gravest public contempt for Murdock and exhibited it freely. Ge[of]frey Gore[r] was at Yale at that time as a visitor[,] too, having gone in part at least to work with John Dollard.

The Anthropology practised in England was very highly regarded here. It was certainly read with interest and with care and I think that there can be no possibility that there was any serious or grave misunderstanding as to what the English Anthropologists were doing. Neither has there been any since. Although I have often said facetiously that the reason English Anthropologists are, on the whole, unaware of what Americans are doing is that it is written in a foreign language which is not ordinarily taught in English schools, and although it may very well be that much of English Anthropology loses something in its translation into American, the fact is that the only Anthropology which we seriously fail to read, (but which we should read) because of language difficulties remains the Anthropology from France.

So your page 15 troubles me very much. On the one hand, relations between some English anthropologists and many American anthropologists were extraordinarily strained personally. On the other hand, the work of the English anthropologists was seriously read, seriously considered and I believe quite clearly understood. It is certainly true that Radcliffe-Brown made a very sharp distinction between "culture" and "social structure," treating culture as having to do with ornaments, styles of hair-dress and skirt-length fashions (as Kroeber and Richardson studied and reported them). It is certainly true that Radcliffe-Brown claimed that he was studying "social structure" and that he formally defined this as the concrete network of actual social relations which existed in a tribe!! It is also true that Malinowski wrote a long and vivid paper for the Encyclopedia of the Social Sciences on "culture" in which he spent much time on interaction, the meeting of biological needs as functions of "culture."

I offer a simply "political" hypothesis: the word "culture" was used by Malinowski as his intellectual "trade mark" and respected as such by Radcliffe-Brown. The words "social structure" were used by Radcliffe-Brown as his intellectual trade-mark and respected as such by Malinowski. There was no love lost between these two, and their antipathy gained them strong allegiance and close followings. But one should certainly not confuse trade-marks with important analytic concepts. Any reasonably close study of the work of the "structuralists" shows, I think, that up to the point recently when Gluckman seized it and retreated full steam ahead, "social structure" was quite properly described as consisting of a potpourri of things, some of which was "social structure"[,] some "culture"[,] and mostly the largely undifferentiated treatment of primitive societies [cf. Stocking 1995: passim]

Only gradually in the work of Evans-Pritchard, Fortes, and then Leach and others, has the notion of "social structure" come to have been refined into what you (and many of us) regard as "culture." That is, Evans-Pritchard in his Zande Witchcraft, Magic and Oracles, in his classic The Nuer, in his Nuer Religion is more and more concerned with what the English sometimes denegratingly refer to as "ideology"—i.e., systems of norms, beliefs and values as they are stated in the norms for the composition of non-existent groups, norms for the understanding of non-existent supernatural forces, and so on.

And the point of central intellectual concern here is that Evans-Pritchard took his inspiration mainly from Levy-Bruhl, Durkheim, Mauss and much less from Radcliffe-Brown than has generally been supposed. At this time, today that is, the intellectual connection between Levi-Strauss, Lienhardt, Needham, Pocock, Evans-Pritchard, Leach and their intellectual progenitors Durkheim, Mauss, Levy-Bruhl, Granet is very close indeed, and clearly acknowledged to be so. On the other hand, Fortes stands firmly with Radcliffe-Brown and Malinowski, so close were they, while Gluckman reverts, as I said, to very early undifferentiated interaction and so feels close kinship with Margaret Mead. Firth has maintained his uncommitted, intellectually eclectic position unsullied throughout.

