

Learning and Social Effects

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A very important question in economics is to what extent individuals are influenced by others around them.

Contexts that are particularly interesting:

- Technology adoption (e.g. spread of technology)
- Fads/Social norms about things like education, contraception, etc.
- Peer effects in crime or other bad behaviors.

There are various reasons why this might happen:

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Finding out whether such phenomena exist and their magnitude is important because:

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1 Learning and peer effects: Identification problems with observational data

But learning about social effects is not easy. The typical regression for individual i belonging to group j is of the form:

$$y_{ij} = X_{ij}\beta + \bar{y}_j + \epsilon_{ij},$$

where \bar{y}_j is the average of the y_{ij} over all the members of the j group, except person i .

Here is an example from the worms paper we saw earlier: parents (and children) can decide not to take the drug (a waiver needs to be signed by the parent to authorize the child to take the drug). So the first column of table 7 regresses whether you take the drug on the fraction of people who take the drug in your school.

Results:

Potential problems with this regression:

-The definition of the “network”: what is the right definition?

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These types of problems are pervasive in all regressions of this form.

-e.g. adoption of a new technology on whether my neighbors adopt

-adoption of a new technology on whether my neighbors who have adopted have good profits

2 Learning and peer effects: How experiments solve the problem

2.1 Creating an exogenous “shock” to a part of a “social network”

The first task is to identify the relevant “social network.” It can be done through:

-presumption (village; school; co-workers)

-asking people who actually belong to their network (who they are talking to; can ask questions such as “who do you trust for agricultural advice?”).

Once the network has been identified, what is a first strategy for using an experiment to identify social effects?

So here, the “treatment” for individual i belonging to group j is that someone in group j got treated.

2.1.1 An example of large peer effects: 401k participation in the USA

The general problem: Americans save little for retirement. There are incentives through the tax code (401k, or IRA), and some employers match employees' savings.

Duflo and Saez (2003) are interested in two questions:

1. Does information play a role in 401k decisions?
2. Do the decisions of your peers (co-workers) play a role in 401k decisions?

They work in a university, which has numerous departments, which can be conceived of as groups of colleagues. In a first paper, they show that:

-in a regression like the one we showed, the 401k participation is correlated with others' decisions in the departments

-this is only along gender lines (women are influenced by women and men by men)

Still, there could be problems...

The experimental design is as follows:

1. First, randomly select departments (two-thirds)
2. Restrict the sample to those not yet enrolled in the 401k (eligible sample)
3. Within each department, select one half of those who are not enrolled, and send them an invitation to attend an information fair organized by the university, with a \$20 incentive if they go.
4. At the meeting, we record the department of every one who came, so we can compute average department attendance
5. We ask all the people who received the incentive letter to sign up to get their incentives, so that we know exactly who among them came.

First piece of evidence: peer effect in fair attendance, and in enrollement in the 401k. The random treatment is receiving the letter.

$$y_{ij} = \alpha + \beta L_{ij} + \gamma D_j + \epsilon_{ij}$$

Table 2, panel B. How do we read the coefficients? (Hint: remember that to get a letter, you must be in a “treated” department.)

Methodological aside: Encouragement design and randomization as IV

Forget about peer effects for a moment, and consider just the intervention of sending the letter with the incentive.

The invitation is called an “encouragement design”: there is an existing treatment (the information) that everybody is free to get, but we are encouraging some people to take it (by providing them with the incentive).

Call Z the “treatment” of receiving the incentive letter ($Z = 0$ or $Z = 1$).

Call Y the participation in the 401k.

Call T a variable equal to 1 if went to the information session, and 0 otherwise.

It is easy to get the effect on 401k participation of getting the incentive letter.

$$E[Y|Z = 1] - E[Y|Z = 0]$$

But this is not the effect of going to the information session, since some people go to the session without incentives, and vice versa. We call that the *INTENTION TO TREAT*.

Suppose now that we want to know the effect of going to the session.

If I compute:

$$E[Y|T = 1] - E[Y|T = 0]$$

Is that the effect? Why or why not? Are there other things we can do?

We can use the observation that, even though not everybody who is invited goes to the session, the probability of going is still much higher for those who get the letter (see table 1 and 2 for the difference).

$$E[T|Z = 1] - E[T|Z = 0] \neq 0$$

Assume that the treatment effect of going to the fair is constant:

$$Y = \alpha + \beta T + \epsilon$$

$$E[Y|Z = 1] - E[Y|Z = 0] = \beta(E[T|Z = 1] - E[T|Z = 0]) + E(E[\epsilon|Z = 1] - E[\epsilon|Z = 0])$$

One additional assumption: Z and Y are independent.

