Abstract

Much anthropological interest in methods derived from the natural sciences has been motivated by a search for tools with which to describe ethnographic settings in more ‘rigorous’ terms. These have generally failed, and these failures are due to implicit conventions in anthropology regarding what to observe and how. I suggest that natural science methods were developed as a result of a process which aimed to solve problems that are generally different from those social scientists pursue and that social scientists need to take the underlying ideas and rebuild them for their own purposes if they want to import them. Likewise, most qualitative anthropologists are also guilty of inappropriate borrowing, in particular in their importation of postmodern philosophy from literary criticism rather than adapting these ideas from the source in a manner more suited to the discipline. I examine some the conditions that would be necessary to integrate the various fractions of the anthropological community within a flexible but common framework. I conclude that anthropology must include a major qualitative perspective, and in response to this suggest that if we are to formalise our study we must adopt some new conventions. These include a broadening of analysis from causal to include interpretive reasoning within a common framework, the addition of deontic operators to our basic formal framework, an emphasis on declarative semantics over procedural semantics, and end by briefly describing some speculative work using Information Theory as a basis for quantitatively evaluating qualitative descriptions, models and analysis.

1 Introduction

Past anthropological forays into the natural sciences have been motivated by a search for tools with which to describe ethnographic settings in more ‘rigorous’ terms. The typical result of these forays has been unsatisfactory - e.g. good descriptions of natural resource management systems were produced that contained but pale reflections of the principal subject, people and their activities. It is suggested here that these failures are due to implicit conventions in anthropology - in particular conventions pertaining to numerical representation - regarding what to observe and how. This is seen most clearly in anthropology’s applications of computer science [Boone and Woods 1992; Fischer 1994a]. There were early successes with simulation models in anthropology, in which the constructive semantics of kinship were played out over whole populations [Coult and Randolph 1965; Dyke 1981], food procurement strategies were reconciled with local ecology [Ricci & Wilson 1978; Buchler & Fischer 1986], and tabla improvisations were related to the constituent ‘formulae’ used by musicians [Kippen, 1988].

These successes were not followed up en masse, however, because the nature of early computer representations was not in a form easily adaptable to conventional forms of representation in

1. This paper is based on research supported by the Nuffield Foundation, the ESRC (UK) and the AHRB (UK)
anthropology [Fischer 1994a:10]. This is consonant with other attempts to incorporate natural science theories, methods and models that have followed much the same pattern [see Bernard 1994].

Depending on the constituency these failures are typically attributed to one of the following reasons:

a) matters involving people are intrinsically “too complex” for natural science derived methods
b) social scientists are too imprecise in their “measurements”, too lax in their methods, and lack any serious form of testable theory

I shall stand to the side and suggest another reason: natural science methods were developed as a result of a process which aimed to solve problems that are different from those social scientists pursue.

Without qualification we could argue that this is a variant of either a) or b) preceding. That is, either a) social phenomena is intrinsically too complex for natural science derived methods to apply or b) social scientists are asking the wrong questions, biting off more than they can chew.

As a scientifically trained anthropologist, who is also a computer scientist, I propose a third reading:

Social phenomena are of a different order from the phenomena that natural science has been successful in addressing. The failure to successfully adopt natural sciences methods has largely been because of a failure to address these differences rather than an intrinsic barrier to adoption.

Natural science has been most successful at describing phenomena which are a) recursive, b) have structure, c) not self-referential, and d) similar to a high degree with respect to different projections (manifestations in different contexts).

By recursive, I mean that the phenomena can be described as the same object over and over again, and where higher order organisation resembles lower-order organisation. The same properties, forces and consequences recur. By structure, I indicate interrelating parts. Self-referential indicates an ability to alter relationships based on a moment-to-moment examination of a process. Projection refers to instances or instantiation in a given context.

Social scientists rarely find most of these conditions. The level of recursion almost always is 1. The different levels of the situation are driven by different principles and act on different objects. Societies are highly self-referential in other than the very short term, and individuals on a scale measured in seconds, minutes or hours.

This does not mean that the fundamental ideas that apply to natural science research cannot be applied to social science, but it does suggest these will have to be formulated in a different way. That is, the typical response to importing a method is to import it directly, and then attempt to fit new or existing data to this method, which generally requires considerable deformation of the data. This is not the manner in which these were developed in the natural sciences, where the mathematics or method is adapted to the data and the research objective, not the converse.

