

14.75: Collective Action Lecture 2

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- Collective action failures stem from misalignment of private and collective incentives (e.g., Olson)
- In the developing world, one way this manifests itself is insufficient monitoring of local officials
 - Teachers and health workers not coming to work
 - Local officials stealing funds from central government projects
 - (much more to come on these issues in the corruption lectures)
- So many suggest that a natural solution to this problem is to increase the ability of citizens to monitor local officials
- In fact, this is precisely what the World Bank suggested in the 2004 *World Development Report*:
 - *“Putting poor people at the center of service provision: enabling them to monitor and discipline service providers, amplifying their voice in policymaking, and strengthening the incentives for service providers to serve the poor.”*

- This lecture: three randomized experiments that sought to increase community-based monitoring of service providers in three different settings – with three very different sets of results
 - Banerjee et al. (2008): education in India – no impact.
 - Björkman and Svensson (2009): health in Uganda – massive impacts.
 - Olken (2007): corruption in road building in Indonesia – impacts only in some circumstances (no free riding, limited elite capture)
- What's going on?

Theoretical background

- Suppose we're in a world of moral hazard:
 - Bureaucrat can exert effort $e \in [0, 1]$ to produce a good, with convex cost of effort $\frac{1}{2}ce^2$. Effort is unobservable.
 - Probability public good is produced is e . Each citizen gets utility $\frac{1}{N}$ if good produced, 0 otherwise.
- Two components of monitoring:
 - One citizen is designated "monitor"
 - The monitor can pay a personal cost $\frac{1}{2}\alpha m^2$ to try to observe whether the public good was not produced. Observe with probability m .
 - If he observes the public good was not produced, he can pay a cost s to share the information with everyone else
 - If he observes that the good was not produced and shares the information with the community, bureaucrat receives punishment p .
- Timing:
 - Monitor announces monitoring plan m
 - Bureaucrat chooses effort
 - Payoffs realized

- Bureaucrat solves

$$\max_e -p(1-e)m - \frac{1}{2}ce^2$$

so

$$e = \frac{pm}{c}$$

- Monitor solves

$$\max_{m_i} \frac{e}{N} - \frac{1}{2}\alpha m^2 - ms(1-e)$$

i.e.

$$\max_{m_i} \frac{pm}{Nc} - \frac{1}{2}\alpha m^2 - ms\left(1 - \frac{pm}{c}\right)$$

so the FOC implies

$$m = \frac{p - csN}{Nc\alpha - 2psN}$$

- Equilibrium is

$$m = \frac{p - csN}{Nc\alpha - 2psN}$$
$$e = \frac{p}{c} \frac{p - csN}{Nc\alpha - 2psN}$$

- Comparative statics:
 - Reducing the cost of monitoring (α) increases monitoring and effort
 - Reducing the cost of sharing information (s) increases monitoring and effort
 - Increasing N lowers monitoring and effort (free rider problem)
- What would happen if bureaucrat could make ex-post side payments to the monitor (elite capture)?

Education in India

Banerjee, Banerji, Duflo, Glennerster, and Khemani (2008): "Pitfalls of Participatory Programs: Evidence from a Randomized Evaluation in India"

- Setting: education in Uttar Pradesh, India
- Baseline situation: substantial problems with teacher absence and teacher laziness, and 39 percent of children age 7-14 could not read and understand a simple (grade 1 level) story
- Scope for collective action: each school has a Village Education Committee (VEC)
 - Consists of three parents, the head teacher, and the head of village government
 - Charged with intermediating between village government and bureaucracy, monitoring performance of schools, and controlling some share of the school budget (e.g., community-based teachers, supplemental allowances)
- But VECs are generally ineffectual:
 - At baseline, most parents did not know the VEC existed
 - Many VEC members did not know their responsibilities

- Treatment 1 (monitoring):
 - Facilitated small group discussions in each hamlet of the village to talk about education
 - Facilitated village-wide meeting to talk about education, providing details about the VEC and the role of it plays. Meeting included villagers, teachers, and village officials
 - Facilitators followed up by visiting each VEC member, gave them a pamphlet on VEC roles and responsibilities, and discussed VEC with them
- Treatment 2 (monitoring + information): Treatment 1 + plus reading report card
 - Villagers taught how to test kids reading levels
 - In each hamlet, villagers tested kids and prepared a report card

Interventions

- Treatment 3 (monitoring + information + remediation): Treatment 1 + treatment 2 +
 - Village volunteers given 4 trainings in how to teach kids to read
 - Volunteers receive about 7 visits per year from NGO to support the activity
- What does Treatment 3 test? Why do it?

