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Formal network methods in history: why and how?

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On 19 May 2008, *Nature News* discussed history (Brumfiel, 2008). Under the title "Social networking gets medieval", the first lines read as follows: "Researchers give a French province the 'Facebook' treatment. The popularity of Internet sites such as Facebook, Bebo and MySpace might make social networking seem relatively modern. But a team of French researchers has challenged this idea by trawling through medieval documents to create the oldest detailed social network ever constructed."<sup>1</sup>

Network research, be it explicitly historical or not, often relies on such modernization theories. Brumfiel's statement mixes the very existence of social ties with the type of awareness and representation of these ties provided by Facebook (did the researchers "create" or reconstruct the network?). Medieval "networking" obviously did not take the form of web-based "friends wheels"; but were medieval networks generally less wide, varied and consciously constructed than those of our contemporaries? And which methods—a "Facebook treatment"—allow us to answer this question?

The most general implicit modernization theories of social ties (often not based on empirical data, and especially not on formal studies) actually appear somewhat contradictory. The "Facebook" vision of "networking", that considers social capital as an integral component of economy in the information age, might conflict with Polanyian theories (and even more general concepts of holist vs. individualistic societies) that equate modernity with impersonal exchange, as opposed to the warmth and/or constraints provided by "traditional" strong ties. As Ogilvie, 2005 puts it, "Past societies are often portrayed as having enjoyed more trust than modern ones. History is mined for examples of the closely-knit and multi-stranded social networks thought to generate particularly rich stocks of social capital". The contradiction in fact vanishes if we concentrate on the precise characterization of ties, not on the "size" or "importance" of networks, "trust" or "social capital" in general: they have always existed, but their structure, their building mechanisms, and the relative weight of different sorts of ties have certainly varied across cultures and periods<sup>2</sup>.

This leaves room for empirical studies centered on the "modernization" of networks, a question addressed by many contributors in this volume: Alfani, Fertig, Langthaler, Munno, Silva, Seiser, and, in an interesting case of modernization accompanied by decreasing resources, Kasarjyan *et al.* "Modernization" of course covers a large range of phenomena: monetarization, sales of common land, political democratization.... Discussing their effects on social ties, or the fostering or hindering effects of social ties on them, is still one of the most powerful incentives to study historical networks, as it was already the case for pioneering microhistorians who, without using the then new formal network methods, put the interplay of class, kinship and other personal relationships on the research agenda (Levi, 1985, Sabeau, 1990).

The availability of a computing power that could barely be imagined in these pioneering times now allows to formally test some of these modernization assumptions; it has however also given birth to researches considering the reconstruction of complex networks as an aim *per se*—which is unfortunately the case of the one that caught *Nature's* attention<sup>3</sup>. Popular interest in social

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<sup>1</sup> This is not fair for Alexander and Danowski, 1990, who treated the much older social network appearing in Cicero's correspondence in an altogether more serious way.

<sup>2</sup> These questions on the characteristics of network "modernization" were already clearly framed in Wellman *et al.*, 1988.

<sup>3</sup> Presented on the website <http://graphcomp.univ-tlse2.fr>.

networking has recently led researchers in various sciences and social sciences to join "network studies" with no knowledge of—or interest in—the sociological and anthropological theories, methods and software developed during the last 35 years. They thus often reinvent indicators; more worryingly, they tend to build an artificial "complexity" by mixing heterogeneous ties on long periods. For example, in the medieval France research, the idea is "to rely on agrarian contracts as a source of information about social bounds between persons" (Boulet *et al.*, 2008). Each bound is actually treated as symmetrical and defined by the fact that two persons appear, in any social role, in the same contract or in related contracts, be they about land hiring, sale or legation. It is difficult to get precise historical results from such premises; there is a high risk to only re-discover very general, structural phenomena, such as the existence of very dense parts in many networks, often described as "small worlds"—in this case, the fact that persons from the same family or village were often present in the same contract does not add much to our understanding of social structure.

This is not to say that the formal network analysis of large datasets built from archival sources has no added value for historical research. The fact that methodological sophistication, or the drawing of very complicated graphs, becomes the aim of the research is a risk that can be controlled. Seriously using formalization, while keeping it in its ancillary role, might allow us to go beyond the loose "relational turn" of the last decades. The use of words like "network", "social capital", "sociability", "relational", "family", "friendship", "trust" or "cooperation" has been steadily increasing in the last 20 years; the explanatory and/or descriptive power imparted to the underlying concepts has often been gained on the expense of "class", "institution" and/or "market". However, the number of papers or books showing network graphs, using specific network statistics (centralization scores, triad counts...) or even the precise definition of some concepts (betweenness, clique...) as found in network analysis textbooks is much lower<sup>4</sup>—certainly in history, but even in the research fields of social network pioneers, such as economic sociology. There, too, general references to "embeddedness" or "the strength of weak ties" do not imply the use of any formal network method. The network vocabulary is often either used in a purely metaphorical way or associated with data supposed to measure social capital at a macro scale (e.g. the existence of voluntary association), not with the sort of "network data" (information on precise ties between specific individuals or organizations) that need to be analysed through graphs and matrices.

This situation has to do with the ongoing debate about the nature of "social network analysis": is it a method (or even a technique), a theory (e.g. in the versions of Burt, 2005, or White, 2008, that have been mostly applied to economic sociology), a paradigm, an "ontology" (White & Johansen, 2004), or something looser, like an approach, a way to view things? Mark Granovetter (e.g. Granovetter, 1990) has often pointed out that "network ideas" were incompatible with at least two sorts of social theories: the "under-socialized" ones, such as the simplest perfect market models, where atomized actors may be influenced by the aggregate behavior of others, but not by specific ties, and the "over-socialized" ones, where people automatically follow widely agreed-upon norms. In this context, he did not plead for a well-defined, unified "network theory" of social life, but for a general use of network methods, as "there really is no way to remain faithful to the fundamental insights of sociology without paying attention to networks of social relationships." This definition of network methodology as a kind of middling approach opposed to limited polar theories, but not itself building a new one is shared by many other authors, e.g. Lazega, 2003 (emphasizing the consideration of "meso-social" phenomena, as opposed to purely "micro" and "macro" ones) or Degenne and Forsé, 1994 (showing that networks are involved in complex feedbacks between agency and structure). If there is not a unified network social theory, there is certainly a shared core

