Does the Monetary Cost of Abstaining Increase Turnout?

Causal Evidence from Peru

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We study the elasticity of turnout on the size of the monetary fines that governments impose on those who fail to vote. We leverage a discontinuity in the size of monetary fines in Peru, where voters in districts above an arbitrary cutoff in poverty rates face higher fines for not voting relative to voters who reside in districts below the cutoff. Using individual-level data on millions of voters for every regional and national election between 2010 and 2016, we find that turnout increases slightly in districts with higher fines—an effect of roughly one percent. This modest effect is similar across socioeconomic groups and elections. Our results highlight a challenge that governments face in designing the sanctions in compulsory voting systems: how to increase turnout without disproportionally hurting the poor or raising turnout inequality.

Keywords: Compulsory voting, Electoral fines, Regression discontinuity, Voter turnout, Peru

More than 20 countries around the world have compulsory voting (CV) laws that require eligible citizens to register and vote in elections (CIA, 2015).[1] The conventional wisdom is that these laws increase turnout and reduce turnout inequality between the rich and the poor, a claim that finds support in cross-national and within-country studies (e.g., Lijphart, 1997; Franklin, 1999; Panagopoulos, 2008; Singh, 2011; Fowler, 2013; Bechtel, Hangartner and Schmid, 2018).

Countries with CV use a variety of penalties to induce people to vote, with monetary sanctions of various amounts being the most common (Jackman, 2001). In Switzerland and Brazil, for instance, failing to vote carries a fine of roughly three dollars US; abstention in Australia and Uruguay, in turn, can lead to penalties of up to twenty dollars US (Table 1). Does increasing the pecuniary cost of abstaining induce people to vote? And, if it does, are there particular groups of individuals who are more adversely affected by these costs? In particular, does CV reduce turnout inequality because the poor are disproportionately hurt by monetary fines?

To answer these questions, we leverage an institutional reform in Peru that established electoral fines that were scaled according to the poverty level of the voter's district of residence.[2] As described below, voters in Peru face sharply different monetary fines from abstaining to vote depending on the district in which they live. Specifically, voters in "non-poor" districts face fines that are double and quadruple the fines in "poor" and "extremely poor" districts, respectively. Using a regression discontinuity design that exploits the arbitrary cutoffs used to assign fines to Peru's electoral districts, we estimate the causal effect of higher fines on the turnout for the electorate at large and for different educational groups (which we use as a proxy for social class). Our information comes from the complete Peruvian voter rolls, which include infor-

[1] This is about 17% of all countries, according to the International Institute for Democracy and Electoral Assistance (IDEA).
[2] Law 28859, August 3, 2006.

mation on turnout, education status, date of birth, and the district of residence for over 20 million citizens for every election between 2010 and 2016.

Table 1: Compulsory Voting Fines Around the World

Country	Level of Enforcement	Fine	Amount	Denial of Access to State Services
Australia	Very Strict	Yes	20 USD	Yes
Brazil	Very Strict	Yes	0.3-1 USD	Yes
Peru	Very Strict	Yes	6-25 USD	Yes
Singapore	Very Strict	Yes	5 USD	Yes
Uruguay	Very Strict	Yes	28-85 USD	Yes
Bolivia	Strict	Yes	22 USD	Yes
Nauru	Strict	Yes	4.6 USD	No
Thailand	Strict	Yes		
Belgium	Moderate	Yes	30-152 USD	Yes
Ecuador	Moderate	Yes	38.6 USD	Yes
Liechtenstein	Moderate	Yes	20.5 USD	No
Turkey	Moderate	Yes		No
Argentina	Very Moderate	Yes	2.5-7.5 USD	Yes
Greece	Very Moderate	No		Yes
Luxembourg	Very Moderate	Yes	123-1230 USD	No
Panama	Very Moderate	No		
Paraguay	Very Moderate	Yes	7-15 USD	No
Costa Rica	No Enforcement	No		No
Demo. Rep. of Congo	No Enforcement			
Dominican Republic	No Enforcement	No		No
Egypt	No Enforcement	Yes	0-28 USD	Yes
Honduras	No Enforcement	Yes	1 USD	No
Mexico	No Enforcement	No		No
Venezuela	No Enforcement	No		No

The table includes all countries for which voting is currently compulsory and details whether abstentions are sanctioned with fines and/or administrative penalties, as well as the extent to which these sanctions are enforced. Empty cells indicate missing information. Source: International Institute for Democracy and Electoral Assistance (IDEA).

Our findings indicate that doubling the size of the fine increases turnout across the board, but only slightly. On average, turnout increases by roughly one percent in districts with higher fines, compared to those with lower fines. The average turnout rate in the "control" districts was 81.6% for the period; thus, we estimate that roughly

8.7% of individuals who abstain from voting in districts with lower fines would vote if they were required to pay twice as much in fines. The effect is similar for both regional and general elections (although only statistically significant at conventional levels for the former) and for voters with different levels of education. Thus, imposing higher fines has a moderate effect on turnout both across types of voters and elections. By raising turnout in districts with fewer poor and extremely poor voters, the practice of scaling fines based on district-level poverty rates may help to explain the recent rise in turnout inequality in Peru.[3]

Our study speaks to and expands on a long strand of research that examines the effect of CV on turnout. For instance, using cross-national data, Panagopoulos (2008) and Singh (2011) find that harsher penalties and stricter enforcement jointly increase turnout. Recent studies by Cepaluni and Hidalgo (2016), in Brazil, and Jaitman (2013), in Argentina, in turn, use individual-level data to estimate the joint effect of administrative and monetary penalties on voter abstention for different social classes. They do this by analyzing specific age groups who are typically too young or too old compared to the median voter. The monetary fines that we study, by contrast, are imposed on all voters between the ages of 18 and 70. We are also able to isolate the causal effect of the monetary fine from other types of sanctions—such as the restriction to access public services.

Our paper also complements three recent studies that examine the effect of the Peruvian CV system on turnout. León (2017) finds that delivering information about the reduction of fines (relative to 2006) prior to the 2010 election had a small, negative impact on turnout. By examining several electoral rounds, our study suggests that the modest effect of the higher fines on turnout is not solely a problem of disinformation or voters' unfamiliarity with the system of penalties. Carpio et al. (2018) and Gonzales, León Ciliotta and Martínez (2019) also analyze the effect of monetary

[3]We return to this issue in the concluding section.

fines on turnout. Unlike these studies, we use individual-level records from the official voter rolls to measure turnout, which allow us to examine the effect of the CV staggered system for voters of different socioeconomic strata. In addition to this, we examine a larger number of elections, including both national and regional races.

