

Social Dynamics

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Economists have the outcome while sociologists focus on individual behavior, the barrier resulting in the economics is based directly on the influence of others, creating a

Interest in social interactions, neighborhood effects, and social dynamics in the last several years has seen a revival. One reason is the widespread perception that many social indicators in the United States have worsened. The increase in wage and income inequality is one of the most prominent of these trends; a decline in the earnings and incomes of those at the bottom of the distribution is another, separate trend concerning absolute rather than relative changes; and an increase in the concentration of poverty and racial segregation is another. While there is often a tendency to assume that everything is getting worse when this is not correct—a view usefully countered by Jencks (1992)—it is unquestionable that some measures of social well-being have deteriorated.

That inequality, concentration of poverty, segregation—and their continued persistence over time—might be a partial result of social interactions—that is, direct nonmarket interactions between individuals—that lead to low-level equilibria, or “traps,” is an old idea that saw its last major discussion in the 1960s and early 1970s. That period saw extensive examination of the notion of a culture of poverty from which the poor cannot escape (Lewis 1966), of externalities in housing markets that lead to prisoner’s dilemmas and Pareto inferior housing equilibria (Davis and Winston 1961), of segregation as a natural sorting and self-reinforcing mechanism (Schelling 1971), and of peer-group effects in schools (Coleman 1966). The recent revival of interest in such models has come from a variety of sources. In sociology, the work of Wilson (1987) almost single-handedly brought the concept of neighborhood effects and role models back into general discussion, a discussion that has spilled over into all the social science disciplines. In economics, the work of Romer (1986), Lucas (1988), and others on the externalities in technology and human capital investment that

promote economic growth has spilled over into more microeconomic concerns with neighborhoods, income inequality, and the like (Benabou 1993, 1996; Brock and Durlauf 1995, forthcoming; Durlauf 1996a,b, to cite the most influential works among many)¹ The growth of game theory in economics has also led to a branch dealing with the development of social norms and conventions as a natural outcome of group interactions (Young 1996).

The new theoretical literature on these issues in economics has spawned a number of papers demonstrating that, under specified conditions and model assumptions, certain policy interventions can be shown to possibly counter the effects of undesirable social interactions and can have social-welfare-improving consequences (e.g., Benabou 1996). In many cases, these interventions have been shown to permit an escape from the low-level equilibria resulting from those social interactions. A natural question is whether there is any empirical evidence that these, or other policy interventions that might be considered, would have the effects hypothesized, and for the reasons hypothesized, if they were in fact implemented. The answer to this question, in turn, naturally leads to an investigation of whether there have been any policy interventions in the past that have had, either intentionally or unintentionally, effects that have operated directly or indirectly on social interactions, and have been shown to produce positive effects of one kind or another. This is the motivating issue for this chapter.

Answering these questions necessarily requires addressing the prior issue of whether the existence of social interactions can be detected with empirical analysis in the first place, which is an obvious first requirement for estimating their magnitudes or whether policy interventions have affected them. This question has played a major role in the discussions in the empirical literature on social interactions—and not just in the recent literature, for there is almost no econometric issue in recent discussions that was not raised, even if much less formally, in the literature in the 1960s and early 1970s. However, the answers have been clarified, or at least formalized, in more recent years, and this has led to a clearer statement of the conditions for identification. Once one has established the conditions necessary for identifying the existence of social interactions, and for estimating their magnitude, one can then ask what the evidence from past policy interventions has to say about them and what kind of future policy interventions might be tested.

The chapter is organized into four sections. The first provides a general discussion of the issues involved in assessing the effects of interventions on social interactions. The second furnishes a discussion of the econometric identification issues surrounding social interactions and whether policy experiments can assist in that identification. That section demonstrates that identification is possible in many models of social interactions. The third briefly surveys the empirical evidence on the existence and magnitude of social interactions gathered from data on private actions in the absence of policy interventions, focusing on two types—peer-group effects in education and neighborhood effects in cities. The fourth section reviews the evidence on two policy interventions: (1) busing and desegregation, and (2) the Gautreaux and Moving to Opportunity (MTO) programs, which provide incentives to low-income families in inner-city neighborhoods to relocate.

3.1 Overview of the Issues

When considering some of the major social policy interventions in the United States since 1935—for example, the Social Security Act and the programs it spawned (Social Security, AFDC, unemployment insurance), other transfer programs like Food Stamps and Medicaid, the G. I. Bill, busing and school desegregation, Affirmative Action, and equal opportunity legislation—the most important basic distinction to be made for present purposes is whether the interventions operate through effects on private incentives—prices, incomes, and public goods—or through social interactions, or both. I shall call the private incentives the “fundamentals” and define them as those variables that an individual would employ in a purely private calculus of decision making, ignoring the characteristics or actions of other individuals.²

Most policy interventions operate on the fundamentals, and this generates an empirical problem for studying social interactions. The major transfer programs just noted, for example—the Social Security Act programs, Food Stamps, and Medicaid—are generally presumed to have had their major effects through prices and incomes, although in most cases there have been suggestions in the literature that the responses of recipients to those programs have generated, through social interactions, the establishment of norms or community responses that have independent effects of their own. Consequently, from the standpoint of assessing the evidence, simply establishing that these programs have had effects on the distribution of individual outcomes in U.S. society

is obviously insufficient, in and of itself, to establish the mechanism by which those effects have occurred. A purely reduced-form policy intervention analysis, therefore, is not very useful for judging the importance of social interactions unless the intervention in question can be certified to have its effects solely, or at least mostly, operating through such interactions.

The interventions other than transfer programs share this problem. The G. I. Bill, which is regarded by some as the most successful social policy intervention in the U.S. postwar era, used as its policy lever a specific price (of education) and its initial effects operated solely through individual and private responses to that change in price, even if later eligible individuals were influenced by the large numbers of others going to college. Even the race-based interventions mentioned—Affirmative Action, equal opportunity legislation, busing, and school desegregation—affect the treatment of individual adults and children in specific situations and environments first and foremost by altering their workplace characteristics and educational inputs, and are not directly operating on social interactions.

A more recent example is the 1996 welfare legislation, which is generally considered to have changed the cash welfare system for single mothers in the most significant and fundamental way since the enabling Social Security Act of 1935. The supporters of the 1996 legislation clearly intended it to have effects operating through social norms—to “send a message” to young women that they should not have children out of wedlock and to change the “culture” of welfare expectations in low-income communities. However, there was very little in the legislation that directly did so, for the legislation primarily changed the prices and incomes faced by recipients and eligibles (i.e., the benefit formulas and eligibility rules and how they are applied).

These distinctions are also important for distinguishing the goals of theoretical and empirical work on social interactions. Many theoretical results in the literature demonstrate that, assuming the existence of social interactions of one type or another, a particular policy intervention that operates on the fundamentals—school finance equalization, for example, as in Benabou (1996)—may, under certain conditions, have beneficial effects on the distribution of individual outcomes and may change lower-level equilibria to higher-level equilibria. But if such a model were accepted and the recommended policy actually implemented, empirically observing that the intervention did indeed affect

outcomes would establish nothing about whether social interactions were responsible, even in part, for those effects because they could have been solely a result of the change in the fundamentals.

The primary empirical implication of the existence of social interactions that work through changes in fundamentals is the presence of multiplier and contagion effects that can generate large responses to small changes in those fundamentals (Schelling 1978). One approach to detecting social interactions even when the fundamentals are the policy lever is to search for these multiplier and contagion effects. However, the difficulty with this approach is the inherent arbitrariness of what is “small” and what is “large,” particularly in light of the fact that most population outcomes do not change very quickly regardless of the reason for change. A formal analysis of what the magnitude of the response should be in the absence of social interactions would seem to be necessary in this approach, and the assumptions built into that analysis about the way in which changes in fundamentals work themselves through the market and the society, the speed with which individuals adjust to changes in the fundamentals, and the existence of multiple equilibria are likely to be critical.³ Consequently, while this approach should not be ruled out entirely, it would appear to be quite challenging to implement convincingly.

A more promising approach to establishing the existence and magnitude of social interactions through policy interventions is to search for interventions whose effects operate directly on those interactions, and not on the fundamentals, and for which no change in population outcomes at all should be expected in the absence of social interactions. At least three classes of such interventions would appear possible: (a) interventions that change group membership, either by forcibly reassigning individuals to schools, neighborhoods, or other groups, or that offer taxes or subsidies for voluntarily changing group membership; (b) interventions that alter the fundamentals for only a subset of a group with the intention of affecting the entire group; and (c) interventions that seek to operate directly on social norms.

