

# SPILOVER EFFECTS

Manuela Angelucci

University of Arizona and IZA

22 May 2009

## Structure of this lecture

- Introduction
- Definition
- Types of spillover effects
- Why do we care
- Design and Identification
- Estimation
- Examples from papers
- Conclusions

## Introduction

- Welfare-enhancing interventions often have a specific target population (e.g. provide incentives to increase schooling for the poor, distribute de-worming drugs for infected children).
- However, the target population is often a subset of the local economy (the village, neighborhood, city, or nation) or of local institutions (the school district, the church, the extended family, etc.).
- The local non-target population may also be indirectly affected by the treatment through social and economic interactions with the treated (e.g. treated children may share textbooks with untreated children, raising enrollment for both groups, or de-worming a group of children may reduce untreated children by reducing disease transmission, lowering infection rates for both groups).

## Definition

- Consider a group of subjects (e.g. individuals, households).
- Some subjects are eligible ( $E = 1$ ) for some treatment ( $T = 1$ ), while others are not ( $E = 0$ ). [Eligibility may or may not be random]
- $Y_0$  and  $Y_1$  are potential outcomes in the absence and presence of the treatment. [e.g. infection rates without and with the drug]
- The Average Indirect Treatment Effect (*ITE*) is the effect of the treatment on the ineligible:

$$ITE = E(Y_1 - Y_0 | T = 1, E = 0)$$

- That is, the ITE identifies the effect of the treatment on subjects that are not meant to be treated. [e.g. effect of treating some pupils on infection rates of pupils not offered the drug]

## Types of spillover effects

- Externalities (e.g. health/contagion)
- General Equilibrium effects (e.g. active labor market policies)
- Peer Effects (loosely meant to encompass learning, social norms, cost-sharing, or resource-sharing - e.g. informal transfers for insurance and investment).

## Why do we care

There are at least three reasons why accounting for spillover effects is useful:

- 1 To correctly identify and estimate the Average Treatment Effect on the Eligibles (*ATE*).
- 2 To learn features of the local “economy” and of human behavior.
- 3 To design successful policies.

## Example: de-worming drugs

- From Miguel and Kremer (2004).
- High intestinal parasites infection rates in developing countries. They may lead to anemia, malnutrition, pain.
- Deworming drugs are cheap and are offered through mass school-based deworming (WHO-endorsed).
- Effect of drug on infection rates works through two channels: direct (through ingesting the drug) and indirect (through reducing chance of contact with contaminated fecal matter).
- Individual-level randomization: 1) underestimates ATE; 2) fails to measure externalities (1) and 2) make treatment appear more expensive/less effective than it actually is); 3) cannot separately identify direct and indirect effects, essential for successful policy design (e.g. partly subsidized treatment or mass compulsory de-worming)

## Spillovers and ATE (1)

- Consider the case of de-worming treatment externalities: treated children receive a de-worming drug, their infection rates drop, and reduce the infection rates of children who do not receive the drug.
- Example: infection rate without the drug is 45%.
- Suppose the drug lowers the infection rate to 5%.
- TRUE effect of drug on eligible children (ATE):  
 $5\% - 45\% = -40\%$

## Spillovers and ATE (2)

- TRUE effect of drug on eligible children (ATE):  
 $5\% - 45\% = -40\%$
- However, suppose we don't know that. Rather, we run the following experiment: 1) go to a school and split pupils randomly into two groups; 2) first group gets the drug, the second does not; 3) after the treatment, infection rates in the treatment and control groups are 5% and 25%
- ESTIMATED effect of drug on treated children:  
 $5\% - 25\% = -20\%$

## Spillovers and ATE (3)

- Problem: beneficial effect of drug is doubly underestimated.
- First, estimated direct effect is half as big as true one.
- Second, existence of indirect effect is entirely missed.
- Implication 1: mistakenly conclude drug is less effective than it actually is (hence reducing infections appears costlier than it is).
- Implication 2: wrong policy recommendations (either drop program/too costly, or try to sell drug - and fail because externality is big).
- Implication 3: experiment gives “wrong” information even if randomization works [i.e. one cannot infer existence of spillovers from data].

