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Turnout, political preferences and information: Experimental evidence from Peru

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ABSTRACT

I combine a field experiment with a change in voting laws reducing the fine for abstention to assess the effects of monetary incentives to encourage voter participation. In a real world election, using experimental variation in the perceived reduction of the fine for abstention I estimate that receiving information about a reduction in the fine by 50 (75) percent causes a decrease in turnout of 2.6 (5.3) percentage points. These estimates imply a cost elasticity of voting of −0.22. The reduction in turnout is driven by voters who are in the center of the political spectrum, hold less political information and have lower subjective value of voting. The increase in abstention does not change aggregate preferences for specific policies. Further, involvement in politics, as measured by the decision to acquire political information, is independent of the level of the fine. Additional results indicate that the reduction in the fine reduces the incidence of vote buying and increases the price paid for a vote.

1. Introduction

Electoral institutions and policies to encourage voter participation in elections are widespread around the world. They aim at ensuring voters' preferences are adequately represented in policies enacted by elected governments. Despite increasing interest in bolstering electoral participation, little is known about the effects of the most obvious type of interventions, i.e. those that affect the cost of voting. Understanding whether the cost of voting affects turnout, the magnitude of these effects, and the profile and preferences of voters who are more likely to respond to these policies will allow us to design better policies to boost turnout, and to understand its effects on preference aggregation.

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 generates variation in the cost of abstention in a real world election, allowing me to estimate the effects of being informed about a reduction in the cost of abstention on turnout, and its elasticity with respect to the cost of voting. Further, I characterize and analyze the heterogeneity of the effects and provide evidence on the specific policy preferences of voters who are swayed to participate due to the change in voting costs.

Peru, as other thirty-four countries around the world, has compulsory voting and abstention is penalized with a fine. In 2006, Congress passed a law reducing the fine for abstention (and introduced regional variation in the fine). However, the new law received 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.) Just before the 2010 municipal election, I provided a randomly selected group of voters information about the *new* levels of the fine. My main finding shows that in districts where the fine was reduced in half, the average voter informed about the new cost of abstention 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 among treated voters.

To interpret my reduced form effects I exploit the individual-level, within village, random variation in the change in the perceived fine for abstention to estimate the cost elasticity of voting. Using the treatment status as an instrument for the change in the perceived value of the fine between the baseline and follow-up surveys, I find this elasticity to be −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.) My 2SLS 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

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about its enforcement. Using information from my follow up survey I show that perceived enforcement was not different in the treatment or control groups. I also show that the treatment did not differentially affect the voter's intrinsic value of voting or primed them to acquire political information.

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. These heterogeneous effects are consistent with the predictions of an extension of the models in Degan (2006), Degan and Merlo (2009, 2011), in which voters are uncertain about the candidates' political position, and – 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.

The change in the composition of the electorate does not necessarily imply that the outcome of the election will be affected. Using detailed data on individual's preferences for policies, I show that the average voter who abstains due to the reduction in the fine does not have different preferences for specific policies, compared to the average voter who participate despite the lower fines. This implies that (under assumptions of perfect commitment by politicians), policies implemented by governments elected under high vs. low cost for abstention regime would implement similar policies. These result is consistent with Hoffman et al. (2017), who find that the elimination of compulsory voting in Austrian states did not lead to significant changes in fiscal policies. Further, I show that 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. The 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 8.4 cents per each Nuevo Sol (75 percent), making it more expensive for politicians to influence the outcome of the election.

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 evidence that campaigns aimed at affecting electoral participation and improving representation might generate differential responses. Changes in the electorate do not unequivocally translate into policies, but rather the way interventions affect participation differentially among groups in the population is a key issue to consider when designing policies to increase turnout.

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.[2] Funk (2007) shows that compulsory voting laws, even under low or zero consequences, affect turnout through the expressive value of the law. 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 introduction of postal voting and shows that it increased turnout, and this increase is mainly driven by uninformed and less educated voters.[3] Another commonly used source of exogenous variation 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. Similarly, Godefroy and Henry (2015) analyze the effects of changes in the cost of voting on participation and policy outcomes and uses the incidence of stomach diseases in constituencies as an unexpected shock that affects participation, and this affects municipal spending. 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, relying on monetary rewards for voting offered by the experimenters. For instance, Gerardi et al. (2016) test several implications of costly voting models in the laboratory, while 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, I am able to quantify the magnitude of voters' responses to changes in the cost of abstention. My reduced form effects show that informing voters about larger reductions in the fine for abstention cause larger abstention rates. The external validity of my estimates is strengthen by the fact that I study voters in a real world election, and an imperfectly executed law provides the opportunity to generate individual level exogenous variation in the cost of voting. I Interpret the magnitude of my reduced form effects by exploiting the changes in the perceived fine for abstention 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.

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). 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

[2] 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, 2001; Gerber et al., 2003, 2003, 2008; Arceneaux and Nickerson, 2009; Gerber and Rogers, 2009; Pons et al., 2014; Chong et al., 2017.) 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., 2017; Gerber et al., 2008; Giné and Mansuri, 2017; Fafchamps et al., 2015.) 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., 2016). 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; Lassen, 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).

[3] Funk (2010) uses the same setting and shows that in smaller communities, where social pressure is higher, the introduction of postal voting decreased turnout, due to the social pressure generated by being seen voting. Other important contributions to this literature exploit changes in the cost of voting in the form of the elimination of poll taxes (e.g. Filer et al., 1991) or literacy tests (e.g. Cascio and Washington, 2014) in the U.S.

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., 2017).

Finally, the results of the paper also speak to the literature analyzing vote buying in developing countries (Finan and Schechter, 2012; Vicente, 2014; Vicente and Wantchekon, 2009). A potential unexpected result of government regulation that mandates voting is that it could affect the market for votes. I find that the reduction in the fine for abstention increase the price of each vote, making it more costly for politicians to influence the outcome of the election. My results are consistent with a downward shift in the supply of votes (caused by a reduction in the cost of abstention), and also with the theoretical predictions in Gans-Morse et al. (2014).