I repeat by picking up a different line. A piece of most important mythology which is widely believed is that American anthropology has to do mainly with the study of culture; English with the study of social structure, these

two being contrasted categories. This is explained, the myth states, by the fact that American anthropology lived on long dead indian tribes, where the culture was recovered from the last surviving senile old man who remembered what his father had told him had been the state of affairs when the father had been a boy. English anthropology, studying "living peoples, and going concerns" (have you ever read the introduction to *The Nuer* and discovered how they had been "pacified" before E-P got there?) working in their own Colonys [sic], studies "whole live societies and their social structure." Evans-Pritchard's little book *Social Anthropology* states this in its nicest form even to an allusion to a school of American Anthropology called "the wind in the palm trees school" (Margaret Mead of course!). But this is all nonsense, except of course, as myth stands for the kinds of relations which American and English anthropologists have to each other. The exchange between Murdock and Firth in 1951 (American Anthropologist) makes the historical facts quite clear.

If one reads American anthropology and English anthropology with the simple guide "culture = social structure" one can read it quite as well as if one reads it "culture  $\neq$  social structure." It is a matter of utter indifference to the sense of the works.

It is only very recently—within the last 10-15 years [--] that the culture-social structure distinction as an analytic distinction of self-consciously employed utility has made its appearance among some of the younger, theoretically more sophisticated anthropologists (Geertz, Fallers, Leach, Levi-Strauss, Dumont, etc.)

It is at this point that linguistics becomes relevant to the discussion again. You must remember, first, that David Aberle wrote a very good little paper for the Leslie White festschrift in which he described the role which linguistic theory and the study of language played in the development of culture-personality theory. This is an excellent and much neglected little paper. It has its counterpart in the summary paper which Dell Hymes wrote for the *Transcultural Studies of Cognition* recently issued by the American Anthropologist as a supplement.

For the fact is that from its earliest period, language as an "institution" has consistently provided certain of the theoretical advances which anthropology has applied to other areas of culture. The early philologists (Grimm et al) complemented the Burkhardts; each dealt with symbol-value systems, these framed within sets of normative "rules" for their manipulation. Then a shot in the arm from the biologically oriented, the "human needs" people, and then stagnation again. Now again linguistic theory—much of it developed in the 1920-30's (by Clyde[']s much admired Roman Jakobson among others at that small, eastern sectarian university, Harvard).

The new surge in anthropological theory has been from structural linguistics via Levi-Strauss in France and England, and via Goodenough, Lounsbury and their followers, Conklin, Frake, Hammel, Metzger etc. The French-English read a few Americans, but not many. Levi Strauss was fully in prolific production by 1945-46.

Hence the problem at Harvard when I was there in 1951-55 over whether a structural linguist was or was not needed on the social relations staff was no mere gambit in a game of empire which Clyde was playing. Clyde believed and believed so on firm, well established evidence, that a constant source of theoretical innovation in what he took to be Anthropology's fundamental intellectual task, the study of culture, was from linguistics. It had been in the time of the early philologists, it continued to be during the reign of Culture-Personality theory, it remained so for his own intellectual hero, Edward Sapir—the master linguist and genius—. When Alex Inkeles walked around and kept saying "Why should social relations hire a linguist? Can't anthropology students learn languages in the field without having a course in linguistics?" he was just simply, wholly and abysmally off the point which Clyde was firmly committed to.

Language was in microcosm "culture" in its entirety. If the theory of language, and linguistic structure, could solve some of the problems of language, the same theory might well work on other parts of culture. And so Jakobson

showed, and Clyde read it and saw it. And when he came back from the Wenner-Gren conference Anthropology Today in New York in 1951 he said that there was only one person there who was really brilliant—who really had something important to say—and that was Levi-Strauss.

You put it correctly: Clyde's affinity was with linguistics and clinical psychology, and against sociology and British anthropology. He was against certain British anthropologists, but not against the anthropology which certain Britains practiced. I hope that is clear now.

But you are in error, I believe, in equating certain work in British Anthropology with that of sociology and contrasting this with the work of American anthropology. I hope I have made that clear now. Clyde's antipathy was to these few Englishmen, and this was not the ground from which it was "generalized" to sociology at all—simply because there is not the link you suggest, there is not the path along which it could be generalized.

