Ethics

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Abstract

1. Challenges of Conducting RCTs

1.1. Overview

- **Need:**
    1. **Ethical approval** (IRB)
    2. **Selecting implementing partners** and securing funding
    3. Ensuring **high-quality data collection** and minimizing attrition
    4. Conducting rigorous data analysis

1.2. When is IRB Approval Mandatory?

- **Human subjects** involved
- **Personal data** used (e.g. GDPR in Europe)

Must **de-identify** and **securely store data**

2. Ethical Considerations

2.1. Is it Ethical to Randomize?

- **Acceptable if:**
    - **Oversubscription** does not reduce total treated
    - **Insufficient funds** to treat all
    - We do not know if the **treatment is beneficial or neutral** (and we do not know which subgroup will benefit more). (sure non negative :))
    - **Cost-benefit analysis**: cost for the sample <<< benefits for society

- **Unacceptable if:**
    - **High certainty of benefit** to everyone
    - **High certainty of benefit to a specific known group** (we can randomize for other subgroups)

2.2. Belmont Report and US IRBs

- Ethical research in the US is guided by **Belmont Report (1978)**:
    - **Inform participants** about risks/benefits
    - Ensure **voluntary participation** (**informed consent**)
    - **Minimize harm** and weight to benefits
    - **Justify deception** (only if risks are minimal and costs from informing large)

2.3. Informed Consent and Participation Bias

- Only participants who give **informed consent** can be included
- Leads to *participation bias* if participants differ from non-participants
- **ATE estimate conditional on participation**, not generalizable (Internal validity is fine and ATE is causal, but no external validity)
- **Compare baseline traits** of participants vs non-participants.

3. Implementing Partners

- Criteria:
    - Large enough scale
    - Commitment to learning and intervention

    Important to have a member of the research team on the field

3.1. Partner Types

- **Government**: Access to administrative data; BUT beware elections and political cycles + strong political pressures (assess gov program... pressure to give positive results). try ot have indpeendent funding
- **NGOs**: Common in development economics
- **Firms**: Useful in organizational economics (how firms are structured and how these structures affect economic outcomes.)
- **Self-implementation**: Risk of low external validity

3.2. Convincing Partners to Randomize

1. **Oversubscription**: Lottery when D > S
2. **Random Phase-In**: random group receives the treatment in the first year, and in the following years other groups are incorporated (deworming project in primary schools). Issues: i) control group might be affected by expectations of treatment, ii) difficult to measure long term effects
3. **Encouragement Design**: Randomize encouragement (e.g. subsidies). Issue you esitmate ITT, need other assumptions for ATE

4. Common Field Issues

4.1. General Challenges and power calculation issues

Underpowered Studies: when statistical power is low, we cannot claim that the intervention does not work even if we cannot reject that the treatment effect is 0.

Common causes:

- "**Failing in the field**": implementing partner tells you he esepcts 10% effect. you decide given N. tru effect is way smaller (or don't find or need way higher N)
- **Poor take-up** (media campaign may fail)
- **Poor implementation**
- **No power calculations** or under-budgeting
- **Attrition**
- **Ignoring outcome variance**, note that with high variance variables you get lower standardized effect sizes. Decreases when we can control for variables that predict the outcome (e.g., baseline outcome) or have more than one measure of the outcome (less measurement error)
- **study of subgroups reduces the effective sample size** (in very specific subgorup you may have few T and C)
- **Unexpectedly high costs**

4.2. Noncompliance

- Recommended: pilot to assess take-up
- Can restrict randomization to likely compliers, usually these are actually the relevant group (trade-off with external validity)

93 • Tools to increase Take-up are context-specific

4.3. Attrition

95 • **Prevention strategies:**

96 – **Flexible surveys** (time, location), **plan several visits**
97 – **Collect contact info + alternate contacts**
98 – **Provide incentives**
99 – Bounds are going to be uninformative if attrition is very
100 large and differential across treatment arms
101 – If treatment and control groups have different response
102 rates, estimates may be biased. one possible startegy: Com-
103 pare only those in treatment and control who respond after
104 same call effort

4.4. Data Collection and Quality

106 • **Pilot questionnaire (cognitive interviews). Check ques-**
107 **tions are understood, survey is not too long, key ques-**
108 **tions are not at the end, as-much-as-possible homoge-**
109 **neous questionnaire btw treatment and control groups,**
110 **etc.**
111 • **Pilot fieldwork** (first week slow): go slow the first week and
112 see how it goes
113 • **Monitor survey collection**:

114 – **Track missing data**
115 – **Back-check at least 10% of answers**

5. Data Analysis

5.1. Best Practices

118 • **Register** experiment (e.g. AEA registry)
119 • **Pre-analysis plan (PAP)**:

120 – **Define outcomes, model, covariates, regressions,**
121 **transformations**
122 – **Include all planned subgroup analyses**
123 – Reduces flexibility, but boosts credibility

124 • **Data and Code Sharing**

6. Application: Jensen (2012)

6.1. Research Question and Motivation

127 • **Question**: Do labor market opportunities for women affect
128 marriage and fertility decisions?
129 • **Importance**:

130 – Helps explain why women in developing countries leave
131 school early, marry, and have children at young ages.
132 – Sheds light on high fertility rates in low-income settings.

