The Language of Ethnohistory

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RÉSUMÉ

La position prise dans cet article est que la méthodologie dans l'éthno-histoire devrait inclure les modes du discours et l'analyse de l'utilisation des concepts. De la précision et de la consistance dans l'utilisation et l'application des concepts sont nécessaires dans une discipline qui de plus en plus tire son information de diverses sources et ses concepts d'autres disciplines.

The language which the historian talks contains hundreds or words which are ambiguous contructs created to meet the unconsciously conceived need for adequate expression, and whose meaning is definitely felt, but not clearly thought out.

> – Max WEBER, *The Methodology of the Social Sciences* (1949). The Free Press: New York, p. 92-3

...without a constant misuse of language there cannot be any discovery, any progress.

> – P. FEYERABEND, *Against Method* (1975) Verso: London, p. 27.

A recent exchange in the literature (Gadacz 1981; Trigger 1982) has made it clear that despite the sophisticated methodologies of some interpretive social histories (e.g. Trigger 1976) philosophical and epistemic considerations remain relatively unexplored. There is no question that concepts like "understanding", "interaction", and "motives", and the use of the "interest group" as a unit of analysis, among others, have utility. Indeed, methodological advances in ethnohistory are sorely needed. It is the innovative ethnohistorian who can successfully write a history, ethnic or otherwise, and who does not lose sight of social science as the *raison d'être* for the account in the first place. The position taken in this paper, however, is that concept use must be accompanied by concept definition and

development.[1] It is argued that a successful methodology must be grounded in a so-called metalanguage which will serve to maintain high levels of precision and consistency in concept use and application. This is important in a discipline which increasingly draws its data from diverse sources and its concepts from other disciplines.

I

There are many définitions of ethnohistory and opinions of what its goals are (Symposium in *Ethnohistory*, volumes 8 and 9 (1961); see also Schwerin, 1976 and Carmack, 1972). Some scholars emphasize that ethnohistory is not a discipline but a method and technique. Others suggest that ethnohistory is indeed a discipline – one wherein "various explanatory approaches and methods of *scientific analysis* are more or less applicable" (Schwerin 1976: 329, italics added). In fact, the scientific approach looked at this way seems to provide for a sort of methodological *carte blanche:* "The healthiest situation for the full development of any discipline's potential is when its practitioners are free to apply *whatever* techniques are best suited to the analysis of a given problem" (Schwerin 1976: 328-329, italics added).[2] Science, however, cannot be approached with such a "natural attitude".

Whether it is called a discipline or technique, the writing of Indian history (or preferably "un-hyphenated history") precludes neither scientific discipline nor rigorous standards. Whether objectivity is attainable, in science let alone in history, is however another matter. Objectivity is no

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1 The present article is not so much intended as a reply to the original exchange as it is an attempt to go beyond the original argument to a more fundamental issue. In the earlier article (Gadacz 1981) I tried to show what could happen when concepts and terms remain undeveloped and in some cases undefined. In this article I hope to show why problems in interpretation can arise and how this can be avoided. My own work of course is no less free of the ambiguous, opaque and even contradictory use of social science concepts and terminology. Finally, the comments offered in the last article and in this one are in the spirit of academic debate, and are least of all to be construed as criticisms of any single individual's scholarship.

2 It would have been helpful had Schwerin defined what he meant by the scientific approach or "systematic scientific analysis", with respect to methodology in science (Nagel 1961). As part of the scientific approach are we to include, for example, dramatic metaphors such as Goffman's (1959) "all the world's a stage" approach to social interaction, or the dramaturgical technique that is Brecht's *Verfremdungseffekt* applied in ethnohistory by Trigger (1975)? These are not so much scientific as they are examples of humanistic approaches (when correctly applied). While not mutually exclusive (Truzzi 1974), science and humanism are *not* the same. An important question would be: Can a humanistic approach be carried out scientifically? See for example Riches (1982), but Martin (1978) is an extreme example.

longer an article of faith in either the hard or soft sciences – an "objective" study is no longer "more scientific". The historical enterprise is not dissimilar to the scientific one and it too is not immune to extremist positions and viewpoints. There are, for example, the scientific historians who seek to describe past events "as they really were" and who approach materials with complete objectivity (e.g. Ranke). There is, at the other extreme, the historical idealist view which sees the historian playing a creative role in writing history (e.g. Dilthey, Collingwood). Perhaps because there seem to be so many kinds of history (a situation probably more apparent than real – see the essays in Gardiner, 1974) anthropologists in the past have been somewhat schizophrenic with respect to the place of history in anthropological research (Hudson 1973: 118, 120-123; Carmack 1972: 228-232).

