Turnout, Political Preferences and Information: Experimental Evidence from Peru

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This version: November 2015

(December 2014)

Barcelona GSE Working Paper Series

Working Paper nº 691

Turnout, Political Preferences and Information:
Experimental Evidence from Peru[*]

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October 10, 2015

Abstract

I combine a field experiment with a change in voting laws reducing the fine for absten- tion to assess the effects of monetary incentives to encourage voter participation. In a real world election, using individual-level experimental variation in the perceived reduction of the fine for abstention and an objective measure of turnout, I estimate that a 10 percent decrease in the cost of abstention reduces the probability of voting by 2.2 percentage points. As predicted by the model presented, the reduction in turnout is driven by voters who (i) are in the center of the political spectrum, (ii) hold less political information and (iii) have lower subjective value of voting. The increase in abstention does not change aggregate preferences for specific policies, on average. Further, involvement in politics, as measured by the decision to acquire political information, seems to be independent of the level of the fine.

Keywords: Voting Behavior; Incentives to Vote, Electoral Politics, Public Choice, Peru
JEL Classification Codes: O10, D72 , O53, D71

1 Introduction

Electoral institutions that encouage voter participation in elections are widespread around the world. These institutions are often believed to help ensure voters' preferences are adequately represented. However, despite the increasing interest in devising effective policies to bolster electoral participation, we still don't have a credible estimate of the magnitude of voter's responses to the most obvious type of interventions, i.e. those that affect the cost of voting and the types of monetary incentives that governments and NGOs provide. Undestanding whether voters respond to changes in the cost of voting and to monetary incentives not only allow us to design effective policies, but also, to be able to interpret the effects of such policies on preference aggregation, we have to be able to undestand which types of voters respond to them. More generally, credible causal evidence on these questions will allow us to deepen our understanding on long standing questions in economics and political science, namely, what motivates voters to participate in elections?

In this paper, I combine a field experiment with a change in electoral laws that reduced the cost of abstention to identify the effect of monetary incentives on turnout, and examine its effects on the composition of the electorate and preference aggregation. The experimental design generated individual level variation in the cost of abstention in a real world election, allowing me to estimate treatment effects on turnout and its elasticity with respect to the cost of voting. In Peru, as in other thirty-four countries around the world, voting is compulsory, and abstention is penalized with a fine. In 2006, Congress passed a law reducing the fine for abstention, and the magnitude of the reduction depended on the poverty level of the district. However, this decision received very little media coverage and knowledge about the reduction in the fine was not widespread at the time of the 2010 municipal election (as confirmed by information from my baseline survey.) This provided the ideal setting to test the effects of monetary incentives on voter behavior. Just before the 2010 municipal election, I provided a randomly selected group of voters information about the *new* levels of the fine. The experiment took place in 29 villages in 10 districts in the region of Lima. In the follow-up survey, right after the election, I collected an objective measure of turnout by asking respondents to show their ID cards with official proof of voting. My reduced form estimates show that in places where the fine was reduced in half, the average voter was 2.7 percentage points less likely to vote, while where the fine was reduced to 25 percent of its previous value, turnout decreased by 5.7 percentage points. Further, exploiting the individual-level, within village, random variation in the perceived fine for abstention, I estimate that the elasticity of voting with respect to the cost is -0.22. Extrapolating these results, if the fine was eliminated, turnout could decrease from 94.2 percent (observed in my sample) to about 72 percent, roughly what we observe in countries where voting is voluntary (e.g. France, Spain, South Korea, etc.)

The reduction in turnout induced by the decrease in the cost of abstention is driven by

voters who (i) are in the center of the political spectrum, (ii) hold less political information and (iii) have a lower subjective value of voting (as measured by their interest in politics). These heterogeneous effects are consistent with the predictions of the theoretical model in the paper. In this model, voters are uncertain about the candidates' political position. Due to this uncertainty, there is a non-zero probability that they choose a candidate whose ideological position is far from the voter's, making a (costly) mistake. The probability of a voting mistake is higher among voters with low subjective value of voting, the uninformed and political centrists, which makes them more likely to abstain upon a decrease in the cost of abstention. This model is derived from the classic *calculus of voting* literature (Downs 1957; Rikker and Ordeshook 1968), and follows Degan (2006) and Degan and Merlo (2009).

However, the change in the composition of the electorate does not necessarily imply that the outcome of the election will be affected. The results indicate the reduction in turnout does not lead to a change in the specific policies preferred by the average voter in the sample, i.e. the policies preferred by the average voter who abstains due to the reduction in the fine are not different from the average voter who participate regardless of the lower fines. These result is consistent with Hoffman, et. al. (2015), who find that the elimination of compulsory voting is Austria's states did not lead to significant changes in fiscal policies. Further, voters who respond to the reduction in the fine by abstaining do not lose interest in politics, as measured by their decision to acquire political information.

Finally, I explore the effects of the reduction in the fine on politicians' ability to distort public choice by buying votes. Suggestive (but imprecise) results indicate that the exogenous change in the fine for abstention does not affect the incidence of vote buying, but it does increase the price politicians pay for the marginal vote at a rate of 6 cents per each Nuevo Sol (49 percent), making it more expensive for politicians to influence the outcome of the election.

The randomization was done within the village, and the data from my follow up survey shows that some voters in the control group reported lower perceived fines than in the baseline, suggesting that there could have been within-village information spillover. If voters with specific characteristics are the ones who also updated their priors about the fine, regressing the change of the perceived on turnout would yield biased results. To deal with this, I use the treatment status as an instrument for the change in the perceived value of the fine between the baseline and follow-up surveys. My IV results are robust to the inclusion of controls and village fixed effects, to different measures of the main dependent variable, and to a number of specification checks that deal with potential violations to the exclusion restriction. For example, one might be concerned that providing information about changes in the fine affected voter's perceptions about its enforcement – voters could perceive that the reduction of the fine signals that the government cares less about voting and will lower its effort to enforce compulsory voting. If this effect is different between the treatment and control groups, my

estimates will not only reflect the effects on the fine, but also of the changes in perceived enforcement. In the follow-up survey, I asked everyone what were the consequences of abstention, and voters in the treatment and control groups did not provide statistically different answers, ruling out the possibility of differential effects on perceived enforcement. Similarly, one can be concerned that the information treatment affected voter's intrinsic value of voting, by making civic duty more or less salient. Again, using data from the follow up survey, I show that there are no differential responses between treatment and control groups.

Overall, the results in this paper highlight the importance of monetary incentives in determining voter behavior. Further, by showing that these effects are not uniform across different groups of the population (in terms of their political preferences), I provide robust evidence that campaigns aimed at affecting electoral participation and improving representation might generate differential responses. Moreover, I show that we shouldn't expect changes in the electorate to inequivocally translate into changes in policies, but rather the way changes in participation are distributed across different groups of the population (with specific policy preferences) is a key issue to consider.

The results in this paper are related to several strands of the literature on voter behavior and electoral institutions. I contribute to literature on the determinants of voter turnout.[1] A few empirical studies use natural experiments to test whether changes in the cost of voting affect the likelihood of going to the polls on Election Day. Brady and McNulty (2011) show that an increase in the cost of voting induced by an unexpected reduction in the number of polling stations in California's 2003 gubernatorial elections generated 3.03 percentage point reduction in polling place turnout, while absentee vote increases by 1.18 percentage points. Hodler et. al. (2015) uses data from Switzerland's intrduction of postal voting to show that the reduction in the cost of voting increased turnout, and this increase is mainly driven by uninformed and less educated voters.[2] Another commonly used source of exogenous variation

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[1]A number of papers have studied voter mobilization campaigns that encourage participation, such as door-to-door canvassing or reductions in registration costs (Gerber and Green 2000 and 2001; Gerber, Green and Nickerson 2003; Gerber, Green and Shachar 2003; Gerber, Green and Larimer 2008; Arceneaux and Nickerson 2009; Gerber and Rogers 2009; Pons et al. 2014; Chong et al. 2015.). On the other hand, it has been documented that social pressure affect voter behavior, and information about encouragement schemes (some times) travel through social networks (See e.g. DellaVigna et al 2014, Gerber et al. 2008, Gine and Mansuri 2012, Fafchamps et al. 2012.). Further, it has been well documented that voting is habit forming: voting in one election significantly increases the probability of going to the polls in the next election (Gerber et al. 2003, Fujiwara et al. 2014). Another strand of the literature emphasizes that access to informative media increases participation. Areas where the TV or radio coverage expanded earlier were more likely to have higher turnout (Gentzkow 2006, Gentzkow, et. al. 2014, Lasen 2005). Likewise, access to news containing specific information about the candidates running in the local election also increases participation (Snyder and Strömberg 2010). This fact has been shown to hold with specific information campaigns at the individual level (Banerjee et al. 2011).

[2]Funk (2010) uses the same setting and shows that in smaller communities, where social preasure is higher, the introduciton of postal voting decreased turnout. Other important contributions to this literature expoloit changes in the cost of voting in the form of the elimination of poll taxes (e.g. Filer, Kenny and Morton 1991) or literacy tests (e.g. Cascio and Washington, 2014) in the U.S.

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in the cost of voting is the presence of inclement weather conditions (Knack 1994, Gomez et al. 2007, Hansford and Gomez 2010, Fraga and Hersh 2010). These studies find that, on average, an additional millimeter of rain tends to reduce turnout by about 1 percentage point. In terms of partisan effects, the results are mixed. One shortcoming of this literature is that the magnitude of the effects found is hard to interpret. Experiments allowing a direct measurement of the monetary costs of voting have been rare and small-scale. So far, they have relied on monetary rewards for voting offered by the experimenters. For instance, Gerardi et al. (2011) test several implications of costly voting models in the laboratory. The few field experiments with real voters also rely in variation in monetary incentives generated by the experimenters (Loewen, et al. 2008; Panagopoulos 2013; Shineman 2014).

Unlike the natural experiments used to study related questions, beyond providing reduced form effects, I am able to quantify the changes in the cost of voting at the individual level. Further, the external validity of my estimates is strengthen by the fact that I study voters in a real election, and an imperfectly executed law provides the opportunity to generate exogenous variation in the cost of voting. The changes in the perceived fine for abstention are induced by a randomly assigned treatment, which allows me to provide the first estimates in the literature of the cost elasticity of voting. This is a parameter necessary for evaluating policy interventions affecting the cost of voting, for example, an increase in the number of polling stations, reduction of transportation costs, electronic or proxy voting, availability of ID cards, among others.[3]

A related literature analyzes how representation and policy making respond to changes in the electorate. The standard median voter model predicts that any change in the composition of the electorate affects who gets elected through a change in the median voter (Persson and Tabellini 2000; Husted and Kenny 1997, Godefroy and Henry 2011). Lizzeri and Persico (2004) argue that increasing turnout is key to shifting political systems away from clientelism toward programatic competition. Miller (2008) and Fujiwara (2015) analyze specific events in which groups of the population with identifiable policy preferences were enfranchised. As a consequence, they observe that policies respond to the new composition of the electorate. Unlike these studies, there is no *a priori* reason to expect that the groups that stop going to the polls due to the reduction in the fine have particular policy preferences. Even though the reduction in the cost of abstention changes the composition of the electorate, I find that the average citizen who stops voting does not have significantly different policy preferences than voters who participate regardless of the reduction in the fine. This result suggests that we

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[3]To a large extent, the lack of large scale experimental evidence on the effects of changes of electoral rules on turnout decisions is mostly due to the fact that these changes apply to every voter, limiting our ability to causally interpret changes in behavior. By using the lack of communication of an actual institutional change, along with randomly allocated information to derive causal estimates of the effects of electoral rules on turnout behavior, my paper is also related to the growing literature that uses field experiments to understand voter behavior (Michelson and Nickerson 2011; Green et. al. 2013; Pande 2011).

should not expect changes in the policies enacted if the fines were reduced, a conjecture that is consistent with evidence from developed countries (Hoffman et al. 2015).

Finally, the results of the paper also speak to the literature analyzing vote buying in developing countries (Finan and Schechter 2012; Vicente, 2013; Vicente and Wantchekon 2009). A potential unexpected result of government regulation that mandates voting is that it could affect the market for votes. My results are consistent with a downward shift in the supply of votes (caused by a reduction in the cost of abstention). The patterns observed suggest that the reduction in the fine for abstention might increase the price of each vote, making it more costly for politicians to influence the outcome of the election.

In the next section, I present a theoretical model that characterizes voter behavior and motivates the empirical analysis. Section 3 provides the institutional background on the Peruvian electoral system and the change in the law that reduced the fine for abstention. Section 4 explains the experimental design and the data used in the empirical analysis, which is presented and discussed in Sections 5 and 6. Finally, Section 7 summarizes and discusses the findings.

2 The Model

In this section I present a variation of the basic model from Degan (2006), Merlo (2006), and Degan and Merlo (2011), in which I introduce an additional term of interest to motivate the empirical analysis. The model provides a framework to characterize marginal voters and provides predictions on how the composition of the electorate should change as a response to a decrease in the cost of abstention.

The theory builds on a rational choice model where voting decisions are based on a threshold strategy: if the cost of voting is lower than the benefits, citizens go to the polls, otherwise, they abstain. I consider an election where voters share a common prior about the distribution of ideological positions of the candidates, but are uncertain about their actual position. Voting is costly, and the net cost of voting has three components: (i) an exogenous benefit of voting, i.e. the warm glow or utility derived from fulfilling one's civic duty, (ii) a fine for abstention, and (iii) an endogenous cost of voting, which is the utility loss due to the possibility of making a *voting mistake*, i.e. voting for a candidate whose ideological position is far from the voter's.[4] This endogenous component drive the predictions of the model. A reduction in the cost of abstention reduces turnout, and this reduction is driven by voters who (i) are in the political center, (ii) have a lower subjective value of voting, and (iii) hold less information about the candidates.

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[4]For simplicity, the model abstracts from the transportation and opportunity cost of going to the polls. Empirically, this assumption does not affect the interpretation of any of the results as long as these costs are balanced between the treatment and control groups.

Assume that there are two candidates, denoted by $j \in J = \{L, R\}$. Each candidate has a position y_j in a uni-dimensional policy (or ideological) space, $Y = [-1, 1]$.[5] Citizens know their own ideological position $y_i \in [-1, 1]$, but are uncertain about the candidate's. From the voter's perspective, the candidate's ideological positions are random variables (y_L, y_R) distributed according to a joint probability distribution $F(y_L, y_R | y_L < y_R)$. Assume that $F(\cdot)$ is uniformly distributed on the support $[-1, 1]$. The main source of heterogeneity between voters is the amount of information each voter i holds about the candidates, $\Omega_i \in \Omega$, a refinement of $F(\cdot)$.[6]

Voters are heterogeneous in the subjective benefit they derive from participating in elections. This utility is represented by d_i, which follows a uniform distribution on the support $[0, 1]$. There is a cost of not going to the polls, a fine for abstention, M_i. Voters observe a noisy signal about the level of the fine for not voting, so each voter has a different perceived fine ($M_i = M + \varepsilon_i$). M_i ranges between zero (no fine) and one (maximum perceived fine).

The voter's decision is a two stage maximization problem. First, she evaluates whether to go to the polls or abstain. The payoff of abstaining is the fine, whereas if she decides to vote, her utility will depend on the candidate chosen. In this choice, the voter minimizes the expected ideological distance between herself and the candidate, conditional on her information set:

$$\underset{t \in \{0,1\}, v \in \{L,R\}}{Max} \, t \left[d_i - C(v_i \,;\, y_i, \Omega_i) \right] - (1 - t) M_i \tag{1}$$

where, $t_i \in \{0, 1\}$ denotes the turnout decision, $v_i \in \{L, R\}$ is the chosen candidate, and $C(v_i \,;\, y_i, \Omega_i)$ is the utility loss associated with making a *voting mistake* by voting for candidate v_i, given the voter's position (y_i) and information set (Ω_i).

