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# INTERVIEWS WITH MATHEMATICAL SOCIOLOGISTS

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# Interviews with mathematical sociologists

Christofer Edling

## *1. Introduction*

It is hard to exaggerate the role of mathematical tools for the advances of science.[1] Indeed one needs only to pick up a copy of a science journal such as *Nature* or *Science* to realize that a god deal of technical skill is needed to follow the frontiers of science. The same can hardly be said about the frontiers of sociology and many other social sciences. Because of the tension between science and literature, a defining characteristic of sociology throughout its history, the role of mathematics in the advancement of sociology has always been, and still is an issue. In this paper I once more revisit the question of the possibility of a mathematical sociology through a set of interviews with mathematical sociologists.

There are many examples of sociology trying to take advantage of mathematics to solve problems (Heckathorn 1984; Coleman 1988; Fararo 1997; Holme, Edling et al. 2004). However it is less easy to find examples where sociological problems, broadly defined, have stimulated mathematical work. Still, we have two powerful examples where it is easy to see how mathematics has contributed to the development of the social sciences.

First there is the well-known example of the development of game theory by the mathematicians von Neumann (Von Neumann and Morgenstern 1944) and Nash (1951) that effectively re-draw the map of neoclassical economics, and eventually also spilled over to the biological sciences (Maynard Smith 1982). Game theory builds on the simple idea that the action of ego is strategically dependent on the action of alter; a Weberian idea that sociologists should indeed find attractive. But however dramatic the effect on economics, game theory has had very little impact on the other social sciences save perhaps for political science (but see Swedberg 2001 for a discussion on game theory in sociology).

[1] I wish to thank Peter Abell, Philip Bonacich, Kathleen Carley, Patrick Doreian, Thomas Fararo, Harrison White, and  the late Aage Sørensen. Related work has been presented at Utrecht University and at the Swedish Collegium for Advanced Study (SCAS) and I thank all participants.

Second, and highly relevant for sociology, there is the less well-known example of social network analysis. Network analysis has grown tremendously during the last 30 years or so into a very vigorous research tradition, spanning so many disciplines (Carrington, Scott et al. 2005; Newman, Barabási et al. 2006) that by some even referred to as a Kuhnian paradigm. Although social network ideas can be traced further back, contemporary network analysis was developed at Harvard in the early 1970's under leadership of Harrison White (Wellman 1988; Freeman 2004) with a great deal of inspiration and motivation from development in graph theory (Harary, Norman et al. 1965). Due to the complexity in analyzing even a small structure of social entities and their interrelations, mathematical tools are at the core of both theoretical and empirical network analysis. Indeed, social network analysis is a prime example of sociology "turning a profit from mathematics" (Freeman 1984). And the most remarkable fact is that while the general experience is that use of mathematics tends to scare sociologists off, network analysis is the home to scholars whose diverse research interests span from personal identity construction to epidemiology, from ethnography to analyzing the structure of the Internet.

Before turning to the interviews, I wish to highlight a few of the utilities that arise from using mathematics as a tool for social science. These will be touched upon also in the remainder of the paper. First of all mathematics is a much more exact language than the written or spoken word that brings clarification and coherence to theoretical arguments, which help eliminate ambiguous interpretation. Thus it is a very powerful tool. Second, mathematics is a unifying language that promotes communication and problem solving among otherwise conceptually separate scientific communities. In his book on the trans disciplinary Santa Fe Institute, Waldrop quote a discussion with Eugenia Singer claiming that "it was mathematics that provided the common language" and "if they had gotten a lot of social scientists in there with no technical background, I'm not sure the gulf could have been crossed" (Waldrop 1992:192). Thirdly, because it is a unifying language, mathematics provide a way to learn from other scientific domains that struggle with structurally similar properties. Mathematics can bring our attention both to the isomorphism between sociology and other sciences and by way of example suggest possible ways to apply mathematics in sociology (Coleman 1988; White 1997).

From social network analysis we learn that mathematics can serve the social sciences in just the same way as it serves the natural sciences. In the development of social network analysis mathematics has provided a tool to tackle complexity, a language to ease communication, and a way to bridge theoretical concepts in sociology, mathematics, anthropology, computer science, physics, etc. So we do know that mathematics can help us make considerable scientific progress. However, the rest of the paper take as s point of departure the fact that by and large, mathematics is rarely put to work in sociology and when it is, it is done by a small number of sociologists.

## *2. Interviews*

My interest in mathematical sociology has to do with the possibility of applying some mathematics in my own work, with the tradition in itself, and the biographies of mathematical sociologists. The first admittedly remains an ongoing struggle. But as a consequence of the latter – and by inspiration from Swedberg's (1990) book of interviews with economists and sociologists on the interface between economics and sociology – I carried out a set of interviews with a small number of influential contemporary mathematical sociologists. In parallel I studied mathematical sociology, trying to get a grip on what type of work mathematical sociologists really did. While that effort was published in a review article some years ago (Holme, Edling et al. 2004) I would like to take this opportunity to present some more of the interview material.

