

Anthropology, History, and the European Joint Family: Reflections on Modes of Research

Recent research on European social structures has made it abundantly clear that historical data about them frequently resemble the data employed in social-scientific disciplines such as demography, sociology, and social and cultural anthropology. There is no absolute identity, of course, because historical sources, by definition, are created by people who are not the ultimate analysts whereas social science data sources are, in a sense, created by social scientists themselves. Yet there is sufficient similarity between the two for there to have emerged such subspecialties as historical demography, historical sociology, and historical anthropology (Wrigley, 1969: 8:28; Imhof, 1975; Laslett, 1977: 1-11; Macfarlane, 1970). No final judgement on these new interdisciplinary fields is as yet possible because, as has been argued, their development is still in a beginning phase (Burke, 1980). Still, in some specializations the progress of work has been rapid enough for a „state of the art“ assessment, and the history of the European family is certainly one of these (Wrigley, 1977). The question I will deal with in the following pages, from the vantage point of an historian, is therefore somewhat narrower than the whole field of historical social-structural investigations; and it is rendered even more narrow by my intention to focus on the ways in which anthropological researchers in English-speaking lands are now formulating the question of the European joint family. Even such a limited review can be instructive, however. An understanding of the lines along which researchers are currently working to establish what the European joint family has been, what it is now, and what further questions have to be raised in order to deepen our knowledge suggests much about not only the joint family as such but also about interdisciplinary currents which have by no means run their course. At some point in the future such a review will have to be repeated, because many of the studies I use as examples are themselves far from being completed.

The formulations which need to be described appear in the writings of those few anthropologists who have turned their attention to evidence about joint families which does not originate in surveys or in participant fieldwork. This evidence comes from nominal entries in hundreds of population enumerations and registers of vital events from different areas of the European continent from the medieval centuries onward. The form in which this evidence frequently presents itself to the researcher of family life resembles closely some of the configurations into which anthropological fieldworkers assemble information gathered about living individuals. There are notations about when an individual was born and married and when he or she died, as well as about the people with whom he or her lived. There is also information about social standing and occupation and, sometimes, about the transactions in which the individual was engaged. Such information is frequently available about all members of an historical community. Some fifteen years

of archival searches throughout the European continent have identified literally hundreds of usable archival sources of this kind, and the efforts spent in their analysis have already produced a rich corpus of methods (Laslett, 1966; Wrigley, 1966). The most important point for our purposes is that this substantial collection of primary and secondary sources deals with individuals living in the near and distant past and therefore, at least in principle, offers a better grounding for a *history* of family life than do the quasi-ethnographic observations of travelers, clergymen, and officials who were relied upon so frequently in earlier writings on the subject. With such sources researchers are now in the position to reconstruct familial configurations from their constituent elements, to classify such configuration by means of schemes of their own devising, and to formulate hypotheses on the basis of such classifications. Moreover, this evidence is available not only about the elites of different historical societies but also about common people, which fact makes it unnecessary for the researcher to make inferences about everyone from the lives of the few people who were constantly in the public eye.

It would not be correct to say that all areas of the European continent have received equal attention in the work done to date. Indeed there is only one country — England — in which the new sources have been explored so thoroughly that the question of structural variability over space and time can be dealt with adequately; and only a few others — France and Scandinavia, for example — that could be placed second according to such a standard (Wrigley and Schofield, 1981; Soliday, 1980; Åkerman, Johansen, and Gaunt, 1978). The standard which requires accounting for variability is, to be sure, a demanding one, yet it is now obvious that it has to be met. We now know enough about what sampling is and what traps historical evidence sets in this respect to hold back generalizations, especially about the past, until at least the first requirements of drawing a sample have been satisfied (Schofield, 1972). In light of these requirements, the conclusion is inevitable that those areas of historical Europe in which simpler forms of family life predominated can be spoken about far more authoritatively than those in which complex forms, including the joint family, appear to have been significant. The best example for illustrating this point is the Russian Empire. Initial researches on familial forms in pre-revolutionary Russia have demonstrated that in a number of localities in this vast ethnically heterogeneous land joint families were indeed the dominant form (Czap, 1982; Plakans, 1975). Yet a closer look at this research will also reveal that regardless of how useful local materials have been for arriving at an understanding of the *dynamics* of joint family life in the localities where it existed, we are not yet in a position to speak with certainty about the spatial distribution of the joint-family type at any period of the past nor about shifts in the relative importance of all represented family forms over the long course of time. The researched areas