Interventions that forcibly reassign group membership or that offer inducements to do so voluntarily alter the composition of groups without changing the fundamentals for any individuals and hence can identify the existence of social interactions by whether the population outcomes within groups are affected. Perhaps the best example of an intervention that forcibly reassigns group membership is court-ordered busing, an intervention that will be discussed further later in the

chapter. Assuming that who gets bused and who does not is exogenous, the remixing of individuals that results furnishes the opportunity to identify social interactions. At least two problems must be circumvented for this means of identifying social interactions to succeed, however. One is that the fundamentals for the individuals in the population must be held fixed, for if those fundamentals change at the same time as group membership changes, a difference in outcomes could occur from that source alone. An example of this problem will be given in the discussion of busing in section 3.4. A second problem is that group membership will often, if not usually, be endogenous; after being forcibly reassigned, individuals can simply move back to where they began or otherwise adjust their behavior to compensate for the change in location. If there is a unique equilibrium to the locational distribution of individuals that is independent of initial conditions, then any government alteration to locational decisions will be ineffective in the long run as the population merely returns to the old equilibrium. The detection of the effects of social interactions in this case requires that the social interaction effects—that is, the responses of individuals to the presence of different types of individuals in their group—occur more quickly than the locational adjustment process.⁴

Policy interventions that tax or subsidize changes in group membership would appear to be more promising because they will alter the equilibrium even when the locational process has a unique equilibrium. On a small scale, the Gautreaux and Moving to Opportunity interventions that will be discussed in section 3.4 below are of this type. In those interventions, families in inner-city low-income neighborhoods were offered housing in other, often higher-income areas. Once again, however, several problems must be overcome for this approach to succeed. One is that, as before, the fundamentals for the individuals involved must be held fixed to avoid their having an independent effect on outcomes. The second is that, again as before, the long-run equilibrium consequences of such a policy must be considered and the speed of adjustment to the new equilibrium considered relative to the speed of response to social interactions. Third, however, a more basic issue would arise as to how the tax or subsidy to group membership would be defined in equilibrium. If, for example, a subsidy for low-income families to move to high-income areas is offered and sufficiently large numbers of low-income families move to those areas—and possibly high-income families move out—those areas will no longer be high income. It would not make sense for the government to offer subsidies

for movement to specific geographic locations independent of the population living there, but if it offers subsidies for movement to areas on the basis of characteristics that are made endogenous by mobility, the equilibrium consequences of that policy are entirely unclear.

The second class of direct social-interaction intervention is that which seeks to change the fundamentals for a subset of the population in a group in an attempt to influence the outcomes of the others in the group. The change in fundamentals should affect the individuals directly involved but should not, in the absence of social interactions, affect the outcomes of other individuals (ignoring changes in market prices that might have an indirect effect). Although this type of intervention bears a superficial resemblance to the intervention mentioned above that seeks to identify large responses to small changes in fundamentals, the difference here is that no distinction between large and small is necessary, nor is the existence of nonlinearities necessary to identify the effects. It is necessary, however, that group membership itself not change quickly relative to the influence of social interactions, and in this respect this intervention shares the problems of those that change group membership more directly.

Interventions that are aimed directly at social norms are a third type of policy. The most common examples of such policies are mass media campaigns. Examples include campaigns to discourage young women from having children (especially out of wedlock), stay-in-school campaigns, antismoking campaigns, public-health campaigns in general, and the like. To some extent, these "hortatory" interventions can be viewed not as an attempt to change social norms but as an attempt to provide information—on the true consequences of teen childbearing, of smoking, or of dropping out of school, for example—on the presumption that, possibly because of the environment in which they are located, correct information is not available or perceived to the individuals targeted for the campaign.⁵ But many are implicitly or explicitly attempts to actually change norms—that is, preferences—by attempting to convince the audience of the correctness of a different standard of behavior. However, a problem for inferring the existence of social interactions from the effects of these types of interventions is that they may simply work on individual preferences and not on norms *per se*. Consequently, a more credible test of social interactions would be hortatory interventions that target a subset of the population in an attempt to change their norms or behavior in the hopes that this will affect others in the population. Indeed, the literature on mass media

campaigns is replete with strategies of this kind, such as interventions targeted at key actors, those in positions of authority or influence, opinion leaders, and so on (Katz and Lazarsfeld 1955; Rice and Atkin 1989). In this sense, these interventions are like those in the second class, for they operate on only a subset of the population, but in this case by changing the preferences, not the fundamentals, of that subset.

Although this discussion has entirely focused on policy interventions that can conceivably identify social interactions, they have direct implications for identifying those interactions from relationships in the data in the absence of such interventions as well. If we imagine randomized trials that could implement each of these three types of policy interventions correctly (i.e., in a manner to minimize the problems that have been revealed for each), then there necessarily exists a class of nonexperimental variables corresponding to each that replicate the experimental conditions nonexperimentally. These variables take the form of exclusion restrictions and instrumental variables in econometric models and constitute what are usually called natural experiments. Although the existence of such nonexperimental variation is not guaranteed, the policy intervention framework used here provides a natural framework within which to guide the search for alternative nonexperimental sources of identifying variation as well.

Subsequent sections of this chapter will formalize these arguments and review the empirical literature in a selected set of research areas in light of these principles. A theme, and conclusion, of the empirical review is that the principles here formulated have not been generally recognized and that serious attempts to identify social interaction effects using these approaches—either in the design of new policy interventions or in the design of econometric models with nonintervention data—have not been used very often or at least not successfully. While there is a certain amount of research attempting to use changes in group membership as a source of identifying information, it generally does not credibly avoid the problems that have been noted with that approach. Even less analysis has been conducted using the two second approaches, at least by econometric modelers (there is ample literature on the evaluation of mass media campaigns, however). Therefore, it would seem that much additional work could be performed along these lines.

Finally, apart from evidence from the empirical literature, what evidence is there for the existence and importance of social interactions, and therefore for their importance in affecting the response to policy

interventions? Perhaps the strongest evidence for social interactions is now, as it always has been, the *prima facie* evidence on the high degree of stratification in the United States by income, education, race, and other characteristics across neighborhoods and schools, and the high variance across areas and schools that this strong sorting implies (Massey and Denton 1993; Jargowsky 1997). The persistence of this sorting over time is another piece of *prima facie* evidence in support of some type of self-reinforcing equilibrium mechanism, of which social interactions are one type. Ethnographic evidence (e.g., Anderson 1991) also supports the existence of strong social interactions. Yet another type of evidence in favor of the existence and magnitude of social interactions are “puzzles” in individual behavior that are difficult to explain with models of individual calculus. The \$50 bill puzzle (see n. 5) and related behaviors is in this *prima facie* class, as is any evidence that individual actions taken by a socioeconomic group make them demonstrably worse off according to criteria that it is presumed they themselves would accept. Also in this class are sudden swings in time series trends in welfare participation, teen childbearing, and other social behaviors that cannot be explained by changes in prices and incomes.

This type of evidence is extremely important and not to be discounted, but it is nevertheless ultimately unsatisfactory. Establishing the existence of a large unexplained residual is not sufficient to establish the source of that residual, no matter how strong the *a priori* plausibility of one explanation over another. The type of evidence that is needed is instead harder evidence based on the testing of alternative hypotheses within models in which alternatives can be falsified. Empirical evidence of this type will be reviewed in sections 3.3 and 3.4.

3.2 Econometric Analysis

The inferential problems in detecting the existence and estimating the magnitude of social interactions have been subject to considerable and long-standing discussion. Here the problems of identification are formalized, and the role of policy interventions in assisting identification and providing a framework for nonpolicy intervention discussed in section 3.1 are presented.

The basic conceptual relationship in models of social interactions is the effect on one individual's actions of the actions of another individual or group of individuals. The archetypal empirical exercise in the

literature therefore relates, usually through regression analysis, the behavior of an individual to the characteristics of some group to which the individual belongs. Thus regressions of educational attainment, teen childbearing, criminal behavior, and so on, on the individual's own characteristics but also the characteristics of a group, are typical. The traditional critique of such exercises is that the group characteristics are, in one sense or another, endogenous or, more generally, correlated with unobservables in the equation. An issue is whether such endogeneity, if present, can be circumvented by some conventional technique such as instrumental variables or two-stage least squares, using some naturally occurring instrument (nonexperimental methods), or whether formal investigator-induced interventions (experimental methods) would permit identification of the parameters of interest. As noted in the last section, the approach here will be to initially determine whether any experiment is possible to identify social interactions effects.

