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Causality in Political Networks

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Abstract

Investigations of American politics have increasingly turned to analyses of political networks to understand public opinion, voting behavior, the diffusion of policy ideas, bill sponsorship in the legislature, interest group coalitions and influence, party factions, institutional development, and other empirical phenomena. While the association between political networks and political behavior is well established, clear causal inferences are often difficult to make. This article consists of five independent essays that address practical problems in making causal inferences from studies of political networks. They consider egocentric studies of national probability samples, sociocentric studies of political communities, measurement error in elite surveys, field experiments on networks, and triangulating on causal processes.

Keywords

social networks, causality, surveys, public opinion, measurement error, field experiments

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Introduction

By Michael T. Heaney

Political networks play a vital role in American politics. Networks shape how citizens receive and interpret political information (Huckfeldt & Sprague, 1987, 1995; McClurg, 2006; Mutz, 2006). Social network ties may prompt people to vote (Nickerson, 2008). They influence legislative cooperation on bill sponsorship (Fowler, 2006), party cooperation across competing factions (Grossman & Dominguez, 2009; Koger, Masket, & Noel, 2010; Schwartz, 1990), and social movement cooperation across coalition boundaries (Heaney & Rojas, 2007, 2008). The degree of influence that interest groups have in the legislative process depends, in part, on their access to networks of other interest groups (Carpenter, Esterling, & Lazer, 2004; Heinz, Laumann, Nelson, & Salisbury, 1993; Laumann & Knoke, 1987). In a recent review of this research, Scott McClurg and I point to the exponential growth of network studies of American politics over the past decade, especially applications pertaining to "the flow of information," "coordination, cooperation or trust," "informal organization," and "multiple levels of organization" (Heaney & McClurg, 2009, pp. 729-730).

While the strong association between political networks and political behavior is well established empirically, determining the exact nature of the causal relationship between networks and politics is a more challenging endeavor. Institutional networks and coalitions are constructed over time by political actors with self-interested political purposes in mind. People may choose their friends and associates, in part, because of agreement on political issues and involvement in politics. Respondents to surveys may think about the political implications of their answers and, thus, modify their reported networks based on their expectations of the political implications of the results. These and other considerations suggest that political networks may be, in part, caused by political behaviors or influenced by common political contexts.

The strategic nature of relationship-formation in politics may make concerns about the causal direction of empirical relationships somewhat more pronounced than in some not-explicitly-political social networks (such as sexual networks or patent-citation networks). To what extent do networks cause political behavior and to what extent does behavior cause political networks? This article is a set of essays that explore practical approaches to observing causal effects in political networks. We do not adopt a single perspective on how to approach issues of causality, but rather embrace the idea that multiple perspectives have validity. Which approach is most appropriate depends heavily on the problem at hand.

Commonly, we understand a network to exert a causal effect on a political behavior if an exogenous modification of the network structure results in a change in political behavior. For example, suppose a person, Ego, has a network of friends who talk about politics, and then one more politically interested person, Alter, joins the network, without any other changes in Ego's situation. If Ego shifts from having a moderate level of interest in politics to having a slightly higher level of interest in politics, then Ego's discussion network can be said to have a causal effect on her interest in politics. Of course, we recognize that a key empirical challenge is determining when the change in the network is truly exogenous from the change in behavior. Indeed, it is often the case that the network changes because of a change in behavior. For example, a person who starts attending political rallies for the first time may start to acquire more friends with perspectives commonly espoused at those rallies. Network researchers struggle to draw valid causal inferences about the direction and strength of these relationships when networks and behavior are observed contemporaneously, or nearly so.

No one approach to making causal inferences is likely to be applicable in all instances. We agree with Henry Brady, who argues that causal arguments are stronger to the extent that they demonstrate four elements: (a) "constant conjunction of causes and effects"; (b) "No effect when the cause is absent in the most similar world to where the cause is present"; (c) "An effect after a cause is manipulated"; and (d) the identification of "Activities and processes [i.e., mechanisms] linking causes and effects" (Brady, 2008, p. 218). To satisfy these ideals in a persuasive fashion, multiple approaches, tailored to the problem at hand, are required to make strong causal inferences.

The authors of the following set of essays make practical recommendations for observing causal effects in research projects important to scholars of American politics. Betsy Sinclair focuses on the problems of making causal inferences about interpersonal discussion networks from egocentric network data in probability samples of large populations. Such problems are particularly present in efforts to track public opinion and voting behavior. James Fowler focuses on the problems of drawing causal inferences from whole networks using sociocentric data that may be collected in studies of specific communities. Sociocentric data are especially valuable for understanding the indirect effects of people on one another in a social network (e.g., the effects of friends of friends), which may be vital to assessing the diffusion of ideas in a network, for example. I examine measurement errors brought about by strategic respondents who wish to shape the outcomes of network studies or who are influenced by the study's design. My analysis may be particularly valuable in studies of elites struggling against one another for power in a

network. David Nickerson outlines procedures for inferring the causal effects of networks from field experiments. Although such experiments may not always be ethical or practical, they may be invaluable for some questions on which laboratory experiments have little external validity, such as the influence of networks on voting behavior. John Padgett concludes the article by explaining how to triangulate on causal processes. While Padgett's advice is widely applicable to network studies, it is especially pertinent to historical studies that draw upon complex relational databases. Scholars investigating the development of bureaucratic agencies, shifting coalitions in Congress, and the emergence of urban power networks, for example, would benefit most readily from Padgett's recommendations.

The Social Citizen: Social Networks via Surveys of National Random Samples

By Betsy Sinclair

The Erie County Study of 1940 and the Elmira Community Study of 1948 report finding positive and significant effects of peers on individual political behavior from detailed panel samples of small, local communities (Berelson, Lazarsfeld, & McPhee, 1954; Lazarsfeld, Berelson, & Gaudet, 1948).[1] In recent years, there has been a renaissance of this original literature, with studies conducted on different community samples to investigate the effect of peer networks on vote choice, social communication, expertise, racial attitudes, and disagreement (Huckfeldt et al., 1995; Huckfeldt, Levine, Morgan, & Sprague, 1998; Huckfeldt & Sprague, 1988, 1991, 1995; Leighley & Matsubayashi, 2009). The applicability of community-based surveys has come into question, as many scholars claim that Americans are increasingly "bowling alone" and replacing neighborhood social interactions with online social interactions (Putnam, 2000; Sunstein, 2001). In particular, if respondents are more likely to have discussion partners who are geographically distant, then knowledge of the respondent's geography is not sufficient to control for the information they would receive from peers. It is then crucial to know something about the structure of the social network if peer-to-peer communication affects political choices.

This concern has resulted in the inclusion of network batteries on surveys of national probability samples, including the General Social Survey (GSS) and the American National Election Study (ANES). National probability sample surveys allow for network analyses on the discussion network of each individual across a wide range of different types of individuals in different types of

geographies, making it possible to test general theories about the relationship between the respondent and her network. These surveys are used to document and explain behaviors beyond the standard decision calculus, which considers behavior solely as a function of individual preference, probability of impact, or civic responsibilities. Use of these surveys to draw causal inferences about the effects of peer networks requires assumptions about selection and homophily—while the 1985 GSS, 1987 GSS, and 2000 ANES all included social network batteries, they are limited in their coverage of demographic and political information about network discussion partners. For example, the 2000 ANES does not include any questions regarding the socioeconomic status of the discussant, the geographic location of the discussant, or the propensity of the discussant to vote. Thus causal inferences regarding influence of the discussants on the respondents' political choices cannot be determined without assumptions regarding the selection process, as it is possible that both respondent and discussant have identical preferences and are connected based on those preferences.[2]

Causal inferences can be drawn only after accounting for four issues and addressing the possibility of unobserved variation driving the results. First, as most of the surveys using national probability samples ask for only a small number of discussants, it is necessary to assume that these few individuals are an appropriate proxy for all the peer influences an individual will receive. Second, identification of peer effects is only possible after assessing network selection based on homophily. Third, it is necessary to assume that the respondent will appropriately recall, and truthfully describe, her network alters' demographics and political preferences. Fourth, it is necessary to elicit the respondent's contextual influences. Here network causality is defined in the context of the Rubin Causal Model (Rubin, 1980), where the effect of a treatment on an individual is defined as the difference between two outcomes: the outcome that manifests when this individual receives the treatment and the corresponding outcome when the treatment is withheld. Often referred to as the "counterfactual" model of causality—as no individual can both receive and not receive treatment—the Rubin Causal Model is often invoked when the treatment variable for a particular variable is not affected by the assignment of treatment and control to other individuals (Heckman, 2005; Rubin, 1980), as is the case with randomized field experiments. In the framework of using surveys to ascertain the effect of the network on each respondent's political preferences or behaviors, it is necessary to use each individual's self-report about their network alters as the treatment variable and to be able to obtain covariates to control for the correlation in the assignment of treatment and control within a particular network. This essay clarifies the assumptions necessary to invoke

the Rubin Causal model with network batteries on national probability samples and conclude with a series of recommendations for best practices.

