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INTRODUCTION

The empirical status of formal analysis is a fundamental conceptual problem for philosophy and a basic methodological problem for science. The problem is especially crucial in the effort to establish a fully empirical version of anthropology.

In the physical sciences, experiments often reveal relationships that can be expressed mathematically or in other highly formalized ways, such as the conceptual models of the atom, evolutionary tree diagrams, or chemical notation. It is the same in the social sciences, but with an added twist. In the physical sciences, there is generally a quite straightforward distinction that can be observed between the formalization as an aspect of our analysis and the data that the formalization represents. For the social sciences, this may also be true in some cases but in others the data may actually consist of formal systems. In this case, the problem is not only to find them and represent them, it is also to do so without contaminating the representations with concepts that are alien to them. Locating and characterizing such systems offers new epistemologies for anthropology and hence takes us to the frontiers of the discipline, where the struggle to recognize new phenomena is inseparable from a struggle to find new ways to think about them. How we do it has been subject to various formulations, and many of these have been fundamentally misleading and distracting.

UNEMPIRICAL FORMALISM

In the recent history of the social sciences, the most prominent of the misleading views of formalization and formal systems is the complex claim that "formal" analysis depends on frameworks that are inherently arbitrary, *a priori*, substantively empty, and must be established before any specific investigation or description can be undertaken. Formal theory was the opposite of substantive or descriptive theory. It was something the analyst defined and then imposed on the prospective data. This is always asserted on the basis of one or another philosophical position, never on the basis of a close analysis of any actual scientific experience or even of ordinary thinking. While the deepest philosophical roots of this position lie in Plato and Aristotle, the philosophers usually cited by anthropologists are more recent. They sometimes go back as far as Locke and Hobbes, but the most important have been in the line of positivist arguments beginning with John Stuart Mill's *System of Logic* (1843, reprinted as Mill, 1950).

Mill did not describe science as a system of experimental knowledge but as a system of "propositions." Propositions "name" things and relations between things, and truth or falsity can be assigned to them. This top-down, deductivist, view of science is astoundingly wrong-headed when taken as any sort of close description of what scientists actually do. Logicians may sit about assigning truth or falsity to propositions in Mill's sense, but scientists certainly do not. *A fortiori,* they do not deal with relations between propositions. Scientists deal with observations and relations between observations, preferably experimentally clear observations. And observations, *per se*, are not true or

false. They are what they are; the problem is to understand them. Such observations cannot be reduced to propositions or any other single form of expression, any more than a three dimensional object can be reduced to a two dimensional representation or a process over time reduced to a static picture —which is why we teach laboratory sciences in laboratories, field methods courses in the field, and so on. Nevertheless, Mill's phrasing had a powerful influence on subsequent academic philosophy, and through that on social science.

Mill divided propositions into those that were merely emotive and those that were "real." He then divided real propositions into those that were "purely verbal" or "essential" and those which were "non-essential":

> An essential proposition, then, is one which is purely verbal, which asserts of a thing under a particular name only what is asserted of it in the fact of calling it by that name, and which, therefore, either gives no information or gives it respecting the name, not the thing. Non-essential or accidental propositions, on the contrary, may be called real propositions, in opposition to verbal. They predicate of a thing some fact not involved in the signification of the name by which the proposition speaks of it, some attribute not connoted by that name. Such are all propositions concerning things individually designated, and all general or particular positions in which the predicate connotes any attribute not connoted by the subject. All these, if true, add to our knowledge; they convey information not already involved in the names employed. (1950: 87-88)

Propositions are to be related to each other by formal logic, which Mill equated with Aristotelian class-inclusion logic. It is not clear whether Mill recognized formal logic itself as propositional knowledge (taking the rules of logic as essential propositions), but he was clear that in his view logic was solely concerned with reasoning from "general propositions" to "particulars" (1950: 12). It was not concerned with reasoning from particulars to general propositions, which is "induction" for Mill, and entirely different. In Mill's schoolbook logic, one can prove something by deriving a particular proposition from a general proposition, but one can never prove a general proposition on the basis of particulars.

Beginning with Ernst Mach, modern logical positivists and their allies have made substantially the same assertions. Rudolph Carnap's *Logical Syntax of Language* (1937), for example, argued that statements or truths that were verifiable were of only two possible kinds. They were either analytic, which was also *a priori*, or they were synthetic, which was also *a posteriori*. Analytic corresponds to Mill's "essential;" synthetic corresponds to his "accidental." Formal systems, like logic and mathematics, could only be analytic, hence also *a priori*. Given these definitions, it does not make sense to say that you can derive a formal system from description. With respect to social analysis, the implication is that we cannot find formal systems in the communities we study; we can only impose them on those communities.

Positivist arguments in anthropology go back to E. B. Tylor, whose conceptions of theory, thought, and social evolution drew directly on the arguments and assumptions of Auguste Comte. In the United States before World War I, such positivism was largely held at bay in serious scientific circles by pragmatism. The reasons were both scientific

and political. Pragmatism was clearly associated with an experimental, rather than deductivist, conception of science, which Americans generally appreciated. But equally important, the prevailing pragmatism was also associated with democratic conceptions of society. Positivism, on the other hand, in addition to being recognized as associated with a deductivist and anti-experimentalist conception of science, was firmly associated with German and French ultranationalist authoritarianism —neo-Hegelian socio-legal theory as represented by Joseph Köhler and by German and French sociology. These schools of thought agreed with one another other absolutely in their rejection of democracy and experimentalism and in the connections they saw between them; they disagreed among themselves only about what forms of administrative absolutism and deductivism should replace them. Leading American thinkers around the time of World War I recognized this and had little sympathy with German/French philosophical imperialism in any form.

