Chronicling Cultures

LONG-TERM FIELD RESEARCH IN ANTHROPOLOGY

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COLLABORATIVE LONG-TERM ETHNOGRAPHY AND LONGITUDINAL SOCIAL ANALYSIS OF A NOMADIC CLAN IN SOUTHEASTERN TURKEY

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Introduction

This chapter describes Johansen's Turkish fieldwork and our collaborative longitudinal study of nomad social dynamics (White and Johansen 2001) based on a 40-year span of field visits, extensive oral histories, and a 200-year genealogical database. On the basis of this case study, we elaborate a paradigm for diachronic network research on social organization in the context of long-term field research (see Brudner and White 1997, White *et al.* forthcoming).

Long-term field studies are not synonymous with longitudinal research, which entails the capacity to track samples of cases through time. Long-term fieldworkers can easily keep track of individuals anecdotally or examine small samples of cases systematically, but computers are needed as the size of populations or complexity of problems outstrips the capabilities of researchers. As a supplement to qualitative "hands-on" research, the capacity to track samples with the aid of computers can be tremendously helpful; especially when the issue is one of comparability of data over time. In the Gwembe Tonga project, for example, years of work were needed to bring the computerization of the project up to a level suitable for longitudinal analysis (White *et al.* 1998, Clark *et al.* 1995). A similar collaborative effort for the Tzintzuntzan project was begun in 1998 and is still in progress (see the Tzintzuntzan website, currently available at *www.santafe.edu/tarasco/Mexican.html).*

Through longitudinal analysis, long-term field sites become crucial laboratories for monitoring changes that may benefit local peoples (Cernea 1996). Longitudinal analysis also offers a better interface between theories, methods, and the richness and embeddedness of ethnographic data. Both qualitative and quantitative data are necessary to capture the rich and shifting quality of people's lives. Longitudinal analysis, in this context, vastly enriches our understanding of social processes.

The aim of this chapter and of our forthcoming book (White and Johansen 2001) is to show how "classical" long-term ethnographic studies of social organization, using genealogical and oral histories among other methods, can be

reformatted and reanalyzed by computer in terms of network analysis. In our book, using the Turkish case as an example, we describe new methods designed to facilitate network analysis of data from genealogies and narratives, and show how to identify emergent structures in kinship networks, such as cohesive groups, using concepts defined in graph theory.

A meeting of family heads in the large black tent of the *Tanidik Kisi*. Photo by Ulla Johansen.

The Turkish study is a case where kinship networks are central to social organization. It has a number of characteristics that make it perfect for longitudinal network analysis of social organization using genealogical data. Its patrilocal residential groupings are determined by genealogical links, as are its patrilineal extended families and linkages. Kinship links provide network supports for leadership positions and cohesion with the larger clan. Here we will discuss how an analysis of kinship networks contributes to understanding the dynamics of social organization in the Turkish case, leaving out the details that can be found in our book.

It is not easy to find the right voice for a pair of anthropologists who work collaboratively but from different perspectives. A formal division of our chapter

into two parts – one by the ethnographer and the other by the analyst – would not be appropriate, as Johansen took a direct interest in the analysis, as did White in the ethnography. We decided to retain our unifying "We" and at times, where appropriate, to break our dialogue into specific "Johansen" and "White" responses within our common enterprise. By emphasizing multiple voices in the ethnographic enterprise, we also hope to provide multiple possibilities for those researchers or members of the Aydinli who will one day read this study and will view the enterprise differently. Perhaps our dialogue will make transparent some of the differences and similarities in our orientations to research and analysis, especially for younger anthropologists considering long-term field research.

Long-Term Fieldwork Among The Aydinli

The nomads of the Turkish Antitaurus are officially named Yürük or Yörük ("nomad"), which is what those in the west wing of the Taurus Mountains, the Taurus proper, call themselves. In contrast, the nomads of the east wing, the Antitaurus, call themselves Aydinli ("people from Aydin"), a town and its hinterland in Southwest-Anatolia. The Aydinli and Yürük were organized in tribes, large political units of several thousand people; some of which, such as the Karakoyunlu ("people with black sheep"), have been documented since the 13th century (Sümer 1967). The Aydinli clan that Johansen (1965, 1994) studied had no traditional name, since they formed a distinctive group in the 19th century. They introduced themselves to her as Karaevli ("people with black houses"), a name which refers to the black goat-wool tents they lived in and which is sometimes given by sedentary people to full-time nomads. After they knew each other better, they advised Johansen to look at them simply as a branch of the Karahacili tribe. This implies a form of cohesion somewhere between a clan identity and an established tribal name.

Johansen: My interest in peoples with Turkic languages began in 1951 when I worked on my Ph.D. thesis on the Yakuts in Siberia. Since systematic fieldwork in the Soviet Union was impossible at that time, I decided to investigate Turkish nomadism in Turkey. The Aydinli study was my first fieldwork training. After having taken courses not only in anthropology but also in Turkish and Islamic Studies at Hamburg University, I studied one semester at Ankara University and became friends with both senior and junior Turkish colleagues. I also was taught how to behave properly in Turkish society.