In the next section, I provide the institutional background on the Peruvian electoral system and the change in the law that reduced the fine for abstention. Section 3 explains the experimental design and the data used in the empirical analysis, which is presented and discussed in Section 4. Section 5 presents the heterogeneity of the main results and Section 6 analyzes the effects of the cost of abstention on preference aggregation, information acquisition and vote buying. Finally, Section 7 summarizes and discusses the findings.

2. Institutional background

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.[4] In order to get back full rights, a fine has to be paid and once it is done, an official stamp is placed on the ID card. As expected, enforcement of these regulations is imperfect: 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.[5]

Until 2006 the fine for abstention was set at S/.144 (144 Nuevos Soles, ∽US$50), which represented about 26 percent of the minimum official monthly wage. That year, Congress issued a new law, which reduced the fine 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).[6] Using census information on the proportion of the district's population with unsatisfied basic needs, districts were classified into one of three poverty (and fine) categories. The new levels were set as follows: abstainers registered in non-poor districts (N=184) are subject to a fine of S/.72 (∽US$25); those in poor districts (N=793) were fined with S/.36 (∽US$12.50) if they abstain, while in extremely poor municipalities (N=852), the fine was reduced to S/.18 (∽US$6).

Importantly, no major media outlet reported the changes in the fine and no campaigns were conducted to spread the information about the new fine structure.[7] In fact, at the time fieldwork was conducted, most of the population was still uninformed about the *new* fine, as I show in Section 3. The fact that electoral laws changed and very few people were informed about it, presented a unique opportunity to investigate the effects of monetary incentives on voter behavior.

In this paper, I study the municipal elections of 2010, where district mayors (and councilors) and regional presidents were elected in a proportional election with no run-off. Municipal governments are 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 have candidates in most local elections, they compete with independent parties, and thus in most places political competition is issue based as much as ideologically based.

3. Experimental design and the data

3.1. Experimental design and the treatment

The field experiment was designed to generate within village, individual level variation in the perceived cost of abstention. Randomly selected voters from 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 their ID card). This design allows me to compare an objective measure of voting of people who are likely to believe that the fine was still at the previous level (control group) with those whose information set had been updated by the treatment. Further, the randomization was done within village, which allows me to hold constant other observable and unobservable characteristics of the village, including supply side factors, such as political competition, campaigning, candidate characteristics, village poverty level, social pressure, etc.[8]

Half of the households sampled in each village were treated. Right

[4] The official ID card is also used for voting and 99% of the adult population has one and is automatically registered to vote. Civil disenfranchisement implies an effective ban on getting official certificates from the national registry, 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 anti-poverty programs.

[5] Effectively, the milder enforcement implies that the expected fine is lower than the actual one. 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 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 (as I show in the next section).

[6] Districts/municipalities (I use these terms interchangeably) are the lowest level of political administration. Districts are divided in neighborhoods (*barrios*, in urban areas), or villages (*centros poblados*, in rural areas). In this paper I use *village* to refer to neighborhoods or villages.

[7] 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.

[8] To be able to make comparisons within and between poverty categories, I sampled districts on each side of the two poverty category thresholds. On the four larger districts, we sampled about 240 households from randomly selected villages, while in the smaller six districts, about 150 households were sampled. Within a village, we determined the number of households to be interviewed proportionally to the number of households in the village, and enumerators chose them by knocking one of each x doors, where x represents the proportion of sampled households to the total number of households. In each household, the head and his/her partner were surveyed (as long as they were between 18 and 70 years old). Overall, I have complete baseline information for 2,837 individuals, from 1,911 households. Table A.1 and Fig. A.2 provide descriptive statistics on the districts in the sample, as well as their geographical location and poverty category, respectively.

after the baseline interview, members of households assigned to the treatment group were informed by the enumerator about the current level of the fine in the district where she was registered to vote. In order to reinforce the message, the enumerator showed a copy of the official newspaper where the law was published and gave a flier to each respondent with the exact text of the script. The script for the treatment group was as follows:

Dear Sir/Madam,.

On August 2006, Congress passed a law in which the fines for not voting were reduced (Law 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].*To avoid 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). The script for the control group was as follows (see Fig. A.1 in the Appendix for Spanish version of both scripts):

Dear Sir/Madam,

*Remember that voting is mandatory in Peru and not voting is subject to a sanction that implies a fine.*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 knowing that voting was compulsory, and that abstention was penalized with a fine.

3.2. The data

The baseline interview took place between one and four weeks before the municipal election. We asked for information on household characteristics, household composition and expenditures. Also, we asked about basic demographics, political preferences, policy priorities for the district, knowledge about the current electoral process and past voting. One key variable collected in the baseline survey is the voter's prior about the value of the fine for abstention. To do this, we first asked whether the respondent knew if there were any consequences for not voting, and if she responded yes, we followed up asking about the consequences (open question). When the respondent mentioned a fine, we inquired about the amount. I assume that the reported value of the fine is the voter's ex-ante perceived fine.[9] At the end of the interview the enumerator provided the treatment.

The follow-up survey was collected between one and three weeks after the election. In this survey, we asked again questions on political preferences, political information and interest in politics. The main variables in the survey were whether or not each respondent voted in the municipal election and their perceived fine. The latter was collected using the exact same string of questions as in the baseline, and the perceived fine in the follow up is assumed to correspond to the information each voter had at the moment of the election (when they decided whether to vote or not). I measure voting through both an objective measure (sticker in the ID card) and a self-reported variable. Not all respondents showed their ID and thus I combine information from these two sources and run exhaustive robustness checks showing that my results are not sensitive to the choice of the source. Importantly, the probability of showing the ID card is uncorrelated with the treatment status, as I show below.

3.3. Descriptive statistics and the effects of the treatment

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. In the baseline survey, the average respondent reports that the fine for not voting is S/.121.9, which is close to its old level (S/.144). This confirms that the majority of voters in my sample were not informed about the law that modified the value of the fine. Importantly, there are no significant baseline differences the perceived fine between the treatment and control group. Fig. 1 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. The distributions in the left column confirm that, not only there were no significant baseline differences in the perceive fine on average, but also the distributions are similar.