As in science, where scientific methods "deliberately and systematically seek to annihilate the individual scientist's standpoint" (Wallace 1971: 14), in history "the multiplicity of viewpoints that have gone into the writing of history over a number of generations ... tends to expose biases and thereby to endow history with a degree of objectivity that refutes its most determined critics" (Trigger 1978: 22). However, "the methodological controls of the scientific process thus annihilates the individual's standpoint, not by an impossible effort to substitute objectivity in its literal sense, but by substituting rules for intersubjective criticism, debate, and ultimately agreement" (Wallace 1971: 14). Methods deemed scientific constitute rules whereby agreement about specific images of the world is reached. Objectivity, then, is agreement and consensus about something. Importantly, criticism (in science though there is no reason why history cannot be included) "is not directed first to what an item of information says about the world, *but to the method by which the item is produced*" (Wallace 1971: 14, italics added; Nagel 1961). The argument, then, is that it is not so much a question of whether anthropology is a kind of science or whether it is a kind of history (Hudson 1973: 111) as it is a question of how anthropology – and historical ethnology or ethnohistory – obtains its facts and how it deals with its materials. As an aside, if there is to be any kind of objectivity *qua* consensus or agreement it would have to be with regards to method (but it would be going too far to say that historical ethnology or indeed anthropology should eschew criticism and debate for rules).

The concern with method in historical ethnology or ethnohistory should be obvious. Historical ethnology is the testing-ground for anthropological theory, and anthropology is holistic. Thus, theories in historical ethnology are derived from a number of disciplines. It follows that historical

ethnology also has multiple methodologies; if ethnohistory is itself a method or technique, it is one which makes use of a number of other methods. What they are, how they are put to use and whether one is more or less appropriate than another are standard scientific considerations. Ethnohistorians essentially depend on documentary evidence, such as chronicles, letters, diaries, records, reports, and so forth. But at the same time they rely "more upon auxiliary sources of data than does the regular historian" (Trigger 1978: 19). The historical ethnologist must therefore make use of and be familiar with the methods, skills and techniques of historians and anthropologists. He or she must, for example, be sufficiently aware of what historiography is, but at the same time know what science, and indeed social science, is all about.

As well, it should be recognized that use of auxiliary sources of data does not *by itself* constitute the inter-disciplinary approach. Archaeological data, for example, no matter what they may reveal about cultural development, are only as useful as the methodology that guided their recovery. The New Archaeology is a case in point. While it completely rejects any appeal to history (presumably in favor of formulating laws, something which critics have bemoaned – but see counterarguments in Salmon 1982: 20-26), its focus on the significance of the data themselves "has laid the foundations for far better interpretations of an historical sort than were possible previously" (Trigger 1978: 21). The methodology of the New Archaeology, despite its philosophy or perhaps because of it, produces data useful only to the ethnohistorian who can appreciate the difference in methodologies. The same should hold for methodology in linguistic analysis, ecological studies, comparative ethnology, physical anthropology, even history. But to discover order where disorder prevails (Hickerson 1970: 2) requires more than just data, however.

While there are many arguments in favor of diversifying the sources of information available to ethnohistorians, little attention is paid to *concept use* in those sources. Are not the terms, concepts and the language that the researcher uses part and parcel of the "other" data? Can we rightfully use ecological concepts in, say, an ecological study in historical ethnology (Bennett 1977, Glassow 1978)? Are we on safer ground when we use sociological terms like "interest group" or psychological terms like "motive"? Contextual criticism in this sense means analyzing or at least recognizing where these terms and concepts come from and what they mean (Fischer 1970: 37), even though they are familiar enough. There is little consolation in using a concept from another discipline in the name of the multi-disciplinary approach or even methodology, only to find that it is a

debated one that has six possible definitions. Operationalism, as this is sometimes called, is not highly developed in anthropology (Pelto and Pelto 1978: 38-53), despite the holistic perspective, the inter-disciplinary approach, and cross-cultural comparative research. It is universally recognized that a fundamental methodological requirement of all scientists is the need to operationalize concepts (Harris 1979: 14). While it is recognized that operationalism can be carried to extremes in science, in social science and even in history (Fischer 1970: xx),

> ...a strong dose of operationalism is desperately needed to unburden the social and behavioral sciences of their overload of ill-defined concepts, such as status, role, superordination and subordination, groups, institutions, class, caste, state, community ... and many others that are a part of every social scientist's basic working vocabulary. The continuing *failure to agree on the meaning* of these concepts is a reflection of their unoperational status and constitutes a great barrier to the development of scientific theories of social and cultural life (Harris 1979: 15, italics added).

We are inclined to worry less about it when we are working in the shelter of a "discipline" than when working on the so-called peripheries. Ethnohistory, which is guided by multiple methodologies and whose objectives we are told are still uncertain, is in a particularly vulnerable position with respect to problems of operationalism. Through no one's fault in particular, it is vulnerable to the use of "personalized concepts and idiosyncratic data languages" (Harris 1979: 15). In summary, the argument is that terminology, concepts and language use are as much data as anything else. How is this type of data to be treated?

