ECON 672: Economics of Development

Week 5: Impact evaluation

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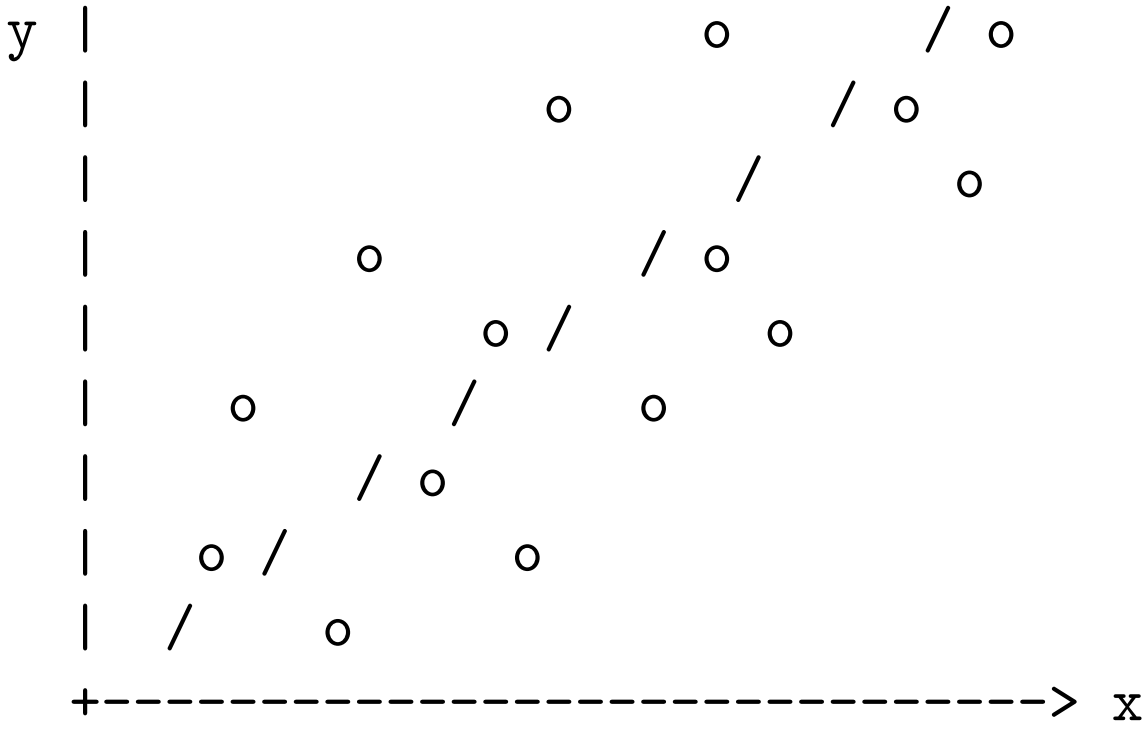
Disclaimer

Warning: econo (economics) metrics (measurement) ahead



Banerjee & Duflo on the Spread of RCTs

> *The number of completed RCTs[1] within J-PAL[2] went from 327 in 2008 to more than 2,204 in 2024, and other organizations, including the World Bank, the Inter-American Development Bank, and many others, also added trials of their own [...] Many of those who had initially expressed skepticism about the value of field experiments, and others who were simply busy with other work, seemed taken by the new insights that were coming out of the RCTs and decided to dip their feet in. Other fields of economics, focused on rich countries rather than the developing world where we were doing most of our work, also discovered or rediscovered the joy of experimentation and brought in a whole slew of new talent.*
>
> — *Banerjee & Duflo, p. x*

[1] Randomized Controlled Trials

[2] Abdul Latif Jameel Poverty Action Lab

What Impact Evaluation Is For

Impact evaluation identifies *causal* effects[3] of a program on relevant outcomes

- Accountability and ex-post cost-benefit analysis of causal impacts for financers by external evaluators
- Results-based management using evidence to tweak or overhaul design for subsequent phase or scale-up (or cancelation)
- General lessons for the field as a public good (e.g. work on conditional cash transfers or CCTs)

[3]We distinguish causal effects from correlational findings; whether or not a program is *associated with* changes in outcomes is much less informative than showing the program *caused* a difference in outcomes.

The Potential Outcomes Setup

Setup:

- Causal impact (δ) of program $P \in \{0, 1\}$ on outcome Y, for units $i \in \text{n} \subset \text{N}$, our sample[4] within the population[5]
- Ideal: $\delta_i = Y_i(1) - Y_i(0)$
- Constraint: the **counterfactual**, or what outcomes would have looked like without the program, is unobservable
- Reality: take averages[6] across units:

$$\delta = \underbrace{E[Y_i(1) - Y_i(0)]}_{\text{Average difference in outcomes}} = \underbrace{E[Y_i(1)] - E[Y_i(0)]}_{\text{Difference in average outcomes}}$$

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[4] Our sample, indexed from $i = 1, \ldots, n$, is ideally *representative*.

