Elections and accountability

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Rethinking elections - again

- The Downsian model assumes voters choose among candidates who want to win; the citizen-candidate model assumes voters choose among candidates with different underlying preferences.
- Another model of electoral behavior holds that candidates are in fact identical, and that the role of elections is simply to discipline politicians who would otherwise steal from the public coffers.
- In this lecture, I will summarize this model and present some empirical evidence about voters' ability to use elections as a mechanism to discipline incumbents.

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Barro and Ferejohn: Modelling retrospective accountability

- In this model, voters observe some information about an incumbent candidate and then decide whether or not to reelect the candidate.
- The challenge is that there is also some variation in the state of the world: i.e., sometimes there are negative shocks, rendering it more challenging for the candidate to deliver public goods.

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Comparing candidates

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- Thus in principle the voters are indifferent between having the incumbent and an alternative.
  - But, the possibility that they could vote the incumbent out will induce him/her to perform well.

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Model framework

- Tax revenue can be used either for government spending or "rents" for politicians.

$$\tau y = g + r$$

- Political candidates value cash rents this period r, as well as the future benefits of continuing to hold office R.
- Objective function of candidate P is written as follows, where p_I is the probability of re-election for the incumbent.

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Citizen preferences

- The policy preferences of citizen i can be written as follows, assuming all citizens have the same income $y^i = y$.

$$W^i(q) = (y - (\theta g + r)) + H(g)$$

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Assumed timing

- The state of the world θ is realized and observed by everybody; when θ is higher, providing public goods is more costly.
- Voters set a reservation (threshold) utility for reelecting the incumbent.
- The incumbent policymaker freely sets policy q_I.
- Elections are held in which voters choose between the incumbent and the opponent.

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Voters' strategy

- Assume voters coordinate on a retrospective voting strategy that entails punishing the incumbent for bad behavior; they will re-elect the incumbent if their welfare is above a certain threshold.

$$p_I = \begin{cases} 1 & \text{if } W(g(\theta), r(\theta)) \geq \bar{\omega}(\theta) \\ 0 & \text{if } W(g(\theta), r(\theta)) < \bar{\omega}(\theta) \end{cases}$$

- The incumbent then has two choices: please the voters to earn reelection, or steal everything.
- If s/he chooses to please the voters, s/he seeks to maximize rents subject to earning reelection. This yields the following:

$$r(\theta) = y - \bar{\omega}(\theta) + H(g^*(\theta)) - \theta g^*(\theta)$$

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Incumbents' strategy

- If the following condition holds, the incumbent prefers to steal moderately and be reelected, rather than stealing everything.

$$r(\theta) + R \geq y$$

- The voters' best strategy will be to ensure this condition holds with equality in every state of the world: i.e., to give the politician as little as possible to ensure s/he still prefers to be reelected.
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Model insight: summing up

- First, considering the possibility that politicians could appropriate public funds (or otherwise misuse their office) adds a whole new dimension to elections: they're no longer a choice among different options, but a mechanism to punish incumbents.
- Second, these models do not suggest voters can eliminate rents, but potentially limit them.
- Third, clearly this mechanism is dependent on voters obtaining accurate information about incumbents' actions while in office.
- The empirical papers analyzed today will examine whether voters do in fact respond to this information - a pattern consistent with the structure of this model.

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- The audit reports enumerated both irregularities associated with corruption, and those associated with poor administration.
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Estimation strategy

- The primary estimating equation is as follows:

$$E_{ms} = \alpha + \beta A_{ms} + X_{ms}\gamma + \nu_v s + \epsilon_{ms}$$

  where E_{ms} denotes electoral performance of an incumbent eligible for reelection in municipality m and state s, A_{ms} is an indicator for pre-election auditing, and X are controls.
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Discussion questions

- What kind of corruption do the audits report? Were you surprised by the forms of corruption observed?
- What is the major potential source of endogeneity bias in the primary equation equation? What evidence do the authors present suggesting that this bias is not evident in the data?
- What are the primary effects of audits?

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- Did you believe the results? Any alternate stories? What do the results suggest about the role of information in elections?

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Banerjee et al., Do informed voters make better choices?

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- Jurisdiction-specific report cards were generated that published information about incumbent performance (legislative activity, committee attendance and spending of discretionary development funds), as well as information about wealth, education and criminal record of the incumbent and challengers.
- In a random sample of slums, households received a free copy of a newspaper with the report card; the authors find evidence that voters not only use the information, they use it in sophisticated ways.

