14.770: Collective Action

Ben Olken

Overview

- 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."*

These lectures

- Olson, the group size paradox, and heterogeneity
- Social capital
  - Where does it come from?
  - Does social capital help solve collective action problems?
- External attempts to improve collective action
  - Can stimulating collective action improve service delivery in developing countries? Why or why not?
  - Can it change institutions?
- Decentralization and local capture

Collective action and group size

Banerjee, Iyer, and Somanathan (2007)

- Olson (1965): "the larger the group, the less it will be able to favor its common interests."
- Let
$$f(\sum^{n} a_i) = [\sum^{n} a_i]^{\alpha}, 0 < \alpha < 1$$
be the probability that a particular collective effort succeeds. a_i is the effort of group member i, and assume that there are n group members.
- Let everyone benefit an amount b from the success of the effort.
- Let the cost of the effort be $v(a) = a^{\beta}, \beta > 1$.
- Then a group member will maximize
$$b[\sum^{n} a_i]^{\alpha} \quad a_i^{\beta}$$
- Then a_i will satisfy
$$\alpha b[\sum^{n} a_i]^{\alpha \quad 1} = \beta a_i^{\beta \quad 1}$$

Collective action and group size

- So in equilibrium

$$\begin{aligned} \alpha b[na]^{\alpha \quad 1} &= \beta a^{\beta \quad 1} \\ \alpha b &= \beta n^{1 \quad \alpha} a^{\beta \quad \alpha} \\ \alpha n^{\beta \quad 1} b &= \beta (A^e)^{\beta \quad \alpha} \end{aligned}$$

  Denoting by $A^e = na$, the total equilibrium collective effort, we see that A is increasing in n.

- The socially optimal choice of effort maximizes

$$nb[\sum^n a_i]^\alpha \quad \sum^n a_i^\beta$$

  which tells us that

$$n\alpha b[na]^{\alpha \quad 1} = \beta a^{\beta \quad 1}$$

  Hence the optimal social effort A^o satisfies

$$\alpha n^\beta b = \beta (A^o)^{\beta \quad \alpha}$$

Collective action and group size

- Recall
$$\alpha n^{\beta \quad 1} b = \beta (A^{e})^{\beta \quad \alpha}$$
- and
$$\alpha n^{\beta} b = \beta (A^{o})^{\beta \quad \alpha}$$
- Which implies
$$\frac{1}{n} = \left(\frac{A^{e}}{A^{o}} \right)^{\beta \quad \alpha}$$
- Hence A^{e}/A^{o} goes to zero as n goes to infinity.

Implications

- Collective action is harder in larger groups because the misalignment of private and social incentives is larger.
- Olson goes on from here to argue that this is why small interest groups tend to get their own way: they are better at collectively articulating their demands
- In this model however A^e is always increasing in n.
- To get at that possibility we need to bring in the idea that smaller groups have higher stakes per capita.
- In other words we now introduce the idea that there is some private component in the returns from collective action.

Adding crowd-out

- A group member will now maximize

$$(b + \frac{w}{n})[\sum^{n} a_i]^{\alpha} \qquad a_i^{\beta}$$

- So in equilibrium

$$\alpha(b + \frac{w}{n})[na]^{\alpha \quad 1} = \beta a^{\beta \quad 1}$$

and

$$\alpha(b + \frac{w}{n})[n]^{\beta \quad 1} = \beta(A^{e})^{\beta \quad \alpha}$$

- Clearly increasing n has two effects and the result can go either way
    - (e.g., $b = 0$ and $\beta < 2$ reverses the previous result)
- Intuitively there is more of a free rider problem in big groups but the bigger group has to put in less effort per capita to get to the same total effort.
- Esteban and Ray provide exact conditions in a setting where they also take into account that the groups are competing against each other.

Collective action and heterogeneity

- Folk wisdom is that it is harder to have collective action in heterogenous groups.
- Suppose there are m groups each of size n_j. $mn_j = n$
- Assume that once again the public good has a public component and a private component, where private means that some group captures it.
- The probability of it being captured by group J conditional on the public good being built is
$$\frac{\sum\limits_{i \in J} a_i}{\sum a_i}$$
- The payoff function is then
$$\left(b + w\frac{\sum\limits_{i \in J} a_i}{\sum a_i}\right)\left[\sum a_i\right]^{\alpha} \quad a_i^{\beta}$$

- At the optimum we will have

$$\begin{aligned} & \alpha b[\sum a_i]^{\alpha \;\; 1} + [\sum a_i]^{\alpha \;\; 1} w \\ & \quad (1 \quad \alpha)\{\sum_{i \in J} a_i\}[\sum a_i]^{\alpha \;\; 2} w \\ = & \quad \beta a_i^{\beta \;\; 1} \end{aligned}$$

or

$$\begin{aligned} & \alpha b A^{\alpha \;\; 1} + A^{\alpha \;\; 1} w \quad (1 \quad \alpha)\frac{A}{m}[A]^{\alpha \;\; 2} w \\ = & \quad \beta a_i^{\beta \;\; 1} \end{aligned}$$

or

$$\begin{aligned} & \alpha b A^{\alpha \;\; 1} + A^{\alpha \;\; 1} w \quad (1 \quad \alpha)\frac{1}{m}[A]^{\alpha \;\; 1} w \\ = & \quad \beta (A/n)^{\beta \;\; 1} \end{aligned}$$

Collective action and heterogeneity

- Recall that keeping n fixed, increasing m increases heterogeneity.
- We just showed that

$$
\begin{aligned}
& \alpha b A^{\alpha \quad 1} + A^{\alpha \quad 1} w \quad (1 \quad \alpha)\frac{1}{m}[A]^{\alpha \quad 1} w \\
= \quad & \beta (A/n)^{\beta \quad 1}
\end{aligned}
$$

- This shows that increasing m (i.e., increasing heterogeneity) increases A. Heterogeneity helps! Intuition?
- Would also work if we set it up such that a group member would maximize

$$
(b + \frac{w}{n^g})[\sum^n a_i]^\alpha \quad a_i^\beta .
$$

- So it is not the structure of intergroup competition that drives the result

Collective action and heterogeneity

- Intuition:
  - The Olson effect operates here as well. Group size in this framework matters only because your incentive to put in effort depend in part on what is happening in your group and bigger groups discourage effort.
  - So having more smaller groups increases effort.
  - In order to capture the intuition that heterogeneity hurts, we need to look for a context where the free-rider problem is not the big problem.
  - Instead, we'll look at a context where the problem is heterogeneity in tastes

Collective action and heterogeneity

Alesina, Baqir, and Easterly (1999): Public Goods and Ethnic Divisions

- Key distinction between this model and the previous model: now there is a *type* of public good, not just an amount of public good
- Individual i utility function given by

$$u_i = g^{\alpha} (1 \quad l_i) + y \quad t$$

  where g is amount of public good, and l_i is distance between individual's most preferred type of public good and the actual type of public good, y is income, and t is lump-sum taxes used to finance the public good. Assume $0 < \alpha < 1$.
- Normalize population size to one, so $g = 1$ Rewrite utility as

$$u_i = g^{\alpha} (1 \quad l_i) + y \quad g$$

- Assume voters vote first on size of public good, and then vote on the type of the public good. In the second stage, type of good is the one preferred by the median voter.