[5] Our true population, indexed from $i = 1, \ldots, N$, may include past, present, future, or hypothetical units and is generally unobservable.

[6] Because the expectation operator $E[\ldots]$ is linear, the average of differences is equal to the difference of averages.

Causal Methods for Impact Evaluation

Causal methods:

- **Randomized controlled trials (RCTs)**, the gold standard of impact analysis, assume generalizability and exogeneity and so may still face problems (Deaton, 2010)
- **Matching methods** including *propensity score matching (PSM)* that construct comparable groups on observables, assuming unobservables are also comparable. This delivers the **treatment on treated (ToT)** estimate, the average difference in outcomes between treated participants and matched non-treated, non-participants[7].

[7] We may employ such a design when it is not feasible or ethical to generate a typical control group of untreated units with which to compare treated units.

Causal Methods: Differences and Panels

Causal methods pt. 2:

- **Difference-in-difference (DiD)** compares changes in outcomes before and after a program for treated and untreated groups, assuming parallel trends:

$$ATE = \Delta E[Y_T] - \Delta E[Y_C] = \left(\bar{Y}_{T1} - \bar{Y}_{T0}\right) - \left(\bar{Y}_{C1} - \bar{Y}_{C0}\right)$$

- **Panel analysis** or staggered dif-in-dif compares program rollout with heterogeneous timing, assuming no changes in context or behavior between units over time outside of program[8]
- **Event study** compares outcomes many periods before and after a policy change for treated and untreated units

[8] Imagine we compare outcomes of a subsidy program delivered either 6 months or 1 year ago; if we believe the transfer sets treated households on a continuous path of income growth, it would not be appropriate to compare early- and late-treated households with a staggered dif-in-dif, as earlier-treated households would have had longer to accumulate additional wealth.

Causal Methods: Discontinuities and Instruments

Causal methods pt. 3:

- **Regression discontinuity (RD)** compares units just above and below a threshold (e.g. Dell, 2010), assuming no sorting and smoothness over threshold. Under **sharp RD** the threshold is fully binding, so assignment perfectly predicts take-up; under **fuzzy RD** it is only mostly binding, so assignment serves as an instrument for take-up
- **Instrumental variables (IV)** approach rescales variation in an endogenous independent variable by an exogenous instrument (e.g. Acemoglu, Johnson, & Robinson, 2001)
- **Natural experiments** are rare but sometimes present themselves, where nature (or policy) has already randomized assignment (e.g. Chattopadhyay & Duflo, 2004)

Experimental Design and the Average Treatment Effect

Experimental design:

- Ethics[9] of choosing control group, non-deception, non-compliance, and withdrawal of consent
- **Average treatment effect (ATE)** can be estimated as $ATE = \bar{Y}_T - \bar{Y}_C$. Our ATE is statistically significant if our two means are *significantly different*[10]
- **Conditional average treatment effect (CATE)** is the ATE for specific values of observables X, $CATE = \left(\bar{Y}_T - \bar{Y}_C\right) \mid X$

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[9] A key step for any experiment with human subjects is approval by the **Institutional Review Board (IRB)**, which ensures proper consent from, treatment of, and disclosure to subjects before, during, and after the project.

[10] We typically test mean differences using a t-test at the 5% level or nonparametric Chi-squared test for percentage differences.

Ordinary Least Squares

Regression is a central tool in empirical economics to estimate relationships between variables, and to test hypotheses about them:

- We are interested in the true relationship between an outcome and one or more predictors in a *population*, but we observe only *sample(s)*, so we must estimate the effect. We call using sample estimates to learn about the population *inference*; whether those estimates recover the *true* effect we care about, rather than a biased one, we call *identification*.
- **Ordinary Least Squares (OLS)** is a common method of estimating this (linear) relationship by minimizing the sum of squared residuals, deviations between observed and predicted outcome values, across sample observations.

Bias and the Standard Error

Key properties of OLS estimation:

- Our estimator is **unbiased** if it is correct (equal to the population value) on average across samples, and **consistent** if it converges to the truth as the sample size grows.
- Our estimator's **standard error** estimates how much our estimate would vary from sample to sample. We use this for constructing confidence intervals and hypothesis testing, so an incorrect standard error leads to improper inference.
- Note that using OLS does not necessarily deliver a *causal* estimate of the relationship between predictor(s) and outcome; this requires additional assumptions.

Randomized Controlled Trials

RCTs are a workhorse tool of development economics: by randomly assigning units to treatment or control, we may estimate a causal treatment effect.

But the field is messy, and even a carefully designed experiment can face problems that threaten its validity.

Today we cover common threats to RCT designs and how to address each, whether we are *designing* an experiment or *evaluating* one. We organize these broadly around two questions:

- **Is our estimate right?** Non-compliance, spillovers, and attrition can *bias* our estimate of the effect (a problem of *identification*); external validity asks whether it *generalizes*.
- **Is our uncertainty right?** Clustering deflates our standard errors, multiple hypothesis testing inflates false positives, and low power hides true effects, all problems of *inference*.