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Experimental design

- Sampling frame consists of polling stations that served slum areas; 200 treatment and 575 control polling stations selected.
- 3 days before newspaper distribution, the NGO distributed a pamphlet to each household in a treatment area, encouraging voters to use report cards to make an informed voting decision.
- 10 days before the election, the newspaper published report cards; NGO workers placed a free copy of the newspaper on every household's doorstep in treatment slums.
- Within next the 48 hours, NGO workers organized a public reading of report cards.

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Brief primer on randomized controlled trials

- Randomized controlled trials have become an increasingly popular empirical strategy in development economics in recent years.
- The fundamental principle is that units of interest - individuals, communities, health clinics, etc. - are randomly assigned to a treatment arm that receives some intervention or to a control arm.
- Remember the fundamental equality that must be established to ensure a regression is unbiased? There is no correlation between X and the unobserved error term ϵ.
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Challenges of RCTs: operational

- First, and most important, it needs to be feasible for the treatment to be randomly provided to some units and not others.
- Sometimes governments or NGOs implementing a program won't agree to this condition.
- Sometimes there are programs that are almost impossible to randomize - e.g., macroeconomic policies.
- It's not (generally) plausible to randomize some states or towns into having a fixed exchange rate, and some states or towns into having some other policy.

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Challenges of RCTs: levels of randomization

- The researcher needs to choose at what level the program is randomized.
- If you randomize at the municipality or neighborhood level, then some municipalities or neighborhoods will receive the program and others will not.
- If you randomize at the individual or household level, then some individuals or households will receive the program and others not.
- Why would a political information program be randomized at a neighborhood level? What is the potential problem with randomization at an individual level?

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Example

- Consider, for example, if you are implementing a RCT of a health intervention in a rural area.
- As a result of your intervention, some newly healthy individuals choose to migrate to the city for higher wages.
- If you fail to observe these individuals at follow-up because they have attrited from the sample, you will **underestimate** the positive impact of this intervention.
- Thus implementing a RCT often entails considerable effort on the part of researchers to track down all participants at follow-up.

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Challenges of RCTs: ethical

- Some researchers believe it's not appropriate to offer programs randomly to some individuals or communities and not others.
- Would it be better to target these programs to those who are most in need? Open question. . .
- Another option is to randomly **delay** the program for some areas or individuals, but ultimately offer it to everyone.
- This is a very commonly used strategy.

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Estimating the impact of an RCT

- Assuming that you've overcome these challenges and are comfortable with the ethical implications, you have individuals randomly assigned to treatment; in this case, an educational intervention.
- There are two (main) strategies that can be used to analyze the data in this case.
- The first option is to directly compare average outcomes in the treatment arm and the control arm. (Mechanically: regress outcomes on a dummy for treatment.)
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Estimating the impact of an RCT, cont.

- A second option is to consider the variable that the treatment is designed to directly shift: in this case, voters' level of information.
- We define voters' information as X; we then estimate a two-stage least squares specification using the treatment assignment as the instrument Z.
- Again, what are the assumptions that we must satisfy to estimate a 2SLS specification?
- Are they satisfied here?

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  - E.g., in this case, if no one read the information provided, there would be no impact of the intervention on voter information.
  - Or, if everyone was already fully informed, there would be no impact of the intervention on voter information.
  - This is something typically explored in piloting.
- The exclusion restriction will be satisfied if the randomization is implemented and it is clear that the intervention affects the outcome of interest through only one channel.