Collective action and heterogeneity

- How does this affect amount of public good?
- Individual i solves

$$\max g^{\alpha} \;\; 1 \quad \hat{l}_i \;\; + y \quad g$$

where $\hat{l}_i$ is the distance of individual i from the ideal type of the median voter. Solution is

$$g_i^* = \left[\alpha \;\; 1 \quad \hat{l}_i)\right]^{\frac{1}{1 \;\; \alpha}}$$

- Define $\hat{l}_i^m$ as the median distance from the type most preferred by median voter. ("median distance from the median").
- Then amount of public good is given by

$$g_i^* = \left[\alpha \;\; 1 \quad \hat{l}_i^m)\right]^{\frac{1}{1 \;\; \alpha}}$$

- This implies that equilibrium amount of public good is decreasing in $\hat{l}_i^m$.
- Polarization increases this distance.

Illustration

Low heterogeneity

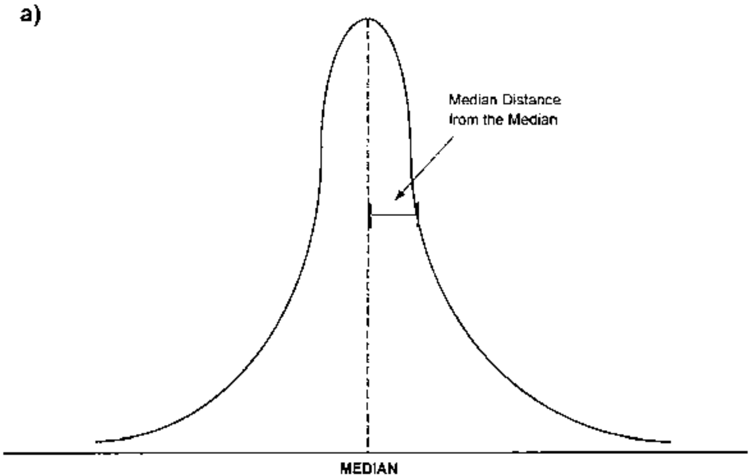
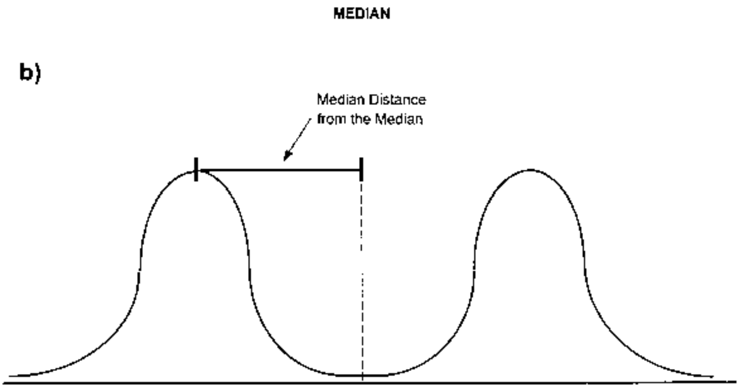


Illustration
High heterogeneity



Evidence

Alesina, Baqir, and Hoxby 2004: "Political Jurisdictions in Heterogeneous Communities"

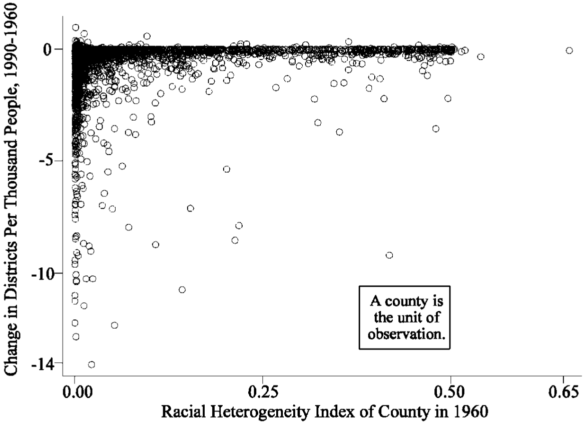
- Setting: US school districts
- Idea: political jurisdictions are formed from a trade-off of economies of scale and homogeneity.
- So number of school districts in a county is:
  - Increasing in county size
  - Increasing in fixed costs measures
  - Decreasing in heterogeneity

OLS results with state fixed effects

	Population Heterogeneity Variables Based on							
	Entire Population				School-Aged Children			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Racial heterogeneity	.288	.279	.280	.284	.260	.228	.216	.204
	(.096)	(.096)	(.100)	(.102)	(.085)	(.089)	(.087)	(.091)
White ethnic heterogeneity		.433		.271		.144		.046
		(.163)		(.163)		(.136)		(.136)
Hispanic ethnic heterogeneity		.065		.053		.015		.010
		(.062)		(.062)		(.056)		(.055)
Gini coefficient household income	1.500	1.369	1.434	1.242	1.511	1.322	1.500	1.284
	(.601)	(.612)	(.600)	(.611)	(.600)	(.624)	(.598)	(.624)
Religious heterogeneity	.032	.041	.024	.009	.036	.054	.015	.065
	(.086)	(.089)	(.086)	(.088)	(.086)	(.092)	(.086)	(.091)
ln(mean household income)	.338	.295	.246	.240	.322	.266	.249	.204
	(.104)	(.105)	(.129)	(.131)	(.104)	(.108)	(.130)	(.136)

Panel data

- Idea: there has been district consolidation. But heterogeneity may prevent it.



Panel data

	Regression			
	(1)	(2)	(3)	(4)
Change in racial heterogeneity	.931	.892	.908	.880
	(.166)	(.164)	(.167)	(.165)
Change in white ethnic heterogeneity		.410		.370
		(.048)		(.049)
Change in Hispanic ethnic heterogeneity		.172		.101
		(.072)		(.071)
Change in Gini coefficient household income	3.269	3.499	1.042	1.401
	(.566)	(.563)	(.605)	(.609)
Change in religious heterogeneity	.359	.137	.258	.066
	(.115)	(.115)	(.114)	(.114)
Change in ln(mean household income)	1.132	.939	1.346	1.177
	(.077)	(.079)	(.087)	(.089)
Change in percentage of adults with at least high school	.021	.021	.027	.026
	(.003)	(.003)	(.003)	(.003)
Change in percentage of population aged 65 or older	.004	.001	.004	.007
	(.005)	(.005)	(.005)	(.005)
20 variables that describe change in population and pattern of population density				
Change in industry share variables				
Observations	2,718	2,670	2,718	2,670

Evidence from Kenya

Miguel and Gugerty (2005): Ethnic diversity, social sanctions, and public goods in Kenya

- Setting: school funding and facilities in rural Kenya
- Slightly different theoretical motivation:
  - They posit no preference heterogeneity over these types of goods
  - Instead, they think about voluntary contributions (not compulsory taxes), with social sanctions for non-payment
  - Assume no ability to impose social sanctions across ethnic groups
- Empirical approach:
  - Low residential mobility implies that ethnic heterogeneity is exogenously determined with respect to public goods provision (e.g., no Tiebout sorting)
  - Compare contributions cross-sectionally