Why should social science adopt methods from a natural science’s vantage at all? If present methods from natural science are not largely applicable to social science, why not develop new methods from the ground up. I believe that we have to do both.

Authority

In many ways this issue arises not for intrinsic reasons but because of how natural sciences and social sciences are applied in the world. Natural sciences have a proud tradition of being perceived to ‘work’. They are seen as the driving force behind ‘progress’ over the past four centuries and thus getting things done. Social science was thought to be capable of working, but largely did not appear to, at least not in the sense that natural sciences worked. Natural scientists could happily assume this was because social scientists were doing something wrong, but it really did not matter much to them. Social science had little impact on their world. The opposite was not necessarily true.
The reason there is an issue now is that this situation has changed. Natural scientists see social scientists as poor but dangerous competitors, both because there is direct competition in some applied areas, and because some social scientists are attempting to undermine the position of natural science as a purveyor of useful knowledge, often in a manner incomprehensible to the natural scientist.

These changes in the positioning of social sciences has arisen because of two factors: greater ability to record, exchange and examine information, in large part due to better communications technology and computers, and examination of the outcomes of attempting to transfer technology to domains different from that in which that technology was developed.

These contexts yielded indications that natural science-based methods of technology production did not always ‘work’ at an optimum level nor in the manner in which they were thought to work. This was the natural sciences in their most exposed position: embedded in the ‘real world’ of people, processes and knowledge.

With their new ‘power’ social scientists responded with several divergent tracks including a movement we might call *scientism* and what has come to be known as *postmodernism*.

### 2 Scientism

There has been considerable comment on the relation between ‘scientism’, scientific knowledge and modes of inquiry and other ‘modes’ of knowledge [Lambek 1998; Herzfeld 1999]. There is bound to be some disagreement between anthropologists, in both their scientific and humanist poses and other scientists. Standing back and looking at different kinds of knowledge, although the question of equivalence is a difficult one, we should have to say that there is an existence proof that most indigenous bodies of knowledge are successful with respect to the particular way of life with which these are associated, and that these same bodies of knowledge will appear defective to most participants in other bodies of knowledge]. However, this is not the case with all possible bodies of knowledge. One of the problem that many people (anthropologists included) appear to suffer from is to assume that any idea that they may have that fits the ‘facts’ as they see them is valid (e.g. Pierce’s abductive logic). Such ‘new’ knowledge requires a means of integration and reproduction wherever it is to persist and add value to an existing knowledge tradition.

Conventionally, anthropologists tend to adopt natural science methods directly from natural science disciplines. In some cases this is appropriate, for instance evolution and genetics apply as well to people as to all other living things. But there is also a great deal of importation into areas not so well suited. One of the earliest failures in importation in anthropology was applying standard evolutionary theory to societies and cultures, languages, and technological development. This is not to say that there is not some intrinsic merit to the notions that societies evolve or change over time, but that the model applied was a bare modification of one which applied to living things, which have entirely different conditions for reproduction, survival and transmission of properties .

About 20 years ago multicultural matters assumed more importance, and gradually much of anthropology has been appropriated from a genre of generalizing situational knowledge to translating situational knowledge from other cultures into situational knowledge for western societies [Weiss 1999; Barth 1987]. This ‘application’ of ethnography and anthropological matters is welcome in many respects. The baseline of a successful locus of knowledge is the ability to incorporate with and between other locii.

### 3 Postmodernism

Another outcome of the same 20 years is what is known as postmodernism. As it is practised in anthropology it seems to focus on three basic principles. First, that the form of anthropology for the past century or so is attributed to the power wielded by a few key men and women in the discipline and their relationship to other wielders of power outside the discipline. Second, that there are no factual, essential or ‘authoritative’ positions. Third, that social phenomena is an illusion non-reductively constructed by the interactions of unique individuals.
None of these principles emerge directly from classical post-structuralist philosophy (e.g. Derrida or Lacan). The first is a direct adaptation from post-structuralist ideas about the political impact on the construction of historical knowledge (as exemplified by Foucault). The second and third are direct adaptations from literary criticism and the later writings of Derrida, Barthes and others on the philosophy of writing. That is, since these are imported technology principally designed to work with literary texts, the use of this technology depends on posing the people in a society as the authors of texts that anthropological postmodernists can discuss (as well as the texts that anthropologists themselves write). Just as with the natural sciences, anthropology imported postmodern technology not as a method starting with anthropological problems, but rather importing both the problems and the technology to deal with these problems from literary criticism. For many anthropologists the importation was authentic to the extent that their articles were written in a style that resembled poorly translated French.