## Spillovers and ATE (4)

- Solution: figure out the relevant unit of analysis and randomize at that unit. [based on theory, observation, or introspection]
- Example: suppose externalities occur at the school level. Then randomly select treatment ( $T=1$ ) and control ( $T=0$ ) schools.
- Pupils offered drug in  $T=1$  schools only
- $ATE = E(Y_1 - Y_0 | T = 1) = E(Y | T = 1) - E(Y | T = 0)$
- Takeaway 1: we need some “theory” of what may cause the spillovers.
- Takeaway 2: to identify/estimate ATE in the presence of spillover effects, randomize at the “local economy” level (school, village, firm), not at the subject level (pupils within same school).

## Measuring Spillovers: *ITE*

- If one's goal is simply to evaluate benefit of treatment on the treated, the above design is good.
- However, by doing that we are missing out on valuable information.
- First, we still underestimate program benefits because we fail to measure program effect on the non-treated who are in contact with the treated. [wrong cost-effectiveness]
- Second, we fail to understand some of the mechanisms that make treatment successful. [social vs. private returns; feasibility of charging for drug in the future]
- Solution: double randomization.

## Measuring Spillovers: Double randomization

- Randomization 1: randomly assign “units” (e.g. schools) to treatment ( $T=1$ ) and control ( $T=0$ ) groups.
- Randomization 2: randomly offer treatment to a group of subjects ( $E=1$ ) in treatment units.
- There are three (four) groups of subjects, depending on unit type and eligibility type.
- $ATE = E(Y_1 - Y_0 | T = 1, E = 1) = E(Y | T = 1, E = 1) - E(Y | T = 0)$
- $ITE = E(Y_1 - Y_0 | T = 1, E = 0) = E(Y | T = 1, E = 0) - E(Y | T = 0)$
- This ATE may differ from one from previous design, because all students were given drug in treated schools; now only a subset of students receives drug in treated schools. [more realistic scenario depends on actual policy - e.g. mandatory treatment or subsidized offer]
- Takeaway 3: randomly assign “units” (e.g. schools) to treatment ( $T=1$ ) and control ( $T=0$ ) groups, then randomly treat a group of subjects ( $E=1$ ) in treatment units.

## Measuring Spillovers: non-random assignment

- Double randomization may be unfeasible in some settings. E.g. some programs may want to target *all* the poor in a given locality.
- Solution: replace second step with non-random assignment.
- Example: Progresa evaluation (rural MX). 1) Census of 506 villages; 2) random assignment of *villages* into treatment and control; 3) *all* households classified into eligibles and ineligibles based on observed data.
- Takeaway 4: when double-randomization not feasible/interesting, randomize at the “local economy” level and then group *all* subjects into eligible and ineligible. Collect data on these 4 groups.

