Social Networks and Political Context

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People are embedded in networks, neighborhoods, and relationships. Understanding the nature of our entanglements and how they shape who we are is fundamental to *social* sciences. Networks are likely to explain important parts of personal development and contemporary decision making. Researchers have found social networks to be important in activities as disparate as voting (Berelson, Lazarsfeld and McPhee 1954), immigration patterns (Sanders et al. 2002), finding a job (Nordenmark 1999), recycling (Tucker 1999), de-worming (Miguel and Kremer 2004), cardiovascular disease and mortality (Kawachi et al. 1996), writing legislation (Caldeira and Patterson 1986), and even happiness (Fowler and Christakis 2008). A wide range of political outcomes could be studied using "social networks," the only limitation is that the outcome be measurable. Ironically, the very ubiquity and importance of social networks makes it very difficult to study. Isolating causal effects is always difficult, but when like-minded individuals cluster together, share material incentives, are exposed to common external stimuli, and simultaneously influence each other, the job of reliably estimating the importance of social ties becomes nearly impossible. Rather than offering a comprehensive overview of the wide number of topics covered by social networks, this chapter focuses on the common empirical challenges faced by studies of social networks by: considering the challenges faced by observational studies of social networks; discussing laboratory approaches to networks; and describing how network experiments are conducted in the field. The chapter concludes by summarizing the strengths and weaknesses of the different approaches and considers directions for future work.

Observational studies

Cross-sections

The social networks literature blossomed during the 1940s and 50s with the works like *The People's Choice* (Lazarsfeld, Berelson, and Gaudet 1948) and *Voting* (Berelson, Lazarsfeld, and McPhee 1954). Utilizing newly improved survey technology, the authors administered surveys to randomly-selected respondents densely clustered in medium sized communities. This strategy provided insight into what the neighbors and friends of a respondent believed at the same point in time allowing correlations in the behaviors and beliefs of friends and neighbors to be measured. The authors found that information flowed horizontally through networks and overturned the opinion leadership model of media effects.

The advent of affordable nationally representative polling largely ended this mode of inquiry. Why study one community when you could study an entire nation? Unfortunately, the individuals surveyed from around the nation had no connection to one another, so the theories developed based on this data generally assumed atomistic voters (for example, Campbell et al. 1964). The political context literature was revived only when Huckfeldt and Sprague returned to the strategy of densely clustering surveys in communities, but adding a new methodological innovation. Huckfeldt and Sprague used snowball surveys, where respondents were asked to name political discussants, who were then surveyed themselves. The technique allowed Huckfeldt and Sprague to measure directly the political views in a person's network rather than infer the beliefs of discussants from neighborhood characteristics. In a series of classic articles and books, Huckfeldt and Sprague and their many students meticulously documented the degree to which political engagement is a social process for most people (for example, Huckfeldt 1983; Huckfeldt and Sprague 1995). It is fair to say that most contemporary observational studies of

political behavior and social networks either rely on survey questions to map social networks or ask questions about politically relevant conversations (for example, Mutz 1998).

These empirical strategies face three primary inferential hurdles making it difficult to account for all plausible alternative causes of correlation. First, people with similar statuses, values, and habits are more likely to form friendships (Lazarsfeld and Merton 1954), so self-sorting rather than influence could drive results. Second, members of a social network are likely to share utility functions and engage in similar behaviors independently of one another. Third, members of social networks are exposed to many of the same external stimuli (for example, media coverage, economic conditions, political events). If the external stimuli influence members of the social network similarly, then observed correlations could be due to these outside pressures rather than the effect of the network. All of these problems can be categorized as forms of omitted variable bias[1] and call into question the results based on cross-sectional surveys. Within the framework of a cross-sectional study, it is difficult to conceive of data that could convince a skeptic that the reported effects are not spurious.

Panels

Part of the problem is that influence within a network is an inherently dynamic process. A person begins with an attitude or propensity for a given behavior and the network acts upon this baseline. Observational researchers can improve their modeling of the process by collecting panel data where the same set of individuals is followed over time through multiple waves of a survey. This strategy allows the researcher to account for baseline tendencies for all respondents and measure movement away from these baselines. Moreover, all measured and unmeasured attributes of a person can be accounted for by including fixed effects for each individual in the sample. In this way, panel data can account for all time-invariant confounding factors.