of the Russian Empire include a handful of serf estates in Central Russia in the eighteenth and nineteenth centuries, and a small selection of similar estates from the highly untypical Baltic provinces. Yet the source from which the empirical data for these researches come – the head-tax censuses or soul-revisions – exists for almost the entirety of the Russian territories from the early eighteenth century to 1858. The Russian example is a sobering one as far as the question of representativeness is concerned, not only because of the dimensions of the evidence which has not yet been explored but also because, in the Baltic case, the evidence has shown the preponderance of the joint family form in an area which traditionally was not known to contain it.

I have underlined the problem of variation in order to suggest that simultaneously with the uncovering of new usable empirical evidence about the history of European family forms has come a set of new attitudes concerning what the evidence does or does not permit us to say at this time about the European familial past, and concerning the language in which permissible statements have to be made. These factors are important in any discussion of the role anthropologists have played in reformulating the question of the joint family. As Milovan Gavazzi and Micheal Mitterauer have pointed out, we now have a much better sense of where in the past of the European peoples the joint family form can be said to have made an appearance, of what range of definitions exists for classifying families either as joint or not joint, and of what is involved in the study of the dynamics of joint family life in those areas where evidence reveals their presence (Gavazzi, 1979/80; Mitterauer, 1981). The scholarly literature they have used to establish these points clearly shows that in the writings about the history of the joint family since the mid-1960's the names of researchers from historical and anthropological disciplines are very prominent. For historians to be involved in what is essentially an undertaking concerning the European past is not particularly surprising, but the presence of anthropologists in these bibliographies requires further exploration. The history of the anthropological disciplines in the twentieth century would not lead to a prediction that their practitioners would turn to an extensive analysis of historical evidence, or indeed that they would come to play a major role in the formulation of questions about the *past* of European social institutions (Kuper, 1973).

Generally speaking the attention which Anglo-American anthropologists recently have come to pay to historical data constitutes something of a *return* by them to questions about the past. Anthropology in the nineteenth century, and in the decades immediately preceding World War I, raised many questions about the historical origins of the social structures being found among non-European and European peoples, about the development of these structures in the course of time, and about the replacement of struc-

tures characteristic of one era by those characteristic of the next one. The answers provided to such question, however, proved entirely unsatisfactory to the generation that dominated the discipline in the interwar and the immediate post-World War II years. The English anthropologist A.R. Radcliffe-Brown, among others, charged that anthropologists were making statements about the history of social-structural forms without having anything like sufficient evidence for them and therefore characterized these efforts as „conjectural history“ (Radcliffe-Brown, 1965, edition of work first published in 1952). History, according to Radcliffe-Brown,

shows us how certain events or changes in the past have led to certain others events or conditions, and thus reveals human life in a particular region of the world as a chain of connected happenings. But it can do so only when there is direct evidence for the preceding and succeeding events or conditions and also some actual evidence of their interconnection (Radcliffe-Brown, 1965 :60).

Anthropologists, in this view, had very little actual proof of any of these matters. Such a critique was wholly warranted, at least for the evidentiary side of the issue. Yet, having been made, and having helped to initiate a strict synchronic analysis of fieldwork data, the critique left unanswered the question of what historical evidence should be like to be acceptable as a basis for a „non-conjectural“ history of social structures. Would any historical evidence be admissible which did not look precisely like a „chain of connected happenings“? The emphasis on synchronic interpretation of field data appeared to be leaving the job of writing the history of social structure to historians or, possibly, to anthropologists who wanted to incorporate into their descriptions the short-term historical „background“ of the communities about which they were writing. As retrospective accounts of the development of anthropology in the twentieth century have shown, however, this strict position was not able to command the absolute allegiance of all anthropologists; and, simultaneously with the elaboration of the synchronist position, anthropologists, even in England, continued to raise questions of an historical nature, though of a more limited sort than had been raised in the nineteenth century (Lewis, 1968).

