Peer Effects and Spillovers: A Short Guide to Identification

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1   Background

Many economic outcomes are positively correlated within groups: test scores are generally correlated within schools, health outcomes within households, and income within neighbourhoods and countries. These positive correlations are often robust to conditioning on observed characteristics. Health outcomes within households may remain positively correlated after controlling for household income and wealth, and for local environmental factors. These conditional within-group correlations are consistent with three classes of explanations:

1. Group members share a common environment, which exerts a causal effect on their outcomes. For example, people living in the same location face similar local labour markets and so may have similar labour market outcomes.

2. Individuals sort into groups based on unobserved characteristics. These unobserved characteristics are in turn correlated with their outcomes, generating within-group correlation in outcomes without any causal relationship. For example, students may form friendships with other students who have similar work attitudes, leading to correlated test scores within social networks.

3. Individual outcomes depend directly on the other group members' outcomes or background characteristics. For example, members of the same household may infect each other with communicable diseases, leading to correlated health outcomes within households.

We typically refer to the second explanation in terms of "sorting" or "matching." We reserve the term "peer effects" for the third explanation. Peer effects can be further sub-divided:

1. Individual outcomes may depend on other group members' outcomes. For example, tuberculosis can easily pass from household member i to household member j.

2. Individual outcomes may depend on other group members' background characteristics. For example, household member i may be more likely to acquire tuberculosis from someone outside the household because member j smokes.

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Manski (1993) refers to the first type of peer effect as "exogenous" or "contextual," and the second type of peer effect as "endogenous." Note that this terminology is not consistent with the use of exogeneous and endogenous in the treatment effects literature.

   This distinction is important. Endogenous peer effects generate feedback loops: i's outcome influences j's outcome, which influences i's outcome, etc. Exogeneous peer effects do not generate peer effects: i's background characteristics influence j's outcome and the causal chain stops. The distinction depends on the definition of the outcome. The causal chain $X_i \rightarrow (X_j, Y_j)$ implies endogenous peer effects when X is the outcome and exogenous peer effects when Y is the outcome.

   These notes discuss identification of peer effects, from a relatively atheoretical perspective.[1] We begin with a simple peer effects model in section 2. We initially assume that the model is linear in peer group characteristics, that individuals are randomly assigned to groups, and that we observe group membership. Sections 3, 4, and 5 extend this model to allow for respectively nonlinearities, groups that are partly or fully unobserved, and groups that are non-randomly assigned. We then discuss when, if ever, results from peer effects can inform policy in section 6. In section 7 we turn to identifying treatment effects in the presence of spillovers. The ideas and methods in this literature are closely related to the peer effects literature, though the empirical applications seldom overlap. Section 8 covers suggestions for further reading.

2   The Linear Peer Effects Model

This section draws heavily on Manski (1993) and Sacerdote (2001). The canonical linear-in-means peer effects model assumes:

$$Y_{ig} = \alpha + \beta X_{ig} + \gamma \bar{X}_g + \delta \bar{Y}_g + \epsilon_{ig}, \tag{1}$$

where i and g index individuals and groups respectively. To make the discussion more concrete, assume that Y and X are student test scores and parents' education respectively. The model assumes that students' test scores are a function of their own parents' education, the mean education of their classmates' parents, and their classmates' mean test scores.[2] For simplicity, assume that X_{ig} and ϵ_{ig} are mean independent, so there are no omitted factors that are associated with student test scores and their parents' education. This assumption is certainly false but not important for our discussion. The parameters γ and δ measure exogenous and endogenous peer effects respectively. We cannot estimate equation 1 because Y appears on the left-hand and right-hand sides of the equation.