II

The concern is not solely with operationalism; such a rational obsession is not advocated here. While the argument will return to operationalism later in the article, the purpose of this short section is to tie operationalism, and thus by implication methodology (of history as well as anthropology) to something else. Concern is with the sociology of knowledge, for it is in this branch of science that most if not all of the substantive issues in historiography actually have their parallel (e.g. critical analysis of documents, Pitt 1972: 46-62; the writing of history, Fischer 1970). Interestingly, Fischer (1970: 218, n. 3) in a long footnote discussing progress in the "new" (i.e. social) history commented that "German academic historiography may be the most backward in the world". The sociology of knowledge, historiography's parallel, deals with the socio-cultural factors associated with thought and its various forms of expression on a number of different levels, and it is the German contributions to the

sociology of knowledge that are by far the best developed and have left their distinctive mark on the whole field (Mannheim 1936; Adler 1966: 399-415 for outlines of the contributions of Szende, Adler, Lerner, Scheler, and Speier among others). Significantly the sociology of knowledge has its roots in the vast accumulation of historical scholarship that is one of the greatest intellectual fruits of 19th century Germany (which includes the work of Max Weber and Karl Marx), and as Berger and Luckmann (1966: 5) note, "the sociology of knowledge takes up a problem originally posited by historical scholarship", that is, the relationship between thought and its historical situations, between ideas and their social contexts.[3] Consequently, there has been no need for 20th century German historians to develop a historiography of a new (whatever that means anyway) social history. The sociology of knowledge *is* their historiography. In North America this situation does not obtain.

The sociology of knowledge as a comprehensive historiography seeks to observe how and in what from intellectual life at a given historical moment is related to existing social and political forces. It is concerned with the way in which systems of thought, whether cognitive or evaluative or both, are conditioned by other social facts (in this discussion the kinds or types or knowledge that exist are not relevant, but see Stark, 1977: 3-45, and Merton, 1973). It is a *comprehensive* historiography because, in combining the writing of social history with its own critique, it evaluates sources of data in the standard ways (e.g. observer bias, which includes the *Zeitgeist*) and examines the historian's own mental processes, common-sense knowledge of life, specialized knowledge, *Weltanschauung* (e.g. interpretational bias), based on his social position, interests, affiliations, and so forth. The sociology of knowledge ought to be ethnohistory's methodology, for it sensitizes the social scientist to a number of distinct modes of discourse, *one of which is his own.* In a multi-disciplinary and empirical setting this is crucial.

III

A reality of a multiple methodology situation such as what obtains in ethnohistory is the diversity of theoretical orientations which are an indicator of cognitive diversity. A consequence of that is a plurality of

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3 Intellectual roots go back to Durkheim, but especially Marx to whom we are grateful for the familiar statement – "it is not the consciousness of men that determines their existence, but on the contrary, their social existence determines their consciousness".

languages, or *modes of discourse*. Language is constitutive of social science practice and its subject domains, and is thus essential to the kinds of reality that there are (Stehr 1982: 47); in fact, concepts and terminology are created, modified and diffused along with theories and procedures so that their study cannot be undertaken independently from the latter (Stehr 1982: 48, n. 1). This is a well-known fact in the sociology of knowledge. This is what was meant when it was suggested that terms, concepts and language, as discourse or as knowledge claims, are data.

The idea of discourse is seen to provide a useful theoretical orientation for the analysis of anthropological knowledge, and is one that creates a linkage between cognitive and social processes, and the development of anthropology and anthropological knowledge. The degree of language formalization (e.g. linguistic homogeneity) in a discipline is generally taken as an indicator of the level of cognitive development of it as a *scientific* discipline. That is, "a discipline comes to be regarded as scientific when it is within the reach of speaking a language free of ambiguity" (Stehr 1982: 49, Scientific discourse is identifiable in terms of its differentiation from everyday discourse, and what differentiates it is linguistic homogeneity. If methods of scientific analysis are applicable in ethnohistory, and if ethnohistory is deemed a scientific enterprise, then important questions (not answered in this article) are whether multiple forms of discourse present an obstacle to the development of scientific knowledge in ethnohistory, or whether homogeneity of language is desirable or even possible.

One of the problems encountered in identifying the different modes of discourse in anthropology or ethnohistory, or in any of the social sciences, on the basis of language used to formulate knowledge claims is that of locating criteria for distinguishing different modes of discourse. Different systems of knowledge codify their claims in distinctive specialized languages which is subject to variation. In general, however, it could be argued that discourses range from the specialized to the everyday. Stehr and Simmons (1979: 146-147) propose a typology for the classification of the different modes. Natural discourse simply refers to knowledge claims formulated in the terms of everyday language concepts present in any given natural speech community. Even within this mode is evidence for specialization, e.g. restricted codes associated with social class or geographic area. Technical discourse (in occupational roles, for example) refers to knowledge which is formulated in a way which clearly distinguishes it from everyday discourse yet which is still intelligible to "outsiders". Finally, knowledge claims which have been formulated in terms of specialized languages of observation and theory refer to formal discourse. To some extent, consensus in formal

discourse becomes necessary as a pre-condition for communication, rather than an outcome of it (controversy over discourse in the sciences, characteristic of their development, is tolerated only insofar as it is regarded as transitory; in the social sciences this situation is endemic and persistent). Lachenmeyer (1971) simply distinguishes between "scientific" and "coventional" language.