In short, anthropological postmodernism, via literary criticism, transformed a philosophical theory questioning the linkage between representation and semantic analysis of symbols and favouring linkage to political pragmatics, into a practice which dealt with texts, and fictional ones at that. One could almost suggest, if one were a postmodernist, that simply transforming social productions into texts was not sufficient, these had to be transformed to literature (fiction) as well to suit the imported methodology of literary criticism.

These assumptions considered together result in a powerful confection which empowers the believer to perform research in any manner which is ethically consistent with the three principles (which is complex and limiting in itself) and to present any transformation of the research material in any form consistent with the three principles. So long as they claim no essentialist or authoritative position, support no existing power structure and present only information about individuals they will produce a valid result, regardless of content. Likewise they are insulated from criticism by non-believers, since these criticisms are almost always based on criteria which violate one or more of the three principles.

Much as anthropologists adopted scientific philosophy after it had been shaped by natural scientists, anthropologists adopted postmodernism after it had been shaped by literary criticism. Note that there is no necessary relationship between postmodernism and literary criticism, other than both deal with language and symbolism to some extent, Derrida, in particular, sometimes dealt with writing, and postmodernism and literary criticism deal with complex semantics and pragmatics. This may make post-structural philosophy applicable to literary criticism, but does not make literary criticism a necessary stage (or a desirable one) to application within anthropology.

To accommodate this importation, early adopters recast the data of anthropology as a text, in the literal sense of an ethnography as a text, conversations as oral texts, and, by extension, the claim that lived lives are like texts. There appears to be little motivation for this other than the need to shape the data to preexisting applications of post-structural theory in literary criticism.

The only possible contender for an independent motivation was the examination of written ethnography as a specific genre, although as a specific movement this came rather late in the day in anthropology, probably introduced by Marcus in 1983 or 1984, although one could credit Boon in the late 1970s with this innovation. This also marked the beginning of the postmodern examination of anthropology as a science, drawing it into the postmodern History of Science movement, which also appears to have been heavily influenced by literary criticism.

Because postmodernist work is unassailable from outside as it neatly removes power from these external positions, the only manner in which we can proceed is to apply some ideas from postmodernism to postmodernism. If postmodernism is like a genre of painting, it is like that painting in student residences in the 1960s; highly personal, rarely communicative, but often interesting. It can provide a basis for injecting new ideas into our ordering of knowledge, but as practised is often implicated in suppressing ideas as well because of the three principles and complex essentialist ethical theory.

Annealing

Restating the converse of the kinds of phenomena that natural science are ‘good at’, the main characteristics of phenomena that social sciences engage are:
a) not generally recursive or very limited recursion,
b) processural (structural unfolding),
c) self-referential (meta-intentional or pragmatic),
d) dissimilar to a high degree with respect to different ‘points of view’ (projections or perspectual unfoldings).

However, this is not really true, any more than the natural science version of this is. One of the points of the critique of science is that it tends to ignore that these are unfolding at the level of technology or science and are embedded in the world of people and their ideational creations. Both kinds of phenomena must be considered, and ‘successful’ analyses will integrate both. Likewise, social scientists, of whatever persuasion, must not imagine that they can ignore the impact of physical phenomena on the social.

This affects social scientists rather more than natural scientists, because there are many phenomena natural scientists can describe where this embedding is not a significant factor. It becomes a factor in natural science in its unfolding into the world of living things, especially people. However social scientists are never isolated from the world of physical forces, because social scientists are always looking at unfoldings or the results of unfoldings. Social scientists sometimes try to get around this by keeping the discussion rather philosophical, or by inventing conceptual forces analogous to physical forces, but this is simply evading the problem.