Results

Explanatory variable	Dependent variable								
	School ELF across tribes	Total local primary school funds collected per pupil in 1995 (Kenyan Shillings)							
	(1) OLS 1st stage	(2) OLS	(3) OLS	(4) IV-2sls	(5) OLS	(6) OLS	(7) OLS	(8) Spatial OLS	(9) Spatial OLS
Ethnic diversity measures									
Zonal ELF across tribes	0.86***		185.7**		145.2***	143.6*			
	(0.07)		(77.9)		(49.6)	(82.1)			
School ELF across tribes		32.9		216.4**					
		(64.0)		(88.4)					
1-(Proportion largest ethnic group in zone)							162.9**		
							(66.6)		
ELF across tribes for all schools within 5 km								174.0**	174.0**
								(76.3)	(80.8)
Zonal controls									
Proportion fathers with formal employment					189.5	220.6*	184.6		142.8
					(165.1)	(120.5)	(170.9)		(167.3)
Proportion of pupils with a latrine at home					431.6***	286.3	429.8***		466.9
					(139.9)	(228.0)	(150.3)		(250.2)
Proportion livestock ownership					120.1	186.2	110.6		116.9
					(136.9)	(130.4)	(148.3)		(117.7)
Proportion cultivates cash crop					35.7	22.2	27.8		85.2
					(61.4)	(106.9)	(62.4)		(78.4)
Proportion Teso pupils						67.9			
						(181.4)			
Geographic division indicators	No	No	No	No	No	Yes	No	No	No
Root MSE	0.14	99.8	96.7	105.5	95.0	93.0	95.4	97.1	95.0
R^2	0.40	0.00	0.06	–	0.14	0.25	0.12	0.06	0.09
Number of schools	84	84	84	84	84	84	84	84	84
Mean dependent variable	0.20	152.6	152.6	152.6	152.6	152.6	152.6	152.6	152.6

Does "social capital" matter?

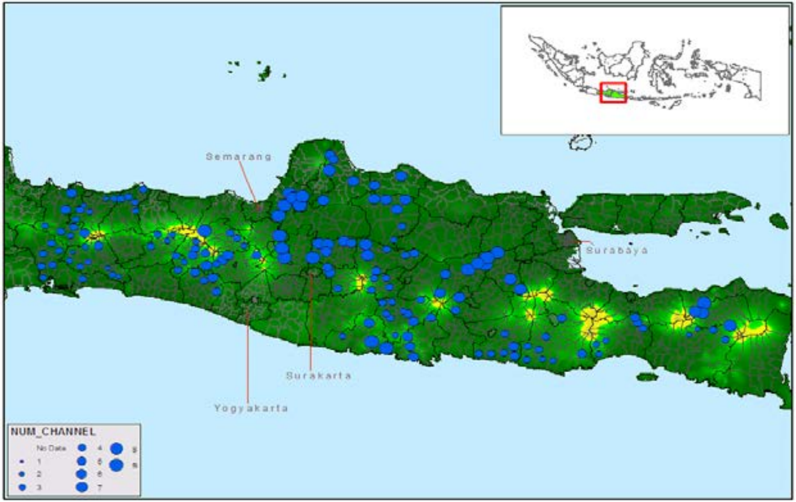
- Miguel and Gugerty paper suggested that contributions are enforced through "social sanctions"
- This is connected to a broader idea, that "social capital" is an important supporter of collective action
  - E.g., Putnam – "Making Democracy Work" and "Bowling Alone"
  - Could be because people trust each other (with trust enforced through links on social network)
  - Could be because social links are a way to exclude people who fail to participate
- Examples of models of this: Ambrus et al (2014) AER show that informal insurance is easier to sustain if groups are more interlinked

Testing social capital's impact using TV

Olken (2009): Does TV and Radio Destroy Social Capital?

- Setting: Examines the impact of television (and radio) on social capital in over 600 Indonesian villages
- Main source of identification: plausibly exogenous variation in signal strength associated with the mountainous terrain of East / Central Java
- Additional sources of identification:
  - Compare social capital in subdistricts before and after introduction of private television in 1993
  - Use model of electromagnetic signal propagation to explicitly isolate impact of topography
- Then: to see if it matters, examine the impact of television reception on corruption in road projects

Map: Variation in television reception



Setting

- Indonesian villages have extremely dense social networks
  - Typical Javanese village of 2,600 adults has 179 groups of various types
  - Types of groups: Neighborhood associations, religious study groups, ROSCAs, health and women's groups, volunteer work
- Television and radio
  - 80 percent of rural households watch TV per week in 2003
  - 11 national TV stations, showing mix of news, soap operas, movies, etc
  - Broadcasting centered around major cities
  - But prior to 1991, only 1 TV channel (gov't channel)
  - Will not separately identify TV and radio as I don't have independent data on radio, and they are likely co-linear in any case

Does better reception translate into increased use?

- Show that in Central / East Java sample, television reception is orthogonal to a large number of village characteristics
- Estimate impact of channels on use at individual level with data from East / Central Java survey:

$$\begin{aligned} MINUTES_{hvsd} &= \alpha_d + NUMCHAN_{sd} \\ &\quad + Y_{hvsd}\gamma + X_{vsd}\delta_1 + \delta_2 ELEVATION_{sd} + \varepsilon_{hvsd} \end{aligned}$$

where:

  - $MINUTES_{hvsd}$ is number of minutes respondent spends watching TV or listening to radio
  - Y_{hvsd} are respondent covariates (gender, predicted per-cap expenditure, has electricity)
  - all specifications include district FE α_d
  - standard errors clustered by subdistrict

Does better reception translate into increased use?

	Individual-level data (Java survey)			
	Total minutes per day (1)	TV minutes per day (2)	Radio minutes per day (3)	Own TV (4)
Number of TV channels	14.243***	6.948***	6.997***	−0.007
	(2.956)	(1.827)	(1.881)	(0.008)
Observations	4,213	4,250	4,222	4,266
R^2	0.18	0.16	0.10	0.17
Mean dep. var.	180.15	124.54	55.82	0.70

Participation in social groups

	Village-level data (Java survey)		Individual-level data (Java survey)	
	Log number of groups in village	Log attendance per adult at group meetings in past three months	Number types of groups participated in during last three months	Number times participated in last three months
	(1)	(2)	(3)	(4)
Number of TV channels	−0.068**	−0.111**	−0.186*	−0.970
	(0.026)	(0.045)	(0.096)	(0.756)
Observations	584	556	4,268	4,268
R^2	0.64	0.49	0.40	0.29
Mean dep. var.	4.94	1.97	4.27	22.77

Qualitatively similar results using introduction of private TV (panel) and using electromagnetic model of signals to instrument for who receives channels

But no impact on actual monitoring...

	Log attendance at meeting (1)	Log attendance of "insiders" at meeting (2)	Log attendance of "outsiders" at meeting (3)	Log number of people who talk at meeting (4)	Number of problems discussed (5)	Any corruption-related problem (6)	Any serious action taken (7)
Number of TV channels	−0.030**	−0.047**	−0.009	0.002	0.019	−0.009	0.000
	(0.015)	(0.020)	(0.032)	(0.020)	(0.059)	(0.008)	(0.003)
Observations	2,273	2,266	2,124	2,200	1,702	1,702	1,702
Mean dep. var.	0.26	0.19	0.26	0.22	0.37	0.15	0.15
	3.75	2.77	2.71	2.07	1.18	0.06	0.02

Or on corruption

	Missing expenditures in road project (1)	Missing expenditures in road and ancillary projects (2)	Discrepancy in prices in road project (3)	Discrepancy in quantities in road project (4)
Number of TV channels	−0.033*	−0.042**	−0.030***	0.003
	(0.019)	(0.019)	(0.010)	(0.021)
Observations	460	517	476	460
R^2	0.35	0.29	0.30	0.32
Mean dep. var.	0.24	0.25	−0.01	0.24

- Not in paper, but I've also checked, and no impact on labor or monetary contributions to the project

Enhancing collective action

- Spurred on by ideas in the 2004 World Development Report, there were numerous attempts to test whether one could somehow increase collective action by reducing the costs of participating in monitoring behaviors
    - Will not solve free ride problem of course
    - But may nevertheless be important if one cannot solve these problems centrally

