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## ABSTRACT

### Program Evaluation and Spillover Effects<sup>\*</sup>

This paper is a practical guide for researchers and practitioners who want to understand spillover effects in program evaluation. The paper defines spillover effects and discusses why it is important to measure them. It explains how to design a field experiment to measure the average effects of the treatment on eligible and ineligible subjects for the program in the presence of spillover effects. In addition, the paper discusses the use of nonexperimental methods for estimating spillover effects when the experimental design is not a viable option. Evaluations that account for spillover effects should be designed such that they explain the cause of these effects and whom they affect. Such an evaluation design is necessary to avoid inappropriate policy recommendations and neglecting important mechanisms through which the program operates.

JEL Classification: C93, C81, D62

Keywords: impact evaluation, spillover effects, field experiments, data collection, Indirect Treatment Effect, program mechanisms

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## **1. Introduction**

This paper is a practical guide to spillover effects: what they are, why it is important to measure them, and how to design an evaluation to measure the average effects of the treatment in the presence of spillover effects on subjects both eligible and ineligible for the program.

Welfare-enhancing interventions often have a specific target population. For example, conditional cash transfer (CCT) programs in Latin America and elsewhere target the poor, and other interventions provide incentives to increase schooling for poor children, from providing equipment to distributing deworming drugs to infected children. However, the target population is often a subset of the local economy, loosely defined as the geographic unit or local institution within which the target population lives and operates. In this sense, we can consider the village, the neighborhood, the city, the municipality, the school district, or the extended family as the relevant local economy.

The treatment may also affect the local non-target population. For example, the recipients of CCTs may share resources with or purchase goods and services from ineligible households who live in the locality, affecting also the ineligibles' incentives to accumulate human capital. Children who receive free textbooks and computers may share them with untreated children (such as relatives, friends, and members of the local church), increasing enrollment levels in both groups. Supplying deworming drugs to a group of children may benefit untreated children by reducing disease transmission, lowering infection rates for both groups. If parasites affect school performance (for example, by reducing emetic iron, causing weakness and inattention) the treatment may result in both treated and untreated children learning more. An intervention that improves the supply and quality of water to only some beneficiary households in a neighborhood is likely to have effects on all residents, ranging from an increase in property values to a decrease in infection rates. In addition, if the intervention includes the provision of hygiene information and education campaigns, we might expect the information to flow from person to person within the community.

While the examples so far describe positive spillover effects, such effects may also be negative. Introducing genetically modified (GM) crops may contaminate neighboring organic crops, reducing their value. Similarly, eradicating pests from one farm may cause them to move to nearby farms, or providing training to a group of people may decrease the employment likelihood and decrease the wages of close substitutes in production. Examples of negative spillover effects are also documented in the literature on crime displacement (see,

among others, Di Tella and Schargrodsky, 2004 and Yang, 2008).

In some cases, spillover effects are intended. For example, agricultural extension programs encourage participants to adopt a certain technology and hope that this will induce further adoption within the community or in neighboring communities. Immunization campaigns among high-risk populations lower the likelihood of contagion, reducing the infection rate among low-risk populations. Whether intentionally or not, nonparticipants can be affected by programs, and these spillover effects should be taken into account when conducting an impact evaluation. Failure to perform such evaluations results in biased estimates of program impacts, leading to inappropriate policy recommendations and incorrect understandings of data-generating models.

For example, suppose that, to study the effect of a deworming drug on school performance we randomly select half the pupils of a school and treat them with the drug. We then compare the grades of treated and untreated children. Since the drug is likely to result in decreased infection rates among both treated and untreated children, it is possible that both groups' performances may change. That is, suppose deworming children increases the average grades of treated pupils by 10 percentage points and of untreated pupils by 4 percentage points. The Average Treatment on the Treated effect<sup>1</sup> is 10 percentage points. However, if we simply compare the grades of treatment and control pupils, we will observe only a six percentage point increase. In sum, the failure to recognize the possibility that the drug may affect untreated pupils also and to design the experiment accordingly will result in a double underestimate of the treatment's effectiveness. Not only will its effect on the treated be underestimated, but its effect on the untreated will also remain unmeasured. This may result in incorrect policy conclusions (for example, a decision to discontinue a program because it is not cost-effective).

Conversely, when the spillover effects are negative, one overestimates the treatment effect on the treated and fails to estimate its negative indirect effects. Indirect effects can be estimated in two ways. At the very least, the evaluation design must select a control group that is not indirectly affected by the program. This will enable researchers to measure the program effect on eligible subjects. If possible and relevant, the design should also allow for the measurement of spillover effects, as these effects are often important policy parameters in themselves.

To design an evaluation that accounts for the presence of spillover effects, one needs

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<sup>1</sup>The Average Treatment on the Treated effect can be defined as the impact of the program on subjects that are eligible for it. In our case this is the effect of the deworming intervention on the treated pupils.

to understand why and how these effects occur. This knowledge would help researchers to identify the subset of nonparticipants is most likely to be indirectly affected by a particular treatment. For example, in the case of a deworming drug for children, one has to know how people become infected (by being in contact with contaminated fecal matter) to understand which other people are likely to benefit from the deworming (classmates, friends, and families of dewormed children, who will be less likely to come into contact with contaminated fecal matter). Importantly, the evaluation must be carefully designed to account for spillover effects before the program is implemented. Spillover effects cannot be detected accurately *ex post* unless the design considers their existence from the start.

The following section describes different types of spillover effects. Section three explains why it is important for an evaluation design to account for the presence of these spillover effects. Section four explains how to design an experiment in the presence of spillover effects. Section five discusses how to use nonexperimental methods when an experiment is not a viable option. Section six provides a specific example using the evaluation of the CCT program PROGRESA. Section seven concludes with a summary of the key recommendations this paper makes to aid the design of evaluations that account for the presence of spillover effects.

## **2. Types of spillover effects**

In what follows, we describe four types of spillover effects that are particularly relevant in the development and health economics literature. We call these four types of spillover effects (1) externalities, (2) social interactions, (3) context equilibrium effects, and (4) general equilibrium effects.<sup>2</sup>

Externalities. These effects operate from the treated subjects to the untreated population. A particularly relevant domain is health. Miguel and Kremer (2004) show that deworming drugs can have an indirect effect in addition to the direct effect of ingesting the drug. Supplying deworming drugs to a group of children may benefit untreated children by reducing disease transmission, lowering infection rates for both groups. If parasites affect school performance (for example, by reducing emetic iron, causing weakness and inattention), the treatment may result in both treated and untreated children learning more.

Besides contagion effects in health-related interventions, externalities may arise from interventions that change the degree of environmental pollution (for example, Lipscomb and

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<sup>2</sup>These labels are somewhat arbitrary, but are a useful way of grouping similar types of spillover effects.

Mobarak, 2013) or that introduce GM seeds, which then alter the genetic makeup of non-GM plants through cross-pollination (for example, Rieben, Kalinina, Schmid, and Zeller 2011).

