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Compulsory Voting Can Increase Political Inequality: Evidence from Brazil

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One of the most robust findings on political institutions is that compulsory voting (CV) reduces the participation gap between poorer and wealthier voters. We present evidence that in Brazil, the largest country to use such a rule, CV *increases* inequality in turnout. We use individual-level data on 140 million Brazilian citizens and two age-based discontinuities to estimate the heterogeneous effects of CV by educational achievement, a strong proxy for socioeconomic status. Evidence from both thresholds shows that the causal effect of CV on turnout among the more educated is at least twice the size of the effect among those with less education. To explain this result, which is the opposite of what is predicted by the existing literature, we argue that nonmonetary penalties for abstention primarily affect middle- and upper-class voters and thus increase their turnout disproportionately. Survey evidence from a national sample provides evidence for the mechanism. Our results show that studies of CV should consider nonmonetary sanctions, as their effects can reverse standard predictions.

1 Introduction

One of the most robust findings in the literature on political institutions is that compulsory voting (CV) compresses inequality in turnout (Lijphart 1997; Jackman 2001). Low-income voters around the world tend to participate in elections at lower rates than wealthier voters, which can induce democracies to cater policy to their more well-off citizens (Fowler 2013; Bechtel, Hangartner, and Schmid 2015). The typical account in the literature is that effective CV—usually enforced by monetary fines—mechanically eliminates inequality by reducing absenteeism to negligible levels and thus eliminates the representational biases stemming from unequal rates of participation.

While fees are the primary means by which CV is enforced around the world, the existing literature frequently overlooks *nonmonetary* sanctions for abstention.[1] In countries as diverse as Greece (prior to 2001), Venezuela (prior to 1993), Bolivia, Peru, and Brazil, failure to turnout can deny citizens access to a range of privileges such as the ability to apply for state employment, carry out financial transactions in banks, travel abroad, and obtain official identification documents. What is notable about these nonmonetary sanctions is that many, if not most, of the penalties affect activities that are primarily valued by middle- and upper-class voters. While abstaining voters can usually restore their access to state services by paying the fine, in most instances, it is simply easier to vote than deal with the bureaucratic hassle of fine payment.

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[1]For an exception, see Power (2009).

The existence of these nonmonetary sanctions complicates standard predictions about the distributional consequences of CV. This complication is especially important in contexts where fines for absenteeism are small or unenforced, leaving substantial room for noncompliance with the law.[2] If the size of the monetary fee is low and the importance upper- and middle-class voters place on the nonmonetary sanctions is high relative to poorer voters, CV may *increase* inequality in political participation. The logic is straightforward: the cost to abstaining poor voters of being denied access to state services is small when sanctioned state services are rarely used by low income voters, especially when the fine is low. For wealthier voters, lack of access to frequently used state services generates incentives to comply even when the monetary sanction is trivial.

We provide evidence that CV increases turnout more among the comparatively well off in the largest country in the world to use CV: Brazil. To show this, we employ a regression discontinuity (RD) design that uses two age-based thresholds in the electoral law which partitions the electorate into voters for whom voting is mandatory and those for whom it is voluntary. To assess how differential exposure to CV increases turnout among voters of distinct socioeconomic groups, we employ a comprehensive voter registry that records turnout, education status, and precise date of birth for over 140 million Brazilian citizens. We find that across both thresholds CV increases inequality in turnout. In addition, we present survey evidence to show that the most likely mechanism is differential use of state services among the comparatively well off. Because less well-off voters tend not to use the state services affected by failure to comply with the law, CV has comparatively weaker effects among these voters. We conclude by discussing limitations of the design and implications for future analyses of the effects of CV.

