(Identity and Difference): What Good are (Ethnographic) Models?

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Computational Models in Anthropology: What Are They Good For? Why Should You Care?
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This is an initial manuscript for a book (with Altamira?): ***The Use of Models in Ethnography***

Abstract. IDENTITY AND DIFFERENCE: WHAT GOOD ARE (ETHNOGRAPHIC) MODELS? There is nothing that goes by the name of research that does not involve models, implicit or explicit. One use of modeling is to bring assumptions to the surface to see whether or to what extent they hold. Models bring together narrative, cognition, history, qualitative and quantitative data, simulations, opportunities to test hypotheses and to challenge reigning methodologies. Models provide points of intersection between the different strands of a discipline. They can provide fulcrums of potential understanding, translatability, comparability, and organize debate and contention, where criticism can lead to model reconstruction. A major research achievement is to "weaken" our models, namely, to state more general or generic understandings, stripped to essentials but also to encompass a wider range of variability. Weaker testable models for different aspects of theory can then be more easily combined. I discuss ethnographic examples from studies of Turkish nomads, the Irish Republic, and other social networks—Australian, Dravidian (Pul Eliya), Natchez, Chuukese, Gailtal Austrian farmers—to highlight propositions about ethnographic modeling. This talk covers *benefits of modeling* (Part 1), useful guidelines for modeling (Part 2: including narratives, exceptions, theorems, computation in people's heads, verifiability, and boundaries), and general perspectives and theoretical framings (Part 3). What I have to say is relevant to modeling generally and computational models specifically.

Introduction

Because I know best a certain range of ethnographic cases, I use my work with coauthors in this section to give examples of ethnographic modeling—Turkish nomads, Irish language, Natchez and Australian social networks—in order to highlight propositions about ethnographic

modeling.[1] These examples will cover: *benefits of modeling* (Part 1); *useful guidelines for modeling* (Part 2: including narratives, exceptions, theorems, computation in people's heads, verifiability, and boundaries); and *general perspectives and theoretical framings* (Part 3). There is nothing that goes by the name of research that does not involve models, although they are often implicit. A major use of modeling as a research activity is to bring to the surface and reexamine the assumptions used in the research. My views about models are heuristic and chosen here by a canon of best or useful practices for empirically grounded modeling (although three of the four models in this section have also involved simulation as a means of model verification). The models discussed here are often explanatory. The ethnographic models I discuss involve observation and systematic data collection, narrative, logic, and computation (where both cognition may suffice, or, with sufficient time, paper and pencil). When I get to network analysis and simulation as methods for extending and testing complex models, we grow more dependent on computing and results can grow richer rather than necessarily more impoverished.[2] I want to stress the importance of "weaker" models—those with fewer assumptions to begin with—that give "stronger results" because they for the diversity and variability that we see ethnographically. For simulation this is similar to John Holland's (1995) injunction that the simpler the mechanism in a simulation the more we learn about the (apparent) complexity of the results. In ethnography, "weak" models are intended to capture general patterns or principles while avoiding overgeneralization, and they allow models to be concatenated or combined (White 1974). I will give some examples below.

Part 1: Some benefits of modeling

The argument in this section concerns network models, cognition, and behaviors. Most of the illustrations involve new types of analysis (structural cohesion, entailment) that do not use standard matrix-based methods of analysis. Many of the findings are backed with verification by simulations, but neither of these two topics are of focal importance here. The main focus is on the benefits of these approaches for grounding and verification of models.

1. **Models travel: Modeling** *Aydınlı nomads.* Models often derive from concepts that begin to

[1] In other sections I use cases—such as Leach's *Pul Eliya* and its distinctively but Dravidian properties, Chuukese [Trukese] residence rules, Gailtal Austrian farmers—as illustrations of marital relinking as a network construction of social class) that illustrate principles that are very poorly understood by ethnographers.

[2] Narrative reasoning and logic is equally involved, but with or without computing, consequential theorems from simpler sets of assumptions are equally relevant. In general, the ratio of thickness of results to thinness and verifiability of assumptions (in those cases where they apply), the more useful the model. Thick description is not necessarily better and has little or no descriptive value in the first place without models in place for interpretation, which provides also for comparison.

emerge as we try to make sense of something relatively large in scale and deep in content. If successful, models can replicate by incorporating themselves into culture as new understandings. Turkish nomads, the subject of my recent book with Ulla Johansen, is a good example of models that travel, meaning that in this case they apply to many areas of the Middle East (Wolfe 2006), that they have flexibility and generality, and that they enter into and in some cases participate in some ways in the culture(s) they model. Our models dealt with aspects of culture that were partly incorporated in several ways into nomad and Turkish culture. Now is a time when Turkish national culture has embraced the idea of nomad ancestries and nomadic lifeways.[3] We based our book on Johansen's forty years of ethnography and narratively reconstructed historical genealogies. They allowed us to trace the ethnogenesis of an *Aydınlı nomad* clan down to the present day, starting from the 1780s with their trek from one of the nomad heartlands, near the town of *Aydın*, eastwards into the Taurus uplands north of Adana.[4] Prior to finishing the book we posted for the book's readers on-line searchable genealogies and colored graphic imagery of the shapes and contours of self-similar repetitions of levels of cohesion that showed how these groups, in terms of their actual networks, were integrated through marriage, and how these inter-group and inter-level marriages were solidified through return marriages into bonds of trust between sets of siblings and siblings-in-law.[5] These, in turn, were shown to form the basic *Aydınlı* cooperative and visiting groups across families, lineages, and clans. We started to get emails from *Aydınlı*, up to the level of members of parliament who had *Aydınlı* ancestries, who wrote us about how various units were constituted through social and political interactions. We incorporated their insights, stories, and data into our book.