I conducted six interviews in the late 1990's. Chronologically, the interviews were done in the following order: Patrick Doreian (May 29, 1998), Harrison White (May 29, 1998), Philip Bonacich (August 22, 1998), Kathleen Carley (February 20, 1999), Peter Abell (May 3, 1999), and Thomas Fararo (August 9, 1999). The interviews did not focus primarily on the substantial sociological contributions of these scholars. Rather I used the interviews in order to find out how they turned to mathematical sociology, and from what background they came. And my prime interest was to get their view on the role of mathematics in sociology at large. I also wanted to hear what they had to say on the evolution, status, and future promises of the field in a broad perspective. I selected my victims on an ad hoc basis, formed by my own readings in mathematical sociology. I wanted to meet with people who had long experience from mathematical sociology and who were still contemporary leaders in the field. Obviously there are several biases in my selection and I wish to stress that even given my selection criteria, the list of people that would have been perfect to interview is quite long. One striking selection bias is my preponderance towards sociologists active in the USA. Another apparent bias is that there is only one woman in my sample. An obvious reason is that there are not many women actively pursuing mathematical sociology. However, a few additional women could easily be included if the set of interviews were to be extended or revised.

A special characteristic of the mathematical approach to sociological problems is that it involves many scholars from outside academic sociology, something that was clearly brought out in some of the interviews. Therefore it might seem awkward that I concentrate only on sociologists. Several persons of the past and present that are not sociologists have made substantial contributions to mathematical sociology. Names that spring to mind are Herbert Simon, Garry Becker, Dirk Helbing, and Robert Axelrod. But there are many others. However, I wanted to make sure that I talked to people who strongly identified both with sociology in itself and with mathematical sociology. As it turned out, at least Peter Abell only hesitantly agreed to the label while others did with some qualifications. This is Abell’s frank answer to my opening question on how he got into mathematical sociology:

> Let me say first that I do not regard myself as a mathematical sociologist. I regard myself as a sociologist who is often led to try and us mathematics because it is the only way of really being clear about complex phenomena. So I am not a mathematical sociologist, and I do not really regard myself as a good enough mathematician to so describe myself. (Peter Abell)

While both Patrik Doreian and Harrison White did identify stronger with the label mathematical sociologist they too did throw in caveats. Doreian, long term and highly dedicated editor of *Journal of Mathematical Sociology*, and since recently the editor of *Social Networks* said that,

> Given that I work in a lot of different substantive areas, and that I always use some mathematics at some point, I would have to say it is mathematical sociology. When I describe what I do, often I use mathematical sociology as a description. I have tried using "playing in a mathematical play pen", but that generates a similar response to saying mathematical sociology. Maybe the conjunction of ‘play’ and ‘mathematical’ was the problem! (Patrik Doreian)

And White, indeed one of the most prominent mathematical sociologists ever and also a key person in the development of contemporary network analysis said that,

> I would prefer sociologist, and theoretical sociologist. But I am also a mathematical sociologist, and that is a fine thing to be. But I do not want to be just a mathematical sociologist. I think mathematical modeling is an auxiliary; it is not the driving thing. I am a sociologist, and that is the whole point, you want to understand social phenomena, their political and economical aspects, and their anthropological aspects. (Harrison White)

These quotations do capture some of the contradictory essence of mathematical sociology. On the one hand, one is hesitant to name oneself a mathematical sociologist and even to claim mathematical sociology as a sub-field in its own because it is the sociological issues that are always at stake, and on the other hand one tends at the same time to value highly mathematical sociology as such. But the driving force is not to carve out a niche space on sociology but to contribute to sociology with a capital s. Rather, one is drawn to applying mathematics out of necessity in the strive for precision, as in Abell's case, or as a means to an end as in White's case, or one is just inclined to do so out of personal preference, as in Doreian's case, just as one might have a personal preference for some method or theory over others. I proceed now by turning first to the mathematical sociology, second to the mathematical sociologists, and finally to some concluding remarks.

## *3. What is mathematical sociology?*

So, what is this thing called mathematical sociology? In a sense the question directly targets the historically ever imminent tension over what is the core of sociology itself. Is sociology a humanist endeavor, the critical and reflexive voice in (post)modern society? Or is sociology a science of the social, similar to the way in which physics is a science of the natural? Is sociology maybe both? It might very well be the case that this tension in itself is really the core of sociology, as some have claimed (Lepenies 1988). Whatever is the case; with some confidence I would say that a mathematical sociologist certainly subscribe to the idea that sociology is a science of the social, possibly to the idea that it can be both. Of course this does not help much as sociology has not decided what it means to be a science of the social. But nor, do I believe, have mathematical sociologists. Just as is sociology at large, mathematical sociology is a very heterogeneous field (Freese 1980). What does constitute its borders is a shared conviction that mathematics can bring leverage to sociological analysis. Even though this might be debated, most sociologists, and including the ones I have interviewed, would make a very clear distinction between statistical modeling and mathematical sociology. Indeed, the literature on mathematical sociology and the interviews with mathematical sociologists suggest that mathematical sociology is strictly a theoretical affair. Thus, it is the application of mathematics to sociological theorizing that defines mathematical sociology. And mathematics is used broadly to include also logic and computer models. Formal theory could therefore be an alternative label and is often used as such. However, it would be at fault not to acknowledge that statistical modeling is the area within sociology in which mathematics has the strongest impact on the field as a whole. The statistical tools being put to work in network analysis, event-history analysis, and hierarchical modeling do bring added sophistication to quantitative sociology.

