Introduction to Evaluations
A randomized evaluation is a type of impact evaluation that uses random assignment to allocate resources, run programs, or apply policies as part of the study design. Like all impact evaluations, the main purpose of randomized evaluations is to determine whether a program has an impact, and more specifically, to quantify how large that impact is. Impact evaluations measure program effectiveness typically by comparing outcomes of those (individuals, communities, schools, etc) who received the program against those who did not. There are many methods of doing this. But randomized evaluations have the benefit of ensuring that there are no systematic differences between those who receive the program and those who do not, thereby producing accurate (unbiased) results about the effect of the program.

For a copy of the following content, download Introduction to Evaluations.

What is evaluation and why evaluate?

Why evaluate?

The purpose of evaluation is not always clear, particularly for those who have watched surveys conducted, data entered, and then the ensuing reports filed away only to collect dust. This is most common when evaluations are imposed by others.
If, on the other hand, those responsible for the day-to-day operations of a program have critical questions, evaluations can help find answers.

As an example, the NGO responsible for distributing chlorine pills may speak with their local field staff and hear stories of households diligently using the pills, and occasionally see improvements in their health. But each time it rains heavily, the clinics fill up with people suffering from diarrheal diseases. The NGO might wonder, “if people are using chlorine to treat their water, why are they getting sick when it rains? Even if the water is more contaminated, the chlorine should kill all the bacteria.” The NGO may wonder whether the chlorine pills are indeed effective at killing bacteria. Are people using it in the right proportion? Maybe the field staff is not telling the truth. Perhaps the intended beneficiaries are not using the pills. Perhaps they aren’t even receiving them. And then when confronted with this fact, the field staff claims that during the rains it is difficult to reach households and distribute pills. Households, on the other hand, will reply that they most diligently use pills during the rains, and that the pills have helped them substantially.

Speaking to individuals at different levels of the organization as well as to stakeholders can uncover many stories of what is going on. These stories can be the basis for theories. But plausible explanations are not the same thing as answers. Evaluations involve developing hypotheses of what's going on, and then testing those hypotheses.

What is evaluation?

The word “evaluation” can be interpreted quite broadly. It means different things to different people and organizations.
Engineers, for example, might evaluate or test the quality of a product design, the durability of a material, efficiency of a production process, or the safety of a bridge. Critics evaluate or review the quality of a restaurant, movie or book. A child psychologist may evaluate or assess the decision-making process of toddlers.

The researchers at J-PAL evaluate social programs and policies designed to improve the well-being of the world’s poor. This is known as program evaluation.

Put simply, a program evaluation is meant to answer the question, “how is our program or policy doing?” This can have different implications depending on who is asking the question, and to whom they are talking. For example, if a donor asks the NGO director “how is our program doing?” she may imply, “have you been wasting our money?” This can feel interrogatory. Alternatively, if a politician asks her constituents, “how is our program doing?” she could imply, “is our program meeting your needs? How can we make it better for you?” Program evaluation, therefore, can be associated with positive or negative sentiments, depending on whether it is motivated by a demand for accountability versus a desire to learn.

J-PAL works with governments, NGOs, donors, and other partners who are more interested in learning the answer to the question: How effective is our program? This question can be answered through an impact evaluation. There are many methods of doing impact evaluations. J-PAL uses a methodology known as randomized evaluation.

At a very basic level, randomized evaluation can answer the question: Was the program effective? But if thoughtfully designed and implemented, it can also answer the questions,
how effective was it? Were there unintended side-effects? Who benefited most? Who was harmed? Why did it work or not work? What lessons can be applied to other contexts, or if the program was scaled up? How cost-effective was the program? How does it compare to other programs designed to accomplish similar goals? To answer these questions, the impact evaluation should be part of a larger package of evaluations and exercises.

Following the framework on comprehensive evaluations offered by Rossi, Freeman, and Lipsy, this package is covered in the subsequent sections:
1. Needs Assessment
2. Program Theory Assessment
3. Process Evaluation
4. Impact Evaluation
5. Cost-Benefit, Cost-Effectiveness, and Cost-Comparison Analysis
6. Goals, Outcomes, and Measurement

The first two assessments (Needs and Program Theory) examine what needs the program or policy is trying to fill and what are the steps by which it will achieve these objectives. Ideally these steps should be formally set out by those implementing the program, before an impact evaluation is set up.

Process evaluations are useful for program managers and measure whether the milestones and deliverables are on schedule. Many organizations have established systems to track processes—often classified as Monitoring and Evaluation (M&E).

Impact evaluations are designed to measure whether programs or policies are succeeding in achieving their goals.
Lastly, Cost-benefit and Cost-effectiveness analyses are useful for the larger policy implications of a program. The first looks at whether the benefits achieved by the program are worth the costs. The second compares the benefits of this program to that of programs designed to achieve similar goals.

In conducting any assessment, evaluation, or analysis, it is imperative to think about how progress can be measured. Measuring indicators of progress – keeping the programs’ goals and expected outcomes in mind—requires significant thought as well as a system of data collection. This is covered in Goals, Outcomes and Measurement.

1. Needs Assessment

Programs and policies are introduced to address a specific need. For example, we may observe that the incidence of diarrhea in a community is particularly high. This might be due to contaminated food or water, poor hygiene, or any number of plausible explanations. A needs assessment can help us identify the source of the problem and those most harmed. For example, the problem may be due to the runoff of organic fertilizer which is polluting the drinking water used by certain communities.

Needs assessment is a systematic approach to identifying the nature and scope of a social problem, defining the target population to be served, and determining the service needed to meet the problem.

A needs assessment is essential because programs will be ineffective if the services are not properly designed to meet the need or if the need does not actually exist. So, for example, if the source of pollution contaminating drinking water is agricultural, investment in sanitation infrastructure
such as toilets and sewage systems may not solve the problem. Needs assessments may be conducted using publicly available social indicators, surveys and censuses, interviews, etc.

2. Program Theory Assessment

Social programs or policies are introduced to meet a social need. Meeting that need usually requires more thought than finding and pressing a single magic button, or taking a pill. For policymakers, it requires identifying the reasons that are causing undesirable outcomes (see Needs Assessment) and choosing a strategy from a large set of options to try to bring about different outcomes.

For example, if people are drinking unclean water, one program might be designed to prevent water from becoming contaminated—by improving sanitation infrastructure—while another may be designed to treat contaminated water using chlorine. One proposed intervention might target those responsible for the pollution. Another might target those who drink the water. One strategy may rest on the assumption that people don’t know their water is dirty, another, that they are aware but have no access to chlorine, and even another, that despite awareness and access, people choose not to chlorinate their water for other reasons (e.g. misinformation, taste, cost, etc).