Exposure to the treatment affected the beliefs about the fine for abstention reported in the follow-up survey. Even though the reduction in the perceived fine is significantly larger for voters in the treatment group, there seem to have been within village information spillovers, and thus the distribution of perceived fines in the control group also shifted to the right, as shown in Panel A of Table 1 and the plots in the right panel of Fig. 1. This was particularly the case for voters in extremely poor municipalities, for whom the average change in the perceived fine is not significantly different between treatment and control groups.[10]

Panel B in Table 1 shows that 94.2 percent of the respondents in my sample voted in the October 2010 elections.[11] Treatment assignment led to lower turnout. On average, respondents in the treatment group were 3.1 percentage points less likely to vote. 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, treatment assignment led to a 2.1 percentage points reduction in turnout. Likewise, in poor districts, where the reduction in the fine was larger, treated voters were 5.4 percentage points less likely to vote.

Treated voters in extremely poor districts, where the fine was reduced the most, are only 1 percentage point less likely to participate than those in the control group, and the difference is not statistically significant. In these districts, the treatment did not differentially affect voter's perceptions of the change in the fine, i.e. voters in the control group updated their priors about the fine as much as those in the treatment group, and thus there is no difference in their voting behavior (on average). The lack of a "first stage" for this group does not allow me to do any valid inference from the differential behavior of the treatment and controls in these municipalities, and therefore in all the subsequent analysis I exclude them and refer to the sample of

[9] 4% of respondents did not know what were the consequences of abstention and 1.3% do not mention a fine as one of the consequences. I assume that they perceive that the fine for abstention is zero. In the few cases in which enumerators were not able to asses an exact value of the perceived fine, respondents were asked to place their beliefs in brackets. For these observations, I use the median value of that range specified using data from those who did mention 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, or if I include an interactive term between the variable of interest and a dummy for having provided a range instead of an exact number.

[10] Overall, it is unclear why information about the new levels of the fine spread more rapidly in these localities. Learning about the new levels of the fine is independent of the size of the village and the number of days between the baseline and follow-up surveys is not statistically different between non-poor, poor and extremely poor municipalities (30 days, on average).

[11] There are two reasons why turnout in my sample is higher than the official statistics: (i) 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), (ii) conversations with government officials suggested that the electoral roster is not updated often, thus there are a number of dead people who's names haven't been removed (and of course, they are absent on Election Day).

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	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	-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 changes in the value of the fine established in the 2006 law were: (i) Non-poor districts, S/.72 (from S/.144 to S/.72); (ii) Poor districts, S/.108 (from S/.144 to S/.36); and Extremely Poor districts, S/.126 (from S/.144 to S/.18). The sample comprises all respondents with information on all relevant covariates in the baseline and follow up surveys. For details on the survey questions used to asses the perceived fine, see Subsection 3.2.

voters in poor and non-poor districts as the *Analysis Sample*.[12]

The descriptive statistics for the analysis sample by treatment and control status are reported in Table 2. Overall, we observe marginal differences between the baseline characteristics of these groups, and the test for joint significance of the covariates rejects the null at the 95 percent. On average, 40 percent of the sample is male, they are 38.7 years old, with 10 years of education and spend S/.288.1 (∽US$110) per capita per month.

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 1,733 individuals from 1,166 households. Table A.2 shows the balance of observables between attrited individuals and those who we were able to track. Overall, the sample of attriters is not statistically different from non-attriters along most of the observable characteristics.[13] In the next section, I perform robustness checks showing that attrition is not correlated with treatment status and that any differential attrition does not significantly affected the qualitative or quantitative results in the paper.

4. Empirical strategy and results

The empirical strategy to estimate the effects of being informed about a reduction in the fine on turnout follows directly from the experimental design. First, I exploit the exogenous variation treatment assignment to identify the effects of being exposed to information about lower levels of the fine on turnout, and identify the main result of the paper, the reduced form effects. Then, after documenting that treatment assignment had an effect on the changes in the perceived fine, I use a 2SLS approach to interpret the magnitude of the reduced for coefficients, and identify the effect of the reduction in the cost of abstention on turnout.

4.1. Reduced form results

The reduced form regression identifies the direct effect of informing voters about the new value of the fine on turnout:

$$Vote_{ikj} = \alpha + \beta_1 Treat{*}NonPoor_{ikj} + \beta_2 Treat{*}Poor_{ikj} + \beta_3 Poor_{ij} + \gamma X_{ikj} + \delta_k + \eta_{ikj} \quad (1)$$

$Vote_{ikj}$ is an indicator of whether voter i, living in village/centro poblado k and registered to vote in district j, voted. The treatment status is given by the indicator variables $Treat{*}NonPoor_{ikj}$ and $Treat{*}Poor_{ikj}$, representing whether voter i was assigned to the treatment group in a Non-Poor or Poor municipality, respectively, and therefore was informed about a reduction in the fine to S/.72 or S/.36. The inclusion of a dummy indicating the level of poverty of the district where the voter is registered to vote allows 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 likely to affect voting decisions: gender, age, years of education, and the log per capita expenditures. Finally, δ_k is a fixed effect at the level of the village where the respondent lives. η_{ikj} is the error term, which I cluster at the level of treatment assignment (the household).

It is not straight forward that we should expect changes in the fine to cause lower turnout. Compulsory voting laws have an expressive value beyond any monetary consequences (Funk, 2007). In the

[12] This sample comprises 1,732 voters instead of the 2,350 shown in Table 1. All the qualitative results from the paper go through if I include this group of the population and a full set of tables is available upon request.

[13] One variable that shows systematic imbalances is gender. Men are less likely to be in the follow up survey. Excluding this variable, a joint F-test of the significance of the difference between covariates does not show overall imbalances. In all regressions gender is included among the controls in the regressions to account for this imbalance.