May I repeat here and in confidence, I hope, another anecdote which I hope will make clear what I think is in part at work in this particular matter. Clyde had the deepest respect for and urged all of his students to read Evans-Pritchard's monumental work on Zande witchcraft. I think you will agree that this is certainly one of the landmarks in the study of Religion. But E.E. Evans-Pritchard himself was simply an unmitigated, unqualified, no good, son of a so and so. You remember that Clyde went to England once around 1951[, and] he had written ahead to Evans-Pritchard saying that he would like very much to visit him at Oxford, that he had been a Rhodes scholar at Oxford, and so on. E-P wrote back and said please come to tea at the Institute for Social Anthropology on such and such a day, at such and such a time. Clyde appeared at the appointed day at the appointed time and the secretary said that she was sorry that Professor E-P would be somewhat late, and would he please help himself to some tea. Fortes was there and at the end of the first hour quite uncomfortable, went to the phone, called E-P, came back looking quite perturbed, and said that although E-P had been at home, he had mumbled something incoherent and it did not seem that he cared to come have tea with Clyde. Meyer Fortes[,] who has always at least been courteous[,] was very put out and very distraught at this obviously staged insult.

When I first came to Harvard in 1947, one of the first series of books that Clyde insisted that I sit down and read was Nadelf[s] Black Byzantium and Evans-Pritchard's The Nuer along with Witchcraft, Oracles and Magic Among the Zande.

Quite distinct, however, from his relations with the "structuralists" were Clyde's very warm friendly relations with Raymond Firth and Audrey Richards from the London School of Economics.

Two problems remain here. First, why were Clyde's relations with Sociology in general luke warm at best, and secondly, why did he have such a passionate aversion to kinship and family as objects of intellectual inquiry.

On the second I will not hazard any guesses of a personal nature. I do not think, however, that one can overlook the fact that "the family" has always been one of Florence [Kluckhohn]'s central intellectual and professional concerns. I also remember very vividly one time having a discussion with Clyde on this subject. I had, you remember, "discovered" kinship for myself in my own fieldwork on Yap [cf. Bashkow 1991]. When I came back it was one of the central elements of my thesis. At one point prior to the completion of the thesis I had a talk with Clyde in which I tried to convince him (having just discovered it myself) that kinship like anything else had a cultural as well as a social aspect and that one need not confound the two or identify them and that in an important sense my thesis was concerned with the cultural aspects of kinship and had very little to do with the social aspects of kinship. I knew that Clyde hated ["kinship"] and I was trying to convince him that after all it was precisely the object that he himself said he was passionately interested in—namely culture. Having delivered what I thought was a perfectly rational, perfectly clear argument, I looked to see Clyde change his mind. (One of my own forms of pathology is to

believe that once people see the facts correctly they will then necessarily change their mind.) Clyde just looked up at me and said, ["Jof course, did you think I didn't know that?["]. And I was somewhat crushed since I thought I had brought to him a brand new insight.

On page 16, therefore, when you say that Clyde minimized the central significance of social organization I think that this statement covers too much. And when you say that he was rather cold to the important contributions of a number of British structuralists, I think that you are factually in error. I do not think Clyde missed these contributions at all. [But] He was certainly not given to loudly proclaiming the praise of individuals who had treated him personally quite badly.

It may be that Clyde's relations to Sociology and the problems of quantification are not entirely unrelated. Certainly much of modern Sociology, particularly in America, has devoted itself to quantitative problems, scales, measurements, sampling, and statistics of all sorts. Equally certainly, as you will recall from Clyde's very important paper on the "Kulturkreise Lehre" in the *Anthropologist* for about 193?[, Clyde's distaste for quantification did not arise from his inability to handle these matters as it does with some of us. Clyde could handle a considerable sophistication of higher mathematics and when he was at the Center 1954-55, took a course that one of the fellows was offering there in higher mathematics which could be of use to Social Scientists. His ambivalence, therefore, to quantification, I suspect, had a great deal to do with the fact that quantification was so closely associated with Sociology.