Panel studies in political science are rare because of the expense involved. Studies focusing on social networks are even less common and nearly always examining families – one of the most fundamental networks in society. Jennings and Niemi's (1974, 1981) classic survey of families over time is the best known panel in political science examining how political attitudes are transferred from parents to children (and vice-versa). More often, political scientists are forced to rely on a handful of politically relevant questions in panel studies conducted for other purposes (for example, Zuckerman, Dasovic, and Fitzgerald 1985).

While a huge advance over cross-sectional data, panel data cannot provide fully satisfactory answers. Even if the type of networks considered could be broadened, dynamic confounding factors, such as congruent utility functions, life-cycle processes, and similar exposure to external stimuli, remain problematic. Furthermore, if the baseline attitudes and propensities are measured with error and that error is correlated with politically relevant quantities, then the chief advantage of panel data is removed because the dynamic analysis will be biased.

Network Analysis

Network analysis is touted as a method to analyze network data to uncover the relationships within a network. Sophisticated econometric techniques have been developed to measure the strength of ties within networks and their effects on various outcomes (Carrington, Scott, and Wasserman 2005). Instead of assuming the independence of observations, network models adjust estimated coefficients to account for correlations found among other observations

[1] Selection bias can be even more pernicious than omitted variable bias (see Achen 1986).

with ties to each other. Network analysis is a statistical advancement, but it does not surmount the core empirical challenge facing observational studies of social networks, which is essentially a data problem. Similar utility functions and exposure to external stimuli remain problems, as do selection effects. Selection effects are not only present but reified in the model and analysis, being used to define the nodes and ties of the network.

To illustrate the challenge facing observational studies of social networks, consider the recent work by Christakis and Fowler (2007, 2008a,b) using the Framingham Heart Study. To supplement health and behavioral data collected since 1948, the Framingham Heart Study began collecting detailed social network data in 1971. Taking advantage of the panel and network structure of the data, Christakis and Fowler found evidence that obesity, smoking, and happiness were contagious. While the claim is entirely plausible, there are three reasons to question the evidence provided and the strength of observed the relationship. First, unobserved factors influencing both alters and egos could drive the results. Second, the strength of the relationship detected violates a few causal models. For instance, Christakis and Fowler find "geographic distance did not modify the intensity of the effect of the alter's obesity on the ego" (2007, p 377). The primary mechanism for jointly gaining and losing weight would presumably be shared meals or calorie burning activities like walking or perhaps competitive pressure to remain thin. However, none of these mechanisms work for geographically distant individuals, raising the concern that selection bias is driving the results.[2] Third, Cohen-Cole and Fletcher (2008) adopt a similar empirical strategy on a similar data set to Christakis and Fowler and find evidence that acne and height are also contagious, which constitute failed placebo tests. None of these points disprove the claims by Christakis and Fowler, but they do call into doubt the evidence provided and the strength of the relationships detected.

The Framingham Heart Study is a nearly perfect observational social network data set. If the answers provided remain unconvincing, then perhaps the observational strategy should be rejected in favor of techniques using randomized experiments. Ordinarily, experiments can get around problems of self-selection and unobserved confounding factors through randomization, but the organic nature of most social networks pose a difficult problem. To test the power of social networks, the ideal experiment would place randomly selected individuals in a range of varying political contexts or social networks. The practical and ethical concerns of moving people around and enforcing friendships are obvious. The time-dependent nature of social networks also makes them inherently difficult to manipulate. Reputation and friendships take a long time to develop and cannot be manufactured and manipulated in any straightforward manner. Thus, the experimental literature testing the effect of social networks on behaviors and beliefs is still in its infancy. That said, the next section discusses the laboratory tradition that began in the 1950s.