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Checking balance

Table 2: Randomization Check

	Full sample		Diff (1) and	Household survey sample		Diff (4) and	Observation sample		Diff (7) and
	Control	Treatment	(2): p-value	Control	Treatment	(5): p-value	Control	Treatment	(8): p-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A: Electoral Rolls									
Total Electors	1013.977	1000.510	0.478	1016.979	1002.546	0.622	933.094	1044.034	0.365
	(321.010)	(305.698)		(327.605)	(305.837)		(379.542)	(374.847)	
Total Female Electors	424.424	427.495	0.783	418.536	428.603	0.482	382.531	440.103	0.223
	(142.392)	(138.124)		(139.553)	(137.951)		(145.307)	(173.622)	
Electors per Household	4.404	4.542	0.488	4.375	4.556	0.356	4.292	4.980	0.307
	(2.069)	(2.135)		(2.288)	(2.153)		(2.859)	(1.697)	
Elector Age	35.501	35.333	0.926	35.215	35.307	0.671	34.686	35.469	0.245
	(2.340)	(2.408)		(2.456)	(2.412)		(1.984)	(2.980)	
Percent Temporary Housing							0.108	0.116	0.960
							(0.208)	(0.229)	
Joint F Test			0.815			0.831			0.328
Observations	575	200		195	194		32	29	
Panel B: Survey Data									
Female				0.496	0.499	0.776	0.547	0.505	0.101
				(0.500)	(0.500)		(0.499)	(0.501)	
Age				36.510	35.918	0.278	34.968	36.722	0.176
				(13.320)	(12.912)		(12.779)	(14.157)	
House Size				5.952	6.097	0.143	5.949	6.194	0.333
				(2.701)	(2.924)		(2.553)	(3.163)	
Monthly Income (INR)				6385.397	6687.185	0.268	5460.000	7499.665	0.038
				(5321.758)	(5933.879)		(3322.642)	(7181.029)	
Ration Card Holder				0.817	0.819	0.958	0.823	0.819	0.940
				(0.387)	(0.385)		(0.382)	(0.385)	
Literate				0.762	0.776	0.461	0.727	0.833	0.047
				(0.426)	(0.417)		(0.446)	(0.374)	
Muslim				0.153	0.182	0.249	0.151	0.174	0.496
				(0.360)	(0.386)		(0.359)	(0.380)	
Low Caste				0.579	0.583	0.891	0.685	0.579	0.194
				(0.494)	(0.493)		(0.465)	(0.495)	
Joint F Test						0.752			0.255
Observations				1946	1952		311	299	

Notes:

Simple impacts

Table 3: Average Treatment Effects: Voter Knowledge, Perceptions and Behavior and Party Campaigning

	Voter Knowledge: Quiz Score			Voter Perceptions		Voter Behavior		Party Campaigning			
	Total	Responsibilities	Qualifications and Spending	Much road spending	Much drain spending	log(Voters)	Incumbent Vote Share	Door to Door Campaign	Public Meeting or Rally	Gift-Giving at Campaign or Rally	Cash Bribe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Treatment	-0.017	0.013	-0.030	0.028	-0.035	0.035**	0.004	-0.034	0.029	0.100	-0.194**
	(0.090)	(0.043)	(0.067)	(0.072)	(0.035)	(0.016)	(0.011)	(0.057)	(0.088)	(0.081)	(0.091)
Educated	0.125*	0.058*	0.067	-0.084	0.000						
	(0.066)	(0.031)	(0.052)	(0.067)	(0.029)						
Treatment*Educated	0.280***	0.096*	0.184**	0.083	-0.031						
	(0.102)	(0.049)	(0.080)	(0.090)	(0.040)						
Treatment*MLA LADS Spending in that category				-0.023	-0.011						
				(0.023)	(0.043)						
Educated*MLA LADS Spending in that category				0.034*	0.013						
				(0.019)	(0.040)						
Treatment*MLA LADS Spending in that category*Educated				-0.038	0.062						
				(0.028)	(0.055)						
Treatment*MLA LADS Spending in that category in Slums				0.061***	0.205***						
				(0.023)	(0.075)						
Educated*MLA LADS Spending in that category in Slums				0.001	-0.031						
				(0.024)	(0.055)						
Treatment*MLA LADS Spending in category in Slums*Educated				0.020	-0.020						
				(0.033)	(0.091)						
Observations	3898	3898	3898	3898	3898	775	775	61	61	61	61
Control Mean	2.707	1.291	1.352	0.327	0.125	0.575	0.463	0.969	0.719	0.375	0.625

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Manacorda et al., Transfers and political support

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- This is a positive (or empirical) question.
- If such a relationship does exist, the next question is what the normative implications are.
  - In this instance, because government transfers are, in principle, a zero-sum game.
  - Voters do seem to be rewarded for receipt of transfers, but this may be at the expense of other voters, who are left with less resources to provide public goods.

