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Kinship, Genealogy, Objectivity, and Ethnocentrism

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Abstract

The collapse of anthropological interest in kinship in the 1980s is commonly attributed to David Schneider's argument that kinship is not objective. It is a Western ethnocentric imputation. L. H. Morgan's initial assumptions were indeed ethnocentric. But the dissonances this generated were immediately noticed in the field studies he stimulated. A widespread drift developed toward representing indigenous ideas of kinship in their own terms. This was derailed when kinship analysis came to be dominated by proponents of epistemologies drawn from Positivist philosophy. These included Schneider himself. The problems Schneider identified flowed from this, not the earlier studies. In fact, kinship relations are formed using indigenous idea-systems that have very specific logical properties. This article reviews what they are. It describes the opposition between the epistemologies of genuine empiricism and Positivism. It describes the empirical beginnings of the anthropology of kinship, the expanding Positivist intrusion and finally the continuation of the same epistemological errors in the self-described "post-Positivist" line of arguments since Schneider's critique. Positivism was a reactionary political ideology wrapped in a dogmatic and anti-empirical conception of science. It cannot be removed from ethnology's past but it should be removed from its future.

Introduction: The Kinship Apocalypse

This article describes the accumulation of mistakes leading to the collapse of anthropological interest in the scientific analysis of kinship and social organization in the 1980s, the persistence of these mistakes since the collapse, and the alternative that avoids them.[1]

Until 1984, socio-cultural anthropologists agreed that kinship is an objective fact of social life in every known human community. They agreed that all kinship systems are similar to one another in some respects but differ in others. One of the important ways they were the same was that they all involved a distinct vocabulary of terms that were used for one's relatives and by one's relations to them in return. They agreed that these terms often did not translate readily into the anthropologists' own languages. They recognized that differences between terminological meanings were connected to differences between the larger social organizations of their societies.

Anthropologists disagreed about how to explain this universality and apparent importance, how to relate it to reproductive biology, what the systemic quality actually consists in, what made a term a kinship term, what a meaning is, and what kinship itself is. But there was no question that kinship was something anthropology had to deal with in order to understand what any individual human community is and what humans in general are.

In 1984, the disagreements overcame the agreements. The catalyst was David M. Schneider's *A Critique of the Study of Kinship*. This capped a line of criticism of two prominent contemporary arguments that he began over 20 years before: the componential analysis of kinship terminologies and the alliance-descent debate. For componential analysis, he argued that the argument was circular. The "components" that the analysts found to organize the meanings of kin terminologies were nothing more than the biological parameters implicit in the kintypes that the analysts used as their translations: "you are what you eat (1984: 3)." For Alliance-Descent, his main conclusion was that neither side had been able to produce a "total system analysis" that its proponents promised. And for social science in general, he concluded that a "quartet" of types of organizations --"kinship, economics, politics, and religion" (1984: 181) -- were "metacultural categories imbedded in European culture which have been incorporated into the analytic schemes of European social scientists" (1984: 184). They are Western European cultural impositions. They are not objective.

It seemed to be a classic insider exposé. Schneider was a leading kinship scholar. He was now declaring that there was no such thing. Who could know better? The mass of anthropological onlookers hastened to agree that the fault lay in the topics rather than the approaches to them. The consequence was a kinship apocalypse. The consensus that "kinship and social organization" was a priority topic that had to be understood for anthropology to advance as a science evaporated. "Kinship and social organization" lost its central place in anthropology curricula. Kinship eventually disappeared as a standard topic for the annual meetings of the American Anthropological Association. Prominent recent efforts to return to the topic continue to accept Schneider's criticism and avoid the specific problems that it focused on: kinship terminologies and marriage rules. They also continue to avoid the associated issues regarding meaning, relating kinship con-

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[1]I am happy to acknowledge the careful readings and very helpful suggestions of the two anonymous readers of this article.

cepts to rules for behavior and rules for behavior to actual observable behavior, and, most importantly, objectivity.

In fact, however, Schneider's criticisms only applied to one stream of anthropological theory based on one system of epistemological assumptions. Other analyses were based on other assumptions. His criticism did not apply to them. Yet the abandonment he triggered has extended to them. One of the main reasons for describing what led to the apocalypse is to restore recognition of this other stream.

Two Epistemologies

The two streams of epistemological assumptions took shape long before anthropology and extend well beyond it. They are idealism vs materialism on the one hand and demonstration on the other. I previously described them (Leaf 1979) under the headings of dualism (AKA dogmatism) versus monism (AKA Skepticism and Empiricism). The differences between them that are especially important here concern their modes of argument and their epistemologies in the broad sense of their theories of meaning, perception, truth and knowledge.

Idealism vs materialism is not a conflict between two epistemologies; it is an argument about alternative forms of one epistemology. The argument began with Plato's theory of defining essences combined with his dichotomous opposition between form and matter. Form is idea; matter is material substance. The Greek term being translated as "form" here is *logos*, which can also be rendered as rule or law (as in *anthropo-logy*). In Stoic and Christian theology, it is also God.

The problem that this is intended to resolve runs through all the Platonic dialogues but is described most dramatically in Plato's parable of the cave in the beginning of the *Republic*. The question is how we know what things are. The parable portrays people as individuals chained in a cave facing a wall. What they experience is shadows cast on that wall by unknown objects being manipulated by unknown means with light from a fire behind and above them that they cannot turn to see. So the problem is to find someone with insight to know what it is beyond experience that generates our knowledge within experience. This the natural ruler; everyone else is his natural subordinate.

Plato's claim was that things were what they were because of their form. Forms were qualities. Matter was shaped into specific things by "participating" in the form of that thing. So knowledge of a thing was knowledge of its form. For government, one had to have knowledge of civic virtues, such as courage, justice, and piety. Such knowledge was inborn. Few people have it. What we think of think of as education is actually just a reawakening this inborn knowledge. It follows that there are few natural leaders and only one person who had the greatest possible ability and that was the natural ruler, the philosopher king.

On the basis of the same view of the problem as explaining how knowledge arises in the minds of isolated individuals, materialism then became the contrary position that it is imposed or created by material causes. Neither form, matter, mind, or any of these processes is defined in such a way as to be directly observable. With the first Christian creeds, the rhetoric was institutionalized as the language of Church authoritarianism.

The alternative view is that knowledge is what can be demonstrated. This is represented in the Platonic dialogues by the Sophists that Socrates argues with. The most important is Pro-

tagoras, in the dialog of that name. In Greek philosophical texts that have come down to us, writers commonly distort the views of their opponents to make them appear less cogent. Plato is no exception. In many of the dialogues, the Sophists are straw men. But Protagoras was extremely well known and Sophism was the philosophical voice of Athenian democracy. So Plato had to be careful. Substantial parts of the dialogue appear to be authentic reproductions of Protagoras's arguments.

After about 400 BPE, the Sophist theory of knowledge was absorbed into Skepticism. After 200 PE, the most important representation of Skepticism was Sextus Empiricus' *Contra Dogmaticus*. Later Skeptical landmarks, such as Boyle's 1661 *Skeptical Chymist*, are now usually considered only as part of the history of science.

From Plato to the 18th ccccc`, idealism vs materialism was the dominant vehicle for academic debate for topics like ethics, psychology, logic, and theology. In countries European where these topics overlapped medicine, law, biology, physics, and the like, faculties occupied with idealist vs materialist arguments were deemed "higher" (Kant 1798). This ended only with Immanuel Kant's response to David Hume's criticism of John Locke (Leaf 1979: 39; Leaf and Read 2020: 16).

Locke sought to explain where we get our idea of cause and effect. His explanation was materialistic: it arises in our mind when we repeatedly perceive the same sequences of phenomena. Hume proposed an experiment. Pick up something and drop it. Do it any number of times. It always falls. Pick it up again. Can you now imagine that it will not fall? The answer is that you can. That is all it takes to disprove Locke's argument. Repetition does not *cause* you think cause and effect. So if we do not get the idea from nature, where does it come from? Hume's answer was "custom and habit."

Hume's conclusion was Kant's starting point. He began, he said, when Hume's skeptical critique "awakened" him from his "dogmatic slumbers" (1783 [1950]: 260). But he criticized Hume for having run "his ship ashore, for safety's sake, landing on skepticism, there to let it lie and rot" (Kant 1783 [1950]: 262-263; Leaf 2009: 42). Kant's aim was to put Hume's finding in a general form and establish a "formal science" (1783 [1950]: 263). Three of Kant's major conclusions are directly related to the subsequent explosion of empirical scholarship in law, linguistics, human geography, experimental and developmental psychology, and anthropology. The conclusions concerned objectivity, physical perception, and moral perception.

Kant proceeded by showing that the dichotomies on which the idealist vs materialist arguments were based did not hold up to experience. For the subjective:objective dichotomy, he argued that they do not actually exclude each other. All knowledge is "initially subjective." It is knowledge that the perceiving subject recognizes as theirs alone. Subjective knowledge becomes objective when it is organized under certain categories perceiving subjects share with each other (Kant 1783: 298, 299; Leaf 2009: 52). These are the categories Kant described as "synthetic *a priori*." They are *a priori* because they seem to be true by definition. But they are synthetic because they are also true substantively. They "expand" our knowledge of what we apply them to. His examples included geometry; I-thou-it; cause and effect; end and means; and the theories of the experimental sciences generally. Kant's understood Newton's physics. In his description, he coined the phrase "Copernican revolution."

For physical perception, Kant rejected the idealist vs materialist problem of explaining the correspondence between geometry as the form of space and actual space. There is not one thing in the mind and another in nature, but just one set of structuring principles in our sensory faculty. Geometry "formalizes," or articulates it. It is the way our senses present the world (including us) to ourselves. Kant continues:

> . . . thought space renders possible the physical space, that is, the extension of matter itself; that this pure space is not at all a quality of things in themselves, but a form of our sensuous faculty of representation; and that all objects in space are mere appearances, that is, not things in themselves but representations of our sensuous intuition. (1783: 288)

This is the basis of Kant's conception of knowledge that is *a priori*. It is also the basis of modern pragmatic theories of the social construction of reality, in that such theories recognize that we develop the categories that constitute our perception in interaction with others, that we impute them with others, and that we impute them to ourselves and others. They are socially developed, socially acquired, and socially situated.

For morality, Kant's *Groundwork of the Metaphysics of Morals* (1785) rejects the notion that moral knowledge rests on a different faculty than physical knowledge. Both depend on reason and reason is the same in all cases. He does not say precisely what reason is, but if you put his examples together there are two constant threads. It is our ability to make choices, to distinguish one thing from another. And it is our ability to recognize self-contradiction or self-negation. Kant's argument is that this is all the power we need to make moral judgments, in the sense of a judgment we make purely out of regard for what is right, out of a sense of duty. It is not to avoid punishment, for convenience, or for approval. Moral judgments that meet this standard are judgments consistent with the one universal maxim: "I ought never to act except in such a way that I can also will that my maxim should become a universal law."(1785: 18 [1956: 70]). So to judge oneself to be acting morally is the same as making the judgement that the principle of one's action can be followed by all others without self-negation. And what this has to mean, in turn, is that Kant is assuming that each such rational being can imagine themself in the place of all other rational beings, and all other rational beings as ones-self. It is a specification of the general principle of reciprocity. Reciprocity presupposes that one has an idea of the relationship between a self and others. The idea of consequences presupposes that one has an idea of cause and effect. Kant expanded on the ideas of self-other and cause and effect in the critiques of pure and practical reason. Here, his conclusion was to argue that the type of community in which laws embodying the recognition of human freedom of this sort could be most fully realized was a republic, with elected representatives.

Idealists struck back, beginning with Fichte and Hegel. Fichte had actually been Kant's student. They could not refute Kant's empirical arguments but they obscured them by construing them as a primitive version of their own idealism, adopting his terminology and twisting his meanings by displacing agency. Instead of recognizing that he had shown how we create our general ideas in communicating about phenomena, they construed him as showing that general ideas make us create phenomena by imposing themselves. This was Hegel's "phenomenology of the spirit" (*Geist*) and state-philosophy. The state is "reason forcing itself in history" by an entirely self-contained process of thesis and antithesis. So, all of our general ideas and all of our shared values come from the state and represent the state. The individual is an unimportant particular.

The most natural form of the state is an absolute dictatorship--as long as the dictator expresses the will of the state. These are the "neo-Kantians." They pretend to be new Kantians. They are actually anti-Kantians.

Kant's analyses were important in several of the political streams that fed into the Revolutions of 1848. These were suppressed by an alliance of resurgent monarchists and new industrial capitalists. Comte's Positivism (1865, 1854), Spencer's Social Darwinism, and Mill's Utilitarianism gained ground as justifications for this suppression. For Anglophone readers, these were not separate. Spencer described himself as a positivist. Mill's *Comte and Positivism* assimilated Positivism to Utilitarianism (Mill 1843, 1865).