This changed in the 1930s. A new and not obviously political type of European positivist—mainly the logical positivists of the Vienna Circle and the similar Berlin "logical empiricists"—became objects of Nazi persecution, and a new generation of Americans welcomed them as heroic refugees. Logical positivism was consolidated as an intellectual movement in important and cohesive groups at the University of Chicago, UCLA, and UC Berkeley. After WWII, it provided the theoretical rationale for the Harvard Department of Social Relations.

Specifically in ethnology, positivist and logical positivist ideas and assumptions were invoked successively in the rationales for the American diffusionism of Wissler, Kroeber, and Lowie; Radcliffe-Brown's comparative sociology; Kluckhohn's conceptions of scientific method; Evans-Pritchard's structural-functionalism; Levi-Strauss's structuralism; Parsons and Shils' general theory; Homans' and many others' search for some version of Freudian psychodynamics; ethnoscience; Geertz's interpretivism (which Paul Friedrich rightly described as crypto-positivism); and Schneider's self-labeled neo-positivism, among many other efforts. Although most of these are now passé, they are not dead. There is wide recognition that what their proponents promised was not delivered, but there is far from consistent recognition of why and how they were doomed to fail in principle.

Although all varieties of positivism involve some version of Comte's and Mill's conception of form as inherently arbitrary and non-substantive and all consider theory to be preeminently formal in this sense, not all varieties are prominently associated with a program for "formal analysis." In philosophy, formal analysis is primarily attached to the varieties of logical positivism and its close allies. In anthropology, it has been primarily associated with ethnoscience, componential analysis, and now cognitive anthropology — and arguably also with Levi-Strauss's "structural" analysis and structuralism.

In the ethnoscience of the 1960s and 70s, the positivistic, referential, theory of meaning was clear and explicit. The focus was on taxonomies, and taxonomies were treated as sets of terms (identified morphologically) and the sets of objects that the terms referred to. Each "object," in turn, was a set of "features" —the counterpart of Mill's sensations or sense impressions. The possibility that ideas—as such—might have an essential role to play in this process was minimized or ignored. As applied in componential analysis of kinship terminologies, the method was to arrange the terms of the taxonomy and their supposed referents in such a way that "no term overlaps or

includes another; every component is discriminated by at least one term; and all terms can be displayed on the same paradigm" (Wallace and Atkins, 1960: 62). The "formal" order in the words, then, was taken as coming from the "formal properties" of what they referred to, on the assumption that each term could have only one referent in this sense — an assumption that is not in fact true.

Proponents of contemporary cognitive anthropology reject the earlier ethnoscientists' positivistic insistence that science can only study "action" or "behavior" and now say they are focusing on "idea systems" (D'Andrade 1995: 12), but the disagreement is superficial. The underlying methodological assumption is still that meaning can only be referential. In consequence, the concept of "idea systems" is still actually the same as when ideas were nominally ignored. They are still not what people actually think, *per se*, but rather the words (or, in some cases, drawings or images) they use and what in the analyst's view those words or images are "really about" in terms of the scheme of supposedly "formal" categories that the analyst imposes. D'Andrade describes the aim of the method as to produce an "emic" analysis, and:

> To carry out an emic analysis one began with a set of categories brought in by the scientific observer and then tried to find out which of those categories really made a difference with respect to the way the natives understood and responded to things. (D'Andrade 1995: 18-19)

What is such an analysis an analysis of? What is it describing? Is it the observers' categories? Is it the observer's categories that make some difference in the behavior of the natives? Is it the native's behavior that the observer's categories pertain to, or some of each of the above? The critical question is why we should think that any categories "brought in by a scientific observer" should make a difference to the way natives understand and respond to things. And more simply, why should we start with such categories at all? Why not start with the natives' own categories, find out what they are, and then see what difference they make?

There is a logical affinity between the positivist conception of meaning in general and taxonomies in particular. Taxonomies generally do have a set of referential meanings corresponding to the order of the terms. But while cultural communities do have taxonomies in this sense at least in some places and for some things, there are in every community many other kinds systems of categories of very different types. There are all sorts of things we readily identify and distinguish from one another without any clear set of exclusive defining features, and conversely there are all sorts of systems of ideas that are not taxonomies. Taxonomies cannot be taken to represent culture in general, and taxonomic meaning cannot represent meaning in general.

When you focus a study on one taxonomy at a time, it is easy to lose sight of the fact that in the community as a whole, taxonomies regularly contradict one another. The Aristotelian elements earth, air, fire, and water are not the periodic table, and the two systems are associated with entirely different cosmologies, yet both are part of "our culture." The difference between Catholic religious cosmology and what physicists call cosmology is not just a matter of different levels of expertise. What philosophers call language is not what linguists call language. Moreover, many cultural idea systems are not taxonomies at all. A myth or ideology is not taxonomy. Neither is a scientific theory.