In the autumn of 1956, I visited a number of families among the Aydinli nomads of Southeastern Turkey to get a first impression of their culture and to choose the lineage to which I would try to gain access. Turkish archeologists Halet Cambel and Bahadir Alkim helped to introduce me to this group. As they did

excavations in the area of nomadic autumn and winter camps, they were able to introduce me to a farmer who had business contacts with the nomads. This farmer became my first guide and consultant. From April to September of 1957, I spent half a year observing the nomads' daily routine: their economy, material culture, patterns of interpersonal relations, and Islamic folk religion. Since no studies on these nomads existed at that time, with the sole exception of a Turkish Ph.D. thesis on their physical anthropology and a Turkish traveler's report, I wished to write an ethnography. As a 29-year-old young woman – no more than a "girl," from the Aydinli perspective – I could not establish my own household in their society, but had to live as a member of an extended family in the tent of my adoptive family. This was a "loner" type of fieldwork (Foster *et al.* 1979:10). I revisited the Aydinli for short periods in 1964, 1970, 1982, 1989, and 1995. Among other things, I studied the social organization of the lineage and the whole clan. The consequence of my involvement – which became a lifelong commitment to document the Aydinli's history – was a very intensive form of studying a nomad society.

Over the years, I worked on many other projects, but always kept my Aydinli connections open by letters. There might be a hundred or so letters in response in my files, dictated by the old people and, in the last decades, written by the young people themselves. My adoptive brother, who became a clan leader, learned to write as a teenager although he never visited a school. Sometimes I have had phone calls from richer members of the clan during winter, when they are in town and if they know I may visit the next summer. We gossip freely in both directions, and I ask for news about their families: Who has married whom? Who has had babies? What professions do young people have now? How do they organize their lives if they give up nomadism? What ties have they still with the clan? Who is most popular now among the leaders? Has anyone died?

My biggest regrets were not to have written down my first field report immediately after my return from the research in 1958, and to have been too self-conscious in asking who would pay my salary, and for what? I became involved in Museum fellowships and Central Asia research positions. My fieldwork proved critical to my subsequent teaching career. Before field research, I was not interested in theories or methods. Only after I could evaluate a theory or method in the context of my field experience did these become exciting for me.

Although my field trips lasted no more than six months at a time, and mostly included summers, it was during summertime that nomadic life ways were most in evidence. The Aydinli winter camps are in the lowland of southeastern Turkey, the so-called Cukurova, only a few meters above sea level, where they live dispersed among other groups. In summer this lowland is very hot and humid, with temperatures rising to 47 centigrade in the shade. Thus, the goats and sheep have to be driven to the mountains, which rise at the inland side of Cukurova to a height

of nearly 4,000m. In winter it is snowy and the temperatures reach -20 centigrade, so herds and their owners have to come down before it gets too cold.

In the first decades of the Turkish Republic, the nomad clans had an interest in registering as settlers of the newly founded villages in former Armenian lands, which meant charge-free access to the projected villages' pasture grounds. In this way, the Aydinli obtained a common summer-pasture, a valley of about 12 km length with surrounding mountain ridges at a height of ca. 1,500m above sea level. This common territory was an important social camp for those groups, but every autumn the patriarchs of the lineages were forced to enter into long negotiations concerning their winter camps with farmers or nomadic groups who had obtained territories in these lowlands. Since the 1980s, spring- and autumn-pastures have had to be paid for as well, due to the need for a pause of about two weeks on the way up or the way down the 100-200 km migration routes.

Migrations were the occasions at which all nomads, men and women, had the most contact with the outside world – with other clans, even tribes or villagers. They were forced to cross the territories of other groups at least two times every year, in spring and in autumn. These migrations revealed in a profound way the cultural changes during the time of the long-term fieldwork. In 1956-57 most of the clans still migrated in the traditional form: the lineage went together wearing their best garb, with beautifully decorated camels. These animals were festooned with tassels, felt-plates with beads, and *kauris* and smaller bells at the heads to control acoustically the regularity of their movements, as well as with large bells at their sides, announcing the coming of the group at a considerable distance. Migrating with the Aydinli, I saw that they had little concern if their herds were damaging the borders of growing cereal-fields along the way, nor did they ask the villagers before they interposed a spring pasture of two to three weeks on their common lands. "Farmers are cowards, they negotiate. Our tongues are in the barrels of our rifles," declared a young man, pointing proudly to the weapon which he, like every male nomad over 14, wore at his shoulder.

This attitude had changed fundamentally by 1970, when I again took part in a migration. The nomad women did not wear their fancy dresses any longer, but tried to look like farmers' women. There was no expression of festivity, joy, or pride left in connection with the first migration day. Joint families – no longer whole lineages – went together. Tractors with racks, which a few of the clan members owned, transported the tent and household utensils of many families. By 1982, only a few nomads were still camel breeders. Their animals were used for the steepest parts of the way and all camel-decorations had already been sold to tourist antique-shops in the bazaars of Adana and Kayseri. In 1995, the herds of some families were even transported by trucks to avoid the hostility of farmers

living along the migration route. The scarce common land of the villages was under tight control of farmers and policemen.

Turkey saw its population grow from about 25 million, when I first met the nomads, to about 65 million in 1995. What such dry numbers mean to the self-consciousness of people can really be understood only through the experience of long-term fieldwork.

White and Johansen: These dramatic changes in the situation of the Aydinli are reflected in the network analysis, which includes demography, migration, and changes in social organization. Because we also have network data from the genealogical reconstructions, combined with oral histories, we have provided in our book a longer-term perspective on demographic and social changes from the clan's establishment in its present territory in the mid-19th century.

