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How Effective Are Monetary Incentives to Vote? Evidence from a Nationwide Policy†

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We study voters' response to marginal changes to the fine for electoral abstention in Peru, leveraging variation from a nationwide reform. A smaller fine has a robust, negative effect on voter turnout, partly through irregular changes in voter registration. However, representation is largely unaffected, as most of the lost votes are blank or invalid. We also show that the effect of an exemption from compulsory voting is substantially larger than that of a full fine reduction, suggesting that nonmonetary incentives are the main drivers behind the effectiveness of compulsory voting. (*JEL* D72, K16, O17)

The effectiveness of monetary incentives has attracted the attention of economists studying a wide array of topics (Gneezy, Meier, and Rey-Biel 2011). But despite the fact that multiple countries around the world have mandatory voting enforced through monetary sanctions, little is known about voters' response to marginal changes to the size of these incentives, especially when applied at a large scale.[1] Even though a larger abstention fine mechanically increases the cost of not voting, a stronger extrinsic incentive may crowd out the intrinsic motivation to vote provided by social image concerns or a sense of civic duty (e.g., Bénabou and Tirole 2003, 2006). This makes the magnitude of the net turnout effect an open empirical question. Whether any resulting change in electoral participation affects election outcomes is also not clear, given that those induced to vote by a marginally larger

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[1] Compulsory voting laws exist in almost 30 countries, but the mandate to vote is only enforced in Argentina, Australia, Belgium, Brazil, Ecuador, Luxembourg, Nauru, Peru, Singapore, and Uruguay (IDEA 2018). The voting-age population in these countries is around 220 million, comparable to that in the United States.

fine are likely to be particularly uninformed or uninterested in the electoral process (Feddersen and Pesendorfer 1996).

A better understanding of the relationship between the value of the abstention fine, electoral participation, and election outcomes can help inform the debate around the desirability of compulsory voting. This debate is far from settled. The introduction of a mandate to vote has been endorsed by political theorists (e.g., Lijphart 1997, Chapman 2019) and by prominent public figures like former US president Barack Obama (Somin 2015). Learning about the role of monetary incentives in the functioning of compulsory voting is particularly important given the potential trade-off between the greater effectiveness of a larger fine and its also greater burden on those who are sanctioned.

In this paper, we study voters' response to marginal changes in monetary incentives to vote. We exploit a nationwide reform to the value of the abstention fine in Peru, a country with more than 20 million voters and compulsory voting since 1933. We provide three main findings. First, a lower fine has a robust negative effect on voter turnout. This effect is partly driven by irregular changes in registration, as spatial variation in the value of the fine causes young voters to increasingly register in districts with lower fines. Second, the reduction in turnout spurred by a lower fine appears to have little impact on representation, since it is largely matched by reductions in the number of blank or invalid votes. Third, evidence from a second natural experiment that exploits an age-based exemption from compulsory voting suggests that monetary incentives are not the main driver behind the effectiveness of compulsory voting.

Until 2006, the value of the abstention fine was homogeneous throughout Peru. A reform that year classified districts into three categories (high, medium, and low fine) and differentially reduced the value of the abstention fine. Our main analysis employs a difference-in-difference design to study the impact of the reform on voter turnout and electoral outcomes. We use administrative data covering 4 national election cycles over 16 years. As they are based on district-level variation, our estimates capture potential spillover effects across individuals (e.g., Nickerson 2008), which would not be possible with variation at the individual level. We contrast our findings with previous experimental work to illustrate some important limitations of small-scale field experiments in political economy.

We show that a smaller fine has a robust negative effect on voter turnout, with an estimated elasticity of 0.03. Equivalently, a 10 Peruvian sol (S/) fine decrease (approximately US$7 at the PPP-adjusted exchange rate) leads to a 0.5 percentage point (pp) drop in turnout. The turnout response varies depending on the type of election (general versus runoff elections) and increases over time, suggesting voter adaptation to new institutional rules. It is also larger in districts with a larger share of voters below the poverty line, for whom the burden of the fine is greater. These heterogeneous effects highlight the potential for context dependence in highly localized or short-lived field experiments studying voter mobilization.

We can decompose the turnout effect into the separate channels of voter registration and the propensity to vote. In Peru, citizens are automatically registered to vote in their district of residence, as recorded in their national identification card (DNI). However, the spatial variation in the value of the fine induced by the reform creates an incentive for low-turnout voters to fraudulently report an address in a district with a lower fine. We find indeed that the number of registered voters increases disproportionately in low-fine districts after the reform, and we estimate a registration elasticity of −0.05. This effect is only present for young adults and is mostly driven by first-time voters with ages 18–20, who can manipulate their reported address at very low cost when they initially apply for a DNI. This type of unintended consequence is also hard to capture in small field experiments. A bounding exercise reveals that the changes in registration explain as much as 57 percent of the overall effect of the fine on turnout, with the rest corresponding to actual changes in the propensity to vote.

