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# Actions and Networks: Sociology that really matters... to me

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# Actions and Networks: Sociology that really matters... to me

by Peter Hedström

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## Introduction

In order to understand the somewhat unusual and self-centered focus of this article, some background information is needed. It was originally presented in a seminars series at Nuffield College organized by me and Duncan Gallie. The seminar presenters were encouraged to describe some of the most important projects they had been working on over the years and to make explicit the reasons for why they had chosen these specific studies and not others. So what I do in this paper is in a semi-informal manner relate some projects that I have been working on and thereby try to delineate the characteristics of the sociology that *really* matters – to me.

Before getting into my own research and my broader research agenda, let me just say a few words about possible relationships that may exist between theory, data, methods, and substantive problems. Figure 1 enumerates some possible configurations.

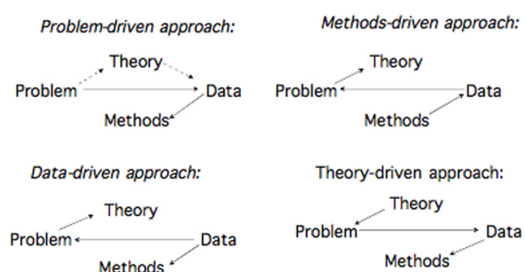


FIG. 1. Common sociological approaches defined on the basis of possible relationships between theory, substantive problems, data, and methods.

In the *problem-driven approach*, one starts with a substantive problem of interest, this dictates the type of data to be used as well as the theories that may be relevant. The type of data in turn will influence the choice of methods being used for analyzing the data.

In what I here refer to as the *methods-driven approach* one instead takes the point of departure in a specific method. The choice of method will dictate what type of data is appropriate, and this in turn the problems that can be considered. Some sociologists have become experts on time-series modeling, for example, and this influence most of what they work on. They study a range of different topics, but the choice always is dictated by the preferred method; unless there are sufficiently long time series available, this method cannot be used, and therefore they do not study it.

In the *data-driven approach*, the choice of research topic instead originates in the data. One has access to a certain data set which determines the problems that can be addressed as well as the methods that are appropriate.

In the *theory-driven approach*, one instead takes the point of departure in some specific theoretical ideas about how a slice of reality is likely to operate. Often the theory suggests that a class of substantive processes is likely to operate in a certain way, and this guides the search for appropriate data and methods. As far as is this type of approach is concerned, it useful to distinguish further between two sub-categories (see Figure 2). They differ from one another in terms of the stage at which data considerations enters the picture.

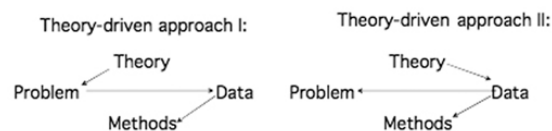


FIG. 2. Two types of theory-driven approaches.

There are arguments favoring all of these approaches, but in my view the arguments are particularly strong for the problem-driven and the theory-driven approaches. The choice between these two approaches perhaps mostly is a matter of taste. Some like to take their point of departure in a specific problem that interests them or in an empirical puzzle that intrigues them, while others take their point of departure in theory.

For better or worse, the approach that always have intrigued me the most is the theory-driven one. With a theory driven approach I do not simply mean research that makes some salutary references to the founders of our discipline. Nor do I mean research where the “theory” simply is a set of hypotheses derived from the existing

literature. What I have in mind is research that takes its point of departure in some very specific ideas about the mechanisms likely to have brought about the type of outcomes that one seeks to explain, and a research design that seeks to minimize the gap between the theoretically postulated mechanisms and the data being used in the empirical analysis [see Hedström 2005 for a detailed discussion of this type of approach].

With these introductory remarks, let me move on to some of my own research.

### **Earnings inequalities within organizations**

One early effort of mine to specify mechanisms and to design research that minimized the gap between theory and empirical research, concerned earnings inequalities within organizations [see Hedström 1988; Hedström 1991]. I took my point of departure in some of the sociology of organizations literature such as Blau [1970] and in some ideas of Herbert Simon [1957]. What I tried to do was to devise a relevant theoretical model, derive predictions from the model, and then test these predictions with empirical data.