   We can evaluate the equation at the group mean, obtaining $\bar{Y}_g = \alpha + (\beta + \gamma)\bar{X}_g + \delta\bar{Y}_g + \bar{\epsilon}_g$. We then solve for $\bar{Y}_g$ and substitute this into equation 1. This yields

$$Y_{ig} = \frac{\alpha}{1-\delta} + \beta X_{ig} + \frac{\gamma + \beta\delta}{1-\delta}\bar{X}_g + \frac{\epsilon_{ig} + \delta\bar{\epsilon}_g}{1-\delta}$$
$$\equiv \pi_0 + \pi_1 X_{ig} + \pi_2 \bar{X}_g + \eta_{ig} \tag{2}$$

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[1] The notes only briefly discuss estimation. Most models in this literature are smooth functions of the data. If the parameters are identified, they can be consistently estimated by sample analogues. We briefly discuss inference, as the group structure of the data naturally leads to non-independent data points.

[2] The same argument holds for a model defined with "leave-one-out" means $\tilde{X}_{ig} = \frac{1}{n-1}\left(X_{1g} + \ldots + X_{i-1g} + X_{i+1g} + \ldots + X_{Ng}\right)$, where n is the group size. The algebra is just much more complex. Some applications use two-person groups, in which case the leave-one-out mean $\tilde{X}_{ig}$ is simply X_{jg}.

We call equations 1 and 2 the "structural" and "reduced-form" models respectively. The structural model is the economic relationship of interest that cannot be estimated. The reduced-form model is the estimable model derived from the structural model. This is consistent with the language of the Cowles Commission but does not correspond to the modern use of "structural." This equation can be estimated by OLS.

How should we interpret the OLS estimator $\hat{\pi}$? If students are randomly assigned to classes, then η_i and $\bar{X}_g$ are independent. $\pi_2 = \frac{\gamma + \beta\delta}{1-\delta}$ is identified and is non-zero if either γ or δ is non-zero. π_2 is thus interpreted as a measure of the strength of peer effects in the linear model.[3] What if students are not randomly assigned to groups? Then students' unobserved characteristics ϵ_{ig} may be correlated with their peers' parents education $\bar{X}_g$. This generates a relationship between η_i and $\bar{X}_g$, so ψ_2 is no longer identified. Random assignment is not necessary for identification of π_2 but it is sufficient.

This simple framework illustrates four general lessons for analysing peer effects models:

1. As $\delta \to 1$, the reduced-form parameters become infinitely large. This is a feature, not a bug, of the model. When $\delta = 1$, equation 1 implies "complete spillovers:" a one-unit change in $\bar{Y}_g$ raises Y_{ig} by one unit, which by definition raises $\bar{Y}_g$ by one unit, etc. This model implies that Y_{ig} equals the maximum feasible value of Y for all i, g. We regard this as implausible and so assume $|\delta| << 1$ in practice.

2. We have three reduced-form estimates and four structural parameters. We cannot separately identify exogenous peer effects γ and endogenous peer effects δ. We can only test the hypothesis that they are not jointly zero. This result is specific to the linear model. These effects can be separated in nonlinear models, such as when Y is binary. See Brock and Durlauf (2007) for a detailed discussion on this point.

3. The error term η_{ig} in the estimating equation 2 includes $\bar{\epsilon}_g$ and so is correlated within groups by construction. The variance estimator should allow for this correlation. If the errors in equation 1 are homoscedastic and independent within groups, then a random effects estimator is appropriate. In practice, the cluster-robust variance estimator is standard in the applied literature. This result is not specific to the linear model.

4. The key variable of interest $\bar{X}_g$ varies at the group level. If there are very few groups, then there is very little variation in $\bar{X}_g$. This will lead to very imprecise estimates of π_2. If group sizes are very large, then random assignment to groups and the law of large numbers mean that $\bar{X}_g \to_p E[X]$ for all g and there is no variation. Peer effects studies are generally more viable with a large number of small groups than a small number of large groups.[4] This result is not specific to the linear model. See Angrist (2013) for a more detailed discussion on this point.