2 CV in Brazil

CV is mandated in the 1988 Brazilian constitution, and the sanctions for abstention are stipulated in the electoral code. Per the constitution, voting and registration are compulsory for literate individuals between the ages of eighteen and sixty-nine, and voluntary for illiterates and for those aged sixteen to seventeen and older than seventy. For citizens required to vote, abstention is only permitted after formally requesting an exemption due to travel or illness. Abstaining voters who fail to receive an exemption are required to visit an electoral judiciary office and pay a fee of 3–10% of the regional minimum wage (roughly \$3.50 BRL or \$1.60 USD). For citizens who fail to pay the fee, the electoral law[3] forbids participation in civil service exams or public bidding processes, working in the government, obtaining a passport, enrolling in a public university, or obtaining loans from state banks.[4]

Are the penalties for abstention sufficient to compel citizens to vote? For most Brazilians, the monetary penalty for abstaining is small. The average monthly wage in 2013 was about \$500 USD, so the fine amounts to less than 0.4% of an average worker's monthly income. The nonmonetary penalties, however, can be more costly, but primarily to voters who use state services that are affected by noncompliance with the law. The services affected by failing to vote, such as obtaining a passport and taking a civil service exam, are primarily those accessed by the middle and upper class. To provide evidence on this point, we asked a national sample of Brazilian voters whether they used any of the state services in the last three years that are affected by failure to comply with the law.[5] Only about 15% of the sample used any of the state services in the previous three years, but this average masks considerable variation by education status, which we use as a proxy for class (see next section). Figure 1 plots the average number of state services used by education status and

[2] Even in contexts with relatively strong CV laws such as Australia, however, residual absenteeism can still create small, but persistent, representational biases (McAllister 1986). The Australian case indicates that robustly enforced financial sanctions for absenteeism may reduce turnout inequality, but need not eliminate it completely.
[3]See Article 7.1 of Law 4.737.
[4]In Online Appendix A, we provide the text of the relevant laws.
[5]The survey was conducted by the survey firm *Instituto Análise* in 2014, and total sample size is 1230 respondents. All figures and uncertainty estimates presented in the article take into account sample weights and the sampling procedure used in the survey. Details about the survey are found in Online Appendix B.

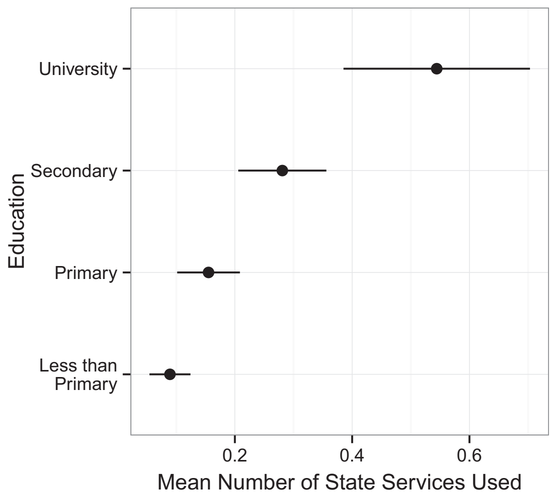

Fig. 1 Average number of state services used by education status. Lines represent 95% confidence intervals.

demonstrates that more educated Brazilians are more likely to use state services that would be affected by failure to vote on Election Day.

Given that the two types of penalties—a monetary fine and access to state services—affect voters of different socioeconomic strata differentially, it is not immediately clear how CV will affect inequality in turnout. If poorer voters are highly sensitive to even a modest fee and wealthier voters do not particularly value access to state services, then the finding that CV compresses inequality in turnout should apply. If, however, wealthier voters do value access to state services affected by abstention, then the standard hypothesis could be wrong and CV could *increase* political inequality rather than reduce it.

3 Research Design and Data

Our main data set (Cepaluni and Hidalgo 2015) of over 140 million registered voters is drawn from the individual-level voter record maintained by the Brazilian Superior Electoral Court (*Tribunal Superior Eleitoral* or TSE). Our analysis is the first to use the full Brazilian individual voter file.[6] Specifically, our data measure whether every registered Brazilian voter turned out for the first round of the 2012 municipal elections for mayor and city councilor.

We use educational achievement to measure socioeconomic status because education is strongly correlated with income (Lam and Levison 1991). According to the 2010 census, for example, Brazilians who completed secondary or higher education earned, on average, twice as much as Brazilians with less education. In our data, education is only measured at the time of registration, which means that it will be mismeasured for voters who received further schooling after the date of registration. To avoid the measurement error induced by failure to update registration records, we coarsen the education variable into whether or not the voter completed primary education.[7] By age sixteen—the earliest age at which citizens are eligible to register—completion of basic primary education should be accurately measured.

