Measuring Interpersonal Influence

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Friends, family members, co-workers and neighbors influence one another's actions and attitudes to some extent. Neighbors tend to vote in blocs[1] and children often follow their parents' lead in political behaviors and beliefs.[2] These types of phenomenon lead some to believe that interpersonal influence is obvious and ubiquitous. On the other hand, the very ubiquity of the phenomenon helps to make accurate detection of interpersonal influence a complicated endeavor. Neighbors and coworkers tend to have similarly structured material interests. How can one know that they haven't reached the same decision independently? Similarly, people choose spouses, friends, workplaces, and neighborhoods. Is it possible that citizens are selecting into homogenous networks? There are a host of explanations for the homogeneity described above that do not involve interpersonal influence. How can one distinguish between the myriad of possible influences on a person's psyche and know which ones are truly important?

The goal of this chapter is to lay out a method of inquiry for accurately gauging various forms of interpersonal influence. The inquiry begins by examining the standard observational techniques that constitute the bulk of the literature on interpersonal influence. Omitted variable bias from unobserved factors may be a problem for regression techniques, accordingly the discussion will turn to selection models. Estimating the necessary components for selection

[1] V.O. Key offers one of the earliest and most famous examples of context effects in voting.
[2] Newcomb and Svehla established this using modern survey techniques in 1937 (Newcomb and Svehla 1937), but the observation dates back at least as far as Plato. Jennings and Niemi (1974) provide a thorough analysis of this topic.

models may not be possible for many interpersonal studies, so cross-sectional time-series data will be considered next. While panel data is able to accommodate static confounding unobserved factors, dynamic factors continue to plague the analysis. Randomized experiments sidestep these problems because the unobserved factors affect the treatment and control groups equally. Three types of experimental protocols will be outlined and examples provided: 1) manipulate the network itself; 2) control communication within an existing network; and 3) provide an exogenous shock to a network. The conclusion reached is that randomized experiments can parsimoniously isolate the magnitude of an individual form of interpersonal influence while observational statistical analysis requires increasingly untestable assumptions and complicated techniques. The philosophy driving the analysis is that a researcher should let the data speak for itself and add as few untestable assumptions as possible.

Background and Setup

The research methodology used to study interpersonal influence has not changed much in the past fifty years. The pioneering work conducted in *The People's Choice* (Lazarsfeld, Berelson, and Gaudet 1948), surveying communities in depth, has not been surpassed in design or execution. Contemporary work emphasizes either the mapping of existing community networks (e.g., Huckfeldt and Sprague 1995) or surveying individuals about their political conversations with friends (e.g., Mutz 1998). Both flavors of inquiry utilize similar methods, relying upon subject answers to survey questions[3], and guiding logic, which is to demonstrate that people take on the values and behaviors of their friends and/or communities at a rate greater than one would expect given sociological predictors. This logic will be laid out in detail over the

[3] Self-reporting unavoidably leads to measurement error as will later be discussed.

next few paragraphs. For the sake of exposition, the discussion will be limited to interactions between two individuals, but the logic is just as applicable to contexts such as neighborhoods.

Measuring interpersonal influence entails determining the extent to which a person, P_1, alters the behaviors or beliefs of another person, P_2. Presumably a person would not advocate a position that she did not hold herself, so researchers typically are concerned with the relationship between the attitudes or actions of person 1, A_1, and the attitudes and actions of person 2, A_2.[4]

It should be noted that possessing data on the attitudes or actions of both parties is an ideal condition that is rarely realized in practice. Studies often settle for questionable proxies. For instance, in *Impersonal Influence* (Mutz 1998, chapter 3) the measure used of interpersonal influence is the survey question "How often do other people talk to you about their unemployment problems, that is, having difficulty finding or keeping a job? Would you say they talk to you about job security or unemployment problems everyday, three or four times a week, once or twice a week, or less often than that?" The survey question contains information about interpersonal influence, but it also measures the respondent's gregariousness, social class and memory. At the very least, the question measures interpersonal influence with error. However, even in the best-case scenario where the researcher has access to an accurate measure the beliefs of both people (or person and a neighborhood), traditional modes of analysis misspecify the relationship between individuals because they fail to account for existing similarities.

The most common method of gauging this relationship is regression analysis using the attitudes or actions of person 1 to explain the attitudes or actions of person 2. Such a relationship can be expressed mathematically as

[4] Naturally a person could influence another person's development in an infinite number of unintended ways. For instance, a very bad date with a Republican may convince someone never to vote for a Republican candidate again. These types of processes may occur, but unfortunately such analysis is beyond the scope of social science at this time.

$$A_2 = c + \alpha A_1 + \varepsilon \tag{1}$$

where c is a constant, α is the coefficient of interest, and ε is the error term. In order for equation 1 to yield an unbiased estimate of α the following condition must hold, $E[\varepsilon \mid A_1] = 0$. In other words, the residual error term must have no relationship with the variable of interest and be essentially random.

An objection to this simple model is that the pairs of individuals in this model may share similar attitudes or actions because of a common trait. In other words, a relevant variable may have been omitted from the analysis. Any similarity in opinions between the pairs is hollow if factors such as religion, socio-economic status, and race are not taken into account. Would it be surprising to discover that Catholics have similar beliefs to other Catholics? This objection can be pictured in the following way

$$\begin{array}{l} A_2 = c + \alpha A_1 + \varepsilon \\ \uparrow \qquad\quad \uparrow \\ X \qquad\quad\; X \end{array} \tag{2}$$

where X is an outside cause of the attitudes of both A_1 and A_2. The residual error term in equation 1 can be re-formulated and divided into component parts to capture the relationship expressed in equation 2 as follows

$$A_2 = c + \alpha A_1 + \varepsilon^* \tag{3}$$

where $\varepsilon^* = f(X) + \varepsilon$. Unless X has no role in the relationship between A_2 and A_1, that is $f(X) = 0$, it is obvious that $E[\varepsilon^* \mid A_1] \neq 0$. Thus, the estimate of α will be biased.

Multivariate Regression

To address precisely this issue, a researcher would move to a statistical model that accounts for exogenous factors.

$$A_2 = c + \alpha A_1 + \beta X + \varepsilon \qquad (4)$$

where X = a set of relevant variables and β = a vector of coefficients estimating the effect of these variables upon A_2.

Model 4 has been the workhorse for scholars of interpersonal influence.[5] Virtually every study of note relies upon analysis of this model. Bells and whistles may be added to account for dichotomous dependent variables or non-linearity in the independent variables, but the core logic of the statistical model remains intact. The primary appeal of the model is that both computation and interpretation are straightforward, and it appears to adequately control for confounding variables.

Unfortunately, the above model does not take into account that the individuals considered rarely are paired together randomly and a selection process creates the relationship. Indeed, virtually all studies focus on pairs of individuals who have a defined relationship such as friends, family members, co-workers, or neighbors. While such pairs of people undoubtedly share common traits such as religion, and socio-economic status, they also share important traits that are difficult to observe and measure, both in practice and in principle. Within a parish or a club not every possible pair of friendships forms despite the homogeneity of the population. Nor does a random subset of friendships form (although contingency plays an important role). Individuals ordinarily pair up with individuals because they share some commonality that often is ineffable. Dating services may do an excellent job of paring down possible mates based on measurable characteristics, but ultimately the single individuals need to meet and discover if there is "chemistry." These unobserved traits play a crucial role in social interactions and should

[5] Examples of this model being used include such classic works as *The People's* Choice (Lazarsfeld, Berelson, and Gaudet 1948), *Personal Influence* (Katz and Lazarsfeld 1955), and *The American Voter* (Campbell, Converse, Miller and Stokes 1960). The model is also employed in more recent pieces of solid and respected scholarship like *Citizens, Politics and Social Communication* (Huckfeldt and Sprague 1995), *Impersonal Influence* (Mutz 1998), and *Democracy in Suburbia* (Oliver 1999).

be accounted for in theories of interpersonal influence. The influence of these hidden traits can be modeled in the following way:

$$\begin{array}{ccc} A_2 = c + & \alpha A_1 & + \beta X + \varepsilon \\ \uparrow & \uparrow & \\ H_2 \Leftrightarrow & H_1 & \end{array} \tag{5}$$

where H_i are unobserved or hidden personality traits that lead to the formation of attitudes or actions. Just as the residual error term could be parsed in the first set of equations in response to observed exogenous variables, X, the same can be done in response to unobserved variables, H.

$$A_2 = c + \alpha A_1 + \beta X + \varepsilon^* \tag{6}$$

where $\varepsilon^* = g(H) + \varepsilon$. Unless H has no role in the relationship between A_2 and A_1, that is $g(H) = 0$, then $E[\varepsilon^* \mid A_1] \neq 0$. Thus, the estimate of α will be biased.

Two terms can usefully be introduced at this point for grappling with the unobserved factors, H. The first is unobserved heterogeneity. Two individuals may be identical across a range of observable variables and very different in an unseen manner. That is, the pair is observably homogenous, but exhibits unobserved heterogeneity.[6] Comparing and contrasting two individuals who appear similar, but are systematically heterogeneous in a relevant manner will lead to the type of biased inference demonstrated in equation 6.

Selection is the second concept to be discussed. The subjects of social science research are autonomous agents who make decisions on their own behalf. When a person chooses to live in a particular neighborhood, or join a certain social club, or befriend a specific coworker, she is acting on volition: rational, emotional, or a matter of taste. Under most circumstances, the reason behind the selection is obscured to the researcher. The net result of the process of

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[6] The econometric literature on unobserved heterogeneity typically refers to differences in response to the variable of interest by subjects (see Hsiao 1986). The concept is similar, but not the particular problem that is being tackled here.

selection is that a similarity exists between subjects, which is unobserved by the researcher. In other words, selection is one way that unobserved heterogeneity occurs.[7]

In most instances, unobserved heterogeneity and selection processes are benign and irrelevant to the analysis conducted. However, when the hidden factors are causally related to the dependant variable and the independent variable of interest (as illustrated in equation 5 above), then the analysis suffers from omitted variable bias. Researchers typically respond to omitted variable bias by including more covariates in the analysis, but, by definition, relevant covariates are not available for unobserved factors. Nevertheless, the difficulty in measuring selection processes and unobserved heterogeneity does not imply the problem can be ignored.