Nevertheless, classifications of mathematical sociology highlight the use of mathematics for constructing *theoretical* models of social phenomena. Even though many mathematical sociologists are using and sometimes even develop quantitative methods, they often point out explicitly that the use of mathematics in sociology should not be equated with statistics. And the people I interviewed all subscribe to the idea that mathematical sociology belongs to theoretical sociology proper. In general there is no agreement on what is theoretical sociology. Many things indeed go into that label. In this respect, mathematical sociologists seem to be a bit more united than the discipline at large.

Many, including myself, take the view that theoretical model building is an act of balancing realism, generality, and precision. In this act of balance, one will have to stand back in favor of the other two simply because all things cannot be achieved (Levins 1966; Heckathorn 1984). If one accepts this premise, one can continue discussing the pros and cons of model building. Mathematical sociology is often accused of sacrificing realism for precision. To some extent this critique is justified, but as was implied by Abell in the opening quotation precision might be what foremost attracts many of the proponents of the field. Moreover the way to manage this balancing act is not agreed upon. And to contrast, Fararo (1989) argues that realism is the driving force in model construction because we build the model only when there is something there to be modeled, and models are deliberately constructed as representations of the real world.

Obviously, model building is about making idealizations of a complex reality by using simplifying, and sometimes false, assumptions. However, few sociologists would base their models on obviously false assumptions if it means distorting the essential feature of the problem. Some of those that I interviewed did express doubts over some scientific endeavors perceived as over-theorized, such as mathematical physics and parts of neo-classical economics. It this respect, mathematical sociologists appear to safe-guard against loosing touch with real sociological issues and are very reluctant to withdraw into pure model development. This, for instance is Peter Abell's reflection on the relationship between mathematical sociology and sociology.

> If I look at contemporary economics, for instance, I am sometimes worried about the extent to which technical facility is so highly rated that people can spend their time immersed in the technical problems and loose sight of the fact that we are really trying to understand a complex world. They have to simplify the world that they want to look at to such a degree that one sometimes wonder whether it is worthwhile. I would *not* like sociology to take that direction. I think the great strength of sociology, if it has any strength, is that is has tried to take empirical complexity seriously, and has not done what some parts of economics have done, and I think we should preserve that. (Peter Abell)

Still, the issue is delicate, as it extremely hard to decide how much complexity and heterogeneity one should account for. I would argue in favor of a sociological model to be general enough to explain social phenomena across time and space, whereas some would claim that sociology is a historical science and as such no model can be applied across time and space. In addition sociological models should be precise, or else they cannot serve as hypothesis generators and consistency checkers in any substantial way; and those are two important functions for theoretical models (Carley 1997). Consequently it can be argued that realism will have to give way to generality and precision. Such a modeling paradigm is characterized by the expectation "that many of the unrealistic assumptions will cancel each other, that small deviations from realism result in small deviations in the conclusions, and that, in any case, the way in which nature departs from theory will suggest where future complications will be useful" (Levins 1966:422). To illustrate the balance of realism, generality and precision, I wish to give two examples of simple mathematical models that successfully walk the line. Incidentally, both models are submitted by non-sociologists, although Watts later transferred to a sociology department. Schelling's model of segregation is a classic example and Watts' and Strogatz's model of the small world is on its way to becoming one.

The motivation for Thomas Schelling's dynamic models of segregation was the observation that people get segregated across many dimensions and often by discrimatory individual behavior. "By 'discrimatory' I mean reflecting an awareness, conscious, or unconscious, of sex or age or religion or color or whatever the basis of segregation is, an awareness that influences decisions on where to live, whom to sit by, what occupation to joint or to avoid, whom to play with or whom to talk to" (Schelling 1971:144). But ethnic segregation was the prime concern at the time when Schelling proposed his model, and it is primarily in such light that it has been discussed. Yet, Schelling is very clear that the model as such is abstract enough that results can be generalized to any preference driven segregation process. Thus following Fararo, the model is fueled by concern for a real social issue and in that sense it is a realistic model. But it is also a general model claiming to shed light on a range of segregation phenomena. However, Schelling remarks early on that some very important factors that explain segregation are not included, such as institutionalized segregation (that he calls organizational) and economically driven segregation. In Schelling's models, the segregation process is driven by individual preferences only. In this respect they are also fairly precise models. Both initial and boundary conditions are clearly defined, but because the analysis was made by hand, agents' movement rules for instance are only loosely defined (but contemporary computer replications give exactly the same results).

One of Schelling's model is a two dimensional lattice of 13 rows and 16 columns on which 25 star-chips and 18 zero-chips are distributed randomly. Assuming that both stars and zeros have a preference for having at least 50 percent neighbors of her own type Schelling rearranges the chips so that this condition is met for every chip. The result is a strikingly segregated pattern in which a star have about 80 percent of her neighbors being stars and a zero has about 83 percent zeros in her neighborhood (in the random initial distribution the corresponding numbers was 46 and 53 percent).