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Overview of the paper

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Discontinuity in program eligibility

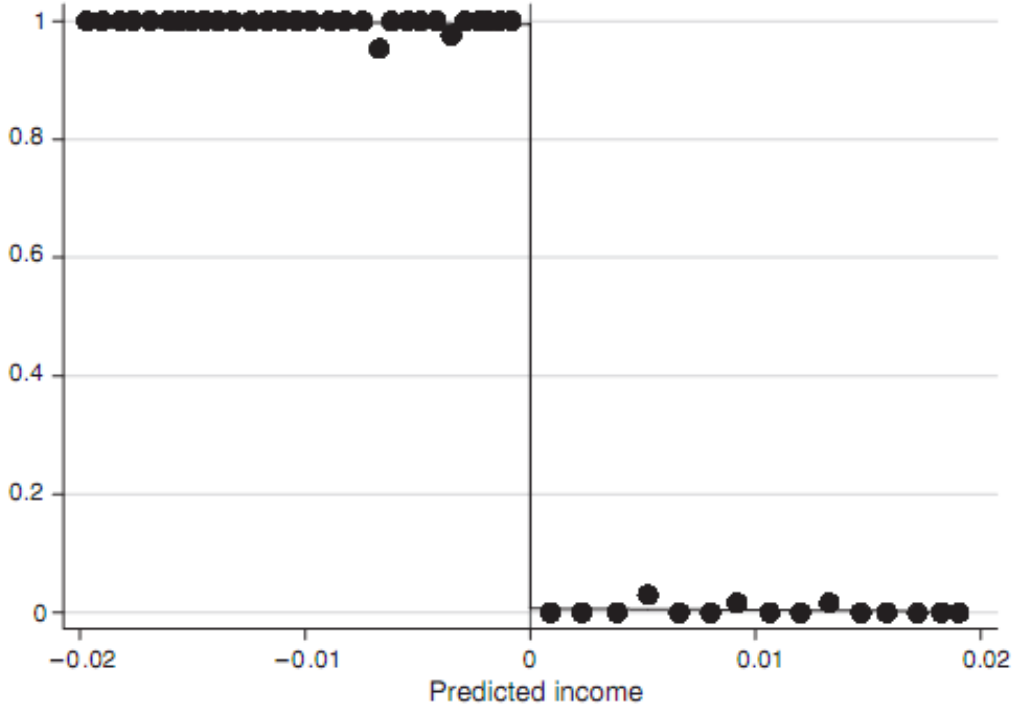

FIGURE 2. PANES PROGRAM ELIGIBILITY AND PARTICIPATION

Notes: The figure reports the proportion of households ever enrolled in PANES as a function of the standardized score (based on administrative data). The fitted plots are linear best fits on each side of the eligibility threshold.

Further verifying discontinuity

- The authors then go further to show that there is no evidence eligibility is correlated at the discontinuity with other variables.

Discontinuity in program eligibility, cont.

Table 2: Program eligibility, pre-treatment characteristics and response rates

Dependent variable:	Mean non-eligibles	Coefficient (s.e.)	Observations
Log per-capita income (2005)	6.33	-0.061 (0.063)	1,876
Household average years of education 16+ (2005)	4.04	0.091 (0.200)	1,887
Household size (2005)	3.05	-0.323 (0.237)	1,938
Household average age (2005)	31.69	-1.599 (2.126)	1,938
Respondent is female (2005)	0.699	-0.016 (0.056)	1,937
Respondent years of education (2005)	6.45	0.294 (0.321)	1,916
Respondent age (2005)	44.10	-1.811 (1.583)	1,938
Survey non-response rate (2008)	0.384	0.071 (0.045)	2,367
Replacement household (2008)	0.349	-0.069 (0.045)	1,938
Voted in 2004 elections	0.924	0.014 (0.024)	1,911
Linear score controls		Yes	

Notes. The table reports results from regressions of various pre-treatment (2005) characteristics on the program eligibility indicator. The specification is equivalent to column 2 in Table 1. See also notes to Table 1.

Measure of government support

- Note that the data on government support is based on the household's response to the question: in relation to the previous government, do you believe the current government is worse, the same or better?
- The authors do not exploit any direct electoral data.
- They do show that higher support for the government persists even after the conclusion of the program; this suggests that households were not merely expressing support out of fear of losing the benefit.