But while they all agreed that they were Positivists, Comte and Mill had opposed epistemologies. Comte's was Hegelian. His program was to create a total unification of science, society, and religion, the "religion of humanity" (1854). Sociology, as his new science of society, was both the explanation and the cause. It created "the final preponderance of Subjectivity in its regenerated form," which is social rather than individual (Comte 1875: 471-472. Capitalization in original). Mill's epistemology was materialist, following Locke. Scientists invent theory to explain associations between materialistic "sense-impressions" (1843). Beginning with Tylor, Positivist anthropologists followed Mill.

Neither Comte, Mill, nor Spencer were university educated. They had no first-hand experience in the expansion of human sciences in research universities that had been underway since the founding of what is now the Humboldt University of Berlin, in 1809. This was the first research university in a modern sense. Humboldt's vision was Kantian (Bahti 1987: 444-445; Leaf 1979: 90-91; Leaf 2019: 50). The university was entirely dedicated to the concern Kant had described only for the "lower faculties:" finding truth. It would not have "higher" faculties, whose claim to superior authority lay in the need to avoid public disagreement (Kant 1798). In this academic environment, the idealist vs materialist arguments had been thoroughly discredited. So Positivism had little credibility. But the Positivist rhetoric included a very important innovation. Instead of presenting their claims as a continuation of the pre-Kantian philosophical arguments, they described them as science and dismissed everything before them as "metaphysics."

More scholarly versions appeared in the beginning of the 20th century: French Sociology of Durkheim's Circle and German Sociology associated with Max Weber. Then the Logical Positivism of the Vienna Circle. During World War II, Vienna Circle Positivism was re-established at the University of Chicago as the Unity of Science Movement (Neurath 1955). And finally, after World War II, self-consciously Positivist programs expanded or were newly created in many major American universities. Those with important impact on anthropology included Chicago, Berkeley, Harvard, Stanford, UCLA, and Yale.

Positivist Anthropology

The first anthropology programs began to crystalize in the 1860s, often in museums. Initial assumptions were demonstrative. The first Anglophone anthropologist to espouse an explicitly Positivist epistemology was E. B. Tylor in *Primitive Culture* (1889a) and On a Method of Investigating the Development of Institutions; Applied to Laws of Marriage and Descent (1889b). Tylor transposed Comte's fetishism into his own animism. Comte's unitary and deterministic conception of society became Tylor's unitary and deterministic definition of "culture." It was "that com-

plex whole which includes knowledge, belief, art, morals, law, custom, and any other capabilities and habits acquired by man as a member of society" (Tylor 1889a: i,1). Cultural explanations and explanations based on rationality and choice are mutually exclusive. The idea of human free will is a "fallacy" that "could be illustrated by the simile of a balance sometimes acting the usual way, but also possessed of the faculty of turning by itself without or against its weights" (1889a: i,3). The main emphasis in the above definition is not the list of elements, it is in the idea of "acquired by" in opposition to "created by." Accepting the Positivist assumption that "subjective" and "objective" were dichotomous opposites, Tylor's culture was "objective." What constituted a cultural trait or feature did not depend on the ideas of those in the community, it depended on Tylor. Culture formed itself by its own laws and imposed itself on individuals. It was what he found by his own objective methods. These were to be identified by Tylor's method of "adhesions" (1889b). Tylor's argument for adhesions reflected Mill's argument, citing Comte, that actual relations of cause and effect were not observable. All that was actually observable was ""constant relations of succession or of similarity" which exist among phenomena" (Mill 1843: 843). All of these ideas were built into the Tylorian definition of culture. All of them shaped the framework Schneider shared with those he criticized, and therefore also the kinship apocalypse.

Primitive Culture was about beliefs rather than social organization. But the article on methods was about kinship. It focused on customs associated with marriage and childbirth, including many that Morgan had discussed previously. These included joking relationships, avoidance of one's spouse's parents, rules of residence, levirate, sororate, marriage by capture, Morgan's "classificatory systems," cross-cousin marriage, matrilineal descent, and patrilineal descent. He found some associations. He did not look for causes.

In the United States, Tylor's Positivism was first taken up by the faction of students of Franz Boas associated with Clark Wissler and "diffusionism," most importantly Alfred Kroeber and Robert Lowie. Lowie's obituary of Ernst Mach (Lowie 1916) endorsed Vienna Circle Positivism. Mach was the only major physical scientist to associate himself with Positivism and explicitly dismiss the importance of experimental proof. Lowie's *History of Ethnological Theory* (1937) devoted an entire chapter to praising Tylor and his definition of culture.

The diffusionists set the central problems for G. P. Murdock, who relied still more openly on Positivist claims about science to resolve them. Murdock's failures in turn gave rise to the still greater reliance on the same ideology in componential analysis/ethnoscience.

Componential analysis and alliance-descent were easily the most prominent debates in kinship and social organization from the 1960s through the 1980s. They were common topics in journals and drew crowds at anthropological meetings, in good part because they made the most sweeping claims for total social or cultural determinism and their own abilities to demonstrate it. Kinship was where the claims would be validated; similar applications could then spread to all other topics. But the claims were not validated. Schneider's criticism of 1984 was not substantially different from what it had been in the 1960s (Schneider 1965a, 1965b, 1969). But in 1960s he was attacking fresh promises of easy gains. By 1984 he was explaining widely recognized failure and frustration.

Alliance theory was built on Levi-Strauss's contemporaneous effort to resuscitate Durkheim's 1912 description of Australian society without its original evolutionary framework. Self-described alliance theorists postulated a most basic society that was totally integrated

through a single, unitary, "prescriptive marriage system." This tied literally all action into a comprehensive system of male and female "prestations" going from clan to clan in opposite directions while all thought was organized into a comprehensive "dualism." It was the beginning of everything and, by implication, the explanation for everything.

The descent side of the debate was not cohesive and self-recognized in the way the alliance side was. But a few of those usually named did argue for a similarly unified, monolithic, and all-controlling "total system model" on the basis of their view of descent groups. The most notable were E. E. Evans-Pritchard and Meyer Fortes. Their Positivism goes back to Radcliffe-Brown's "comparative sociology."

Schneider's Positivism reflected his graduate training in the Harvard Department of Social Relations. The Department was formed in 1947 as an "interdisciplinary" program to bring together self-identified Positivist scholars from across the university, plus Freudian psychologists. So students in the program were exposed to multiple substantive disciplines but only Positivist epistemologies.

I was Schneider's student at the University of Chicago. In 1964, after I had turned in my draft dissertation and he had read it, he told a story. I paraphrase slightly for brevity. Three umpires were discussing balls and strikes. The youngest said, "I calls them the way I sees them." The middle umpire said, "I calls them the way they are." The senior umpire paused for a moment and then said, emphatically, "They *ain't nuthin'* till I calls them!" It was clear that he was identifying me with the young umpire and himself with the old umpire, and that he considered the old umpire to be right and the young one naive. Richard Feinberg recounts the same story (Feinberg and Ottenheimer 2001:10-11; Leaf 2001: 60-64). In fact, Schneider's position was Comte's idealistic imposition of subjectivity, but Schneider did not know it. On a couple of occasions, I tried to ask him about his philosophical readings. He was not interested.

I have described philosophical Positivism at greater length elsewhere (Leaf 1979: 13-77; Leaf 2001; Leaf and Read 2021: 13-19). The descriptions include its preservation in Schneider's turn to symbolism and convergence with Clifford Geertz's interpretivism (Geertz 1973; Schneider 1984, 1995). Geertz was another product of Harvard Social Relations. He described his position as "anti-positivism." Paul Friedrich explained why it is more accurate to describe it as "crypto-positivism" (Friedrich 1992). He did not reject the assumptions of Positivism. He perpetuated them in an alternate form.

Five important Positivistic dicta concerning science shaped the path to the kinship apocalypse. 1) That science is a unified system of laws. 2) It cannot be about ideas; it must be about material (external) things. 3) There is no order in nature; order is imposed by the mind of the scientist. 4) The meaning of a term (or statement) is the thing it refers to, or denotes, not what it connotes. 5) Scientific explanation requires determinism rather than human choice and agency.

Kinship Maps: Demonstration is Elicitation

In fact, kinship organizations are constructed by those who use them with systems of kinship ideas that are synthetic *a priori* in Kant's sense. They provide categories that enable people who use them to make their mutual relationships objective. They enable them to agree on what the relationships are and make each person's perspective consistent with every other person's perspective. Understanding how they do this provides important new insights into the social creation

of objective knowledge itself. Failure to understand this was the single most important error that led to the kinship apocalypse of the 1980s.

I will first say what these kinship ideas are. Then we can go back and trace the steps by which anthropologists dealing with kinship and social organization became increasingly enmeshed in the tangle of Positivist assumptions that obscured them.

The core kinship ideas that members of a community use to construct kinship relations and organizations are what I have described as the "kinship map" (Leaf 1971, 1972, 1984, 2006, 2009; Leaf and Read 2012, 2021). These are the ideas that kinship terms invoke. Other ideas, such as rules of inheritance, are incorporated into kinship organizations by being associated with positions or relations defined in the kinship map.

Kinship maps can be elicited by cultural frame analysis (Leaf 2006; Leaf and Read 2021: 106-115). This uses part of an ordered conceptual system as a frame to elicit the rest of the system. The label and my formalization are new but the basic technique began with Carl von Savigny's comparative method in law and has been applied many ways in linguistics. For kinship, it involves finding what are regarded as a persons' "direct" kin and diagramming it. Figure 1 is the core diagram for English.
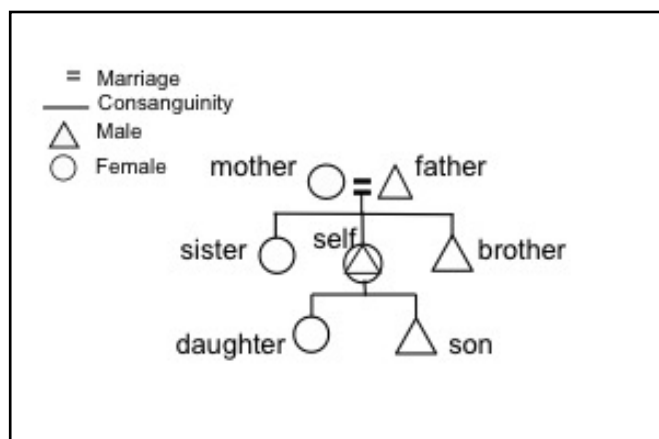

Figure 1: Core positions for American English.

To avoid ethnocentric imputations, it is important to be sure that every symbol that it uses stands for and faithfully follows the logic of an indigenous idea with indigenous names.

This is the eliciting frame for the rest of the map. It is used in steps. The first is to elicit the direct relations of these direct relations. For each direct relation, the basic eliciting question (in the indigenous language) is "what is this direct relation of this direct relation to you?" In English, for example, one would ask "what is the father of your father to you?" "What is the mother of your father to you?" And so on around. If the answer is a term already obtained, the diagram is redrawn to include its meaning. If the answer is a new term, the diagram is expanded to include its meaning. Father of father (in English) yields "grandfather." So this added. It will be queried in turn by asking "What is father of grandfather to you?", "What is mother of grandfather to you?"-- and so on (Leaf 2006). The process is repeated until it reaches a boundary. This means that respondents say there are no relations beyond it, or there is a rule that tells you how to go on forev-
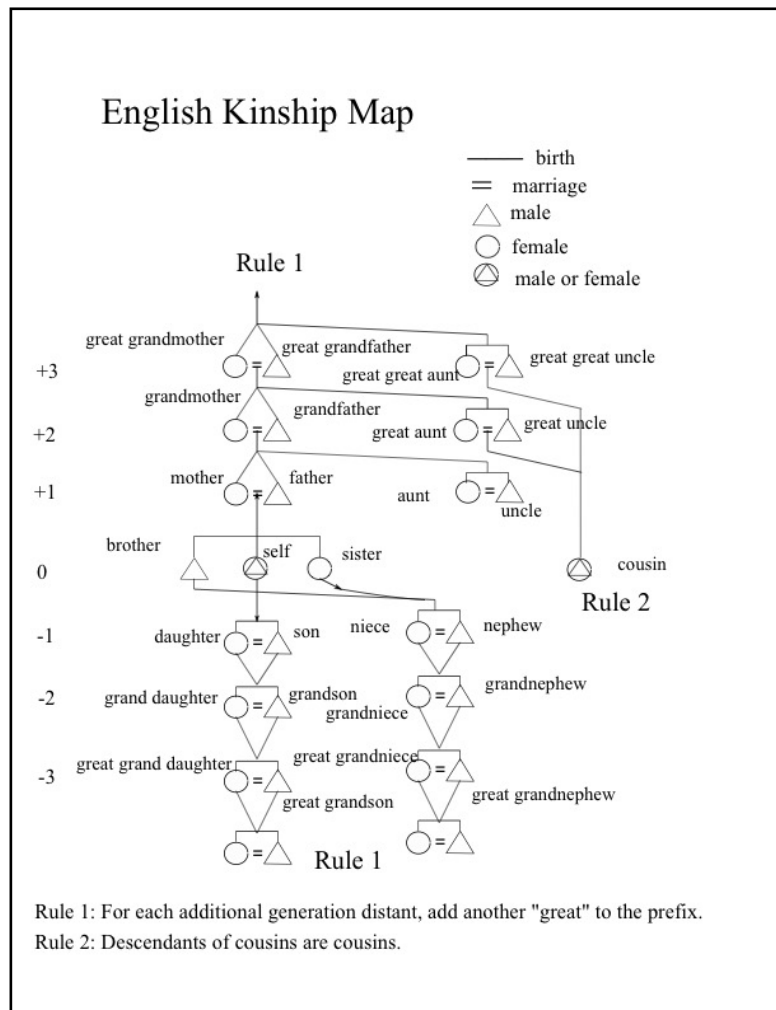
English Kinship Map

	birth
=	marriage
△	male
○	female
⬚	male or female

Rule 1

+3 great grandmother great grandfather great great aunt / great great uncle

+2 grandmother grandfather great aunt / great uncle

+1 mother father aunt / uncle

0 brother self sister cousin **Rule 2**

-1 daughter son niece nephew

-2 grand daughter grandson / grandniece grandnephew

-3 great grand daughter great grandniece / great grandson great grandnephew

Rule 1

Rule 1: For each additional generation distant, add another "great" to the prefix.
Rule 2: Descendants of cousins are cousins.