A Model of Compulsory Voting

Most research on turnout focuses on understanding the decision to vote in countries where voting is voluntary. This has led researchers to explore the parameters affecting the individual costs of voting, such as the probability of casting a decisive vote and the extrinsic and intrinsic benefits obtained from supporting a victorious candidate. Under compulsory voting, the individual decision to vote is shaped not only by the cost of voting, but also by the cost of abstaining, through penalties that governments impose on those who fail to cast a ballot. To analyze the first-order implications of CV penalties on turnout, we adapt the turnout model proposed by Panagopoulos (2008). For any individual i the cost of abstention is:

$$C_{abstain,i} = q \times p + (1 - q) \times z_i \times p, \tag{1}$$

where q is the probability that the state will collect the fine, z_i is the individual level probability of using a service denied to non-voters (if any), and p is the monetary fine applied to non-voters.[4]

Higher fines increase the cost of abstention, incentivizing citizens to vote. Fines may be one strategy through which CV reduces the participation gap between the rich and the poor, since the incidence of monetary sanctions should decrease with income. In our model, "enforcement" is effectively a function of both the state's capacity to collect the fine and the ability of the state to restrict government services to

[4]Our model does not include a parameter for social norms, but CV laws may increase turnout by changing norms about what is "normal" or "good" behavior. For instance, less motivated voters often vote in countries where turnout is high (Gerber and Rogers, 2009).

non-voters. We think of q as a country-level parameter that relies on the development of a state bureaucracy that is able to collect fines (for example, by deducting the fine amount from an individual's bank account). For high values of q—when the state can easily collect the fine—the second term is close to zero and an increase in the value of the fine applies equally to all potential voters. Our measure of the probability of using those state services that are restricted to non-voters, z_i, is an individual level parameter. In countries with low values of q, the effect of monetary fines should be larger for individuals with higher values of z_i. For example, when the state can restrict access to services that are more likely to be used by middle- and upper-income voters, such as renewing a passport or depositing a check, the costs of abstention will be higher for wealthier voters (Cepaluni and Hidalgo, 2016). In other countries, services may include those used more frequently by the poor.

Compulsory Voting in Peru

The Peruvian Constitution establishes that voting is compulsory for all registered citizens between the ages of 18 and 70, and voluntary for those older than 70.[5] All Peruvian citizens are automatically registered to vote by the National Registry of Identification and Civil Status (RENIEC) when they formally acquire their government identity card (DNI, for its Spanish acronym) at the age of 18. Before each election, RENIEC sends this information to the National Office of Electoral Processes (ONPE). Historically, residents of rural or poor areas were excluded from voting rolls because they lacked identity cards. Stimulated by the emergence and implementation of several social programs, RENIEC initiated an aggressive program to provide identity cards to those who did not have them, effectively enfranchising a large portion of the population. By 2011, only 0.7% of eligible voters were undocumented (RENIEC 2012). Citizens who fail to vote under mandatory voting laws face monetary fees and

[5]Exceptions include being sick on Election Day, the death of a family member, and being the victim of a natural disaster. The vote is also granted to married citizens who are sixteen-years-old or older.

extensive administrative consequences—access to many state services is prohibited until the fines are paid.

Until the presidential elections of 2006, the fine for those who failed to vote was 4% of a tax unit (UIT; equivalent to around 27% of the monthly minimum wage that year). However, in August 2006, the Peruvian Congress passed a bill to reduce the fine's value, in large part due to the onerous burden it placed on the poor.[6] As a national legislator stated during debate about reducing the fine, "the penalties are no trifle [*ni moco de pavo ni chancay de a veinte* in the original]."[7] The law introduced a new system that adjusted the level of the fine to the poverty rate of each electoral district. Specifically, it established the classification of Peruvian districts into three categories depending on their level of poverty—"non-poor," "poor," and "extremely poor"—and specified a different fine for each of these types of districts.

An initial classification of electoral districts occurred in 2006 but, shortly before the 2010 election, was replaced by an updated classification that employed data from the 2007 census. In "non-poor" districts, voters pay a fine that is 2% of a tax unit (UIT; around $28USD in 2011); in poor districts, the fine is just 1% of a UIT ($14USD); in extremely poor districts, voters pay only 0.5% of a UIT ($7USD). Despite the law's intention to reduce the financial burden of the fine, these monetary penalties are still high for most Peruvians. Peru's monthly minimum wage was $218USD in 2011; in a "non-poor" district, the fine represents 12% of the minimum wage. More than 78% of Peruvians—27.8 million in 2007—live in "non-poor" districts. However, 28% of the individuals in these "non-poor" districts are themselves poor or extremely poor.[8] Thus, a majority of poor Peruvians still face a steep fine for not voting.

[6]See Law 28859. The law also suppressed the administrative sanctions, known as "civilian death." However, the electoral agencies kept enforcing them, since they are included in regulations unaffected by Law 28859; see Law 26497 and Law 26859.
[7]Diario de los Debates, 11/03/2005, 16th reunion.
[8]Of all individuals considered poor or extremely poor (around 10.6 million people), 57% live in "non-poor" districts.

Failure to pay this monetary penalty restricts access to a variety of official services, including any state services for which users need to present their DNI, banking transactions (such as cashing a check), buying or selling a house or car, changing a home address, obtaining or renewing a passport or driver's license, getting married/divorced, accepting a job in the public sector, and enrolling in social security and social programs. These restrictions are likely to affect citizens of different economic status equally. To validate this claim, we use data from Peru's National Household Survey (ENAHO) from 2013. In this survey, the National Institute of Statistics and Informatics (INEI) asks a national sample of Peruvian citizens (31,690 households) whether they used a battery of state services in the last 12 months. Less educated Peruvians use services at similar rates as the more educated.[9] In addition, using our regression discontinuity design (described below), we examine whether people in districts with high and low fines use services at different rates. We find that, on average, people use state services at similar rates across either side of the discontinuity.[10] We find that, on average, people use state services at similar rates at both sides of the discontinuity.

To increase compliance, the national government established an agency exclusively responsible for the collection of fines in 2012. The process to collect the fine starts with a formal notification, sent by the JNE to the non-compliant individual's home, stating that the individual has fifteen days to pay the fine (which they can do at any branch of the JNE or the National Bank, a public bank with 600 offices across the country).[11] Once the opportunities for voluntary payment are exhausted, the JNE starts the process of coercive collection.[12] If the individual has a bank account (as roughly 40% of Peruvians did as of 2018), the JNE will seize the individual's accounts

[9] See Appendix, Section C.

[10] While the ENAHO is not representative at the district level, it does include sampled households for a large number of districts. See Appendix, Section A.2.

[11] The details of the process are specified in Resolution N 052-2012-P-JNE.

[12] Whereas the fine formally prescribe four years after the election, it no longer does so once the JNE starts the process of coercive collection.

and collect the fine. If the individual does not have a bank account, the JNE will instead block their credit and debit cards. In cases of individuals without a bank account or a debit/credit card, the JNE will place the individual on a debtors' list until the fine is paid. Peruvian banks charge a fee for collecting the fine as part of this coercive collection process. The fee increases the cost of the fine significantly; while the amount charged varies by bank, it is generally between $40 to $60 USD.

Data and Research Design

We use voter files containing turnout and demographic data for more than 20 million voters for two regional elections (2010, 2014) and two presidential races (2011, 2016).[13] These data are maintained and validated by the three Peruvian institutions that oversee every electoral process: the JNE, the ONPE, and the RENIEC. Our data measures turnout for each voter and includes information on voters' year of birth, the district in which they live, their gender, and their education level.[14] We obtained anonymized versions of these voting records (i.e., without individuals' national identity card numbers, addresses, signatures, fingerprints, or first names and last names) after filing a public information request with ONPE, following the procedures established in Law 27806.