Social Interactions. The local nontarget population may also be indirectly affected by the treatment through social and economic interaction with the treated. For example, the recipients of CCTs may share resources with, and affect the incentives to accumulate human capital of, ineligible households in treated localities (Angelucci and De Giorgi, 2009). Recipients of human-capital-enhancing programs who receive free textbooks and computers may share them with untreated children (such as relatives, friends, and members of the local church), increasing enrollment for both groups. Similarly, Bobonis and Finan (2009) identify neighborhood peer effects on children's school enrollment decisions within the Mexican CCT PROGRESA program. In particular, they find that peers have considerable influence on the enrollment decisions of program-ineligible children, and these effects are concentrated among children from poorer households.

Context equilibrium effects. These spillover effects stem from an intervention that affects the behavioral or social norms within the contexts (for example, a locality) in which these interactions are relevant. For example, Avitable (2012) shows that exogenously increasing the rate of cervical cancer screening among eligible women in rural Mexico increases the screening rate for ineligible women, changing the previous social norm by which husbands prevented women from being screened by male doctors.

General equilibrium effects. These effects stem from interventions that affect equilibrium prices through changes in supply and demand. They naturally arise in active labor market programs (for example, Heckman, LaLonde, and Smith, 1999). With few exceptions (for example, Angelucci and De Giorgi, 2009; Mobarak and Rosenzweig, 2014), most studies of spillover effects in developing countries focus on the other mechanisms discussed above. Nevertheless, studying general equilibrium effects has both policy and economic relevance. At the policy level, such studies determine whether the general equilibrium effects reinforce or offset the partial equilibrium effect of an intervention. At the economic level, they provide insights into the local economy and people's preferences.

Consider the case of PROGRESA. Cash transfers may increase goods prices through an increase in demand. Price increases would change consumer and producer welfare in opposite directions, influencing the distribution of benefits and either reinforcing or offsetting the partial equilibrium effect of the transfer on its recipients, depending on their consumer and producer mix. The more segmented the local goods market, the stronger the price effects. The program might also change wages and labor income through two different channels.

First, the transfers may have an income effect for households that would have sent their children to school regardless of the program, reducing labor supply. Second, the program may reduce income for households that would not have sent their children to school in the absence of the program, increasing their labor supply. The net effect on labor supply, wages, and labor earnings is unclear a priori.

While one can classify the types of spillover effects, the evaluation designs we will discuss do not necessarily enable one to distinguish one from the other. One way to disentangle a specific type of spillover effect is to carefully design survey instruments to elicit information on the relevant type of unintended response. This highlights once more the importance of understanding which type of spillover effect might occur and how before designing the evaluation (and the program).

### **3. Identification with an experimental design**

#### **3.1 Treatment effects in the presence of spillovers**

Accounting for spillover effects is necessary for correct identification and estimation of direct/intended and indirect/unintended treatment effects. Measuring these two effects enables one to design more effective policies and to learn features of the local economy and of human behavior.

First, it is useful to define the two parameters of interest. Consider a group of subjects (for example, individuals, households) who live in areas where a treatment is offered ( $T=1$ ) or not offered ( $T=0$ ). In  $T=1$  areas, some subjects are eligible for the treatment ( $E=1$ ) while others are not ( $E=0$ ). The variables  $Y_0$  and  $Y_1$  are potential outcomes in the absence and presence of the treatment, respectively. The Average Treatment Effect on the Eligibles ( $ATE$ ) is the effect of the treatment on eligible subjects:

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

That is, the  $ATE$  identifies the effect of the treatment on subjects that are meant to be treated.<sup>3</sup>

While the outcome in the presence of the treatment is the observed outcome for eligible subjects in the treated area  $E(Y_1|T=1, E=1)=E(Y|T=1, E=1)$ , the potential outcome in the absence of the treatment,  $E(Y_0|T=1, E=1)$ , is unknown. This missing counterfactual problem can be solved if the two following non-testable assumptions hold: first, that the

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<sup>3</sup> If all eligible subjects take up the programme, then this is the average treatment on the treated effect.



treatment assignment is independent of the potential outcomes and second, that the treatment status of any subject does not affect the potential outcomes of other subjects. In statistics, these assumptions are called the unconfoundedness assumption and the Stable Unit Treatment Value Assumption (SUTVA).<sup>4</sup> Randomizing eligibility ensures that unconfoundedness holds. Therefore, absent spillover effects in  $T=1$  areas, then  $E(Y_0|T=1, E=1)=E(Y|T=1, E=0)$ . However, the presence of spillover effects violates the SUTVA. Therefore, the average observed outcome for the ineligible in  $T=1$  areas is not the average potential outcome in the absence of the treatment.

To solve this problem, one can use a double randomization or conduct a partial population experiment (Moffitt, 2001).<sup>5</sup> The idea of double randomization is to first identify the relevant local economy, that is the socioeconomic unit within which the spillovers occur. Then, one randomly assigns units (for example, localities, schools) to the treatment ( $T=1$ ) and control ( $T=0$ ) groups. Lastly, one randomly assigns eligibility ( $E = 1$ ) in all units.

There are thus three distinct groups defined by treatment and eligibility: eligible and ineligible subjects in treatment units and (all) subjects in control units. Absent spillover effects from treatment to control units, if the two randomizations are effective one can identify the ATE as follows:

$$ATE=E(Y_1-Y_0|T=1,E=1)=E(Y|T=1,E=1)-E(Y|T=0) \quad (1)$$

because unconfoundedness and the SUTVA hold for subjects in control units.

Besides providing unbiased estimates of the ATE, this design enables measurement of spillover effects. Define the Indirect Treatment Effect as the average effect of the treatment on ineligible households. If the two previous assumptions hold, one can estimate this parameter by comparing the observed outcomes of ineligible in treatment units with subjects in the control group:

$$ITE=E(Y_1-Y_0|T=1,E=0)=E(Y|T=1,E=0)-E(Y|T=0) \quad (2)$$

While this design solves the problem of the missing counterfactual for the identification and estimation of the ATE and ITE parameters, it is important to emphasize that the SUTVA is still violated in treatment units. Indeed, testing that  $ITE=0$  is a test of the SUTVA violation. In this case, one can think of the estimated ATE as the combination of the true ATE (the ATE under the SUTVA) and the spillover effect, and both parameters may depend on the share of the treated unit. Under the additional assumption that these two effects

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<sup>4</sup> To identify average treatment effects, a weaker assumption than unconfoundedness is required, namely  $E(Y_0|T=1)=E(Y_0|T=0)$ .

<sup>5</sup> For example, Bobonis and Finan (2009) follow this approach to estimate education peer effects in PROGRESA.

are additive, one can estimate the true ATE by subtracting the estimated ITE from it.

To identify how the spillover effects vary with the proportion of the treated unit, one must further randomize the share of the treated unit. Crepon, Duflo, Gurgand, Rathelot, and Zamora (2013) use this type of experimental design to study the displacement effects of labor market policies. Baird, Bohren, McIntosh, and Ozler (2014) discuss various parameters that can be identified with this design.

This last point has important policy implications. If the spillover effects are a function of the share of the local economy that is treated, the estimated ATE will vary as the program is scaled up (for example, by treating all subjects in one unit). To understand how spillover effects vary with the share of the treated population, a different design, in which the fraction of eligible subjects in a given area is treated, is also randomized.