In the process of her forty years of mutual acquaintance with the Aydinli, Johansen changed in their eyes from being a young foreign "girl" – albeit conversant in Turkish – to an elder "auntie" of the clan members with whom she had stayed so many times. The long-term fieldwork not only made her an observer of becoming old together with the Aydinli, but she had also shared their feelings at many occasions. Thus the young people increasingly took her for a witness of their tribal life in the "good old time," because enormous changes had taken place since her early visits. The people of her age and their children, for example, had no school training, but now all children – girls and boys – go to school for at least five years. Many of the nomads, who have obtained permanent houses in the lowlands, now watch TV for some months of the year, and are increasingly connected to elements of the global system.

Johansen: It may be useful to give younger anthropologists a sense of the field methods I used in collecting the genealogical data. Younger men, when asked, said at once to ask their fathers or fathers' brothers. To get genealogical information from the Aydinli patriarchs proved to be an easy task. Even the most prominent among them would give his genealogy and draw it into my large book in the manner anthropologists do. They reacted eagerly to my request to tell their genealogies, because quite obviously this gave them the opportunity to make known once more the long history of their families and the influential position of their ancestors. They immediately understood the genealogical drawings and wished to correct them by having me review them. And as soon as the man just talking named relatives whom he had in common with other men present in the tent, the latter began to supplement or correct the information he had given.

Women, even elderly ones, laughed in embarrassment when I put the same question to them, and said they did not know anything of the older times, either of

the families they had married into or the families of their descent, which they had usually left at a young age. But when asked the names of people of their own generation, and especially those of their children and their children's children, the elderly women were better informed than the men. They could tell the exact sequence of children, even for those who died as infants. In this way, they could even estimate the age of the children and young people by counting a regular distance of two years between each birth for a woman.

In later fieldwork, when I came into tents of the clan's summer pasture for the first time in the season, the host would announce in the presence of as many men as possible: "Go to it, draw out my roots, let us see!" The recitation was for the host a question of reputation. Sometimes, when I did not want to waste too much time, I showed the already written genealogy to my host and asked him to correct the data. I then recited his full genealogy in a loud voice waiting for his corrections, and men always laughed in amazement that a woman knew their genealogy. As my data grew, I knew it even better than did some of them. By the constant repetitions I had good control of the data and could eliminate errors. In this way, I traced back ancestral lines of the informants to ten "roots" and their descendants (White and Johansen 2001).

Over the years, the focus of my study of social organization has remained enigmatic. In 1982, I decided to integrate all of my genealogical field notes and diagrams into a single large scroll consisting of successive sheets of graph paper, taped together, organized lineage by lineage. This compilation of genealogical data provided me with a way of assessing my data, but it formed a scroll some forty feet long! The scroll served my research purpose of being able to assess genealogical connections as I wrote about different aspects of nomad life, but it was not in a publishable format. While the males were neatly organized in lineages, females each appeared in *two* places: once as a daughter and elsewhere as a wife. Across the bottom of the scroll I had drawn parallel colored lines that connected upward to the positions in which each woman appears once as daughter and once as wife. Had I to do it over again, whatever the method of keeping the genealogies, I would from the beginning assign unique identification numbers to each individual and, in addition to genealogical drawings, keep a numerically ordered file on individuals that also showed at least their parents and spouses, if not also their children by each spouse and siblings by each parent.

In formulating the goals of our collaboration, we had to answer two interwoven questions. Mine, deriving from the fieldwork perspective, was: What happened to the nomads in the last 200 years? White's was: *Why and by what processes* did it happen? What role did social networks play in the dynamics of changing social organization and adaptation?

With a network approach that fitted naturally to an ethnographer's concerns with social organization, we reconstructed the developments resulting from the social rules of Aydinli society. The network analysis was able to make these developments visible in a set of graphs that formed the basis for our analysis of changes in social organization and leadership.

Data Issues

Johansen: The problem of "minimum core data" in social anthropology was raised by A. L. Epstein (1967), then by T. Scarlett Epstein (1979), and more recently by Emilio Moran (1995). It was not an idea I took to the field back in 1956, but like many anthropologists I improvised on the spot. Exact birth dates, for example, were impossible to obtain from archival records, but general time frames could be inferred from narratives. In some societies it is important to have accurate birth order even if birth dates are not available, so where possible in the genealogies I listed children in order of birth.

Because the data regarding genealogies were systematically collected, with updating of genealogies during each visit since 1964, they are accurate throughout a period of nearly forty years. Discrepancies in memories about earlier ancestries have been closely examined, and corrected where possible, for the full 200-year period of their coverage.[1] The genealogical data provide a longitudinal framework for analysis of social organization that encompasses but also goes beyond the framework of my field visits.

White: Not every field worker has genealogical data complete enough over a long enough time period to support a network analysis of social organization. On my visits to Cologne throughout the 1990s, after first meeting Johansen, it became evident to me that her genealogical scroll for the Aydinli, which had become well-known at Cologne's Institute of Ethnology, could support a richly documented diachronic network analysis. For several years, I asked if she would simply assign unique ID numbers to each of the individuals on her scroll, which would allow the genealogical network to be transferred to computer. Our joint research project on the network analysis of her genealogies was able to begin when she did so in 1997.