3   More General Model Specifications

The linear model in equation 1 has many substantive limitations. For example, it assumes that there are no interactions between students' own characteristics and their peers' characteristics. It

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[3] We may also have $\psi_2 = 0$ if $\beta = -\gamma/\delta$. Most empirical and theoretical work rules this out by implicitly or explicitly assuming that β, γ and δ have the same sign.

[4] Using leave-one-out means generates slightly more variation in finite samples but does not solve the problem that $\tilde{X}_{ig} \to_p E[X]$ as group sizes go to infinity.

assumes that all relationships are linear, ruling out concave or convex relationships between parents' education and students' test scores. It assumes that only the mean peer group characteristics matter for peer effects, ruling out the possibility that the highest- or lowest-performing member of the group exerts a disproportionate influence.

There are many papers that test and reject the linear model in favour of models with nonlinear relationships and multiple measures of peer group composition. Sacerdote (2011) has a helpful review of this literature for education and presents a taxonomy of different nonlinear models adapted from Hoxby and Weingarth (2006).

It is difficult to distinguish between exogenous and endogenous peer effects in these more general models. In the linear model the two types of effects are well-defined but difficult to distinguish empirically. In nonlinear models the two types of effects may not even be well-defined. As a simple example, include an interaction between $\bar{X}_g$ and $\bar{Y}_g$ in equation 1 and try to derive a reduced-form model similar to 2. You will find that the reduced-form model is not even estimable.

Fully nonparametric identification of peer effects models is in principle possible. Consider, for example:

$$Y_{ig} = f\left(X_{1g}, \ldots, X_{ng}\right) + \epsilon_{ig}, \tag{3}$$

where n is the group size, G is the number of groups, and $N = G \times n$. We assume only that ϵ_{ig} is independent of X_{jg} for all $i \neq j$ (which holds under random group assignment). If $f_j\left(x_{1g}, \ldots, x_{ng}\right) \neq 0$ for some $j \neq i$ at some values $\vec{x}$ in the support of the random variable $\vec{X} = (X_{1g}, \ldots, X_{ng})$, then this model exhibits peer effects. This model allows interactions between own and peer characteristics ($f_{ij} \neq 0$ for $i \neq j$), nonlinearities ($f_{jj} \neq 0$), and for peer effects to occur through features other than the mean.

In practice, this model would be difficult to estimate. This is a fully nonparametric n-dimensional model, and the dimension would rise to $K \times n$ if there are K observed characteristics in X. Recall from the nonparametrics discussion that these estimators converge to the true value at a rate that depends on the dimension. Here the ratio of the sample size to the dimension is $\frac{N}{K \times n} = \frac{G \times n}{K \times n} = \frac{G}{K}$. So estimation will be completely infeasible unless the number of groups is much larger than the number of observed characteristics. Few datasets satisfy this condition. Hence, nonparametric estimation of peer effects model is extremely rare.[5] See Shalizi and Thomas (2011) for a discussion of nonparametric identification and Pinto (2011) for an example of semiparametric estimation.

Note that it may be possible to derive a valid test for the existence of peer effects in a nonparametric model even if we cannot identify the parameters of the model. See Graham (2008) for an example.

4   Defining Peer Groups

Most papers assume that every individual i is assigned to exactly one peer group g and that this group is observed. For example, peer groups are often defined in terms of administrative units (schools), geographic units (villages), or self-reported networks (friendship networks).

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[5] This discussion is sloppy on a number of dimensions. The rate of convergence depends on the specific nonparametric estimator (local polynomial, sieve, etc.) and choices of the the tuning parameters as well as the dimension of the regressors. This discussion is simply intended to build intuition and is not a substitute for serious independent reading.