Unobserved heterogeneity is not a new concept in the social sciences, nor has it been confined to obscure niches of political science. Unobserved heterogeneity and selection processes play a crucial role in the interpersonal influence literature. One of the most prominent examples is Anthony Downs' theory of rational ignorance and informational shortcuts (Downs 1957). Collecting information about parties and candidates to inform a vote is costly, so Downs theorized that citizens reduce information costs by asking the opinion of friends, neighbors, and colleagues *whom they know to share similar beliefs*. The rational voter assumes that the informed friend will have reached a similar conclusion to what the rational voter would have had she taken the time to be well versed on the issues. Notice that the correlation between the opinion of the rational voter, A_2, and the informed friend, A_1, is not caused solely by the communication. Instead, the homogeneity of opinion causes the information to be sought out in

[7] Since selection processes tend towards homogeneity, it may seem counter-intuitive to say that selection is a source of unobserved heterogeneity. However, heterogeneity and homogeneity are different sides of the same coin. The point is that the effects may not be entirely random (or idiosyncratic), are unseen, and typically go unnoted by the researcher.

the first place. Studies utilizing model 4 suffer from precisely this type of specification error and consistently over estimate the influence of interpersonal communication, α.

Rational ignorance nicely dovetailed with early findings and theories in the interpersonal influence literature. The most enduring legacy of *The People's Choice* (Lazarsfeld, Berelson, and Gaudet 1948) has been the two-step flow hypothesis. Lazarsfeld, Berelson, and Gaudet were baffled by the small influence the mass media and the enormous influence of interpersonal communication displayed in their study of Elmira, New York on opinion formation. They hypothesized that the media influenced attentive opinion leaders who in turn influenced the inattentive members of the public.[8] Despite a dearth of empirical evidence to support the theory,[9] the two-step flow spawned an entire literature attempting to map social networks and trace the dissemination of media content.

The core empirical finding of the interpersonal influence literature is the high correlation between opinions of members of a social network. A bewildering array of social networks exhibit this empirical regularity regardless of whether the network is large or small, high or low salience, or political or apolitical.[10] Interpretation of the correlation can be divided into two types: a) selection hypotheses where homogeneity is the result of self-selection and affinity for persons with similar traits; or, b) influence hypotheses where homogeneity is generated by pressure to conform to group norms.[11]

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[8] Opinion leaders themselves are an excellent example of unobserved heterogeneity. Virtually every study of opinion leaders finds them to be identical to the opinion followers in every measurable respect (see Rogers and Shoemaker 1971, Köpller 1989, or Brosius 1996 for reviews of the two-step information flow hypothesis). And yet, these opinion leaders are different in a very political salient respect.

[9] At least one of the authors acknowledged this to be the case when Katz wrote, "Of all the ideas in *The People's Choice*, however, the two-step flow hypothesis is probably the one that was least well documented by empirical data" (Katz 1957, 62).

[10] An important exception to this rule is coerced groups such as conscripts in the military.

[11] Genetic similarity is a third possibility, though not often discussed in the interpersonal influence literature. Pinker (2002) offers an interesting argument as to the unseen role of genetics in a wide range of behavioral traits.

The selection hypothesis argues that individuals seek out friendships, groups, and neighborhoods with similar value structures as their own. Despite a heterogeneous society, people are attracted to and actively surround themselves with like-minded individuals. Festinger, Schacter, and Back's study of the Westgate Housing Development (1950) offers an excellent and early example of the selection hypothesis. Married veterans newly enrolled at M.I.T were placed in an apartment complex away from the main campus. Apartment proximity was the biggest factor in explaining the friendship groups that formed. However, even among the set of close apartments a person would not form friendships with a random assortment of neighbors. Those who expressed "deviant" opinions were far less likely to be named by other members of the community as one of their three closest friends. Even within this very homogenous environment, a stringent selection process determined the social circles in which people operated.[12] Therefore, the selection hypothesis would suggest that a positive coefficient α in equation 4 is not caused by communication and influence so much as through a selection process.

The influence hypothesis argues that individuals come to resemble other members of the group. Often individuals have no firm opinion on a subject and gradually come to form an opinion based on the opinions to which they are exposed. Exposure to opinions happen frequently within social networks, so it is hardly surprising that individuals come to share the opinions of their families, neighborhoods, and clubs.[13] MacKuen and Brown (1987) provide a good example of interpersonal influence analysis using macro and micro level analysis. During the 1980 Presidential primaries, subjects were polled January, June, and September on their

[12] Theodore Newcomb noticed this same process nearly twenty years earlier in his Bennington study. Girls with a strong attachment to home remained conservative whereas girls who wanted to be part of the Bennington community adapted to the more liberal surroundings.
[13] The model of survey responses consisting of answers drawn from a pool of considerations obviously had not been explicitly formulated prior to the publication of *The Nature and Origins of Mass Opinion* (Zaller 1992), but a similar set of ideas were present in the interpersonal influence literature.

opinions of Carter and Reagan. To uncover macro-level influence, MacKuen and Brown checked the percentage of votes Carter and Reagan received in the November general election in the counties where the subjects lived. The respondent's attitudes towards the two candidates, A_2, more strongly reflected her county's vote share, A_1, in September than in January. On the micro-level, respondents were asked about the partisan loyalty and vote intentions of three friends. It turns out that the subject's vote intention, A_2, is well predicted by the vote intentions of her neighbors', A_1. MacKuen and Brown conclude that "individuals' political views are subject to social influence" and "the empirical estimates indicate that social processes rival the power of psychological mechanisms in guiding attitude change" (484-485). There is no note of uncertainty in the analysis or discussion of alternative hypotheses.

Alternative hypotheses that explain the empirical findings of MacKuen and Brown are readily available. The micro-level correlation could be the result of peers with similar economic profiles independently recognizing their self-interest or the selection of like-minded individuals as friends. At the macro-level, the convergence in vote intention could be a measure of improved information from the campaign decreasing individual uncertainty about the candidates. Interpersonal influence among residents of the county is an explanation of the correlations that MacKuen and Brown uncover, but not the only ones.

It is possible that both the selection and the influence hypotheses are correct and play a crucial role in opinion formation and social network construction. There is nothing in the theories that need be mutually exclusive. Since the correlation between the beliefs of individuals and their political contexts can be explained by both theories, disentangling the two effects is extraordinarily difficult.

Unfortunately, the results from most empirical studies performed to date are incapable of differentiating between the selection and influence effects. In fact, the studies focusing on interpersonal influence are designed in such a way as to ignore selection processes. Huckfeldt and Sprague (1995), for instance, are almost silent on the degree to which residents of South Bend select the neighborhoods in which they live. The few oblique mentions of selection bias imply that such forces are not an issue in the analysis of political context in the South Bend study (see for example pages 9, 48, 84-85, 128). All of the analysis of context that follows assumes there is no selection bias present.[14] The design of the South Bend study is, therefore, entirely focused on detecting contextual influence with carefully constructed surveys, while questions about selection are not capable of being answered. As a result, the statistical models are potentially misspecificd (by using an analogue of equation 4) and overstate the degree of influence communities have over the political attitudes. That is to say, the estimate for α will be inflated to an unknown extent.

Recognizing that unobserved heterogeneity and selection are factors in social interaction does not ensure that the bias will be accounted for. Huckfeldt & Sprague do acknowledge that individuals select partners with whom to discuss politics in constructing a social network. In fact, chapter 8 is entirely devoted to the creation and discussion of a model of discussant partner selection. This admission is odd since most of the analysis in the rest of the book assumes no unobserved heterogeneity.[15] Occasionally, descriptive statistics are provided about selection on

[14] This is especially odd since Huckfeldt and Sprague surveyed "sixteen different South Bend area neighborhoods purposely selected to maximize social homogeneity within the neighborhoods, and social heterogeneity between the neighborhoods" (1995, 129).

[15] The articles published by Huckfeldt and Sprague prior to *Citizens, Politics, and Social Communication* suffer the same defect (see for example Huckfeldt, Plutzer, and Sprague 1993, Huckfeldt and Sprague 1987, or Huckfeldt and Sprague 1991).

observable characteristics, but they are not incorporated into statistical models.[16] Such a correction would not address the issue of *unobserved* similarities, and nor does it explain why the authors fail to act upon the evidence of observable selection.

An excellent example of the same cognitive dissonance is provided in two recent articles from the American Political Science Review. In March 2001, Mutz and Martin published an insightful article analyzing descriptive statistics to argue that people are very selective with regards to discussion partners, but less selective in exposure to mass media. One year later in the *APSR* and using the same data, Beck, Dalton, Greene, and Huckfeld argue that "interpersonal discussion outweighs the media in affecting the vote" (Beck et al. 2002, 57) and a logit model perfectly analogous to model 4 is used as the empirical support. The Mutz and Martin article is cited, but the problem is seen as "whether political bias should be measured at the source or at the receiver" (p. 59).[17] In other words, Beck et al. worry about measurement error (which is a concern) and completely ignore selection and unobserved heterogeneity and the degree to which it confounds estimates of interpersonal influence.

Studies that recognize unobserved heterogeneity as an omitted variable problem, mistakenly attempt to account for selection by adding more control variables.[18] The reasoning is

[16] The explanation of the statistical estimators used in the analysis leaves something to be desired. Huckfeldt and Sprague develop a model of selection in chapter 8 (p. 148-151) and then use a "logistic regression model" referencing the entirety of Hanushek and Jackson 1977 to generate estimates (p. 152). Nothing in the analysis or discussion suggests that the logistic model utilized is anything other than the standard logit model. The suggestion is that the logistic analysis is only to derive estimates for two parameters in the model. Unfortunately, the discussion never returns to the selection model derived by the authors. In short, the authors may have used a selection model in chapter 8, but the presentation is ambiguous enough that a reader is unsure.

[17] Beck, Dalton, Greene, and Huckfeldt are not unique in this regard. Even Katz and Lazarsfeld (1955) failed to recognize the degree to which the selection hypothesis undercuts estimates of influence. To establish the value of primary groups, numerous studies are cited (including Cooley's *Social Organization* published in 1909) arguing that people seek out others with similar values (p. 59-61). The fact that this type of selection process undercuts the empirical results reported in every single study cited in the rest of the book utterly escaped Katz and Lazarsfeld.