These programs must simultaneously navigate the capacity constraints (financial, human, and institutional) and political realities of their context. In conceiving an appropriate response, policymakers implicitly make decisions about what is the best approach, and why. When this mental exercise is documented explicitly in a structured way, policymakers are conducting what can be called a program theory assessment,
or *design assessment*.

A Program Theory Assessment models the theory behind the program, presenting a plausible and feasible plan for improving the target social condition. If the goals and assumptions are unreasonable, then there is little prospect that the program will be effective. Program theory assessment involves first articulating the program theory and then assessing how well the theory meets the targeted needs of the population. The methodologies used in program theory assessment include the *Logical Framework Approach* or *Theory of Change*.

3. **Process Evaluation**

Before it is ever launched, a program exists in concept—as a design, description or plan (see Program Theory Assessment). But once launched, the program meets on-the-ground realities: Is the organization adequately staffed and trained? Are responsibilities well-assigned? Are the intermediate tasks being completed on schedule? If the program is designed to provide chlorine tablets to households to treat unclean water, for example, does the right number of chlorine tablets reach the appropriate distribution centers on time?

Process evaluation, also known as *implementation assessment* or *assessment of program process*, analyzes the effectiveness of program operations, implementation, and service delivery. When process evaluation is ongoing it is called *program monitoring* (as in Monitoring and Evaluation, or M&E). Process evaluations help us determine, for example:
- Whether services and goals are properly aligned.
- Whether services are delivered as intended to the appropriate recipients.
- How well service delivery is organized.
• The effectiveness of program management.
• How efficiently program resources are used.¹

Process evaluations are often used by managers as benchmarks to measure success, for example: the distribution of chlorine tablets is reaching 80% of the intended beneficiaries each week. These benchmarks may be set by program managers, and sometimes by donors. In many larger organizations, monitoring progress is the responsibility of an internal Monitoring and Evaluation (M&E) department. In order to determine whether benchmarks are being met, data collection mechanisms must be in place.


4. Impact Evaluation

Programs and policies are designed to achieve a certain goal (or set of goals). For example, a chlorine distribution program may be implemented specifically to combat high-incidence of waterborne illness in a region. We may want to know whether this program is succeeding in its goal. This isn't the same thing as asking, “Does chlorine kill bacteria?” or “Is the consumption of chlorine harmful?” Those questions can be answered in a laboratory. For our program to achieve its goal of stopping illness, money must be allocated, tablets must be purchased, distribution mechanisms must be put in place, households must receive the tablets, households must use the tablets, and households must not consume untreated water. A program evaluation helps us determine whether all of these requirements are being met and if our goal is actually being achieved as intended.

As a normal part of operations, e.g., basic bookkeeping,
certain information is produced, such as how many boxes of
chlorine tablets have been shipped. This type of information
can be used for process evaluation. But it cannot tell us
whether we've successfully reduced the incidence of
diarrhea. To measure impact, we must use more direct
indicators such as the number of people who report suffering
from diarrhea in the last two months.

Impact evaluations gauge the success of a program—where
success can be broadly or narrowly defined. They help us
weed out less effective interventions from successful ones
and also help us improve existing programs.

The primary purpose of impact evaluation is to determine
whether a program has an impact on a few key outcomes,
and more specifically, to quantify how large that impact is.

What is impact? In our chlorine example, impact is how much
healthier people are because of the program than they would
have been without the program. Or, more specifically, impact
is how much lower the incidence of diarrhea is than it would
have been otherwise.

Getting this number correct is more difficult than it sounds. It
is possible to measure the incidence of diarrhea in a
population that received the program. But “how they would
have been otherwise” is impossible to measure directly—just
as it is impossible to measure the United States economy
today had the Nazis won World War II, or to determine
today's most deadly disease if penicillin was not discovered in
Alexander Fleming's laboratory in 1928 in London. It is
possible that Germany would have become the dominant
economy in the world; alternatively, the Nazis may have
fallen just a few years later. It is possible that minor wounds
would still be one of the largest killers; alternatively, some
close relative of penicillin could have been discovered in another laboratory in a different part of the world. In our chlorine example, it is possible that, without chlorine, people would have remained just as sick as they were before. Or it is possible that they would have started boiling their water instead, and the only thing chlorine distribution did was substitute one technology for another—suggesting that people are not really any healthier because of the program.

Impact evaluations usually estimate program effectiveness by comparing outcomes of those (individuals, communities, schools, etc) who participated in the program against those who did not participate. The key challenge in impact evaluation is finding a group of people who did not participate but closely resemble the participants had those participants not received the program. Measuring outcomes in this comparison group is as close as we can get to measuring “how participants would have been otherwise.” There are many methods of doing this and each method comes with its own assumptions.

5. Cost-Benefit/Effectiveness/Comparison Analyses

Two organizations may come up with very different strategies to tackle the same problem. If a community's water supply, for example, was contaminated and led to a large incidence of diarrhea, one NGO may advocate for investments in modern water and sanitation infrastructure, including a sewage system, piped water, etc. Another NGO may propose a distribution system where households are given free chlorine tablets to treat their own water at home. If these two methods were shown to be equally effective—each reducing diarrhea incidence by 80%—would local policymakers be just as happy implementing one versus the other? Probably not.
They would also need to consider the cost of each strategy. It is highly likely that modern infrastructure investments in an otherwise remote village would be prohibitively expensive. In this case, the choice may be clear. However, the options are not always so black and white. A more realistic (but still hypothetical) choice would be between an infrastructure investment that reduces diarrhea by 80% versus a chlorine distribution program that costs 1/100th the price, and reduces diarrhea by 50%.

A cost-benefit analysis quantifies the benefits and costs of an activity and puts them into the same metric (often by placing a monetary value on benefits). It attempts to answer the question: Is the program producing sufficient benefits to outweigh the costs? Trying to quantify the benefit of children’s health in monetary terms, however, can be extremely difficult and subjective. Hence, when the exact value of the benefit lacks widespread consensus, this type of analysis may produce results that are more controversial than illuminating. This approach is most useful when there are multiple types of benefits and agreed ways of monetizing them.

A cost-effectiveness analysis takes the impact of a program (e.g. percent reduction in the incidence of diarrhea), and divides that by the cost of the program, generating a statistic such as the number of cases of diarrhea prevented per dollar spent. This makes no judgment of the value of reducing diarrhea.

Lastly, a cost comparison analysis will take multiple programs and compare them using the same unit, allowing policy makers to ask: per dollar, how much does each of these strategies reduce diarrhea?
See the paper on "Comparative Cost-Effectiveness Analysis to Inform Policy in Developing Countries: A General Framework with Applications for Education" for more information.

6. Goals, Outcomes and Measurement

When conducting a program evaluation, governments and NGOs are often asked to distill a program's mission down to a handful of outcomes that, it is understood, will be used to define success. Adding to this difficulty, each outcome must be further simplified to an indicator such as the response to a survey question, or the score on a test.