After the persons in the scroll were numbered, we followed established methods (White, Batagelj, and Mrvar 1999; see also White and Jorion 1992) to code and analyze their genealogical links and attributes by computer. There were 1,309 individuals, including 364 married males and 386 married females in 412 marriages. The earliest generation, going back to seven of the founders (or remembered roots) of informants' ancestries, dates back to 1785.

Schneider's (1986) critique of genealogical studies notes the strong bias of many kinship studies to assume that biological ancestry (an obsession of societies

that emphasize legitimacy in rights to inherited private property) constitutes the core of human kinship. Too often, genealogical data collection has been tied to some master-scheme for reconstructing social structure.[2] Frameworks for minimum data should be constructed so as not to force rigid or inappropriate taxonomies of residence types, descent groups, and the like. Coding social organization as a collection of individuals and groups with networks and attributes has enormous advantages in steering clear of the pitfalls of many kinds of analysis previously used in kinship studies.

Johansen: While network methods have seemed to most anthropologists to serve only for specialized synchronic studies in places like cities, our study aimed at using diachronic network methods to study organizational change within a "traditional" population. Moreover, we used some of the most traditional and widely available types of ethnographic data such as genealogies and oral histories supplemented by biographical and actor-oriented information.

White: The requirements for minimum core data for genealogical studies used in a longitudinal network framework are rather simple. One must: (1) identify each male and each female uniquely so as not to confuse those with the same name or similar names, (2) create an inventory of all marriages and multiple marriages of individuals, and (3) identify all children, keeping careful track of their parents, and, where known, children who died prior to reaching adulthood or died without marrying or having children. Even where exact birth, marriage, and death dates are lacking, the network-ordering of genealogical generations creates a longitudinal framework for the analysis of different time periods. The result is a "rough cut" for assigning historical periods to the levels in the network. In the present case, these levels are highly correlated with Johansen's estimates from narrative sources as to the generational levels of individuals in their historical cohorts.

Johansen: Looking back to the first volume on long-term research (Foster *et al.* 1979), it is surprising that "minimum core data" (Table 15.1, p. 333) did not include the minimal kinship links (mother and father) needed to construct genealogical networks. How could such an important dimension of ethnographic data have been ignored?

White: Our study shows that a traditional technique of anthropology, the genealogical census, is now feasible within a database for a population; whereas in 1979 relational or network databases were still well beyond our capabilities. Another factor that made genealogy problematic as a basis of analyzing social

organization in 1979 was the link between the "personal data" of names and identities and the possibility of "open data banks" that were considered a potential time bomb "whose explosive potentialities cannot be predicted" (Foster *et al.* 1979:336). What we have seen, however, is an "explosion" of Internet-based public-domain genealogies and archival materials on over fifty million people, and still expanding. The creation of genealogical software now makes possible the exchange, merging, and analysis of genealogical data. Finally, network analysis software (such as Pajek, Batagelj and Mrvar 1997) now allows new approaches to the analysis of social organization using large-scale network data.

Network Analysis

Johansen: Most anthropologists are convinced that network analysis is something exquisitely synchronic, but in our work, White has demonstrated that network analysis also could be used to discover diachronic processes – even over more than 200 years, as in the case of the Aydinli.

White: We used the ethnographic context of successive marriage events to define both long-term and immediate impacts of marriage events and relationships. We also examined the impact of environmental factors in shaping family relationships, alliances, social groupings, political support networks for emergent leaders, likelihood of migration, transmission of cultural knowledge, etc. While such an approach may be complemented by the study of other events, it still is commensurate with an understanding that "the essence of culture is change . . . and there seems to be only one way to get necessary data for an adequate anthropological analysis: through long-term research" (Pospisil 1979:142). We have tried to provide a dynamic representation of events and their impacts through time that, when coupled with a rich ethnographic database and the ethnographer's long-term experience of change, can contribute to the development of a dynamic theory of culture.

Thus, we were interested not only in demographic changes, but also in how genealogical linkages created network patterns relevant to social organization and social practices. The graph theoretical concept used to identify cohesive groups in the kinship networks was that of *bicomponents,* defined in graph theory as sets of nodes (here, marriages) in which each pair had two or more independent paths connecting them through ties of descent. Since the nodes are marriages, multiple paths of relationship trace out what is called in social anthropology either "marital relinking" between families, or blood marriages.

The graph in Figure 1 shows the entire network of intermarried descent lines associated with relinked marriages, starting from marriages of clan founders at the top of the graph to those of the present day at the bottom. In this graph, marriages

