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Causality in Political Networks

James H. Fowler, Michael T. Heaney, David W. Nickerson, John F. Padgett, and Betsy Sinclair †

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Abstract

As the study of political networks becomes more common in political science, greater attention to questions of causality is warranted. This essay explores competing visions of causality in political networks. Independent essays address issues of statistical model specification, identification of multi-step personal influence, measurement error, causality in historical perspective, and the insights of field experiments. These essays do not agree entirely on the nature of causality in political networks, though they commonly take seriously concerns regarding homophily, time-consistency, and the uniqueness of political network data. Serious consideration of these methodological issues promises to enhance the value-added of network analysis in the study of politics.

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1 Introduction
By Michael T. Heaney

The study of social networks has grown exponentially across the social and natural sciences over the past decade. A *Web of Science* search on the term "social networks" shows that the annual number of articles published on this topic rose from about 200 in 1998 to over 800 in 2008. In political science, articles on networks once appeared only occasionally, but now articles using network theory, data, and methodology appear regularly in the leading journals in the discipline. Network studies span the subfields of political science, exploring the outcomes of individuals, institutions and policies through both direct and indirect relationships.

Although the metaphor of "network" has long resonated in political science as a way of thinking about systems of power, influence, and communication, it has only recently gained currency as a formal model of political behavior. We see several factors are behind this growth in political network analysis. First, recent studies offer compelling explanations of observed phenomena, including research on social capital (Putnam 2000), sexual contacts (Laumann et al. 2004), small worlds (Watts 1999), power laws (Barabasi 2003), and business success (Burt 2005). A second factor is the availability of inexpensive and easy-to-learn computer tools for social network analysis, such as UCINET (Borgatti, Everett, and Freeman 2009), as well as increased ease of access to large network data sets using electronic tools. Third, the emergence of on-line social networks (e.g., Facebook, LinkedIn) has drawn attention to networks by mass media and citizens across the world. A final factor is the contagion that comes from an innovation spreading through formal and informal networks – appropriately enough. More and more people – inside and outside of political science – are coming to see the applicability of social networks in their day-to-day personal, political, and professional lives.

With the emergence of any new research paradigm, a reasonable amount of caution is warranted. If social network analysis is to endure within political science – rather than being a brief flash in the pan – it must approach theoretical and methodological development carefully. Specifically, we believe that not enough attention has been given to the nature of causality in political network analysis. Political scientists who study networks have drawn heavily upon advances in other disciplines – especially sociology and physics – yet scholars in these areas tend to have very different ideas of what constitutes an adequate causal argument than do many scholars in political science. As methods travel from discipline to discipline, do causal ideas travel as well? Is there anything special about the substance of politics that merits a different kind of approach than, say, the questions in epidemiology or population biology? Many network ideas and techniques travel, but perhaps some require modest translation.

This article is a series of independent essays that address questions of causality in political networks. The authors are not in full agreement over the requirements for assessing causality in political networks, only over the primacy of the question. The authors similarly confront the dilemmas arising from the fact that social networks are embedded in recursive causal processes. For example, a person inherits her/his political views, in part, from her/his network – parents, teachers, friends – but then subsequently chooses new members of the network based, in part, on her/his political views. Confounding the situation is the principle of homophily, which posits that individuals prefer to connect with others who are like themselves. As a result, standard statistical models (e.g., OLS regression) do a poor job of representing network effects. But, if the standard models are insufficient for dealing with networks, then which models are? How should causality be examined in the presence of political networks? The five authors of this article offer different answers to this question.

The first three essays in the article – by Betsy Sinclair, James H. Folwer, and Michael

T. Heaney, respectively – focus on statistical analysis in answering this question. Sinclair explores the assumptions used in drawing inferences about the effects of political networks in national random samples. She considers, in particular, the relevance of small sample sizes, identification, recall and truthfulness, and contextual effects. Fowler, in contrast, focuses more narrowly on pinpointing indirect personal influence in large social networks, accounting for random clustering, homophily, contextual effects, and influence in this examination. While Sinclair and Fowler search for statistical models to improve our understanding of causality, Heaney targets the measurement process to resolve these puzzles. Sinclair claims that it is necessary to assume that data are reported truthfully in order to undertake analysis, while Heaney considers what happens when we know that those data are not reported truthfully, or are at least reported with some error. He summarizes the advantages of obtrusive measurement of political networks, but then proposes methods to reduce measurement errors generated in the process, including the use of comparative modeling, multiple measures, explicit error models, and reframing interview/survey instruments.