133 • **Expected mechanism**: More job opportunities ⇒ higher op-
134 portunity cost of early marriage/childbearing ⇒ delays in those
135 decisions. But what if high value woman ⇒ more demanded by
136 men?

6.2. Why a Field Experiment is Needed

138 • **Omitted Variable Bias**: Women with job opportunities may
139 also differ in unobservables like wealth, ability, or ambition, all
140 charactetistics affecting marriage decisions fertility.
141 • **Reverse Causality**: Women who plan to delay marriage may
142 also seek more job opportunities. Or women who antiicate mar-
143 riage may be discriminated by employers.
144 • **Location Selection Bias**: Comparing areas with high vs low
145 job access may be biased by correlated area traits (e.g., school
146 quality, income) all factors possiblt related ot marriage adn fer-
147 itly.

6.3. Intervention Design

149 • Context: rural India, very low employment opportunities
150 • **Randomization**: 160 villages randomized 80 to treatment, 80
151 to control.
152 • **No stratification**.
153 • **Treatment**: three years of recruiting services to women in ran-
154 domly selected rural areas to increase awareness of job opportu-
155 nities. Details:

156 – secondary school , english known, computer skills
157 – Basically only young girls (18-24) had those qualifications.
158 Plus,

6.4. Findings

160 • **First Stage**: Women aged 1521 in treated villages were more
161 likely to work in BPO jobs or work at all vs. control.

162 – **Human Capital Investments**:
163 * Increased school enrollment for younger girls.
164 * Higher BMI (indicator of parental investment). they
165 are investing in girls. how do we know they are not
166 simply getting richer? placebo on boys

167 • **Causal effect: Marriage and Fertility**:
168 – Delayed marriage and childbearing

6.5. Empirical Specification

• **Main Specification**:

$$Y_i = \beta_0 + \beta_1 \cdot Treatment_i + \varepsilon_i$$

• **With Controls**:

$$Y_i = \beta_0 + \beta_1 \cdot Treatment_i + \sum_j \gamma_j X_{ij} + \varepsilon_i$$

170 where X includes parental education, household expenditures,
171 family size, and age dummies.
• **Change Specification**: this absorbes time invariant unob-
served heyerogeneity

$$\Delta Y_i = \beta_0 + \beta_1 \cdot Treatment_i + \varepsilon_i$$

172 • cluster at the village level
173 • Controls and change-specifications were moved to the appendi,
174 why? no need in good RCT. they can help in precision.

6.6. Balance Checks and Internal Validity

176 • Table 1: tests for baseline balance between treatment and con-
177 trol.
178 • Report F-test for joint significance of covariates explaining treat-
179 ment assignment: p-value = 0.77 ⇒ no significant imbalance.

6.7. Robustness

181 • Nice placebo test: no treatment effect for men or older women.

6.8. Threats to Internal Validity
6.8.1. Attrition

184 • Similar attrition rates across treatment and control
185 • Check baseline characteristcs of attrictos: Attrition mostly
186 driven by migration of younger, poorer, landless households.
187 • **Interact baseline covariates with treatment to test if in-**
188 **teractions jointly predict attrition: suggest aattrition is**
189 **differential and correlated with treatment**!
190 • Use of IPW confirms robustness

6.8.2. Partial Compliance

- Treatment defined as exposure to village-level intervention. Randomization was at village level.
- All treated villages received the recruiter visit. SO there was no partical compliance
- No control villages were visited. SO there was no partical compliance

6.8.3. Externalities

- Where to look: Since the unit of randomization is village, spillovers across villages (e.g., control village individuals learning about BPO opportunities from treated village individuals) violate the Stable Unit Treatment Value Assumption (SUTVA). Treatment is assigned at the village level: Entire villages were randomized into treatment or control. All women in treated villages are considered treated, regardless of whether they attended info sessions.
- Spillovers: refer to control individuals (in control villages) possibly being influenced by treated neighbors (e.g., via word-of-mouth or attending nearby sessions).
- Would expect downward bias.
- ery few control group women worked in BPO.
- In such setting, it is interesting to check spilloevrs at the village level also for the marriage market. if marriage marke tis not village level bu broader: women from T village don't marry, Men in T billage loook for women in C village

6.9. Alternative Mechanisms

- Carefully tested other channels:
  - Not driven by 1) household income effects or 2) time reallocation by adults (1 The intervention did not increase total household expenditure, indicating no significant household income gain, 2) adutls work more or less and affect girls (role model or opposite: somebody needs to take care of the hosue)).
  - Cannot fully rule out effects via teachers (e.g., more encouragement to girls); what if treatment lead teachers to incentivise girl to 1) study to work and 2) postpone marrigae
- what other mechanisms could explain the observed results? Nicolò: decline in fertility is a mechanical effect from elevating community and working more

6.10. External Validity

- Findings apply mainly to white-collar BPO jobs: safe, non-manual, socially acceptable.
- We are in India
- However, the role of information about job availability can matter in many contexts.