[18] Huckfeldt and Sprague do not proceed down this route and provide very few covariates in their analysis. However, their reasoning is somewhat dubious. "It should come as no surprise that the inclusion of highly correlated explanatory variables often proves to weaken statistical purchase. That is, additional individual-level

that since hidden factors, *H*, are the problem, adding all available demographic variables will perhaps capture a portion of the unobserved heterogeneity. Unfortunately, this strategy may backfire and create further bias in estimates. If the included variables are orthogonal (i.e., unrelated) to the unobserved selective factor, then the added variables neither hinder nor advance the analysis (though degrees of freedom will shrink and collinearity among independent variables may be introduced). If the included variables capture a portion of the unobserved selection factor, then the added variables *increase* the bias of the model. By controlling for relevant factors but ignoring the selection process, the model treats dissimilar observations as equivalent.

For instance, Huckfeldt and Sprague discover that friends often vote for the same candidate even when controlling for demographic factors such as party identification in model 4 (1995: Chapter 9). Most sets of friends share party identification, and it is not surprising the vote choices are the same.[19] However, for those friends who are members of opposing parties, it initially appears surprising that they vote in a similar fashion. Presumably, friendships form because of shared values and life experiences that form a bond. These bedrock values inform a person's choice of party, so a party identification variable should be related to the friendship selection process. It is not terribly surprising that two people who share common values and experiences vote in the same fashion. By holding the discussant's party constant in the analysis, Huckfeldt and Sprague end up comparing a Republican subject's Democratic friend to the pool of other Democrats in South Bend sample with whom she has less in common.[20] Thus, the

controls run the very real danger of producing exececessive collinearity and, thus, a misplaced willingness to accept null hypotheses" (Huckfeldt and Sprague 1995, 133).

[19] Huckfeldt and Sprague were concerned about endogeneity between a person's vote choice and party identification, so they use a two-stage least squares estimator. So, A_1 represents the purged estimate of P_1's vote choice rather than the vote choice itself.

[20] Achen (1986) makes this argument formally in the appendix to chapter 2.

interpersonal influence is over-stated because the baseline commonality between friends is under-stated. The exact same argument can be made for other demographic variables such as religion, education, or income. This particular problem is model specification rather than the lack of the adequate proxy variables. The process of interpersonal influence is not being modeled appropriately, and piling on new control variables will not rectify the bias (see chapter 2 of Achen 1986 for a more complete discussion of this point).

Selection Models

Nearly fifty years of econometric progress has created selection models that attempt to model the non-random assignment of treatment. In particular, the Heckman selection models are well suited to studying interpersonal influence because the selection process need not be a function of the dependent variable (as in the Tobit models for censored and truncated data). The particular formulation of the selection model that Heckman advanced in 1978, calculating the Inverse Mills Ratio, has come under attack (see Manski 1993), but all of the proposed replacements use the same basic logic of calculating the probability that subjects are selected into the sample.

Before describing the logic of selection models, it is useful to restate the objection to standard regression techniques for interpersonal influence studies in terms of expectations. When estimating the coefficient α , scholars searching for interpersonal influence need to estimate the attitudes of a person, A_2 , given the attitudes of a friend, A_1, and demographic characteristics, X. This can be expressed mathematically as $E(A_2 \mid A_1, X)$. However, data are only collected for pairs of friends.[21] Define F as a dichotomous variable that equals 1 when two

[21] This same argument can obviously be extended to include any form of relationship between two individuals: neighbors, coworkers, parishioners, or Moose Lodge brothers.

individuals are friends and 0 otherwise. The goal of the interpersonal influence estimation can now be expressed as:

$$E(A_2 \mid A_1, X) = E(A_2 \mid A_1, X, F=1)\Pr(F=1 \mid A_1, X) + E(A_2 \mid A_1, X, F=0)[1-\Pr(F=1 \mid A_1, X)] \qquad (7)$$

The first term on the right hand side of the equation is the expectation of the attitudes of the subject given demographic characteristics, the attitudes of her friend, and the fact that they are friends. This is precisely the model prior scholars have estimated with their research ignoring the other terms in the equation. This is an analogue of equation 4 and biased for the same concerns about unobserved heterogeneity and selection.

The second and last term of equation 7 is the probability that the given persons are friends. In principle, it should be possible to calculate this term. In practice, this term may be difficult to estimate. Similarly, the counter-factual third term of equation 7 requires estimating the effect of a person not in the sample upon the subject’s attitudes and is typically impossible to calculate. In some cases it may be possible to assume the counter-factual is zero, that is to say that people not in contact with the subject have no effect upon her behaviors and beliefs, but one should be cautious about making unverifiable assumptions. The difficulty in calculating a parameter in no way mitigates its importance.

However, in certain instances it may be possible to calculate the required probabilities. For instance, suppose that a researcher wanted to know the degree of influence co-workers have upon one another’s political beliefs. An employer agrees to allow its employees to be surveyed by the researcher. If one assumes that employees working different shifts have no effect upon each other, the variance in opportunity to interact among employees because of corporate hierarchy and office geography allows the researcher to detect influence. However, proximity and interaction are likely to be correlated with pre-existing character traits. The employer may have hired roughly similar people for similar positions. If people with the same job description are located in the same area and frequently interact with one another, the resulting correlation of attitudes may be spurious. However, if the researcher knew the corporate structure and the decision rule used to assign offices and cubicles, she might be able to predict how interactions take place within the office. This information would allow for the estimation of a sufficiently accurate selection equation to control for proximity. Such information would allow for a Heckman-like estimation[22]:

$$\begin{aligned} A_2 &= c + \alpha A_1 + \beta X + \hat{F} + \varepsilon \\ F &= \gamma Z + u \end{aligned} \tag{8}$$

where Z is a vector of predictors of the hiring process and proximity within the firm and F is the same as in equation 7 but here represents proximity within the office. In order for this estimation to be unbiased, it must be true that $\operatorname{cov}(\varepsilon, u) = 0$, which may be an assumption that strains credulity. It is also necessary for γZ to predict F with sufficient accuracy to purge the variance in F correlated with A_1. In the example above where the employer provided the decision rule for the firm's geography and shifts, this estimation may be possible, but it may prove difficult in other settings.

It is important to note how the outlined study of the firm differs from interpersonal research as it is commonly undertaken. Typically, a survey respondent would be asked to name three co-workers with whom she discusses politics and these co-workers would then be surveyed

[22] The model below is identical to the Heckman model except that the Inverse Mills Ratio, $\frac{\varphi(\gamma Z)}{\Phi(\gamma Z)}$ is replaced by any estimation of the probability of proximity and interaction, $\hat{F}$. The Inverse Mills Ratio has been correctly criticized as imposing a number of unlikely constraints upon the data. However, every proposed replacement continues to focus upon the probability of the observation being selected. The deficiencies and advantages of various analogues are irrelevant to the current discussion.

as to their beliefs.[23] This method of surveying is often referred to as a "snowball" survey. The researcher would examine the correlation between the two sets of answers. Because people have an affinity for person with similar traits and seek to discuss politics with those friends with the most in common politically, this research methodology will suffer from the unobserved heterogeneity problems discussed in equation 4. Disentangling the selection and influence effects in the correlation between attitudes may require hefty assumptions. As a practical matter, selection models are unlikely to be able to assist removing the bias from the estimation of snowball surveys. Modeling the process by which co-workers befriend each other and deriving a probability that any two people will become friends is likely to prove intractable. At the very least, it would require a rich set of covariates that social science has yet to produce. Selection models are only of utility when the researcher can be fairly certain of that the process that generated the data has been modeled correctly.

Panel Data

In panel studies, unobserved factors are handled by looking for changes and using fixed effects. The intuition behind the strategy is that the same unobserved factors would be present at time 1 and time 2 and therefore cannot cause changes in the dependent variable. Therefore, by focusing on change rather than absolute levels, it is possible to eliminate the influence of fixed traits. This can be shown easily using data collected twice (with subscripts denoting the time period) and adapting model 4:

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[23] Huckfeldt and Sprague (1995) use this technique as does Mutz (1998). However, Mutz (2001) does provide something similar to a selection model when analyzing the effect of intimacy and influence. Mutz uses a two-stage least squares model to first estimate the degree of intimacy between discussant pairs and then uses the purged intimacy measure to estimate influence. This does not address the problem of being a pair in the first place, but the analysis is a step in the right direction.

$$
\begin{aligned}
A_{21} &= c + \alpha A_{11} + \beta X_1 + g(G_1) + \varepsilon_1 \\
A_{22} &= c + \alpha A_{12} + \beta X_2 + g(G_2) + \varepsilon_2 \\
\hline
\Delta A_2 &= \alpha \Delta A_1 + \beta \Delta X + g(\Delta G) + \Delta \varepsilon
\end{aligned}
\tag{9}
$$

where G_i are the unobserved factors for the time period (as in the above examples). If one assumes that the unobserved factors influencing attitudes are fixed, that is $G_2 = G_1$, then the unobserved traits play no role in the change of A_2 since $\Delta G = 0$. Collecting cross-sectional time series data (i.e., panel data) therefore controls for the subset of unobserved factors that are fixed. Panel studies therefore represent a step towards the goal of isolating interpersonal influence from pre-existing beliefs and selection.

It should be noted that under the first differences model (i.e., fixed effects) outlined in equation 9, the only explanatory factors that are allowed in the model are those that change. Static variables will be washed out for the same reason that the constant term does not need to be estimated. This limits the model’s usefulness in practical applications. The majority of studies in interpersonal influence estimate a model where the subject’s opinions converge to the opinion of the community, which is assumed to be unchanging (e.g., MacKuen and Brown 1987). This means that few studies of interpersonal influence to date have utilized a fixed effects model.[24] The basic cross-sectional models are typically what are used for analysis.