Much of my recent work, as in this book, focuses on models that operate on multiple levels, such as deriving significant social properties linked to data like Johansen's on kinship networks. For the *Aydınlı* nomads, one use of these models was to explore the implications of concepts as they expand or shrink their meaning *in relation to the network(s) they refer to*. The *Aydınlı* have sets of concepts such as those roughly translated as clan and family—*kabile* and *aile*—that operate as shifting signifiers for actual *scalable groups. Kabile* (Lebanese Arabic~*lineage*) is used for maximal lineages or large sublineages but also for clans composed of cohesively intermarried lineages, and for tribes as cohesively linked clans. Similarly, the term *aile* (Lebanese Arabic~*family*) is shown to be a signifier for cohesive family levels that shift meaning from a densely intermarried lineage segment, down to an extended family, to a nuclear

[3] The Turkish government published Ulla Johansen's Turkish language *Nomadic Life in Turkey fifty years ago*, after our book was published in English.

[4] These genealogies were searchable on-line by family, with family lines for men and women, by name and named lineage, clan, or town. We also provided an on-line Aydınlı ethnographic photo gallery and Johansen was invited to show her photo exhibition in one of the national museums of ethnology.

[5] A summary is found in White and Houseman 2002.

family, to a broadly metaphorical "we're all family" (i.e. cohesively intermarried at some level). We showed how these sliding signifiers, when synchronized with changes in actual kinship networks, corresponded to sliding scales of social cohesion in which the size of kinship groups expands or contracts with changes in their cohesive boundaries and densities that operate through *structural endogamy. A s**tructurally endogamous group** is one whose marriages and blood linkages extend so as to connect every married pair by redundant (i.e. multiple) kinship-marriage paths*. New vocabularies and network models such as these allow us to find and visualize how social configurations map out ethnographically, which we do in successive chapters, as in, for example:

Ch6 on how *structural endogamy* as social cohesion continually reconstructs the Aydınlı clan and deconstructs it over time into different segments

Ch7 on the *flexible scalability of cohesion*—in which scale-up or scale-down can enlarge when links are formed, without adding extra cost in numbers of links per person, or cohesive groups can shrink as certain ties are dissolved

Ch8 on *fractally scalable* network integration, which applies to the *Aydınlı* but is also found throughout the Middle East when generated by preferential decay for marriages with blood relatives ranked by kinship distances

Ch9 on how judiciousness and *structural cohesion* among generational sibling-in-law sets and elders acts as an equivalent of "electability" in a decentralized system of emergent leadership, in this case one of people simply gathering in a certain tent, a meeting place that typically stabilizes for an extended tenure of an emergent leader, typically lasting until voluntary retirement after a long tenure.

The modeling of effects of emergent structural cohesion, here (reviewed by Wolfe 2005), as elsewhere,[6] has been found to be predictive in a great variety of social contexts, ranging from high school attachment to collaborations among firms and organizations, as recognized in two major journal article prizes in sociology.[7]

In explicating the complex structures and dynamics of the *Aydınlı* clan, we chose to publish the book with a publisher that would allow a redolence of graphs and imagery, and would let us explicate our concepts along with the formal definitions we were developing for these **network models** in the framework of complexity theory. The publisher let us present the measures that we applied to the ethnographic study, the tables that summarized the results, and glossaries that explicated concepts from complexity models and network theory. This intricate context for reworking social anthropological and ethnographic concepts allowed us to recast kinship and marriage networks in an entirely new light. We showed in our book how *Aydınlı* ethnogenesis and ethnogenealogy related to their own complex concepts and behavioral instantiations contained in their own descriptions of cohesion-building social strategies. Our

[6] D R White & U C Johansen, 2005. *Network Analysis and Ethnographic Problems: Process Models of a Turkish Nomad Clan*. Lanham, MD: Lexington Press. (Review by Alvin Wolfe in *International Journal of Middle East Studies* 38(4):603-605). James Moody & D R White. 2003. Structural Cohesion and Embeddedness: A Hierarchical Conception of Social Groups. *American Sociological Review* 68(1):1-25. Walter W. Powell, D R White, Kenneth W. Koput, & Jason Owen-Smith. 2005 Network Dynamics and Field Evolution: The Growth of Interorganizational Collaboration in the Life Sciences. *American Journal of Sociology* 110(4):901-975.

[7] Moody and White (2003), Powell, White, Koput and Owen-Smith (2005).

models of the dynamics of cohesively emergent social entities dealt with their evolution, their spinoffs in migration of nomads to towns and cities, and the dissolution of cohesion, all of which we were able to match to the narratives and histories provided by the *Aydınlı* themselves. We did not intend to write a popular book, but these models have proven to be popular (Google: "aydinli johansen") and have travelled to a broad range of disciplines and problems.[8] The concepts of structural cohesion and structural endogamy are not particular to the *Aydınlı* but now commonly accepted bases for network analysis in the social sciences.[9] The use of analytic tools to study cohesion and systemic integration also occurs in the next example.

2. In this second example I use a policy-model study on factors sustaining the Irish language to illustrate **models for study through time** The use of models to filter the most culturally pertinent factors in a problem is illustrated in the Irish language study begun in 1971 by anthropologist Lilyan Brudner and myself. We were selected by a national panel of experts for an Irish Ministries of Finance and Gaeltacht (native Irish) award of $1.1 million to do research and to direct the Irish Republic's "Irish Language Attitudes Project." We collected not only attitudes data but longitudinal census data, family histories, institutional studies, and network data, which proved to be decisive. Our six (Irish Government Printing) book-length publications,[10] coauthored with research-team members after two years of research, prompted the Irish Parliament to repeal the Irish requirement for high school graduation and the Civil Service, and to replace a host of compulsory language policies and dependencies with government support for civic and voluntary organization. Six weeks of Irish Times editorials and reviews of the modeling efforts represented in our project publications helped to reformulate national language policy and to reenvision the host of elements in Irish culture that could support the Irish language and literature without the harsh elements of coercive policy and schooling. The resulting policy changes formed part of the historic cultural reform period from the 1970s forward[11] that helped Ireland to edge toward a more cohesive and economically successful Republic, moving from a semiperipheral position in the world economy in 1965 to a secondary core sector of the EU in 2000.[12] In the last three decades of successful reformulation of national identities and culture, the Republic also joined the EU and has ranked as one of its success

[8] About 120 pages (1/4) of the book (and 5 pages of our article) now appear in limited on-line preview in http://books.google.com/ "ulla johansen".

[9] Here, as in the use of entailment analysis in the next example (footnote 14), these measures of cohesion are not based on matrix analysis, as is so common in the social sciences, but on graph theoretic and discrete structure.