In practice this requires two things:

1. Those who receive the treatment are no different than those who do not.
2. Z does not have an effect on Y except for the fact that it affects T (there is no *direct* effect of the encouragement design except for affecting the variable we care about).

(1) is true here by construction, why?

But this is not true of (2)!! When in this situation, you must ALWAYS check that the instrument does not have a direct effect on the variable of interest. Having an experiment (such as an encouragement design) does *not* guarantee that you have a valid two-stage strategy for the effect of another variable.

Independence implies: $(E[\epsilon|Z = 1] - E[\epsilon|Z = 0]) = 0$

So:

$$E[Y|Z = 1] - E[Y|Z = 0] = \beta(E[T|Z = 1] - E[T|Z = 0])$$

And therefore:

$$\beta = \frac{E[Y|Z = 1] - E[Y|Z = 0]}{E[T|Z = 1] - E[T|Z = 0]}$$

An estimate of β is obtained by replacing the expectations by their sample counterpart (sample average). This is called the *WALD ESTIMATE*.

The intention to treat is called a “reduced form.” The variable Z is the “instrument”; the effect of the instrument of the treatment variable is called the “first stage.”

Why independence may fail even with an instrument:

Look at table 2. We show the number of people who have come to the fair in “treated department,” where a treated department is a department where 1/2 the members received a letter.

Can I use “being in a treated department” as an instrument for “went to the fair”?

In this case, there are two treatments of interest:

-being in a treated department

-going to the fair

If we find that going to the fair has a large effect, but being in a treated department has no effect?

We also have two instruments (since we varied along two dimensions)

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We can generalize the Wald estimate with a more general instrumental variables procedure (2-stage least square):

- First we regress going to the fair on the two instruments (being in a treated department and receiving the letter).

- Then we run fair attendance on the predicted value from that regression, and the dummy for whether the person is in a treated department.

(In practice, if you tell Stata what the instruments are, and what the treatments of interest are, it will do that in one step).

Results, column 1, table III. What do these results suggest?

Methodological aside (2): Interpretation of IV when the treatment effects are not constant

The equation:

$$Y = \alpha + \beta T + \epsilon$$

assumes constant treatment effect. Instead, the treatment effects could vary by people:

$$Y = \alpha + \beta_i T + \epsilon$$

Why could they vary in these contexts?

In this case, the simple logic we have been using to derive the Wald estimate does not apply any more.

Yet it turns out that the IV estimate still has an intuitive interpretation (the proof we leave for some other time, recitation, or the interested readers.... it takes some algebra). It requires one more interpretation: no one is discouraged to go to the fair because of the incentive.

The interpretation is that the IV is the treatment effect for *the compliers*. The compliers are those for whom $T = 0$ if $Z = 0$, and $T = 1$ if $Z = 1$. (i.e. they do not go to the fair if they do not get the incentive, but they do if they get it).

Can we know who the compliers are in our data?

Can we know what fraction of the sample are compliers.

Another interpretation of our results is that the fair has an effect, but the effect is stronger for those who decide to go with a friend (the social interaction compliers) than for those who decide to go just because of the incentive (the financial incentives compliers). Why might that be?

We cannot identify both the different treatment effects and the social effects. We have to make an assumption. In column 2-4 we assume no social effects, and we use the two instruments to separately identify the effect of going to the fair for those who go because of the incentive, and those who go because of their friends.

2.1.2 An example of NO peer effects: Fertilizer in Kenya

Regardless of the interpretation of the second stage, this example had large peer effects: the resulting participation in the 401k is as large in the social network as it is among the treated people.

Here is another example where we may have expected very large peer effects: learning about a technology. This is in fact the archetypal example of the type of problems that are studied in development economics.

Set up: fertilizer adoption in Western Kenya. The technology has been around for a while, but not very many people use it (about 20% in any given season).

Different possibilities for low use:

- Not profitable
- No information
- Difficulty in saving

Today we will study the first two reasons.

Experimental set up: farmers were randomly selected from the parents' list in a school.

Selected parents were given fertilizer for a plot of a size of 30 square meters. The fertilizer was applied with the help of a field officer, who also explained all of the procedures. At the end of the season:

Result 1: Fertilizer is profitable

Result 2: Farmers learn from experience in their field

Follow farmers' adoption of fertilizer and other seeds this season and next.

Result 3: No social effect

Farmers were asked who they talked to about agriculture. In order to study whether there are any social effects, who should be compared to whom?