Although the essays by Sinclair, Fowler, and Heaney place faith in statistical models to address questions of network causality, the final two essays in the article – by John F. Padgett and David W. Nickerson – mutually question the adequacy of statistical analysis of observational data (either in a cross-sectional or panel framework) to address satisfactorily the questions at hand. Nonetheless, Padgett and Nickerson embrace radically different solutions to the problem. For Padgett, causality is understood properly over long historical periods using relational databases that may be incomplete in significant ways. This approach requires the analyst to gather multiple sources of evidence and triangulate on the causal processes at hand. For Nickerson, causality is understood properly by using experiments in organic social networks. Thus, the political scientist is able to pinpoint changes in micro-political behavior and short-term adjustments in social networks. Nickerson argues that best practices using this method demand that networks be measured in advance of any experiments and, potentially, following the experiment as well, if feasible. This approach – because of its concern for the pervasive confounds produced by dynamic political networks – is anathema to the simulation and analytical identification methods proposed above by Fowler and Sinclair.

This article makes no pretense to reconcile the varying views of its contributors on the question of causality in political networks. The historian and the experimentalist are engaged in very different projects that seek answers on fundamentally different scales. The statistician is stuck in the middle, seeking a widely generalizable result over a moderate timeframe. Nonetheless, the views presented here collectively map out the major issues and approaches confronting causal inference in political networks. Both current and future scholars should give careful attention to these questions when using network ideas to study politics.

2 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 (Lazarsfeld, Berelson and Gaudet 1948; Berelson, Lazarsfeld and McPhee 1954).[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, and disagreement (Huckfeldt and Sprague 1988, 1991, 1994; Huckfeldt et al. 1995; Huckfeldt 1995; Huckfeldt et al. 1998; Mondak, Mutz, and Huckfeldt 1996). However, the applicability

[1]For a review of this literature and the Columbia School findings, see Eulau 1980.

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). These surveys are now being 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. Analyses from these surveys may still be problematic, for example, while the 1985 GSS, 1987 GSS, and 2000 ANES all included social network batteries asking respondents to provide demographic and political information about their discussion partners, these surveys do not elicit enough data to draw causal inferences about the effects of peer networks without additional assumptions about selection and homophily. 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 upon those preferences.[2]

Analyses of these data need to address four key issues. First, as most of the surveys

[2]The GSS includes variables which account for socioeconomic status, but neglects the geographic location of the discussant and some key political variables.

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 upon 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. Causal inferences can be drawn only after accounting for all four of these issues and addressing the possibility of unobserved variation driving the results.

2.1 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. In order to rely upon these surveys, it is necessary to assume that these discussants are representative of the respondent's larger social sphere. Literature which attempts to estimate network size suggests that the average network contains somewhere between 290-750 people (McCarty et al. 2001; Killworth et al. 1990; Zheng, Salganik and Gelman 2009). If we anticipate that individuals are most likely to be persuaded by weak ties, then it is possible that these weak ties are not included in the small set of individuals identified by the respondent (Mutz 2006). However, we observe the consistent presence of some disagreement across these survey responses; while this may be an underestimate of the actual amount of political disagreement an individual is exposed to from their broader network it does include some heterogeneity of preferences. Analysis of network surveys requires the assumption that these respondents are representative of – or at least the primary influences within – an individual's political network.

2.2 Identification

Homophily is a well-documented phenomena within social relationships where there is a tendency for individuals to form social ties with others who are similar (Lazarsfeld and Merton 1954; Coleman 1958; McPherson, Smith-Lovin and 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, Smith-Lovin and Cook 2001). Little research has investigated the extent to which relationships will form based upon 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 upon shared political values (Lazar et al. 2008). If social relationships are formed based upon shared political preferences or behaviors, then the correlation that is present between the respondent and her network is based upon 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 upon availability and not solely on personal choice (Mollenhorst, Volker and Flap 2008, 2007). 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 are some fraction of discussants who are chosen based upon availability – relationships which were selected not based upon shared political preferences. Accounting for the degree of shared characteristics is key in the analysis of peer effects; it is within those relationships where there is not homophily that is possible to identify peer effects.