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<sup>4</sup> Some of the technical terms will be explained below. The reader who would like to be introduced to network analytical methods should begin with short texts written for historians and surveying historical research, e.g. Erickson, 1997 in English, Lipp, 2003 in German, Lemercier, 2005 in French. Simple manuals, written for precise software but more generally applicable, are Hannemann & Riddle, 2005 and de Nooy, Mrvar & Batagelj, 2005. The most popular textbooks, including theoretical discussions, are Wassermann & Faust, 1994 in English, Schweizer, 1996 in German, Mercklé, 2004 in French. Recent methodological developments are covered by Carrington *et al.*, 2005 in English and Lazega, 2007 in French. The International Network for Social Network Analysis provides various online resources (<http://www.insna.org>).

of concepts that have been translated to indicators and implemented in software. Many of the tools in this well-defined set are not intrinsically related to any special sociological or anthropological theory; some are or have been (like R. Burt's "structural holes" or D. White's "structural endogamy" measures), but it is often possible to make good use of the theory without any data fit to the tool, or of the tool for a purpose that the theorist had not anticipated<sup>5</sup>.

Therefore, using formal network analysis and asking questions about social relations are relatively independent research choices. Does it imply that historians interested e.g. in family strategies or in the embeddedness of economic relationship should not bother with strange and complicated tools? Social network analysis still offers concepts and methodological warnings that valuably complement more qualitative and other quantitative methods. Knowing its basic concepts helps to refine, and sometimes to test, hypotheses on the construction and evolution of social ties and on their influence on behaviors. Methodological precision is all the more necessary at a time when the network vocabulary is at risk of being considered as nothing more than an intellectual fashion: showing what exactly a relational view adds to our understanding or history, and even measuring it when it is possible, is crucial in this context.

I will try to show this usefulness of formal techniques by referring to studies on the themes of interest for rural historians that are addressed in this volume: the embeddedness of markets, migration, alliance and family strategies, innovation and political mobilization. I will first discuss two frequently asked questions: Are network methods applicable to historical sources? What is the "added value" of a formal study of networks as compared to a more qualitative one? I will then present several network methods and their theoretical implications, following the choices that have to be made in the course of a research.

## Sources

Historians often seem to fear that their sources are not suited to the complicated tools of network analysis. This is especially the case for "structural" network analysis: as it involves the study of so-called "complete networks", this methodology stresses the need for exhaustive information. It is however routinely used to treat contemporary surveys with a given proportion of missing data (the methodological journal *Social Networks* regularly deals with this question): the historians' problems are not very specific on this point. In addition, sociologists are often confronted with reluctant interviewees who do not want to talk about some sorts of relationships. Conversely, in many cases, the traces of ties that historians find (e.g. alliances, land sales, membership in associations) have been recorded through a relatively homogenous process, with reasonably limited or known biases. In most of the cases, the cost of computing additional information on one link or individual is also much smaller when reading archival material than in face-to-face interviews. It is generally easier to find a source suited for the reconstruction of a "complete" network in archival records, e.g. all the sales or marriages taking place in a village during a decade, than to conduct interviews asking e.g. 100 people about their ties with each of their 99 potential partners.

Historians are generally less well equipped than sociologists to perform random surveys (if only because they lack a complete list of their population), therefore confined to more "micro" case studies. In the case of networks, where random sampling is not very useful (as it does not allow to consider ties between surveyed and non-surveyed persons), except in the case of purely ego-centered networks (see below), this peculiarity of our sources might become an advantage. As shown here by Kasarjyan *et al.*, sampling problems in the study of contemporary networks are often solved by "snowball" strategies (interviewing the contacts of the first interviewed persons)<sup>6</sup>, that can

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<sup>5</sup> The complex relationships between theories of Pierre Bourdieu—one of the inventors of "social capital"—and formal network methods, that have long been rejected by his French followers, is a particularly good example of this relative, but only relative independence between theories and techniques. See e.g. de Nooy, 2003.

<sup>6</sup> On snowball sampling, see esp. Doreian & Woodard, 1992 and on the notion of "sampling" in network studies Rothenberg, 1995.

also be used with some historical sources (basically, the ones that are indexed by name); but this involves costly and time-consuming investigations and conveys the risk to center the study on the most connected individuals. Historical sources thus offer much potential for network research, including formalized studies. However, the nature of ties and their possible interpretation require just as careful a specification as in sociology, especially as the fact that ties are already described in a written source could tend to make the researcher less sensitive to definitional problems.

### *Conduits and fluxes*

Let's, for a moment, think of social networks as metaphorically referring to hydrographic or computer networks. In the latter, there is a distinction between the infrastructure (river beds, that might be empty at some times of the year; computers and cables or waves) and something that circulates through it, in changing volumes and patterns (water, data). Social reality offers fuzzier limits between the "container" and "contents", although the river metaphor has its value as a reminder of possible feedback (as in the case of a flood widening the bed). We can nevertheless think of polar forms, on the one hand of purely potential relationships (as in the case of alumni having kept no contact in years) and on the other hand of pure exchange/circulation of resources following no previously established social tie and creating none (as in a very simple model of anonymous market).

More generally, it is useful to distinguish two senses of the words "tie", "link" or "relationship", referring either to an actually observed interaction at a given moment or to potential for exchange, in turn created by previous exchanges of the same type (as in the case of a repeated marriages between two lineages), of another type (a kinship tie making an economic association easier), or by a pure similarity (e.g. of religion) or a shared past experience (alumni, members of the same large association). As András Vári puts it in this volume, "no doubt there were networks, but these functioned in terms of an eventuality, as something that may happen under certain conditions". If we want to understand the structure of a network, knowing if we deal with "eventuality" or with actual interaction or exchange of a precise resource is important: some tools might apply to both cases, but the interpretation will surely differ. In Langthaler's paper, for example, "being neighbours" is quite a different relationship from "being the borrower and the guarantor of a loan according to the debt relief application". Such distinctions should be kept in mind, especially when the researcher tries to aggregate or to compare relationships of different types.