Figure 2: English Kinship Map.

er. It is not necessary to have a set or list of "kinship terms" before beginning this elicitation. The elicitation itself will produce them.

Since the elicitation process asks respondents to identify concatenated definitions as they are produced, the kinship map must be a generative system. As such, it can be further analyzed to ask exactly what the generative premises are and how they are applied. This is an analytic process rather than an elicitation, but it is still subject to empirical verification. It has been developed by Dwight Read in two versions. One uses basic algebra represented with set theory notation or a graphic form (Read 1984, 2000, 2001, 2007, 2010, 2013, 2018; Leaf and Read 2012). The other is a computer program that can be used by anthropologists without Read's mathematical background. This is the Kinship Algebra Expert System (Read and Behrens 1990; Read 2006; Leaf and Read 2021: 274-278). Either way, the generated output is called the "kin term map." If it is correct, the kin term map and kinship map will be topologically isomorphic. There will be a one for correspondence of the positions defined in the kin term map with the positions elicited in the kinship map.

The kin term map shows the generative logic that enables kinship maps to be held in the kind of universal, self-perpetuating consensus that anthropologists have always observed. Since the generative patterns fall into just three major groups, it appears to have important implications for our understanding of how such idea systems have evolved historically (Read 2016, 2018; Leaf and Read 2021: 282).

Kinship maps show how the meanings of kinship terms are interrelated. To draw them, we need symbols for positions and for relations. Symbols for positions indicate male, female, or either. Symbols for relations usually indicate descent and marriage, but there are exceptions and variations. The basic convention for drawing the map is that each position should have a single name, so the indigenous meaning of that name can be read by out from the meanings assigned to the relational symbols and its connections to the other positions on the chart. All positions are arrayed around a "self" position. Every term has at least one reciprocal. Charts may require imaging in three dimensions, not just two. All of this and more is what the Positivist epistemologies of Schneider and those he criticized prevented them from recognizing. Yet all of this and more is exactly what gives the kinship map and the ideas associated with it the power it has to enable its users to place themselves conceptually within organizations that are objective, stable, useful, intellectually compelling and self-perpetuating.

Figure 2 is the full elicited kinship map for American English. For indigenous English speakers, it should be easy to understand how to read it. English speakers invoke this imagery when they speak of their ancestry "going back to Adam," of "direct descendants," of "lineal relations," when they consider all ancestors and descendants to be kin, and when they speak of lineal versus collateral relations. It is also what Anthropologists invoke in their usual definitions of lineage, clan, phratry, and tribe.

Rule 1 in the diagram is an example of a boundary. It marks both the upper and the lower boundaries. It is a rule for etcetera, which means continuing with the information you already have rather than requiring any new information. The rule is "Add another 'great' for each additional generation." Rule 2 is the etcetera rule for cousin. It is that children of cousins are cousins. The other boundary condition is that there is no relation defined beyond uncle, aunt, or cousin.

The arrows in the diagram mean that computations can be made in the direction indicated but not in the reverse direction. So, for example, a great aunt is always a sister of a great grandmother or the wife of a great uncle, but the sister of a great aunt is not always a great grandmother.

Notice that the visual pattern repeats the patterns of linguistic stems, prefixes and suffixes (none, grand-, great-, great-grand-, and so on). The visual grouping corresponds to the phonological and syntactic frames. This is apparently a universal feature of such maps.

Every kinship map implies its own sense of what kinship in general is. These differ subtly but pervasively between systems. This kinship map associates the idea of kinship with descent. Many others do not. So descent is a fact of some kinship idea-systems. It is not a fact of nature. But Lewis Henry Morgan took it for a fact of nature. So did those who followed him. This, precisely, was the starting point for the subsequent series of developments leading to the kinship apocalypse of the 1980s.

Kinship maps implicitly demonstrate the necessity for cultural pluralism. The vocabulary of kinship has to be different from vocabularies of other kinds of social relations and organiza-
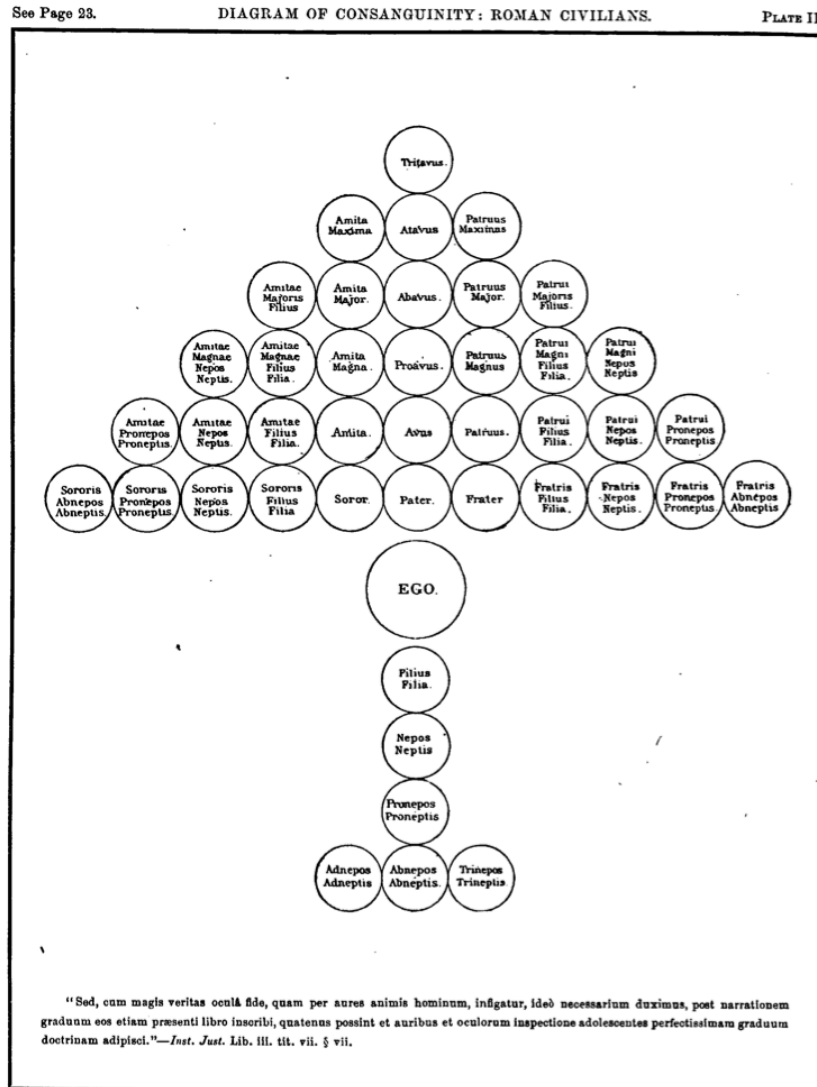
tions because the ideas have to be kept separate. If they are not separate, they cannot be bounded.

Figure 3. Morgan's Diagram of Roman Consanguinity.

If they are not bounded, they cannot be coherent (Leaf and Read 2021: 93).

So how did anthropologists come to think kinship terms had to be defined by some objective thing they referred to and why did they think total system models were a good thing? We can trace the steps.

From Morgan to Schneider

Robert Trautmann credits Morgan with "the discovery of kinship." It is more correct to say that Morgan first called attention to the central importance of kinship terminologies for investigating it.

Figure 4. Roman Civilian version of Figure 3.

Morgan's epistemological assumptions are demonstrative. He was a lawyer participating in a debate on the origins of law that flowed directly from Kant's analysis of the ways law arises from community consensus. On these assumptions, Carl von Savigny, Professor of Law at the University of Berlin, had demonstrated that there had been a continuous development of Roman law in Europe from the collapse of the Roman Empire to the "reception" of the 15th century (Savigny 1829; Leaf 1979: 83-85). Bachofen, Main, Morgan, and McLennan then took up the question this posed. What came before Roman Law (Leaf 1979: 106-116)? Roman legal theory had said that it was founded in the *Patria Potestas*, the power of the father. This focused attention on rules of inheritance. Morgan knew that Western rules of inheritance require separating out every descending collateral line and that this went back to Rome. He was working with the Seneca, avocationally and as a lawyer. He discovered that Seneca terminology militated against this. He connected this to the importance of clans and the fact that clan membership was ob-

DIAGRAM OF CONSANGUINITY: ENGLISH.   PLATE III.
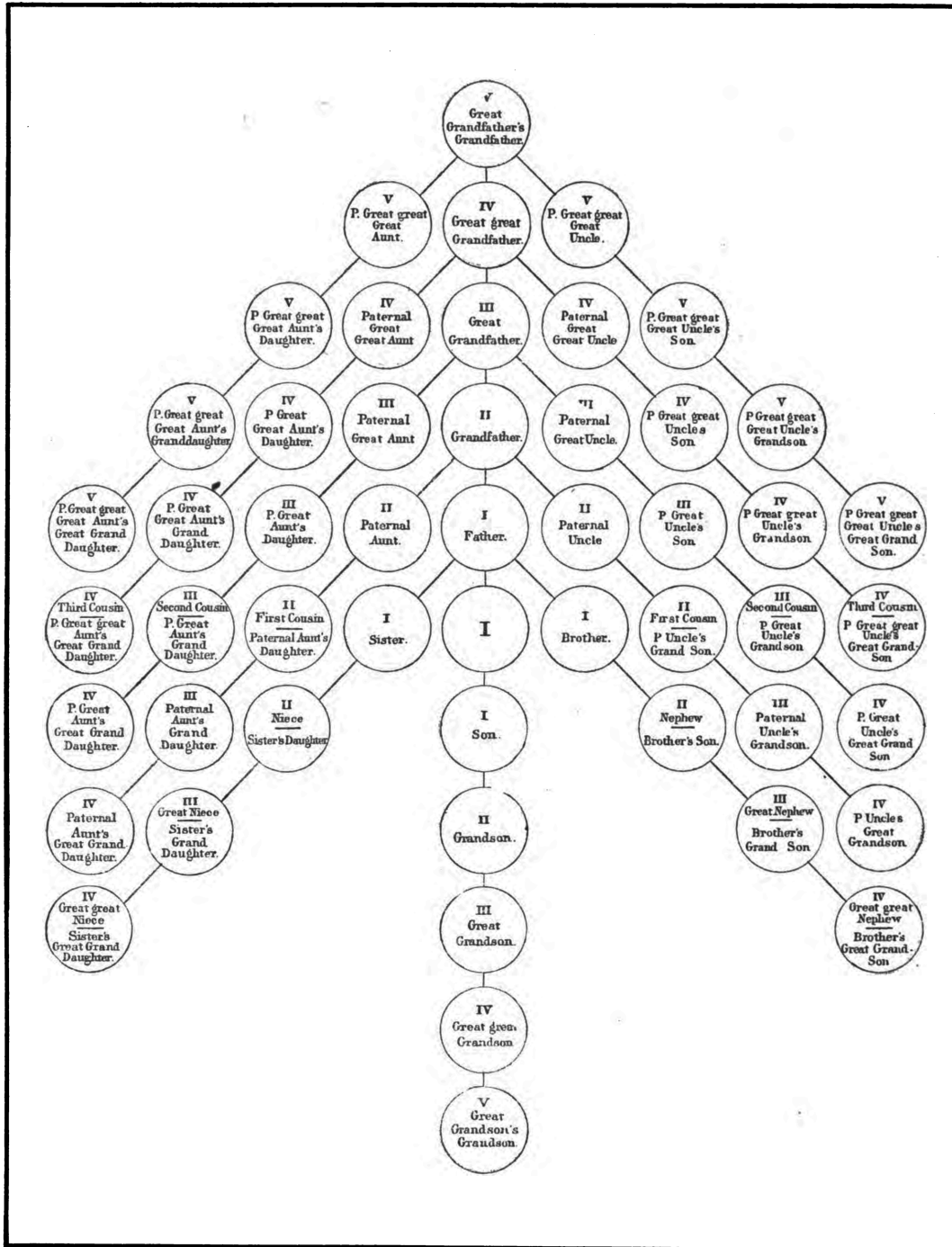