Seeds and Crop Yields

Suppose a development agency wants to know whether a new *experimental seed technology* raises farmers' crop yields.

We randomly assign farmers to treatment (the new seed) or control (their usual seed), and measure each farmer's yield during harvest:

$$Y_i = \beta_0 + \delta P_i + \beta_1 X_{1i} + \cdots + \varepsilon_i \quad \text{(population)}$$
$$Y_i = b_0 + d P_i + b_1 X_{1i} + \cdots + e_i \quad \text{(sample)}$$

Here Y_i is farmer i's yield, P_i indicates the program, δ is the true causal effect, and d estimates the seed's causal effect in the sample (unbiased under proper randomization)[11].

[11] Additionally, ε_i and e_i are the error term and residual, capturing unobserved factors affecting yields in the population and sample, respectively. X_{1i} is an observable covariate with coefficient β_1, and "$\cdots$" allows for further covariates. As Deaton (2010) points out, proper randomization should make inclusion of covariates unnecessary to obtain unbiased estimates of δ, though their inclusion may still improve the precision of d and thus aid inference.

Hypothesis Testing and the p-value

Estimating an effect is not the same as establishing that there is one, so we frame the question as a **hypothesis test**:

- The **null hypothesis** H_0 says there is no effect, $\delta = 0$, and the **alternative** H_1 says there is one, $\delta \neq 0$.
- Our **test statistic** rescales the estimate by its standard error, $t = \frac{d}{s_d}$, asking how many standard errors our estimate sits away from the null.
- The **p-value** is the probability of seeing an estimate at least this extreme *if the null were true*. A small p-value means our data would be surprising in a world with no effect.
- We **reject the null** when the p-value falls below a chosen **significance level** α, conventionally 5%. Failing to reject is not evidence of no effect; we may simply lack the power to detect one.

Why Randomization Helps

Random assignment aims to make the treatment and control groups comparable from the start: because treatment is decided purely by chance, it is unrelated to any farmer-specific factors, observed or unobserved.

This means P_i is uncorrelated with the error term, and d is an unbiased, consistent estimate of the average treatment effect.

Even valid randomization is not airtight (Deaton, 2010). *Hawthorne* and *John Henry effects*, where farmers behave differently because they are observed or untreated, and *experimenter demand effects*, where farmers offer the outcomes they perceive we are looking for, can both bias d; blinding and unobtrusive measurement help.

The threats that follow are the ways this clean result breaks down in the field. They either *bias* d, a problem of *identification*, or leave d unbiased while undermining the *inference* we draw from it.

Checking Balance

Balance checks. Before trusting our estimate d, we confirm randomization actually produced comparable groups by comparing *pre-treatment* characteristics across arms: baseline plot size, soil quality, and prior yield.

Sharp differences suggest the random assignment failed or was compromised; we can then control for these covariates or investigate further.

With many covariates, though, we would expect *some* to look different purely by chance, roughly 5% of them when we test at the 5% level, so we typically judge balance with a *joint* test rather than variable by variable.

Balance on *observables* is reassuring but not decisive: randomization is what secures balance on *unobservables*, in expectation.

Non-compliance

Non-compliance. In the field, not everyone assigned to treatment (P_i) actually takes it up (W_i). Suppose only the most experienced farmers plant the new seed when assigned to treatment.

Comparing those who *actually used* the seed against everyone else no longer compares randomly assigned groups, so d is biased by *who chose to comply*.

We call those who always take up the program regardless of treatment assignment **always-takers** ($W_i = 1$ always), while those who never take up the program are **never-takers** ($W_i = 0$ always). **Compliers** are those who behave as assigned ($W_i = P_i$) while those who behave *opposite from* assignment are **defiers** ($W_i = 1 - P_i$ for $W_i, P_i \in \{0, 1\}$).

Non-compliance: ITT and LATE

Two methods keep randomization intact under non-compliance:

- **Intention-to-treat (ITT):** compare groups by their *original assignment*, ignoring take-up. This measures the effect of being *offered* the seed, often the policy-relevant question.
- **Local average treatment effect (LATE):** use assignment as an *instrument* (a nudge that shifts take-up but affects yields only through it) to recover the effect of *using* the seed, for the ("local") *compliers* who adopt only when assigned.

A **randomized encouragement design** uses the same idea when treatment cannot be randomized (e.g. program is already available but has low take-up), as long as the nudge never pushes anyone the *opposite* way[12] (we call this condition *monotonicity* or "no defiers").

▸ ITT and LATE specifications

[12] *Defiers* in this case would be farmers who adopt the new seed when assigned to control or vice versa.

Spillovers and SUTVA

Spillovers (interference). Our comparison assumes one farmer's treatment does not affect another's *outcome*: the **Stable Unit Treatment Value Assumption (SUTVA)**, specifically the *no-interference* part[13].

But if treated farmers share their seed, or what they learn, with neighbors (*peer effects*), the control group becomes partly treated.

This **spillover** shrinks the gap between groups and biases our estimate *toward zero*[14].