[10] Brudner and White (1973, 1974, 1974).

[11] Brown (1981), Tovey (1978).

[12] Network models of position and mobility in the world economy done with Smith and Reichardt, respectively in Smith and White (1992) and Reichardt and White (2007), show Ireland moving from semiperiphery in 1965 to a secondary core sector of the EU in 2000.

stories. The language reemerged as stronger today as a result of rethinking and repositioning its multifarious bases of support in different kinds of social networks.

The first of many research subprojects produced in the Irish project was a **longitudinal model** of the age-cohort census data reflecting language changes for each of 14 decades from 1840 through 1970. These data showed historical consistency in how Irish as a widespread native language in 1840, as reported in the 3-4 year old (pre-school) cohorts, gave way to a school-only language that went into decline after declaration of its status as the first national language along with independence from England in 1922. [13] Our census cohort model was the first to show the uncontestable effects of time: the pre-school cohorts, once with 29% native fluency (58% in the eight most intensively Gaeltacht counties) had fallen by 1970 to 3% (7% in the Gaeltacht) and artificial supports. Artificial language incentives as government policy were not working. In 1971, the start of our project, government supports for the language were coercive: "if a student failed Irish in their Leaving Certificate they were deemed to have failed the whole exam. We found that this had the effect of closing off access to progressing one's education. The requirement was abolished in 1973"—i.e., at the end of public discussion of our project results[14]. "In 1974 Irish was removed as a requirement for entry to the civil service." But we had shown, from our **survey-based and network models**, using from data that we collected for relevance to national policy, that Irish had massive support in some networks, and that voluntary efforts, which aroused support but no backlash, showed more promise for language resurgence than coercion. Here we used entailments (White and McCann 1988) among statements about Irish language attitudinal and belief statements and their relation to networks and behavior to give a logically coherent view of variation rather than a polarizing view.[15] It was in this form that the Irish public got their first coherent view from the social sciences—including sociolinguistics and anthropology—on where they stood as a nation in their identities and different views on their "First" language, the Irish.

As of today (2005-2006), things have changed radically, with numbers of native bilinguals, by self-report, having more than doubled from 1970. In Ireland today, "85,076 (7.1%) speak Irish on a daily basis (2006 census of the Irish Public), [and another] 97,089 (8.1%) weekly". The language is thriving and extensively supported by voluntary organizations such as those listed below in Table 1 as quoted in the Government of Ireland (2006:34) website.

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[13] This is the statement of the Wikipedia page of the Language Freedom Movement (11-18-07), whose views were opposed by the IRA, which was expectedly pro-Irish. As our project began, both groups opposed our project. The IRA initially proposed, as The Irish Times reported, to blow up our project headquarters on Fitzwilliam Street. We promised both groups and others, publically, only a well-designed and objective social scientific study.

[14] The quote correctly continues here "although students are still obliged to study Irish as part of the Leaving Certificate programme."

[15] Matrix-based analysis using SVD, MDS, for PCA, for example, are designed to set up polarized dimensional spaces for analysis,

In "13 June 2005, EU foreign ministers unanimously decided to make Irish an official language of the European Union. The new arrangements came into effect on 1 January 2007, and Irish was first used at a meeting of the EU Council of Ministers, by Minister Noel Treacy, T.D., on 22 January 2007. Since then, it has been regularly used by Irish government ministers."[16]

The five major monographs of the project[17] produced a series of concatenated models that gave support to a positive realignment of policy, culture and networks, rather than to compulsory and artificial policy incentives for support of Irish. Each model presented evidence of coherent variation in attitudes and beliefs about the Irish language that had a shared core with systemic variants, but also a great deal of consensus. The normative areas of consensus and the best predictors of variance in cognition and behavior were provided by studies of social networks.

Organisation	Role	Website
Conradh na Gaeilge	Promotion of Irish language in all aspects of life	www.cnag.ie
Comhdháil Náisiúnta na Gaeilge	Support for the Irish language as a living language and development of ability to speak it	www.gaelport.com
Gael-Linn	A Foundation that promotes the language in the culture and business sectors	www.gael-linn.ie
Coláiste na bhFiann	Clubs to provide opportunities for members to enjoy leisure activities through medium of Irish	www.colaistenabhfiann.ie
Glór na nGael	A national competition with language preservation and development as central objectives	www.glornagael.ie
Comhluadar	Provision of support for parents who wish to raise their children through Irish	www.comhluadar.ie
An Taibhdhearc	National Irish–language theatre	www.antaibhdhearc.com
An tOireachtas	Festival of native Irish language art and culture	www.antoireachtas.ie
An Cumann Scoildrámaíochta	Promotion of school drama in Irish	
Gaelscoileanna	Co-ordination organisation for all-Irish schools	www.gaelscoileanna.ie
Eagraíocht na Scoileanna Gaeltachta	Umbrella and support group for Gaeltacht schools	
Ógras	Irish language Youth Organisation linked to Conradh na Gaeilge	www.ogras.ie
Feachtas	Summer camps and other activities through Irish organised for young people	www.feachtas.ie
Concos	Over 25,000 students attend Irish Colleges every Summer	www.concos.ie

"There are other organisations which focus more on aspects of Irish culture than on the language itself, such as the Gaelic Athletic Association and Comhaltas Ceoltóirí Éireann. These organisations have a very important role arising from their ability to promote the language as part of their primary role."

Table 1: Network-based voluntary supports for Irish, 2006

In both the Irish language project and the analysis of Aydınlı nomads, it important to note the use of analytic tools to study systemic integration in a system with a great deal of internal

[16] Wikipedia page 11-18-07 Irish language.