Enhancing collective action

- To investigate this: 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)
- Second generation of experiments sought to unpack this puzzle
  - Pradhan et al (2014) - education in Indonesia
  - Björkman, de Walque, and Svensson (2014) - health in Uganda take 2

Education in India

Banerjee, Banerji, Duflo, Glennerster, and Khemani (2010): 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

Interventions

- Treatment 3 (monitoring + information + remediation): Treatment 1 + treatment 2 +
  - Village volunteers given 4 training 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

$$\widehat{\beta}_k = \frac{1}{k} \sum_{k=1}^{K} \frac{\widehat{\beta_{tk}}}{\widehat{\sigma_{tk}}}$$

Results

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

TABLE 1—VEC AWARENESS AND ACTIVISM

	Baseline		Endline comparison	OLS: Impact of treatment in endline				
	Mean	N	Group mean	Treatment 1	Treatment 2	Treatment 3	Any treatment	N
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A. Dependent variables—VEC members information about their role								
Mentioned that they are in the VEC unprompted	0.383	248	0.247	0.084	0.083	0.030	0.066	237
	(0.024)		(0.038)	(0.060)	(0.061)	(0.058)	(0.046)	
Mentioned that they are in the VEC when prompted	0.753	248	0.602	0.065	0.095	0.047	0.070	237
	(0.020)		(0.044)	(0.067)	(0.061)	(0.064)	(0.051)	
Had heard of SSA	0.258	248	0.209	0.101	0.062	0.065	0.075	237
	(0.018)		(0.033)	(0.056)	(0.053)	(0.058)	(0.042)	
Knew that their school can receive money from SSA	0.210	248	0.179	0.119**	0.048	0.072	0.078	237
	(0.017)		(0.033)	(0.056)	(0.049)	(0.057)	(0.041)	
Had received VEC training	0.132	248	0.046	0.118***	0.135***	0.148***	0.134***	237
	(0.016)		(0.020)	(0.042)	(0.044)	(0.041)	(0.030)	
Average over family of outcomes (in SD)				0.387***	0.345***	0.320**	0.350***	
				(0.138)	(0.125)	(0.141)	(0.098)	
Panel B. Dependent variables—VEC member activism								
Complained	0.171	254	0.102	−0.035	0.033	0.017	0.005	235
	(0.014)		(0.024)	(0.034)	(0.042)	(0.038)	(0.031)	
Raised money	0.076	254	0.029	−0.015	−0.005	−0.006	−0.009	235
	(0.010)		(0.012)	(0.016)	(0.027)	(0.022)	(0.018)	
Number of school inspections reported	9.356	242	9.041	−0.161	−1.948	−1.204	−1.117	214
	(0.696)		(1.201)	(1.723)	(1.550)	(1.864)	(1.435)	
Distributed scholarships	0.082	254	0.054	−0.039	0.018	−0.013	−0.012	235
	(0.012)		(0.020)	(0.038)	(0.042)	(0.040)	(0.033)	
Implemented midday meal	0.147	254	0.122	0.006	0.001	0.029	0.012	235
	(0.015)		(0.029)	(0.030)	(0.024)	(0.027)	(0.021)	
Average over family of outcomes (in SD)				−0.090	−0.002	0.005	−0.030	
				(0.092)	(0.093)	(0.092)	(0.076)	

Results

No impact on parent knowledge

TABLE 2—PARENTS' AWARENESS AND ACTIVISM

	Baseline		Endline comparison	OLS: Impact of treatment in endline				
	Mean (1)	N (2)	Group mean (3)	Treatment 1 (4)	Treatment 2 (5)	Treatment 3 (6)	Any treatment (7)	N (8)
Panel C. Dependent variables—parental knowledge of education								
Said "don't know" when asked how many children can read paragraph	0.200 (0.009)	2,660	0.172 (0.016)	−0.007 (0.023)	−0.044** (0.020)	−0.006 (0.024)	−0.018 (0.018)	1,920
Said "don't know" when asked how many children can write sentence	0.212 (0.009)	2,660	0.175 (0.016)	−0.012 (0.023)	−0.033 (0.021)	−0.008 (0.024)	−0.017 (0.018)	1,920
Perception minus reality of how many kids can read paragraphs	0.123 (0.007)	2,146	0.042 (0.012)	−0.014 (0.018)	0.018 (0.018)	−0.040** (0.017)	−0.012 (0.014)	1,671
Perception minus reality of how many kids can write sentences	0.109 (0.006)	2,113	−0.020 (0.012)	−0.019 (0.018)	0.025 (0.018)	−0.035 (0.018)	−0.010 (0.014)	1,662
Overestimated own child's ability to read	0.419 (0.009)	2,503	0.336 (0.015)	0.007 (0.022)	0.006 (0.023)	−0.026 (0.022)	−0.005 (0.018)	1,815
Overestimated own child's ability to write	0.254 (0.007)	2,466	0.196 (0.011)	−0.023 (0.018)	−0.003 (0.018)	−0.027 (0.017)	−0.018 (0.014)	1,794
Average over family of outcomes (in SD)				−0.047 (0.040)	0.005 (0.042)	−0.097** (0.041)	−0.047 (0.033)	

Banerjee, Abhijit V. and Banerji, Rukmini and Duflo, Esther and Glennerster, Rachel and Khemani, Stuti, Pitfalls of Participatory Programs: Evidence from a Randomized Evaluation in Education in India (September 5, 2008). MIT Department of Economics Working Paper No. 08-18. Available at SSRN:

Results

Zero impact on schooling status

TABLE 3—SCHOOLING STATUS AND STUDENT ATTENDANCE

	Baseline		Endline comparison	OLS: Impact of treatment in endline				
	Mean (1)	N (2)	Group mean (3)	Treatment 1 (4)	Treatment 2 (5)	Treatment 3 (6)	Any treatment (7)	N (8)
Panel A. Dependent variables—type of school students attend								
Out of school	0.069	17,530	0.079	0.008	0.006	0.013**	0.009**	16,455
	(0.003)		(0.006)	(0.005)	(0.005)	(0.005)	(0.004)	
In private or NGO school	0.373	17,530	0.387	0.009	0.019	−0.006	0.007	16,455
	(0.009)		(0.017)	(0.016)	(0.017)	(0.017)	(0.014)	
Any tutoring			0.069	−0.006	−0.018**	−0.002	−0.008	17,530
			(0.007)	(0.009)	(0.009)	(0.010)	(0.008)	
Read class	N/A		0.005	−0.001	0.002	0.077***	0.009**	16,412
			(0.001)	(0.002)	(0.003)	(0.010)	(0.004)	
Panel B. Dependent variables—students' enrollment and presence (government schools)								
Log (boys enrollment)	4.568	301	4.522	0.041	0.027	−0.020	0.017	276
	(0.033)		(0.062)	(0.048)	(0.050)	(0.069)	(0.045)	
Log (girls enrollment)	4.625	301	4.636	0.001	0.020	0.013	0.012	277
	(0.032)		(0.075)	(0.077)	(0.074)	(0.075)	(0.071)	
Fraction boys present	0.530	300	0.528	0.029	−0.004	−0.053	−0.008	244
	(0.015)		(0.028)	(0.041)	(0.042)	(0.041)	(0.032)	
Fraction girls present	0.496	301	0.522	0.053	−0.006	−0.027	0.006	249
	(0.014)		(0.022)	(0.043)	(0.035)	(0.035)	(0.028)	
Average over family of outcomes (in SD)				0.127	0.007	−0.105	0.011	
				(0.097)	(0.086)	(0.085)	(0.071)	
Panel C. Dependent variables—students' attendance as reported by parents								
Days present in last 14: all children	7.335	5,984	6.058	−0.279	−0.599	−0.314	−0.395	5,555
	(0.086)		(0.239)	(0.355)	(0.351)	(0.371)	(0.285)	
Days present in last 14: only male children in school	7.894	2,947	6.672	−0.264	−0.550	−0.255	−0.353	2,669
	(0.099)		(0.254)	(0.398)	(0.391)	(0.409)	(0.312)	
Days present in last 14: only female children in school	8.137	2,518	6.642	−0.221	−0.657	−0.152	−0.340	2,306
	(0.099)		(0.263)	(0.393)	(0.394)	(0.397)	(0.308)	
Average over family of outcomes (in SD)				−0.077	−0.153	−0.052	−0.094	
				(0.086)	(0.087)	(0.092)	(0.069)	