Figure 5. English consanguinity. Female left, male on right.

tained matrilineally. He assumed that the Seneca represented what could have existed before Rome. He also assumed that relatives who were grouped together must have the same relative social roles, if not observably then at least when that family system originally took shape. So the way to decisively answer the legal question was to survey terminologies world-wide, in societies

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DIAGRAM OF CONSANGUINITY: SENECA–IROQUOIS.   PLATE VII.
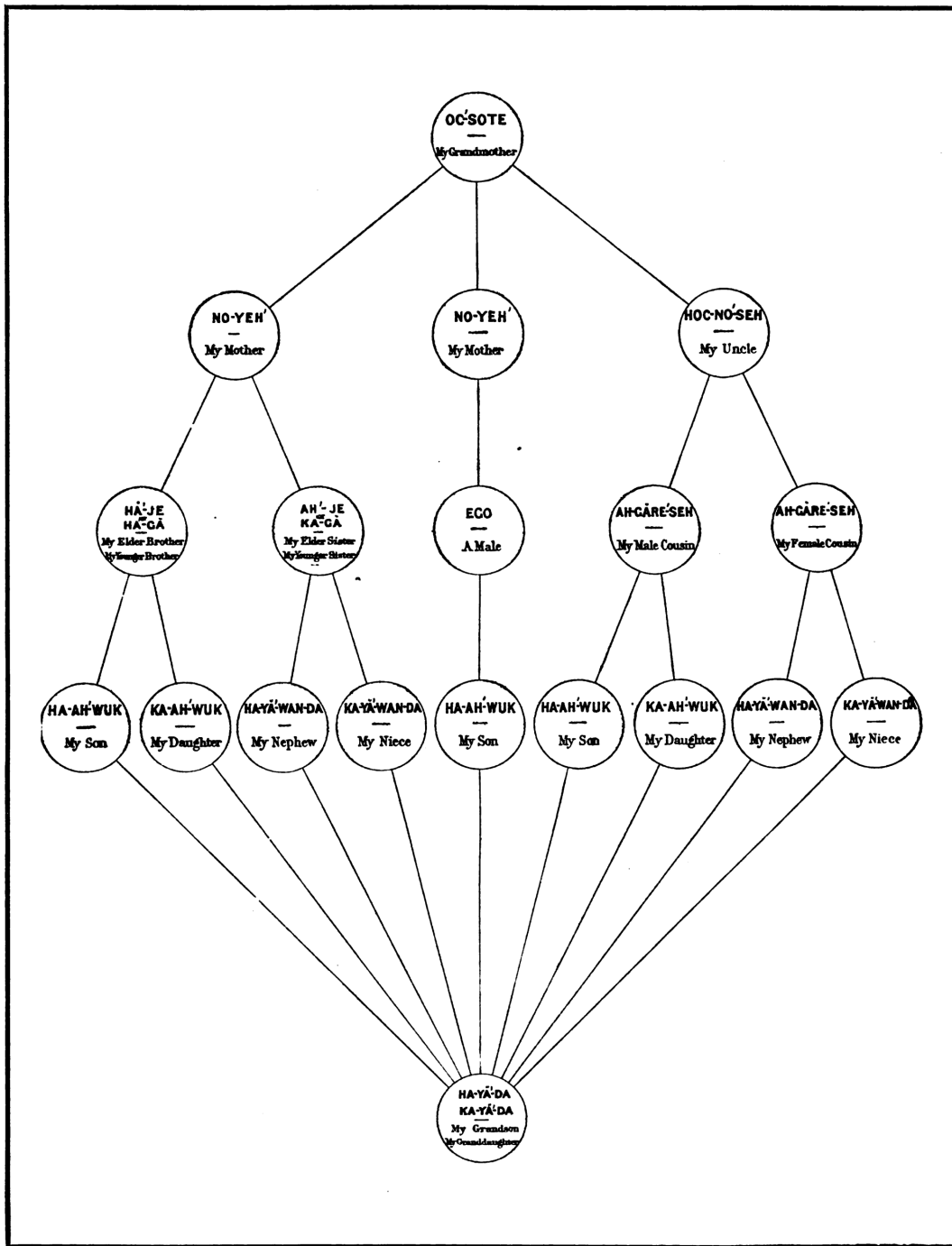
Lineal, and Second Collateral Lines: Male and Female: Mother's Side.
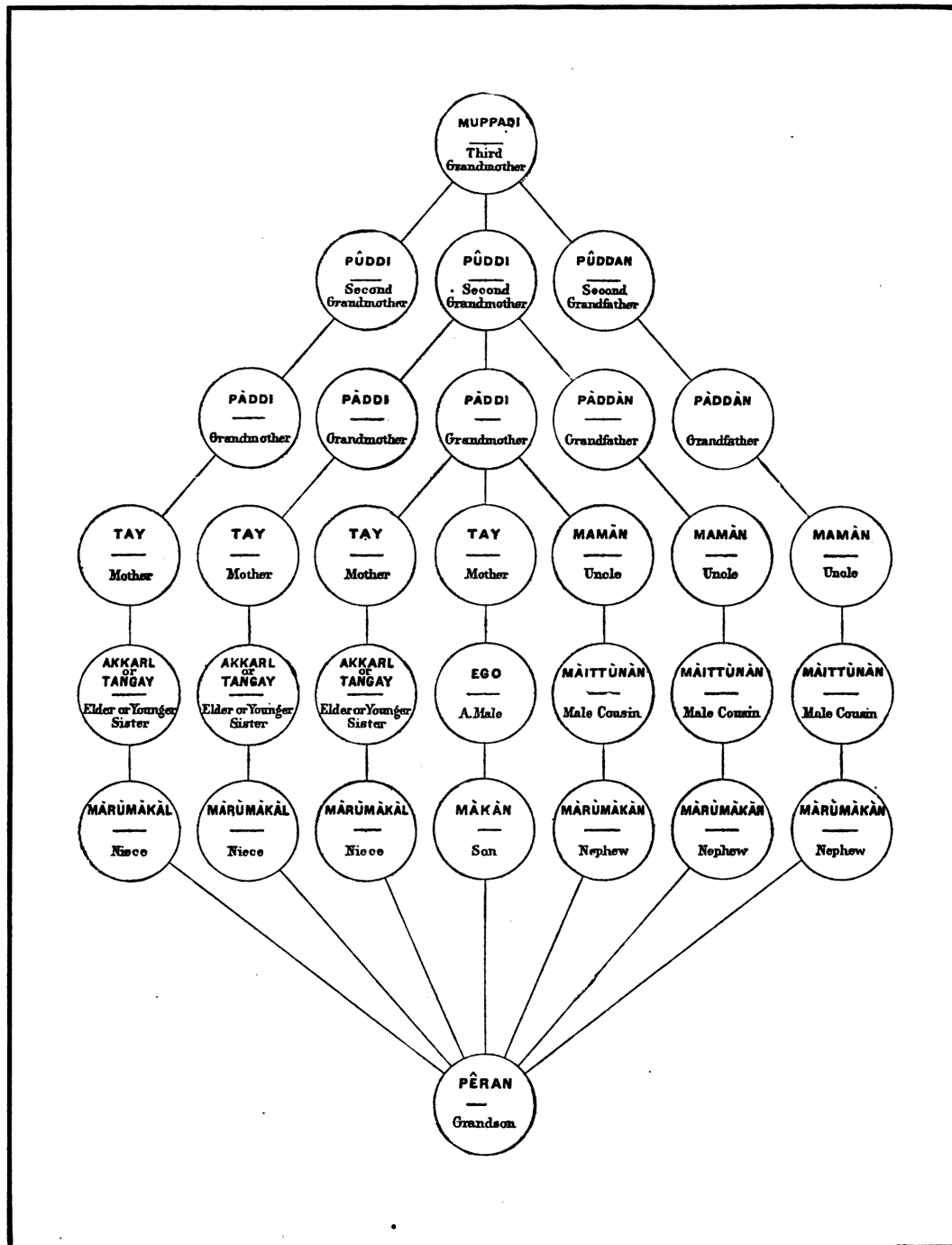*Ego*, a Male.

Figure 6. Seneca lineal to second collaterals male speaking mother's side

of all levels of development. There is no reliance on idealist vs materialist epistemology in this argument.

DIAGRAM OF CONSANGUINITY: TAMIL.   PLATE XIV.

Lineal, and Second, Third, and Fourth Collateral Lines: Male and Female.   Mother's Side.
Ego, a Male.

Figure 7. Tamill lineal to fourth collaterals male speaking mother's side.

Morgan's Diagrams

Morgan's *Systems of Consanguinity and Affinity of the Human Family* was published in 1871. This divided the evolution of the family into two main stages: group marriage and "monogami-

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an" marriage. The division turned on his distinction between terminologies that are "classificatory" and those that are "descriptive." Classificatory terminologies are associated with group marriage; descriptive terminologies go with monogamian marriage. His argument reconstructs the progressive differentiation of relationships in group marriage leading to monogamian marriage.

Subsequent anthropological discussions of Morgan's work have focused on his evolutionary claims and his terminological glosses, given in many tables throughout the work. But they are the outputs. The inputs are indicated by fourteen plates included at the end. The first three exemplify the descriptive system: Roman, Roman Civilian, and English. Figures 3 and 4 are two versions of the Roman system. English is Figure 5. The resemblance to the English kinship map in Figure 2 should be obvious. His two key examples of classificatory systems, Tamil and Seneca are given in Figures 6 and 7. The Tamil and Seneca diagrams differ according to male or female speaker and mother's or father's side. The gender differences do not affect their basic similarities in shape that are relevant here. These are male versions.

The chart is pyramidal because one side represents males and the other females. Another common form, still more obviously reflecting the kinship map of Figure 2, is a half-pyramid, in which male and female names for position are provided together.

Figure 4 is Morgan's Plate II. A "Civilian" is a Roman Civil Law theorist. Morgan credits this particular drawing to the Common Law jurist Blackstone. It has the same function as Figure 3, but with a few different terms.

Figure 5 is Morgan's Plate III, representing English in the same way as Figure 3, with female relations on the left, male on the right

All the figures have a central descent line with ego at the mid-point. The differences are that in the descriptive group collateral lines diverge from each node in the central lineage and remain separate. In the classificatory group the lines diverge from the central line above ego and converge at the bottom. This was Morgan's driving discovery and the basis of his theory of the evolution of the family from group marriage to monogamy. In the first, groups of brothers married groups of sisters, so there is no distinction between lineals and collaterals. In the "monogamian" system, all lineal and collateral lines can be distinguished.

The English and Roman diagrams are legal models. They measure the "degrees of consanguinity"-- "common blood"-- that in European law determines eligibility to inherit and to marry. The degrees are quantitative. The quanta are given by the Roman numerals. He explains in the text why there are no numbers in the "civilian" diagram: degrees can be determined from the arrangement of rows (p. 23).

Figures 6 and 7 are what Morgan got when he diagrammed Tamil and Seneca in the same way. His successive diagrams expand to include progressively more collateral lines, from one to four. His Tamil and Seneca diagrams showing the same numbers of collateral lines have the same shapes, so Morgan considered them identical. The two plates reproduced show Morgan's lineal to second collateral for Seneca and lineal to fourth collateral for Tamil. Both contain positions with different degrees of consanguinity that have the same names. This is why he considered them "classificatory." As Trautmann says:

> His project, then, becomes the study of the way in which relationships existing in nature are, as he would say, "classified" (merged under the same name or vocable) or "described" (not merged). (1984: 95; parenthetical remarks are Trautmann's.)