While there are several ways in which endogeneity can arise, and more than one way in which even the same basic concept can be formulated, the major types of problems with estimating the effect of group characteristics on individual characteristics can be grouped into three categories:

- the simultaneity problem
- the correlated unobservables problem and the related errors-in-variables problem
- the endogenous membership, or mobility, problem

The third of these is perhaps the most commonly discussed. The first two problems can arise, however, even if group membership is exogenous

3.2.1 Simultaneity

The simultaneity problem is mentioned occasionally in the empirical literature (e.g., Case and Katz 1991), although less frequently than the endogenous group membership and correlated unobservables problem, and has been considered formally recently by Manski (1993).⁶ The problem arises if person A's actions affect person B's actions and vice versa. This generates a conventional simultaneous equations problem if we attempt to regress person A's actions on person B's or

person B's on person A's. To illustrate the problem, suppose we have $g = 1, \dots, G$ groups and that there are only two individuals ($i = 1, 2$) per group. Let y_{ig} be the outcome variable of interest for individual i in group g , x_{ig} be an individual socioeconomic characteristic of individual i in group g , and ε_{ig} be an unobservable. Assuming linearity for the relationship, the true structure is assumed to be

$$y_{1g} = \theta_0 + \theta_1 x_{1g} + \theta_2 y_{2g} + \theta_3 x_{2g} + \varepsilon_{1g} \quad (1)$$

$$y_{2g} = \theta_0 + \theta_1 x_{2g} + \theta_2 y_{1g} + \theta_3 x_{1g} + \varepsilon_{2g} \quad (2)$$

We assume only that ε_{1g} and ε_{2g} are orthogonal to both x_{1g} and x_{2g} and that group membership is exogenous. The social interaction coefficients are represented by θ_2 and θ_3 , which represent, respectively, the presence of endogenous and exogenous social interactions. The model could be made more realistic by considering more than two individuals per group, by adding a set of observable group-specific variables to the equation, and by other extensions, but this would not alter any of the results to be discussed here. Note as well that the linearity of the model implies that, in the absence of degenerate and other special solutions, there will be a single unique equilibrium, not multiple equilibria.⁷

Equations (1)–(2) constitute a simple linear simultaneous equations problem and can be analyzed using conventional rules for identification. As noted by Manski, the parameters in (1) and (2) are not identified.⁸ This can be seen either by applying the usual exclusion condition rule—namely, the rule requiring that at least one exogenous variable be excluded from each equation (there are no such exclusions)—or by considering the reduced form, which is

$$y_{1g} = \alpha + \beta x_{1g} + \gamma x_{2g} + v_{1g} \quad (3)$$

$$y_{2g} = \alpha + \beta x_{2g} + \gamma x_{1g} + v_{2g}, \quad (4)$$

where

$$\alpha = \theta_0 (1 + \theta_2) / [1 - \theta_2^2] \quad (5)$$

$$\beta = (\theta_2 \theta_3 + \theta_1) / [1 - \theta_2^2] \quad (6)$$

$$\gamma = (\theta_2 \theta_1 + \theta_3) / [1 - \theta_2^2] \quad (7)$$

$$v_{1g} = (\varepsilon_{1g} + \theta_2 \varepsilon_{2g}) / [1 - \theta_2^2] \quad (8)$$

$$v_{2g} = (\varepsilon_{2g} + \theta_2 \varepsilon_{1g}) / [1 - \theta_2^2]. \quad (9)$$

The coefficients in equations (3) and (4) are the same and hence can be estimated consistently by pooling the data on the individuals and regressing the values of y_{1g} in the data set on each individual's own x and the x of the other individual in the group. But estimates of the three parameters α , β , and γ do not allow separate identification of the four parameters θ_0 , θ_1 , θ_2 , and θ_3 . Thus endogenous and exogenous interactions cannot be separately identified.

An important question is whether identification can be achieved using the covariance of the values of the residuals for different individuals within a group conditional on the values of x_{1g} and x_{2g} , namely, the covariance of v_{1g} and v_{2g} . Equations (8) and (9) imply this is possible only if ε_{1g} and ε_{2g} are independent, in which case θ_2 can be identified from that covariance (the individual variances of ε_{1g} and ε_{2g} can be simultaneously identified from the variances of v_{1g} and v_{2g}). For example, if $\theta_2 = 0$, that covariance is zero if ε_{1g} and ε_{2g} are independent. However, the difficulty is that ε_{1g} and ε_{2g} are probably strongly correlated in most applications, either because of endogenous group membership and the sorting of individuals across groups that results or, more generally, from the presence of the unobserved correlated effects that will be discussed momentarily. To assume independence of ε_{1g} and ε_{2g} is to implicitly assume that all of the correlation of values of y among individuals in a group who have the same x values arises from social interactions, and this ignores the basic identification problem in the model—namely, how to distinguish within group correlations that arise from social interactions from correlations that arise for other reasons.

Many studies in the literature assume one form of interaction only—endogenous or exogenous—and obtain identification by that restriction. Unfortunately, if the assumed form of interaction is incorrect, the resulting estimates are either biased or simply misinterpreted. For example, if exogenous interactions are assumed to be zero ($\theta_3 = 0$) when they are not, and if the system is estimated by two-stage least-squares using estimates of equations (3)–(4) to form instruments for the “other” y in equations (1)–(2), it can be shown that the coefficients on predicted “other” y in equations (1)–(2) are unbiased estimates of (γ/β) and hence are biased estimates of θ_2 . On the other hand, if endogenous interactions are assumed to be zero ($\theta_2 = 0$) when they are not, then estimation of equations (1)–(2) leaving out the “other” y is equivalent to estimating the reduced form, and hence the social interaction coefficient—that on the “other” x —is an unbiased estimate of γ ; this would be incorrectly interpreted as estimating θ_3 .

A key point is, however, that the existence of social interactions in general is identified in this model (Manski 1993). The coefficient γ indicates whether any type of social interaction is present, for if $\theta_2 = \theta_3 = 0$ then $\gamma = 0$. Thus if the exogenous characteristics of individuals in a group are correlated with the values of y of others within the group (holding fixed own values of x), interactions must be present in this model, although one cannot determine whether it is because those characteristics have direct effects or they have indirect effects working through outcomes. To the extent, therefore, that it does not matter for the purposes at hand whether social interactions are of the endogenous or exogenous type, estimation of the reduced form equations (3)–(4) is sufficient. However, this form of inference will again founder on the presence of unobserved correlated effects or endogenous group membership, which will induce a relationship between y and x across individuals that arises from other sources.

It is useful to approach the question of identification by asking whether there are any randomized trials of policy interventions that could, even in principle, identify the model, a perspective not taken in the literature to date on social interactions. By “in principle”, we mean randomized trials that use the observed and known values of all x and y of all individuals in a population (assumed free of measurement error), and their initial group membership, and that alter either x , y , or that group membership in different ways for different individuals. If we take group membership as fixed and seek to manipulate experimentally the values of x and y within groups, the structure of the model as given in equations (1)–(4) and the nonidentification results we have obtained for it necessarily imply that no such experiment is possible. In fact, the only experimentally manipulatable variables are the individual values of x_{1g} and x_{2g} , and we have already noted that this permits only the estimation of the reduced form in equations (3) and (4), which does not identify all the parameters (the experimental manipulation of these variables would merely break any correlation they have with the error terms, which is not the source of the problem we are discussing in this section). The values of y_{1g} and y_{2g} , like all endogenous variables in a model, cannot be directly experimentally manipulated; they are chosen by the individuals and, even if they could be temporarily altered by the government, would, if the system were allowed to adjust, simply return to their equilibrium values.⁹

Experimentally altering group membership, however, would allow identification. Randomly matching a set of 2G individuals into pairs of

individuals would result in independence of ε_{1g} and ε_{2g} , and hence θ_2 could be identified from the correlation of residuals across individuals within a group. The identification of θ_2 permits the identification of θ_3 from the other reduced form coefficients. The randomization of group composition implies that any within-group correlation must be the result of endogenous social interactions. As noted in the last section, however, the ability of individuals to resort themselves if the assumption of exogenous group membership is relaxed is the main difficulty with this approach. We shall therefore return to this issue in the discussion of endogenous group membership below. We shall also consider at that point whether there are nonexperimental counterparts to random assignment of individuals.