[17] These were: the Dublin language attitudes study pretest, the Interim report to the Minister, the language attitudes national survey, the Gaeltacht communities studies, the national 6th form student surveys, and the national 1st form surveys.

variation by the use of analytic tools that show how of core or shared features connect to diverse variants. In the Aydınlı case the central tools were those of structural cohesion (cohesive-k core[18]s and noncohesive outliers of the networks). In the case of Irish beliefs, attitudes and behavior, the central tools for this purpose were the entailment relations whereby an element X with distribution |X| in the population would be connected to Y with distribution |Y| such that ≈X→Y (if all X or almost all X, designated by ≈X) then |≈X|~⊆|Y|. The use of ≈ for a statistical model of entailments requires that the logical proposition that ≈X→Y is not only equivalent to the set theoretic statement that the instances of X are all or almost all instances of Y. It is these tools that provide the means for seeing how a majority of elements in a network may be systematically connected through shared or core structures. For policy related research, as in the Irish project, this may be essential to understand insofar as there are connections between common core elements and policy objectives, to that there can be agreement on policy objectives that are not at odds with what is commonly agreed about basic properties of system. Identification of this core and shared values concerning its elements is what make it possible to reach a common evaluation by policymakers and experts on basic properties of the existing system and common evaluation in the populations of different institutions and communities that were affected by policy decisions. In the U.S. political system, in contrast, each of the two major parties, instead of trying to find bases of agreement for common problems and policy objectives for legislative or executive decisions, the tendency has been to find the divisive issues that mobilize a partisan base.

3. **How paying attention to (what may be wrongheaded) models that produce anomalies may lead to identifying errors in ethnographic (or other models), such as Eurocentric biases**. This is what led, in this example, to a reformulation of the classical "Natchez Paradox" which, up to 1972, was found in many anthropology texts. Here, various modelers had labored mightily to show how Swanton's (1911) description of the kinship system could be reconciled with the demographic imbalances that it implied. In what Swanton described, the descent and marriage rules would lead to an exponential increase in number of nobility so that Nobility, obliged to marry commoners, would outnumber commoners in relatively few generations. Swanton described a society whose remaining members fled or were sold into slavery after the French-Indian wars of the early 1700s. But no one would have thought to question his description were it not for **mathematical models and simulations** that set out to "explain" the demographic anomaly rather than question the interpretations that Swanton used to derive his ethnographic facts, including an insistence on gender symmetry in descent. These models,

[18] A cohesive-k core is a maximal set of nodes that cannot be disconnected by removal of fewer than k nodes.

faithful to Swanton (e.g., Hart 1943), however, were what called attention to the anomalies in his description.

The alternative view of Swanton's model was that he had imposed European biases about gender symmetries in descent groups (i.e., that sons and daughters were automatic members of matrilineal descent group) and failed to distinguish rank from class for First Americans.[19] These were biases that needed to be removed. The **ethnographic modeling** that solved the Natchez Paradox posed a problem of two competing and incompatible axiom sets, those of Swanton (1911) and my own (White 1974), as shown in Table 2. My axioms—greatly weakened from those of Swanton—were derived from rereading the historical sources. Basic assumptions differed about what constituted descent, nobility, and rank.

In the Natchez case, as in every ethnography, **networks as ethnographic models can check facts against ethnographer assumptions about patterns**. To summarized, "weakened assumptions" of the model that resolved the Natchez paradox[20] were tested by network modeling based on rereading the original sources. This resulted in the findings that: (1). there were *no Honored women mentioned in the full corpus of historical texts on the Natchez (!*), contrary to what Swanton had supposed (1931); and (2) asymmetric inheritance of rank by the "honored" sons of Noble men, as described in contemporaneous French sources and rejected by Swanton, was consistent with asymmetric rank-descent also found among the neighboring Caddo. The detailed historical network prosopography as a **reconstructive model of actual genealogical networks** examined nobility in terms of actual named individuals, occupying specific gendered ceremonial roles.

	Swanton's (1911) Model	**White's (1974) Model**
Axioms	Matridescent lines in 4 groups Nobility marry commoners	Matriline for Sun royalty with rank decay for nobility Nobility marry commoners
Groups	Sun matrilineage, children of ♂ are Nobles	Suns' royal lineage, children of ♂ are Nobles, matriline rank for 3rd generation ♀s falls to Noble rank.
	Noble matrilines, children of ♂ are Honored	Noble lines, children of ♂ are Honored, matriline rank for 3rd generation ♀s falls to *Commoner*
	Honored lineages, children of ♂ are Commoners	Honored: (♀-*no Honored women!!*) sons (♂) of Noble men
	Honored (♂) titles	Honored titles (♂), achieved (*no Honored women*)
	Commoner (♀♂) lineages	Commoners (♀♂)

Table 2: Weakening the Axioms of Descent for Natchez Nobility

[19] All sources agree on an "honored" status of males whose fathers were Nobles but who did not inherit Noble status because their mothers, by inter-class marriage rules, were commoners. Because the French term "Les Honores" was unmarked for gender, Swanton somehow inferred a nonexistent class of matrilineal Honored descent groups, assuming that daughters of Noble fathers were also Honoreds.

[20] White, Murdock, and Scaglion (1971)

4. The final example of this section gives another of the many examples where **weakening the assumptions of models can be useful**: here, in the social anthropology of Australian section systems. Radcliffe-Brown's (1913, 1931) comparative models of section systems have been fiercely resisted by ethnographers, many following Malinowski's predilection for "kinship of flesh and blood" over abstract models, a predilection that can be incorporated through network models for real people and not just genealogical stick-figures. Elkin (1941:91) wrote, for example, "generally speaking, the study of merely the formal element in Australian kinship is hardly worth doing … After all, it is of little satisfaction and affords no real understanding of the life of the tribe." And muddles in the models show up even in the most basic assumptions about section systems. Hammel's (1960), like Radcliffe-Brown's model, can be cited as wrongheaded. It is that: "section organization results from the *permutation* of lineal kinship affiliations; … all groups of affiliates are *exogamous*; and … the entire model is *endogamous*." Lineage groups are thus organized into closed cycles of exchange such that the whole system is self-contained and prescriptively endogamous, *within the model.* Hammel's model implied that each *person* in a section system must be related to every other through either common ancestors or prior marriages *between* ancestors. This assumption is materially incorrect.

