

Articles

The Myth of Social Network Analysis as a Special Method in the Social Sciences

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THE DENIAL

In the history of public speaking, there have been many famous denials. One sunny day in 1880, Karl Marx declared: "I am not a Marxist". On a less auspicious occasion in 1973, Richard Nixon insisted "I am not a crook". Neither Marx's nor Nixon's audience gave much credence to their denials, and you too may respond with disbelief when I tell you that "I am not a networker!".

You may want to know what has driven me to such a claim. In response I tell you that for seventeen years, I have been haunted by a specter -- but not the specter of communism. It is instead a specter that has insinuated itself into my life on every occasion since 1973 when I have been introduced to a new acquaintance unfortunate enough to know some sociology.

On such occasions, these new friends have thrown etiquette to the winds. They do not say "Hello" or "How nice to meet you", and introduce themselves, as in normal civilized discourse. Instead, a glazed look comes over their eyes and they say, as if uttering some incantation, "Oh yes, The Strength of Weak Ties!". Some simply say "weak links!" -- they've mixed me up with Oscar Mayer. When I hear this, I know what Richard Nixon must feel when a new acquaintance says: "Nixon, Nixon -- oh yes, Watergate!".

So this is my specter: the specter of weak ties. And it helps to explain why I have always resisted being classified as a "network analyst". Perhaps it is because of my flight from being typecast as a networker, that this is the first Sunbelt meeting I have attended. And after my first few publications that were explicitly on network analysis, a period that ended in 1976, I believed that this period of my career was behind me and that I was moving on to my more substantive interests in stratification, economic sociology and sociological theory.

A FUNNY THING HAPPENED ON THE WAY OUT OF NETWORK ANALYSIS

But a funny thing happened on my way out of network analysis: as I innocently pursued these other interests, which I approached from various directions, I always found that as I got more deeply into any subject, network ideas kept coming in the back door. I would write an article that I thought was completely innocent of any network ideas, and someone who had read it would say: oh yes, that's the network approach. I once gave an entire course that I thought didn't have a single network idea in it, and then overheard the TA telling his students, "Of course, Granovetter represents the network approach to this subject".

Let me give you some examples: In the late 1970's, I began to pursue my interests in stratification, by looking more closely at the way labor markets distribute income to workers. In the process, I became quite interested in comparing the way economists and sociologists had approached these questions, and read up on economics. I came to the conclusion that two fallacies dominated both the economic and sociological literature on income differences.

One was the assumption that these differences derived entirely from the demand side of the labor market: that the characteristics of the type of work you do is what determines your rewards. This fallacy is most obvious in the functional theory of stratification, which suggests that the most important work in a society must receive the highest rewards in order for the society to function well. There are elements of this fallacy in much Marxist literature on stratification as well.

The other, and symmetrical error is to suppose that inequality derives entirely from the supply side, as both the theory of human capital and that of status attainment, would lead you to believe.

But it was clear to me that both the characteristics of positions and those of people must be critical to establishing inequality, and it then follows that what is most crucial is how people with certain characteristics get matched to positions with high or low potential for rewards -- what I came to call the problem of matching processes (see Granovetter 1981).

But you don't have to think very long about matching processes before you see that they basically concern location in social networks and how these locations shape peoples' work lives and the kinds of positions that are available to them. And this led me to see that a question that is simply finessed in classical and neoclassical economics -- that of how supply and demand are made to mesh -- is actually the central question, and is one that can only be understood in the framework of social networks.

More generally, as soon as you become dissatisfied with an economics that is built on the comparative statics of equilibrium states, you have to start talking about what happens when systems are out of equilibrium. This means talking seriously about how changes occur. And what happens in such a dynamic account is that you have to look at how people make use of their location in social networks to mobilize resources in order to achieve their economic goals. They may well act rationally, but this rational action is highly constrained by the structure of those networks and the resources available in them. Probably my most systematic attempt to articulate this is in the article I have written with Charles Tilly on inequality and labor processes (Granovetter and Tilly 1988).