Result 4: There can be learning from friends

To check that learning was possible, we randomly invited the neighbors to the demonstration plot. To check the effect of this intervention, who should be compared to whom?

Note: here, if we did a simple regression of fertilizer adoption on friends' adoption, what do you think we would get?

2.1.3 An example with negative peer effects!: Worms in Kenya

Our last example of this strategy will show us that the peer effects can be different from what we expect....

Remember the example of whether people take deworming drugs that we just saw at the beginning of the lecture. The non-experimental estimate showed a positive effect.

One note of warning comes from the same table: they also asked them who their friends were (they call these the “links”). When we look at adoption among friends, the coefficient is actually lower, even though it should have probably been higher if the mechanism was really peer effects.

The deworming experiment generates exogenous variation in the number of contacts that got treated.

Remember timing: there were “early treatment schools” (group 1 and 2) and late treatment schools (group 3). They surveyed groups 2 and 3, and asked parents who their links were. A parent had about 10 links, including about 3 links in an early treatment school that was not their own.

Then, controlling for total number of links, they asked whether the child was more likely to take the deworming drug in group 2 and 3 if they knew more children in group 1.

What do they find?

What are possible explanations for this result?

This shows that one needs to be particularly careful with these types of analysis: non-experimental estimates of peer effects are highly suspicious!

2.2 Randomly assigning an individual to the social network

All three examples we saw were based on the same principle: take the social network as given, and assign a shock to a part of the network to see what happens to the other part.

Another possibility (in some settings) is to study what happens when you randomly assign people to a social setting.

Examples: assigning students to classes; assigning students to roommates. Then you can ask what happens to me as I get assigned to a roommate with particular characteristics.

It turns out that randomly assigning students to roommates is done by some universities, e.g. Dartmouth, and a big state university. There are three papers that take advantage of this fact.

First paper is by Bruce Sacerdote, on Dartmouth. Conditional on some revealed preferences, assignment to a roommate is random at Dartmouth, since it is done by lottery.

For students i and j belonging to pair g , the regression is of the form:

$$GPA_{ig} = \alpha + \beta GPA_{jg} + X_{ig}\gamma + \epsilon_{jg}$$

Where X is the vector of control variables (everything you have to say that can influence your matching). He finds a strong effect of the other roommate's GPA on my GPA.

Can there be a problem with this regression?

The random assignment does not solve all the problems! We still have the issue that the roommates are subject to common influences, and they can also influence each other.

So this is not a great thing to do.

What we can do instead, is to see whether the characteristics of one roommate *before* they entered college affect the behavior of the other roommate once in college or after.

They study a big state university.

Kremer and Levy: Look at alcohol use.

If your roommate drank in high school, how does that affect your GPA (and what are the channels)?

Strategy: pre-frosh students can indicate choices about their residence in freshman year: location, gender of corridor, smoking room, single/double/triple occupancy.

Regression is:

$$GPA_{ig} = \alpha + \beta Drank_{jg} + X_{ig}\gamma + \epsilon_{jg}$$

Here X is a set of dummies for all the possible combinations of choices (+gender and race of the student).

NB: The roommate who was initially assigned is not always the roommate that I end up with: 5% of people switch. What do they use? What could they do instead? Would the results be very different?

Boisjoly and others: Look at attitudes. The question here is whether being exposed to someone from another group makes you more cooperative or less cooperative.

It is a central question for policy: the key Supreme Court decisions on affirmative action (Grutter vs. Bollinger) say that affirmative action is not permissible as a way to compensate for past discrimination, but it upheld affirmative action programs based on the value of diversity in education.

Existing laboratory studies (Aronson in particular) suggest that whether being exposed to the other group makes you more or less cooperative depends on the context. Experiments in camps, where you can get members of different tents to positively hate each other just by ascribing them an identity (description in Cialdini's book).

Interesting, but it does not provide much theoretical guidance as to what will happen on a college campus (real world situation... competitive or not?). It is important to see whether in practice, affirmative action makes people more or less cooperative. This university has a strong affirmative action policy, with black students having test scores about one standard deviation below those of white students. So if there is a place to look for animosity created by affirmative action, it will be there.

The data:

1. Roommate assignment
2. A survey filled out when they arrived on campus
3. A post survey done by telephone or internet after 2-5 years (some are still in college, some are gone). Questions are the same as those asked in other surveys of racial attitudes.

Regression is:

$$Attitude_{ig} = \alpha + \beta Char_{jg} + X_{ig}\gamma + \epsilon_{jg}$$

Results: see table.