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[13] SUTVA also requires *no hidden variations of treatment*: every treated farmer receives the same, well-defined version of the seed. If instead some experimental seeds were rotten and thus ineffective, this assumption would be violated.

[14] The direction of bias depends on the spillover's sign: positive spillovers onto the control group, as here, pull the estimate toward zero, while negative spillovers, such as treated farmers out-competing their neighbors for buyers, can make controls worse off and push the estimate away from zero.

Spillovers: Design Fixes

We can limit spillovers at the *design stage*, through how we set up the experiment:

- **Randomize whole villages** instead of individual farmers, so spillovers stay *within* units
- **Vary the share treated** across villages to measure spillovers directly and include this in the analysis (Baird et al., 2018)
- Introduce **buffer zones** between areas to limit spillover contamination

Attrition

Attrition bias. Some farmers may also drop out of the sample before we measure their yields: they migrate, refuse follow-up surveys, or cannot be found.

If attrition is *differential*, tied to both treatment and outcome, the remaining sample is no longer randomly assigned.

If discouraged treated farmers leave, the treated who *remain* look unusually successful, and d overstates the true effect.

Attrition: Corrections

ITT/LATE does **not** fix attrition; it still needs an outcome for everyone, which is exactly what is missing. Instead we can:

- **Recover it** by intensively tracking and re-surveying a (random sub)sample of those lost.
- **Bound it** with **Lee (2009) bounds**: if drop-out runs one way, trim the better-retained group to the other's response rate, giving a worst-to-best-case range.
- **Reweight** survivors to stand in for similar dropouts based on their predicted likelihood of attrition (*inverse-probability weighting*).

External Validity

Site-selection bias. An RCT cleanly estimates the effect only *for the units that took part*; whether it *generalizes* beyond them is a separate question of *external validity*.

Suppose skeptical, poor-soil villages decline to enroll. Our estimate may still be *internally valid*, measuring in our design what we think we are measuring for the villages that participated, the *identification* concern from before. It need not carry over to villages that opted out, or to other regions, times, or implementers.

External Validity: Scale and Remedy

Scale is a second reason our results may not travel (Deaton, 2010). Our treatments usually study one side of a market at a time, so what we recover is a *partial equilibrium* effect; when the whole economy adjusts, the *general equilibrium* impact may look quite different.

If every farmer in the region plants the new seed, the harvest glut may push prices down far enough to undo the income gain we measured on a handful of villages.

No estimator fixes either problem; the remedy is at the *sampling stage*: we choose study sites that resemble our population of interest, and explicitly acknowledge any remaining threats to external validity.

de Janvry & Sadoulet on Validity and Policy

A great deal of attention is given in impact evaluation to internal validity. This consists of showing that the control group has been properly chosen against the treatment group within the selected population of eligible control individuals, households, institutions, or communities. But usefulness for policy purposes requires that results also achieve some reasonable level of external validity with respect to the broader population of eligible individuals, households, institutions, or communities. Establishing the external validity of an impact can be obtained by replicating the experiment over different selections of eligibles, by estimating behavioral parameters (e.g. elasticities), by exploiting heterogeneity in impact, or by planning experimentation across a variety of contexts.

— *de Janvry & Sadoulet, pp. 129-130*

Banerjee & Duflo on Experiments vs. Non-Experiments

[...] the concerns about experiments are not new. However, many of these concerns are based on comparing experimental methods, implicitly or explicitly, with other methods for trying to learn about the same thing [...] Note that, although some of these issues are specific to experiments (we point these out along the way), most of these concerns (external validity, the difference between partial equilibrium and market equilibrium effects, nonidentification of distribution of effect) are common to all microevaluations, both with experimental and nonexperimental methods. They are more frequently brought to the forefront when discussing experiments, which is likely because most of the other usual concerns are taken care of by the randomization.

— *Banerjee & Duflo, 2009, p. 159*

Clustered Standard Errors

Clustering. Having randomized by *village*, our estimate may be unbiased but its *standard error* could now be wrong.

Farmers in a village may share weather, prices, and leadership, so their errors are correlated, while those in different villages may not.

OLS assumes *independent* errors, so this within-village correlation makes the conventional standard error *too small*, and we overstate our certainty.

Clustered Standard Errors: Two Caveats

The fix is **cluster-robust standard errors**, which allow correlation *within* villages but assume independence *across* them. Two caveats:

- **Independence across clusters must hold**: a shared region-wide shock (e.g. war) would force us to cluster at the region level rather than the village level, which may make inference challenging with a small number of regions.
- **We need enough clusters**: cluster-robust standard errors rely on having *many* villages (the more the better), with 30 to 50 as a common floor. With fewer, the *wild cluster bootstrap*[15] is the standard fix. We also watch out for *thin*, *unequal*, or *few treated* clusters, which can make inference unreliable.

[15] Bootstrapping generally is a technique of treating the sample as the population and resampling many times with replacement, mimicking the true unobserved sampling process. The wild cluster variant does something slightly different: rather than resampling villages, it re-draws the data by randomly flipping the sign of each village's residuals.