### **3.2 A numerical example**

A numerical example can illustrate the importance of an experimental design that accounts for the presence of spillover effects. Consider the case in which the treatment is a deworming drug and the subjects are pupils of a school. The outcome of interest,  $Y$ , is the rate of infection with intestinal parasites. Since pupils in the school study and play together, providing a group of students with deworming drugs may also affect the infection rate of pupils that are not directly treated with the drug.

The drug affects infection rates directly—by ingesting the drug, a child becomes parasite free—and indirectly—worms die off over time, so worm loads naturally decrease in the absence of reinfection. Therefore, when some pupils take the drug, the re-infection rate for other children drops, as do the chances of coming into contact with contaminated fecal matter. At the same time, there is considerable seasonal and inter-annual variation in worm infection rates and intensity due, for example, to weather variation.

Consider a school in which a group of pupils is offered the deworming drug ( $E = 1$ ) and another group is not ( $E = 0$ ). The ATE is the average effect of the deworming drug on the infection rate of eligible pupils, while the ITE is the effect of treating the eligible pupils on the infection rates of pupils not offered the drug. In this particular example, the ATE will be some combination of the first and second effects described above (the deworming effect of the drug and the lowered re-infection rates), while the ITE will be only caused by the decreased contact with contaminated feces. That is, offering the deworming drug generates a positive externality: treated children receive a deworming drug, decreasing direct infection rates and reducing the infection rates of children who do not receive the drug.

Suppose that the baseline infection rate was 70 percent and the rate in the absence of the treatment was 80 percent, that is, the infection rate would have increased over time and this is unknown to the evaluators. Assume that treating a random group of pupils in a school decreased this rate to 10 percent for the eligibles and 60 percent for the ineligibles. In this case, the ATE and the ITE would be -70 and -20 percentage points. However, the policy evaluators are unaware of these numbers. To measure this effect, they split the school pupils into two randomly assigned groups and administer the drug to one group only. The naïve evaluators compare the infection rates of the two groups of pupils, exploiting the randomization; that is, they assume that the infection rate for the control group is the same rate we would observe in the absence of the treatment, or that

$$60\% = E(Y | T = 1, E = 0) = E(Y_0 | T = 1, E = 1).$$

As mentioned, we know this is untrue. The naïve evaluator would estimate an ATE of -50 percentage points, given by

$$E(Y | T = 1, E = 1) - E(Y | T = 1, E = 0) = 10\% - 60\% ,$$

underestimating the true ATE (in absolute value) by 20 percentage points. Moreover, the evaluator would fail to notice that the ineligibles are also indirectly benefiting from the deworming drug, as their infection rate has also dropped.

To understand the magnitude of this double underestimation of the program effect and its negative policy implications, suppose the school has 1,000 pupils and the treatment and control groups (with pupils assigned randomly to each group) have an equal size of 500 students each. In the absence of the treatment, 800 pupils would have been infected (equally distributed between treatment and control group). After treating 500 students, only 50 treated pupils (500x10%) and 300 control pupils (500x60%) are infected, for a total of 650 healthy children, compared to 200 in the absence of the treatment. However, the naïve evaluator compares the infection rates of the two groups after administering the drug and erroneously believes that the drug was successful for 250 pupils in the treatment group and none in the control group. That is, first the evaluator fails to notice that 100 other students in the treatment group have benefited from the drug, and then s/he fails to notice that the drug has also reduced the infection rates for the control group, with only 300 infected pupils rather than the original 400. In total, 200 more children are not infected compared to the evaluator's

estimate.<sup>6</sup>

Suppose the drug costs US\$1 per child and the expected benefits of not having worms is 1.5. The naïve evaluator would conclude that this treatment is not cost effective, because its cost of US\$500 is higher than its benefit of US\$375 (1.5x250). In fact, the drug *is* cost effective, because when 500 out of 1,000 pupils are treated, 450 pupils stop being infected,<sup>7</sup> for a total benefit of US\$675 (1.5x450), exceeding the drug's cost. The drug would also be evaluated as cost effective if the evaluator were to properly measure only its ATE, as the benefits for the treated would be US\$525 (1.5x350).

Randomizing treated schools first and then randomly choosing eligible pupils within the treated schools would have solved this problem, as the infection rate in the control schools would have been 80 percent in expectation, providing the evaluators with the right counterfactual outcome.

To summarize the lessons from this simple example, failing to implement an evaluation design that accounts for spillover effects results in a double underestimation of the program's impact: first, the average effect on the eligible children is underestimated; second, the effect of the treatment on the ineligible children goes completely unnoticed. This double underestimation can have result in negative policy implications. Researchers may mistakenly conclude that the treatment is less effective than it actually is (and that reducing infections is more costly than it is). Incorrect policy recommendations could be made as a result (for example, to drop the program, because it is too costly). Importantly, the experiment yields wrong information, even if the randomization is performed successfully. That is, one cannot easily infer the existence of spillover effects from data alone. A conceptual framework of how the treatment may indirectly affect the outcomes for ineligible subjects is essential for understanding whether and the extent to which spillover effects may occur.

### 3.3 A different design

In many interventions, eligibility for the program within the unit is decided on the basis of predetermined criteria (that is, poverty score), meaning that randomization within the unit is unavailable in these settings. However, it remains possible to measure the spillover effects of the treatment on ineligible subjects living (or studying, working, and so forth) in treatment

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<sup>6</sup> Having baseline (t=0) as well as follow-up (t=1) data does not help, as the difference-in-difference estimate of the naïve ATE is still the same, given that  $ATE = [E(Y|T=1, E=1, t=1) - E(Y|T=1, E=1, t=0)] - [E(Y|T=1, E=0, t=1) - E(Y|T=1, E=0, t=0)] = E(Y|T=1, E=1, t=1) - E(Y|T=1, E=0, t=1)$ .

<sup>7</sup>In principle, it is also possible to define a total treatment effect as the sum of ATE and ITE. In this simple example this would equal 45 pupils who stopped being infected. However, in practical application it is not always immediately clear how to define the total treatment effect.

units. The only difference in the design is that the second step has a nonrandom assignment. There are now four distinct groups, depending on unit type and eligibility type: eligible subjects ( $E=1$ ) in treatment ( $T=1$ ) and control ( $T=0$ ) units and ineligible subjects ( $E=0$ ) in treatment ( $T=1$ ) and control ( $T=0$ ) units.

Treating the eligible subjects may affect ineligible subjects' outcomes. Therefore, as long as we observe outcomes for the four groups we can measure the treatment effect on both eligible and ineligible subjects:

$$ATE = E(Y_1 - Y_0 | T=1, E=1) = E(Y | T=1, E=1) - E(Y | T=0, E=1) \quad (3)$$

$$ITE = E(Y_1 - Y_0 | T=1, E=0) = E(Y | T=1, E=0) - E(Y | T=0, E=0) \quad (4)$$

These comparisons identify the two average treatment effects as long as: (1) the unit randomization works, that is, there are no ex-ante systematic differences in the characteristics of eligible subjects in treatment and control units and of ineligible subjects in treatment and control units; and (2) any spillover effect of the treatment occurs only within the treated unit.