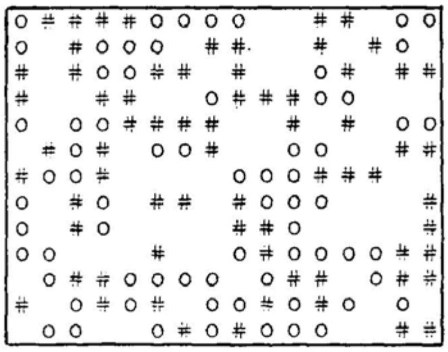

Fig.7

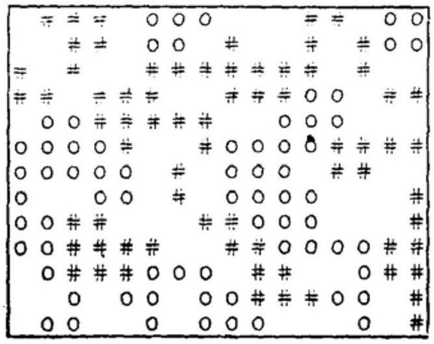

Fig.9

*Figure 1. Initial random distribution and final equilibrium distribution in Schelling's dynamic two-dimensional spatial proximity model of segregation Schelling 1971:155-157).*

Schelling's dynamic model of segregation is, if the unintended pun is excused, a role-model in mathematical sociology in many ways. Not only does it strike the balance between realism, generality, and precision perfectly it also generates surprising, and even counterintuitive results. Recall that in the model, segregation emerges solely from the action of uncoordinated individual actors and in that respect is a beautiful example of the link between micro-interaction and macro-dynamics.

The rationale for Duncan Watts' and Steven Strogatz's model of small world dynamics (Watts and Strogatz 1998) is not motivated by a pressing social issue such as segregation. Rather it is the answer to a puzzling question of how to understand a particular network structure. For a long time the ideal type networks that mathematical sociologists worked with were either random networks or ordered networks despite the fact that all sociologists knew empirically that social networks are nor random or ordered. For one, social networks are clustered, that is, people tend to form tight knit groups that are distinguishable from other groups. Trying to capture this intuition mathematically, for instance by modeling biased networks, is hard. A particularly interesting and popularized network communication phenomenon is the so-called small-world effect. This effect was illustrated experimentally in the 1960's with the implication that only six intermediary steps separate any two Americans. A question that had deluded sociologists since these experiments and that captured Watts' imagination was this: what makes a network small? In Watts' model actors are put in an ordered circle and every actor is connected to its four nearest neighbors. One of the actors is selected and at random one of its connections re-connected to another randomly selected actor in the ring. As process of re-connection is repeated, the ring transform from an ordered network structure into a random network structure as in Figure 2 (Watts and Strogatz 1998).

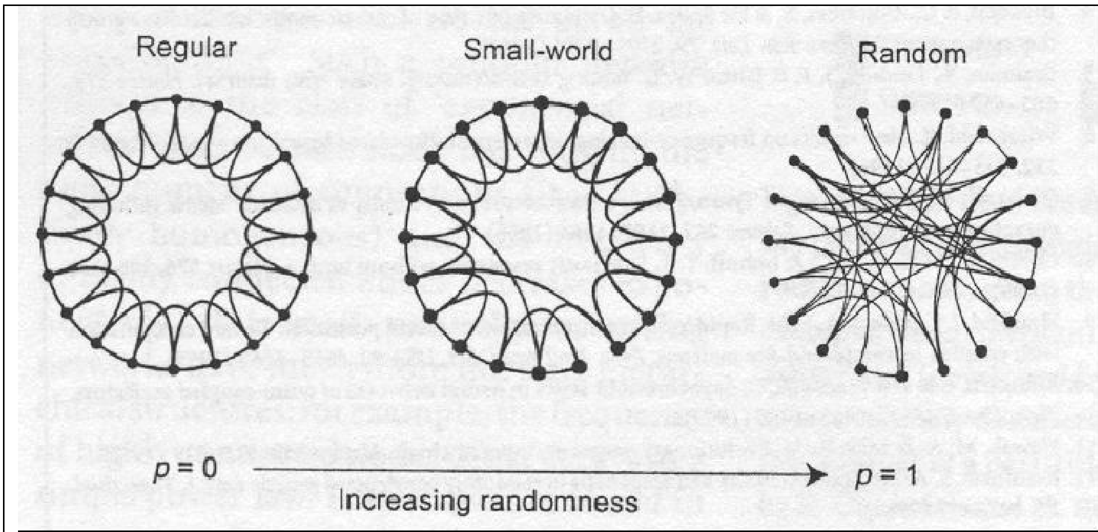


*Figure 2. Watts' and Strogatz's small-world model. An ordered network (left) is transformed into a random network (right) by a process of randomly re-connecting actors. The small-world is situated in between these extremes (Watts and Strogatz 1998).*

Starting from a very simple model of an ordered network structure Watts could demonstrate that what is characteristic of a small-world network is both a high level of clustering and short average distance between actors in the network. Thus a small-world network seems to capture both the property of random networks, i.e., short path lengths, and ordered networks, i.e., high clustering. What is really interesting with Watt's model is that parameter *p*, the degree of randomness, need only be very small for the ordered structure to transform into a small-world network.

Watts' small-world model is even simpler than Schelling's model of segregation but still very elegant. In terms of the balance between realism, generality, and precision the judgment is perhaps not as clear cut. The model as such is not particularly realistic in the sense that it doesn't even abstract from observed social reality. Few sociologists would feel at home at this level of abstraction. It is on the other hand general way beyond sociology; the network properties under analysis are present in many biological and physical networks, both natural and designed. And it is precise and well-defined. In contrast to Schelling's model this one needs a fair deal of interpretation before it can be mapped onto social reality. But it does highlight important properties in almost all social networks that are now, because of this model, easier to understand and theoretically scrutinize.