In the decades immediately following World War II some prominent anthropologists put forward such ideas as that „social anthropology is a kind of historiography“ (Evans-Pritchard, 1961) and that „anthropology and history share the same subject, which is social life; the same goal, which is a better understanding of man; and in fact the same method, in which only the proportion of research technique varies“ (Levi-Strauss, 1963:18). The distance between the intellectual work involved in the two disciplines appeared in these perceptions to be minimal. And, what is more, there was in these statements more than just a suggestion that the evidence with which historians

and anthropologists work could be thought of as essentially the same. The criss-cross of influences and inspirations of these years is still very difficult to unravel, but there is no doubt that by the 1960's a number of Anglo-American anthropologists were ready to start assessing the meaning of historical evidence when it had to do with social microstructures, even if the evidence came from the distant past. The most prominent interdisciplinary publication on the history of the family to appear at the end of this period — Laslett and Wall's *Household and Family In Past Time* — included among its twenty-three contributors five who were associated with anthropology departments (Laslett and Wall, 1972). The interdisciplinary mood exemplified by this work has continued during the decade since its publication. Anthropologists now routinely employ in their research what in the past might have been regarded as purely historical sources (e.g. Netting, 1979; 1981); they perceive the social processes exhibited in historical documents from as far back as six hundred years as having theoretical significance (e.g. Hammel, 1980); and they tend, by and large, to evaluate the future possibilities of historical information positively:

Historical material enables us to trace thousands of individuals rather than tens, and to follow them through their whole lives in a number of cases. Both in terms of quality and quantity there is much for a sociologist or social anthropologist to envy, though a historian will also envy the sociologist's ability to ask questions, to create his own data (Macfarlane, 1977: 202).

We might also note, without examining the point in detail, that historians, on their side, have become highly conscious of the fact that an alliance with such disciplines as sociology and social anthropology alters significantly the way they perceive their own tasks and view their evidence. In his broad survey of the state of the historical professions in the modern world, the historian Geoffrey Barraclough discusses these changes in the historian's outlook under twenty-one separate headings, of which two are of particular importance to our present concerns. „Anthropologists and sociologists“, Barraclough maintains, „have demonstrated that the alleged ‚uniqueness‘ of historical facts, which is supposed to make the scientific study of history impracticable (and if practiced, misleading), is a myth“ (Barraclough, 1978: 53). Moreover, he continues:

the anthropologist is concerned first and foremost with domestic and community relations, with family, kinship, law, and other determinants of social conduct (e.g. taboo); the sociologist is concerned mainly with the normative structure of contemporary society . . . But these or similar institutions and relationships manifestly played an equally important role in the societies about which historians write. Why, then, should they be left to sociologists and anthropologists? The simple answer is that traditional historical methods provided no satisfactory techniques for dealing with them. The importance of anthropology and sociology is, therefore, not merely that they directed the historian's attention to such matters, but also that they indicated how they could be handled and evaluated (Barraclough, 1978: 54-55).

These developments make it easier to understand why the new anthropological view of the joint family problem has not been simply the product of methodological changes *within* the anthropological disciplines, but rather the result of interaction, at the level of data analysis, between historians and anthropologists. On the one hand, historians interested in the familial past could not proceed very far with manifestly relevant data unless they used social-scientific approaches to them; on the other, anthropologists, now favorably inclined toward the study of the past, saw in the evidence being uncovered by historians the possibility of writings some form of „non-conjectural history“ of the social microstructures in which they were interested. The result has been a kind of writing about the past to which the terms „anthropological history“ and „historical anthropology“ could equally apply, depending upon how the researcher has set up the problem and upon whether or not an effort is made in the analysis to actually study the changes undergone over time by a community, population, or institution. Actually, the distinction between anthropological history and historical anthropology is a difficult one to make at the present time, nor is it of major importance that the distinction be drawn sharply. It is useful, however, that something like this distinction be kept in mind. While historical anthropology, in its simplest variants, may mean simply that a body of data from a moment in the past is being analyzed primarily through the use of anthropological concepts; anthropological history, to be properly so called, has to retain in a central position the effort to describe change in the long term. This is a major challenge, because neither anthropologists nor historians themselves have succeeded in spelling out what sequences one needs to look at when, for example, the object of the endeavor is to describe change over time of such microstructures as families, households, and kinship groups. Are the important considerations the structure, composition, and size of such groups and how these change over time; or should one instead keep an eye on the changing experiences of the persons comprising such groups? In order to discuss change over time, is it sufficient to have information about such groups at several points in past time, with „changes“ inferred from comparisons among these points; or is it necessary to have a running record – Radcliffe-Brown’s „chain of connected happenings“ – covering all moments of a structure’s existence before the claim can be made that the history of the structure has been described? The present consensus on this question, formed no doubt on the basis of what has become known about the attributes of historical data, is to treat all studies of this general type as being equally useful as long as they are reliable and informative.