Laboratory Experiments

Assign Context

Many tactics have been used to study social networks in laboratories. The central logic behind all of them is for the researcher to situate subjects in a randomly assigned social context. One of the most famous examples is Asch's series of classic experiments on conformity. Subjects were invited to participate in an experiment on perception where they had to judge the length of lines. Control subjects performed the task alone, while subjects in the treatment group interacted with confederates who guessed incorrectly. Subjects in the control group rarely made

[2] A similar problem arises when happiness is found to be more contagious for neighbors than co-workers or spouses.

mistakes, while individuals in the treatment group parroted the errors of the confederates frequently. The initial study was criticized for relying on a subject pool of male undergraduates, who may not be representative of the population as a whole. However, the Asch experiments have been replicated hundreds of times in different settings (Bond and Smith 1996). While the conformity effect persists, it: a) varies across cultures; b) is stronger for women; c) has grown weaker in the United States over time; and d) depends on parts of the experimental design (for example, size of the majority, ambiguity of stimuli) and not others (for example, whether the subject's vote is public or private). Thus, the Asch experiments constitute evidence that peers – even ones encountered for the first time – can shape behavior.

Many literature in psychology employ tactics similar to those used by Asch. For instance, social loafing (Karau and Williams 1993) and social facilitation (Bond and Titus 1983) can boast equally long pedigrees and replications.[3] While these experiments measure conformity, how strongly the findings apply in real-world setting is unclear. First, the participants are inserted into a peer group with no real connection or bond. These essentially anonymous and ahistorical relationships may accurately characterize commercial interactions, but differ in character from social networks classically conceived. Second, subjects are presented with an artificial task with limited or no outside information on the context (for example, estimating the length of a line). Thus, participants may have little stake in the proceedings and may not take the exercise seriously (that is, subjects want to avoid arguments on trivial matters or think they are playing a joke on the experimenter). Asch-style experiments measure a tendency to conform, but how and under what conditions do the results translate to political settings where individuals know messengers and think more is at stake.

Randomly Constructed Network

Creative strategies have been designed to respond to these criticisms about external validity. A recent tactic embraces the isolation of the laboratory and utilizes abstract coordination games with financial incentives for subjects linked to the outcome of the game. The advent of sophisticated computer programs to aid economic games played in the laboratory has facilitated a number of experiments that directly manipulate the social network and the subject's place in it (for example, Kearns, Suri, and Montfort 2006). Researchers can now isolate the factors of theoretical interest within social networks. For instance, researchers can manipulate the degree of interconnectedness, information location, preference symmetry, and external monitoring.

The downside of this strategy is that the networks are not only artificial but entirely abstracted and may not approximate the operation of actual networks. Strategy convergence among players may reflect the ability of students to learn a game rather than measure how social networks operate. That said, such experiments serve as a useful "proof of concept" for formal theories of social networks. If people are in networks like X, then people will behave like Y. The challenge is to link real-world phenomena to particular games.

Role Playing

To create more realistic social networks, researchers can have subjects engage in collaborative group tasks to create camaraderie, share information and views about a range of subjects to simulate familiarity, and anticipate future encounters by scheduling post-intervention

[3] Social loafing dates back to at least 1913 when Ringelmann found individuals pulled harder on a rope than when working in concert with others.

face-to-face discussion (for example, Visser and Mirabile 2004). These efforts to jump start genuine social connections or mimic attributes of long standing relationships are partial fixes. If organic social networks generated over years behave differently than those constructed in the laboratory, it is unclear how relationship building exercises blunt the criticism.

To address some concerns about external validity, some laboratory experiments allow subjects great freedom of action. By randomly assign subjects roles to be played in scenes, researchers hope to gain insight into real-world relationships. The most famous example of this strategy was Zimbardo's 1971 Stanford Prison Experiment where students were asked to act out the roles of prisoners and guards.[4] Most role-playing experiments are not so extreme but the same criticisms often apply. If subjects consciously view themselves as acting, the degree to which the role-play reflects actual behavior is an open question. Behaviors may differ substantially when subjects view participation as a lark and divorced from reality, A common critique of laboratory experiments is that they draw on undergraduates for their subject pool, but the critique has added bite in this setting. Whether more mature individuals would behave similarly given the roles assigned is an open question. Many role-playing experiments incorporate features of real-world relationships in order to approximate reality. However, not all details can be incorporated and researchers must make decisions about what features to highlight. The down-side of this drive for verisimilitude is that the highlighted attributes (for example, "parents" providing "allowance") may shape the behaviors of subjects, who take cues and conform to expected behaviors. These framing decisions therefore affect experiments and potentially make the results less replicable. Thus, the degree to which role-playing social network experiments approximate organic social networks found in the real world is open to question.