It is possible that identification could be achieved if this linear model were made nonlinear in a way that permitted multiple equilibria (see, e.g., Brock and Durlauf 1995 and Durlauf 1996b for examples). For each of the stable equilibria there will be a reduced form counterpart to equations (3)–(4) that describes the relationship of the group distribution of x values to the y values, and nonlinearities may result in more parameter identification. A major problem with models of multiple equilibria is, however, detecting which equilibrium the observed data correspond to, assuming that the system is in equilibrium. This is a higher level of identification problem than any present in the linear, single-equilibrium model.¹⁰

While random assignment of group membership is a possible identification mechanism, there are, in fact, other policy interventions that can identify the model even without manipulation of group membership. However, the structure of the model must be changed. Specifically, partial-population experiments in which only a portion of the individuals within each group are given a treatment are in this class. Modifying equations (1)–(2) to introduce policy variables that affect one individual but not the other can be illustrated by letting p_{1g} be a government “price” (subsidy, tax, or other instrument) administered only to individual 1, a price variable that is independent of the unobservables in the model. Then we replace equation (1) with

$$y_{1g} = \theta_0 + \theta_1 x_{1g} + \theta_2 y_{2g} + \theta_3 x_{2g} + \theta_4 p_{1g} + \varepsilon_{1g} \quad (10)$$

The absence of p_{1g} in equation (2) permits all parameters in the model to be identified. As can be seen from the reduced form (not shown), the parameter θ_2 is identified from the effect of p_{1g} on y_{2g} ; this again

leads to identification of θ_3 as well. The difference in this model and the previous one is that here there exists an exogenous variable that affects one individual directly but affects the other only through the endogenous social interaction. The identifying restriction is that individual 2 is not directly influenced by p_{1g} and there is no social interaction induced by that variable. Implicit in this restriction is the notion that the exogenous social interactions originally specified in equations (1)–(2) exist only for certain types of characteristics of individuals, and that the unique prices that some of them might face are not in that category. Indeed, this example suggests that there might be a larger class of exclusion restrictions consisting of characteristics of individuals that can be argued on some basis to not have a direct influence on others. Judging the plausibility of such restrictions, as well as that of the partial-population policy intervention suggested here, requires a more careful consideration of what is meant by exogenous social interactions and what the deeper source of such interactions is.

While the possibility of randomized trials of such policy interventions is reasonably clear, it is also possible that nonexperimental counterparts to such policy interventions exist. Any government program or any private market event that affects only a subset of the individuals in a group for reasons unrelated to the unobservables in the model (i.e., unrelated to y conditional on x) is a candidate in this class, if it can also be reasonably assured that such programs or events also have no direct social interaction effect on the other individuals in the community.

The hortatory policy interventions discussed in section 3.1 are also in this class, provided they aim to change the preferences of only a subset of the individuals with a group, and hence likewise can offer identification of the model parameters. Simply replace p_{1g} in equation (10) by a treatment dummy for a randomized trial of a hortatory campaign to affect the preferences of individual 1. As before, the additional restriction implicit in the approach is that being subjected to such a campaign is not a characteristic that has direct influence on the others in the group who are not subjected to it. Pure mass media campaigns that are directed at the entire population are consequently not in this class, but efforts to affect subsets of the population such as key influential persons are. In this case, however, there are no obvious natural experiment counterparts to mass media campaigns, although conceivably subsets of group populations might be discovered who were exposed to extra information for exogenous reasons.

3.2.2 Correlated Unobservables and Errors-in-Variables

The problem of correlated unobservables arises if there is some group-specific component of the error term, call it μ_g , that varies across groups and that is correlated with the exogenous characteristics of the individuals (x) (Manski 1993). The suggestion that the presence of such unobservables could account for much of the evidence on social interactions has a long history dating back to the 1960s (see section 3.3) and is one of the most common biases referred to in empirical studies. The unobservables could arise from a variety of sources and depend partly on the application. Often the unobservables are assumed to arise from unobserved preference components (neighborhoods) or abilities (classrooms) that are correlated across individuals within those groups. These correlations can be motivated by the endogenous group membership model, as described below—that individuals tend to locate where there are other individuals of the same type, in the most common case—but can in principle arise even in an exogenous group model. Alternatively, the unobservables may represent contextual, or environmental, influences that are measurable in principle but may not be in practice, such as school resources, crime rates, and employment opportunities in the neighborhood.

Modifying the previous model by allowing $i = 1, \dots, N_g$ individuals per group, the reduced forms in equations (3)–(4) can be rewritten as

$$y_{ig} = \alpha + \beta x_{ig} + \gamma x_{(-i)g} + \mu_g + \eta_{ig} \quad i = 1, \dots, N_g, \quad (11)$$

where $(-i)$ denotes the individuals in the group other than i and $x_{(-i)g}$ denotes a weighted mean of the values of x of the individuals in $(-i)$.¹¹ The component μ_g can be thought of as representing the covariance between ε_{1g} and ε_{2g} in equations (1)–(2) but modified for many individuals, that is, capturing an intraclass covariance. Then, assuming

$$E(x_{ig}\mu_g) \neq 0, \quad (12)$$

least-squares estimation of equation (11) will yield inconsistent estimates of both β and γ . In particular, it can be shown that the least-squares coefficient on $x_{(-i)g}$ is biased upward if the covariance between $x_{(-i)g}$ and μ_g is sufficiently larger than the covariance between x_{ig} and μ_g .¹² This is likely to be the case if $x_{(-i)g}$ represents some average across individuals that is more highly correlated with the unobservable than is any single observation. Thus in the presence of correlated unobservables even the weak form of identification obtainable from the

reduced form in the simultaneity model—of the existence of any form of interaction, endogenous or exogenous—is lost.

A related model, not formally considered in the literature to this author's knowledge, arises if there are errors-in-variables in the measured individual characteristics x but the true values are correlated across individuals.¹³ A typical example occurs where x_{ig} is the income of the family of child i in group g , $x_{(-i)g}$ is the mean family income in the rest of the group, and y_{ig} is some child outcome, but where x_{ig} measures transitory rather than permanent income and it is permanent income that matters. We can write the model as

$$y_{ig} = \alpha + \beta x_{ig}^* + \gamma x_{(-i)g}^* + v_{ig}, \quad (13)$$

$$x_{ig} = x_{ig}^* + \zeta_{ig}, \quad (14)$$

$$x_{ig}^* = \mu_g + \xi_{ig}, \quad (15)$$

where the variables with asterisks measure true but unobserved variables and those without asterisks are the observed, error-filled variables. Assuming all errors are independent across i and g and of each other, a correlation between x_{ig} and $x_{(-i)g}$ arises only from the presence of the common unobservable μ_g in equation (15). In the presence of that factor, it can be shown that a regression of y_{ig} on the observables x_{ig} and $x_{(-i)g}$ yields in the population a nonzero coefficient on $x_{(-i)g}$ even if $x_{(-i)g}^*$ does not truly affect y_{ig} . The simple reason for this result is that the other individuals' weighted mean of x serves as a proxy for μ_g . To be precise, the least-squares coefficient on $x_{(-i)g}$ in such a regression is biased upward if the variance of $\zeta_{(-i)g}$ is sufficiently smaller than the variance of ζ_{ig} , that is, if measurement error is smaller in the weighted mean $x_{(-i)g}$ than in the individual x_{ig} .¹⁴

Consistent estimation of γ requires in either model breaking the correlation between $x_{(-i)g}$ and μ_g . Consideration of policy interventions that might induce this result requires that thought be given to the source of μ_g and that a distinction be made between two generic sources of such correlated unobservables. The first is that which arises from sorting and endogenous group membership, and from preferences or other forces leading certain types of individuals to be grouped together. The second is that which arises from common environmental factors in the neighborhood such as crime, schools, and employment opportunities, which are different because their relationship to the population composition of a group is more complex. Crime, for example, may be partly a function of the fraction of group individuals with low income; school

characteristics are determined through a political process where the influence of population composition is not entirely clear, particularly in cases where population in the area is fairly heterogeneous; and the proximity of employment opportunities to a neighborhood are likely fixed in the short run but will change over time as the population composition of a neighborhood changes if employer location decisions are affected by the location of workers.