We study the effect of monetary sanctions on turnout, focusing on variation in the level of monetary sanctions across Peru's 1,816 districts.[15] The classification of districts into different poverty categories is done by INEI based on the poverty rates derived from the last national census available.[16] The INEI first classifies the percentage of individuals in a district that fall into each of three poverty categories—

[13] We study only first-round elections. Regional elections occur every four years. Presidential elections are held concurrently with national legislative elections every five years. Presidential candidates must obtain at least 50% of the votes to win.

[14] This information is self-reported by every citizen at the time of processing their national identification card.

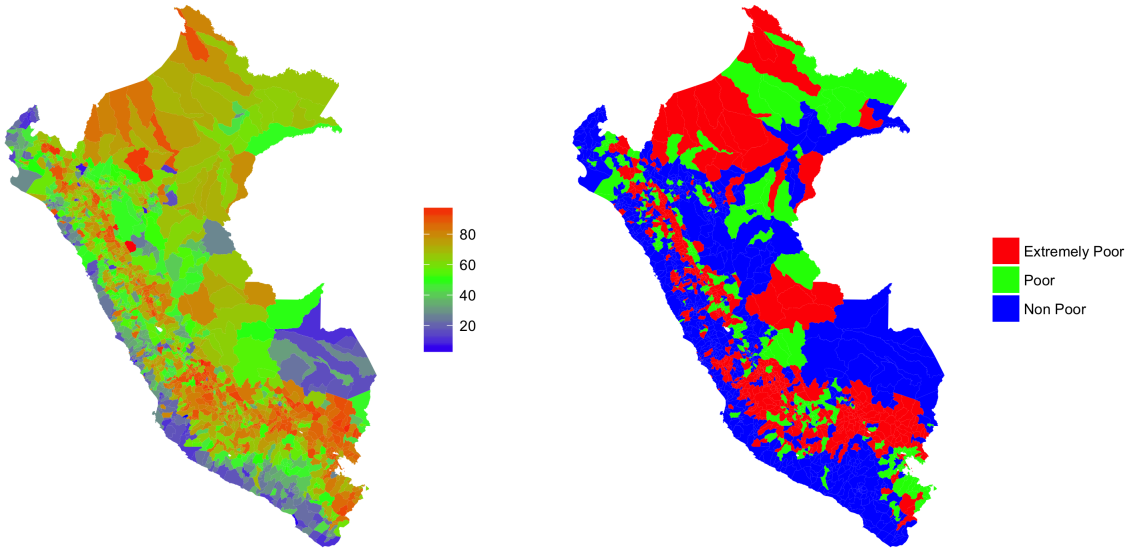
[15] This number includes both provincial capitals and municipal districts.

[16] For the analysis in this paper, the 2007 national census.

"extremely poor," "poor," and "non-poor."[17] It then classifies districts according to the poverty category that accounts for a plurality of the district population. To give one example, districts where the number of "non-poor" residents exceeds both the number of "poor" residents and the number of "extremely poor" residents are classified as "non poor."

We exploit the discontinuity generated by the fact that, conditional on the two categories with the largest share of the district population, districts with similar poverty rates for those categories are given different poverty classifications based on the category with the largest share of the population. This can be seen in Figure 1, which shows that similar levels of district poverty can lead to strikingly different classifications depending on this discontinuity.

Figure 1: Geographic Distribution of Poverty and District Classification



(a) Total poverty (%) (b) INEI's classification of districts

Total poverty is defined as the sum of "poor" and "extremely poor" residents. Data from INEI's 2007 National Census.

[17] Poverty is defined as having an income below the cost of a basket of minimum goods and services; extreme poverty is defined as having an income below the cost of a minimum food basket.

To estimate the effect of increasing the level of monetary sanctions on turnout, we use a regression discontinuity (RD) design. We construct the running variable by identifying the two population groups that account for the largest shares of each district's population. For each district j, we denote the share of the population corresponding to the *least poor* of these two categories as $popshare_{j1}$ and the share of the population corresponding to the poorest of these two categories as $popshare_{j2}$. The forcing variable z_j is $popshare_{j1}$ minus $popshare_{j2}$. Positive values of the forcing variable indicate that those who fail to vote in district j pay the highest possible fine among the two top categories, whereas negative values indicate that individuals would pay the lowest possible fine among the two top categories. Thus, we define the "high fine" treatment as,

$$highfine_j = \begin{cases} 1 & \text{if } popshare_{j1} > popshare_{j2} \\ 0 & \text{if } popshare_{j1} < popshare_{j2} \end{cases}$$

Table 2 summarizes the three possible comparisons between $popshare_{j1}$ and $popshare_{j2}$, the fine amounts that correspond to each case (in USD, according to 2011 values), and the number of districts in each. Our main analysis focuses on pooling districts with a majority of the population in poverty or extreme poverty and poverty or non-poverty—i.e., the top two rows in Table 2. This allows for a straightforward interpretation of the treatment, since in both cases those who fail to vote in districts assigned to the "high fine" owe the state twice the amount than those in the "low fine" districts.[18]

To estimate the effect of the "high fine" treatment on turnout, we first subset the voter file to individuals for whom voting is compulsory, i.e., Peruvians between the ages of 18 and 70. We then aggregate our individual-level data to the level of the

[18] In Appendix B.2, we show that our results are robust to including the remainder 46 districts. We also show results by type of discontinuity.

Table 2: Districts by Top Poverty Categories

Top Poverty Categories in District	High Fine	Low Fine	Nr. of Districts
"Extremely Poor" & "Poor"	\$14 USD	\$7 USD	691
"Poor" & "Non-Poor"	\$28 USD	\$14 USD	1080
"Extremely Poor" & "Non-Poor"	\$28 USD	\$7 USD	46

Values correspond to the 2011 General Election.

district.[19] We fit two, separate local-linear regressions above and below the cutoff, using a bandwidth around the cutoff that minimizes the mean squared error of the regressions. The RD effect is merely the difference between the two estimated intercepts. We include fixed effects for year—since we pool data from every election since 2010—and for each of the two types of districts—since, as shown in Table 2, we can think of the design as a blocked randomization depending on the top poverty categories in the district. We report the bias-corrected confidence intervals and p-values developed by Calonico, Cattaneo and Titiunik (2014). The key outcome variable is the district turnout rate, which we examine in the aggregate, for regional and general elections, and for voters with different education levels. We follow Cepaluni and Hidalgo (2016) and use voters' level of education to proxy for socioeconomic status. We construct a measure indicating whether voters have less than primary education or primary education or more; less than high school education or high school education or more; and less than college education or college education or more. When comparing turnout rates between sub-samples, we compute the standard error of the difference using the sum of the variances of each point estimate as an estimate of the variance of the difference.