Experimental Design

- Experimental design: 280 villages randomly allocated into 4 groups (65 in each treatment and 85 in control):
 - Treatment 1: facilitated discussions
 - Treatment 2: facilitated discussions + village monitoring tool
 - Treatment 3: facilitated discussions + village monitoring tool + village reading tool
- Are these the right interventions? What else might you have wanted to do?
- Why more villages in control group?

Multiple outcomes

- They examine about 70 different outcome variables
- What's the problem?
- What are solutions?
- Their solution (following Katz, Kling, Liebman 2007):
 - Group indicators into "families" of similar indicators k
 - Regression specification for each family of indicators k :

$$y_{ijk} = \alpha + \beta_{1k} T_1 + \beta_{2k} T_2 + \beta_{3k} T_3 + X\gamma_k + \varepsilon_{ijk}$$

- Compute the average standardized effect

$$\widehat{\beta}_t = \frac{1}{k} \sum_{k=1}^K \frac{\widehat{\beta}_{tk}}{\widehat{\sigma}_{tk}}$$

Results

"First stage": VEC knew more but did little more

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=a U[ Yg:fYa cj YX'Xi Y'hc 'Vednf][ \hfYgfh]M]cbg" GYY. '6UbYf'YYz'5V\.]q'z'Fi _a ]b]'6UbYf'jz'YhU""'D]hZU"gcZDUfh]WdUhc'fm  
Dfc[ fUa g.'9j ]XYbW'Z'ca 'U'FUbXca ]nYX'9j Ui Uh]cb ]b'9Xi WUh]cb ]b'='bX]U""'B69F 'K cf_]b[ 'DUdYf'Bc"%' '%f&$$, £"
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Health in Uganda

Björkman and Svensson 2009: "Power to the People: Evidence from a Randomized Field Experiment on Community-Based Monitoring in Uganda"

- Setting: 50 health centers ("dispensaries") in rural Uganda
- Each dispensary provides preventive care, outpatient care, maternity, lab services to a population of about 2,500 households
- Situation is similar to the Indian education context in Banerjee et al. in many ways:
 - Many problems at baseline – stockout rate of 50% of basic drugs, only 41% use any equipment at all during examinations
 - Scope for collective action through Health Unit Management Committee (HUMC), which consists of health workers and non-political representatives of community. Supposed to monitor but does not have hiring/firing power. Very similar to VECs.

Intervention

- Single intervention with two goals: increasing information about health problems and service delivery failures and strengthening citizen monitoring
- Specifics of intervention
 - Conduct baseline survey of health problems and quality of services
 - Create facility-specific report card of service delivery, including comparison to other facilities
 - Use community-based organizations to hold facilitated meetings with:
 - Community. Two-day event, including about 150 people. Discussed patient's rights, how to improve service delivery, etc. Culminated in "action plan" of improvements.
 - Health providers. One-afternoon with all staff. Discussed report card findings.
 - "Interface meeting" of both. Discuss results of two meetings and wrote a "community contract", which included promised changes in service and a plan for community monitoring.
 - Follow-up meeting six months later by community-based organization.
 - How is this comparable to the Indian experiment? How different?

Experimental design

- 50 dispensaries, randomized into 2 groups of 25
- Estimate effects as

$$y_{ijd} = \alpha + \beta T_{jd} + X_{jd}\pi + \theta_d + \varepsilon_{ijd}$$

where X are pre-intervention facility covariates and θ_d are district fixed effects

- For variables with pre-data, they can also estimate

$$y_{ijd} = \gamma POST_t + \beta_{DD} T_{jd} * POST_t + \mu_j + \varepsilon_{ijd}$$

How is this different from the Banerjee et al. specification?

Results

Results on Service Quality

=a U[Yg fYa cj YX Xi Y hc Vēdnf][\hfYgrf]Wicbg" GYY. '6^ f_a Ubz A UfhjbUz UbX'>U_cV'Gj Ybggcb""Dck Yf hc h\Y
DYcd'Y 9j]XYbW Zfca 'U'FUbXca]nYX :]Y'X'9l dYf]a Ybh'cb'7ca a i b]m6UgYX'A cb]rcf]b[]b'l [UbXU""
Ei UfhYf'm>ci fbU'cZ9Vēbca]Vg%&('bc"& f&\$\$- L '+')! * -"
HUV'Y' =Dfc[fUa 'a dUVh'cb HFYUha Ybh DfUVh]Wg UbX'A UbU[Ya Ybh
HUV'Y' =Dfc[fUa 'a dUVh'cb =a a i b]nUh'cb
HUV'Y' J'Dfc[fUa 'a dUVh'cb'l h]nUh'cb#7cj YfU[Y
HUV'Y' J=Dfc[fUa 'a dUVh'cb<YU'h' Ci H'ēa Yg

Reconciling with India?

- How do we reconcile this with the India results?
 - What differences in the treatment might be important?
 - What differences in the setting might be important?