Banerjee, Abhijit V. and Banerji, Rukmini and Duflo, Esther and Glennerster, Rachel and Khemani, Stuti, Pitfalls of Participatory Programs: Evidence from a Randomized Evaluation in Education in India (September 5, 2008). MIT Department of Economics Working Paper No. 08-18. Available at SSRN:

Why zero?

- Is the "zero" persuasive? What would you want to know to believe it?
  - Standard errors – is this a "tight" zero?
  - "First stage" – would other interventions have mattered more?
  - In this setting can anything be done?

Results

Treatment 3 really did teach kids how to read

TABLE 4—READING AND MATH RESULTS

	Baseline	Endline comparison	OLS: Impact of treatment in endline			First stage	IV
	Mean	Group mean	Treatment 1	Treatment 2	Treatment 3	Attend read class	Impact of read class
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Reading results—all children (n=15,609)							
Could read letters	0.855	0.892	0.004	0.004	0.017**	0.077***	0.223**
	(0.004)	(0.007)	(0.007)	(0.007)	(0.007)	(0.010)	(0.093)
Could read words or paragraphs	0.550	0.635	0.005	−0.003	0.018**		0.232**
	(0.006)	(0.009)	(0.008)	(0.008)	(0.008)		(0.101)
Could read stories	0.391	0.499	0.004	0.003	0.017		0.224
	(0.006)	(0.011)	(0.009)	(0.010)	(0.010)		(0.137)
Panel B. Reading results—children who could not read at baseline (n=2,288)							
Could read letters		0.432	0.041	0.032	0.079**	0.131***	0.602**
		(0.023)	(0.031)	(0.034)	(0.035)	(0.023)	(0.304)
Could read words or paragraphs		0.056	−0.006	−0.013	−0.007		−0.051
		(0.010)	(0.015)	(0.012)	(0.014)		(0.106)
Could read stories		0.028	−0.006	−0.013	−0.008		−0.063
		(0.007)	(0.010)	(0.008)	(0.009)		(0.074)
Panel C. Reading results—children who could only read letters at baseline (n=3,539)							
Could read letters		0.919	−0.008	−0.015	0.021	0.132***	0.162
		(0.010)	(0.016)	(0.014)	(0.013)	(0.020)	(0.097)
Could read words or paragraphs		0.253	−0.011	−0.025	0.035		0.269
		(0.014)	(0.022)	(0.021)	(0.022)		(0.171)
Could read stories		0.086	−0.001	−0.010	0.033**		0.261
		(0.011)	(0.014)	(0.014)	(0.017)		(0.135)

Health in Uganda

Bjorkman 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

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

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

Results

Results on Service Quality

TABLE III

PROGRAM IMPACT ON TREATMENT PRACTICES AND MANAGEMENT

Spec.	Dep. variable	Model	Program impact	2005	Mean control group 2005	Obs.
(1)	Equipment used	DD	0.08**	−0.07***	0.41	5,280
			(0.03)	(0.02)		
(2)	Equipment used	OLS	0.01		0.41	2,758
			(0.02)			
(3)	Waiting time	DD	−12.3*	−12.4**	131	6,602
			(7.1)	(5.2)		
(4)	Waiting time	OLS	−5.16		131	3,426
			(5.51)			
(5)	Absence rate	OLS	−0.13**		0.47	46
			(0.06)			
(6)	Management of clinic	OLS	1.20***		−0.49	50
			(0.33)			
(7)	Health information	OLS	0.07***		0.32	4,996
			(0.02)			
(8)	Importance of family planning	OLS	0.06***		0.31	4,996
			(0.02)			
(9)						

TABLE IV
PROGRAM IMPACT ON IMMUNIZATION

Group Specification:	Newborn (1)	Under 1 year (2)	1 year old (3)	2 years old (4)	3 years old (5)	4 years old (6)
Average standardized effect	1.30* (0.70)	1.44** (0.72)	1.24** (0.63)	0.72 (0.58)	2.01*** (0.67)	0.86 (0.80)
Observations	173	929	940	951	1,110	526

Results

Results on Utilization of Facility

TABLE V
PROGRAM IMPACT ON UTILIZATION/COVERAGE

Dep. variable	Outpatients	Delivery	Antenatal	Family planning	Average std effect	Use of project facility	Use of self-treatment/ traditional healers	Average std effect
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A: Cross-sectional data								
Program impact	130.2**	5.3**	15.0	3.4	1.75***	0.026*	−0.014	1.43*
	(60.8)	(2.1)	(11.2)	(3.2)	(0.63)	(0.016)	(0.011)	(0.87)
Observations	50	50	50	50	50	50	50	50
	(9)	(10)			(11)	(12)	(13)	(14)
B: Panel data								
Program impact	189.1***	3.48*			2.30***	0.031*	−0.046**	1.96**
	(67.2)	(1.96)			(0.69)	(0.017)	(0.021)	(0.89)
Observations	100	100			100	100	100	100
Mean control group 2005	661	9.2	78.9	15.2	–	0.24	0.36	–

TABLE VI
PROGRAM IMPACT ON HEALTH OUTCOMES

Dependent variable	Births	Pregnancies	U5MR	Child death	Weight-for-age z-scores	
Specification:	(1)	(2)	(3)	(4)	(5)	(6)
Program impact	−0.016	−0.03**	−49.9*		0.14**	0.14**
	(0.013)	(0.014)	(26.9)		(0.07)	(0.07)
Child age (log)						−1.27***
						(0.07)
Female						0.27***
						(0.09)
Program impact × year of birth 2005				−0.026**		
				(0.013)		
Program impact × year of birth 2004				−0.019**		
				(0.008)		
Program impact × year of birth 2003				0.003		
				(0.009)		
Program impact × year of birth 2002				0.000		
				(0.006)		
Program impact × year of birth 2001				0.002		
				(0.006)		
Mean control group 2005	0.21	0.29	144	0.029	−0.71	−0.71
Observations	4,996	4,996	50	5,094	1,135	1,135

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

Accountability Meetings

Interventions

- 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 has 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:

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

Measuring Corruption

- Goal
  - Measure the difference between *reported expenditures* and *actual expenditures*
- Measure of theft:

$$THEFT_i = Log(Reported_i) \quad Log(Actual_i)$$

  - 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

Measuring Corruption

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***	13.47***	.743***	.286***
	(1.35)	(1.25)	(.188)	(.079)
Invitations plus comments	11.48***	10.28***	.498***	.221***
	(1.35)	(1.27)	(.167)	(.069)
Meeting 2	5.32***	4.00***	.163	.024
	(1.11)	(1.06)	(.155)	(.084)
Meeting 3	4.29***	5.78***	.431**	.158*
	(1.20)	(1.13)	(.172)	(.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	.027**	.003
	(.063)	(.013)	(.008)
Invitations plus comments	.104	.026**	.015**
	(.064)	(.012)	(.008)
Meeting 2	.187***	.002	.020**
	(.066)	(.013)	(.009)
Meeting 3	.428***	.036***	.029***
	(.074)	(.012)	(.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

Results

Corruption

TABLE 11
PARTICIPATION: MAIN THEFT RESULTS

Percent Missing[a]	Control Mean (1)	Treatment Mean (2)	No Fixed Effects: Treatment Effect (3)	No Fixed Effects: p-Value (4)	Engineer Fixed Effects: Treatment Effect (5)	Engineer Fixed Effects: p-Value (6)	Stratum Fixed Effects: Treatment Effect (7)	Stratum Fixed Effects: p-Value (8)
A. Invitations								
Major items in roads (N = 477)	.252	.230	.021	.556	.030	.385	.026	.448
	(.033)	(.033)	(.035)		(.034)		(.034)	
Major items in roads and ancillary projects (N = 538)	.268	.236	.030	.360	.032	.319	.029	.356
	(.031)	(.031)	(.032)		(.032)		(.032)	
Breakdown of roads:								
Materials (N = 477)	.209	.221	.014	.725	.008	.839	.005	.882
	(.041)	(.041)	(.038)		(.037)		(.037)	
Unskilled labor (N = 426)	.369	.180	.187*	.058	.215**	.024	.143*	.098
	(.077)	(.077)	(.098)		(.094)		(.086)	
B. Invitations Plus Comments								
Major items in roads (N = 477)	.252	.228	.022	.455	.024	.411	.015	.601
	(.033)	(.026)	(.030)		(.029)		(.030)	
Major items in roads and ancillary projects (N = 538)	.268	.238	.026	.409	.025	.406	.027	.385
	(.031)	(.026)	(.032)		(.030)		(.031)	
Breakdown of roads:								
Materials (N = 477)	.209	.180	.028	.414	.022	.496	.010	.754
	(.041)	(.032)	(.034)		(.032)		(.033)	
Unskilled labor (N = 426)	.369	.267	.099	.255	.132	.131	.090	.323
	(.077)	(.073)	(.087)		(.087)		(.091)	

Results

Interactions with elite capture

TABLE 12
Interactions of Participation Experiments with How Invitations Were Distributed

Percent Missing[a]	Control Mean (1)	Treatment Mean (2)	No Fixed Effects: Treatment Effect (3)	No Fixed Effects: p-Value (4)	Engineer Fixed Effects: Treatment Effect (5)	Engineer Fixed Effects: p-Value (6)	Stratum Fixed Effects: Treatment Effect (7)	Stratum Fixed Effects: 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
B. Invitations Plus Comments — Invitations Plus Comment Forms Distributed via Neighborhood Heads								
Major items in roads ($N = 242$)	.252 (.033)	.278 (.036)	.025 (.036)	.483	.038 (.036)	.294	.022 (.041)	.602
Major items in roads and ancillary projects ($N = 271$)	.268 (.031)	.277 (.039)	.010 (.039)	.792	.024 (.038)	.535	.023 (.040)	.569
Invitations Plus Comment Forms Distributed via Schools								
Major items in roads ($N = 242$)	.252 (.033)	.179 (.036)	.070* (.041)	.093	.086** (.038)	.023	.052 (.036)	.150
Major items in roads and ancillary projects ($N = 267$)	.268 (.031)	.198 (.034)	.064 (.042)	.127	.077* (.039)	.052	.078* (.041)	.056

Discussion

- 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?

Improving Collective Action 2.0

Pradhan et al (2014), Improving Educational Quality through Enhancing Community Participation

- The previous papers suggest that the details matter
- Pradhan et al conduct an experiment to try to tease this out, testing four interventions aimed at improving Indonesian school committees in 420 communities:
    - *Block grants*. School committee receives grant of \$870. Supposed to develop plan for expenditure with assistance of faciltiators (13 visits). What does this test?
    - *Training*. Two day training of 4 school committee members (principal, teacher, parent, village rep). Focused on creating plan for how to spend block grant, but also taught active learning, school-based management, visit to model school etc. What does this test?
    - *Elections*. Broadened participation in election of committee members (so not de facto appointed by principals). What does this test?
    - *Linkages*. Linked school committee with local village parliament, in attemt to broaden their influence. What does this test?
- How does this design help answer the questions raised by the version

Matrix design explores interactions

TABLE 2—ALLOCATION OF SCHOOLS TO TREATMENTS (*Number of Schools*)

	No election		Election		
Receiving block grant	Linkage	No linkage	Linkage	No linkage	Total
No training	50	90	50	50	240
Training	45	45	45	45	180
Total	95	135	95	95	420

Control group, not receiving block grant, no intervention: 100 schools

Courtesy of Menno Pradhan, Daniel Suryadarma, Amanda Beatty, Maisy Wong, Arya Gaduh, Armida Alisjahbana, and Rima Prama Artha.

Findings

Table 5—Impact on Drop Out, Repetition, and Test Scores

	Pre/post mean and SD (1)	Grant, G OLS (2)	Election, E OLS (3)	Linkage, L OLS (4)	Training, T OLS (5)	L+E OLS (6)	L+T OLS (7)	T+E OLS (8)
Panel A. Dropout and repetition rates								
Dropout	0.002/0.01	−0.005	−0.003	−0.002	0.007	−0.005	0.003	0.004
	[0.01/0.05]	(0.005)	(0.006)	(0.006)	(0.006)	(0.011)	(0.006)	(0.006)
Repetition	0.02/0.03	−0.004	−0.001	0.007	−0.006	0.007	0.001	−0.007
	[0.04/0.06]	(0.007)	(0.005)	(0.005)	(0.005)	(0.008)	(0.009)	(0.008)
Panel B. Language test scores (*average, by gender*)								
Average	11.66/13.09	0.129	0.053	0.173**	−0.042	0.234**	0.134	0.015
	[4.32/6.34]	(0.094)	(0.069)	(0.068)	(0.069)	(0.094)	(0.087)	(0.103)
Boys	11.38/13.16	0.085	0.025	0.156**	−0.044	0.187*	0.115	−0.020
	[4.13/6.57]	(0.105)	(0.078)	(0.078)	(0.078)	(0.101)	(0.101)	(0.115)
Girls	11.93/13.04	0.167*	0.073	0.191***	−0.040	0.271***	0.152*	0.039
	[4.48/6.13]	(0.093)	(0.069)	(0.068)	(0.069)	(0.099)	(0.088)	(0.101)
Panel C. Mathematics test scores (*average, by gender*)								
Average	16.42/8.88	−0.015	−0.008	0.070	−0.029	0.061	0.040	−0.036
	[5.61/3.17]	(0.080)	(0.050)	(0.050)	(0.050)	(0.075)	(0.068)	(0.066)
Boys	16.38/8.98	0.002	−0.030	0.026	−0.021	−0.003	0.001	−0.051
	[5.67/3.10]	(0.085)	(0.058)	(0.058)	(0.059)	(0.089)	(0.080)	(0.071)
Girls	16.49/8.78	−0.032	0.009	0.113**	−0.035	0.121	0.079	−0.025
	[5.55/3.23]	(0.088)	(0.053)	(0.053)	(0.053)	(0.074)	(0.072)	(0.074)

Courtesy of Menno Pradhan, Daniel Suryadarma, Amanda Beatty, Maisy Wong, Arya Gaduh, Armida Alisjahbana, and Rima Prama Artha.