Small Groups

Experiments where subjects deliberate in small groups are an important sub-set of role-playing experiments. For example, the Deliberative Polls conducted by Fishkin (Luskin et al. 2002) invite randomly selected members of a community to discuss a topic for a day. Participants are typically provided briefing materials and presentations by experts. The experimental component of the exercise is that subjects are randomly placed into small groups to discuss the topic at hand. Thus, subjects could be placed in a group that is ideologically like-minded, hostile, or polarized. By measuring attitudes before and after the small group deliberation, it is possible to estimate the shift in opinion caused by discussion with liberal, conservative, or moderate citizens. The random assignment to small group discussion ensures that a subject's exposure to the opposing or supporting viewpoints is not correlated with any characteristics of the individual.[5] In this way, researchers can infer how the viewpoint of discussion partners effect an individual.

The evidence of attitudinal contagion from these experiments is mixed (Farrar et al. 2009), but the model is useful to consider. Since these experiments consist of randomly selected citizens talking to other randomly selected citizens, many concerns about external validity are alleviated. The subjects are representative of the community (conditional on cooperation) and the conversation is unscripted and natural (depending on the moderator's instructions). On the other hand, the setting itself does not occur naturally. People discuss political matters with members of their social networks, not randomly selected individuals – much less a set of people

[4] The experiment was halted after six days because of physical and psychological abuse by guards.
[5] Many mock jury experiments (for example, Sunstein et al. 2003) share this characteristic.

who have read common briefing materials on a topic. In fairness, the hypothetical nature of the conversation is precisely Fishkin's goal, because he wants to know the decisions people would make were they to become informed and deliberate with one another. However, the hypothetical nature of the conversation limits the degree to which the lessons learned from small-group activities can be applied to naturally occurring small groups.

Field Experiments

Observational studies examine naturally occurring social networks, but may suffer from selection processes and omitted variable biases. Laboratory experiments of networks possess internal validity, but the social networks studied are typically artificial and possibly too abstract to know how the results apply to real world settings. Intuitively, conducting experiments in the field could capture the strengths of both research strategies. The reality is more complicated, given the difficulty of conducting experiments in the field, the lack of researcher control, and unique concerns about the external validity of field experiments themselves.

Three strategies can be applied to study social networks experimentally. Researchers can provide an external shock and trace the ripple through the network, control the flow of communication within a network, or randomize the network itself. While the three categories cover most field experiments, the categorization does not apply to lab settings where researchers often manipulate all three analytic levers simultaneously. For instance, in the Asch experiments, subjects are randomly assigned to a network with no confederates, 8 confederates providing the wrong answer, or a group with a minority of confederates providing the correct answer. The presence or absence of confederates and their role defines the social network and manipulates the communication within the network. The task of judging the line length is the external shock used to measure the power of social influence. In theory, experiments conducted in the field could also pursue multiple randomization strategies since the categories are not mutually exclusive. In practice, a researcher will have difficulty manipulating even one aspect of the social network. Organic social networks are difficult to map and manipulate, so researchers have far fewer analytical levers to manipulate compared to the laboratory.

Logistical and Ethical Concerns

Before discussing each of the experimental strategies, it is worth considering a few of the practical hurdles that apply to all three research designs. The first difficulty is in measuring the network itself. The researcher has to know where to look for influence in order to measure it and the strategy employed will inherently depend on the setting. For instance, snowball surveys are a good technique for collecting data on social networks in residential neighborhoods or mapping friendships. Facebook and other social networking sites can be used on college campuses. Cosponsored bills in state legislatures are another possibility. Many studies of interpersonal influence rely on geography as a proxy for social connectedness, assuming that geographically proximate individuals are more likely to interact than geographically distant people (Festinger, Shachter, and Bach 1950). Each of these measurement techniques defines the network along a single dimension and will miss relationships defined along alternative dimensions. Thus, every study of social networks conducted in the field will be limited to the particular set of ties explicitly measured.