More than daunting, this task can appear impossible and the request, absurd. In the process, evaluators can come across as caring only about data and statistics—not the lives of the people targeted by the program.

For certain goals, the corresponding indicators naturally follow. For example, if the goal of distributing chlorine tablets is to reduce waterborne illness, the related outcome may be a reduction in diarrhea. The corresponding indicator, incidence of diarrhea, could come from one question in a household survey where respondents are asked directly, “Has anyone in the household suffered from diarrhea in the past week?”

For other goals, such as “empowering women,” or “improving civic mindedness” the outcomes may not fall as neatly into place. That doesn't mean that most goals are immeasurable. Rather, more thought and creativity must go into devising their corresponding indicators. For an example of difficult-to-measure outcomes, see article.
What is randomization and why randomize?

What is randomization?
In its most simple sense, randomization is what happens when a coin is flipped, a die is cast, or a name on a piece of paper is drawn blindly from a basket, and the outcome of that flip, cast, or draw determines what happens next. Perhaps, the outcome of the coin flip determines who has to do some chore; the roll of the die determines who gets a pile of money; the draw of a name determines who gets to participate in some activity or a survey. When these tools (the coin, the die, the lottery) are used to make decisions, the outcome can said to be left to chance, or randomized.

Why do people let chance determine their fate? Sometimes, because they perceive it as fair. Other times, because uncertainty adds an element of excitement. Statisticians use randomization because, when enough people are randomly chosen to participate in a survey, conveniently, the attributes of those chosen individuals are representative of the entire group from which they were chosen. In other words, inferences can be made from what is discovered about them to the larger group. Using a lottery to get a representative sample is known as random sampling or random selection.

When two groups are randomly selected from the same population, they both represent the larger group. They have comparable characteristics, in expectation, not only to the larger group but also to each other. The same logic carries forward if more than two groups are randomly selected. When two or more groups are selected in this way, we can say that individuals have been randomly assigned to groups. This is called random assignment. (Random assignment is also the appropriate term when all individuals from the larger
group divided randomly into different groups. As before, all groups represent the larger group and, in expectation, have comparable characteristics to each other.) *Random assignment* is the key element of randomized evaluation.

What happens next in a simple randomized evaluation (with two groups) is that one group receives the program that is being evaluated and the other does not. If we were to evaluate a water purification program using this method, we would randomly assign individuals to two groups. At the beginning, the two groups would have comparable characteristics on average (and are expected to have equivalent trajectories going forward). But then we introduce something that makes them different. One group would receive the water purification program and the other would not. Then, after some time, we could measure the relative health of individuals in the two groups. Because the groups were comparable at the beginning, differences in outcomes seen later on can be attributed to one having been given the water purification program, and the other not.

Randomized Evaluations go by many names:
- Randomized Controlled Trials
- Social Experiments
- Random Assignment Studies
- Randomized Field Trials
- Randomized Controlled Experiments

Randomized evaluations are part of a larger set of evaluations called impact evaluations. Like all *impact evaluations*, the primary purpose of randomized evaluations is to determine whether a program has an impact, and more specifically, to quantify *how large* that impact is. Impact evaluations typically measure program effectiveness by
comparing outcomes of those (individuals, communities, schools, etc) who participated in the program against those who did not participate. There are many methods of doing this.

What distinguishes randomized evaluations from other non-randomized impact evaluations is that participation (and non-participation) is determined *randomly*—before the program begins. This *random assignment* is the method used in clinical trials to determine who gets a drug versus who gets a placebo when testing the effectiveness (and side effects) of new drugs. As with clinical trials, those in the impact evaluation who were *randomly assigned* to the “treatment group” are eligible to receive the treatment (i.e. the program). And they are compared to those who were randomly assigned to the “comparison group” –those who do not receive the program. Because members of the treatment and comparison groups do not differ systematically from each other at the outset of the evaluation, any difference that subsequently arises between them can be attributed to the treatment rather than to other factors. Relative to results from non-randomized evaluations, results from randomized evaluations can be:

- Less subject to methodological debates
- Easier to convey
- More likely to be convincing to program funders and/or policymakers

Beyond quantifying the intended outcomes caused by a program, randomized evaluations can also quantify the occurrence of unintended side-effects (good or bad). And, like other methods of impact evaluation, randomized evaluations can also shed light on why the program has or fails to have the desired impact.
1. Randomization in the Context of “Evaluation”
Randomized evaluations are a type of impact evaluation that use a specific methodology for creating a comparison group—in particular, the methodology of random assignment. Impact evaluations are program evaluations that focus on measuring the final goals or outcomes of a program. There are many types of evaluations that can be relevant to programs beyond simply measuring effectiveness. (See What is Evaluation?)

2. Methodology of Randomization
To better understand how the methodology works, see "how to conduct a randomized evaluation."

Why randomize?
What is impact? In our chlorine example, impact is how much healthier people are because of the program. Or, more specifically, it is how much lower the incidence of diarrhea is than it would have been otherwise.

Getting this number correct is more difficult than it sounds. It is possible to measure the incidence of diarrhea in a population that received the program. But “how they would have been otherwise” (termed, the counterfactual) is impossible to measure directly; it can only be inferred.

Constructing a Comparison Group
Impact evaluations estimate program effectiveness usually by comparing outcomes of those (individuals, communities, schools, etc.) who participated in the program against those who did not participate. The key challenge in impact evaluation is finding a group of people who did not participate but closely resemble the participants, specifically, the participants if they had not received the program.
Measuring outcomes in this comparison group is as close as we can get to measuring “how participants would have been otherwise.” Therefore, our estimate of impact is only as good as our comparison group is equivalent to the treatment group.

There are many methods of creating a comparison group. Randomization generates a comparison group that has characteristics that are comparable to the treatment group, on average, before the intervention begins. It ensures that there are no systematic differences between the two groups and that the primary difference between the two is the presence of the program. This produces unbiased estimates of the true effect of the program.

Other methods may produce misleading (biased) results and rely on more assumptions than do randomized evaluations. When the assumptions hold, the result is unbiased. But it is often impossible, and always difficult, to ensure that the assumptions are true.

Beyond escaping debates over whether certain assumptions hold, randomized evaluations produce results that are easy to explain. More information can be found in our 'Why Randomize' document. A table comparing common methods of evaluation can be found here.

When to conduct a randomized evaluation

The value added by rigorously evaluating a program or policy changes depending on when in the program or policy life cycle the evaluation is conducted. The evaluation should not come too soon: when the program is still taking shape and kinks are being ironed out. And the evaluation should not come too late: after money has been allocated, and the program, rolled out, so that there is no longer space for a
An ideal time is during the pilot phase of a program or before scaling up. During these phases there are often important questions that an evaluator would like to answer, such as, How effective is the program? Is it effective among different populations? Are certain aspects working better than others, and can “the others” be improved? Is it effective when it reaches a larger population?