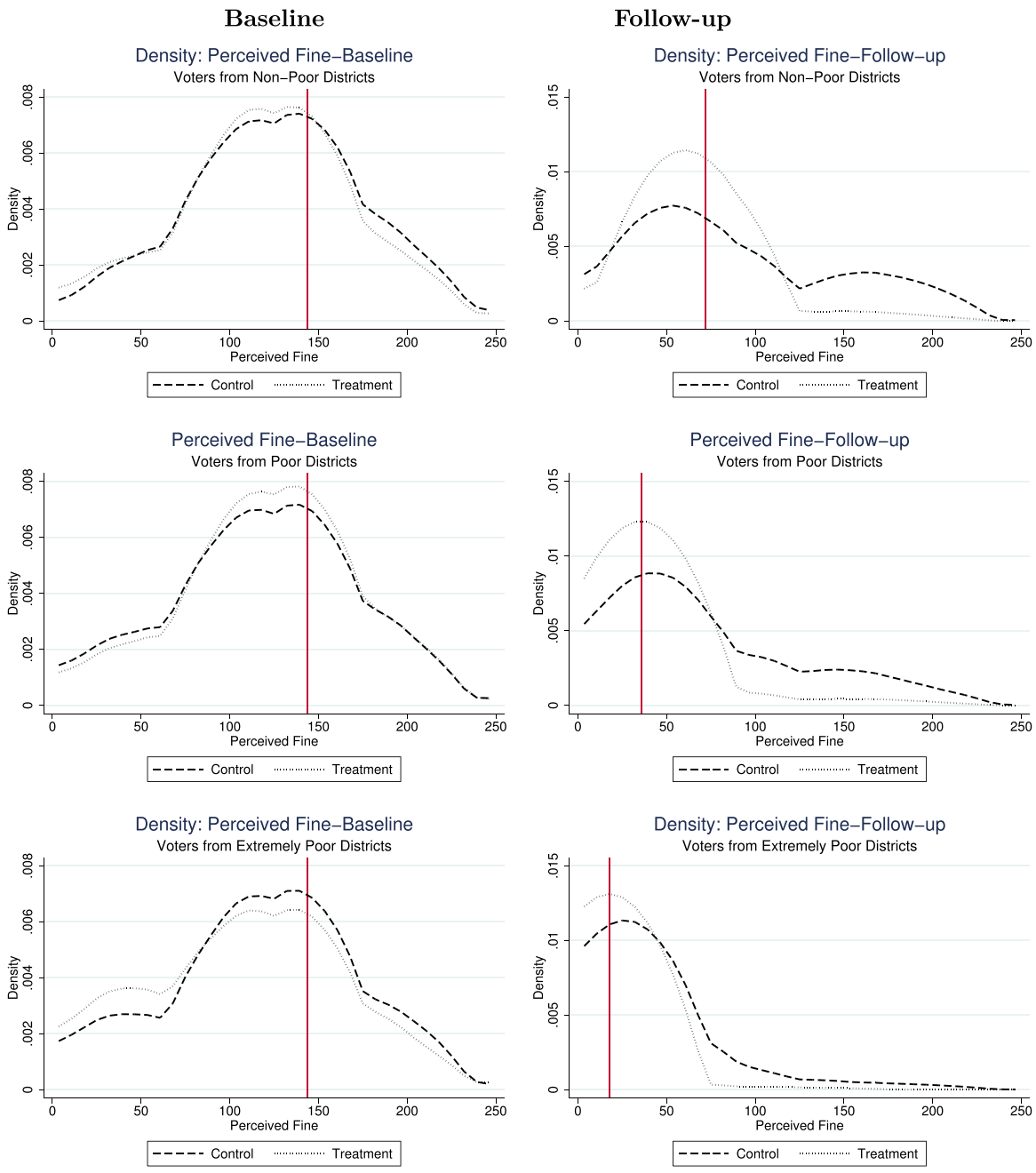


Fig. 1. Perceived fines, by treatment and poverty status.*Notes*: Kernel density estimates of the perceived value of the fine for abstention, as reported in the baseline and follow-up surveys. The data in the figures in the left panels come from the baseline survey and the vertical lines indicate the "old" levels of the fine (S/.144); the information in the right panels comes from the follow-up survey, and the vertical lines denote the value of the "new" fine for each poverty category. For details on the survey questions used to asses the perceived fine, see Subsection 3.2.

Peruvian context, where mandatory voting has been in place for more than 80 years and turnout is consistently high, habit formation (e.g. Gerber et al., 2003, Fujiwara et al. 2014) or social pressure (e.g. Gerber et al., 2008; Funk, 2010) might dominate the effects of the reduction in the fine. Column (1) in Table 3 presents the reduced form estimates of the effects of treatment assignment on turnout (ITT). Being informed about a lower fine reduces the likelihood of voting, and when the information conveys a larger reduction of the fine, the probability of voting is even lower. Treated voters in non-poor municipalities are 2.6 percentage points less likely to vote than the controls in this poverty category. Likewise, voters in poor districts, where the fine was reduced to one fourth of the original amount, showed up at the polls 5.3 percentage points less often than the ones in the control group. Noticeably, these effects are almost exactly the same as the unconditional differences from Panel B of Table 1. These reduced form effects are consistent with the hypothesis that the cost of abstention is an important determinant of turnout.

4.1.1. Robustness

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 (only three out of 21 variables are significantly different, see Table A.2), if (conditional on observables) attrition is correlated with the treatment, it could partially account for the results observed. Column (1) in Panel A of Table 4 shows that, controlling for individual level observables and village fixed effects, respondents assigned to the treatment are not more likely to be absent at the moment of the follow-

Table 2
Balance between treatment and control groups.

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 who are considered in the analysis sample, i.e. voters registered in poor or non-poor districts. The test of joint significance between treatment and control for voters in rejects the null at the 0.95 (χ^2=28.74).

up survey.[14] Despite the insignificant coefficient, its magnitude is not trivial, and thus, to further alleviate concerns about differential attrition, in Panel A of Table 4 I perform a bounding exercise following Lee (2009). This procedure estimates the upper and lower bounds of the effect of the treatment on the main outcome variable assuming the best and worst case scenario about the voting behavior of the attriters, i.e. that all the differential attrition is either composed of voters or of abstainers.[15] Column (2) shows the unconditional effect of the treatment on voting in the full sample, while columns (3) and (4) show the results of the bounding exercise. The estimate for the lower bound is extremely close to the one for the full sample and still significantly different from zero, while the upper bound is about 65 percent larger (and statistically significant) showing that attrition is not likely to be major concern for my main results.