And finally and most importantly, neither are idea systems that define reciprocal social relations, like kinship terminologies and most other terminologies that we use to form organizations and distinguish one organization from another. Taxonomies of the sort that cognitive anthropologists have described are unquestionably real and sometimes important. But they are not all there is to culture. Culture is not the sum of all taxonomies but it is also not a "mazeway" or singularity that produced a single personality type. There is, in fact, no evidence that it is a singularity or totality of any kind, and there is abundant evidence that it is not.

CRITIQUE OF "ANALYTIC"

The analytic-synthetic distinction as articulated by logical positivists has many philosophical predecessors, including Aristotle's dichotomy between *a priori* analysis and *a posteriori* analysis and Locke's distinction between relations of ideas and matters of fact. As such, it had been criticized and rejected by many major thinkers before the logical positivists took it over. It has been criticized by still more since then. One of the more important recent critiques was W. V. O. Quine's 1953 article titled "The Two Dogmas of Empiricism." For Quine, empiricism meant logical positivism and its several allied schools —philosophical empiricism rather than scientific empiricism. He uses this identity repeatedly without noting what might be misleading about it. The two dogmas were reductionism and the analytic-synthetic distinction. Quine's aim was to show that they were "ill-founded" in order to replace them with an alternative "empiricism" which was "more thoroughly pragmatic" (1953: 20).

Reductionism was the "belief that each meaningful statement is equivalent to some logical construct upon terms which refer to immediate experience" (ibid.). Quine's definition thus clearly takes in the line of argument that includes Mill's sensationalism and carries through what Otto Neurath (1955) and Rudolph Carnap (1959), among others, were arguing for under the heading of "physicalism." Quine's criticism was that it involved a basic self-contradiction —namely, that it violates its own premise that all statements can be reduced to logical and experiential components. This is because the idea of a set of qualities cohering at some point in space-time involves the idea of "is at" that is neither an immediate sensation nor an operator in any recognized logic. It may not seem like a very impressive argument from an empirical perspective, but from Quine's "logical" perspective, it neatly hoists the logical empiricists by their own petard.

While Quine's criticism is regarded as devastating by many philosophers, it does not explain what is wrong with the underlying notion that "logical" systems or schemes that seem to be true by definition cannot also be based on fact. It is true that many important formal schemes seem to have the properties of being pure matters of definition, including mathematics in general as well as such systems of mathematics as the various recognized geometries. By far the easiest way to explain why the sum of the angles of a triangle is 180° is to say that it is so by definition, and you cannot argue with a definition. It is also true that once we recognize that Euclidean geometry can be stated as a deductive system of postulates, theorems, and corollaries, it is possible to say that it might have been invented as a pure flight of definitional fantasy. But, is it only this? We can assert something only by definition, and we can assert something only on the basis of

observation, but why should we think that stating that something is true by definition *precludes* the possibility that it is also a matter of fact?

The logical positivists' program to unify science by constructing a single uniform logical language into which the propositions of all of the various sciences could be translated depended on their assumption that all formal or "analytic" systems were the same. They were the same, according to the positivists, because they were all equally matters of (arbitrary) definition. Because geometry could have been constructed as a purely intellectual exercise, their suggestion was that it could be regarded, in principle, as actually having been so constructed. And if all other formal systems were also the same, for the same reasons, then it must also be the case that one could invent a single, more comprehensive, system that would encompass them all and from which, in turn, they all could be deduced. If the different formalizations were grounded in different realms of fact, then their program had no conceptual justification.

To me, the positivist argument has always seemed to be little more than a wishful play on the ambiguities in the idea of "might" or "possible." "Might be" in the logical sense of "not inconceivable" or "not a self-contradiction" is very different from "might be" in the practical sense of "probably did happen that way" or "a method that has a good likelihood of succeeding." Euclidean geometry is not only a tight and coherent system of definitions. It is also a system that *works*. It gives us a grip on real shapes. Historically, it provided the most fundamental basis of reasoning for the astronomy and physics of Copernicus, Galileo, and Newton. Once it is learned, it articulates properties of nature in such a way that thinking about shape, size, and motion without it becomes almost impossible, at least when we want to be precise.

Any distinction, if followed consistently, can be the basis of a system of formal deduction. Many such schemes have been invented, and there are an exceedingly large number of possible distinctions that have not been developed. There is in principle no necessity that such distinctions should make sense to us; any set of symbols will do. It follows that the probability that any one such scheme will actually work as a geometry, in the way Euclidean geometry does, is exceedingly small. The ratio is, in fact, "exceedingly large" (or whatever that may translate to as a number) to about three — three being the most common number of alternate geometries that are actually useable, and they are in fact all variants of Euclidian geometry. It follows that even if creating formalizations by purely arbitrary methods without a descriptive target is always "possible," once you have such a target this is an exceedingly inefficient way to try to hit it.

EMPIRICAL FORMALISM

There are alternative views of the nature and basis of systems of analysis like geometry, and mathematics in general, both in and out of philosophy. Up to the Renaissance, the predominant view was, in fact, that mathematics was empirical: a formalization of relationships that actually existed in nature. That it actually had been derived this way historically is beyond doubt, but the view I am referring to was not only historical but also epistemological. Galileo was a physicist and a mathematician, with no line between them. The chemical conception of the atom associated with the periodic table, and its application in chemical analysis, is chemistry and mathematics. Relativity

physics is physics and mathematics, and, as Dwight Read, Michael Fischer, and I have previously argued, the mathematical analysis of kinship terminologies is kinship and mathematics. Linguistic grammar is certainly formal, although we can argue about whether it is mathematical.