Bjorkman and Svenson 2.0

Bjorkman, de Walque, and Svensson (2014): Information is Power

- Goal of this paper
  - Track long-run impacts of their first intervention (participation + information)
  - Run another experiment with participatory component, but not information component
- Findings:
  - Original effects of first intervention persist
  - But second intervention has no effect
- What do you think?

Multiple hypothesis testing and pre-analysis plans

Casey et al (2012), Reshaping Institutions: Evidence on Aid Impacts Using a Pre-Analysis Plan

- One potential concern with empirical exercises is that you have many, many potential outcomes
  - Go back and look at a standard survey and see how many questions there are
- Moreover, if you are interested in heterogeneous effects, you have many possible regressions
  - In an RCT for a given y_i, not that many choices in how to run:

$$y_i = \alpha + \beta T_i + \varepsilon_i.$$

  - But if you're interested in

$$y_i = \alpha + \gamma X_i + \beta T_i + \psi T_i \times X_i + \varepsilon_i$$

    then now you can run this a zillion ways, with different interaction variables X
  - Examples?

Pre-analysis plans

- Given these concerns people have started to write "pre-analysis" plans to commit to which hypotheses they will test. Standard in medical trials. How might this help?
  - Reduce the number of y_i and deal with concerns about data-mining and multiple hypothesis testing
  - Pre-commit to which X you will interact with
  - Pre-commit to regression specifications.
- Why are these more common in RCTs than non-RCTs?
- Helpful to the extent they limit you. But you may not want to be too limited. Current area where people are actively working things out.
- P-set will talk about one example (Casey et al) related to institutions and building colletive action. Other recent examples include Alatas et al (2012), Finkelstein et al (2012), etc. See also Olken (2015).

Improving collective action

- Substantively, several recent studies (including this one) have looked at whether external programs that sought to improve collective action had spillovers to institutions more broadly
- Broadly speaking: find impact only under limited circumstances
  - Casey et al (2012) – community driven development in Sierra Leone: no impact
  - Fearon et al (2014) – community driven development in Liberia. Find some evidence that program increased contributions in a matching game, but only when both men and women were asked to be included.
  - Beath et al (2013) – exogenous creation of elected councils. Find that when they deliver wheat to the villages and have councils deliver them, aid is delivered with better targeting and less leakage. But without clear specification of control rights, worse outcomes.
- Common theme of last two papers: you can set up new institutions that deal better with new problems, but not existing problems.

Decentralization and Local Capture

- Broadly speaking, there are two ways of framing decentralization:
- The public finance framework (e.g. Tiebout 1956).
  - Heterogeneity in preferences for local public goods (amounts/quantities)
  - But, economies of scale in production of those public goods, which are presumed to be heterogeneous within jurisdictions. This leads to a tradeoff for optimal size of jurisdictions.
  - Idea is that people sort into whichever jurisdiction offers the bundle of tax / public goods they want.
- The political economy framework (e.g. Bardhan and Mookerjee 2000)
  - Better local information at the local level (about either preferences for public goods, or facts about how best to implement them)
  - But, there may be capture by local elites. Not clear why we think local elites will capture more than non-local elites, but people do tend to think this.
  - Mechanism design question: can you get the information out of local elites without capture?

Local capture is a very old idea

Federalist Papers #10 (1787)

"Men of factious tempers, of local prejudices, or of sinister designs, may, by intrigue, by corruption, or by other means, first obtain the suffrages, and then betray the interests, of the people. The question resulting is, whether small or extensive republics are more favorable to the election of proper guardians of the public weal; and it is clearly decided in favor of the latter by two obvious considerations:

In the first place, it is to be remarked that, however small the republic may be, the representatives must be raised to a certain number, in order to guard against the cabals of a few; and that, however large it may be, they must be limited to a certain number, in order to guard against the confusion of a multitude. Hence, the number of representatives in the two cases not being in proportion to that of the two constituents, and being proportionally greater in the small republic, it follows that, if the proportion of fit characters be not less in the large than in the small republic, the former will present a greater option, and consequently a greater probability of a fit choice."

Local capture is a very old idea

Federalist Papers #10 (1787)

"In the next place, as each representative will be chosen by a greater number of citizens in the large than in the small republic, it will be more difficult for unworthy candidates to practice with success the vicious arts by which elections are too often carried; and the suffrages of the people being more free, will be more likely to centre in men who possess the most attractive merit and the most diffusive and established characters."

Putting it together

- How to think about these issues?
  - Is there local information?
  - Do local elites capture resources, and if so, how?

Is there local information?

Alatas et al (2012): Targeting the Poor: Evidence from a Field Experiment in Indonesia

- Alatas et al are interested in the question of targeting, i.e.
  - We are trying to give aid to the poor
  - How do we figure out who is poor?
- Two approaches:
  - Use limited information available to the central government.
    - Survey of assets: 48 variables total. Examples: type of wall, roof, etc; have a car, sofa, refrigerator, etc.
    - Predict consumption from assets using a regression, and eligible if $\hat{y} = X'\beta < \bar{y}$
  - Try to elicit information from the community

Test of local information

- To test whether there is local information, we:
  - Randomly surveyed 9 households in each neighborhood. Asked about consumption (unobservable to central government), assets (observable to central government), and subjective well being
  - Asked each of these households to rank the other 8 households from richest to poorest.
  - Then ran the following regression:

$$r_{ij} = \alpha + \gamma y_i + X_i' \beta + \varepsilon_{ij}$$

    where j is ranker and i is the rankee, y is within village rank based on consumption, and $X'\beta$ is what the central government observes
  - What does this test?

Test of local information

Table 11—Information

	Community survey rank (r_c) (1)	Community survey rank (r_c) (2)	Survey rank (2 continued)
Rank per capita consumption within village in percentiles	0.132***	0.088***	
	(0.014)	(0.012)	
Rank per capita consumption from PMT within village in percentiles	0.368***		
	(0.014)		

Do they use this local information?

- This says that there is some local information beyond what the government observes.
- But, the question is, can we extract that local information.
- To test this, we held community meetings where, in a public forum, people ranked each other from richest to poorest. Poorest ones got the money

What happens?

- Ran an RCT in which $\frac{2}{3}$ of villages used community approach, $\frac{1}{3}$ of villages used traditional asset-based approach. Gave out US$3 if you were chosen.
- Examine rank correlation between these meetings and 4 different survey based metrics:
  - Consumption
  - Survey ranks of each other (at home)
  - Elite ranks of households (at home)
  - Self-assessemnt
- Compare to the ranks generated from the asset test

Results

Table 9—Assessing Targeting Treatments Using Alternative Welfare Metrics

	Consumption (r_g) (1)	Community survey ranks (r_c) (2)	Subvillage head survey ranks(r_e) (3)	Self-assessment (r_s) (4)
Community treatment	−0.065**	0.246***	0.248***	0.102***
	(0.033)	(0.029)	(0.038)	(0.033)
Hybrid treatment	−0.067**	0.143***	0.128***	0.075**
	(0.033)	(0.029)	(0.038)	(0.033)

OK, but what about capture?

Alatas et al (2013): Does Elite Capture Matter? Local Elites and Targeted Welfare Programs in Indonesia

- This shows the plus side: local information exists, and in fact in can be used
  - Although here they implement their own social welfare function, which is not identical to consumption
- What about elite capture?
- What might this mean in this context?