Quantification was antithetical to another one of Clyde's fundamental values and that was his insistence that Anthropology in its methodology centered upon the "clinical" relationship between the Anthropologist and informant or (as in the Benedict) his data. This was a relationship tied in with the notions of insight, understanding, empathy, etc. and this in turn is related to the fact that he saw culture as the central and major dominating problem of Anthropology. There was only one way in which the material and data that was necessary to understand or study culture could be obtained and that was by this "clinical" relationship between the student and the object of study.

Here then is still another link, I think[, between Clyde's affinity for personality and clinical psychology and Anthropology. I recall many, many times in the discussions of field work, the nature of field work and why field work was central to Anthropology as it was clearly not central to Sociology[, Clyde's insistence that the field experience was in itself a kind of psychoanalysis[, that the field experience was fundamentally revealing not only about the other culture but about the instrument of study i.e. the Anthropologist himself.

The fundamental problem of Anthropology for Clyde was the problem of culture. The fundamental methodology of Anthropology was field work and the field-clinical relationship between the Anthropologist and his informant.

Here then is another aspect to the problem of meaning and understanding which I think dominated Clyde's view of the nature of Anthropology and most important in this connection with the nature of the difference between Anthropology and Sociology.

It should be unnecessary for me to go on in detail any more about what I recall to have been Clyde's main commitments in Anthropology. Certainly, his concern with culture, and values as an aspect of culture became, as you say, the object of most intense concern for him. And for me the key phrases have always been ones that Clyde used, that is that culture is "patterns for behavior and patterns of behavior." Although I frequently argued with him that patterns of behavior did not deal with exactly the same thing, he insisted that this remained to be seen and could not be omitted.

It is here, I think, that on page 28 when you quote Clyde's dissenting opinion [cf. Parsons & Shils 1951: 26-27] that I read into his words somewhat more than you do. I am not certain I am right in this matter, but I would suggest that there were two things involved in his ambivalence to the theoretical status of Sociology as a discipline. The first was his somewhat different interpretation of the biological element than appears readily in much of the Sociological literature. Here [on] page 18 the word you use is "de-emphasize" when you say "in this context it is not surprising that there has been an increasingly broad trend among Anthropologists to de-emphasize the biological especially the hereditary concerns of psychoanalysis in favor of a cultureology." It is less, I think, a matter of de-emphasizing biological than of emphasizing the fundamentally biologically determined plasticity and open endedness that is and was for Clyde the central point of biology. I have a feeling that it is in this sense that Clyde was suggesting in the dissent which you [quote] "that there may have been a more clearly culturally patterned aspect to the interactive social processes" than you seemed to be willing to concede. His dissent was, therefore, of the order of leaving this more in the nature of an open problem than anything else.

The second source, I think, of his dissent arises from first principles. I think that he felt, as others have felt, that if one takes action theory as one's fundamental problem then culture becomes very much a question of its role in determining action. There are, however, other questions that can be asked about culture and in Kroeber's famous phrase "the nature of culture," than those entirely confined to its role in determining action. The nature of the patterns, ways in which patterns become determined, etc. are not immediately relevant as determinants of action and I suspect that Clyde felt that questions of this order and questions of the order that Kroeber continually raised along with those of Redfield and others were in some way slighted or in some sense treated as secondary. And in this some of his ambivalence toward Sociology is explained. To repeat and put it simply he felt that there were many Anthropological problems which Sociology as a theoretical discipline simply did not face or attempt to cope with and, therefore, his own relations with it were correspondingly uncertain.

Finally, of course, one should not neglect the fact that in the Department of Social Relations there was an unequal distribution of charisma. At times some people suspect that Clyde's dissent was in fact aimed at precisely that problem. I know that I had a very long and very intense, slightly bitter[,] argument with Cliff Geertz in the middle of Harvard Square one day in 1953 or 1954 over this very problem. I think that I was able to convince him at that time that this did not explain anywhere near all of Clyde's ambivalence with Sociology. And I would insist that it only accounts for, in fact, a small portion of it.