Read and I have compared the Tamil and Seneca kinship maps in detail (Leaf and Read 2021: 223-238). They are actually not identical, although Morgan was right in seeing some common features. One similarity is that there is one map for a male speaker and another for a female speaker. However, since the difference between the male and female versions is subtle, the Tamil kinship map for male speaker will suffice to show the resemblance to Morgan's diagrams here. This is Figure 8.

In the kinship map, unlike Morgan's diagram, each name applies to one and only one position. So the links of each position to all other positions represents its definition in terms of the
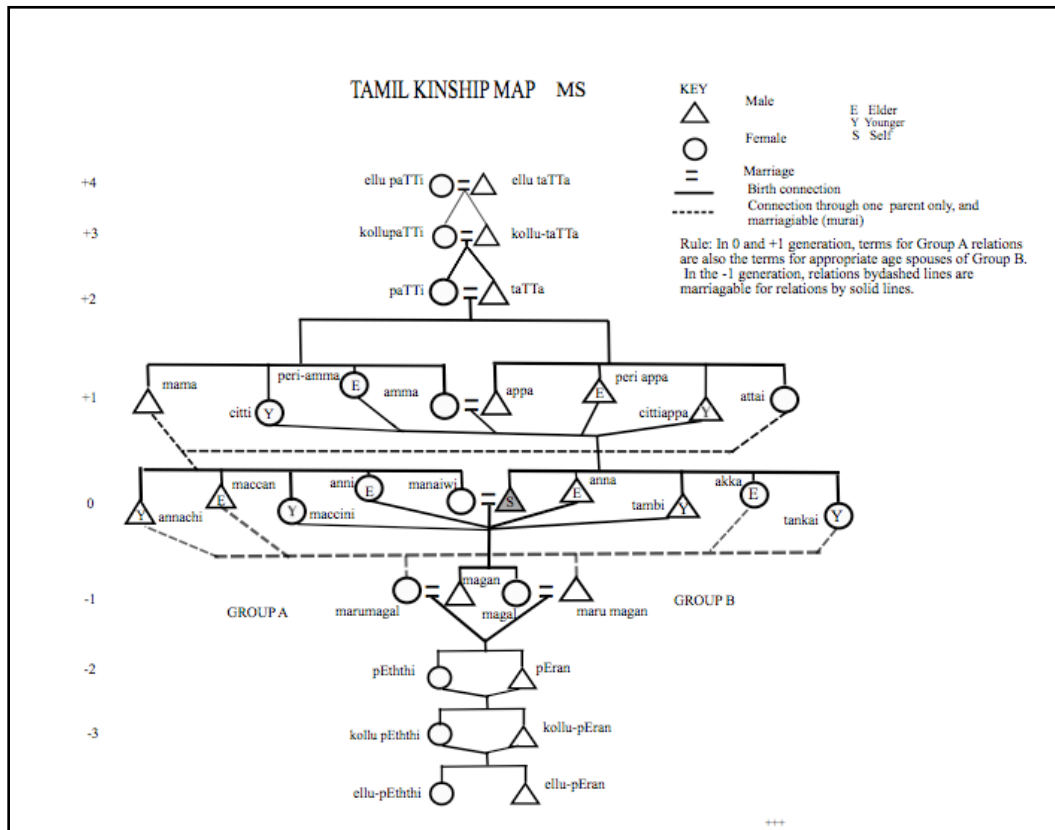

Figure 8. Tamil kinship map, male speaker.

other positions. Morgan could have asked himself if he could draw his diagrams to meet this requirement. Had he done so, the history of anthropology would be very different.

Consanguinity to Genealogy

Morgan's data was collected with a questionnaire. He developed it from discussions first with his Iroquois collocutors and then representatives of other native North American groups. Morgan used it himself in four trips to reservations in the upper midwest from 1859 to 1872 (White 1951; Trautmann 1987: 115). In 1860, the Smithsonian Institution sent the third version to American and other Anglophone officials, missionaries, and professionals around the world.

The forms had three columns. The column headings are the "description of the relation" using English terms, the indigenous term, and its English "translation." There were 218 descriptions of relations (Trautmann 1987: 99; Gardner 2016: 4, 10). They are divided into groups. Trautmann describes them but does not note their connection to the diagrams of consanguinity. It is that they are organized the same way, top to bottom. For example, in the "Table Exhibiting the System of Consanguinity of the Seneca and Dakota" (Morgan 1871: 167) he has five groups. From the top, they are "Lineal Line," then each "Collateral Line" from first (Ego's own generation, starting with "brother's son") to fourth (starting with "my father's father's father's brother"), and finally "Marriage Relatives." The lineal definitions are arranged from top to bottom: great grand-father or father of great grandfather down to great grandson or son of great grandson, with Ego at the mid-point. So the consanguinity diagrams provide the theory of the questionnaires.

Why 218? Plate III has only 41 positions as drawn, but it is easy to see how it was expanded. Morgan doubled it by asking for all lineals below the Ego position going down to the great grandson, with their branching collaterals. He added more collaterals by recognizing, presumably from his experience with the Seneca, that descendants from elder siblings can be separated from descendants from younger siblings and that terms may differ with sex of speaker. He also included "step" relations among the relations by marriage.

Morgan does not describe any difficulty using the form himself but often reports difficulty getting good information from others. These difficulties provoked the second step toward the kinship apocalypse: the development of "the genealogical method." Recent work by Helen Gardner shows how it happened. She starts with Rivers:

> It is a commonplace of anthropological history that British psychologist W. H. R. Rivers developed the genealogical method for the collection of kinship data during the Torres Strait Expedition of 1898. (2016: 294)

But the process actually began sooner and had much more to with Morgan's questionnaire. Rivers published two articles with "genealogical method" in the title. The first, in 1900, describes it only as a system for personal identification. His only mention of indigenous kin terms is to caution the reader against allowing their informants to use them. The first part of the 1910 article repeats the 1900 article. Rivers then adds a section saying that the genealogies should also be used to collect "terms of relationship." He gives a list. It is sixty-one out of Morgan's original 218, arranged in reciprocal pairs. But he does not mention Morgan's survey form. Instead, he recommends using several "pedigrees" to obtain a complete list and allow for cross-checking. Further questions concern how the individual terms are used. There is no sense of trying to find if the terms form a complete and coherent system and if collected as he proposes they never could since they are limited to only two generations above and below ego. He concludes the section:

> The terms given in the list are sufficient to determine the general character of a system, but it will be well to obtain a certain number of terms for more distant relationships such as the father's father's brother and sister together with their children and grandchildren. Among these more distant relationship the sister's son's wife and sister's daughter's husband and their children are sometimes of special interest. (1910: 6)

He concludes the article by describing the method as providing verifiable data of great consistency and precision with "no knowledge of the language and with very inferior interpreters" (1910: 10).

The 1910 article was reproduced in Kinship and Social Organization, published in 1913. It was somewhat revised to supply two sections in the Royal Anthropological Institute's *Notes and Queries in Anthropology*. The 6th edition is still current. The list of relations is the same as in 1910.

Gardner shows that the of method of using "pedigrees" that Rivers described was developed in 1873 by A. W. Howitt and Tulaba, "a man of the Bruthen clan of the Brabralung division of the Gunnai/Kŭrnai people and a leader of those who escaped the massacres of Gippsland Aborigines in the previous 20 years" (2016: 295).

Howitt was a local magistrate in Gippsland, Victoria. Tulaba was his employee and friend. Lorimer Fison was an Australian Methodist missionary who had received a copy of Morgan's questionnaire while living and working on Fiji (Gardner 2016: 296). When he returned to Australia in 1871, a local settler suggested that the indigenous kinship system of people around Northern Queensland was similar. Fison then printed a shortened version of Morgan's schedule to circulate locally. Howitt had received Fison's questionnaire and Tulaba was trying to fill it out for him. Howitt reported the results back to Fison. Gardner quotes from Fison's report:

> He found it impossible to make any headway in my schedule. Toolabar soon grew hopelessly bewildered, utterly failing to take in the idea conveyed by a term such as 'my father's, father's, sister's, son's daughter'. But Mr. Howitt, after getting what terms that he could did not abandon the attempt in despair after the manner of but too many of my correspondents. He hit upon a simple, yet ingenious plan which produced admirable results. On the floor he constructed a sort of family tree representing the members of his own family, with which Toolabar was well acquainted. Each individual was represented by a piece of stick, and Toolabar gave the words by which one stick would address another. The results given here in the accompanying memoranda which I have made are … families (no fewer than four) whose diagrams have been ascertained by means of Toolabar's sticks.' (2016: 297).

Tulaba's answers were then used to fill in Fison's version of Morgan's questionnaire. Gardner reproduces a completed version. It carries two official stamps. One is dated Au 25 but the year in unreadable. The other is "Melbourne Ja 1, 77," evidently January 1877. If we consider Morgan drawing his Seneca and Tamil diagrams to be the first near-recognition of kinship maps, this was another. Gardner connects the investigators' failure to describe the full process of elicitation to their unwillingness to share intellectual credit with their indigenous informants.

Gardner also describes the way Fison and Howitt involved E. B. Tylor in their correspondence. Tylor in turn connected them to Baldwin Spencer and Frank Gillen as they began their own researches. So the "genealogical method" attributed to Rivers was well established and widely circulated as a way to obtain the kinship terms sought by Morgan's questionnaire decades before Rivers described it. Even more importantly, it was much more responsive to the problem of finding indigenous kinship ideas than Morgan's list:

> But Rivers continued to suggest the collector work with as few terms as possible and to seek the 'real' father despite recognising (sic.) that it was probable that 'father' might be applicable to a large number of people (1910: 119–20). It was essentially a different task from that envisaged by Morgan and developed further by Tulaba, Fison and Howitt.

(2016: 316; internal reference is Gardner's.)

People answer questions by attributing meaning to them. If you hear one of Morgan's more complicated "definitions of persons" without something like an English kinship map in mind, there is no way to follow the links to the position it is asking the name for. But if you attribute the links to your own relatives, and thereby invoke your own kinship map, the question becomes intelligible. It can be answered. So the answer can be recorded. But it cannot be explained. To explain it, you would have to relate it to the other answers you are providing. Gardner recognizes that Fison, Tulaba, and Howitt were making these connections in their own discussions. But they were lost once the answer was transferred to the questionnaire.

More Steps

No community has 218 separate terms for Morgan's 218 separate definitions. Kinship maps usually have between twenty and thirty-five discrete positions. So if one of Morgan's returned questionnaires was rewritten with each indigenous term appearing just once, several of Morgan's definitions of types persons would appear against that term as a group of positions or relations it designated. Morgan recognized this in his text. It was what he focused on to reconstruct his supposed different family organizations.

The next step was to simplify comparing such groupings. This was done by Josef Köhler, in *Zur Urgeschichte der Ehe; Totemismus, Gruppenehe, Mutterrecht,* published in 1887. It was translated into English by J. Barnes in 1979 as *The Prehistory of Marriage.* Köhler made two major contributions to the eventual kinship apocalypse. One was that his notation was incorporated into the idea of a kintype. The other was that his interpretation of Australian kinship was the prototype total system analysis for Alliance Theory. Köhler dedicated his book to Tylor.

Köhler was a prominent legal polemicist. He began as a self-described Hegelian and later decided that he was a Positivist. Comte had called for a world-wide Positivist dictatorship centered in France. Köhler's argument was that it should be centered in Germany. As Comte grounded his argument in a portrait of animism as the first religion, Köhler's claims were grounded in a portrait of clan totemism. Everything was in the beginning only one thing: the totemic clan in a system of group marriage as Morgan had described. Hegel had argued that we are products of the State forcing itself in history--so free will and democracy are delusions. Köhler argued that Australian totemism is the original form.

Durkheim reviewed the book with high praise twice in his *Année Sociologique.* The order of argument in his own *Elementary Forms of the Religious Life* (1912) closely follows Köhler's. But Durkheim's paucity of citations to Köhler would easily lead a reader to miss the connection (Leaf and Read 2021: 36).

Köhler's argument was a thicket of *non-sequiturs*. One of these was imbedded in the notation. He wrote the alternative definitions of positions as sets of equations. For example, if the same term is applied to ego's father and father's brother, Köhler wrote this as father = father's brother. If the same term applied to the brother of wife's sister and to sister's husband, he wrote this as brother of wife's sister = sister's husband. The equations are also illustrated by diagrams. They are drawn on the model of Morgan's diagrams of consanguinity, with a central descent line to ego and other lines branching off and merging in various patterns.

Köhler's notation implies that all things equated are equal. This encourages an interpretive error that Morgan had avoided. Morgan recognized, for example, that members of the communities who used the same terms for father and father's brother nevertheless distinguished between a person's actual father and others so designated. He also recognized that contemporary groups with terminologies he associated with group marriage did not actually have group marriage. Köhler selectively obscured this.