Small Sample Size

Within most surveys which include network batteries, respondents are asked to identify a small number of discussants, usually three to six, who compose their social network. To rely on these surveys, it is necessary to assume that these discussants are representative of the respondent's larger social sphere or to test hypotheses regarding each respondent's close social network. Research that attempts to estimate network size suggests that the average network contains somewhere between 290 and 750 people (Killworth, Johnsen, Bernard, Shelley, & McCarty, 1990; McCarty, Killworth, Bernard, Johnsen, & Shelley, 2001; Zheng, Salganik, & Gelman, 2009). Weak ties are not likely to be included in the small set of individuals identified by the respondent (Mutz, 2006). There are two possible assumptions here: either an individual is "treated" by her close ties only (the ones identified in the survey) or this small network is representative of her broader network. We observe the consistent presence of disagreement across these survey responses, so while this difference may be an underestimate of the actual amount of political disagreement to which an individual is exposed from her broader network, there is at least some heterogeneity of preferences. Analysis of network surveys requires the assumption that these respondents are representative of—or at least the primary influences within—the component of individual's political network that is the scope of the research question.

Identification

Homophily is a well-documented phenomenon within social relationships where there is a tendency for individuals to form social ties with others who are similar to them (Coleman, 1958; Lazarsfeld & Merton, 1954; McPherson, Smith-Lovin & Cook, 2001). Homophily is most likely to occur with respect to race and ethnicity, age, religion, education, occupation, and gender, roughly in that order (McPherson et al., 2001). Individuals are also likely to form ties with others who have a similar number of friends (Huckfeldt, 2009). Little research has investigated the extent to which relationships will form based on political behavior or preferences, but there are high degrees of correlation between an individual's party identification and those of her discussants, suggesting that some relationships may form based on shared political values (Lazar et al., 2009). If social relationships are formed solely based on shared

political preferences or behaviors, then the political correlation that is present between the respondent and her network is based on selection, and not on peer influence.

Fortunately, homophily fails to characterize all of an individual's social relationships, as many social ties are formed based on availability, and not solely on personal choice (Huckfeldt & Mendez, 2008; Mollenhorst, Volker & Flap, 2007, 2008). It is likely that an individual will choose some of her peers based upon shared politics. Identification of peer effects is only possible when there is some fraction of discussants who are chosen based upon availability—relationships that were selected not based on shared political preferences. If it were possible to capture all characteristics which drive the selection of personal relationships, then even though those characteristics are also correlated with politics, it would still be possible to observe causal political network effects. However, survey time is limited. The shared characteristics that are observed enable us to control for selection of those relationships—where there is homophily—and the residual effect is either a peer effect or the effect of an unobserved characteristic. It is necessary to document and control for the shared characteristics between individuals so that the correlation is attributable to observed characteristics between characteristics and preferences, either from structural or choice homophily, is controlled for in the calculation of the network effect. The remaining network effect on political preferences or behaviors is then attributable to the effect of the network alters.

Recall and Truthfulness

Survey respondents usually are asked to provide names of individuals with whom they have discussed "government, elections, and politics."[3] They are then asked to identify a full set of characteristics of each discussant, including their socioeconomic and demographic characteristics, their relationship to the respondent, and their political preferences and choices. One large concern with the analysis of these data is that the respondent will fail to identify correctly and recall all of this information.

Huckfeldt and Sprague (1987, 1988) and Huckfeldt, Sprague, and Levine (2000) conducted snowball surveys on political discussion partners where they conducted a second wave survey of the discussants. They found that approximately 80% of all respondents were able to correctly identify the political preferences of their discussants, and that of the 20% misidentified; approximately three fourths believed the discussant agreed with the respondent. Analysis of the self-reported social network data should consider these findings as a mechanism to adjust for the level of disagreement in the network. However, it

is likely that the belief of the respondent, regarding the discussants' preferences within the misidentified relationships, is the key variable—not the true discussant preferences. The belief of the respondent is particularly key when asking for the "network treatment" variable, as these data are only applicable to studies of causal inference insofar as the respondent is able to identify the discussant's preferences, behaviors or characteristics.

Contextual Effects

One advantage of the national probability sample surveys is that they elicit independent networks, where spillovers of treatment effects are not a concern from one network to another.[4] These data have the added benefit of providing variation in the context of the respondents, so that all individuals within the network are not exposed to a common, unobserved variable. Yet these data are exposed to a standard criticism of observational network data, which is that unaccounted environmental factors or other unobserved factors could drive the relationship between the respondent and her discussants (Cohen-Cole & Fletcher, 2009).[5] The sole solution for this problem in survey research is to ask additional questions of the respondent to control for context, as well as to be able to locate the respondent geographically and append additional data to the respondent's physical location.

Conclusion: Causal Inferences

Drawing causal inferences about the effect of an individual's social network on her political behavior, when using survey data from national probability samples, requires both significant data resources as well as several assumptions. Under the best of circumstances, the available data would document the presence of heterogeneity in characteristics of the network members with respect to the respondent, include a full set of individual characteristics, and document and control for the respondent's context and susceptibility. It is further necessary either to assume that the network data are reported truthfully or to model explicitly inaccuracy in data reporting, as discussed below by Michael Heaney. Thus these surveys are most useful in studies where we focus on intense, socially proximate relationships.

A panel survey would provide an ideal research design. In this case, it would be possible to draw causal inferences using national probability sample survey data. There are enormous advantages to being able to test theories regarding the effects of networks in national probability sample data because of the generalizability of the sample and because the individuals in the survey are unlikely to interact with each other. We know that individuals may be

influenced by discussants and/or may influence discussants. In a single time period, solicitation of discussants' political preferences can only provide correlations with the respondent unless a particular structure for influence is assumed.[6] Analysis of panel data, where the respondent is asked to identify her network, preferences, and the preferences of those in her network in at least two different points in time, allows for observation of change in ideology, candidate preference, and party identification for both the respondent and the discussants. Existing panel studies have been able to document the relationship between social networks and civic participation, for example (Klofstad, 2009). A panel study would enable researchers to estimate a difference-in-differences estimator where the "treatment"—the politics of the social network ascertained at the first point in time—could be seen to have an effect by comparing the difference between individuals who either agreed with their network or disagreed with their network during the first survey, for example, with the second panel. Sensitivity analysis could be conducted in this framework using Rosenbaum bounds (Rosenbaum, 2002). Panel analysis helps to relax the assumptions necessary regarding homophily and selection.

Even with the ideal set of data, it would still be necessary to test for sensitivity to the presence of unobserved confounders before interpreting any causal evidence of discussant preferences or behavior on the respondent. Yet, when all of these conditions are met, it is possible to draw causal inferences on survey data without randomization of discussant treatment. Analyzing network surveys on national probability samples has a number of benefits. In particular, there is a high degree of geographic variation in the location of the discussants with respect to the respondent across these surveys—for example, only 20% of the discussants identified in the 2006 ANES share a household with the respondent. Thus one benefit of conducting these surveys is to elicit the identities of individuals with whom the respondent discusses politics. Using geography as a proxy for a social network may be insufficient in these cases where communication about politics can take place via phone, Internet, and other technologies. After careful consideration of the assumptions involved, surveys provide one way to understand the impact of political networks.

Evidence for Causal Relationships in Observational Network Data

By James H. Fowler

For decades, political scientists have been measuring direct person-to-person influence of friends and family on political attitudes and behaviors using egocentric data (Huckfeldt & Sprague, 1995; Lazarsfeld, Berelson, & Gaudet,

1948). But only recently have we begun to consider how influence may spread from person to person to person (Fowler, 2005). Experimental evidence suggests that these indirect effects are real, with 60% of the effect of one person on a second person being passed on to a third in the case of voter turnout mobilization (Nickerson, 2008). But experiments—as discussed by David Nickerson below—are expensive and have limited external validity. The growing availability of cheap and large-scale social network data (Lazer et al., 2009) means that methods to identify possible causal effects in observational data will be increasingly important for understanding the role that social networks play in politics.

Here I outline some possible confounds that complicate causal inference in observational studies of network effects and how researchers have begun to deal with them in both egocentric and full network data sets. If people connected to each other in a social network exhibit similar political attitudes and behaviors, it could be attributed to at least four processes. (a) *Random clustering* may result when many people with the same characteristics happen to be connected to one another by chance. (b) *Homophily* (which literally means "love of like") occurs when individuals choose to become connected to those who have similar characteristics (McPherson et al., 2001). (c) *Contextual effects* may result when connected individuals jointly experience contemporaneous exposures, such as seeing the same political advertisement. (d) *Influence* occurs when political attitudes or behaviors in one person cause a connected person to adopt the same attitudes or behaviors.

Random Clustering

To estimate the influence of an "alter" (a social contact) on an "ego" (the focal individual), we must be sure that similarity of a characteristic between them is not simply due to chance. Standard techniques, such as Pearson correlation, that assume independence of the observations are not adequate because of the complex interdependencies in the social network. To take the network into account, we must first measure the empirical probability of observing a characteristic in ego conditional on the same characteristic being present in alter. This effect can be calculated by summing the total number of dyads (all ego-alter pairs) in the observed network where *both* ego and alter exhibit the characteristic and then dividing by the number of dyads in which the alter exhibits the characteristic. We then repeat this procedure in 1,000 randomly generated networks in which the network topology and the overall prevalence of the characteristic are exactly the same, but we randomly shuffle the assignment of the value to each node. This procedure generates a theoretical distribution

of conditional probabilities that could have resulted due to chance. We can then use these values as a baseline to generate differences between the observed network and each of the random networks. Confidence intervals can be obtained by sorting the results and taking the appropriate percentiles (e.g., 95% confidence intervals can be obtained by looking at the 25th and 975th ranked values).