Road Building in Indonesia

Olken 2007: "Monitoring Corruption: Evidence from a Field Experiment in Indonesia"

- Setting:
 - 608 villages in rural Indonesia, each of which was building a 1-3km road
 - Roads are built by a 3-person village implementation committee
 - Three village-wide "accountability meetings" where the committee has to account for how they spent the funds, after 40%, 80%, and 100% of funds allocated.
- Scope for improvement:
 - Like India and Uganda, these meetings do not look very effective: village head typically only invites the elite, and they almost always approve the accountability report
 - Baseline estimates: 25% of funds can't be accounted for, so potentially pervasive corruption
- Question: does improving the functioning of these monitoring meetings reduce corruption in the project?
- Note: the same project also investigated top-down audits: we will discuss more in the corruption lectures

- Invitations
 - Idea: number and composition of people at meeting affects information, bias
 - Intervention: distribute hundreds of written invitations 3-5 days before meeting to lower cost of attending, to reduce elite dominance and increase participation at meetings
- Comment Forms
 - Idea: anonymity reduces private cost of revealing corruption
 - Intervention: invitations + distributed anonymous comment forms
 - Forms have questions on information, road quality, prices, financial management, plus open-ended questions
 - Collect forms 1-2 days before meeting in sealed drop-boxes, and read summary of comments at meeting
- Sub-variants of both treatments:
 - Number: 300 or 500 invitations
 - Insiders: Distribute invitations via village government or primary schools

Experimental design

- What would you do differently? Does this get at the questions you'd want to answer?
- 608 villages randomly allocated into:
 - Invitations
 - Invitations + Comments
 - Control
- Within invitations and invitations + comments, villages randomly allocated into:
 - 300 or 500 invitations
 - Distribute invitations via village government or primary schools
- Orthogonal randomization into audits or control, by subdistrict
- Regression:

$$y_{id} = \alpha_d + INVITE_{id} + COMMENT_{id} + \varepsilon_{id}$$

Measuring Corruption

- Goal
 - Measure the difference between *reported expenditures* and *actual expenditures*
- Measuring reported expenditures
 - Obtain line-item reported expenditures from village books and financial reports
- Measuring actual expenditures
 - Take core samples to measure quantity of materials
 - Survey suppliers in nearby villages to obtain prices
 - Interview villagers to determine wages paid and tasks done by voluntary labor
- Measurement conducted in treatment and control villages

Measuring Corruption

- Measure of theft:

$$THEFT_i = \text{Log}(\text{Reported}_i) - \text{Log}(\text{Actual}_i)$$

- Can compute item-by-item, split into prices and quantities
- Assumptions
 - Loss Ratios - Material lost during construction or not all measured in survey
 - Worker Capacity - How many man-days to accomplish given quantity of work
 - Calibrated by building four small (60m) roads ourselves, measuring inputs, and then applying survey techniques
- All assumptions are constant – affect levels of theft but should not affect differences in theft across villages

Results

First stage: attendance at meetings

TABLE 9
PARTICIPATION: FIRST STAGE

	Attendance (1)	Attendance of Nonelite (2)	Number Who Talk (3)	Number Nonelite Who Talk (4)
Invitations	14.83*** (1.35)	13.47*** (1.25)	.743*** (.188)	.286*** (.079)
Invitations plus comments	11.48*** (1.35)	10.28*** (1.27)	.498*** (.167)	.221*** (.069)
Meeting 2	-5.32*** (1.11)	-4.00*** (1.06)	.163 (.155)	.024 (.084)
Meeting 3	-4.29*** (1.20)	-5.78*** (1.13)	.431** (.172)	-.158* (.089)
Stratum fixed effects	Yes	Yes	Yes	Yes
Observations	1,775	1,775	1,775	1,775
R^2	.39	.38	.47	.28
Mean dependent variable	47.99	24.15	8.02	.94
p -value invitations = invitations + comment forms	.03	.03	.21	.43

Results

Discussions at meetings

TABLE 10
PARTICIPATION: IMPACT ON MEETINGS

	Number of Problems (1)	Any Corruption- Related Problem (2)	Serious Response Taken (3)
Invitations	.072 (.063)	.027** (.013)	-.003 (.008)
Invitations plus comments	.104 (.064)	.026** (.012)	.015** (.008)
Meeting 2	-.187*** (.066)	.002 (.013)	-.020** (.009)
Meeting 3	-.428*** (.074)	-.036*** (.012)	-.029*** (.009)
Stratum fixed effects	Yes	Yes	Yes
Observations	1,783	1,783	1,783
R^2	.50	.31	.22
Mean dependent variable	1.18	.07	.03
p -value invitations = invitations + comment forms	.60	.96	.02