To try to isolate anthropological writings on the history of the joint family from this general context is unproductive; it could even be misleading if taken to mean that the joint family was a problem only in those areas of traditional Europe on which recent anthropological work has focused. It has

now become clear that the family form which we classify as „joint“ – that is, involving two or more kin-related nuclear families (Gavazzi, 1979/80: footnote 1) – appears with greater or lesser frequency in all European areas which have been investigated to date. I hasten to add, of course, that this does not mean that the joint family was a *dominant* form in all these areas. But we do have a range of frequencies running from the less than 5 % found in early modern England and France to the 80 % found in central Russia in the nineteenth century (Laslett and Wall, 1972:85; Czap, 1982). Findings such as this, ranging over the map of Europe as well as over a high to low numerical scale, have meant that anthropologists researching a joint-family locality have had to remind themselves to conduct their discussion and present their data with due recognition of the comparative dimension. In doing so, they have produced analyses that differ significantly from both traditional ethnographic descriptions as well as from traditional historical accounts of family life. The shifts, or reformulations, have operated in three main ways.

The most readily identifiable of these shifts involves quantitative evidence, especially of the demographic kind. The historical data which in the 1960's came to be seen as relevant to a historical understanding of European family life took the form of census-like enumerations, often with rich biographical information about each listed person. Such information had not been considered of great significance in the fieldwork tradition, as the anthropologists Alan C. Swedlund and George J. Armelagos point out:

Twenty years ago the number of demographic studies in anthropology was very small. In most cases, early analyses of demographic data were cursory and done without serious consideration of their own ultimate utility. As often admitted by anthropologists, demographic statistics (a census or house-count) were collected as „busy work“ until the anthropologist had gained the trust and rapport necessary with the subject group to study the actual problem desired. These basic population counts would then appear in the appendix of a monograph on the culture, with little effort to integrate demography into the analysis of the primary problems studied (Swedlund and Armelagos, 1976:18).

The historical data coming to the fore in the 1960's thus required that the statistics concerning population and social structure be brought from appendices of monographic surveys forward to a central place; otherwise, it could not be given the attention necessary to make it into usable data. Moreover, anthropologists had to become familiar with the techniques of numerical analyses and had to assent to the premise that usable „facts“ about social structure and demographic development could be obtained from numbers standing alone. Then there was also the matter of data selection. More often than not, historical data of the new kind presented itself to the analyst as data about entire communities (villages, manors, etc.): all individuals of an historical community would appear in an enumeration, not only the few

who were living in certain kinds of coresidential structures or who occupied a certain kind of status. In approaching such a collection, anthropologists had to consider individuals, and configurations of individuals, within a local comparative framework and had to give „negative cases“ (if the joint family was being studied) as much attention as „positive cases“. Correspondingly, researchers working on European areas where familial complexity had not figured as a cultural attribute had to explain, in their data, the appearance of joint or extended structures. In such collections it would have been an arbitrary decision to select only those configurations for analysis which, on first glance, seemed to be appropriate to the researcher's main concerns.

The use of quantification methods has now been generally accepted among anthropologists and they are normally portrayed in a positive light: „Quantitative measurement can increase the effectiveness of anthropological description by increasing reliability; increasing comparability; retaining negative cases; expressing intracultural diversity; increasing the precision of theoretical propositions; and increasing the power of statistical tests“ (Johnson, 1978:43). I cannot describe in detail how each of these suggested improvements in method has helped research on the history of the European joint family, but certain examples stand out. I have already mentioned that this approach imposes upon the researcher an obligation to consider the significance of negative cases; and also that the preparation of data for quantitative analysis has led to the identification of joint family formations in areas where they had not been known to exist, thus helping to lay out the evidence for „intracultural diversity“. Johnson's point concerning increased precision of theoretical propositions can be illustrated in the following fashion. Throughout recent work done by anthropologists on historical data there has been a noticeable tendency to eschew flat descriptive statements of the type „Among the NN in the nineteenth century the joint-family form was widely diffused“ in preference for statements which are still descriptive but seek to include all the information alternative ways of quantitative assessment of the data have yielded. The concern is now to show how a particular kind of institution, while appearing very important from one point of view, can appear otherwise when looked at from another. Thus the anthropologist Joel M. Halpern has reviewed in detail the nominal-level data from the Serbian census of 1863 in order to „give attention . . . to the specifics of size and kinship composition“ of the famed *zadruga* in the past (Halpern, 1972:401). He investigated the appropriate historical statistics from eight villages and one emergent market town and arrived at the conclusion that „the complex kin relationships which characterized the *zadruga* were ordinarily participated in at any one time by less than half of the population (p. 408)“. In this analysis a simple reversal of the usual perspective – a look at the family household not from the viewpoint of family form but from the life experience of participants in coresidence – produced a descriptive statement of an un-