There is a great deal of overlap between the idea of a formal system in the sense of these types of analyses and the idea of structure. The idea of structure is most clearly expressed with a system of relationships that we can represent with a diagram of some sort. It is also very often the case that formal systems are most readily understood when converted into diagrams. And when we have something we can draw or diagram as a structure it is also generally possible to describe that structure in terms of a set of clearly separated categories with stated mutual relations that will generate it, hence as a system of formal rules.

Systems of ideas that are both formally cohesive and genuinely descriptive, including geometry, are what Immanuel Kant was concerned with under the heading of the "synthetic *a priori.*" Kant distinguished these from ideas or statements that were analytic *a priori* as well as from those that were synthetic *a posteriori*. Analytic *a priori* statements or definitions were those that were true by definition alone. In Kant's terms, they were statements or definitions in which the predicate simply repeated the subject. Mill and Carnap's conceptions of *a priori* propositions or truths parallel this. Synthetic *a posteriori* were Mill's "real" or accidental propositions or statements or definitions that were straightforwardly factual. In Kant's terms, they were definitions or statements in which the predicate added information to the subject, but depended on experience and hence were not true *a priori*. Both were real, but not very interesting. Synthetic *a priori* was the interesting case that seemed to bridge between them but was also more than either. These were ideas or systems of ideas that seemed to be true by definition but in which the predicate did not merely repeat the subject but rather seemed to "expand" or add to it.

Synthetic *a priori* statements involve different kinds of what in English translations of Kant's writings are called "judgments." By this he meant a conscious perception —a judgment that some thing is something. These judgments were of four main types, each discussed in one or more distinctive *critiques*. He described judgments of physical phenomena in his *Critique of Pure Reason*, judgments regarding human action and the relation of means to ends in his *Critique of Practical Reason*, ethical judgments independent of judgments based on experience of relations of means to ends in his *Groundwork of the Metaphysics of Morals* and the *Metaphysics of Morals*, and aesthetic judgments in the *Critique of Judgment*. In each case, his analysis exposed certain basic categories that appeared to be grounded in the nature of rationality itself (hence *a priori*) but were not simply matters of definition. For physical judgments, these were the categories of "mathematics," exemplified by but not limited to geometry from Euclid through Newton. For morals, they were fundamentally the ideas of reciprocity, freedom of choice, mutual interdependence, and the consequent necessity to recognize all people as means as well as ends in a kingdom of ends.

One of Kant's most general recurrent points was that objective knowledge or judgment was not the opposite of subjective judgment. Objective judgment was built upon subjective judgment. All judgments are "initially subjective," referring only to

ourselves. But some of these subjective judgments can be made objective by construing them under systems of categories that we can share. The categories that are synthetic *a priori* are objectifying categories of this sort, and Kant's great contribution was to show not only that this was so but also why it was. It was because they responded to both the constraints of practical action in a community and the internal requirements of coherent thought and communication. They had to represent general properties of what they were judgments of in a way that would conduce to effective action and interaction, and they also had to reflect the general requirements of coherent thought and communication.

The categories that were synthetic *a priori* were thus not independent of experience or what came before experience. They were, rather, what coherent, communicable experience must presuppose. Kant did not argue that they were arbitrary, as a definition might be, but rather that they were necessary, even though they were learned, could change over time, and were (in our terms) socially constructed. This was the conclusion that the positivists of all stripes have persistently attempted to set aside. They did not succeed, for three main interrelated reasons. First, the positivists did not address his criticism of the previous philosophical schemes whose assumptions they accepted. Second, their criticisms of Kant were wrong on textual grounds, to the extent that they even tried to make them on textual grounds. Kant simply did not say what positivists say he said. And third, they did not address his constructive, substantive, analysis. These disagreements are still salient and Kant's arguments continue to be widely misrepresented, mainly by those who persist in trying to do the kind of speculative philosophy that Kant's empiricism showed to be groundless.

Substantively, what Kant was doing was finding the formal bases of the traditionally recognized branches of philosophy, which were also considered to be the divisions of organized knowledge in general. His aim was not to support or to reify them, as Richard Rorty has claimed repeatedly (1979: 8, 11, 262; 1982: xv, 143, 167, 172), but to see what, if anything, they rested on. Kant's aim was, as he said in the *Prolegomena to Any Future Metaphysics,* to build a "formal science" (1783: 262-263), and he meant this in the present empirical sense. That is why he called his main analyses *critiques*.

In our terms, Kant's critiques described the conceptual foundations of physics, ethics, social interaction in general, aesthetics, and logic. These are, of course, systems of cultural knowledge—or at least bodies of cultural knowledge—and Kant recognized this. Like previous philosophers, Kant found these foundations in the nature of rational thought. He focused especially on the way they formed natural philosophies and epistemologies that were powerfully consistent from the perspectives of all their users, as opposed to the philosophies and epistemologies of his philosophical predecessors that were full of contradictions and often made no sense from the point of view of most of those they were supposed to apply to. The concepts of space and time, for example, are such that we can all share them without contradiction and thereby all use to communicate a common framework; but they are nevertheless concepts. But like David Hume and unlike most of his other predecessors, Kant found rational thought itself to be inherently social and activity-oriented, as opposed to private and passive. W. T. Jones 's *Kant and the Nineteenth Century* describes Kant's pivotal break this way: "Abandoning the traditional view that minds are the essentially passive contemplators of independently existing objects, Kant held that objects are constructs in which the activity of minds plays

an essential part" (1975: xx). In the traditional view, which we still see in positivist epistemology, a mind does nothing but perceive or think. As such, it is inherently alone; it does not do work, it does not communicate. For Kant, minds direct action and interaction; they communicate and they do work. Objects form in this process; take on their identities in this process. On this basis, Kant's formal systems, i.e., his analyses of ideas that were true *a priori* but also synthetic and not *merely* analytic, were the ideas that this constructive process had to presuppose. Perhaps more to the point, they had the kind of consistency that this process had to presuppose, which is why and how they seemed to be inherent in nature as well in our definitions and perceptions.