To estimate the effect of CV, we use two samples. For younger voters, we define the treatment variable T_i for voter i as 1 if the voter is eighteen or older on Election Day and 0 otherwise. For

[6]We obtained the data after submitting a request to the Brazilian Superior Electoral Court (TSE). While initially denied access to the data out of privacy concerns, an appeal to a judge at the court resulted in release of the data. Earlier data were not available.

[7]Specifically, we code voters who declare their highest education level as "illiterate," "read and write," and "primary education incomplete" (*Ensino Fundamental Incompleto*) as having less than primary education. For the older sample, education should be less mismeasured, as all voters were re-registered in 1986. In the Online Appendix K, we present separate results for all education levels among this older sample.

older voters, T_i is 1 if the voter is sixty-nine or younger on Election Day. For each sample, we define the forcing variable X_i as the number of days between voter i's birthday and Election Day multiplied by –1 for those voters with $T_i = 0$. In all estimation samples, we exclude illiterates, as they are not affected by the law.

3.1 *Specification and Inference*

Our research design identifies the average treatment effect for Brazilian citizens born on October 7, 1994, and October 7, 1942. The age thresholds in the election law allow for the estimation of treatment effects using an RD design under the assumption that the potential outcomes of voters are continuous at the threshold (Hahn, Todd, and Van der Klaauw 2001). This assumption is plausible here because the timing of births is unlikely to be affected by the precise date of the election.[8] Furthermore, covariates appear balanced at the cutoff (Online Appendix E) and estimates are generally close to 0 at placebo thresholds (Online Appendix J).

Because the finest resolution with which we observe time of birth is the day of birth, we collapse our individual-level data by aggregating turnout by birth date (or birth date by level of education) for all individuals born within one year of $X_i = 0$ (Lee and Card 2008). To estimate treatment effects, we follow conventional practice and use a local linear regression with triangle kernel weighting. In the main text, we present results using a sample-specific bandwidth—the range of data kept around $X_i = 0$—selected by the Calonico, Cattaneo, and Titiunik (2014) algorithm. In addition, we estimate standard errors using their robust standard error estimator, as well as present their bias-corrected confidence intervals. Finally, to compute the standard error and confidence interval around the difference between subsamples (more versus less educated), we use the sum of the variances of each point estimate as an estimate of the variance of the difference. In Online Appendix G, we show that our conclusions are robust to an alternative approach that assumes local randomization in a two-day bandwidth around the threshold.

For our sixty-nine- to seventy-year-old sample, we estimate treatment effects conditional on being registered. Conditioning on registration could risk the introduction of post-treatment bias because the decision to register could be affected by eligibility for CV. For our sample of older voters, however, bias is not a concern, due to the way that Brazilian electoral law regulates voter registration. According to the law, a voter is removed from the voter rolls only when he or she fails to vote in three consecutive elections. Consequently, among voters turning seventy, the absence of CV cannot cause their registration status to lapse in the first election after they are no longer obliged to vote. To check this empirically, we show in Online Appendix D that there is no discontinuity in the number of registered voters around the threshold.

Among voters turning eighteen, CV does have an effect on registration, as it induces younger voters to register so as to comply with the law. This voter registration effect means we cannot condition on being in the voter registry for voters born in 1994. While the voter registration data gives us complete data on all voters who registered and their turnout decision, to compute the effect of CV, we also require data on the number of voters who *failed* to register, at least for the seventeen- to eighteen-year-old sample. To our knowledge, no administrative data exist that measures the total number of citizens born on each day. Instead, we use four waves from the Brazilian National Household Sample Survey (*Pesquisa Nacional por Amostra de Domicílios* or PNAD) to estimate this quantity. PNAD is a large household survey of over 350,000 individuals, and it records birth date for members of each sampled household.

[8] A potential threat to our inferences is other age-based discontinuities that occur at the eighteenth and seventieth birthday. Because we compare voters born within days of each other and the fact that the election occurs on a Sunday, bias from other discontinuities seems unlikely as most age-based policies, unlike CV, do not immediately go into effect. Furthermore, the consistency of our results across both thresholds suggests that other age-based discontinuities do not affect our overall conclusions. For example, citizens become eligible for a driver's license at age eighteen, but it is unlikely that individuals obtain their license immediately after their birthday, as passing the exam and processing paperwork would take several days, at a minimum. At the seventy-years-old threshold, public servants must retire, but because the election occurs on a Sunday, effects of retirement would not be immediate for the public servants in our sample.

