PAL

ABDUL LATIF JAMEEL
Poverty Action Lab

TRANSLATING RESEARCH INTO ACTION

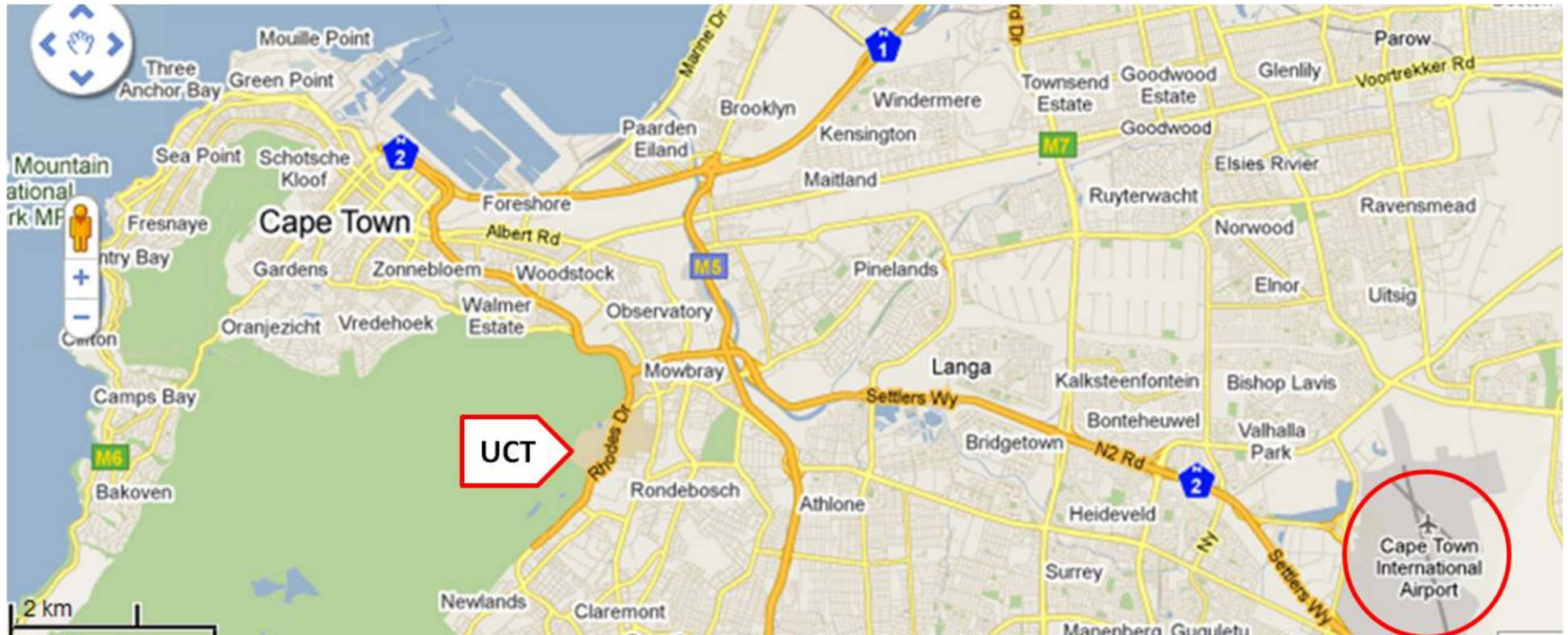University of Cape Town

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PROGRAMME

J-PAL Executive Education Course in Evaluating Social Programmes, 18 – 21 January 2011

Leslie Science Building, Auditorium LS 1E, University of Cape Town

	Tuesday 18 January 2011	Wednesday 19 January 2011	Thursday 20 January 2011	Friday 21 January 2011
8:30 – 10:00	Welcoming Remarks Lecture 1*: What is Evaluation* by Nava Ashraf, Harvard Business School Lecture followed by group picture in front of Jameson Hall	Lecture 3: *Why Randomise* by Marc Shotland, J-PAL	Lecture 5: *Sampling and Sample Size* by Rebecca Thornton, University of Michigan	Lecture 7: *Project from Start to Finish* by Bruno Crépon, ENSAE and École Polytechnique
10:30 – 12:30	Introduction to group members (10min) Group work on case study 1: *Women as Policymakers* Decision on group project (45min)	Group work on case study 3: *Extra Teacher Programme* (60min) Group work on presentation (60min)	Continue Lecture 5 (30min) Group exercise B on sample size estimation (60min) and Group exercise C on mechanisms of randomisation (30min)	Group work to finalise presentations
	Lunch	Lunch	Lunch	Lunch
13:30 – 15:00	Lecture 2: *Measuring Impacts* by Esther Duflo, MIT	Lecture 4: *How to Randomise* by Abhijit Banerjee, MIT	Group work on case study 4: *Deworming in Kenya* (60min) Group work on presentation (30min)	Group presentations (each group: 15 min presentation, 15 min discussion)
15:30 – 17:00	Group work on case study 2: *Learn to Read* (45min) Group work on presentation (45min)	Group exercise A on random sampling (30min) Group work on presentation (60min)	Lecture 6: *Threats and Analysis* by Clara Delavallade, UCT	
		Braai (South African barbecue) at UCT (18:00 – 19:30)	Group work on presentation	

Cape Town – University of Cape Town

Directions to the UCT Upper Campus from the airport
To reach the university from the airport, proceed on the N2 towards Cape Town and take the Muizenberg (M3) off-ramp. Continue until you reach and turn off at the Woolsack Drive / University of Cape Town off ramp. Turn right at the traffic lights on Woolsack Drive and go under the bridge and round the hairpin bend to the northern entrance of the campus (E10 on map on next page).

Directions to the Upper Campus from Cape Town
UCT's Upper Campus (Groote Schuur Campus) is situated on the slopes of Devil's Peak in the suburb of Rondebosch. To reach the upper campus from the city, drive along De Waal Drive or Eastern Boulevards, passing Groote Schuur Hospital on the way. Just past the hospital the road forks. Take the right-hand fork (M3 to Muizenberg). Just beyond Mostert's Mill (windmill) on your left, take the Woolsack Drive / University of Cape Town turn-off. Turn right at the traffic lights on Woolsack Drive abd go under the bridge and round the hairpin bend to the northern entrance of the campus (E10 on map on next page).

# UCT – Upper Campus: Executive Education Course Venue

J-PAL Launch Dinner (Sunday, 16 January), Smuts Hall

The Sunday Launch Dinner will be held at Smuts Hall on the UCT Upper Campus (E 8 on the map). From the UCT North Entrance (see map 1) continue to the visitor parking lot opposite the Visitor Centre (D 12 on the map). Smuts Hall is located directly behind the Visitor Centre. Walk around the building to enter from the side farthest from the Visitor Centre. You can also park directly at parking lot P3 (E 6/7 on map) at the Smuts Hall entrance.

Executive Education Course (Tuesday – Friday, 18-21 January), Leslie Science Building, Auditorium LS 1E

The Executive Education Course (Tuesday – Friday) will take place in Leslie Social Science Building at UCT's upper campus. Walk past the Jameson Hall (the building with the pillars and big stairs) to reach the Leslie Commerce Building (C 3 on the map). The lecture hall is Auditorium LS 1E in the basement of the building. We recommend parking on Ring Road which is at the back of UCT. Go to the entrance where the guard sits (D11 on the map) for access.

UCT Lower and Middle Campus: Conference Venue and Campus Accommodation
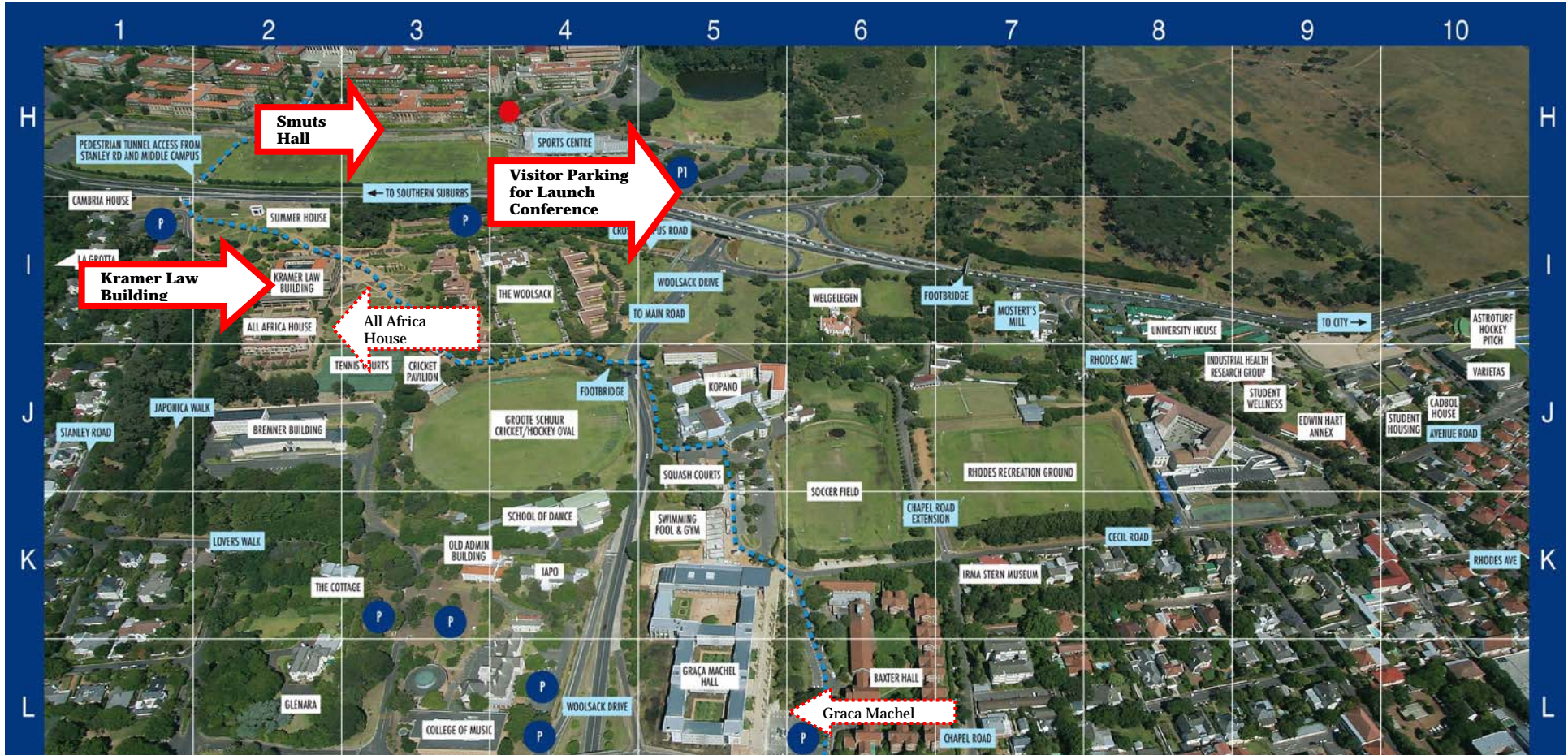
1-Day J-PAL Africa Launch Conference (Monday, 17 January), Kramer Law Building
The Launch Conference on Monday will take place at the Kramer Law Building (I 2 on the map) on UCT's middle campus. We recommend parking at the visitor parking lot opposite the Visitor Centre on upper campus (H5 on the map). The entrance to the parking lot is the road next to the Sports Centre. From visitor parking, make your way down to the Kramer Law Building. (5 minute walk.)

On-Campus Accommodation
The All Africa House is located right next to the Kramer Law Building (I 2 on map). The Graça Machel Hall is located on middle campus (L 5 on map). You can follow the pathway indicated by the blue dotted line to get to Kramer Law Building and upper campus.

Course Objectives

Our executive training programme is designed for people from a variety of backgrounds: managers and researchers from international development organisations, foundations, governments and non-governmental organisations from around the world, as well as trained economists looking to retool.

The course is a **full-time course**. It is important for participants to **attend all lectures and group work** in order to successfully complete the course and receive the certificate of completion.

Course Coverage
The following key questions and concepts will be covered:

- Why and when is a rigorous evaluation of social impact needed?
- The common pitfalls of evaluations, and how randomization can help.
- The key components of a good randomised evaluation design
- Alternative techniques for incorporating randomisation into project design.
- How do you determine the appropriate sample size, measure outcomes, and manage data?
- Guarding against threats that may undermine the integrity of the results.
- Techniques for the analysis and interpretation of results.
- How to maximise policy impact and test external validity.

The programme will achieve these goals through a diverse set of integrated teaching methods. Expert researchers will provide both theoretical and example-based classes complemented by workshops where participants can apply key concepts to real world examples. By examining both successful and problematic evaluations, participants will better understand the significance of various specific details of randomised evaluations. Furthermore, the programme will offer extensive opportunities to apply these ideas ensuring that participants will leave with the knowledge, experience, and confidence necessary to conduct their own randomized evaluations.