For example, the Kenya deworming project excluded older girls (ages 13 and older) from deworming due to the risk that the drugs could cause birth defects. To measure spillover effects, the first step is, as before, to randomly group schools into a treatment and control group. The second step is to offer the drug to all boys and younger girls in treatment schools. The four groups are boys and younger girls ( $E=1$ ) and older girls ( $E=0$ ) in treatment ( $T=1$ ) and control ( $T=0$ ) schools. There may be spillover effects of the deworming drug among the older girls attending the treatment school. Therefore, one should collect data on all pupils in the two groups of schools. In this way, one can measure the effect of offering the drug to boys and younger girls on the infection rates of both eligible and ineligible students.

In some cases, determining eligibility is more complex and involves the collection of multiple variables. For example, for conditional cash transfer (CCT) programs, eligibility is determined by poverty status. In this case the evaluator needs to collect enough data to compute a continuous poverty indicator for both eligible and ineligible subjects in treatment and control units. This process of ex-ante data collection for eligible and ineligible subjects, as well as ex-post collection of information on the outcomes of interest, makes the estimation of spillover effects more costly and time consuming.

### 3.4 Understanding the mechanisms

A well-conceived evaluation design should be based on some hypothesis about whether

spillovers are present and why they exist. After documenting the existence of spillover effects and measuring their magnitude, it is useful to understand the mechanisms that generate them and in so doing attempt to verify the initial hypothesis. A clear idea of such mechanisms may help in the design of effective policies.

Understanding the causes of spillover effects almost always requires more data collection. For example, consider again the case of treating younger girls and boys with a course of deworming drugs using the experimental design discussed in the previous section. Suppose that, after noticing that infection rates among older girls are also lower because of the treatment, the evaluator wants to understand whether this is caused by a reduction in contaminated fecal matter or by a change in basic health behavior, such as hand washing. In that case, the evaluator would need to collect data on these two additional variables, measuring the contamination rates and the frequency of hand washing.<sup>8</sup> If the evaluator were to find that hand-washing frequency does not differ between older girls in treatment and control schools, but the level of fecal contamination is lower in treatment schools, the result would suggest this latter channel is the main cause of the effect.

In this specific example, the additional evidence is, at most, suggestive. If one wanted to compare the benefits of basic health education to the effects of deworming drugs and the interaction of the two treatments, the ideal experimental setting would be one in which there were four random groups of schools. Younger girls and boys in these four groups would receive respectively: (1) the drug only, (2) the health education only, (3) both the drug and the health education, and (4) neither. Comparing pupils' infection rates in each of the first three groups with the fourth would provide estimates of the effect of each of the three treatments on eligible pupils, while the comparison for older girls would provide estimates of the effect of each of the three treatments on the ineligible pupils.

A clear idea of the causes of the spillover effects can also lead to a better understanding of what ATE and ITE are really capturing, especially in cases in which different types of spillover effects are coupled with a general equilibrium effect. For example, an intervention that endows a certain group within the local economy with more resources might have both a spillover effect in terms of sharing of resources between treated and untreated subjects (interaction effect) and a general effect on the local economy's prices (general equilibrium effect). The setup for estimating the spillover effect will not change, although knowledge that there is more than one mechanism behind the spillover will help to

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<sup>8</sup> This is the approach followed in Miguel and Kremer (2004). They collect specific data to show that several dimensions of worm avoidance behaviour did not change in the treatment group.

correctly interpret the estimated ATE and ITE. In this case, the ATE would be the sum of the direct effect and the general equilibrium effect, and the ITE would be the sum of the indirect (interaction) effect and the general equilibrium effect.

### **3.5 Important considerations**

As discussed, the key assumptions for the identification and estimation of treatment effects are that (1) the distribution of characteristics of subjects in treatment and control groups is ex-ante identical; and (2) any spillover effect of the treatment to ineligible subjects is restricted to the local economy, that is, the unit of the initial randomization.

Neither of these assumptions is directly testable. However, while one may obtain indirect evidence that the randomization worked by comparing the means or the distribution of characteristics of subjects in the treatment and control groups, it is often difficult to test whether there are no spillover effects between two randomized units. Therefore, it is extremely important to have a good understanding of how local markets and institutions work before designing the experiment.

Knowledge of the local economy is crucial for a successful experimental design. For example, in understanding at what level the deworming drug externalities operate, knowledge of sanitary habits and facilities in the school and at home is essential. Suppose schools had high-quality sanitary facilities and monitored pupils' hand washing, while houses had low-quality facilities. In this case, the risk of contact with contaminated feces may be higher at home than at school. Therefore, it is possible that siblings, rather than classmates of treated children, may benefit indirectly from the treatment. In this case, a double randomization at the school level may detect no spillover effect, although the drug has actually caused a reduction in the infection rates of treated children's siblings.

In sum, a theory regarding the potential causes of spillover effects and knowledge of local social and economic features and interactions are both essential for increasing confidence in the experimental design's suitability to control for or measure spillover effects.

### **3.6 Design and estimation**

An important issue for the experimental design is the optimal share of units and subjects to randomize. Hahn and Hirano (2010) show that assigning approximately two-thirds of the units to the treatment group ( $T=1$ ) and 50 percent of the subjects in those units to the eligible group ( $E=1$ ) provides the most precise estimates of the ATE and ITE.

To discuss estimation issues, we consider the following regression model in which, as

above,  $T$  indicates the treatment status and  $E$  the eligibility. The subscripts  $i$  and  $v$  refer to the household and the local economy.  $X_{iv}$  is a vector of observable characteristics at the household and local economy level and  $\varepsilon_{iv}$  represents unobserved determinants of the outcome.

$$Y_{iv} = \alpha_0 + \alpha_1 T_v + \alpha_2 E_i + \alpha_3 T_v E_i + \alpha_4 X_{iv} + \varepsilon_{iv}$$

Under the above assumptions, the parameters  $\alpha_1 + \alpha_3$  and  $\alpha_1$  identify the ATE and the ITE when we have eligible and ineligible households.<sup>9</sup>

Estimation issues can vary depending on the type of data available and the method used. With an experimental design and no baseline data (that is, the case summarized by the regression model above) the ATE and ITE can be estimated with a simple OLS. Conditioning on predetermined determinants of the outcome  $Y$  improves estimation precision. In the case of nonexperimental methods and ex-post data, controls are needed to control for the differences in treatment and comparison groups.

The availability of baseline data is important to confirm the lack of systematic differences between treatment and control groups at time  $t = 0$  in the case of experimental design (and to check whether randomization was successful), as well as to assess the differences between treatment and comparison groups in the case of the nonexperimental designs that we discuss in the next section. In addition, with baseline data, a difference-in-difference (DID) strategy can be used. The basic idea of DID is to use the difference between outcome values in the counterfactual group (control or comparison group) before and after the program ( $Y_{T=0,t=1} - Y_{T=0,t=0}$ ) as an indication of the trend the outcome would have had in the treatment areas had the program not been in operation. After controlling for this trend, the remaining difference between treatment and counterfactual group can then be attributed to the program. This strategy may be especially useful when using nonexperimental methods in which outcome values are likely to be different at the baseline. A further advantage of using a DID strategy is that it improves the precision of both the experimental and non-experimental estimates. In a regression framework, the DID can be written as:

$$Y_{ivt} = \alpha_0 + \alpha_1 T_v + \alpha_2 t + \alpha_3 E_i + \alpha_4 T_v t + \alpha_5 E_i t + \alpha_6 T_v E_i t + \alpha_7 X_{iv} + \varepsilon_{ivt}$$

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<sup>9</sup> The parameter  $\alpha_2$  is not identified when eligibility does not vary in the control group.