RD designs rely on a basic "continuity" assumption: at the exact point where a district j is classified as "non poor," or either "poor" or "extremely poor"(i.e., when

[19] This approach increases standard errors relative to standard approaches that treat individual observations as independent and identically distributed (I.I.D).

the score is zero), counterfactual outcomes should be continuous. This identification assumption would be broken if districts were able to sort around the threshold; this is unlikely since the INEI is responsible for measuring poverty rates and is independent from the interests of local stakeholders. In the appendix, we show that density tests around the cutoff fail to reject the null of no sorting ($p = .37$).[20] Similarly, municipalities on both sides of the cutoff should be, on average, identical on all pre-treatment variables except for those that are affected by treatment status (Appendix A.2). We find no statistically significant differences for districts with and without a high fine for a number of relevant pre-treatment covariates. These include, critically, turnout in the 2006 pre-treatment elections, the mean household income in each district in 2007, and the percentage of the district population above the poverty line, living in poverty, facing extreme poverty. We also fail to find statistically significant differences in the number of polling stations and the average number of state services used by people in the district (for districts included in ENAHO's sample), all of which may affect the cost of turnout.

Results

We begin our discussion with a graphical analysis, focusing on the relationship between turnout rates and the running variable—the difference in the size of the two largest socioeconomic groups. Figure 2 shows binned averages of the turnout rate at the district level in every election between 2010 and 2016. The figure includes lines on each side of the treatment cutoff that represent local-linear estimates of the relationship between turnout rates and the running variable. The shift in the outcome variable around the zero threshold indicates that the difference in turnout rates between treatment and control districts is a direct effect of the difference in fines within these two groups of districts. The upward shift appears to be about one percentage point.

[20] See Appendix A.1.

Figure 2: Effect of High Fine Treatment on District Turnout, All Elections

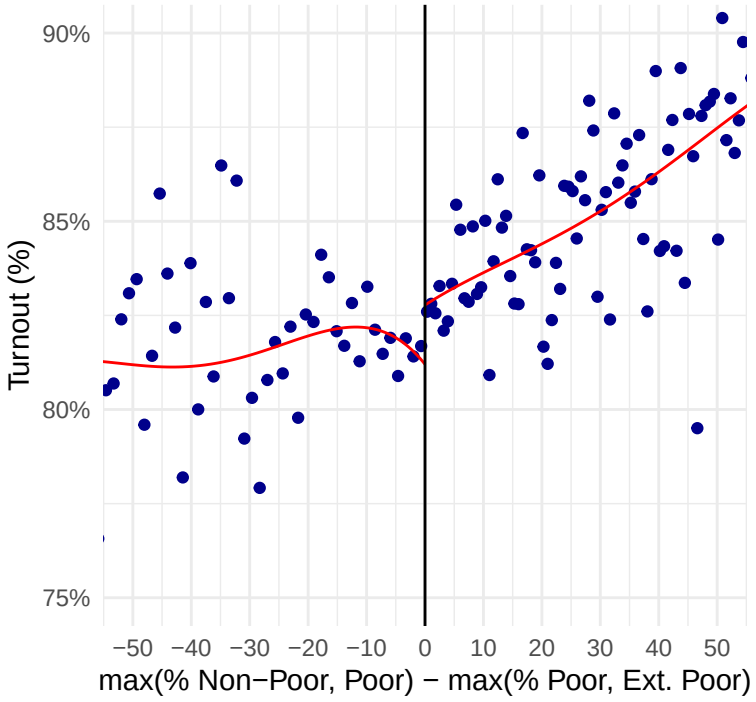


Table 3 reports local-linear regression estimates for the difference in turnout rates between treatment and control districts. The higher fine increases turnout rates by 1.2% relative to the control group, with robust 95% confidence intervals ranging from .1% to 2.1%. This represents an 8.7% increase in turnout among those who would abstain from voting in the absence of the treatment. The table also presents results broken down by type of election—regional or general—and education groups. The estimated effect is somewhat similar for regional and general elections, though the effect is only statistically significant at conventional levels for the former. Turnout is usually lower in "second-order" elections (Norris et al., 2004, p. 163), which means more voters may show up to the polls in order to avoid a higher penalty. The difference, however, is negligible and not statistically significant.

Our analysis sheds light on whether and how CV reduces the participation gap between poorer and wealthier voters (Lijphart, 1997; Jackman, 2001). One possi-

Table 3: Effect of High Fine Treatment on District Turnout

	Est.	95% CI	$p-val$	$n_c \mid n_t$	h
All Elections (2010-2016)	0.012	[0.001:0.021]	0.031	1499 | 1690	16.4
Panel A: Election Type					
General Elections	0.011	[-0.007:0.027]	0.25	815 | 934	18.8
Regional Elections	0.012	[0.003:0.023]	0.014	818 | 934	18.7
difference	-0.002	[-0.022:0.018]	0.861		
Panel B: Education Group					
Primary or More	0.012	[0.001:0.025]	0.04	1639 | 1874	19
Less Than Primary	0.008	[-0.002:0.015]	0.113	1361 | 1531	14.4
difference	0.003	[-0.012:0.018]	0.658		
High School or More	0.012	[0.002:0.024]	0.016	1635 | 1870	18.9
Less Than High School	0.01	[0:0.019]	0.051	1499 | 1690	16.4
difference	0.002	[-0.012:0.017]	0.764		
College or More	0.012	[0.001:0.021]	0.029	1499 | 1690	16.4
Less Than College	0.011	[-0.002:0.024]	0.105	1595 | 1806	17.7
difference	0.001	[-0.015:0.018]	0.884		

The running variable is the difference in the size of the two largest socioeconomic groups in each district. The estimate is the average treatment effect at the cutoff, estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. Columns 3–7 report, respectively, 95% robust confidence intervals, robust p-values, units in treatment and control, and main optimal bandwidth. Fixed effects by year and type of comparison (non-poor vs. poor and poor vs. extremely poor).

ble reason for this is that the marginal cost of the fine for not voting decreases with income. On the other hand, the range of state services that the Peruvian government denies to non-voters affects both poor and non-poor voters, increasing the likelihood of enforcement across income groups (Appendix C). In other words, the distance between the expected and nominal value of the fine should be similar for people in different income brackets. Thus, we expect that the high fine treatment has the largest effect on poor voters.

The last panel in Table 3 shows results for the effect of the "High Fine" treatment for several subgroups of voters. For those with primary, high school, and col-

lege education or higher, respectively, we find that the treatment increases turnout by 1.1%-1.2%. These effects are significant at the .05 level. When we define groups instead by their lack of education we find similar effects, ranging from .8% to 1.1%. These effects, however, are not statistically significant at conventional levels, with p-values ranging from .051 to .11. Moreover, these small differences between subgroups are not themselves statistically significant. Thus, monetary fines are unlikely to reduce the gap between the rich and the poor. If anything, the fact that higher fines are charged in districts with fewer poor voters suggests that turnout inequality increases as a result of the staggered monetary fines.

Are these estimated effects big or small? In 2011, the minimum wage was $218USD and the mean household income was $131USD. Thus, doubling the size of the fine—up to a maximum of $28USD—represented a sizable increase in the fine's share of Peruvian's average incomes. In light of this, the effect of the treatment is not trivial, especially considering that the last three presidential elections in Peru were decided by razor-thin margins (2.9%, 0.2%, and .3% in 2011, 2016, and 2021, respectively). However, there are two caveats that we should keep in mind: First, RD designs identify a "local effect"—i.e., for units whose score in the running variable is essentially zero. Thus, we cannot extrapolate our finding to units far from the cutoff. Second, while we identify a similar effect for both regional and presidential races, the point estimate is only significant for the former. The competitiveness of regional elections varies by district.