There are several problems with this assumption. First, administrative or geographic units may be poor proxies for "true" peers. Foster (2006) considers the implications of this problem. Second, group membership may be entirely unobserved in the data. It may be possible to estimate group membership under additional assumptions. See Manresa (2014) for an example. Third, data on group membership may be incomplete. Many surveys include a network module that asks respondents about their K closest friends, to name K people they ask for advice about farming, etc. This omits data about links $K+1, K+2, \ldots$. Chandrasekhar and Lewis (2011) consider remedies for this missing data problem.

5 Alternative Approaches to Identification

Random group assignment is the simplest sufficient condition to identify the parameters in equation 2, or non-linear generalizations of this model. But it is not necessary. In general, *peer effects can be identified whenever individuals' own unobserved characteristics are not correlated with their peer observed characteristics.* For example, Imberman, Kugler, and Sacerdote (2012) study peer effects generated when students were reallocated across schools following a natural disaster, arguing that reallocation process allowed no scope for students to sort strategically into schools. We consider four approaches that the literature has used to identify peer effects without random or quasi-random assignment.

First, peer effects can be identified if (1) there is variation in the size of peer groups and (2) peer effects have a specific functional form. See Lee (2007) for an example of this approach. This approach has not been widely used in practice.

Second, peer effects can be identified from non-overlapping peer groups. Assume students have two types of peers, classmates and neighbours, and that these groups are not identical. Then student i has first-order peers who are in her class or neighbourhood and second-order peers who are in her classmates' neighbourhoods or neighbours' classes. If peer effects occur from first-order peers but not from second-order peers (and some other more technical conditions hold), then second-order peers' characteristics are a valid instrument for first-order peers characteristics in equations like 2. See Di Giorgi, Pellizzari, and Redaelli (2010) and Bramoullé, Djebbari, and Fortin (2009) for detailed discussion on these approaches.

Third, we can follow Graham (2008) in using an entirely different approach to identification. He refers to models like 2 as "excess sensitivity" models, where peer effects are identified by the sensitivity of individual outcomes to group composition. He lays out an "excess variance" framework, where peer effects are identified by the differential variance of individual outcomes across different types of groups. This approach makes a series of identification assumptions that are typically *stronger* than random assignment to groups. However, the assumptions can in principle hold in settings with self-selection into groups. This approach has not yet been widely used in practice.

Fourth, peer effects can be identified in application-specific structural models (where we now use "structural" in the more contemporary sense). We can in principle identify peer effects if (1) we correctly specify the group formation process and either (2) impose functional form assumptions on the outcome or group formation equation or (3) have an instrument that affects group formation but not individual outcomes. See Blume, Brock, Durlauf, and Ioannides (2011) for a review of several such models.

As a final note, we can in principle separate endogenous and exogenous peer effects in equation 1 if we observe an instrument for the mean peer group outcome $\bar{Y}_g$. This variable mst directly affect the outcomes of some but not all group members, but be unrelated to X_{ig} and $\bar{X}_g$. For example,

assume that Y and X are current and lagged test scores. We could randomly assign students to study groups to identify the π parameters in 2 and then randomly offer some members of some groups an incentive to study harder. The incentive is randomly assigned and so independent of own and peers' lagged test scores. It should also influence test scores directly by manipulating effort.[6]

6 Optimal Group Assignment

Some papers try, explicitly or implicitly, to infer policy implications from peer effects and particularly discuss better or worse ways of assigning students to groups. For example, assume $\pi > 0$ in equation 2. This implies that individuals in groups with higher values of $\bar{X}_g$ will have higher outcomes. We can in principle rearrange groups to change the distribution of outcomes. However, this is a problematic conclusion to draw.