One of the many points of Kant's analysis that separates it from the line leading to the positivists was his notion of logic. For Mill, logic is essentially Aristotelian class inclusion logic. For the logical positivists, it was some more abstract scheme that would include Aristotelian logic and from which Aristotelian logic could be derived as a special case (Carnap, 1955a; 1955b; 1967). So Aristotelian logic is still the prototype and Aristotle's vision of a single, unitary, deductive system of knowledge fixed for all time is still the goal. There is no empirical basis for this; it is simply assumed. For Kant, there were many logics, many systems of formal or formalizable reasoning. What was important for creating such a system was not any one particular formalization, but rather the fundamental principle that all formalizations had to depend on and could not exist without: self-consistency.

Kant recognized self-contradiction in many ways and his requirements for consistency had many more dimensions that those of the logical positivists. Simple logical self-negation in a propositional sense was only one —you cannot assign a predicate to a subject and also assign the opposite predicate to that subject and have both assignments be true. In the *Critique of Pure Reason* he recognized that the consistency of concepts of space and time was such (and had to be such) that they have the same meaning for all users from all perspectives, and allow the shared intellectual construction of selves, series, and the universal encompassing totalities. In the *Metaphysics of Morals* he recognized that a principle of action is self-contradictory as a basis of moral obligation if it is self-nullifying, and it is self nullifying if it contradicts the categorical imperative: "Act only according to that maxim whereby you can at the same time will that it should become a universal law." This is, of course, the principle of reciprocity itself, universalized. You could not lie as a way to extricate yourself from a problem without supporting the principle that all people should lie to extricate themselves from problems, because to do so would destroy credibility and thereby obliterate the possibility of gaining by lying. You could not treat others as only a means to your ends without nullifying the possibility of others treating you as an end in yourself, so reason required that we recognize the principle of treating everyone not only as a means but also as an end. In the *Critique of Practical Reason*, he recognized self-nullification in the relations of means to ends; you cannot rationally use a means to achieve an end that experience shows to be destructive of that end. That is, Kant's search for systems of ideas that were synthetic *a priori* was the same as the search for ideas that provided the underlying logics of the various bodies of knowledge he was concerned with, and was in that sense a formal analysis of each of these fields, in the sense of an exposition of their formal principles. But it was also an empirical analysis, since the requirement for self

consistency is not only a rational necessity but also an observable one. We not only cannot conceive of a conceptual system that lacks this principle, we also cannot find one.

CONSEQUENCES OF KANT

The subject matters Kant dealt with in his several critiques are not, in fact, the only subject matters, and the synthetic *a priori* categories of judgment that he described are not the only such synthetic *a priori* categories. Natural philosophy, moral philosophy and logic may be the most general classes of organized knowledge in terms of Western scholarship, but they are not the only systems of knowledge, either in the West or elsewhere.

Kant's most revolutionary accomplishment was to demystify epistemology. He took it out of the realm of the merely subjective and into the realm of the objective just in the way he argued that the same thing had happened in all other areas of knowledge. This was a major stimulus for the development of a wide range of what we now recognize as the modern social and behavioral sciences: human geography, physiological psychology, linguistics, empirical jurisprudence, and of course ethnology itself. The milieus of Malinowski and Boas were both solidly and self-consciously of this tradition. But even with all that Kant did and all that his successors did, there is much more still to do.

ETHNOLOGY

With respect to the history of ethnology, it is of definitive importance that Kant's analyses did not lead him to delineate the kind of idea system that is most important for understanding the nature and basis of our central subject matter, by which I mean the idea systems that human beings use to define the roles and positions that make up social organizations. Kant's analyses included recognition of the ideas of the self, series, and other as culturally constituted social-psychological organizing principles in a generic sense and he recognized the importance of the general idea of reciprocity in many ways beyond what I have described. But he did not recognize the nature and function of social-organizational ideas that we used to define our selves with specific content in specific in relation to specific others in specific behaviorally salient contexts: self as father in family, self as professor in university, self as citizen in state, and so on. In general, he did not recognize the need to systematically delineate and analyze the systems of relational concepts that constitute kinship, government, religion, business, economics, or the professions. This analysis is now being developed, but it will be an enormous task to carry it to the level of thoroughness in regard to the formal properties of the idea-systems and to their consequences in the fine-grained details of daily thought, communication, and interaction that Kant exemplified for the ideas he did describe.