Köhler applied his convention to all terminologies of all groups. He did not say they all had group marriage now, but he insisted that it had been universal in the past, that the Australian clan system was the first form of it, and that the Australians still had it--even though his Australian sources had explicitly asked their informants about group marriage in response to Morgan's original assertion and they consistently denied it. Their most common response was that a man cannot get a wife without giving up a sister in exchange. Köhler quoted this and rejected it. His reason was simply that Morgan was right.

Spencer and Gillen published *Native Tribes of Central Australia* in 1899. Its treatment of kinship reflects the perspective of Fison and Howitt, not Morgan or Köhler. They explain kin terms by their uses in their organizational context. Chapter II is titled "The Social Organization of the Tribes." The first sentence is "the fundamental feature in the organization of the central Australian, as in that of other Australian tribes, is the division of the tribe into two exogamous intermarrying groups." (1899: 55). The chapter goes on to describe how the cross-cutting division of these two divisions by generation creates what anthropologists recognize as a section system. They describe eight tribes, one tribe with a two-section system and seven with a four-section system. They recognize there are others with more than four sections but say they can all be recognized as variations on the same principles.

Their description of the section system is followed by a "table of relationship terms" specifically for the Urabunna tribe. This is the two-section system. It gives the terms listwise but the list is organized by native terms. These make up the left-hand column. The central column gives the "actual relationship expressed in English terms" for each term in the left column. The third column heading is "English terms, included wholly or partly in the native terms." This extends the description of the central column in order to be very clear about the relationship between the native ideas and any English ideas used. So, this is absolutely not based on Morgan's purposes. Its purpose is to explain the indigenous terms by explaining the indigenous ideas they invoke. A following table for the remaining tribes is organized the same way. They also include genealogical diagrams in Fison and Howitt's sense to indicate how actual people would categorize themselves.

A. R. Radcliffe-Brown's description of Kariera terminology in *Three Tribes of Western Australia*, 1913, goes still farther. His descriptions are diagrams that combine the functions of Spencer and Gillen's tables of kinship terms and diagrams of section relations. He called them "genealogical tables" but they are not genealogies in Rivers' sense. He also could not have used Rivers' genealogical lists to elicit them. They showed precisely the kind of clear and learnable spatial imagery that list-wise elicitation obscured. Figure 9 is a copy of his Table 1, representing the Kariera system, male speaking (Radcliffe-Brown 1913: 152). His table for a female speaker is a mirror image. So, the two make a single logically coherent system in the sense that any man
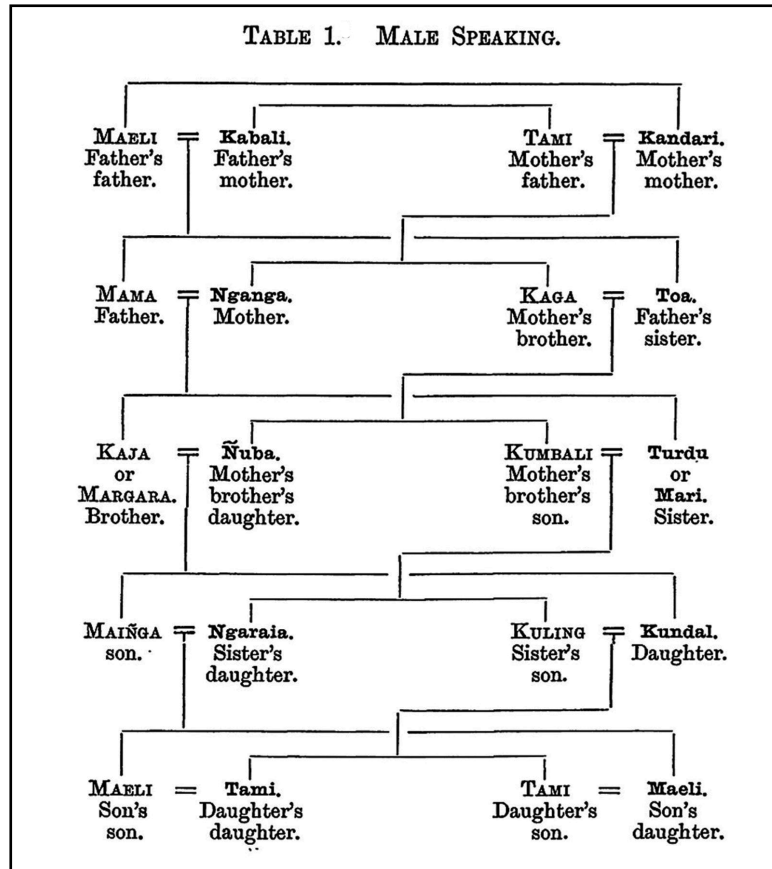
Figure 9. Radcliffe-Brown's Table 1, Kariera MS.

can compute the relation of any person to himself from that person's relation to his sister and vice versa.

The Kariera kinship terminology is clearly integrated with the Kariera section system. In the diagram, each column represents a patrilineal lineage. Each lineage can be divided into two sections, each represented by alternate generations: even generations (including the 0 generation) would be a section and odd generations would be a section.

Apparently, he constructed it in conversation with local informants in a manner like tracing kinship maps. The main difference is that to elicit a kinship map the analyst works outward from an explicit "self" position. These diagrams do not have such a position, although one is implied. It is in the middle row. For the man-speaking kinship map, the male self is between the kaja/margara ('ascending/descending brother'). For the female-speaking kinship map, the female self between the corresponding ascending/descending sister terms. The orientation toward finding the systematic relations between indigenous ideas in Australian kinship still continues.

Similar shifts toward more of a focus on indigenous ideas were being made in the United States. In 1911, the Bureau of American Ethnology published *The Omaha Tribe, Two Volumes*, by Alice Fletcher and Francis La Flesche. Fletcher was ethnically American born in Cuba. La Flesche was Omaha. This was another historically important intellectual partnership between an

anthropologist and a native "informant" who shaped the questions that were asked as well as the answers they received. Chapter 8 of 16 is titled "Social Life." The first section is "Kinship Terms." Beginning on page 313, this has a three-page table titled "Omaha terms of relationship," arranged in four columns. The headings are: 1) English term, 2) Spoken of by one who is not related, 3) Spoken of by relatives of various kinds, 4) Spoken to by relatives of various kinds. The terms listed for "spoken of" by various relatives are definitely different from those listed as "spoken to" by those relatives. So, it is an inherent distinction for the Omaha, not just a theoretical attribution. There are 36 rows. Each row is devoted to a unique Omaha term. The first 28 are consanguines, except for three of the English glosses the table explicitly says there are no Omaha terms for. These are, "uncle (Father's brother)," "uncle by marriage," and "uncle who is direct descendant of mother's brother." The only "uncle" for which there is an Omaha term is mother's brother. These specifications address and exclude the possibility that it is the same as Tamil, as Morgan had held Seneca to be. Relations 29-36 are by marriage.

Over the next two decades, this use of English glosses became normal. Concurrently, Morgan's division between descriptive and classificatory systems was redefined to drop the legal ideas of degrees of closeness and his evolutionary claims were restated as about historic change. One notable example was Elsie Clews Parsons' synthesis of studies of kinship terminologies of Pueblo communities in Arizona and New Mexico in relation to the possible past existence of moiety systems. Another was Fred Eggan's synthesis of studies of kinship systems of the Northeast and Midwest concerned with the difference between "Omaha systems" and "Crow systems." While both are classificatory in Morgan's sense, in the Omaha system more relations in one's father's group are distinguished by generation than on one's mother's side. In the Crow system, the contrast is reversed.

E. C. Parsons' 1932 study drew on field work going back to 1914, by herself and five others including Kroeber (1917) and Lowie (1929). Her table has thirteen columns. The first has no heading but if there were one it would be something like "English approximation, subject to qualification." The other column heads are pueblos, grouped by language.

There are 45 rows. The first 33 are arranged by the indigenous terms the English gloss translates, so each row shows the parallel terms across all the communities. The rows are also arranged in reciprocal pairs. So, for example, the first row has "Mother" and beneath it, still in the same row, "Mother's sister." The reason is clear from looking at the information provided. In most of the pueblos, both glosses represent the same term. But in four of them, mother and mother's sister have different indigenous terms. So one can look across the columns and see whether this varies by language (it does not). The second row is the reciprocal for the term in the row above it. The third row is an apparent exception: "Stepmother." But again, the explanation is clear from reading across the columns. All but one have no term at all, or the term is not unique. In the one case where there is a distinct term, the entry that spells it out also gives the reciprocal.

The 34th row is "Parallel Cousins." None of these terms are unique either. The entry for each column says what previously given terms are used. The other ten terms are for cross-cousins. These again are not unique; the entries say which already stated terms they are classified with. The purpose is to respond to contemporary interest in the possible relation between terminologies and marriage rules, which she discusses in the text. The 43rd row is "Other affinity terms" and only has one entry, for Isleta. This is distinctive, and the entry includes a further spec-

ification of the gloss: parent in law/child in law. The last two rows are terms for wife and husband.

Lowie's 1929 article on Hopi terms that Parsons cited had been built around considering whether sexual teasing between a man and a group of female relatives that included his mother's brother's daughters reflected a past practice of mother's brother's daughter marriage, even though the Hopi denied this (1929: 365). Lowie had concluded it did not.

Parsons returns to this question and others like it. Most discussions are inconclusive. What is important here is not what was concluded but what was considered: connections between the terminologies and differences in kinship organizations, systems of marriage and pueblo government, which in turn were connected to the differing levels of Spanish acculturation. This is seeing kinship as depending on ideas.

Eggan's study (1937) compared changes in the Choctaw terminology from 1830 on to changes in the other major related groups who had also been in Southeast and were, with a few exceptions, also later removed to Oklahoma: Chickasaw, Creek, Cherokee, Yuchi, and Chitimacha. He also compared ideas associated with the terminologies. These included ideas of descent, inheritance, clans, the division of labor, and the organization of the household. His conclusion was that the change responded to the Choctaw adoption of white American political and educational institutions. His argument was that all of these originally had a Crow type terminology and the extent to which they change to the Omaha type terminology corresponded directly to their degree of "acculturation" to American society. The Choctaw were the most acculturated and made the most complete change. So again the focus on what the terms mean to those who use them.

This effort to understand indigenous kinship terminologies gave the word "genealogy" two very different meanings that were not clearly distinguished from one another. The first was Rivers' personal "pedigree" defined by English core terms. The second was kinship relationships as indigenously conceived, using English relational terms to convey what this was. The ambiguity presents two important empirical problems. The first is that there really is no minimal sense of the English relational terms that is culture free any more than there is a minimal sense of anyone else's core relations that is culture free. The second is that there is no known kinship term whose indigenous definition is just a string of genealogical relations. So either way, the idea that you can build up a sense of the meaning of an indigenous kinship term by concatenating genealogical relations is empirically unwarranted. What happened next on the path to the apocalypse was that G. P. Murdock ignored the problem by redefining the genealogical method in yet another way entirely.

George Peter Murdock

Murdock was a student of the Social Darwinist A. G. Keller at Yale. He identified with the Positivist streams that Keller advocated and tried to develop a comparative study of cultures using Mill's epistemology and a statistical version of Tylor's method of "adhesions." His principal work was *Social Structure* (1949). The Preface describes the project as beginning with creating the Cross-Cultural Survey in 1937. He says he began "the present study" in 1941, by deciding that he needed data on: 'kinship terminology, on sex and marriage, and on familial, kin, and local

groups" (1949: ix). The Survey at that point only yielded 85 societies with enough information. So, he combed the available literature for more. This yielded a total of 250.

Murdock ordered his description as a hierarchy of theorems and postulates. From these he would deduce specific propositions. It is model Positivistic science, "one of the very few examples in anthropology of formulating and testing a complex deductive system" (Whiting et al. 1988: 23). His method was to treat societies as units and ask which traits were associated with which other traits in them. He was not concerned with whether the traits were recognized indigenously.

If Murdock were really laying out a science, these theorems and postulates should have had the properties that Kant described for judgments that are synthetic *a priori*. To be synthetic they should be substantive. To be *a priori* they also should be true by definition. In the experimental sciences, statements acquire the character of being true by definition when their definitions are logically consistent with other relevant descriptions. This enables each statement to be inferred by implication from the others related to it. So, these other relevant descriptions should have descriptive precision and operational clarity. Murdock's argument does not recognize these strictures.