There is a continuum of voters of measure 1 (there is no pivotal voter). This means that all the costs and benefits of voting are realized at the time of the election, even before the winner is announced. The uncertainty in the candidate's ideological position generates the possibility of making a mistake by voting for the *wrong* candidate, which carries a utility loss. Given the information held by citizen i (Ω_i) and her ideological position (y_i), the voter's expected utility loss of voting for candidate L is given by $C(L \,;\, y_i, \Omega_i) = E\left[\mathbf{1}\left\{u(y_i, y_L) < u(y_i, y_R)\right\} \cdot \left(u(y_i, y_R) - u(y_i, y_L)\right) \mid \Omega_i\right]$

where, the utility function is given by $u(y_i, y_j) = -(y_i - y_j)^2$.[7] The utility loss of voting for candidate R is symmetric. Note that utility loss is strictly greater than zero only when a

voting mistake occurs, i.e. when a vote for candidate L is cast while she should have voted for R (i.e. when $u(y_i, y_L) < u(y_i, y_R)$). This utility loss is realized when casting the vote, and can be thought of as a feeling of regret for choosing the *wrong* candidate.[8]

Working backwards through the voter's problem from Equation (1), the candidate choice is characterized by:

$$v^*(y_i, \Omega_i) = \begin{cases} L & if \ C(L\,;\,y_i, \Omega_i) < C(R\,;\,y_i, \Omega_i) \\ R & if \ C(R\,;\,y_i, \Omega_i) < C(L\,;\,y_i, \Omega_i) \end{cases} \tag{2}$$

if $C(R\,;\,y_i, \Omega_i) = C(L\,;\,y_i, \Omega_i)$, the citizen randomizes between the two options. Simplifying the expression above, citizen i votes for candidate L *iff* $C(L\,;\,y_i, \Omega_i) - C(R\,;\,y_i, \Omega_i) < 0$: $E\left[u(y_i, y_L) - u(y_i, y_R) \,|\, \Omega_i\right] > 0$

Substituting the previous expression and making the condition bind, one can derive τ_i, the ideological position that will make the voter indifferent between choosing either candidates: $\tau_i = \frac{E\left[y_R^2 - y_L^2 \,|\, \Omega_i\right]}{2E[y_R - y_L \,|\, \Omega_i]}$.

The optimal voting rule for voter i, $v^*(y_i, \Omega_i)$, is completely specified by the voter's ideological position (y_i), and her ideological cut-off point (τ_i). Voter i chooses candidate L *iff* $y_i < \tau_i$, candidate R *iff* $y_i > \tau_i$, and flips a coin when $y_i = \tau_i$. If the information set held by citizen i is $\Omega_i = (y_L, y_R)$, the cut-off will be exactly the midpoint between the two ideological position of the candidates: $\tau_i = \frac{y_L + y_R}{2}$, and when $\Omega_i = F(\cdot)$, the cut-off is zero.[9]

Using this result, we can characterize the turnout decision, given that the utility loss of voting is $C(y_i, \Omega_i) \equiv C(v_i^*(y_i, \Omega_i))$:

$$t(y_i, \Omega_i) = \begin{cases} 1 & if \ C(y_i, \Omega_i) - d_i \leq M_i \\ 0 & if \ C(y_i, \Omega_i) - d_i > M_i \end{cases} \tag{3}$$

The model predicts that an exogenous change in the cost of abstention (M_i) will cause lower turnout. Further, voters at the margin between going to the polls and abstaining can be characterized in terms of the three key dimensions of heterogeneity. Hence, upon a reduction in M_i, we will observe that citizens who abstain will more likely be those who:

1. *Have an ideology closer to τ_i:*

   Note that the utility loss of voting $C(y_i, \Omega_i)$ peaks at the ideological cutoff τ_i. Intuitively, the closer a citizen is to her ideological cut-off, the more likely she is to make a *voting*

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[8]If a voter is perfectly informed, she always votes for the *correct* candidate and does not face any utility loss, thus $C(L\,;\,y_i, \Omega_i) = C(R\,;\,y_i, \Omega_i) = 0$. Voters who hold less information have a higher probability of making a *voting mistake*, and hence are more likely to face a utility loss.

[9]Given the assumption on the distribution of $F(\cdot)$, τ_i is symmetrically distributed with mean zero. Note that the previous formulation always leads to sincere voting, i.e. there is no strategic voting in this model (Feddersen and Peserdorfer, 1996)

mistake, for any pair (y_L, y_R). Hence, the payoff loss associated with voting is higher for voters closer to τ_i.

Given that τ_i is symmetric with mean zero, voters with centrist ideology will face a higher expected loss from voting, and will be at the margin.

2. *Have a lower subjective benefit of voting (d_i):*

   The parameter d_i follows a uniform distribution, which is independent of Ω_i (and thus of the utility loss of voting). From Equation (3), a lower d_i implies a higher net cost of voting, and thus, for any ideology or information set the probability of voting is strictly lower.

3. *Have less information:*

   $C(y_i, \Omega_i)$ is decreasing on Ω_i, implying that less informed voters are more likely to make a *"voting mistake,"* and hence have a higher expected utility loss of voting for any given y_i.

3 Institutional Background

Since 1933, voting in Peru (as in most Latin American countries) is mandatory for all citizens between 18 and 70 years old. Abstention is penalized with civil disenfranchisement, i.e. citizens who are unable to show proof of voting with an official stamp on the ID card are denied public or private services for which official identification is required.[10] In order to get back full citizenship, a fine has to be paid, and once the payment is done, an official stamp is placed on the ID card. De facto, enforcement is mixed: it is usually stronger at banks, the judiciary, public notary, municipalities, or the public registry, while a milder enforcement is usually observed at lower levels of government or basic service delivery, such as police stations, birth or death registry, among others. Effectively, the milder enforcement implies that the expected fine is lower than the actual .[11]

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[10]The official ID card is also used for voting, and 99% of the adult population has one and is automatically registered to vote. Votes can only be cast in person on Election Day (usually a Sunday), and citizens can only vote in the district where they are registered. Voting by mail or other mechanism for remote or delayed voting is non-existent. Civil disenfranchisement implies an effective ban on getting official certificates from the national registrar, taking part in any judiciary or administrative process, signing a contract, taking a government job, getting a passport, being part of the social security system, getting a driver's license, doing any transaction in public or private banks, registering a birth or a marriage, etc. Not having voted in an election does not restrict the right to vote in any other election or access to a government anti-poverty programs.

[11]The mild enforcement is reflected in the percentage of the population that actually pays the fines. For example, in the November 2006 local elections, out of the 12.4 percent of abstainers, 14.1 percent of them had paid their fines as of July 2010. In urban districts, this proportion is higher. For example, in the region of Lima, the abstention rate was 11.87 percent, and out of the abstainers, 17.9 percent paid the fine (these official statistics are no longer available). Note that, in terms of the empirical analysis presented on the next section, the lower enforcement probability would introduce a bias in my estimates *only if* the perceived

Until 2006 the fine for abstention was S/.144 (144 Nuevos Soles, $\backsim US\$50$), which represented about 26 percent of the minimum official monthly wage. That year, Congress started discussing a change to a voluntary regime, with strong proponents on both sides. A final agreement was reached in August, when the new law was issued, and even though voting was still compulsory, the fine was reduced for everyone, with larger reductions for citizens registered in poorest districts.

The poverty level of the district was determined based on a ranking generated by the national statistical institute (INEI).[12] Using census information on the proportion of the district's population with unsatisfied basic needs, the 1,834 districts were classified into one of three poverty (and fine) categories. The new levels of the fine then were set as follows: abstainers registered in non-poor districts (N=184) are subject to a fine of S/.72 ($\backsim US\$25$); those in poor districts (N=793) were fined with S/.36 ($\backsim US\$12.50$) if they abstain, while in extremely poor municipalities (N=852), the fine was reduced to S/.18 ($\backsim US\$6$).

Importantly, no major news outlet reported the changes in the fine, and no campaigns were conducted to spread the information about the new fine structure.[13] In fact, at the time fieldwork was conducted, most of the population was still uninformed about the *new* fine, as will be shown in Section 4. The fact that electoral laws changed, and very few people were informed about it, presented a unique opportunity to explore the effects of monetary incentives on voter behavior, and to test the predictions of the model.

In this paper, I study the municipal elections of 2010, where district mayors and regional presidents were elected. In each municipality, the mayor is in charge of basic public good provision, e.g. street pavement, local security, garbage collection, street cleaning, and local management of education and health services. Also, some municipalities run development programs, e.g. workshops for farmers, job training programs for youth, etc. Even though national political parties usually have a candidate in local elections, they mostly compete with *independent* parties, and thus in most places political competition is issue based as

probability of enforcement is different in the treatment and control group, whereas if the perceived probability of enforcement is similar for those groups, the interpretation of the quantitative results provided below holds (in the next section I present evidence in this direction).

[12]Districts/municipalities (I use these terms interchangeably) are the lowest level of political administration. Each municipality elects a mayor and the council in a proportional election with no run-off. Districts are divided in neighborhoods (*barrios*, in urban areas), or villages (*centros poblados*, in rural areas). In this paper I use *village* to refer interchangeably to neighborhoods or villages.

[13]The new law was issued at a few days after a new president took power, hence news outlets were focused on this rather than other news. *El Comercio*, the major newspaper in the country only published two very short articles about this on July 6th (when the law was still under debate) and on November 20th, 2006 (the day after local elections were held). Additionally, the government offices in charge of publicizing electoral rules and providing electoral information, the ONPE (National Office of Electoral Processes) and the JNE (Electoral Jury), get a share of their annual revenues from the collection of these fines and use turnout as a performance indicator, hence they did not have incentives to publicize the new law. In 2004, 24.5 percent of ONPE's budget came from the collection of fines, while for the JNE, this share was 30.5 percent. Informal conversations with government officials at the time indicated that the heads of both offices were committed to keeping high turnout in elections, so no efforts were made to publicize the law.

much as ideologically based.

4 Experimental Design and the Data

One obvious way to estimate the causal effect of changes in the cost of abstention on turnout in this setting is to compare voting behavior of citizens in districts with different levels of the fine for abstention. However this strategy faces two major challenges. On the one hand, voters were not informed about the new levels of the fine (as I show in the next section), and therefore voters in different districts perceive that the fine was at its old level (common for all districts). Even if some voters knew about the new levels of the fine, information acquisition would probably be correlated with other relevant variables, such as political information, or interest in politics, which would lead to biases in the estimated effects. Additionally, it would be impossible to disentangle the effect of district specific characteristics, such as the electoral context (candidates running for office, availability of polling stations, etc.) or poverty, from the effect of the different fine levels. For instance, given the well documented association between wealth and turnout (Matsusaka 1995, Perea 2002, Frey 1971), a comparison of turnout in the average poor district with that in the average non-poor district would be capturing both the wealth effect as well as different levels of the fine.

One way to isolate the effect of district specific characteristics from different levels of the fine is to compare districts at the threshold between being classified as poor and non-poor and between extremely poor and poor. In expectation, districts just on the border should be comparable in all relevant (observable and unobservable) characteristics. Further, if we believe that the monetary cost of abstention matters in the decision to vote, had voters been informed about the reduction in the fine, we would observe a decrease in turnout in the elections that took place after the reduction in the fine, i.e. the November 2006 and October 2010 municipal elections. On the other hand, this change in turnout would not be present in any elections before the law came into effect, e.g.. in the 2002 municipal election.

Figure 1 shows the results of a regression discontinuity analysis for the last three municipal elections (2002, 2006, 2010). For each of them, districts are ranked from richest to poorest, plotting their turnout rates, and fitting a cubic polynomial for municipalities in each of the three poverty levels. The vertical lines indicate the thresholds at which a district is categorized as non-poor, poor, or extremely poor. There is no statistically significant difference in turnout between districts located at each side of the thresholds in any of the elections analyzed.

The results presented in Figure 1 can be interpreted as evidence that changes in the monetary cost of abstention do not influence the decision to participate in elections. Alternatively, it could mean that the cost matters for turnout decisions, but voters were not informed about the change in the fine.

4.1 Experimental Design

The field experiment was designed to generate within village, individual level variation in the cost of abstention. Randomly selected voters in a sample of 29 villages in 10 districts in the region of Lima received information on the actual levels of the fine *just before* the municipal election of October 3rd 2010. After the election, all subjects in the treatment and control groups were re-interviewed, and asked to show official proof of voting (sticker in the ID card). The advantage of this strategy is that I can compare an objective measure of voting of people who likely to believe that the fines were still at the previous level (control group) with those whose information set had been updated by the treatment.

To be able to make comparisons within and between poverty categories, I sampled districts on each side of the two poverty category thresholds, i.e. as close as possible to both sides of the vertical lines in Figure 1. Within each district, I randomly sampled villages and within each village the team of enumerators applied a random sampling algorithm to choose households. In each household, all individuals eligible to vote (between 18 and 70 years old) were surveyed. Overall, I have complete baseline information for 2,837 individuals, from 1,911 households, and 29 villages. By stratifying the randomization at the village level, I can make comparisons within villages, thus isolating the effect of any district and village specific characteristic, such as political competition, candidate characteristics, village poverty level, social preasure, etc. The unit of observation is the individual, but the sampling and treatment status are determined at the household level, hence in the empirical analysis I allow for correlation of the errors within the household by clustering the standard errors at that level. Table A.1 and Figure A.1 provide descriptive statistics on the districts in the sample, as well as their geographical location and poverty category, respectively.

The baseline interview took place between one and four weeks before the municipal election. It contains information on household characteristics, household composition and expenditures. Also, it collects information about basic demographics, political preferences, policy priorities for the district, knowledge about the current electoral process, past voting, and usage of public services. Importantly, the survey contained a question where respondents were asked whether they knew if there were consequences for not voting and what were these consequences (open question). If the respondent mentioned that there was a fine, a follow-up question inquired about the amount. I assume that the reported value of the fine is the voter's ex-ante perceived fine. At the end of the interview, the enumerator provided the treatment.

Household assigned to the treatment group were informed by the enumerator about the current level of the fine in the district where each voter was registered to vote.[14] To avoid

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[14]In the questionnaire, we asked every respondent the district where she is registered to vote. This information was cross checked with the subject's ID card. Every enumerator had a list of the 1,834 districts in the country, with their corresponding poverty level, and thus, the applicable level of the fine. In order to reinforce the message, the enumerator showed a copy of the official newspaper where the law was published, and gave

differential salience of compulsory voting, the control group received a reminder that voting is mandatory and that there is a fine for not voting (without mentioning the amount).[15] In practice, the script for the control group did not provide any new information, since in the baseline survey 94.5 percent of respondents reported that they knew that voting was compulsory, and that abstention was penalized with a fine.

The follow-up survey was collected between one and three weeks after the election. The main variables in the survey were whether or not each respondent voted in the municipal election and how much did they perceive the fine for abstention was. I measure voting through a self-reported variable, but also collected an objective measure of voting by asking each respondent to show their ID card, where the enumerator confirmed if it had the official stamp.[16] The questions on the perceived value of the fine in this survey are assumed to correspond to the information each voter had at the moment of the election (when they decided whether to vote or not).

Table 1 shows the descriptive statistics of my main independent and dependent variables. The perceived fine in the baseline and follow-up surveys (as well as the change in the perceived fine) by poverty level and treatment status are shown in Panel A.[17] In the baseline survey,

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a flier to each respondent with the exact text of the script. An SMS with the same script was sent during the week before the elections (for those who reported having a cell phone).

The script for the treatment group was as follows (see Figure A.2 for Spanish version):

Dear Sir/Madam,.

On August 2006, Congress passed a law in which the fines for not voting were reduced (Ley No. 28859). According to this law, those who do not vote are no longer subject to a fine of S/.144, but the fines are now lower for everyone, and they vary according to the poverty level of the district where you are registered to vote.

According to the information that you just provided me, if you do not vote in the upcoming elections you will be subject to a fine of S/.[AMOUNT IN THE DISTRICT WHERE SHE'S REGISTERED].