In studies of obesity (Christakis & Fowler, 2007), smoking (Christakis & Fowler, 2008), happiness (Fowler & Christakis, 2008a), and loneliness (Cacioppo, Fowler, & Christakis, 2009), this method shows a significant correlation between directly connected egos and alters, and alters up to three degrees of separation. For example, if we know that your friend's friend's friend is happy, then we can do better than chance at predicting whether or not you will also be happy. This method can be used for both full network data and egocentric data, though in the latter case it is obviously only possible to evaluate associations at one degree of separation.

Homophily

As Betsy Sinclair explains above, if people with the same political attitudes or behaviors tend to befriend one another, it might create a cluster of like-minded people in the network that is not driven by influence. To control for homophily, analysts must model the friendship-formation process that occurred prior to the current interactions that may be generating influence. Analysts must therefore focus only on those pairs that have sustained a relationship in two or more consecutive periods. This method requires repeated measures of each person's characteristics and longitudinal information about their network ties (Carrington, Scott, & Wasserman, 2005; Fowler, & Christakis, 2008b).

A simple statistical model that controls for homophily regresses the ego's characteristic on the alter's characteristic in the current period, and includes as controls the characteristic of both the ego and the alter in the previous exam:

$$Y_{t+1}^{ego} = \alpha + \beta_1 Y_t^{ego} + \beta_2 Y_{t+1}^{alter} + \beta_3 Y_t^{alter} + \gamma_{ego}$$

Inclusion of the ego characteristic in the previous period typically eliminates serial correlation in the errors (as demonstrated by a Lagrange Multiplier Test—see Beck 2001) and also substantially controls for ego's genetic endowment (for political examples, see Alford, Funk, & Hibbing, 2005; Dawes & Fowler, 2009; Fowler, Baker, & Dawes, 2008; Fowler & Dawes, 2008; Fowler & Schreiber, 2008; Settle, Dawes, & Fowler, 2009) and any stable tendency to

exhibit the characteristic. The alter's characteristic in the previous period helps control for homophily (Carrington et al., 2005). The key coefficient in these models that measures the influence effect is the coefficient on the variable for alter's characteristic in the current period (β_2). Analysts should use generalized estimating equation (GEE) procedures to account for multiple observations of the same ego across periods and across ego-alter pairings (Liang & Zeger, 1986) and assume an independent working correlation structure for each ego (Schildcrout, 2005), though Huber-White sandwich estimates with clustering on egos also yield very similar results. One might also wish to control for correlated observations in alters, though in practice we have never found that this significantly changes the model output.

To better understand this method, Monte Carlo simulations of the model can be used to test whether homophily tends to bias the estimate of the influence effect under a set of assumed conditions for the process of network formation (R code for these simulations is available at http://jhfowler.ucsd.edu/homophily_and_influence_monte_carlo.R). In these simulations, a population of individuals is generated, each with a feeling thermometer score (say, for the current President) drawn from a normal distribution (this is just an example—we could use any measure of political attitudes or behaviors). Individuals are then allowed to form ties with a probability that is a sum of two variables: a variable that is inversely proportional to the absolute difference in the two individuals' feeling thermometer scores and a uniform random variable. The weight on these two variables can be varied so that ties are either formed purely due to homophily, purely due to the random variable, or some combination of the two. In other words, the weight is the percentage contribution homophily makes toward the formation of social ties.

Each individual receives an exogenous shock to his or her score that is drawn from a normal distribution, and the ego's new feeling is equal to a weighted combination of his or her own previous feeling with the shock and the average of their friends' feelings (with their shocks). The weight on friends' feelings is the influence effect (how much do one person's feelings about the President influence another's?). The regression model can then be used on the simulated data to see whether the degree of homophily affects the estimated influence effect. This analysis is accomplished by repeating these steps thousands of times and then comparing the "true" parameters used to simulate the data to the estimated parameters inferred from the regression model. Figure 1 shows two sample tests from this analysis. On the left, the "true" influence effect is held constant at 0, and the way people form friends is slowly changed from random formation of friendships to those that are purely driven by homophily. Note that under the assumed conditions for network formation, the

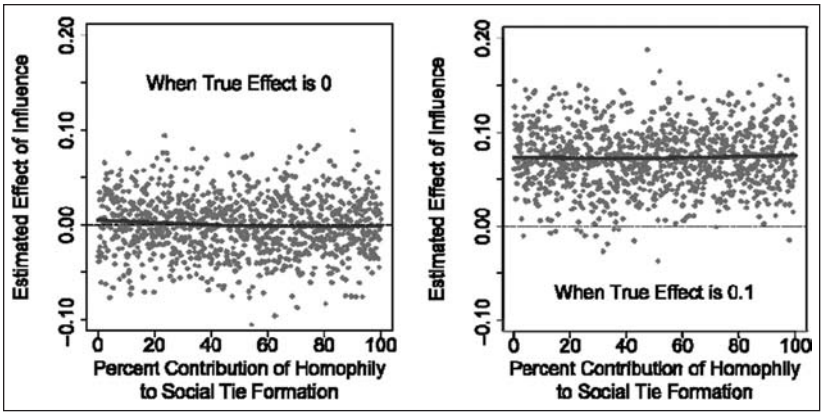

Figure 1. Monte Carlo simulations of network regression model show homophily does not affect estimate of induction effect

Note: Each point represents the estimated effect of alter's feeling thermometer score on ego's feeling thermometer score in one simulation of a network with the homophily weight indicated on the x-axis. The dark line shows best fitting LOESS curve to the observed points. "True effect" refers to the influence parameter used in the model to simulate the influence of alter on ego. 1,000 simulations of a 1,000 person network are shown. Baseline distribution of feeling thermometer scores is assumed to be normally distributed with mean 50 and standard deviation 10. The shock distribution is assumed to be normally distributed with mean 0 and standard deviation 5. Other simulations (not shown) suggest the results are robust to different distributional assumptions.

regression model produces unbiased estimates of the effect that average to 0. And, importantly, it does so even when network formation relies 100% on homophily to generate social ties! Thus, even if people in an observed sample form friendships solely on the basis of similarity in feelings about the President, we would not tend to find a peer-to-peer influence effect where none truly existed.

On the right, the "true" influence effect is held constant at 0.1 and once again the way people form friends is changed from random formation of friendships to those that are purely driven by homophily. Here the results show that peers exert a small effect on average, and once again the result is not biased upward by the presence of homophily. Even if people in the sample tend to form friendships solely of the basis of similarity in feelings, the regression model will not tend to overestimate the size of a peer-to-peer influence effect. In fact, the main drawback to the model is that the estimated effect tends to be lower than the true effect on average but, in this setting, this just means that the model is conservative for effects of this size.

Finally, although people may choose friends based on an attribute other than the outcome of interest, this will only complicate the story in cases where this attribute is omitted and moderately correlated with the outcome. If the correlation between the omitted variable and the outcome is low, then the omitted variable will not be a source of confounding. If it is high, then the lagged outcome will be a good proxy for controlling homophily due to the omitted variable.

Contextual Effects

The last, and most difficult, confound is the presence of contextual effects. An omitted variable (like a campaign) may influence both ego and alter, causing their outcome variable (e.g., feelings toward a candidate) to move in synchrony even when alter has no influence on ego. One method of controlling for contextual effects is to add a fixed effect for each ego to the model. However, adding fixed effects to dynamic panel models with many subjects and few repeat observations creates severe bias toward zero coefficients. This bias has been demonstrated both analytically (Nickell, 1981) and through simulations (Nerlove, 1971) for OLS and other regression models. Therefore, failure of a test with fixed effects included does not necessarily mean that influence effects do not exist.

An alternative method is to analyze how the effect size of the association between ego and alter changes with the direction of the social contact (Bramoulle, Djebbari, & Fortin, 2008). If unobserved factors drive the association between ego and alter friendship, then directionality of friendship should not be relevant. That is, if Susan names Paul as a friend, then we expect Paul to have an effect on Susan. However, if Paul does not reciprocate by naming Susan as a friend, then Paul may not be affected by Susan's political attitudes or behaviors. If contextual effects were spuriously driving the relationship between Paul and Susan, then one would not expect a directional result. The context would cause the named friend and the namer to move up and down simultaneously; hence, the expectation is for the namer to have an influence on the named friend. But in studies of obesity (Christakis & Fowler, 2007; Fowler & Christakis, 2008b), smoking (Christakis & Fowler, 2008), happiness (Fowler & Christakis, 2008a), and loneliness (Cacioppo et al., 2009), researchers find that namers do not have a significant influence on named friends, suggesting that the effect of named friends on namers is at least partly due to influence.

One final way to assess contextual effects is to study the role of geographic distance. If contextual factors are more likely to be jointly experienced by people who live near one another than those who live far away, then we would

expect the size of the effect of alter on ego to diminish with distance. Behaviors such as obesity (Christakis & Fowler, 2007) and smoking (Christakis & Fowler, 2008) do not exhibit this relationship—socially close friends who live hundreds of miles away have as much effect on behaviors as friends who live next door. On the other hand, affective states like happiness (Fowler & Christakis, 2008a), and loneliness (Cacioppo et al., 2009), do exhibit decay with distance, but this is more likely due to the need for frequency of contact for these outcomes to spread. Importantly, those studies show that next-door neighbors influence one another, but same-block neighbors do not, suggesting that neighborhood, street-level, and even block-level effects cannot explain the effect of alter on ego.