To account for individual level effects, a researcher might move to a random effects model, which permits the use of static independent variables. Rather than allowing the constant to vary across subjects as in the fixed effects model, the random effects model decomposes the error term into a static component for each person and a random component that can vary across people. At first blush, the random effects model would appear to be the correct specification

[24] Jennings and Niemi (1974, 1981) create an impressive panel dataset but do not utilize a fixed effects model because the analysis conducted is primarily descriptive in nature.

because it assumes that observations are drawn from an infinite population, which most random sample studies approximate. However, for studying interpersonal influence the random effects model has a key drawback. While the subject may be drawn from a larger population, the subject did not select the neighborhood or workplace or friends randomly. The same flexibility that allows static independent variables to be included in the analysis will fail to purge the analysis of the unobserved heterogeneity and the resulting estimation will suffer from omitted variable bias (see Hausman and Taylor 1981 for a formal explanation). In other words, relaxing the fixed effects restrictions ends up defeating the purpose of conducting a panel study in the first (namely, removing unobserved heterogeneity).

While the econometric sub-field concerned with panel data has made great strides deriving estimation techniques to account for unobserved heterogeneity, there are two major drawbacks to using panel studies to grapple with interpersonal influence and neither can be sidestepped by writing new estimators. The first is that most beliefs and habits are relatively stable over time. Attitudinal change generally takes place over a long period of time and in subtle ways (Page and Shapiro 1992). Most of the variance in survey responses is the result of measurement error or vacillation in weakly held beliefs rather than genuine movement in opinion (see Achen 1975 and Krosnick 1991). Capturing genuine changes in beliefs is difficult and rare.

The second roadblock to using panel studies is accounting for unobserved heterogeneity that varies over time. For instance, an unknown but persuasive candidate may decide to knock on doors in a neighborhood to meet voters in person. When measured in the first time period there may be considerable variance in answers to questions on the unknown candidate. But after the candidate has met the voters in a neighborhood, the residents may have all formed the same opinion about the candidate. This perceived consensus could be the result of discussions among neighbors, but it could also be the result of gaining more information on the candidate. The same argument could be made about any number of exogenous effects such as media coverage, the state of the economy, and crime.

Including dummy variables for time periods in the analysis will be able to account for only the sample wide shocks and will miss idiosyncratic movements. Many of the exogenous shocks in interpersonal studies would be localized and not present across the whole dataset. For instance, a candidate may target certain areas more than others – in fact, the candidate may visit only a few blocks personally. This could mean that individual subjects converge in response to an exogenous event that is not accounted for by a time variable. Time dummies solve unobserved heterogeneity only when the researcher assumes that simply shifting the intercept for each observation sufficiently accounts for the dynamic component. Controlling for time period is a step in the right direction, but does not address the heart of the omitted variable bias.

Randomized Experiments

Randomized experiments can side-step all of the problems discussed above. By randomly manipulating a variable of interest, the experimenter can remove correlation between the variable of interest and all of the other observable and unobservable causal factors. The need for researcher control limits the scope of the questions asked and makes experiments far more difficult to carry out than observational studies. However, the precision of the answers obtained more than justifies the increased planning and expense of overseeing the execution of the experimental protocol.

An experiment is any randomly determined application of a factor of interest on a pool of subjects. In its classic form an experiment randomly divides subjects into a treatment group, which receives an intervention, and a control group, which does not receive the intervention.

Since division into the two groups is randomly determined, the treatment group and control group should have identical compositions on average. This means that receiving the treatment is in no way related to age, income, education, gregariousness, curiosity, or any other measurable or unmeasurable trait when compared to the control group. An experiment has been designed correctly if knowing the assignment to treatment or control provides no further information about the subject.

The application of treatment need not be dichotomous. Application of the treatment can be split as many ways as the researcher desires.[25] Medical researchers often experiment with different doses of the same drug. Lotteries like Powerball could be thought of income experiments with the range of winnings constituting the different treatment groups (and the losers serving as the control group). Nor does the treatment need to involve a single factor. Factorial design can easily accommodate interactions between multiple treatments. For instance, suppose one wanted to know whether citizens are mobilized by leaflets, a knock on the door, or a combination of both. To test this, registered voters in a neighborhood could be randomly assigned to receive only a leaflet, only a knock on the door, both the leaflet and the knock on the door, or no contact at all from the campaign. The voter turnout rates between the four groups could then be compared to determine which technology is more cost effective increasing turnout. This type of protocol could be extended to any number of treatments. Sample size is the only limitation upon the use of the technique.

The chief attraction of experiments is the ease with which they can isolate causal influences. To demonstrate this, suppose that equation 6 correctly captures the causal model of interpersonal influence. Suppose further that an experimenter devised a study where subjects were randomly divided into a treatment group, who were provided information on the opinions

[25] In fact, the treatment variable could even be continuous.

of their neighbors, $A_1 = 1$, and a control group, who were not informed of how neighbors felt, $A_1 = 0$. The analysis proceeds by comparing those subjects assigned to the control group, denoted by subscript C, and the subjects assigned to the treatment group, denoted by subscript T.

$$\begin{aligned} A_{2C} &= c_C + \alpha A_{1C} + \beta X_C + g(G_C) + \varepsilon_C \\ \underline{A_{2T}} &\underline{= c_T + \alpha A_{1T} + \beta X_T + g(G_T)} + \varepsilon_T \\ A_{2T} - A_{2C} &= c_T - c_C + \alpha(A_{1T} - A_{1C}) + \beta(X_T - X_C) + g(G_T) - g(G_C) + \varepsilon_T - \varepsilon_C \end{aligned} \tag{10}$$

Since the treatment and control groups are randomly assigned, the manipulated factor, A_1, should be the only independent variable that differs between the two groups. Thus, with sufficiently large sample sizes the observed, unobserved, and contingent factors balanced out. This can be expressed mathematically as $E(c_T - c_C) = 0$, $E(X_T - X_C) = 0$, $E[g(G_T) - g(G_C)] = 0$, and $E(\varepsilon_T - \varepsilon_C) = 0$. Thus, equation 10 can be reduced to (bearing in mind that the treatment is dichotomous):

$$A_{2T} - A_{2C} = \alpha(A_{1T} - A_{1C}) = \alpha \tag{11}$$

To estimate $\hat{\alpha}$:

$$\hat{\alpha} = E(A_{2T} - A_{2C}) = E(A_{2T}) - E(A_{2C}) = \bar{A}_{2T} - \bar{A}_{2C} \tag{12}$$

Thus, calculating the value of α, which was so cumbersome using regression techniques on observational data, is merely a matter of subtracting the mean of the control group from the mean of the treatment group when a randomized experiment is conducted. Because of the effort put into the design of the experiment and monitoring the implementation of the protocol, the simple subtraction of two averages can yield an unbiased estimate of a particular form of interpersonal influence.

The analysis above reduces the number of assumptions required in the analysis. No assumptions were required about the process that generated the data, the distributions of the variables, or the relationship between the variables. Experimental analysis is not devoid of assumptions. For instance, equation 10 could not be simplified without $E(c_T - c_C) = 0$ and $E(\varepsilon_T - \varepsilon_C) = 0$. That is, it is presupposed that the treatment and control groups have the same intercepts and residual errors on average. However, these two assumptions rest upon the Law of Large Numbers and executing the protocol successfully. It should also be pointed out that observational studies make the same assumptions, so experimental interventions are not adding to the presuppositions of the model. The uncertainty associated with the experimental estimate of α is due to sampling error and not model misspecification.

The simplicity of the unbiased estimator of α in equation 12 does not imply that more complicated econometric estimators cannot be used. In fact, modeling assumptions are better satisfied by experimental rather than observational data. Where observational data requires covariates, multiple stages, and modeling assumptions to claim $E[\varepsilon \mid A_1] = 0$, the condition holds by construction of the variable A_1 in experiments. Adding covariates to the analysis reduces variance and thereby shrinks standard errors, but covariates are unnecessary to achieve unbiased estimates of α because omitted variable bias is not a concern.

The power of experimentation is well established in the natural sciences but until recently ignored in the social sciences. This does not mean that experiments have not been conducted to study interpersonal influence. A grass roots voter mobilization campaign was studied by Eldersveld in 1956 to determine the influence of phone calls on voter turnout. The study was well received, but Eldersveld’s peers did not follow his lead and conduct follow up experiments. The neighborhood surveys used by Berelson, Lazarsfeld and McPhee proved a more popular methodology for field research. During the early 1950s, Stuart Carter Dodd conducted a series of impressive field experiments concerning the transmission of propaganda (see Dodd 1952). However, Dodd's international survey research had a more lasting effect.

Experiments have found much wider acceptance in the laboratory setting. For example, experiments have been conducted to examine the effect of discussing news with others (e.g., Iyengar and Kinder 1987), the role of personal information in trust games (e.g., Wilson), exposure to discordant ideas on political tolerance (e.g., Mutz 2001), and the spread of rumors (e.g., Allport and Postman 1947). One thing laboratory studies have in common is that they remove the person from social networks in order to measure the effect of social networks. The laboratory setting is necessarily artificial, so there is reason to be dubious that lab findings translate to the organic social networks in which subjects are embedded. Lab experiments are more likely to provide interesting insights into interactions in anonymous settings (of which there are many in our society), but social networks differ in that they are characterized as being familiar: personalized settings with repeated interactions among persons with knowledge of each other and multiple mutual acquaintances. It seems that laboratory experiments provide a different type of treatment than subjects are confronted with in organic situations. Ultimately, the extent to which laboratory findings translate to actual neighborhoods is an empirical matter and should be tested. If the goal is to describe interactions in settings in which subjects exist on a day-to-day basis, field experiments are essential to confirm and extend results from the lab.

Randomized field experiments can be designed to study interpersonal influence in three ways: 1) create a social network where none existed before; 2) manage interaction and contact within an existing social network; or 3) introduce exogenous shocks into the network and trace the effect of the shock. The first method manipulates the network itself, the second controls communication within the network, and the third imposes external events upon the network. The three categories of protocols are not mutually exclusive. A researcher could design an experiment where communication about a controlled external event takes place within a constructed social network. However, given the difficulty in executing each experiment on its own, combining the different types may be overkill.

Randomized Networks

The first way of conducting a field experiment on interpersonal influence directly controls and measures the social network itself. Oliver (2001) and Huckfeldt and Sprague (1995) cannot obtain unbiased estimates of influence and participation because subjects are self-selecting the communities and neighborhoods in which they live. But what if it were possible to randomly assign residents of South Bend to neighborhoods? Random assignment would solve the confounding influence of selection, and the models Huckfeldt and Sprague analyze would yield unbiased estimates of the influence of political context. Obviously, such an experiment would be infeasible and unethical.