One basic foundation of the analysis was the observation that organizational rank and pay often are closely linked to one another, that is, people higher up in the organizational hierarchy typically are better paid than those at lower ranks. Another basic foundation was the observation that most organizations are hierarchically structured with gradually fewer positions at higher organizational levels. The theoretical model that I developed took these common features into account and I sought to theoretically examine what that might imply for the earnings distributions we are likely to observe within organizations.

Let me briefly describe the model. If we let  $n_i$  be the number of jobs at the  $i$ -th level of the hierarchy (where level 1 is the lowest organizational level), the model is built upon the assumption that the shape of the organizational hierarchy is governed by a single parameter  $S$ ,

$$S = \frac{n_i}{n_{i+1}}$$

and the smaller this parameter is, the leaner and more peak-formed the hierarchy will be. For example, if  $S = 3$  and the total number of jobs in the organization were 13, the hierarchy would look like the one in Figure 3.

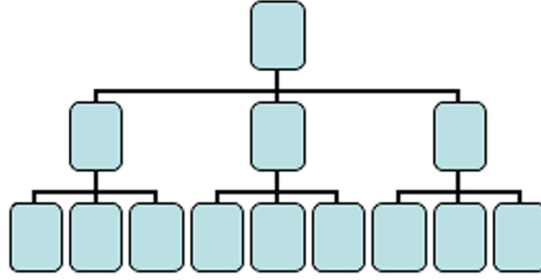


FIG. 3. Hypothetical organizational hierarchy with a span of control equal to 3 and a size equal to 13.

The next step in the theoretical analysis was to build a pay structure into this organizational model that takes into account the fact that rank and pay are tightly coupled in most organizations. Simon offered a particularly attractive solution to this problem in suggesting that managers' and supervisors' pay can be approximated by taking their immediate subordinates' pay and adding a constant percentage. The pay at the  $i$ -th hierarchical level ( $w_i$ ), then simply can be expressed like this,

$$w = w_1 \beta^{i-1}$$

where  $w_1$  is the pay at the lowest hierarchical level, and  $\beta$  is the relative difference in pay between two successive hierarchical levels.

On the basis of this, one can then derive how the earnings distribution within an organization is related to its organizational shape. If we characterize the earnings distribution with the coefficient of variation, a commonly used inequality measure, after some tedious but straight forward math, we find that the relationships look like this:

$$CoV_i = \frac{\beta_i - S_i}{\beta_i^L - S_i^L} \sqrt{\left( \frac{\beta_i^{2L} - S_i^L}{\beta_i^2 - S_i} \right) \left( \frac{S_i^L - 1}{S_i - 1} \right) - \left( \frac{\beta_i^L - S_i^L}{\beta_i - S_i} \right)^2}$$

This equation shows how the earnings distribution within an organization (CoV) is related to the shape of its hierarchy (S), the total number of levels in the hierarchy (L), and the pay difference between two successive levels ( $\beta$ ). Clearly it is not easy to tell from this equation what the relationships actually look like, but if we plot the equation holding the value of  $\beta$  constant, the predicted relationships are more easily discerned (see Figure 4).

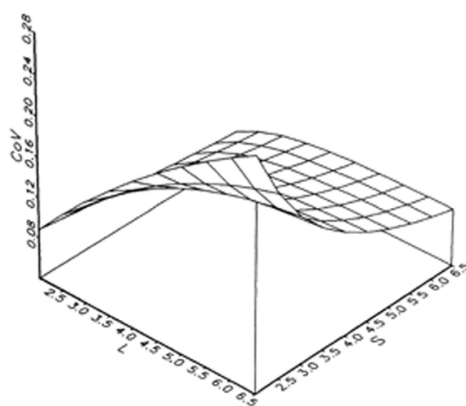


FIG. 4. Predicted relationships between earnings dispersion (CoV) and shape (S) and height (L) of the organizational hierarchy.

Figure 4 shows how the earnings distribution is likely to vary with the height (L) and shape (S) of the formal hierarchy. Furthermore, it suggests that if we knew the height and shape of an organization's hierarchy, we should be able to predict reasonably well the extent of earnings inequality within it.