In sum, these methods represent possible ways to tease out causal effects from observational data. They are likely to become increasingly important as we study how political attitudes and behaviors spread from person to person to person in complete social networks.

Measurement Error and Causal Effects in Political Network Analysis

By Michael T. Heaney

The Heisenberg uncertainty principle in physics can be loosely stated as the idea that when studying a microscopic phenomenon, the precision of the measurement tools affects the degree of certainty with which the properties of the phenomenon can be observed (Heisenberg, 1949; Hofstadter, 1985, p. 464). While macroscopic phenomena of politics differ considerably from the electrons observed by Heisenberg, his principle applies analogously in political science: the use and precision of measurement instruments affects what is observed about politics. For example, if subjects are able to observe the measurement instrument, then being exposed to political research may cause subjects to reflect on their political identities, positions, and strategies. Consequently, they may change their political behavior as a result of knowing that they are being observed. The study in which they participate *itself* may be an opportunity for the respondent to make a statement about identity and political affiliation.[7]

As political scientists rush headlong into the study of networks, it is worth reflecting upon how the tools for measuring networks potentially change their structures—or at least their *observed* structures. A growing literature has addressed questions of measurement of social networks, generally, though it has not yet turned to measuring *political* networks, specifically. This essay claims that

political networks—or at least certain types of political networks—are subject to peculiar measurement errors that are worthy of caution during research design. Significantly, respondents may answer survey questions about the structure of their networks in such a way that attempts to raise or lower the political status of other actors. Under these circumstances, the survey itself may be a cause of the observed network structure—thus requiring some self-conscious adjustment on the part of the investigator.

This essay explores the problem of measurement error and the implications that it may have for assessing causal effects in political networks. It begins by making the case for methods where the researcher and the network participant interact directly (such as surveys, interviews, and direct observation) relative to unobtrusive methods (such as archival analysis and indirect observation). Then it considers common problems of network measurement, situates them within this type of political research, and provides an empirical illustration of these problems. Finally, a series of methodological solutions are proposed for reducing the causal effects of network measurement on observed network structures.

Obtrusive versus Unobtrusive Methods

A wide variety of political networks can be measured unobtrusively using institutional records or other forms of archives. Examples of recent research using this approach include studies of international conflict (Hafner-Burton & Montgomery, 2006), legislative cosponsorship (Fowler, 2006), caucuses in the U. S. Congress (Victor & Ringe, 2009), multiplex interest group and political party networks (Grossman & Dominguez 2009; Koger et al., 2010), and the rise of institutional innovations during the Renaissance in Florence (Padgett & McLean, 2006). In these cases, the observed actors do not have the opportunity to react to the research and, thus, cannot cause the reported structure of the network.

While these studies benefit from the advantages of relatively "objective" measurement and have the ability to explore the historical evolution of networks, they face several drawbacks as well. First, by relying only on official records, they may miss the informal—but crucial—interactions that reflect the networks that may be most relevant for politics. Second, these studies are limited in time increments of their network measures—they may only be able to measure the network on an annual basis, for example—thus missing relevant changes that may occur between the intervals. Third, these studies cannot capture political developments as they occur, leaving researchers to wait passively for data to arrive with a lag of (potentially) several years.

In contrast, other scholars directly survey political actors about their networks. Respondents in these studies are asked to report on their political discussion partners (Huckfeldt & Sprague, 1987), communication and influence among interest group lobbyists (Carpenter et al., 2004; Heaney, 2006; Heinz et al., 1993; Laumann & Knoke, 1987), organizational memberships of antiwar protesters (Heaney & Rojas, 2007), and the political involvement of youth activists in Brazil (Mische, 2008). In these studies, networks are measured in one of two ways. One method asks respondents to list their discussants or contacts in an open-ended question (Heaney & Rojas, 2007, 2008; Huckfeldt & Sprague, 1987; Mische, 2008). The other method shows respondents a list of network members and asks them to indicate with which members the respondent communicates, shares resources, views as influential, or has other kinds of network ties (Heaney, 2006; Heinz et al., 1993; Laumann & Knoke, 2007).

Obtrusive methods for gathering political network data have special advantages. First, they solicit individuals' firsthand accounts of their network involvements, thus capturing informal ties that may be neglected in archival and institutional research. Archival research can only analyze ties that are officially recorded. But surveys and interviews can ask respondents about any kind of tie: Who do you like or dislike? Whose political advice is useful or unreliable? Whom do you consider to be your allies or adversaries? Second, surveys and interviews facilitate the analysis of relevant political actors who might not be the subject of official recordkeeping. Record-based studies of legislative networks appropriately examine connections among members of Congress, but surveys may be the only way to reveal similarly important connections among staff members, constituents, and other less-visible actors. Third, they have the potential to capture politics "in action," measuring networks as they unfold dynamically. Heaney and Rojas (2008), for example, follow antiwar organizations over time from 2005 to 2007, thus allowing them to observe the consequences of the breakup of a major antiwar coalition. Since this kind of activity leaves few formal records, the dynamics of these politics would be lost to history in the absence of on-the-ground collection of network data. In summary, surveys and interviews allow the investigator to see networks that are otherwise invisible to unobtrusive methodologies.

Despite the advantages of collecting network data through direct contact with participants, this approach introduces new forms of error into the analysis, especially when respondents suspect that their answers may have political consequences. In the following section, I outline these sources of error and suggest the how they are likely to alter causal inferences drawn from the analysis of political networks.

Pitfalls in Network Surveys and Interviews

When respondents are asked to report on their ties, these reports are vulnerable to errors and misrepresentation, as mentioned above by Betsy Sinclair. Respondents may either fail to list ties or may make false reports of ties, yielding an accuracy rate of 40% to 60% in the measurement of most communication networks (Marsden, 1990). First, the extent of these problems is, in part, a function of the survey/interview instrument. Open-ended (recall-based) questions about network ties are more prone to forgetting than are list-response (recognition-based) questions, though open-ended questions may also be less prone false reports (Brewer, 2000). Second, whether ties are reported may depend of the strength of tie in question, as strong ties are more likely to be reported than weak ties (Butts, 2003). Third, reporting of ties may depend on characteristics of respondents and the characteristics of members of the network, which Feld and Carter (2002) explain with their distinction between "expansiveness bias" and "attractiveness bias." Expansiveness bias occurs when respondents have a tendency to over- or underreport their ties with others. Alternatively, attractiveness bias occurs when some members of a network are likely to be under- or overreported by others in the network. Fourth, further complications may be caused by data missing as a result of boundary specification, nonresponse, and fixed-choice survey questions (Kossinets, 2006).

Measurement error may lead to substantial distortions in the structure of the network. Network size, range, and density may fluctuate as a result of measurement error (Brewer & Webster, 1999; Feld & Carter, 2002). This problem can be illustrated by examining concordance in multiple reported network ties (Adams & Moody, 2007). In a study of a whole network, each respondent is asked to report on ties with every other network member. Where there is concordance among respondent reports—when both respondents agree that they either have or do not have a tie—then the likelihood of measurement error is relatively low.[8] However, when one respondent reports a tie and the other does not—an uncorroborated tie—then it is likely that someone is mistaken. The researcher then must make a judgment about whether to insist on concordance as the basis of a tie, or to allow discordant ties to serve as the basis of ties. Reanalysis of data on networks among health care lobbyists, that I reported in Heaney (2006), shows that this distinction makes a difference, as represented in Figure 2. If corroboration is required, the network centralization is reduced by 27.44%, heterogeneity is increased by 25.31%, density falls by 63.49%, transitivity is reduced by 28.04%, and average degree drops by 63.49%.[9] These results illustrate the substantial changes in the structure of a network produced by differences in measurement.

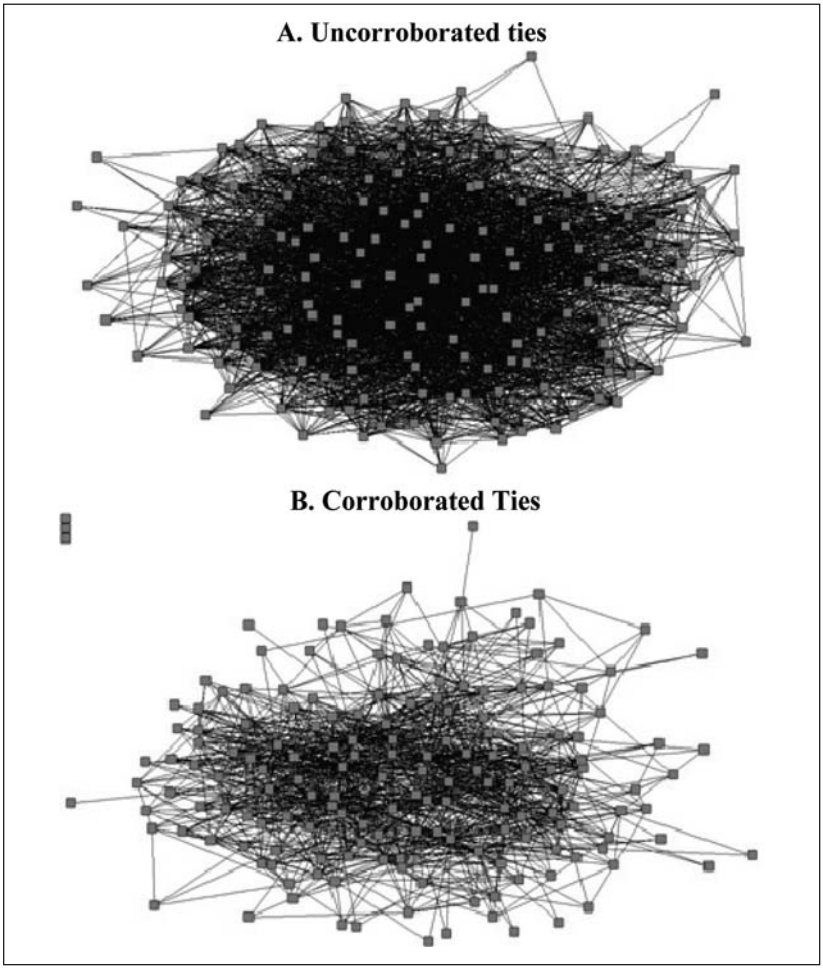

Figure 2. Comparison of uncorroborated and corroborated ties among health care interest groups in Washington, DC, 2003