The subscript  $t$  takes the value of 0 for the baseline and one for follow-up observations.

In this specification  $\alpha_4 + \alpha_6$  estimates the ATE and  $\alpha_6$  estimates the ITE. These parameters are identified under the assumption that, in the absence of the treatment, the trend for eligibles and ineligibles in the treatment group would have been the same as the observed trend for eligibles and ineligibles in the control group. This is often called a parallel trend assumption. When estimating the parameters of both equations, the standard errors should be clustered at the unit level (Bertrand, Duflo, and Mullainathan, 2004).

### 3.7 Total effect of the program

One might be interested in calculating the total treatment effect of the program. This parameter, which we call the *TATE* (Total Average Treatment Effect), is a weighted average of the ATE and ITE where the weights,  $w_{ATE}$  and  $w_{ITE}$ , are the sample proportion of the E=1 and E=0 groups:

$$TATE = w_{ATE} ATE + w_{ITE} ITE$$

## 4. Spillover effects in non-experimental designs

In some cases, a randomized design may not be a viable option. In what follows we focus on how to correctly estimate spillover effects without randomized control trials.

Without randomization, the main challenge in the evaluation design is to find a group (defined as the comparison group<sup>10</sup>) that can be compared to the treatment group. In the case of the presence of spillover effects, a further complication is that the outcome for this comparison group must not be affected by the treatment. Moreover, to estimate the spillover effects, we must identify the subject group for which spillover effects are likely (the ineligibles in the local economy) and find a valid comparison group from within local economies that is not affected by the treatment but is comparable to the treated group.

We follow here the same process as above, that is, we have an intervention offered only to a subgroup of subjects (E=1) living in the treatment units (T=1) where the program is active. However, here the assignment of a unit (locality, municipality, and so forth) to treatment and the assignment of subjects to the program in treatment units are not random and eligibility is decided according to certain criteria, for example, a poverty score. On one

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<sup>10</sup>In experimental designs we used the terminology control group but in nonexperimental designs it is more correct to speak about a comparison group to stress the fact that this group, which can be compared to the treatment group, was not selected by a random assignment.

hand, a sample of non-treatment units ( $T=0$ ) would not necessarily be comparable to a sample of treatment areas; on the other hand, the group of subjects participating in the program ( $E=0$ ) cannot be directly compared to those who are ineligible ( $E=1$ ), as many observable and unobservable characteristics might differ.

There are several non-experimental methods that may estimate the program's impacts in this case. However, these methods require additional and stronger non-testable assumptions than the experimental settings. Therefore, non-experimental methods are generally less credible than experimental methods.

As for the experimental setting, we require samples of four groups: eligibles in treatment units ( $T=1, E=1$ ), eligibles in comparison units ( $T=0, E=1$ ), ineligibles in treatment units ( $T=1, E=0$ ) and ineligibles in comparison areas ( $T=0, E=0$ ) (or just comparable control units if the eligibility is randomized within the treatment units). Typically, areas excluded from the program are those that can be used for the comparison groups. The complication in a nonexperimental setting is that these areas are, by construction, different from treatment areas. For instance, if the treatment units are the poorest localities (for example, in the design for the evaluation of the urban component of Mexico's conditional cash transfer program), then the units left out ( $T=0$ ) are, by definition, less poor than those who are included in ( $T=1$ ).

#### **4.1 Matching**

One nonexperimental design is to employ a matching approach. The key additional non-testable assumption in this method is that the differences between subjects in the treatment and comparison units are observed; therefore, conditional on these assumptions, the treatment assignment is independent of the potential outcomes. This assumption is often called the Conditional Independence Assumption (CIA).

There are two main empirical issues with this approach. First, researchers must find a set of observable characteristics such that the CIA holds. We do not know what such variables are a priori. Researchers have to use their judgment and their knowledge of economic theory and of the local socioeconomic and institutional features. This assumption fails if there are characteristics that predict both treatment take up and outcomes but that are not included in the model. Second, researchers must find a method to match subjects in the treatment group with similar subjects in the comparison groups. A common method

employed is propensity score matching (PSM).<sup>11</sup> This method entails predicting participation in the program with a probit or a logit regression using many predetermined correlates of program take up. Predictions from these regressions provide a propensity score that can range between 0 and 1 (which can be thought of as the probability of participating in the program) for each subject in treatment and comparison units. Researchers then match treated and untreated subjects in the treatment units with subjects with similar propensity scores in the comparison units to estimate the ATE and ITE parameters.

Two main differences between the PSM method and a simple linear regression are that the PSM only imposes minimal parametric restrictions and that the PSM clearly shows whether there is common support—that is, whether each subject in the treatment units has matched or comparable subjects in the comparison units.

Following the example above, while treatment (T=1) and comparison (T=0) areas are different by definition, this would not prevent one from finding households within T=1 that are comparable (that is, with a similar propensity score) to households in T=0 before the intervention occurs. Intuitively, a PSM approach estimates the program’s impact comparing units that have been made more comparable by being matched on the basis of propensity score.

In our setting, we can estimate the ATE matching those who participate in the program (E=1) in treatment areas (T=1) with those who were eligible to participate (E=1) in comparison areas (T=0) and the ITE matching those who were ineligible for the program (E=0) living in treatment areas (T=1) with those who were ineligible (E=0) in comparison areas (T=0). Note that in the following equations P(X) is the propensity score:

$$ATE = E(Y_1 - Y_0 | T = 1, E = 1, P(X)) = E(Y | T = 1, E = 1, P(X)) - E(Y | T = 0, E = 1, P(X))$$

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

Here, in the absence of randomization, the key assumption is that we can estimate ATE and ITE, because the CIA holds conditional on the propensity score, P(X).

## 4.2 Regression discontinuity

A different non-experimental method is the regression discontinuity (RD) approach.<sup>12</sup> In the RD approach, programs are often assigned on the basis of a score (for example, a poverty score or a credit score), and there is a cutoff point above which units (individuals,

<sup>11</sup>See, among others, Hendrick, Maffioli, and Vazquez (2010) and Caliendo and Kopeining (2008).

<sup>12</sup>See, among others, Chay, Ibarraran, and Villa (2010) and Lee and Lemieux (2009).

households, localities, and so forth) are eligible for the program and below which they are not. Intuitively, units that are just above this cutoff would not differ greatly from units that are just below the cutoff, with the only difference being eligibility for the program. An RD approach exploits the nontestable assumption that unconfoundedness holds in a small enough neighborhood of the cutoff point. That is, in this neighborhood treatment assignment is not systematically related to subjects' characteristics.

