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ESTIMATING THE CAUSAL EFFECT OF SOCIAL CAPITAL: A Review of Recent Research

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■ **Abstract** Although there is a large literature on social capital, empirical estimates of the effect of social capital may be biased because of social homophily, the tendency of similar people to become friends with each other. Despite the methodological difficulties, a recent literature has emerged across several different disciplines that tries to estimate the causal effect of social capital. This paper reviews this recent empirical literature on social capital, paying close attention to the statistical and theoretical assumptions involved. Overall, there is evidence that genuine progress has been made in estimating the effect of social capital. The reviewed articles should provide useful examples for future research.

INTRODUCTION

The purpose of this review is to examine recent attempts to estimate the causal effect of social capital. As a number of recent authors have pointed out, there is a considerable amount of ambiguity regarding the precise definition of social capital (Portes 1998, Fischer 2005, Manski 2000, Kadushin 2004). The literature that I review focuses on what we might call "network" social capital, i.e., the effect of characteristics of friends, acquaintances, or groups on individual outcomes. Portes (1998) provides a useful definition of this form of social capital as "the ability of actors to secure benefits by virtue of their membership in social networks or other social structures," stressing that whereas "economic capital is in people's bank accounts and human capital is inside their heads, social capital inheres in the structure of their relationships" (p. 7). The key concept here is that social capital is not an individual characteristic or a personality trait but a resource that resides in the networks and groups to which people belong.

The focus on network social capital in this review is not intended to dismiss alternative definitions of social capital that stress the importance either of norms and values or of group-level outcomes. Fukuyama (1999, p. 16), for instance, provides a coherent definition of social capital that emphasizes the role of norms on group outcomes. Instead, the reason I focus on network social capital is because, as discussed below, the problem of causality for network social capital is particularly clear, and it taps into an existing literature on the role of endogenous friendship formation and self-selection in the estimation of peer effects. In addition, as reviewed here, there is a growing body of research on network social capital and peer effects that takes the problem of endogeneity seriously and, I believe, makes progress in estimating the true effect of social capital.

The papers covered in this review indicate that genuine progress can be made in estimating the causal effect of social capital. Although many of the papers refer to the estimation of peer effects, social interactions, or some form of neighborhood effects without explicit reference to social capital, most of these would be subsumed under the definition of social capital by Portes (1998) referred to above. All, however, are useful because they present creative ways of dealing with the question of causality under endogenous social interaction and can serve as useful examples for future research on social capital.

A fundamental challenge in the estimation of the effect of social capital is posed by the central fact that individuals choose who they want to be friends with and what groups they want to join. Why does this matter? The principle of social homophily argues that people tend to choose others who are similar to them as friends. As a result, it is quite possible that much of the estimated effect of social capital simply reflects selection effects based on the myriad of nonrandom ways in which people become friends.

Of course, estimating a causal effect is difficult, if not impossible, with the kind of nonexperimental data that most social scientists work with in general, so this sort of problem is endemic in the social sciences. Given the general nature of this problem, why should we be concerned about the causality of social capital in particular? Three reasons: First, as mentioned above, social homophily suggests a competing explanation of the social capital findings. Second, it is time; in the past decade there has been an enormous outpouring of empirical studies on the effect of social capital on a variety of outcomes. At this point in the maturation of the social capital literature, it is worthwhile to ask whether we can adjudicate between alternative theories that are also consistent with much of the same evidence. As it stands, much of the literature resorts to asserting by fiat, or by faith, that estimates of the effect of social capital are correct by ignoring or minimizing the potential problems posed by nonrandom friendship formation. Third, the very name social capital invites a comparison to the human capital literature. Although some scholars have argued that the term social capital is counterproductive because social capital is not really capital at all (Fischer 2005), the popularity of the term is likely due, at least in part, to the equivalence that it suggests with other forms of capital.

Posing questions about causality should not be construed as picking on social capital unfairly, as the same questions have been raised regarding human capital. Just as one can argue that estimates of the effect of social capital are biased because of social homophily, one can argue that estimates of the effect of human capital on wages are biased because of unobserved factors that affect schooling and labor market productivity (e.g., if smarter workers earn more and find it easier to succeed in school, then the correlation between education and earnings will partly reflect higher pay that is due to higher natural ability). Economists have taken this question seriously; a recent review of the causal effect of education on earnings has a bibliography listing several pages of attempts to estimate the true effect of human capital (Card 1999; see also Card 2001).

By arguing that social capital needs to confront the problem posed by friendship choice, I am not arguing that concerns about causality should induce research paralysis, where one avoids tackling any subject that goes beyond a simple descriptive analysis. At the same time, I am also not advocating turning to complicated statistical models, such as structural equations, as a solution for the lack of experimental data. As we shall see, these models make strong assumptions that cannot be tested, and they may provide misleading estimates. Many of the most interesting studies reviewed below use fairly simple methods combined with innovative use of quasi-experimental data. Researchers should be careful with the statistical assumptions they make in estimating the effect of social capital, and these assumptions should be made transparent to the reader, so he or she can decide the validity of the technique.

In the next section, I provide a brief overview of why nonrandom friendship acquisition may bias social capital models before I turn to a review of the recent literature.

THE BASIC PROBLEM

Sobel (1995) and Winship & Sobel (2004) provide useful discussions of causality in the social sciences. Ideally, an experimental approach to causality is preferable, manipulating one variable while holding everything else constant. When this is not possible, natural experiments and quasi-experiments provide an alternative approach. In all the literature discussed below, a careful understanding of the assumptions involved in each modeling approach is critical, as is an understanding of the contingent and restricted nature of the resulting causal claims. Nonetheless, many of the papers discussed represent a substantial improvement in the estimation of the effect of social capital.

A fundamental claim of the social capital literature is that the characteristics and resources of friends, contacts, and groups may affect individual outcomes. For example, a positive correlation between the average characteristics of one's friends and some individual outcome is interpreted as evidence of the effect of social capital. However, because, for the most part, individuals choose their friends and the groups they belong to, some, if not all, of the positive correlation may simply be due to the fact that similar people tend to associate with one another. There is a large literature on the principle of social homophily (see McPherson et al. 2001 for a review), suggesting many dimensions along which individuals with similar characteristics socialize with each other. As a result, despite the strong intuitive appeal of the importance of social capital, skepticism may exert its own appeal, particularly for those inclined to attribute social behavior to individual choice rather than to social interaction (for example, Manski 1993, pp. 531–32).