During the pilot phase, the effects of a program on a particular population are unknown. The program itself may be new or it may be an established program that is targeting a new population. In both cases, program heads and policymakers may wish to better understand the effectiveness of a program and how it might be improved.

Almost by definition, the pilot program will reach only a portion of the target population, making it possible to conduct a randomized evaluation. After the pilot phase, if the program is shown to be effective, leading to increased support, and in turn more resources allocated, it can be replicated or scaled up to reach the remaining target population.

One example of a well-timed evaluation is that of PROGRESA, a conditional cash transfer program in Mexico launched in 1997. The policy gave mothers cash grants for their family as long as they ensured their children attended school regularly and received scheduled vaccinations. The political party, which had been in power for the prior 68 years, the Institutional Revolutionary Party (PRI), was facing inevitable defeat in the upcoming elections. A probable outcome of electoral defeat was the dismantling of incumbent programs such as PROGRESA. To build support for the program’s
survival, PRI planned to clearly demonstrate the policy's effectiveness in improving child health and education outcomes.

PROGRESA was first introduced as a pilot program in rural areas of seven states. Out of 506 communities sampled by the Mexican government for the pilot, 320 were randomly assigned to treatment and 186 to the comparison. Comparing treatment and comparison groups after one year, it was found to successfully improve these child-level outcomes. As hoped, the program's popularity expanded from its initial supporters and direct beneficiaries to the entire nation.

Following the widely-predicted defeat of PRI in the 2000 elections, the new political party, PAN took power and inherited an immensely popular program. Instead of dismantling PROGRESA, PAN changed the program's name to OPORTUNIDADES and expanded it nation-wide.

The program was soon replicated in other countries, such Nicaragua, Ecuador, and Honduras. Following Mexico's lead, these new countries conducted pilot studies to test the impact of PROGRESA-like programs on their populations before scaling up.

When is randomized evaluation not appropriate?

Randomized evaluations may not be appropriate:

1. *When evaluating macro policies.*

No evaluator has the political power to conduct a randomized evaluation of different monetary policies. One could not randomly assign a floating exchange rate to Japan and other nations and a fixed exchange rate to the United States and a different group of nations.

2. *When it is unethical or politically unfeasible to deny a program*
to a comparison group.
It would be unethical to deny a drug whose benefits have already been documented to some patients for the sake of an evaluation if there are no resource constraints.

3. If the program is changing during the course of the evaluation.
If, midway through an evaluation, a program changes from providing a water treatment solution to providing a water treatment solution and a latrine, it will be difficult to interpret which part of the program produced the observed results.

4. If the program under evaluation conditions differs significantly from how it will be under normal conditions.
During an evaluation, participants may be more likely to use a water treatment solution if they are encouraged or given incentives. In normal conditions, without encouragement or incentives, fewer people may actually use the water treatment solution even if they own it and know how to use it. As a caveat, this type of evaluation may be valuable in testing a proof of concept. It would simply be asking the question, “can this program or policy be effective?” It would not be expected to produce generalizable results.

5. If a randomized evaluation is too time-consuming or costly and therefore not cost-effective.
For example, due to a government policy, an organization may not have sufficient time to pilot a program and evaluate it before rolling it out.

6. If threats such as attrition and spillover are too difficult to control for and hurt the integrity of the evaluation.
An organization may decide to test the impact of a deworming drug on school attendance at a particular school. Because deworming drugs have a spillover effect (the health of one student impacts the health of another), it will be
difficult to accurately measure the impact of the drug. In this case, a solution could be to randomize at a school level rather than at a student level.

7. If sample size is too small.
If there are too few subjects participating in the pilot, even if the program were successful, there may not be enough observations to statistically detect an impact.

How to conduct a randomized evaluation

Of the various methods of impact evaluation, randomized evaluations require fewer assumptions when drawing conclusions from the results. There are a number of steps involved in conducting a randomized evaluation, including convincing program implementers to randomize, thinking about the appropriate evaluation design, ensuring the sample size is sufficient to detect an effect if there is one, ensuring the integrity of the evaluation design (random assignment) is maintained, and figuring out why the program does or does not work. Many of these steps are not unique to randomized evaluations.

Planning an evaluation
In planning an evaluation, it is important to identify key questions the organization may have. From these, we can determine how many of those questions can be answered from prior impact evaluations or from improved systems of process evaluation. Assuming we haven't found all our answers, we must then pick a few top priority questions that will be the primary focus of our impact evaluation. Finally, we should draw up plans to answer as many questions as we can, keeping in mind that fewer high quality impact studies are more valuable than many poor quality ones.
The first step in an evaluation is to revisit the program's goals and how we expect those goals to be achieved. A logical framework or theory of change model can help in this process. (See Program Theory Assessment) As part of assessing the purpose and strategy of a program, we must think about key outcomes, the expected pathways to achieve those outcomes, and reasonable milestones that indicate we're traveling down the right path. As expected in an evaluation, these outcomes and milestones will need to be measured, and therefore transformed into “indicators” and ultimately data. (See Goals, Outcomes, and Measurement.) Only after we have a good sense of the pathways, the scope of influence, and a plan for how we will measure progress, can we think about the actual design of the evaluation.

How to design an evaluation

An evaluation design requires a considerable amount of thought. First comes the conceptual pieces: what do we plan to learn from this evaluation? What are the relevant questions? What outcomes are expected? How can they be measured?

Next come the design questions:
• What is the appropriate level or unit of randomization?
• What is the appropriate method of randomization?
• Beyond the political, administrative and ethical constrains, what technical issues could compromise the integrity of our study, and how can we mitigate these threats in the design?
• How would we implement the randomization?
• What is the necessary sample size to answer our questions? (How many people do we need to include in the study, both as participants, but also as survey respondents?)

1. Unit of Randomization
In designing our evaluation, we must decide at what level we will randomize: what unit will be subject to random assignment? Will it be individuals or groupings of individuals, such as households, villages, districts, schools, clinics, church groups, firms, and credit associations? (When we randomize groups of individuals—even though we care about and measure individual outcomes—this is referred to as a cluster randomized trial.) For example, if we managed to secure enough chlorine pills for one thousand households to treat contaminated water (out of, say, ten thousand households who use the same contaminated source of drinking water), do we expect to randomly assign *households* to the treatment and comparison groups? This means that some households will be given chlorine pills, but some of their immediate neighbors will be denied chlorine pills. Is that feasible? Ethical?