If archaeology, anthropology or ethnohistory are scientific in their approaches and methods, then they ought to be characterized by formal discourse. We acknowledge this is not the case. As an aside, it is interesting to note that modes of discourse are manipulable. Formal discourse is part of the dominant conception of science by both scientific and lay persons alike, and a discipline's credibility and ability to obtain political and/or economic support from socially powerful groups in society is most certainly contingent upon the level of formalization of its mode of discourse (Stehr and Simmons 1979: 149). Anthropology as a social science is not exempt from this. At any rate, there is no method of selecting *a priori* a mode of discourse most appropriate for formulating particular knowledge claims. Winch (1958: 88-89), for example, has advocated a natural or everyday discourse for anthropology and sociology, while others like Nagel and Hempel view that as undesirable. Lachenmeyer (1971: 48) has observed that sociological language more closely approximates conventional (i.e. natural discourse) than scientific language; the same would hold true for anthropological language. Unlike sociology, however, the structure of anthropological discourse at first blush does not include as considerable a variation of specialized languages. Communication across sub-disciplines may be less problematic than between sociology and anthropology. This may be so because communication across specialties within sociology, where the variation of specialized languages ranges conceptually from naturalistic to highly formalized formulations, is said to contribute to practical difficulties in sociological research (Stehr 1982: 48). With respect to language/concept usage, excursions into sociology by anthropologists have therefore to be made with caution.

A brief comparison of conventional and scientific language usage would be helpful. Scientific language systems demand a much greater control over language usage than do conventional language systems. By control over language usage it meant the use of devices to increase precision of usage, and precision refers to the degree or extent of consensus in the use of language and linguistic elements such as words, phrases, *etc.* Agreement between users of linguistic elements has, according to Lachenmeyer (1971: 23) three components: semantic agreement, grammatical agreement, and

contextual agreement. Semantic agreement refers to the judgement by users of a term that the term's object predicate reliably designates the empirical reality they are supposed to designate. Contextual agreement, for instance, refers to how the object predicates of one term are affected by the object predicates of another term. In addition, there are two devices which increase the precision of usage, namely *definition* and *systematization* (Lachenmeyer 1971: 24-25). Definition of terms is the most common way to obtain semantic agreement, and definitional *chains* increase contextual agreement (e.g. the simplest chain would include definitions of each component, actual or implied, or a term, for example, for the term "interest group", both "interest" and "group" would have to be defined). In addition, there are two kinds of definitions – nominal and operational (Lachenmeyer 1971: 53-57). Nominal definition is definition by agreement, where "users of a term agree that a particular set of predicates are the most appropriate defining predicates of a term". Operational definitions are those which interpret the nominal definitions of terms, concepts and so forth in to terms that are acceptable for research. Very often in the social sciences distinctions between the two types are not made, so that the danger increases that arbitrariness occurs in definition formation (Salmon 1982: 143-150). What occurs is the "nominalist fallacy". Though it is recognized that researchers are under obligation to define terms explicitly and to remain consistent with the definitions, it is not uncommon and is indeed acceptable for terms to be defined in ways deemed "most appropriate" to a given situation (this parallels Schwerin's idiosyncratic attitude to techniques in a scientific approach, and Trigger's (1978: 18) chastisement of amateur historiographers who prefer those sources that are most congenial to their own interpretations). The nominalist fallacy fails to consider the constraints placed on all definition formation:

> Any definition is only as good as its empirical utility in a theory language. This utility has two dimensions. First, the defined terms must be used to facilitate the formation and derivation of theoretical statements that enhance explanation of the phenomena in question. Second, those terms that are defined must designate or be used to permit the designation of recurrent, stable, and discriminable empirical objects, properties, or relations ... [T]hese are limits to the arbitrariness of nominal definition formation (Lachenmeyer 1971: 54-55).

The second device having to do with precision in language usage is systematization, which refers to "the ordering of linguistic elements and specifying the exact relations between them so that the deductive and inductive logical processes are facilitated" (Lachenmeyer 1971: 24; Salmon 1982: 150-157). Rephrasing and axiomatization to form sets or hierarchies of statements are examples of this procedure.

These are only some of the linguistic problems facing discourse in the social sciences. Unlike scientific language, social science language has minimal control over definition and systematization which function as a "metalanguage", or language control procedures. Insofar as the social sciences lack this control, theirs is like a conventional language system. Conventional language systems exhibit greater tolerance to language use in a number of other respects too:

> *Vagueness* exists when a term or expression has multiple, equiprobable, specifiable referential meanings. *Ambiguity* exists when a term or expression has multiple, equiprobable, specified referential meanings. *Opacity* exists when a term or expression has no referential meaning. *Contradiction* exists when a term or expression has logically inconsistent referential meanings (Lachenmeyer 1971: 30-36, 58, italics added).