2.3 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% mis-identified, approximately 3/4 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 possible that the belief of the respondent regarding the discussants' preferences within the mis-identified relationships is the key variable – not the true discussant preferences.

2.4 Contextual Effects

One great 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 fac-

[3]Surveys often ask for discussants of "important matters". There are minimal differences between these two name-generators (Klofstad, McClurg and Rolfe 2009).

[4]For an example of such an effect, see Nickerson 2008.

tors could drive the relationship between the respondent and her discussants (Cohen-Cole and 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.

2.5 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 particular care, significant data resources, and 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 to assume that the network data are reported truthfully. A panel survey would provide an ideal research design.

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] One potential resolution to this problem is through analysis of panel data. This approach allows for observation of change in ideology, candidate preference, and party identification for both the respondent and the discussants.

Within the set of surveys on networks, there is seldom sufficient structure to the data

[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, pg 129).

or resources available to draw causal inferences without strong assumptions. Under 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 have 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 geographic proxies may be insufficient in these cases where communication about politics can take place via phone, Internet, and other technologies – and surveys, with a set of assumptions, provide one way to understand the impact of political networks.

3 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 (Lazarsfeld, Berelson and Gaudet 1944; Huckfeldt and Sprague 1995). 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 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. 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. 1) *Random clustering* may result when many people with the same characteristics happen to be connected to one another by chance. 2) *Homophily* (which literally means "love of like") occurs when individuals choose to become connected to those who have similar characteristics (McPherson, Smith-Lovin and Cook 2001). 3) *Contextual effects* may result when connected individuals jointly experience contemporaneous exposures, such as seeing the same political advertisement. 4) *Influence* occurs when political attitudes or behaviors in one person cause a connected person to adopt the same attitudes or behaviors.

3.1 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 (for example, 95% confidence intervals can be obtained by looking at the 25th and 975th ranked values).

In studies of obesity (Christakis and Fowler 2007), smoking (Christakis and Fowler 2008), happiness (Fowler and Christakis 2008), and loneliness (Cacioppo, Fowler and 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.

3.2 Homophily

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 and Wasserman 2005; Fowler and Christakis 2008a).

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 current and previous exam. 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 and Hibbing 2005; Fowler, Baker and Dawes 2008; Fowler and Dawes 2008; Fowler and Schreiber 2008; Settle, Dawes and Fowler 2009; Dawes and Fowler 2009) and any stable tendency to exhibit the characteristic. The alter's characteristic in the previous period helps control for homophily (Carrington 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. Analysts must use generalized estimating equation (GEE) procedures to account for multiple observations of the same ego across periods and across ego-alter pairings (Liang and 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.

To assess the validity of this method, Monte Carlo simulations of the model can be used to test whether homophily tends to bias the estimate of the influence effect. 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 their feeling thermometer score 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 towards the formation of social ties.

Each individual receives an exogenous shock to his/her score that is drawn from a normal distribution, and the ego's new feeling is equal to a weighted combination of his/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 the 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.

Figure 1 Goes Here

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.

3.3 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 towards 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 towards 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 (Bramoullé, Djebbari and 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 and Fowler 2007, Fowler and Christakis 2008a), smoking (Christakis and Fowler 2008), happiness (Fowler and Christakis 2008), and loneliness (Cacioppo, Fowler and Christakis 2008), 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 and Fowler 2007) and smoking (Christakis and 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 and Christakis 2008b), and loneliness (Cacioppo, Fowler and Christakis 2008), 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.

4 Measurement Error and Causality in Political Network Analysis
By Michael T. Heaney

The Heisenberg uncertainty principle in physics can be loosely stated as the idea that to observe a phenomenon is to change it (Heisenberg 1949). While the subject matter is somewhat different when studying politics, this principle is relevant to observational methods in political science. 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 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.

As political scientists rush headlong into the study of networks, it is worth reflecting upon how the measurement of networks potentially changes 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 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 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 and situates them within this type of political research. Finally, a series of methodological solutions are proposed for reducing the causal effects of network measurement on observed network structures.