André Weil (1949, 1963), unlike Hammel (1960), had long since derived a model for sections where *marriages*, not individuals, were subject to the marriage rules, so that *only one person in a marriage needs to share ancestries to "count" within the system.* Here, the system is not assumed to be endogamous:—surprisingly, it is consistent with this model that *anyone* within the system can marry an outsider and automatically be assigned a kinship category without a change in the kinship terminology or logic. Weil's model escapes from many of the Eurocentric biases that prevent overly specific and misleading models that fail to cut back on unneeded assumptions. [21] Hammel's model, in effect, is too strong, too presumptive, too prescription and, so, precludes an ethnographic understanding of the complexities, dynamical flexibility, and intersocietal transmission of Australian kinship terminology and section systems. The lack of robustness of Hammel's model is shown by slight changes in the model that radically alter our concept of Australian logics of identity and difference.

To examine this model empirically, there are data on complete genealogies, section memberships, and kin-term usage for the Alyawarra of Central Australia, collected in two years

[21] Weil's (1949) assumptions are followed by several models of classificatory systems, starting from Bush (1963) and Kemeny, Snell and Thompson (1962), and continuing to the *opus magnum* of Harrison White (1963). Although begun from weaker assumptions – ones that did not include endogamy – and further weakened axioms that better fit and comprehend the ethnographic data, some of these models forget to note the lack of prescriptive closure. The notion of closure in these models is reintroduced in the notion of abstract roles rather than concrete groups closed under endogamy. Even Harrison White confuses the abstract closure of a classificatory kinship system with prescribed marriage.

of fieldwork by Woody Denham (1978). These data showed extensive and systemic intermarriage with the neighboring Aranda, a pattern that contradicts those models that assume prescriptive endogamy in section systems. His findings are consistent with Dousset's (2005) findings, for Western desert societies, who documents widespread intersocietal marriages and diffusion of moiety, section and subsection names that involve re-alignments of systemic aspects of kin terms and section names. Denham's findings, however, were transformed once again by modelers (Denham, McDaniel, and Atkins 1979)—with McDaniel a student of Hammel—using the modeling assumption once more that these were closed systems, even if the marriages were generationally skewed and the exchange cycles represented a double helix. This was a brilliant model but with a fatal flaw in understanding.

Denham and I, in a Field Methods article (2005:89) newly reconsidered Denham's field data to come up with a "weaker" and more general model of Australian kinship and section logic[22] as an open system to which the Alyawara and Alyawara-Aranda marriages fit perfectly and which incorporates intersocietal marriages generally; but here, to understand the abstract model *and* the conceptual system, both had to be seen in terms of how they were empirically *instantiated* within the genealogical networks of actual people, as recorded in Denham's data. This was a simple ***cognitive model of chains of sibling and siblings-in-law sets in which marriages sequentially connect to "same generation," not in the European sense of "contemporaries" but linking back or ahead in history if wives are much younger on average than husbands, or can jump two generations above or below as allowed by alternating generation moieties.*** This is a "weak model" that concurs with a great range of variation in actual behavior. Models *can't be like this* of course if you expect symmetries and neatness in human behavior, as did Swanton and, in this case, Hammel (1960). But jumping generations and running generations backwards into history until history fades as forgotten when ancestries are not memorized – as against the European notion that a generation consists of *contemporaries* – is exactly what Australians with a section system logic are free to do. And this model "travels" in that generations interconnect networks across different societies.

This model allowed us to compare and reconcile cognitive egocentric categories with actual networks of marriage and descent.[23] Thus, Weil's abstraction – for *marriages* not

[22] Not unlike the egocentric model of Dumont (1983).

[23] The "weaker" model that we found, as did Weil, takes marriages and sibling sets as the units of the logic and the network. It made fewer assumptions and allowed more variability in how networks connected or disconnected, and how genealogies were remembered or forgotten. This provided a vastly easier way to conceptualize the complex patterns in the genealogies. It leads, however, to radically different ethnographic understandings of identity and difference in social relations. In White and Denham (2007) we show how, by eliminating the restrictions and flaws in the previous models, a weaker but conceptually more general and more consistent model vastly helps our understanding of section and kin-term logic for the Alyawara but the Australian continent generally. It *classifies marriages correctly although the multiple marriages of individuals* may fall into different historical and egocentric

individuals – is carried down to the concrete level in the form of a very loosely regulated *network model* that allows a huge amount of internal variation even within an abstract set of rules.

5. To conclude this section **Network models are heuristic tools but also part of complex heuristic theory**. For Linton Freeman (1988:38), "the network idea… provides a firm foundation up[on which theoretical and methodological efforts can be built…. It can facilitate the building of the critical link between very simple ideas about social relations and extremely complicated notions about [social identities], social roles, social groups, and social structures. Second, the network idea is heuristic… —it more or less maps the terrain." His idea of the "structure experiment" is that it "can be used as a guide to help us think about possibilities and limitations in both theoretical and analytical work." As heuristic theory, however, network models for integrate empirical facts, cognitive models elicited from narratives, interviews, conversations, and reconstructed networks in complementary ways —allowing us through the use and testing of models to see *how* people's cognition and their *use of ideas* relates to actual behavior and the contexts of behavior. Careful examination of cognitive models as empirical formalisms (Leaf 2007) which can be verified in their own right and put into relation with observed instantiations of the use of an empirical formalism model in social and network contexts. Second, as in the Natchez case, where the social instantiation model also unfolded in a time dimension that could be simulated or modeled in its own right, the dynamics of the instantiated model (network dynamics) is tested against simulated social interaction consistent with the empirical formalism observed to be in use. If the model does not fit, there remains the possibility that Elkin noted that it "affords no real understanding of the life of the [group]."

Some see a model as a knowledge system or also as a measurement device allowing us to work through a series of interlocked puzzles and to check the fit of our models. That definition falls on the "scientific" side of modeling—which I of course endorse, but I see *ethnographic modeling* as equally necessary also on the "humanistic" side. Sometimes "fit" is obtained directly by elicitation (Leaf 2007). In addition, the fact that ethnography deals with the interplay of individuals and collectives, this opens the possibility of using models for the study of "historical" dynamics as interactive processes operating over time and space that involve multiple interacting levels (meaning, cognition, behavior, social groupings, roles, identity, and contexts).

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generations, and marriages with outsiders are absorbed into the kin terminologies and sections. Unlike the positivistic "componential analysis" of the 1960s, these are relational models which are fit to explain the flexibilities, dynamics, and lines of transformation and borrowing of these complex systems, which share very basic and simple conceptual rules that not even the best of the Australian ethnographers have managed to explain and verify in discussions with the members of different indigenous groups, perhaps because their implicit models are still too strong and too tight.