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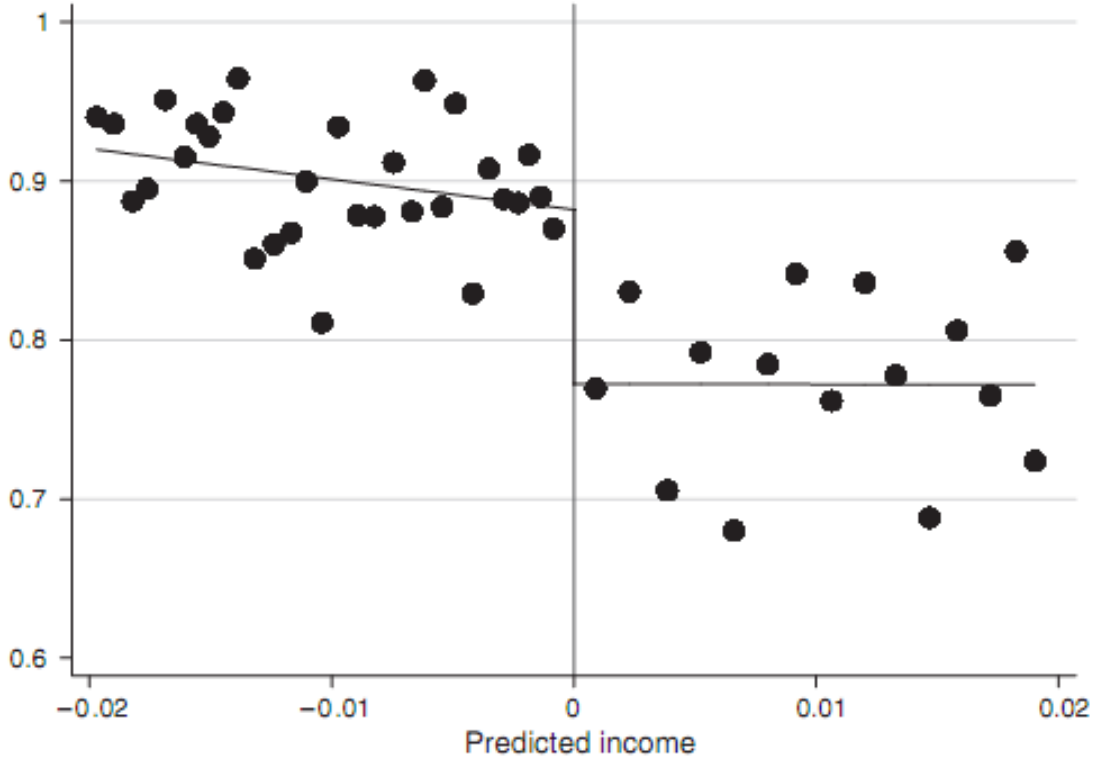

FIGURE 3. PANES PROGRAM ELIGIBILITY AND POLITICAL SUPPORT FOR THE GOVERNMENT, 2007 FOLLOW-UP SURVEY ROUND

Increased government support: Regression results

TABLE 1—PANES PROGRAM ELIGIBILITY, PARTICIPATION, AND POLITICAL SUPPORT FOR THE GOVERNMENT

Dependent variable	Mean non-eligibles	Coefficient (standard error) (1)	(2)	(3)	(4)	(5)	(6)	Observations
1. Ever received PANES, 2005–2007	0.004	0.993*** (0.002)	0.987*** (0.005)	0.995*** (0.005)	0.993*** (0.003)	0.988*** (0.005)	0.998*** (0.005)	2,232
2. Government support, 2007 (during program)	0.77	0.129*** (0.013)	0.110*** (0.026)	0.130*** (0.040)	0.126*** (0.014)	0.103*** (0.027)	0.125*** (0.043)	2,089
3. Government support, 2008 (post-program)	0.73	0.118*** (0.030)	0.100*** (0.043)	0.093** (0.016)	0.119*** (0.031)	0.096*** (0.045)	0.081* (0.045)	1,948
Score controls		None	Linear	Quadratic	None	Linear	Quadratic	
Other controls		No	No	No	Yes	Yes	Yes	

Potential sources of bias

- Remember that an RD design requires that potential outcomes with and without treatment do not change discontinuously at the threshold.
- In this case, the threshold was designed by program architects **after** the baseline survey was collected, and the threshold was not revealed to households or enumerators.
- Is it plausible that there is a discontinuous change in outcomes at the cutoff?

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- The interpretation is not as obvious!
- The authors suggest that voters are learning about politician preferences: voters don't know how much a given party prefers redistributive spending, but once relatively poor voters observe their own receipt of transfers and learn that parties value this type of program, they change their level of political support.
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