TABLE 11
PARTICIPATION: MAIN THEFT RESULTS

PERCENT MISSING ^a			NO FIXED EFFECTS		ENGINEER FIXED EFFECTS		STRATUM FIXED EFFECTS	
	CONTROL MEAN (1)	TREATMENT MEAN (2)	Treatment Effect (3)	p-Value (4)	Treatment Effect (5)	p-Value (6)	Treatment Effect (7)	p-Value (8)
A. Invitations								
Major items in roads (<i>N</i> = 477)	.252 (.033)	.230 (.033)	-.021 (.035)	.556	-.030 (.034)	.385	-.026 (.034)	.448
Major items in roads and ancillary projects (<i>N</i> = 538)	.268 (.031)	.236 (.031)	-.030 (.032)	.360	-.032 (.032)	.319	-.029 (.032)	.356
Breakdown of roads:								
Materials (<i>N</i> = 477)	.209 (.041)	.221 (.041)	.014 (.038)	.725	.008 (.037)	.839	.005 (.037)	.882
Unskilled labor (<i>N</i> = 426)	.369 (.077)	.180 (.077)	-.187* (.098)	.058	-.215** (.094)	.024	-.143* (.086)	.098

Results

Corruption

	B. Invitations Plus Comments							
Major items in roads ($N = 477$)	.252 (.033)	.228 (.026)	-.022 (.030)	.455	-.024 (.029)	.411 (.030)	-.015 (.030)	.601
Major items in roads and ancillary projects ($N = 538$)	.268 (.031)	.238 (.026)	-.026 (.032)	.409	-.025 (.030)	.406 (.031)	-.027 (.031)	.385
Breakdown of roads:								
Materials ($N = 477$)	.209 (.041)	.180 (.032)	-.028 (.034)	.414	-.022 (.032)	.496 (.033)	-.010 (.033)	.754
Unskilled labor ($N = 426$)	.369 (.077)	.267 (.073)	-.099 (.087)	.255	-.132 (.087)	.131 (.091)	-.090 (.091)	.323

TABLE 12
INTERACTIONS OF PARTICIPATION EXPERIMENTS WITH HOW INVITATIONS WERE DISTRIBUTED

PERCENT MISSING ^a	CONTROL MEAN (1)	TREATMENT MEAN (2)	NO FIXED EFFECTS		ENGINEER FIXED EFFECTS		STRATUM FIXED EFFECTS	
			Treatment Effect (3)	p-Value (4)	Treatment Effect (5)	p-Value (6)	Treatment Effect (7)	p-Value (8)
A. Invitations								
Invitations Distributed via Neighborhood Heads								
Major items in roads (N = 246)	.252 (.033)	.222 (.044)	-.030 (.042)	.469	-.043 (.039)	.274	-.042 (.043)	.324
Major items in roads and ancillary projects (N = 271)	.268 (.031)	.255 (.045)	-.013 (.043)	.761	-.015 (.041)	.712	-.004 (.043)	.924
Invitations Distributed via Schools								
Major items in roads (N = 233)	.252 (.033)	.239 (.046)	-.009 (.050)	.854	-.014 (.048)	.774	-.003 (.045)	.950
Major items in roads and ancillary projects (N = 263)	.268 (.031)	.216 (.040)	-.048 (.044)	.282	-.051 (.043)	.245	-.056 (.039)	.155

Results

Interactions with elite

	B. Invitations Plus Comments							
	Invitations Plus Comment Forms Distributed via Neighborhood Heads							
Major items in roads (<i>N</i> = 242)	.252 (.033)	.278 (.036)	.025 (.036)	.483	.038 (.036)	.294	.022 (.041)	.602
Major items in roads and ancillary projects (<i>N</i> = 271)	.268 (.031)	.277 (.039)	.010 (.039)	.792	.024 (.038)	.535	.023 (.040)	.569
	Invitations Plus Comment Forms Distributed via Schools							
Major items in roads (<i>N</i> = 242)	.252 (.033)	.179 (.036)	-.070* (.041)	.093	-.086** (.038)	.023	-.052 (.036)	.150
Major items in roads and ancillary projects (<i>N</i> = 267)	.268 (.031)	.198 (.034)	-.064 (.042)	.127	-.077* (.039)	.052	-.078* (.041)	.056

- Summary of results
 - Interventions affected the process at meetings
 - But effects were too small to matter overall – if taking a "serious action" eliminated corruption entirely, impact of comment forms would be to reduce missing expenditures by 0.68 percentage points
- But important heterogeneity suggests that details matter for combating free riding and elite capture
 - Invitations reduced theft of labor, and laborers are the ones with high personal returns to reducing corruption
 - Comment forms worked only if distributed via schools where elite capture was lower (in fact comment forms were more negative, but corruption was lower!)
- Does this help us reconcile India vs. Uganda? What would?

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14.75 Political Economy and Economic Development

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