(And that is where the talk ended. Some quotes from readers are included below).

"I think most people who even have an inkling of scientific background know why models are useful in an abstract sense. What is more interesting is in the social sciences where the problems are so complex and the data is hard to come by, how do you build empirically testable models that actually test a given hypotheses and thereby move the field forward? So any emphasis you can put on that side of the paper would be good.

- What does it mean to build a model that is not statistical, i.e. attempts to quantify uncertainty, in nature? Are non-statistical models useful and why? [DRW: See Leaf 2007]

- Kinship seems like a special kind of modeling problem in anthropology because it lends itself so naturally to mathematics and quantitative analysis. And most of the examples in your talk involve modeling kinship. But for the problems I am working on, the phenomena to not lend themselves so neatly to network analysis or dynamic modeling. Are all your points still valid?

- What can physics learn from social sciences and what can social sciences learn from physics with regards to modeling not just in an abstract sense but also in a real concrete sense?"

– Reader 1

Part 2: Some useful guidelines for modeling

7. **Modeling injunction 1: Listen to the Narrative**. Michael Houseman and I observed (1998), for Leach's (1961:flyleaf) village-wide genealogy, a tendency for Pul Eliya marriages to take place as if they were "sided" in the Australian manner, i.e., as if men mostly married women in opposing "sides" with consistent rules of inherited membership on each "side" and exchange of wives between "sides." There were, however, no named moieties and no ethnographer had elicited rules corresponding to moiety membership. When we asked South Asian ethnographers about this (e.g., Leaf and F. K. L. Chit Hlaing) we learned that for many Dravidian-speaking groups "when two people don't know how they are sided" (and there were explicit names for this both in the structure of kin terms and the concept "wrong sided" for certain marriages), "they trace their genealogies to see if they have a common ancestor." We immediately realized that network analysis alone would not solve our modeling problem. What was needed was to take the descendants of each apical ancestor born in the village and check whether any of their married descendants were "wrong sided", such that there were an odd-number rather than an even number of female links in the two descent chain. The female-sided rule, that is, operates where women change sides at marriage.[24] A parallel cousin marriage would then be "wrong sided" but so would a FZ (*one* female descent link) but not a ZD (*two* female descent links), and so forth for

[24] An opposite rule of sidedness is formulated as male-sided, where odd/even numbers of male links are counted.

all the genealogical kin types for genealogical depth up to four ancestor links (great-great grandparent). Only one error showed up in the entire genealogy of 198 people. We formulated a weak model of sidedness: you marry someone *sided* by female links (female crossovers between sides), a completely *local cognitive rule*, without any necessary knowledge of the overall village-level memberships in opposing *sides*! But since this local rule defines *balance* as a double crossover (back to *your* side), the balance theory holds: *if everyone acts in a balanced way locally, then the whole network is balanced* (see injunction 5). And since all but one marriage was *locally balanced*, the village as a whole was almost perfectly divisible into two opposing *sides* that are globally balanced.

Find the empirical formalism. This is verifiable model that corresponds to what people do and say they do, at some critical levels of abstraction (Leaf 2007). It is not always "literal" in the sense of a fixed or written code. It may contain shifting signifiers, rules of combination or recombination with other "weak" or partial models.

8. **Modeling injunction 2: Pay attention to the exceptions, if they are valid**. In Pul Eliya, if you started counting sidedness through *affinal* (i.e., marriage links) rather than strictly ancestral links, the balance principle no longer holds: affines of affines are not always affines. These errors are systematic. They are also *recognized* in Pul Eliya: marriages are called *diga* if residence is with the husband and *binna* if with the wife, and irrigation rights and residential compounds are *supposed* to be inherited through the male line. But what if there were daughters but no sons? Then the *daughter* would inherit, as if her *identity* was as a son. But then she needs to reside by *diga*, and have her husband join her in the compound but: she should not marry someone from the village or from near the village, or—as we came to understand—a man who had common close ancestors with those in the village. This rule, reckoning from Leach's genealogies, was never broken, and *sidedness* operated in three cases as if these daughters were male, although the occurrences were rare. It was these three marriages that occasionally "messed up" perfect agnatic sidedness, and that worked against agnatic sidedness as a recognized, articulated rule. Instead, people said, according to Leach, that a fundamental principle in Sinhalese (state-level) law was that "if agnatic land and water rights are passed through a daughter rather than a son, it is expected that a return of that property is arranged through a marriage *back into* that compound's agnatic line." All of this led to a great deal of *ambiguity* about claiming land and water rights from other lines because of an earlier owner having an alternate descent line, through either males or females, to an ego's household as claimant. Leach comments on how these issues of land and water rights constitute some of the major dramas and thick narrative strands within the village. And along with such conflicts of interest (and alliances between siblings-in-law being more powerful than between descent lines), this system "worked"

because land was often reallocated in ways that sometimes balanced claimants, while cooperative groups among affinals were larger and more effective than those dividing land and water as descendants within a common compound and having to consider splitting up into separate residential groups.

9. **Modeling injunction 3: Theorems are still valid even in a narrative world** (Pul Eliya). There is a definition of balance in networks for which the theorem is valid that *if everyone acts in a balanced way locally, then the whole network is balanced.* And conversely, *if a whole network is balanced then everyone in the network is acting in a balanced way.* This says nothing about either the *boundaries* of the network or that *balance is a stable condition* or that once in balanced world you can't get out. People who are used to *balancing* in this way understand that if you break the existing structure of balance, you can begin to construct a new balance. So if you look at this network over time, where you find those breaks, look further to see if they lead to a new balance, a new alignment. Exceptions may show you where the boundaries are, those outside the system, or they may show you where the internal dynamics are, inside the system. The theorem doesn't freeze time; instead it offers a means of testing alternative interpretations.