The dependent variable is constructed based on a combination of objective and self-reported measures of voting, which raise concerns about potential biases. First, individuals who show their ID might have different observable or unobservable characteristics that could be correlated with voting.[16] In Panel B of Table 4 I show the results from the main specification 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. The results using the self-reported measure of voting is slightly attenuated, but still large and economically significant. Second, if the treatment differentially affected the probability of showing the ID card, this could introduce bias. Column (1) of Panel C in Table 4, 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. As with the case of attrition, one might worry that differential rates of showing the ID card to the enumerator affect my main results, and thus as an additional robustness check, I compute the Lee (2009) bounds for the (unconditional) reduced form effects using only the information of those who showed their ID cards. The lower bound is 40 percent smaller than the effect for the full sample and is statistically insignificant, however one must take into account that the assumptions used to compute these bounds are quite extreme, especially in a context where turnout is high. One the other hand, the upper bound computed is very close to the effect observed in the full sample. I thus conclude that by using the combination of the objective and self-reported measures of turnout is unlikely to affect my results.

4.2. 2SLS results

Now that we have established that informing voters about a reduction in the fine affects their voting behavior, and moreover, those voters who received information about a larger reduction in the fine voted less often, it is important to interpret the magnitude of voters' responses, thus in this section, I use a 2SLS model to estimate the cost elasticity of voting.

Voters update their beliefs differentially, even within the same treatment conditions. In order to be able to say something about the magnitude of voter's 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:

[14] The unconditional correlation between the treatment and attrition is statistically significant, with slightly more attriters among treated respondents. However, this difference dissapears once I control for observables (and especially the ones that are imbalanced in Table A.2, which are included in the main regressions) showing that attrition should not affect my main estimates.

[15] 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-respondant 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-respondants correspond to non-voters in the control group, so I trim them from the control group. The estimation does not allow to include controls in the regressions and the standard errors shown are bootstrapped.

[16] Table A.3 in the Appendix shows that the observable characteristics of respondants who showed their ID or not are mostly similar, and a test for the equality of all the variables rejects the null hypothesis of equality at the 95% confidence.

Table 3
Main regression results.

	Reduced form	First stage	2SLS	
	Dependent Variable:			
	Voted in the 2010 Election	Perceived fine (follow-up)	Voted in the 2010 Election	Voted in the 2006 Election
Δ Perceived Fine			0.0017	0.00008
			(0.0005)***	(0.0005)
Treatment: Fine S/.72	−.026	−17.420		
	(0.015)	(4.851)***		
Treatment: Fine S/.36	−.053	−28.848		
	(0.016)***	(4.644)***		
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean Dep. Var.	0.944	−58.00	0.944	0.945
First Stage F-stat			25.63	20.84
Obs.	1732	1732	1732	1548

Robust standard errors clustered at the household level in parentheses. Column (1) presents a linear probability model, while in Column (2) I use OLS, and in Column (3) and (4), I present 2SLS regressions where Δ Perceived Fine is instrumented by the two treatment dummies (Treat*NonPoor and Treat*Poor). Regression equations are specified in Eqs. (1), (2) and (3). The sample size in Column (4) 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 controls for gender, age, years of education, log(PC Expenditures), and a dummy for being registered to vote in a poor district. The overidentification test is unable to reject the joint null hypothesis is that the instruments are valid instruments (Sargan test with a p-value=0.7649; Basmann test p-value=0.7673). * Significant at 10%; **Significant at 5%; *** Significant at 1%.

$$\Delta Fine_{ikj} = \alpha + \beta_1 Treat*NonPoor_{ikj} + \beta_2 Treat*Poor_{ikj} + \beta_3 Poor_{ij} + \gamma X_{ikj} + \delta_k + \nu_{ikj} \quad (2)$$

$\Delta Fine_{ikj} = (Fine_2 - Fine_1)_{ikj}$ is the change in the perceived fine between the follow-up and baseline surveys.[17] β_1 and β_2 represent the difference in the average change in the perceived fine between the treatment and control groups, among voters from non-poor and poor municipalities, respectively. The results from the first stage regression are displayed in Column (2) in Table 3. The difference in the change in perceived fine for the treatment and control groups is S/.17.4 in non-poor municipalities, while the treatment effect for voters in poor districts is a reduction in the perceived fine of S/.28.8. Treatment assignment affected the beliefs about the fines in the way we expect them, but the magnitude of the changes between the treatment and control groups is lower than the actual changes in the fines, which is potentially a result of the information spillovers described in Section 3.3. These results provide a strong first stage for 2SLS 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 dummies) on turnout:

$$Vote_{ikj} = \alpha + \beta_1 \Delta Fine_{ikj} + \beta_2 Poor_{ij} + \gamma X_{ikj} + \delta_k + \varepsilon_{ikj} \quad (3)$$

β_1 represents the marginal effect of a change of S/.1 in the fine for abstention on the likelihood of voting. The 2SLS results are presented in column (3) of Table 3. An exogenous *decrease* of S/.1 in the fine for abstention reduces the likelihood of voting by 0.17 percentage points. Using these estimates, I can back out the cost-elasticity of voting. The average voter in the analysis sample reports that the fine was reduced by S/.58 (46.8 percent from the 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 to 84.6 percent and a cost-elasticity of −0.22. Column (4) of Table 3 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. 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, providing support to the assumption that voters in the treatment groups were not already less likely to vote than those in the control group in previous elections.