The method by which we can identify the most fundamental self-consistent assumptions of systems of cultural ideas like those of kinship, government, and religion is two-pronged. First, we identify a corpus of ideas that are apparently interrelated for the people we are trying to understand, and that seem to fit together logically and consistently. Most often, this will be ideas on or about what is indigenously recognized as a single topic, and that topic will have a name. In the case of organizations, it will be the idea of one organization or, more often, one type of organization. The topic must be of

their designation, not our designation. We then first try to identify the underlying principles that hold that topic together by what amounts to critical textual and contextual analysis. Second, we check this analysis by basing an elicitation procedure on those principles to see if we can, in fact, elicit the ideas as a system that is internally consistent and logically bounded.

Internally consistent means that it avoids self-contradiction. Logically bounded means that each element in the system will either be recognized as tied to other elements in the system or will be an endpoint, beyond which one is dealing with "something else" or "another matter" —another type of relationship or no relationship. And of course we continue to check our analysis in the run of ongoing behavior, looking to be sure that the ideas recur regularly together but do not regularly recur with ideas of other systems while retaining the same implications and associations. It might, for example, be argued that "father" as a title for a priest is the same idea, and not merely the same term, as "father" as a male progenitor, but it cannot be found that the two ideas in the two contexts have the same implications and associations. A pilot who flies an airplane may arguably be said to involve the same idea as a pilot who guides a boat into a harbor, but again the implications and associations—the ideas that define how the two pilots relate to others in their organizations, are observably entirely distinct. The idea of a president in government is defined in relation to an entirely different set of "others" from a president of a business firm.

Essentially, ideas that are within a well-formed cultural idea system will always have clear relationships of association, contrast, or complementary distribution with one another. These are structural relations. Ideas that are not defined in the same cultural idea systems may occur together sometime in conversations, but they will not have this kind of structural patterning. They might occur together or they might not, and if they do occur together they may or may not have the same conceptual relations. A person who is brother to A will never be a father or cousin to A, and this applies to all other people who are brothers to A or anyone else (in a kinship context). A person who is brother to A may be a Democrat, but this will not apply to all other people who are brothers to A, or anyone else. Being a brother to A implies that A is brother in turn, and this has definite conceptual associations —which make up implications. Being brother to A and a Democrat has no such definite associations. So brother, cousin, and father are part of one system of ideas, but Democrat is not part of that system.

The most important of the systems of relations that anthropologists have dealt with over the years is kinship. The core of every kinship idea system is what is called— rather unfortunately—a kinship terminology. In 1971, I published description of the way I had elicited terminologies in the field that did not involve treating them as taxonomies in the way that componential analysis had. The method treated them forthrightly as sets of interdependent definitions of positions and relationships between positions, and led directly to a diagrammatic representation that informants themselves could recognize and use. Strictly as descriptions and leaving aside the doubtful epistemological assumptions, the componential analyses had had three major weaknesses. They never gave a complete account of all kinship positions in any system, meaning a description that included all positions and all possible ways they would be defined. They did not represent positions whose definitions could not be stated genealogically. And they could not deal with

reciprocals. They did not represent reciprocals because componential analysis began by separating "terms of address" from terms for "reference" and focusing only on the latter (cf. Goodenough 1965; 261). It is in the address terms (or better the addressing function) that the reciprocal character of the definitions becomes most inescapable —and by the same token where the idea that meanings are simply denotata becomes most immediately untenable. The terminology I described was Punjabi. The description was complete for all positions, it included positions defined by other than genealogical criteria, and it included recognition of all reciprocals for every position. Sylvia Vatuk quickly followed with a very similar representation of Hindostani (1972). One might have thought this would be decisive, but it wasn't and componential analyses continued to appear. In retrospect, perhaps I should have been more outspoken about the philosophical basis of the argument, but at that time extended discussion of philosophical alternatives was not part of anthropological discourse.

In the Kantian manner, my method began with the recognition that the formal bases of these systems could not be something imposed upon them, but must be something found within them. It was, in fact, inherent in the most universally recognized feature of kinship terminologies, which (again) the componential analyses had ignored because it did not fit with their narrowly referential conception of meaning: that terminologies were ego-centric. They always assumed an "ego" or user, whose relatives were being designated, and were in this sense clearly "subjective" rather than "objective" in the sense the componential analysts were trying to impose.

In my graphic elicitation procedure, this subjectivity was both the starting point of the elicitation in a visual sense and the central premise in a logical sense. Around this ego were a series of direct relatives immediately defined in relation to it. In English, they are father, mother, brother, sister, son, daughter, and spouse. Other systems have others. Spouse may be such a direct relation, or it may not. But there is always such a core and once the core is identified all other positions can be elicited as relative products of the relations within it, with added qualifications as one proceeds outward. Reciprocals are defined across this central position. The definition of each position is the chain of links to it from ego, and the reciprocal of that term is the set of links obtained by reading the first chain in reverse, taking the first endpoint as ego in turn. To get the reciprocal of father's father you take the latter (the person who is grandfather) as ego and go through the links to son and son: grandson (if the initial ego is male).

The idea of relative products, *per se*, was not new when I first applied it (my field analysis was actually done in 1964). As White notes, Lounsbury had used the idea in 1956 and Burling used it in his critical reply to Goodenough's analysis of English kinship terms in 1970 (White 1974, 416-417; Lounsbury 1956; Burling 1970). In mathematics and relational logic it was far older. Bertrand Russell made the idea a central theme in the notation he developed in *Principles of Mathematics* (1903). But without also recognizing reciprocity, it was not possible to see the true formal power and importance of either idea. A formal system is in the nature of the case a wholly constrained system, meaning one in which the definition of every element is determined by or entailed in the definitions of the other elements. The idea of a relative product mainly embodies the idea of transitive consistency, which is one general type of formal constraint. Reciprocal consistency is another. For seeing how kinship conceptions worked it was necessary to

connect the two ideas to one another formally. This can be done, but only if one takes both ideas as part of what has to be analysed.