For this type of program, it probably wouldn’t be feasible to randomize at an even smaller unit than the household, for example the individual level. It would imply that some children within a household are given chlorine pills and some of their siblings are not. If all household members drink from the same treated tank of water, individual randomization would be physically impossible, regardless of the ethical considerations. Perhaps the appropriate unit of randomization is the community, where some communities will receive chlorine, other communities will not, but within a “treatment” community all households (implying all neighbors) are eligible to receive the pills.

There are many things to consider when determining the appropriate level of randomization, of which ethics and feasibility are only two. Seven considerations are listed below.
• What unit does the program target for treatment? If chlorine tablets are meant to be dissolved in water storage tanks that in our region all households typically already own, then some households could be selected to receive chlorine, and others not. In this case, the unit of randomization would be at the household level. However, if the storage tank is typically located outside and used by a cluster of households, then it would be impossible to randomly assign some households in that cluster to the comparison group—they all drink the same (treated) water as the treatment households. Then, the most natural unit of randomization may be the “clusters of households” that use a common water tank.

• What is the unit of analysis? If the evaluation is concerned with community-level effects then the most natural level of randomization is probably the community. For example, imagine our outcome measure is incidence of “hospitalization” due to diarrhea, and it is most economical to measure this using administrative records at community clinics, and, furthermore, those records remain anonymous. We would not be able to distinguish whether people who were hospitalized were from treatment households or comparison households. However, if the entire community is in the treatment group, we could compare the records from clinics in treatment communities against those of comparison communities.

• Is the evaluation design fair? The program should be perceived as fair. If I’ve been denied chlorine pills but my immediate neighbors receive them, I might be angry with my neighbors and the NGO, and I might be less willing to fill out a questionnaire on chlorine usage when surveyors knock at my door. The NGO might also not be enthusiastic about upsetting its community members. On the other hand, if my
entire community didn’t get it, but a neighboring community did, I might never hear of the program and have nothing to complain about, or I could think that this was just a village-level choice and my village chose not to invest. Of course, people may be equally upset about a community-level design.

- **Is a randomized evaluation politically feasible?** It may not be feasible politically to randomize at the household level. For example, a community may demand that all needy people receive assistance, making it impossible to randomize at the individual or household level. In some cases, a leader may require that all members of her community receive assistance. Or she may be more comfortable having a randomly selected half be treated (with certainty) than risk having no one treated (were her village assigned to the comparison group). In one case she may comply with the study and in another, she may not.

- **Is a randomized evaluation logistically feasible?** Sometimes it is logistically impossible to ensure that some households remain in the comparison group. For example, if chlorine distribution requires hiring a merchant within each village and setting up a stall where village members pick up their pills, it may be inefficient to ask the distribution agent to screen out households in the comparison group. It could add bureaucracy, waste time, and distort what a real program would actually look like. Or even if the merchant could easily screen, households may simply share the pills with their neighbors who are in the comparison group, in which case, the comparison group would be impacted by the program. In this case, it would make sense to randomize at the village level, and then simply hire merchants in treatment villages and not in comparison villages.
• What spillovers and other effects will need to be taken into account? Even if it is feasible to randomize at the household level—to give some households chlorine tablets and not others—it may not be feasible to contain the impact within just the treatment households. If individuals in the comparison group are affected by the presence of the program—they benefit from fewer sick neighbors (spillover effects), or drink the water from treatment neighbors (don’t comply with the random assignment and cross over to the treatment group), they no longer represent a good comparison group.

• What sample size and power do we require to detect effects of the program? The ability to detect effects depends on the sample size. When more people are sampled from a larger population, they better represent the population. For example, if we survey two thousand households, and randomize at the household level (one thousand treatment, one thousand comparison), we effectively have a sample size of two thousand households. But if we randomized at the village level, and each village has one hundred households, then we would have only ten treatment villages and ten comparison. In this case, we may be measuring diarrhea at the household level, but because we randomized at the village level, it is possible we have an effective sample size closer to ten (even though we are surveying two thousand households)! In truth, the effective sample size, could be anywhere from ten to two thousand, depending on how similar households within villages are to their fellow villagers. (See: sample size.) With an effective sample size closer to ten, we may not be sufficiently powered to detect real effects. This may influence our choice as to the appropriate level of randomization.
There are many considerations when determining the appropriate level of randomization. Evaluators cannot simply sit at a computer, press a button, produce a list, and impose an evaluation design on an organization from thousands of miles away. Evaluators must have a deep and broad understanding of the implementing organization, their program, and the context and work in partnership to determine the appropriate level of randomization given the particular circumstances.

2. Different Methods of Randomization

If my organization can secure one thousand chlorine pills per day so I can treat one thousand out of an eligible two thousand households per day, I could choose to treat the same one thousand households in perpetuity. Alternatively, I could rotate recipients so that each household gets clean water every other day. I may feel that the latter option makes no sense. If everyone is drinking dirty water half the days, I may expect zero impact on anyone. So I may choose one thousand households that will receive the pills daily. If randomizing, I may perform a simple “lottery” to determine which thousand households get the pill: I write all two thousand names onto small pieces of paper, put those pieces of paper into a basket, shake the basket up, close my eyes, and pull one thousand pieces of paper out. Intuitively, this is called a lottery design.

Alternatively, I could rotate households every year instead of every day and randomly assign the order in which they get treated. In this case, one thousand households would be in the treatment group in the first year and in the comparison group in the second year, while the reverse would be true of the other one thousand households. I could then compare
outcomes between the two groups at the end of each year. This set-up is called a rotation design. Note that rotation designs are most workable when the primary concern is what happens when households have access to the program, in this case clean water, and when the treatment effects do not remain after the treatment ends.

Say I can secure five hundred pills per day this year, but next year I expect to secure one thousand per day, and the following year two thousand per day. I could randomly choose five hundred households to get the pill in the first year, another five hundred to be added in the second year, and the remaining thousand get it in the third year. This would be called a phase-in design.

There are seven possible randomization designs—the lottery design, phase-in design, rotation design, encouragement design, the varying levels of treatment design, and two-stage randomization. These designs are not necessarily mutually exclusive. Their advantages and disadvantages are summarized in this table.

3. Threats to the Design

A spillover effect occurs when a program intended to help targeted participants unintentionally impacts the comparison group as well (either positively or negatively). The comparison group is supposed to represent outcomes had the program not been implemented (see counterfactual). If this comparison group has been touched by the program, its role mimicking the counterfactual is now compromised, and the ensuing impact measure may be biased. There are ways of mitigating spillover effects, such as by changing the level of randomization.

For example, one source of sickness may be drinking
contaminated water. But another source is playing with neighboring children who are themselves sick. If I am in the comparison group, and the program treats my neighbors so that those neighbors are no longer sick, my changes of getting sick are reduced. As such, I have now been affected by the treatment of my neighbors, despite being in the comparison group, and would no longer represent a good counterfactual. This is known as a spillover effect, in this case a positive spillover. To mitigate the possibility of spillovers, we could randomize at the community level so that everyone in the same community shares the same status of being in either the treatment or comparison group. Children in a treatment group community would be less likely to impact children in a comparison group community.