The raw PNAD survey estimates are noisy even with a large sample size, so we smooth the data using a semiparametric smoother to provide stable estimates of the number of unregistered voters born on each day in a one-year window around October 7, 1994. Specifically, to compute these quantities, we fit a flexible generalized additive model (Hastie and Tibshirani 1990) on the PNAD data. The model treats the number of people born on each day as smooth function of day of the year and includes day-of-the-week fixed effects. The number of unregistered voters with a given birth date is simply the difference between the predicted value and the number of people registered according to the voter registry.[9]

4 Results

We begin by focusing on citizens born in 1942 because—for this sample—we need not account for the effect of CV on the decision to register, thus making analysis simpler. The overall result of the article is summarized in Fig. 2, which shows the effect of CV among voters with less than primary education (left) and those with primary education or more (right). Citizens past their seventieth birthday on Election Day and for whom turnout is voluntary are plotted on the left of 0, whereas sixty-nine-year-olds are on the right of each plot. Unsurprisingly, CV increases turnout in both samples. Against the expectations of the existing literature, however, the effect is larger on voters who have completed primary education. Turnout levels under voluntary voting are similar across the two groups, but CV causes the average turnout level to increase more among the more educated.

Formal estimates of the pattern observed in Fig. 2 can be found in the top panel of Table 1. For these specifications, we show estimates from local linear regressions estimated separately on each side of the discontinuity in the full sample, the sample of voters with primary education or greater, and the sample of voters with less than primary education. These estimates can be interpreted as the proportion of each sample near the threshold that votes because of CV ("marginal voters," to use the terminology of Fowler 2015).[10] The RD effect averaged over all citizens, irrespective of education, is reported in the first column. We find that CV increases the probability of turning out by about 0.044.

As shown in the second column in Table 1, the RD effect among voters with primary education or more is about 0.067, whereas the effect among less educated voters (third column) is about 0.034. In other words, the estimated effect among the more educated is almost two times the effect among those who never completed primary education. As indicated by the fourth column, this difference is statistically significant. These results imply that 13% of more educated voters near the threshold would still not vote under CV, whereas among less educated voters, 16% would not vote. The proportion of voters, however, who would vote in the absence of CV ("always voters") is 81% in both strata. To characterize these results slightly differently, the proportion of citizens induced to vote by CV that are educated is 0.45, which is a substantially higher rate than the proportion of educated citizens among those who never vote (about 0.25) and those who always vote (about 0.29) irrespective of CV.[11] In sum, when subject to voluntary voting, more educated and less educated voters at the threshold turn out at almost identical rates, but the CV effect is larger for the more educated and consequently increases inequality in turnout.

While we rely on the Calonico, Cattaneo, and Titiunik (2014) algorithm for choosing the mean square error-optimal bandwidth for our main estimates, our point estimates are somewhat sensitive

[9]In Online Appendix H, we include specifications that incorporate the extra modeling uncertainty of this step via a nonparametric bootstrap that respects the sampling structure of the PNAD survey.

[10]To make this interpretation, one must make a monotonicity assumption that voters are not demobilized by CV (or mobilized by voluntary voting).

[11]The proportion with primary education among "marginal voters," "always voters," and "never voters" was calculated using the method described in Angrist and Pischke (2008, 171) to characterize "compliers" in instrumental variables analyses. For marginal voters, for example, we use the ratio between the turnout effect among educated voters to the effect among the full population (with $X_i = 0$) to estimate the relative prevalence of education among marginal voters to the prevalence of education among all citizens. We then multiply this ratio by the estimated proportion of educated voters in the full population to obtain an estimate of the proportion of marginal voters who are educated. Estimates for always voters and never voters are calculated in a similar fashion.