Clear evidence on the role of homophily comes from longitudinal research on adolescent friendships. Kandel (1978) and Cohen (1977) use longitudinal data on adolescent friendships to show that much of the similarity in attitudes and behavior among friends or within cliques is due to friendship choice rather than to peer effects. Kandel (1978), for instance, studies the change in similarity in best friends' attitudes and behavior at the beginning and end of the school year and finds that about half of the increase in similarity is due to adolescents changing friends during the course of the year rather than adapting their behavior/attitudes to resemble their initial friend. Cohen (1977, p. 237) concludes that most of the similarity in adolescents' attitudes is due to initial homophilic selection.

Explicit depiction of the estimation problem posed by individual choice extends back to Blalock (1984) in sociology. Writing about contextual effects—the effect of aggregate group variables on individual outcomes—Blalock notes that "it is indeed possible that persons will select their contexts on the basis of their own preferences" making the direction of causation difficult to determine (Blalock 1984, p. 369). Blalock stresses the importance of careful theoretical consideration to account for the relationship between individual- and group-level variables. In the case of social capital, this points to the importance of a detailed understanding of how individuals choose their friends as a precursor to estimating empirical models on the effect of friends' characteristics with nonexperimental data. In a study of the effect of peer influences on occupational and educational aspirations, Duncan et al. (1968) recognize the problem posed by the self-selection of friends by arguing that "if assortment on the basis of aspirations proves to be important, our estimates of the mutual influence of friends on each other's aspirations are not merely wrong; they become irrelevant" (p. 135). As discussed by Friedkin (1990), the flip side of a concern about the role of unmeasured individual characteristics on social capital effects is the impact that unmeasured network factors may have on estimates of the effect of individual variables. Although he largely assumes that the networks of friends and peers are exogenous, he also considers the implications of the networks themselves being endogenous to individual characteristics and preferences (Friedkin 1990, Figure 5, p. 325).[1]

Moffitt (2001), Manski (1993, 2000), Durlauf (2002), and Durlauf & Fafchamps (2004) all depict the difficulties of estimating social capital models under the presence of friendship, group, or neighborhood choice. Moffitt (2001) presents a particularly lucid account. He points out that the estimation problems stem from two distinct sources: simultaneous effects, in which peer or group effects are overestimated because the outcomes of friends or group members simultaneously

[1]Doreian (2001) provides a general treatment of the question of causality in the structure of social networks. Although beyond the scope of this review, this general treatment would be useful for anyone who wanted to try explicitly to model the process of friendship formation with social network data.

effect each other, and correlated unobservables, in which friendship is related to unobserved factors that also affect the dependent variable. Here, I present a simple model that clarifies the basic issues.

Equation 1 depicts a preference for friendship homophily along two fixed characteristics, x_i (observed by the researcher) and w_i (unobserved). Homophily preferences, i.e., the desire to associate with people who are similar to you, are expressed (for convenience) by taking the absolute value of the difference between potential friends' characteristics,

$$I_{ijt} = -\beta_1|x_i - x_j| - \beta_2|w_i - w_j| + \varepsilon_{ijt}, \qquad 1.$$

where ε_{ijt} is an error term and I_{ijt} represents the preference that individual i has to choose j as a friend at time t, with higher values indicating more desire to be friends.[2] In Equation 1, individuals who are similar in terms of x and w are more likely to become friends. Equation 2 depicts the outcome Y as a function of individual characteristics and the average outcomes of one's friends, contacts, or fellow group members (see Blalock 1984, p. 363, for a discussion of this model),

$$Y_{it} = \alpha_1\bar{Y}_{-it} + \alpha_2\bar{x}_{-it} + \alpha_3 x_i + \alpha_4 w_i + \varphi_{it}, \qquad 2.$$

where both $\bar{Y}_{-it}$ (the average outcome of i's friends/group at time t) and $\bar{x}_{-it}$ (the average x of i's friends/group at time t) are possible measures of social capital. Because w_i is unobserved to the researcher, what he/she ends up estimating is depicted in Equation 3:

$$Y_{it} = a_1\bar{Y}_{-it} + a_2\bar{x}_{-it} + a_3 x_i + \phi_{it}. \qquad 3.$$

The difficulties in getting accurate results by estimating Equation 3 can be depicted as follows. One must first address the simultaneity or reflection problem (see Manski 1993; Moffitt 2001, p. 54). If there is a social capital effect via $\bar{Y}_{-it}$, then it is difficult to get an accurate estimate of α_1 because all the Y_i s are determined simultaneously. As Manski puts it, "the problem is similar to that of interpreting the almost simultaneous movements of a person and his reflection in a mirror. . . . [D]oes the mirror image cause the person's movements or reflect them?" (p. 129). With cross-sectional data, one must look for a variable to serve as an instrument for Y, i.e., something that is correlated with the Ys of your friends but not your own Y. With longitudinal data, it is possible to use the lagged outcomes of your friends as an instrument for their current outcomes.[3]

The problem of correlated unobservables occurs when there is an unobserved factor that is correlated with friendship (or group) choice and with the outcome variable. In Equation 3, the unobserved factor w_i will bias the estimate of a_1

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[2]The negative signs in front of β_1 and β_2 ensure that individuals who are similar (i.e., smaller absolute differences in x and w) are more likely to become friends.

[3]If one uses a first difference model (see Halaby 2004), the first difference of the one-period lagged outcome of your friends will be correlated, by definition, with the first difference of the error term.

(the coefficient on $\bar{Y}_{-it}$) if w_i is correlated with Y_{it} (i.e., $\alpha_4 \neq 0$) and I_{ij} ($\beta_2 \neq 0$). Similarly, the estimate of a_2 (the coefficient on $\bar{x}_{-it}$) will be biased if w_i is correlated with x_i and $\alpha_4 \neq 0$.

How much might this matter? Table 1 presents an example of how much bias could result. In Table 1, I present results from simulated data for an imaginary school of 5000 individuals.[4] The results presented in Table 1 are merely suggestive, as they start from (arbitrary) known parameters and then generate the data to be analyzed. In this simulation, x_i and w_i are jointly normally distributed with a correlation of 0.25. In Equation 1, the values of β_1 and β_2 are set to 1. In Equation 2, $\alpha_1 = 0$, i.e., no effect of $\bar{Y}_{-i}$, and $\alpha_2 = \alpha_3 = \alpha_4 = 1$.[5] As discussed above, the estimates of the coefficients for observed independent variables will be biased because of a correlated unobservable, w_i. The second column of Table 1 shows the true values of the coefficients on the independent variables. Based on the results of Equation 1, each simulated student selected 10 classmates as friends with the highest values of I_{ij}. The average values of Y and x for these 10 friends are used to calculate $\bar{Y}_{-i}$ and $\bar{x}_{-i}$ for Equation 2.