Within-Village Clustering

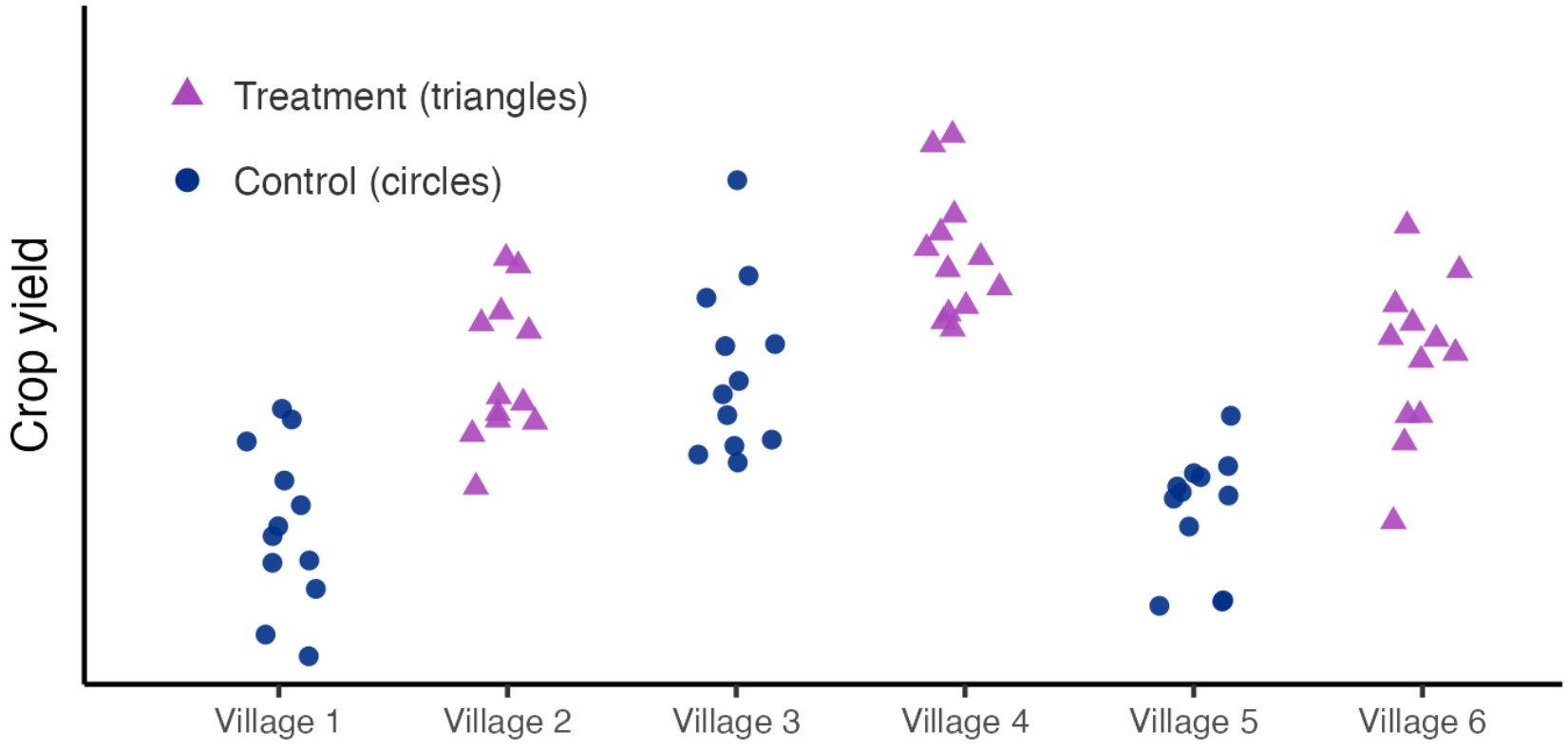

Figure 1: Whole villages are randomized to treatment or control; the two groups are comparable, and yields cluster tightly within each village.

Testing Many Outcomes

Multiple hypothesis testing. Field experiments typically record many outcomes: yield, plant height, disease resistance, farmer income, and we may want to estimate treatment effects for all of them. Remember that for *each* hypothesis test, we have an α chance of a *false positive* (a significant result when the null is true) when testing at the α level.

If we test enough at the 5% level, this becomes a serious problem: with 20 independent true nulls, the chance of at least one false positive is $1 - \Pr[\text{No false positives}] = 1 - (1 - 0.05)^{20} \approx 0.64$.

Reporting only the tests that "worked" invalidates our inference.