Allowing respondents to report on their network ties permits them to determine, partially, the structure of the network under investigation. The consequences of this causal effect may depend on the motivations of respondents. If respondents elect to "perform" for the survey, the consequences may be particularly troubling. Expansiveness bias is a problem if some low-status respondents

attempt to raise their status by overreporting their ties, while high-status respondents prefer to downplay their influence by underreporting their ties. Attractiveness bias is a particular concern because certain network members may be especially visible contacts, while others are less visible, thus drawing or discouraging ties that either did or not exist in fact, depending on the visibility of the alter. In either case, surveys of political actors may be especially problematic, as these respondents may wish to use the survey instrument to send a political message or to influence the ultimate reports about which actors are most influential or central.

An Empirical Example

To illustrate problems of measurement error in political networks, I reanalyze data reported in Heaney (2006). I interviewed lobbyists at 168 of the leading health care interest groups. As part of the interview, I asked them to fill out a survey about their contacts with other leading health care interest groups. They reported on which contacts they had, as well as which ones they perceived to be influential over health care policy. In addition, I collected data on groups' lobbying spending, partisanship, campaign contributions, advertising in congressional newspapers, grassroots lobbying in Congress, organizational age, comembership in coalitions, organizational types, and issue niches.[10]

I sought to determine if there was systematic measurement error in the responses given by these health care lobbyists. First, I hypothesized that (consistent with expansiveness bias) respondents representing interest groups with stronger reputations for influence would be likely to underreport their contacts with less influential actors (so as to avoid the appearance of ties with lower status actors), while respondents representing reputedly less influential interest groups would overreport their contacts with more influential actors (so as to create the appearance that they are more important than they really are), other things equal. Second, I took advantage of a feature of the survey in which members of the network were presented in a fixed list without variation in the order. I hypothesized that (consistent with attractiveness bias) interest groups toward the top of the list would be cited more frequently, while interests groups toward the bottom of the list would be cited less frequently, other things equal.

An Exponential Random Graph Model (ERGM) can test for expansiveness and attractiveness basis in these data. Following Hunter, Handcock, Butts, Goodreau, and Morris (2008) and Frank and Strauss (1986), the directed network of contacts, **Y**, is parameterized as follows:

$$P_{\theta,\lambda}(Y = y) = [\ \exp\{\theta^T, g(y, X)\ ] / [\ X\ (\theta,\lambda)\ ],\ \mathbf{y} \in \lambda$$

where $\theta \in \nabla \subset R^q$is the vector of model coefficients and $\mathbf{g}(\mathbf{y}, \mathbf{X})$ is a q-vector of statistics based on the adjacency matrix **y** and the **X** matrix of covariates. The **y** matrix contains edges (akin to the "constant" of an ERGM) and mutual ties, while the **X** matrix contains measures of organizational influence, proximity to the end of the survey, groups' lobbying spending, partisanship, campaign contributions, advertising in congressional newspapers, grassroots lobbying in Congress, organizational age, comembership in coalitions, organizational types, and issue niches. The results of the analysis are reported in Table 1.

The ERGM results support the view that the contact data contain significant, systematic measurement errors. The significant, negative coefficient on first parameter of the model shows that when A's influence reputation is greater than B's, A is less likely to cite B than B is to cite A. Importantly, this parameter is estimated while holding constant the level of an organization's influence (the first node attribute in the model), which should and does correlate positively and significantly with more network contacts. Thus, as an interest group's influence reputation rises, it is less likely to list an unreciprocated contact, but it is also more likely to receive an unreciprocated contact. This finding indicates that more influential interest groups systematically underreport their contacts with less influential groups and/or less influential groups overreport their contacts with more influential groups. Either story is consistent with the strategic reporting based on influence reputation expected by expansiveness bias. Consistent with attractiveness bias, the results also show that interest groups were more likely to be cited if they appeared early in the survey and less likely to be cited if they appeared late in the survey, other things equal. Other variables (e.g., grassroots lobbying) are included in the ERGM to demonstrate the inherent biases in the use of surveys of political networks are present after accounting for these alternative explanations for influence citations.

A goodness-of-fit analysis is reported in Figure 3 to demonstrate the appropriateness of the estimated ERGM for these network data. Following Hunter, Goodreau, and Handcock (2008), I graph actual versus simulated network for three network properties: in degree, minimum geodesic distance, and edge-wise shared partners. In addition, I include an analysis of triad census. The black line in each graph represents the statistics from the actual network, while the gray lines represent the upper and lower bounds of 100 simulations of the model. Overall, the goodness of fit of the ERGM is strong.

Table 1. Exponential Random Graph Model of Report Communication Among Health Care Interest Groups in Washington, DC, 2003

	Coefficient	*SE*	MCMC *SE*	Significance
Dependent variable				
A Reports communication with B (= 1 if yes)				
Independent variable				
Expansiveness bias				
A's influence reputation is greater than B's (= 1 if yes)	–0.40130	0.03486	0.00045	$p < .001$
Attractiveness bias				
B's proximity to end of survey (higher numbers closer to end)	–0.00074	0.00012	0.00000	$p < .001$
Node attributes				
B's influence reputation (in number of peer citations)	0.01258	0.00017	0.00000	$p < .001$
B's Federal Lobbying Spending (in millions of US$)	–0.01469	0.00111	0.00001	$p < .001$
B's Partisan bias in lobbying contacts (the absolute value of Democratic contacts minus Republican contacts)	0.00724	0.00085	0.00001	$p < .001$
B's PAC campaign contributions (in millions of US$)	–0.00457	0.00393	0.00004	
B's Advertising in Congressional newspapers (= 1 if yes)	0.03819	0.00889	0.00009	$p < .001$
B's Grassroots lobbying in Congress (in number of staff citations)	0.00143	0.00029	0.00000	$p < .001$
B's Organizational age (in years)	–0.00006	0.00011	0.00000	
Edge attributes				
A and B are comembers of a coalition (= 1 if yes)	0.98800	0.03744	0.00049	$p < .001$
A and B have the same organizational type (= 1 if yes)	0.53490	0.03686	0.00100	$p < .001$
A and B have overlapping issue niches (= 1 if yes)	0.64470	0.04434	0.00100	$p < .001$

(continued)

Table 1. (continued)

	Coefficient	*SE*	MCMC *SE*	Significance
Structural parameters				
Edges	−3.39100	0.04795	0.00100	$p < .001$
Mutual ties	1.72900	0.07175	0.00100	$p < .001$

g = 168 Health care interest groups
n = (g × g)—g = 28,056 Dyads of interest groups
Newton-Raphson iterations = 14
MCMC sample of size = 10,000
Akaike's information criterion (AIC) = 20,331
Bayesian information criterion (BIC) = 20,446

Note: Data are from Heaney (2006). MCMC stands for Markov Chain Monte Carlo. Estimates are produced using a maxium likelihood approximation based on MCMC using the ERGM package in R (Huter, Handcock, Butts, Goodreau, & Morris, 2008).