Further, as I stated above, when "the dynamics of the instantiated model (network dynamics) is tested against simulated social interaction consistent with the empirical formalism observed to be in use," one result is that a divergence between an empirical formalism observed to be in use at time 1, and assumed to be constant from time 1 to time 2, may show a discrepancy in the simulated model with the observed behavioral interactions over that time period. The use of this alternative to Elkin's foreshortened dismissal of models generally is that something changed. Did cognition change? Did the context change? Or, if not, how and why did behavior change?

Models are used by people in their computations. If those models that *are* used can be elicited or discovered, they not only help to *make sense* of what the ethnographer sees, but they have their own internal logic, representable by some internally consistent form of computation. For these logics there are *theorems* that follow as consequences. If the model is followed in cognition, in behavior, *those theorems are real, they are real consequences*, ones that the ethnographer will never be aware of unless they work out the logic.

10. **Modeling injunction 4: Models are verifiable, so figure out how to verify**. In my 1999 article in the journal JASSS (concerned with simulation), I created a "randomized" comparison group for Pul Eliya village where everyone kept their agnatic affiliation and generation, but women's marriages were "rewired" as filling empty musical chairs within each generation. The statistical comparisons between actual and "rewired" marriages were astronomically on the side

of consistent ancestral sidedness, including the "rewiring" of generationally skewed marriages.

11. **Modeling injunction 5: Weak models are better than strong, in part, because they cover more general and partial principles that may be combined with others.** The goal of a model is not to be fully detailed and complete. Natchez, Alyawara, Pul Eliya: the same modeling principles are at work each time, but the successful models are the *weak* and *partial*, paying attention to detail and exceptional cases that could be explored in terms of ethnographic context (White 1974). Reconnoiter all three cases: It's called renormalizing. For good measure, in the JASSS article, I tested in a "rewired" comparison of the rules we discovered for Austrian farmers in the Gailtal village of Feistritz (Brudner and White 1998). There marriages within the village were aligned with heirships of the farmsteads, and the heirs could be either male or female, and non-heirs were given quitclaims to balance their inheritance rights, so the correlation was between migrants and outmarriage, heirs and inmarriage. Inmarriage was defined as *structural endogamy*: marrying someone that you are already linked to through bilateral ancestries *and* marriages among those ancestors. We found a way to calculate the boundaries of this well-defined concept because for any body of genealogical data, there is a theorem of bicomponents that, when applied to a network of *marriages*, where there are maximal connected sets of marriages connected by parent-child links that cannot be separated by removing just two of the marriages, each such set is identical to a maximal set in which every *pair* of marriages has two or more independent parent-child ("relinking") paths that connect them.[25] In Feistritz, as for most communities, there was one giant bicomponent with no competitor at that scale. The puzzle in modeling this marriage system, with bilateral descent, was: how do heirs "reckon" how to form a marriage that will be "in" the bicomponent given that most people migrate out (as your fiancee's family might do, leaving you effectively outside the bicomponent). Do people marry "close" while still marrying sufficiently "distant" so as not to be in a small bicomponent insufficiently ties to the core community. The JASSS "rewiring" simulation showed that all statistically significant "relinking" occurred in each generation either within circles of siblings-in-law or mixed cousins-and-siblings-in-law, with no marriages between blood kin. In other words, there was *synchronization* for generation-specific structural endogamy, ignoring the kin-ties back to dead ancestors (or ancestors likely to be dead).

12. **Modeling injunction 8: Boundaries are important**. For Australia, territorial groups compete. That doesn't mean they should be considered as strictly endogamous groups. Yet groups not only compete, confront, fight, argue… but they migrate through each other's

[25] Structural endogamy is the same *abstract concept* for the *Aydınlı* marriages between consanguines and European "relinking" marriage between affines. See White (1998).

territories, meet, exchange, intermarry, discuss how to realign kinship terminology after intermarriage, borrow one another's kinship terms. Dousset's (2005) Western Desert world is all about this. Renormalizing is going on all the time, from scale to scale.

More to say here? "Definitely don't drop the "Boundaries are important" point. I would in fact expand upon it and talk about boundary conditions from physics and how they are used to derive the dynamics and then think about how that can be applied to social science" – Reader 1

Part 3: Perspectives on modeling

13. **Perspective 1: Different cultural realities do not lack comparability as the interpretivists had asserted**. Even to make the claim of non-comparability (e.g., Geertz 1973) requires a comparative frame. Rather, it's the lack of good models that lead to lack of ethnographic understanding. If you cannot compute what members of the community are computing, you are not able to understand, and you will have an implicit model that is wrong. Had we not been able to understand how *sidedness* is computed in these communities in Australia and Sri Lanka, or *the concept of structural endogamy*, or *balance*, which are easy for *people* to compute (but not always easy for computers) you wont' have a model that works to understand marriage, social class, role behavior, why some things are regarded locally as exceptions, what the competition is about, and so forth.

14. **Perspective 2: Reality is neither so impossible to think about nor so arbitrary as the structuralists had asserted**. Lévi-Strauss (1970:125) took this view of modeling, and, with hindsight, it is only partly right:

> [T[here must be a way to approach the study of kinship systems which avoids their apparent and impossible complexity…. This ought to be the sociologist's point of view, but … it might also be that of the natives…. [The] Maricopa themselves conceived of kinship as a clearly defined system….
> '[I]t is partly as a theoretical formula that subsections are carried intertribally' (citing Stanner). 'Those who doubt the aborigines' power of such abstract reasoning can never have heard them expounding to their tribe's fellows how ŋinipun (subsections) should work, by inference from the theory to the case under attention. In this way an abstraction becomes a flesh-and-blood reality.'

Where Lévi-Strauss was right is to pay attention to "the theoretical nature of the native's conception of his own marriage [or other] system." His structuralism, however, never matured to get beyond their own parochialisms, e.g., such as favoring dual oppositions as the fundamental human process of thought, or, in some sense, regarding all models as equally arbitrary because abstract. Where he was wrong is to assume that people lack concepts to understand the very

social networks in which they live, work, and play. We have many more emergent concepts in social network studies that we had 50 years ago, but each individual has their own emergent concepts, some of which are shared across subgroups, larger groups, or in cultural, organizational, or institutional configurations. Aspects of each of these can be abstracted as models, compared, and decomposed or combined.