### *Observing and defining ties*

Historians encounter specific difficulties to define some of the ties that look more like "potential for exchange" than like "actual interaction". It is the case, for example, of distant kinship ties. Any use of kinship ties in a formal network analysis or in other calculations (e.g., are sellers and buyers akin?) should first question not only the "effects of kinship", but also the knowledge that actors had of their own ties. If third cousins interact without knowing that they are akin, we cannot directly infer that they do so because "they trust kin": we will have to think of a mechanism, maybe a combination of relationships with acknowledged kin including one or two bridging persons, that could have caused this choice of interaction—or to dismiss kinship ties as a possible factor enabling interaction.

Sociologists and ethnologists who want to reconstruct "objective" genealogies have to confront the interviewees' subjectivity, imperfect recollection and different cultural definition of kinship. Conversely, historians reconstructing kinship from acts of civil status generally ignore who was aware of each tie (and how they called it). They can therefore discuss the effects of an "objective" kinship network, not a "subjective" one. The study by Bearman, 1993, on English 17th-century nobility provides an interesting exception. As genealogies made partly contradictory kinship claims

(e.g. a nephew being recognized by the uncle as such, but not the other way around), Bearman considered these claims as a network, which gave new insights on the meaning of kinship for the actors.

In the more frequent case of "objectively reconstructed" kinship, independent evidence should be looked for in other sources (such as diaries or legal or religious interdictions) to justify the definition of kinship used by the historian in graphs or calculations. Are we dealing with reconstructed, available, mobilized, united kinship, with kinship acknowledged by the individual? The interest for family outside the household, much increased among historians by the early results of microhistory (Levi, 1985), has too often led to a simplistic, reified definition of wider "groups" (Plakans & Wetherell, 2003). Using names as proxies for "families", while it is sometimes necessary because of the lack of other data, calls for special caution.

An even more complicated situation occurs when the historian has to use mere hints, or "proxies", as statisticians would put it, to approximate enduring ties that seem important as "potential for exchange" but are difficult to observe. The most obvious case is friendship. Many studies have considered marriage witnesses as the best possible proxy for friends—or indeed acquaintances, as witnessing has often been treated as a "weak tie". However, choosing a witness might also be seen as a precisely dated strategic choice, variously constrained in different social contexts, which makes the use of this source less than straightforward (Gourdon, 2006).

Despite these ambiguities, written sources altogether tend to offer more opportunities to observe actual, precisely dated and qualified exchanges than fieldwork in contemporary societies. Alfani and Munno, in this book, show the interest of cadastral surveys and sales registers in this respect. In the fields of political history and innovation, other sources list precisely defined interactions that have still too rarely been treated as networks, like citations (not only in scientific papers, but in any sort of text) or sponsorship for entry in a club or association. Formally analysing them would help to understand recruitment and innovation-spreading processes.

### *Ties between places...*

When we think of "social networks" in general, we generally refer to ties between human beings; some of them are difficult to observe in historical sources, especially if we want to follow precise individual across different social spheres, or to be sure to survey the whole population of a community. The interest of formal network methods in history is however not limited to inter-individual ties. Networks of firms, towns or regions can also be considered. Studies of exchanges, especially of goods, benefit from a real "added value", as formal network analysis can take the orientation, volume and type of ties into account to generate a synthetic but detailed image of the structure underlying the observed exchanges—and of its changes (see e.g. Smith & White, 1992 on international trade). The same is true for migration: we often think of "networks and migration" at the individual level (e.g., do local dense kinship relationships prevent young people to leave?), but migration networks between villages are also fit for formal studies. Rich and complete information can be rapidly gathered at this scale, and graphs, "blockmodels", etc. help to test competing models of preferential directions, such as rural depopulation, chain migration or local, culturally-constructed clusters of villages (Lemerrier & Rosental, 2000, 2008).

Still other types of relationships between places, depending on the available sources, might expand this view of historical geography and thus complete the more limited community studies that up to now form the majority of studies on historical networks. Historians have for example analysed the correspondence between town councils (Buchholzer-Rémy, 2006), and constructed subtle indicators of intra- and inter-village relationships based on court records (Barkey & van Rossem, 1997). This last case is interesting: one mention of friendship, help or feud between two individuals in the middle of a testimony does not tell us much. Reconstructing individual networks from such a source would be painful and they would certainly not approximate the "complete social networks" of these persons. Aggregating these ties at the village scale makes more sense and allows

to confront this view of the structure of a network of places with independent evidence about collective local reactions to State centralization.

... and organizations

Traces of precise interactions between individuals, places or organisations thus seem more suited to formal network analysis than e.g. genealogies or mentions of friendship in various sources. They are often easier to compute, but above all, they deal with more homogeneous relationships, that are described with useful details (date, amount, orientation...), allowing us to choose indicators suitable for our hypotheses (for example, the creditor of two persons is not a broker between two communities in the same way as the creditor of one and debtor of the other). In addition, we generally have information about the concrete consequences and meaning of the interaction for actors, which helps to build more precisely specified and more realistic causal models. For example, the huge literature on "interlocking directorates", i.e. bonds (supposedly) created between firms by having the same persons sitting on their boards, has often be criticized for the lack of such models (Mizruchi, 1996). In many studies, not enough time had been spent considering, e.g., who decided to create interlocks (who appointed members? with which legal restrictions? did some people refuse to be appointed?), if board members actually met and what they did together, and if interlocks could be considered as *directed* ties between firms (e.g. according to dates of entry in boards). This points to the fact that interlocks *per se* are a loosely defined tie, and constitute more a potential for interaction than a type of interaction.

The same could be said about kinship ties (when loosely defined) and common involvement in events or associations reconstructed from members lists. This is not to say that we should not use such sources in network analysis. On the contrary, formal studies of networks of members and organizations<sup>7</sup> have proved very useful for research on social and political movements, clarifying their structure, its evolution, and pointing to "central" or "intermediary" key persons or organizations, thus reassessing the role of, e.g., feminists in women's organizations (Rosenthal *et al.*, 1985). Such a systematic study of overlap between lists of members (successive lists for the same organization, lists from different sorts of associations, involvement in important events...) might help, in the future, to extend Guzzi-Heeb's analysis of the political scene in a Swiss region (in this volume). It was one of the bases of Carola Lipp's impressive study of the political structures and mobilizations in the German town of Esslingen during the 19<sup>th</sup> century (e.g. Lipp & Krempel, 2001). Among other indicators, she computed a level of "exposure" to political activism (based on connections between people through petitions) at different points of time and confronted it with the network of organizations as well as with other data (such as professions).