First, equation 1 implies that mean outcomes will be identical across all possible group configurations. Moving an individual with high X_{ig} from one group to another will reduce the first groups' mean outcome and increase the second groups' mean outcome by the same amount. Rearranging groups can change the distribution of outcomes but not the mean. This is true only of the linear model. In nonlinear models, rearrangements can shift the mean but these estimates are sensitive to the observed range of the data. Consider a quadratic-in-means model estimated using randomly assigned groups:

$$Y_{ig} = \phi_0 + \phi_1 X_{ig} + \phi_2 \bar{X}_g + \phi_3 \bar{X}_g^2 + \epsilon_{ig} \qquad (4)$$

If $\phi_3 > 0$, then returns to peer group means are convex. Mean outcomes will then be higher if some groups have very high and other groups have very low means than if all groups have approximately the same mean. The "optimal" policy seems to be sorting individuals into homogeneous groups. (If $\phi_3 < 0$, then the opposite is true.) But random assignment with moderate-sized or large groups only generates groups with substantial within-group heterogeneity and approximately equal means. So creating these "optimal" homogeneous groups requires us to extrapolate outside the observed range of the data. Estimates of nonlinear models like 4 can be very sensitive to the range of the data.

Second, patterns of peer group interaction may be different under different policies for organizing groups. If individuals want to interact with people similar to themselves, then the amount of within-group interaction will be higher with homogeneous groups than with heterogeneous groups, The size of peer effects estimated with one type of group may not generalize to settings with the other type of group.

See Bhattacharya (2009), Carrell, Sacerdote, and West (2013), Garlick (2013), and Graham, Imbens, and Ridder (2013) for more discussion on these issues.

7 Identifying Treatment Effect Spillovers

Many papers study experiments whose effects may plausibly spill over from treated units to control units. For example, conditional cash transfers paid to treated households may be transfered to

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[6] This specific example is valid only if there are no direct peer effects from effort. This assumption is unlikely to hold in practice and illustrates the challenge with finding an instrument that generates no exogenous peer effects but affects outcomes and hence generates endogenous peer effects.

related control households, or may indirectly affect control households by changing local prices and/or wages. In Miguel and Kremer (2004), worm infections spread easily between children who live close to each other, so deworming some children may reduce infection rates for nearby control children.

The experimental literature deals with spillovers in three ways. *First, spillovers may be viewed as avoidable nuisance parameters.*[7] Consider identifying the effects on food consumption of cash transfer programme with randomly assigned eligibilty. Transfers raise treated households' income and may raise local food prices, which in turn reduces neighbouring control households' food consumption. Comparing food consumption between the treated and control households will then overstate the direct effect that the programme would have if prices were held constant. This can be (partially) avoided by applying treatment at the village level, as was done by Progresa/Oportunidades. Many education interventions adopt this approach, assigning schools to treatment and control groups rather than individual students within schools.

The first approach only works if (1) we can assign treatment in ways that minimize the risk of spillovers, and either (2a) we know (roughly) which control individuals are at risk of experiencing spillovers might occur or (2b) we know (roughly) through what channels spillovers might occur. If #1 fails, the we face *the second case, where spillovers are unavoidable nuisance parameters.* Consider again the example of Progresa. The rural component of Progresa was randomized at the village level, and villages are sufficiently far apart that spillovers seemed less likely. But the same approach was infeasible for the urban component of Progresa. It is very difficult to identify neighbourhoods within a city that are sufficiently "separate" that spillovers will be minimal. Conditions #1, #2a and #2b are unlikely to hold in this setting. More generally, many papers rely on definitions of "markets" that are unlikely to be independent of each other.

In this second case we may still be able to assess whether spillovers are a major problem for identifying the main treatment effect. We continue with the example of Progresa. Village-level randomization to treatment still does not rule out cash or food transfers between relatives in treatment and control villages. We can then compare both food consumption and food/cash gifts received across households in control villages who have immediate relatives in treatment villages and households in control villages who do not. If their outcomes differ, this suggests that treatment effects did spill over onto related control households (see #2a) via the transfer channel (see #2b). We can potentially exclude these related households from the control group when we identify the main treatment effect, though this approach has other limitations. Similarly, we can compare food prices in treatment and control villages. If they differ, this indicates that spillovers may occur onto households in the same village (see #2a) via the price channel (see #2b).