In order to test the predictions or implications of this model, I used a Swedish data set from 1976 which contained data on basically all white- and blue collar employees in Swedish manufacturing industry. All in all, it included information on about 700,000 individuals who worked in about 5,600 establishments. I estimated a series of ordinary linear regression model where the size-distribution of earnings within the organizations (measured with the coefficient of variation) was regressed on measures of the height (number of levels) of the hierarchy, and the shape of the hierarchy. I will not go into any details about the results, but the predictions from the theoretical model were confirmed, the regression model accounted for a substantial part of the differences in earnings dispersions between different organizations, and the results were fairly robust also when controlling for educational and experience variations within the organizations as well as their gender composition.

I must admit that I am still rather fond of and believe in this general idea, i.e., that earnings inequalities in part can be explained as an unintended outcome of actors' efforts to design what they believe to be appropriate organizational structures. An implication of this is also that changes over time in the overall earnings dispersion in a society may in part be the result of changes in the organizational demography of the society.

I also did some related studies on how organizational careers and earnings attainments were influenced by the organizational structures in which the individuals were embedded [Hedström 1992; Hedström 1994a]. In all of these studies I had rather clear ideas about how the social structure was likely to influence the outcomes

I sought to explain, but the actors themselves remained passive and silent or at least implicit and under-theorized.

### **Action-based explanations**

In my view, it is essential to try to explain social outcomes with reference to the actions that brought them about, without losing sight of the fact that actions are constrained and influenced by the social structures in which the actors are embedded. The main reason for adopting an action approach is that we thereby get a deeper understanding for why we observe what we observe because all societal changes are the intended or unintended outcomes of individuals' actions.

As soon as one seeks to develop action-based explanations of social outcomes, rational-choice theory crops up as a strong contender. Initially I was extremely optimistic about the role rational-choice theory could come to play in sociology, and I was one of the founding associate editors of *Rationality & Society* when it started in the late 1980s. But over the years my views gradually have become more tempered. To a considerable degree this skepticism has its roots in the instrumentalism that characterizes so much of rational-choice theorizing. In my view, one should always strive for explanations that detail the type of mechanisms that actually were at work in producing whatever it is that one tries to explain, and this realist view is not shared by most rational-choice theorists. In fact, I think it is fair to say that instrumentalism is an important part of the theoretical heritage of rational-choice-based analyses.

The classic arguments for the predictability-motivated form of instrumentalism were presented by Milton Friedman [1953]. According to him, the idea of basing theories on realistic assumptions is an illusion and therefore the choice of theoretical assumptions should not be guided by how realistic they are, but by how accurate the predictions they generate are. As-if stories are fine according to Friedman as long as they allow us to predict what we try to explain.

Most rational-choice theorists are not instrumentalists in the same sense as Friedman, however. They justify their choice of theoretical assumptions neither on the basis of what appears to be realistic nor upon what generates good predictions. Rather their choice of theoretical assumptions is, at least in part, dictated by their preference for simple models with clear analytical solutions.

This form of instrumentalism, in which assumptions are seen as instruments that can be freely tinkered with until one arrives at simple and elegant models, is not uncommon among mathematically oriented rational-choice sociologists. One ex-

ample is Coleman's analysis of school grades, published in the *Foundations of Social Theory* [1990]. Although I would hold Coleman to be the most important sociologist of the second half of the 20<sup>th</sup> century, in some of his work he displayed an unfortunate instrumentalist tendency. In these analyses of school grades, for example, without any real justification, he simply assumed that the relationship between a teacher and his or her students was identical to that which exists between buyers and sellers in a perfect neoclassical market. Introducing these assumptions allowed him to use mathematical models and perform analyses that he otherwise would not have been able to do, but it also meant that his analysis came to be based upon clearly false premises. No matter how elegant the resulting model was, the explanations and results derived from it must be called into question because the mechanisms and processes assumed in the model had little or nothing to do with the actual processes through which the grades he was trying to explain had been brought about.