J-PAL Lecturers

Nava Ashraf
Associate Professor of Business Administration
Harvard Business School
Nava Ashraf is an Associate Professor at Harvard Business School and a
J-PAL affiliated professor. She has conducted randomized evaluations of
many programs around the world, focused on innovations that can
promote behavior change in microsavings, agriculture and health.
Professor Ashraf has been working in Zambia since 2005 on several
randomized evaluations in health services delivery and health
technology adoption.
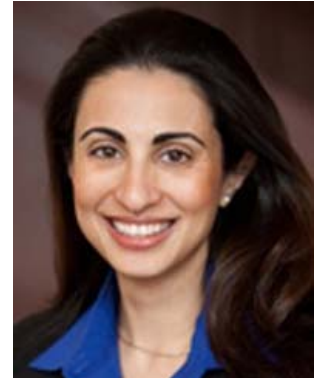
Abhijit Banerjee
Ford Foundation International Professor of Economics
Massachusetts Institute of Technology (MIT)
Abhijit Vinayak Banerjee is a founder and director of J-PAL. Banerjee is a
past president of the Bureau for the Research in the Economic Analysis
of Development, a Research Associate of the NBER, a CEPR research
fellow, International Research Fellow of the Kiel Institute, a fellow of the
American Academy of Arts and Sciences and the Econometric Society
and has been a Guggenheim Fellow and an Alfred P. Sloan Fellow. J-PAL
received the inaugural BBVA Frontiers of Knowledge Award for world-
class research, and Professor Banerjee received the Infosys Prize 2009 in
Social Sciences and Economics. His areas of research are development
economics and economic theory. He has authored two books as well as a
large number of articles and is the editor of a third book. He finished his first
documentary film, "The Name of the Disease" in 2006.

Bruno Crépon
Associate Professor
ENSAE and École Polytechnique
Bruno Crépon is a researcher at Centre de Recherche en Économie et
Statistique (CREST) and an Associate Professor at ENSAE and École
Polytechnique. The focus of his research is on policy evaluation with
special attention to labor market policies.

Clara Delavallade
Senior Lecturer
University of Cape Town
Clara Delavallade is a Senior Lecturer at the University of Cape Town.
She headed J-PAL South Asia between 2008 and 2010. She is involved in randomised evaluations of health, education and agriculture programs in India and Mauritania.

Esther Duflo
Abdul Latif Jameel Professor of Poverty Alleviation and Development Economics
Massachusetts Institute of Technology (MIT)
Esther Duflo is a founder and director of the Abdul Latif Jameel Poverty Action Lab (J-PAL). Duflo is an NBER Research Associate, serves on the board of the Bureau for Research and Economic Analysis of Development (BREAD), and is Director of the Center of Economic Policy Research's development economics program. Her research focuses on microeconomic issues in developing countries, including household behavior, education, access to finance, health and policy evaluation. Duflo has received numerous academic honors and prizes including the John Bates Clark Medal (2010), a MacArthur Fellowship (2009), the American Economic Association's Elaine Bennett Prize for Research (2003), the "Best French Young Economist Prize" (Le Monde/Cercle des economistes, 2005), the Médaille de Bronze (Centre National de la Recherche Scientifique, 2005), and the Prix Luc Durand-Reville (Académie des Sciences Morales et Politiques, 2008). In 2008-2009 she was the inaugural holder of the international chair "Knowledge Against Poverty" at the College de France.

Marc Shotland
Senior Research Manager
J-PAL Global
Marc Shotland holds a Masters in Public Administration in International Development (MPA/ID) degree from Harvard University's Kennedy School of Government and a Bachelors degree in Economics from Williams College. He first joined Professors Duflo and Banerjee in the summer of 2002 to run randomized evaluations of education interventions as a field research associate in India. In 2004 he joined the Poverty Action Lab's Cambridge office as a research manager. He left in 2006 to earn his Masters at Harvard before rejoining J-PAL in 2008 in his current position.

Rebecca Thornton
Assistant Professor of Economics
University of Michigan
Rebecca Thornton began her appointment as an assistant Professor at the University of Michigan economics department in 2008. Her research focuses on education and health as well as how individuals respond to financial incentives in these areas. She has worked on a randomized evaluation of a merit-based scholarship in Kenya. She is also working on randomized evaluations examining HIV testing and prevention and menstruation and education in Nepal.

List of Participants

	Name	First Name	Organization	Country
1	Asaoka	Hiroaki	Japan International Cooperation Agency (JICA)	Japan
2	Bailey	Angela	International Rescue Committee (IRC)	Uganda
3	Batonon	Isatou	International Rescue Committee (IRC)	Kenya
4	Bayoumi	Dalia	The Social Contract Center	Egypt
5	Chikezie	Chukwu-emeka	Microfinance Investment & Technical Assistance Facility	Sierra Leone
6	Christensen	Colin	Innovations for Poverty Action (IPA) Kenya	Kenya
7	Cole	Gabrielle	International Rescue Committee (IRC)	Congo
8	Diga	Kathleen	School of Development Studies, University of KwaZulu-Natal	South Africa
9	Dladla	Timothy	Department of Trade and Industry, South Africa	South Africa
10	Durieux	Monet	National Treasury SA	South Africa
11	Gray-Johnson	Wilfred	Liberian Peacebuilding Office / Ministry of Internal Affairs	Liberia
12	Guerrero Ruiz	Alejandro	World Bank	United States
19	Hamainza	Busiku	Government of Zambia	Zambia
13	Hazell	Eleanor	MIET Africa	South Africa
14	Herholdt	Roelien	JET Education	South Africa
15	Kakande	Nelson	Clinical Operational & Health Services Research (COHRE)	Uganda
16	Kiwala	Yusuf	National Agricultual Advisory Services (NAADS)	Uganda
17	Koppenhaver	Todd	USAID Southern Africa Regional HIV/AIDS Program	South Africa
18	Kuganab-Lem	Robert	University for Development Studies, Tamale	Ghana
20	Mabirizi	Daniel	National Agricultual Advisory Services (NAADS)	Uganda
21	Marera	Double-Hugh	JET Education	South Africa
22	Msichili	Grace	SFH Zambia	Zambia
23	Muhammad	Enas Ali	Information and Decision Support Center – Cabinet	Egypt
24	Mulenda	Lazarus	Government of Zambia	Zambia
25	Nansamba	Aisha	BRAC Uganda	Uganda
26	Nayyar-Stone	Ritu	The Urban Institute	Egypt
27	Ogo	Isaac	Planned Parenthood Federation of Nigeria	Nigeria
28	Olwenyi	Martha	Technoserve	Uganda
29	Owusu	Victor	Kwame Nkrumah University of Science and Technology	Ghana
30	Pitman	Robert	The Carter Center	United States
31	Roberts	Gareth	Wits University, AMERU	South Africa
32	Sellu	Andrew	International Rescue Committee (IRC)	Sierra Leone
33	Shejavali	Kandi	Millennium Challenge Account Namibia (MCA-N)	Namibia
34	Shimeles	Abebe	African Development Bank	Tuneria
35	Toki	Damilola	Planned Parenthood Federation of Nigeria	Nigeria
36	Torres da Costa	Ricardo	The World Bank	Mozambique

Transport from Hotels

The shuttle bus will pick up course participants in the morning from Vineyard hotel and Southern Sun Hotel to take you to the UCT venues. A return bus will leave the venue immediately after the program ends. There will only be <u>one trip each way</u>. Please try to be ready 5 min in advance of the departure time.

Timetable for shuttle:

Sunday, 16 January, Dinner: depart from Southern Sun at 18:00 and Vineyard at 18:15; return after dinner.

Mo., 17 January, Conference: depart from Southern Sun 7:45 and Vineyard at 8:00; return after conference.

Tu.-Fr., 18-21 January, Course: depart from Vineyard 7:45 and Southern Sun at 8:00; return after course.

Taxis:

If you miss the shuttle, you can take a taxi. Standard rates are R 10 per km. If you need a receipt, please tell the cab driver before getting into the car. Local cab companies include:

Cabs on Call: 021 5226 103
Cab Xpress: 021 448 1616
Excite Meter Cab: 021 418 4444

Case 1: Women as Policymakers
Measuring the effects of political reservations
Thinking about measurement and outcomes

Massachusetts
Institute of
Technology

India amended its federal constitution in 1992, devolving power to plan and implement development programs from the states to rural councils, or Gram Panchayats (Village Councils). The Village Councils now choose what development programs to undertake and how much of the budget to invest in them. The states are also required to reserve a third of Village Council seats and Village Council chairperson positions for women. In most states, the schedule on which reserved seats and positions cycle among the Village Councils is determined randomly. This creates the opportunity to rigorously assess the impact of quotas on politics and government: Do the policies differ when there are more women in government? Do the policies chosen by women in power reflect the policy priorities of women? Since randomization was part of the Indian government program itself, the evaluation planning centered on collecting the data needed to measure impact. The researchers' questions were what data to collect and what data collection instruments to use.

Empowering the Panchayati Raj

Village Councils, known locally as Panchayats, have a long tradition in rural India. An assembly *(yat)* of five *(panch)* elders, chosen by the community, convened to mediate disputes between people or villages. In modern times Village Councils have been formalized into institutions of local self-government.

This formalization came about through the constitution. In 1992, India enacted the 73rd amendment, which directed the states to establish a three-tier Panchayati Raj system, with Village Councils as the grassroot unit[1] elected every five years. Councilors are popularly elected to represent each ward. The councilors elect from among themselves a council chairperson called a Pradhan. Decisions are made by a majority vote and the chairperson has no veto power. But as the only councilor with a full-time appointment, the chairperson wields effective power.

The 73rd amendment aimed to decentralize the delivery of public goods and services essential for development in rural areas. The states were directed to delegate the power to plan and implement local development programs to the Village Councils. Funds still come from the central government but are no longer earmarked for specific uses. Instead, the Village Council decides which programs to implement and how much to invest in them. Village Councils can chose programs from 29 specified areas, including welfare services (for example, public assistance for widows, care for the elderly, maternity care, antenatal care, and child health) and public works (for example, drinking water, roads, housing, community buildings, electricity, irrigation, and education).

Empowering women in the Panchayati Raj

The Village Councils are large and diverse. In West Bengal, for example, each has up to 12 villages and up to 10,000 people, who can vary by religion, ethnicity, caste, and, of course, gender. Political voice varies by group identities drawn along these lines. If policy preferences vary by group identity and if the policymakers' identities influence policy choices, then groups underrepresented in politics and government could be shut out as Village Councils could ignore those groups' policy priorities. There were

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[1] Village councils, called Gram Panchayats, form the basic units of the Panchayat Raj. Village council chairs, elected by the members of the village council, serve as members of the block—subdistrict—council (*panchayat samiti*). At the top of the system is the district council (*zilla parishad*) made up of the block council chairs.

fears that the newly empowered Village Councils would undermine the development priorities of traditionally marginalized groups, such as women. To remedy this, the 73rd amendment included two mandates to ensure that investments reflected the needs of everyone in the Village Council.

The first mandate secures community input. If Village Council investments are to reflect a community's priorities, the councilors must first know what those priorities are. Accordingly, Village Councils are required to hold a general assembly every six months or every year to report on activities in the preceding period and to submit the proposed budget to the community for ratification. In addition, the Chairpersons are required to set up regular office hours to allow constituents to formally request services and lodge complaints. Both requirements allow constituents to articulate their policy preferences.

The second mandate secures representation in the council for women. States are required to reserve at least a third of all council seats and Chairperson positions for women. Furthermore, the states have to ensure that the seats reserved for women are "allotted by rotation to different constituencies in a Panchayat [Village Council]" and that the chairperson positions reserved for women are "allotted by rotation to different Panchayats [Village Councils]." In other words, they have to ensure that reserved seats and chairperson positions rotate evenly within and among the Village Councils.

Randomized quotas in India: What can it teach us?

Your evaluation team has been entrusted with the responsibility to estimate the impact of quotas for women in the Village Councils. Your evaluation should address all dimensions in which quotas for women are changing local communities in India. What could these dimensions be? What data will you collect? What instruments will you use?

As a first step you want to understand all you can about the quota policy. What needs did it address? What are the pros and cons of the policy? What can we learn from it?

Discussion Topic 1: Gender quotas in the Village Councils

1.	What were the main goals of the Village Councils?
2.	Women are underrepresented in politics and government. Only 10 percent of India's national assembly members are women, compared to 17 percent worldwide. Does it matter that women are underrepresented? Why and why not?
3.	What were the framers of the 73rd amendment trying to achieve when they introduced quotas for women?

Gender quotas have usually been followed by dramatic increases in the political representation of women. Rwanda, for example, jumped from 24th place in the "women in parliament" rankings to first place (49 percent) after the introduction of quotas in 1996. Similar changes have been seen in Argentina, Burundi, Costa Rica, Iraq, Mozambique, and South Africa. Indeed, 17 of the top 20 countries in the rankings have quotas.

Imagine that your group is the national parliament of a country deciding whether to adopt quotas for women in the national parliament. Randomly divide your group into two parties, one against and one for quotas.

4.	Debate the pros and cons of quotas. At the end of the debate, you should have a list of the pros and cons of quotas.

What data to collect

First, you need to be very clear about the likely impact of the program. It is on those dimensions that you believe will be affected that you will try to collect data. What are the main areas in which the quota policy should be evaluated? In which areas do you expect to see a difference as a result of quotas?

What are all the possible effects of quotas?

Discussion Topic 2: Using a logical framework to delineate your intermediate and final outcomes of interest

1.	Brainstorm the possible effects of quotas: positive, negative, and no effects. Hint: Use your answers to Discussion Topic 1 as a starting point.
2.	What evidence would you collect to strengthen the case of those who are for or against quotas? For each potential effect on your list, list also the indicator(s) you would use for that effect. For example, if you say that quotas will affect political participation of women, the indicator could be "number of women attending the General Assembly."