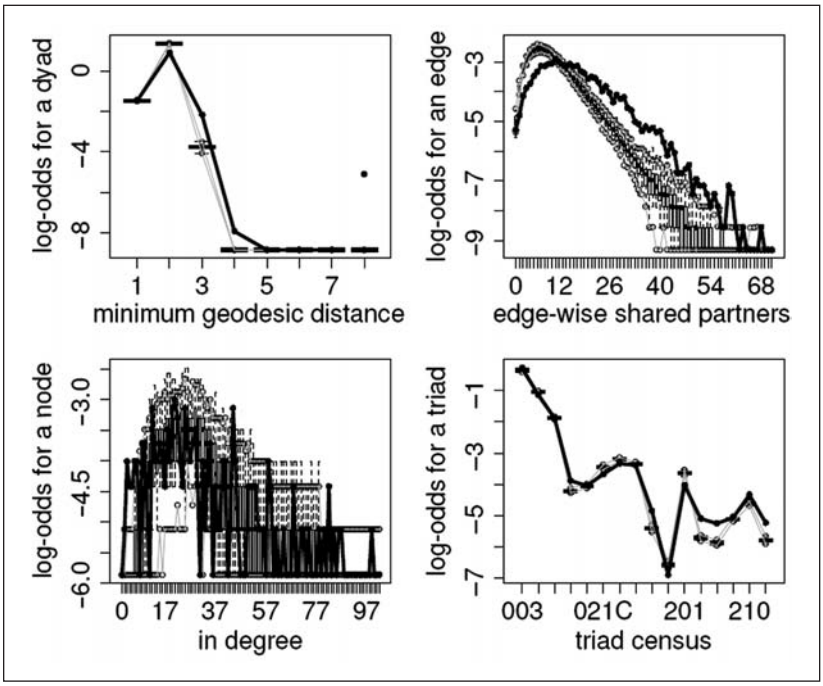

Figure 3. Goodness-of-fit diagnostics for exponential random graph model

The actual network and the simulated models match closely for the minimum geodesic distance, in degree, and triad census statistics. However, the simulated model systematically overestimates the number of cases with a small number of edge-wise shared partners, while systematically underestimating the number of cases with a large number of edge-wise shared partners. The model could possibly be improved by including additional structural network parameters, beyond edges and mutual ties, which are already included in the model. Although the estimated ERGM is not a perfect fit, the close match on three of four goodness-of-fit indicators suggests confidence in the ERGM as a representation of the network-formation process.

Strategies to Address Measurement Error

If measurement interferes with causal inferences about the network, what are some methods of correcting this problem? It is essential to recognize that the approach to measuring political structures in the network is a choice in the research design, rather than an inherent feature of the data. Thus it is possible to alter or supplement network measures to create a clearer picture of the causal process. First, researchers relying on directed reports of undirected data—the kind of data where concordance is at issue—should estimate their models two ways to see how the results depend on whether or not ties are corroborated. If either corroborated or uncorroborated ties yield the same result, this finding suggests that causal inferences are not strongly influenced by the measurement process. If the results using corroborated and uncorroborated ties differ, then the researcher should investigate the discrepancy further. When this approach is used in Heaney (2006), the results show that while there are significant measurement errors in reporting the communication network, the errors do not affect the conclusions drawn about its effects on interest group influence.

A second strategy is to create multiple measures of the same network. Multiple interviews with the same respondent may increase reliability (Adams & Moody, 2007). If informants are used for an organization, multiple respondents within the same organization may be more reliable than a single respondent for that organization. Turning to the third parties outside of an organization may be a way to uncover "hidden" ties—that is, when "everyone knows" that two actors are in communication, even when they mutually refuse to admit it. A third strategy is to factor measurement error directly into the model in question. By including variables that capture the effects of status, network position, or desire to influence the survey result, the researcher may be able to parcel out the effects of network measurement. A Bayesian approach may be particularly

well suited to weighting observations according to the degree of certainty associated with their measurement (Butts, 2003). Alternatively, explicitly measuring networks as directed or by retaining two-mode structures (rather than collapsing two-mode data to one mode) is a way to make the "errors" a part of what is being investigated.

A final strategy is to rethink the setting or framing of the interview/survey. Are there elements of the research design that prompt the respondent to "think politically"? One approach is to be sure to ask network questions before asking substantive questions about politics or policy. Another approach would be to deemphasize the political focus of the interview/survey by combining questions about political networks with questions about other kinds of networks (e.g., alumnae, friendship). The less explicitly the respondents are primed to think about the politics, the less they may see the study as an opportunity to assert their political identity and views beyond what is called for by the questions. Experiments that vary framing and priming within the survey may be a way to assess the nature and extent of these effects.

While any method of asking respondents about their political networks is likely to influence the reported structure of networks, this essay suggests strategies for reducing these effects. While observing political networks often changes them, it may be possible for political scientists to gain greater understanding of how and to what extent these networks are changed.

Conducting Experiments in Organic Social Networks

By David W. Nickerson

The way in which networks are formed makes drawing causal inferences about the effect of social networks on behavior and attitudes difficult. People self-select into social networks and are often exposed to common outside influences (e.g., particular candidates, factory closings, media programming). Disentangling the competing effects of selection, and unobserved common causes, from the effect of the social network using observational data in a manner that will convince skeptics, may prove impossible. Randomized experiments can often be used to surmount problems with selection bias and unobserved heterogeneity, so conducting experiments to establish properties of social networks is an attractive analytic strategy.

Laboratory experiments allow researchers to control and manipulate every part of a social network: the composition, structure, and communication within the network. Such studies are good means of establishing properties of networks

in the abstract and confirming that people conform to hypothesized behavioral strategies in assumed network structures. That said, if researchers care about real-world networks (e.g., neighborhoods, work places, families, legislators), then the degree to which results from social networks constructed in laboratories apply to the real-world network of interest is an open question. Conducting experiments on these organically formed, real-world networks can solve some of these concerns about external validity.

Given the difficulty and expense of measuring social networks, researchers conducting experiments on networks may be tempted to map the social network while measuring the outcome variable of interest, thereby eliminating one round of data collection. This essay explains why network data should be collected prior to randomization if at all possible when studying organic social networks. The lack of researcher control over the behavior and interactions of subjects makes the up-front investment necessary. The transparency of the analysis, improved statistical power, and ability to withstand problems encountered in the field are all improved by mapping the networks to be studied in advance. I begin by applying the logic of experimentation to social networks and briefly describing the form of most experiments. I then explain the reasons for pre-mapping networks and, in the process, describe problems that can arise when conducting studies of social networks. This essay concludes by discussing the drawbacks of the strategy.

The logic behind randomization makes a compelling case for the use of experiments when trying to establish causality. In the most basic form, experimental subjects are randomly assigned to receive the treatment of interest or to a control group that is not exposed to the treatment. Because treatment is assigned, self-selection by subjects is not an issue. Because the assignment is random, the subjects receiving the treatment should be comparable to subjects in the control group with regard to both observable (e.g., age, education) and unobservable characteristics (e.g., genetics, psychological dispositions, exposure to unseen causes). Thus systematic differences in outcomes variables can be attributed solely to the treatment. By constructing theoretically perfect data, causation can be established with a minimum of modeling assumptions through experimentation.

Compared to settings where researchers can assume that subjects are atomistic, applying the logic of experiments to organic social networks is not straightforward. The most obvious problem is the lack of researcher control when studying naturally occurring networks. In most instances, researchers cannot randomly assign subjects positions within social networks. An exception to this rule are studies of freshman roommates (e.g., Sacerdote, 2001) or soldiers (e.g., Goette, Huffman, & Meier, 2006) where subjects have extremely

limited autonomy over living arrangements, but it is unclear how the results of such studies translate to more typical networks where self-selection by members plays an important role (e.g., neighborhoods, churches, work places). Similarly, researchers generally cannot randomly manipulate communication within a social network, so the content of conversations and interactions are not only endogenous but also in a black box (see Nickerson, 2007, for an exception, though experimentally initiated conversations may have a different effect on subjects than everyday conversations).

As a result, most experimental studies of social networks involve imposing an external shock to a node of a network and tracing its ripple through the network (e.g., Miguel & Kremer, 2004; Nickerson, 2008). Under this strategy, researchers assign nodes (i.e., egos) to treatment and control conditions and then measure the change in the outcome variable of interest induced by the intervention by comparing the two types of nodes. The outcomes are then measured for network members of both treatment and control nodes (i.e., alters). Systematic differences in network behaviors or attitudes can be attributed to diffusion of the treatment or outcome through the network. It should be noted that a necessary precondition for this ripple strategy to work is that the experimental treatment provided must change the behavior of the initial node treated; otherwise, there is no ripple to trace.

Experiments conducted in the field face logistical hurdles that are not problems in laboratory based research. As a result, the types of questions that can be addressed by field experiments is constrained somewhat. All field experiments require a clearly defined subject population that can be tracked, a treatment that can be manipulated by the researcher, and outcomes that are measurable for both the treatment and control groups. Experiments on organic social networks pose special problems. Precisely administering the treatment to particular subjects may be complicated by interactions within the network. Outcomes need to be measured in such a way that the act of answering does not cue network norms or influence the answers of others. Most problematic, however, is that researchers very rarely find readily observable networks and must explicitly measure the network to be studied for the purposes of the experiment. Identifying the links between subjects is often the most time consuming and costly portion of an experiment, so finding the most efficient means is highly desirable. Defining the network is also essential to making inference, and, therefore, is not a place to corners.

Theoretically, researchers need not map networks prior to assigning and applying treatment. Just as randomization assures balance between treatment and control subjects (i.e., treatment and control groups are comparable for both measured and unmeasured characteristics), on average, treatment and control

egos should reside in networks with identical size, connectedness, and alter characteristics. The only requirement is that the networks and outcomes be measured for the treatment and control groups in the exact same manner, to avoid differences in measurement biasing results.[11] As a design principle, however, measuring and defining the social network prior to randomization is strongly preferred for five reasons.

First, measuring the network prior to randomization provides analytic clarity. The quantity of interest is the behavior of the network. Thus defining the unit of analysis and randomization in advance adds a level of transparency to the experimental protocol. By stating upfront the network to be analyzed and its structure, the researcher removes an area of discretion that can lead to curve-fitting and Type I errors.