In my attempt to move away from network analysis, I started to feel like Alice in *Through the Looking Glass*. She had already gone through the mirror into the "looking-glass house", had gone out the door, and was trying desperately, but without much success, to get away from the house: resolutely turning her back upon the house, she set out once more down the path, determined to keep straight on till she got to the hill. For a few minutes all went on well, and she was just saying "I really shall do it this time --" when the path gave a sudden twist and shook itself (as she described it afterward), and the next moment she found herself actually walking in at the door. "Oh it's too bad!" she cried. "I never saw such a house for getting in the way! Never!" (Carroll 1871 [1984]: 169).

Like Alice, the harder I tried to run away from the house of social networks, the faster I found myself back there. I can't say that I still fully understood this in the early 1980's, and so I set out on an ambitious project (now at least five years behind schedule) to think through the social basis of economic life. I read a lot about industrial organization, and other aspects of markets, and came to feel that the field was dominated, again, by two fallacies:

One had been identified by Dennis Wrong as the "oversocialized" conception of human action in modern sociology -- the notion that people automatically follow widely agreed-upon norms in their daily life, and that this is what guarantees social order (Wrong, 1961). Wrong meant to be criticizing the structural-functional sociology of Talcott Parsons. But most standard economic arguments implicitly make the same assumption as well, since the old Hobbesean question of why it is that people do not pursue their self-interest by force and fraud does not arise. Instead, as Albert Hirschman has pointed out, it is normally assumed that the pursuit of economic goals is a "gentle, civilized activity" (Hirschman 1982).

But classical and neoclassical economics also could be characterized as harboring an "undersocialized" conception, since they insist upon viewing individuals as atomized actors, whose behavior might be influenced by the aggregate outcome of the behavior of others, but not by their ties to any particular others. Economic man, instead, moves through life wearing social blinders, in the rational pursuit of individual gain.

As I thought this through, I realized that these two apparently opposite fallacies actually went together very easily, because both depict individuals as unaffected by any ongoing social relationships. This is the more obvious for the undersocialized account, but is true in the oversocialized one as well because once actors, conceived in this way, have internalized the prevailing norms and values, they also become oblivious to the social life in which they are embedded. They move through their daily round like those poor souls in the movie, *Invasion of the Body Snatchers*, who have been taken over by pods from outer space.

What I came to argue is that one has to thread one's way between these under and over-socialized views by seeing that all economic activity is in fact embedded in complex, ongoing networks of social and economic relationships, and the trust that makes such activity possible, but also the malfeasance that can bring it to a halt, can only be explained by a close analysis of how people function in such networks. This argument, made in my 1985 *American Journal of Sociology* article, will be greatly elaborated in my forthcoming book *Society and Economy: The Social Construction of Economic Institutions* (to be published by Harvard University Press).

THE MYTH OF NETWORK ANALYSIS AS A SPECIAL METHOD IN THE SOCIAL SCIENCES

By this time I had realized that my flight from the house of social networks was futile. But I also realized something else, that made such flight less urgent. To explain this, I should begin by admitting that the title of my talk, like most good titles, is stolen: in this case, from a 1959 paper by Kingsley Davis, which had been his keynote address to the 1959 ASA meetings: "The Myth of Functional Analysis as a Special Method in Sociology and Anthropology" (Davis 1959). In this paper, Davis argued that although you couldn't always be sure what "functional analysis" was supposed to be, that "examination of the features most commonly mentioned and of the work actually done under the label shows it to be, in effect, synonymous with sociological analysis" (1959: 757).

Without recapitulating his entire argument, I simply note his claim that functional analysis is basically about the consequences of social patterns and the impact of these consequences in maintaining or changing the patterns themselves. He goes on to say that we can hardly conceive of any social science that isn't about this.

I will abstain on whether Davis was correct about functional analysis. If he was, I wonder what he thinks about the recent revival of functionalism under the name "neofunctionalism", in a city not far north of here. But for my purpose, I want only to borrow the title, because what I came to realize was that it shouldn't be any surprise to me that I kept coming back to network analysis, since I am a sociologist, and there really is no way to remain faithful to the fundamental insights of sociology without paying attention to networks of social relationships.