These two examples do bring out both the attractions and the potential shortcomings of mathematical sociology that I believe is nicely captured by the balance of realism, generality, and precision. Another way to phrase this problem is in terms of a conflict between sociological intuition and formal theorizing, as Fararo does in this quotation.

> I always think about it, as we always know more than we can say. And we always can say more than we can really formally put down in more exacting terms. So as you go further and further from the fundamental intuitions in the interest of being logical and mathematical, you can potentially loose contact with the governing intuitions. But the main gain would be to try to bring the mathematics back into, and as close as possible to the basic intuitions of the field. Trying to represent those intuitions in some way. It is what Bourdieu calls a habitus. It is a kind of a sociological habitus you acquire by exposing yourself to classical ideas and postclassical ideas and so on. You know, you think sociologically, and then you think mathematically. But these are often hard to fit together. The mathematics enforces a discipline that the other discipline does not really value in the same way. It has it is own forms of rigor but they are not the same. To bring those two into conjunction has always been the sort of think that I thought of as important. I do not think we've been that successful really, but collectively we are trying. (Thomas Fararo)

This challenge might be especially tricky for sociology, a subject that attracts students from a wide population, not seldom driven by strong political and social interests rather than analytical.

> The problem is that many people come into sociology not really seeing it as a hard science. They see it as a sort of spiritual adventure of knowing about society in a qualitative, grounded way, to be able to reflect upon the big political and social policy issues. In a sense, I came to sociology a bit like that, and I

> have respect for that. I think that if we would loose that, we would loose something important. What we need to do is to add to that the idea that technical grounding is important if you really want to make a contribution. By and large, my generation of sociologists has failed. There are a few pimples on the top that look good: James Coleman, and probably Harrison White. But it is not a story of great success. (Peter Abell)

As far as I am aware no extensive piece on the history of mathematical sociology is available. Some notes are spread around in reviews and books (e.g., Fararo 1997) and there seem to be agreement that mathematical sociology as we apply the term today is a child of the 1950's. I believe the recant revival and interest in mathematical sociology is mainly due to the enormous interest in structural and relation sociology as it is approached by social network analysis. Admittedly, network analysis is both methodology and theory, but its theoretical components have always been mathematically charged (Freeman 2004). Network analysis has been constantly growing within sociology since the late 1970's and with the advent of the "new science of networks" in the early 2000's (Watts 2004) interest has sky-rocketed. Other developments also spur the re-vitalization of mathematical sociology, such as the growing use of computer simulation models across the social sciences (Macy and Willer 2002) and current theoretical debates in European sociology on social mechanisms (Barbera 2004) and analytical sociology (Hedström 2005). In the mid 2000's, mathematical sociology is visible in state of the art journals, and the range of problems approached and the level of sophistication are striking.

In my review of mathematical sociology, following Sørensen (1978) I classified mathematical sociology along three broad strains -- process, structure, and action -- based partly but not entirely on the different types of mathematics involved (Holme, Edling et al. 2004). With models of processes one typically study social change over longer or shorter time-periods using various types of discrete and continuous mathematical models. Mathematically, these models have a lot in common with various system-analyses in other fields, ranging from physics to ecology, and usually they do not allow for much in terms of structural or individual heterogeneity. Diffusion models, such as James Coleman's famous analysis of medical innovation (Coleman 1988), and models of organizational change as developed within organizational ecology (Hannan and Freeman 1989), are typical examples of process models.

If Coleman can be said to be the father of the mathematical sociology of process, then Harrison White would undoubtedly be named the father of mathematical sociology of structure. Models of structure tie directly into social network analysis, their most important field of application in which graph theory and matrix algebra are primarily used. However, as more and more attention is directed towards understanding system dynamics and structural change, it makes little sense to uphold a clear cut distinction between process and structure as research enterprises. Describing a structure without accounting for its emergence and stability is simply not satisfactory. Patrik Doriean expressed this clearly in our interview.

> It is interesting that you have this history of James Coleman coming in from one side with his kind of dynamics models, and Harrison White coming in from another side with his kind of structural models. It would be great if we could synthesize those two traditions and really model social structure and social process in an integrated and coherent fashion. […] My sense about mathematical social science is this: let us do it. If we are successful great, if we are not successful, then we admit that we failed. I think that if we model successfully the dynamics of structured systems – networks with social objects and relations between them with levels and multiple populations – we will have done very well. Structures evolve through time and we need to be attentive to both the structural characteristics and the social actor characteristics. If we succeed, that is when I think we will really have successful mathematical social science and successful social science. It sounds very methodological and programmatic, but I think that is what we should be aspiring to. And that would be a synthesis of the White and Coleman traditions. The physicists at the Institute for Theoretical Physics in Stuttgart that have contributed to the Journal of Mathematical Sociology (Helbing 1994; Weidlich 1994) and use partial differential equation models that moves us into the whole debate of complexity and chaos. At the moment they seem more like buzzwords. I think that if we can successfully use that kind of modeling and be genuinely concerned with structure, and genuinely concerned with process, then we are going somewhere. Let me add that I am skeptical that the models from physics in an *unmodified* form being useful. But I think they need to pursue their program – just as mathematical sociologists need to use mathematics. If they succeed then we will have learned much. (Patrik Doreain)

Dorian also draws our attention to the third type of models, models of action. Prototypical examples of such models would be those of utility maximizing individuals. Indeed, James Coleman proposed that the simple and well defined assumptions about individual rational choice should provide the foundation for social theory (Coleman 1988). This is not yet the case, nor in sociology at large (but see Goldthorpe 2007) nor in mathematical sociology. On the contrary, if there is one thing that my earlier review (Holme, Edling et al. 2004) clearly show it is that mathematical sociology span across a broad range of phenomena and apply an equally broad set of mathematical tools to try and understand those phenomena.