Model 1 of Table 1 shows the ordinary least square (OLS) results of estimating Equation 3 for this data, where $\bar{Y}_{-i}$ and $\bar{x}_{-i}$ are the average Y and x values for each student's 10 friends. Including $\bar{Y}_{-i}$ in the model results in a statistically significant effect, even though the true value for the coefficient on $\bar{Y}_{-i}$ is zero. In addition, the coefficient on $\bar{x}_{-i}$ is downwardly biased. In Model 2, only the effects of x_i and $\bar{x}_{-i}$ are estimated, and again the social capital effect is badly biased (1.702 versus 1.0 for the true value). The point here is not to argue that these results are definitive, but merely to suggest that even in a very simple model, traditional OLS estimates of social capital effects may be very misleading.

A REVIEW OF THE EMPIRICAL EVIDENCE ON CAUSAL EFFECTS, BY METHOD

Table 2 provides an overview of the studies reviewed in this paper grouped by the different method used. I have grouped the papers by method rather than by substantive topic to provide a coherent critique of each methodological approach. The methods discussed here are fixed effects models, instrumental variables (IVs), structural equations, randomly assigned roommates, and quasi-experimental designs. In each of the sections that follow, I provide a brief discussion of the general methodological approach, followed by a detailed review of each of the different papers. None of these methodological approaches is new or unique in their application to social capital models. However, as depicted in Table 2, each of the

[4]The data was simulated using Stata. The computer files used in the simulation are available on the author's website, http://www.tedmouw.org, for readers who wish to see the effect of changing some of the parameters.

[5]In both equations, the error term is normally distributed with a variance of 3.

TABLE 1 Monte-Carlo estimates of social capital with correlated unobservables[a]

		OLS		Fixed Effects	Cross-Lagged Regression	
		Model 1	**Model 2**	**Model 3**	**Model 4**	**Model 5**
Variable	True values[b]	Y_i	Y_i	ΔY_i	Y_i	Y_i
x_i	1.0	0.920	0.859		0.774	1.402
		(0.096)**	(0.098)**		(0.094)**	(0.097)**
Social capital variables:						
$\bar{Y}_{-i}$	0	0.485				
		(0.039)**				
$\bar{x}_{-i}$	1.0	0.699	1.702	1.080	1.546	
		(0.193)**	(0.178)**	(0.171)**	(0.168)**	
$\bar{x}_{-i,t-1}$	N.A.[c]					0.262
						(0.177)
$\bar{Y}_{i,t-1}$	0				0.120	0.127
					(0.014)**	(0.014)**
Constant	0	0.031	0.058	–0.077	–0.026	–0.028
		(0.045)	(0.045)	(0.060)	(0.044)	(0.045)
Obs		5000	5000	5000	5000	5000
R-squared		0.29	0.27	0.01	0.30	0.29

[a]Standard errors in parentheses significant at 5% level; double asterisk (**) indicates significant at 1% level; all the computer files for this analysis are available on the author's web page, http://www.tedmouw.org, if readers wish to repeat the analysis with different parameters.
[b]See Equation 2 in the text.
[c]N.A., not applicable, no direct effect in Equation 2.

different approaches makes certain assumptions, and the goal of this review is to focus attention on the relative advantages and disadvantages of making these assumptions in estimating the true effect of social capital.

Fixed Effects Models

If the bias in the social capital model is caused by fixed, unobserved characteristics, such as w_i in Equations 1 and 2, then a "first difference" or fixed effects model using longitudinal data can provide an unbiased estimate. Halaby (2004) provides a clear discussion of the benefits of fixed effects models vis-à-vis other estimation strategies such as random effects and lagged dependent variables for longitudinal data. A first difference model estimates a regression of changes in the dependent variable on changes in the independent variables for two time points, whereas a fixed effects model uses each variable's difference from its within-individual mean. Conveniently, time-invariant variables, including unobserved ones, drop out of the model. For example, Equation 2 becomes

$$Y_{it} - \bar{Y}_i = \alpha_1 \left(\bar{Y}_{-it} - \bar{Y}_{-i}\right) + \alpha_2 \left(\bar{x}_{-it} - \bar{x}_i\right) + \vartheta_{it}, \quad 4.$$

TABLE 2 Overview of studies reviewed in the paper

Method/"trick" used to get around the problem of endogeneity	Primary assumption needed to estimate the "causal" effect of social capital	Examples, by topic:	Overall evaluation
A. Longitudinal and/or fixed effects models	Assume that unobserved factors do not change.	Peer effects on education: Hanushek et al. (2003), Arcidiacono & Nicholson (2005), Angrist & Lang (2002), Hoxby (2000) Peer effects on delinquency: Kandel (1978), Cohen (1977), Rose et al. (1999) [sibling fixed effects], Gordon et al. (2004), Bayer et al. (2003) Migration: Palloni et al. (2001) Labor market: Mouw (2002), Yakubovich (2005)	A reasonable approach. Mixed results for peer effects in education, but the size of coefficients is modest. Overall, the decline in the size in the coefficient points to substantial effects of homophily, but also evidence of peer or social capital effects.
B. Instrumental variables (IVs), structural equation models, cross-lagged models	Assume that the error term is not correlated with the instrument (These models require strong assumptions; see text)	Peer effects on delinquency: Aseltine (1995), Evans et al. (1992), Schulenberg et al. (1999), Matsueda & Anderson (1998)	Evaluation depends on whether one believes the assumption that the IV is a good one. This assumption rests on theoretical grounds. The reader should be cautious of models in which the underlying assumptions are not clearly discussed.