Testing Many Outcomes: False Positives Multiply

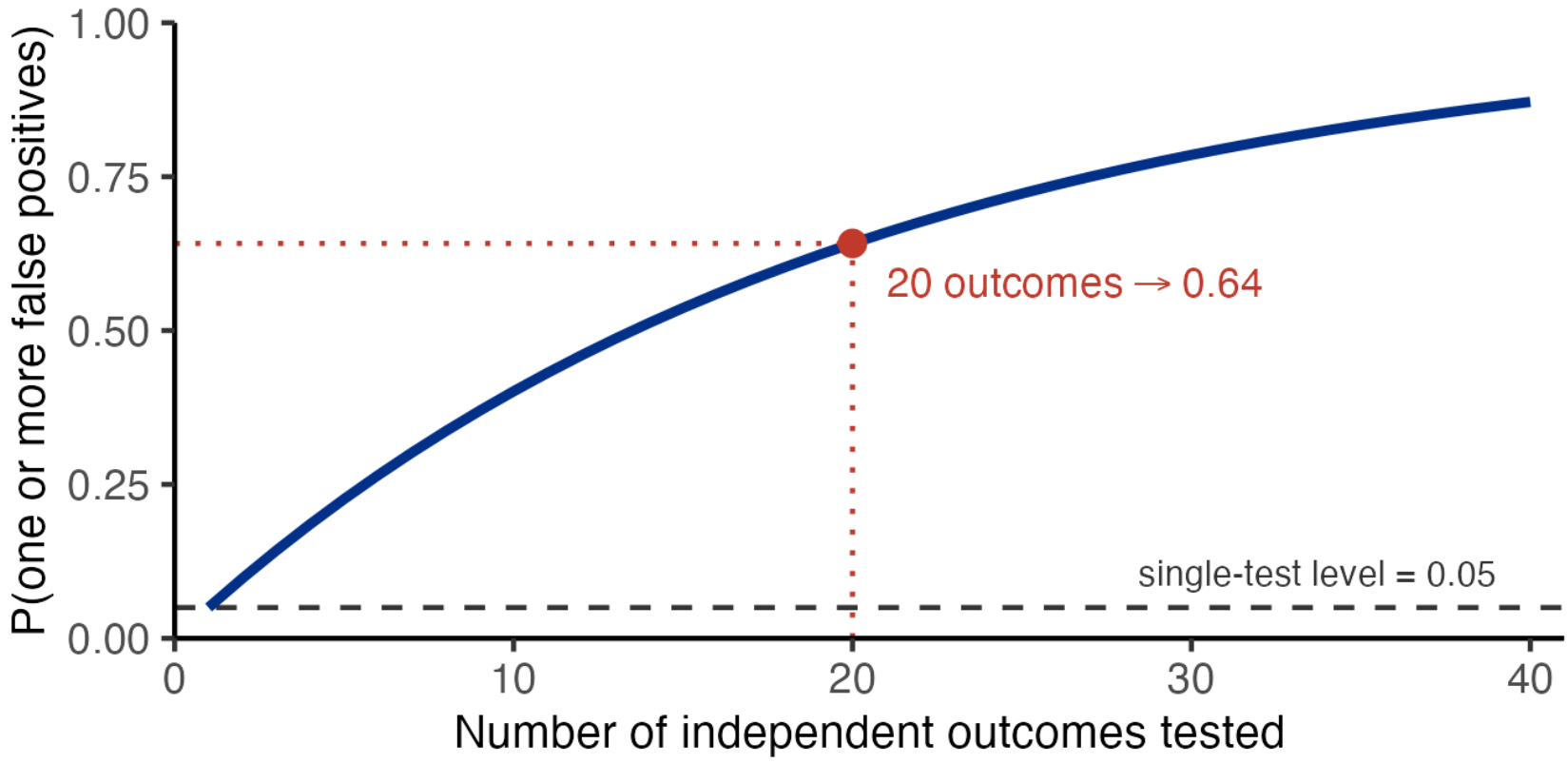

Figure 2: The chance of at least one false positive climbs quickly as we test more outcomes.

Testing Many Outcomes: Safeguards

With many outcomes, we can protect our inference in three ways:

- **Pre-register** a *pre-analysis plan* naming the main outcomes in advance, ruling out *p-hacking* (searching the data for whatever turns out significant and not reporting nulls).
- **Adjust the threshold** by applying a correction[16] for *multiple hypothesis testing* that raises the bar for significance as we test more outcomes, keeping the overall false-positive rate in check.
- **Aggregate** related outcomes into a single *summary index*, typically an average of the standardized outcomes, so one test replaces many, e.g. "harvest success".

[16] One such correction for multiple hypothesis testing is the *Bonferroni*, a quite conservative adjustment to p-values that often leads to weakly significant results becoming insignificant.

Statistical Power

Power is the chance of detecting a real effect when there is one (i.e. the null is false). The **minimum detectable effect (MDE)** is the smallest true effect a study can reliably detect; researchers often target a specific MDE (e.g. a 10% yield increase) at the design stage, yet can still end up underpowered due to unexpected variation, attrition, or low take-up.

Underpowered studies are doubly dangerous. They usually *miss* real effects, and because their standard errors are large, only an unusually big estimate can clear the significance bar. So the significant findings that survive *overstate* the true effect.[17]

Power also falls as we *cluster*: correlated farmers add less than one independent observation each, so the number of *villages* is the more relevant unit of observation. Always run a *power calculation before* collecting data (many grants will also ask for this and the MDE).