[15]Respondents in the control group received the reminder using exactly the same methods as those used for the trreatment group, namely, a flier repeating the script and those with a cell phone received SMS reminders. The exact script for the control group was as follows (see Figure A.2 for Spanish version):

Dear Sir/Madam,

Remember that voting is mandatory in Peru and not voting is subject to a sanction that implies a fine.

[16]The option to pay the fine and get the official stamp in the ID card is only available once the full voting record is centralized, which usually happens more than a month after the elections. The only way a respondent could have the stamp at the moment of the interview was by having voted.

[17]In the complete panel sample, 4% of respondents did not know what were the consequences of abstention. Out of the ones who knew the consequences, only 1.3% do not mention the fine as one of them. I take a conservative approach and assume these respondents think that there is no fine (i.e. it is S/.0). When assessing the current value of the fine, enumerators were trained in different strategies to try to get an exact number for the respondent's belief. However, in case the respondent refused to provide it, we asked they could place their beliefs about the value within certain ranges (< S/.25; S/.25 - S/.50; S/.50 - S/.100; S/.100 - S/.150; and > S/.150). In order to get an individual level belief about the exact value of the fine, for respondents who provided a range, I use the median value of that range specified using data from those who did mention

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the average respondent reports that the fine for not voting is S/.121.93, which is close to its old level (before August 2006, S/.144). This confirms that the majority of voters in my sample were not informed about the change in voting laws. There is significant dispersion in the data, ranging from voters who think that there is no fine for abstention (4 percent of the sample), to those who think that the fine is around S/.300. There are no significant baseline differences the perceived fine between the treatment and control group.

Figure 2 shows the distribution of the perceived fine in the baseline and follow-up surveys, for the control and treatment group, by poverty level of the district where each respondent is registered to vote. In each graph, the vertical line represents the current level of the fine. In the left panel, for the baseline survey, the vertical line represents the old level of the fine (S/.144), while in the graphs in the right, the lines are set at the new levels of the fine: S/.72 for voters in non-poor districts, S/.36 for those in poor districts, and S/.18 for voters in extremely poor districts. The distributions in the left column confirm that, not only there weren't significant baseline differences in the perceive fine on average, but also the distributions are similar (and this is confirmed by the Kolmogorov-Smirnov test).

Treatment assignment affected the beliefs about the fine for abstention reported in the follow-up survey. However, not only those in the treatment group learned that the fines had decreased. The average respondent registered in a non-poor district who received the treatment reports at follow-up that the fine for not voting is S/.65.88, while non-poor voters in the control group report a perceived fine of S/.88.43, significantly lower than the S/.126 reported at baseline. For voters registered in poor districts, the pattern is similar. The average voter in the treatment group reports that the fine is S/.41.36, which is close to the actual S/.36, while the average voter in the control group reports that the fine is S/.68.82. Voters from extremely poor districts are more likely to learn about the new levels of the fine. While the treatment group reported a perceived fine of S/.19, the mean for control group is S/.36. As shown in the graphs in the right column of Figure 2, these patterns not only hold on average, but the distributions are shifted to the left for both the treatment and control groups.

Overall, the treatment had the desired effect of updating treatment voter's beliefs about the new level of the fine, however some voters assigned to the control group also learned about the new fine, and this is especially true for people registered in extremely poor districts. It is unclear why information about the new levels of the fine spread more rapidly in extremely poor districts. Learning about the new levels of the fine is independent of the size of the village, for example. The number of days between the baseline and follow-up surveys is not statistically

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an exact value. The main results from the paper do not change if I restrict the sample to only respondents who reported an exact number for the fine. Further, in additional regressions, I included an interactive term between the variable of interest and a dummy for having provided a range rather than an exact number, and this term is statistically insignificant and close to zero in all cases.

different between non-poor, poor and extremely poor municipalities (30 days, on average). In Table A.3, I explore the correlation between the number of days between the baseline and follow-up survey and the changes in the perceived fine. As expected, the time between surveys is only significantly related to changes in the perceived fine among control households (the treatment group received the information just after the baseline survey). Interestingly, learning in the control group seems to have happened after the election, and not between the baseline survey and the election. This means that most voters in the control group hadn't updated their information about the changes in the fine by the time they made the decision of whether to vote or not. The evidence from Table A.3 suggests that the perceived fine reported by controls in the follow-up should be taken as an upper bound of their beliefs about the fine and the estimates in the regressions, as lower bounds.

Panel B in Table 1 shows that 94.2 percent of the respondents in my sample voted in the October 2010 elections.[18] The reduction in the cost of abstention driven by the treatment led to lower turnout. On average, respondents in the treatment group were 3.1 percentage points less likely to show up to vote on Election Day. This result can be interpreted as a reduced form effect, or the direct (unconditional) effect of the treatment on turnout. The magnitude of this effect is roughly proportional to the reduction of the perceived fine. In non-poor districts the reduction in the fine led to a difference of 2.1 percentage points in turnout rates between the treatment and control groups. Likewise, in poor districts, where the reduction in the fine was larger, treated voters were 5.4 percentage points less likely to vote. Finally, voters in extremely poor districts, where the fine was reduced the most, the difference between voters in the treatment and control groups is only 1 percentage point, and its not statistically significant.

In extemely poor districts, the treatment did not differentially affect voters in the treatment and control groups (on average). Voters learned about the new levels of the fine, regardless of their treatment status. Consistent with this, given that a good share of voters in extremely poor districts perceived that the fine was reduced, they turned out to vote at least 2 percentage points less often than voters in the control groups in poor and non-poor districts (93.5 percent versus 96.7 percent and 95.9 percent, respectively). Provided that the experiment did not differentially affect the perceived fines for the treatment and control respondents in extremely poor municipalities, I am not able to do any valid inference from the differential behavior of these groups. Hence, in all the subsequent analysis, I exclude voters from extremely poor districts, and refer to the sample of voters in poor and non-poor districts

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[18]There are two reasons why turnout in my sample is higher than the official statistics. First, I only sampled voters between 18 and 70 years old, whereas the official turnout rate is computed among all registered voters, including voters older than 70 (who are no longer mandated to vote). Second, conversations with government officials in Peru have suggested that the electoral roster is not updated often, thus there is a number of dead voters who's names are still in the official roster (and of course, they are absent on election day).

as the *Analysis Sample*.[19]

Even though the time between the baseline and follow-up surveys was short (30 days, on average), in the *analysis sample* we were able to track down 1,732 individuals from 1,166 households. Table A.2 shows the balance of observables between attrited individuals and those who we were able to track, breaking down the analysis sample by poverty categories. Overall, the sample of attriters is not statistically different from those who we were able to track along most of the observable characteristics.[20]

The main variable collected in the follow-up survey was whether or not each respondent voted in the municipal election. I measure voting through a self-reported variable, but also collected an objective measure of voting by asking each respondent to show their ID card, where the enumerator confirmed if it had the official stamp or not. Among the 1,732 respondents in the analysis sample, only 3 of them refused to tell the enumerator whether they voted or not. 65.2 percent of the respondents agreed to show their ID cards, and the probability of showing it is independent of the treatment status of the respondent (See Panel A of Table 7).[21] There does not seem to be a tendency to lie about voting. Out of those for whom I have self-reported and objective measures of voting, only 6 respondents reported that they did not vote, while their ID card had the official stamp. The opposite happened in 6 other cases. Given the low lying rate, I maximize the sample size in the analysis by defining the turnout variable based on the objective measure of voting for those who showed their ID, while taking the self-reported values for those who did not. In the empirical analysis in the next section I show that the results are robust to using only the self-reported or objective measure of voting. The follow-up survey also included questions about political preferences, information about the political process, the candidates and parties running, and a battery of questions about vote buying.

4.2 Descriptive Statistics

The descriptive statistics for the analysis sample, and by poverty category, are reported in Table A.4. On average, 40 percent of the sample is male, they are about 38.7 years old, with 10 years of education, and spend S/.288.1 ($\smile US\$110$) per capita per month. In order to test the heterogeity of the effects predicted by the theoretical model presented, I collected information on (i) political preferences, (ii) intrinsic value of voting, and (iii) political information. The

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[19]This sample comprises 1,732 voters instead of the 2,350 shown in Table 1

[20]One variable that shows systematic imbalances is gender. Men are less likely to have been found in the follow up survey. Excluding that variable, a joint F-test of the significance of the difference between covariates does not show overall imbalances (when I include gender, the test is marginally significant). In all the subsequent analysis, gender is included among the controls in the regressions. In the next section, I run a bounding excercise to evaluate the size of the effect in the best and worst case scenarios, and the main results hold.

[21]Further, the probability of showing the ID card is unaffected by the treatment even among those who self-report having voted in the election.

ideological position of the population is highly concentrated in the center, with 8 percent locating themselves in the left and 22 percent in the right. This outcome comes from self-reports in a scale ranging from left (1) to right (5). I take the categories in the middle (2, 3 and 4) to represent the political center. Ideology is not unidimensional, and as mentioned earlier, much of the competition in Peruvian local election is as issue-based, thus I use a second measure that uses policy preferences to capture a broader range of "ideological" distributions. In the survey, I asked voters to name (in order) the first five policies that they would implement if elected mayor of the district. This was an open question, and the enumerators had to place the answers in one of twenty eight policy categories. For each of these categories, the policy preferences are ordered from not mentioned (zero) to most preferred (five). I aggregate these questions by taking the first principal component, and dividing the sample into quintiles. The center is defined by those in the quintiles 2, 3, and 4, while the first and fifth quintiles define the ideological extremes.[22] The Policy Extreme 1 is related to preferences for public goods, such as health and education infrastructure, roads, etc. On the other hand, the Policy Extreme 2 is associated with club goods (publicly provided goods that are easily appropriated by a group of agents), such as youth labor training, security, promotion of private investment, etc. The questions that define the ideological position of each voter were asked at baseline, before the treatment was administered, so they are taken as predetermined.

The subjective value of voting is a difficult concept to quantify. I approximate it by using different variables that measure the voter's interest in (i) politics, (ii) the current electoral race, and (iii) the current campaign. Few people (7 percent) declare themselves to be very interested in politics, while 47 percent are somewhat interested, and 46 percent are not interested at all. The relatively little interest in politics is also apparent from the small proportion of voters who are very interested in the results of the election, or in the campaign (38 percent and 9 percent, respectively). Respondents who are somewhat interested in the results represent 46 percent of the analysis sample, while 55 percent are somewhat interested in the campaign. Finally, 16 percent and 35 percent are not interested in the results or the campaign, respectively.

Political knowledge and information are measured in several ways. I included open ended questions asking respondents to name all the candidates (and/or parties) running for the mayor's seat in the municipality where they are registered to vote. All measures of political information are expressed as ratios of the number of candidates (and/or parties) that the respondent is able to name as a proportion of the total number of candidates (and/or parties) running for office. The average respondent in the analysis sample is able to name 36 percent of the candidates and 29 percent of the parties running. As an alternative measure of political information, I asked 17 questions about knowledge of the political structure of the country, electoral institutions and rules.[23] The average respondent got 9.3 questions right (55 per-

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[22]The coefficients for each policy item loading into the principal component analysis are listed in Table A.5.

[23]The questions include information about the length of the term, reelection possibilities for two consecutive

cent). Importantly, these political information measures are uncorrelated with the baseline knowledge about the fine. For instance, the correlation between the absolute value of the error in the perceived fine (|perceived - actual fine|) and the index of knowledge of candidates is 0.0005.

In the next section, I outline a formal framework for the empirical analysis, and use the information described here to test the predictions of the theoretical model.

5   Empirical Strategy and Results

The empirical strategy to test the predictions of the model follows directly from the experimental design. My strategy exploits the exogenous variation in the change in the perceived fine provided by the treatment status to first identify the effect of the reduction in the cost of abstention on turnout in a 2SLS framework. Further, I test the predictions of the theoretical model, characterizing voters who are more likely to abstain upon a reduction of the cost of abstention. This allows me to identify what are the predicted changes in the composition of the electorate, and its effects on preference aggregation. As mentioned in the previous section, given the low and insignificant change in the perceived fine between treatment and control voters in extremely poor villages (see Table 1), the empirical analysis focuses on voters registered in non-poor and poor districts (*analysis sample*).[24]

5.1   Reduced Form Effects

The reduced form regression identifies the direct effect of the treatment on turnout:

$$Vote_{ikj} = \alpha + \beta_1 NonPoor_{ij} \cdot Treat_{ikj} + \beta_2 Poor_{ij} \cdot Treat_{ikj} + \beta_3 Poor_{ij} + \gamma X_{ikj} + \delta_k + \eta_{ikj} \quad (4)$$

$Vote_{ikj}$ is an indicator of whether voter i, living in village k, and registered to vote in district j, voted in the election of October 3rd, 2010. The treatment status is given by the indicator variable $Treat_{ikj}$. Given that there are two distinct treatment groups depending on the poverty level of the district where voter i is registered, in all the regressions I interact the treatment dummy with the poverty level of the district ($NonPoor_{ij}$ and $Poor_{ij}$). The inclusion of the dummies indicating the level of poverty of the district where voting allows

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periods, length of the term, and existence of run-off elections for president, congressmen and mayor, the official minimum and maximum age for which voting is mandatory, and which are the government institutions in charge of the elections, ID cards and political claims.

[24]The reduced form, first stage and two stage least squares including the extremely poor are shown in Panel A of Table 6. The qualitative and quantitative results for the pooled sample are similar than the ones shown here. Also, the main results in all other tables can be replicated including the extreme poor, and all the results hold. A full set of tables including this sub group is available upon request.

restricting the comparison to treatment and control units *within* the same poverty status/level of the fine, $NonPoor_{ij}$ is the excluded category. X_{ikj} is a vector of individual level characteristics that are are likely to affect voting decisions: gender, age, years of education, and the log per capita expenditures. Finally, δ_k denotes a fixed effect at the level of the village where interview took place. η_{ikj} is an error term, for which I allow to be correlated within household (the treatment unit).[25]

It is not straight forward that we should expect a reduction in the fine to cause lower turnout. In the Peruvian context, where mandatory voting has been in place for more than 80 years, and turnout is consistently high, habit forming (e.g. Gerber et al 2003, Fujiwara et al 2014) or social pressure (e.g. Gerber et al 2008, Funk 2010) might dominate the monetary effect. Panel A in Table 3 presents the reduced form estimates of the effects of the treatment on turnout, showing that this is not the case. Treated voters in non-poor municipalities are 2.7 percentage points less likely to vote than the controls in this poverty category (Column 1). Likewise, voters in poor districts showed up at the polls 5.2 percentage points less often than the ones in the control group (Column 2). Pooling voters does not affect the magnitude or significance of the results (Column 3). Further, the effects on turnout are roughly proportional to the official decrease in the fine, and are practically the same as the descriptive statistics shown in Panel B of Table 1, as expected, given the random allocation of the treatment condition. These reduced form effects are consistent with the hypothesis that the reduction in the cost of abstention is an important determinant of turnout.

5.2 IV Results

Voters update their beliefs differentially, and to be able to say something about the magnitude of their response to different changes in the fine for not voting, we need to scale the reduced form effects by the change in the perceived fine caused by the treatment. The first stage regression estimates exactly this:

$$\triangle Fine_{ikj} = \alpha + \beta_1 NonPoor_{ij} \cdot Treat_{ikj} + \beta_2 Poor_{ij} \cdot Treat_{ikj} + \beta_3 Poor_{ij} + \gamma X_{ikj} + \delta_k + \nu_{ikj} \quad (5)$$

$\triangle Fine_{ikj} = (Fine_2 - Fine_1)_{ikj}$ is the change in the perceived fine between the follow-up and baseline surveys. The specification is similar to the one in equation (4). In this case β_1 and β_2 represent the difference in the average change in the perceived fine between the treatment and the control group, among voters from non-poor and poor municipalities, respectively. The results from the first stage regression are displayed in Panel B in Table 3. The difference in the change in perceived fine for the treatment and control groups is S/.16.9

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[25]The results are robust to clustering the standard errors at the village level.

in non-poor municipalities (Column (1)), while the treatment effect for voters in poor districts (Column (2)) is a reduction in the perceived fine of S/.28.9. Column (3) pools the results showing that the we do have a strong first stage for IV strategy, with an F-statistic for the excluded instruments of 25.6.