However, randomized assignment may be possible within specialized communities. There are many settings in which persons are assigned to groups; recruits are assigned to camps and barracks in the military; poor families are assigned to housing developments, apartment buildings, and specific apartments (see Katz, Kling and Liebman 2001); prisoners are assigned to prisons, cells and blocks; and, college students are assigned to dorms and rooms (see Sacerdote 2001). The case of college students and prisoners is particularly interesting because the governing authority not only assigns rooms but also the people with whom to live. Once the assignments are made, people explore the social topography and form relationships and institutions with those around them.[26]

Usually assignments within these communities are made according to a conscious decision making process. For instance, many colleges ask incoming students to answer surveys of varying lengths to determine the compatibility of roommates and to create demographic balance within dorms. But in the instances where assignments are made randomly (which is not to be confused with haphazardly), an experiment has been conducted and all a researcher has to do is collect the data. All the persons of a certain type have been distributed randomly across the community, so there is no fear that civically engaged persons opted into the same apartment building or that all the violent criminals were place in a particular cellblock. If interpersonal influence effects are strong, then pockets of norms, behaviors, and opinions should form and be distinguishable from random chance. On the other hand, if the context effects are weak, then the pockets of homogeneity noticed will likely be the result of sampling variance (i.e., a group of Republicans placed together by random chance) and estimates of influence will fail to reject the null hypothesis of atomistic individuals unaffected by the views of their roommates.[27]

It is important to note what is measured by controlling assignment within a social network. Such experiments measure context effects and not individual interactions. While neighbors (and perhaps roommates) will be externally imposed, the subject remains free to select the people with whom they interact the most – their friends. Regression analysis considering the influence of the consensus within an apartment building will yield accurate estimates of the effect of the building on the behavior and beliefs of the subject. In contrast, a survey comparing

[26] This set up differs from lab experiments in that the subjects are living within the social context as opposed to operating in it for brief periods of time. The interactions are not artificial by definition because the constructed framework really is the social network in which the subject resides.
[27] Well, the null hypothesis will be rejected 95% of the time.

the attitudes of friends remains at the mercy of self-selection and unobserved heterogeneity and, consequently, such studies will be biased. The randomization of one facet of social interaction does not purge the entire process of selection and bias.

It is perfectly acceptable for the randomization to occur within strata. There may be important policy concerns that trump the researcher's desire for a purely random sample: the severity of the crime and the predicted risk of future violence help to determine the assignment of prisoners to wards; rooms (and sometimes floors) of dorms are gender segregated; and, the military provides different training and assignment to specialized troops such as intelligence or communications. While such stratification needs to be accounted for and limits the conclusions to be drawn, it in no way invalidates the experiments as a whole. The researcher simply needs to examine the context effect within the strata created rather than in the system as a whole. This can be easily accomplished through the use of dummy variables and is not complicated. The net effect is conducting several concurrent experiments on slightly different populations.

An example of using randomized social network creation to study contagion of norms was conducted during the 2002-2003 school year at a college that randomly assigned entering students to housing. The college is a somewhat selective, mid-sized private school in a suburban East Coast setting. Unlike students at Dartmouth, Reed and Williams College (i.e., the sites of the grade point average studies), the students at this college do not fit the definition of academic elite. Racially, the student body mirrors broader society, however, with tuition, room and board costing more than $30,000, the students come from wealthier than average families. In short, the school is typical of residential campuses across the country.

Students were randomly assigned to rooms within suites of dormitories. Rooms generally contained two students and suites consisted for four rooms sharing a living space and bathroom. Thus, one could reasonably expect to observe behavioral contagion among both roommates and suitemates given the relatively intimate accommodations. At the beginning of the school year, there should be no correlation between the behaviors and beliefs of randomly selected roommate pairs, since every student has an equal chance of being placed with a given roommate of the same gender.[28] Some students will be given apathetic roommates with no real political opinions, while others will be presented with strident Democrats or Republicans. The predisposition of your roommate (and suitemates) constitutes the treatment a student is exposed to. By the end of the school year, if nonrandom clusters of opinions and behaviors are found (i.e., roommates possess correlated behaviors and beliefs), then one can conclude that roommates are influencing each other over the course of the year.

Despite the advantages of randomization, studies of college freshmen have trouble isolating the unique effect of interpersonal influence for two reasons. The first problem is determining the degree to which roommate A is influencing roommate B and vice versa, also known as reflection. Even if the researcher discovers clustering for the dependent variable of interest (e.g., grade point average), the degree and nature of the contagion cannot be estimated without imposing stringent assumptions about the direction of the contagion, the functional form of the relationship (e.g., linear or non-linear) and the importance of each reference group (see Manski 1993). Randomization eliminates selection as a confounding factor and the layout of the dorms creates natural units to examine for interpersonal influence, but reflection still poses problems.

The second problem is that pairs of roommates and suitemates may have similar exposure to outside influences. For instance, the media consumption of one roommate is likely to affect

[28] It is possible that men and women differ with regards to a behavior or a belief on average. As a result, it is important to control for gender when studying roommate pairs.

the media the other roommate is exposed (e.g., reading one another's magazines or watching TV at the same time). Events that are highly localized, like campus security breaking up a party, will potentially alter the opinions of each member of the suite and not other residents of the building. Over the course of an academic year, roommates and suitemates will be exposed to many of the same exogenous factors and such exposure may cause convergence in opinions without any interpersonal influence.

A solution to both of these problems is to consider traits measured prior to the school year and before any interpersonal influence could take place. Such traits serves as a meaningful metric by which to measure the influence roommates have upon one another.[29] Applying this design principle, one can model student *i*'s end of year attitudes, A_{2i}, are a function of her attitudes in the beginning of the year, A_{1i}, and the starting opinions of her roommates, A_{1R_i}, and suitemates, A_{1S_i}. To account for the single sex nature of rooms and suites, a dummy variable for gender is also included in the model. Equation 7 summarizes the regression equation to be used,

$$A_{2i} = c + \beta_1 A_{1i} + \alpha_1 A_{1R_i} + \alpha_2 A_{1S_i} + \beta_2 Female + \varepsilon_i \qquad (7)$$

where *c* is the constant and ε_i represent the unobserved, idiosyncratic causes of a student's opinion at the end of the year.[30]

[29] For instance, Sacerdote initially regresses a student's freshman year GPA on her roommate's freshman GPA (Sacerdote 2001, Table 3, column 1) and finds that every point a roommate scores boosts a students GPA by 0.12 points. This analysis suffers from the reflection and exogenous factor problems, so Sacerdote moves to a model where a student's GPA is regressed upon her roommate's "high school academic score" and intention to graduate with honors. He finds that living with a roommate the Dartmouth admission department deems in the top 25% of the class boosts a GPA by 0.04 points and roommates intent on graduating with honors can boost turnout by as much as 0.32 points (Sacerdote 2001, Table 3, column 6). The characteristics of a student at the beginning of the year is not a perfect measure of the studiousness of a roommate, but characteristics prior to the school year are not endogenous and provide a valid point of comparison.

[30] Following the lead of Sacerdote (2001), when a student has two roommates, the views of both roommates will be averaged together to calculate A_{1R_i}. Similarly, A_{1S_i} represents the average views of person *i*'s suitemates. Averaging loses information but in no way biased the analysis.

Data for the study came from a 28 question, two-wave panel survey administered during the second day of freshman orientation and the week prior to final exams in the spring (see Appendix). Roughly half of the entering class took both the first and second wave of the survey. However, 45% of the students did not complete the full year of study and not all respondents had roommates and suitemates complete the survey. As a result, the sample of students answering both surveys with pre-survey information for roommates and suitemates is relatively small (N=230). Fortunately, beginning of the year attitudes explain most of the variance in end of year attitudes, so the study has sufficient statistical power to detect contagious attitudes.[31]

Table 1 presents the results for those questions exhibiting contagion.[32]

Table 1 OLS Regression Results Modeling the Peer Effects of Roommates and Suitemates

Variable	Discuss Politics	Number of days a week you read a newspaper	Number of days a week watch television	Drinking Age	Favorable / Unfavorable Pot Smokers	Legalize Marijuana	Favorable / Unfavorable Homosexuals	Homosexual Teachers
Constant	-0.06	-0.10	-0.33	-0.59***	0.23	0.09	0.20	-0.53***
	(0.14)	(0.26)	(0.47)	(0.13)	(0.14)	(0.14)	(0.12)	(0.12)
Pretest Answer	0.57***	0.25***	0.34***	0.37***	0.70***	0.66***	0.56***	0.36***
	(0.07)	(0.03)	(0.04)	(0.06)	(0.05)	(0.06)	(0.05)	(0.04)
Pretest Roommate	0.18**	0.12***	0.18***	0.15**	0.19***	0.14**	0.24***	0.10**
	(0.07)	(0.04)	(0.05)	(0.06)	(0.06)	(0.07)	(0.05)	(0.05)
Pretest Suitemate	0.21*	0.13**	0.15*	0.20**	0.24**	0.16	0.16	0.14
	(0.12)	(0.06)	(0.08)	(0.10)	(0.10)	(0.12)	(0.10)	(0.09)
Female	-0.24	-0.12	-0.33	0.30*	0.05	-0.02	0.46**	-0.38***
	(0.18)	(0.16)	(0.20)	(0.17)	(0.20)	(0.18)	(0.19)	(0.12)
N	204	230	230	202	197	198	204	202
Adj-R-sq	0.30	0.23	0.26	0.20	0.51	0.38	0.51	0.36

The dependent variable is the post-test response of the student.
Unless otherwise stated, questions were on a 5-point agree/disagree scale.
Favor/unfavorable ratings are on a 7-point scale.
* implies $p<0.1$; ** implies $p<0.05$; *** implies $p<0.01$

To understand the magnitude of the reported coefficients, imagine two students who begin the year indifferent to homosexuals (i.e., 0). One student is placed with a roommate who

[31] No meaningful correlation was found between pre-existing attitudes among either roommates or suitemates.
[32] The Q-statistic testing the homogeneity of treatment effect across questions is firmly rejected. Thus, it is extremely likely that

has an unfavorable view of homosexuals (i.e., -2) and the other student is placed with a roommate with a favorable view of homosexual (i.e., +2). By the end of the year, Table 1 predicts that that one student will have a slightly unfavorable view of homosexuals (i.e., -0.5) and the other would have a slightly favorable view of homosexuals (i.e., +0.5). The same calculation can be performed for suitemates and repeated for each category in Table 1. The bottom line is that roommates and suitemates can shift a person's opinions, but the shift is not radical and does not necessarily breed like-mindedness.