The term equiprobable refers to when a term can have a number of equally legitimate meanings. Arbitrariness in definition formation, especially in nominal definitions, contributes to all four of the above language control problems. These problems are not present in scientific language. We have only to consider Kroeber and Kluckhohn's (1952) thorough critical review of the concepts and definitions of the term "culture" to appreciate the argument (see also *ibid.*, 41-42). Examples of vague terms would be "social structure", "function", *etc*; ambiguous terms would be "status", "power", "role", "group", "culture", *etc*; opaque terms are those like "social system", "role", "institution", "norm", *etc.,* which are used as if they designate directly observable things. These terms do not refer directly to empirical events – there is no one-to-one correspondence with physical reality. Other problem terms are "rights", "obligations", "values", "order", "competition" – the list goes on. The way each researcher uses a term will determine whether it is vague, ambiguous, or whatever.

There are two possible solutions to language problems such as these in a conventional language system. One solution is to repeatedly use particular terms in ways that can narrow the gap between nominal and operational definitions, provided that definitions are even offered. Their range of empirical referents must also remain fairly constant for any agreement in usage to result. Another solution is to call for extensive explication and definition to account for and to anticipate diverse and dissimilar referential meanings (predicates). However, this task is virtually impossible to accomplish since predicates that explicate a particular term are themselves subject to explication of infinite regress (Lachenmeyer 1971: 110). Definitions must therefore (unfortunately) remain open-ended. Nevertheless, definitions may themselves be offered and consistency maintained within a single empirical study or theoretical statement.

That language use is of concern to those interested in the growth of knowledge (Stehr 1982: 54-55) can now be more fully appreciated. In addition, inter-disciplinary "borrowing" of terms as well as concepts without definition and systematization, and unfamiliarity with possible problems connected with the term(s) or concept(s) even within the "donor" discipline (Salmon 1982: 165-166; Lachenmeyer 1971: 112) also presents an obstacle to raising the conventional language of anthropology (or any social science) to the status of a more scientific language.

IV

Returning to ethnohistory, it is felt that empirical research will in the future become more complex and methodologies increasingly sophisticated. If so, then ethnohistorians would do well to "watch their language". The issue is not simply one of rhetoric, however. Hickerson (1970: 1-2) wrote that "the work of the anthropological ethnohistorian must be highly interpretive. He is inclined to find clans in societies in which they were not described as such ... He must be prepared to *conjure up* formal structures where none seem to exist ..." (italics added). The anthropological ethnohistorian is likely, then, to conjure up interest groups, roles, social interaction, personality types, and even motivations from the documents (Trigger 1976: 1-26). A comprehensive historiography is required for this enterprise, which must include language and concept analysis involving, at the very least, attempts at definition formation at one or both of the two levels that were described. For the purposes of a particular study, it suffices to offer either nominal or operational definitions – nominal if the terminology derives from elsewhere other than the study itself or the discipline in which the study is conducted, and operational if from either. Internal consistency should be maintained, especially if use of terms and concepts are intended to be the same in more than just one study.

There is no reason why, in ethnohistory or in anything else, research results and interpretations should be accepted without question. Knowledge producers in our century are neither authoritarians nor mystics (Wallace 1971: 11). Particularly in a situation where replicability is impractical – a situation that obtains in ethnohistory – agreement on concept use and terminology is critical. If a scholar takes ten years to sift through documents, analyze the "data" and write an interpretive account, it might take a scholar of comparable skill (and endurance) a similar length of time to verify, reanalyze or reinterpret the same documents (if they are even accessible to him or her) to see whether different data might emerge. Few scholars will

devote time and energy to reanalysis as a consequence, and would prefer to write their own accounts. Reanalysis is not impossible, but it is unlikely. My own work, which will take the form of a comprehensive and exhaustive anthropological ethnohistory of the Montagnais-Naskapi of Labrador and Quebec, even at present involves more original research than reanalysis or earlier works (unless they were of sufficient "antiquity" that made it worthwhile to see "how far we have come" in analysis). It has already involved five years of sporadic effort, the last one of which has included study in such areas as sociology of knowledge, cognitive anthropology, phenomenology, symbolic interaction, and others. I, too, am interested in discovering order where disorder seems to prevail, to use Hickerson's phrase, and I see the challenge in methodology and theory.

It has been said that the only study worth pouring over is the one that stimulates thought and excites the imagination, not because of what it contains, but because of its possible influence and effect on the future of the subject matter or its methodology. Trigger's (1976) *The Children of Aataentsic* is such a study. Concern has been with the terms and concepts employed in that study (Gadacz 1981). Several of the problems associated with some of them are outlined as follows.