are represented by nodes (circles), distributed across eight historical network-generations, while the dashed and solid lines, respectively, show lines of male versus female descent. Graphs such as these become the object of a network analysis of kinship and marriage.
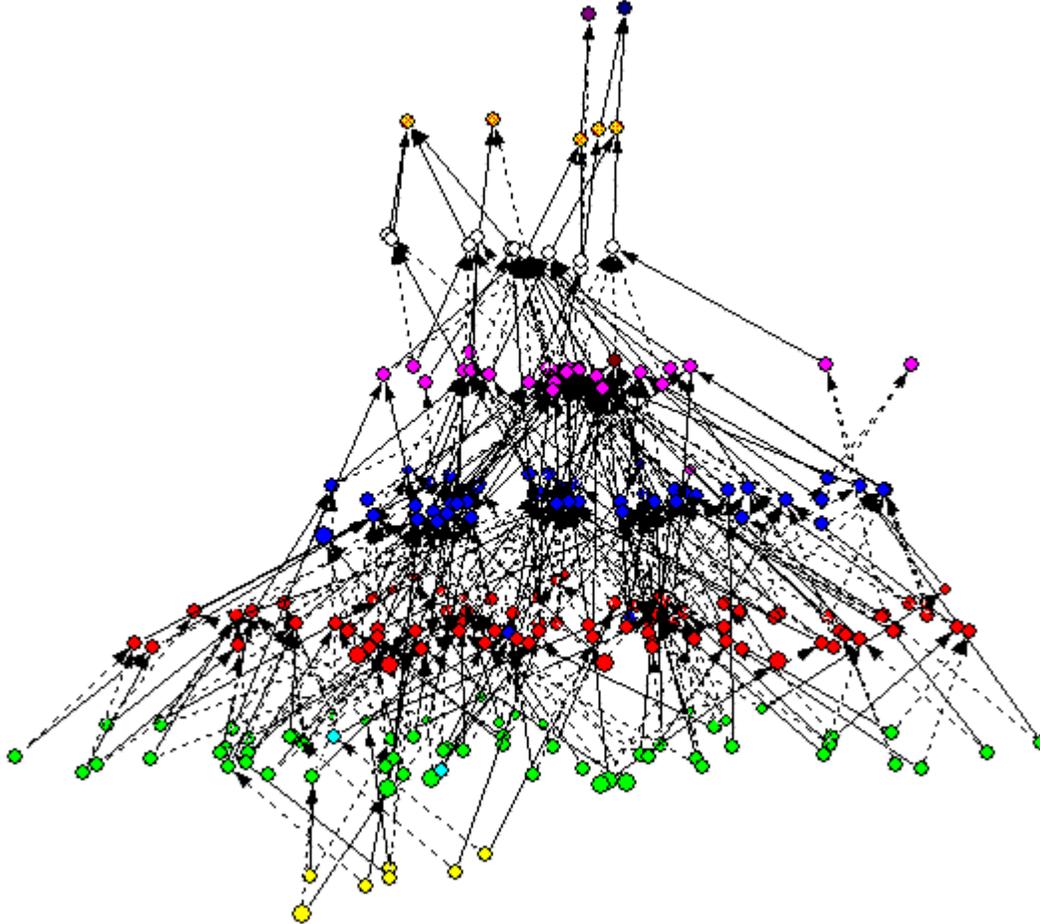
Figure 4.1: A three-dimensional graphic of the relinking marriages among the Aydinli

Johansen: As an ethnographer, I found the term "structural endogamy" (White 1997, Brudner and White 1997) particularly apt for describing the pattern and boundaries of intra-group marriage among the nomads, as defined by relinking. Here was a way to describe what many ethnographers observe about the effects of marriage patterns on social cohesion, while making precise the delimitation of boundaries of cohesive groupings within the society. Detailed results can be found in White and Johansen (2001).

White: A network approach such as ours, focusing on the genealogical network and how other social processes connect to it, is still partial, and a full

ethnography of the Aydinli nomads based on the existing field material would occupy many volumes. The problem of "longer" and "thicker" data generally is especially characteristic of long-term field studies. It is still true, as Hofer (1979) noted for Hungarian ethnography, that present forms of publication could hardly begin to make available all the basic data. For the Aydinli project, however, Johansen and I have begun to make a sample of her photographic collection available as an Internet publication, with each photo dated and annotated in time. We plan to post to the Web the complete genealogies – not for living persons, but for the ancestral generations of Aydinli – as a supplement to our book. In this way, we will overcome a much-neglected consideration in long-term studies: the creation of systematic datasets that can, eventually, become historical data in the public domain.

Johansen: In the photo book I kept for the Aydinli, I annotated the content of every one of about 2,500 photos according to the persons or groups, and the place where the photo was made. Since 1982, I have used some additional tapes for long reports on the nomads. On a daily basis, I wrote field notes, partly in a sort of shorthand, with a list of the signs used in them so that others could later decipher them. Many of these notes were later copied out in full and made into a data catalogue, parts of which are now on computer.

White: Hence, we can expand the concept of network representation to include all kinds of data, including not only diachronic data but also links from individuals, groups, and nodes at other levels of organization, as expressed in both visual and textual data files. Thus, we have the beginnings of suitable computer-based methodologies for longitudinal analysis of field data.

Networks, Migration, Leadership, And Conflict

It is obvious that longitudinal analysis of a kinship and marriage network can yield important insights about marriage choices, marriage rules, and marriage systems (White 1999). Our work shows how the analysis of marital relinking is involved in many of the fundamental structures and processes of the formation, adaptation, and potential dissolution of any society. For the Aydinli of Turkey, our longitudinal network analysis demonstrates that:

1) In the process of clan formation, the relinking of marriages by a limited set of ancestral families created the basis of the cohesion and solidarity of the clan, from the earliest generation to the present.
2) The formation of multiple networks – whose intersections and contending principles define both the multiplex and heterogeneous relationships among individuals – is dynamically involved in the

continual emergence and shifting re-emergence of patterns of social grouping and cohesion. New marital relinkings in each generation have the effect of altering or reconstituting groupings of relatives vis-à-vis others, thereby influencing patterns of migration and leadership coalitions.

3) Shifting network groupings provide an explanation for the Aydinli's use of loosely-structured concepts such as *aile* (for families, extended families, minimal lineages, and larger joint families) and *kabile* (for tribes or larger lineages).