The study of this sort of "ties" nevertheless implies, as in the case of kinship and interlocks, that the researcher pays attention to the meaning and significance of each "tie" (in this case, e.g. do members often meet? should successive involvements be treated as directed or undirected ties between organisations?) before considering them as an homogeneous series suited for calculation. Economic studies of social capital, such as that of Wolz & Fritzsche in this volume, often aggregate measures of individual interactions and of membership or leadership in more or less formal institutions (associations, interest groups...). To go one step further, it would be useful to draw a finer conceptual distinction between the benefits of face-to-face interaction (more thoroughly investigated here by Kasarjyan *et al.*) and other consequences of membership in organizations, e.g. information given in internal journals or even financial support, as in the case investigated by Seiser. Organizations, but also individual involvements in them, differ in this balance between direct interaction and more impersonal collective services: it is important not to directly mix these different sorts of resources in a loose concept of "network" or "social capital". Compagnone, 2005 thus investigates networks of advice on wine growing practices in five villages, distinguishing

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<sup>7</sup> This is called a "2-mode" network, as it can be considered in two ways: common members create a tie between organizations; being members of one or several organizations together creates a tie between people.

between advice provided by organizations and by more personal networks, that have different structures and consequences.

### *The map is not the territory*

This discussion of ties that historians can reconstruct from their sources should lead us to reconsider an often implicit aim of network reconstitution and analysis that has certainly contributed to make it attractive to researchers, but that can become very misleading in practice. Social network analysis does not allow to "draw a map" of an individual's network or of all the relationships inside a community, to describe *the* network of this person or *the* social structure of this group. There are too many sorts of possible ties, continuously changing: it is not possible to list them all (let alone to qualify them). Aggregating them under the general word "tie" or "relationship" only leads to impoverish possible interpretations. It is in fact possible to "draw maps" of networks, but only if we remember that the map is not the territory: it concentrates on some precisely defined phenomena, momentarily forgetting everything else. Even in the richest "community studies", making use of exceptional sources and research workforce, such as Rutman & Rutman, 1984, that follows the growth of a Virginian village from 1650 to 1750, trying to reconstruct all sorts of interaction, maps, tables and narratives use changing scales and successive focuses to build a composit—some would say cubist—image: they do not "show the social structure" as such. Choices in "boundary specification" (whom do we observe? which ties? when?) heavily constrain the sort of questions that can be analyzed by network analysis: "carelessness in system specification is probably a more serious issue for network analysis than for much survey analysis" (Laumann *et al.*, 1992).

As Guido Alfani puts it in this volume, "observability of the social space is a problem", as each different tie gives a different picture and adding new ties is time-consuming; there is a high risk "of getting an incomplete or distorted picture of how people interacted". A more optimistic view however would consider that our aim as historians is not to "observe the social space" generally, but—as Alfani himself does it in his paper—to understand the patterns of precisely defined ties, by deliberately abstracting them in order to carefully consider their effects, their origins (as they are sometimes dependent on legal constraints, sometimes strategically constructed with more freedom), and their changes in response to external events.

### **Why formal methods?**

The abstraction that is involved in concentrating on some ties and momentarily forgetting the others is common to all formal and quantitative methods<sup>8</sup>: a phenomenon is defined with a precision that allows to state if it is present or absent in given situations (in the case of networks, a tie of a given type exists or does not exist between two individuals during the period studied); its attributes are also defined according to precise and homogeneous scales or classifications. These very general requirements generate distinctive advantages: an incentive to consider symmetry, and the possibility to test two sorts of hypotheses. First, we might want to test the idea that interesting network patterns in the data are not created by pure chance, so that some social mechanism must be at play. In many cases, we also have more complex causal, even multicausal, hypotheses on the origin of ties and of their structure.

### *Symmetry*

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<sup>8</sup> On the difference between "formal" and "quantitative", the former being more inclusive, and on the place of network analysis in the realm of formal methods, see Tilly, 2004. Abstraction is in fact always involved in research, even the most ethnographic case studies, whereas it is even less often made explicit in this context.



Symmetry refers to the advice: "don't forget the negative cases!". Historical texts about "networks" that only rely on genealogies and narration too often only tell the story of one or a handful of "successes with networks". For example, after having selected politicians with a particularly impressive career, the author looks for ties that could help to explain it. And he finds some, of various sorts: alliances, friendships, common acquaintances, involvement in social events with powerful persons... Hence the importance of "social capital".

Even when qualitative studies are more rigorous than in this sketch, when they distinguish ties that really played a role and take chronology and other attributes of the person into account, they often lack what only more systematic research can offer: a comparative consideration of the three other possible cases, namely success without network, network without success, and neither. This strategy compels the researcher to define a set of comparable actors that are "at risk" of experiencing the same trajectory: e.g., she will not only look for ties of some sort between innovators, but between a wider set of "potential innovators". In the case of Langthaler's study in this volume, it would be interesting to confront the diffusion of "debt relief" applications with a full network of neighborhood relationships (that would thus look like a map), which might allow to consider possible resistance to the influence of neighbors<sup>9</sup>. The same is true for Guzzi-Heeb's paper, that could have more systematically confronted lineages (with all their members, even politically inactive ones) and political activity. This lack of symmetry is a classical limit of innovation and mobilization studies, also found in the pioneering—and otherwise very stimulating—research by Banti, 1989 on the introduction of artificial fertilizers in 19th-century Italy.

Negative questions are often difficult to investigate, and formal methods do not give all the answers here, but we should not simply obliterate them. For example, the rich literature on social networks and mobilization (Diani & McAdam, 2003) often focuses on how different sorts of ties increase the probability to mobilize. Actors with less network incitations are also generally taken into account: other theories are needed to explain their choices. There is, however, another possible case, with ties hindering mobilization, or conflicting ties—e.g. a conservative family and liberal friends (McAdam & Paulsen, 1993). The same ambiguity can be found in the economic sphere: does the observation of exchange, especially between family members, prove the existence of trust, here influenced by kinship ties, or did exchange occur *despite of* mistrust or indifference (Fertig, 2005)? Likewise, the detailed qualitative study of the building of patronage ties by Silva, in this volume, shows that the acceptance of these ties by the clients was more linked to a fear of sanctions or lack of alternatives than to any positive choice.