Another possibility is that my household has been assigned to the comparison group, but my neighbor is in the treatment group, and my mother knows their water is clean and sends me to their house to drink. In a sense, I am finding my way into the treatment group, even though I was assigned to the comparison group. This is called a crossover effect, which happens when people defy their treatment designation (knowingly or unknowingly) and outcomes are altered as a result. As with spillovers, by crossing over I no longer represent a good comparison group—since I have clearly been affected by the existence of the program. As before, changing the level of randomization could mitigate crossover effects.

4. Mechanics of Randomization

Once the unit and method of randomization have been determined, it is time to randomly assign individuals, households, communities, or any unit to either the treatment
or comparison group.

a) Simple Lottery
Generally, to start with, we need a list of (individual, household head, or village) names. We then randomly select, such as by flipping a coin or pulling names out of a hat, those who will be in the treatment group, with the remaining names in the comparison group (or vice versa). This could also be done as part of a public lottery. However, we don't always divide the study population exactly in half. We may wish to include 30 percent in the treatment group and 70 in the comparison. Or if we had a phase-in method with three periods, we may want to divide the population into three groups. We may also wish to test multiple treatments at the same time, which would also require several groups. In these more sophisticated evaluation designs, a coin flip will not suffice. Instead, randomization is typically done through a computer program.

b) Spot Randomization
Sometimes we do not have a list beforehand. For example, if individuals enter a clinic with symptoms of malaria, the decision of whether to administer the World Health Organization’s standard “DOTS” treatment or an enhanced alternative must be made on the spot. The treatment could be determined by the nurse at the clinic using the flip of a coin. Alternatives could include computerized or cell-phone based randomization.

c) Stratified Randomization
Frequently, the target population is divided into subgroups, known as strata, before randomizing. For example, a group of individuals can be divided into smaller groups based on gender, ethnicity, or age. This division into subgroups before
randomization is called stratification. Then the randomization exercise takes place within each of the strata. This is done to ensure that the proportion of treatment and comparison groups are balanced within each group so that researchers can understand whether the effect of the treatment varies by subgroup. For example, researchers may be interested in knowing whether a treatment affects female headed households differently than male headed households, but it is conceivable that without stratification we would end up with too few female headed households to be able to draw any conclusions about heterogeneous effects. Stratifying the sample according to the gender of the household head avoids this problem. The primary purpose of stratification is statistical and relates to sample size. The decision to stratify has no bearing on whether the results are biased.

5. Sample Selection and Sample Size

Whether an evaluation can detect outcome differences between the treatment and comparison groups depends on statistical power. Among other factors, statistical power depends on the number of units in the sample, or the sample size.

Once again, let's take our example of waterborne illness in a community, and let us assume that we have chosen to distribute chlorine tablets to households to test their impact on the incidence of diarrhea. But let us also assume that we only have a very limited budget for our test phase, so we would like to minimize the number of households that are included in the survey while still ensuring that we can attribute any changes in incidence to the chlorine tablets and not to random chance. How many households should receive the tablets, and how many should be surveyed? Is five
households enough? 100? 200? How many households should be in the comparison group? Power calculations help us answer these questions.

For more information on how to estimate the required sample size, see:

How to implement and obtain results

How to implement
Once an evaluation design has been finalized, the evaluator must remain involved to monitor data collection as well as the implementation of the intervention being evaluated. If respondents drop out during the data collection phase, the results are susceptible to attrition bias, compromising their validity. Attrition is covered in this section. Other threats in the data collection phase such as poor measurement instruments, reporting bias, etc, are equally important, but are not covered here. For best practices on data collection see:
In the implementation of the intervention, the integrity of the randomization should remain intact. Unless intentionally incorporated into the study's design, spillovers and
crossovers should be minimized, or at the very least, thoroughly documented. (See Threats to the design for background.)

1. Threats to Data Collection

Attrition occurs when evaluators fail to collect data on individuals who were selected as part of the original sample. Note that the treatment and comparison groups, through random assignment, are constructed to have comparable characteristics, on average, at the beginning of the study. The comparison group is meant to resemble the counterfactual, or what would have happened to the treatment group had the treatment not been offered. (See: Why Randomize?). If the type of individuals who drop out of the study are not systematically different in the treatment versus comparison groups, the (smaller) comparison group will still represent a valid counterfactual to the (smaller) treatment group. This will reduce our sample size and may change the target population to which our results can be generalized, but it will not compromise the “truth” of the results, at least applied to the restricted population. That is, our estimates of the effect of the program will remain unbiased.

For example, suppose our study area is rural and that many household members spend significant portions of the year working in urban areas. Suppose further that we created our sample and collected baseline data when migrant household members were home during the harvests and available for our study. If we collect our endline data during the off-peak season, the migrant family members will have returned to their city jobs and will be unavailable for our survey, so our study will now be restricted to only non-migrants. Assuming there is no systematic difference between the type of person
who migrates in the treatment versus comparison groups, the non-migrant population in the comparison group will represent a good counterfactual to the non-migrant population in the treatment group. Our measure of impact will be valid, but only applicable to the non-migrant population.

If, however, attrition takes a different shape in the two groups, the remaining comparison group no longer serves as a valid counterfactual, which will bias our results. Using our example of waterborne illness, suppose that, due to random chance, more children and mothers are ill in the comparison group. As a result, the young men who typically migrate to the cities during off-peak seasons stay back to help the family. Households that were assigned to the comparison group contain more migrants during our endline. It is entirely feasible that these migrants, of peak working age, are typically healthier. Now, even though our treatment succeeded in producing healthier children and mothers on average, our comparison group contains more healthy migrant workers than does the treatment group. When measuring the incidence of diarrhea, outcomes of the healthy migrants in the comparison group could offset those of their sicker family members. Then, when comparing the treatment and comparison groups, we could see no impact at all and may conclude the treatment was ineffective. This result would be false and misleading.

In this simplified example, we could forcibly reintroduce balance between the comparison and treatment groups by removing all migrants from our sample. Frequently, however, characteristics that could dependably identify both real and would-be attrits (those who disappear) have not been measured or are impossible to observe. Predicting attrition
can be difficult, and attrition bias can result in either overestimating or underestimating a program's impact.