4) The potential dissolution of the clan is reflected by disentanglement, or increasing scarcity of intermarriage (relinking) within the group, involving not just an increase of marriages with other groups, but also a breakup of the structurally endogamous core of Aydinli society.

Presenting the empirical results that support our argument about the centrality of network dynamics to social change and adaptation among the Aydinli is beyond the scope of this chapter (see White and Johansen 2001).

Johansen: One of the advantages of long-term fieldwork was that I became witness to the passing of leadership across three generations, and the competition for leadership among four lineages. Among the Aydinli, leadership is neither hereditary nor formal; there are no formal offices of clan or lineage leaders, only leading *tanidik kisi* – "known persons" – whose influential personalities provide informal leadership to the clan during succeeding generations. Nor is leadership restricted to any one lineage. The dispersion of successive leaders across different lineages is a source of divisiveness, and there are often rivals for leadership in other lineages.

White: One of my first structural observations from the marital relinking calculations was that every one of the *tanidik kisi* over all generations had married so as to relink with the giant cohesive bicomponent of the clan (as in the graph in Figure 1). This led me to investigate the means by which cohesive factions supported each leader and how these factions operated, within the overall bicomponent of the clan, in competing with one another for the passing of leadership from one generation to the next.

Johansen: Nomad leadership, as an activity, consists of group discussion with those who voluntarily come to gather in the tent of a *tanidik kisi*. It includes mediation of disputes, common problem-solving, and coordination of activities. Because there is respect for age and experience, however, it is primarily the older men who discuss in the *tanidik kisi*'s tent, while the younger men listen. A great

amount of time is spent in discussion over monies to be exchanged at marriage, thus showing the importance of marriage bonds among the nomads. The informal councils often consider marriage decisions that add flexible adaptations of reciprocity rules in building social solidarity. For example, if two marriages are arranged simultaneously, such as the exchange of daughters, the payments of bride-money are canceled, and interfamily reciprocity is direct rather than indirect.

White: Every marriage may act as a new relinking that creates or reinforces a locally cohesive subgroup. I developed the hypothesis that cohesive subgroups of the clan and central positions of leadership within the clan are established dynamically by the denser cohesiveness of the subgroups whose kinship and marital relinking provide the basis of their support. The bottom line for clan leadership is that a successful candidate for *tanidik kisi* can have a small but intense following whose members span the clan, or a large and less intense following; but in any case the members of the cohesive set must span the clan and cannot simply occupy a solidarity group with very close ties that excludes other subsets in the clan. Through successive generations, emergent loci of leadership slowly rotate around the clan center as different segments of the clan are articulated to the center. Given a clan with a relatively stable center, each successive leadership faction (after the settlement in eastern pastures ca. 1875) is focused on a different lineage, but also links both to the center of the clan, and reaches out to other subgroups. Rotating around a central axis of clan cohesion, shifts of leadership over time sweep through different segments or factions of the clan to augment overall political connectivity of each successive lineage group (and their closer allies) to both the clan center and its peripheries.

Johansen: Long-term fieldwork also enabled me to observe conflict, expressed through feuding, in relation to cohesion and leadership. I saw four episodes of shootings and ensuing blood-revenge during each of my first three stays before 1964. Feuding had not altogether ceased in 1970, but there were no feuds by 1982. Our network analysis of changes in the kinship basis of political leadership found an unexpected correlation with the decline of feuding. Above all, the rules for feuding respect kinship distance, reckoned first of all by patrilineal connections. The closer the kin involved in potential disputes, the greater is the potential for disruption of local groups, the more serious are the consequences, and the more feuding is avoided and mediation sought in the case of disputes. In contrast, the more distant the relation between the parties, the more severe the feuding and the larger the group mobilized (Boehm 1987). Feuds between two clans or a clan and a village were important up to about 1980 (cf. Johansen 1995),

a time when leadership structure changed away from kinship cohesion and toward education and external political-economic ties as the basis of leadership.

White: In developing our ideas of how the nomads adapted to changing environmental pressures, we took into account elements like feuding and solidarity within a general framework for understanding the co-evolution of social behaviors and social rules that "let people forge new bonds, invent new institutions, and find better ways of doing things" (Postrel 1998:112). Our findings on political factions and social cohesion led me to hypothesize that solidarity cannot be so strong within lineages as to exacerbate rivalries into feuding. There are advantages to exchange, trust, and cooperative relationships between lineages established through marriage (e.g., bride-money payments), and the ensuing cooperation between siblings-in-law. The rule that every woman must be paid for by bride-money does not give lineage members privileged status, but creates an egalitarian basis for creating cooperation and exchange through marriage.

A relinking marriage is a signal of commitment to stay within the nomad group. It equips subsequent children with two parents experienced in the nomad way of life, whose relatives have been members of the group; and thus with a variety of role models for cultural socialization. The cohesion of the nomad groups arises out of relinking, but the bicomponent created by relinking does not act like a magnet nor prevent the children of relinking marriages from leaving the clan. Such decisions are voluntary.

The various kinds of social groupings in nomad society – such as clans, lineages, and cohesive groupings of leaders and their followers – divide the loyalties of individuals, and prevent the bonds of solidarity from becoming overly restrictive. In this context, we used Lindenberg's (1998) framing approach to social solidarity to help show how shifting groups and levels of cohesion operate dynamically within nomad society, with marital relinking as one of the important dynamic operators.