Let me try to summarize some of these points. Clyde was primarily committed to Anthropology in both its senses: as a discipline of certain intellectual questions, and as a profession. The history of that discipline was certainly closely related to the members of the profession. Clyde held Boas, Kroeber, Sapir and Redfield dear above other men. You do not mention these men and perhaps should. Clyde held these men dear for both their intellectual and their personal worth. The intellectual problem was seen as the study of culture—sometimes phrased as the study of man, and man's distinctive species, and specific characteristic, culture.

In the history of both the discipline and the profession, linguistics and linguists and language played a central role. The linguists (the early philologists) had been among the first to state the symbolic, "patterns for behavior" aspects of culture—in the 1800's, though often the late 1800's. The source of much of "anthropological theory" had been and continues to be linguistics. Boas and his texts came long before Malinowski and his. And Boas and his texts now bridge to the next point, namely, that to study culture takes an empathetic, intimate, soul-shaking experience in the field in relationship with informants. One learns to think and see the world as "they" do. So the kinship Clyde felt for both linguistics, language, and psychology (clinical). Finally, to cap it off, there was the special relationship to a particular linguist, Edward Sapir, who within himself contained all of the "good" things—genius at linguistics, relating it to personality theory, master of Navajo as a substantive body of material which Clyde shared, and so on and so forth. So too the link with Yale.

May I add one, final criticism: you have all the elements to state Clyde's central intellectual problem but you do not put it clearly enough for me. The alignment of the study of culture with clinical psychology and linguistics makes sense, as I say, for the link they have through field work as the basic setting for the informant-anthropologist relationship as the methodological tool of Anthropology.

But it is precisely the confusion between the "systems" of culture and personality, rather than their systematic separation[,] which wrecks so much of Clyde's work. His books with Leighton and his Navajo Witchcraft are all marred by this. It was only as he began to go back to structural linguistics, to formal analytic techniques and consider these as "cognitive" that this dilemma and confusion began to diminish. Focusing on values left too much "personality" kicking around; but he had begun to fight free of it I think. His death cut that development short.

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In the copy I have, Schneider's text breaks off at this point, at the bottom of its tenth page, without any formal phrases of closing. There was, however, a two and a half page sequel (not reproduced here), on January 18, 1965, in which Schneider, responding to a letter from Parsons, backed off just a bit from certain formulations where "I think now I overstated the case"—but insisted that "so much of Clyde's intellectual work can best be seen and understood with reference to his personal situation—the object of Peabody's venom [i.e., the more traditional anthropologists in the Peabody Museum], the thin-skinned butt of E-P's snubs, needing desperately to have an intellectual identity and an intellectual commitment, but having much of it located in aspects of his own personal relations with Sapir." Parson's draft (of which I have no copy) is presumably an early version of an essay that eventually appeared in the 1973 Kluckhohn commemorative volume [Taylor et al. 1973] as "Clyde Kluckhohn and the Integration of Social Science." As published, it appears to have been substantially modified—although the only explicit reference to comments made by Schneider ("in personal correspondence") refers to a specific theoretical issue on which Parsons "was entirely ready to accede": namely, that "another basis of Kluckhohn's dissenting note [in Parsons & Shils 1951] was probably his conviction that the relevance of culture to the theory of action by no means exhausted its importance in human affairs." [Taylor et al. 1973:55] It is worth noting that the January 18<sup>th</sup> letter to Parsons was signed by Schneider as "Chairman" (of the Anthropology Department), and was typed by "pb," which suggests that both letters to Parsons may have been dictated or transcribed from a handwritten draft—and raising thus yet another question about the historiographical issues suggested in the title of this re-presentation, including especially the relation of oral and written sources.