It is important to note that the measurement of the social network cannot be related to the application of the treatment in any way. Both treatment and control groups need to have networks measured in identical manners. In most instances, this is accomplished by measuring

social networks first and then randomly assigning nodes to treatment and control conditions. This strategy also has the benefit for preserving statistical power by allowing for prematching networks to minimize unexplained variance and assuring balance on covariates (Rosenbaum 2005). Given the small size of many networks studied, statistical power is not an unimportant consideration.

While defining the network identically for treatment and control variables may appear obvious, it imposes considerable logistical hurdles. Letting networks be revealed through the course of the treatment imposes a series of unverifiable assumptions and confuses the object of estimation. For instance, in his classic 6 Degrees of Separation experiment, Milgram (1967) mailed letters to randomly selected individuals and requested that they attempt to mail letters to a particular individual in a separate part of the country. If the subject did not know the individual (and they would not), they were instructed to forward the letter to a person who would be more likely to know the target. Milgram then counted the number of times letters were passed along before reaching the target destination.

Revealed networks research designs such as Milgram's creates data where the networks measured may not be representative of the networks of interest. If network characteristics (for example, social distance) correlate with the likelihood of subject treatment regime compliance (that is, forward/return the letters), then inferences drawn about the nature of the network will be biased (that is, Milgram probably overestimated societal connectedness). The treatment could also be correlated with the measurement of the network. Treatments may make certain relationships more salient relative to other relationships, so the networks measured in the treatment group are not comparable to networks assigned to control or placebo conditions. The potential bias introduced by these concerns suggests that researchers should measure the networks to be studied prior to randomization and application of the treatment. The downside of defining the network in advance is that the analysis will be limited only to the networks the researcher measured ahead of time; less obvious connections and dynamic relationships will be omitted from the analysis. However, the avoidance of unnecessary assumptions and clarity of analysis from clearly defining the network upfront more than compensate for this drawback.

The second major problem facing field experimental studies of networks is the inherent unpredictability of people in the real world where behavior cannot be constrained. This lack of researcher control poses two primary problems for experiments. First, if the behavior of a volunteer network node is part of the experimental treatment (for example, initiating conversations), then planned protocols may be violated. The violation is not necessarily because of noncompliance on the part of the subjects whose outcomes are to be measured (for example, refusing to speak with the experimental volunteer about the assigned topic) but because the person designated to provide the treatment does not dutifully execute the protocol in the way that laboratory assistants typically do. Overzealous volunteers may speak to more people than assigned; under motivated volunteers may decide to exclude hard to reach members of their network; or, the treatment may deviate substantially from what researchers intend. To contain these problematic participants and prevent biasing the overall experiment, researchers can build in safeguards into the initial experimental design (Nickerson 2005). For instance, blocking on the nodes of the network providing the treatment can allow the researcher to excise problematic participants without making arbitrary decisions as to what parts of the network to remove.

A second problem that unpredictable behavior creates is that network experiments may be far more contingent and have less external validity. Suppose two people are observed to have a strong relationship when the network is initially defined. If these people do not interact much

during the course of the experiment itself, then the two individuals are unlikely to pass the treatment along to each other and the detected strength of the network will be weak. If the waxing and waning of interactions is random, such differences will balance out across pairs of individuals and the researcher will achieve an unbiased estimate of the average network characteristic to be measured. However, the waxing and waning could be a function of a range of systematic factors. For instance, experiments conducted on student networks are likely to find dramatically different results should the treatments be conducted at the beginning, middle, or end of a semester. Political interest varies during and across elections, so experiments of voting and social networks may be highly contingent. Thus, external validity is a large concern and replication is an especially important aspect of advancing the science of real world networks.