C. “Exogenous” IVs	Same as above, but (arguably) more believable. This distinction (between B and C) is based on theoretical grounds; see text.	Migration: Munshi (2003) (Instrument: rainfall) Labor market: Bayer et al. (2004) (Instrument: census tract of residence)	The same assumptions as B apply, but in these two papers the choice of IVs may be better, suggesting evidence of social capital effects.
D. Randomly assigned roommates	Assume that like/dislike of roommate does not affect the roommate correlation among outcomes.	Peer effects on education: Sacerdote (2001), Zimmerman (2003), Stinebrickner & Stinebrickner (2004), McEwan & Soderberg (2005) Peer effects on delinquency: Kremer & Levy (2003), Duncan et al. (2005) Labor market: Marmarosa & Sacerdote (2002)	Good methodological approach, but null or modest effects of social capital.
E. Quasi-experimental evidence, (e.g., randomized housing studies)	Assume that you are measuring a true change in social capital due to residential mobility.	Randomized housing studies and social capital: Pettit & McLanahan (2003), Sanbonmatsu et al. (2006), Ludwig et al. (2005), Kling et al. (2005)	This approach does not provide direct evidence on social capital effects, as moving neighborhoods does not imply an exogenous change in social capital. Null or modest effect of changing neighborhood characteristics on individual outcomes.

where $\bar{Y}_{-i}$ and $\bar{x}_{-i}$ are mean values of $\bar{Y}_{-it}$ and $\bar{x}_{-it}$. In Equation 4, changes in social capital over time, i.e., in $\bar{Y}_{-it}$ and $\bar{x}_{-it}$, are caused by fluctuations in the composition of one's friends that are due to the error term in Equation 1, which is uncorrelated with the fixed, unobserved factor, w_i. A key advantage of fixed effects is that they allow the fixed, unobserved factor to be correlated with the independent variables, whereas random effects assume independence (Halaby 2004; Gordon et al. 2004, p. 65). The principal assumption of fixed effects models is that the fixed effects do not change over time.

Model 3 of Table 1 illustrates the accuracy of a fixed effects estimation of Equation 4, provided these assumptions hold. The data were simulated in the same way as for Models 1 and 2, but for two time periods. Although x_i and w_i are fixed over time, the error term in Equation 1 results in change in friendship rankings over time, and hence in variation in $\bar{Y}_{-it}$ and $\bar{x}_{-it}$. Model 3 shows that the fixed effects model provides a good estimate of the effect of $\bar{x}_{-it}$, as the fixed effects eliminate the fixed, unobserved effect, w_i.

PEER EFFECTS ON EDUCATION The estimate of peer effects is problematic because a positive correlation between students' test scores and grade point average (GPA) could be caused by the sorting of students across schools that is based on factors unobserved by the researcher. Given the high degree of sorting by race and class in schools in the United States, any cross-sectional estimate of peer effects is likely to be upwardly biased. One way to tackle the problem of endogeneity in estimating peer effects is to use longitudinal data on individual students and schools to study how changes in the composition of schools and classes affects changes in individual students' test scores. The central problem is convincing the reader that the unobserved factors are time invariant, i.e., that changes in the composition of peers are not simply a reflection of the changing school selectivity. Hoxby (2000), Hanushek et al. (2003), Arcidiacono & Nicholson (2005), and Angrist & Lang (2002) use fixed effects models to control for fixed, unobserved factors at the school and/or individual level.

Hoxby (2000) uses a rich data set from the Texas Schools Microdata Sample that allows her to follow individual students in more than 3000 schools from 1990–1999. She argues that idiosyncratic changes in the gender and race composition of classrooms—differences by cohorts of students within the same school—reflect random, exogenous variation in peer composition, as parents would be unlikely to move in or out of a school district because of random, year-to-year perturbations in the demographic composition of a classroom. Provided that this assumption is true, these small variations in class composition can be used to estimate peer effects. Hoxby finds significant peer effects based on these essentially random fluctuations in classroom demographic composition. For example, a random 10% increase in the proportion of females in the class increases third grade girls' reading scores by 0.037 points and boys' reading scores by 0.047 points, with similar effects for math scores. She notes that this effect may be due either to the fact that girls tend to score higher on both tests at this age in Texas (hence, an increase in the percentage female generates a peer effect by increasing the average test scores of students' classmates) or to the effect that changing the gender composition of the classroom has on classroom behavior.

Also using the longitudinal Texas schools data, Hanushek et al. (2003) estimate a model of the effect of average classmates' test scores on the change in individual test scores. They use individual, school, and school-by-grade fixed effects. Much of the year-to-year fluctuation in their data is driven by annual mobility rates of students across schools of about 20%. They use classmates' test scores from two years before to mitigate what Manski (1993) calls the "reflection problem."[6] Hanushek et al. (2003) find small but significant peer effects in their full fixed effects models. A one-unit increase in the average math score of peers (standard deviation 0.35) results in a 0.15-point increase in the change in the average student's math scores (standard deviation 0.63). It is worth noting, however, that in earlier estimates of changes in math scores without the individual, school, and grade fixed effects, Hanushek et al. find a negative effect of peers' scores on individual scores (–0.07), which runs counter to the basic intuition about peer effects (i.e., that they should be upwardly biased because of sorting).[7]

The Texas microdata that Hoxby (2000) and Hanushek et al. (2003) use are obviously quite remarkable and allow them to estimate models and obtain results that, if taken together, are much more convincing than previous estimates of peer effects on academic achievement. Other research with different data has found different results, however. Angrist & Lang (2002) analyze longitudinal data on Boston's Metco program, which is a voluntary desegregation program that buses (primarily low-income, black) inner-city students to suburban schools. They find that although the schools where the Metco students are placed experience a decline in their average test scores (which has made the program contentious politically), the decline is due entirely to a composition effect. They find no evidence of negative peer effects: The change in the scores of non-Metco students are not correlated with the number of Metco students in the classroom. Arcidiacono & Nicholson (2005) study peer effects on medical students and find no evidence of an effect of average peer MCAT scores on fourth year board scores after they include school fixed effects; they argue that the correlation between students' scores is due entirely to different levels of selectivity of the schools themselves.

Overall, the recent evidence on peer effects using fixed effects and longitudinal data is mixed. In addition, although these fixed effects models are a significant improvement over cross-sectional estimates of peer effects, one must always be cautious of the possibility that changes in unobserved factors such as the selectivity or desirability of the school are driving the estimates of peer effects.

[6]Hanushek et al. (2003) use classmates test scores from two years before to avoid building in a mechanical correlation with the dependent variable, which is the individual's change in test scores over the past year.

[7]One possible explanation for this negative effect is, as Hanushek et al. (2003) note, a ceiling effect on the standardized test scores. Nonetheless, it should make the reader cautious about accepting the overall results.