4.1 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 and Montgomery 2006), legislative co-sponsorship (Fowler 2006), caucuses in the U. S. Congress (Victor and Ringe 2009), multiplex interest group and political party networks (Grossman and Dominguez 2009), and the rise of institutional innovations during the Renaissance in Florence (Padgett and McClean 2006). In these cases, the observed actors do not have the opportunity to react to the research and, thus, cannot cause the 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 are 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 question political actors about their networks. Respondents in these studies are asked to report on their political discussion partners (Huckfeldt and Sprague 1987), communication and influence among interest group lobbyists (Carpenter, Esterling, and Lazer 2004; Heaney 2006; Heinz, Laumann, Nelson, and Salisbury 1993; Laumann and Knoke 1987), organizational memberships of antiwar protesters (Heaney and 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 and Rojas 2007, 2008; Huckfeldt and 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, Laumann, Nelson, and Salisbury 1993; Laumann and Knoke 2007).

Obtrusive methods to gathering political network data have special advantages. First, they solicit individuals' first-hand 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 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. 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.

4.2 Pitfalls in Network Surveys and Interviews

When respondents are asked to report on their ties, these reports are vulnerable to errors and misrepresentation. Respondents may either fail to mention ties or may make false reports of ties, yielding an accuracy rate of 40% to 60% in the measurement 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, non-response, 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 and Webster 1999; Feld and Carter 2002). This problem can be illustrated by examining concordance in multiple reported network ties (Adams and 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.[7] 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, collected by Heaney (2006), shows that this distinction makes a difference. If corroboration is required, the network centralization is reduced 27.44%, heterogeneity is increased 25.31%, density falls 63.49%, transitivity is reduced 28.04%, and average degree drops by 63.49%. These results illustrate the substantial changes in the structure of a network produced by differences in measurement.

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. Attractiveness bias is a particular concern because certain network members may be seen as desirable network contacts, while others are *persona non grata*, thus drawing or discouraging ties that either did or not exist in fact, depending on the desirability of the alter. 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. In either case, surveys of political actors may be especially problematic,

[7]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.

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.

A reanalysis of Heaney's (2006) data using an exponential random graph model (ERGM) revealed that expansiveness and attractiveness biases patterns are present in the data.[8] Consistent with attractiveness bias, the results show that interest groups with a stronger reputation for influence are cited as having more communication ties than would be expected, other things equal, as interest groups with weaker reputations are cited less frequently than would be expected, other things equal. Consistent with expansiveness bias, informants that have lower ranks within their organizations cite fewer communication ties than would be expected, while informants with higher ranks report more communication ties than would be expected. These patterns suggest that respondents report their ties in a way that responds to their political view of and position within the network, thus altering the causal process affecting the reported network structure.

4.3 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 in order 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 is suggestive that causality is not strongly influenced by the measurement process. If the results using corroborated and uncorroborated ties differ, then the researcher should

[8] The ERGM model is estimated using Hunter, Hancock, Butts, Goodreau, and Morris 2008's method in R. The results are not reported here to conserve space, but are available from the author upon request.

investigate the discrepancy further.

A second strategy is to create multiple measures of the same network. Multiple interviews with the same respondent may increase reliability (Adams and 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 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. Laboratory 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 these networks, this essay suggests a series of strategies for reducing these effects. The Heisenberg principle applies – to observe political networks is to change them – yet it may be possible for political scientists to gain greater understanding of how and to what extent these networks are changed.

5 Triangulation 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 are biased and lie. Even to see the bias – and hence to avoid being captured by it – one needs another biased source against which to triangulate. Data may or may not out-and-out contradict the reality to which they refer. But all data, quantitative or qualitative, were produced for a purpose. This purpose of the recorder imposes severe constraints on any subsequent researcher's vision. They blind him or her to concepts as well as facts that were not measured, as much as they enable him or her to see a narrow slice of the recorder's world.[9] To rely on only one archive or source is to be captured by the purpose of the person who originally collected the data (perhaps most insidiously, oneself), with no chance to step outside it to see what is real.[10]

The point of this note is not to try to convince us to become historians. Still less is it to launch into some post-modern canard about the impossibility of (at least relative) truth. I have little sympathy with that. Instead I want to make the case that we should

[9] March and Simon (1957) called this "uncertainty absorption". Namely, the subliminal absorption of theories or cognitive categories about the world into the format of the data transmitted.