## Identification and Estimation

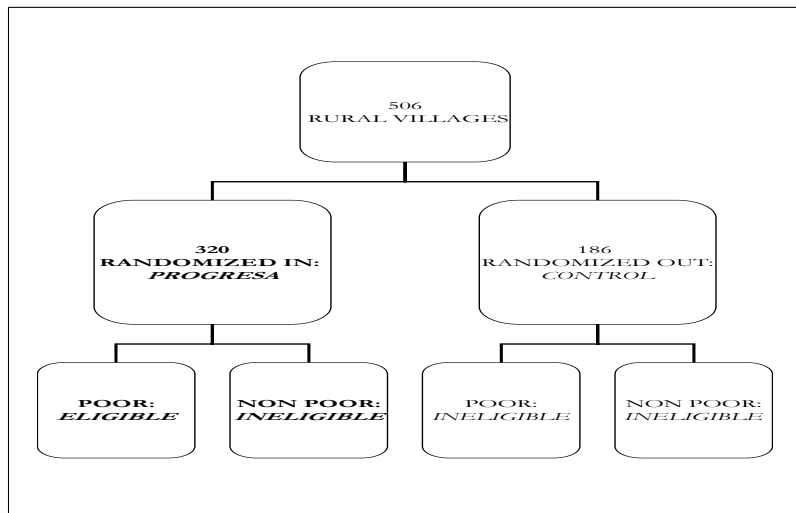
- Consider the non-random assignment of eligibility.
- $ATE = E(Y_1 - Y_0|T = 1, E = 1) = E(Y|T = 1, E = 1) - E(Y|T = 0, E = 1)$
- $ITE = E(Y_1 - Y_0|T = 1, E = 0) = E(Y|T = 1, E = 0) - E(Y|T = 0, E = 0)$
- $Y_{iv} = \alpha_0 + \alpha_1 T_v + \alpha_2 E_i + \alpha_3 T_v E_i + \epsilon_{iv}$
- The parameters  $\alpha_1 + \alpha_3$  and  $\alpha_1$  identify the ATE and the ITE.
- OLS consistently estimates these parameters under the assumptions that 1) the randomization “works” and 2) there are no spillovers to  $T = 0$  areas.
- You may condition on predetermined characteristics to improve the precision of the estimates.

## How to choose group shares

- Select “local economy units” and randomly assign a share  $p$  to treatment.
- Within treatment units, consider a share  $q$  of eligible subjects.
- How to choose  $p$  and  $q$  to estimate the coefficients of interest most precisely?
- Suppose we are interested in both the ATE and the ITE.
- Hahn and Hirano (2009) show that if we want to minimize the variance of both the ATE and the ITE estimators (and we care about them equally),  $q \approx 0.4$  and  $p \approx 0.7$ .
- Takeaway 5: rule of thumb for group shares is  $q = \frac{1}{2}$  and  $p = \frac{2}{3}$ .

## An existing program: Progresa

- From Angelucci and De Giorgi (2009).
- Objectives: improve education, health, and nutrition for poor households in rural Mexico.
- Average monthly grants are 200 *pesos* per household, i.e. about 143% and 100% of the average food consumption per adult for the poor and the non-poor in control villages.
- Evaluation design: 506 villages, of which 320 randomized to receive program from May 1998 ( $p \approx \frac{2}{3}$ ). Poor households (52%;  $q \approx \frac{1}{2}$ ) in Progresa villages eligible for treatment.
- Data: 1) program starts in May 1998; 2) census of 506 villages interviewed before and during program implementation.



## Objective: measure effect on consumption

- Program should increase consumption for the eligibles.
- Could there be an effect on the ineligibles too?
- If so, the existing design enables one to measure it.

## Need for informal insurance

- Income is very volatile in the sampled villages.
- Need for insurance/credit, but no formal markets.
- Households resort to informal consumption-smoothing activities (e.g. share resources with other households in the village), consistent with consumption being much smoother than income.

## Evidence on social networks

- No direct info on network members, however:
  - 1 Very low migration rates (about 5% left households, of which 20% moved within same village).
  - 2 About 80% of households have relatives in the same village.
  - 3 Within-village family networks are 50-50 mix of eligible and ineligible households.
  - 4 30% of transfers are from within village; 70% of loans are from family and friends.
- Therefore, village (or extended family within the village) is important unit of analysis for informal risk-sharing.

## A simple risk-sharing model

*Key idea: in a standard risk-sharing framework, when a household has a good shock, the consumption of **all** its social network increases through higher loans/transfers to the other members.*

## Testable hypothesis

- If households share risk within the village, then when eligible households receive Progresa grant, the consumption of ineligible households should go up.
- We can test this hypothesis by estimating the consumption ITE.
- If the randomization “works” and the program has no effects in control villages, then  $E(C|T = 1, E = 0) - E(C|T = 0, E = 0)$  identifies the ITE.

Average *peso* monthly food consumption per adult equivalent - levels and differences.