MIGRATION Social capital, in the form of migrant networks, may provide information and help that facilitates migration. Prima facie evidence on the importance of social networks in migration is given by the high rates of geographic and occupational concentration among different immigrant groups. Hagan (1998), for example, documents the role of migration networks in the growth of an immigrant community in Houston. For a general depiction of the role of networks in migration, see Massey (1993).

Palloni et al. (2001) test for the role of social capital on the probability of migration by using sibling data from the Mexican Migration Project. A high rate of migration within a family or community may represent the effect of social capital on migration, or it may be due to shared unmeasured characteristics. Palloni et al. get around this problem by estimating what amounts to a sibling fixed effects model, comparing the probability of migration of individuals before and after one of their siblings migrates. Because their dependent variable, first migration, is a discrete, nonrepeated event, a fixed effects model similar to Equation 4 is not possible. Instead, the key consideration of Palloni et al. is to allow for a latent variable to pick up unobserved heterogeneity in the hazard of migration. To see why this is important, consider the following example. Imagine a two-period model of migration with two groups of sibling pairs, A and B, in which the underlying probability of migration is higher for group A than for B ($p_A > p_B$). If the group is unobserved by the researcher, then even in the absence of social capital effects, the probability of migrating in period two will be higher if your sibling migrated than if he/she did not simply because relatively more As than Bs will have a sibling migrate in period one (versus having no siblings migrate). Palloni et al. (2001) note that their model fits the data well with only two latent states, representing high and low unobserved migration propensities. Overall, they find that having a sibling migrate substantially increases the hazard of the other sibling migrating. The model is still sensitive to concerns that temporal changes, such as a local recession or drought, will increase the joint probability of migrating, but this is the same basic assumption that all fixed effects models make.

PEER EFFECTS ON DELINQUENCY Recognition of the difficulty of differentiating between peer effects and social homophily in criminology extends back at least to Glueck & Glueck (1950). In an analysis of the friends of 500 delinquent and nondelinquent boys, they found that the majority of the friends of 98.4% of delinquent boys were delinquents, compared with only 7.4% of the friends of nondelinquent boys. However, because the boys in the study were selected from largely the same neighborhoods, and hence had similar opportunities to forge friendships with both delinquents and nondelinquents, Glueck & Glueck argued that their results reflected the tendency that "birds of a feather flock together" and not of a causal effect of peer influence (p. 164). Gottfredson & Hirschi (1987) provide a critique of common methods of controlling for friendship selection in criminology, such as the cross-lagged regression model (p. 597).

Gordon et al. (2004) use longitudinal data on gang membership and delinquent behavior and find strong effects of gang membership on self-reported behavior of adolescents using fixed effects models. Their research indicates that the behavior of gang members cannot be entirely attributed to the self-selection of already delinquent teens to join gangs, as criminal behavior increased when the respondents joined the gang and declined after they left it. Again, the benefit of this approach is that it narrows the parameters of the debate between peer effects and selection effects. The effects Gordon et al. (2004) report could be caused by the idiosyncratic changes in the disposition to commit delinquent acts if, say, some adolescents pass through a period when they are more likely to commit crimes and, as a result, choose to join gangs, or, alternatively, by the effect of gang membership on crime. The only way to go further in resolving the question is to conduct an actual social experiment or to suggest a plausible IV that picks up an exogenous change in gang membership.

Rose et al. (1999) use data with peer and sibling reports of smoking and find an effect of peer smoking on respondents' smoking after controlling for sibling fixed effects using hierarchical linear modeling (HLM). Of course, this does not rule out the possibility that differences in peer smoking levels among siblings are caused by different friendship preferences among the siblings, but it is useful in indicating that the observed peer correlation in smoking is not solely attributable to fixed tendencies to smoke based on shared genetic and/or home environment among siblings. Finally, Bayer et al. (2003) use longitudinal data on 8000 juveniles serving time in different correctional institutions in Florida to study the role of peer effects on recidivism. Using fixed effects for the correctional institution to control for selectivity of assignment to different facilities, they find that the probability of recidivism for specific crimes varies according to the criminal histories of the peers who are serving in the same institution at the same time.

LABOR MARKET SOCIAL CAPITAL Mouw (2003) provides an overview and critique of the literature on the effect of social capital in the labor market. He proposes an indirect test of whether a proposed social capital variable is truly causal or whether it is simply picking up a spurious effect owing to social homophily. He shows that, if all else is equal, workers with more social capital should be more likely to use contacts to find work. His results suggest that oft-used measures of labor market social capital, such as the average education or job prestige of your contacts, have a spurious, rather than causal, effect on wages. This does not necessarily mean that social capital does not matter, but merely that we need to find better measures of it.

Fixed effects models have also been used to assess the role of social networks in the labor market. Mouw (2002) uses longitudinal data to assess racial differences in the effect of using job contacts to find work. He finds that whereas black workers who used contacts have lower wages in cross-sectional models, this is not the case when one follows workers over time; workers who used contacts in one period did not do any better when they did not use contacts to find work. This suggests that it is not the use of contacts per se but rather poorer job opportunities in general (part of which could be due to differences in social capital) that contributed to the racial wage gap. Yakubovich (2005) uses a fixed effects approach in an innovative way to test the "strength of weak ties" hypothesis. He collected data on multiple potential contacts for each worker and found that, net of individual fixed effects, weak ties were more likely than strong ties to have led to a job. Although neither of these papers directly tests the effect of social capital on labor market outcomes overall, they illustrate the potential benefits of a fixed effects approach.

Instrumental Variables and Structural Equations

One solution to the problem of correlated unobservables depicted in Equations 1–3 is to find an IV that is correlated with the independent variable of interest but not with the unobserved factor. In Equation 2, this would entail finding a variable z_i that was correlated with x_i but not with the unobserved factor, w_i. The problem with this approach is that because w_i is unobserved, there is no way to prove that a potential z_i variable is actually a good instrument. Instead, the researcher must rely on theoretical justifications. In many instances, researchers have chosen instruments on weak theoretical grounds. As Moffitt (2001) notes, "thus far the instruments chosen have generally been ad hoc in nature and not based on a strong, or at least explicitly formulated theory" (p. 70). If an instrument is poorly chosen such that it is still correlated with the error term, then the estimates may be as biased, if not more so, as models that do not try to account for selectivity. In addition, the results may depend on the researcher's arbitrary choice among a number of potential instruments, with little theoretical difference justifying the choice.