[17] This selection-driven inflation of statistically significant estimates is known as the *winner's curse*, or *Type M (magnitude) error* (Gelman & Carlin, 2014).

Statistical Power: Power vs. Type II Error

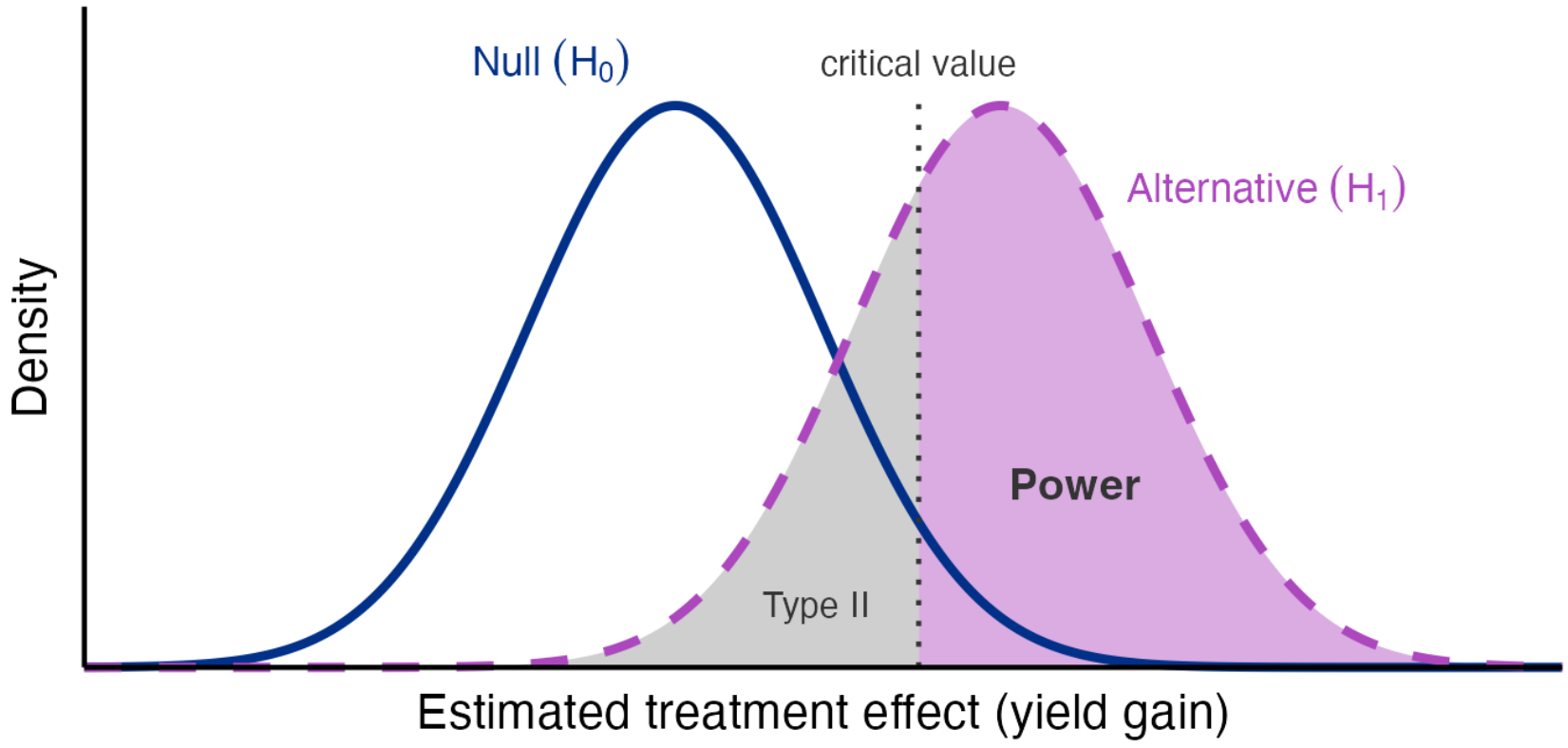

Figure 3: Power is the share of the alternative distribution that falls beyond the critical value. We can increase power by increasing the sample size.

Summary: Designing and Evaluating Field Experiments

To review, we ask two broad questions when we design or evaluate a field experiment:

- **Is the estimate right?** *(identification)* Watch for *non-compliance* (report ITT and LATE), *spillovers* (randomize at the right level), and *attrition* (track, bound, or reweight); and consider *external validity*, whether the effect generalizes.
- **Is the uncertainty right?** *(inference)* Cluster at the level of randomization, ensure sufficient clusters, adjust for *multiple hypothesis testing*, and sufficiently *power* the study (i.e. recruit a large enough sample) ahead of time.