Whereas empirical answers are difficult to find, such questions are important, as they depart from a too often unilaterally positive view of networks in the literature. Partly for technical reasons, negative ties, even strong ones, that were present in early small-groups research, have been quickly forgotten in formal network analysis. Even if we do not directly include them in datasets, we must however be aware that conflict, both internal and external, is often an integral part of the daily life of densely knit communities—as reminded by Guido Alfani and András Vári in this volume, and demonstrated, in the case of family, in the classical book by Sabeau, 1990. To translate it from the vocabulary of historical anthropology to that of economic sociology, if networks, as forms of organization, really provide a variety of economic and social benefits, why do markets and hierarchies still survive in many fields (Podolny & Page, 1998)?

"Network studies" deal with hypotheses about the possible influence of networks on various phenomena; they do not have to lead to the conclusion that this influence is always positive or even present. Fertig, 2007 and Boudjaaba, 2008 for example found only limited effects of family ties in rural land and credit markets of the 18<sup>th</sup> and 19<sup>th</sup> centuries—be it because there was no market at all, because efficient institutions (laws, banks...) made close transactions less interesting, or because other forms of "closeness" than family ties played a more important role. In this respect, quantitative studies are useful because they help to assess negligible or negative effects as well as

<sup>9</sup> The study of (especially spatial) diffusion of innovation in rural areas has ancient roots. Hägerstrand, 1965 gives a good example of how to take potential adopters (not only actual adopters) into account, of how to deal with neighborhood in an heterogeneous space, and of the merits of simulation in testing such hypotheses.

positive ones. Especially when dealing with the probably too fashionable concept of "social capital", they should neither dismiss the weight of "negative" ties nor the possibly negative effect of the accumulation of strong ties, the "not-so-desirable consequences of sociability (...): exclusion of outsiders, excess claims on group members, restrictions on individual freedom, and downward leveling norms" (Portes, 1998).

### *Networks and chance*

The general aim of testing hypotheses might be seen as common to all quantitative methods, or even to any sort of research. "Purely descriptive" studies themselves at least implicitly refer to hypotheses on what will be found, which allows us to distinguish confirmatory from original, counter-intuitive results. That is to say, not very innovatively, that we all have implicit theories. Well-conducted quantitative research often helps to make them more explicit, as the researcher has to define which factors she takes into account, how she defines them, which are the dependent and independent variables, etc. For example, investigating a question such as "does family matter in 19th-century Parisian economic institutions?" implies not only to find a proper assessment technique, but also to define "family" and to differentiate several hypotheses on the way it could "matter" (Lemerrier, 2006): Were new members tied to previous ones? Loosely or through real succession processes? Did family groups sometimes control an entire institution? Answers to such questions will never be 100% "yes" or "no", but will help both to find out which part of the actual events these processes might account for, and to identify exceptions that should be investigated in a more qualitative way—as Vári points out at the end of his paper.

In the case of Parisian institutions, as in many others, to go further in the direction of a real test, we should consider the fact that family members were sometimes chosen not because they were family, but for other reasons, including the simple fact that they were available. The question "are family members often chosen?" then becomes "are they chosen more often than chance would have provided?" This very simple twist of the question changes many things, especially in studies of relatively closed communities, where marrying a parent or selling him land often might have been a likely option, even with no specific preference, trust or strategy involved. It is not always easy to define the set of potential partners; the possibility of "chance network" should however be kept in mind, and tested, when possible, against simulated chances: this has been done e.g. by White & Johansen, 2004 (chances to observe various sorts of endogamous alliances) and Fertig, 2005 (chances to find land sales between kin).

The idea underlying this test is at the heart of most of the new techniques developed in network analysis during the last decade. Although scarcely used, up to now, in empirical research, especially on historical data, they should not be confined to the most experienced users, as they allow a more robust assessment of network patterns. The general idea is to use simulation to generate many random networks sharing basic properties (such as the number of individuals and ties, in order to allow comparison). It is thus possible to assess if some properties of the observed network differ significantly from those of the random networks. This idea is present in QAP-correlation, used in this volume by Kasarjyan *et al.* It helps to assess if two sets of ties between the same individuals (e.g. kinship ties and sales of land) are significantly correlated<sup>10</sup>. Another technique, "triangle counts", groups all triads of the network (all possible sets of three individuals) in one of the 16 possible triad forms (e.g., no sale; A sells to B who sells to C and buys from C; A sells to B and nothing else, etc.) and assesses which of these forms are significantly under- or over-represented in the observed network. This allows to test hypotheses about reciprocity, hierarchy (A sells to B and

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<sup>10</sup> It might seem easier to perform a standard regression, each dyad (possible couple of two individuals, or potential tie) being used as a statistical unit, and it has indeed often been done. However, regression theoretically applies to independent individuals, and independence between dyadic ties in the same network is a strong hypothesis: if e.g. friends of my friends tend to become my friends—a common structural effect called transitivity—, then dyadic relationships are not independent from one another.

C, B and C do not sell to A) or "generalized exchange" (A gives to B, who gives to C, who gives to A), for example: such local patterns will be present in many networks, but if they are much more present than would happen by chance, this might signal an underlying social norm, or at least a constraining social and economic structure shaping the network.

### *Ties, attributes, behaviors and discourses*

It is important to be reasonably sure that the patterns we observe in networks are not due to pure chance. Understanding why and how they appeared is however a more complicated matter—especially important for historians, who often can reconstruct at least a part of the evolution of the ties they study: this makes causal questions more acute.

Early network studies, especially those dealing with historical data, have been criticized for either being too deterministic (seeing *the* cause in the network structure, forgetting both agency and cultural norms) or lacking any coherent causal model (Emirbayer & Goodwin, 1994). The radical, deterministic view was actually shared by some early promoters of the method, especially Ronald Burt, who more or less clearly stated that "attributes" (class, age, sex...) did not matter, as social structures only relied on relations (and especially on their local patterns): his 1980s software Structure did not allow to include any data on attributes. As a result, network models are often regarded "as graphs of relationships taken out of context and out of time, a mere dried skeleton of social life. Network models are often misunderstood as something divorced from the social context that lends a more proper interpretation" (White & Johansen, 2002). Fortunately, the situation has changed since, and researchers do not hesitate to integrate both networks and attributes in various multicausal models. In this volume, for example, Munno could simultaneously test the effects of betweenness in a godparental network and membership in the upper class on the participation in an auction, which led her to conclude that the latter effect captured most of the former.