Evans et al. (1992) study the effect of peer influences on teenage pregnancy using National Longitudinal Survey of Youth (NLSY) data, using the percentage of students in the respondent's school who are classified as disadvantaged as their peer group measure. Preliminary models indicate that the peer group measure is significantly associated with the respondent's probability of teenage pregnancy, but Evans et al. provide a good discussion of why the peer effect may be over-estimated owing to correlated unobservables, and they argue that an IV approach is needed. They note that they need a set of IVs that are correlated with peer group characteristics and are uncorrelated with the error term on teen pregnancy. The instruments that they choose are the metropolitan area unemployment rate, median family income, poverty rate, and the percentage of adults with a college degree. Although they note that these variables are not strong predictors of teenage pregnancy, this does not mean that they are not correlated with the error term. This cannot be proven; it can only be argued theoretically. The IV results show that the peer group effect disappears. However, the reader is left to wonder whether these variables really are good indicators of exogenous changes in peer group characteristics and what sort of biases may result by using them as instruments. As Moffitt (2001) suggests, they seem ad hoc. The benefit of the Evans et al. (1992) paper is that it shows there can be a wide divergence between OLS and IV estimates, even if the IV results are based on assumptions that may be as problematic as the original OLS results.

Structural equation models (SEMs) represent an elaboration of the basic IV approach. SEMs are useful in dealing with problems of measurement error and latent variables with multiple indicators, both of which may apply, obviously, in social capital research. They cannot, however, resolve the debate between social capital and social homophily without making assumptions about the underlying causal structure, assumptions that are ultimately untestable and subject to second-guessing. In this sense, they merely transform the nature of the debate into an argument over the validity of instruments, causal diagrams, or assumptions about the distribution of the error term. I believe a key consideration is that the assumptions that are made in order to identify the model may seem innocuous to a naive reader but could turn out to have profound empirical consequences. As argued above, I believe that it is critically important that the assumptions used to identify the model, i.e., the assumptions behind the use of the IV, be made explicit to the reader. McDonald (1997) shows that even with a simple data set, different, seemingly arbitrary assumptions about the path diagram can lead to substantively different parameter estimates.

With longitudinal data, a frequent approach to the endogeneity problem has been to use "cross-lagged" SEMs to try to estimate reciprocal effects between peer selection and individual behavior. In these models, measures of peer and individual behavior are regressed on lagged measures of both variables. Building on our previous discussion of Equations 1–4, a cross-lagged model would look like this:

$$\bar{x}_{-i,t} = b_1 Y_{i,t-1} + b_2 \bar{x}_{-i,t-1} + b_3 x_i + e_t, \qquad 5.$$

$$Y_{i,t} = c_1 Y_{i,t-1} + c_2 \bar{x}_{-i,t} + c_3 x_i + u_{t,} \qquad 6.$$

where $\bar{x}_{-it}$ is the average value of the x_i of individual i's friends at time t (although x does not change, each individual's friends may change), and $\bar{Y}_{it}$ is the respondent's outcome at time t. The lagged value of Y in Equation 6 is intended to control for unmeasured factors that affect Y, which in our case would be the unobserved variable w_i from Equation 2. However, Halaby (2004, pp. 52–62)[8] shows that a regression on Equation 6 is not a general solution to the problem of unobserved fixed effects (in contrast to fixed effects models), nor does it solve the problem of nonstationary, unobserved effects.[9] In the presence of a fixed effect, such as w_i, the

[8]Note that page numbers for Halaby (2004) refer to a longer, online version of the review, which may be viewed at http://www.ssc.wisc.edu/soc/faculty/pages/Ann_Rev13 total_web.pdf.

[9]In the case of true state dependence, taking first differences of all the variables in Equation 6 will eliminate the fixed effects, but bias is still introduced because the change in the error term is correlated with the change in the lagged dependent variable.

lagged dependent variable will be, by definition, correlated with the composite error term ($\alpha_4 w_i + \varphi_{it}$ from Equation 2). As Halaby (2004) notes, "serially correlated errors in a model with a lagged dependent variable render least squares estimates biased and inconsistent" (p. 57). Rogosa (1980) is critical of the claim of cross-lagged models to measure reciprocal causation, showing that even in a simple two-variable case without measurement error, the cross-lagged model may produce inconsistent results. He argues that cross-lagged SEMs "should be set aside as a dead end" (p. 257).

Models 4 and 5 of Table 1 illustrate what can go wrong with a lagged dependent variable. Model 4 estimates Equation 6 for the simulated data that were used to estimate the fixed effects model in Model 3, whereas Model 5 estimates a modified form of Equation 6 where the current value of $\bar{x}_{-it}$ is used rather than the lagged value. The estimate of the true social capital effect $\bar{x}_{-it}$ is badly biased in both models. Models 4 and 5 show that even in a well-behaved, simulated example, a lagged dependent variable is not a solution for the problem of fixed unobserved effects.

Matsueda & Anderson (1998) and Schulenberg et al. (1999) use cross-lagged models to estimate peer influences on delinquent behavior. Although both papers provide good discussions of the central issue of selection versus influence, inspection of the proposed causal path diagrams reveals that each is making strong assumptions regarding the nonexistence of paths between certain variables in order to identify the model. As with the Evans et al. (1992) paper, these papers are useful because they illustrate the range of estimates that can be found given semiplausible assumptions about the modeling strategy. Aseltine (1995) estimates a SEM of reciprocal effects between peer and respondent delinquency and marijuana use with three-wave panel data, using lagged values as IVs of the contemporaneous measures. His paper is insightful because it gives a detailed account of the assumptions that are necessary to estimate the model, in particular the assumption that there is no direct effect between lagged peer behavior and respondent behavior (i.e., it all operates via current peer behavior). Given Halaby's (2004) argument that lagged variables are not an adequate solution for the problem of unobserved fixed effects, Aseltine's approach may not suffice to take care of the possibility that the relationship between peer and respondent behavior is the result of correlated unobservables, i.e., the w_i in Equation 2. In sum, given that one has to accept the assumption that the path diagram is specified correctly in order to trust the results (i.e., the results of a SEM do not prove that the model is correct), none of these papers is likely to convince skeptics that they have uncovered the "true" effect of peer influence unless they can make a strong case that the assumptions they make are tenable.

"Exogenous" Instrumental Variables

The distinction between this section and the previous one is a matter of degree. In some cases, the use of a particular IV may be a reasonable solution to the endogeneity problem. In this section I discuss two papers in which this approach seems credible.