In the second stage, I analyze the effect of the changes in the perceived fine (instrumented by the treatment status in each poverty level) on turnout:

$$Vote_{ikj} = \alpha + \beta_1 \triangle Fine_{ikj} + \beta_2 Poor_{ij} + \gamma X_{ikj} + \delta_k + \varepsilon_{ikj} \qquad (6)$$

β_1 represents the marginal effect of a change of S/.1 in the fine for not voting on the likelihood of voting. The instrumental variables results are presented in Table 4. An exogenous *decrease* of S/.1 in the fine for abstention reduces the likelihood of voting by 0.16 and 0.18 percentage points for voters in non-poor and poor districts, respectively (Column (1) and Column (2)). Pooling the results, the average voter in the sample is 0.17 percentage points less likely to go to the polls when the fine drops S/.1 (Column (3)). The results in Table 4 can be interpreted as the local average treatment effects (LATE) of a marginal reduction in the cost of abstention on voters whose priors about the fine were updated by the treatment.

The estimates from Table 4 allow me to back out the cost-elasticity of voting, an important parameter for policy applications. The average voter in the analysis sample reports that the fine has been reduced by S/.58 (46.8 percent from her baseline perception of S/.124), hence the effect of the drop in the perceived fine on turnout for the average voter is -9.86 percentage points (10.43 percent). These results imply that the observed reduction in the perceived fine lead to a drop in turnout from 94.5 percent to 84.6 percent, and cost-elasticity of voting of -0.22.

Extrapolating these results to the whole population, a reduction in the fine of 50 percent would reduce turnout in about 11 percentage points, while driving the fines to zero could lead turnout to 72.5 percent, a level comparable to the one observed in some countries where voluntary voting is in place. To put these results in context with the previous evidence, Gerber et al. (2008) find that reminders to vote emphasizing social pressure messages cause an increase in turnout between 4.8 and 8.1 percentage points.

The model presented in section 2 predicts that voters in the political extremes, those who have a lower subjective value of voting, and the uninformed are more likely to make a voting mistake, and thus abstain more often. This means that upon a reduction in the fines for abstention, these marginal voters should be the ones to abstain. The results shown TablesA.6 and A.7 are robust to the inclusion of my preferred variables proxying for these categories. Consistent with the model (as well as with other models of turnout), leftists and rightist voters are less likely to vote than centrists (on average). Also more interested, and informed voters participate more often in elections, however, the only results that come though as statistically

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significant are those for political information. When testing the main predictions of the model in sub-sections 5.4, 5.5, and 5.6, I include this set of variables as controls in all regressions.

5.3   Robustness and Validity Checks

The exclusion restriction for the IV results to be valid require that the treatment only affects turnout through a reduction in the perceived fine. One potential violation of this restriction would happen if the treatment generated differential changes not only in the perceive fine, but also on the perceived probability of enforcement. This would be the case if voters in the treatment group perceived that, by reducing the fine for abstention, the government is signaling that voting is less important, and thus reducing enforcement. If this were the case, the treatment group would expect to be subject to lower sanctions than the control group, in case of abstention. In the follow-up survey I asked voters to name all the possible consequences of abstention, this is, which services they believed they would be denied access to in case of abstention. In Table 5, I regress the perceived consequences of not voting on the treatment variables, and the controls used in the preceding analysis. The results show that voters in the treatment group are not more likely to think that the chances that they are denied a particular service are higher than those in the control group.

As can be seen at the bottom of Table 5, while 98.3 percent of respondents report that they would have to pay a fine if they fail to vote, the proportion of respondents that report that they will loose access to other services is relatively low. This indicates that voters (whether treated or not) are unaware that they can avoid the fine by not accessing these services, hence alleviating concerns of selection into abstention being driven by voters who don't expect to use other services. An alternative potential violation of the exclusion restriction is that the information treatment can differentially affect the salience of the importance of voting. To test whether this is the case, I follow a similar strategy, and run my main specifications, but using as the dependent variable proxies for the importance of the elections, the electoral campaigns, and the results of the elections collected in the follow-up survey (after the treatment was administered). The results show that the treatment did not differentially affect voters in the treatment or control groups.[26]

Given that the endogenous regressor in the IV estimation is a continuous variable, and at baseline voters have heterogeneous beliefs about the value of the fines, one might worry that the treatment affects voters beliefs in different ways. If this was the case, the interpretation of the 2SLS results as local average treatment effects would be threatened by violations to the monotonicity assumption, in which case, IV estimator is not guaranteed to estimate a weighted average of the underlying individual causal effects, and the LATE would not converge to the IV estimator (Angrist and Pischke 2009, pp-154-158). More precisely, the monotonicity

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[26]These results are not shown, but are available upon request to the author.

assumption implies that, compared to the counterfactual, all voters in the treatment group should update their beliefs downwards ($\Delta F_i \leq 0$), i.e. otherwise, this might imply the presence of *defiers* in the sample. Even though the presence of *defiers* is an untestable assumption, I can provide evidence that, if they were present, this wouldn't generate a significant biases to my main estimates.[27]

The only group in the analysis sample where we might (plausibly) find defiers is among voters whose initial beliefs about the fine were below the new level. 11% of voters in the analysis sample fall in that category. In terms of the potential outcomes framework, this 11% (besides defiers) can also include *never takers*. Panel B in Table 6 presents the main regressions of the paper excluding this group, showing that the point estimates of the reduced form, first stage, and IV regressions are remarkably similar to those in Tables 3 and 4, providing evidence that the potential presence of defiers in the sample would not bias in my estimates.

As mentioned in the previous section, attrition rates were not trivial in the experiment, with about 15% of households not present in the follow-up survey. Even though attriters do not look different than non-attriters in most observable characteristics, we still do not observe the main outcome variable for those who we weren't able to track down. In Panel A of 6, I run a bounding excercise following Lee (2009), showing that even in the worst case scenario, the estimated effects are still statistically different from zero. Lee bounds assume that receiving the treatment makes individuals more or less likely to respond to the survey. Under this assumption, I estimate the lower and upper bound for my main estimate, assuming the best and worst case scenario. The attrition rate in the treatment group is 21.75 percent, while in the control group, it is 17.77 percent. The estimation of the lower bound is equivalent as saying that any additional treatment non-responded in the sample would be equivalent to control observations in the lower part of the distribution of the outcome variable (i.e. voters). Similarly, the upper bound is computed assuming that any additional treatment non-respondents correspond to non-voters in the control group, so I trim them from the control group.[28] The estimate for the lower bound is extremely close to the main effectes estimated in Table 4, while the upper bound is 80 percent larger.

The dependent variable is constructed based on a combination of objective and self-

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[27]In this analysis, always takers would be those voters who adjust their beliefs on the value of the fine downward regardless of receiving the treatment or not. Never takers are those who did not update their beliefs, regardless of their treatment status. The compliers are voters who, if they were informed about the reduction in the fine, they'd adjust their beliefs downwards. Finally, defiers are those voters that, if treated, update their beliefs upwards.

[28]To implement this, and compute the lower bound, I trim from the control group the difference in attrition divided by the number of control group observations that are observed (4.83 percent=3.98/(100-17.77).) In this case, this is equivalente to 67 observations. Given that the outcome variable is discrete, and I need to drop 67 observations who voted, I run a bootstrap, dropping 67 randomly chosen voters from the control group. Similarly, to compute the lower bound, I need to drop 67 non-voters. In the control group I only have 32 abstainers, so I drop them all, and estimate the main regression equation. These results are reported in Panel A of 6.

reported measures of voting, which raises two important concerns. First, if the treatment differentially affected the probability of showing the ID card, my point estimates might be biased. Panel A in Table 7, regresses the probability of showing the ID on the treatment variables and the same controls as above, obtaining point estimates very close to zero and statistically insignificant, alleviating this concern. Further, there could also be a "social desirability effect", e.g.. voters who reported having voted, if treated, are less likely to show their ID. In columns (2) and (3) I run the same regression as before, splitting the sample between voters who reported having abstained or voted, still finding a zero treatment effect. Second, individuals who show their ID might have different observable or unobservable characteristics that could be correlated with voting. In Panel B of Table 7 I show the results from the main specification, but using both self-reported and objective measures of voting as a dependent variable. The results are very similar across the different samples and voting measures. In the sample for which I have both self-reported and objective voting measures ("comparable sample", in the table), turnout is higher, since people who reported not having voted were less likely to show their ID. In this sample, the results using the self-reported measure of voting is slightly attenuated, but still large and economically significant.[29]

Panel C in Table 6 presents a validity test for the effect of the treatment on turnout. If the treatment did affect the perceptions about the magnitude of the fines, it should have affected turnout in 2010, but it had no way of affecting past behavior. Here, I show the results of running the same specifications as in Table 4, but using a self-reported measure of turnout in 2006 as the dependent variable. The change in the perceived fine do not have a statistically significant effect on the self-reported measure of voting in 2006, and the coefficients are very close to zero in the different samples.

Finally, one might be worried that the choice of the main independent variable could affect the results. Table A.8 shows the results of the main IV regression using as the endogenous regressor, instead of ΔF_{ij}, the level of the perceived fine in the follow-up (in levels or logs, while controlling for the baseline perceived fine), or the fine as a percentage of per capita expenditures. The results are robust to the choice of the independent variable.

In the following section, I show the heterogeneity of the main effect, highlighting different voter characteristics that predicted by the model above.

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[29]It is standard in the literature to use self-reported voting data (e.g.. Chong et al 2014, Perea 2002, Vicente 2013, Fefchamps and Vicente 2012, among others). If I did that, the results in the paper would remain unchanged. However, objectively measuring turnout by observing whether voters have the corresponding sticker in their ID cards is an improvement over previous literature, and this improvement comes at the cost that not everyone showed their ID card to the enumerator.

5.4 Ideological Position

The model predicts that voters with a centrist ideology are more likely to abstain upon a reduction in the fine. The random variation in the cost of not voting provided by the treatment allows me to causally interpret the effect of changes in the cost of abstention induced by the treatment on turnout for different ideological position categories. That is, introducing in my main regression the interactive term between the change in the perceived fines and the ideological position, instrumented by the treatment dummies and their interactions, provide causal evidence of whether people with centrist ideologies are more likely than those with extreme political preferences to react to a change in the cost of abstention, as the model predicts. Given the three ideological positions, left, center and right, denoted by P_{ij}^l ($l = 1, 2, 3$), the effect of the reduction in fines on turnout for each ideological position is identified by equation (7).

$$Vote_{ij} = \alpha + \sum_{n=1}^{3} \beta_n \triangle Fine_{ij} \cdot P_{ij}^n + \sum_{n=1}^{3} \beta_{n1} Poor_{ij} \cdot P_{ij}^n + \sum_{n=2}^{3} \beta_{n2} NonPoor_{ij} \cdot P_{ij}^n + \gamma X_{ij} + \delta_k + \epsilon_{ij}$$

(7)

In order to compare voters within the same fine level, the regression model in Equation (7) includes interactions between all the relevant coefficients and the poverty/fine level dummies. The only effects that I constrain to be constant across poverty categories are the control variables (X_{ij}), where I include the same set of covariates as in Tables 3 and 4. For completeness, I further control for other variables that the theory predict that will affect turnout behavior, as interest in politics and political information. The coefficients of interest in this case are β_n and, if the predictions of the model hold, we should observe that the coefficients associated with the interaction between the indicators of political extremes (P_{ij}^1 and P_{ij}^3) with the change in the perceived fines are not different from zero (β_1 and β_3), while the coefficient testing for the effects of changes in the fine on voting *among* centrists voters (β_2, associated with P_{ij}^2) should be positive.

Panel A in Table 8 shows the results from Equation (7). In Column (1) I use the self-reported measure of political ideology, and find that the bulk of the effect of the change in the fine on turnout observed in Table 4 is mainly driven by voters who place themselves in the political center. Voters on both political extremes seem to be unresponsive to changes in the fine for abstention. The results in Column (2) use the second measure of ideological position (based on policy preferences) are even more stark. Voters in the second through fourth quintiles of the policy preference scale account for the whole effect of changes in the fine for not voting, while voters in the political extremes show effects close to zero and statistically insignificant. Overall, the results from Panel A in Table 8 are consistent with the first prediction of the model, and show that people in the political extremes are less likely to

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respond to a change in monetary incentives to vote. [30]

This result has important implications for thinking about incentives to vote and its potential effects on political competition and social conflict. In the medium run, the political supply should respond to changes in the electorate. If this is the case, a reduction in turnout among centrists might lead parties to bunch in the extremes of the political spectrum, which can cause coordination problems, polarization and social conflict.

5.5   Interest in Politics / Subjective Value of Voting

Voters with a higher subjective value of voting need lower incentives to attend to the polls, compared to those who derive lower utility from voting. The subjective value of voting is an unobserved individual characteristic, and I use a battery of questions on interest in politics, in the results of the current election, and in the campaign (all of these questions were asked before the treatment was administered).

In Panel B of Table 8 I run a similar regression as the one in Equation (7), again here I control for observables as well as other variables that are predicted to affect turnout, such as political ideology and information. The results show that voters who are more interested in politics attend to the polls regardless of the change in the perceived fine. People who report being somewhat interested in politics are less likely to vote when the fine for abstention is reduced. Consistent with the model, the effect is smaller in magnitude than the one observed for voters who are not interested in politics. Similarly, voters who are very interested in the political campaign or in the results of the election are unlikely to respond to a reduction in the fine, while for those who are somewhat interested we observe a significant effect, but again, lower in magnitude than for low interest voters. This result is consistent with the second prediction of the model.

Using the results from Panel B in Table 8 I am able to provide revealed preference estimates of the subjective value of voting. Voters who are less interested in politics are much more sensitive to a change in the fine for abstention, with an elasticity of -0.247. This result implies that in order to increase their probability of voting from the observed 93.5 percent to 100 percent, we would need to increase in the fine for abstention in S/.92.4 (~US$33). Likewise, voters who are interested in politics have an elasticity of -0.159, hence to achieve full participation, the fine would have raise by S/.77.4 (~US$27). Finally, voters who are very interested in politics are hardly sensitive to changes in the fine, with an implied elasticity of -0.13, and they would vote even if the fine was set at zero.

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[30]This result is also consistent with the predictions in Glaser et al. (2008).

5.6 Political Information

The model predicts that $C(y_i, \Omega_i)$ is decreasing in Ω_i, which implies that less informed voters are more likely to make a *"voting mistake"*, and hence have a higher expected cost of voting for any given y_i. Empirically, I test this prediction by interacting different measures of political information collected at baseline with the change in the perceived fine. More precisely, I run the test for this prediction through the following equation:

$$Vote_{ij} = \alpha + \beta_1 \triangle Fine_{ij} + \beta_2 \triangle Fine_{ij} \cdot Info_{ij} + \beta_3 Poor_{ij} \cdot Info_{ij} + \beta_4 NonPoor_{ij} \cdot Info_{ij} + \beta_5 Poor_{ij} + \gamma X_{ij} + \delta_j + \epsilon_{ij}$$
(8)

As before, in Equation (8) I am only comparing people within poverty categories, and include controls for demographics, political preferences, subjective value of voting, and information. We expect to observe that the effect of reductions in the cost of not voting is steeper for voters who hold less political information, and thus β_2 should be negative.