15. **Perspective 3: Models for comparison are no different than those for ethnography**. This is contrary to what Goodenough (1970) argued in *Description and Comparison*, and I take issue with him. His view was biased by the linguistic idea that that "componential features" must be imposed from outside to represent the inside *emic* structure of contrastive features used to create meaningful utterances. This was wrong, but it led him to oppose the *etic*-comparative as what you could describe from the outside in absolute terms of the observer without the *emics*. His was linguistic (Saussurian) bias led him to err by concluding that because emics represent the emergent contrasts that carry signals. But they do not reach to meaning or grammar. His notion of cumulative scalograms of features of meaningful contrasts of position in a system suffers the same failure: they are not comparable across cultures and are not meaningfully interpretable within a culture. They focus on models located too far down into linguistic structure to be useful ethnographically, and the various missions of ethnoscience and the "new" ethnography failed in consequence. His dilemma with the residence controversy (with fellow ethnographer Fischer) illustrates why they were both wrong: they were arguing between arbitrary contrastive features *correlated* with resident "types." They failed to construct the networks in which residence, social obligations, commitments, relational positioning, and claims over others and over resources were situated. David Schneider[26] was correct, as shown by Skyhorse (1998, 2003), that what determined residence on Chuuk is strength of claims through the kinship network to those relatives that allow access to land.

Comparison of models as relational structures with elements, relations, qualities, quantities, etcetera (rather than just componential elements that intersect) make possible comparisons between different ethnographic contexts and are of a sort that is essential for understandings within each. In each case we are dealing with similar issues of complexity and emergence.

16. **Perspective 4: Network and relational models are necessary**. Mathematics forms part of indigenous systems of cognition. It is necessary to understand this if we are to understand our

[26] Like Goodenough (1956) and Fischer (1958), Schneider was one of the Chuukese research team taken to the field with Murdock's Micronesian field project, but then went on to study the Micronesians of Yap, where he discovered the connection between land rights and kinship.

own ethnography. As Leaf (2007:6) says:

> Up to the Renaissance, the predominant view was, in fact, that mathematics was empirical: a formalization of relationships that actually existed in nature. That it actually had been derived this way historically is beyond doubt, but the view I am referring to was not only historical but also epistemological. Galileo was a physicist and a mathematician, with no line between them. The chemical conception of the atom associated with the periodic table, and its application in chemical analysis, is chemistry and mathematics. Relativity physics is physics and mathematics, and, as Dwight Read, Michael Fischer, and I have previously argued, the mathematical analysis of kinship terminologies is kinship and mathematics.

Ethnographers have more difficulty to "see" the complex effects of interactions that are going on beyond their view, however, beyond their participation in particular narratives. They cannot *compute* the implications of interaction, which is equally hard for the ordinary citizen. And if there is an the *indigenous mathematics* of interaction such as a balance principle, the Western bias is not to perceive it. If there is a dynamical balance principle, it is hard to detect even for the best observer. Here, we need network analysis and dynamical modeling.

Part 4: A Partial Conclusion

Ethnographic modeling is about what people say, interactively; and do, interactively, and how the saying and the doing relate, in the network of interaction, which forms a structure that changes through time, and a dynamic, which is the change through time, and which can be productively modeled, but only if you pay close attention. Its not just about computing: people also have and use models: in many cases they can be elicited. Knowing what these models are is important. Sometimes getting at them is hard, and we have to relax, and vary, permute, our models, until they fit. Then understanding is possible. Models also exist for understanding regularities in behavior that cannot be consciously elicited or make explicit in narrative accounts. The modeling problem here is not fundamentally different: we have to relax, and vary, permute, hopefully simplify our models, until they fit. In some cases our models become complex: we have to think about ways to find the simpler elements and relations that compose them. We can start with the complexity and try to find the simpler compositional principles (the model is an emergent), or we can start with the simpler principles by which interactive elements are composed, and look for complexity in the effects of composition (the model is an emergent). In any cases, anything you do in or with anthropology involves models, and the study, use, application, and critique of models can lead to productive outcomes as emergents, for which we also need a better theory and models of how this process works in our discipline. What you pay attention to, however, may depend on the goals of your research.

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Talks in order this session:

KOEHLER, Timothy. TESTING OPTIMALITY IN SITE LOCATION IN THE ARCHAEOLOGICAL RECORD: LOCAL EXAMINATION OF A GLOBAL QUESTION.

WHITE, Douglas. (UCI) (IDENTITY AND DIFFERENCE): WHAT GOOD ARE MODELS?

READ, Dwight. W. (UCLA) CULTURE: THE CHALLENGE FOR COMPUTATIONAL MODELING

GESSLER, Nicholas. TRANSCENDING THE LIMITS OF NATURAL LANGUAGE – EVOLVING ARTIFICIAL CULTURES VIA SIMULATION AND FACILITATING EMERGENCE THROUGH INTERMEDIATION

KUZNAR, Lawrence A. (Indiana University – Purdue University) APPLIED VERSUS PURE RESEARCH IN COMPUTATIONAL MODELING: IS THERE A DIFFERENCE?

MCNAMARA, Laura A. (Sandia National Laboratories, Albuquerque, New Mexico.) COMPUTATIONAL CRYSTAL BALLS AND THE SEDUCTION OF PREDICTION.

Early Title and Outline:

"Using Models in Ethnographic Falsification and Reconstruction"

Checking ethnographic descriptions by Logical Contradiction, Simulation, and Reconstruction: The Natchez Example

Resolving Ethnographic Disputes and Reconstructions: The Case of Chuukese (Trukese) Residence Rules (Schneider vs. Murdock, Goodenough and Fischer)

The Sociocentric/Cognitive (Dravidian) Controversies in South Asia (e.g. Pul Eliya) and South America: Controlled Simulation in Ethnographic Reconstruction

Reconstructing Australian Closed and Open Network Prehistories: The benefits of weak models for Cognition (Read) and Behavioral Dynamics (White, Denham, Leach, and Houseman)

Using Behavioral Dynamic Models in constructing alternative planetary futures

Toward better understandings of Identity, Difference, and Indigenous Rights

Original Paper Title: "Revisitng Questions of Identity and Difference: Eurocentric Biases in Modeling Australian and Dravidian Social Networks"