	Ineligibles		Eligibles	
	May 1999	Nov. 1999	May 1999	Nov. 1999
Control	213.69 [212.19]	206.71 [232.56]	159.92 [158.33]	153.7 [126.72]
Treatment	233.06 [303.79]	224.08 [285.61]	185.66 [193.81]	184.31 [172.25]
ITE	20.72 [10.19]**	18.84 [9.42]**	ATE 24.42 [5.64]***	29.86 [4.79]***

10 peso=1 dollar

## Alternative explanations

$$\Delta Y_i + \Delta L_i = \Delta C_i + \Delta S_i + \Delta I_i$$

The program may affect:

1. Consumption (C): increases by 20 pesos
2. Loans/gifts (L): increase by 15 pesos
3. Labor market (Y): no change
4. Goods market (Y): no change
5. Savings/Investment (S/I): 4 peso drop in S

## Results

- The 20 peso increase in consumption for the ineligible is caused by an increase in gifts/informal loans received.
- No change in goods and labor markets (e.g. no general equilibrium effect).
- Small drop in precautionary savings.
- Conclusion: the ITE on consumption works through informal transfers/loans (risk-sharing).
- Takeaway 6: to understand the *mechanisms* that cause the ITE, think about potential competing explanations and collect data on relevant outcomes.

## Implications

- 1 Program effect is underestimated.
  - Not measuring *ITE* underestimates the treatment effect on consumption by 12%.
- 2 *ITE* teaches us how households cope with high income variability absent formal credit/insurance.
- 3 Learning about mechanism suggests what to expect from scaling up (there will be positive consumption *ITE* as long as informal risk-sharing is common in local economy)
- 4 Change in policy design (with risk-sharing, target locality rather than household)?

## Conclusions

- 1 Have a “theory” of what may cause the spillovers.
- 2 Have an experimental design that accounts for spillover effects (at the very least) or enables researcher to measure them (because they are important/useful).
- 3 Collect data on eligibles and ineligibles in *both* treatment and control units.
- 4 Rule of thumb for group shares is  $p = \frac{2}{3}$  and  $q = \frac{1}{2}$ .
- 5 Collect data to explore potential competing causes for the spillovers.

## Conclusions

- 1 Have a “theory” of what may cause the spillovers.
- 2 Have an experimental design that accounts for spillover effects (at the very least) or enables researcher to measure them (because they are important/useful).
- 3 Collect data on eligibles and ineligibles in *both* treatment and control units.
- 4 Rule of thumb for group shares is  $p = \frac{2}{3}$  and  $q = \frac{1}{2}$ .
- 5 Collect data to explore potential competing causes for the spillovers.

## Conclusions

- 1 Have a “theory” of what may cause the spillovers.
- 2 Have an experimental design that accounts for spillover effects (at the very least) or enables researcher to measure them (because they are important/useful).
- 3 Collect data on eligibles and ineligibles in *both* treatment and control units.
- 4 Rule of thumb for group shares is  $p = \frac{2}{3}$  and  $q = \frac{1}{2}$ .
- 5 Collect data to explore potential competing causes for the spillovers.

## Conclusions

- 1 Have a “theory” of what may cause the spillovers.
- 2 Have an experimental design that accounts for spillover effects (at the very least) or enables researcher to measure them (because they are important/useful).
- 3 Collect data on eligibles and ineligibles in *both* treatment and control units.
- 4 Rule of thumb for group shares is  $p = \frac{2}{3}$  and  $q = \frac{1}{2}$ .
- 5 Collect data to explore potential competing causes for the spillovers.

## Conclusions

- 1 Have a “theory” of what may cause the spillovers.
- 2 Have an experimental design that accounts for spillover effects (at the very least) or enables researcher to measure them (because they are important/useful).
- 3 Collect data on eligibles and ineligibles in *both* treatment and control units.
- 4 Rule of thumb for group shares is  $p = \frac{2}{3}$  and  $q = \frac{1}{2}$ .
- 5 Collect data to explore potential competing causes for the spillovers.