expected sort. After this study, and other studies of this type, the question is pertinent as to what kinds of descriptions should enter into the formulation of a theory of the joint family in the past. One proposal, made by Peter Laslett, is to consider „a form of the coresident domestic group . . . to be *the* familial institution of a society in permanency over time if a sufficient number of persons being born into that society are being continuously socialised within domestic groups of the structure nominated“ (Laslett and Wall, 1972: 65). Whether this standard is the appropriate one or not can be debated, but what is absolutely clear is that the testing of theoretical proposition of this type will have to take place by means of quantitative data. Without an effort to obtain numerical data that allow the analyst to examine descriptive statements of contemporary observers or of later researchers it becomes impossible to assess the truth-content of such descriptions and their relative importance for theory-building.

An example of how quantitative evidence enables researchers to use powerful statistical tests to weigh the relative significance of pertinent variables can be found in the attempt by Eugene A. Hammel and Djordje Šoć to establish „what lies behind the relative strength of lineages in an area“ (Hammel and Šoć, 1973: 812). The data they used came from southern and eastern Yugoslavia and were contained in a collection of thirteen ethnographic studies of the origins and migrations of modern Yugoslavian peasants. The information in the studies could be used, however, to explore the problem of why certain local lineages encompassed a greater number of individual households than others. Using regression analysis and partial correlations Hammel and Šoć demonstrated that „the variables most closely associated with the mechanics of the cycle of lineage development are time depth and fertility . . . if you want to know what lies behind the relative strength of lineages in an area, the first question to ask is how long they have been there, and the second is how fast they have been growing“ (1973: 811-12). In this study it was the lineage rather than the family household that was the focus, but the manner in which the problem was set up contains a lesson for familial analysis. The inclusion in the analyzable data file of quantitative information concerning duration and fertility (history and demography) made for a more powerful test than would have been possible had only cultural and economic attributes been used. For our purposes here the conclusions are less important than the procedure. An approach which set up the problem by using variables common to traditional discussions of the question as well as variables of a relatively new kind transformed the question itself. The experience with lineages is clearly a warning for family analysts.

The second area in which questions concerning the history of joint families have undergone reformulation is that of short-term familial processes. As far as the *history* of European familial forms is concerned, the idea that a familial

configuration reported in a particular historical record should be conceptualized in terms of „process“ was put forward in a forceful and convincing manner by several historians and anthropologists roughly at the same time, although, if we were after an accurate pedigree for this way of thinking we would need to go back as far perhaps as the anthropologist Meyer Fortes' essay, first published in 1949, on „Time and Social Structure: An Ashanti Case Study“ (Fortes, 1970:1-32), and even to earlier anthropological writings. How analysis of this kind could be carried out on European historical records was demonstrated effectively in the historian Lutz Berkner's often-cited essay on the stem family in the eighteenth-century community of Heidenreichstein in Austria (Berkner, 1972). In this, Berkner drew upon a collection of essays on the developmental cycle of the domestic group, edited by the Cambridge anthropologist Jack Goody in 1971 (Goody, 1971). The subject of the developmental cycle of the household was discussed at length by Laslett in *Household and Family In Past Time* (1972:32-34 and passim); and in that same volume the anthropologist Eugene A. Hammel argued that the South Slav *zadruga* can best be understood if thought about in terms of process: „the surface structures of social forms are the results of an interplay of conflicting forces on fundamental processes; a social institution is not its end products but rather the procedural rules or principles that generate those products under varying constraint“ (Hammel, 1972:373). How this approach could be used with historical data was demonstrated by Hammel through close analysis of household listings from fourteenth, nineteenth, and twentieth century Serbia. Since the first statements of these ideas a decade ago, few analyses of historical familial materials have assumed that single-year censuses as such present sufficient evidence for the discussion to proceed *directly* to the creation of a typology of family forms in a given locality. It is now well understood that the structural forms in such material have „frozen“ familial processes which operated before the census was taken and continued to work their way afterwards. Or to use the theoretical formulation of the anthropologist S.H. Nadel: „Social structure is implicitly an event-structure . . . the time dimension is not only implicit in the social structure, but constitutes an explicit condition of it. Both have to do with the recruitment of people into roles, relationships, and groups so-and-so structured, that is, with the mechanics of intake and circulation“ (Nadel, 1957:128-29). The recognition by anthropologists that historical data, when manipulated, permit roughly the same kind of diachronic analysis that is performed with fieldwork data from living populations has opened new possibilities while at the same time underlining the dangers of literal interpretations.