The most striking feature of Table 1 is that a student's attitude at the beginning of the year is by far the best predictor of the student's opinion at the end of the school year (see row 3).[33] The coefficients are extremely large relative to the peer effect coefficients, with most pre-test coefficients in the range of 0.35 to 0.56, and statistically significant. For the homosexual question, the pre-test coefficient is over twice as large as the roommate coefficient; and, for the pot smokers question, the coefficient is nearly three times as large as the suitemate coefficient. Thus, while roommates and suitemates may affect a student's opinion about homosexuality and the use of marijuana, the best predictor of their attitudes remains the beliefs present at the beginning of college.

It should be pointed out that most of these survey questions concern behaviors. Few would be surprised that students whose roommates enjoy discussing politics also report discussing politics (though, interestingly, they are no more interested in politics at the end of the year). Similarly, students whose suitemates regularly read the newspaper or watch television are more likely to consume such readily available media. Even opinion questions concerning the

[33] This finding also validates the survey instrument; suggesting that the responses are measures of genuine opinion and not an artifact of taking the survey itself.

legal drinking age and marijuana may be capturing behaviors. Again, roommates who procure illicit substances may alter attitudes simply by lowering barriers to usage.

Attitudes concerning homosexuality are the only exceptions to the pattern of behaviors being contagious and attitudes not changing in response to roommates. Why might tolerance for homosexuals be different from attitudes about political parties, gender roles, or tax rates? A likely explanation is that, freed from direct parental oversight, college represents the first opportunity for young persons to encounter homosexuality. The intimate living conditions with non-family members may also increase the salience of the topic. The combination of a topic being both unfamiliar to the student and highly salient probably creates an environment where interpersonal influence is strongest.

The results of the study are not surprising, but instructive for understanding the role of interpersonal influence in shaping beliefs and behaviors. By far the best predictors of a student's attitudes at the end of the year are her attitudes at the beginning of the year. Moreover, on bedrock political attitudes such as partisan identification, taxes, and welfare no peer effects were detected whatsoever. However, there were questions exhibiting measurable contagion. The views of roommates and suitemates matter, but only on behavioral, unfamiliar or highly salient topics and the effect is seen only on the margins.

Randomized Interactions within an Existing Network

If answers about common residential life are desired, then constructing the social network is not the proper mode of inquiry. One would have to study behavior and interactions within the social network. The second experimental design seeks to do precisely this by controlling and measuring interaction between subjects. The literature on interpersonal influence generally posits two kinds of mechanisms for convergence in attitudes and actions. In the first, friends and neighbors communicate directly and explicitly transfer information and ideas. This mode of influence fits the classic Norman Rockwell image of neighbors talking over the fence or meeting at the corner store. Politics are probably touched upon only briefly during informal conversations such as these, but the communication of ideas is explicit. The second mechanism involves a diffusion of norms and expectations among peers. Friends and neighbors do not talk about politics as such, but subtle cues of approbation and disapproval of ideas and actions are sensed by individuals. Eventually, residents internalize community norms without resorting to formal educative processes. The two mechanisms are mutually reinforcing and involve learning and/or conditioning by people.

The first mechanism is easier to tackle experimentally because of the direct and explicit transfer of ideas.[34] The researcher simply needs to force communication in the network to comply with a protocol that dictates the content of conversations. This can occur in one of two ways: a) the researcher initiates the communication herself; b) members of the network are recruited to participate in the experiment and agree to abide by the protocol. Both methods have advantages and drawbacks.

In researcher initiated communication members of the social network to be studied are randomly divided into various treatment groups. The researcher then provides the correct treatment to the correct subject and measures the outcome. The 1998 New Haven study conducted by Gerber and Green (2000) offers an excellent example of such a model. Registered voters in New Haven were assigned to receive encouragement to vote via a knock on the door, a phone call, direct mail, or some combination of the above (including a control group that

[34] Experiments testing the gradual diffusion of norms are better off examining constructed social networks.

received nothing). Gerber and Green then checked to see whether voter turnout increased among the various treatment groups.

The chief advantage of this research strategy is that the researcher can control every facet of the experiment. The danger of contaminating the control group or failing to treat the treatment group is greatly diminished. Two related drawbacks stem from this researcher control. Firstly, a researcher initiated and directed intervention may seem artificial and the external validity of the findings may be a concern. Secondly, the communication between subject and researcher is necessarily anonymous. Familiarity is one of the hallmarks of interpersonal communication and very difficult to simulate. Centralized protocols are ideal for measuring the influence of political campaigns, which was the goal of Gerber and Green, but do not capture fully the nature of interpersonal influence. Such research may obliquely inform theories of interpersonal influence, but because centralized experiments do not directly involve the flow of information through social networks, the answers are necessarily unsatisfying.

This concern can be resolved by recruiting members of the social network to participate in an experiment. The model is similar to how Huckfeldt and Sprague ask subjects to provide the names of people with whom they discuss politics. However, where Huckfeldt and Sprague ask subjects questions about *past* interactions with the friends listed (and then interview the friends in the same fashion), the volunteers in the experiment are asked to *initiate* communication with the select set of friends. The list of friends is randomly divided into the various treatment groups and the volunteer is then instructed to execute the protocol faithfully. The type of treatment could vary by topic (e.g., donations to candidates, voting, volunteerism, trimming the lawn, or simply holding an opinion) or by mode of delivery (e.g., face-to-face conversations, phone calls, or e-mail). The important thing is that the researcher not the volunteer determines who is contacted and how. This ensures treatment and control group do not differ in a subtle way.

This experimental design represents an improvement upon the Huckfeldt and Sprague model in two key respects. Firstly, the treatment and control groups are drawn from the same population, so selection bias is not a concern. A person is not selected to receive a particular treatment until after she is listed by the subject as someone with whom politics are discussed. On average, the set of friends who receive the treatment are identical to the set of friends who receive a different treatment (or are in the control group). The effect of the communication can therefore be isolated from unobserved confounding factors. In contrast, Huckfeldt and Sprague have no way of determining whether similarity is caused by conversation or an affinity for similarity.

Secondly, contact in the Huckfeldt and Sprague research design is subject to the vagaries of human memory. There is substantial empirical evidence that people are more likely to recall information that agrees with previously held beliefs. This systematic bias is on top of the processes of forgetting and fabricating events. Thus, the key independent variable of interest has substantial measurement error and yields biased results.[35] Additionally, the standard errors associated with the regression coefficient do not convey the uncertainty associated with the estimate and the naïve reader has no hint of the bias or the uncertainty.

In contrast, measurement error in the assignment to experimental treatment groups rarely occurs. What can happen is that subjects are provided the incorrect treatment (e.g., members of the control group are treated). If the communication in the treatment has an effect, then the

[35] Huckfeldt and Sprague should be commended for taking the time and trouble to interview the friends and neighbors listed to verify information. Diana Mutz (1998) relies entirely upon the recollection of survey respondents. This adds another layer of measurement error. Not only is there no outside verification of the information provided, but Huckfeldt have shown that people are generally good at guessing the partisanship of friends, but tend to err on the side of homogeneity.

misapplication of treatment will lead to underestimates of the magnitude of the effect. Members of the control group who receive the treatment will more closely resemble the treatment group and members of the treatment group who do not receive the treatment will more closely resemble the control group. The standard error associated with the estimate will also be larger to represent this uncertainty, so experiments where the protocol is not executed faithfully yield obviously poor estimates. However, when the protocol is implemented correctly, measurement error is not an issue.[36] Recruiting members of the social network to implement the protocol may appear similar to tracing political conversations through surveys, but the precision and ability to isolate causal connections make it a vastly superior methodology for uncovering influence.

Another problem that can occur is contamination of the control group, or, phrased another way, contagion of the treatment. One of the downsides of working in the field rather than the laboratory is that researcher control is diminished. It is possible for members of the control group to be inadvertently applied the treatment by other subjects in the experiment. Contagion and contamination confound inferences by making the control group resemble the treatment group. The unwitting researcher may presume incorrectly that the difference between the two groups is the application of the full treatment, when in fact the difference is something less.

The problem of contagion can be bypassed in two ways. First, the researcher can simply select a set of subjects who are unlikely to infect one another. The distance need not be spatial; it could be along any dimension, such as temporal. The second solution is to randomly vary the density of the treatment (i.e., the subjects in one set receive a high dosage of the treatment while subjects in another set receive a low dosage). If the treatment is contagious, then the estimated

[36] Thus, the lesson is that one should not trust the results of an experiment where there were severe lapses in the execution of the protocol.

treatment effects will be smaller for populations with a high percentage of subjects assigned to the treatment group than populations with a low percentage of subjects provided the treatment. While this will not eliminate contamination of the control group by the treatment group, it does provide a means of detecting the source of error.

An experiment controlling the flow of information through social networks was conducted during the 2002 Congressional election. The goal of the experiment was to determine the extent to which explicit appeals from friends and coworkers could increase voter turnout. Past field experiments had demonstrated that a brief face-to-face conversation with a stranger could increase vote turnout by 8 to 10 percentage points (Gerber and Green 2000; Green, Gerber, and Nickerson 2003). Given reputation effects and increases in social pressure, blandishments to vote from friends should be more effective than similar appeals from strangers. Laboratory experiments have found this to be the case across a wide range of setting (e.g., Davis and Rusbult 2001; Mackie, Worth, and Asuncion 1990; Brock 1965) and political organizations have acted upon this reasoning (e.g., the AFL-CIO's labor-neighbor program; MoveOn.org's friendship networks; the GOP pilot tested peer-to-peer mobilization in 2004). There is good reason to believe that organizations can harness social networks to change norms of voter turnout.

An alternative hypothesis is that friends sufficiently committed to voting to volunteer time operate in social circles where few people can be mobilized. The majority of the volunteer's friends will already vote habitually, so impossible to motivate. The flip-side of the coin is that serial abstainers situated in social networks where voting is the norm are likely to have a reason for not voting and will be extremely resistant to persuasion. For instance, they may be ineligible or simply detest politics. Thus, selection processes may limit the effectiveness of peer-to-peer efforts to increase voter turnout.