Panel C in Table 8 tests this hypothesis using the four different measures of political information described in Section 4.2. In all four columns, the interaction between the information indices and the change in the perceived fine (instrumented by the treatment and the relevant interaction) is negative and significant, meaning that people who have higher levels of information are less likely to change their turnout decision when they learn that the fine has been reduced. Moreover, Moreover, the magnitude of these coefficients line up remarkably well with the model. Fully informed voters are unaffected by the changes in the fine. Previous evidence shows that more informed voters are more likely to hold the elected officials accountable and less likely to elect corrupt politicians (e.g. Ferraz and Finan, 2008; Banerjee et al., 2011, Pande, 2011) It is possible that by reducing the cost of not voting, and allowing less informed voters to select out of the voters' pool, we could increase the quality of elected officials.

6 Policy Preferences, Information Acquisition and Vote Buying

The results in the previous section are consistent with the predictions of the theoretical model of turnout presented in Section 2 and have important implications for the design of voters' incentives. A lower fine for not voting draw a lower share of the population the polls. This effect is particularly important for centrist voters, those who have lower subjective value of voting (or who are less interested in politics), and the uninformed. However, the natural question following these findings regards its implications for the aggregation of preferences and electoral outcomes.

6.1 Policy Preferences

Electoral institutions in democratic societies are designed to maximize voter representation and to ensure that policies are catered towards the interests of the majority. Mandating citizens to participate in elections imposes a cost on society, and it could be justified if the incentives to vote lead to improved representation. Theoretical arguments are mixed. Depending on the assumptions on the type of information available to voters, different authors have argued that compulsory voting can be welfare increasing or decreasing. Krishna and Morgan (2014) present a theoretical model showing that under voluntary voting, information aggregation holds, and mandating people to vote imposes a net cost to society. Along the same lines, Borgers (2004) reaches a similar conclusion based on a model with simple private value majoritarian elections. On the other hand, Ghosal and Lockwood (2009) use a model with common values to show that compulsory voting pareto-dominates voluntary participation.

Even though I am not able to rule out any of these models, I can provide suggestive evidence to help us think about how different incentive schemes to participate in elections affect policy outcomes. One way to address this issue is to analyze whether people who prefer certain type of policies are more likely to respond to monetary incentives to vote. If that is the case, a reduction of the fine for abstention will lead to under-representation, and thus the policies preferred by this group will not be enacted (assuming perfect commitment by politicians). To implement this test, I use the policy preference questions, aggregating them into 10 categories that represent broad policy issues, and analyze whether voters with specific policy preferences are more or less likely to respond to changes in the fine.

The results from this analysis are presented in Table 9. The interaction terms between policy preferences and change in the fine for not voting are not statistically significant and very close to zero, suggesting that voters with particular policy preferences are not over-represented among those who stop voting due to the treatment. The only interaction coefficient that comes through statistically and economically significant is the one for voters who have preferences for policies that promote agricultural activities (i.e. water projects, investment in improved seeds, etc.) This result is related to the literature showing empirically how the enfranchisement of different groups of the population leads to changes in policies (Husted and Kenny 1997, Fujiwara 2015, Miller 2008, Fowler 2013.) However, when the cost of voting is reduced, there is no *a priori* reason to expect that voters who select out of the pool of voters have different policy preferences, and thus allowing them to abstain should not lead to significant changes in policies. This is consistent with Hoffman et al. (2015), who show that the elimination of compulsory voting in some Austrian states does not lead to a significant change in the composition of expenditures.

Overall, these results suggest that the average voter who abstain when facing a lower fine for not voting does not have significantly different policy preferences than the average voter

who still votes despite the reduction in the fine. Assuming perfect commitment by politicians, this implies that the change in the electorate due to lower incentives to vote will not cause a change in the policies implemented by elected officials.

6.2 Information Acquisition

Proponents of mandatory voting argue that mandating people to vote not only increases participation, but also involves them in the political process, for example by providing incentivizing information acquisition. The underlying model for this claims is one similar to the one proposed here, but it endogenizes information acquisition (Martinelli, 2005, Degan, 2011, Oliveros, 2013). The intuition behind these models is that for sufficiently high penalties for not voting, abstention will drop and people might demand more political information to avoid making a *"voting mistake"*.

In the follow-up questionnaire, I included questions assessing the level of political information held by each respondent, which allow me to test if voters who stop voting due to a lower perceive penalty are less likely to acquire political information. In Panel A of Table 10 I regress the change in the different measures of political information on the change in the perceived fine, instrumented by the treatment status. The effect is very close to zero and not statistically significant. Voters who stop voting due to a lower costs for abstention do not acquire information differentially than their peers who face a higher fine. This result is consistent the predictions of the models in Degan (2006) and Degan (2013), as well as with the empirical findings in Loewen et al (2008) and Leite Lopez de Leon and Rizzi (2014), but contradict those in Shineman (2014), who shows that an increase in political participation leads to more information acquisition. However, my results must be taken with a grain of salt for two reasons. First, even though around the elections is when voters are more likely to get informed about the candidates and the political process overall, we must keep in mind that the average time between surveys was short (30 days). Second, in the medium or long-run people who stop voting might also change their behavior in terms of information acquisition.

6.3 Vote Buying

Electoral processes in developing countries are often prone to vote buying.[31] In my sample, 31 percent of respondents reported having been offered and accepted cash or a gift from a political operator. Vote buying represents a net welfare loss for society since it tends to distort voters preferences, affecting the results of an election. In electoral systems with mandatory voting, voters who go to the polls because of the mandate are more likely to accept money for their votes. If this were the case, the mandate generates a negative externality. Using the exogenous variation in the cost of not voting, I am able to test whether a reduction in

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[31]See: Vicente, 2013; Vicente and Wantchekon, 2009; Finan and Schechter, 2012.

the cost of not voting affects the incidence of vote buying and the price paid for each vote. I do this by using information collected in the final section of the follow-up survey, where I asked respondents if they were offered any in-kind gift (and their value) or cash by someone associated with any candidate or political party before the election. I also asked if the money or in-kind gift was given directly to the person, or indirectly as in a mass rally.

Panel B in Table 10 shows the effects of the change in perceived fines (instrumented by the treatment) on whether the voter accepted money for her vote, and the amount of money accepted. As a result of a reduction of the fine, we observe a lower share of the population attending to the polls, and thus the pool of potential votes to be bought is reduced. As shown before, voters still attending to the polls despite the lower sanctions of abstention are more likely to be well informed, have a strong political position and are interested in politics. Arguably, these voters are less willing to sell their vote, and when they do, a higher amount of money would be demanded.

The reduction in turnout due to the treatment generates an exogenous shift in the supply of votes. The results in Column (1) show that a decrease in the fine for abstention does not have a economically or statistically significant effect on the incidence of vote buying. Results in Column (2) show the effect on the amount of money received directly from a candidate or her representatives before the election. A change in the fine for abstention of S/.1 leads to an increase in the price of the vote of S/.0.03. Given the small number of observations, the coefficient has a large standard error, and its not statistically significant at the conventional levels, but its magnitude is still economically significant. The average voter, who perceived that the fines were reduced by S/.56, would have to be paid 49 percent more than before (S/.7.03) to buy her vote.

As a robustness check for this result, in Column (3) I use as a dependent variable the amount of money indirectly received by the voter. If there is a negotiation between the voter and the political operator about the price of the vote, I do not expect this negotiation to affect the amount received in a public rally. Indeed, I find a statistically and economically insignificant effect. Overall, the a reduction in the fine for abstention does not affect the incidence of vote buying, however each vote becomes more expensive, making it more costly to politicians to have influence on the outcome of the election through vote buying.

7   Summary and Discussion

Electoral institutions that encourage or mandate citizens to vote are widespread around the world. Such institutions are often introduced in the spirit of democratization, hoping to achieve better representation, and to involve the citizenship in the political process. However, since both voting and enforcing institutions are costly, there could be significant welfare losses if the objectives of higher participation and more involvement are not achieved.

In this paper I combine a natural experiment provided by a change in Peruvian voting laws with a field experiment to identify the effect of fines for abstention on voting. I find that a reduction in the cost of abstention decreases turnout, and that this reduction is more than proportional among (i) centrist voters, (ii) those who have a lower subjective value of voting, and (iii) voters who hold less political information. These results are consistent with the predictions of the rational choice model of voter behavior with imperfect information presented in the paper.

The estimates imply that cutting the fines for not voting by half leads to a 10 percentage point reduction in turnout. Further, the experimental design allows me to compute the elasticity of voting with respect to the cost, which I find to be -0.22. To my knowledge, this is the first paper to be able to estimate this parameter, which is key to evaluate policy interventions that attempt to affect the cost of voting, such as increasing in the number of polling stations, implementing electronic voting, among others.

Even though we observe a change in the electorate due to the reduction in the fine for not voting, this does not necessarily imply that the outcome of the election will be affected. On average, voters who stop going to the polls due to the reduction in the fine do not seem to have different policy preferences than their peers who do not respond to the change in the cost of abstention. This result implies that a reduction in the incentives to attend to the polls will likely not lead to a change in the policies enacted. Further, the fact that some people do not vote as a response to the treatment does not lead them to acquire less political information.

Additionally, I find suggestive evidence that a decrease in the fine for not voting decreases the externalities on related markets. More specifically, the reduction in the fine for abstention reduces the pool of voters who are willing to *sell* their vote, thus increasing the price paid by politicians to buy votes. Hence, lowering the incentives to vote reduces the chances politicians have to influence the election by making each vote more expensive.

In this paper, I contribute to the growing literature that uses field experiments to understand voter behavior in developing countries. Experimenting with the salience and information about an institutional change is a promising research tool to get causal estimates from specific institutional features. New laws are passed frequently, and for different reasons, they are not always publicized or citizens are not aware of them because of selective and limited attention. Even though it is nearly impossible to randomize an institution, we can experiment with its salience and information about it.

The results presented have strong implications for the design of electoral institutions. First, voters respond to monetary incentives to go to the polls, and the extent in which they respond is non-negligible. Second, the experimental evidence presented suggests that the objectives of mandatory voting, namely ensuring representation and involvement in politics, do not seem to be affected by the reduction in the incentives. If these results holds when the incentives are completely eliminated, mandatory voting would lead to a welfare loss to

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society. However, if the polarization of society has a negative weight in the policymaker's objective function, mandating voting might dominate, since it will ensure that centrists stay in the pool of voters.

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Figure 1: Discontinuity Analysis: Effect of Non-Voting Fine Law on Turnout
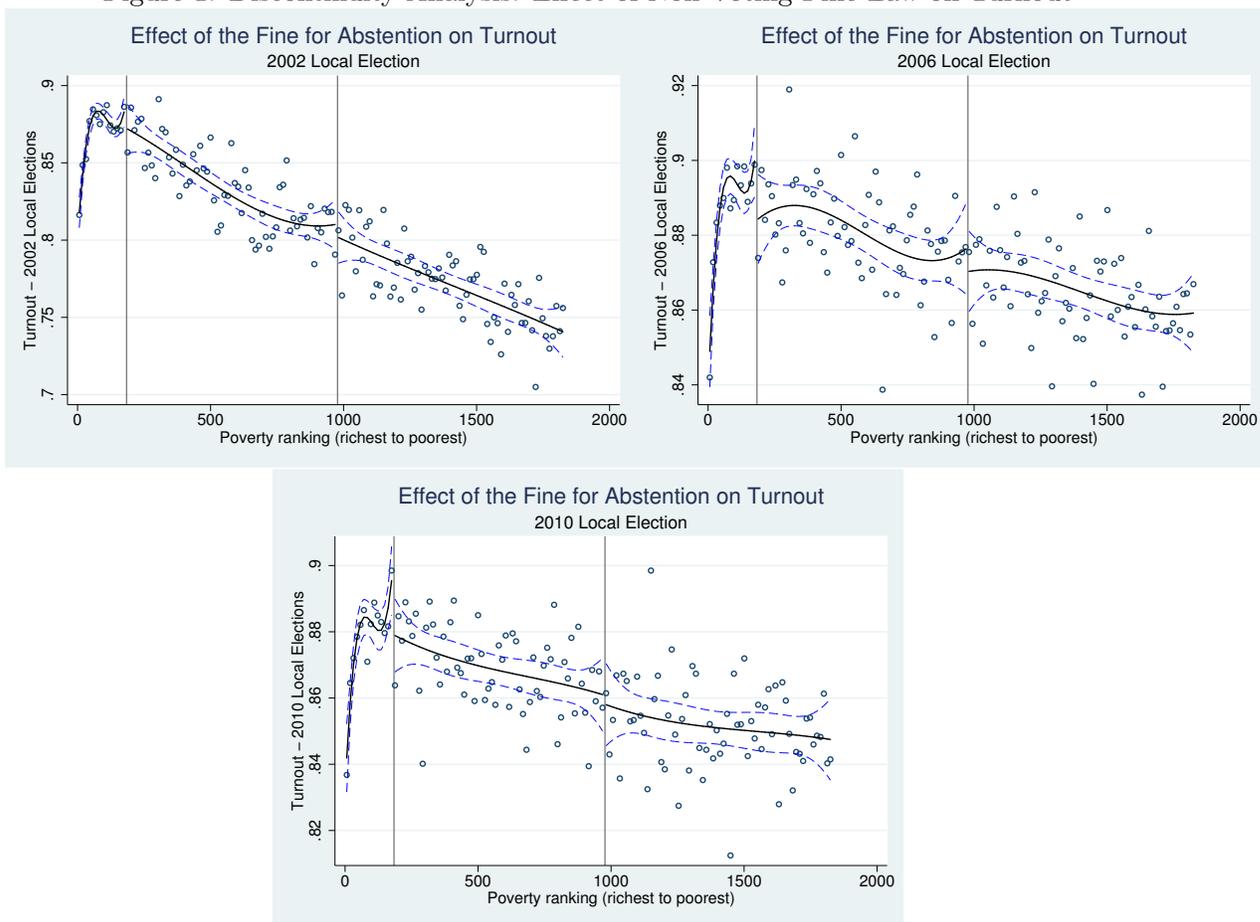
Notes: This figures plots the official turnout rates at the district level in the 2002, 2006, and 2010 municipal elections. Districts are ranked from richest to poorest using the poverty rankings computed by the Statistical office (INEI) for the electoral office (JNE). The vertical lines indicate the thresholds at which a district is categorized as non-poor, poor or extremely poor. For the 2010 elections, I exclude the 10 districts where I run the experiment from the sample to allow a cleaner comparison. The plots for 2002 and 2006 include these districts, but the basic results remain the same if I exclude them.