The following example shows how RD can work. We have been assuming that assignment of areas to treatment is nonrandom. In most cases, this type of targeting implies that poorer or needier areas are included in the program. Typically, characteristics related to the relevant targeting objectives are used to compute a score, which is then employed to rank the areas. A cutoff point is then chosen,<sup>13</sup> and only areas with a score below (or above<sup>14</sup>) this cutoff will be included in the program. In our setting, RD can be employed to estimate both the ATE and the ITE. We would use only areas that are just above and just below the cutoff point, assuming that these areas have both comparable characteristics and that the only relevant difference between them is that those just above the cutoff (T=1) receive the program and those just below (T=0) do not. We can write:

$$ATE(\text{"just above cutoff"})=E(Y_1-Y_0|T=1, E=1, \text{"just above cutoff"})= \\ E(Y | E=1, \text{"just above cutoff"})- E(Y | \text{"just below cutoff"})$$

and

$$ITE(\text{"just above cutoff"})=E(Y_1-Y_0|T=1, E=0, \text{"just above cutoff"})= \\ E(Y | E=0, \text{"just above cutoff"})- E(Y | \text{"just below cutoff"})$$

While the identification assumption that unconfoundedness holds around the cutoff may often seem plausible, the RD approach identifies local parameters, the ATE, and the ITE for subjects around the cutoff point, which are not necessarily the general population of interest. RD has a strong internal validity but weak external validity. In our setting, this means that RD is a powerful way to estimate the direct and indirect impact on the areas around the cutoff point (high internal validity), but this result cannot be easily extended to the other areas that are farther from the cutoff point (low external validity). This is even truer if

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<sup>13</sup>The cutoff point is often chosen according to criteria that are related to the program budget. For example, if only a certain number of localities can be included due to budgetary restrictions, then the cutoff point can be chosen accordingly.

<sup>14</sup>Inclusion of below or above the cutoff areas depends on the definition of the score. For example, if a higher value score means that the areas are poorer, and the program wants to include poorer areas, then only areas above the cutoff will be included.

there are reasons to believe that areas around the cutoff are different from areas far from the cutoff point. This would be the case in our example, as areas just above the cutoff ( $T=1$ ) are those units that are only marginally poor and the other areas, substantially above the cutoff, are by definition poorer.

### 4.3 Instrumental variables

Another method that could be used to estimate the direct and indirect effects of a program is the instrumental variables (IV) approach.<sup>15</sup> Typically, any nonrandom assignment to a program creates a bias. For instance, program administrators of a health intervention may assign localities to a program because those areas are better equipped to provide the health packages. The comparison of localities with and without the program would then be biased by definition, since one would be comparing groups that are constructed differently. The impact of any relevant outcome would then be a sum of the true impact and this bias. One can control for some of these differences, including many observed characteristics in the model, but in practice the choice of inclusion in the program would be based on criteria that were not necessarily observable, such as convenience or political, logistical, or budgetary considerations.

The IV approach tries to mitigate this bias by means of another variable (an instrument,  $Z$ ), which determines the assignment to treatment variable ( $T$ ) but has no direct effect on the outcome of interest  $Y$ . For example, suppose we want to study the impact of providing incentives on screening for breast cancer. Eligible women ( $E=1$ ) in some areas ( $T=1$ ) are offered incentives to get screened for breast cancer. Areas with better health infrastructure are more likely to be assigned to the treatment group ( $T=1$ ). Therefore, if we find that the screening rates are higher in the treatment than in the comparison group, we do not know the extent to which this is due to the better infrastructure vs. the effect of the incentives.

However, other variables might explain the assignment to treatment but do not necessarily drive differences in screening for breast cancer. For example, suppose that the political affiliation of the locality leader or the distance of the locality from the municipality center affects the likelihood that the locality is treated, but otherwise have no direct effect on screening rates. Using more formal language, we can say that the instruments  $Z$  are correlated with the treatment variable  $T$ , but, conditional on  $T$ , are not correlated with the outcome  $Y$ .

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<sup>15</sup>A full treatment of an IV approach can be found in chapter four of Angrist and Pischke's book (2009).

An IV approach exploits these variables to estimate the impact of the program<sup>16</sup> In our setting we can employ an IV approach to estimate ATE and ITE as follows:

$$\begin{aligned}ATE &= E(Y_1 - Y_0 | T = 1, E = 1) = E(Y | T(Z) = 1, E = 1, X) - E(Y | T(Z) = 0, E = 1, X) \\ITE &= E(Y_1 - Y_0 | T = 1, E = 0) = E(Y | T(Z) = 1, E = 0, X) - E(Y | T(Z) = 0, E = 0, X)\end{aligned}$$

An intuitive explanation of what IV does in our setting is that it will estimate the impact of the program for those localities that have different values of T because of changes in Z ( $T[Z]=1$  vs.  $T[Z]=0$ ). As in the RD case, this means that the validity of the estimated impact is only specific to this group of localities, and it cannot be extended to the total universe of interest (low external validity).<sup>17</sup>

## **5. An example: PROGRESA**

Consider the case of the Mexican conditional cash transfer (CCT) program, PROGRESA.<sup>18</sup> The program's objectives are to improve education, health, and nutrition for poor households in rural Mexico. It provides cash transfers to eligible households under the condition that they send their children to school, have periodic health checks, and participate in informal health and nutrition classes.

Eligible households in the villages sampled for the policy evaluation receive monthly grants of about 200 pesos per household, or 32.5 pesos per adult equivalent. This is about 23 percent and 16 percent of the average food consumption per adult equivalent for the eligible and ineligible in control villages.

### **5.1 Effect of PROGRESA on consumption**

Suppose the goal of the evaluator is to measure the effect of PROGRESA on consumption. As discussed, one of the things to consider when designing the experiment is whether there may be spillover effects. To determine if there are likely to be spillover effects, it is important to understand some of the features of rural Mexico. First, the villages that the program targets are small and marginalized (a combination of financial and geographic isolation), and the

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<sup>16</sup>In practice, the most difficult part is to find valid instrumental variables. It is always possible to come up with arguments against the validity of instrumental variables, including those we are using as an example in this chapter.

<sup>17</sup>Following this interpretation, the estimated impacts are only local. We can call them: Local Average Treatment Effect (LATE) and Local Indirect Treatment Effect (LITE).

<sup>18</sup>The following discussion is related to Angelucci and De Giorgi (2009).

residents' income tends to be very volatile. It is conceivable that households may want to borrow money or buy insurance to ensure their consumption is stable even though their income varies over time. However, there are no formal credit or insurance institutions such as banks. Therefore, it is highly likely that these households will resort to informal activities to stabilize their consumption, consistent with the evidence that consumption is much more stable over time than income.