The first of these is the unit of analysis – the interest group. Fischer (1970: 216) provides examples of fifteen types of groups, each of which possesses at least five properties (cf. Trigger 1975: 52). Fischer's groups, not intended as a formal classification, range from "civilizations" to "caravans", and the interest group (not listed by Fischer) is favored by Trigger in analysis. The work of George C. Homans is also cited in support of this choice. The difficulty is not with the unit of analysis but with its derivation. How does it differ from the voluntary association? In what contexts has the interest group, or any other type of group, been dealt with in anthropology? What is their treatment in sociology? In other words, we are presented with what appears to be a nominal definition of *group*, not specifically with a definition of an *interest* group (Anderson 1971; Zeigler 1964, but see Trigger 1975: 23). There are a variety of perspectives on groups within sociology. Zisk (1969: 78 ff.) discusses four alternative frameworks for the study of groups: structural-functional, communication theory, role analysis, and interaction theory. Warriner (1956) discusses four major orientations in writings that deal with groups: nominalist, interactionist, neo-nominalist, and realist. These various orientations differ from one another with respect to the individual/group dichotomy, the role of individual psychology (e.g. reductionism), and even such things as game theory, e.g. the rational individual who reciprocates with others, and so on. These approaches are

incorporated into definitions and color their empirical use (and usefulness), and the debates are old ones in sociology.

The fact that Homans was cited provides a clue to the general orientation. Homans has, throughout most of his work, advocated a formal social science premised on the principles of exchange theory (itself developed as an alternative to systems theory), where the notion of bargaining is highlighted. Interaction between individuals is considered partly in terms of reciprocal behavior which is shaped by its pay-off function. Homans' work is thus behavioral psychological. The book *The Human Group* (1950) is functionalist and we are given a mechanical equilibrium model which is dependent on the actual behavior of actual social actors. In *Social Behavior: Its Elementary Forms* (1961), Homans claims the task of sociology is to see how relationships between men are created and maintained by individual human choices. Homans therefore proposes a *methodological individualism*. In view of this fact, it is curious that he is cited as an authority when it is "preoccupation with the personalities and idiosyncratic behavior of individual members of interest groups" (Trigger 1982: 21) that is being rejected. Furthermore, Homans had waffled in his position. He combines the "nominalist" and "interactionist" orientations in *The Human Group*, and the "interactionist" and "realist" positions in his later writings (*Sentiments and Activities*, 1962). Realism, for example, is theoretical, analytical and is *anti*-reductionist. It is a structural-functional position which denies men the power to construct their own social realities and make their own choices. This contradicts any notions of methodological individualism and is something Homans never fully reconciled (1962: 22-35; 48-49). The scientific sociology he advocated is reductionist, or behavioral psychological – where the emphasis is on the individual. Structural-functionalism denies the role of the individual in the social system. Homans changed his mind about the relation between individuals and groups at least once in the course of his writings; in addition, the cost-benefit notion, or the *economic* exchange analogy in social exchange has been in disfavor in sociology for a long time now (Heath 1976).

Conceptual confusion might have been avoided by providing a nominal or operational definition of *interest* group in *sociology* (with more appropriate references). The reader could have been automatically directed to the theoretical literature on social exchange, game theory, and perhaps even transaction analysis, and someone might have been encouraged to pursue a fine-scale analysis within one of the theoretical approaches. Or, they might have been on firmer ground to even reject the unit of analysis.

Another term with which there is difficulty is "motive". It is a term that appears from time to time throughout Trigger's work, but there is neither a nominal nor an operational definition for it. Fischer (1970: 187-215) provides an excellent discussion of motives and motivation. Perhaps Peter Winch's idea of what a motive is is indeed not wholly acceptable (Giddens 1976: 44-51); more than anyone however, it is C. Wright Mills (1940) who has done the most to clarify the concept and has even suggested how one could *empirically* impute motives in given historical situations. Neither Trigger nor Fischer cite this rather important source that might have led to additional analyses. Interestingly, Fischer (1970: 214) suggests that only individuals have motives, not groups (though individuals may share motives to some extent). If true, it might be difficult to reconcile this with the interest group as the unit of analysis. Otherwise, we are back with idiosyncratic individuals and personalities. The whole idea of motives and motivations is a rather complex one and needs to be studied and developed if it is to be a more powerful analytic concept. Again, the difficulty is not with the concept but with its lack of explication.

Lastly, the notion of "understanding" requires something more than conventional language treatment. The appropriateness of the term is not questioned however. In one article, the term understanding occurs ten times in two pages (Trigger 1975: 54-55), and in Chapter One of *The Children of Aataentisic* it occurs at least 27 times between pages 11 and 26 (seven times on page 26). Of all the contexts in which the term is used, it refers most often to the following: a process, interaction, a situation, "the Indian", motives, personality, history, Huron behavior, and the documentary sources themselves. Nowhere is the term understanding defined. It is unclear whether the term/concept is to be used as an analytical tool or heuristic device, or in empirical validation or verification (see the debates over this issue in Truzzi 1974).