Extrapolating these results to the whole population (with the obvious caveats), 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 voting is voluntary. 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.[18]

4.2.1. Interpretation of the 2SLS results and the exclusion restriction

The interpretation of my 2SLS estimate as the cost elasticity of the fine relies on the assumption that the information treatment only affected turnout through the change in the perceived fine. One concern is that the information also affected the perceived enforcement probability. 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, voters in the treatment group would have a lower expected value of the fine driven by the both, the lower perceived fine and enforcement. 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 the first six regressions of Table 5, I regress the perceived consequences of not voting on the treatment variables and the controls used in the previous analysis. All coefficients are very close to zero and are statistically insignificant, showing that voters in the treatment group are not less likely to think that the chances that they are denied a particular service are higher than those in the control group.

A potential violation of the exclusion restriction would happen if the treatment differentially affects the salience of the importance of voting. To test whether this is the case, I follow a similar strategy as before, and run my main specifications, but using as the dependent variable proxies for the perceived value of voting, measured in the follow-up survey, eg. the importance of the elections and the electoral campaigns. The results in the last two columns of Table 5 show that the treatment did not differentially affect the voters' value of elections.[19]

[17] The main regressions in the paper use a specification in which I focus on the effects of $\Delta Fine$ on voting, rather than the effect of $Fine_2$. The reason behind this choice is because there seems to be anchoring in the way individuals update their beliefs about the fine, and this is particularly the case among those in the control group, for whom the correlation between $Fine_1$and $Fine_2$ is 0.14 (statistically significant). This leads us to believe that accounting for the differential updating in the first stage is important. However, all the results in the paper remain quantitatively and qualitatively unchanged if I did not include $Fine_1$ in the regression equation. A full set of results is available upon request.

[18] The reduced form, first stage and 2SLS results remain unchanged if we include in the estimation voters from Extremely Poor municipalities, as shown in Panel A of Table A.4. Likewise, most of the qualitative results of the paper go though if I include these observations. These results are available upon request.

[19] Another threat along the same lines is that the treatment induced lower information acquisition and thus affected turnout. In Table 9, Section 6, I show evidence that voters in the treatment group did not acquire less political information than those in the control group.

Table 4
Robustness checks: attrition and measurement of voting.

	Panel A: Attrition				
	Dep. Var:		Lee (2009) Bounds Dep. Var: Voted in the 2010 Election		
	Attrited	Full Sample	Lower Bound		Upper Bound
Treatment	0.043	−0.037	−0.035		−0.062
	(0.0282)	(0.0113)***	(0.0126)***		(0.0204)***
Controls	Y	N	N		N
Village FE	Y	N	N		N
Mean dep. var.	0.218	0.944	0.944		0.944
Obs.	2214	1732	2214		2214
	Panel B: 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)***
Controls	Y	Y	Y	Y	Y
Village FE	Y	Y	Y	Y	Y
Mean dep. var.	0.944	0.944	0.971	0.971	0.971
F-statistic	25.627	25.248	15.573	15.148	15.148
Obs.	1732	1729	1130	1127	1127
	Panel C: Measurement of Voting				
			Lee (2009) Bounds		
	Dep. Var:		Dep. Var: Voted in the 2010 Election (Sticker)		
	Not Showed ID	Full Sample	Lower Bound		Upper Bound
Treatment	−0.0183	−0.0352	−0.0192		−0.0365
	(0.0243)	(0.0101)***	(0.0179)		(0.0119)***
Controls	Y	N	N		N
Village FE	Y	N	N		N
Mean dep. var.	0.347	0.971	0.971		0.971
Obs.	1732	1130	1732		1732

Robust standard errors clustered at the household level in parentheses. In Column (1) of Panel A, I use and OLS regression. Columns (2)–(4) I run the reduced for regression without controls or fixed effects, 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 Eq. (3), including controls and village fixed effects. 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) and (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). In Panel C, Column (1) uses an OLS model with controls and fixed effects. Columns (2)–(4) are analogous to those in Panel A. * Significant at 10%; ** Significant at 5%; *** Significant at 1%.

The spread of information about the new level of the fine between the treatment and the control group represent a threat to the interpretation of my estimates as long as this information was acquired *before* the election (otherwise, this information wouldn't have affected voting decisions). Table 6 explores the correlation between the number of days between the baseline and follow-up survey and Election Day, 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 and the election. This means that most voters in the control group had not 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 6 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.

The results in Column (3) of Table 3 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. This interpretation relies on assumptions on how people update their information sets under different treatment conditions, i.e. I assume that the treatment leads everyone to update their beliefs about the fine in the same direction. If the monotonicity assumption does not hold, the interpretation of the 2SLS results as LATE would be threatened, in which case, the 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, in our case, the monotonicity 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 would not generate a significant biases to my main estimates.

One group in the analysis sample where we *might* find defiers is among voters whose initial beliefs about the fine were below the new level. 11 percent of voters in the analysis sample fall in that category. In terms of the potential outcomes framework, this 11 percent (besides defiers) can also include *never takers*. Panel B in Table A.4 in the Appendix presents the main regressions of the paper excluding this group of voters. The point estimates in the reduced form, first stage, and 2SLS regressions are remarkably similar to those in Tables 3, providing evidence that either there are few defiers in the sample or that their presence would not bias my estimates.

Table 5
Effect of the treatment on perceived consequences of abstention.

	Dep. Var: Consequence of Abstaining:			
	Fine	Can't use banks	Can't use pubic inst (municipality, police, etc.)	Can't use notariat
Treatment: Fine S/.72	-.0056	0.0188	0.0108	0.0005
	(0.0051)	(0.0271)	(0.0209)	(0.0168)
Treatment: Fine S/.36	-.0076	-.0023	-.0013	-.0107
	(0.0051)	(0.0218)	(0.0179)	(0.0154)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean dep. var.	0.994	0.155	0.096	0.062
Obs.	1712	1712	1712	1712

	Dep. Var: Consequence of Abstaining:			
	Can't use registry (contracts)	Other: (travel, jail, jobs, etc.)	V. Interested in Campaign (Follow-up)	V. Interested in Results (Follow-up)
Treatment: Fine S/.72	0.0178	0.0252	-.0054	0.0399
	(0.0152)	(0.0329)	(0.0242)	(0.0344)
Treatment: Fine S/.36	-.0013	0.0306	-.0109	-.0133
	(0.0131)	(0.0259)	(0.0271)	(0.0324)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean dep. var.	0.047	0.287	0 .153	0.357
Obs.	1712	1712	1725	1729

*significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. All 2SLS models 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: $Consequence_{ikj} = \alpha + \beta_1 Treat*NonPoor_{ikj} + \beta_2 Treat*Poor_{ikj} + \beta_3 Poor_{ij} + \gamma X_{ij} + \delta_k + \eta_{ikj}$.