Alliance and godparenthood, investigated in several chapters of this volume, are obvious cases of ties for which attributes such as fortune or social status matter: most of the researchers concentrate their descriptions of such networks on interactions between ties and attributes. Topics like agricultural practices and political mobilization involve even more complex interactions, that should be observable at a micro level, between attributes (e.g. education), networks (co-involvement in organizations, professional discussions and advice...), behaviors and discourses justifying these behaviors. In various, mostly qualitative ways, Langthaler, Seiser and Vári strongly point in this direction. The complexity of interactions yet does not forbid the use of formal methods, be they graphs integrating these four aspects, as demonstrated in contemporary studies of agricultural practices (Compagnone, 2004), or multilevel regressions including regional or religious specific representations of the family as factors, among others, influencing the frequency of individual interaction with siblings (Bras & van Tilburg, 2007).

Statistically testing multicausal models in order to explain patterns in networks and the evolution of these patterns has also become possible in the last few years, once again thanks to the comparison of observed networks with many randomly simulated ones. When the data are rich enough, it is possible to simultaneously test hypotheses on "purely structural" phenomena (that is, in fact, social constraints or cultural norms operating independently of attributes), such as the tendency to reciprocate ties, to form closed cliques or to be attracted to already popular godfathers, and on the effect of "attributes" (that may, in fact, include elements that we would call behaviors or discourses) on networks, e.g. a tendency to choose a rich godfather, or a godfather of the same neighborhood as the father. Some models even allow to test the mutual effects of networks on behaviors (adopting the political position of a friend) and vice versa (choosing friends among people of the same opinion)—provided that we have data on the evolution of both ties and behaviors or discourses (Agneessens & Wittek, 2008, Burk *et al.*, 2008). In the case of rural migration in a region of 19th-century France (Lemerrier & Rosental, 2008), this type of model allowed to control for obvious effects such as the attraction of industrializing towns and yielded interesting results, including an

unexpected tendency to reciprocation of attraction between villages, a strong transitivity (many moves between A and B and between B and C tended to foster moves between A and C) and an increasing closure of linguistic sub-regions. This method, currently implemented in the software Siena (Snijders et al., 2007), seems to offer great potential to historical studies, especially in rural history, as reciprocity and generalized exchange in particular are key concepts both in kinship anthropology and economic anthropology.

### **Choosing methods... and theories**

Such models make the implicit choice of focusing on only one possible view, or scale, of the network: what is modeled is the absence, presence, creation or destruction of each tie, so that the basic unit is the dyad (a pair of individuals, be they human beings or villages). Even if a wider structure is somehow included in the model (e.g. transitivity considers triads), this doesn't give us a summary picture of the whole network, or allow to take into account hierarchies and borders between groups other than those defined by attributes.

These multicausal models in fact concentrate on a "meso" scale, on local patterns, whereas "micro" or "macro" approaches are more suited to other important research questions on historical networks. In the pioneering period of network analysis, many authors were content with showing that ties did matter and with calculating every possible indicator and trying to make sense of it. We are now faced with a huge number of possible treatments and views of our networks—and with more demanding colleagues, who want to know what exactly networks are supposed to do, and where they come from. This calls for a better specification of causal models, for an informed choice of methods and scales of study and for complementarity between qualitative and quantitative studies. Choices have to be made, and to be made explicit to readers. I will now concentrate on five questions related to these choices.

The first one has already been mentioned when I discussed sources: Which ties are we interested in? This is probably the point where a good qualitative knowledge of the case (and especially of the sources) is most needed, so as to avoid building "a network" out of totally heterogeneous ties, symmetrizing obviously directed ties, etc. The other four choices deserve longer comments.

#### *Causes and consequences*

Is the network supposed to be the cause, the consequence, or both? Most historians are deservedly proud to "describe and understand" the past before jumping to general explanatory models. We however sometimes go too far in this direction, when pretending to draw maps of "the social reality". A network structure is never interesting *per se*, or as a mere "reflection" of some deep, unobservable social structure: our curiosity is based on hypotheses that should be made explicit. Are we interested in alliances between families because their pattern seems to reveal generating strategies, such as repeated marriages between lineages that would aim to reconstruct fragmented pieces of land, in the context of a given inheritance system? Are we trying to spot the effects, if any, of an event such as an institutional change, a war or a plague on the network? Or do we want to know how "embeddedness" influences individual behaviors, e.g. on a market, or collective ones, such as the propensity of a community to adopt an innovation?

#### *Scales*

Do we focus on dyads, on limited or complete ego-centered networks, or on complete networks?

By dyads, I mean that the unit studied is a relationship, such as the ones depicted in land selling contracts or marriage records. I also mean that we do not try to reconstruct a more general structure

from these dyads, but stay focused on them. This leads to the use of cross-tabulations or regressions, basically referring to the concept of endogamy: are partners in a transaction, godparents and godchildren, etc., also kin, neighbors, etc.? In network terms, it is a question about "multiplexity", but it is treated without using network methods. Sabeau, 1998, systematically used this approach to assess the changing place of kinship in various sorts of exchanges in Neckarhausen, from 1700 to 1870. We should however not forget what is lost when we concentrate on dyads. Structural phenomena (such as triad patterns or the structure of the whole network) are unobservable at this scale. In addition, the technique is not symmetrical, as we do not deal with the relationships between pairs that do not appear as tied in our source (are couples of people who *do not* exchange land akin?). Tests should therefore be conducted to assess whether correlations between two sorts of ties are due to preference or to mere chance.

There is much ambiguity in the definition of "ego-centered networks". Sociologists often include ties between ego's contacts in them, whether reported by ego or independently reconstructed, which allows to consider local structures such as Burt's "structural holes" (contacts with people not tied to each other). Questions on the contact's contacts in field studies of entrepreneurs, although they are probably biased by the interviewee's incomplete information, have proved useful. For example, in the case of small French building contractors, different forms of ego-networks are correlated with different specialties and performances (Comet, 2007). These ties between contacts are not always observable in historical sources considered as ego-centered, such as correspondences or diaries, neither in the more serial documents used to build indexes of "social capital"—although Rosenthal, 1999, built a sort of structural index on the basis of acts of civil status, contrasting "auto-centered" and "exo-centered" families.