Impact And Benefits Of Long-Term Research

White: One of the major implications of a long-term study is that other researchers or community members can use the data to ask new questions. Although Johansen characterizes her fieldwork as a typical "loner" study, it was the systematic character of the long-term data and genealogies that later attracted me to ask new questions about her materials on social organization, leadership, and the dynamics of adaptation in nomad life ways. And once the data are in a computerized form, researchers from the next generation also can pose new questions or continue the fieldwork.

The use of genealogical methods in our monograph (White and Johansen 2001), for example, provided us with the ability to trace not only the genesis of the nomad clan from its origins in the 18th century, but also the movement of clan and village peoples back and forth as the clan grew in size. Whether this process will lead to the dissolution of the clan as a nomadic society is an ongoing question for analysis.

Johansen: I am excited by the possibility of new questions and new research, although there are other motivations for continuing a long-term study. Like Scudder and Colson (1979:251), I didn't go back to the field only to check on various hypotheses, but also out of concern for the people who had been so generous to me in my studies, and to know what was happening to them as their situations were changing. One of my family members, my adopted son Ralf, went with me on a summer visit and was heartily welcomed, especially in that he was half-Turkish and had a Turkish second name by which he was called, "Yildiray." Ralf's presence changed my status as well: no one referred to me as "girl" after that, although for many younger people I was *hala* ("father's sister") because I was the sister of Dede and his brother Aliboz. Other younger people called me *teyze* ("mother's sister"), the familiar way to address elder women; older people used my first name, Turkicized, with the meaning of "the rosy." Over the years, as many members of the clan quit nomadic life and moved to different places, it became hard to visit old friends. And as many of my colleagues at Ankara University have died, my relations to Turkish colleagues have become more distant in recent years.

I am looked on as a fairly rich old "auntie" by the young people, especially since the Turkish *lire* is low in value and my salary seems astronomical for Turks – and of course I am a sort of relative at least for the Kirbasa and Koca bey lineages. Now, I am a member of the old generation, and have to take care of the young generation in a patriarchal way. I have made contributions many times at weddings and funerals, have helped persons to avert little insolvencies, and even have assisted in buying a car for a crippled young man. But these people have helped me to have success in my life, and so I do whatever I can for them.

My presence and my help have had an impact of the Aydinli and, of course, on the value people place on the many benefits of nomadic life, although it is increasingly difficult these days. Perhaps some of the girls have been inspired to go further in school because of me, and people have been delighted when I bring photos or show them my published articles, especially with photographs and captions. Perhaps their greatest amusement came in 1982 when I brought about 100 slides from my visits in 1956, 1957, and 1964; which I showed to members of the clan by means of a battery-operated projector. Ralf ran a tape recorder, and I wrote down the commentary on the pictures. A great many came to the tents in the

evenings when I showed the pictures. Everybody wanted to see them again and again, and most discussions went on about identification of people in this way: "Look, there is Crazy Ahmet" (the use of nicknames is common). "No, this cannot be Crazy Ahmet. Crazy Ahmet never wore such a coat." "But it is Crazy Ahmet, only he was much younger then." And so on.

My general questions about the difference in life between then and now were disregarded in all the fun, because people were so delighted to see themselves or their late relatives in the pictures. My plans for this approach to studying culture change had to be abandoned. Who was I to impose, the friendly and dear old auntie, when the culture itself takes over?

White: Johansen's story illustrates beautifully that methods also have an experimental character, and that well-laid plans can go awry. Serendipity often plays a beneficial role in research (Foster 1979:182), as with the creation of the genealogical scroll: who would have expected that it would give new results via network analysis? I would emphasize as well that, like all research results, ours are provisional. Nowhere is the provisional character of research more evident than in long-term research, where the passage of even a few years can lead to a change in perspective on what is and has been happening in people's lives (Meggitt 1979:116-122). On the one hand, Kemper (1979:206) has noted the benefits of long-term research: "Long after today's theoretical fads and methodological innovations have been tossed aside, the data collected carefully and patiently over several decades in key ethnographic settings will continue to provide a basis for testing new ideas. On the other hand, Lamphere (1979:42) has argued that "Anthropology has been characterized by major shifts in theoretical interest from one decade to the next, so that despite increasingly sophisticated theory and concomitant methodology, data collected one decade may be of limited use to the next generation of researchers . . . we need to avoid this and consider what kinds of data will be most useful 10 to 20 years after a project has been started." With today's changing theoretical interests, changing technologies, and rapid turnover of ideas and methodologies, long-term research projects continue to invite new research strategies.

NOTES
1. There is some selection bias here since it is the ancestors of those living in the 1950s who are remembered in this genealogy, and some ancestors who were members of the early clan in its new location may not have left descendants. But since residence is strictly virilocal, and only females marry in, the memory of ancestry within the clan itself is likely to be highly accurate, especially since it is reconstructed from multiple sources.

2. The founder of "the genealogical method" in anthropology, W. H. R. Rivers, moored his advocacy of genealogical studies in the idea that actual behaviors, such as those reflected in the events of marriage practices, would be consonant with symbolic forms in language, such as kinship terminology.

References Cited

Batagelj, Vladimir, and Andrej Mrvar
(1997) *Networks / Pajek: Program for Large Networks Analysis*. University of Ljublana, Slovenia.