The final practical hurdle facing researchers conducting experiments on social networks is that special attention must be paid to how the measurement of outcomes can affect the network itself. A researcher may want to see how inserting a piece of information into a network alters beliefs, but the insertion may also spur discussion in its own right. That is, the experiment could provide an unbiased estimate of how the *insertion* of the information affects the network, but cannot say how the *existence* of the information within network alters beliefs. Early social network experimenters were aware of this fact and therefore conducted their research under the label propaganda.[6] An extreme example of this dynamic is Dodd's Gold Shield Coffee study where randomly selected residents of a community were told the complete coffee slogan. The next day, a plane dropped 30,000 leaflets on the town of 300 households. The leaflets said that representatives from the Gold Shield coffee company would give a free pound of coffee to anyone who could complete the slogan and that 1 in 5 households were already told the slogan. The next day, everyone in the community was interviewed by researchers to map the spread of the information (Dodd 1952). The Gold Shield Coffee experiment does not capture how company slogans diffuse through neighborhoods, but it does measure how information diffuses when a plane drops a huge number of leaflets over a very small town.

A more common problem is the measurement of baseline attitudes. Researchers often worry about testing effects among subjects in pre/post test designs, but it is possible that administering the pretest changes the nature of the network. Subjects taking the survey may be more likely to discuss the topics covered in the survey than they would in the absence of the pretest. Even if no discussion is spurred by the pretest, subjects may be primed to be especially attentive to treatments related to the topics covered in the pretest. This increased sensitivity may compromise the external validity of such experiments. Incorporating time-lags between pretreatment measurements and the application of treatment can alleviate these concerns, as can creating pretest measures that cover a wide range of topics.

Related to the practical problems in conducting experiments on social networks are the ethical problems. Setting aside obviously unethical practices (for example, forced resettlement), many practices common in political science research are problematic in the context of social networks. The revelation of attitudes about hot button issues (for example, abortion, Presidential approval), the existence of sensitive topics (for example, sexually transmitted diseases, financial distress, abortion), and holding socially undesirable views (for example, racism, sexism, homophobia) could fracture friendships and negatively impact communities and businesses. Selectively revealing information to subjects about neighbors can answer many interesting questions about social networks (for example, Gerber, Green, and Larimer 2008), but should

[6] The fact that the military funded much of this research in order to understand the effectiveness of propaganda techniques assisted this decision.

only be practiced using publicly available information or after achieving the explicit consent from subjects. Maintaining strict confidentially standards is much more important when studying social networks than in atomistic survey conditions. Even revealing the presence or absence of network connections during a snowball survey could affect relationships, so researchers need to think carefully about the presentation of the study and how the assistants administering the survey can assure absolute privacy.

With these hurdles in mind, the three types of field network experiments can now be discussed.

External Shocks to the Network

The first experimental strategy for studying networks is for researchers to provide an external shock to an existing network and track the ripple (for example, Miguel and Kremer 2004; Nickerson 2008). The process involves introducing a change in a behavior/attitude at one node of the network and then examining other points on the network for the change as well. In principle, this strategy is not experimental per se and is like throwing a rock in a lake and measuring the waves. By throwing a large number of rocks into a large number of lakes, good inferences are possible. Randomly sampling nodes in the network only helps generalizability, much like random sampling does not make surveys experimental. To make the strategy truly experimental, multiple networks need to be examined simultaneously and the treatment then be randomly assigned to different networks. This random assignment allows the researcher to account for outside events operating on the networks (for example, the news cycle) and processes working within the network (for example, life-cycle processes). However, remember that the unit of randomization is the network itself and not the individuals within the networks. Thus, the analysis should either be conducted at the network-level or appropriately account for the clustered nature of the treatment.