4 Integration

There are several possibilities for integration that are in principle compatible with both scientific and postmodern approaches. These would not be treated in precisely the same way by a ‘true-believer’ of either (and might be rejected by both), but make possible research and analytic strategies for those anthropologists wanting to integrate principles from both approaches.

How might we integrate these two perspectives? We generally think in terms of causality as a primary principle of the natural sciences. This has some value from a conceptual point of view, since causality is highly associated with explanation. In part this might be due to the relation of occurrence to process. Causality is intrinsically a processural concept, and there is probably no useful notion of process without causality, despite the co-concurrence of process with absence of causality proposed by some anthropologists.

We generally reckon that history is part of the context of a particular event. This is not directly so in many cases. Although one can always retroactively demonstrate how the situation and result were possible because of historical processes, it is difficult to convincingly argue that a given assemblage of events (e.g. history) caused this result. When a situation is complex it is difficult to assign causality in any kind of useful predictive format. Instead we provide ‘reasons’, a weaker concept, and more or less the interpretative analogue to causes. I am not suggesting that causality be abandoned as an important goal in social analysis, but rather the social sciences are impoverished if establishing causality is the only goal; for a whole raft of reasons (practical and ethical) we often simply cannot accumulate enough data to establish causality for many areas of research, nor is it clear that we ever could. By adding a weaker formal goal, establishing retrodictive reasons relating historical circumstances to later events, we make it possible to do a wider range of research, establish a broader basis for social modelling, and we close the ‘gap’ between scientific and postmodern social science to a considerable extent.

We also require a more complex approach to process. Causality tends to imply relatively simple processes; a → b → c → d. In practical research we deal with imperfect causality by constraining causal links as having specific probabilities. Interpretative analysis involves what did happen, and how this relates to one or more outcomes. From looking at a range of cases we can establish some sense of likelihood for these contingent events, but it is difficult to present this in a conventional framework of causality and implicational logic.

Can we broaden the concept of process with declarative semantics? Declarative systems have no causality, since these are not temporal systems in the strictest sense, but we can show that causal systems can be described, and explained, in terms of declarative derivations. That is, we subsume
time and process in a ‘static’ derivation that documents the order of application (and records time if necessary), while retaining all the processural information in a recoverable form.

Fischer [1997] briefly discusses some methodological approaches to this end. In order of concreteness, non-linear multimedia textual/documentation approaches to fieldwork, simulation, and the adoption of two techniques developed for representing complex information on computers, declarative resolution logic and production systems.

But we require something more if we are to deal both with ‘ordinary’ causal systems and interpretive reasoning in a common framework. In particular we need a way to formally represent contingency without having to establish probabilities, while possessing a quantitative measurement which permits us to compare our formulation to data or alternative formulations.

5 Deontic Reasoning

One of the motivations for interpretive analysis is that in complex contexts many more ‘events’ could have happened than did, and often these ‘events’ could follow in many different orders (and often in parallel). By introducing deontic operators (Oblige and Permit) to our formal framework we can accommodate this complexity, and at the same time describe events and contexts in a form that facilitates both interpretive and causal analysis where applicable. Deontic operators also help to ‘processuralise’ our ‘static’ declarative representation.

Permit x and NOT Oblige x introduce contingency by indicating enablement and potential enablement. That is, Permit x indicates that the proposition x is allowed but indicates nothing with respect to causing x to occur. NOT Permit x indicates that x is not allowed, and will block the instantiation of x until Permit x is instantiated. Thus IF y THEN Permit x states that y is one (of possibly many) necessary conditions for x. Deontic operators are one of the key features of Modal Action Logic (M[A]L) [Maibaum 1987], a specification language for specifying complex processural potentialities. I adapted this for ethnographic research [Fischer & Finklestein 1991, Fischer 2003] by implementing a Prolog vocabulary for representing ethnographic descriptions and then testing micro specifications against the description database. This facilitated identifying areas of weakness in the ethnographic research while in the field, indicating focused topics that should be addressed.