The appendix reports several robustness tests. One key decision in RD designs is the selection of the bandwidth. We show that our point estimates are increasing on the bandwidth and relatively stable and statistically significant for larger bandwidths (roughly above 10%-15%). The point estimates reported in Table 3 that are not significant at a conventional level become significant for bandwidths larger than those selected by the Calonico, Cattaneo and Titiunik (2014) algorithm (Appendix B.1). In

addition to so-called "density" and "balance" tests, we also examine a number of variables that may affect the costs and benefits of turnout, including the average number of state services accessed by individuals, the number of polling stations, and the number of candidates in regional elections. We also run a placebo test using two fake cutoffs at plus and minus 10% of the running variable. These fake treatments do not lead to a statistically significant effect on turnout. All of these tests are included in Appendix A.2. Finally, we show that our point estimates remain largely the same (and statistical precision increases) once we include the 46 districts for which the two largest socioeconomic groups are the "Non-Poor" and "Extremely poor" (Appendix B.2), as well as when we break down the analysis by type of discontinuity ("Poor" and "Non-Poor," "Extremely Poor" and "Poor," and "Non-poor" and "Extremely poor"). The effect of the "high fine" treatment is somewhat larger (roughly 1.5%) and statistically significant for the "Poor" and "Non-Poor" (doubling the fine) and "Non-poor" and "Extremely poor" (quadrupling the fine) discontinuities. However, the point estimate becomes smaller and statistically insignificant for the "Extremely Poor" and "Poor" discontinuity (Appendix B.3).

Discussion and concluding remarks

Higher levels of electoral participation are thought to be associated with a variety of outcomes, including less economic inequality and more support for labor parties and social public spending. (Bechtel, Hangartner and Schmid, 2016; Chong and Olivera, 2008; Fowler, 2013; Mahler, 2008). In turn, low participation rates are frequently associated with deficits in the democratic process, such as the political exclusion of marginalized groups and the poor (Lijphart, 1997).

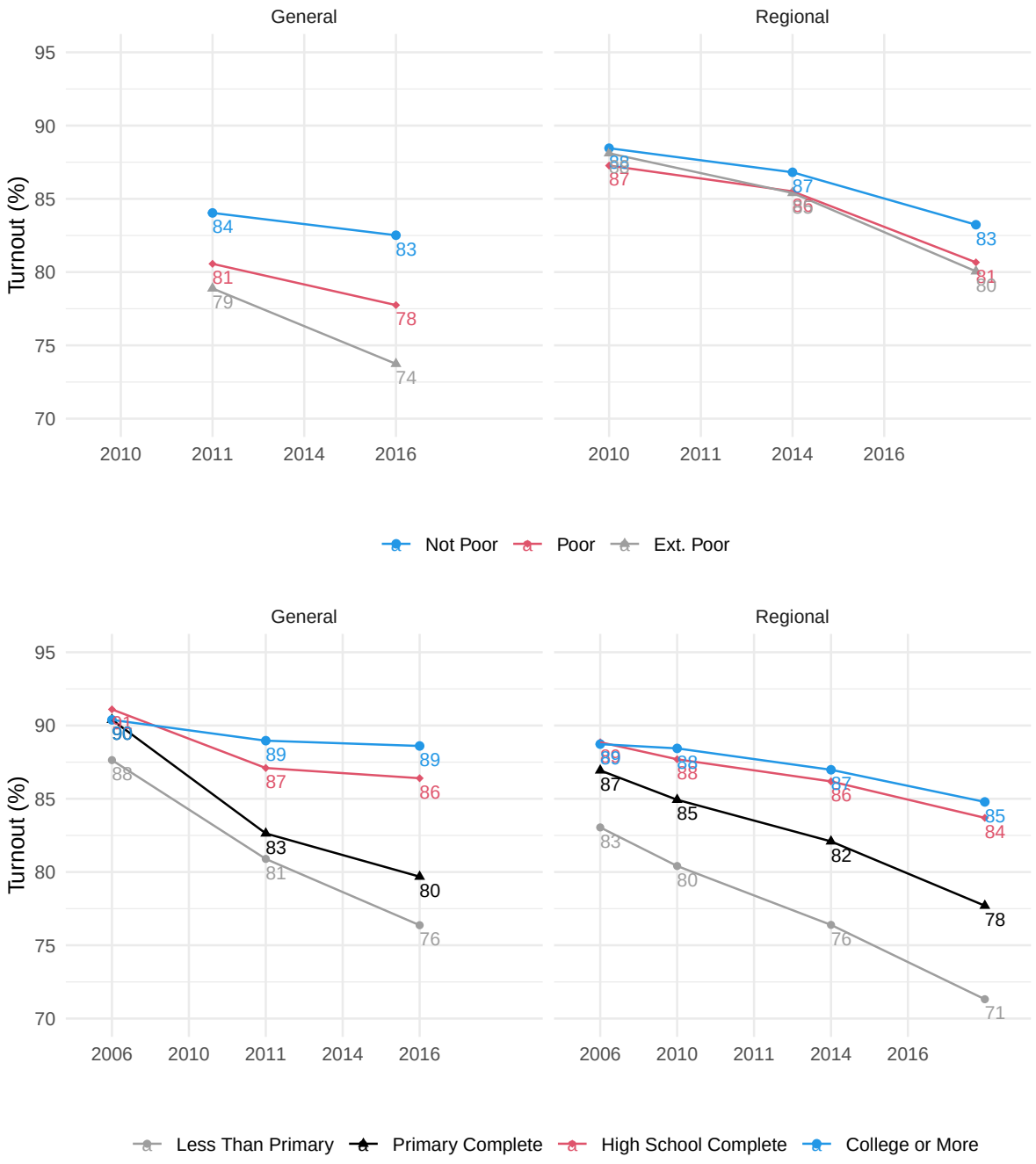
Our analysis suggests that higher fines in some districts in Peru cause a modest increase in voter turnout, an effect that is similar across voters of different socioeconomic status. Our design allows us to isolate the effect of the size of monetary fines,

while leaving other factors——including administrative penalties—unaffected. This system of staggered fines—which raises participation in districts with a larger proportion of wealthier voters relative to poorer districts—may explain in part the increase in the turnout gap between socioeconomic groups in Peru. There were 27.9 million Peruvians in 2010, of which 23.1 million lived in "high fine" districts and 4.8 million resided in "low fine" districts. In "low fine" ("high fine") districts, 42% (6.7%) of people were extremely poor; 34% (23%) were considered poor; and 22% (69%) were not poor.[21] Turnout in the "extremely poor" and "poor" districts has fallen more rapidly than in non-poor districts, particularly in regional elections (Figure 3). The turnout of the less educated—those voters who did not complete a high school education—has fell steadily in every election held after the implementation of the staggered system in 2010 until 2018, while turnout among more educated groups also declined, though at a lower rate.