What I call limited ego-centered network is a description limited to the number and sort of ties "available" to an individual, with no either consideration of ties between his or her contacts or of indirect contacts (of "second", "third", etc. order: see Boissevain, 1973). This is essentially an individual, not a structural characteristic, defining a "social capital" "owned" by a given person<sup>11</sup>. This capital has often been integrated in cross-tabulations, regressions or event history analysis models. This research strategy has been used in particular in migration studies (e.g. Manfredini & Breschi, 2005, Bras & Neven, 2007, Bonneuil *et al.*, 2008). A wide range of "network attributes", or "social capital attributes" (e.g. being the elder of only female siblings, having an aunt who has already migrated...) can be tested along with more classical individual attributes to explain the decision to migrate.

Both dyad and ego-network studies thus make little use of network techniques and software as such. The latter are only really useful when "complete network" data are available. That is not to say, as we have already seen, that all sorts of links should be included, but that we should be reasonably sure of the existence or absence of each potential link between two individuals in the group that we study. However, a network is never complete, if only because the individuals generally have other ties outside of the studied group. This should not be forgotten when defining the limits of the study (that should ideally be wide enough to allow to find external borders of the group and to assess their porosity) and above all when interpreting the observed structure: is it plausible that it would dramatically change if other individuals, e.g. foreigners acting as brokers between two local groups, had been included? This question, however difficult to address, must be kept in mind in the case of community studies for which the sources do not allow to take ties with "foreigners" into account.

The same problem, in an even acuter form, ensues from time scales and time boundaries. Network methods, based on graph theory, are intrinsically static, although it is now possible to visualize network evolutions as films. There is no miracle solution to deal with time, although several reasonable approaches have been developed, such as (most often) carefully chosen

<sup>11</sup> Although, as already pointed out, this capital might be constraining as well as enabling. Some famous proponents of "social capital", e.g. Putnam, have defined it as a quality of groups or even nations, rather than individuals; this approach generally leads to the use of very general measurements of "civility" rather than to any analysis based on recorded ties and precise causal specification (Portes, 1998).

"snapshots" of successive periods, overlapping cross sections (Hammou, 2009), time-oriented graphs (Rosenthal *et al.*, 1985), a time-decreasing measure of the intensity of ties... As discussed above, statistical models allow a precise study of network dynamics, but they still rely on discretely defined successive networks. The key underlying problem remains the characterization of ties with a date (of beginning and, more challengingly, of end), that can only be based on strong assumptions from a qualitative study. For example, do marriage ties between families disappear after the death of one of the spouses? During which period does a satisfying sale experience with someone make you more likely to trade with them again? Defining the time borders of a network always entails the risk to include long-forgotten ties that in fact do not influence anything, or to exclude central actors whose death or migration does not immediately invalidate all effects of their past structural position. Even simply avoiding to take "future" ties into account in calculations (e.g. of the part of godparents who were kin *at the time of the baptism*, in Fertig, 2009) or being able to provide images of kin network for a given month (as does Munno in this volume) requires particularly well-constructed datasets.

Provided that we have made choices on boundaries (including time boundaries), it is possible to use formal concepts and techniques ("equivalence", "blockmodeling") to construct a synthetic image of the structure of our "complete" network. This use seems to become less and less common in empirical network studies—quite ironically, as software overcoming the main flaw of the technique (it produced a simplified structure, but was unable to assess the quality of this simplification) has been recently developed (Snijders & Nowicki, 2007). This certainly has to do with a "rational choice" turn in sociology in general and network analysis in particular, that has moved the focus to more directly explicative techniques and to more local structures. For historians, however, especially but not only historians of relatively closed rural communities, the purpose of equivalence measures, namely indentifying meaningful social roles and positions constructed by a global pattern of ties, still makes sense. As compared to more micro approaches, blockmodeling is more concerned with hierarchies than with the study of local patterns, and emphasizes borders between blocks, while social capital scales are more continuous and individual. Classical results of this technique are the discovery of one or several cores and peripheries in the network, often with different internal structures (e.g. a cohesive core and a more "open" one) and connecting brokers (individuals or groups, between core and periphery or between cores...). This makes sense for example in the case of witnessing ties between village households, characterized by two parallel hierarchies only partly based on occupation (Gourdon, 2005)—as well as in the aforementioned cases of migration, commercial or other ties between villages or countries.

### *Drawings and calculations*

In cases where formal methods seems useful, which sort of graphs and/or calculations should be chosen? Graphs often enhance intuition, especially when the researcher has a good qualitative knowledge of the group studied, its important actors, etc. They produce ideas on what should be more precisely tested (the existence of a pattern, the centrality of an actor or group...). One should however not forget that many readers are still not accustomed to interpreting them: they often give inaccurate impressions of density, of centrality of some actors, they tend to symmetrize oriented ties, etc.<sup>12</sup> The most useful graphs are probably the simplest, most synthetic and/or abstracted and clearly constructed ones—the ones that do not convey the idea that they are photographs of "the social reality", but simply help to visualize properties that are difficult to describe with words. For instance, projecting observed networks on maps, i.e. confronting ties with distances, can be illuminating, perhaps the more so in rural settings (Houdart *et al.*, 2007). Graphs based on explicit

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<sup>12</sup> Computer scientists have been intensively working on network visualization and exploration in the last few years, some of them along with social scientists exploring contemporary or historical network. This might lead to an improvement of software in the next few years. See e.g. Henry, 2008 and the webpage <http://www.aviz.fr/~nhenry/Research.html>

construction rules—like those in Langthaler's chapter in this volume, or of Lipp & Krempel, 2001—are also an interesting choice, perhaps even more so in the case of kinship ties, that have often been the focus of rural history.