For the first type of common unobservable, the randomized group assignment intervention discussed in the context of the simultaneity model will also eliminate the intragroup correlations that arise from endogenous group membership (with the same caveats regarding subsequent resorting). The additional element here is that it will also eliminate the correlation of x_{i-ng} and the reduced-form error term, which was not an issue in the simultaneity model. All structural parameters could be identified with this type of intervention and in this sense there is no difference between the simultaneity problem and the correlated unobservables problem. In addition, partial-population interventions that introduce a price or change the preferences of a subset of the population are likewise sufficient to identify the endogenous social interactions coefficient θ_2 even in the presence of correlated unobservables, so long as those policy interventions are constructed to be independent of all observables and unobservables. However, this is not sufficient to identify θ_3 because these interventions do nothing to remove the correlation of x_{i-ng} and the error term. For that purpose a randomized alteration of x_{i-ng} is necessary. It was not needed in the simultaneity model because x_{i-ng} was assumed uncorrelated with the error term in that case.

If the common unobservable is of the second type, identification is not so simple and, indeed, it is not even clear what the object of estimation is. This is because, in all the examples given, μ_g is a function of the distribution of x_{ig} (if not y_{ig}). If, for example, crime rates are a simple function of the low-income portion of the group population, then it is not clear that it will ever be possible to separate the effects of low income per se from the effects of crime. If a certain quality of local school necessarily follows the presence of sufficient numbers of high-income families, then it is not clear that it will ever be possible to separate the effects of high income per se from the effects of schools. One might take the position that such separation is not needed because it does not matter for policy purposes what the source of the influence of the low-income or high-income families is, but in fact there are policies that operate separately on the crime, schools, and other environmental

variables that do not work through the characteristics of the neighborhood population. These policies might be used to separate the effect of the two, but this will be application-specific. There would not seem to be any general solution to this problem that will work for all possible environmental influences.

One possible line of attack to this generic problem is through the assumption of nonlinearities in the relationship between μ_g and the group population characteristics. If instead of $\mu_g = \delta x_{i-ng} + \omega$, where ω is a white-noise unobservable, we assume the relationship is nonlinear. If school resources in a community are determined by the median voter, for example, then changes in x_{i-ng} that do not change the identity of that voter will not change those unobserved resources; if variables like crime rates and employment opportunities are determined by the value of x_{i-ng} in the dominant, or majority, part of the x distribution, then changes in x_{i-ng} that do not affect the composition of that majority will not affect those rates and opportunities. The best example of this latter case is one in which the values of x within a minority of the population change, those within the majority remain fixed, and the question is whether the values of y of the majority respond to changes in the values of x among the minority.

Reliance on these types of nonlinearities for identification has the disadvantage of forcing reliance on assumptions that are difficult if not impossible to test and also restricts the range of x_{i-ng} over which social interactions can be tested (namely, only over ranges within which μ_g does not change). It also makes the definition of groups even more important than it usually is, for the choice of definition affects whether a change in the values of x in a subpopulation within a group is "large" or "small." If the distribution of x alters for only 1 percent of the individuals in a school district, and does not materially affect (unmeasured) school resources in the district, it might still affect the residents in a particular block if the entire 1 percent whose x values have changed live in that block; there they might constitute a majority and might affect the values of different other types of μ_g on that block. The general problem is that different types of effects, both arising from social interactions (x_{i-ng}) and unobservables (μ_g), may have different group definitions.

3.2.3 Endogenous Group Membership

The endogenous group membership issues are particularly familiar and have, again, been discussed since the 1960s. The simplest way to

set up the model is in the framework of the familiar two-equation switching regression model of econometrics consisting of an equation for outcomes y_{ig} conditional upon a group membership assignment of the population and an equation for the group membership assignment itself. An illustrative example of the first equation, again maintaining linearity, is

$$y_{ig} = \theta_0 + \theta_1 x_{ig} + \theta_2 y_{(-i)g} + \theta_3 x_{(-i)g} + \varepsilon_{ig}, \quad (16)$$

where we now, for simplicity, assume that $y_{(-i)g}$ and $x_{(-i)g}$ are the means of the individual values of y and x in each group excluding that of individual i . The reduced form of equation (16) is necessarily also linear and of the same form as considered previously, namely,

$$y_{ig} = \alpha + \beta x_{ig} + \gamma x_{(-i)g} + v_{ig} \quad (17)$$

As for the second equation, we define the utility to individual i from locating in a group g conditional on the locational decisions of the rest of the population and hence conditional on mean exogenous characteristics $x_{(-i)g}$ and mean structural residuals $\varepsilon_{(-i)g}$ —we assume these residuals to be observed by individual i but not by the econometrician—in each group g as¹⁵

$$U_{ig} = f(x_{ig}, \varepsilon_{ig}, x_{(-i)g}, \varepsilon_{(-i)g}) + \eta_{ig}, \quad (18)$$

and with the following decision rule:

$$\text{individual } i \text{ chooses location } g \text{ iff } U_{ig} \geq U_{ig'} \forall g' \quad (19)$$

The usual presumption is that the function f in equation (18) picks up conformity effects as individuals prefer to locate near individuals like themselves, but there is nothing in this general structure that requires it. Assuming that a unique locational equilibrium exists—that is, a single allocation of individuals to groups in which each individual's preferred location is consistent with that of all other individuals—equations (17)–(19) represent an internally coherent description of a social interactions model with endogenous group membership.¹⁶

That estimation of equation (17) on the assumption that x_{ig} and $x_{(-i)g}$ are independent of the error term in that equation yields inconsistent parameter estimates is familiar from the econometric literature on selection bias, for equation (18) clearly indicates that there will be a relationship between the error terms ε_{ig} and $\varepsilon_{(-i)g}$ (which are contained in the reduced form error term in that equation) and x_{ig} and $x_{(-i)g}$, which is induced by the locational decision mechanism

The parameters of equation (16) or (17) are unidentified in this model just as they were in the correlated unobservables model discussed in the last section. The group membership selection equation gives specific form to such correlated unobservables, but the econometric implication is the same as any such model. However, this particular mechanism for the problem suggests a class of solutions along the conventional lines used for switching regression models, namely, exclusion restrictions. In this case, for identification of at least the coefficients of equation (17) (ignoring the intercept) a variable is needed that appears in equation (18) but not in equation (17), and no such variable appears (recall that the structural residuals terms ε_{ig} and $\varepsilon_{(-i)g}$ are not observed by the econometrician). A randomized trial of a policy intervention that offers subsidies (or penalties) to locate in groups g that differ across individuals i is sufficient for such a purpose. Denoting such a subsidy level as b_{ig} , we have

$$U_{ig} = f(x_{ig}, \varepsilon_{ig}, x_{(-i)g}, \varepsilon_{(-i)g}, b_{ig}) + \eta_{ig}. \quad (18')$$

We suggest that the presence of such a variable should permit identification of the coefficients in equation (17), although without demonstrating it formally.¹⁷ We also suggest that it is certainly possible that there exist nonexperimental counterparts to these subsidies in the form of differential moving costs across individuals and other constraints on mobility that differ cross-sectionally.

A major question surrounding this approach concerns the design of the subsidies b_{ig} and how they are tied to the characteristics of the areas g (they are presumably not tied literally to the index variable g itself, which is arbitrary and meaningless). How the subsidies are designed will affect the equilibrium of the model and make the analysis of identification more complicated still. For example, as noted in section 3.1, if subsidies are based on the population composition of the group, those will change as individuals change group, and hence the value of the subsidies will not stay fixed as the system moves toward equilibrium.

As will be noted in the review of empirical work below, the most common approach to this problem is to ignore the equilibrium implications of the model and simply analyze the effects of moving a small subset of the population from one location to another, holding the locations of the rest of the population fixed. As also noted in section 3.1, this is not an unreasonable strategy but does require that the speed of locational adjustment that results be slower than the speed at

which the social interactions take form and have their influence. It also requires an a priori judgement on what constitute "small" versus "large" changes in the population composition of a group.

3.2.4 Summary

The identification method that works most often in all of these situations we have discussed is by means of a randomized policy intervention that provides subsidies or taxes to group membership or that otherwise induces changes in group membership unrelated to the sorting motivations that occur in the absence of such interventions. Nonexperimental counterparts that achieve the same end are also candidate approaches. Such interventions break the correlation between observed and unobserved group characteristics, and they generate independence of errors within groups that can identify endogenous social interactions. The major unresolved problem in this approach is the presence of environmental variables that are unobserved but tied to the distribution of group characteristics themselves, and here the best approach is to rely on nonlinearities that are plausibly related to specific theories of concrete environmental variables.