Admittedly the introduction of the idea of „process“ into historical analysis has rendered the job of description far more difficult, not only for anthropologists working with historical sources but also for historians analyzing European joint family areas. Thus for example, the historian Peter Czap, whose

expertise lies in the analysis of historical-demographic data from the Russian province of Riazan in the eighteenth and nineteenth centuries, has found it advantageous to deal with such processes as marriage, childbearing, and household formation separately, though the material used in each kind of analysis comes from the same data base (Czap, 1978, 1982). This strategy is adopted, with data bases of this magnitude and detail, not because the processes are hypothesized to be operating independently of each other, but because of the complexity of each. By avoiding synthesis in the early stage of analysis of a data base, the strategy permits the researcher to move to the forefront a series of questions which are quite different from those that loomed large in the thinking of an earlier generation of writers. If earlier researchers dealt with the questions of how „typical“ family formations in a particular Russian community were related to overall patterns of socio-economic change, how these formations were representative of a certain stage of societal evolution, and whether the „joint-family regime“ was in the course of time transformed into a „simple-family regime“, — questions which dealt with macro-processes —, recent approaches have found it more informative to start with microprocessual problems and to postpone asking of „larger“ questions until a better understanding is achieved of the nominal-level empirical evidence. It might be noted that this order of priorities (in this case involving an historian working with a joint-family area) resembles that which is followed in many anthropological monographs dealing with community analysis. The progression is from micro-structures and micro-processes to more inclusive structures and processes; in the present case, however, we are talking about analyses which though constituting a series may be separated from each other (from the viewpoint of publication) by relatively long spans of time.

The experience of the last decade has shown that there is every reason for circumspection in the attempts to understand and describe processes at this level. To stay with the Russian example, in the Riazan serf estates analyzed by Czap, a high proportion of joint family households appears to go together with very early ages at first marriage for both men and women. In the Russian Baltic province of Kurland, however, ages at first marriage of both women and men were normally higher than in the Russian case by some four to five years. Yet in Kurland, complexity levels in coresidential groups varied greatly, in some cases almost reaching the very high levels exhibited by the Riazan evidence and in others reaching the low levels of Western communities (Plakans 1975). Moreover, in the adjoining Baltic province of Estland, ages at first marriage, though similar to those of Kurland, seem to have been accompanied by uniformly low levels of household complexity and household size (Palli, 1980). In the Russian case, it is relatively clear that the shorter time between marriages in the same male line created demographic conditions favoring household complexity: the microprocesses of family formation and

household formation were thus linked. Yet in the Baltic cases no such clear picture emerges. These cases of different interactions among microprocesses — all within a broad geographical area traditionally associated with family complexity — raise the question of what, ultimately, should be the characterization of the area as a whole. Should it be dealt with as a political unit or subdivided into ethnic units? Should we accept the proposal of Peter Laslett that the Baltic provinces belong to an 'intermediary' area of European family history, that is, to an area with its own particular characteristics; or should we see the Baltic provinces historically as some sort of periphery to an area which has central Russia as its core? The timing of events within microprocesses, and the timing of the moments when microprocesses intertwine with each other, has not been worked out for a sufficiently large number of areas within this large encompassing purportedly joint-family region for these questions to be answered with any degree of certainty at this time.