2. Spillovers and Crossovers

Spillovers occur when individuals in the comparison group are somehow affected by the treatment. For example, if certain children in the comparison group of a chlorine dispensing study play with children who are in the treatment group, they have friends who are less likely to be sick, and are therefore less likely to become sick themselves. In this case, they are indirectly impacted by the program, despite having been assigned to the comparison group. Individuals who “crossover” are those in the comparison group who find a way to be treated. For example, if the mother of a child in the comparison group sends her child to drink from the water supply of a treatment group household, she is finding her way into the treatment group. Impartial compliance is a broader term that encapsulates crossovers and also treatment individuals who deliberately choose not to participate (or chlorinate their water, in this example).

When a study suffers from spillovers and crossovers, in many circumstances it is still possible to produce valid results, using statistical techniques. But these come with certain assumptions—many of which we were trying to avoid when turning to randomization in the first place. For example, if spillovers can be predicted using observed variables, they can be controlled for. With impartial compliance, if we assume that those who did not comply were unaffected by the intervention, and, by the same token, the individuals who crossed over were affected in the same way as those from the treatment group who were treated, we can infer the impact of our program. But, as discussed in the Why
Randomize section, the more assumptions we make, the less firm ground we stand on when claiming the intervention caused any measured outcomes.

How to obtain results
At the end of an intervention (or at least the evaluation period for the intervention), endline data must be collected to measure final outcomes. Assuming the integrity of the random assignment was maintained, and data collection was well-administered, it is time to analyze the data. The simplest method is to measure the average outcome of the treatment group and compare it to the average outcome of the comparison group. The difference represents the program’s impact. To determine whether this impact is statistically significant, one can test the equality of means using a simple t-test. One of the many benefits of randomized evaluations is that the impact can be measured without advanced statistical techniques. More complicated analyses can also be performed, such as regressions that increase precision by controlling for characteristics of the study population that might be correlated with outcomes. However, as the complexity of the analysis mounts, the number of potential missteps increases. Therefore, the evaluator must be knowledgeable and careful when performing such analyses.

It is worth noting that, when a result is obtained, we have not uncovered the true impact of the program with 100 percent certainty. We have produced an estimate of the truth and can say, with a certain degree of probability, whether the program had an effect. The larger our sample size, the smaller our standard errors will be, and the more certain we are that our measure is close to the truth. But we can never be 100 percent certain.
This fact leads to two very common pitfalls in analysis:

• **Multiple Outcomes**: Randomization does not ensure the estimated impact is a perfect measure of the true impact of the program. The measured impact is unbiased, but it is still an estimate. Random chance allows for some margin for error around the truth. Depending on the sample size and the amount of variation in the outcome, the estimate may be very close to the truth. If we have correctly calculated the sample size required to be able to say, with some degree of certainty, whether the program had an effect, then it is unlikely that we will draw incorrect inferences about the impact of the program on a single outcome. If we look at many outcomes, however, the chances of drawing incorrect inferences will increase. The more outcomes we look at, the more likely one or more of our estimates will deviate significantly from the truth, simply due to random chance.

For example, assume the chlorine pills we distributed to fight waterborne illness in our water purification program were faulty or never used. If twenty outcome measures are compared, it is in fact very likely that at least one comparison will suggest a significant change in health due to our program. If we look at enough outcome measures eventually we will stumble upon one that is significantly different between the treatment and comparison groups, simply due to random chance. This is not a problem, per se. The problem arises when the evaluator “data mines,” looking at outcomes until she finds a significant impact, reports this one result, and fails to report the other insignificant results that were discovered in the search.

• **Sub-group analysis**: Just as an evaluator can data mine by looking at many different outcome measures, she can also
dig out a significant result by looking at different sub-groups in isolation. For example, it might be that the chlorine has no apparent impact on household health as a whole. It may be reasonable to look at whether it has an impact on children within the household, or girls in particular. But we may be tempted to compare different combinations of boys and girls of different age groups and living in households with different demographic compositions and assets. We may discover that the program improves the health of boys between the ages of 6 and 8, who happen to have one sister, one grandparent living in the household, and where the household owns a TV and livestock. We could even concoct a plausible story for why this subgroup would be affected and other subgroups not. But if we stumbled upon this one positive impact after finding a series of insignificant impacts for other subgroups, it is likely that the difference is due simply to random chance, not our program.

How to draw policy implications

Having performed a perfect randomized evaluation, and an honest analysis of the results, with a certain level of confidence we can draw conclusions about how our program impacted this specific target population. For example: “Our chlorine distribution program caused a reduction in the incidence of diarrhea in children of our target population by 20 percentage points.” This statement is scientifically legitimate, or internally valid. The rigor of our design cannot tell us, however, whether this same program would have the same or any impact if replicated in a different target population, or if scaled up. Unlike internal validity, which a well-conducted randomized evaluation can provide, external validity, or generalizability, is more difficult to obtain. To extrapolate how these results would apply in a different
context, we need to depart from our scientific rigor and begin to rely on assumptions. Depending on our knowledge of the context of our evaluation, and other contexts upon which we would like to generalize the results, our assumptions may be more or less reasonable.

However, the methodology we chose—a randomized evaluation—does not provide internal validity at the cost of external validity. External validity is a function of the program design, the service providers, the beneficiaries, and the environment in which the program evaluation was conducted. The results from any program evaluation are subject to these same contextual realities when used to draw inferences for similar programs or policies implemented elsewhere. What the randomized evaluation buys us is more certainty that our results are at least internally valid.

**Common questions and concerns about randomized evaluations**

Is it ethical to assign people to a comparison group, potentially denying them access to a valuable intervention?

If there is rigorous evidence that an intervention is effective and sufficient resources are available to serve everyone, it would be unethical to deny some people access to the program. However, in many cases we do not know whether an intervention is effective (it is possible that it could be doing harm), or if there are enough resources to serve everyone. When these conditions exist, a randomized evaluation is not only ethical, but is also capable of generating evidence to inform the scale-up of effective interventions or to shift resources away from ineffective interventions.

When a program is first being rolled out or is oversubscribed,
financial and logistical constraints may prevent an organization from serving everyone. In such a case, randomization may be a fairer way of choosing who will have access to the program than other selection methods (e.g. first-come, first-served). Conducting a randomized evaluation may change the selection process, but not the number of participants served.

It is also possible to conduct a randomized evaluation without denying access to the intervention. For example, we could randomly select people to receive encouragement to enroll without denying any interested participants access to the intervention. In other cases, it may be useful to compare two different versions of an intervention, such as an existing version and a version with a new component added.

Is it possible to conduct randomized evaluations at low-cost without having to wait years for the results?

Collecting original survey data is often the most expensive part of an evaluation, but it is not unique to randomized evaluations. Likewise, it is increasingly possible to conduct evaluations at relatively low cost by measuring outcomes using existing administrative data, instead of collecting survey data.

The length of time required to measure the impact of an intervention largely depends on the outcomes of interest. For example, long-term outcomes for an educational intervention (e.g. earnings and employment) require a lengthier study than shorter-term outcomes, such as test scores, which can be obtained from administrative records.