The expected cost of abstention depends not only on the monetary cost of the penalty but also on the likelihood of enforcement (Panagopoulos, 2008; Singh, 2011). Peru is considered a case of moderate to high enforcement (Table 1), particularly following the creation of the collection agency in 2012 (an increase of q in our simple model), due to the large number of state services denied to non-voters (a higher z_i) across a range of socioeconomic groups. The fact that we fail to find an effect for less educated groups, who should be especially sensitive to the cost of the fine, may indicate another case of forbearance towards the poor (Feierherd, 2020; Holland, 2015). What matters, however, is voters' perception of enforcement. León (2017) shows that randomly informing voters of the change in penalties for abstaining to vote in 2010 did not lead voters in the treatment and control groups to alter their perceptions about the consequences of not voting (p. 63). In line with this, Gonzales, León Ciliotta and Martínez (2019) shows that districts with higher monetary fines have higher turnout,

[21] See Table D in Appendix.

Figure 3: Electoral Turnout by District Type and Education Level (2002-2018)



even after controlling for district characteristics that may correlate with higher enforcement, such as being a provincial capital.

To conclude, our results highlight a dilemma that governments face in designing the sanctions associated with CV systems. One motivation to implement CV laws

is to reduce the participation gap between the rich and the poor (Lijphart, 1997). But imposing monetary and administrative costs on those who abstain from voting in order to encourage their participation can be particularly costly for the poor, who by definition lack economic resources and rely on the state to meet their most basic needs. Peru's 2006 reform, which stratified the fines based on socioeconomic indicators, was in part motivated by legislators' concerns about the financial burden of CV fines at the time. During their debates, Peruvian legislators described a tension between increasing turnout rates and limiting the economic costs that poor voters face when they skip the polls. One legislator noted: "Can you imagine what the old system of penalties meant for citizens in extreme poverty, many of whom could not vote without traveling long distances?"[22] Another legislator argued: "What we are doing is helping the poorest people (...) Those enrolled in *Juntos* [Peru's conditional cash transfer program] receive 100 soles. The old fine was 132 soles. The fine is higher than the help they receive from the state!" (p. 1335). The fine is higher than the help they receive from the state!" (p. 1335). Other legislators challenged these arguments: "We are fomenting—probably, unintentionally—electoral absenteeism and favoring political apathy" (ibid, p. 1337).

The solution settled on by Peruvian legislators—to lower fines in poorer districts—is not unique to Peru; other countries also implement a system of differential fines to reduce the burden of failing to vote for poor voters. Brazil, for example, maintains fines that are a function of the regional minimum wage. Our results highlight that this solution hides a tradeoff. While lowering fines for poor voters can limit the extent to which CV sanctions block access to state services, it can also subvert the effects of CV on turnout inequality. In Peru, higher fines increase turnout; however, by lowering fines in poor districts, the Peruvian system discourages turnout in districts with a disproportional number of poor voters.

[22]Diario de los Debates, 11/03/2005, 16th reunion, p. 1331

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Online Appendix

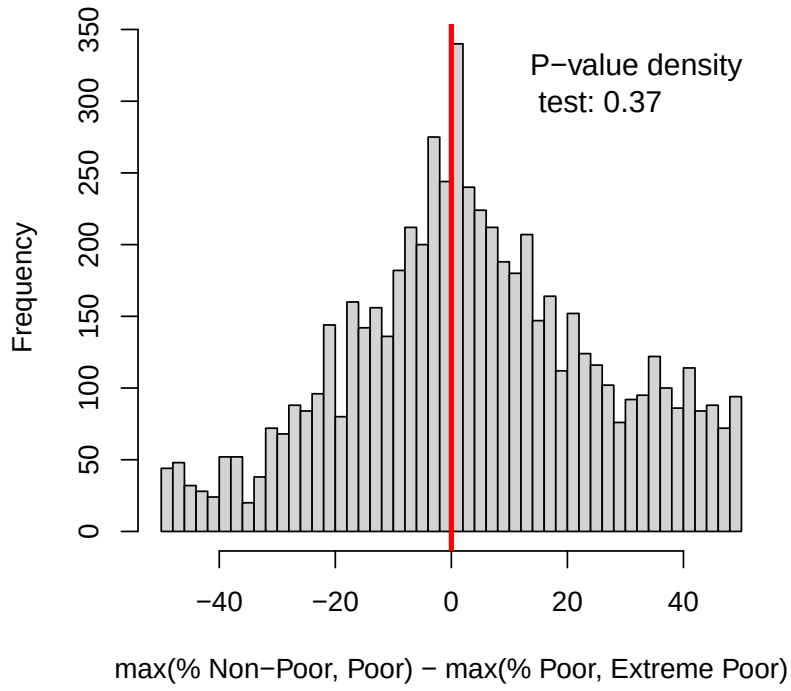
Contents

A Tests of design

A.1 Density test

A potential threat to the regression discontinuity design comes from the possibility that units—in our case, districts—can sort near the threshold. Figure A1 shows the histogram for the running variable. The figure also includes the p-value of the null hypothesis that the density of the running variable is continuous at the cutoff using the local density estimator developed by Cattaneo, Jansson and Ma (2016). The density test fails to reject the null hypothesis of no sorting ($p > .37$).

Figure A1: Histogram of the forcing variable.



A.2 Balance and placebo tests

We also find no statistically significant differences for districts with and without a high fine for a number of relevant pre-treatment covariates. This include the turnout in 2006, the share of non-poor, poor, and extremely poor people in 2007, and the mean household income that year. We also include three variables which are not pre-treatment but that may affect the cost of voting: the number of polling stations and polling booths in 2010 and the mean number of state services used by people in the district according to the ENAHO 2013 survey. Finally, we examine whether the political supply of electoral competition in regional elections by looking at the number of candidates (and the its logged transformation) for the 2010 and 2014 rounds.

Table A1: RD effect on covariates.

	Est.	95% CI	$p-val$	$n_c \mid n_t$	h
% Turnout in 2006	0.008	[-0.007, 0.027]	0.237	357 | 395	15.2
% of Extreme Poor	0.471	[-1.431, 1.997]	0.746	361 | 407	15.7
% of Non-Extreme Poor	0.467	[-0.81, 1.91]	0.428	303 | 345	12.8
% of Non-Poor	-0.537	[-1.938, 1.209]	0.65	408 | 467	18.6
Household income	-6.864	[-17.929, 2.174]	0.125	359 | 404	15.5
Polling stations	-0.123	[-0.596, 0.385]	0.674	359 | 401	15.3
Polling booths	-1.719	[-9.141, 6.19]	0.706	350 | 388	14.7
Services used	-0.13	[-0.42, 0.115]	0.263	237 | 284	19.7
Number of candidates	0.164	[-0.254, 0.666]	0.38	788 | 835	19.8
Number of candidates (log)	0.029	[-0.033, 0.109]	0.291	816 | 853	20.5

The running variable is the difference in the size of the two largest socioeconomic groups. Estimate is average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. Columns 3–7 report, respectively, 95% robust confidence intervals, robust p-value, units in treatment and control, and main optimal bandwidth. Fixed effects by type of discontinuity (non-poor vs. poor and poor vs. extremely poor).