Testing for elite capture

- We measure elite capture by looking at relatives of formal and informal elites, and seeing whether they are more or less likely to get on the program conditional on their actual consumption level.

Testing for elite capture

- We measure elite capture by looking at relatives of formal and informal elites, and seeing whether they are more or less likely to get on the program conditional on their actual consumption level. How to find elites?

Testing for elite capture

- We measure elite capture by looking at relatives of formal and informal elites, and seeing whether they are more or less likely to get on the program conditional on their actual consumption level. How to find elites?
- Consider:
  - The small-stakes program
  - A large-stakes version of the same program ($150 / yr for 6 years)
  - Randomly varying the meetings to invite whole community, or just elites
  - A variety of existing government programs for the poor (e.g. subsidized rice, health insurance, cash transfers)

Results for formal elites

Table 3: Elite Capture by Formal Versus Informal Elites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel A: Government Transfer Programs — Formal Elites						
	Receives Benefits				Targeting Lists		
	BLT 05	BLT 08	Jamkesmas	Raskin	PPLS 1	PPLS 2	PPLS 3
Elite	0.049***	0.047***	0.082***	0.032**	0.026	-0.011	-0.008
	(0.018)	(0.018)	(0.018)	(0.014)	(0.017)	(0.015)	(0.010)
Observations	3,985	3,985	3,996	3,996	3,996	3,996	3,996
Dependent Variable Mean	0.362	0.387	0.425	0.751	0.359	0.262	0.102
	Panel B: Government Transfer Programs — Informal Elites						
	Receives Benefits				Targeting Lists		
	BLT 05	BLT 08	Jamkesmas	Raskin	PPLS 1	PPLS 2	PPLS 3
Elite	-0.069***	-0.066***	-0.064***	-0.061***	-0.017	-0.030*	-0.017
	(0.020)	(0.021)	(0.023)	(0.017)	(0.021)	(0.018)	(0.012)
Observations	3,985	3,985	3,996	3,996	3,996	3,996	3,996
Dependent Variable Mean	0.362	0.387	0.425	0.751	0.359	0.262	0.102

Courtesy of Vivi Alatas, Abhijit Banerjee, Rema Hanna, Benjamin A. Olken, Ririn Purnamasari, and Matthew Wai-Poi. .

Results for formal elites

Table 3: Elite Capture by Formal Versus Informal Elites

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Panel C: PKH Experiment — Formal Elites						
	Receives PKH				Targeting Lists		
	PMT	Community	Community		PMT	Community	Community
Elite	-0.034**	-0.042***	-0.021		-0.017*	-0.018	-0.017
	(0.015)	(0.015)	(0.023)		(0.009)	(0.012)	(0.018)
Elite x Elite Subtreatment			-0.042				-0.003
			(0.031)				(0.024)
Observations	1,863	1,936	1,936		1,996	2,000	2,000
Dependent Variable Mean	0.110	0.142	0.142		0.0431	0.0770	0.0770
	Panel D: PKH Experiment — Informal Elites						
	Receives PKH				Targeting Lists		
	PMT	Community	Community		PMT	Community	Community
Elite	-0.033*	-0.020	-0.018		-0.011	-0.040***	-0.051**
	(0.017)	(0.018)	(0.026)		(0.011)	(0.014)	(0.021)
Elite x Elite Subtreatment			-0.004				0.022
			(0.038)				(0.029)
Observations	1,863	1,936	1,936		1,996	2,000	2,000
Dependent Variable Mean	0.110	0.142	0.142		0.0431	0.0770	0.0770

Courtesy of Vivi Alatas, Abhijit Banerjee, Rema Hanna, Benjamin A. Olken, Ririn Purnamasari, and Matthew Wai-Poi.

But does it matter?

- How would you answer this question?
- Shortcut: how much would elite capture change average consumption level of beneficiaries?
- Suppose average person receives program with probability β and elites have extra chance of receiving the program$\Delta\beta$. Fraction α are elites. Then the percent difference in consumption due to elites is

$$
\begin{aligned}
& \frac{\frac{(1 \quad \alpha)\beta c_b + \alpha(\beta+\Delta\beta)c_e}{(1 \quad \alpha)\beta + \alpha(\beta+\Delta\beta)} \qquad c_b}{c_b} \\
= \quad & \frac{\alpha\Delta\beta\frac{(c_e \quad c_b)}{c_b}}{\beta + \alpha\Delta\beta} \\
< \quad & \alpha\frac{\Delta\beta}{\beta}\frac{(c_e \qquad c_b)}{c_b}
\end{aligned}
$$

Welfare calculation

$$\alpha \frac{\Delta\beta}{\beta} \frac{(c_e \quad c_b)}{c_b}$$

- Why is this useful? Because it says how much this matters is bounded above by the product of
  - How many elites there are in the population (α).
  - How much more likely they are to get the programs than everyone else ($\frac{\Delta\beta}{\beta}$)
  - How much richer they are than everyone else ($\frac{(c_e \quad c_b)}{c_b}$)
- In our data:
  - $\alpha = 0.15$
  - $\frac{\Delta\beta}{\beta}$ is at most 0.19
  - $\frac{(c_e \quad c_b)}{c_b}$ is about .08
- So the net effects of elites on average consumption of beneficiaries is at most $0.15 \times 0.19 \times 0.09 = 0.003$. So elite capture increases average consumption of beneficiaries by less than 1 percent.

External validity

$$\alpha \frac{\Delta\beta}{\beta} \frac{(c_e \quad c_b)}{c_b}$$

- So even if estimates of extent of elite capture is slightly different in different contexts, mechanically it can't be very big because elites are not that much richer than everyone else, and there are not that many of them.
- In particular almost by definition the product of how much richer they are and how many of them there are cannot be large.

Welfare analysis

- A more formal welfare approach is to estimate the welfare loss more formally using a CRRA welfare framework. I.e. assume that utility is

$$\int \frac{(c_i + b_i)^{1 \quad \rho}}{1 \quad \rho}$$

and calculate utility with actual program allocations b_i, and with hypothetical program allocations if we set $\Delta\beta = 0$.

Results

Table 7: Simulated Social Welfare under Different Levels of Capture

	(1) PKH Experiment	(2) BLT05	(3) BLT08
		Panel A: Elites	
Utility…			
Without program	-6.689	-6.689	-6.689
With Elite on	-6.600	-6.296	-6.268
With Elite off	-6.601	-6.296	-6.266
Under perfect PMT-targeting	-6.550	-6.171	-6.148
Under perfect consumption targeting	-6.354	-6.005	-5.991
Share of possible utility gain…			
With Elite on	26.51%	57.40%	60.23%
With Elite off	26.28%	57.39%	60.50%
Under perfect PMT-targeting	41.37%	75.73%	77.39%
		Panel B: Formal Elites	
Utility…			
Without program	-6.689	-6.689	-6.689
With Elite on	-6.600	-6.296	-6.269
With Elite off	-6.600	-6.292	-6.263
Under perfect PMT-targeting	-6.550	-6.171	-6.149
Under perfect consumption targeting	-6.354	-6.005	-5.991
Share of possible utility gain…			
With Elite on	26.58%	57.37%	60.20%
With Elite off	26.35%	57.98%	60.98%
Under perfect PMT-targeting	41.37%	75.73%	77.26%

Other types of capture

- Of course this is only one type of capture
- Other types of capture that might matter more?