Munshi (2003) studies the effect of network size on employment among Mexican immigrants using rainfall patterns in origin communities as an instrument for network size at the destination. Munshi uses data from the Mexican Migration Project (MMP) to estimate fixed effects models of unemployment and occupational choice. Although the MMP is a repeated cross-sectional survey conducted in Mexico, it has life history data on respondents' location and employment status. From these data, Munshi constructs person-year longitudinal data indicating whether the respondent is living in Mexico or the United States and whether the respondent worked more than one month during the year (which serves as the measure of employment).

For the agricultural communities in the MMP data, Munshi shows that lower levels of rainfall are empirically associated with higher numbers of migrants in the United States one to three years later. Intuitively, low rainfall would reasonably decrease agricultural wages and make migration to the United States relatively more attractive. As these migrants establish themselves in the United States, they can serve as sources of job information and help for subsequent migrants who arrive from the same origin community. Munshi uses individual fixed effects models to account for the possibility that rainfall affects the selectivity of migration. Migrants who anticipate poor employment prospects in the United States might migrate only in the event of a drought in their origin community, thereby downwardly biasing models that do not include individual fixed effects. In models in which rainfall is used as an IV for the number of established migrants from the origin community in the United States, Munshi finds that predicted network size has a substantial effect on the employment probability of immigrant workers.

The only problem with Munshi's (2003) paper is that the calculated employment rates for the migrants in the United States are very high, about 95%–96%. This may be partly due to the fact that, because of data constraints, Munshi counts as employed anyone who worked at least one month in the United States during the calendar year. As Munshi notes, the high employment rates and the use of fixed effects models mean that his results are being driven by the small percentage of migrants who were unemployed during one year in the United States and employed in another year (generating variation in the dependent variable). Given that the immigrants in the data have come to the United States to work and many will have paid substantial sums of money to cross the border, it is hard to believe that any of them would spend a full year in the United States without working at least one month or that many would return voluntarily without having earned enough to recoup the expenses of crossing the border. Hence, it is possible that the 5% who are unemployed were deported before they could work or are special cases in some other way. In supplemental models, Munshi uses occupation in the most recent trip to the United States as a dependent variable and shows that increasing the network size increases the probability of securing nonagricultural employment, again using rainfall as the instrument for the size of the migrant network.

Although using the same approach in other contexts may be difficult, Munshi's paper is a splendid example of how progress can be made in social capital research by the creative use of existing data. The unique twist to this paper is that it is able to exploit the geographic distance between the origin and destination communities to use weather as an IV.

Bayer et al. (2004) use census data on residential and job location from Boston to study the effect of referral networks on employment. They find that workers from different households who live in the same census block (roughly the equivalent to a city block) are 50% more likely to work in the same location than are workers who live in the same block group but different blocks. Because there are, on average, ten census blocks in each block group in their data, they argue that this result is due to the sharing of job information among people who live in the same geographic area. They use block-group fixed effects to account for the possibility that workers select their neighborhood based on unobserved factors that are also correlated with where they work. The underlying assumption is that the selectivity operates at the block-group level but not at the block level within block groups. Another possible interpretation of their results is that they are due to reverse causality—the tendency of coworkers to share house or apartment information. They try to account for this possibility by restricting some models to workers who have lived in the neighborhood for at least 2 years and who worked less than 40 weeks during the previous year as a proxy for a spell of unemployment preceding a new job. Although census data are not ideal for picking apart temporal order, the results seem to support their basic argument.

Randomly Assigned Roommates

Recently, an important literature has developed that uses randomly assigned college roommates as a natural experiment to estimate the effect of social capital. Although many colleges assign roommates based on answers to a list of preferences (i.e., smoking/nonsmoking), provided the researcher knows the answers to these questions, the assignment is essentially random on unobserved factors.

Sacerdote (2001), Zimmerman (2003), McEwan & Soderberg (2005), and Stinebrickner & Stinebrickner (2004) (hereafter S&S) estimate models of the effect of (randomly assigned) roommate's precollege academic achievement on respondent's college GPA. The striking result is that there is no evidence of a linear effect of roommate quality on college achievement. Although Sacerdote (2001) has been cited as evidence of a positive finding of peer effects because he finds a positive correlation among freshman GPAs, he notes that this cannot be treated as evidence of a causal peer effect (p. 694) because of the reflection problem and because it might indicate mutually experienced events. A better estimate is the relationship between roommate high school academic achievement and freshman GPA (because high school achievement is obtained prior to the common experience as roommates). The coefficient of roommates' high school test scores on freshman GPA is found to be –.001 (s.e. 0.001) (Sacerdote 2001, model 1, table 3).

Zimmerman (2003), S&S, and McEwan & Soderberg (2005) all find similar results.[10] As S&S discuss, there is slight evidence of a nonlinear effect in both Sacerdote (2001) and Zimmerman (2003), but the results are not consistent across the different articles.[11]

What should we make of these findings? Randomly assigned roommates are, arguably, the cleanest estimates we will get of social capital effects because they avoid the question of friendship choice that complicates the rest of the literature. It could be, however, that the effect is simply too small to show up; hence, the null findings would not indicate that social capital does not matter, but merely that these models are not picking it up. In addition, S&S note that the amount of time you spend with your roommate depends on how well you get along, and hence potential negative effects of bad study habits and/or poor academic preparation are mitigated by simply avoiding your roommate.[12] S&S suggest that more attention should be paid to the concrete ways in which roommate effects might operate, arguing that study habits rather than achievement test scores are where a causal effect might lie. In their study of roommates at Berea College, S&S find that although achievement scores have no effect, there is evidence of a positive effect of roommates' high school GPA on freshman GPA for females (but not for males), and they claim that this represents the benefit that students with poor study skills may obtain by observing roommates with good study skills.

The use of roommate data revolutionizes the estimation of social capital effects with quasi-experimental data. Clearly, it is a special situation because the random assignment of roommates is undertaken by colleges for other reasons; nonetheless, this is precisely the kind of creative use of data that lends hope to the idea of estimating causal effects of social capital. At the moment, the lack of a clear finding of peer effects in these studies should give the researcher pause even if he/she is not working in the social capital and education literature.