What, after all, is the distinctive contribution of the founders of modern sociology, such as Durkheim, Weber and Simmel? It is precisely that one cannot understand social life as the summing up of individuals' motives and traits, as they are given by the study of psychology. This was the fundamental insight of Durkheim in his classic study of suicide -- that this most individual of acts is to be explained by the way people are or are not integrated into social networks; and in *The Division of Labor in Society*, Durkheim argued that modern societies with an advanced division of labor can only be held together by the complex networks of complementarity that this division produces.

If social relations and the structure of networks of relations are practically coterminous with social science analysis, then how can it be that social network analysis could seem so separate to so many from mainstream work? I believe that part of the answer is that for thirty years, American sociology and, to some extent, anthropology, was dominated by the followers of Talcott Parsons, who had a very different view.

Parsons's "discovery" in *The Structure of Social Action* (1937) was that four great thinkers -- the sociologists Durkheim, Weber and Pareto, and the economist Alfred Marshall -- were all converging on a single proposition that Parsons had finally brought to light: exactly the one criticized by Dennis Wrong -- that society is integrated by common value orientations held by all its members. In saying this, Parsons believed that he was upholding the classic sociological tradition, and moving away from a conception of atomized actors.

But in his argument, there was hardly any room at all for particular people or relations; they were relegated to a minor and subordinate role in the conceptual scheme, and Parsons, for this reason, saw Durkheim's progression away from an emphasis on concrete relations toward vague ideas about the "collective conscience", as progress toward a higher level of argument, rather than just a higher level of abstraction.

The founders of network analysis, to some extent, were rebelling against this excessively abstract and oversocialized view of social life. In the Parsons-dominated atmosphere of the 1950's and 1960's when network analysis had its formative period, network analysis had to be rebellious and iconoclastic, since there was no room for it in the received wisdom. This explains, I think, some of the sectarian features of the earlier period of networking. And even now I'm sure many of us think of ourselves as crusading outsiders.

This sense of a crusade has been salutary; it has helped sustain us through the period of consolidation, and brought us to our present strength -- this is the largest Sunbelt conference ever, and there is a veritable explosion of network-related writings. But I think it is time for us to take note that the Parsonian synthesis has long since declined in American social science, and this means that we should think about changing our stance.

If we remember that the insights of network analysis are not peculiar or sectarian, but in fact the rightful heirs to those put forward by the founders of modern social science, we can begin the task of reorienting social science research toward the proposition that no part of social life can be properly analyzed without seeing how it is fundamentally embedded in networks of social relations. In doing this, we need to remember that there are many scholars outside the house of social network analysis who think in a relational way but don't see the kinship with network methods and ideas. I see us as having a mission to join with these kindred souls.

There has long been dissatisfaction with the oversocialized notion that society is integrated by mental harmonies, and also with the undersocialized one that rational, atomized individuals, pursuing their own self-interest, explain all there is to know about social life. We as the self-conscious core of relational analysis are in a unique position to offer a solution to both kinds of dissatisfaction, and bring large numbers of others under our roof. This was part of my motive for undertaking the editorship of the Cambridge University Press series *Structural Analysis in the Social Sciences* -- to try to bring together both outstanding exemplars of network analysis, and also other relational work not previously thought of as belonging to the same school of thought.

So my message is that we have had an outstanding success as a separate method in the social sciences, but that there may be diminishing returns to this strategy. On the other hand, we are now in a strategic position to bring our insights to the more general social science community in a way that will reorient it in our direction. Many of us have already begun to do this. Just one look at our program will show how many papers take standard social science topics and demonstrate the power of a relational approach.

This does not mean we need to dissolve as a separate interest group, or that no one should continue to work on social network methods as such. On the contrary, it is exactly such work that lays the ground for what will eventually be our successful attack on the ramparts of orthodox, non-network-oriented social science. And when we have breached those ramparts and reached that plateau, there will be no one left who will have to stand before a group and declare: "I am not a networker". Instead, the slogan of the day will be "We are all networkers now!".

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