[10] This position is the essence of Wittgenstein's argument against "private language". Without an external perspective on themselves, communicators cannot distinguish movement in the world from movement in their categories of the world.

listen carefully to what our historian colleagues are telling us in order to become better scientists. To listen to what they say requires thinking about triangulating across multiple sources. And it requires a slight adjustment in our core concept of causality.

In my opinion, "causality" is not an estimation technique. Causality is a process or mechanism that does work in the world – i.e., that produces something. Two corollaries are implicit in this definition: "Produces something" necessarily implies change. Change that is mere static reproduction usually is less revealing of underlying causality than is observable movement. And "world" is the thick and variegated contexts in which processes operate. These contexts are richer than any single source or archive or measuring instrument can reveal. Processes have consequences that ramify in multiple contexts, not only in the context on which our attention is focused.

This article is about causality in political networks. In the context of networks, my definition of causality implies collecting network data on (a) multiple networks (b) from multiple sources (c) over time. In addition to offering material for triangulation, multiple networks provide researchable contexts for making more precise the search for each network's causal dynamics.

Let us bring these points down to tangible research practice. To lay out two usefully oversimplified stereotypes: Social scientists analyze flat-file data structures, in statistical fashion, and historians analyze relational databases, in detective fashion. Both styles have their strengths, but both styles also have their weaknesses. Because I am writing for an audience of social scientists, I do not feel the need to linger on the strengths of that approach. My emphasis in this note is rather one-sidedly on the comparative advantages of the historians' approach to causality.

The flat-file approach is designed for hypothesis testing. The researcher is assumed to know ahead of time what is going on in the data. Data, selected from a precisely defined and homogeneous sampling frame, are complete, with all variables filled in. Data collection is assumed to be a perfect camera, which takes an accurate picture.[11] Given such heroic assumptions, probability foundations kick in to measure numerically the degree of deviation of data from hypothesis. The emphasis always is on finding a close match between one well-defined theory and one fixed-data slice. But the omniscience and foresight assumed about the researcher in this approach are, in my opinion, unrealistic and even narcissistic.

Historians, by contrast, deal in relational databases, whether they formalize them as such or as 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.

Despite its holes and traps, the comparative advantages of the relational database structure are two: greater ability to learn from the data, and more robust explanations.

The greater ability to learn from relational databases comes from their capacity to pick up the object of study and 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, even though it does not enforce that. More importantly, 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 encourages the

[11]Or if it does not take a perfect picture, at least one assumes that the researcher knows how to correct its imperfections.

mentality of trying to learn from the data, rather than trying to measure the data against oneself. My experience is that this is, in fact, what most social scientists do; they are just not permitted to admit it.

If convergence does emerge through triangulation, the explanation or interpretation so derived will be more robust to the extent that it has been evaluated against multiple heterogeneous data sources, rather than against just one. Instead of being based on the matching of one data source perfectly, confidence in explanation becomes based on the variety and heterogeneity of the data structures confronted.

I see no reason to resist, from the scientific perspective, the historian's emphasis on heterogeneity of data and sources. Good science also strives for robust explanations, so defined. It is just that our current statistical tools are not up to the task. These presume flat files – namely, case rows with a good sampling structure, and variable columns that are completely filled in. But as the data management world evolves beyond flat files to relational databases, so must our methodological thinking about statistics and causality.

I put forward an illustration of one effort to do causally oriented statistics, while thinking historically. It may be useful to others to sketch how one designs an historical social- and political-network data set.

Step 1: Collect data on multiple networks from multiple sources. For example, using primary-source archives, Padgett and McLean (2006) coded about 100 data files (one separate data file coded for each archival source) about social, economic and political networks over time in Renaissance Florence, and cross-linked them through case IDs into a relational database. Covering the time period from 1280 to 1500, these files include tax records, kinship and marriage networks, economic partnership and credit networks, guild records, political factional networks, and republican political elections and office-holdings.

Step 2. Identify some phenomenon that changed. This could include state centralization (Padgett and Ansell 1993), business organization in international finance (Padgett and McLean 2006), economic credit (Padgett and McLean, forthcoming) , and family structure and marriage (Padgett, forthcoming) . If one's research goal is the search for causal mechanism, the main thing is to choose to study not a static network, but one that changed. The first descriptive task is then to identify precisely the exact date or dates of change. This requires considerable archival knowledge of the sampling bias in diverse sources in order to construct comparable data structures over time. But once achieved, it is amazing how many complicated theoretical debates become clarified by the simple act of dating change.