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## SOURCES FOR THE HISTORY OF ANTHROPOLOGY

**Archives of the Berlin Anthropological Society--** The Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte, founded in 1869, was the most important institution for the study of physical and cultural anthropology and European prehistory in Germany before the second World War. Remembered in the United States as a context of Franz Boas' earliest anthropological work, it merits attention as a peculiarly German school of anthropology, distinct from United States traditions of cultural anthropology. Its archive has survived in a single attic room in the Museum für Vor- und Frühgeschichte in Berlin. Consisting of largely unordered boxes hastily packed up during World War II, the archive holds many buried treasures for historians willing, literally, to get their hands dirty digging.

Among the materials in the archive are letters from the Prussian Ministry of Culture regarding the founding and funding of the society and minutes from the meetings of the board of directors and steering committee, as well as the card catalogue of the society's library (which disappeared during World War II), and documents relating to the exclusion of Jews in 1933, when the society willingly cooperated with the Nazi Gleichschaltung, or "coordination," of German public life. Among the 19<sup>th</sup> century materials are letters from (and to) variety show impresarios, arranging for the study of non-European and deformed people performing in Berlin—which were an important source material in the pre-fieldwork period.

The archive also contains documents of the scientific activities of Berlin anthropologists, including sketches made by Rudolf Virchow in 1872 of the Neanderthal skull which he argued (to the satisfaction of many) was an injured and diseased human skull, rather than representing an intermediary race or species between apes and humans. Also important are documents from the Virchow Foundation between 1905 and 1909, which contain plans and budgets of scientific travelers, as well as the Foundation's evaluations. And there is a large photography collection including pictures of artifacts and "representatives" of various races, as well as birth defects and Wilhelm von Gloeden's erotic studies of Sicilian boys.

In short, the archive is valuable, and full of surprises, and scholars conducting research in the history of German anthropology will surely come across interesting documents not mentioned here. For more information, contact Dr. Gustav Mahr, who directs the archive, and has done a great deal of research on the Berlin Anthropological Society, at the Museum für Vor- und Frühgeschichte, Schloss Charlottenburg, D-10459, Berlin, Germany. [Andrew Zimmerman]

## RESEARCH IN PROGRESS:

Matthew Engelke (University of Virginia) is beginning an oral history project on Victor Turner's life and work, Edith Turner's anthropological career, and their academic relationship with one another.

Andrew Zimmerman (University of California, San Diego) is completing a dissertation on "Anthropology and the Place of Knowledge in Imperial Berlin" based on a wide range of written and visual sources, including government and museum archives, material culture artifacts, newspaper and police files relating to displays of living "natives" in zoos and cabarets, a basis for the reconstruction of anthropology not just as a system of ideas, but as a culturally embedded phenomenon involving anthropologists, the people they studied, their audiences, and their political and financial patrons.



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### I. Recent Dissertations (Ph.D. except where otherwise indicated)

Hannah Augstein (University College, London) has defended a dissertation on "James C. Prichard's views of mankind: An anthropologist between the Enlightenment and the Victorian age."

Jennifer Hecht (Columbia University, 1995) has completed a dissertation on late 19<sup>th</sup> and early 20<sup>th</sup> century French physical anthropology, entitled "Anthropological utopias and republican morality."

Susan Krook (University of Colorado, 1993) "An analysis of Franz Boas' achievements and work emphasis during the last five years of his life, based on documentation and interpretation of the Federal Bureau of Investigation file maintained on him from 1936 to 1950."

Filippo M. Zerilli (Universita' degli Studi di Roma 'La Sapienza', Dipartimento di Studi Glottoantropologici), "Alle origini dell' ethnologia francese: Elementi per una biografia intellettuale di Paul Rivet."

### II. Recent Work by Subscribers

[Except in the case of new subscribers, for whom we will include one or two orienting items, "recent" is taken to mean within the last two years. Please note that we do not list "forthcoming" items. To be certain of dates and page numbers, please wait until your works have actually appeared before sending offprints (preferably) or citations in the style used in History of Anthropology and most anthropological journals]

Bieder, Robert 1995. Due gentlemen di Filadelfia e il loro contributo alle origini dell'antropologia americana. La Ricerca Folklorica 32:29-36.