Partial-population price interventions and partial-population hortatory interventions are also candidates for identifying some of the social interaction parameters. These methods do rely on exogenous group membership and are therefore candidates if population reallocations occur more slowly than within-group social interaction influences. The relative speeds of adjustment issue comes up sufficiently often in the discussion of identification approaches that it would probably be fruitful to model those dynamics explicitly, which has rarely been done in the literature, and to search for testable implications of the types of dynamics assumed.

3.3 Evidence: Private Actions

For lack of a better term, I denote studies of social interactions that arise naturally in society and without any exogenous government intervention as studies of private actions. In this category, two literatures on social interactions stand out in size relative to all others; these are the literatures on peer-group effects in education, and on neighborhood effects in cities. Both literatures are sizable and both, especially that on peer group effects, have already been surveyed or summarized

multiple times. I will survey the surveys, as it were, but also note specific studies that are either representative or unusual in some respect.¹⁸

3.3.1 Peer-Group Effects in Education

The literature on peer-group effects in education dates from the publication of the Coleman report (Coleman et al. 1966). Coleman's analysis of US schools indicated that educational achievement of black students was positively related to the fraction of students in their school that were white. This was one of the most controversial findings of the Coleman report—among many that were controversial—and has been held primarily responsible for the subsequent movement toward forced busing in the United States in the late 1960s and early 1970s (Heckman and Neal 1996).¹⁹

From the standpoint of the detection difficulties described above in section 3.2, such a peer-group association has all the problems noted above except the simultaneity problem (because the percent of a school that is white is an x , not a y , in such a model). It is quite possible, in fact extremely plausible, that greater-percentage white schools have better (unmeasured) resources and inputs than lesser-percentage white schools, that the percentage white might be correlated with unmeasured characteristics of black students, and that the types of black students whose parents choose to get them into high-percentage white schools are systematically different than other black students in unmeasured ways; in short, that the biases discussed in section 3.2 are all present.

These points are sufficiently obvious that they constituted a theme repeatedly made in the subsequent volume on the Coleman report published not long afterward (Mosteller and Moynihan 1972). Individual papers by Christopher Jencks, Eric Hanushek and John Kain, David Armor, Marshall Smith, and David Cohen, Thomas Pettigrew, and Robert Riley all mentioned these possible biases and interpretative problems with Coleman's result.

However, the papers in the Mosteller-Moynihan volume went further than this in their critiques of the Coleman peer-group finding. First, several authors (Jencks; Cohen, Pettigrew, and Riley; Armor) found that the peer-group effect became either insignificant or inconsequential in magnitude once individual and family background characteristics like family SES were controlled for (which Coleman had not done). Second, and even more damaging, a replication of Coleman's

work by Smith revealed that Coleman had made a coding error that greatly affected the estimated peer group effect; namely, it became insignificant when the error was corrected.

The importance of these early findings cannot be underestimated because the biases noted above and by the authors of the Mosteller-Moynihan volume are likely still present and hence the effects estimated by such methods are arguably an upper bound; therefore, a small estimated effect could easily reflect a true zero effect.

Since the Mosteller-Moynihan volume, a large number of studies of peer group effects have been conducted. Most of those in the education literature are summarized in a recent review by Schofield (1995), who surveys the surveys as well as updates them (some of the more widely cited earlier surveys are those by Cook et al. 1984; Mehard and Crain 1983; St. John 1975; and Weinberg 1977). For someone whose expectation is that peer effects are likely to be strong, the literature Schofield surveys will be surprising because of the extremely weak nature of the findings. Many, if not a majority, of studies find small or weak effects of peers. The findings are fragile and nonrobust to specification and to the inclusion of other controls. Schofield concludes her survey by stating that "research suggests that desegregation has had some positive effect on the reading skills of African-American youngsters. The effect is not large, nor does it occur in all situations" (Schofield 1995, 610). She also finds that there is no evidence for any effect on mathematics skills.²⁰

In the economics literature, which overlaps somewhat but not completely with that surveyed by Schofield, the majority of studies again find weak or nonexistent peer group effects. Two exceptions are the studies by Summers and Wolfe (1977) and Henderson, Meiskowski, and Sauvageau (1978), the first of whom found positive effects on the test scores of a sample of Philadelphia students of the achievement levels of the other students in the school, and the second of whom found similar effects in a study of Montreal schools. It is easy to note the biases that could lead to such positive correlations, but this does not explain why these studies yielded stronger effects than the majority of other studies, which also were subject to those biases. Henderson, Meiskowski, and Sauvageau speculated that one reason for the difference of their results with those in U.S. schools was the extreme social and cultural homogeneity of their French-speaking population.²¹

This literature continues and is thriving in economics. The major difference between the newer studies and the older ones is that the former

regularly use instrumental variables to account for endogeneity of the characteristics of the peer group. However, thus far the instruments chosen have generally been ad hoc in nature and not based on a strong, or at least explicitly formulated, theory, and not on the types of identifying variables discussed in this chapter. A typical example is Gaviria and Raphael (1997), who regress various student social outcomes (drug and alcohol use, cigarette smoking, etc.) on the characteristics of students in their schools, but instrumenting the latter with whether the families are long-term or recent residents of the area. The argument is that long-term residents are less likely to be selected because they moved sufficiently far in the past as not to be affected by current locational characteristics. Not only does this ignore the fact that long-term residents are a self-selected set of families who have chosen *not* to move out of the area, it presumes them to be out of equilibrium, an assumption that needs defense and demonstration, at best. It also assumes incorrectly that the instrumental variables technique will correct for student self-selection, and it ignores most of the correlated unobservables problem.

3.3.2 Neighborhood Effects

As noted in the introduction, perhaps the strongest evidence for neighborhood effects thus far is simply the *prima facie* descriptive evidence on strong sorting within U.S. cities by neighborhood, as well as its persistence and the actual increase in isolation of minority poor (Massey and Denton 1993; Wilson 1996; Jargowsky 1997).²² Considering instead the literature testing falsifiable hypotheses for the existence and magnitude of social interactions, it should again be noted that much of the empirical literature on neighborhood effects was spawned by the work of Wilson (1987) who suggested that disadvantaged black families left behind in inner-city neighborhoods have no successful role models to emulate (his argument is much more complex than this simplistic description implies). A large body of literature has ensued, the vast majority of which has consisted of simple regressions of family outcomes on own characteristics and some measure of the characteristics of the other families in a localized area. Once again, the problems in doing so have been recognized in the literature, even if relatively little has been done about them. A survey of the literature up to 1990 is provided by Jencks and Mayer (1990), who find the evidence to be surprisingly weak and fragile, in light of the

presumed bias toward stronger effects (not that there are not many studies that show positive correlations).²³ Subsequent studies that similarly make no adjustment for potential bias include those of Crane (1991), Mayer (1991), Corcoran et al. (1992), Brooks-Gunn et al. (1993), Solon, Page, and Duncan (forthcoming), and many others. While many of these studies find positive neighborhood effects, it is difficult to conclude that anything at all has been learned except that raw intraneighborhood correlations often hold up when individual characteristics are controlled (though some studies such as Solon, Page, and Duncan (forthcoming) find that most of the effect goes away when such controls are added).²⁴

A small number of studies have attempted instrumental variable methods but, once again, with problematic choice of instruments. Case and Katz (1991) instrument the neighborhood variables with lagged x variables; Evans, Oates, and Schwab (1992) instrument school composition with citywide variables for the unemployment rate, median income, and the like; and Cutler and Glaeser (1997) instrument a racial segregation index with a variety of variables, including prior residence and citywide governance variables. What is absent in these discussions is any formal description of a residential choice model that could justify the instruments, a dynamic model that could justify the use of lags, an accounting for both correlated unobservables and area selection effects, or, in some cases, simply a good defense for why the variables should not affect the outcome variable directly. The instruments have an ad hoc flavor because they are not based on any explicit theory or model.

3.4 Evidence: Policy Interventions

The number of policy interventions that can be fairly described as having effects on social interactions as a major goal is quite small, and the number that have been evaluated or studied seriously is even smaller. In this section, I will focus on two upon which a considerable amount of ink has been spilled: school desegregation and busing, and the Gautreaux/MTO programs. While this is not an exhaustive list of those that have been studied, these two will illustrate the issues involved in using policy interventions to study social interactions. In addition, I will note two additional types of policy interventions that are germane to the estimation of social interactions but less widely known (hortatory campaigns and I Have a Dream programs).