Boehm, Christopher
(1987) *Blood Revenge: The Enactment and Management of Conflict in Montenegro and Other Tribal Societies.* Second Edition. Philadelphia: University of Pennsylvania Press Publications.

Brudner, L. A., and D. R. White
(1997) "Class, poverty, and structural endogamy: Visualizing networked histories." *Theory and Society* 26:161-208.

Cernea, Michael
(1996) "Social organization and development anthropology: the 1995 Malinowski award lecture." *Environmentally Sustainable Development Studies and Monographs Series,* No. 6. Washington, D.C.: World Bank.

Clark, Samuel, Elizabeth Colson, James Lee, and Thayer Scudder
(1995) "Ten thousand Tonga: A longitudinal anthropological study of southern Zambia: 1956-1991." *Population Studies* 49:91-109.

Epstein, A. L. [editor]
(1967) *The Craft of Social Anthropology*. London: Tavistock.

Epstein, T. Scarlett
(1979) "Mysore villages revisited." Pp. 209-226 in George M. Foster, Elizabeth Colson, Thayer Scudder, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology*. New York: Academic Press.

Foster, George M.
(1979) "Fieldwork in Tzintzuntzan: The first thirty years." Pp. 165-184 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V.

Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Foster, George M., Thayer Scudder, Elizabeth Colson, and Robert V. Kemper [editors] (1979) *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Hofer, Tamás
    (1979) "Hungarian ethnographers in a Hungarian village." Pp. 85-102 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Johansen, Ulla
    (1965) "Die Nomadenzelle Südost-Anatoliens." *Bustan* 2:33-37.
    (1994) "Feldenfahrungen bei den Aydinli." Pp. 27-40 in Albert Kunze (editor), *Yürük: Nomadenleben in der Türkei.* Munich: Trickster Verlag.
    (1995) "Regeln der Blutrache." Pp. 401-420 in D. Schorkowitz (editor), *Wege und Lehrjahre eines Philosophen: Festschrift für Lawrence Krader zum 75. Geburtstag.* Peter Lang.

Kemper, Robert V.
    (1979) "Fieldwork among Tzintzuntzan migrants in Mexico City: Retrospect and prospect." Pp. 189-208 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Lamphere, Louise
    (1979) "The long-term study among the Navajo." Pp. 19-44 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Lindenberg, Siegwart
    (1998) "The microfoundations of solidarity: A framing approach." Pp. 61-112 in Patrick Doreian and Thomas Fararo (editors), *The Problem of Solidarity: Theories and Models.* Amsterdam: Gordon and Breach Publishers.

Meggitt, M. J.
    (1979) "Reflections occasioned by continuing anthropological field research among the Enga of Papua New Guinea." Pp. 107-126 in George M. Foster,

Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Moran, Emilio F.
(1995) *The Comparative Analysis of Human Societies: Towards Common Standards for Data Collecting and Reporting.* Boulder, CO: Lynne Rienner.

Pospisil, Leopold
(1979) "The Tirolean peasants of Obernberg: A study in long-term research." Pp. 127-144 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Postrel, Virginia
(1998) *The Future and Its Enemies: The Growing Conflict over Creativity, Enterprise, and Progress.* Chicago: The Free Press.

Rivers, W. H. R.
(1910) "The genealogical method of anthropological enquiry." *Sociological Review* 3:227-234.

Schneider, David M.
(1984) *Critique of the Study of Kinship.* Ann Arbor: University of Michigan.

Scudder, Thayer and Elizabeth Colson
(1979) "Long-term research in Gwembe Valley, Zambia." Pp. 227-254 in George M. Foster, Thayer Scudder, Elizabeth Colson, and Robert V. Kemper (editors), *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Sümer, Faruk
(1967) "Kara Koyunlular." *Türk Tarih Kurumu Yayinlarindan* VII, 49. Ankara.

White, Douglas R.
(1997) "Structural endogamy and the graphe de parenté." *Informatique, Mathématique et Sciences Humaines* 137:107-125.
(1999) "Controlled simulation of marriage systems." *Journal of Artificial Societies and Social Simulation* 2(3). http://www.soc.surrey.ac.uk/ JASSS/ JASSS.html

White, Douglas R., Vladimir Batagelj, and Andrej Mrvar
(1999) "Analyzing large kinship and marriage networks with Pgraph and Pajek." *Social Science Computer Revie*w 17(3):245-274.

White, Douglas R. and Paul Jorion
(1992) "Representing and analyzing kinship: A new approach." *Current Anthropolog*y 33:454-462.

White, Douglas R. and Ulla Johansen
(2001) *An Introduction to Network Analysis of Genealogy and Politics: Social Dynamics in a Nomadic Society*: Walnut Creek, CA: AltaMira Press.

White, Douglas R., M. Schnegg, L. A. Brudner, and H. Nutini
(2002) "Conectividad múltiple y sus fronteras de integración: Parentesco y compadrazgo en Tlaxcala rural," in Jorge Gil and Samuel Schmidt (editors) *Redes Sociales: Teoría y Aplicaciones.* México, D.F.: Universidad Nacional Autónoma de México.

White, Douglas R., Thayer Scudder, and Elizabeth Colson
(1998) NSF Proposal (funded): "Long term dynamic assessment of impacts of world-system, development and ecological processes on local socioeconomic change in Gwembe Valley, Zambia 1988-90."