In 1974, I presented a diagrammatic analysis of English constructed by the same method to a seminar in the Department of Anthropology at UCLA. The shape and logic of the terminology were readily seen to be very different from Punjabi. Dwight Read was in the audience, understood the underlying significance of a kinship terminology having a generative structure in this full, reciprocal, sense, and developed the analysis from there. In 1974 he published "Kinship Algebra: A Mathematical Study of Kinship Structure" in Genealogical Mathematics, edited by Paul Ballonoff. The argument was that terminologies had their own logic which was not dependent on reference to genealogy, and that this logic could be precisely specified through mathematical structures called algebras that are defined using the language of set theory. The advantage of the algebraic formalism over the kind of graphic form I had produced in my field elicitation is that it makes it possible to prove the logical coherence of the system by generating the structure displayed in the graphical form from irreducible concepts, rather than simply showing visually the outcome of the underlying logic. In 1984 he published "An Algebraic Account of the American Kinship Terminology" in *Current Anthropology*, which applied the formalism of algebraic structures defined through set theory to English and showed in detail the parallels of the algebraic structure generated from irreducible kinship concepts with the diagrammatic form I had produced. (At this point, my analysis of English had been circulated informally but had still not been published.) Then, working with Clifford Behrens, Read developed the first version of the Kinship Algebra Expert System (KAES) computer program, which generalized the analysis of the AKT by identifying what would be the generative components of any kinship terminology and the means to reproduce the component positions of the terminology from generative premises. The first description of the KAES program was published in 1990 (Read and Behrens, 1990) and development has continued.

The current version of KAES, written in Java by Read and Michael Fischer, is available on the internet at http://kaes.anthrosciences.net/. It can be downloaded with files for the kinship maps of American English, Punjabi, Shipibo, Omaha, and Trobriand (male terms only). The files are "zipped". They should be downloaded to a single directory. When unzipped, a file labeled KAES with the program KAES and a second file containing the kin term maps will be produced. If KAES has no file-type label, it should be renamed kaes.jar. It will then run when clicked on in an Apple or other Linux system and on Microsoft operating systems if the Java Run Time Environment (JRE) has been installed. (The latter is available for download at no charge from Sun Microsystems.)

The KAES program allows the user to input a kinship map in graphic form. It asks for the generative terms and then uses them to ask for the names of new positions exactly as in the field method I have described. It also, however, consolidates the results as it proceeds, building a graphic analysis on-screen. One important advantage of this method over the pencil-and-paper field procedure is that the computer version makes it easier to identify redundant generative positions, and thereby arrive at the one set of generators that is most economical and in that sense logically most basic. Once the generative terms are identified and the full set of positions that they generate is obtained, the program can automatically produce various analyses, including both an algebraic

form and a number of different reductions depending on the logical structure of the system itself. For example, if the system has sets of positions that are all defined the same way but differ by just one feature, like gender, it can identify that and replace the differentiated terms with a common cover term —like "aunt-uncle" for aunt and uncle. If the system uses terms that mark gender but are not structurally similar it can also (of course) recognize that and analyze out just a male or just a female terminology. The software can also produce a matrix of kin-term glosses in which every position is defined in terms of relative products of the generative terms, such as "father's father" as the definition of grandfather. It can determine if the analysis is simple or complex (capable of being reduced), and if it is complex can reduce it. Once it is simple, the basic algebraic relationships can be displayed. Before KAES, every one of these operations was a matter of the deepest controversy packed solidly around with unresolvable methodological and philosophical argument, if it was conceivable at all.

The KAES analyses confirm beyond any reasonable doubt that these kinship maps are formal systems. Notwithstanding their complexity, they are deeply and many-sidedly self-consistent. The mathematics would not apply and the computer program would not run if this were not the case. Because of this self-consistency and complexity, and also because of what they define substantively, they are powerfully generative. They do not merely replicate the application of a few terms out of a larger set of terms to a few genealogical relations out of a usually much larger world of possible relations, as the componential analyses do, but, rather, regenerate whole worlds of appropriately interfaced and mutually consistent reciprocal relations. This is a great advance.

Since Lewis Henry Morgan, anthropologists have sought to represent other systems of kinship ideas on the assumption that what was designated could be somehow indicated apart from the system of designation: *first* say what the terms apply to, *then* look for their conceptual patterns or significance. The result, from Morgan to componential analysis, was that on this basis no one could provide a complete account of such a system or, what is the same thing, describe it in such a way that one could reconstruct it fully from the description. Moving beyond this to developing an actual, culturally neutral, way to compare any kinship system with any other cross-culturally was never more than a distant goal. Now the problem of providing a complete and coherent analysis is solved and the goal of a system of comparison is attained. We have an accurate theory of kinship in the way that the periodic table embodies accurate theory in chemistry and the double helix is accurate theory in biology.