To adjudicate between these two hypotheses an experiment was conducted where volunteers were asked to list friends, neighbors, and coworkers believed to be unlikely to vote in the Congressional election. These listed individuals were then randomly assigned to be contacted by the volunteer about voting, or left alone.[37] After the election, the volunteers reported who they spoke with regarding the election, and official voter files were checked to determine whether subjects assigned to the treatment or control group were more likely to vote.

Volunteers were recruited from two sources. The first source was a 501(c)3 organization named Vote for America that mobilized voters via peer-to-peer networks. The organization had been active during the 2000 Presidential election in Rhode Island and eager to evaluate its model in an area where its volunteers were experienced, well-trained, and received robust organizational oversight and support. In the end, thirty-one volunteers were recruited, who listed 481 friends to be mobilized. One-fourth of each volunteer's list was assigned to the control group and the remaining three-fourths were targeted for mobilization.

The second sample of volunteers was recruited on-line through email appeals. Ultimately, 65 volunteers successfully completed the protocol and lived in regions where voter databases were readily accessible. These 65 volunteers listed a total of 374 friends and neighbors, who were evenly divided into treatment and control conditions. Given the lack of training and support for internet volunteers, one might expect the volunteers recruited over the internet to be less effective than Vote for America participants, so the two experiments will be analyzed separately. Table 2 reports the results for both experiments.

[37] Randomization tests indicated that the treatment and control were extremely similar with regards to observable characteristics. The only noticeable difference is that the control group for the internet volunteer sample was slightly less likely to be registered than the treatment group. However, this difference did not cross traditional thresholds of statistical significance and there is no reason to assume the randomization failed in any sense.

Table 2 Effect of Peer-to-Peer Mobilization

	Internet Volunteers			Vote for America (Rhode Island)		
	Model 1	Model 2	Model 3	Model 1	Model 2	Model 3
Discussion (α)	0.048 (0.064)	-0.022 (0.051)	-0.046 (0.051)	-0.030 (0.048)	-0.038 (0.035)	-0.032 (0.033)
Prior Voter History		Yes	Yes		Yes	Yes
Member of Major Party			0.190** (0.045)			0.275** (0.033)
Captain Dummies	Yes	Yes	Yes	Yes	Yes	Yes
Constant	0.412** (0.149)	0.268* (0.121)	0.219 (0.118)	0.224 (0.205)	0.078 (0.152)	-0.062 (0.143)
N	374	374	374	481	481	481
Adj-Rsq	0.13	0.43	0.47	0.06	0.48	0.55

Dependent variable is voter turnout in the 2002 general election.
Coefficients are derived from two-stage least squares using assignment as an instrument for contact.
Numbers in parentheses represent standard errors.
* implies $p<0.05$ and ** implies $p<0.01$

The influence of conversations about voting is measured by the coefficient for discussion (row 1). Once slight imbalances between treatment and control groups with regards to past voter history and party affiliation are accounted for neither experiment demonstrated any mobilization effect. In fact, individuals who were approached by a friend regarding the election were on average 4.6 percentage points less likely to vote in the internet experiment and 3.2 percentage points less likely to vote in the Vote for America experiment (see row 1, model 3). Pooled together, these experiments estimate the mobilization to be –3.6 percentage points with a standard error of 2.8 percentage points. A demobilization effect from peer-to-peer contact is simply not believable and the negative coefficient is a result of sampling variability. However, the upper bound of the 95% confidence interval for the pooled result is a meager 1.7 percentage points. Thus, the experimental results strongly suggest that peer-to-peer mobilization is less effective than traditional mobilization from a stranger.

Given the consensus in the literature on the strength of personal ties, this null finding is surprising. Two explanations for the ineffectiveness of peers at mobilizing voters immediately suggest themselves. The first is that selection into homogenous social networks outweighs the importance of reputation and persuasion. Many people declined to participate in the experiment because "everyone I know votes." For such individuals, contacting members of their social circles will do little to increase turnout because everyone is planning to vote already. Participating volunteers were instructed to target people who they thought might not vote. It might be possible that individuals operating in social circles where voting is the norm but regularly abstain cannot be mobilized.

A second explanation is that friends and neighbors actually are less effective at mobilizing voters. Perhaps developed trust between two individuals has narrow bounds and that requests outside of these boundaries have little influence. More likely, the knock on a door from a friend to asking civic participation seemed a little artificial. Why bring up voting after many years? The appeal for turnout may have even been deemed insulting. If this explanation is true, then it is possible that norms are transferred not through direct conversations, but rather through subtext and social expectations. The study is obviously small, but extremely suggestive of the shape and form of interpersonal influence.

Exogenous Shocks to a Network

It is not always possible to manipulate the communication within a network. The ubiquity and contingency of interactions within a social network often make it impossible to control sufficiently and study directly. An alternative strategy allows the researcher to place the complicated system of communication in a black box and simply watch the influence diffuse through the network. The third method of studying interpersonal influence experimentally accomplishes this by introducing an exogenous shock and watching it process through the system.

Diffusion protocols offer a straightforward design, but attention to detail is required to avoid selection bias. The key step in the design and analysis is to establish baseline rates for the behavior or belief in question. In order to measure change, the researcher needs to know what the subject is different from.[38] There are two primary methods of establishing a reference group.

The more traditional method involves taking a measure of the network prior to the introduction of the exogenous shock (using a survey or whatever tool is appropriate). The results taken after the intervention by the researcher are then compared to this baseline. This method of analysis is perfectly analogous to discussion of panel data above. Panel surveys are not randomized experiments per se, but offer a scientific mode of analysis in this setting. The researcher is actively providing a treatment rather than passively measuring the effect of interactions that have already taken place. The social network is initially measured at equilibrium and the researcher introduces a disturbance and then watches the ripples through the system. The recorded pattern of diffusion then can be replicated by other researchers at different times, places, and concerning different topics. This model of analysis is akin to a physicist dropping a rock in a pool and attempting to model the waves.[39]

The second model involves using a placebo as the reference group. Two parallel exogenous interventions are made and they each serve as the other's point of reference. The treatment and the placebo need to meet two stringent requirements. Firstly, the two interventions must not lead to the same causal outcome. If the treatment of interest is expected to improve political knowledge, then the placebo should be unrelated – even indirectly – to that area. The

[38] This point may sound pedantic and simplistic, but it is often ignored.
[39] A fabulous example of this protocol was provided by Dodd (195X). One in five people in a town of 950 people were told a six word slogan for a coffee company and were told that anyone able to repeat the slogan in two days would receive a free pound of coffee. The next day a plane dropped thirty thousand leaflets over the town. The leaflets stated that one in five people in town knew a six word slogan and anyone able to repeat the slogan would receive a free pound of coffee. The day after the massive literature drop, researchers knocked on every door in the town to see who knew the slogan (and hand out the coffee). Dodd conducted similar experiments in other cities and using different modes of mass communication (see Dodd 1952, 195X).

second requirement is that the two treatments must travel through the network and roughly the same rate. A placebo that does not travel far from the point of its introduction would make a poor comparison for a treatment that spreads through a network quickly. Selecting two treatments that meet both requirements can be tricky. For instance, encouraging families to recycle paper is not a very good placebo for a treatment encouraging families to recycle aluminum because the two behaviors are likely to be linked. A rumor concerning police brutality is likely to spread through a neighborhood more quickly than a piece of information on the mayor's electoral platform. The extent to which a design meets these two criteria is an empirical matter and difficult to gauge prior to conducting the study.

There is no reason why the two protocol designs cannot be combined. The panel framework has the advantage of mapping the network prior to the intervention so the system is more contained and easier to trace. The placebo design has the advantage of controlling for all temporal effects since both interventions take place at the same time. The strengths of each design cover the weaknesses of the other, so the dual design is optimal. The added complexity makes the execution of the protocol more difficult, but that is the only downside.

The exogenous shock design is versatile and can be constructed to address a range of topics. The diffusion of information, the contagion of behaviors, and the passage of opinions through influence would all be designed and analyzed the same way. The topics about which a subject would be informed or persuaded or indoctrinated are also flexible.

Given the straightforward nature of the experimental design and the ready applicability to most facets of interpersonal influence, it is surprising that numerous studies have not already been conducted using this model. However, most of the explicitly political analysis relies on naturally occurring interventions and do not offer a control. Relying on a natural intervention as widespread as candidate information during an election is fraught with problems. The first is that all subjects are liable to be treated to some extent. This means there is no control group to use as a reference. A second, and related problem, is that exposure to the treatment is correlated with politically relevant variables. Those individuals who are most interested in politics are the ones most likely to follow the coverage of the election in the media. The third important problem is that because the media blankets the area, it is impossible to know each subject's exposure without error. Measurement error in the variable of interest biases results in unpredictable ways. The problem is compounded when researchers rely upon self-reported media exposure. The net result is that the influence of interpersonal communication cannot possibly be disentangled from the effect of the natural intervention.

An experiment utilizing the exogenous shock design was conducted during the 2002 Congressional Primaries in Denver and Minneapolis. The specific social network considered was very small, households with two registered voters, and the behavior traced was voter turnout. Two voter households in Denver and Minneapolis were randomly selected to receive face-to-face encouragement on either of two subjects: recycling or voting.[40] The mobilization effect of face-to-face canvassing is well established (see Gerber and Green 2000 or Gerber, Green, and Nickerson 2003), so it makes an excellent exogenous shock to introduce into the household. The idea behind the design is that the people who answer the door and receive the Get Out The Vote (GOTV) message will turn out at higher rates than people who answer the door and receive the recycling message. But what then happens to the person in the household who did not answer the door? How much of the boost in turnout from canvassing is passed onto the other person in the household?

[40] Households were also assigned to a control group that received no visit from the campaign. The purpose of the control group was simply to verify that the mobilization effect detected was genuine.