Multiple outcomes are difficult to interpret, so define a hypothesis

Quotas for women could produce a large number of outcomes in different directions. For example, it may improve the supply of drinking water and worsen the supply of irrigation. Without an *ex-ante* hypothesis on the direction in which these different variables should be affected by the quota policy, it will be very difficult to make sense of any result we find. Think of the following: if you take 500 villages and randomly assign them in your computer to a "treatment" group and a "control" group, and then run regressions to see whether the villages look different along 100 outcomes, would you expect to see some differences among them? Would it make sense to rationalize those results *ex-post?*

The same applies to this case: if you just present your report in front of the commission who mandated you to evaluate this policy, explaining that the quota for women changed some variables and did not change others, what are they supposed to make of it? How will they know that these differences are not due to pure chance rather than the policy? You need to present them with a clear hypothesis of how quotas are supposed to change policymaking, which will lead you to make predictions about which outcomes are affected.

Discussion Topic 2 continued…:

3.	What might be some examples of key hypotheses you would test? Pick one.
4.	Which indicators or combinations of indicators would you use to test your key hypothesis?

Use a logical framework to delineate intermediate and final outcomes
A good way of figuring out the important outcomes is to lay out your theory of change; that is, to draw a logical framework linking the intervention, step by step, to the key final outcomes.

Discussion Topic 2 continued…:

5.	What is the possible chain of outcomes in the case of quotas?
6.	What are the main critical steps needed to obtain the final results? What are the conditions needed to be met at each step?
7.	What variables should you try to obtain at every step in your logical framework?
8.	Using the outcomes and conditions, draw a possible logical framework, linking the intervention and the final outcomes.

ABDUL LATIF JAMEEL
Poverty Action Lab

Case 2: Learn to Read Evaluations
Evaluating the Read India Campaign
How to Read and Evaluate Evaluations

Massachusetts
Institute of
Technology

Why Learn to Read (L2R)?

In a large-scale survey conducted in 2004, Pratham discovered that only 39% of children (aged 7-14) in rural Uttar Pradesh could read and understand a simple story, and nearly 15% could not recognize even a letter.

During this period, Pratham was developing the "Learn-to-Read" (L2R) module of its Read India campaign. L2R included a unique pedagogy teaching basic literacy skills, combined with a grassroots organizing effort to recruit volunteers willing to teach.

This program allowed the community to get involved in children's education more directly through village meetings where Pratham staff shared information on the status of literacy in the village and the rights of children to education. In these meetings, Pratham identified community members who were willing to teach. Volunteers attended a training session on the pedagogy, after which they could hold after-school reading classes for children, using materials designed and provided by Pratham. Pratham staff paid occasional visits to these camps to ensure that the classes were being held and to provide additional training as necessary.

Did the Learn to Read project work?

Did Pratham's "Learn to Read" program work? What is required in order for us to measure whether a program worked, or whether it had impact?

In general, to ask if a program works is to ask if the program achieves its goal of changing certain outcomes for its participants, and ensure that those changes are not caused by some other factors or events happening at the same time. To show that the program *causes* the observed changes, we need to simultaneously show that if the program had not been implemented, the observed changes would not have occurred. But how do we know *what would have happened*? If the program happened, it happened. Measuring *what would have happened* requires entering an imaginary world in which the program *was never given to these participants*. The outcomes of the same participants in this imaginary world are referred to as the *counterfactual*. Since we cannot observe the true counterfactual, the best we can do is to estimate it by mimicking it.

The key challenge of program impact evaluation is constructing or mimicking the counterfactual. We typically do this by selecting a group of people that resemble the participants as much as possible but who did not participate in the program. This group is called the comparison group. Because we want to be able to say that it was the program and not some other factor that caused the changes in outcomes, it is important that the only difference between the comparison group and the participants is that the comparison group did not participate in the program. We then estimate "impact" as the difference observed at the end of the program between the outcomes of the comparison group and the outcomes of the program participants.

The impact estimate is only as accurate as the comparison group is successful at mimicking the counterfactual. If the comparison group poorly represents the counterfactual, the impact is (in most circumstances) poorly estimated. Therefore the method used to select the comparison group is a key decision in the design of any impact evaluation.

That brings us back to our questions: Did the Learn to Read project work? What was its impact on children's reading levels?

In this case, the intention of the program is to "improve children's reading levels" and the reading level is the outcome measure. So, when we ask if the Learn to Read project worked, we are asking if it improved children's reading levels. The impact is the difference between reading levels after the children have taken the reading classes and what their reading level would have been if the reading classes had never existed.
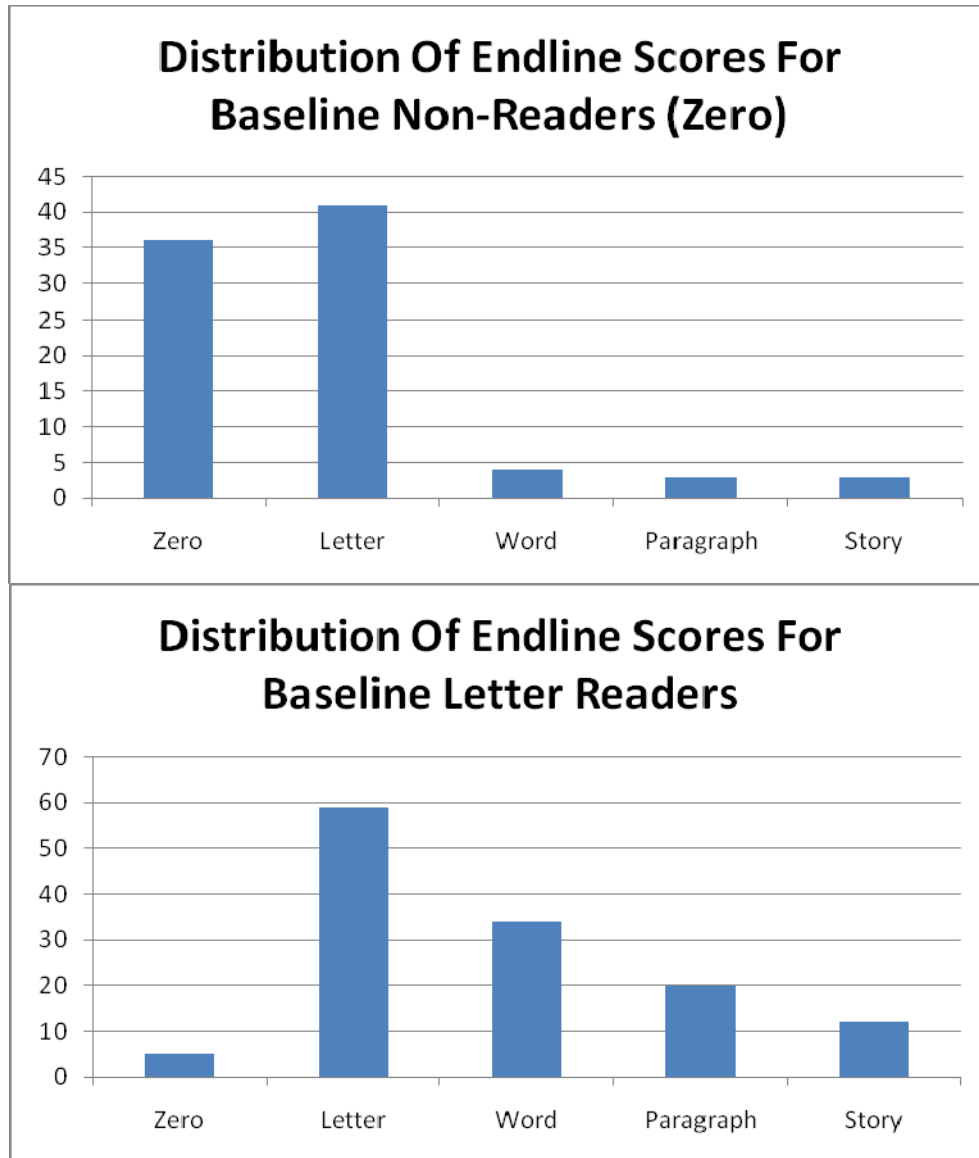
What comparison groups can we use? The following experts illustrate different methods of evaluating impact.

Estimating the impact of the Learn to Read project

Method 1:

News Release: Read India helps children Learn to Read.
Pratham celebrates the success of its "Learn to Read" program—part of the Read India Initiative. It has made significant progress in its goal of improving children's literacy rates through better learning materials, pedagogical methods, and most importantly, committed volunteers. The achievement of the "Learn to Read" (L2R) program demonstrates that a revised curriculum, galvanized by community mobilization, can produce significant gains. Massive government expenditures in mid-day meals and school construction have failed to achieve similar results. In less than a year, the reading levels of children who enrolled in the L2R camps improved considerably.

Distribution Of Endline Scores For Baseline Non-Readers (Zero)

A bar chart showing values for Zero ≈ 36, Letter ≈ 41, Word ≈ 4, Paragraph ≈ 3, Story ≈ 3. Y-axis from 0 to 45.

Distribution Of Endline Scores For Baseline Letter Readers

A bar chart showing values for Zero ≈ 5, Letter ≈ 59, Word ≈ 34, Paragraph ≈ 20, Story ≈ 12. Y-axis from 0 to 70.

Just before the program started, half these children could not recognize Hindi words—many nothing at all. But after spending just a few months in Pratham reading classes, more than half improved by at least one reading level, with a significant number capable of recognizing words and several able to read full paragraphs and stories! *On average, the literacy measure of these students improved by nearly one full reading level during this period.*

Discussion Topic 1:

1. What type of evaluation does this news release imply?
2. What represents the counterfactual in this example?
3. What might be the problems with this type of evaluation (use concrete examples)?

Method 2:

Opinion: The "Read India" project not up to the mark

Pratham has raised millions of dollars, expanding rapidly to cover all of India with its so-called "Learn-to-Read" program, but do its students actually learn to read? Recent evidence suggests otherwise. A team of evaluators from Education for All found that children who took the reading classes ended up with literacy levels significantly below those of their village counterparts. After one year of Pratham reading classes, Pratham students could only recognize words whereas those who steered clear of Pratham programs were able to read full paragraphs.

Comparison of reading levels of children who took reading classes Vs. reading levels of children who did not take them
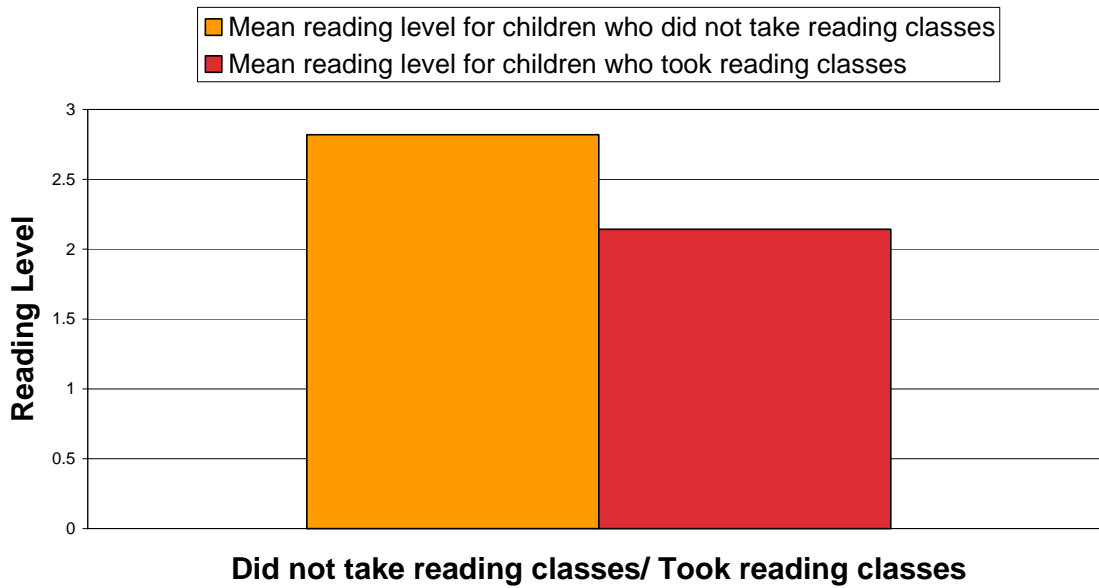
Notes: Reading Level is an indicator variable that takes value 0 if the child can read nothing, 1 if he knows the alphabet, 2 if he can recognize words, 3 if he can read a paragraph and 4 if he can read a full story.

If you have a dime to spare, and want to contribute to the education of India's illiterate children, you may think twice before throwing it into the fountain of Pratham's promises.

Discussion Topic 2:

1. What type of evaluation is this opinion piece using?
2. What represents the counterfactual?
3. What might be the problem with this type of evaluation (use concrete examples)?

Method 3:

Letter to the Editor: EFA should consider Evaluating Fairly and Accurately
There have been several unfair reports in the press concerning programs implemented by the NGO Pratham. A recent article by a former Education for All bureaucrat claims that Pratham is actually hurting the children it recruits into its 'Learn-to-Read' camps. However, the EFA analysis uses the wrong metric to measure impact. It compares the reading *levels* of Pratham students with other children in the village—not taking into account the fact that Pratham targets those whose literacy levels are particularly poor at the beginning. If Pratham simply recruited the most literate children into their programs, and compared them to their poorer counterparts, they could claim success without conducting a single class. But Pratham does not do this. And realistically, Pratham does not expect its illiterate children to overtake the stronger students in the village. It simply tries to initiate improvement over the current state. Therefore the metric should be *improvement* in reading levels—not the final level. When we repeated EFA's analysis using the more-appropriate outcome measure, the Pratham kids improved at twice the rate of the non-Pratham kids (0.6 reading level increase compared to 0.3). This difference is statistically very significant.