Second, it is always possible that measurement of the network posttreatment could be correlated with the provision of the treatment. If treatments cause certain relationships to become more salient or networks to change composition, then many strategies for defining networks (e.g., snowball surveys or Facebook links) may cease to be equivalent for treatment and control groups. At the very least, failing to measure networks prior to treatment assignment requires the researcher to make parallel measure an assumption, rather than a feature, of the experimental protocol.

Third, statistical efficiency can be gained by matching egos with similar backgrounds and network characteristics, and then randomizing within these matched strata. By creating strata (or even pairs) of subjects as similar as possible, and randomly assigning treatment within these strata, the amount of unexplained variance in the experiment is decreased. As a result of decreasing the uncertainty around the outcome variable, experiments can gain considerable precision from prematching (Cox & Reid, 2000; Rosenbaum, 2005). These gains in efficiency from prematching measured networks are likely to be especially important in experimental studies of networks for two reasons. Typical experiments have only one layer of variation by focusing on atomistic subjects (i.e., egos). Experiments looking at networks are exposed to variation in alter and network characteristics, so the reduction in unexplained variance may be even greater in these settings. Statistical power is at a premium in social network experiments because the quantity of interest is not the effect of the initial treatment, but the diffusion of the effect through the network. In most settings, the diffusion of the treatment effect will be lesser than the initial treatment itself. As such, gaining efficiency through prematching can be especially important to detect small ripples of the treatment effect.

Fourth, measuring networks prior to randomization allows for designs that preserve efficiency in the face of problems encountered in the field. Despite the best laid plans, researchers may not be able to execute the protocol exactly as planned. Applying treatments to subjects in their natural habitats may be time consuming, expensive, and lead to unexpected problems. In most cases, problems can be solved by expending more resources to address the issue (e.g., low initial rates of treatment uptake or subject attrition), but that may force the researcher to treat fewer subjects than planned. By measuring networks in advance, and carefully structuring the randomization procedure, protocols can be designed where untreated subjects are rolled into the control group or excised from the experiment (Nickerson, 2005). The reasons for the lack of treatment must be orthogonal to response to the treatment and the dependent variable, but advance work can preserve statistical efficiency in an unbiased manner.

Finally, knowing the structure of networks can allow the researcher to avoid contamination of the treatment and control groups. The whole point of the experiment is to measure the diffusion of the treatment, but if alters in control networks are exposed to the treatment inadvertently, then results will be biased towards zero. This type of stable unit treatment value assumption (SUTVA—see Rubin, 1978) violation will lead to Type II errors in most instances. By mapping in advance and conducting the experiment using networks as disjoint as possible, researchers can help avoid this type of contamination. Given the wide variety of connections that people hold, complete separation of networks is unlikely, but limiting the number of known connections can minimize inadvertent treatment.

Each of these five reasons for measuring networks prior to randomization, and taking advantage of that information, is enough to suggest a best practice. Taken as a whole, the suggestions not only nearly require mapping in advance but also provide a blueprint on how to utilize the information. Clearly defining the unit of analysis and randomization will lead to clear and convincing demonstrations of network effects. The downside of the strategy is that the analysis becomes far more static. The analysis will miss the dynamics of people joining and dropping out of the network. The researcher constrains the analysis to focus on a particular type of network (e.g., neighborhood, work place, friendship) when it may not always be clear which network will be most salient until the treatment is provided. A partial solution to this problem is for the researcher to measure network structure and composition before and after the treatment is provided (which may be inexpensive depending on how the outcome variable is collected). In this way, the researcher can take advantage of the rigor premapping

and capture some of the dynamics and hypothesis generation possibilities of posttreatment measurement.

Field experiments on organic social networks are an exciting technique for understanding social ties. However, a few words of caution about external validity are in order. First, the populations and treatments amenable to experimentation may be limited. Researchers should be careful about extrapolating the results from one population (e.g., students, neighbors) or treatment (e.g., voter mobilization) to other settings since the results are likely to be highly contingent. Second, networks may behave differently in response to a treatment than they behave in their normal state. That is, researchers should be careful to define the object of estimation to be the diffusion of the treatment and not the diffusion of norms, information, or behaviors in general. Even with these concerns about external validity, the ability to establish causation firmly should cause more political scientists to study networks experimentally.

Triangulating on Causal Process

By John F. Padgett

Historians have a joke. How do you review an article when you don't have time? Answer: Don't read any argument or data in the text. Just look in the bibliography and footnotes and see how many different archives have been consulted. The point of their joke is that all data were produced for a purpose. No subsequent reader of any data can escape the purposes, biases, cognitive categories, and omissions of the original collector of that data.[12] Capture by hidden assumptions can only be avoided by the self-conscious search for alternative sources of data with different hidden assumptions.[13] A robust interpretation is one that finds confirmation in many diverse sources, all of which are biased. The more diverse the sources, the more confidence the historian has in the interpretation. Objectivity means the triangulated search through always biased sources for the perspective of a stable focal point.

The thesis of this section is that science's search for causality is essentially no different from this. I share with many the view that science seeks to explain through mechanisms.[14] Causality, on this view, is not an estimation technique. It is a mechanism in the world that does work. Hedstrom (2005) lists seven very similar definitions of mechanism that he has found in the social science literature. The version I find most felicitous is this: "A mechanism is a process in a concrete system which is capable of bringing about or preventing some change in the system" (Bunge, 1997, p. 414). To explain something in a system is to find the process that generated it.[15]

Taking mechanism seriously as the goal of science, I argue, requires taking triangulation seriously as the goal of data analysis. Hypothesized mechanism without triangulated verification across diverse data may be worthy of interesting conversation, but it is not worthy of deep respect as a statement about how the world works. This is because causal processes are always just outside the vision of observers. This could be because of bad science: either analysts are satisfied with observed facts and correlations without looking deeper for any generative process,[16] or else they are satisfied with simple assertions of consistency between observations and speculative mechanisms.[17] Good science seeks to burrow down (or burrow up) to an ever closer observational focus on the mechanism itself. But even with good science, poor focus is inevitable, because of limited instruments and because process is a movement, not an object. This argument applies fittingly to political network analysis although it is also applicable to a wide range of social investigations.

No need for philosophical contortions, however, for there are at least three empirical methods that can give confidence that progress toward uncovering mechanism is being made.[18] These three methods are critical tests, process tracing, and detective work. All three approaches involve triangulating on causal process.

If critical test is interpreted narrowly as "one violation and the theory is dismissed," then the classic Popperian interpretation of falsification has rightly gone out of favor. No theory would last long under that brutal rule. But the essential intuition of trying to distinguish between two mechanisms that can produce the same observation remains important to anyone trying to uncover generative processes. The logic is fairly well known, but a diagram wouldn't hurt for clarity. The analyst observes that A is correlated with B, but then tries to make this into a causal claim by adducing some hypothesized mechanism M. The problem is that someone else can come along and explain the same observation by N:

$$\begin{array}{c} A \rightarrow B \\ \uparrow \\ M \end{array} \qquad\qquad \begin{array}{c} A \rightarrow B \\ \uparrow \\ N \end{array}$$

The argument between the two analysts cannot be resolved at this level. To solve the problem, they need to find a different phenomenon, which usually means a different data set, under which

$$\begin{array}{c} C \rightarrow D \\ \uparrow \\ M \end{array} \qquad\qquad \begin{array}{c} C \rightarrow E \\ \uparrow \\ N \end{array}$$

Preferably this would not be done just once, but multiple times, across diverse data.

Stinchcombe's (1968, pp. 18-22) spin on this is that the main reason that verified predictions of a single mechanism across a variety of diverse phenomena are so impressive (besides just elegance) is that implicitly one has greatly reduced the likelihood that any alternative candidate mechanism can do better, through the very diversity of the predictions.[19] This is different from mere replication of the same finding across similar designs. The volume of replications builds confidence in the correlation A—B, but it is the diversity of predictions that builds confidence in M. And without M, one is not justified in interpreting an observed correlation A—B as the causal claim that A → B. In the example of critical-test methodology, triangulation across diverse phenomena and data is not just nice, it is essential.[20]

Critical tests are an indirect approach to the search for causal process, essentially by whittling down plausible alternatives. Process tracing is the more direct approach. This takes the mechanism bull by the horns by gathering data on multiple levels of observation at once. There are a number of disciplinary variants on this: "following markers" is how the (experimental and evolutionary) biologist tends to think of process tracing (Griesemer, 2007); "reconstructing context" and narrative is how the historian tends to do it (Bloch, 1953); "process modeling of human protocols" is the psychologist's style (Einhorn, Kleinmuntz, & Kleinmuntz, 1979; Gregg & Simon, 1967); "give quotations" from subjects is a literary sensibility; and "thick description" of archetypal examples is the anthropologist's modus operandi (Geertz, 1973). The point here is certainly not to argue for the superiority of these methods to the statistical work traditionally done by social scientists. The point is to argue for mixing multiple methods in the same research, in particular to mix statistical data on observed correlations with direct observation (as close as one can, with whatever tools are available) of hypothesized process. Each method gives a different slant or perspective into the phenomenon of interest. The more eyes we have to see with, the more confident we are that what we see is in the world, not in our mind. In particular, the closer we get to observing through different lenses the process we hypothesize, the more confident we become that this is indeed what is generating our data.