The concept of understanding, or *Verstehen*, which emerged out of debate between a number of German historians, was developed and used primarily by Max Weber in his historical investigations. Weber was interested in three types of subjectivity – concrete purposes, motives, and meanings attributed by people to specific social actions; common meaning of something given to it by a group of people; and, meanings attributed to an "ideal" actor in a symbolic model of action constructed by the social scientist. In pursuing these, Weber considered our capacity for empathy, our capacity for rational understanding, and our capacity to formulate and test causal models. In his work, Weber aimed to acquire a causal explanation of social action and to achieve empathetic appreciation. *Verstehen*, then, was a

device that could generate hypotheses concerning the connection between subjective states and human action, but it could not validate them (Weber 1949). Unfortunately, there is no mention of Weber in Trigger and not even a hint about *Verstehen* as it is used in historiography (links with the sociology of knowledge are obvious). Understanding is very closely related with motives and motivation, but how they can be operationalized together to make what would be a formidable analytic tool is never pursued. There is considerable debate both in sociology and in the philosophy of the social sciences whether *Vestehen* is applicable to groups or individuals. Some fear a psychological reductionism (Truzzi 1974). If this is a consequence of *Verstehen*, then the concept is not at all useful if explanations premised on idiosyncratic individuals and personalities are to be avoided. Finally, what kind of causal models can we formulate or test? The formulation of models and the generation of hypotheses is replaced instead by something called a "materialist orientation" (Trigger 1982: 23-24).

A critique of historical materialism would require another article, so any remarks have to be limited ones. We note that the unit of analysis in historical materialism is not the rational actor, as in exchange theory, nor the formally defined role player, as in structural-functionalism. The unit of analysis is the mode of production (Wilson 1983: 177). Depending on which version of Homans one is inclined to follow, historical materialism is incompatible with Homans the methodological individualist, but may be compatible with Homans the realist. The materialist orientation also seriously neglects the role of human agency in social life (Wilson 1983: 208); there is the tendency toward sociological reductionism, where individual actors are collapsed into social structures. People are conceived as stepping into already conceived systems (sometimes of the researcher's own making) such that they do not create their own motivation or ideological structures (around family, community, or in political groups) (Wilson 1983: 211-212). Historical materialism lacks on adequate phenomenology of social action (Wilson 1983: 212). Harris (1979: 225) makes the well-taken point that "what we encounter in ... the entire corpus of dialectical materialist theory is the inevitable ambiguity associated with any views of sociocultural causality that fails to distinguish between the mental and behavioral and the emic and etic components of sociocultural systems". Thus, a materialist orientation may be compatible with one version of Homans but not with the other. It is, however, incompatible with the notion of motives or motivations *except* those which have been imposed from outside. Futhermore, the kind of understanding (or *Verstehen*) that might obtain from this context is quite different from what Weber and others

intended it to be. Finally, the dialectic method is not exclusive to historical materialism. Writes Wilson (1983: 208), "sociologists working in a variety of research traditions are fond of pointing out that the consequences of human action are frequently the opposite of what was intended" (see Trigger 1975: 56; 1976: 850; 1982: 22). The idea that social life is ordered by basic contradictions can be incorporated without serious modification into functionalism, where the idea of "structural strain" already plays a prominent part (Wilson 1983: 209).

V

In conclusion, if ethnohistory is to be a valid scientific endeavor as a discipline or a method, and if knowledge in the subject is to be cumulative, language and concept-use analysis should be an integral part of our work. Most ethnohistorians rely on documentary evidence. In their analyses they "transform" them and produce yet another type of document. Thus, the accounts and "histories" they produce are data themselves and are no less subject to historiographic analysis than the so-called primary sources they work with. Far from being an obsessed operationalism, what I am proposing is a historiography of our own work. This is not new (e.g. Fischer 1970), but as I have tried to point out, the tendency to "borrow" from other disciplines requires much tighter control over the use of language and especially the development of concepts.

REFERENCES

ADLER, F.
1966 "The Range of Sociology of Knowledge", in *Modern Sociological Theory*, ed. by H. Becker and A. Boskoff. Holt, Rinehart and Winston: New York. Pps. 396-423.

ANDERSON, R.
1971 "Voluntary Associations in History", *American Anthropologist* 73: 209-222.

BENNETT, J. W.
1977 "Ecosystem Analogies in Cultural Ecology", in *The Concept and Dynamics of Culture*, ed. by B. Bernardi. Mouton Publishers: The Hague. Pps. 177-207.

BERGER, P. and T. LUCKMANN
1966 *The Social Construction of Reality*. Anchor Books: New York.

CARMACK, R. M.
1972 "Ethnohistory: A Review of its Development, Definitions, Methods, and Aims", *Annual Review of Anthropology* 1972: 227-246.

FISCHER, D. H.
1970 *Historians' Fallacies: Toward a Logic of Historical Thought*. Harper and Row, Publishers: New York.