Information Theory

For our immediate purposes, deontic logic in conjunction with a declarative semantics for representing our data provides a framework within which we can represent and analyze our data from both a conventional (causal) and historical (interpretive) perspective. However, we require a means of evaluating and comparing our results that is not simply qualitative. This is a long standing problem in the social sciences, and anthropology in particular. High quality quantitative data is difficult to collect, and in subjects such as anthropology where opportunity tends to drive research, often minimal or peripheral in quantity. Thus much of anthropology has been driven by qualitative analysis of situations, with minimal quantitative support for establishing the ‘norms’ and ‘deviations’ in cultural processes, more often relying on the judgements of indigenous actors in these processes. In the case of purely interpretive studies we are often faced with the dilemma of ‘one-hand clapping’ analysis. While it is obvious that whatever we come up with will be weaker than conventional quantitative indicators of significance, this weaker guidance is still a desirable goal.

I have been experimenting with Information Theory [Shannon & Weaver 1949] in this capacity. Although Information Theory (IT) has had its principle applications in evaluating how to transmit and receive information over channels, it has been applied in many disciplines, such as biology, physics and economics for other purposes. I will briefly describe how IT can be used to evaluate tabular data, documents and finally ethnographic data and agent-oriented simulations in a comparable fashion. [For expanded examples and software examples see http://real.anthropology.ac.uk/EMCSR2002/Fischer].

IT provides a means of calculating the entropy of an information source. In this case entropy best translates to interpretative variability (though IT is not concerned with the content of the information,
only its structure). Entropy of 1 indicates maximum variability, where all possible interpretations under consideration have an equal probability of being used. An entropy of 0.0 indicates that only one interpretation is possible. Intermediate values indicate our ability to 'know' something about a message without having the full message. This is also sometimes expressed as redundancy. It also indicates the level of ambiguity. High entropy yields ambiguity that prevents useful interpretation, too little entropy limits the dynamic range of the interpretation (which may or may not be desirable depending on context) without knowing the full range of information.

For tabular data we can calculate three values for entropy:

\[ H_{\text{max}} \] - maximum entropy given no information other than number of choices = \( \log_2 p_i \)

\[ H_{\text{Ind}} \] - entropy knowing the marginal totals = \( \sum p(ij) \log_2 p(ij) \)

\[ H_{\text{Markoff}} \] - entropy knowing the distribution of values in the table = \( \sum p(ij) \log_2 p(ij) \)

For example, given a 2x2 table we might get the results in Table 1. \( H_{\text{max}} \) indicates entropy of 2 bits based on four choices. Expressing entropy as a ratio of \( H_{<\text{type}}/H_{\text{max}} \), in Table 1 \( H_{\text{ind}_r} \) is 0.53, indicating that from the marginal totals alone, we have reduced entropy by 0.47 from our reference \( H_{\text{max}_r} \), which will always be 1.0 (e.g. \( H_{\text{max}}/H_{\text{max}} \)). \( H_{\text{markoff}_r} \) is the entropy taking the distribution of cell values into account. The rest of Table 1 is output from a program that samples a random selection of tables with the same marginal totals to produce a significance value for \( H_{\text{markoff}_r} \) computed from the original table. This significance value tends to agree with the \( \chi^2 \) value for the table, though not identically - some table configurations result in a significant value for one and not the other. Indeed, the \( H_{\text{markoff}_r} \) value tends to work well even with low frequency cells (where \( N > 30 \)), whereas \( \chi^2 \) does not.

This permits us to evaluate the correlation of the two tabular variables in a different way, producing a parameter we can use to directly compare with other relationships. We have a clear indication in \( H_{\text{ind}_r} \) how much we 'know' just from the independent distribution of values for each variable. \( H_{\text{Markoff}_r} \) indicates the reduction of uncertainty resulting from the interaction of the two variables. If this were all we could do it would at present be a curiosity, at least until more work was done on the resulting statistic and its distribution and reliability. But it provides a basis for working with purely qualitative results as well.

