Lecture 22: Experimental Design

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14.310x

- What is being randomized?
  - The intervention(s)
- Who is being randomized?
  - The level of randomization (schools, individuals, villages, cells)
  - The sample over which you randomize (eligible people, population, people who applied,etc)
- How is randomization introduced?
  - Method of randomization
  - Stratification
- How many units are being randomized?
  - Power

- Introducing randomization when it may not be otherwise obvious
- Answering specific question(s)

- Simplest randomization: define your sample frame and your unit of randomization, use software to randomly assign one group to treatment, one to control
- Stratification: create group that are similar ex-ante. You will compare outcomes within each strata. It will help power by reducing variance
- Clustering: randomize instead at the group level. It will hurt power (since people who are similar share the same treatment status) but may be the only option.

- Phase in design
- Randomization “in the bubble’
- Encouragement design

- Choose target individuals or communi7es to be covered over several years
- Randomize the order in which they are phased in
- Those not yet phased in are the comparison

Randomized phase-in

- Choose target individuals or communities to be covered over several years
- Randomize the order in which they are phased in
- Those not yet phased in are the comparison

Randomized Phase-in diagram removed due to copyright restrictions. Please see *Running Randomized Evaluations: A Practical Guide* by Rachel Glennerster and Kudzai Takavarasha.

Randomization Around the Cutoff

Diagram removed due to copyright restrictions. Please see *Running Randomized Evaluations: A Practical Guide* by Rachel Glennerster and Kudzai Takavarasha.

Encouragement Design

Diagram removed due to copyright restrictions. Please see *Running Randomized Evaluations: A Practical Guide* by Rachel Glennerster and Kudzai Takavarasha.

- Estimating general equilibrium effects
- Unpacking the effect of an intervention to understand it better

- High unemployment: a promising labor market policy is job placement assistance (Card Kluve Weber, 2010)
- Several randomized evaluation exists: usually similar workers are assigned to a group versus another.
- An important criticism against the existing evaluations of these programs (and similar such as training program): gains can be offset by displacement effects (queue-jumping)

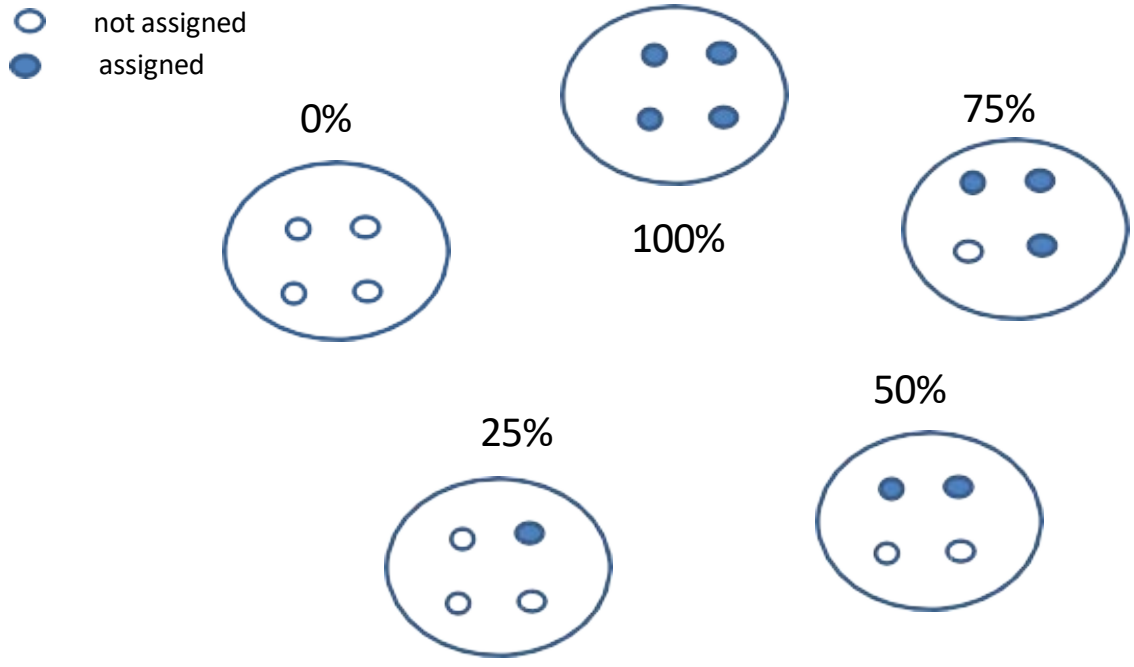
- We take advantage of a large-scale search assistance program which was implemented in France in 2007 (targeted half of administrative regions)
- Two-step RCT: randomly assign the proportion of treated to areas ; randomly assign treatment status to individuals within areas