Had the EFA evaluators thought to look at the more appropriate outcome, they would recognize the incredible success of Read India. Perhaps they should enroll in some Pratham classes themselves.

Discussion Topic 3:

1. What type of evaluation is this letter using?
2. What represents the counterfactual?
3. What might be the problem with this type of evaluation (use concrete examples)?

Method 4:

The numbers don't lie, unless your statisticians are asleep
Pratham celebrates victory, opponents cry foul. A closer look shows that, as usual, the truth is somewhere in between.

There has been a war in the press between Pratham's supporters and detractors. Pratham and its advocates assert that the Read India campaign has resulted in large increases in child literacy. Several detractors claim that Pratham programs, by pulling attention away from the schools, are in fact causing significant harm to the students. Unfortunately, this battle is being waged using instruments of analysis that are seriously flawed. The ultimate victim is the public who is looking for an answer to the question: is Pratham helping its intended beneficiaries?

This report uses sophisticated statistical methods to measure the true impact of Pratham programs. We were concerned about other variables confounding previous results. We therefore conducted a survey in these villages collecting information on child age, grade-level, and parents' education level and used those to predict child test scores.

Table 1: Reading outcomes

	Level		Improvement	
	(1)	**(2)**	**(3)**	**(4)**
Reading Classes	-0.68 **	0.04	0.24 **	0.11
	(0.0829)	(0.1031)	(0.0628)	(0.1081)
Previous reading level		0.71 **		
		(0.0215)		
Age		0.00		-0.01
		(0.0182)		(0.0194)
Sex		-0.01		0.05
		(0.0469)		(0.0514)
Standard		0.02		-0.08 **
		(0.0174)		(0.0171)
Parents Literate		0.04		0.13 **
		(0.0457)		(0.0506)
Constant	2.82	0.36	0.37	0.75
	(0.0239)	(0.2648)	(0.0157)	(0.3293)
School-type controls	No	Yes	No	0.37

Notes: The omitted category for school type is "Did not go to school". Reading Level is an indicator variable that takes value 0 if the child can read nothing, 1 if he knows the alphabet, 2 if he can recognize words, 3 if he can read a paragraph and 4 if he can read a full story

Looking at Table 1, we find some positive results, some negative results and some "no-results", depending on which variables we control for. The results from column (1) suggest that Pratham's program hurt the children. There is a negative correlation between receiving Pratham classes and final reading outcomes (-0.68).   Column (3), which evaluates improvement, suggests impressive results (0.24). But looking at child outcomes (either level or improvement) *controlling for* initial reading levels, age, gender, standard and parent's education level – all determinants of child reading levels – we found no impact of Pratham programs.

Therefore, controlling for the right variables, we have discovered that on one hand, Pratham has not caused the harm claimed by certain opponents, but on the other hand, it has not helped children learn. Pratham has therefore failed in its effort to convince us that it can spend donor money effectively.

Discussion Topic 4:

1. What type of evaluation is this report using?
2. What represents the counterfactual?
3. What might be the problem with this type of evaluation (use concrete examples)?

TRANSLATING RESEARCH INTO ACTION

Case 3: Extra Teacher Program
Designing an evaluation to answer
three key education policy questions

Massachusetts Institute of Technology

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Confronted with overcrowded schools and a shortage of teachers, in 2005 the NGO International Child Support Africa (ICS) offered to help the school system of Western Kenya by introducing contract teachers in 140 primary schools. Under its two year program, ICS provided funds to these schools to hire one extra teacher each year. In contrast to the civil servants hired by the Ministry of Education, contract teachers are hired locally by school committees. ICS expected this program to improve student learning by, among other things, decreasing class size and using teachers who are more directly accountable to the communities they serve. However, contract teachers tend to have less training and receive a lower monthly salary than their civil servant counterparts. So there was concern about whether these teachers were sufficiently motivated, given their compensation, or qualified given their credentials.

What experimental designs could test the impact of this intervention on educational achievement? Which of these changes in the school landscape is primarily responsible for improved student performance?

Over-crowded Schools

Like many other developing countries, Kenya has recently made rapid progress toward the Millennium Development Goal of universal primary education. Largely due to the elimination of school fees in 2003, primary school enrollment rose nearly 30 percent, from 5.9 million to 7.6 million between 2002 and 2005.

Without accompanying government funding, however, this progress has created its own set of new challenges in Kenya:

1) **Large class size:** Due to budget constraints, the rise in primary school enrollment has not been matched by proportionate increases in the number of teachers. (Teacher salaries already account for the largest component of educational spending.) The result has been very large class sizes, particularly in lower grades. In a sample of schools in Western Kenya, for example, the average first grade class in 2005 was 83 students. This is concerning because it is believed that small classes are most important for the youngest students, who are still acclimating to the school environment. The Kenyan National Union of Teachers estimates that the country needs an additional 60,000 primary school teachers in addition to the existing 175,000 in order to reach all primary students and decrease class sizes.

2) **Teacher absenteeism:** Further exacerbating the problem of pupil-teacher ratios, teacher absenteeism remains high, reaching nearly 20% in some areas of Kenya.

   There are typically no substitutes for absent teachers, so students simply mill around, go home or join another class, often of a different grade. Small schools, which are prevalent in rural areas of developing countries, may be closed entirely as a result of teacher absence. Families have to consider whether school will even be open when deciding whether or not to send their children to school. An obvious result is low student attendance—even on days when the school is open.

3) **Heterogeneous classes:** Classes in Kenya are also very heterogeneous with students varying widely in terms of school preparedness and support from home.

   Grouping students into classes sorted by ability (*tracking*, or *streaming*) is controversial among academics and policymakers. On one hand, if teachers find it easier to teach a homogeneous group of students, tracking could improve school effectiveness and test scores. Many argue, on the other hand, that if students learn in part from their peers, tracking could disadvantage low achieving students while benefiting high achieving students, thereby exacerbating inequality.

4) **Scarce school materials:** Because of the high costs of educational inputs and the rising number of students, educational resources other than the teacher are stretched, and in some cases up to four students must share one textbook. And an already over-burdened infrastructure deteriorates faster when forced to serve more children.

5) **Low completion rates:** As a result of these factors, completion rates are very low in Kenya with only 45.1% of boys and 43.3% of girls completing the first grade.

All in all, these issues pose new challenges to communities: how to ensure minimum quality of education given Kenya's budget constraints.

What are Contract Teachers?

Governments in several developing countries have responded to similar challenges by staffing unfilled teaching positions with locally-hired contract teachers who are not civil service employees. The four main characteristics of contract teachers are that they are: (1) appointed on annual renewable contracts, with no guarantee of renewed employment (unlike regular civil service teachers); (2) often less qualified than regular teachers and much less likely to have a formal teacher training certificate or degree; (3) paid lower salaries than those of regular teachers (typically less than a fifth of the salaries paid to regular teachers); and (4) more likely to be from the local area where the school is located.

Are Contract Teachers Effective?

The increasing use of contract teachers has been one of the most significant policy innovations in providing primary education in developing countries, but it has also been highly controversial. Supporters say that using contract teachers is an efficient way of expanding education access and quality to a large number of first-generation learners. Knowing that the school committee's decision of whether or not to rehire them the following year may hinge on performance, contract teachers are motivated to try harder than their tenured government counterparts. Contract teachers are also often more similar to their students in terms of geographic and cultural roots as well as socio-economic status. Opponents argue that using under-qualified and untrained teachers may staff classrooms, but will not produce learning outcomes. Furthermore the use of contract teachers de-professionalizes teaching, reduces the prestige of the entire profession, and

reduces motivation of all teachers. Even if it helps in the short term, it may hurt efforts to recruit highly qualified teachers in the future.

While the use of contract teachers has generated much controversy, there is very little rigorous evidence regarding the effectiveness of contract teachers in improving student learning outcomes.

The Extra Teacher Program Randomized Evaluation
In January 2005, International Child Support Africa initiated a two year program to examine the effect of contract teachers on education in Kenya. Under the program, ICS gave funds to 140 local school committees to hire one extra contract teacher to teach an additional first grade class. The purpose of this intervention was to address the first three challenges: class size, teacher accountability, and heterogeneity of ability. The evaluation was designed to measure the impact of class-size reductions, the relative effectiveness of contract teachers, and how tracking by ability would impact both low and high-achieving students.

Addressing Multiple Research Questions through Experimental Design
Different randomization strategies may be used to answer different questions. What randomization strategy could be used to evaluate the following questions? Concentrate on the appropriate unit (level) of randomization for each.

Discussion Topic 1: Testing the effectiveness of contract teachers

1. What is the relative effectiveness of contract teachers versus regular government teachers?

Discussion Topic 2: Looking at more general approaches of improving education

2. What is the effect of smaller class sizes on student performance?
3. What is the impact of grouping students by ability on student performance?

Discussion Topic 3: Addressing all questions with a single evaluation

4. Could a single evaluation explore all these issues at once?
5. What randomization strategy could do so?

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TRANSLATING RESEARCH INTO ACTION

Case 4: Deworming in Kenya
Managing threats to experimental integrity

Massachusetts Institute of Technology

Between 1998 and 2001, the NGO International Child Support Africa implemented a school-based mass deworming program in 75 primary schools in western Kenya. The program treated the 30,000 pupils enrolled at these schools for worms—hookworm, roundworm, whipworm, and schistosomiasis. Schools were phased-in randomly.

Randomization ensures that the treatment and comparison groups are comparable at the beginning, but it cannot ensure that they remain comparable at the end of the program. Nor can it ensure that people comply with the treatment they were assigned. Life also goes on after the randomization: other events besides the program happen between randomization and the end-line. These events can reintroduce selection bias; they diminish the validity of the impact estimates and are threats to the integrity of the experiment.

How can common threats to experimental integrity be managed?

Worms—a common problem with a cheap solution

Worm infections account for over 40 percent of the global tropical disease burden. Infections are common in areas with poor sanitation. More than 2 billion people are affected. Children, still learning good sanitary habits, are particularly vulnerable: 400 million school-age children are chronically infected with intestinal worms.

Worms affect more than the health of children. Symptoms include listlessness, diarrhea, abdominal pain, and anemia. Beyond their effects on health and nutrition, heavy worm infections can impair children's physical and mental development and reduce their attendance and performance in school.

Poor sanitation and personal hygiene habits facilitate transmission. Infected people excrete worm eggs in their feces and urine. In areas with poor sanitation, the eggs contaminate the soil or water. Other people are infected when they ingest contaminated food or soil (hookworm, whipworm, and roundworm), or when hatched worm larvae penetrate their skin upon contact with contaminated soil (hookworm) or fresh water (schistosome). School-age children are more likely to spread worms because they have riskier hygiene practices (more likely to swim in contaminated water, more likely to not use the latrine, less likely to wash hands before eating). So treating a child not only reduces her own worm load; it may also reduce disease transmission—and so benefit the community at large.

Treatment kills worms in the body, but does not prevent re-infection. Oral medication that can kill 99 percent of worms in the body is available: albendazole or mebendazole for treating hookworm, roundworm, and whipworm infections; and praziquantel for treating schistosomiasis. These drugs are cheap and safe. A dose of albendazole or mebendazole costs less than 3 US cents while one dose of praziquantel costs less than 20 US cents. The drugs have very few and minor side effects.

Worms colonize the intestines and the urinary tract, but they do not reproduce in the body; their numbers build up only through repeated contact with contaminated soil or water. The WHO recommends presumptive school-based mass deworming in areas with high prevalence. Schools with hookworm, whipworm, and roundworm prevalence over 50 percent should be mass treated with albendazole every 6 months, and schools with schistosomiasis prevalence over 30 percent should be mass treated with praziquantel once a year.

Primary School Deworming Program

International Child Support Africa (ICS) implemented the Primary School Deworming Program (PSDP) in the Busia District in western Kenya, a densely-settled region with high worm prevalence. Treatment followed WHO guidelines. The medicine was administered by public health nurses from the Ministry of Health in the presence of health officers from ICS.

The PSDP was expected to affect health, nutrition, and education. To measure impact, ICS collected data on a series of outcomes: prevalence of worm infection, worm loads (severity of worm infection); self-reported illness; and school participation rates and test scores.

Evaluation design — the experiment as planned

Because of administrative and financial constraints the PSDP could not be implemented in all schools immediately. Instead, the 75 schools were randomly divided into 3 groups of 25 schools and phased-in over 3 years. Group 1 schools were treated starting in both 1998 and 1999, Group 2 schools in 1999, and Group 3 starting in 2001. Group 1 schools were the treatment group in 1998, while schools Group 2 and Group 3 were the comparison. In 1999 Group 1 and Group 2 schools were the treatment and Group 3 schools the comparison.

Figure 1:   The planned experiment: the PSDP treatment timeline showing experimental groups in 1998 and 1999

	1998	1999	2001
Group 1	Treatment	Treatment	Treatment
Group 2	Comparison	Treatment	Treatment
Group 3	Comparison	Comparison	Treatment

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Managing attrition—when the groups do not remain equivalent

Attrition is when people join or drop out of the sample—both treatment and comparison groups—over the course of the experiment. One common example in clinical trials is when people die; so common indeed that attrition is sometimes called experimental mortality.

Discussion Topic 1: Managing Attrition

You are looking at the health effects of deworming. In particular you are looking at the worm load (severity of worm infection). Worm loads are scaled as follows: Heavy worm infections get a worm load score of 3, medium worm infections a score of 2, and light infections a score of 1.