<table>
<thead>
<tr>
<th>Table 1: Entropy indicators for a sample 2x2 table</th>
</tr>
</thead>
<tbody>
<tr>
<td>H_max_b=2.0</td>
</tr>
<tr>
<td>H_max_r=1</td>
</tr>
<tr>
<td>H_ind_r=0.53</td>
</tr>
<tr>
<td>H_markoff_r=0.416</td>
</tr>
<tr>
<td>( \chi^2 )=5.916</td>
</tr>
<tr>
<td>df=1.0</td>
</tr>
<tr>
<td>Average H_Markoff=0.447</td>
</tr>
<tr>
<td>Max H_Markoff=0.53</td>
</tr>
<tr>
<td>Min H_Markoff=0.413</td>
</tr>
<tr>
<td>Markoff sig=0.006</td>
</tr>
</tbody>
</table>

For the past few years we have been organizing our field data using XML, a standard for document description [Fischer, Kortendick and Zeitlyn 2002; see also http://csac.anthropology.ac.uk/XML]. Basically this consists of marking up a data unit (which is often a text or notes, but can be an object
description, or even a table of values) using a phrase-structure grammar defined in a Document Template Definition (DTD) or XML Schema. The resulting document can then be parsed and interrogated in a very rigorous fashion. This works very well for improving access to data, but there are presently no formal standards for evaluating the overcapacity of a DTD with respect to a given data set (undercapacity is easy to identify, since the DTD will be inadequate to represent the data set). Using a similar procedure to that used for the tabular data above, we can derive our entropy measures by parsing the DTD and identifying the number of different document structures that can be generated from the DTD. Since some DTDs have trivially infinite productions, a heuristic is applied so that no nodal branch is evaluated deeper than the maximum for that branch in an actual document from the set under evaluation. $H_{\text{max}}$ is simply the cross-product of the elements to the maximum depth for each instance of each element. Each element in the DTD can be individually identified with respect to its role in expanding or limiting message choice. Since DTDs can contain deontic (optional) elements, the hypothetical document instances must be generated recursively. This results in a tree of values from which we can calculate $H_{\text{ind}}$, which takes the hierarchy into account. $H_{\text{markoff}}$ is calculated across the set of actual documents collected, e.g. the frequency of each nodal type in the same position. From this we can quantitatively evaluate how permissive a given DTD mark-up scheme is relative to the data actually collected, where we assume that a less permissive scheme is better than a more permissive scheme for the purposes of data description; we would like to be able to describe all the possible data configurations while not generating structures that cannot or do not occur. This results is a quantitative parameter that specifically indicates the extent of parsimony between a theoretical data description and actual data structures. We can also examine in detail the contribution to expressiveness of each element and each hierarchical definition. This provides an avenue for examining each document and expressing the complexity of the data in each quantitatively, and identifying just where the complexity (or simplicity) is located. This is useful for directing attention both to the theory underlying the classificatory scheme employed and the behavior of the different data elements making up our dataset.

Finally, this can be applied to agent-oriented analysis over time, either from direct data collection or from simulated data. This is done by a) creating a DTD that represents the phenomena to be observed and/or recorded (including the ‘rules’ one is examining), and b) coding of time for each value change in a given agent - each element has a time signature as a parameter. Adapting the previous method by limiting analysis to a given time interval, we can trace the progress of a given situation, tracking changes in the overall entropy of that stage of the situation. That is, we can evaluate the model being applied to the data - sequences of low entropy imply areas where predictability is high, areas of high entropy where it is low. In the case of simulations, we have the added advantage that we can record the entire run of a complex agent-oriented simulation, and examine a given run from a number of different perspectives, since we can effectively recreate the simulation by animating the XML document that contains the record. (http://real.anthropology.ac.uk/EMCSR/Fischer contains some examples).

6 Conclusion

In each of these cases we gain a quantitative parameter in entropy that meets the criteria set out. For causal relationships we expect very low entropy (ideally 0 once the conditions are met), and can test this expectation. For interpretative analysis, we can evaluate and modify our representational model using the degree of entropy to guide us in making the model more closely representative of the situations we are analyzing. Additionally, we can evaluate the contribution to lowering or increasing entropy in the model for each model element and context individually. A great deal of work remains to be done... calculating the potentialities of even a simple phrase structure grammar is a time-consuming task, and currently requires a number of heuristics that potentially degrade the results. Further exploration of the properties of entropy in these specific contexts needs to be done to access how to interpret the results. But even at its current state of development it is a great improvement over the existing situation in principally qualitative research, since all it requires is that you represent your data using a form that depends only on your ability to describe your classificatory scheme, which should be a part of any properly reflexive research.
References