Finally, one might be worried that the choice of the main independent variable could affect the results. Table A.7 in the Appendix 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 quantitative and qualitative results are robust to the choice of the independent variable.

5. Political preferences, value of voting, and information

The reduced form and 2SLS results show that exposure to the treatment had impacts on turnout, and that the magnitude of the change in the fine significantly affected participation. However, to understand the effects of the cost of abstention on preference aggregation and its potential impacts on public policies, we need to look beyond the effects on the level of turnout and dig further into what happens with the *composition of the electorate*. The identity of the marginal voter and her preferences determine whether changes in turnout will translate into substantial effects on who gets elected and what policies get implemented.

Theoretical models in the literature (see e.g., Martinelli, 2005; Merlo, 2006 for critical reviews) use a few common dimensions of heterogeneity among voters: (i) ideological positions, (ii) subjective value of voting and (iii) political information. In this section, I analyze the heterogeneous effects of the changes in the cost of abstention to qualify the identity of marginal voters who responded to the policy. Using the 2SLS framework from the previous section, I introduce in my main regression interactive terms between the (instrumented) change in the perceived fines and variables that proxy for the relevant characteristics being evaluated (while controlling for the others).

5.1. Ideological positions

Models in which voters derive utility from expressing their preferences usually assume that those with more intense preferences (i.e. extremists) will be more likely to vote than centrists. However, if voters have uncertainty about the candidate's position and can make voting mistakes (see eg. Degan, 2006; Degan and Merlo, 2011), or in models based on strategic extremism (Glaeser et al., 2005), centrists are more likely to participate. In the presence of costly voting, these two broad families of models provide different predictions on the identity of the marginal voter who will be affected by changes in the cost of abstention.

In local elections it is not straightforward to understand the role played by ideological preferences. While in the Peruvian case, national parties with well define ideologies compete in these elections, a good part of the candidates are independent, and base their campaigns on specific proposals rather than in the standard ideological scale, but in

Table 6
Spread of information and days between surveys.

	Dep. Var.: Δ perceived fine			
	Treatment	Control	Treatment	Control
Num. days: Baseline-Follow-up	−.0719	−.7557		
	(0.2003)	(0.242)***		
Num. days: Baseline-Election			−.0841	−.0916***
			(0.2243)	(0.2728)
Num. days: Election-Follow-up			−.0336	−2.6682
			(0.4156)	(0.4573)***
Obs.	862	868	862	868

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

general we see some regularities with, e.g. left leaning candidates proposing public works while right leaning candidates propose to help promote small entrepreneurs. To empirically approximate ideological preferences, I use two distinct measures that are intended to capture these dimensions. First, I use standard self-reports in the ideological scale, ranging from left (1) to right (5) and take the categories in the middle (2, 3 and 4) to represent the political center. As can be seen in Table 2, there is a concentration of voters in the center. Alternatively, I create a second measure that uses policy preferences to capture a broader range of ideological distributions. In the baseline survey, I asked voters to name (in order) the first five policies that they would implement if they were elected mayor of the district. This was an open question and 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 of the first PC. The center is defined by those in quintiles 2, 3 and 4, while the first and fifth quintiles define the ideological extremes (the coefficients for each policy item loading into the principal component analysis are listed in Table A.5.) The Policy Extreme 1 is related to preferences for public goods, such as health and education infrastructure, roads, etc. On the other hand, Policy Extreme 2 is associated with club or targeted goods, such as youth labor training, security, promotion of private investment, etc. These two variables are meant to represent different dimensions in which voters preferences can be represented.

Panel A in Table 7 shows the results from estimating Eq. (3), including interactions between the variables representing voter's ideological position and the perceived change in the fine, instrumented by the treatment status and the relevant interactions (I don't include $\Delta Fine$ in the regression). 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 is 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 7 show that people in the political extremes are less likely to respond to a change in monetary incentives to vote.

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.

5.2. Interest in politics/subjective value of voting

Models of expressive voting suggest that voters with a higher subjective value of voting need lower incentives to attend to the polls. 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 from the baseline survey to proxy for it. 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 (see Table 2). 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).

In Panel B of Table 7 I run a similar regression as in Subsection 5.1. The results show that voters who are more interested in politics attend to the polls regardless of the change in the perceived fine, and the magnitude and significance of the effect decreases with the interest in politics. A similar pattern is observed when using the alternative proxies for the subjective value of voting.

Using the results from Panel B in Table 7 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 for this group 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 among these voters, 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.

5.3. Political information

Are informed voters more or less likely to respond to changes in the cost of abstention? Both common and private values models predict that informed voters are more likely to participate in elections (see, eg. Feddersen, 2004; Degan and Merlo, 2011, among others), hence we should expect uninformed voters to be more likely to abstain upon a reduction in the fine. In this subsection, I test this empirically by interacting different measures of political information collected at baseline with the change in the perceived fine.

Political knowledge and information about politics are measured in several ways. In the baseline survey, I included open ended questions asking respondents to name all candidates running for the mayor's seat in the municipality where they are registered to vote (and their parties). 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 they are running for. As an alternative measure of political information, I asked 17 questions about knowledge of the political structure of the country, electoral institutions and rules.[20] The average respondent got 9.3 questions right (55 percent). 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 at baseline (—perceived - actual fine—) and the index of knowledge of candidates is 0.0005.

Panel C in Table 7 shows the results of running Eq. (3) including the interactions between the perceived change in the fine and our measures of political information. In all four columns, the interactive terms are 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, 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.