The program is school-based, so it is natural and cost-effective to collect data at the schools—the children are gathered in one place, so the enumerator does not have to travel to every child's home. The enumerator takes the measurements on all children in school on a randomly chosen day (the school authorities are not given prior warning).

There are 30,000 children: 15,000 in treatment schools and 15,000 in comparison schools. After you randomize, the groups are equivalent, children from each of the three categories are equally represented.

Protocol compliance is 100 percent: all children who are in the treatment get treated and none of the children in the comparison are treated. Deworming at the beginning of the school year results in a worm load of 1 at the end of the year because of re-infection. Children who have a worm load of 3 only attend half the time and drop out of school if they are not treated. The number of children in each worm-load category is shown for both the pretest and posttest.

Worm Load	Pretest		Posttest	
	Treatment	Comparison	Treatment	Comparison
3	5,000	5,000	0	Dropped out
2	5,000	5,000	0	5,000
1	5,000	5,000	15,000	5,000
Total children tested at school	15,000	15,000	15,000	10,000

1.	a. What is the average pretest worm load for the treatment group? b. What is the average pretest worm load for the comparison group? c. Are the groups equivalent?
2.	a. What is the average posttest worm load for the treatment group? b. What is the average posttest worm load for the comparison group? c. What is the difference?
3.	a. Calculate the outcome differences at the beginning and at the end of the year. b. Is this outcome difference an accurate estimate of the impact of the program? c. If it is not accurate, does it overestimate or underestimate the impact?
4.	Because the treatment was treated, you expected there to be a difference between the groups at the end of the year. a. If this difference is an effect, what is the source of attrition bias, if any? b. How can you solve the problem to get a better estimate of program impact?
5.	a. What is the average posttest worm load for the comparison group if you also tested the 5,000 dropouts (assuming all would have had worm loads of 3)? b. Calculate the impact of the program. c. What is the size of the attrition bias?
6.	The PSPD also looked at school attendance rates and test scores. a. Would differential attrition bias either of these outcomes? b. Would the impact be underestimated or overestimated?
7.	In Case 1, you learned about other methods to estimate program impact, such as pre-post, simple difference, differences in differences, and multivariate regression. a. Discuss if and how the issues explored above exist for each of these methods. b. Are the threats to experimental integrity unique to randomization?

Managing partial compliance—when the treatment does not actually get treated or the comparison gets treated

Some people assigned to the treatment may in the end not actually get treated. In an after-school tutoring program, for example, some children assigned to receive tutoring may simply not show up for tutoring. And the others assigned to the comparison may obtain access to the treatment, either from the program or from another provider. Or comparison-group children may get extra help from the teachers or acquire program materials and methods from their classmates. Either way, these people are not complying with their assignment in the planned experiment. This is called "partial compliance" or "diffusion" or, less benignly, "contamination." In contrast to carefully-controlled lab experiments, diffusion is ubiquitous in social programs. After all, life goes on, people will be people, and you have no control over what they decide to do over the course of the experiment. All you can do is plan your experiment and offer them treatments. How then can you manage threats arising from partial compliance?

Discussion Topic 2: Managing partial compliance

All the children from the poorest families don't have shoes and so they have worm loads of 3. Though their parents had not paid the school fees, the children were allowed to stay on in school during the year. Parental consent was required for treatment, and to give consent, the parents had to come to the school and sign a consent form in the headmaster's office. Because they had not paid school fees, the poorest parents were reluctant to come to the school. So none of the children with worm loads of 3 were actually treated. Their worm loads scores remained 3 at the end of the year. No one assigned to comparison was treated. All the children in the sample at the beginning of the year were followed up, if not at school then at home.

	Pretest		Posttest	
Worm Load	Treatment	Comparison	Treatment	Comparison
3	5,000	5,000	5,000	5,000
2	5,000	5,000	0	5,000
1	5,000	5,000	10,000	5,000
Total children tested	15,000	15,000	15,000	15,000

1. **a.** Calculate the impact estimate based on the original assignments.
   b. What does this "intention to treat" estimate measure?
   c. This is an accurate measure of the effect of the program, but is it a good measure? What are the considerations? When is it useful? When is it not useful?

You are interested in learning the effect of treatment on those actually treated.

2. Five of your colleagues are passing by your desk; they all agree that you should calculate the effect of the treatment using only the 10,000 children who were treated.
   a. What is the impact using only the treated?
   b. Is the advice sound? Why? Why not?

3. Another colleague says that it's not a good idea to drop the untreated entirely; you should use them but consider them as part of the comparison.
   a. What is the impact estimate based on this strategy?
   b. Is the advice sound? Why? Why not?

4. Another colleague suggests that you use the compliance rates, the proportion of people in each group that complied with the treatment assignment. You should divide the "intention to treat" estimate with the difference in the compliance rates.
   a. What are the compliance rates in the treatment and comparison groups?
   b. What is the impact estimate based on this strategy?
   c. Is the advice sound? Why? Why not?

5. The program raised awareness of worms, so some parents in the comparison bought the drugs and treated the children at home. Altogether 2,000 comparison children were treated.

   What is the "treatment on the treated" impact estimate?

Managing spillovers—when the comparison, itself untreated, benefits from the treatment being treated

People assigned to the control group may benefit indirectly from those receiving treatment. For example, a program that distributes insecticide-treated nets may reduce malaria transmission in the community, indirectly benefiting those who themselves do not sleep under a net. Such effects are called externalities or spillovers.

Discussion Topic 3: Managing spillovers

In the PSPD, randomization was at the school level.

People in the evaluation areas lived on farms close together. Clusters of farms can be divided into areas of 3km radius. Three such areas—A, B, and C—are shown in the diagram below.[*]Farms are close enough for children from neighboring farms to play with one another. Families also had a choice of primary schools.

There are three schools in area A, three in area B, and five in area C. It was common for children from neighboring farms, or even siblings, to go to different schools. Some of the schools in each cluster were treatment, others were control. Group 1 schools were the treatment in year 1, and group 2 and 3 were the comparison.

Each school has 100 children. Protocol compliance is 100 percent: all the children in treatment get treated and all the children in comparison do not get treated.

1. You estimate impact by comparing average worm loads at treatment and comparison schools.

   Would this estimate be an underestimate or overestimate of the impact?

2. The treatment density is the proportion of treated to untreated in a given grouping of people.
   a. What is the treatment density at the treatment schools in year 1?
   b. What is the treatment density of comparison schools?
   c. What are the treatment densities in areas A, B, and C in year 1?
   d. What are the treatment densities in areas A, B, and C in year 2 and year 3?

3. a. If there are any spillovers, where would you expect them to come from?
   b. Is it possible for you to capture spillover effects within the schools?
   c. If you don't expect to be able to capture the spillover effect, what would you need to be able to capture them?
   d. Is it possible for you capture cross-school spillovers?

4. Rank the areas A, B, and C in terms of the amount of treatment spillover effects expected in years 1, 2, and 3.

5. a. If you had randomized at the individual level, what could you have done to capture interpersonal spillover?
   b. If you had randomized at the school level what can you do to capture cross-school spillovers?
   c. What general strategy does this suggest?

Discussion Topic 3: Managing spillovers
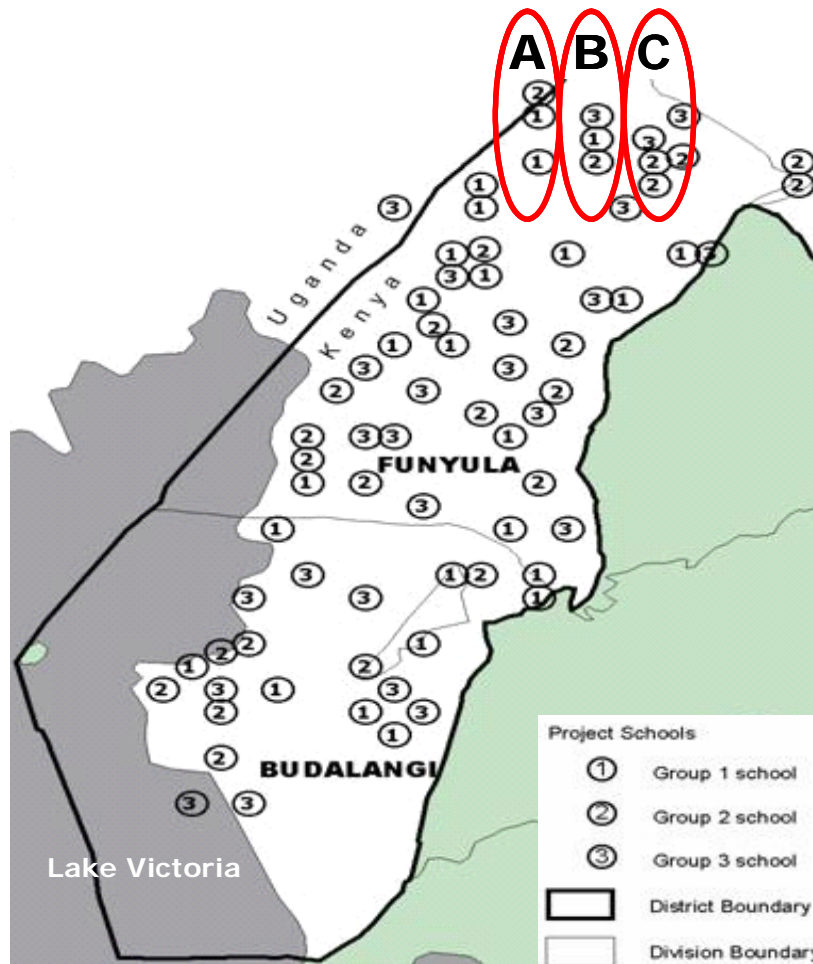
* **The GPS locations were collected before May 2000, when the U.S. was still downgrading international GPS accuracy. Readings may only be accurate to within several hundred meters. So one Group 3 school appears to be in Uganda, but it's actually on the Kenyan side of the border. The school that appears to be in Lake Victoria is actually on a very small island.**

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Exercise A: Understanding random sampling and the law of large numbers

In this exercise, we will visually explore random samples of different sizes from a given population. In particular, we will try to demonstrate that larger sample sizes tend to be more reflective of the underlying population.

1) Open the file "ExerciseA_SamplingDistributions_NEW.xlsm".

2) If prompted, select "Enable Macros".

3) Navigate to the "Randomize" worksheet, which allows you to choose a random sample of size "Sample Size" from the data contained in the "control" worksheet.

4) Enter "10" for "Sample Size and click the "Randomize" button. Observe the distribution of the various characteristics between Treatment, Control and Expected. With a sample size this small, the percentage difference from the expected average is quite high for reading scores. Click "Randomize" multiple times and observe how the distribution changes.

5) Now, try "50" for the sample size. What happens to the distributions? Randomize a few times and observe the percentage difference for the reading scores.

6) Increase the sample size to "500", "2000" and "10000", and repeat the observations from step 5. What can we say about larger sample sizes? How do they affect our Treatment and Control samples? Should the percentage difference between Treatment, Control and Expected always go down as we increase sample size?

Key Vocabulary:

> **1. Power**: the likelihood that, when the program has an effect, one will be able to distinguish the effect from zero given the sample size.
> **2. Significance**: the likelihood that the measured effect did not occur by chance. Statistical tests are performed to determine whether one group (e.g. the experimental group) is different from another group (e.g. comparison group) on the measurable outcome variables used in the evaluation.
> **3. Standard Deviation**: a standardized measure of the variation of a sample population from its mean on a given characteristic/outcome. Mathematically, the square root of the variance.
> **4. Standardized Effect Size**: a standardized measure of the [expected] magnitude of the effect of a program.
> **5. Cluster**: the level of observation at which a sample size is measured. Generally, observations which are highly correlated with each other should be clustered and the sample size should be measured at this clustered level.
> **6. Intra-cluster Correlation Coefficient**: a measure of the correlation between

Exercise B: Sample size calculations

The Extra Teacher Program (ETP) case study discussed the concept of cluster randomized trials. The Balsakhi example used in the prior lecture introduced the concept of power calculations. In the latter, we were interested in measuring the effect of a treatment (balsakhis in classrooms) on outcomes measured at the individual level—child test scores. However, the randomization of balsakhis was done at the classroom level. It could be that our outcome of interest is correlated for students in the same classroom, for reasons that have nothing to do with the balsakhi. For example, all the students in a classroom will be affected by their original teacher, by whether their classroom is unusually dark, or if they have a chalkboard; these factors mean that when one student in the class does particularly well for this reason, all the students in that classroom probably also do better—which might have nothing to do with a balsakhi.

Therefore, if we sample 100 kids from 10 randomly selected schools, that sample is less representative of the population of schools in the city than if we selected 100 random kids from the whole population of schools, and therefore absorbs less variance. In effect, we have a smaller sample size than we think. This will lead to more noise in our sample, and hence larger standard error than in the usual case of independent sampling. When planning both the sample size and the best way to sample classrooms, we need to take this into account.

This exercise will help you understand how to do that. Should you sample every student in just a few schools? Should you sample a few students from many schools? How do you decide?

We will work through these questions by determining the sample size that allows us to detect a specific effect with at least 80% power. Remember power is the likelihood that when the treatment has an effect you will be able to distinguish it from zero in your sample.

In this example, "clusters" refer to "clusters of children"—in other words, "classrooms" or "schools". This exercise shows you how the power of your sample changes with the number of clusters, the size of the clusters, the size of the treatment effect and the Intraclass Correlation Coefficient. We will use a

software program developed by Steve Raudebush with funding from the William T. Grant Foundation. You can find additional resources on clustered designs on their web site.