One such activity may be to share resources with other households in the village. Households may share assets or labor, grant each other loans, or give each other gifts. Abundant evidence suggests that these informal activities are common and frequent among the poor (for example, Rosenzweig 1988, Townsend 1994, Udry 1995, Fafchamps and Lund 2003, Dubois, Jullien, and Magnac 2008, Fafchamps 2008). In particular, we have reason to believe that these informal resource sharing networks may be important in the villages in which PROGRESA is implemented. For example, these villages have very low migration rates (in 1999, only about 5 percent of households had at least one member leave in the previous five years, of which, 20 percent moved within the same village). Subsequently, about 80 percent of households have relatives in the same village. In these villages, groups of related households (parents' offspring and siblings, as well as more distant relations) live and work in close proximity. Therefore, it is highly likely that when one household experiences high earnings, be it a good harvest or a government transfer, it might share some of those earnings with extended family members.

This discussion suggests the following: First, to evaluate the effect of PROGRESA on eligible households, researchers should not compare their consumption with that of ineligible households living in the same village, but randomly select a group of treatment and control villages, choosing eligible households in these villages (if one believes these spillover effects occur mainly at the village level) and collecting data on eligible households in both treatment and control villages. Second, to measure spillover effects, researchers should also collect information on ineligible households living in both treatment and control villages.

The design of the experiment is exactly as described. Because of logistical constraints, the program was started in a limited number of localities and gradually expanded to cover all targeted localities. The evaluator exploited this gradual phasing in of the program to create the following experiment. Out of a sample of 506 villages, 320 were randomized to receive the program from May 1998. The remaining 186 villages started receiving the program at the end of 1999. A village level census conducted before the randomization ensured there were data on all households in the 506 villages. Each household was classified as eligible or

ineligible based on a poverty score computed from observable characteristics (ranging from education to assets owned to dwelling characteristics). Thus, we have information on eligible ( $E = 1$ ) and ineligible ( $E = 0$ ) households in treatment ( $T = 1$ ) and control ( $T = 0$ ) villages, as shown in Figure 1.

Importantly, there were both eligible and ineligible households in the same extended family. Thus, for example, it is likely that a woman in a household whose income increased by 25 percent because of the PROGRESA grant shared some of it with her sister's household. This leads to the formulation of our testable hypotheses.

- Hypothesis 1: PROGRESA increases the consumption of eligible households—that is, the consumption ATE is positive:

$$ATE^C = E(C_1 - C_0 | E = 1, T = 1) > 0.$$

- Hypothesis 2: PROGRESA has a positive indirect effect on the consumption of ineligible households—that is, the consumption ITE is positive:

$$ITE^C = E(C_1 - C_0 | E = 0, T = 1) > 0.$$

Because of the experimental design, to test these hypotheses one can simply compare the average consumption of eligibles and ineligibles in treatment and control villages:

$$\begin{aligned} & E(C | E = 1, T = 1) - E(C | E = 1, T = 0) \\ & E(C | E = 0, T = 1) - E(C | E = 0, T = 0) \end{aligned}$$

To increase the precision of the estimates, it is common practice to estimate the above parameters using a regression, adding predetermined determinants of consumption. These are variables that affect consumption (for example, socioeconomic characteristics), but which are not correlated with the treatment dummy, because they are measured *before* the beginning of the program. That is, one can estimate the following regression in which, as above,  $T$  indicates the treatment status and  $E$  the eligibility. The subscripts  $i$  and  $v$  refer to the household and the local economy, respectively.

$$Y_{iv} = \alpha_0 + \alpha_1 T_v + \alpha_2 E_i + \alpha_3 T_v E_i + \alpha_4 X_i + \varepsilon_{iv}$$

The parameters  $\alpha_1 + \alpha_3$  and  $\alpha_1$  identify the ATE and the ITE. The variables ( $X$ ) are: household poverty index and land size; locality poverty index and number of households; and the gender, age, and literacy of the head of household and whether he or she speaks an



indigenous language. All the  $X$  variables are at 1997 values.

However, before estimating average treatment effects it is necessary to check that the randomization worked—that is, that the characteristics of subjects in treatment and control villages did not differ systematically, especially in those cases in which randomization was at the village/local economy level and relevant outcomes were mostly at the individual/household level. Berhman and Todd (1999) perform this exercise, confirming that the baseline characteristics are balanced between treatment and control villages.

Table 1 shows the actual data. As hypothesized, both effects are positive and significant. The consumption ATE is between 24 and 30 pesos, a 15 percent to 20 percent increase in monthly food consumption per adult equivalent, compared to the levels in control villages (which, by assumption, are not affected by the program's existence). The consumption ITE is also positive: PROGRESA causes ineligibles' monthly food consumption to increase by about 20 pesos, which is roughly a 10 percent increase compared to the consumption level of ineligible households in control villages.

## **5.2 General equilibrium effects**

While we have established the existence of positive spillover effects on consumption, provided the village randomization was successful and the consumption spillover effects occurred only within villages, it is not yet clear whether the proposed mechanism (informal gifts and loans between villagers, especially family members) causes these indirect effects. Besides working through informal gifts and transfers between relatives and friends, the program may affect the labor and goods markets as well as savings. Higher goods prices may increase the incomes of ineligible households if they produce the commodities for which prices have increased. Similarly, higher wages may increase their labor earnings.

Given the village randomization, we can test the hypothesis that PROGRESA generates general equilibrium effects by comparing average prices, wages, and income in control and treatment villages. We observe unit values for 36 food commodities, the consumption of which accounts for about two-thirds of an eligible household's nondurable consumption in control villages. These unit values are observed for the first time in March 1998, right before the distribution of the first transfers, and then again in October 1998 and May and November 1999, after which the control villages also start to receive cash transfers.

We estimate the parameters of the following equation:

$$Y_{iv} = \alpha_0 + \alpha_1 T_v + \alpha_2 E_i + \alpha_3 T_v E_i + \alpha_4 X_i + \varepsilon_{iv}$$

Where the variable  $Y$  is the log price of the  $i$ -th commodity in village  $v$ ,  $T$  is a dummy for treatment village, and  $E$  is a dummy for three data waves after the beginning of the program. For a robustness check, we also present the estimates of the effect of PROGRESA on prices, dropping the baseline and using the cross sectional variation only.

Table 2 shows no significant difference in prices by village type, as the coefficient of the interaction between the village and time dummy is  $-0.01$  and is not statistically significant. That is, we cannot reject the hypothesis that there are no differential effects on goods prices by village type, and any effect is likely small (as the standard error is  $0.01$ ). Since grocery stores likely serve clusters of control and treatment villages—only 10 percent of purchases of chicken, meat, and medicines and 53 percent of purchases of staples such as milk, corn, and flour take place within one's village of residence—prices may increase in both control and treatment villages. We would not be able to identify this effect.

We repeat the analysis for median village wages, the results of which are reported in Table 3. For prices, the point estimate is  $-0.02$  (small and negative) and the standard error is  $0.03$ . Thus, the effect is not statistically different from zero and is, at most, small.

### **5.3 Other mechanisms**

Having ruled out any sizable general equilibrium effect on prices and wages, it is useful to provide direct evidence that the increase in consumption for ineligible households is caused by an increase in the number of loans and gifts received. Angelucci and De Giorgi (2009) find increases in informal loans and transfers to ineligible households in treatment villages (and a small decrease in the value of their savings). The implications of these findings are that failing to account for the program's spillover effects results in a 12 percent underestimation of the effect of PROGRESA on consumption in treatment villages. More generally, this exercise teaches us how households cope with high levels of income variability and the absence of formal credit and insurance.