3.4.1 School Desegregation and Busing

Perhaps the most daunting feature of the literature on the effects of school desegregation and busing on student achievement is that it greatly overlaps with the literature on peer-group effects in education. In particular, the majority of studies of school desegregation consist of studies regressing black student outcomes on the racial composition of their schools, and thus do not examine directly the effect of the intervention in question (namely, the intervention of school desegregation). In these studies, the differences in school racial composition that generate the independent variable of primary interest occasionally arise from court-ordered desegregation plans; they more frequently arise from voluntary residential location patterns and school choice patterns; from school board rezoning decisions; and from student transfers subsequent to school openings or closings. From a program evaluation standpoint, only variation that directly arises from court-ordered desegregation plans provides direct evidence on the policy intervention of interest. Yet this issue appears to have escaped most of the analyses in the literature, which rarely note that it matters which source of variation in generating the treatment variable of interest is utilized for estimation.

To be sure, there would be problems in studying court-ordered desegregation by itself because most of those orders are by now twenty years old and one could only conduct historical studies (though this would certainly be of interest). In addition, studying current outcomes in areas that were once under court order is problematic because significant resegregation of those school districts has occurred, exactly as a sorting-equilibrium model with a single stable equilibrium would predict (Schofield 1995, 598). In any case, based on the studies that have been conducted, the literature has concluded that the effects of school desegregation, like those of peer effects, are at best modest and at worst zero, as described previously.

There are exceptions to the methodological objection that has been made here. A good example is the study by Boozer, Krueger, and Wolkon (1992) whose treatment variable of interest is also racial composition but who instrument it with a dummy breaking the data into pre-1964 and post-1964 periods, which is a good indicator of when court-ordered desegregation began. The authors found that using this instrument reduced the coefficient on racial composition to insignificance. Thus, at least for this one study, an improvement in the

methodology did not change the general empirical conclusions from the prior literature.

While the literature hence suggests relatively few effects of desegregation, it should be noted for the purposes of this chapter that court-ordered desegregation is a problematic treatment for the evaluation of peer effects in education in the first place. The treatment in this case is transferring a disadvantaged minority student to a majority-white school. Because majority-white schools differ in many ways from the schools from which the minority students came, school resources by themselves differ and are therefore part of the treatment; this is the problem of correlated unobservables noted in section 3.2. Solving this problem, it could be argued, would require an experiment in which white students were bused to minority schools, not the other way around; in that type of experiment, the general school environment of minority students would not change, and the effects of being in the same classroom or school with white students could be more readily isolated.²⁵ But on top of this are the effects of transportation to school by bus itself on student functioning, as well as any short-run stigma or social adjustment effects experienced by minority students bused into majority white schools. Both of these effects could have deleterious effects on achievement and hence could mask positive peer effects.

3.4.2 *Gautreaux/MTO Programs*

The well-known Gautreaux program in Chicago was the result of a housing discrimination lawsuit settled in the 1970s (Rosenbaum 1992; Rosenbaum and Popkin 1991; Popkin, Rosenbaum, and Meaden 1993). In the program that followed the settlement of the suit, public housing residents who volunteered for the program were put on a waiting list and offered new housing in other parts of the city that were less than 30 percent black as such housing became available. Rosenbaum has argued that the locations of the offered apartments were randomly assigned and that residents therefore randomly ended up in different types of neighborhoods. Rosenbaum's analysis has shown that those who ended up in suburban neighborhoods had more successful outcomes on a number of dimensions (employment rates, wage rates, school attendance and completion, etc. for either adults or children) than those who ended up in city neighborhoods.

The Gautreaux program has the usual set of biases that are associated with almost any social experiment. Only volunteers were enrolled,

for example; the offered housing was obtained by the program operators only from landlords who agreed to participate; and some of the families moved back to the city or attrited from the experiment (Rosenbaum 1995). But aside from these biases, the Gautreaux program has properly received considerable attention from researchers and policymakers who have seen it as a reasonable attempt to experimentally alter group membership.²⁶

Partly as a result of the success of the program, the U.S. Housing and Urban Development (HUD) has mounted an ambitious set of randomized trials in five cities to test a similar program. These programs, called Moving to Opportunity (MTO), offer inner-city public housing residents who volunteer one of two options—a voucher to use for housing in a high-income area of the city, and a voucher to use anywhere in the city. Public housing residents are randomly assigned one of these options, and a third group is randomly assigned to control status. The evaluations are still being conducted but the early results show positive effects on some outcomes (Katz, Kling, and Liebman 1999; Ludwig, Duncan, and Hirschfield 1999; Ludwig, Duncan, and Pinkston 2000). HUD has termed the MTO demonstrations “the most significant social experiment HUD has implemented in the last 25 years” (Goering and Feins 1997).

While the Gautreaux and MTO programs have the strongly desirable feature of randomization or quasi-randomization (in the case of Gautreaux), they do have problems as a test of the presence of neighborhood effects. For example, while the Gautreaux/MTO programs remove sorting bias, they do not solve the correlated environmental unobservables problem because the neighborhoods into which the participants move differ from their old neighborhoods in more ways than simply the socioeconomic composition of their neighbors. Differences in labor market opportunities, in school quality, in crime rates, and even in housing quality (the new units are presumably better quality than the old) exist. In fact, with regard to the first of these, the programs are motivated as much by the spatial mismatch problem (i.e., job opportunities) as by social interactions. While it is sufficient for some policy purposes to know simply the composite and net effect of all these factors taken together, the multiplicity of factors hinders the detection of effects of social interactions *per se*.

Once again, a better strategy to measure social interactions would be to measure the change in the outcomes of the initial residents of the areas into which the inner-city public housing residents moved,

for it could be argued that the contextual-environment factors were approximately fixed for them before and after the change²⁷ Unfortunately, the evaluations of the programs appear not to have collected data on that population. One could argue that the MTO movers are such a small part of their new neighborhoods that their effect on aggregate moments of the destination neighborhood distributions would be negligible, but this ignores the issue noted in section 32 that some types of neighborhood effects may be quite local (e.g., on a single city block). The crude proxies for neighborhood effects that are used in the empirical literature, which are solely the result of data limitations, should not lead to a conclusion that no social interactions are present in smaller geographic areas. More generally, the theory is consistent with a small intervention affecting only a small number of individuals.

A larger, more difficult problem to solve is the possibility of subsequent residential mobility of both MTO participants and initial residents of the destination areas. While these considerations may be discounted because they require long-run adjustments, implying that the programs can at least measure short-term responses, it must also be considered whether the most important social interaction effects are not also long-run in nature. In addition, as just discussed, the relevant mobility of the initial residents of the destination areas may be that of those on a particularly city block, not those in the rest of the neighborhood.

3.4.3 Other Policy Interventions

Much literature on policy interventions works through diffusion and network processes and other forms of social interactions, but two are worth mentioning here given the types of substantive applications we are primarily concerned with. One is the substantial literature on media campaigns as well as interventions that intend to have effects working through social networks (Katz and Lazarsfeld 1955; Rice and Atkin 1989; Valente and Saba 1998). In the sociological literature, the most well-known model of network effects is that of Granovetter (1973), who argued that there is "strength of weak ties" in the sense that information can flow rapidly in communities even if the social ties between individuals are weak. Models of diffusion in this literature tend to be more specific in their modeling of how individuals within a community interact than those in the economics literature, and they often

model the influence of individual opinion leaders and followers, for example, as well as quantifying the "density" of personal information networks by the closeness of the ties between individuals (Valente 1995).

However, the evaluation of these interventions appears to be in its infancy. While there has been substantial work on advertising, commercial marketing, public relations, and public health campaigns, relatively little work has been conducted within the framework of randomized trials or quasi-experiments. By and large the evaluation method of choice has been a simple before-and-after, or evaluation by means of a crude comparison group, with consequent weak inferences. The microstructure of the diffusion of change induced by such campaigns also appears to be rarely evaluated by formal methods.