I think it is important to make a distinction between descriptively *false* and descriptively *incomplete* statements. The distinction can be described in the following way. If we have a set  $A = \{a, b, c, d\}$  and we assume that  $A = \{e, f\}$ , our assumption will be descriptively false, while if we assume that  $A = \{a, d\}$ , our assumption will be descriptively incomplete. In the former case we ascribe to  $A$  characteristics which it does not have, while in the latter case we assume  $A$  to be what it is only in part, that is, we accentuate certain aspects by ignoring others. While descriptive incompleteness appears to be a defining characteristic of all theories because they always focus on limited aspects of a complex totality, there cannot be any advantage in basing theories on fictitious assumptions, as Friedman and many other have implied [see also Sen 1980].

Being a social scientist can often be frustrating because our subject matter is such that we are rarely able to specify theories that are as precise and mathematically elegant as we would like them to be. But the temptation to invent entirely *fictional* worlds because in such worlds we can formulate more elegant theories is something that should be resisted. We should always aim for precision, but not for excessive precision if that simply entails fictional accounts or assumptions. As Tukey [1962, 15-16] once put it, "far better an approximate solution to the *right* question than (...) an exact answer to the *wrong* question".

But being a skeptic about the value of rational-choice theory does not mean that I am a skeptic about action-based theorizing in general. In fact, as I see it, sociological theories should always be based on clearly explicated and plausible theories of action – that is, plausible theories of why actors do what they do – and they should also include clear and plausible ideas about the social structure in which the actors are embedded.



## **An agent-based model for analyzing games in networks**

The most straight-forward way of showing why action logics and network structures are so important is by using a so-called agent-based model. Agent-based modeling is a formalism designed for analyzing the relationship between individual- and social-level phenomena, whatever these phenomena may be, and the core idea is to use computer simulations to assess the social outcomes that groups of virtual agents are likely to bring about [see Macy and Willer 2002 for a brief overview]. What distinguishes agent-based analyses from other simulation approaches is that they are actor-based and that they explain social phenomena from the bottom up, that is, agent-based models make predictions about social outcomes, given different assumptions about the actions and interactions among the actors (see Epstein 2006 for a range of examples).

The example I will use analyzes the evolution of cooperation in different types of networks, and this is based on joint work with Lorien Jasny at the University of California, Irvine. The setup is as follows:

- We analyze the actions of 100 actors.
- They are embedded in a network and can only interact with those to whom they are directly linked.
- At each point in time we randomly select one of these actors.
- Among the actors to which this actor is linked, we randomly select another actor, and these two actors play a prisoners' dilemma game.

In the prisoners' dilemma game there exist four possible outcomes (see Table 1). If the actors were rational egoists they would always defect in this kind of situation because no matter what they think that the other may do, it is better for them to defect. Assuming that Actor 1 is a rational egoist, the numbers in the cells give his preference ordering over these outcomes (and the preference ordering of Actor 2 is parallel to that of Actor 1).

A wealth of experimental research by Ernst Fehr and others [e.g., Fehr and Fischbacher 2004; Fehr and Gächter 2002] suggest that most people are not egoists, however. The vast majority rather appears to be “reciprocators” or “conditional altruists”. A conditional altruist starts out by being cooperative and continues to cooperate if she believes that the other will cooperate. But she will defect if she believes that the other will defect. In our analysis we model the actions of the conditional altruists by assuming that the probability that they will cooperate is directly proportional to their beliefs about how others are likely to act which in turn depends upon their past experiences.

TAB. 1. Possible outcome of the prisoners' dilemma game

		Actor 2:	
		Cooperate	Defect
Actor 1:	Cooperate	2	4
	Defect	1	3

At the start of the simulation, a conditional altruist only knows herself. She is inclined to cooperate and she has no reason to believe that others are different. Therefore she will act cooperatively. If she runs into a defector in the first round, there is a fifty-fifty chance that she will cooperate also in the second round because now her experience is based on two people (herself and the defector in the first round) and half of them were defectors and half were cooperators. These proportions then will change for each individual as the simulation proceeds and they will continuously adjust their beliefs and actions in the light of this information.