Figure 2: Perceived fines, by treatment and poverty status
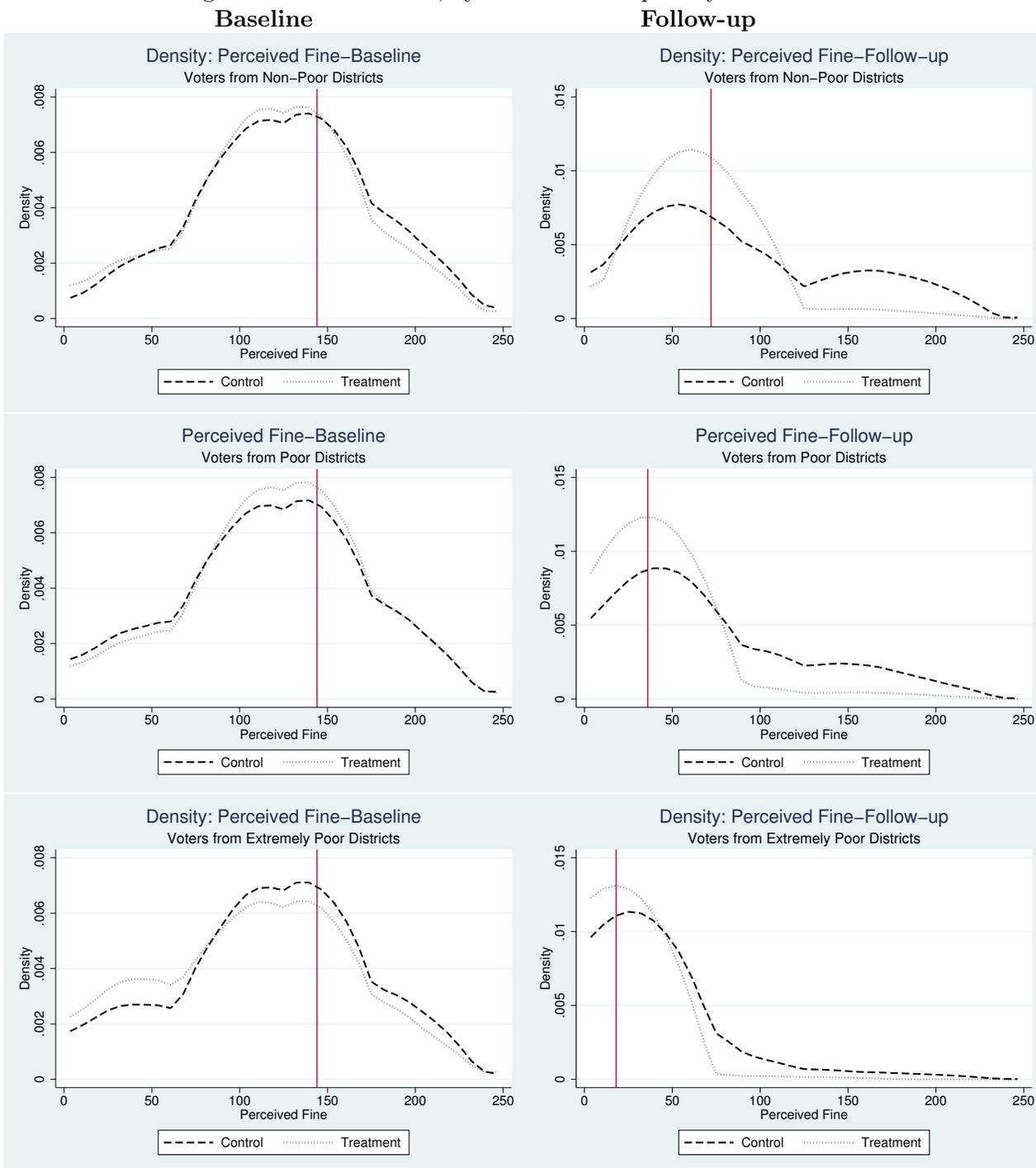
Notes: Kernel density estimates of the perceived value of the fine for abstention, as reported in the surveys. The data in the figures in the left panels come from the baseline survey, collected in before the election and before providing the treatment. The information in the right panels comes from the follow-up survey, collected after the election. The vertical lines denote the value of the fine for each poverty category.

Table 1: Turnout and Perceived Fine, by Treatment and Poverty Status

	Obs.	Total	Treatment	Control	T - C	P-value
		PANEL A: Perceived Fines				
Baseline						
Non-Poor	850	126.85	124.57	129.30	-4.73	(0.18)
Poor	882	121.74	121.90	121.58	0.32	(0.93)
Extreme Poor	541	115.30	111.35	119.32	-7.97	(0.13)
Total	2273	121.93	120.01	123.85	-3.85	(0.12)
Follow-up						
Non-Poor	850	76.84	65.88	88.43	-22.55	(0.00)
Poor	882	55.56	41.36	68.82	-27.47	(0.00)
Extreme Poor	541	27.16	19.14	35.32	-16.19	(0.00)
Total	2273	56.76	45.45	68.05	-22.60	(0.00)
Change						
Non-Poor	850	-49.51	-57.69	-40.87	-16.82	(0.00)
Poor	882	-66.18	-80.54	-52.76	-27.78	(0.00)
Extreme Poor	541	-88.14	-92.21	-84.00	-8.22	(0.15)
Total	2273	-65.17	-74.55	-55.80	-18.75	(0.00)
		PANEL B: Turnout				
Non-Poor	850	0.948	0.938	0.959	-0.021	(0.175)
Poor	882	0.941	0.913	0.967	-0.054	(0.001)
Extreme Poor	541	0.935	0.930	0.940	-0.010	(0.641)
Total	2273	0.942	0.927	0.958	-0.031	(0.002)

Notes: The actual changes that occurred were: for people voting in Non-poor districts, S/.72 (from S/.144 to S/.72); for those voting in Poor districts, S/.108 (from S/.144 to S/.36); and for people registered to vote in Extremely Poor districts, S/.126 (from S/.144 to S/.18). The sample comprises all respondents with information on all relevant covariates.

Table 2: Balance Between Treatment and Control Group

Variable	Obs.	Full Analysis Sample			Non-Poor			Poor		
		Treat	Control	Diff. p-val	Treat	Control	Diff. p-val	Treat	Control	Diff. p-val
Gender (Male=1)	1732	0.41	0.40	(0.64)	0.40	0.37	(0.37)	0.42	0.42	(0.87)
Age	1732	38.51	38.81	(0.63)	38.12	38.11	(0.99)	38.90	39.44	(0.55)
Yrs. of education	1732	10.05	10.00	(0.78)	10.35	10.28	(0.80)	9.75	9.74	(0.98)
PC Expenditures	1732	300.99	275.27	(0.12)	331.21	308.94	(0.30)	269.98	244.77	(0.32)
Center	1665	0.68	0.73	(0.05)	0.66	0.70	(0.18)	0.71	0.75	(0.17)
Left	1665	0.09	0.06	(0.01)	0.10	0.07	(0.09)	0.09	0.06	(0.08)
Right	1665	0.22	0.21	(0.54)	0.24	0.23	(0.72)	0.21	0.19	(0.68)
Policy Extreme 1 (Pub. goods)	1732	0.14	0.16	(0.28)	0.05	0.07	(0.22)	0.24	0.25	(0.83)
Policy Center	1732	0.58	0.61	(0.20)	0.54	0.54	(0.79)	0.62	0.66	(0.17)
Policy Extreme 2 (Club goods)	1732	0.28	0.23	(0.02)	0.41	0.38	(0.42)	0.14	0.09	(0.02)
Very Interested in politics	1713	0.07	0.07	(0.90)	0.08	0.08	(0.90)	0.07	0.06	(0.79)
Interested in politics	1713	0.48	0.45	(0.30)	0.46	0.45	(0.80)	0.50	0.46	(0.21)
Not Interested in politics	1713	0.45	0.48	(0.27)	0.47	0.47	(0.85)	0.43	0.48	(0.17)
Very Interested in the results of this election	1732	0.39	0.37	(0.29)	0.37	0.32	(0.12)	0.41	0.41	(0.93)
Interested in the results of this election	1717	0.47	0.45	(0.37)	0.47	0.47	(0.85)	0.46	0.43	(0.31)
Not Interested in the results of this election	1732	0.14	0.18	(0.02)	0.15	0.21	(0.04)	0.12	0.16	(0.15)
Very Interested in the campaign of this election	1714	0.09	0.09	(0.88)	0.10	0.09	(0.65)	0.08	0.09	(0.75)
Interested in the campaign of this election	1714	0.57	0.54	(0.12)	0.53	0.52	(0.86)	0.62	0.55	(0.03)
Not Interested in the campaign of this election	1714	0.33	0.37	(0.08)	0.37	0.39	(0.64)	0.30	0.36	(0.04)
Name recall- Candidates running	1732	0.36	0.36	(0.78)	0.29	0.27	(0.23)	0.43	0.45	(0.37)
Name recall- Parties running	1732	0.28	0.30	(0.35)	0.22	0.21	(0.60)	0.34	0.37	(0.23)
Name recall- Candidates+Parties running	1732	0.32	0.33	(0.54)	0.26	0.24	(0.36)	0.38	0.41	(0.27)
Political information score	1732	0.56	0.55	(0.42)	0.56	0.55	(0.21)	0.55	0.56	(0.95)

Notes: The table includes all subjects interviewed in the baseline and follow-up surveys in the Analysis sample, i.e. the 1,732 voters in poor or non-poor districts. Table A.4 in the Appendix shows the covariate balance for the full sample.

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Table 3: Reduced Form and First Stage Regressions

	Panel A: Reduced Form Regressions		
	Dep. Var: Voted in the 2010 Election		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-.027 (0.015)*		-.026 (0.015)*
Treatment: Fine S/.36		-.052 (0.016)***	-.053 (0.016)***
Gender (Male=1)	-.0009 (0.016)	0.018 (0.016)	0.013 (0.011)
Age	0.001 (0.0007)	0.001 (0.0006)**	0.001 (0.0005)***
Yrs. of education	0.002 (0.002)	0.004 (0.003)	0.004 (0.002)**
Log(PC Expenditures)	0.004 (0.008)	0.011 (0.013)	0.007 (0.008)
Votes in Poor district			0.0006 (0.022)

	Panel B: First Stage Regressions		
	Dep. Var: $\triangle$ Perceived Fine		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-16.962 (4.903)***		-17.420 (4.851)***
Treatment: Fine S/.36		-28.908 (4.707)***	-28.848 (4.644)***
Gender (Male=1)	-2.740 (4.972)	-2.578 (4.706)	-2.941 (3.387)
Age	0.32 (0.203)	0.391 (0.178)**	0.346 (0.133)***
Yrs. of education	0.455 (0.749)	-.535 (0.69)	-.031 (0.498)
Log(PC Expenditures)	-4.280 (3.524)	-1.093 (3.533)	-2.155 (2.528)
Votes in Poor district			0.486 (6.260)
Village FE	Y	Y	Y
Mean: Voted in the 2010 Election	0.948	0.941	0.944
Mean: $\triangle$ Perceived Fine	-49.51	-66.18	-58.00
First Stage F-stat	13.08	41.90	27.81
Obs.	850	882	1732

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Panel A presents linear probability models, while in Panel B we use OLS. Regression equations are specified in equations (4) and (5).

Table 4: IV - Effect of Change in Perceived Fines on Turnout

	Dep. Var: Voted in the 2010 Election		
	Non-Poor	Poor	All
$\triangle$ Perceived Fine	0.0016	0.0018	0.0017
	(0.001)*	(0.0006)***	(0.0005)***
Gender (Male=1)	0.0035	0.023	0.0184
	(0.0179)	(0.0172)	(0.0126)
Age	0.0005	0.0008	0.0007
	(0.0008)	(0.0007)	(0.0005)
Yrs. of education	0.0009	0.0053	0.0039
	(0.0025)	(0.0031)*	(0.002)*
Log(PC Expenditures)	0.0111	0.0133	0.0108
	(0.0113)	(0.0147)	(0.0089)
Votes in Poor district			-.0038
			(0.0219)
Village FE	Y	Y	Y
Mean dep. var.	0.948	0.941	0.944
Obs.	850	882	1732
F-statistic	11.96	37.72	25.62

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. All columns present 2SLS estimations where Δ Perceived Fine is instrumented by the two treatment dummies (Treatment: Fine S/.72 and Treatment: Fine S/.36, from Table 3). The regression equation is specified in equation (6).

Table 5: Robustness: Effect of the Treatment on Perceived Consequences of Abstaining

			Dep. Var: Consequence of Abstaining:			
	Fine	Can't use banks	Can't use pubic inst (municipality, police, etc.)	Can't use notariat.	Can't use registry (contracts)	Other: (travel, jail, jobs, etc.)
Treatment: Fine S/.72	-.0056 (0.0051)	0.0188 (0.0271)	0.0108 (0.0209)	0.0005 (0.0168)	0.0178 (0.0152)	0.0252 (0.0329)
Treatment: Fine S/.36	-.0076 (0.0051)	-.0023 (0.0218)	-.0013 (0.0179)	-.0107 (0.0154)	-.0013 (0.0131)	0.0306 (0.0259)
Gender (Male=1)	0.0054 (0.0042)	0.0143 (0.017)	0.0185 (0.0133)	-.0002 (0.0114)	0.0096 (0.0103)	0.0215 (0.0193)
Age	-.0002 (0.0001)	0.0014 (0.0007)**	0.0009 (0.0005)*	0.0013 (0.0005)***	0.0011 (0.0004)***	0.0025 (0.0008)***
Yrs. of education	-.0008 (0.0006)	0.0078 (0.0024)***	0.0035 (0.0019)*	0.0014 (0.0017)	0.004 (0.0015)***	0.0038 (0.0029)
Log(PC Expenditures)	0.0002 (0.0017)	-.0004 (0.012)	0.0003 (0.01)	0.013 (0.0086)	0.0062 (0.008)	-.0116 (0.0132)
Votes in Poor district	-.0034 (0.0048)	0.0306 (0.0299)	0.0087 (0.023)	0.0122 (0.0188)	-.0036 (0.0164)	-.0409 (0.0367)
Village FE	Y	Y	Y	Y	Y	Y
Mean dep. var.	0.994	0.155	0.096	0.062	0.047	0.287
Obs.	1712	1712	1712	1712	1712	1712

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. All 2SLS models omit the constant, include village fixed effects and controls, and the endogenous regressor is instrumented by the two treatment dummies. The regression equation for the second stage is given by: $Consequence_{ij} = \alpha + \beta_1 NonPoor_{ij} \cdot Treat_{ij} + \beta_2 Poor_{ij} \cdot Treat_{ij} + \beta_3 Poor_{ij} + \gamma X_{ij} + \delta_k + \eta_{ij}$.

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Table 6: Robustness checks

	Panel A: Including Voters from Ext. Poor Districts		
	Reduced Form	First Stage	IV
		Dependent Variable:	
	Voted in 2010	△ in Perceived Fine	Voted in 2010
△ Perceived Fine			0.0016
			(0.0005)***
Treatment: Fine S/.72	-.0208	-17.4542	
	(0.0157)	(4.8582)***	
Treatment: Fine S/.36	-.0508	-28.5876	
	(0.0161)***	(4.6360)***	
Treatment: Fine S/.18	-.0091	-7.7912	
	(0.0201)	(5.8322)	
Mean dep. var.	0.9424	-65.17	0.9424
Obs.	2273	2273	2273
F-statistic			17.46
	Panel B: Potential Violation of Monotonicity		
	Reduced Form	First Stage	IV
		Dependent Variable:	
	Voted in 2010	△ in Perceived Fine	Voted in 2010
△ Perceived Fine			0.0014
			(0.0005)***
Treatment: Fine S/.72	-.0240	-20.0418	
	(0.016)	(4.6314)***	
Treatment: Fine S/.36	-.0434	-28.4220	
	(0.0169)**	(4.2909)***	
Mean dep. var.	0.945	-69.66	0.945
Obs.	1540	1540	1540
F-statistic			31.28
	Panel C: Changes in Perceived Fine and Past Turnout (IV)		
	Dep. Var: Voted in the 2006 Election		
	Non-Poor	Poor	All
△ Perceived Fine	-.00018	0.0007	0.00008
	(0.0012)	(0.0006)	(0.0005)
Mean dep. var.	0.9459	0.9444	0.9451
Obs.	757	791	1548
F-statistic	9.49	29.64	20.84

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation in Panel A follows the structure detailed in the main text in equations (4),(5), and (6), but it includes an indicator for voting in an extremely poor district, and the corresponding interactions. In Panel B, we consider the analysis sample, but exclude all voters from poor and non-poor municipalities whose baseline perceived fine was below the actual fine (192 voters, who correspond to 11% of the analysis sample, i.e. the sample in which we might find *defiers*.) In Panel C, the dependent dependent variable is a (self-reported) dummy equal to one if the respondent reported having voted in the 2006 municipal election. The sample size in Panel C is smaller than the analysis sample because there are a number of voters in the sample who voted for the first time in 2010. All regressions include village fixed effects and controls.