Finally, the time and expense of conducting a randomized evaluation should be balanced against the value of the evidence produced and the long-term costs of continuing to
implement an intervention without understanding its effectiveness.

Can a randomized evaluation tell us not just whether an intervention worked, but also how and why?

When designed and implemented correctly, randomized evaluations can not only tell us whether an intervention was effective, but also answer a number of other policy-relevant questions. For example, a randomized evaluation can test different versions of an intervention to help determine which components are necessary for it to be effective, provide information on intermediate outcomes in order to test an intervention's theory of change, and compare the effect of an intervention on different subgroups.

However, as with any single study, a randomized evaluation is just one piece in a larger puzzle. By combining the results of one or more randomized evaluations with economic theory, descriptive evidence, and local knowledge, we can gain a richer understanding of an intervention’s impact.

Are the results of randomized evaluations generalizable to other contexts?

The problem of generalizability is common to any impact evaluation that tests a specific intervention in a specific context. Properly designed and implemented randomized evaluations have the distinct advantage over other impact evaluation methods of ensuring that the estimate of an intervention’s impact in its original context is unbiased.

Further, it is possible to design randomized evaluations to address generalizability. Randomized evaluations may test an intervention across different contexts, or test the replication of an evidence-based intervention in a new context. Combining a theory of change that describes the conditions
necessary for an intervention to be successful with local knowledge of the conditions in each new context can also inform the replicability of an intervention and the development of more generalized policy lessons. Learn more about addressing generalizability.

History of randomized evaluations

1. Clinical Trials
The concept of a treatment and comparison group was introduced in 1747 by James Lind when he demonstrated the benefits of citrus fruits in preventing scurvy using a scientific experiment.¹ As a result of his work, Lind is considered to be the father of clinical trials. The method of randomly assigning subjects to comparison and treatment groups, however, was not developed until the 1920s.

2. Agricultural Experiments
Randomization was introduced to scientific experimentation in the 1920s when Neyman and Fisher conducted the first randomized trials in separate agricultural experiments. Fisher’s field experimental work culminated with his landmark book, The Design of Experiments, which was a main catalyst for the much of the growth of randomized evaluations.²

3. Social Programs
Randomized trials were introduced to government-sponsored social evaluations between 1960 and 1990. Rather than small-scale studies conducted on plants and animals, these new social evaluations were significantly larger in scale and focused on people as the subjects of interest. The idea of conducting social policy evaluations grew out of a 1960s debate over the merits of the welfare system. The
model was later applied both in Europe and the United States to evaluate other programs such as electricity pricing schemes, employment programs, and housing allowances. Since then, similar types of evaluations have been used across disciplines and in a variety of settings around the world to guide policy decisions. ³


³ Ibid

Who conducts randomized evaluations?

J-PAL was founded in 2003 as a network of affiliated professors who conduct impact evaluations using the randomized evaluation (RE) methodology to answer questions critical to poverty alleviation. J-PAL affiliates also conduct non-randomized research, and many other people and organizations conduct REs. Since J-PAL’s founding, more than 200 organizations have partnered with a J-PAL affiliate on an RE. Amongst key players in poverty alleviation and development, the idea of REs is now fairly well-known.

Of the top ten U.S. foundations,¹ four of the six that work on international development have partnered with a J-PAL affiliate on a RE. These include the Bill & Melinda Gates Foundation, the Ford Foundation, the William and Flora Hewlett Foundation, and the John D. and Catherine T. MacArthur Foundation.²

Of the top ten multilateral organizations,³ four have partnered with a J-PAL affiliate on a RE (the World Bank, the
Asian Development Bank, Unicef, and the Inter-American Development Bank), and six of the ten have sent staff to J-PAL’s training courses.

Of “The Big Eight” relief organizations, 4 Save the Children, Catholic Relief Services, CARE, and Oxfam have partnered with a J-PAL affiliate on an RE. The International Rescue Committee is doing REs on its own. And six of the eight have sent staff to J-PAL’s training courses.

Governments also partner with J-PAL affiliates. Major donor country partners include the United States (USAID, MCC), France (Le Ministre de la Jeunesse et des Solidarités Actives), Sweden, and the United Kingdom (DFID). Developing country government partners have been both at the national level (e.g. Kenya’s Ministry of Education and the Government of Sierra Leone’s Decentralization Secretariat) and the sub-national level (e.g. the Government of Andhra Pradesh, the Gujarat Pollution Control Board, and the Rajasthan police).

A number of research centers have been established with the support or under the direction of J-PAL affiliates. These research centers often run affiliates’ REs and employ the staff associated with each RE. These research centers include: Innovations for Poverty Action (IPA), Centre for Microfinance, Center for International Development’s Evidence for Policy Design, Center for Effective Global Action, Ideas42, and the Small Enterprise Finance Center.

Private companies also conduct randomized evaluations of social programs. Mathematica Policy Research and Abt Associates are two examples.

1 When measured by endowment.
2 The other two that work on international development but
have not partnered with J-PAL are the W.K. Kellogg Foundation and the David and Lucile Packard Foundation.
The four that we have judged as having a domestic U.S. focus are the Getty Trust, the Robert Wood Johnson Foundation, the Lilly Endowment Inc., and the Andrew W. Mellon Foundation.

3 When measured by official development assistance granted, including The World Bank, the African Development Bank Group, The Global Fund, the Asian Development Bank, the International Monetary Fund, Unicef, UNRWA, Inter-American Development Bank, the United Nations Development Program, and the World Food Program.

4 When measured by annual budget. These are World Vision, Save the Children, Catholic Relief Services, CARE, Médecins Sans Frontières, Oxfam, International Rescue Committee, and Mercy Corps.

For more resources on randomized evaluations, see:

- Running Randomized Evaluations - R. Glennerster and K. Takavarasha, November 2013
- Field Experiments: Design, Analysis and Interpretation. A. Gerber and D. Green, May 2012
- Evaluating Social Programs: Executive Education at J-PAL
- Lecture videos and course content from a past Executive Education Course
- A free online version of the course
- Randomized Evaluations of Educational Programs in Developing Countries: Some Lessons. M. Kremer
- Field Experiments in Development Economics. E. Duflo, January 2006
- Use of Randomization in the Evaluation of Development
Effectiveness. E. Duflo and M. Kremer, July 2003
- Scaling Up and Evaluation. E. Duflo, May 2003
- Nonexperimental Versus Experimental Estimates of Earnings Impacts. S. Glazerman, D. Levy and D. Myers, May 2003

Please note that the practical research resources referenced here were curated for specific research and training needs and are made available for informational purposes only. Please email us for more information.

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https://www.povertyactionlab.org/research-resources/introduction-evaluations