Section 1: Using the OD Software

First download the OD software from the website (a software manual is also available):

http://sitemaker.umich.edu/group-based/optimal_design_software

When you open it, you will see a screen which looks like the one below. Select the menu option "Design" to see the primary menu. Select the option "Cluster Randomized Trials with person-level outcomes," "Cluster Randomized Trials," and then "Treatment at level 2." You'll see several options to generate graphs; choose "Power vs. Total number of clusters (J)."
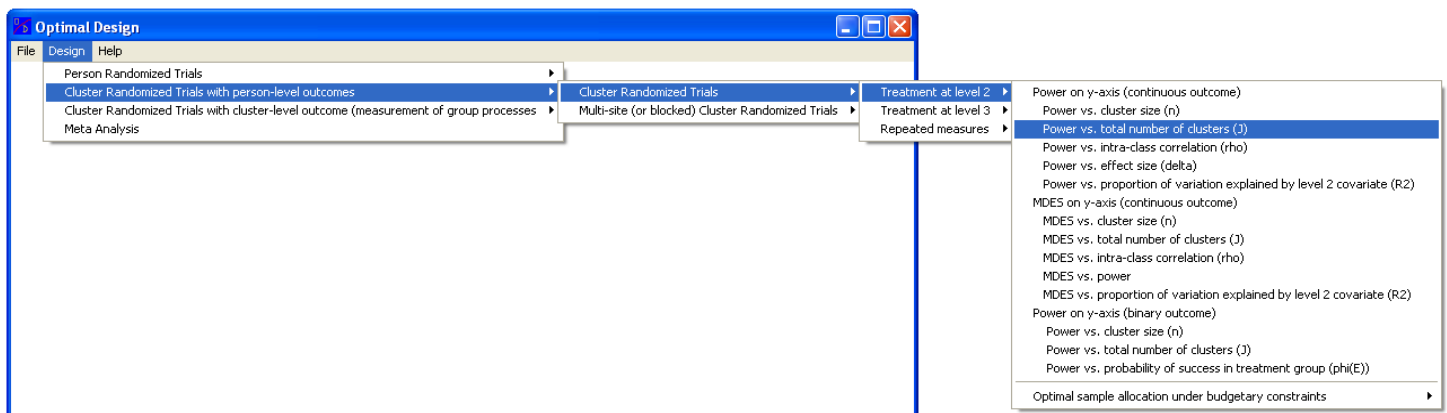
A new window will appear:

Select **α** (alpha). You'll see it is already set to 0.050 for a 95% significance level.

First let's assume we want to test only 40 students per school. How many schools do you need to go to in order to have a statistically significant answer?

Click on **n**, which represents the number of students per school. Since we are testing only 40 students per school, so fill in n(1) with 40 and click OK.

Now we have to determine δ (delta), the standard effect size (the effect size divided by the standard deviation of the variable of interest). Assume we are interested in detecting whether there is an increase of 10% in test scores. (Or more accurately, are uninterested in a detect less than 10%) Our baseline survey indicated that the average test score is 26, with a standard deviation of 20. We want to detect an effect size of 10% of 26, which is 2.6. We divide 2.6 by the standard deviation to get δ equal to 2.6/20, or 0.13.

Select δ from the menu. In the dialogue box that appears there is a prefilled value of 0.200 for delta(1). Change the value to 0.13, and change the value of delta (2) to empty. Select OK.

Finally we need to choose *ρ* (rho), which is the intra-cluster correlation. *ρ* tells us how strongly the outcomes are correlated for units within the same cluster. If students from the same school were clones (no variation) and all scored the same on the test, then ρ would equal 1. If, on the other hand, students from the same schools are in fact independent—and there was no differences between schools, then *ρ* will equal 0.

You have determined in your pilot study that ρ is 0.17. Fill in rho(1) to 0.17, and set rho (2) to be empty.

You should see a graph similar to the one below.
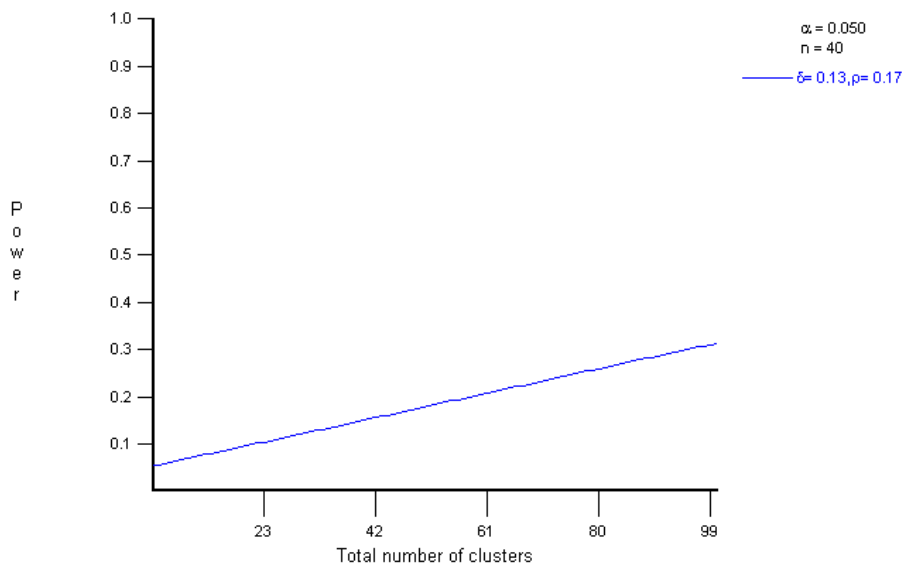
You'll notice that your x axis isn't long enough to allow you to see what number of clusters would give

you 80% power. Click on the [≤⊠≤] button to set your x axis maximum to 400. Then, you can click on the graph with your mouse to see the exact power and number of clusters for a particular point.

α = 0.050
n = 40
δ = 0.13, ρ = 0.17

J=283.387218 power=0.703593
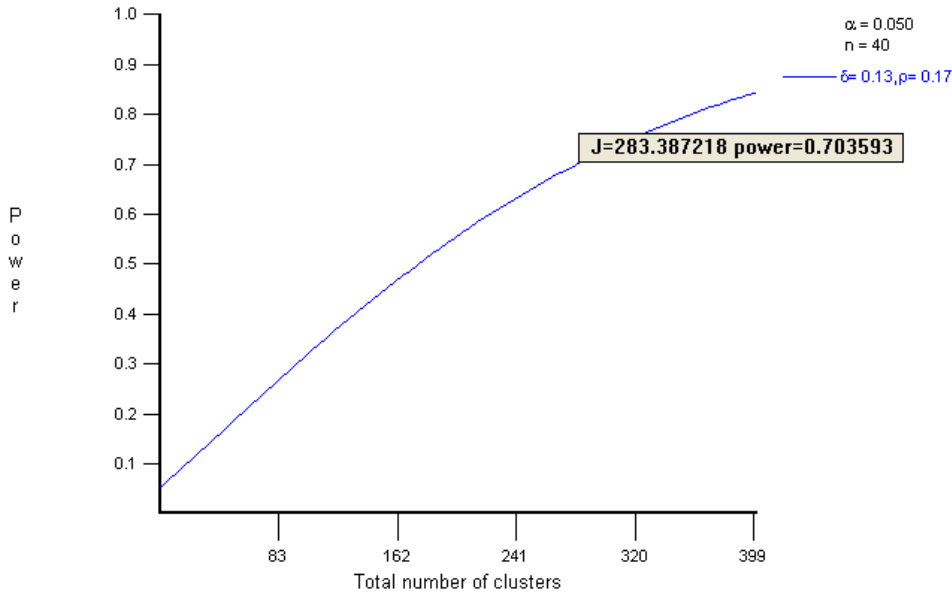
Exercise 3.1:
How many schools are needed to achieve 80% power? 90% power?

Now you have seen how many clusters you need for 80% power, sampling 40 students per school. Suppose instead that you only have the ability to go to 124 schools (this is the actual number that was sampled in the Balsakhi program).

Exercise 3.2:
How many children per school are needed to achieve 80% power? 90% power?
Choose different values for n to see how your graph changes.

Finally, let's see how the Intraclass Correlation Coefficient (ρ) changes power of a given sample. Leave rho(1) to be 0.17 but for comparison change rho(2) to 0.0.

You should see a graph like the one below. The solid blue curve is the one with the parameters you've set - based on your pretesting estimates of the effect of reservations for women on drinking water. The blue dashed curve is there for comparison – to see how much power you would get from your sample if ρ were zero. Look carefully at the graph.

Exercise 3.3:
How does the power of the sample change with the Intraclass Correlation Coefficient (ρ)?
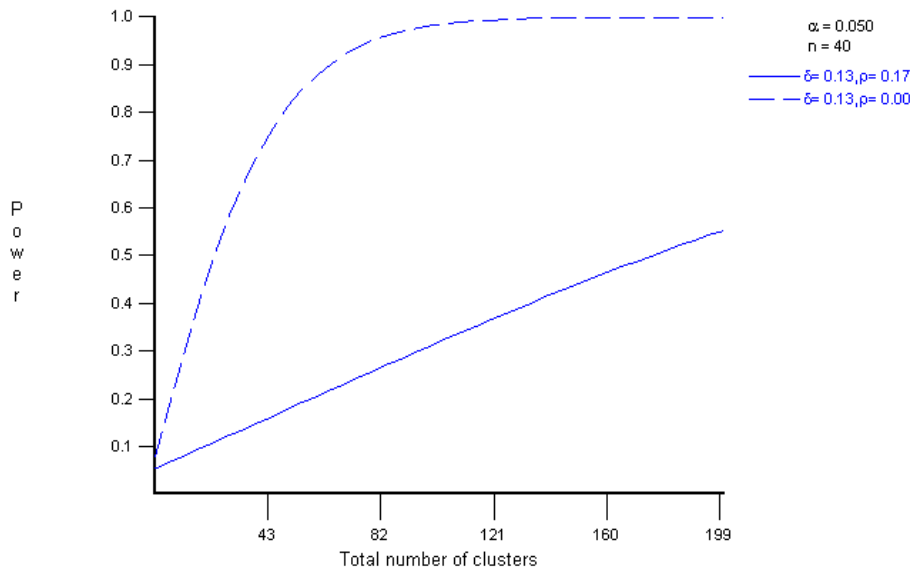
To take a look at some of the other menu options, close the graph by clicking on the ⊠ in the top right hand corner of the inner window. Select the Cluster Randomized Trial menu again.
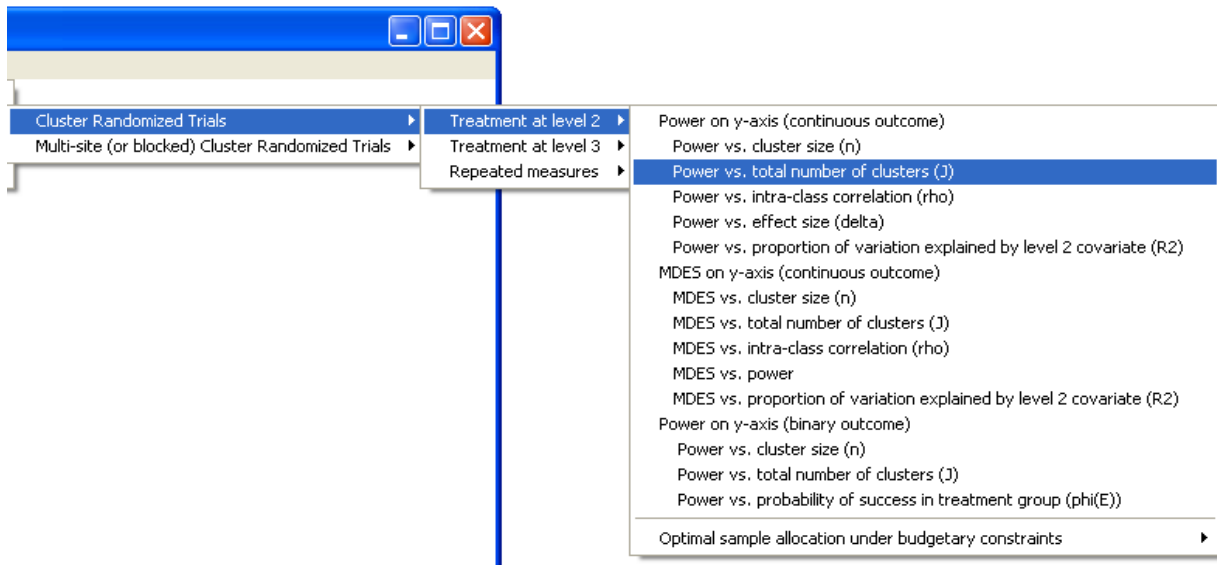
Exercise 3.4:
Try generating graphs for how power changes with cluster size (n), intra-class correlation (rho) and effect size (delta).
You will have to re-enter your pre-test parameters each time you open a new graph.

Exercise C: The mechanics of random assignment using MS Excel

Part 1: simple randomization

Like most spreadsheet programs MS Excel has a random number generator function. Say we had a list of schools and wanted to assign half to treatment and half to control

(1) We have all our list of schools.