GADACZ, R. R.
1981 "Understanding and Interpretation in Historical Ethnology", *Anthropologica* N.S. 23 (2): 181-189.

GARDINER, P.
1974 *The Philosophy of History*. Oxford University Press: Oxford.

GIDDENS, A.
1976 *New Rules of Sociological Method*. Hutchinson: London.

GLASSOW, M. A.
1978 "The concept of Carrying Capacity in the Study of Culture Process", in *Advances in Archaeological Method and Theory*, Volume 1, ed. by M. Schiffer. Academic Press: New York. Pps. 31-48.

GOFFMAN, E.
1959 *The Presentation of Self in Everyday Life*. Anchor Books: New York.

HARRIS, M.
1979 *Cultural Materialism: The Struggle for a Science of Culture*. Random House: New York.

HEATH, A.
1976 *Rational Choice and Social Exchange*. Cambridge University Press: Cambridge.

HICKERSON, H.
1970 *The Chippewa and Their Neighbors: A Study in Ethnohistory*. Holt, Rinehart and Winston: New York.

HOMANS, G. C.
1950 *The Human Group*. Harcourt, Brace and Company: New York.

1961 *Social Behavior: Its Elementary Forms*. Harcourt, Brace and World, Inc.: New York.

1962 *Sentiments and Activities*. Free Press of Glencoe: New York.

HUDSON, C.
1973 "The Historical Approach in Anthropology", in *Handbook of Social and Cultural Anthropology*, ed. by J. J. Honigmann. Rand McNally College Publishing Company: Chicago. Pps. 111-141.

KROEBER, A. and C. KLUCKHOHN
1952 *Culture: A Critical Review of Concepts and Definitions*. Papers of the Paebody Museum (Volume 47), Number 1). Cambridge, Mass.

LACHENMEYER, C.
1971 *The Language of Sociology*. Columbia University Press: New York.

MANNHEIM, K.
1936 *Ideology and Utopia: An Introduction to the Sociology of Knowledge*. Harcourt Brace Jobanovich: New York.

MARTIN, C.
1978   *Keepers of the Game.* University of California Press: Berkeley.

MERTON, R.
1973   "Paradigm for the Sociology of Knowledge", in *The Sociology of Science*, ed. by R. Merton. University of Chicago Press: Chicago. Pps. 7-40.

MILLS, C. Wright
1940   "Situated Actions and Vocabularies of Motive", *American Sociological Review* 5: 904-913.

NAGEL, E.
1961   *The Structure of Science.* Harcourt, Brace and World: New York.

PELTO, P. and G. PELTO
1978   *Anthropological Research: The Structure of Inquiry.* Cambridge University Press: Cambridge.

PITT, D. C.
1972   *Using Historical Sources in Anthropology and Sociology.* Holt, Rinehart and Winston: New York.

RICHES, D.
1982   *Northern Nomadic Hunter-Gatherers: A Humanistic Approach.* Academic Press: London.

SALMON, M. H.
1982   *Philosophy and Archaeology.* Academic Press: New York.

SCHWERIN, K. H.
1976   "The Future of Ethnohistory", *Ethnohistory* 23 (4): 323-341.

STARK, W.
1977   *The Sociology of Knowledge.* Routledge and Kegan Paul: London.

STEHR, N.
1982   "Sociological Languages", *Philosophy of the Social Sciences* 12: 47-57.

STEHR, N. and A. SIMMONS
1979   "The Diversity of Modes of Discourse and the Development of Sociological Knowledge", *Zeitschrift für allgemeine Wissenschaftstheorie* 10 (1): 141-161.

TRIGGER, B. G.
1975   "Brecht and Ethnohistory", *Ethnohistory* 22 (1): 51-56.

1976   *The Children of Aataentsic.* Volumes I and II. McGill-Queen's Printer: Montreal.

1978   "Ethnohistory and Archaeology", *Ontario Archaeologist* No. 30: 17-24.

1982   "Interest Groups, Alienation, and Humanity: A Reply to René Gadacz", *Anthropologica* 24: 19-25.

TRUZZI, M.
1974   *Verstehen: Subjective Understanding in the Social Sciences.* Addison-Wesley Publishing Company: Menlo Park.

WALLACE, W. L.
1971   *The Logic of Science in Sociology*. Aldine Publishing Company: New York.

WARRINER, C. K.
1956   "Groups are Real: A Reaffirmation", *American Sociological Review* 21: 549-554.

WEBER, M.
1949   *The Methodology of the Social Sciences*. Translated and edited by E. Shils
       and H. Finch. The Free Press: New York.

WILSON, J.
1983   *Social Theory*. Prentice-Hall: Englewood Cliffs.

WINCH, P.
1958   *The Idea of a Social Science*. Routledge and Kegal Paul: London.

ZEIGLER, H.
1964   *Interest Groups in American Society*. Prentice-Hall: Englewood Cliffs.

ZISK, B. H.
1969   *American Political Interest Groups*. Wadsworth Publishing Company: Belmont
       (California).