If all actors were conditional altruists we would expect them all to cooperate, and if all actors were egoists we would expect them all to defect. But as also underscored by the experimental work of Fehr and his collaborators, most real-life populations are heterogeneous. Some individuals are egoists, some are true altruists, and the vast majority is conditional altruists.

The results of large number of simulations are summarized in Figure 5. We varied the density of the random networks linking the actors to one another and recorded their actions in different structural settings. The figure reports the average outcomes from a large number of such analyses. The outcomes recorded were the last actions the actors performed before the end of the simulation.

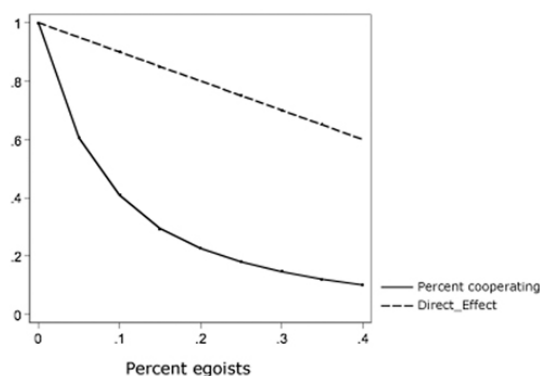


FIG. 5. Interaction-effects in prisoners' dilemmas games: Averages from 27,000 simulations in each of which 100 actors play 20 games.

What we first of all can observe in Figure 5 is that all actors will indeed cooperate if they all are conditional altruists. The graph then shows what happens when we introduce egoists into this population. The straight line shows what would have happened had there not been any network- or social-interaction effects. That is, had 10 percent been egoists, the rate of cooperation would have fallen from 100 to 90 percent; had 20 percent been egoists, the cooperation rate would have fallen to 80 percent; and so on. But as the graph makes evident there are very pronounced social interaction-effects because the egoists also influence the actions of the conditional altruists. The introduction of 5 percent egoists reduces the cooperation rate from 100 to about 60 percent and if we introduce 10 percent egoists, during the time span analyzed here, the cooperation rate would have been reduced to 40 percent.

Examples like this help bringing home a point which I consider to be a fundamental importance for explanatory sociology: social outcomes always should be explained as the result of actions embedded in networks (where “network” is broadly defined to also include the actors’ ties to various social contexts or generalized others). These analyses show on the importance of simultaneously considering networks and individual action logics, because it is the specific combination of action logics and interaction patterns that influence the social outcomes the actors bring about.

## **Empirical research**

Let me then turn to some more examples of my own empirical research. As should be evident by now, I believe it is essential:

- to focus on processes that unfold over time because causal processes operate in time and studying cross-sections of these processes can mislead us in numerous ways;

- to focus on individuals’ actions, because it is actions that generate whatever outcomes we seek to explain and we arrive at a deeper understanding of what is going on by explicating these actions; and

- to focus on the networks that link individuals to one another because social interactions and social influences are important for explaining why individuals do what they do.

If we take these desiderata seriously, they pose formidable requirements on the type of data that should be used. Essentially we need longitudinal data about the relevant actions of *all* individuals in the sub-populations that we are studying, and we furthermore need information on these individuals’ relationships to one another at different points in time.

If we do not have access to this kind of data, we can of course use our theory to make predictions about what is to be expected in, say, a cross-section. But it seems that we could do much better than just testing theories by checking the correctness of their predictions – and this relates back to what I just said about the limitations of instrumentalism; I believe that we should seek to achieve a high degree of isophormism between theoretical and statistical models, and not by adopting our theories and theoretical models to the statistical models, but the other way around [Coleman 1986; Sørensen 1998 make similar points].

One of the areas that I have studied concerns the spread and spatial diffusion of social-movement organizations – my specific focus was on the spread of Swedish trade unions and Social Democratic party organizations in the beginning of the 20<sup>th</sup> century. In these studies I focused on individuals' decisions whether or not to start up a local movement organization, and I examined how these decisions were influenced by similar decisions of others and the effects this had for the growth trajectory of the movement.