Nickerson (2008) provides an example of the strategy by looking for contagion in voter turnout. Households containing two registered voters were randomly assigned to one of three treatments. The first treatment involved face-to-face encouragement to vote in the upcoming election. The second treatment was face-to-face encouragement to recycle that served as a placebo. The final condition was a control group that received no visit from researchers, but could verify that the voter mobilization detected by the experiment was genuine. The placebo condition served to define the network. Voter turnout for the people answering the door in the voting condition would be compared to turnout among people answering the door in the recycling group. Similarly, turnout for the registered voter not answering the door could then be compared across the voting and recycling conditions. The degree to which the canvassing spilled over could then be estimated by comparing the indirect treatment effect (that is, cohabitants of people who opened the door) to the direct treatment effect (that is, for the people who opened the door). The design requires the assumption that subjects do not preferentially open the door for one of the treatments and that only the people opening the door are exposed to the treatment.

A more common strategy was employed by Miguel and Kremer (2004) in their study of a de-worming program in Kenya by using an institution, schools, as the network node to be treated. The order in which rural schools received a de-worming treatment was randomly determined.[7] Miguel and Kremer then compared health outcomes, school participation, and school performance for pupils at the treatment and control schools finding cost-effective gains in

[7] This strategy also avoids ethical concerns about denying subjects treatment.

both school attendance and health. The most interesting effect, however, came when the researchers looked beyond the pupils in the experimental schools to villages and schools not included in the study. Untreated villages near treated schools also enjoyed health benefits and increased school attendance, confirming that worms are a social disease. This strategy of relying on institutional nodes of networks can be applied in a wide number of settings. The major hurdle to employing the strategy is collecting a sufficiently large number of networks/institutions to achieve precise and statistically meaningful results.

Controlling the Flow of Communication within a Network

A second strategy is to control the flow of communication within a neighborhood or network. The idea is to recruit participants who apply a treatment to randomly selected members of their social networks. Nickerson (2007) provides an example where volunteers were recruited to encourage friends and neighbors to vote in Congressional elections. Volunteers listed people who may need "encouragement" and volunteers would be comfortable talking to. The people listed were then randomly assigned to be approached (treatment) or not (control). The same design principle has been applied to proprietary studies of campaign donations and the adoption of consumer products. A major advantage of the design is that the list provided by the volunteers clearly defines the social network to be examined. Since individuals within networks are the unit of randomization, the design can also be much more powerful than designs that randomize across networks.

The biggest problem with controlling the flow of communication within a network is that the experimental interaction may artificial and not approximate conversations that occur organically. Neighbors, friends, co-workers rarely make explicitly political appeals to each other. Most of the hypothesized mechanisms for the diffusion of norms and peer effects are subtle and take time. It is possible that friends have a great deal of influence over each other but recoil from explicit prodding. Thus, such experiments measure the effect of aggressive word of mouth campaigns within networks and not the workings of social networks in their natural state.

Inadvertent contamination is a serious problem within social networks that needs to be considered. Volunteer may bring up the experiment in the course of everyday conversation following innocent questions such as "What's new?" Subjects may cross-contaminate themselves by discussing the unusual behavior of the volunteer applying the treatment. These problems can be avoided by randomizing across networks (that is, some volunteers treat everyone and others treat no one), but at the cost of considerable statistical power. The difficulty in controlling communication in social networks makes this type of experiment very difficult to conduct in the field and probably better suited for the laboratory.

Randomizing the Network Itself

The final strategy randomizes the position of people within networks. The steps involve measuring people's opinions, attributes, or tendencies at time 0, assigning a place in a network at time 1, measuring opinions at time 2, and modeling time 2 opinions for one person as a function of opinions at time 0 of both the subject and the others in the network. Obviously, there are limited settings where subjects can be randomly assigned to places in social networks. The most common use of this strategy has been to examine the effect of roommates among college freshmen looking at outcomes such as grades (Sacerdote 2001) or drug use and sexual behavior (Boisjoly et al. 2006). Less common are experiments where inmates are randomly assigned security levels in prisons (Bench and Allen 2003; Gaes and Camp 2009), which generally find

that prisoners assigned to more secure prisons are no more or less likely to commit crimes within prison but more likely to commit crimes upon release. The key to these empirical strategies is establishing baseline characteristics prior to assignment to achieve identification. Once the assignment is made and peers are residing together, outside forces could cause conformity independent of any peer effects thereby creating spurious relationships.