	A	B	C	D
		Exercise 2 data.xls [Compatibility Mode] - Microsoft Excel		
1	SchoolID	SchoolName	Random#	T-C
2	101	Babajipura G.M.M.Kumar shala No. 1		
3	103	Babajipura Kanya Shala No. 3		
4	107	Babajipura Mishra Shala No. 7		
5	108	Babajipura Mishra Shala No. 8		
6	112	Babajipura Marathi Mishra Shala No. 12		
7	113	Babajipura Kanya Shala No. 13		
8	114	Babajipura Mishra Shala No. 14		
9	117	Babajipura Kumar Shala No. 17		
10	118	Babajipura Mishra Shala No. 18		
11	119	Babajipura Mishra Shala No. 19		
12	120	Babajipura Mishra Shala No. 20		
13	121	Babajipura Mishra Shala No. 21		
14	125	Babajipura Kumar Shala No. 25		
15	126	Babajipura Kanya Shala No. 26		
16	127	Babajipura Mishra Shala No. 27		
17	128	Babajipura Mishra Shala No. 28		
18	130	Babajipura Hindi Mishra Shala No. 30		
19	131	Babajipura Mishra Shala No. 31		
20	132	Babajipura Mishra Shala No. 32		
21	201	Fatehpura Kumar Shala No. 1		
22	202	Fatehpura Mishra Shala No. 2		
23	209	Fatehpura Mishra Shala No. 9		
24	210	Fatehpura Kanya Shala No. 10		
25	211	Fatehpura Mishra Shala No. 11		
26	213	Fatehpura Kumar Shala No. 13		
27	215	Fatehpura Hindi Mishra Shala No. 15		
28	216	Fatehpura Mishra Shala No. 16		
29	218	Fatehpura Mishra Shala No. 18		
30	219	Fatehpura Mishra Shala No. 19		
31	301	N. Sayajiganj Mishra Shala No. 1 (center)		

(2) Assign a random number to each school:

The function RAND () is Excel's random number generator. To use it, in Column C, type in the following = RAND() in each cell adjacent to every name. Or you can type this function in the top row (row 2) and simply copy and paste to the entire column, or click and drag.

	A	B	C	D
1	SchoolID	SchoolName	Random#	T-C
2	101	Babajipura G.M.M.Kumar shala No. 1	=RAND()	
3	103	Babajipura Kanya Shala No. 3		
4	107	Babajipura Mishra Shala No. 7		
5	108	Babajipura Mishra Shala No. 8		
6	112	Babajipura Marathi Mishra Shala No. 12		
7	113	Babajipura Kanya Shala No. 13		
8	114	Babajipura Mishra Shala No. 14		
9	117	Babajipura Kumar Shala No. 17		
10	118	Babajipura Mishra Shala No. 18		
11	119	Babajipura Mishra Shala No. 19		
12	120	Babajipura Mishra Shala No. 20		
13	121	Babajipura Mishra Shala No. 21		
14	125	Babajipura Kumar Shala No. 25		
15	126	Babajipura Kanya Shala No. 26		
16	127	Babajipura Mishra Shala No. 27		
17	128	Babajipura Mishra Shala No. 28		
18	130	Babajipura Hindi Mishra Shala No. 30		
19	131	Babajipura Mishra Shala No. 31		
20	132	Babajipura Mishra Shala No. 32		
21	201	Fatehpura Kumar Shala No. 1		
22	202	Fatehpura Mishra Shala No. 2		
23	209	Fatehpura Mishra Shala No. 9		
24	210	Fatehpura Kanya Shala No. 10		
25	211	Fatehpura Mishra Shala No. 11		
26	213	Fatehpura Kumar Shala No. 13		
27	215	Fatehpura Hindi Mishra Shala No. 15		
28	216	Fatehpura Mishra Shala No. 16		
29	218	Fatehpura Mishra Shala No. 18		
30	219	Fatehpura Mishra Shala No. 19		
31	301	N. Sayajiganj Mishra Shala No. 1 (center)		

Typing = RAND() puts a 15-digit random number between 0 and 1 in the cell.

	A	B	C	D
1	SchoolID	SchoolName	Random#	T-C
2	101	Babajipura G.M.M.Kumar shala No. 1	0.80541713	
3	103	Babajipura Kanya Shala No. 3	0.53078382	
4	107	Babajipura Mishra Shala No. 7	0.92449824	
5	108	Babajipura Mishra Shala No. 8	0.81342515	
6	112	Babajipura Marathi Mishra Shala No. 12	0.59650637	
7	113	Babajipura Kanya Shala No. 13	0.58563987	
8	114	Babajipura Mishra Shala No. 14	0.6486176	
9	117	Babajipura Kumar Shala No. 17	0.46206529	
10	118	Babajipura Mishra Shala No. 18	0.18134939	
11	119	Babajipura Mishra Shala No. 19	0.69772005	
12	120	Babajipura Mishra Shala No. 20	0.83992642	
13	121	Babajipura Mishra Shala No. 21	0.85501349	
14	125	Babajipura Kumar Shala No. 25	0.30572517	
15	126	Babajipura Kanya Shala No. 26	0.53388093	
16	127	Babajipura Mishra Shala No. 27	0.46003571	
17	128	Babajipura Mishra Shala No. 28	0.27464658	
18	130	Babajipura Hindi Mishra Shala No. 30	0.02073858	
19	131	Babajipura Mishra Shala No. 31	0.77709404	
20	132	Babajipura Mishra Shala No. 32	0.2362122	
21	201	Fatehpura Kumar Shala No. 1	0.91552715	
22	202	Fatehpura Mishra Shala No. 2	0.95669543	
23	209	Fatehpura Mishra Shala No. 9	0.48508217	
24	210	Fatehpura Kanya Shala No. 10	0.62054343	
25	211	Fatehpura Mishra Shala No. 11	0.17807564	
26	213	Fatehpura Kumar Shala No. 13	0.36389518	
27	215	Fatehpura Hindi Mishra Shala No. 15	0.03446481	
28	216	Fatehpura Mishra Shala No. 16	0.51526826	
29	218	Fatehpura Mishra Shala No. 18	0.17860571	
30	219	Fatehpura Mishra Shala No. 19	0.04501407	
31	301	N. Sayajiganj Mishra Shala No. 1 (center)	0.93881649	

**(3) Copy the cells in Colum C, then paste the values over the same cells**

The function, =RAND() will re-randomize each time you make any changes to any other part of the spreadsheet. Excel does this because it recalculates all values with any change to any cell. (You can also induce recalculation, and hence re-randomization, by pressing the key F9.)

This can be confusing, however. Once we've generated our column of random numbers, we do not need to re-randomize. We already have a clean column of random values. To stop excel from recalculating, you can replace the "functions" in this column with the "values".

To do this, highlight all values in Column C. Then right-click anywhere in the highlighted column, and choose Copy.

Then right click anywhere in that column and chose Paste Special. The "Paste Special window will appear. Click on "Values".
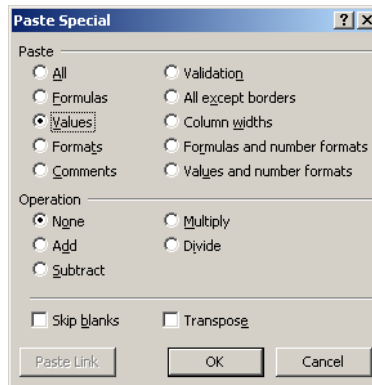
(4) Sort the columns in either descending or ascending order of column C:

Highlight columns A, B, and C. In the data tab, and press the Sort button:
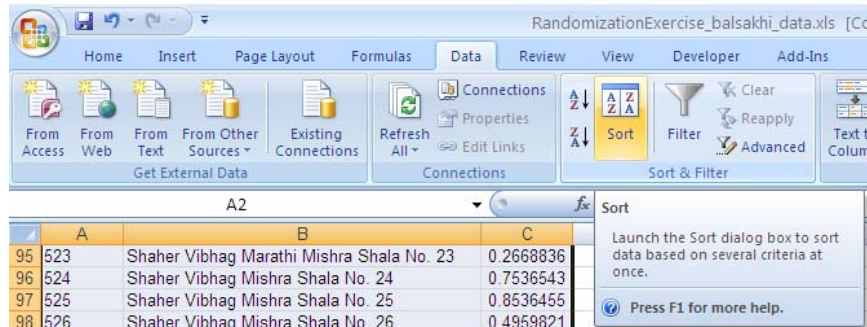
A Sort box will pop up.
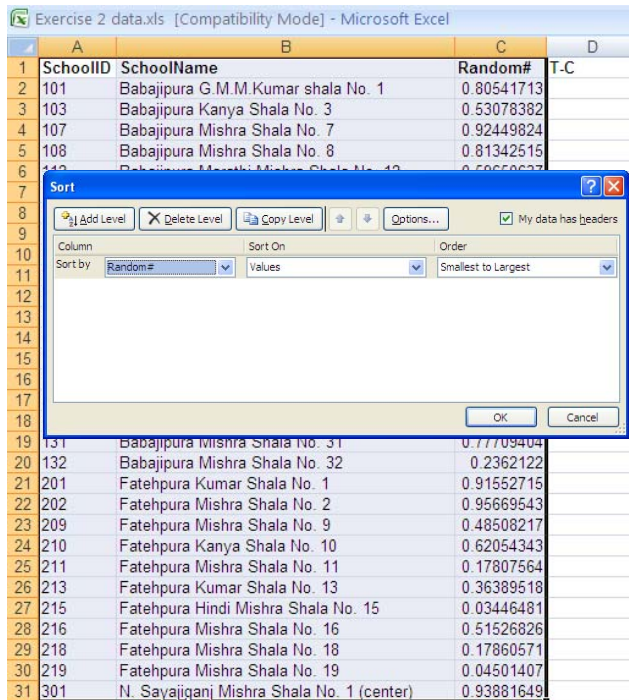
In the Sort by column, select "random #". Click OK. Doing this sorts the list by the random number in ascending or descending order, whichever you chose.

There! You have a randomly sorted list.

(5) Sort the columns in either descending or ascending order of column C:

Because your list is randomly sorted, it is completely random whether schools are in the top half of the list, or the bottom half. Therefore, if you assign the top half to the treatment group and the bottom half to the control group, your schools have been "randomly assigned".

In column D, type "T" for the first half of the rows (rows 2-61). For the second half of the rows (rows 62-123), type "C"

Re-sort your list back in order of school id. You'll see that your schools have been randomly assigned to treatment and control groups

	A	B	C	D
1	SchoolID	SchoolName	Random#	T-C
2	101	Babajipura G.M.M.Kumar shala No. 1	0.80541713	C
3	103	Babajipura Kanya Shala No. 3	0.53078382	T
4	107	Babajipura Mishra Shala No. 7	0.92449824	C
5	108	Babajipura Mishra Shala No. 8	0.81342515	C
6	112	Babajipura Marathi Mishra Shala No. 12	0.59650637	C
7	113	Babajipura Kanya Shala No. 13	0.58563987	C
8	114	Babajipura Mishra Shala No. 14	0.6486176	C
9	117	Babajipura Kumar Shala No. 17	0.46206529	T
10	118	Babajipura Mishra Shala No. 18	0.18134939	T
11	119	Babajipura Mishra Shala No. 19	0.69772005	C
12	120	Babajipura Mishra Shala No. 20	0.83992642	C
13	121	Babajipura Mishra Shala No. 21	0.85501349	C
14	125	Babajipura Kumar Shala No. 25	0.30572517	T
15	126	Babajipura Kanya Shala No. 26	0.53388093	C
16	127	Babajipura Mishra Shala No. 27	0.46003571	T
17	128	Babajipura Mishra Shala No. 28	0.27464658	T
18	130	Babajipura Hindi Mishra Shala No. 30	0.02073858	T
19	131	Babajipura Mishra Shala No. 31	0.77709404	C
20	132	Babajipura Mishra Shala No. 32	0.2362122	T
21	201	Fatehpura Kumar Shala No. 1	0.91552715	C
22	202	Fatehpura Mishra Shala No. 2	0.95669543	C
23	209	Fatehpura Mishra Shala No. 9	0.48508217	T
24	210	Fatehpura Kanya Shala No. 10	0.62054343	C
25	211	Fatehpura Mishra Shala No. 11	0.17807564	T
26	213	Fatehpura Kumar Shala No. 13	0.36389518	T
27	215	Fatehpura Hindi Mishra Shala No. 15	0.03446481	T
28	216	Fatehpura Mishra Shala No. 16	0.51526826	T
29	218	Fatehpura Mishra Shala No. 18	0.17860571	T
30	219	Fatehpura Mishra Shala No. 19	0.04501407	T
31	301	N. Sayajiganj Mishra Shala No. 1 (center)	0.93881649	C

60

Part 2: stratified randomization

Stratification is the process of dividing a sample into groups, and then randomly assigning individuals within each group to the treatment and control. The reasons for doing this are rather technical. One reason for stratifying is that it ensures subgroups are balanced, making it easier to perform certain subgroup analyses. For example, if you want to test the effectiveness on a new education program separately for schools where children are taught in Hindi versus schools where children are taught in Gujarati, you can stratify by "language of instruction" and ensure that there are an equal number schools of each language type in the treatment and control groups.

(1) We have all our list of schools and potential "strata".

Mechanically, the only difference in random sorting is that instead of simply sorting by the random number, you would first sort by language, and then the random number. Obviously, the first step is to ensure you have the variables by which you hope to stratify.

(2) Sort by strata and then by random number

Assuming you have all the variables you need: in the data tab, click "Sort". The Sort window will pop up. Sort by "Language". Press the button, "Add Level". Then select, "Random #".

(3) Assign Treatment – Control Status for each group.

Within each group of languages, type "T" for the first half of the rows, and "C" for the second half.