Table 7: Robustness checks: Attrition and Measurement of Voting

	Panel A: Lee (2009) Bounds for Attrition		
	Dep. Var: Voted in the 2010 Election		
	Full Sample	Lower Bound	Upper Bound
△ Perceived Fine	0.0017	0.0016	0.0031
	raisebox.7ex[0pt](0.0005)***	(0.0005)***	(0.0006)***
Village FE	Y	Y	Y
Mean dep. var.	0.944	0.942	0.962
Obs.	1732	1665	1700	
F-stat	25.6271	24.2043	26.4584	

	Panel B: Probability of not Showing the ID and Treatment		
	Dep. Var: Didn't Show ID Card		
	Full Sample	Self Reported: Voted	Self Reported: Abstained
Treatment: Fine S/.72	-0.033	-0.034	-0.063
	(0.036)	0.036	(0.155)
Treatment: Fine S/.36	-0.004	-0.019	-0.000
	(0.032)	(0.032)	(0.172)
Village FE	Y	Y	Y
Mean dep. var.	0.347	0.329	0.667
Obs.	1732	1633	96

	Panel C: Different Measures of Turnout				
	Dep. Var: Voted in the 2010 Election				
		Available Sample		Comparable Sample	
	Benckmark	Self Reported	Sticker	Self Reported	Sticker
△ Perceived Fine	0.0017	0.0014	0.0015	0.0011	0.0016
	(0.0005)***	(0.0005)***	(0.0005)***	(0.0005)**	(0.0005)***
Village FE	Y	Y	Y	Y	Y
Mean dep. var.	0.944	0.944	0.971	0.971	0.971
Obs.	1732	1729	1130	1127	1127
F-statistic	25.627	25.248	15.573	15.148	15.148

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. In Panel A I run the main 2SLS regression as specified as in Equation 6, dropping some observations from the control group under different assumptions, following Lee (2009). In Panel B, all linear probability models follow the same structure as in equation (4), including controls and village fixed effects. The dependent variable is a dummy equal to one if the respondent did not show her ID card to the enumerator. Columns (2) and (3) include voters who reported having voted or abstained in the follow-up, respectively. In Panel C, the 2SLS models are specified as in Equation 6. The dependent variables are self reported or objective measures of turnout. Columns (1)-(3) use all the analysis sample for which the dependent variable is available. Column (1) presents the benchmark specification from Table 3, Columns (2)-(3) use as dependent variables a self reported and objective (i.e. Sticker) measure of turnout, respectively. Columns (4) and (5) repeat the exercise from Columns (2)-(3), but restricting the sample to voters for whom both outcomes are observed (i.e. Comparable sample).

Table 8: Effect of Changes in Perceived Fine on Turnout, by Political Preferences, Interest in Politics and Information

	Dep. Var: Voted in the 2010 Election			
	PANEL A: Political Preferences			
△ Fine*Left	-.0006 (0.0024)			
△ Fine*Center	0.0015 (0.0006)**			
△ Fine*Right	0.001 (0.0008)			
△ Fine*Policy Extreme 1		0.0003 (0.0013)		
△ Fine*Policy Center		0.002 (0.0007)***		
△ Fine*Policy Extreme 2		0.0008 (0.001)		
Obs.	1650	1650		
	PANEL B: Interest in Politics			
△ Fine*Very interested in politics	0.0003 (0.0017)			
△ Fine*Interested in politics	0.0012 (0.0007)*			
△ Fine*Not interested in politics	0.0019 (0.0007)***			
△ Fine*Very interested in results		0.0007 (0.0006)		
△ Fine*Interested in results		0.002 (0.0008)**		
△ Fine*Not interested in results		0.0038 (0.0019)**		
△ Fine*Very interested in pol. campaign			0.0024 (0.002)	
△ Fine*Interested in pol. campaign			0.0008 (0.0005)	
△ Fine*Not interested in pol. campaign			0.0024 (0.0011)**	
Obs.	1650	1654	1651	
	PANEL C: Political Information			
△ Perceived Fine	0.0024 (0.0008)***	0.0021 (0.0007)***	0.0024 (0.0007)***	0.006 (0.0027)**
△ Fine*Candidate recall	-.0029 (0.0012)**			
△ Fine*Party recall		-.0026 (0.0011)**		
△ Fine*Candidate and Party recall			-.0032 (0.0012)**	
△ Fine*Pol. Info. Score				-.0081 (0.0045)*
Obs.	1650	1650	1650	1650

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Information on political preferences, interest in politics, and political information was collected in the baseline survey. All 2SLS models include village fixed effects, demographic controls, controls for political preferences (right, left), interest in politics (Very interested in politics, interested in politics) and information (Candidate recall), and the endogenous regressor and its interactions are instrumented by the two treatment dummies and the relevant interactions. The regression equations for the second stages are specified in equations (7) and (8).

Table 9: Effects by policy preferences

| | Dep. Var.: Voted in the 2010 Election | |
| | Coeff. on | Coeff. on |
Policy	△ Perceived Fine	△ Perceived Fine*Policy
(1) Health	0.002	-.0005
	(0.0008)**	(0.001)
(2) Education	0.001	0.0012
	(0.0006)*	(0.001)
(3) Infrastructure	0.0011	0.0007
	(0.0012)	(0.0014)
(4) Order and Security	0.0023	-0.0013
	(0.0008)***	(0.0011)
(5) Promote micro-enterprises/training	0.0017	0.0002
	(0.0006)***	(0.0012)
(6) Agriculture	0.0024	-0.0022
	(0.0007)***	(0.0009)**
(7) Youth/Women	0.0014	0.0013
	(0.0006)**	(0.0012)
(8) Cleaning/Environment	0.0014	0.0009
	(0.0006)**	(0.0011)
(9) Institutions	0.0019	-0.0010
	(0.0006)***	(0.0011)
(10) Social/work programs	0.0018	-0.0004
	(0.0006)***	(0.0011)

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. The coefficients shown in each row come from separate 2SLS regressions models, include village fixed effects and controls, and the endogenous regressor and its interactions are instrumented by the two treatment dummies and the relevant interactions. The table reports the coefficients for the instrumented endogenous regressor and the relevant interaction. Policy preferences include: (1) Health: Infrastructure, health professionals, and training for health workers; (2) Education: Infrastructure, teachers, and training for teachers; (3) Infrastructure: Roads and access to them, sewage, water, electricity and telecommunications infrastructure, build markets, churches, community building, main square; (4) Order and Security: Traffic, more policemen in the streets, fight drugs and gangs; (5) Promote micro-enterprises/training: promote micro/small firms, train local entrepreneurs, promote private investment, promote tourism; (6) Agriculture: Build dams and irrigation infrastructure, technical assistance to agriculture, seed banks, support livestock farmers; (7) Youth/Women: Women empowerment and equality, youth policies, sporting events; (8) Cleaning/Environment: street cleaning, increase green areas, promote recycling; (9) Institutions: Transparency in managing the municipality, fight corruption, modernize the bureaucracy, participatory decision-making, land titling; (10) Social/work programs: Job training programs, help those in poverty, food aid, child care, generate jobs. For each of these categories, the dependent variable is a dummy indicating whether the respondent named at least one of the policies in this category as one of her five priorities for the district.

Table 10: Effects of Fines on Information Acquisition and Vote Buying

	Panel A: Effect of Fines on Information Acquisition			
	Dep. Var.:			
	$\triangle$ Candidate Recall	$\triangle$ Party Recall	$\triangle$ Cand.+Party Recall	$\triangle$ Pol. Info Score
$\triangle$ Perceived Fine	-.0003 (0.0005)	-.0006 (0.0006)	-.0004 (0.0005)	-.00002 (0.0004)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Obs.	1732	1732	1732	1732
F-Stat	25.6271	25.6271	25.6271	25.6271
	Panel B: Effects of Fines on Vote buying			
	Dep. Var:			
	1=Accepted Money or a Gift	Amount Accepted Directly	Amount Accepted Indirectly	Amount Accepted Total
$\triangle$ Perceived Fine	-.0002 (0.0009)	-.0657 (0.0485)	0.0105 (0.0176)	-.0633 (0.0505)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean dep. var.	0.310	7.03	3.22	10.80
Obs.	1732	537	537	537
F-statistic	25.62	11.40	11.40	11.40

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. In Panel A, the regression equation is:
$\triangle Info_{ij} = \alpha + \beta_1 \triangle Fine_{ij} + \beta_2 Poor_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$, where $\triangle Info_{ij}$ represents the change in the political information between the baseline and follow-up surveys, and $\triangle Fine_{ij}$ is instrumented using the treatment dummies. In Panel B, the regression equation is: $Y_{ij} = \alpha + \beta_1 \triangle Fine_{ij} + \beta_2 Poor_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$. In Column (1), Y_{ij} is an indicator for whether voter i accepted money from a politician or his/her representative for her vote. In Column (2) through (4), the dependent variable measures the amount of money accepted (directly or indirectly) to buy a vote. All 2SLS models include village fixed effects and controls.

# APPENDIX (Not intended for publication)

Figure A.1: Geographic location of the districts in the survey
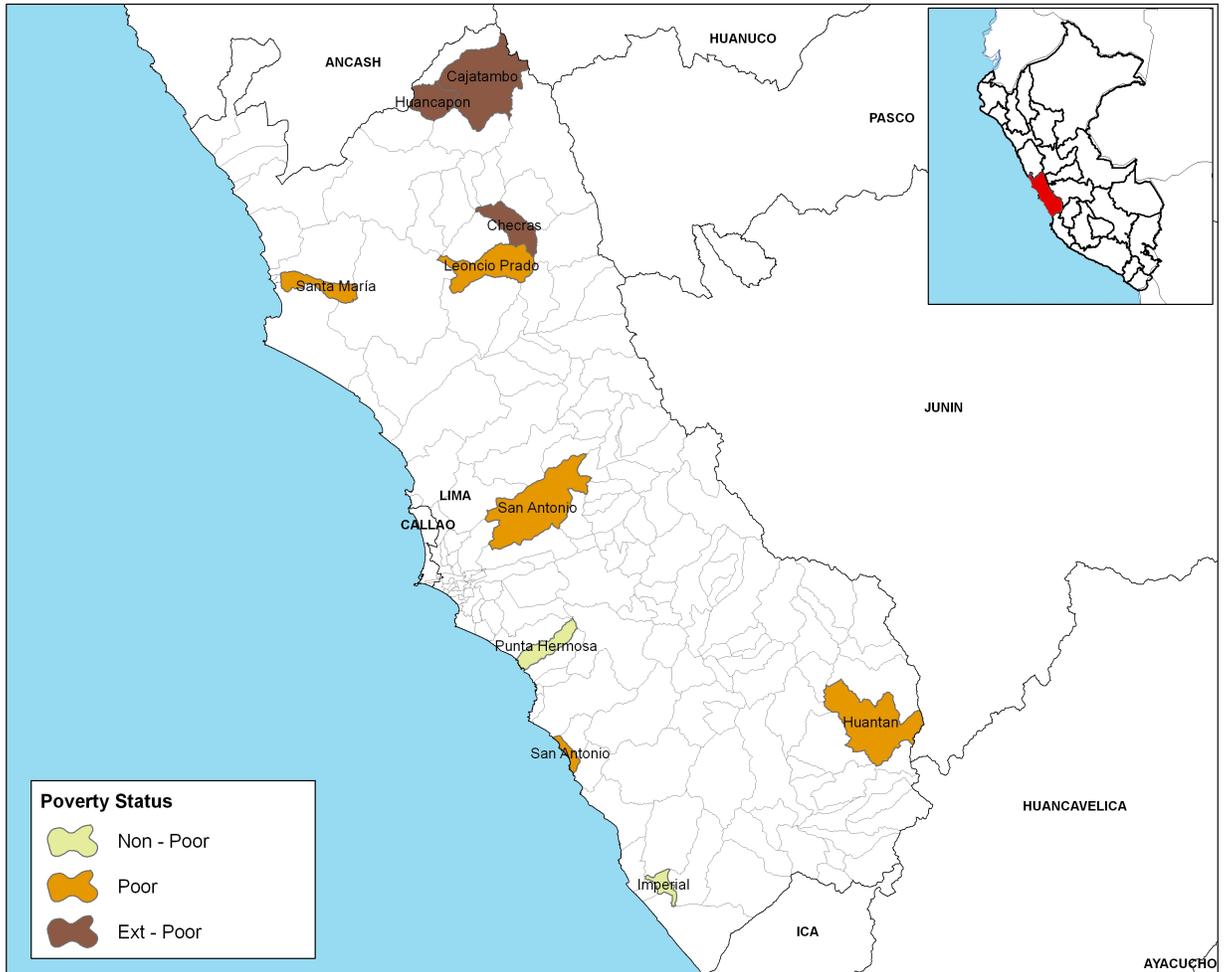
Figure A.2: Fliers for the Treatment and Control Groups

Flier for the Treatment group:

Estimado Sr(a):

En Agosto del 2006, el Congreso de la República aprobó una reducción de las multas para los omisos al voto (Ley No. 28859). Según esta ley, aquellos que no voten ya no estarán sujetos a una multa de S/. 144, sino que la multa es menor para todos, y escalonada de acuerdo al nivel de pobreza del distrito.

De acuerdo a la información que Ud. me ha dado, si es que Ud. no vota en las elecciones de octubre de este año, tendrá que pagar una multa de S/._____.

*Si tiene alguna consulta sobre esta información, puede contactarse con el coordinador de este proyecto, el Sr. Gianmarco León, al teléfono 99985-0252

Flier for the Control group:

Estimado Sr(a).:

Recuerde que en el Perú votar es obligatorio, y estar omiso al voto, es decir, no asistir a cualquier elección, está sujeto a una sanción que implica el pago de una multa.

Table A.1: Descriptive Statistics, Districts sampled

		Lima Lima Punta Hermosa	Lima Cañete Imperial	Lima Huaura Santa Maria	Lima Cañete San Antonio	Lima Huarochiri San Antonio	Lima Huaura Leoncio Prado	Lima Yauyos Huantan	Lima Cajatambo Huancapon	Lima Cajatambo Cajatambo	Lima Huaura Checras
Region											
Province											
District											
Poverty											
Poverty Ranking		173	176	191	201	841	876	938	978	1042	1047
Poverty Category		Non-poor	Non-poor	Poor	Poor	Poor	Poor	Poor	Ext. Poor	Ext. Poor	Ext. Poor
Number of HHs		1767	8170	6429	897		717	311	461	791	593
Sampled HHs		251	248	240	241	240	146	93	166	166	121
Sampled Individuals		632	930	756	776	668	418	264	460	462	308
Electoral variables											
Registered voters	2006-1	4,291	24,668	17,577	2,200	5,564	872	543	724	1,857	490
	2006-2	4,291	24,668	17,577	2,200	5,564	872	543	724	1,857	490
	2006-3	4,297	25,006	18,183	2,255	6,478	1,017	568	778	1,891	550
	2010	4,615	26,873	20,630	2,525	13,640	1,367	683	921	1,820	664
Turnout	2006-1	88.9%	92.6%	92.1%	95.0%	93.9%	91.2%	86.0%	77.1%	73.6%	86.5%
	2006-2	88.4%	92.2%	91.4%	94.8%	93.7%	90.5%	88.0%	79.0%	78.3%	85.5%
	2006-3	87.5%	91.1%	90.1%	94.5%	92.5%	91.6%	83.1%	82.5%	75.4%	88.7%
	2010	86.7%	90.0%	88.2%	93.7%	89.9%	91.4%	82.0%	83.6%	79.0%	85.4%
Blank Votes (%)	2006-3	3.1%	9.1%	7.0%	8.4%	11.5%	5.5%	8.9%	8.9%	13.9%	12.3%
	2010	3.5%	7.4%	9.3%	6.8%	14.3%	11.6%	10.2%	9.0%	15.5%	4.6%
Invalid Votes (%)	2006-3	3.8%	3.7%	9.8%	13.2%	9.2%	2.5%	4.9%	34.3%	5.4%	2.0%
	2010	2.6%	5.7%	5.4%	12.3%	8.0%	0.7%	4.5%	4.3%	4.4%	39.3%
Num. of candidates running	2006	10	11	13	7	8	9	8	6	7	9
for the local government	2010	5	13	20	5	12	2	7	6	6	6

Notes: 2006-1 and 2006-2 refers to the first and second round of the presidential elections held in April and June 2006, respectively. 2006-3 refers to the municipal elections held in November, 2006, the first elections under the law that reduced the fines. Information on the number of households comes from the national census, while that on turnout, invalid votes and political competition comes from the ONPE (electoral office).