## *4. Who is the mathematical sociologist?*

The interviews can be summarized along four themes: the scholarly background of the interviewees, their attraction to mathematical sociology, their thoughts on the strengths and weaknesses of mathematical sociology, and their thoughts on institutional problems and limitations.

With respect to their background the six scholars fall nicely into two categories. I realize it is dubious to classify this small number of observations, but nevertheless I will do so as it serves the purpose of my presentation. Three of the interviewees immigrated to sociology from a relatively speaking, mathematically strong background. Peter Abell came to sociology at Essex via philosophy with a PhD in physical science to do philosophy of sciences. Patrick Doreian earned a Bachelors degree in mathematics before deciding to enter a cross-disciplinary program in mathematical social science, and Harrison White switched to sociology after completing a PhD in physics. The other three came into sociology much earlier in their academic career and mathematical and computational modeling was brought into their thinking parallel to or even after their maturation as social scientists. Philip Bonacich says he was heavily influenced by a strong role model at Harvard, namely Harrison White, Cathleen Carley was early on seduced by the possibility of modeling man, and Thomas Fararo was philosophically committed to axiomatic science. One of the nicest stories about the attraction of mathematical sociology is indeed that of Kathleen Carley:

> [My] interest in modeling started […] when I was in high school, when I read Isaac Asimov's the *Foundation Trilogy*. I thought this is what I want to do:

> Build models of people! So then I went to college and tried to take every course I could to make that happen. […] Actually I was very interested in artificial intelligence; but they did not have a degree in it yet in that point in time. And I did not realize that you could get what would become the degree by going through engineering, so I did not do that. So, I do not have a degree in AI, and I was actively discouraged from going into mathematics because I was female. I was told: You'll never make it because you're a woman". And I though, Ok, they must know what they are saying. (Kathleen Carley)

Incidentally, Carley shares her fascination with Asimov’s novels with Duncan Watts, who used Asimov’s *Robot* series as an inspiration for one of his models of emergent system behavior (Watts 2003:74). But Carley’s account is of course also a sad reminder of the kind of gender stereotyping that helped and still helps pushing male and female students into different college programs.

The difference between a science and a social science background becomes somewhat visible in the view on mathematics in sociology. For example, the three persons with a science/mathematics background are quite pragmatic and non-programmatic in arguing that mathematics has its place but that it is not an end in itself. The three persons with a social science background much more strongly beliefs that mathematics is intrinsically good and something to generally strive for. One reason the first group has a more relaxed attitude could be that they had an early training in mathematics which it is both easier and more natural for them to call upon when it seems fit. The other group on the contrary, had a less thorough, and sometimes more shallow, training and therefore they had to, and have to, fight harder for the mathematical leverage.

One should of course be cautious with personal accounts of ones own career. It is all too easy to find a pattern in ones personal history, and perhaps even easier to paint this pattern in self-flattering strokes. For instance, how come Abell, Doreian, and White left the hard sciences to go into sociology? Is not the most probable answer that they had failed in these areas, or that they where sensing failure coming? Assuming they wished to pursue an academic career, both physics and mathematics holds much more status and resources than do sociology, so the sensible thing would be not to transfer. Only if failure is certain would it pay off to switch a career in physics for one in sociology. On the other hand, is it so strange that people actually hold a strong interest in the subject matter? Being a sociologist myself, I actually find that quite easy to believe. Sociology is a rich and fascinating discipline and it relates to everyday experiences and intuitions in a myriad of ways. Still, it is probably safe to assume that none of the three immigrants was doing remarkably well in their original fields. If they were, they would not have been allowed to repel but would have been sucked deeper into the attraction of disciplinary, departmental and collegial structures. But, picking up on the rationality once again, the fact that among all alternatives they choose sociology actually strengthens the interest-hypothesis, because if they wanted a trade-off from their training, engineering, business administration, or economics would have been a much more reasonable pick.

In some sense, the social science group is more interesting. People migrating from science and mathematics into sociology do formal theory because this is how their training wired them to think. But people that start out in social science and decide to bring in mathematics and other formal tools actually turn away from the highway and pick the straight and narrow road. I think Abell is right in his analysis that the rational choice in contemporary sociology is to not do formal theory.