Recognizing the generative power of these formal systems is the key to escaping the simplistic choice between idealistic or materialistic conceptions of meaning that bedeviled the debates between the componential analysts and David Schneider. Once we see the many-faceted and subtle generative implications of the formal premises that define systems of kinship relations, we realize that neither option can possibly be correct, any more than it could be correct to say that the theorems of Euclidian geometry must either be imposed on objects to make them triangles or must be derived from triangles as they actually exist. The ideas can't be imposed because the system as a whole is far too powerfully generative to "keep in mind" —to hold as a single image in any one person's imagination. On the other hand it can't be simply derived from its objects because it is very clear that those "objects" are not concrete things in any simple sense but rather

classes, classes of classes, and even classes of classes of classes whose "existence" is in a very large part predicated on the mutual distinctions among them. Moreover, the relation between these classes and their objects is deeply imbedded in patterns of reciprocal behavior to which we have strong conceptual commitments and upon which we have powerful practical dependencies.

Empirical-formal analysis in this sense is not limited to kinship. I have described five such systems of ideas for the one Punjabi village where I did the kinship analysis, each entirely separate and distinct (Leaf 1972). These were not by any means the only such conceptual systems in use the village, only the ones that were in substantially universal consensus. Many more were held by smaller sets of individuals. Comparable analyses by others thinking and working along these same lines include a model of place that underlies Tongan conceptions of direction (Bennardo 1999), Malay conceptions of intersubjectivity (Mckinley 2001), planning algorithms for South Asian farms (Leaf 2000), factional conflict in Pakistan Punjab (Lyon 2002), and Lansing and Kremer's explication of the underlying strategic logic of Balinese water temple arrangements (1993). And of course many of the classical "structural" studies in social anthropology did the same without recognizing what they were doing in the present terms. Laura Bohannan's "A Genealogical Charter" (1952), Paul Bohannan's 1957 *Justice and Judgment among the Tiv*, Turner's explication of the logic of rites of passage (1969), Leach's explication of the Kachin Gumsa, Kachin Gumlao, and Shan organizational models in highland Burma (1954), and his *Structural Implications of Matrilateral Cross Cousin Marriage* (1951) are especially clean examples. In fact, when Houseman and White (1998) reanalyzed Leach's *Pul Eliya* (1968) data using the assumption that the equations of spouses with matrilateral cross-cousins in its kin terminology described actual marriage rules and using network analysis to represent what the logic of those rules would be in the context of the constraints of their demography, system of property-holding, and social rank, they were able to show that the kinds of networks that followed from the conceptions did exist in fact. The Pul Eliyans' kinship conceptions were imbedded in and corresponded to a system of marriage exchange that they had said they had but that Leach himself had not been able to see.

CONCLUSION

Of course my argument is not simply that Kant was a heavyweight revolutionary thinker and Mill and the logical positivists were reactionary lightweights, or even that a Kantian analysis is more elegant, truer, and more consistent than the kind based on the assumptions of positivism. I am also concerned with payoffs, what physical scientists sometimes call a theory's productivity. My argument is that an analysis that recognizes form as a property of systems and seeks to expose that form as the deepest inner order of those systems will lead to accounts of those systems that are enormously more powerful for capturing observable, predictable, and verifiable realities that will in turn lead to still further discoveries, in the kind of self-corrective, experientially and experimentally grounded discipline that fully deserves the label "science."

I think I am right to believe that even the most enthusiastic proponents in the heyday of ethnoscience recognized that what they were capturing of the way culture worked was only a pale shadow of what they knew it actually did. Culture is not simply a

screen or template between me and some set of objects, like a pair of tinted lenses or set of labels with a specific twist. It defines me as the observer, all the options by which I might view what is around me, all possible things I might view, all of the ways that all others can view me and my viewing in turn, and allows me and all those I communicate with to bring all of this together in what seems to be a coherent whole. It does not merely describe reality; it is powerfully generative of it. Yet it is not fiction. It is food and fire, my dependence on you and your dependence on me, success and failure, life and death. By trying to characterize indigenous cultural formalizations by first imposing our own cultural formalizations upon them, we inevitably truncate them and obscure this power. It is exactly like setting off on a photographic expedition with film that is already exposed. It is only by seeing how cultural idea systems are based on their own formal structures that we can understand this generative power.

To say that these systems are *generative* means that they are much more than just a few interrelated ideas that we can represent with formalizations if we want to. They are systems that require such representation and cannot be understood otherwise, just in the way that is true of systems of logic, mathematics or models of the atom. They are not simple taxonomies, static stereotypes, or linguistic frames like the different images of time built into different systems of linguistic tense. They are well-formed and highly structured systems of conceptual premises that generate large—usually unendingly large—numbers of possible inferences. Moreover, they are not only found here and there in culture; they are the basis of it. They are what generate and support many of the more ordinary stereotypes, beliefs, and the rest, just as the premises of geometry generate and support the specific figures of geometry. They are the basis of the distinctively expansive nature of human communicative abilities, compared to those of other species and what make cultural and organizational consensus possible.

All organizations must have a form in the sense described. There must be some most basic set of logically consistent and generative relationships among its parts. Without this, people could not imagine themselves as "in" them, just as we could not imagine ourselves within space and time if space and time did not rest on formal concepts of a similar sort but with a different logic. Although the character of formal systems in culture has been obscured by the application of theory and metatheory that made it impossible for them to be uncovered in their own terms, they are not mysterious when approached without such an intellectual overburden.

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