Famous RCT Designs in Development

Famous RCT designs (many we will read):

- Deworming programs for schoolchildren
- (Conditional) cash transfers on household incomes
- Microcredit and microfinance on entrepreneurs
- HIV testing information on sexual health behavior
- Improving management practices in firms

de Janvry & Sadoulet on the Black Box

An important limitation of simple impact analyses is their "black box" nature: they provide a measure of the impact of a very specific program in the way it was implemented, but cannot tell us, for example, why people responded the way they did, nor how they would have responded had the level of subsidy been different.

— de Janvry & Sadoulet, p. 113

Deaton on Learning What Works

[...] my main concern is with how we should go about finding out whether and how assistance works and with methods for gathering evidence and learning from it in a scientific way that has some hope of leading to the progressive accumulation of useful knowledge about development.

— *Deaton, 2010, p. 425*

Banerjee & Duflo on Waiting for the Spark

[...] we are largely incapable of predicting where growth will happen, and we don't understand very well why things suddenly fire up [...] Given that economic growth requires manpower and brainpower, it seems plausible, however, that whenever that spark occurs, it is more likely to catch fire if women and men are properly educated, well fed, and healthy and if citizens feel secure and confident enough to invest in their children and to let them leave home to get the new jobs in the city. It is also probably true that until that happens, something needs to be done to make that wait for the spark more bearable.

— *Banerjee & Duflo, p. 314*

5-minute Break

Deaton (2010)

Randomly selected presenter: **Oliver**

1. What is the research question?
2. How do the authors answer it?
3. What do they find?
4. Are you convinced by the design and results?
5. How does the paper connect to our other readings?

Group Discussion

Bringing together our lecture material and academic article, I have prepared the following suggested discussion questions:

- We have seen a few of these causal estimation methods in our required and recommended readings; how were they used, what did they assume, and how did they find causality?
- How should we reconcile Deaton's concerns with RCTs against Banerjee, Duflo, and others' success with them in development economics?
- We have previously discussed Rodrik's caution to tailor interventions to a place in the design and implementation stages of an experiment. Using what we have seen this week, what steps should we also take *after* an experiment has been done to ensure prudent and valid impact evaluation?

Roadmap

Looking Ahead to Week 6

What do we have on the horizon before next Tuesday?

- Our **Preliminary Literature List** assignment will be due on Friday at 6pm
- My office hours for ECON 672 will be held Tuesday before class, 12:15-2:15pm in MCL 108 or virtually by appointment
- Our sixth topic will be *Poverty, vulnerability, and poverty traps*. Our textbook reading will be Chapter 5. Our *Poor Economics* reading will be Chapter 2. Our required journal article will be Kraay & McKenzie (2014), "Do Poverty Traps Exist? Assessing the Evidence," JEP.
- Our **Weekly Reading Response** assignment for this paper will be due Tuesday at 2:40pm before class. One student will be randomly selected to **present their response** to the class in 5-8 minutes.

Appendix: ITT Specification

Let P_i denote random *assignment* (1 if offered the new seed) and W_i actual *take-up* (1 if the farmer plants it). Under full compliance $P_i = W_i$, so the P_i of the main model captures both; under non-compliance they diverge.

Intention-to-treat regresses yield on *assignment*:

$$Y_i = \alpha_0 + \tau_{ITT} P_i + u_i$$

Because P_i is randomized, OLS recovers τ_{ITT}, the effect of being *offered* the seed.

Appendix: LATE Specification

LATE instruments *take-up* W_i with random *assignment* P_i (2SLS):

$$\text{first stage:} \quad W_i = \pi_0 + \pi_1 P_i + \nu_i$$

$$\text{second stage:} \quad Y_i = \gamma_0 + \tau_{LATE}\, \hat{W}_i + \xi_i$$

The second stage regresses yield on the fitted $\hat{W}_i$, so τ_{LATE} uses only assignment-driven take-up. When just-identified[18] it equals the Wald ratio $\tau_{LATE} = \frac{\tau_{ITT}}{\pi_1}$, recovering the effect of *using* the seed for compliers[19].

[18] A just-identified case is one with the same number of instruments (P_i) as endogenous variables (W_i).

[19] Valid under instrument relevance ($\pi_1 \neq 0$), the exclusion restriction, and monotonicity (no defiers).

Appendix: ITT vs. LATE

We may consider whether to report the ITT or LATE estimate; in practice these are often presented together.

ITT measures the direct impact of treatment assignment (e.g. being offered seed) on the outcome, while the LATE rescales this effect by the strength of the first stage[20]. These both utilize the same identifying variation, but are relevant for different research questions.

We may prefer ITT estimates when considering the effect of a policy *as implemented* (for always-takers, compliers, never-takers, and defiers), while LATE estimates are more relevant for understanding the effect of *actually utilizing* the treatment (for compliers[21]).

◂ Back to ITT and LATE

[20] The first stage is equal to the raw take-up rate only under one-sided non-compliance, e.g. when no farmers in the control group get the new seed.

[21] Remember that the validity of the LATE estimate relies on the no-defiers assumption, e.g. no farmers who take up the treatment only when assigned to the control group and vice-versa.