The quantitative approach to microprocesses in the European familial past has led, among anthropologists, to a noticeable amount of hesitation regarding the ultimate purposes of their work in the historical realm. This then is our third, and final area of concern. The problem goes deeper than the question of whether in their use of historical data anthropologists should be after „rules of human behavior“, for that goal can be realized readily enough in the mode of research I earlier designated as historical anthropology. That this can be done with effect and elegance has been demonstrated in such works as Alan Macfarlane's study of the kinship relations of a seventeenth-century English clergyman (explicitly designated by him as „an essay in anthropological history“), and many other works cited above (Macfarlane, 1970). Still, when this and similar studies are considered as a corpus, it almost seems as if anthropologists working with historical sources have adopted the position of S.H. Nadel, who maintained, toward the end of his theoretical statement of what social-structural analysis should be, that

what makes structural analysis really information . . . is not the final positional picture at all, but the steps that lead to it. Our gain lies in the application of the appropriate analytical methods, not in the gathering together, schematically, the results. For it is in the course of this application that we achieve a penetrating insight into the workings of society. Every step in the many abstractions and comparisons we have to make reveals crucial interdependencies — between individuals in their roles, between the roles and the rest of society, and between groups built out of roles (Nadel, 1957: 154).

It would be difficult to fault this statement of goals for any compartment of historical familial study, including that of joint families, for far more is not known than is known. Even studies which deemphasize the significance of ultimate findings, or make them very tentative, will be informative if well-

conceived and well done. There are, however, indications that for some anthropologists the question of what „non-conjectural history“ of social structures is has become very significant. We can find the start of an answer to this question in the work of the Cambridge anthropologist Jack Goody and in his reconsideration of the value of the approaches taken to social structure by nineteenth-century evolutionary anthropologists. These approaches, Goody argues, intended to deal with the problem of long-term change in human societies; and though the intention was not fulfilled in ways that were acceptable to later generations, these results should not be allowed to permanently discredit the question itself. The „long-term“ should be revived, Goody continues, but the investigations that accompany it should be carried out by means of modern statistical methods:

I argue that the approach I employ is not only respectable in its forbears, general in its interest, but that it is of value even for structural or functional analyses of particular societies, as well as for the more general theory on which these studies are based. Any human institution is best understood if one can examine not only its meaning and function in a particular society but its distribution in space and time. I do not mean to substitute one approach for another, but to bring back another dimension, whose elimination from our analytic repertoire impoverishes not only the total effort toward explanation but also the individual approaches themselves . . . (Goody, 1976:2).

Goody is speaking here less about particular family forms and their history than about the investigation of all aspects to social life and their correlations. His work focuses on questions of concubinage, heirship, adoption — all aspects of what he calls the ‚domestic domain‘ — and therefore the question he raises is a significant one for the subject we are dealing with here. In order to introduce the long-term into the discussion, Goody uses evidence from the *Ethnographic Atlas* prepared by Murdock (Murdock, 1967), a form of comparative analysis, and „linkeage and path analysis in an attempt to test possible lines of development“ (Goody, 1976:8). His comparisons are made among social institutions and practices on different continents in order to see whether, over time, there are correlations between different ways of transmitting property and other aspects of society. Because his range of evidence is so wide, his hypotheses are correspondingly all-embracing and perhaps somewhat more general than historians would be comfortable with. Nonetheless, the effort presents a challenge to the belief that the only way the past can be used by anthropologists is for the purposes of historical anthropology and not for the purposes of anthropological history. The challenge lies precisely in Goody’s effort to ‚introduce a sequential perspective in . . . cross-sectional studies‘; or, to put in another way, to show that it is possible to deal with ‚the long term‘ while being cognizant of short-term processes, and to analyze both in a quantitative fashion.

Whether an approach of this kind is possible in the more limited area of European family history, or in the even more limited domain of the history of the European joint family remains to be seen. Something like this perspective is present in the effort by the anthropologist Joel M. Halpern to give a numerical expression to the long-term changes undergone by the structure of the *zadruga* in the period between the nineteenth and twentieth centuries. He found in this study not so much the replacement of the *zadruga* form by some simpler form of the family but rather a shift from lateral to lineal complexity (Halpern and Anderson, 1970). Studies of this kind move a step closer to what I would call anthropological history, namely, a history of a social institution for which the evidence takes the form of, to use Radcliffe-Brown's terms again, a chain of connected happenings. At this point in the development of structural analysis there may be more reason for optimism about the emergence of this kind of history than ever before.

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