Marmarosa & Sacerdote (2002) apply the randomly assigned roommate technique to labor market outcomes. Using data on randomly assigned freshman year roommates at Dartmouth and information on respondents' first job after college, they find no correlation between roommates in the probability of receiving a high-paying job. There is, however, an effect of freshman year hallmates' average salary

[10]The corresponding estimates of the linear effect of high school achievement index on freshman grades are .007 (s.e. .009) for SAT scores in McEwan & Soderberg (2005), .007 (s.e. .006) using SAT scores in Zimmerman (2003), and .0009 (for males, s.e. .0052) to .0015 (for females, s.e. .0044) using ACT scores in S&S.

[11]In a related approach, Kang (2005) studies the peer effects on education using the random assignment of students to classrooms within middle schools in Korea. Kang finds that a 1 standard deviation in peer math scores is associated with a 0.27 standard deviation increase in students' math scores, controlling for school fixed effects.

[12]Of course, once one accepts the fact that the amount of time roommates spend together, and consequently the effect they have on each other, depends on how well they get along, then this introduces some of the problem of endogeneity of friendship choice back into the model.

on the respondent's salary (t-statistic 2.27). They conclude that this is due to peer effects on career choice and help in obtaining jobs. Although this interpretation seems plausible, it is hard to reconcile the positive finding on hallmates with the null results for roommates.

Kremer & Levy (2003) and Duncan et al. (2005) study the effect of randomly assigned roommates on grades and alcohol and drug use during college. Kremer & Levy (2003) find that among students who reported drinking frequently in high school, random assignment of a roommate who also reported drinking frequently reduced the student's GPA by a full point. Duncan et al. (2005) find that, among male (but not female) college students, if two high school binge drinkers are paired together, they are more likely to binge drink in college than if a binge drinker is paired to a nonbinge drinker.

Quasi-Experimental Evidence

Recent evidence from the Moving to Opportunity (MTO) program can be used to estimate the role that neighborhood effects play on outcomes such as work, crime, and education. The MTO program is a large-scale housing mobility experiment conducted in five cities. Interested families were randomly assigned to an experimental or control group. Experimental group families were given housing vouchers to live in a low-poverty neighborhood, whereas control group families did not receive the vouchers (Kling et al. 2005). Because both the experimental and control groups consist of families who wanted to receive housing vouchers, a comparison of the results for families who did receive vouchers (the experimental group) and those who did not (the control group) arguably overcomes much of the problem of correlated unobservables that plagues the analysis of neighborhood effects. Sanbonmatsu et al. (2006) study the effect of receiving MTO housing vouchers on children's academic achievement for over 5000 children 4–7 years after the start of the experiment. They find that although receiving a MTO voucher resulted in families moving to neighborhoods with significantly lower poverty rates (33% versus 45.6% for the control group), there was no difference in the children's achievement test scores or measures of school or behavioral problems for any age group (p. 23). These results appear to reverse the findings of the three-year follow up of the Baltimore MTO program, which found evidence of positive results (Ludwig et al. 2005). Sanbonmatsu et al. (2006) argue that their null results are "particularly surprising" for the youngest children who spent a larger proportion of their lives living in the new neighborhood environment. They suggest that one explanation for the findings is that the effect on school quality of receiving vouchers, though significant, was more modest than the effect on neighborhood poverty rates (the average percentile rank of the school on state exams was 4.1 points higher for the experimental group). At the same time, they conclude that the evidence from nonexperimental data is probably overestimated because of selection effects and that the true impact of moving families from poor neighborhoods is probably small. The null results for children's outcomes are similar to the magnitude of the findings on employment and earnings for adults (Kling et al. 2005, Ludwig et al. 2005). These MTO findings are corroborated by similar housing voucher results from Chicago (Jacob 2004).

The results from the MTO program do not directly address the role of social capital on these outcomes, as the underlying mechanism behind neighborhood effects is not clearly theorized or measured. Indeed, simply moving to a new neighborhood with a lower poverty rate will not increase your social capital unless it is accompanied by actual social integration into the networks of the neighborhood. Pettit & McLanahan (2003), for example, study the effect of residential mobility on two measures of social capital in the MTO program in Los Angeles, the frequency that parents talk to the parents of their children's friends and the number of extracurricular activities the child participated in. They find that a smaller percentage of the experimental group had talked to friends' parents over the past month compared with the control group (72.7% versus 76.6%) but that the percentage participating in activities was the same. So, a reasonable interpretation of the MTO results is that a network-based measure of social capital (talking to friends' parents) indicates little change for those families who moved, thereby indicating that the MTO program is not a direct test of the role of social capital on these outcomes. Nonetheless, because neighborhood characteristics such as the poverty rate are often used as proxies for neighborhood social capital, the MTO results suggest that we should exercise caution in interpreting the estimated effect of these measures with nonexperimental data. The randomized housing studies provide an important example of both the difficulty and potential benefits of quasi-experimental data.

CONCLUSION

If individuals choose friends who are similar to them, then one may reasonably suspect that the effects of many social capital variables are overestimated because of unobserved, individual-level factors that are correlated with friendship choice and the outcome variable of interest. This is not an argument that social capital does not matter, but merely a suspicion that many existing empirical estimates of the effect of social capital are not much of an improvement over our intuition or anecdotal conviction that it does matter. Overall, the evidence reviewed here suggests that when the problem of endogenous friendship choice is taken into account by a method that attempts to deal with it explicitly, the resulting estimates of social capital effects are modest in size, ranging from essentially zero for the majority of the estimates using randomly assigned roommates (see row D of Table 2) to the small, but significant, coefficients reported in fixed effects models of peer effects in education or juvenile delinquency (row A of Table 2). Again, as depicted in column 2 of Table 2, each of the different methodological approaches involves making certain assumptions, and the reader should be cautious about interpreting the results of models in which the assumptions are strong and/or not clearly explained. The evidence reviewed here, combined with the theoretical argument behind why friendship choice may bias estimates of network social capital, should lead social capital researchers to take the problem of causality seriously. Munshi (2003) provides the best example of a credible IV because, as discussed above, rainfall patterns in Mexico provide a reasonably exogenous instrument for the size of migrant networks in the United States several years later, as they affect local opportunities but do not reflect changes in opportunities in the United States.

However, the primary purpose of this review is to show that the problem of nonrandom friendship choice, although posing serious problems for empirical research, is not insoluble. The literature reviewed in this paper addresses the endogeneity of social capital directly and presents innovative attempts to improve the estimation of social capital effects. Although the papers reviewed here are dispersed across several disciplines and substantive topics, their creative methodological approaches may provide a useful template for future researchers to arrive at a more definitive conclusion regarding the effect of social capital on individual outcomes.

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