The biggest problem with this research strategy is that researchers rarely have the power to randomly assign the residence of subjects.[8] The cases where random assignment is practiced may not generalize to more typical living conditions. The kinds and intensity of interactions a person has in dormitories or cell blocks may be qualitatively different than interactions that people typically have at work or in their neighborhood. College students and prisoners are often young and may also be more impressionable than older individuals. As a result, these types of studies can tell us a great deal about the dynamics of these particular networks, but how the results apply to other settings is an open question.

A step-below randomizing the network itself is randomly providing the opportunity to opt in or out of a network (for example, change neighborhoods or schools). The most famous of these experiments is the Moving to Opportunity (MTO) study where randomly selected residents of public housing were provided vouchers to move where they see fit. The experiments then compared the outcomes of families receiving the vouchers to those in the control group with no voucher. The MTO experiments found that subjects electing to move felt safer and healthier, but made few differences with regards to criminal activity, employment, and educational attainment (Kling, Ludwig, and Katz 2005; Kling, Liebman, and Katz 2007). The same type of experiment has been conducted with regards to schooling where randomly selected families are provided vouchers to attend schools of their choosing (Howell et al. 2002).

All experiments provide a complier average treatment effect to some extent, but the dilemma is highlighted in these "choice" experiments. Many policy analysts would like to know the effect of living in certain types of neighborhoods or attending particular schools on the average person. However, choice experiments can only speak to how the *move* out of one environment and into another effects the *type of person* who would move. Both the treatment and control group also contain people who would stay put given the opportunity and the experiment is uninformative about these subjects. These nonmovers are revealed in the treatment group, but not the control group where randomization only assures that the proportion of nonmovers is the same as the treatment group. Thus, carefully defining the estimand and designing treatment-on-the-treated analysis plays a very special role in choice experiments. How best to model the decision making process is not always obvious and researchers have more discretion than is typically found in the analysis of experiments.

Subject attrition is a special challenge for choice experiments in two ways. First, subjects who take advantage of the voucher program may opt to move out of the area where researchers can easily track behavior. If outcomes for subjects moving out of the area differ from outcomes achieved locally, then the estimated treatment effect will be necessarily biased because movement is inherently correlated with the treatment. Second, subjects not enrolled in the experimental program (that is, the control group) have little reason to comply with researcher requests for information and may be more likely to drop out of the study. This process could result in a control group that is no long comparable to the treatment group. Both of these problems can be solved with sufficient resources to acquire information and incentivize

[8] People randomly assigned to living quarters typically have limited autonomy (for example, prisoners and soldiers) and, therefore, enjoy additional human subjects protections.

participation, but researchers seeking to conduct choice experiments should take steps to address these two forms of attrition.

Conclusion

Social networks have been studied throughout the history of social science, but new analytic tools are providing fresh insights into how people are tied together. Unsurprisingly, no single approach can lay claim to being preferred and all methods have their drawbacks. Observational studies allow researchers to collect large amounts of data and study the real-world relationships of interest, but may be plagued by spurious correlations that are impossible to eradicate. Laboratory experiments suffer from no omitted variable bias and can randomly manipulate the theoretically interesting aspects of social networks. The results in the laboratory will generally be theoretically abstract and anonymous networks. The types of real-world networks the results apply to is an open empirical question that researchers will need to establish. In theory, field experiments should combine the strengths of the both the observational and laboratory strategies, but the reality is far messier. The cases where field experiments can be applied to social networks are necessarily limited, so the external validity of the findings is open to question. The amount of control researchers have over the network is limited and many theoretically and practically interesting questions will prove impossible to study.

Thus, a combination of the three approaches is likely to prove the most fruitful. As data becomes more ubiquitous and available to researchers, observational studies will be able to address an increasing range of issues. Just as lab experiments have helped to guide the theoretical development of game theory, laboratory experiments on social networks will answer increasingly complicated theoretical questions about network density, information flow, strength of ties, and a host of other factors. As randomized trials become more accepted in a range of policy settings (for example, education, housing, legal enforcement, environmental protection), the number of opportunities to conduct field experiments on social networks will also increase. Little experimental work on networks has been done to date, but that leaves many fertile avenues for researchers.

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