The basic theoretical ideas were straightforward [see Hedström 1994b]. I started by assuming that individuals are distributed in some sort of space. Their decisions in part are influenced by their own attributes, in part by the local conditions at their respective points in space, and in part by the actions of other individuals whose actions they are aware of. Some individuals are closer to one another which mean that it is more likely that they are aware of and influence one another.

One important reason for expecting network links to be important is that actions and experiences of others provide information about the likely costs and benefits of founding an organization. Particularly before a social movement has become institutionalized and fully legitimate the choice situation can be highly ambiguous. The impact of the movement as well as the reactions of other actors opposed to the movement then often are impossible to predict. In ambiguous situations like these we are particularly prone to be influenced by the actions of others, at least if we believe that they have good reasons for doing what they do [see Coleman, Katz and Menzel 1957].

We can formalize these ideas by assuming that each individual's decision can be described by the following logistic equation:

$$\ln\left(\frac{p_{it}}{1-p_{it}}\right) = a + \sum \gamma_k x_{itk} + \sum \beta_{ij} q_{i,t-1}$$

where

$p_{it}$  = the probability that actor  $i$  will start a local movement organization at time  $t$ ,

$x_{itk}$  = factors likely to influence actor  $i$ 's decision, and

$$q_{j,t-1} = \begin{cases} 1 & \text{if actor } j \text{ participated at time } t-1 \\ 0 & \text{if actor } j \text{ did not participate at time } t-1 \end{cases}$$

In order to highlight how the network is likely to influence the mobilization process, one can simplify this expression by collecting all individually specific factors into a single factor which we call  $a_i$ . The network dimension now can be introduced into the model:

$$\ln\left(\frac{p_{it}}{1-p_{it}}\right) = a_i + \beta \sum \pi_{ij} q_{j,t-1}$$

the change in actor  $i$ 's propensity to found a movement organization resulting from contact with someone who has joined the movement.

$\pi_{ij}$  = the probability of contact between actor  $i$  and actor  $j$  (assumed to be a decaying function of the distance between  $i$  and  $j$ ).

So we assume that actor  $i$ 's propensity of starting a local movement organization partly is a function of factors describing the actor and the actor's local environment (collected in the  $a_i$ -term), and partly is a function of the behaviour of the other actors.

We can translate these ideas into an agent-based model, and then we get results like those in Figure 6. This figure shows 100 actors who are distributed in a two-dimensional space, and how the mobilization process unfolds given the assumptions I just mentioned. The dots represent the actors and the circles highlight the actors who started a movement organization at a particular point in time.<sup>1</sup>

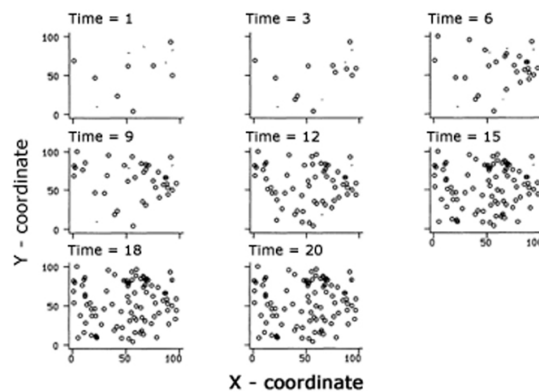


Fig. 6. Spatial diffusion of collective action organizations: simulation results.

<sup>1</sup> The individual intercepts were drawn from a normal distribution with a mean of  $-1.5$  and a standard deviation of  $1$ . The influence parameter  $\beta$  was set equal to  $.011$ .

As can be seen from the figure, eight actors had started a movement organization at time 1. They are the actors with  $a_i$ -values greater than zero. At time 3, four additional actors have decided to do the same; most of them are located nearby those who formed organizations at time 1. The contagious process continues, first rather slowly and then more rapidly, to finally reach 92 organizations at time 20.

How can these ideas and predictions be tested empirically? A decisive test would require not only relevant longitudinal individual data but also information on the individuals' relationships to all other individuals in the relevant population. Unfortunately, such data did not exist. So what I did was instead to use data on the formation of trade-union organizations in 371 different geographical areas during the period 1890 to 1940. The data set contained more or less every local union organization that existed during this time period. Totally about 17,000 local union organizations were included in the data file.