Despite the fact that the independent results of the heterogeneity analysis are consistent with several theoretical models in the literature, few predict at the same time that voters who are more likely to respond

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

Table 7
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			0.0023	
			(0.0021)	
△ Fine*Interested in pol. campaign			0.0008	
			(0.0005)	
△ Fine*Not interested in pol. campaign			0.0029	
			(0.0011)**	
Obs.	1650	1654	1651	
PANEL C: Political Information				
△ Perceived Fine	0.0024	0.0021	0.0024	0.006
	(0.0008)***	(0.0007)***	(0.0007)***	(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

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.
* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

to changes in the cost of voting are those that (i) are in the political center, (ii) have low subjective value of voting, and (iii) are uninformed. One such model is an extension of the one in Merlo (2006) and Degan and Merlo (2011), which I present in Appendix B. In this model, voters know their political preferences, but are imperfectly informed about the candidate's position. They derive utility from fulfilling their civic duty, from voting for the candidate that is closest to their political preferences and from money. Information is exogenous in the model and the fact that voters are imperfectly informed about the candidates makes them prone to make a *voting mistake*. In this set up, marginal voters, i.e. those who are more likely to respond to a decrease in the cost of abstention will be the centrists, the ones with a lower subjective value of voting and the uninformed.

6. Preference aggregation, information acquisition and vote buying

While the conceptual framework in Appendix B helps us think through the heterogeneity results, the model is silent about the effect of changes in the composition of the electorate on the aggregation of preferences and electoral outcomes. Further, an extension of the model in the appendix can include endogenous information acquisition, as in Degan (2013), and this test is also presented below.

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 this led 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 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 directly test the specific predictions of these models, I can provide 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 if 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 commit-

Table 8
Effects by policy preferences.

	Dep. Var.: Voted in the 2010 Election	
	Coeff. on Δ Perceived Fine	Coeff. on Δ Perceived Fine*Policy
Policy		
(1) Health	0.002	−0.0005
	(0.0008)**	(0.001)
(2) Education	0.001	0.0012
	(0.0006)*	(0.0011)
(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	−0.001
	(0.0006)***	(0.0011)
(10) Social/work programs	0.0018	−0.0004
	(0.0006)***	(0.0011)

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.

* Significant at 10%.
** Significant at 5%.
*** Significant at 1%.

ment by politicians). To implement this test, I use the policy preference questions, aggregating them into 10 categories that represent particular policy issues and analyze whether voters with specific policy preferences are more or less likely to respond to changes in the fine. I do this by running my baseline regression in Eq. (3), and interact the (instrumented) change in the perceived fine with a dummy for whether each respondent has a preference of each groups of policies.

The results from this analysis are presented in Table 8. The interaction terms between policy preferences and changes 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; Naidu, 2012.) 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. (2017), who show that the elimination of compulsory voting in Austrian states does not lead to a significant change in the composition or levels of expenditures.

Overall, these results suggest that the average voter who abstains 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 incentivizes people to be more involved in the political process, for example by acquiring information. The underlying model for these claims is one similar to the one Appendix B, but it endogenizes information acquisition (Martinelli, 2005; Degan, 2006, 2013; 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 9 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, 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. These results must be taken with a grain of salt, since even though around the elections voters are more likely to get informed about the candidates and the political process overall, the average time between surveys was short (30 days). Further, 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 or turnout buying (Vicente, 2014; Vicente and Wantchekon, 2009; Finan and Schechter, 2012; Stokes, 2009; Stokes et al., 2013; among others.) This specific type of clientelism generates distortions in the aggregations of preferences, inducing voters to participate or to vote for someone they wouldn't have otherwise by transferring resources from candidates to voters. In my sample, 31 percent of respondents reported having been offered and accepted cash or a gift from a political operator. In electoral systems with compulsory voting, voters who go to the polls because of the mandate are more likely to

Table 9
Effects of fines on information acquisition and vote buying.

	Panel A: Effect of Fines on Information Acquisition			
	Dep. Var.:			
	Δ Candidate Recall	Δ Party Recall	Δ Cand. +Party Recall	Δ Pol. Info Score
Δ Perceived Fine	−.0003	−.0006	−.0004	−.00002
	(0.0005)	(0.0006)	(0.0005)	(0.0004)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Obs.	1732	1732	1732	1732
F-Stat	68.57	68.57	68.57	68.57
	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
Δ Perceived Fine	−.0002	−.0780	0.0105	−.0692
	(0.0009)	(0.0388)	(0.0176)	(0.0394)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean dep. var.	0.310	6.48	3.22	10.08
Obs.	1732	537	537	537
F-statistic	25.62	11.40	11.40	11.40

Robust standard errors clustered at the household level in parentheses. In Panel A, the regression equation is: $\Delta Info_{ij} = \alpha + \beta_1 \Delta Fine_{ij} + \beta_2 Poor_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$, where $\Delta Info_{ij}$ represents the change in the political information between the baseline and follow-up surveys, and $\Delta Fine_{ij}$ is instrumented using the treatment dummies. In Panel B, the regression equation is: $Y_{ikj} = \alpha + \beta_1 \Delta Fine_{ikj} + \beta_2 Poor_{ikj} + \gamma X_{ikj} + \delta_k + \epsilon_{ikj}$. 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. * Significant at 10%; **Significant at 5%; *** Significant at 1%.

accept money for their votes (Gans-Morse et al. 2013.) 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 the fine 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 e.g. in a mass rally.).

Panel B in Table 9 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/.0.08. The average voter, who perceived that the fines were reduced by S/.58, would have to be paid 75 percent more than before 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, e.g. in a massive rally. 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 indirectly. 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, a result that is consistent with the theoretical framework in Gans-Morse et al. (2013).

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 citizens 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.

The reduced form results show that being informed about a larger reduction in the fine for abstention leads to lower turnout. This results contribute to the literature on the effects of monetary incentives to vote. I interpret the magnitude of these results using a 2SLS model, and show that reducing 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 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 composition of 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 evidence that a decrease in the fine for not voting decreases the externalities on related markets. 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. 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 could lead to a welfare loss to 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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Appendix A. Supplementary data

Supplementary data associated with this article can be found in the online version at http://dx.doi.org/10.1016/j.jdeveco.2017.02.005.

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