Third, spillovers may be viewed as objects of interest. Some experimental studies explicitly try to measure spillovers. For example, an experiment might offer incentive pay to some teachers at a school or some nurses at a clinic and try to measure spillovers on their colleagues who do not receive incentives. To illustrate this approach, assume that the outcome for individual i in group g is generated by

$$Y_{ig} = \psi_0 + \psi_1 T_{ig} + \psi_2 \bar{T}_g + \epsilon_{ig} \tag{5}$$

where $T_{ig} = 1$ if and only individual i is assigned to treatment and $\bar{T}_g$ is the fraction of individuals in group g assigned to treatment. Assume that treatment is randomly assigned, so ϵ_{ig} is independent of T_{ig} and $\bar{T}_g$.

This set-up illustrates four general points about estimating experimental spillovers:

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[7] Spillovers may be nuisance parameters for the researcher, as they complicate identification of the main effects, but desirable for the policymaker.

1. If treatment varies only at the group level, then $\bar{T}_g = 1$ in treatment groups and 0 in control groups and so is identical to T_{ig}. Thus we cannot identify the spillover effect ψ_2 and we cannot separately identify the main effect ψ_1 from the treatment effect. This is critically important for understanding many experiments that use group-level treatment assignment. We can generally only identify spillovers if at least some groups have partial treatment. See Angelucci and di Giorgi (2009) for a detailed discussion on this issue.

2. If treatment varies at the individual level but the treatment rate is approximately constant across groups, there is very little variation in $\bar{T}_g$. Any estimate of ψ_3 will be very imprecise. So experimenters that want to identify spillover effects should consider deliberately varying the treatment rate across groups. At minimum, identifying spillover effects requires two values of the treatment rate. The simplest approach has control units (e.g. clinics where no nurses receive incentives) and partial treatment units (e.g. clinics where some but not all nurses receive treatment).

3. The model as written assumes that spillover effects are linear in the fraction treated: moving from 10 to 20% group treatment has the same effect on individual outcomes as moving from 80 to 90% group treatment. If this is true, we only need to observe two values of $\bar{T}_g$ to identify ψ_3. If spillovers are nonlinear, we need groups with at least three different treatment rates to test for nonlinearity and we need continuous variation in the treatment rate to fully characterize the nonlinearity. See Baird, Bohren, McIntosh, and Ozler (2011) for a detailed discussion on this issue.

4. If group membership is observed at only baseline or endline and is time-varying, then the observed $\bar{T}_g$ suffers from measurement error relative to the true $\bar{T}_g$. If group membership changes through time independent of treatment, then we have a classical measurement error problem and $\hat{\psi}_3$ is attenuated. If group membership is affected by the treatment, then we have a non-classical measurement error problem and the bias on $\hat{\psi}_3$ cannot in general be signed. This is potentially very important for papers that try to measure spillovers via social networks, agricultural advice networks, etc. See Comola and Prina (2014) for a detailed discussion on this issue.

There is a large literature spanning multiple social sciences (and biostatistics) on identifying treatment effects in the presence of spillovers. Conventional treatment effects models are only identified under the assumption that there are no spillovers; statisticians refer to this as the "stable unit treatment value assumption" or SUTVA.

8 Further Reading

There are a variety of recent handbook chapters on peer effects and spillovers in economics. All of these discuss identification issues to some degree. Blume, Brock, Durlauf, and Ioannides (2011) review theoretical models of peer effects and consider their empirical implications. Epple and Romano (2011) review empirical work on peer effects in education, covering both theoretical models and empirical work. Graham (2011) reviews identification of peer effects and spillovers. His discussion is considerably broader than the focus of this note. In particular, he considers cases where peer effects are not identified but can be inferred from observed sorting behaviour under additional assumptions. Sacerdote (2011) reviews empirical work on peer effects in education, with a focus on experimental and quasi-experimental work.

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