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### III. Suggested by our Readers

[Although the subtitle does not indicate it, the assumption here is the same as in the preceding section: we list "recent" work--i.e., items appearing in the last several years. Occasionally, readers call our attention to errors in the entries, usually of a minor typographical character. Typing the entries is a burdensome, and under the pressure of getting HAN out, some proofreading errors occasionally slip by. For these we offer a blanket apology, but will not normally attempt corrections. Once again, we call attention to the listings in the Bulletin of the History of Archaeology, the entries in the annual bibliographies of Isis, and those in the Bulletin d'information de la SFHSH [Société française pour l'histoire des sciences de l'homme]--each of which takes information from HAN, as we do from them--although selectively]

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## GLEANINGS FROM ACADEMIC GATHERINGS

### Teaching the History of Anthropological Theory: Strategies for Success

Paul A. Erickson  
Saint Mary's University

[Due to space considerations, this account was omitted from our June number.]

History of Anthropological Theory can be one of the most challenging anthropology courses to teach. The material is abstract, detailed and, for many students, just plain boring. One teacher describes her students' expectations as "one dead guy a week." The unenviable reputation of this course is unfortunate, because the course is at the core of college and university anthropology curricula throughout North America. Many anthropology departments consider their course in History of Anthropological Theory to be the "capstone" of their students' careers. If the course is ineffective, large numbers of students are being ill-served.

In November, 1995, at the 94th Annual Meeting of the American Anthropological Association, Paul A. Erickson (Saint Mary's University) chaired a session on "Teaching the History of Anthropological Theory: Strategies for Success". The session, co-sponsored by the Council on General Anthropology and the Society for Anthropology in Community Colleges, identified challenges faced by teachers of History of Anthropological Theory and showed, by example, how those challenges can be overcome with success. The session comprised 11 papers followed by a commentary and discussion.

The opening paper by Erickson, "Teaching the History of Anthropological Theory: State of the Art", framed the session by presenting results of a survey of History of Anthropological Theory courses taught across Canada and The United States. Approximately 80 anthropology departments provided information on a number of course features, including purpose, level, prerequisites, enrollment, theoretical orientation, format, readings and manner of selection of instructor. Erickson found that while enthusiasm for the course varies, among both students and teachers, there are common challenges and rewards, most often "getting students to think".

Papers by Mark Moberg (University of South Alabama) and Mary W. Helms (University of North Carolina, Greensboro) showed how student resistance to History of Anthropological Theory can be overcome by teaching it from the perspectives of the philosophy of science and natural history. Moberg's paper, "Philosophy of Science in Anthropology: Overcoming Student Resistance to Disciplinary History", showed how the concepts of paradigm and scientific revolution help students understand major theoretical shifts in anthropology, and draw their attention to the historical and social contexts in which anthropologists produce knowledge. Moberg reinforces this approach with role-playing assignments in which students make team presentations of works that uphold or criticize past and present paradigms. In her paper, "Teaching Anthropology as Natural History: From James Hutton to Lévi-Strauss", Helms placed the history of anthropology within the broader framework of the history of natural science from the Renaissance through the 19th century. Helms helps her students understand the difference between faith and reason while she traces the history of the discovery of natural processes in geology, biology and, then, anthropology. This approach makes anthropology seem less exotic.

Papers by Jay K. Johnson (University of Mississippi) and Franklin O. Loveland (Gettysburg College) showed how Marvin Harris' widely used (and avoided) textbook The Rise of Anthropological Theory can be made more accessible to students. In his paper, "Fifteen Years of Teaching Anthropological Theory: An Evolving Strategy", Johnson characterized Harris' volume as "a demanding book that has an obvious theoretical bias which is presented in a style that invites disagreement." Johnson uses the book to teach critical reading skills by means of weekly assignments and research papers that inform classroom discussion. In his paper, "The Rise or Demise of Anthropological Theory:

Teaching Marvin Harris's Theory Book to Undergraduates", Loveland described accommodations to the book ranging from panel discussions and videos to an anthropological "quiz show" in the format of "Jeopardy".