Thus for instance, our view of kinship is deeply influenced by a more common graphic representation, namely the family tree. As any representation, family trees have their implicit meanings (centered on lineages, and often gendered) and practical drawbacks (such as not helping to identify relinking, i.e. repeated marriages involving two or more lineages). Alternative versions, like the "p-graphs" promoted by Douglas White and already applied in various empirical cases, try to overcome these problems. They especially aim to discover endogamous "cores" in long-term, complex marriage networks. When additional data are rich enough, as in the historical and ethnographic long-term studies of the Austrian village of Feistritz (Brudner & White, 1997) and of a Turkish nomad clan (White & Johansen, 2004), strategies creating endogamy can be hypothesized (in connection with inheritance norms and role models), and their viability when confronted with historical changes can be observed. In addition, the counter-intuitive character of White's representation (individuals appear as lines and marriages as dots) helps to question the implicit assumptions of more classical representations.

### *So what?*

Why do ties matter? It is as for now essentially a question for future research, and answers will probably come from the joint improvement and better specification of both theories and methods. Once a correlation between a tie and a behavior—or a tie and another tie—, such as a higher propensity to migrate with one's brother, to offer credit to kin, or common fertility patterns between neighbors, has been established, and does not appear to be caused by mere chance, this still does not tell us why ties matter, in terms of mechanisms—to use a concept that is currently spreading in sociology—or strategies—to point to an already heated debate in the history of the family.

As compared to more general, often vague assessments of "social capital", "trust" or "cooperation" at a macro level, supposed to characterize entire societies, or even cultures or periods, a network approach (the will to systematically gather precise data on interaction and possibly to treat them in a formal way) is at least a first step in the direction of a better specification of such mechanisms. In his critical account of the vague and even often obscuring concept of "trust", Guinnane, 2005, pointed that trust only concerns specific individuals in specific contexts. In his view, it cannot be usefully distinguished from more solidly established mechanisms hindering opportunism and therefore fostering economic exchange, such as the availability of information and effective sanctions. However, it is still interesting for historians to know if such mechanisms in turn relied on established relationships (creating "canals" or potential for exchange): considering the concept of "trust" with caution will then urge them to specify in which areas and ways it played a role, instead of trying to measure it on a simple, general scale (Rousseau *et al.*, 1998). With proper data, it is also possible to compare the effects of "trust in the system" and "trust in persons", although the latter often cannot easily be distinguished from the effects of internalized norms of solidarity, or even of an economically rational decision (Fertig, 2005).

If network analysis has something to say about the elusive notion of "trust", what about "strategies"? Network sociologists, and those who used a similar vocabulary in history, especially in microhistory, have often been accused of overestimating the strategic character of networking, and/or of hypothesizing collective strategies, especially at the level of the family, without any proof of their conscious and concerted character (Viazzo & Lynch, 2002). It is generally difficult to directly test the existence of conscious strategies in network analysis: qualitative investigation remains necessary on this question. However, carefully interpreted network data may allow precise hypotheses on the limits of strategic behaviors. For example, in a study of social mobility in a village, Fertig, 2009, found important "network effects", but only a part of them could be interpreted as the effect of deliberate "networking", as captured by the (probably) strategic choice of

godparents: other "networks", acquired by birth or by the choice of others, were of a different nature. As shown by this last example, it might in fact be useful to distinguish between two possible objects of "network strategy": strategic network-building and management on the one hand, and strategic use of available ties on the other. While the former would seem to entail the latter, the latter may well exist without the former, so that pieces of evidence about one sort of strategy do not always imply that the other is likely to have played a role<sup>13</sup>.

What network theorists and network simulation more generally have to offer is proof that robust macro structures can also durably emerge from local behaviors not intended to produce them. This theme of "emergence" in networks has particularly been addressed, in an often technical way, by Douglas White (e.g. White & Johansen, 2002): "A cohesive group does not need a plan and a selection of social ends to resist its disruption and destruction. Cohesion has "etic" as and "emic" aspects that do not always match." This is both an useful warning against systematically strategic interpretations of "networking" and a definition of the value added by network analysis, in that it allows to detect structures that might not be recognized by all actors, but still in some ways influence their behaviors.

Stating the existence of meso level groups or networks sharing some behaviors however leaves at least three important interpretive questions open. Are conscious, individual or collective strategies of networking and/or of network use involved? Do we have "network" effects, due to interaction, leading to the notion of "contagion", or "context" effects, due to a shared environment (be it physical, economic or normative), which is a quite different thing, in social life as in public health? The latter interpretation leads to the use of other formal methods, especially "multilevel" models<sup>14</sup>; when it is possible, comparing the effect on behaviors of different "networks" or "groups", of which some share the same environment and others don't (e.g. coresident and non-coresident kin), also provides hints helping to choose between network and contextual explanations (Campbell & Lee, 2008). Finally, even if networks (interaction) play a role, can we disentangle "influence" (the fact of becoming similar to friends) and "selection" (the fact of choosing similar friends)? Up to a certain point, the recently developed simulation models provide answers—although this involves heavy data requirement which may cast doubt on the representativity of case studies (Agneessens & Wittek, 2008).

A perfect example of the importance of these interpretive and theoretical questions and of the possibility, however limited, to devise modeling strategies to deal with them is given by a study on contemporary migration (Palloni *et al.*, 2001). The authors consider the observed correlation of migration behaviors across members of a social group as only an "apparent" network or "social capital" effect: it could be due to a direct interpersonal help or imitation, but there are competing explanation that could lead to the same observation. Members of the group could migrate in the same way because they share some characteristics and constraints influencing the expected net return of migration (they take the same decision, independently from each other); they could collectively decide to migrate, in order to maximize collective returns; and, if we think of friends rather than kin, they could have formed a group because they shared some characteristics that also lead them to migrate (because of similarity, not of interaction, i.e. because of a "selection", not "influence" process). The careful study of pairs of siblings allows the authors to discriminate network effects from context effects, but it appears much more difficult to decide on the existence of collective strategies.

Such studies, as well as the whole corpus of empirical research and theoretical debates that network tools have helped to developed, nevertheless give us some hope that we will be able to go beyond the simple statement of "embeddedness" and address these questions of structure and

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<sup>13</sup> This last important distinction, that, to my knowledge, is absent from the relevant literature, was aptly pointed out by one of the referees of this paper.

<sup>14</sup> See Courgeau, 2007 for a general presentation and Bras & van Tilburg, 2007, for an application to the study of ties between siblings in 20th-century Dutch farming families: multilevel analysis is used there to capture "contextual" effects at the scale of the region, considered as one of the possible approximations of social norms related especially to inheritance customs.



agency that are as fundamental in history as in the other social sciences.

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