Table A2: RD effect on pre-treatment covariates.

	Est.	95% CI	$p-val$	$n_c \mid n_t$	h
Panel A: % Running variable + 10%					
All Elections (2010-2016)	-0.011	[-0.022, 0.002]	0.106	1134 | 2201	20.1
General Elections	-0.011	[-0.032, 0.007]	0.205	616 | 1260	23.8
Regional Elections	-0.01	[-0.022, 0.002]	0.103	600 | 1172	21.9
Panel B: % Running variable - 10%					
All Elections (2010-2016)	-0.002	[-0.014, 0.009]	0.683	1851 | 1198	16.2
General Elections	0.004	[-0.015, 0.018]	0.884	1097 | 684	20.2
Regional Elections	-0.003	[-0.016, 0.008]	0.482	966 | 626	17.2

The running variable is the difference in the size of the two largest socioeconomic groups plus/minus 10%. Estimate is average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. Columns 3–7 report, respectively, 95% robust confidence intervals, robust p-value, units in treatment and control, and main optimal bandwidth. Fixed effects by type of discontinuity (non-poor vs. poor and poor vs. extremely poor).

B Robustness tests

B.1 Alternative bandwidths

Figure A2: Effect of High Fine Treatment on District Turnout, All Elections

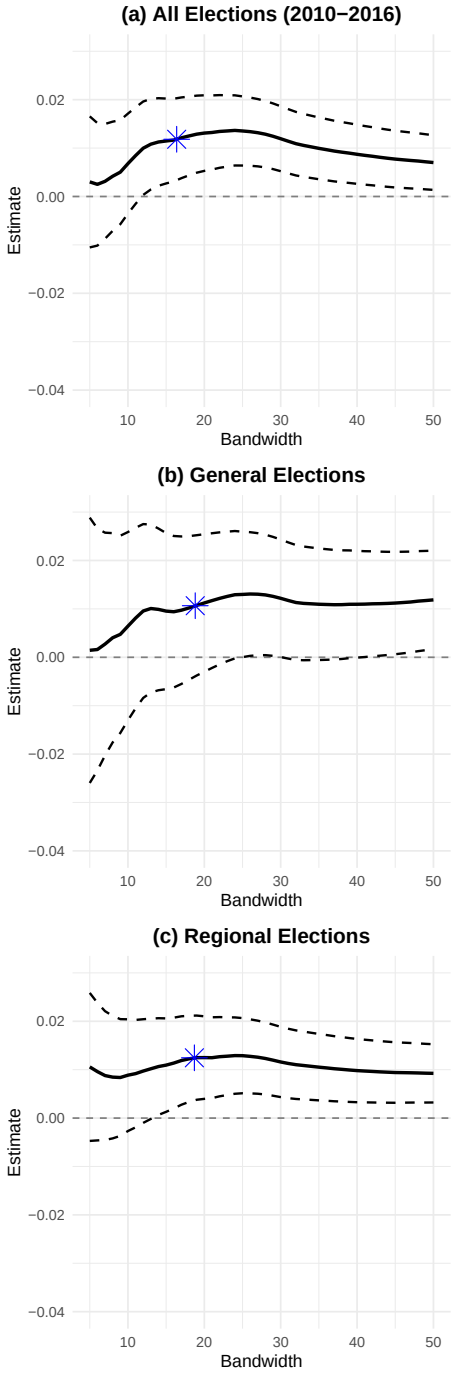
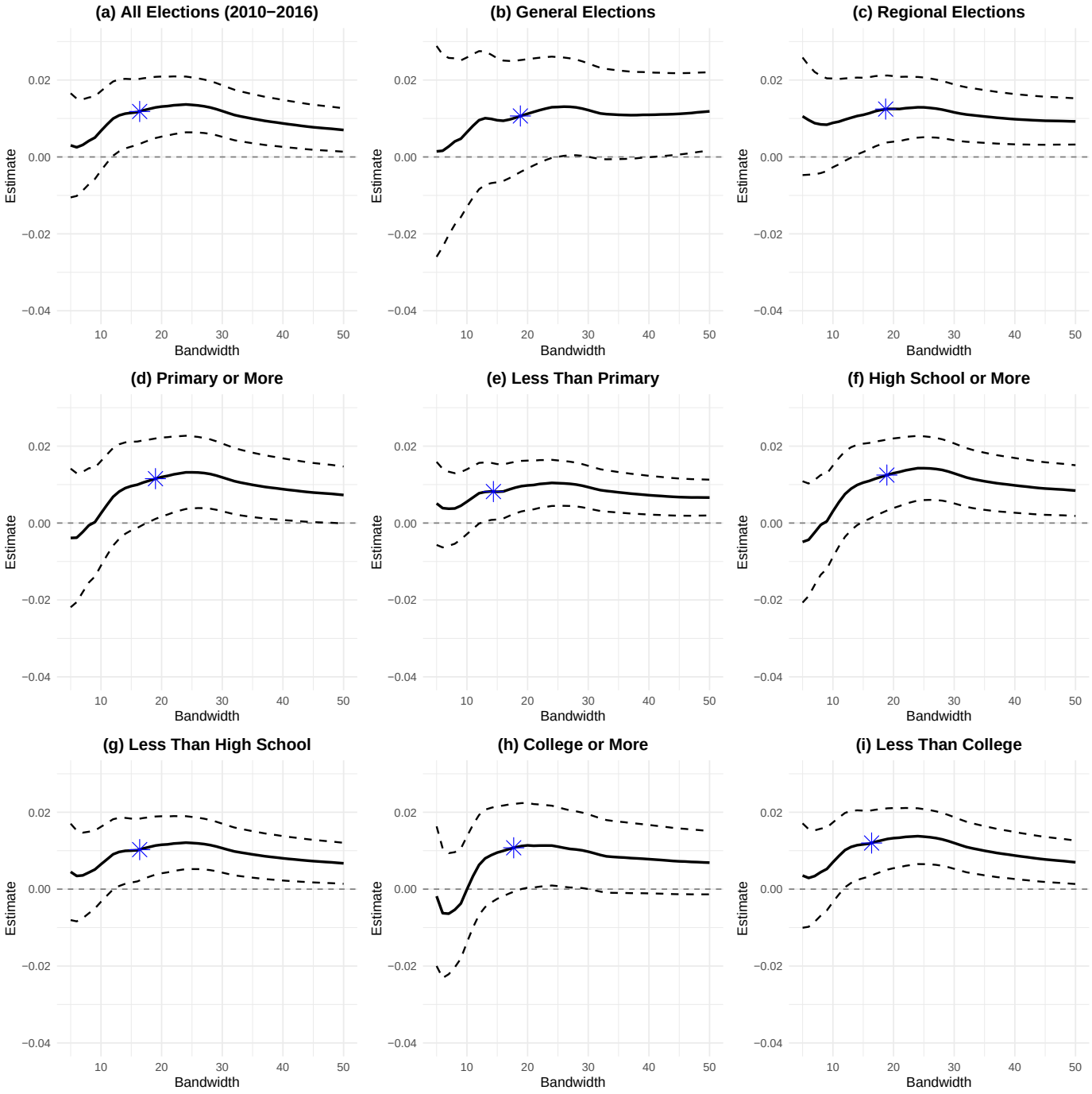