Papers by Alan Sullivan (University of Cincinnati) and William R. Fowler (Vanderbilt University) addressed problems teaching the history of archaeological theory either on its own or as part of traditional four-field anthropology. In his paper, "Archaeological Theory in American Anthropology: Strategies for Teaching the History of Subfield Tensions", Sullivan contended that his students have difficulty adjusting to courses in archaeological theory because much of archaeological theory derives from theory in cultural anthropology. Sullivan overcomes this difficulty by showing students how archaeology can contribute to the solution of mainstream anthropological problems. In "A Dialogue with the Ancestors: A Strategy for Teaching the History of Archaeological Theory", Fowler explained how he teaches the history of American archaeology by having students critique papers published in American Antiquity and American Anthropologist. Fowler's course is organized chronologically, with these papers grouped into periods of approximately five years. In addition, each student assumes the identity of a major figure in the history of archaeology and argues from the perspective of that authority in a research paper and oral presentation.

The remaining papers showed how special strategies can make History of Anthropological Theory come alive in the classroom. In his paper, "'You Mean Lévi-Strauss Did More than Invent Blue Jeans?': Using the 'Field Guide' Approach to Teaching Anthropological History and Theory", James Stanlaw (Illinois State University) explained how he has students augment a handbook of key anthropological theories with biographies of major figures and summaries of seminal works. Stanlaw's aim is to let students know that the history of anthropological theory is not "static", but grows out of contested debates. Karen Field (Washburn University) began her paper, "Good Morning, I'm Dona Marina: Fostering Student Identification with a History of Theory Curriculum", by observing how students complain that the history of anthropological theory is "dry." Field responds to these complaints in a variety of ways: adding women and non-Europeans to the list of canonical theorists; asking students to convey the ideas of chosen thinkers orally "in character"; and inviting practicing social scientists to discuss the importance of theory in their lives and work. Fifteen years of course evaluations indicate that her strategy works.

In his paper, "Competing Paradigms and Hungry Hippos: The Search for the Elusive Marble of Truth in Anthropological Theory", Bruce Roberts (University of Southern Mississippi) discussed how an unusual pedagogical device – the children's game "Hungry, Hungry Hippos"—can be employed to illustrate the notion of competing paradigms in the anthropological quest for knowledge. Based on James Lett's book The Human Enterprise: A Critical Introduction to Anthropological Theory, Roberts' strategy invites students to employ this game as a metaphor for examining rival paradigms that appear incommensurable. In her paper, "Trying to Beat 'One Dead Guy a Week'", Julia Harrison (Trent University) described another unusual pedagogical device. Harrison has students stage, in writing, a conference on "Balancing the Local and the Global in the 21st Century", featuring three early anthropological theorists ("summoned from the world beyond") as speakers. Harrison finds that students respond to this exercise creatively and report that the course far surpasses the dreaded "one dead guy a week."

Discussant for the papers was Aram A. Yengoyan (University of California, Davis). Reflecting on the richness of teaching strategies presented, Yengoyan was pleased to observe that there is theory in History of Anthropological Theory courses and that many of the courses are designed to be capstones of anthropology curricula. At the same time, he was surprised that certain anthropological topics, notably kinship and cultural relativism, were virtually ignored. Yengoyan's comments led to a lively discussion among paper presenters and members of the audience, including Marvin Harris, who responded to some of the criticisms of his book.

HAN readers who want more information on the session can contact Paul A. Erickson, Department of Anthropology, Saint Mary's University, Halifax, Nova Scotia (TEL 902-420-5627, FAX 902-20-5119, E-MAIL perickso@shark.stmarys.ca). Requests for copies of papers should be addressed to authors. There is widespread desire to have the papers published. Suggestions for avenues of publication are welcome.