> I think at some stage sociology just has to face up to that. Sociology is so diverse, and because it shades of into history, there will always be this other end of things. But it has to change. I am not optimistic about it changing in the foreseeable future, because I think the present "social construction" of sociology is such that there is a mutual interest between teachers, students, publishers, etc. to maintain the present paradigm. In my view it is intellectually sub optimal. Nevertheless, it is equilibrium. Everybody wants it and everybody in it would be worse off by moving away from it. So, you find it very difficult to break that. Human history is littered with failed intellectual tradition. Not empirical sociology, but social theory is a failed intellectual tradition, and future generations will necessarily see it as such. At the moment, though, it is relatively stable. (Peter Abell)

If Abell is right, one might wonder why mathematical sociologists continue to pursue mathematical sociology. One possible route to answering that question is to look at what these persons see as the strength of mathematical sociology. Philip Bonacich admits to hold what he calls a mystical belief saying,

> Well, part of it is kind of a mystical belief. It is a non-rational belief that if there is a deep structure in sociology, and deep truths, they will only be discovered through mathematics. But then apart form that, in a more mundane kind of way, I think that mathematics leads to precise thoughts and precise thinking. The concepts, as opposed to English, are completely unambiguous. There is tremendously powerful machinery developed over hundreds of years to draw implications from. I personally think that the finest things about western civilization are music and mathematics. So I think there is this powerful logical apparatus that we should avail ourselves in. […] I think that if we are going to become a science, we have got to use mathematics. (Philip Bonacich)

In Bonacich's case it is a firm conviction that sociology will make progress from the power of mathematics. Thomas Fararo gives a rather thorough account of how he got into mathematical sociology and what he sees as the attraction. Mainly it is the power of axiomatic theory construction and the possibility of theoretical unification. Fararo have presented arguments for unification both with and without formalization, but really strong axiomatization cannot be achieved without formal arguments and so, in Fararo's case, mathematics becomes a necessity out of the preferred style of scientific explanation.

> In the summer of 1960 when I was a graduate student, I needed some funding, and I was recommended for a sociological research project that was just beginning. That was the study of community power structure in Syracuse under the direction of Linton Freeman. That for me was really a transformative experience because it was knowledge as a problem of something to be constructed rather than something read in the library. It really put me in touch with the constructive research process. So sociology then became a place for me where knowledge is constructed rather than just read about. […] But in the context of the community power structure research we were confronted with kind of a structural problem. We had a massive sociogram, as it were, of claims about who was involved in what. We were looking for some kind of mathematical apparatus; we did not even know the name of it, but something to analyze this kind of thing. We did not know of anything, so I undertook that as a direction of my thesis research. I spent a summer reading the *Bulletin of*

> *Mathematical Biophysics*, which was a place where people like Anatol Rapaport were writing. In my thesis I really just practice and applying the idea of mathematics and mathematical model building to this community power structure data. Basically, that whole idea of constructing and testing a mathematical model, which was totally new as I did not have any classroom training in it at all. […] Remember, I came to mathematics from philosophy so I was very interested in things like; what is a mathematical entity, what is the relationship between mathematics and logic? So I read a lot about the philosophy of mathematics too. I taught Patrick Suppes' book, *Introduction to logic* (Suppes 1957). A fabulous book. And I was very attracted to this idea of axiomatization within set theory. So actually, the first two years of my post-doctoral I worked formally under the sponsorship of Patrik Suppes at Stanford. (Tomas Fararo)

Indirectly, Kathleen Carley who went to MIT inspired by the vision of modeling man in society, argues that the greatest utility from mathematical sociology arise from the fact that the substantial research funding goes to projects with some mathematical and computational sophistication, and therefore a formal approach increases the survival chances of sociological research (having spent a great deal of her career at Carnegie-Mellon University, Carley is probably affectively colored by this engineering heavy environment). Bonacich, Carley, and Fararo are all convinced, but not at all by the same arguments, that mathematics is necessary.

Nor Patrick Doreian or Harrison White would go as far as to argue that mathematical sociology is necessary. White admits that he find it hard to believe that progress will be achieved without mathematics, but Doreian simply rest his case, does his thing, and leave it for others to decide on the utility of what he is doing. Peter Abell, who also came from science to sociology, share with the social science group a strong belief in the value of mathematics. He actually argues that social scientists are morally obliged to be “clear and precise” in their thinking, and that mathematics is needed to guarantee this.

Some of the interviewees also commented specifically on weaknesses and potential risks of mathematical sociology. Peter Abell for instance, compares sociology to economics and warns that in economics the mathematics is sometimes valued so highly that it overshadows the substantial issues. He argues in effect that our curiosity of empirical complexity always should have the upper hand over technical proficiency in defining what problems to study. In the same line of reasoning, Harrison White points out that even if the mathematical approach by itself is praiseworthy we need to be aware that the mathematics we need might not actually be the mathematics we have access to at the moment. As a result, sociologists must have a flexible attitude towards mathematics and, which of course is much, much harder, sociologists must keep a constant eye on development in mathematics. Looking at the same issue from another angle, Thomas Fararo sees a problem in bringing mathematics and sociology together. To his mind they represent two different modes of thinking, two intuitions, which are not always easy to link. The risk arises when one continues to construct mathematical models even when the mathematical and the sociological intuition are not on touch with each other. This leads to the same problem that Abell identified in economics, which arises when the models no longer represent the sociological intuitions.

## *5. Concluding Remarks*

Obviously, a very positive impression of applying mathematical models to sociology emerges from these interviews. All six scholars I talked to share the view that perhaps the most prime benefit of a formal approach is to achieve coherence and logical consistency in theory construction. Also, there are several signs that mathematical sociology in the beginning of the new millennium is stronger than it has been since the late 1960s. New publication outlets, a section in the American Sociological Association, and an increased exposure of mathematical and computational models in general sociology journals serve to prove this point. My interviews bear witness that there are positive adherents of mathematical sociology, and my literature review (Holme, Edling et al. 2004) provides references to impressing work on many timely problems. At the same time, it is obvious that mathematics has not established itself as a general tool in the discipline.