- Youth unemployment an important issue in many countries (18% in the US, 23% in France or the UK, 36% in Italy, more than 50% in Spain and Greece)
- In 2007, new job search assistance program for 10,000 young job seekers
- Private counseling firms contracted with the objective to bring job seekers back to long-term jobs (idea of stepping stone)
- Target population:
  - Less than 30 years old
  - Unemployed for more than 6 months (or cumulating more than 12 months over the last 18 months)
  - Diploma after 2 years of college

- Payment conditional on objectives:
  - 25% if the job seeker enrolled
  - 40% if the job seeker signed a stable contract within less than 6 months
  - 35% if the former job seeker is still employed six months after the job has been found

Two-step randomization

1. At the local employment agency (LEA) level:
   - One LEA in each city of more than 30,000 inhabitants
   - Partition 235 LEAs into 47 homogenous quintuplets
   - Randomly assign within each quintuplet the assignment proportions 0%, 25%, 50%, 75% and 100% Diagram
2. Next, at the individual level: each individual is randomly assigned to the treatment or control, the assignment rate depending on the LEA to which he belongs


not assigned
assigned
0%
100%
75%
25%
50%

- *Super control group*: individuals in 0% assignment areas
- Comparing assigned to control and super control
  → Displacement effect
- Comparing assigned to treatment and super control
  → Effect on the treated

	Unemployed
Program Participation	0.441***
	(0.010)
Number of meetings	0.658***
with a counselor	(0.086)
Control mean	2.934
Received help with CV, coaching	0.114***
for interviews, etc.	(0.010)
Control mean	0.260
Help with matching (identify	0.007
job offers, help with transportation)	(0.008)
Control mean	0.194
Observations	9890

$$y_{ic} = \alpha + \beta Z_{ic} + d_c + X_{ic}\gamma + c_{ic}$$

Outcome: fixed-term contract with a length of more than 6 months

	Unemployed		
	All	Men	Women
Assigned to program	0.020*	0.051***	0.005
	(0.011)	(0.019)	(0.015)
Control mean	0.213	0.172	0.237
Observations	9890	3716	6174

Outcome: fixed-term contract with a length of more than 6 months

	Unemployed		
	All	Men	Women
Assigned to program	0.028***	0.051***	0.016
	(0.010)	(0.015)	(0.012)
In a program area	-0.009	-0.039**	0.008
	(0.011)	(0.016)	(0.015)
Net effect of program assignment	0.019**	0.012	0.024*
	(0.009)	(0.013)	(0.013)
Control Mean	0.213	0.172	0.237

- Estimating general equilibrium effects
- Unpacking the effect of an intervention to understand it better

- They examine the Raskin program in Indonesia, which provides eligible households with 15kg per month of heavily subsidized rice
- Right now information about the program among citizens is low:
    - Only 30% of eligible households know that they are actually Raskin eligible, and beneficiaries believe the copay is 25% higher than it actually is
    - Eligible only receive 1/3 of intended subsidy
- Given low levels of information, officials may have an advantage in bargaining with villagers
- Question: Will program transparency increase the amount of subsidy eligible households receive? And why?

- Randomized trial in 572 villages working with the Indonesian government
- In 378 randomly chosen villages eligible households received Raskin identification cards, which informed them they were eligible and the amount of rice

Sample card


KARTU RASKIN
SEPTEMBER 2012 - DESEMBER 2013
KABUPATEN BANDUNG

Nama KRT: Agus Budi
Nama PKRT: Siti Jasnah
Nama ART: Habib
Alamat: Gg. Markisa No.24
Kampung Ciwedi, Saketi

Tanda Tangan / Cap Jempol
Pemegang Kartu

HAK PEMEGANG KARTU RASKIN:
1. Pemegang kartu ini berhak untuk menerima beras Raskin sebanyak 15kg per RTS-PM per bulan selama bulan September 2012-Desember 2013
2. Harga tebus beras Raskin adalah Rp. 1.600 per kg di Titik Distribusi.

KETENTUAN:
1. Pembayaran Raskin dari RTS-PM kepada Pelaksana Distribusi Raskin dilakukan secara tunai
2. Kartu harus disimpan dengan baik, kehilangan atau kerusakan kartu menjadi tanggung jawab pemegang kartu
3. RTS-PM harus dapat menunjukkan kartu Raskin pada saat pengambilan beras.