If one prefers Lakatos's (1978) vision of science as fertile research programs to Hacking's (1983) vision of scientific realism, then theory building and elaboration cannot help but be challenged and thus stimulated by multiple observational perspectives on any phenomenon. Conversely, intradisciplinary debate over the "most perfect method" signals a research program's defensiveness

and stasis, rather than its exploration and expansiveness. To me, combining multiple observational methods just seems like common sense, but it is striking how much disciplinary boundaries and prejudices get in the way by insisting upon standardized research. Too often we act as if "truth" resides not in the phenomenon we are trying to understand, but in the method we are trying to employ.

Let me close by discussing detective work. Juggling multiple data sets and methods stretches the mental and perceptual reach of scientists. This is good. But it also places demands on the armature that holds it all together. I think the future in systematic empirical research lies not in flat files but in relational databases.

In the traditional flat-file approach, the social scientist is presumed to know ahead of time what is important to measure. Data are selected from a precisely defined and homogeneous sampling frame and are complete, with all variables filled in. The omniscience and foresight assumed about the researcher in this approach are, in my opinion, unrealistic. More important, the capacity of researchers to learn anything fundamentally new is severely constricted by the categories comprising the data collection, which they cannot escape.

Historians, in contrast, deal in relational databases, whether they formalize them as such or as old-fashioned index cards. Many heterogeneous data files are recorded—one or more file for each source. These heterogeneous files are interlinked through case IDs—usually peoples' names, but other units of cross-reference are possible as well. Missing data are the rule not the exception. The researcher is assumed *not* to know ahead of time what is going on in the data; hence the preferred self-conception of the researcher becomes that of the careful detective, instead of that of the omniscient theorist.

Relational databases allow the researcher flexibly to juggle and to maneuver through linked multiple data sets, even data sets at different levels of analysis. Hence they are an invaluable tool for triangulation. The greater ability to learn from relational databases comes from their capacity to pick up the object of study and to examine it from multiple perspectives. Of course there are the biased perspectives of the various original sources themselves. But there are also the hybrid data structures that relational databases enable to be assembled out of these sources, in customized fashion. The query-and-response format of relational databases encourages self-consciousness about the structure of one's detective strategy. This does not remove the need for statistical verification, on the basis of customized flat files assembled from relational databases. But new questions can be flexibly asked with new slices of data, once old

questions are resolved. None of this guarantees that triangulation in detective search will converge, but at least query-and-response-then-test encourages the mentality of trying to learn from the data, rather than trying to measure the data against one's fixed and unbending ideas.

These triangulation recommendations apply to all sorts of quantitative data, not just to social network data. But it is not hard to draw out the implications of these prescriptions for research on social networks. The main point is to think about and to measure generative process. Network data are, by definition, interactional in character, so "search for mechanisms" means search for interactional mechanisms. Within social network analysis, if one is in the interactionist tradition, this tends to mean search for micro-interactionist mechanisms. If one is in the structuralist tradition, this tends to mean search for macro-structural mechanisms (Wellman & Berkowitz, 1988). Either way, the critical test approach pushes for specifying more than one mechanism and then searching for diverse data sets that can discriminate among them.[21] If behavior is the dependent variable and network is the independent variable, then search for various network settings that exhibit similar behavior is nice, but search for various behaviors across similar network settings is even better. If network is the dependent variable and macrostructural context (including other networks) is the independent variable, then search for various contexts that generate similar networks is nice, but search for various networks produced by the same macrostructural context produces deeper understanding of mechanism.

The process-tracing prescription for network analysis is simple: don't only collect quantitative data on independent and dependent variables, get quotes, talk to your subjects, build historical context, follow markers or traces over time—anything to get a different observational angle on the process that generated your data.

In my own research, I operationalize these ideas mostly in the context of historical network data (economic ties, political ties, and kinship ties) over time. Multiple data sets and observational methods in that context means multiple archival sources, coded and computerized in large relational databases, which integrate quantitative and qualitative information. Up to this point I have not mentioned my own research, but readers interested in a sample of research that tries to follow the above precepts may consult Padgett (1980, 1981,1990, 2010), Padgett and Ansell (1993), Padgett, Lee, and Collier (2003), Padgett and McLean (2006, 2011), and Padgett and Powell (in press).

Theoretically look for mechanism, empirically look for diversity. The historians' joke is a serious one, which applies as much to the natural and social sciences as it does to the discipline of history.

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Notes

1. For a review of this literature and the Columbia School findings, see Eulau (1980).
2. The GSS includes variables which account for socioeconomic status but neglects the geographic location of the discussant and some key political variables.
3. Surveys often ask for discussants of "important matters." There are some differences between these two name-generators but they do not produce meaningfully different data on social networks (Klofstad, McClurg, & Rolfe, 2009).
4. For an example of such an effect, see Nickerson (2008).
5. For a potential way to address these criticisms methodologically in the context of rich network data, see Christakis and Fowler (2008).
6. This is the "reflection" problem, where trying to determine the causal mechanism of an individual with a group is similar to the problem of interpreting the almost simultaneous movements of a person and his reflection in a mirror. Does the mirror image cause the person's movements or reflect them? An observer who does not understand something of optics and human behavior would not be able to tell. (Manski, 1995, p. 129)
7. Hofstadter (1985, p. 485) points out that it often is possible devise unobtrusive methods of social observation so as to prevent respondents from knowing that they are observed. While that is sometimes the case, at other times it may be infeasible to be unobtrusive. For example, it is possible to follow a person's social interactions continuously using a sensor to obtain a direct measure of his or her social network. However, such interventions are expensive and raise obvious ethical concerns. Surveying the respondent about his or her networks may instead be the most feasible approach.

8. Of course, it is possible that both respondents mutually refuse to admit their tie, or, similarly claim to know each other when they do not. In these cases, concordance may exist even though measurement error is present.
9. Network centralization, heterogeneity, density, transitivity, and average degree are all commonly used concepts in network analysis. Network centralization measures the extent to which a single node controls the links in a network (Wasserman & Faust, 1994, pp. 175-177). Heterogeneity is the variance in the standardized actor closeness indices (Wasserman & Faust, 1994, p. 187). Density is the proportion of actual lines in a graph to the number possible lines (Wasserman & Faust, 1994, p. 101). Transitivity is a proportion of tripples of nodes in a graph with transitive relations (i.e., A→B and B→C implies A→C) (Wasserman & Faust, 1994, p. 165). Average degree is the average number of ties to each node in a network (Wasserman & Faust, 1994, p. 100).
10. Detailed information on the research design, survey questions, and variable operationalization are not given here, but are available in Heaney (2006).
11. While not strictly experimental, Milgram's (1967) six-degrees-of-separation experiment strategy of defining the network as the treatment diffuses obviously violates this principle.
12. See March and Simon (1958, p. 165) on "uncertainly absorption." This is the subliminal absorption into recipients of the cognitive categories and perceptual biases of boundary spanners who collect and code information.
13. Perhaps the most insidious capture of them all is when one has designed one's own data. It is possible to disconfirm priors this way (although the flexibility of statistical techniques should not be underestimated), but it is impossible in this way to learn something genuinely new. This problem with self-reflexivity is similar to the argument of Wittgenstein against "private language" (Kripke, 1982). Namely, without an external perspective on themselves, communicators cannot distinguish movement in the world from movement in their categories of the world.
14. Proponents of the mechanism view of causality include Stinchcombe (1968), Gregg and Simon (1967), Elster (1983), Epstein and Axtell (1996), Bunge (1997), Hedstrom (2005). The mechanism approach is closely aligned to Hacking's "scientific realism." Much (not all) of Popper can be reinterpreted as consistent with scientific realism (Hacking, 1983, p. 146).
15. Some proponents of the mechanism approach, like Elster and Hedstrom, often seem to imply that the primary mechanisms of interest to them are choice mechanisms, either rational or behavioral. But actually the definition is much broader than this. See Padgett and Powell (in press) for two lists of social network mechanisms, one for organizational genesis and one for organizational catalysis, which do not involve choice.

16. I probably do not need to emphasize the point that most empirical social science is like this. "Report a correlation or regression, and go home." The optimism that science can be built up solely through the inductive assembly of epiphenomenal observables is associated with the positivism of Rudolf Carnap (but not that of Karl Popper). Bunge (1997, p. 423) observes caustically in response: "Imagine what would have happened if Newton had abstained from positing unobservables, such as mass and gravitation. . . It has been noted that, luckily for science, the very concept of statistical regression was unknown in Newton's time." I do not go as far as Bunge, but I see his point. The problem is not regression per se, it is the confusion of regression with theory.
17. The well known, but often ignored, problem with this is that any observed phenomenon can be generated by many hypothesized mechanisms. Given this, and the fact that no alternatives were considered, the claim that an observed phenomenon was produced by the favorite mechanism of the analyst is only hubris.
18. Not many still hold onto the fantasy of ultimate "truth" ever being attainable. But scientific realism (Hacking, 1983) maintains that better and worse approximations between theory and instrumented "reality" are measurable. One can know whether or not one is going uphill without knowing where the highest mountain is. If one doesn't believe at least this, then no reason to be a scientist.
19. This does not mean that this likelihood is ever reduced to zero. Remember Newton and Einstein.
20. See Padgett (1980, 1981) for one application of critical-test methodology in political science.
21. Rather than one always winning and one always losing, there may well be a mixed report card. That result is fine, productive of deeper theorizing that pushes beyond original presuppositions.

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