	A	B	C	D	E	F
100	132	Babajipura Mishra Shala No. 32	Gujarati	Mishra	0.8931975	C
101	615	Wadi Mishra Shala No. 15	Gujarati	Mishra	0.9142383	C
102	618	Wadi Kumar Shala No. 18	Gujarati	Kumar	0.9229356	C
103	408	Raopura Kanya Shala No. 8	Gujarati	Kanya	0.9285077	C
104	502	Shaher Vibhag Mishra Shala No. 2	Gujarati	Mishra	0.9549163	C
105	311	Sayajiganj Mishra Shala No. 11	Gujarati	Mishra	0.9595266	C
106	344	Sayajiganj Mishra Shala No. 44	Gujarati	Mishra	0.9688854	C
107	347	Sayajiganj Hindi Mishra Shala No. 47	Hindi	Mishra	0.0163449	T
108	332	Sayajiganj Hindi Mishra Shala No. 32	Hindi	Mishra	0.1528766	T
109	342	Sayajiganj Hindi Mishra Shala No. 42	Hindi	Mishra	0.2646791	T
110	215	Fatehpura Hindi Mishra Shala No. 15	Hindi	Mishra	0.3142377	T
111	326	Sayajiganj Hindi Mishra Shala No. 26	Hindi	Mishra	0.4291559	T
112	638	Wadi Hindi Mishra Shala No. 38	Hindi	Mishra	0.6772441	C
113	130	Babajipura Hindi Mishra Shala No. 30	Hindi	Mishra	0.7053783	C
114	315	Sayajiganj Hindi Mishra Shala No. 15	Hindi	Mishra	0.7955641	C
115	626	Wadi Hindi Mishra Shala No. 26	Hindi	Mishra	0.8918818	C
116	346	Sayajiganj Hindi Mishra Shala No. 46	Hindi	Mishra	0.9051467	C
117	303	N. Sayajiganj Marathi Mishra Shala No. 3	Marathi	Mishra	0.0354843	T
118	523	Shaher Vibhag Marathi Mishra Shala No. 23	Marathi	Mishra	0.1834626	T
119	409	Raopura Marathi Mishra Shala No. 9	Marathi	Mishra	0.7676874	T
120	611	Wadi Marathi Mishra Shala No. 11	Marathi	Mishra	0.8847497	T
121	329	Sayajiganj Marathi Mishra Shala No. 29	Marathi	Mishra	0.8992905	C
122	112	Babajipura Marathi Mishra Shala No. 12	Marathi	Mishra	0.9430188	C
123	327	Sayajiganj Marathi Mishra Shala No. 27	Marathi	Mishra	0.9515261	C
124	617	Wadi Marathi Mishra Shala No. 17	Marathi	Mishra	0.9648498	C

Group Presentation

Participants will form 4-6 person groups which will work through the design process for a randomised evaluation of a development project. Groups will be aided in this project by both the faculty and teaching assistants with the work culminating in presentations at the end of the week.

The goal of the group presentations is to consolidate and apply the knowledge of the lectures and thereby ensure that participants will leave with the knowledge, experience, and confidence necessary to conduct their own randomised evaluations. We encourage groups to work on projects that are relevant to participants' organisations.

All groups will present on Friday. The 15-minute presentation is followed by a 15-minute discussion led by J-PAL affiliates and staff. We provide groups with template slides for their presentation (see next page). While the groups do not need to follow this exactly, the presentation should have no more than 9 slides (including title slide, excluding appendix) and should include the following topics:

- Brief project background
- Theory of change
- Evaluation question
- Outcomes
- Evaluation design
- Data and sample size
- Potential validity threats and how to manage them
- Dissemination strategy of results

Please time yourself and do not exceed the allotted time. We have only a limited amount of time for these presentations and follow a strict timeline to be fair to all groups.

Groups

Group 1: Labour and Finance
TA: Martin Abel, J-PAL Africa
Global
Room: LS 4A
Abebe Shimeles
Gareth Roberts
Andrew Sellu
Monet Durieux
Timothy Dladla

Group 2: Health (I)
TA: Kartik Akileswaran, J-PAL

Room: LS 4B
Aisha Nansamba
Grace Msichili
Damilola Toki
Enas Ali Muhammad
Busiku Hamai

Group 3: Health (II)
TA: Willa Brown, J-PAL Africa
Room: LS 4C
Miriam Libetwa
Todd Koppenhaver
Nelson Kakande
Ogo Odeyovwi
Robert Kuganab-Lem
Kathleen Diga

Group 4: Agriculture
TA: Mike Duthie, IPA Sierra Leone
Room: LS 4D
Daniel Mabirizi
Kandi Shejavali
Martha Olwenyi
Hiroaki Asaoka
Yusuf Kiwala

Group 5: Education
TA: Luke Crowley, J-PAL Africa
Room: LS 4G
Double-Hugh Marera
Eleanor Hazell
Dalia Bayoumi
Roelien Herholdt
Ricardo Torres da Costa

Group 6: Youth
TA: Suleiman Asman, IPA Kenya
Room: LS 4J
Alejandro Guerrero Ruiz
Ritu Nayyar-Stone
Chikezie Chukwu
Victor Owusu
Colin Christensen

Group 7: Conflict and Governance
TA: Kelly Bidwell, IPA
Room: LS 4G
Angela Bailey
Gabrielle Cole
Isatou Batonon
Wilfred Gray-Johnson
Robert Pitman

Group Presentation Template

Title

List your Team Members

You don't have to follow this
exactly, this is just a guideline.

Background

- Talk briefly about general context, needs assessment, problem you want to solve.

Theory of Change

- Describe the specific intervention that you are evaluating.
- Talk about how it will solve part of the problem you described in the background.
- You may want to mention other causes of a problem that your intervention will not solve.
- (You can use the TOC template in the appendix.)

Evaluation Questions and Outcomes

- These should be directly linked to the TOC described above.
- What outcomes do you need to measure to test your research hypothesis?

Evaluation Design

- Unit of randomization, type of randomization (why did you choose these?)
- The actual randomization design- i.e. specific treatment group(s)

Data and Sample Size

- Outcomes
- Tell us where you will get the data – survey? Administrative?
- Power calcs
  - Justify where you got effect size and rho from, don't make it up.
  - You may need to do separate power calcs for separate outcomes.

Potential challenges

- Talk about threats (attrition, spillover, etc.) and how you want to manage them.
- You may need to revise your power calcs.

Results

- Why (and for whom) they would be useful.
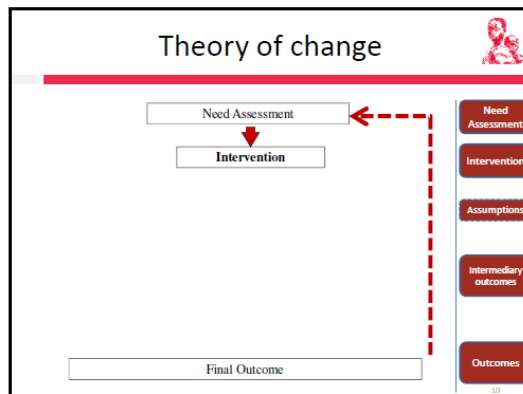- How would you disseminate them.

Appendix

Theory of change

Practical Tips

<u>Shopping</u>

On campus: There are cafeterias near the centre of Upper Campus selling a range of food products, from sandwiches to sushi.

Off campus:
Southern Suburbs (if your hotel is in Rondebosch or Claremont)
Cavendish Square is located just off Claremont Main Road and is bound to satisfy all your shopping desires. If one is looking to do a little grocery shopping, the Woolworths food is located on the bottom floor. Alternatively, if you wish to avoid the rush of the mall, you can find a Pick n' Pay supermarket a little further down Main Road towards Rondebosch. A smaller Pick n' Pay and Woolworths food are also situated closer to the university on Rondebosch Main Road and are a 15 minute walk from UCT.

<div style="border:1px solid black; padding:10px;">

<u>Cavendish Square</u>
Open times: 09:00-19:00 (Mon-Sat), 10:00-17:00 (Sundays)
Address: Dreyer Street, Claremont
Website: www.cavendish.co.za
Call: 021 657 5620

<u>Pick n' Pay (Claremont)</u>
Open times: 08:00 – 19:00 (Mon-Sat), 08:00- 17:00 (Sun)
Address: Corner Campground & Main Road, Claremont
Call: 021 674 5908

<u>Pick n' Pay (Rondebosch)</u>
Open times: 08:00-22:00 (Mon-Sun)
Address: Shop No 1,Village Centre, Main Road, Rondebosch
Call: 021 685 4001

<u>Woolworths (Rondebosch)</u>
Open times: 09:00-21:00 (Mon-Fri), 09:00-20:00 (Sat-Sun)
Call: 021 685 4416

</div>

Other large malls in Cape Town:
<u>The Waterfront</u> (for when you are closer to town or visiting Robben Island)
Website: www.waterfront.co.za
Call: 021 408 7600

<u>Century City</u> (for when you want to visit the biggest mall in Africa)
Website: www.centurycity.co.za
Call: 021 552 6889

Alternative places to buy groceries and snacks:
Many of the petrol stations around Cape Town have little food stores.

<u>Restaurants</u>

Cape Town is known for its diverse array of dining and cuisine. Here is but a small list of well-known restaurants that you may wish to try.

* These restaurants are fairly close to the Vineyard Hotel, Southern Sun, Graca Machel and the All Africa Hotel.

Budget
(Main meal under R50)

1) **Eastern Food Bazaar**
   Cuisine: Indian, Chinese
   Location: City Bowl
   Contact: 021 461 2458

2) **Mzolis**
   Cuisine: African, BBQ
   Location: Gugulethu
   Contact: 021 638 1355
   You will need a guide or someone from Cape Town who knows the area!

3) ***Food Lovers Market**
   Cuisine: Deli, Buffet – Basically everything
   Location: Claremont
   Contact: 021 674 7836

Medium price range
(Main meal between R50 and R100)

1) ***Col Cacchio Pizzeria**
   Cuisine: Pizza
   Location: Claremont (Cavendish), Camps Bay
   Contact: 021 674 6387/ 021 438 2171

2) ***Kirstenbosch Tea Room**
   Cuisine: Coffee Shop
   Location: Kirstenbosch Gardens, Newlands (Not for dinner)
   Contact: 021 797 4083

3) ***Rhodes Memorial Restaurant**
   Cuisine: Bistro, Coffee Shop
   Location: Rhodes Memorial Restaurant (Not for dinner)
   Contact: 021 687 0000

4) ***Fadela Williums**
   Cuisine: Cape Malay
   Location: Claremont
   Contact: 021 671 0037

5) ***Hussar Grill**
   Cuisine: Grills
   Location: Rondebosch
   Contact: 021 689 9516

6) **Addis in Cape**
   Cuisine: Ethiopian
   Location: City Bowl
   Contact: 021 424 5722

Higher End
(Main meal - R100 and above)

1) ***Die Wijnhuis**
   Cuisine: Mediterranean, Italian
   Location: Newlands
   Contact: 021 671 9705

2) ***Barristers Grill**
   Cuisine: Grill and Seafood
   Location: Newlands
   Contact: 021 671 7907

3) **Panama Jack's Taverna**
   Cuisine: Seafood
   Location: Table Bay harbour
   Lunch rates are lower. For example they offer a half-kilo of prawns for only R60 during the week
   Contact: 021 448 1080

4) **Olympia Cafe**
   Cuisine: Deli, Bakery, Coffee Shop
   Location: Kalk Bay
   Contact: 021 788 6396

5) ***Bihari**
   Cuisine: Indian
   Location: Newlands
   Contact: 021 674 7186

6) **Jonkershuis Constantia Eatery**
   Cuisine: Bistro
   Location: Constantia
   Contact: 021 794 4813

7) **Moyo**
   Cuisine: African
   Location: Stellenbosch
   Contact: 021 809 1133

Internet Access
Most hotels will have access otherwise ask for directions to your nearest internet café.

Electricity
Voltage: 220/230 V
Adapter: You will need an adaptor for Plug M and sometimes plug C. Plug C is the two-pin plug commonly used in Europe.

Money
Withdraws: We suggest that you use the campus ATM machines. They are situated on Middle Campus (next to the cafeteria), and Upper Campus (ground floor of the Leslie Social Science building and next to the library).

Credit Cards: When paying by credit card, we suggest that you ask vendors to swipe the card in your presence.

Exchange Rate: The current exchange rate is 6.81 South Africa Rand to the US-Dollar.

Map – Southern Suburbs

HOTELS ⬤ SHOPPING ⬛ RESTAURANTS ➡

1. Vineyard Hotel
2. Southern Sun
3. Graca Machel
4. All Africa Hotel
5. Cavendish Square (Woolworths food inside)
6. Pick n' Pay (Claremont)
7. Pick n' Pay (Rondebosch)
8. Woolworths (Rondebosch)
9. Food Lovers Market (See No. 5)
10. Col Cacchio Pizzeria (See No. 5)
11. Rhodes Memorial Restaurant
12. Fadela Williams
13. Hussar Grill
14. Die Wijnhuis
15. Barristers Grill
16. Bihari

Health and Emergencies

On campus:
1) Campus Protection Services: 021 650 2222/3
2) UCT Emergency Controller: 021 650 2175/6

Off Campus
1) Kingsbury Hospital (Wilderness Road, Claremont): 021 670 4000
2) Constantiaberg Medi-Clinic Hospital (Burnham Road, Plumsted): 021 799 2911 / 021 799 2196 (Emergency number)
3) Kenilworth Medicross (67 Rosmead Avenue, Kenilworth): 021 670 7640 – for doctor's visits

State Emergency Number (Police and Ambulance Services): **10111**
Private Ambulance Services: Netcare911: **082 911**

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