Another well-known intervention that has not been evaluated much is the I Have a Dream program. The initial program was begun by Eugene Lang in 1981, a businessman-turned-philanthropist who offered to help pay the college expenses of a group of disadvantaged sixth-grade students in a New York school if they would complete high school. The program attracted considerable media attention and led to the formation of a Foundation that currently oversees about 160 projects in 63 cities. The programs all follow the original idea of offering college financial assistance to elementary school students if they finish high school. Unfortunately, relatively few evaluations of these programs have occurred to date, and those that have been conducted have not attained a high level of rigor. Other similar programs have also sprung up to provide aid to students for higher education (U.S. General Accounting Office 1990), but these have likewise seen little evaluation effort. Nevertheless, these programs all clearly fall into the class of interventions whose intended effect is to provide examples of successful educational completion to other students and hence to work through social interactions.

3.5 Conclusions

In the literature on social interactions, theory has run considerably ahead of empirical testing, the development of policy interventions that work through social interactions, and the evaluation of such interventions. Although strong *prima facie* evidence for the existence of social interactions is present in many different types of evidence, the importance and magnitude of those interactions remain largely unknown,

and hence the desirability of developing policy interventions aimed primarily at those interactions has not yet been established. While there has been considerable empirical work on social interactions, particularly in the areas of peer group effects in education and neighborhood effects, the literatures have not successfully confronted the several basic identification and estimation problems summarized here and in much prior work. In addition, several relevant major policy interventions—desegregation and busing, for example, and the Gautreaux/MTO programs—have not been properly designed, or the appropriate outcome data collected, to measure social interactions *per se*.

Despite the rather discouraging empirical record to date, however, several methods of identifying social interactions and estimating their magnitude have been presented here. Methods for inducing exogenous variation in group members, and methods for inducing partial-population price and hortatory variation often permit the separate identification of exogenous and endogenous interactions. These methods have not been seriously applied in the empirical literature to date.

In addition, while past policy interventions have not been well designed to measure social interactions *per se*, some of the many opportunities to do so are outlined in the chapter. Some of the interventions that seem most promising for social interaction effects (e.g., the I Have a Dream programs) have not been evaluated at all. In designing new interventions, the most difficult problem is controlling for the presence of unmeasured contextual-environmental unobservables, but interventions that hold those unobservables relatively constant but that alter group composition in such a way as to allow measurement of social interactions to appear quite feasible. In addition, more attention must be paid to data collection at the local level, and to a more careful theoretical framework in mind, in order to make significant progress on many of the policy interventions of interest. These possibilities offer the prospect of learning much more about social interactions than is now known.

Notes

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1 See also Pollak (1976) for a well-known study of interdependent preferences, which is another form of social interaction.

2 Individual actions do, of course, influence other actions through the market (determination of prices, etc.) but these are not what are usually defined as social interactions, which occur through direct nonmarket influences.

3 The possible existence of multiple equilibria is especially important because it could be regarded as the key feature of social interactions models. The empirical difficulty in detecting the existence of multiple equilibria solely from the observation of nonlinear responses to small policy interventions is that it is unclear in general what size of stimulus is needed to move a society from one equilibrium to another. Indeed, one's intuition is that a particularly massive intervention may be necessary to move a society off of a long-established low-level equilibrium.

4 If there are multiple equilibria, then the population will settle down to a new equilibrium, and it is more likely that social interactions can be detected from the change in population outcomes.

5 This problem is related to the "\$50 bill puzzle." A \$50 bill lies on the sidewalk and the individual does not pick it up—why not? Most calculations show the return to additional education to be extremely high, even for disadvantaged youth, but their actions do not appear to respond to it. One hypothesis is that the youth do not correctly perceive the payoff.

6 See also Manski (1997) for several extensions.

7 We ignore the issue of how groups are defined and take that as exogenous. Manski (1993) emphasizes that the need to know group composition *a priori* is a fundamental problem in social interaction models. This requirement is driven by the need to exclude the group identifiers themselves from x , a requirement implicitly assumed in this model. An example of the problems that arise when this requirement does not hold is given below in the discussion of unobserved correlated variables.

8 The notation and model setup here differ somewhat from those of Manski. The most important difference is that Manski replaces y_{2k} and x_{2k} in equation (1), and y_{1k} and x_{1k} in equation (2), by their expected values in the group. The reduced form of such a model would be the same as equations (3) and (4) except that the reduced-form error terms would not contain the "other" error, namely, ε_{2k} would not appear in equation (8) and ε_{1k} would not appear in equation (9). As noted below, these error terms cannot be used for identification of social interactions in any case without unacceptable independence restrictions, so there is no important difference in the models in this respect. The analysis of identification on the basis of reduced-form coefficients is essentially the same here as in Manski (although Manski does treat correlated unobservables using a different notation; these are not considered here until section 3.2).

9 It is conceivable that the government may have the means to manipulate residuals, an issue that will be further discussed below. But knowledge of the residuals v_{1k} and v_{2k} , which the government could presumably estimate, do not identify the underlying residuals ε_{1k} and ε_{2k} , and hence manipulation of v_{1k} and v_{2k} would be meaningless.

10 If the system is thought to be out of equilibrium, more possibilities for identification are possible if there are lags. However, once again, it is unclear how a determination is made whether the data describe an in-equilibrium or out-of-equilibrium state. Assuming the incorrect equilibrium to describe the data will result in incorrect inference.

11 Most of the analysis here could be generalized to allow $x_{(-1)k}$ to be a nonlinear function of the x values in the set. However, the reduced form in that case would not be linear.

12 This assumes that the covariance in equation (12) is positive. The exact bias is $\{Cov(x_{i-g}, \mu_g)Var(x_{ig}) - Cov(x_{ig}, \mu_g)Cov(x_{ig}, x_{i-g})\}/A$, Where A is a positive number

13. Although not formally considered, it is nevertheless frequently mentioned in the empirical literature. For recent examples of such reference, see Datcher (1982) and Case and Katz (1991)

14. This assumes $\beta > 0$. The exact bias in the coefficient on x_{i-g} is $\{\beta Var(\zeta_{ig})Var(\mu_g) - \gamma Var(\zeta_{i-g})Var(x_{ig})\}/A$, where A is a positive number. In the absence of the term μ_g , least squares still produces an inconsistent estimate of γ but this is solely from the presence of measurement error and the direction of bias is toward zero

15. The mean y_{i-g} is not appropriate because it will be affected by whether individual i does or does not choose to join group g

16. These models often have multiple equilibria, however, which raises the issues discussed earlier in the context of the simultaneity problem

17. This model is more complex than the standard switching regression model because the actions of others are not exogenous and will themselves respond indirectly to the set of b_{ig} faced by all other individuals. A more formal analysis would require working out the equilibrium of the model, which is beyond the scope of this chapter.

18. I should stress that while these two literatures are larger than others in volume, this section does not do justice to the extensive literature across many social science disciplines where the concept of social interactions, or related concepts, has been discussed

19. An earlier study of Coleman (1961) reported the results of an extensive examination of social cliques in schools and presaged his interest in peer effects

20. For a similar conclusion in a recent survey, see Jaynes and Williams (1989, 374)

21. On the other hand, Henderson, Mieszkowski, and Sauvageau also had data at the classroom level, unlike most other studies

22. The study of Glaeser, Sacerdote, and Scheinkman (1996) should be considered to fall within this descriptive class, for that study decomposes the unexplained residual from a group-level outcome regression into a portion due to social interactions and a random portion, using parametric restrictions on the form of each to separately identify them, rather than constructing a testable hypothesis for social interactions

23. "Our first and strongest conclusion is that there is no general pattern of neighborhood or school effects that recurs across all outcomes" (174)

24. See the volume edited by Brooks-Gunn, Duncan, and Aber (1997) for more studies of this kind. See also the chapter by Duncan, Connell, and Klebanov (1997) for a discussion of the biases in studies of neighborhood effects

25. Alternatively, one could examine the effect on white students in schools where minority students were bused in, on the presumption that the contextual unobservables were fixed and hence any change in the outcomes for white students would likely reflect social interactions. The school peer-group effects literature indicates that there were relatively few effects of minority racial composition on white achievement scores (Schofield 1995)

26. A special problem in Gautreaux was that some of the moved minority families experienced harassment by white families after the move, which compounded the adjustment

problems of moving to a new environment (Rosenbaum 1995). The important question for evaluation is whether these effects are long-run or short-run, and whether they would be greater or smaller if a different number of families have moved in

27. As in the desegregation case, a better experiment to detect social interactions might be to move a large number of white residents into a minority-dominant public housing unit

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