- Suppose the cards “worked”. What else might you want to know?
- To elucidate mechanisms, within treatment villages varied 4 aspects of the card program
    - Public information about eligibility and cards (in addition to private information)
    - What information was printed on the cards (copay price or not)
    - Who received the cards (all eligible households or a subset) to test whether physical card matters
    - Whether cards contained clipoff coupons to examine perceived accountability effects

Public vs. private information

- Public vs. private information. Designed to test whether common knowledge facilitates collective action.
  - Private information: village head gets list and one copy posted.
  - Public information: in addition, many copies of list and posters about cards posted

Price vs. no-price

- Price vs. no-price: Designed to test precisely whether varying information on cards matters
  - Varied whether cards contained information on co-pay price or noy

Who received cards

- In all villages, full list of eligible beneficiaries was distributed
- But, varied
  - Whether cards were sent to all eligibles
  - Cards only send to bottom 10% of the population (about poorest 1/3 of beneficiaries)
- Designed to test role of physical card in bargaining

Coupons

- Coupons or no: Designed to test whether implied checking on the part of the government changed the results

- Within the 378 card villages, we want to run 4 different dimensions on 4 dimensions (so 16 possibilities):
  - Public vs. private information
  - Information on the cards
  - Who received cards
  - Tear-off coupons or no

		Public		Private	
		Price	No price	Price	No price
Cards to All	Coupons				
	No Coupons				
Cards to B10	Coupons				
	No Coupons				

- Data comes from three follow-up surveys:
  - Conducted 2 months, 8 months, and 18 months after cards distributed
  - Oversampled beneficiaries
  - Also interviewed the village leader
- Administrative data on eligibility status
- Baseline consumption data from the previous experiment

Impact on card receipt and use

Table 2: Reduced Form Effect of Card Treatment on Receipt and Use

	Eligible Households			Ineligible Households		
	Received Card	Used Card	Correctly idenfities own status	Received Card	Used Card	Correctly idenfities own status
	(1)	(2)	(3)	(4)	(5)	(6)
Card	0.28***	0.14***	0.09***	0.02**	0.03**	0.04*
Treatment	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Observations	5,693	5,693	5,691	3,619	3,619	3,619
Control Mean	0.06	0.06	0.30	0.05	0.05	0.35

- Note that only 28% of eligibles received card.

- Results suggest cards had a substantial impact
  - Increase in subsidy for eligibles of 25%
  - And this is with only 28 pp increase in cards. With full penetration of cards, could have been higher
  - Cost effective: increase in subsidy is well over 5 times the cost of the cards over the period of the study.
- Investigate mechanisms:
  - Kinds of information
    - Providing public vs. private information
    - Providing more information on the cards about the program
    - Providing physical cards with the information
  - Testing accountability effects through coupons

- Public information
  - Cards provide individual information on one's eligibility status.
  - But, if I am thinking of protesting, I may need to know if others would join me if I protested: common knowledge matter
  - To test this we varied the information about the program:
    - Standard information: List sent to village head and one poster with beneficiary lists posted
    - Public information: 3 posters per hamlet and mosque radio announcements
  - test whether this indeed changed people's beliefs, and whether it in turn affected outcomes

Knowledge and beliefs

Table 7: Effect of Public Information on Seeing the Eligibility List

	Eligible (1)	Ineligible (2)	Village officials (3)	Informal Leaders (4)
Panel A: Respondent has seen the list				
Public Info	0.14***	0.10***	0.20***	0.14**
	(0.02)	(0.02)	(0.06)	(0.05)
Standard Info	0.02	0.01	0.03	0.02
	(0.01)	(0.01)	(0.06)	(0.05)
Difference:				
Public - Standard	0.11***	0.10***	0.17***	0.12**
	(0.02)	(0.02)	(0.06)	(0.05)
Observations	5,685	3,619	496	385
Control Mean	0.07	0.06	0.36	0.12
Panel B: Respondent believes that stated category of individuals has seen the list				
Public Info	0.35***	0.26***	0.24***	0.24***
	(0.04)	(0.03)	(0.05)	(0.05)
Standard Info	0.07	0.01	0.03	0.06
	(0.04)	(0.03)	(0.05)	(0.04)
Difference:				
Public - Standard	0.28***	0.25***	0.22***	0.18***
	(0.05)	(0.04)	(0.06)	(0.05)
Observations	9,304	9,304	9,304	9,304
Control Mean	0.31	0.15	1.04	0.47

Note: This table provides the reduced form effect of the public information treatments on seeing the eligibility list. In Panel A, the sample is the stated category in the column and the outcome is a dummy indicating whether the individual has seen the eligibility list; "Do not know" answers are coded as zero (not seen). Panel B includes all survey