Figure A3: Effect of High Fine Treatment on District Turnout, All Elections



B.2 Including *"Ext. Poor" vs. "Non-Poor"* districts

Table A3: Effect of High Fine Treatment on District Turnout (All Districts)

	Est.	95% CI	$p-val$	$n_c \mid n_t$	h
All Elections (2010-2016)	0.012	[0.002, 0.022]	0.019	1603 | 1786	16.8
Regional Elections	0.012	[-0.004, 0.029]	0.136	909 | 1008	20.3
General Elections	0.01	[0.001, 0.02]	0.037	894 | 1008	20
Less Than Primary	0.007	[-0.003, 0.014]	0.21	1397 | 1567	13.9
Primary or More	0.013	[0.002, 0.026]	0.021	1735 | 1946	19.1
Less Than High School	0.01	[0.001, 0.019]	0.035	1619 | 1794	17
High School or More	0.013	[0.003, 0.025]	0.011	1727 | 1942	19
Less Than College	0.012	[0.002, 0.022]	0.018	1603 | 1786	16.9
College or More	0.015	[0.002, 0.029]	0.021	1591 | 1782	16.7

The running variable is the difference in the size of the two largest socioeconomic. Estimate is average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. Columns 3–7 report, respectively, 95% robust confidence intervals, robust p-value, units in treatment and control, and main optimal bandwidth. Fixed effects by type of discontinuity (non-poor vs. poor and poor vs. extremely poor).

B.3 Results by type of discontinuity

Table A4: Effect of High Fine Treatment on District Turnout (by Discontinuity)

	Est.	95% CI	$p-val$	$n_c \mid n_t$	h
"Poor" & "Non-Poor"	0.015	[0.002, 0.026]	0.026	506 | 943	13.5
"Ext. Poor" & "Poor"	0.004	[-0.017, 0.019]	0.887	691 | 496	12
"Ext. Poor" & "Non-Poor"	0.141	[0.024, 0.396]	0.027	36 | 12	1.7

The running variable is the difference in the size of the two largest socioeconomic. Estimate is average treatment effect at cutoff estimated with local linear regression with triangular kernel and MSE-optimal bandwidth. Columns 3–7 report, respectively, 95% robust confidence intervals, robust p-value, units in treatment and control, and main optimal bandwidth. Fixed effects by type of discontinuity (non-poor vs. poor and poor vs. extremely poor).

C The importance of administrative sanctions

One way that non-monetary fines influence people to vote is through access to state services or public procedures. Thus, in cases where states have limited capacity to collect fines, they can force citizens to pay them by denying them access to a variety of public services until the fine is paid. Therefore, it is logical to assume that it is the citizens who use certain services or public procedures the most that are more sensitive to this type of sanction. In short, these voters know with a greater degree of certainty that they will have to pay the fine, one way or another.

In an exploratory way, it could be affirmed that it is the citizens with the greatest resources who would be most affected. This is because most notarial, judicial, and banking proceedings are primarily a part of the daily dynamics of individuals who are fully integrated into the formal circuits of the economy. In short, voters belonging to the middle and upper class socio-economic strata would be the most sensitive to this type of sanction.

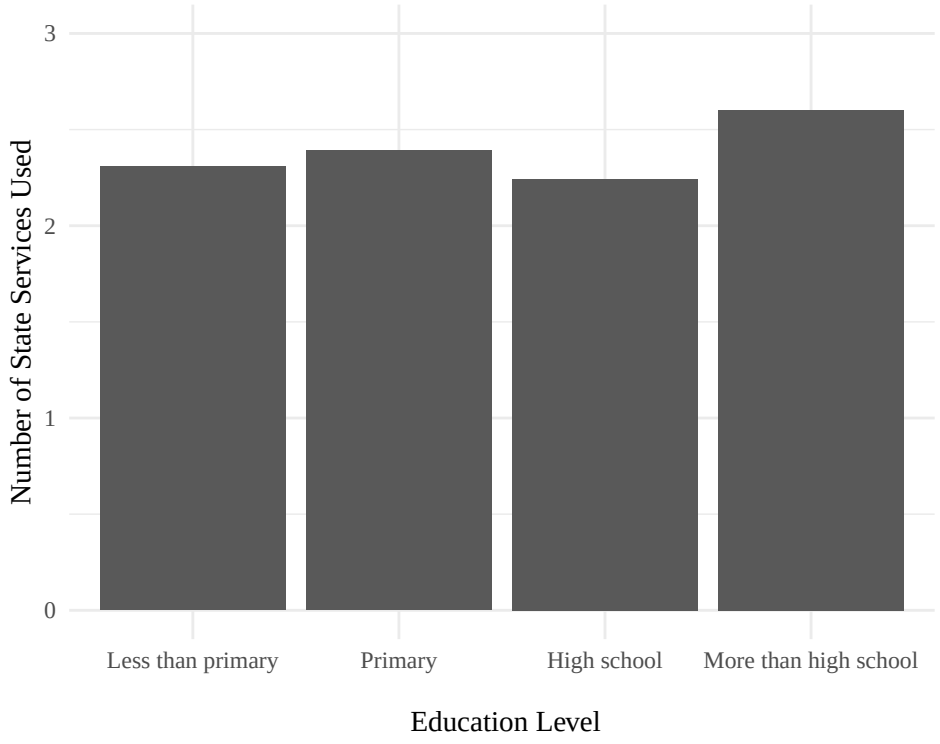
This insight has been corroborated in other countries that maintain similar restrictions for residents who fall into a status of omission. In Brazil, for example, citizens who do not pay the fine cannot take public administration exams, participate in public bidding processes, obtain a passport, enroll in a public university or apply for loans from state banks. Cepaluni and Hidalgo (2016) show that in this context, CV increases participation, especially of the more educated voters, since they are more likely to be affected by the restrictions that we have just described.

Returning to the Peruvian case, are citizens with greater purchasing power the ones who would be mainly affected by non-monetary sanctions? To provide some empirical evidence about the fact that in Peru, restrictions on state services affect citizens with a different economic status, we use data from 2013's National Household Survey of Peru (ENAHO). In this survey, the INEI asked a national sample of Peru-

vian citizens (31,690 households) if they used a battery of state services in the last 12 months.

In Figure A4, it can be observed how the use of state services varies according to educational levels, variables that we use as a proxy for socioeconomic status. The results reveal that people with a higher socio-economic position (a higher educational level) use on average a greater number of public services than other citizens (approximately 2.6 services). However, unlike that country, in Peru the less educated also use state services at a similar rate.

Figure A4: Average Number of Public Services Used by Education Groups



D Population and income by district type

Table A5: Population (2010) and mean household income (2007) by district type.

	Not poor	Poor	Extreme poor	Income (2007)	Total population
"Non-Poor"	158850901	4931896	1155937	290.6	21972924
"Poor"	786231	1139773	739043	167.9	2665047
"Ext. Poor"	565434	987351	1694307	136.3	3247093
Total	17236756	7059020	3589287	766.6	27885064