The design is notable in a few respects. Firstly, nothing in the collection of data relies upon responses from the subject. The paid canvasser recorded who answered the door (if anyone) and the county clerk recorded who voted. Any measurement error will be rare random errors in transcription. Secondly, the placebo-controlled design offers a perfectly comparable reference group for measurement. Persons who answered the door are compared only with other persons who answered the door. Similarly, the contagion is sought only among persons whose cohabitants answered the door. The only difference between the groups is the treatment provided.[41] Finally, two voter households are convenient to work with for the experiment, but this analysis would be impossible with non-experimental data. Researchers often ignore selection with regards to city or neighborhood, but there is no avoiding the selection of spouses and roommates. The selection of cohabitants is not an issue in the slightest since the randomization of the treatment occurs only within the set of people who have selected each other. The experiment considers a unique and small social network, but it measures voter contagion very precisely.

In Denver, the campaign contacted 33% of the households in both the GOTV and recycling conditions for a total of 563 households receiving either the treatment or the placebo. In Minneapolis, 45% of the targeted households were successfully contacted, so 394 households received one of the two treatment measures. Among the people who answered the door in Denver, those receiving the voting appeal were 8.6 percentage points more likely to vote than people answering the door and hearing the recycling message. In Minneapolis, the measured direct treatment effect was a similarly robust 10.9 percentage points. Pooled together, the two

[41] The similarity of the treatment and placebo groups on observed characteristics is striking. Every randomization check supports this conclusion.

experiments uncovered a 9.8 percentage point boost in turnout from being exposed to the GOTV message.

Surprisingly, over 60% of this boost in turnout was passed onto the other person in the household. Persons not answering the door in Denver households exposed to the voting appeal were 5.5 percentage points more likely to vote than the comparison group in households assigned to the recycling condition. In Minneapolis, the boost in turnout was 6.4 percentage points. Pooled together, we find registered voters in the household were 6 percentage points more likely to vote even though they were not directly exposed to the voting message.

Table 3 Treatment Effect among Contacted Households

	Denver		Minneapolis		Pooled	
	Direct	Secondary	Direct	Secondary	Direct	Secondary
Percent Voting in GOTV Group	47.7% (3.0)	42.4% (2.9)	27.1% (3.1)	23.6% (3.0)		
Percent Voting in Recycling Group	39.1% (2.9)	36.9% (2.9)	16.2% (2.7)	17.3% (2.7)		
Estimated Treatment Effect	**8.6% (4.2)**	**5.5% (4.1)**	**10.9% (4.1)**	**6.4% (4.1)**	**9.8% (2.9)**	**6.0% (2.9)**
P-Value	0.02	0.09	<0.01	0.06	<0.01	0.02

Numbers in parentheses represent standard errors.
P-values test the one-tailed hypothesis
Pooled estimates are weighted averages of results for both cities.

The magnitude of the effect is substantial and indicates that voting is a highly contagious behavior. A person who may have been 20% likely to vote in the primary becomes 80% likely to turnout should their housemate vote. Explicit appeals to vote from friends and neighbors may not be effective, but the Denver and Minneapolis experiments suggest that the behavior of spouses and housemates has an extremely large impact on the decision to vote.

Conclusion

Similarities between pairs of people can be the result of interpersonal influence, similar pre-existing dispositions, similar structural incentives, exposure to identical exogenous factors, or a selection process that weeds out dissimilar individuals. As Huckfeldt and Sprague note, "The observational data that would be necessary to disentangle each of these component parts fully and directly are unavailable, and very nearly unimaginable" (Huckfeldt and Sprague 1995, 164). In order to estimate the effect of interpersonal influence, α , scholars using observational data are required to make unverifiable assumptions. In contrast, experiments require minimal assumptions about the process that generated the data or the relationship between the variables. Table 4 summarizes the assumptions made by the analytic strategies discussed in this chapter.

Table 4 Assumptions by Analytic Strategy

Assumptions	Regression	Selection Models	Panel Data	Experiments
No unobserved heterogeneity/selection	X			
Selection process can be modeled		X		
Unobserved heterogeneity is static		X	X	
Intercepts and errors have no relationship to the variable of interest	X	X	X	X

In many settings, the assumptions required to estimate a model may be very reasonable. However, when an experiment can be conducted to answer a question, the experiment will always offer more convincing evidence because of the lack of assumptions. The externally randomized intervention prevents spurious correlations from arising.[42]

Part of the reason is that experiments have not been used more in social science is that the questions asked are necessarily focused. The required control over the factor of interest limits the number of questions a researcher can answer in a given study and increases the difficulty in conducting research. An experimental research agenda does not lend itself to writing the definitive magnum opus on interpersonal influence (or any topic for that matter). However,

[42] Though sampling error is always a possibility.

experiments allow small questions to be answered accurately. By emulating the natural sciences, the researcher is forced to emulate the incremental notion of intellectual progress found in the natural sciences. The three experiments described in this paper are intended to be illustrative of how one might utilize experiments to study social networks. The findings are interesting and informative, but readers anticipating the final word on interpersonal influence will be sorely disappointed.

The tenor of these findings differs greatly from most of the literature on interpersonal influence. A reader could conclude from reading Oliver's *Democracy in Suburbia* (2001) and Huckfeldt and Sprague's *Citizens, Politics and Communication* (1995) that we reside in a world where citizens are constantly buffeted by the attitudes and actions of our neighbors. Every person is an abeyant party switcher, who just happened to move into a particular neighborhood in a particular suburb. In these books, the coefficients are too large because the authors have paid no attention to selection bias, which inflates the estimates. In contrast, the experiments paint a picture of subtle and slow processes. The coefficients are smaller because they are purged of confounding factors. The best predictor of a person's beliefs is what they believed at an earlier time period, but neighbors can mold the specific beliefs.

Three major hurdles face researchers who want to employ experimental methods in the field. The first is compliance with the experimental protocol. Humans are hard to track down in their natural habitats. People move, engage in activities away from their residences, keep odd hours, screen calls, fail to open doors, and generally make themselves difficult to reach. Once the would-be experimenter contacts the intended subject, people are often unwilling to participate in the experiment. Phone calls are ended prematurely and doors closed. People present a number of hurdles that corn, automobiles, and lab rats do not.

The second problem facing experiments in the field is a dearth of proven interventions to utilize. Voter mobilization was utilized for two of the experiments presented here because the value of face-to-face canvassing campaigns has been established in past experiments. How many other activities have demonstrated robust consequences and been validated experimentally? Scholarships increase educational attainment, but the expense and lengthy timeframe make it impractical for most researchers to use as an intervention. It would be fascinating to study the transference of opinion through social networks, but an exogenous shock that alters a person's attitudes is a requirement. The tool capable of consistently shifting a person's behavior has not yet been discovered.

A call for an increased number of experiments without good interventions would be a call for null results. Interpersonal influence might occur, but the poor treatments would be incapable of detecting the underlying process. To prevent such an outcome, experiments must first be conducted to find useful interventions and generate a tool kit. A good place to begin might be laboratory experiments. Researchers in laboratory settings have developed a number of techniques for eliciting responses from subjects and some of them could be portable to the outside world. Until a set of proven interventions, like face-to-face canvassing, is developed, there will be severe limits to the types of questions that experiments can answer about interpersonal influence.

The final hurdle to conducting field experiments is patience and perseverance. Most experiments do not pan out for one reason or another. Several dozen schools were approached before one kindly allowed its students to be surveyed for the experiment. The Vote for America study was intended to involve hundreds of volunteers across three states. However, the organizers in North Carolina and Georgia were just starting operations and were uncomfortable recruiting volunteers to participate in the experiment. Even in Rhode Island many volunteers did not actively target specific individuals, preferring to hand out pledge cards to people during chance encounters. Finding volunteers willing to strictly adhere to the experimental protocol was not easy, and ultimately the experiment was less than 5% of the initially planned size.

The point is that anything can happen in the field and researchers wanting to employ experiments as a methodology would be well advised to adopt a philosophical view of failures. Despite these potential problems, experiments represent the ideal research strategy for avoiding the problems of selection bias and unobserved heterogeneity encountered in the study of interpersonal influence. It is the hope of the author that these modest examples included in this paper will spur other scholars to pursue more ambitious experiments in the field.

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Appendix Roommate Survey

1. Please circle the category that best describes **YOURSELF**:

A						
Strong Republican	Republican	Lean Republican	Independent	Lean Democrat	Democrat	Strong Democrat

B						
Extremely Liberal	Liberal	Slightly Liberal	Moderate	Slightly Conservative	Conservative	Extremely Conservative

2. Please indicate how favorably you feel towards each of the following people, issues, and groups.

	Very Favorable	Favorable	Slightly Favorable	Neutral / Indifferent	Slightly Negative	Negative	Very Negative
Republicans	1	2	3	4	5	6	7
Democrats	1	2	3	4	5	6	7
George W. Bush	1	2	3	4	5	6	7
Hillary Clinton	1	2	3	4	5	6	7
Labor Unions	1	2	3	4	5	6	7
Affirmative Action	1	2	3	4	5	6	7
CEOs	1	2	3	4	5	6	7
Homosexuals	1	2	3	4	5	6	7
Immigration	1	2	3	4	5	6	7
Microsoft	1	2	3	4	5	6	7
Pot Smokers	1	2	3	4	5	6	7
Christians	1	2	3	4	5	6	7
Racial Profiling	1	2	3	4	5	6	7

3. Please indicate whether you strongly agree (1), somewhat agree (2), neither agree nor disagree (3), somewhat disagree (4), or strongly disagree (5) with the following statements.

	Strongly Agree	Somewhat Agree	Neither Agree nor Disagree	Somewhat Disagree	Strongly Disagree
I often discuss politics with friends.	1	2	3	4	5
Taxes are too high in the U.S.	1	2	3	4	5
Marijuana should be legalized.	1	2	3	4	5
Racial diversity is an important goal in college admissions.	1	2	3	4	5
I am not interested in politics.	1	2	3	4	5
Some liberties should be curtailed for security.	1	2	3	4	5
The drinking age should be kept at 21.	1	2	3	4	5
Homosexuals should not be allowed to teach elementary school.	1	2	3	4	5
Redistributing wealth and income is a proper role for the federal government.	1	2	3	4	5
Men should bear equal responsibility for child rearing in a family.	1	2	3	4	5

4. How many days a week (0-7) do you do the following?

A. Read a newspaper? __________

B. Watch the news on TV? ____________

C. Listen to news on the radio? _____________