Impacts

Table 9B: Effect of Public Information on Rice Purchases and Price

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Public Info	0.03	1.54***	-79***	9,081***	-0.07***	0.09	-50*	657
	(0.02)	(0.30)	(21)	(1,665)	(0.03)	(0.23)	(27)	(1,256)
Standard Info	0.01	0.79***	-41*	4,778***	-0.04	0.07	-26	527
	(0.02)	(0.30)	(22)	(1,690)	(0.03)	(0.22)	(25)	(1,222)
Difference:								
Public - Standard	0.01	0.75**	-38*	4,303**	-0.03	0.03	-24	129
	(0.02)	(0.36)	(22)	(1,999)	(0.03)	(0.25)	(25)	(1,338)
Observations	5,685	5,684	4,873	5,684	3,619	3,619	2,283	3,619
Control Mean	0.79	5.29	2,276	28,605	0.63	3.46	2,251	18,754

Note: This table provides the reduced form effect of public information treatment groups on rice purchases, by eligibility status, as compared to the that did not receive a card under the bottom ten treatment are dropped from the sample and we reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month. The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice, whereas the price is calculated among purchasing households. Data are pooled from the first and second follow-up survey. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies and dummy variables for the previous experimental design. We also provide the difference in the two card treatments. Standard errors are clustered by village.

*** p<0.01, ** p<0.05, * p<0.1

Information about prices

- Changing the information on the cards is the cleanest test of information
- Everything held constant except we added a single extra line to the cards with co-pay price information

Impacts of price information

Table 11B: Effect of Printing Price on Cards on Rice Purchases and Price

	Eligible Households				Ineligible Households			
	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)	Bought in the Last 2 Months	Amount Purchased (Kg)	Price (Rp.)	Subsidy (Rp.)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Cards with Price	0.01	1.13***	-55**	6,708***	-0.04	0.12	-37	881
	(0.02)	(0.36)	(25)	(2,056)	(0.03)	(0.26)	(29)	(1,415)
Cards without Price	0.01	0.46	-34	2,935	-0.04	0.08	-7	451
	(0.02)	(0.32)	(24)	(1,797)	(0.03)	(0.25)	(27)	(1,349)
Difference:								
Price - No Price	0.00	0.67*	-21	3,773*	-0.01	0.03	-31	430
	(0.02)	(0.36)	(25)	(2,031)	(0.03)	(0.24)	(25)	(1,279)
Observations	5,688	5,687	4,877	5,687	3,615	3,615	2,281	3,615
Control Group Mean	0.79	5.29	2,276	28,605	0.63	3.46	2,251	18,754

Note: This table provides the reduced form effect of belonging to the Price and No Price treatment groups on rice purchases by eligibility status. Data are pooled from the first and second follow-up survey. Eligible households that did not receive a card under the bottom ten treatment are dropped from the sample and we reweight the treatment groups by sub-district so that the ratio of all three income groups is the same. For each household, the variables for amount purchased, price and subsidy are averages over the past four months; the current month is dropped if the interview occurred before the 25th day of the month. The amount and subsidy are set equal to zero if the household does not purchase any Raskin rice, whereas the price is calculated among purchasing households. Each column in this table comes from a separate OLS regression of respective outcome on the two treatments, sub-district fixed effects, survey sample dummies, dummy variables for the previous experimental design, and a dummy for whether the village was also in the public information treatment. We also provide the difference in the two card treatments. Standard errors are clustered by village. *** p<0.01, ** p<0.05, * p<0.1

- In all card villages, village heads received a letter with the complete list of eligible households, and all lists that were posted publicly had the complete list
- But, the government varied who received the cards
  - In half of villages, cards were mailed to all beneficiaries
  - In the other half of village, cards were mailed only to the bottom 10% of all households (about bottom 1/3 of beneficiaries)
- We can analyze our data separately for these three groups of households – eligible bottom 10, eligible non-bottom 10, and ineligible
- This isolates the role of getting a card per se

Who receives cards

	Subsidy received by….		
	Bottom 10 households	Other eligible households	Ineligible households
	(1)	(2)	(3)
Cards to Bottom 10	4,662**	1,624	691
	(1,911)	(1,783)	(1,338)
Cards to All	4,484**	4,779**	690
	(2,238)	(1,869)	(1,409)
Bottom 10 - All	178	-3155*	1
	(2091)	(1833)	(1257)
Observations	3,682	2,966	3,619
Control Group Mean	29457	27941	18428

14.310x Data Analysis for Social Scientists
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