Chapter 5 was "Analysis of Kinship." Chapter 6 was "Determinants of Kinship Terminology." Two important points can illustrate the lack of empirical and logical precision. First, citing an earlier discussion by Kroeber, he redefined Morgan's distinction between descriptive and classificatory terminologies. He argued that what is descriptive or classificatory is not the terminology as a whole but a specific term within it. Any given terminology could have some terms that were descriptive and some that were classificatory. The argument depends on some linguistic fudging. First, he slips in Mill's term "denotation" as though it meant the same as "descriptive." Then he contrasts denotation with classificatory in a way that changes the meaning of "classificatory." For Mill, denotation was the thing a term referred to as opposed to its "connotation." Connotation was its emotional or subjective significance for the speaker. Only denotations could be the objects of scientific statements. Murdock makes the contrast instead between denotative and classificatory. He says that a denotative term "applies only to relatives in a single kinship category as defined by generation, sex, and genealogical connection" 1949: 99). A classificatory term "applies to persons of two or more kinship categories, as these are defined by generation, sex, and genealogical connection" (1949:99). So in English, "father" is denotative and "cousin" is classificatory. This is not what Morgan was pointing to.

But Murdock is focused on reducing meanings to biology. Continuing, he restates the parameters:

> The pioneer researches of Kroeber and Lowie have led to the recognition of six major criteria which, when linguistically recognized as a basis of terminological differentiation, yield denotative terms but the ignoring of any one of which produces classificatory terms. These criteria are generation, sex, affinity, collaterality, bifurcation, and polarity. They are the criteria employed above in calculating the number of potential categories of primary, secondary, and tertiary relatives. (1949: 101)

Given this, he goes on to say that there are seven primary terms and just 33 secondary terms. After that the number becomes unspecifiable. For the primary terms, he created a list of abbreviations: Mo for mother, Fa for father, Br for brother, Si for sister, So for son and Da for

daughter, Wi for wife, Hu for husband with some additional qualifications including y and e for elder and younger. So, mothers brother would be MoBr. Then, using Köhler's idea of equations but replacing equal marks with commas, if the same term was used for mother's brother and wife's father, it would be written MoBr, WiFa. These are "kintypes." The genealogical method for Murdock then becomes the "method" of writing the meanings of terms as concatenations of kintypes.

Murdock also classified kinship terminologies and societies. One part of the scheme was Lowie's four-way classification into bifurcate collateral, lineal, bifurcate merging, and generational (See Leaf and Read 2021: 25). Lineal corresponds roughly to Morgan's "descriptive" and bifurcate merging corresponds to Morgan's Tamil. Another was his own six major types: Hawaiian, Eskimo, Iroquois, Crow, Omaha, and Sudanese. None of these describes any actual kinship map.

He then calculated Chi-square statistics for how often these different features appeared together in different societies. He found many associations. They were almost entirely spurious, artifacts of the way he set up the variables (see Leaf 1979: 218). The statistical analysis has not been repeated (Whiting et al. 1988: 24), but kintypes became the data of componential analysis.

Componential Analysis

Ward Goodenough was Murdock's student. As an undergraduate at Yale, his first teachers were solidly in the Kantian tradition: Malinowski and George Trager. But during World War II, he served in a unit headed by the sociologist Samuel Stauffer doing statistical surveys on the attitudes of American troops. He then returned to Yale, enrolled in the graduate program in Anthropology, did field work with Murdock on Truk Island in 1947, and completed his Ph.D. in 1949.

Goodenough published Componential Analysis and the Study of Meaning in 1956. His illustrations were from the Truk kinship terminology. The method involves writing the meanings of the terms as strings of kintypes and then comparing the strings that represent the different terms to find distinguishing contrasts. He could, thereby, construe the kinship "term" as a unit of sound and its meaning as the physical thing it "signified." Thus:

> The problem of determining what a linguistic form signifies is very well illustrated by kinship terms. In essence it is this: what do I have to know about A and B in order to say that A is B's cousin? Clearly, people have certain criteria in mind by which they make the judgment that A is or is not B's cousin. What the expression his cousin signifies is the particular set of criteria by which this judgment is made. (p. 195)

Componential analysis would show what the anthropologist "had to know" in order to determine the criteria in the minds of A and B--other than by a procedure that would involve asking them. This assumes that one already has all the terms, all of their "significata," and nothing else mixed in with them. It does not provide a way to find them.

The theory of meaning is from Mill by way of Murdock. It does not allow for the possibility that "significata" could signify you back. So Goodenough restricted his description to terms of reference rather than terms of address--as the quotation indicates. It is about identifying kin, not addressing, relating to, or interacting with them. So for English, the terms "relative" and "family member" could be kin terms, just as "father" or "brother" could. The Yap terms Goodenough described in most detail seemed to be of the former sort. In concluding, Goodenough implied that what led people to signify kin would be important for their behavior toward kin, and

	c1		c2		c3	
	a2	a1	a2	a1	a2	
b1	grandfather	grandmother	uncle	aunt		
b2	father	mother				
b3	ego		brother	sister	cousin	
b4	son	daughter	nephew	niece		
b5	grandson	granddaughter				

Figure 10: Componential Analysis of English.

that the method could also apply to many types of significata in addition to kin. So cultural determinism merges with linguistic determinism.

This caught on. Many of those it attracted had recently obtained their PhDs on the G.I. Bill. Prominent advocates included A. K. Romney (Harvard), Roy D'Andrade (Harvard), Floyd Lounsbury (Yale), Harold Conklin (Yale), Charles O. Frake (Yale), Duane Metzger (Harvard), and Anthony F. C. Wallace (Penn).

As they tried more examples, difficulties multiplied. As difficulties multiplied, variations multiplied. Componential analysis became part of the larger group of studies described as ethnoscience. But all the variations followed Goodenough in what they took as data: lists of terms as physical sounds whose referential meanings were kintypes.

In The Meaning of Kinship Terms, Anthony Wallace and John Atkins provided an exceptionally readable illustration of how completely the idea of the "meaning" of kinship terms had come to be identified with kintypes. I quote from his opening paragraph:

> The meaning of kinship terms in foreign languages… Has traditionally been rendered by English-speaking ethnologists by a simple and direct procedure: each term is matched with a primitive English term (e.g., " mother") .… Each primitive English term and each English relative product denotes an English "kin type." Thus the meaning of the term is given by list of nonredundant English kintypes, each of which includes one or more individuals in the group of persons to which the foreign term refers, and none of which includes any individual outside the group of persons to which the term refers. The validity of the matching derives, in general, from the prior use of a genealogical method of inquiry and from a general knowledge of the language and culture of this society. (1960: 58).

"Denotes" is the same as "signifies." So "the genealogical method" is equated with the use of combinations of simple English terms to translate indigenous kinship terms which is equated with the rendering the meanings of all kinship terms with kintypes. None of these equations is empirically justified, but they bring together all the elements that Schneider criticized.

The article illustrated the process with English, Figure 10. This reduces the components of English kinship terminology to distinctions on three dimensions that form what they refer to as a paradigm: sex of relative (A) male (al), female (a2); generation (B): two generations above

ego (b1), one generation above ego (b2), ego's own generation (b3), one generation below ego (b4), two generations below ego (b5); lineality (C): lineal (cl), co-lineal (c2), ablineal (c3). (1960: 61).

Comparing this with the kinship map of Figure 2 shows how much it leaves out: many specific positions, the overall shape, the boundaries and the generative principles. If this is all they can get out of a kinship terminology they actually know, how much less would they get from one they did not? By 1984, nothing had changed. The apocalypse followed.

Perpetuating Failure

Schneider first rejected biology (1965c, 1968), then kinship (1984) and then "the institutional notion" (1995: 194). But he continued to accept the underlying Positivist dogma that the meaning of a term was what it referred to and the idealist vs materialist, individualistic, conception of the problem of knowledge that this responded to. So if the something beyond experience that kinship terms referred to was not biology, it had to be something else. Others continued to agree.

A 2016 article by Robert A. Wilson surveyed these arguments. Wilson is a philosopher, not an anthropologist. His interest is in biological taxonomies. His conception of meaning is referential. He assumes that the problem in identifying any thing is to identify its defining essence. He sees Schneider as rejecting "biological essentialism" and succeeding anthropologists as agreeing by replacing it with more social kinds of essentialism:

> As Western conceptions of kinship were pried from whatever forms of bio-essentialism rigidified them, anthropologists subsequently came to speak more freely of relatives than of kin, of relationships rather than of kinship. Relative and relatedness came to be the preferred terms of cultural analysis for emerging forms of kinship (p. 574).

He specifically cites Maurice Bloch (2013), Janet Carsten (2004, 2013), David Eng (2010), James Faubion (1996), Sherry Ortner (1984), Nancy Levine (2008), Marshall Sahlins (2011a, 2011b), Warren Shapiro (2010), Marylin Strathern (2005), and Robert Trautmann (2001). His counter-suggestion is to return to biology but "redirect theoretical discussion of what kinship is by applying a view, the so-called homeostatic property cluster view of kinds, to kinship" (p. 572). Instead of a few rigid features, they should see its defining essence as a cluster subject to adjustment.

Similarly, Adam Kuper's "We need to talk about kinship," started with the observation that anthropologists now talk much less about kinship than the people they try to understand. We spend less time on describing it and say less when we do describe it. He cites the hollowness of Sahlins's What Kinship is--and is Not (2013). This, he says:

> … takes off … from the taken for granted premise that kinship is a symbolic discourse. It discourses about "mutuality of being" or "common substance" -- the currently ok terms for what used to be thought of as "kinship relationships." (Kuper 2018: 4)

Kuper's alternative is "families," since people talk about their own families and there are families that hold enormous wealth and political power.

There is no empirical problem that these are solutions for. What has to be explained is how people in every society, whether they communicate frequently or are meeting for the first time, can consistently and unambiguously agree on their mutual relations as kin and their respective places in their kinship organizations. This cannot be done by finding some material thing

"out there" that kinship refers to. It cannot be done by ignoring the ideas of kinship that they share.

Conclusion

Families are a human universal. Families are always imbedded in a larger system of related families. These make up kinship systems. Kinship systems are never the only kind of organizations in a society. Each type of organization is defined with distinctive ideas. These distinctive ideas interconnect to form the definitions of a distinctive vocabulary. Kinship terminologies are the core of the distinctive vocabularies of kinship systems. Morgan's survey was intended to establish the universality of such vocabularies. It did so. But it also generated the beginnings of an alternative to Morgan's underlying assumptions about what kinship was.

The genealogical method was described by Rivers as another way to get the information to answer Morgan's questions. In fact, it provided more. A genealogy in the way it was being developed was a diagram. A diagram is not a list. It has a spatial ordering among its parts. So, assigning terms to positions in a diagram calls for comprehensive information on their relationships to each other in a way that a list cannot.

As analysts focused on the indigenous meanings of kinship terms and organized their elicitations spatially rather than just list-wise, they inevitably encountered kinship maps. That is what a kinship map is: a spatial representation of the interrelated ideas associated with a kinship terminology. But to understand them requires recognizing that the meaning of any given term does not lie in what it refers to. It lies in the connected terms that make up its definition--which may or may not involve some concept of reference. Beginning with Fison and Howard, the accounts noted responded to the tension without acknowledging the underlying epistemological choice.

Radcliffe-Brown described the Kariera kinship map but called it a "genealogy." Fletcher and La Flesche (1911) simply described "Omaha terms of relationship." Raymond Firth (1933) laid out the Tikopia kinship map as a diagram with the terms in place of the positions they name but called it a "schematic list." The two who came closest to recognizing that they were dealing with pure systems of ideas were Laura Bohannan's description of Tiv negotiations over inherited land rights in "A Genealogical Charter" (1955) and Mischa Titiev's argument (1967) that Hopi kin terms primarily express "sociocultural values" rather than biological relationships. These studies, and many more, have enough information to allow us to recover the indigenous kinship maps and their uses. *Introduction to the Science of Kinship* does so for Purum, Seneca, Tamil, Hopi, and Kariera (Leaf and Read 2021).

Ignoring the mounting evidence for kinship maps, Murdock's 1949 *Social Structure* flatly asserted that the meaning of kinship terms could be reduced to materialist "kintypes." This had no experimental proof. It was ethnocentrism rationalized by Positivist dogma. In 1956, Goodenough made kintypes the basis of componential analysis. In 1968 Schneider rejected the biology and argued that kinship referred to symbols. In 1984 he rejected kinship altogether. It was not objective. This left nothing. But kinship is not nothing.