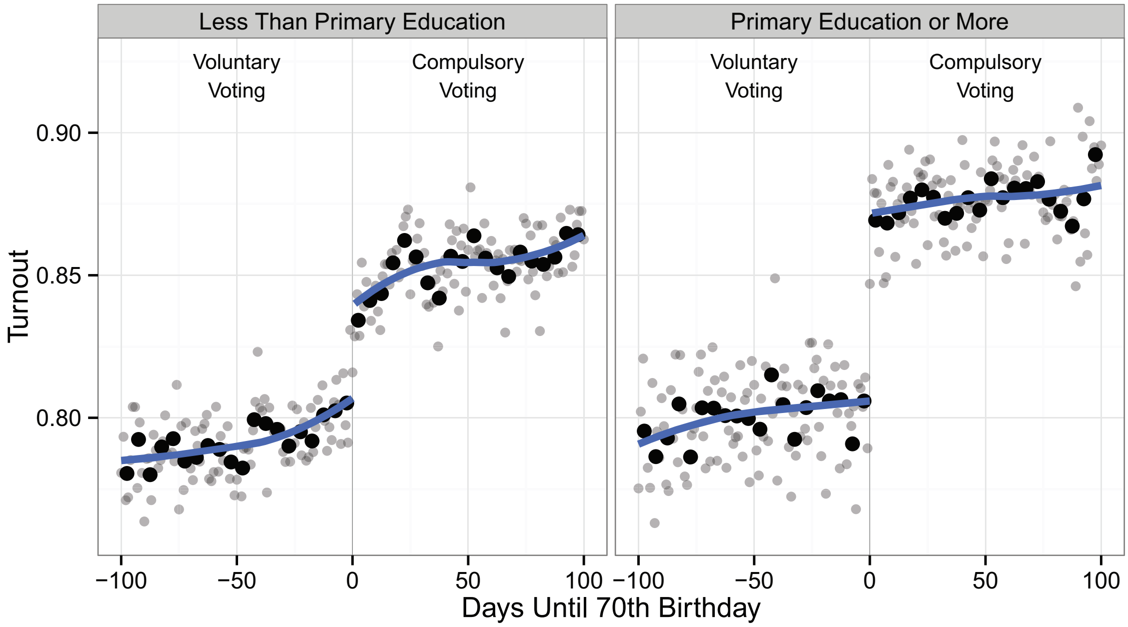

Fig. 2 Effect of CV by education status. Black points represent average turnout in five-day bins, whereas gray points represent average turnout in one-day bins. Lines are estimates from a Loess model.

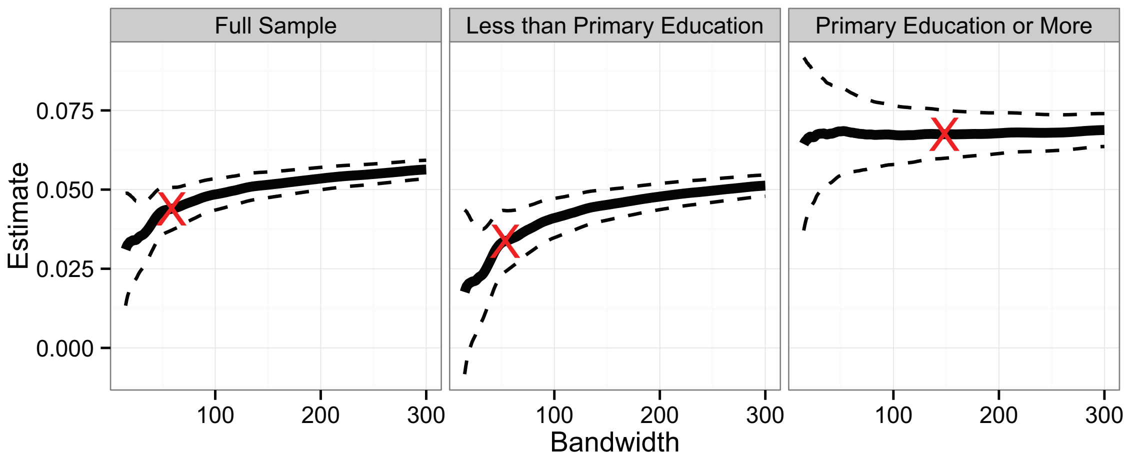

Fig. 3 Robustness to alternative bandwidth choices for the sixty-nine- to seventy-year-old sample. This plot shows how coefficients vary by using bandwidths ranging from fifteen to 300 days for estimates using the full sample, citizens with primary education or more, or citizens with less than primary education. Points marked with an "X" are bandwidths selected using the Calonico, Cattaneo, and Titiunik (2014) procedure. Dashed lines are 95% confidence intervals.

to other bandwidth choices, especially for voters with less than primary education. In Fig. 3, we show how our estimates vary for all bandwidths between fifteen and 300 days. As is evident in the first two panels, point estimates decrease with smaller bandwidths for the full sample and for voters with less than primary education. The difference in effects between more and less educated voters, however, remains consistently large across all bandwidths.