Several people interested in mathematical sociology testify to the institutional fragility of mathematical sociology, evident en many ways, and most likely derived from the small number of adherents of this sub-discipline. For example, both Peter Abell and Patrick Doreian got into mathematical sociology through a program at University of Essex. This lead me to believe that there was actually something exciting going on in mathematical sociology in England in the late 1960’s. Apparently, this was not the case. Apart from these two, no lasting contribution to the field evolved out of Essex. Doreian’s unconcerned and laconic observation that the program "died" lead me to conclude that although there where certainly an inter-personal xenogamy of ideas going on (Doreian 1970; Abell 1971) the program in itself is not much to be spoken of. Indeed, Doreian left for a position in USA in the early 1970's and Abell, who remained in England, become devoutly disillusioned of the future of any version of mathematical application in sociology.

Patrick Doriean became the editor of the *Journal of Mathematical Sociology* in 1982 and still is. It is beyond doubt that the journal has survived much through his hard work. While this has saved the journal from going under, it is possible that it has also increased its vulnerability by attaching survival chances to the energy and professional contact network of one single individual. Again, this is the problem of a small field. A parallel case is the International Network for Social Network Analysts (INSNA), which was founded and for several years single handedly run by Berry Wellman. With the growing popularity of network analysis, INSNA has become a firmly established organization and is no longer dependent on one dedicated person, but for a long time it surely was. But, according to Doreian's account, even after over 30 years since its inception the *Journal of Mathematical Sociology* struggles to stay alive. A small field simply cannot support a specialist journal, and especially so at times when institutional subscription rates are sky-high and academic libraries experience budget shrinkages year after year. It is not the case that there are no outlets for mathematical sociology, several papers in general sociology journals draw on mathematical or computational modeling. Indeed, this is a very positive thing for mathematical sociology.

Another example of institutional fragility surfaces most clearly in the interview with Phil Bonacich in the discussion about the section for mathematical sociology in the American Sociological Association (ASA). This might sound as if a national assemblage, but as several of the other ASA sections, this section is truly international and thus it can be regarded as the international forum for mathematical sociologists. The ASA only saw the formation of this dedicated section for Mathematical Sociology in 1996. The section is still one of the smallest in the ASA and in 2003 the section had just over 160 members (to be compared to the mean of all sections, which is approximately 470 members). However, the bylaws of ASA stipulate that the minimum requirement for holding section status is 300 members, so there is an actual risk that the section will have to shut down. Although in practice, according to Bonacich's experience, this will be highly dependent on section activity. In recent years sessions at the annual ASA meetings have been quite successful, so if this is true perhaps there is no real reason to worry. But still, small is weak when it comes to making lasting contributions in an organizational environment with competition for members.

It might be interesting to compare with other small sections in the ASA. Like most sections of the association many of these small sections define pretty narrow interests. In 2003, the following sections had less than 300 members: Animals and Society, Communication and Information Technology, Ethnomethodology/Conversational Analysis, History of Sociology, Latino/a Sociology, Mathematical Sociology, Peace, War and Social Conflict, Rationality and Society, Sociological Practice, and Sociology of Emotions. Among these, the sections for Ethnomethodology and Animals and society are new, instituted in the 21st century. The oldest is Peace and War that goes back to the mid 1970s followed by Sociological Practice that got started in the late 1970s. Some of these sections have to be considered to be of minor interest, given the nature of sociology. The interest for Latino sociology, Etnomethodology or Animals and Society for example simply isn’t that widespread among sociologists. However, all three of these had more members that the section for Mathematical Sociology, whose members would claim that they represent were broad and general sociological interests. In fact, among the small sections, only Rationality and Society had fewer members than Mathematical Sociology in 2003. Really large sections, with over 900 members are few. Here we find Organizations, Occupations and Work, and Sociology of Culture, Medical Sociology. Largest, with over 1000 members, is the section for Sex and Gender.

But the biggest obstacle to any substantial expansion of mathematical sociology within sociology is the firstly fact that sociology students have very little technical training when they enter sociology, and are given very little further training once they are in. Secondly, and perhaps more surprisingly, in most universities sociology students have no exposure to formal theory construction. And for students with low technical motivation and no training in theory construction mathematical models in sociology are perceived to be so esoteric that nobody needs to care. But the proper use of mathematics is not a question of choice. When the problem is formulated precisely enough the use of mathematics is unavoidable. One should not simply take the mathematics and apply them to a sociological problem. The problem has to be thoroughly worked through and is necessary, then be given a mathematical formulation. The reason we do not se (and perhaps do not need) much mathematical sociology is simply a reflection of the nature of contemporary sociology. A reasonable expectation is that mathematical sociology will be taken care of by other disciplines, and as a consequence it will be difficult to find people in the future, who like the ones I have interviewed, are dedicated both to sociology and to mathematical sociology.

On the other hand, as we have learned from the example of social network analysis, given a dedication to solve a set of real world problems, the turn towards mathematical tools will be unavoidable and come natural if needed. It seems natural to end this paper with the following advisory note from James Coleman’s *Introduction to Mathematical Sociology* (1964:54); “the necessarily difficult task of developing mathematical sociology can best be performed when our concentration remain upon the sociological problem, and the mathematical tools remain means to an end.”

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