My dissertation research was intended to demonstrate that people in communities created their social organizations using well-established conceptual idea-systems functioning as "message sources" as described in information theory (Leaf 1972). Kinship maps were the core of

kinship idea-systems. The field work was done in 1964-66. The elicitations were experimental tests of the theory. It worked. Respondents agreed that the result was a "map" of their relationships (*naksha*, in Punjabi). The first published description was in 1971. Sylvia Vatuk independently replicated the results with Hindustani from a neighboring Indian state (Vatuk 1972). In 1984, Read showed how to expose the underlying mathematical structure. This is a formal proof that kinship maps are true by definition in their own terms, hence *a priori* as well as substantive. Since the analysis itself has the same properties, and is therefore also synthetic *a priori*, the complete result is not just a new paradigm (Read 2007; Read et al. 2014). It is properly described as a new experimental science (Leaf and Read 2012, 2014, 2021).

Kinship is objective for those who share the ideas that make it so. The task of a science of human society is to make the process of creating this social objectivity itself objective.

This is important because without an understanding of social organization, there is nothing to bring all the other subjects of anthropology together. Kinship is the type of social organization that has evolved to enable us to learn all the others.

References

Bahti, T. 1987. Histories of the University: Kant and Humboldt. *MLN* 102(3): 437-460. German Language Issue. http:// www.jstor.org/stable/2905581.

Bloch, M. 2013. What kind of "is" is Sahlins' "is"? *Hau: Journal of Ethnographic Theory* 3(2):253–257.

Bohannan, L. 1952. A genealogical charter. *Africa Journal of the International African Institute* 22(4): 301–315.

Burnyeat, M., ed. 1983. *The Skeptical Tradition*. Berkeley, Los Angeles, London: The University of California Press.

Carsten, J. 2004. *After Kinship*. New York: Cambridge University Press.

Carsten, J. 2013. What kinship does—and how. *Hau: Journal of Ethnographic Theory* 3(2):245–251.

Comte, A. 1851. *The Positive Philosophy of Auguste Comte*. Trans. by Harriet Martineau. Undated edition. *The Catechism of Positivism*. London: Savill and Edwards.

Eggan, F. 1937. Historical changes in the Choctaw kinship system. *American Anthropologist*: 39:34-42.

Eng, D. 2010. *The Feeling of Kinship: Queer Liberalism and the Racialization of Intimacy*. Durham: Duke University Press.

Faubion, J. D. 1996. Kinship is dead. Long live kinship: A review article. *Comparative Study of Society and History* 38(1):67–91.

Feinberg, R. & M. Ottenheimer, eds. 2001. *The Cultural Analysis of Kinship: The Legacy of David M. Schneider*. Urbana: University of Illinois Press.

Firth, R. 1966. *We, the Tikopia: Kinship in Primitive Polynesia*. Abridged by the author. Third Printing. First Published 1933. Boston: Beacon Press.

Fletcher, A. & F. La Flesche. 1911. *The Omaha Tribe, Two Volumes*. First published by the Bureau of American Ethnology. Republished by Bison Book, 1972. Lincoln: University of Nebraska Press.

Friedrich, P. 1992. Interpretation and vision: A critique of cryptopositivism. *Cultural Anthropology* 7:211-231.

Gardner, H. 2016. The genealogy of the genealogical method: Discoveries, disseminations and the historiography of British Anthropology: Ethnography's challenge to British anthropology. *Before the Field: Colonial Ethnography's Challenge to British Anthropology* 86(3):294-319. https://doi-org.libproxy.utdallas.edu/10.1002/ocea.5144.

Geertz, C. 1973. *The Interpretation of Cultures*. Basic Books: New York.

Goodenough, W. H. 1956. Componential analysis and the study of meaning. *Language* 32(1): 195-216.

Kant, I. 1783 [1950]. *Prolegomena to Any Future Metaphysics*. Edited and with an Introduction by Lewis White Beck New York: Bobbs Merrill.

Kant, I. 1785 [1958]. *Immanuel Kant Groundwork of the Metaphysic of Morals*. Trans. H. J. Patton. New York: Harper Perennial.

Kant, I. 1798 [1979]. *The Conflict of the Faculties*. Translation and Introduction by M. J. Gregor. Lincoln & London: University of Nebraska Press.

Kuper, A. 2018. We need to talk about kinship. *Anthropology of this Century* 23. http://aotc-press.com/archive/issue-23/

Leaf, M. J. 1971. The Punjabi kinship terminology as a semantic system. *American Anthropologist* 73:545-554.

Leaf, M. J. 1972. *Information and Behavior in a Sikh Village*. Berkeley: University of California Press.

Leaf, M. J. 1979. *Man, Mind and Science: A History of Anthropology*. New York: Columbia University Press.

Leaf, M. J. 1984. *Song of Hope: The Green Revolution in a Sikh village*. New Brunswick: Rutgers University Press.

Leaf, M. J. 2001. "Schneider's idealism, relativism, and the confusion of kinship," in *The Cultural Analysis of Kinship: The Legacy of David M. Schneider*. Edited by R. Feinberg & M. Ottenheimer. Urbana: University of Illinois Press.

Leaf, M. J. 2006. Experimental analysis of kinship. *Ethnology* 45:305-330.

Leaf, M. J. 2009. *Social Organizations and Social Theory*. Urbana: University of Illinois Press.

Leaf, M. J. & D. Read. 2012. *Human Thought and Social Organization: Anthropology on a New Plane*. Lexington Books. (Division of Rowan and Littlefield).

Leaf, M. J. & D. Read. 2014. Intelligibility and unintelligibility in *Human Thought and Social Organization:* Response to Professor Mithen. eScholarship. University of California. July 20. https://escholarship.org/uc/item/0c54s3w8.

Leaf, M. J. & D. Read. 2021. *Introduction to the Science of Kinship*. New York: Lexington Books. (Division of Rowan and Littlefield).

Levine, N. E. 2008. Alternative kinship, marriage, and reproduction. *Annual Reviews in Anthropology* 37:375–389.

Lowie, R. H. 1916. Ernst Mach: A scientist's scientist. *New Republic* 6:335-37.

Lowie, R. H. 1937. *The History of Ethnological Theory*. New York: Holt, Rinehart & Co.

Mill, J. S. 1843. *A System of Logic, Ratiocinative and Inductive*. Vol. 1. London: John W. Parker.

Mill, J. S 1865. *August Comte and Positivism* 4th ed., 1891 London: Kegan Paul, Trench, Trubner.

Morgan, L. H. 1871. *Systems of Consanguinity and Affinity in the Human Family*. Washington, DC: The Smithsonian Institute.

Murdock, G. P. 1949. *Social Structure*. New York: Macmillan.

Neurath, O. 1955. Unified science as encyclopedic integration. *International Encyclopedia of Unified Science* 1:1-23.

Ortner, S. B. 1984. Theory in anthropology since the sixties. *Comparative Studies in Society and History* 26(1):126–166.

Parsons, E. C. 1932. The kinship nomenclature of the Pueblo Indians. *American Anthropologist*, 34(3):377-389. http://www.jstor.org/stable/661896 Accessed: 19-06-2018 22:59.

Radcliffe-Brown, A. 1913. Three tribes of western Australia."*Journal of the Royal Anthropological Institute (N.S.)* 43:143-194.

Read, D. 1984. An algebraic account of the American kinship terminology. *Current Anthropology* 25:417-449.

Read, D. 2000. Formal analysis of kinship terminologies and its relationship to what constitutes kinship (complete text). *Mathematical Anthropology and Cultural Theory* 1.

Read, D. 2001. "What is kinship?" in *The Cultural Analysis of Kinship: The Legacy of David Schneider and Its Implications for Anthropological Relativism*. Edited by R. Feinberg & M. Ottenheimer, pp. 78-117. Urbana: University of Illinois Press.

Read, D.. 2006. Kinship Algebra Expert System (KAES): A software implementation of a cultural theory. *Social Science Computer Review* 24:43-67.

Read, D. 2007. Kinship theory: A paradigm shift. *Ethnology* 46(4):329-364.

Read, D. 2013. A new approach to forming a typology of kinship terminology systems: From Morgan and Murdock to the present. *Structure and Dynamics* 6(1) https://escholarship.org/uc/item/0ss6j8sh

Read, D. 2016. The deep history of kinship. Paper presentation. *Kinship Cognition and Practic,* Max Planck Institute for Social Anthropology. Halle, Germany, September 2016.

Read, D. 2018. The generative logic of Crow Omaha terminologies: The Thonga-Ronga kinship terminology as a case study. *Mathematical Anthropology and Cultural Theory* 12(1). Www.Mathematical Anthropology.org.

Read, D. & C. A. Behrens. 1990. KAES: An Expert System for the algebraic analysis of kinship terminologies. *Journal of Quantitative Anthropology* 2:353-393.

Read, D., M. D. Fischer, & F. K. L. Chit Hlaing. 2014. The cultural grounding of kinship: A paradigm shift*. L'Homme* 2102: 63-89.

Rivers, W. H. R. A. 1900. Genealogical method of collecting social and vital statistics. *The Journal of the Anthropological Institute of Great Britain and Ireland* 30:74-82.

Rivers, W. H. R. A. 1910. The genealogical method of anthropological inquiry. *The Sociological Review* 3:1-12.

Rivers, W. H. R. A. 1913. *Kinship and Social Organization*. Republished in 1968 with commentaries by R. Firth & D. Schneider. LSE Monographs on Social Anthropology, No. 4. New York: Humanities Press.

Sahlins, M. 2011a What kinship is (part one). *Journal of the Royal Anthropological Institute (N.S.)* 17(1):2–19.

Sahlins, M. 2011b. What kinship is (part two). *Journal of the Royal Anthropological Institute (N.S.)* 17(2):227–242.

Sahlins, M. 2013. *What Kinship Is—And Is Not*. Chicago: University of Chicago Press.

Savigny, C. v. 1829. *The History of the Roman Law During the Middle Ages*. 1978 Trans. by E. Cathcart. Vol. 1. First Pub. by A. Black, Edinburgh. rept. Westport: Hyperion Press.

Shapiro, W. 2010. "What Is Malay kinship primarily about? Or, the new kinship studies and the fabrication of ethnographic fantasy," in *Kinship, Language and Prehistory: Per Hage and the Renaissance in Kinship Studies*. D. Jones & B. Milicic, eds. Pp. 141–151. Salt Lake City: University of Utah Press.

Schneider, D. 1965a. "Some muddles in the models," in *The Relevance of Models for Social Anthropology*. Edited by M. Banton. New York: Taplinger.

Schneider, D. 1965b. A critique of Goodenough's componential analysis. *American Anthropologist* 67(5, Part 2):288-316.

Schneider, D. 1965c. "Kinship and biology," in *Aspects of the Analysis of Family Organization*. A.D. Coale, L. A. Fallers, M. Levy, D. M. Schneider, & S. Tomkins, editors, Princeton: Princeton University Press.

Schneider, D. 1968. *American Kinship: A Cultural Account*. Englewood Cliffs: Prentice Hall.

Schneider, D. 1969. "Componential analysis, a state of the art review." Paper prepared for Symposium on Cognitive Studies and Artificial Intelligence Research, March 2-8, University of Chicago Center for Continuing Education.

Schneider, D. 1984. *A Critique of the Study of Kinship.* Ann Arbor: University of Michigan Press.

Schneider, D. 1995. *Schneider on Schneider: The Conversion of the Jews and Other Anthropological Stories*. As told to Richard Handler. Durham and London: Duke University Press.

Strathern, M. 2005. *Kinship, Law and the Unexpected: Relatives Are Always a Surprise*. New York: Cambridge University Press.

Titiev, M. 1967. The Hopi use of kinship terms for expressing sociocultural values. *Anthropological Linguistics* 9(5): 44-49.

Trautmann, T. R. 1987. *Lewis Henry Morgan and the Invention of Kinship*. University of Berkeley, Los Angeles, London: University of California Press.

Trautmann, T. R. 2001. The whole history of kinship terminology in three chapters. *Anthropological Theory* 1(2):268–287.

Tylor, E. B. 1889a. *Primitive Culture*. New York: Henry Holt and Company. First published 1877.

Tylor, E. B. 1889b. On a method of investigating the development of institutions: Applied to laws of marriage and descent. *Journal of the Anthropological Institute* 18: 245–72.

Vatuk, S. 1972. Kinship terminology in northern India. *American Anthropologist* 74(3): 791-793.

Wallace, A. F. C., & J. Atkins. 1960. The meaning of kinship terms."*American Anthropologist*. 62(1):58-80.

White, L. A. 1951. Lewis H. Morgan's western field trips. *American Anthropologist* 53(1):11-18.

Whiting, J. W. M., M. L. Burton, A. K. Romney, C. C. Moore, & D. R. White. 1988. A reanalysis of Murdock's model for social structure based on optimal scaling. *Behavior Science Research 22*. http://escholarship.org/uc/item/8p41q9vx

Wilson, R. A. 2016. Kinship past, kinship present: Bio-essentialism in the study of kinship. *American Anthropologist*: 118(3):570-584.