I related the timing of the founding of the first local organization within a district to various characteristics of the district and to the activities taking place in other districts by estimating the parameters of the logit model:

$$\ln\left(\frac{p_{it}}{1-p_{it}}\right) = a_i + \sum \lambda_k x_{ikt} + \beta Y_{i,t-1}^*$$

where

$p_{it}$  = the hazard rate, or the conditional probability that the first association in district  $i$  will be formed at time  $t$ .

$x_{ikt}$  = district  $i$ 's value at time  $t$  on the  $k$  factors likely to influence the likelihood of an association being formed; and

$Y_{i,t-1}^*$  = a weighted sum of the number of union members in other districts during the previous year.

It is not appropriate to here discuss the details of the empirical results, but the core finding was that there were clear signs of interaction or network effects in that the probability of a union organization being founded was significantly influenced by union activities in other districts, and these effects remained even when controlling for a range of covariates.

In a related piece [Hedström, Sandell and Stern 2000] we extended this framework to consider the role of multiple overlapping networks (see Figure 7). In addition to the types of network effects just described, we were also interested in the importance of a particular type of meso-level network which was brought about by the routes that various political agitators formed when they travelled around the nation during these years (Figure 7 just describes one hypothetical route where an agitator

travels from A to B, etc and thereby carries information and creates links through which these distant areas can come to influence one another). We coded all of these trips and examined the impact this network had on the propensity of a movement organization being founded.

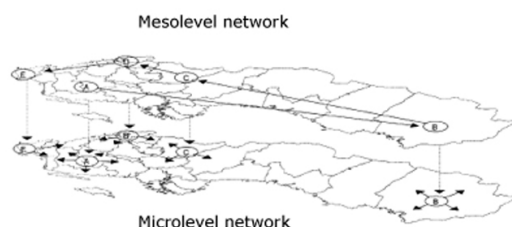


FIG. 7. Hypothetical micro- and mesolevel networks illustrating the operation of a multilevel diffusion process.

What we found was that both types of networks indeed were of importance and that the agitators had a dual effect on the probability of a party organization being founded. First, their visits to a district in it self seemed to increase the probability of an organization being founded there, but in addition, they had an impact by linking distant districts to one another.

## Concluding remarks

Currently I am engaged in several other studies concerning entirely different subject matters such as suicides and labor market networks, but the basic approach is similar to the studies I have described here. Let me therefore end with briefly summing up the most important characteristics of a sociology that really matters to me. What I consider to be important sociology has less to do with the subject matter being studied and more to do with the way in which we go about explaining whatever it is that we wish to explain. The type of approach that intrigues me the most is a theory-driven approach where one starts with clear ideas about how a certain process is likely to operate and then goes from there.

The type of theory-driven research that I think one should strive for tries to intimately link micro and macro and theory and empirical research. It can be summarized as follows:<sup>2</sup>

- The theory should be clear and precise.

<sup>2</sup> See Hedström 2005 for a detailed discussion.

- The theory should specify the mechanisms that we believe have brought about whatever it is that we try to explain.
- The theory should be concerned with explaining change (or lack thereof).
- The theory should include a clearly explicated and plausible theory of action.
- The theory should also be founded upon precise and plausible ideas about the structure of social interaction.
- The empirical research should be designed in such a way as to minimize the gap between the theoretically postulated mechanisms and the data being used in the empirical analyses.



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## **Actions and Networks: Sociology that really matters... to me**

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Abstract: Although the degree of specialization varies a great deal within the profession, many sociologists work on a range of different subject matters. In fact, the broad scope of the discipline is according to many one of its greatest attractions. Working on a range of different subject matters is not necessarily an indication of eclecticism, however, because underlying them all often is a coherent approach reflecting the sociologist's notion of how sociology should be done and what type of sociology is important. That is, we all do what we do for a reason, and although we may study entirely different subject matters, our basic approach often will be similar from one study to another. In this paper I use my own research over the last 20 years to illustrate this point.

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