Table A.2: Balance Between Attrited and non-Attrited, by Poverty Category

Variable	Full Analysis Sample				Non-Poor				Poor			
	Obs.	Non-Attrited	Attrited	Diff. p-val	Obs.	Non-Attrited	Attrited	Diff. p-val	Obs.	Non-Attrited	Attrited	Diff. p-val
Perceived Fine (Baseline)	2214	124.00	125.99	(0.48)	1733	126.34	131.40	(0.19)	481	121.74	119.14	(0.53)
Gender (Male=1)	2214	0.40	0.49	(0.00)	1733	0.39	0.47	(0.02)	481	0.42	0.52	(0.01)
Age	2214	38.67	39.14	(0.49)	1733	38.14	38.23	(0.92)	481	39.18	40.29	(0.28)
Yrs. of education	2214	10.02	9.65	(0.07)	1733	10.31	10.00	(0.23)	481	9.74	9.21	(0.09)
PC Expenditures	2214	288.19	319.06	(0.15)	1733	320.58	332.90	(0.67)	481	256.95	301.50	(0.16)
Center	2142	0.70	0.68	(0.41)	1666	0.68	0.66	(0.52)	476	0.73	0.72	(0.75)
Left	2142	0.08	0.08	(0.90)	1666	0.08	0.08	(0.78)	476	0.07	0.07	(0.96)
Right	2142	0.22	0.24	(0.32)	1666	0.24	0.26	(0.38)	476	0.20	0.21	(0.75)
Very Interested in politics	2186	0.07	0.06	(0.44)	1714	0.08	0.05	(0.12)	472	0.07	0.08	(0.60)
Interested in politics	2186	0.46	0.46	(0.78)	1714	0.45	0.42	(0.35)	472	0.48	0.51	(0.45)
Not Interested in politics	2186	0.46	0.48	(0.50)	1714	0.47	0.53	(0.08)	472	0.46	0.42	(0.31)
Very Interested in the results of this election	2214	0.38	0.35	(0.20)	1733	0.35	0.32	(0.44)	481	0.41	0.38	(0.43)
Interested in the results of this election	2193	0.46	0.48	(0.47)	1718	0.47	0.49	(0.51)	475	0.44	0.45	(0.81)
Not Interested in the results of this election	2214	0.16	0.17	(0.55)	1733	0.18	0.18	(0.96)	481	0.14	0.16	(0.44)
Very Interested in the campaign of this election	2191	0.09	0.11	(0.37)	1715	0.10	0.09	(0.52)	476	0.08	0.13	(0.04)
Interested in the campaign of this election	2191	0.55	0.50	(0.04)	1715	0.52	0.48	(0.19)	476	0.59	0.53	(0.16)
Not Interested in the campaign of this election	2191	0.35	0.39	(0.10)	1715	0.38	0.44	(0.08)	476	0.33	0.34	(0.81)
Name recall- Candidates running	2213	0.36	0.39	(0.15)	1733	0.28	0.26	(0.48)	480	0.44	0.54	(0.00)
Name recall- Parties running	2213	0.29	0.29	(0.70)	1733	0.22	0.20	(0.28)	480	0.35	0.41	(0.03)
Name recall- Candidates+Parties running	2213	0.32	0.34	(0.32)	1733	0.25	0.23	(0.36)	480	0.40	0.48	(0.00)
Political information score	2214	0.55	0.56	(0.83)	1733	0.55	0.55	(0.69)	481	0.55	0.57	(0.45)

Notes: The table includes all subjects interviewed in the baseline survey, and who are considered in the analysis sample, i.e., voters registered in poor or non-poor districts. The table does not show the balance for the policy preference variables, since the pricipal component is computed only among those who enter the analysis (non-Attritters). Overall, there are 481 respondents in the analysis sample that were not found in the follow-up survey (21.7% of the sample). In Non-Poor districts, we could not find 269 respondants (24%), and in Poor districts, 212 were missing in the follow-up (19.4%).

Table A.3: Learning and Days between Surveys

| | Dep. Var.: △ Perceived Fine | | | |
	Treatment	Control	Treatment	Control
Num. days: Baseline-Follow-up	-.0719 (0.2003)	-.7557 (0.242)***		
Num. days: Baseline-Election			-.0841 (0.2243)	-.0916 (0.2728)
Num. days: Election-Follow-up			-.0336 (0.4156)	-2.6682 (0.4573)***
Obs.	862	868	862	868

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. OLS regressions include dummies for poverty category.

Table A.5: Coefficients for Policy Preference First Principal Component

Policy issues	Coefficients
Health: infrastructure	-0.109
Health: personnel and services	-0.146
Education: infrastructure	-0.123
Education: teachers and services	-0.081
Transport: Ordering transit	0.042
Transport: Infrastructure (roads, access, etc.)	-0.358
Basic services: Water, electricity, sewage, communications	-0.452
Promote tourism	-0.055
Economics: Support micro and small enterprises	-0.025
Economics: Training to local enetrepeneurs	-0.028
Economics: Agriculture - technical assistance, and training to local producers	-0.274
Economics: Agriculture - infrastructure projects for agriculture	-0.111
Economics: promote private investment	-0.029
Youth: Sport activities and infrastructure	-0.037
Youth: Labor training programs	0.018
Women: empowerment and programs	0.003
Social: More participation, participatory budgets	-0.024
Security: More policemen	0.153
Security: Fight gangs and drugs in the streets	0.225
Environment: Cleaning the district Garbage trucks	0.045
Environment: More green areas	-0.066
Environment: Recycling of solid residues	0.001
Institutional: Transparency in procedures	-0.025
Institutional: Modernize procedures	-0.030
Infrastructure: Markets, public buildings	-0.052
Social: Children and elderly programs, school lunches, etc.	-0.028
Social: work for the poor	-0.017
Housing: land and house titling	-0.035

Table A.4: Balance Between Treatment and Control Groups, by Poverty Category

Variable	Full Analysis Sample			Treatment			Control			T - C	P-value
	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.		
Perceived Fine (Baseline)	1732	124.00	54.54	863	122.74	53.08	869	125.25	55.97	-2.51	(0.34)
Gender (Male=1)	1732	0.40	0.49	863	0.41	0.49	869	0.40	0.49	0.01	(0.64)
Age	1732	38.66	13.13	863	38.51	13.06	869	38.81	13.21	-0.30	(0.63)
Yrs. of education	1732	10.02	3.93	863	10.05	3.85	869	10.00	4.00	0.05	(0.78)
PC Expenditures	1732	288.08	346.28	863	300.99	400.01	869	275.27	282.66	25.72	(0.12)
Center	1665	0.71	0.46	832	0.68	0.47	833	0.73	0.45	-0.04	(0.05)
Left	1665	0.08	0.27	832	0.09	0.29	833	0.06	0.24	0.03	(0.01)
Right	1665	0.22	0.41	832	0.22	0.42	833	0.21	0.41	0.01	(0.54)
Policy Extreme 1 (Pub. goods)	1732	0.15	0.36	863	0.14	0.35	869	0.16	0.37	-0.02	(0.28)
Policy Center	1732	0.59	0.49	863	0.58	0.49	869	0.61	0.49	-0.03	(0.20)
Policy Extreme 2 (Club goods)	1732	0.26	0.44	863	0.28	0.45	869	0.23	0.42	0.05	(0.02)
Very Interested in politics	1713	0.07	0.26	854	0.07	0.26	859	0.07	0.26	0.00	(0.90)
Interested in politics	1713	0.47	0.50	854	0.48	0.50	859	0.45	0.50	0.02	(0.30)
Not Interested in politics	1713	0.46	0.50	854	0.45	0.50	859	0.48	0.50	-0.03	(0.27)
Very Interested in the results of this election	1732	0.38	0.49	863	0.39	0.49	869	0.37	0.48	0.02	(0.29)
Interested in the results of this election	1717	0.46	0.50	858	0.47	0.50	859	0.45	0.50	0.02	(0.37)
Not Interested in the results of this election	1732	0.16	0.37	863	0.14	0.34	869	0.18	0.38	-0.04	(0.02)
Very Interested in the campaign of this election	1714	0.09	0.29	853	0.09	0.29	861	0.09	0.29	0.00	(0.88)
Interested in the campaign of this election	1714	0.55	0.50	853	0.57	0.49	861	0.54	0.50	0.04	(0.12)
Not Interested in the campaign of this election	1714	0.35	0.48	853	0.33	0.47	861	0.37	0.48	-0.04	(0.08)
Name recall- Candidates running	1732	0.36	0.36	863	0.36	0.35	869	0.36	0.36	-0.00	(0.78)
Name recall- Parties running	1732	0.29	0.32	863	0.28	0.32	869	0.30	0.33	-0.01	(0.35)
Name recall- Candidates+Parties running	1732	0.33	0.32	863	0.32	0.32	869	0.33	0.33	-0.01	(0.54)
Political information score	1732	0.55	0.17	863	0.56	0.17	869	0.55	0.17	0.01	(0.42)

Notes: The table includes all subjects interviewed in the baseline and follow-up surveys, and who are considered in the analysis sample, i.e.. voters registered in poor or non-poor districts.

Table A.6: Reduced Form and First Stage Regressions with Additional controls

	Panel A: Reduced Form Regressions		
	Dep. Var: Voted in the 2010 Election		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-.023 (0.015)		-.025 (0.015)*
Treatment: Fine S/.36		-.045 (0.015)***	-.045 (0.016)***
Gender (Male=1)	0.003 (0.015)	0.018 (0.015)	0.016 (0.011)
Age	0.001 (0.0007)*	0.001 (0.0006)**	0.001 (0.0004)**
Yrs. of education	0.001 (0.002)	0.004 (0.003)	0.003 (0.002)
Log(PC Expenditures)	0.006 (0.008)	0.011 (0.014)	0.009 (0.008)
Left	0.003 (0.029)	-.022 (0.035)	-.011 (0.023)
Right	-.019 (0.018)	0.01 (0.017)	-.011 (0.013)
Interested in politics	0.068 (0.039)*	-.016 (0.021)	0.029 (0.022)
Not Interested in politics	0.081 (0.039)**	-.011 (0.023)	0.038 (0.023)*
Name recall Candidates	0.026 (0.036)	0.132 (0.043)***	0.097 (0.028)***
Votes in Poor district			-.020 (0.022)

	Panel B: First Stage Regressions		
	Dep. Var: △ Perceived Fine		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-18.375 (4.948)***		-19.328 (4.919)***
Treatment: Fine S/.36		-29.730 (4.812)***	-29.145 (4.750)***
Gender (Male=1)	-2.118 (5.105)	-3.463 (4.826)	-2.455 (3.470)
Age	0.3 (0.206)	0.438 (0.194)**	0.347 (0.138)**
Yrs. of education	0.174 (0.771)	-.421 (0.687)	-.144 (0.511)
Log(PC Expenditures)	-3.392 (3.508)	-1.310 (3.842)	-1.816 (2.634)
Left	-.190 (10.150)	18.315 (9.085)**	8.549 (6.770)
Right	-12.845 (6.157)**	5.016 (6.133)	-4.561 (4.327)
Interested in politics	4.233 (9.334)	3.379 (9.914)	3.992 (6.740)
Not Interested in politics	6.549 (9.509)	-2.809 (10.209)	1.279 (6.929)
Name recall- Candidates running	7.586 (9.689)	1.844 (9.679)	4.263 (6.621)
Votes in Poor district			-2.491 (6.463)
Village FE	Y	Y	Y
Mean: Voted in the 2010 Election	0.952	0.946	0.949
Mean: △ Perceived Fine	-48.97	-66.03	-57.55
First Stage F-stat	14.75	41.75	28.38
Obs.	820	830	1650

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Panel A presents linear probability models, while in Panel B we use OLS. Regression equations are specified in equations (4) and (5).

Table A.7: IV - Effect of Change in Perceived Fines on Turnout with Additional controls

| | Dep. Var: Voted in the 2010 Election | | |
	Non-Poor	Poor	All
△ Perceived Fine	0.0012	0.0015	0.0015
	(0.0009)	(0.0006)***	(0.0005)***
Gender (Male=1)	0.0056	0.0232	0.0194
	(0.0164)	(0.0161)	(0.012)
Age	0.0007	0.0006	0.0005
	(0.0008)	(0.0007)	(0.0005)
Yrs. of education	0.0011	0.0042	0.0029
	(0.0024)	(0.003)	(0.002)
Log(PC Expenditures)	0.0105	0.0126	0.0114
	(0.01)	(0.015)	(0.0087)
Left	0.0028	-.0494	-.0231
	(0.0316)	(0.0393)	(0.0251)
Right	-.0031	0.0022	-.0042
	(0.0208)	(0.0199)	(0.015)
Interested in politics	0.063	-.0215	0.0235
	(0.0407)	(0.0229)	(0.0246)
Not Interested in politics	0.0732	-.0070	0.0362
	(0.0414)*	(0.0245)	(0.0251)
Name recall-Candidates	0.0166	0.1294	0.0905
	(0.0367)	(0.0438)***	(0.029)***
Votes in Poor district			-.0192
			(0.0219)
Village FE	Y	Y	Y
Mean dep. var.	0.952	0.946	0.949
First Stage F-stat	14.75	41.75	28.38
Obs.	820	830	1650

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. All columns present 2SLS estimations where △ Perceived Fine is instrumented by the two treatment dummies (Treatment: Fine S/.72 and Treatment: Fine S/.36, from Table 3). The regression equation is specified in equation (6).

Table A.8: IV - Effect of Change in Perceived Fines, Alternatives measures of fines

| | Dep. Var: Voted in the 2010 Election | | | | |
	(1)	(2)	(3)	(4)	(5)
△ Perceived Fine	0.0017 (0.0005)***				
P. Fine (Follow-up)		0.0016 (0.0005)***			
Log(1+P. Fine (Follow-up))			0.1426 (0.0416)***		
△ P. Fine/PC Exp.				1.0764 (0.3809)***	
P. Fine (Follow-up)/PC Exp.					1.3742 (0.4191)***
P. Fine (Follow-up)		-.00008 (0.0001)			
Log(1+P. Fine (Baseline))			0.001 (0.0094)		
P. Fine (Baseline)/PC Exp.					-0.3193 (0.0878)***
Gender (Male=1)	0.0184 (0.0126)	0.0112 (0.0115)	0.0111 (0.0118)	0.0143 (0.0132)	0.0054 (0.0125)
Age	0.0007 (0.0005)	0.0013 (0.0005)***	0.0013 (0.0005)***	0.0011 (0.0005)**	0.0009 (0.0005)*
Yrs. of education	0.0039 (0.002)*	0.0042 (0.0019)**	0.0041 (0.0019)**	0.0036 (0.0022)*	0.0043 (0.0021)**
Log(PC Expenditures)	0.0108 (0.0089)	0.0087 (0.0083)	0.0077 (0.0084)	-.0505 (0.0224)**	0.0422 (0.0186)**
Votes in Poor district	-.0038 (0.0219)	0.0054 (0.0207)	0.023 (0.0225)	-.0016 (0.0216)	0.0121 (0.0219)
Village FE	Y	Y	Y	Y	Y
Mean dep. var.	0.952	0.946	0.949		
Obs.	1732	1732	1732	1714	1721
F-Stat	25.6271	68.5704	48.8389	13.4935	27.921

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. All columns present 2SLS estimations where the independent variable of interest is instrumented by the two treatment dummies (Treatment: Fine S/.72 and Treatment: Fine S/.36, from Table 3). Columns (4) and (5) exclude outliers (indepepndent variable is lower than -2 or greater than 2.) The regression equation is specified in equation (6).