Learning about the causes of spillover effects is useful for at least two additional reasons. First, it helps us understand what to expect when implementing a program at the national level. In this case, the findings suggest the program will continue to have sizeable spillover effects on consumption as long as informal sharing institutions—such as the extended family—are as important in the rest of Mexico as in the sampled villages. Second, the findings can help policy makers design more cost effective policies.

Suppose one were to find that all village members benefit from the program because they redistribute the PROGRESA grant in the village regardless of the actual identity of the households that receive the transfer. This would mean that costly and time consuming household level targeting may not be required. Geographic targeting, using poverty maps to identify poor areas, could be a cheaper and faster alternative.

## **6. Conclusions**

Subjects who are ineligible for a particular treatment will often benefit from it indirectly in a wide variety of interventions. Measuring these spillover effects (or Indirect Treatment Effects) and understanding the mechanisms that generate them can be crucial to the design of effective policies. Failing to adopt an evaluation design that accounts for spillover effects can result in a double underestimation of a program's impact, as the average effect on eligible subjects is underestimated and the effect of the treatment on ineligible subjects goes completely unnoticed. This double underestimation may lead to incorrect policy recommendations (for example, dropping the program because it does not seem cost effective).

This paper makes some key recommendations to aid the design of evaluations that account for the presence of spillover effects. Here is a summary of these recommendations:

1. Develop and/or adopt a theory about what may cause the spillover effects, who may be affected by them, and how. Knowledge of the local economy and of the types of interactions among its members is crucial for the design of an evaluation that at least accounts for the existence of spillover effects. Ideally, the evaluation should enable researchers to measure the effects and understand why they arise. Having a hypothesis about spillover effects often means having better assumptions about how the program works, which should help in the design of better policies.
2. To measure spillover effects effectively, the evaluation design must take them into account from the start. This often means collecting more data, for instance by surveying geographical areas that are not affected by the intervention. Evaluation designs that do not consider these issues from the very beginning are typically unable to measure spillover effects.

3. To identify and estimate the average treatment effect on eligible subjects (ATE) in the presence of spillover effects, researchers should select a control group that is not affected by the program. Theory will guide in this choice. Consider, for example, the case in which the indirect effects operate at village level. With an experimental design, randomize at village level. With a selection-on-observables design (matching), select treatment and control groups from different villages.

4. To identify and estimate the indirect treatment effect (ITE), that is the effect of the treatment on individuals who are not directly affected by the treatment, have two groups of subjects with similar characteristics (the same distribution of characteristics if there is random assignment) but only one group that is indirectly affected by the treatment. Consider, for example, the case in which the indirect effects operate at village level. With an experimental design, conduct a double randomization across and within villages. With a selection-on-observables design (matching), select subjects that live in treated villages and who are indirectly affected by the treatment and compare them with similar subjects from different villages.

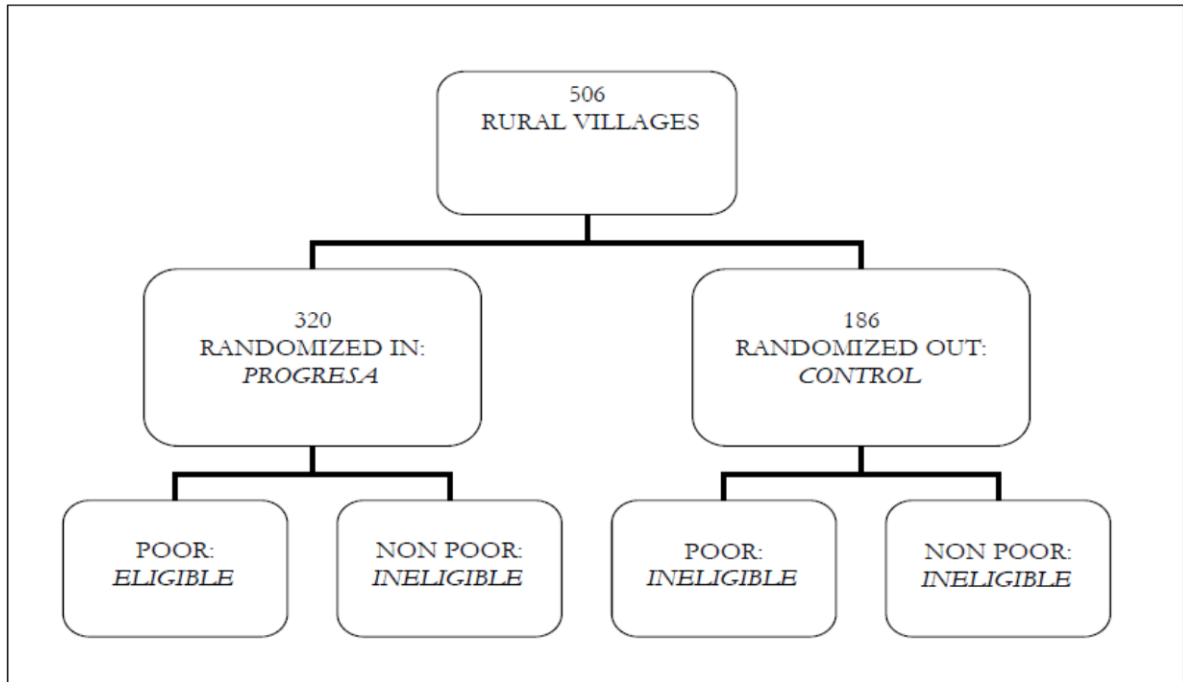
5. To understand the *mechanisms* that cause the spillover effects, think about potential competing explanations and collect data on relevant outcomes. In many cases, unveiling the mechanisms behind the spillover effects will lead to a better understanding of how the program works in general.

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**Figure 1. PROGRESA: Data structure**



**Table 1: 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]***

From Angelucci and De Giorgi (2009). Monthly pesos per adult equivalent at Nov. 1998 prices; the exchange rate is roughly 10 pesos per USD. We report the standard deviations of the means and the standard errors, in brackets, of the treatment effects. The latter are clustered at village level with \*\*\* and \*\* indicating significance at the 1 percent and 5 percent levels respectively. The set of conditioning variables we add to the regressions in the left panel are: household poverty index, land size, head of household gender, head of household age, whether head of household speaks indigenous language, head of household literacy; at the locality level, poverty index and number of households. All variables are at 1997 values.



**Table 2: Effect of PROGRESA on log village prices, standard errors (in parentheses) clustered at the village level.**

Dependent variable: log price	Double difference	Simple difference
Effect on log prices	-0.008	0.004
	(0.011)	(0.006)
Average log prices in control villages	1.68	1.72
	(0.01)***	(0.01)***
Observations	72216	54036

**Table 3: Effect of PROGRESA on log median village wages, standard errors (in parentheses) clustered at the village level.**

Dependent variable: log wage	Double difference	Simple difference
Effect on log wages	-0.02	-0.01
	(0.03)	(0.03)
Average log wages in control villages	3.23	3.44
	(0.022)***	(0.019)***
Observations	2006	1500