To show that this heterogeneity by education is not limited to those turning seventy, we study the age-based discontinuity at eighteen. As discussed earlier, estimating the effect of CV among this population is more challenging because turning eighteen affects the propensity both to register and

Table 1 Effect of CV on turnout

	Age 69–70			
	All	*Educated*	*Less educated*	*Difference*
Estimate	0.044	0.067	0.034	0.034
Std. error	0.004	0.005	0.005	0.007
95% CI	[0.035, 0.050]	[0.058, 0.076]	[0.021, 0.042]	[0.020, 0.048]
Bandwidth	58	148	53	
No. of individuals	237,721	175,553	154,260	
Observations	117	297	107	
	Age 17–18			
	All	*Educated*	*Less educated*	*Difference*
Estimate	0.117	0.144	0.057	0.086
Std. error	0.011	0.017	0.01	0.02
95% CI	[0.096, 0.138]	[0.113, 0.180]	[0.033, 0.074]	[0.047, 0.125]
Bandwidth	92	89	46	
No. of individuals	1,775,921	1,019,661	363,329	
Observations	185	179	93	

Notes. Top and bottom panels show estimates for the Age 69–70 and Age 17–18 subsamples, respectively. Table reports coefficients from a triangle kernel weighted local linear regression on turnout rates by day of birth. MSE-optimal bandwidths and robust confidence intervals

to turn out. While we account for this issue by imputing the number of unregistered voters using survey data, our inferences for seventeen- to eighteen-year-olds are necessarily more model dependent than the earlier analysis. Furthermore, by not conditioning on registration, the causal estimand for citizens born in 1994 applies to a different population than estimates that only use registered voters.

The bottom panel of Table 1 shows our RD estimates for seventeen- and eighteen-year-olds. The first column indicates that CV increases the overall probability of turning out by about 0.117. Furthermore, as indicated by the second through fourth columns, we again find evidence of statistically significant heterogeneity by education. We find that the effect among the more educated (roughly 0.144) is about two times the size of the effect among the less educated.[12] The results for this younger sample parallel the results for the older sample, which increases our confidence in the generalizability of these treatment effects.[13]

5 Conclusion

CV is often recommended as a powerful means to reduce persistent inequalities in political participation that are commonplace in democracies around the world. While CV undoubtedly reduces these inequalities in many settings, discussions of the effects of CV often gloss over variations in how CV is implemented in different societies. Just as other efforts to increase participation may inadvertently exacerbate inequality (Berinsky 2005; Enos, Fowler, and Vavreck 2014), evidence we present in this article indicates that the types of sanctions used to incentivize voters can have negative implications for the distributional effects of CV. Yet to date, most studies of CV have understood variation in the effects of CV to mainly reflect differences in enforcement and fines. Future research should consider the full menu of sanctions that are employed to punish abstainers. If these punishments include restrictions in access to state services used by wealthier voters, CV may have unwelcome consequences with respect to political inequality.

With respect to the broader consequences of CV, it is important to stress that our results are conditional on the overall institutional framework already in place. It is possible that if CV were

[12]In Online Appendix I, we show the robustness of this result to alternative bandwidths.

[13]In Online Appendix L, we rule out an alternative explanation that differences by education status reflect differences in information about the thresholds and not differential sensitivity to nonmonetary sanctions.

removed, the response of parties and other political actors would result in changes to voter mobilization that would exacerbate political inequality in political participation, rather than reduce it, as our results might suggest. A dramatic change in overall turnout levels if CV were to be removed might lead political parties to disproportionately focus on mobilizing wealthier voters, thus undercutting the equality-promoting effects of removing nonmonetary sanctions that drive up turnout among the nonpoor. Unfortunately, our design cannot provide firm evidence on these broader consequences without very strong assumptions. Future research on the consequences of CV with strong nonmonetary sanctions that accounts for changes in the mobilizational strategies of political elites would provide valuable evidence for better understanding the institutional determinants of turnout.

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