Step 3. Contextualize the phenomenon of interest. If one is studying change in economic networks, put that in the context of the political and kinship networks of the time. If one is studying change in kinship, put that in the context of economic and political networks. If one is studying political networks, put that in the context of kinship and economic networks. One does this through using relational database technology to assemble from diverse sources customized data sets that answer specific questions.

Step 4. Search for the proximate causal mechanism that produced the change.[12] Pre-existing theories are helpful as intuition pumps early in the search process. But it may surprise some to learn how quickly search devolves into an inductive detective exercise, once abstract theory confronts complex particulars. To avoid getting stuck in dead ends, intuition-pump theories should be plural, not singular. Multiple-network data, much less reality, are too complicated for any single theory to exhaust.

Step 5. Process mechanism in hand, construct parallel statistical estimation exercises and repeat them for multiple time periods, including both before and after the change. For science, this is the ultimate objective – not just tracing change in some dependent

[12] As an example, Padgett and McLean (2006) found the proximate mechanism that generated a business revolution in international finance to be political co-optation of local bankers into republican political offices, following a class revolt. This mechanism is called "transposition and refunctionality."

phenomenon of interest, but also identifying change in underlying causal relationships within the system that produced that change.[13] Timeless covering laws are ahistorical mirages. But that does not negate the systematic search for period-bound causal principles. These parallel and iterated regressions become the "confirmatory proof" of the causal mechanism that emerged through inductive detective work.

In sum, the historically-oriented, political-network methodology that I recommend is this: Micro-detective search for causal mechanisms through heterogeneous sources, quantitative and qualitative; then confirmation through statistical evidence, arrayed within carefully controlled and repeated designs. Ultimately, confidence in one's findings derives not from any single test, but from the diversity and range of sources and tests to which one's interpretation is subjected. The historians' joke is a serious one.

6 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.

[13]To continue the example of the previous footnote, change in business organization was then connected to demonstrated change in dowries and in clientage to reveal how transformation in multiple networks were linked as different facets of macro transformation in elite structure. This process permeated multiple domains.

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 regards 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 2003) or soldiers (e.g., Goette, Huffman, and 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 and 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 pre-condition 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.

Theoretically, researchers need not map networks prior to assigning and applying treatment. Just as randomization assures balance between treatment and control subjects, 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, in order to avoid differences in measurement biasing results.[14] 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 post-treatment 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 cre-

[14]While not strictly experimental, Milgram's six degrees of separation experiment (Milgram 1967) strategy of defining the network as the treatment diffuses obviously violates this principle.

ating 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 pre-matching (Cox and Reid 2000; Rosenbaum 2005). These gains in efficiency from pre-matching 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 pre-matching 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 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 pre-mapping and capture some of the dynamics and hypothesis generation possibilities of post-treatment 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.

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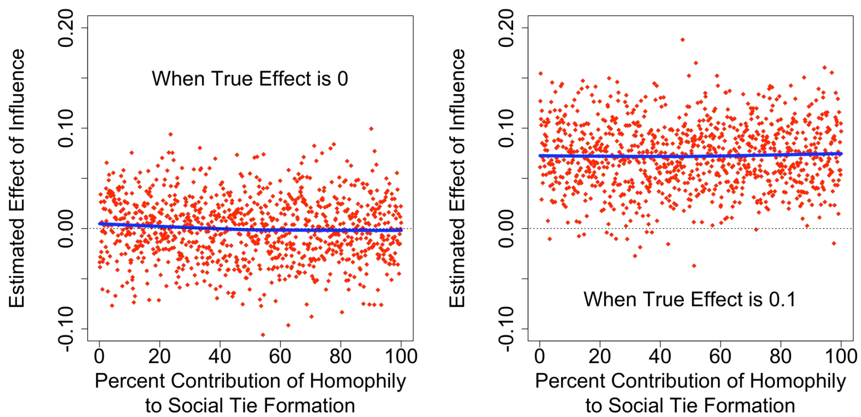
8 Tables and Figures

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 blue 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. 1000 simulations of a 1000 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.