A comparison of our estimated elasticity with that reported by León (2017), based on a field experiment providing information on the modified value of the abstention fine in our same setting, reveals substantial "voltage drop" in the effect of the large-scale policy (Al-Ubaydli et al. 2017). We hypothesize that informational frictions surrounding the modified policy incentives are an important determinant of the reduced effect. Using data from Google searches, we show that the relative frequency with which people search for information about the abstention fine steadily increases after the reform. This suggests that people are imperfectly informed about the fine and slowly increase their demand for information following the change in regulation. It underscores a significant limitation to the use of experimental results in political economy as a guide for policy scale-up, namely that informational frictions hinder adaptation to large-scale institutional changes.

In a setting with few barriers to electoral participation, abstention is likely driven by uninformed or uninterested voters (Feddersen and Pesendorfer 1996). Hence, a higher abstention fine should draw voters who are more likely to spoil their vote. We find that a S/10 lower fine leads to a 0.37 pp decrease in the share of spoiled (i.e., blank or invalid) votes in the presidential first round, equivalent to 86 percent of the estimated effect on turnout for this type of election (0.43 pp). While these votes may also correspond to mistakes by the less educated (Fujiwara 2015) or to a form of political protest (Ujhelyi, Chatterjee, and Szabó 2021), what seems certain is that the induced change in electoral participation has a very small impact on representation. Additional evidence on political attitudes and behaviors from a large household survey further shows that a larger fine leads to reduced interest in politics.

The pecuniary incentive provided by the abstention fine is one of several incentives to vote provided by compulsory voting. These include the restriction on government services faced by nonvoters (i.e., nonpecuniary cost) and the expressive function of the law as a signaling device for socially desirable behavior (Funk 2007). It is difficult to gauge the magnitude of the effect of marginal fine changes on voter turnout without knowing the aggregate effect of compulsory voting. To answer this question, we use detailed data at the "voting booth" level and leverage idiosyncratic variation in the age of voters across voting booths in 2016. We exploit the exemption from the mandate to vote for citizens with ages of 70 or more and compare turnout rates in voting booths with varying shares of voters around the threshold, while flexibly controlling for the age structure of all other voters. This comparison takes place within the same district or even the same polling station. We find that the exemption from compulsory voting leads to a decrease in voter turnout of almost 10 pp at age

70, 20 pp by age 72, and 40 pp by age 75. Using individual-level data on turnout in Chile in 2017 (a neighboring country without compulsory voting), we show that the natural decline in electoral participation at these ages is substantially smaller.

A back-of-the-envelope calculation based on our estimates of the elasticity and the aggregate effect of compulsory voting yields that a 100 percent fine reduction leads to a drop in turnout only 18 percent as large as the one caused by the exemption from compulsory voting. This indicates that the nonmonetary incentives are the main drivers behind the effectiveness of compulsory voting. The conclusion does not fundamentally change if we adjust for the probability of enforcement, plausible growth of the elasticity in the future, or the specific age group affected by the compulsory voting exemption. The ensuing policy implication is that compulsory voting with moderate fines can substantially reduce the burden on nonvoters without incurring large losses in effectiveness.

This paper contributes to the vast literature studying voter turnout.[2] Several papers have studied the effects of monetary incentives but only through field experiments (Loewen, Milner, and Hicks 2008; Panagopoulos 2012; León 2017; Shineman 2018). Our first contribution is to estimate the effectiveness of marginal monetary incentives to vote "in the wild."[3] More broadly, we connect the literature on extrinsic incentives to vote with a separate strand studying intrinsic incentives, such as voters' sense of civic duty (Gerber, Green, and Larimer 2008), habit formation (Coppock and Green 2016; Fujiwara, Meng, and Vogl 2016), and social image concerns (Funk 2010, Dellavigna et al. 2017). Our findings suggest that nonmonetary incentives significantly outweigh pecuniary costs (in the form of fine reductions) in the calculus of voting.

Another large body of work has studied a wide array of voter mobilization initiatives (Gerber and Green 2017), but most of these interventions have only been tested through small-scale field experiments. Our paper links this literature with a separate body of research analyzing the usefulness of experimental studies for policy scale-up (Deaton 2010; Al-Ubaydli et al. 2017; Banerjee et al. 2017; Muralidharan and Niehaus 2017; Al-Ubaydli, List, and Suskind 2021; Vivalt 2020). In particular, the heterogeneous effects we uncover illustrate the potential for context dependence in small experiments. We also shed light on informational frictions as a source of "voltage drop" in the response to large-scale institutional changes.[4]

Our paper also complements the empirical literature on compulsory voting (Funk 2007; Fowler 2013; Jaitman 2013; Cepaluni and Hidalgo 2016; Hoffman, León, and Lombardi 2017; Bechtel, Hangartner, and Schmid 2018). Previous research has largely focused on the effects of the introduction or elimination of a mandate to vote (i.e., extensive margin). Some of these studies show that compulsory voting leads to higher turnout even with a very low fine (e.g., Funk 2007) but are uninformative

[2] Classic treatments include Downs (1957) and Riker and Ordeshook (1968). For more recent overviews, see Blais (2000) and Feddersen (2004).

[3] Panagopoulos (2008) shows that countries with stricter penalties or enforcement have higher turnout. Concurrent work by Carpio et al. (2019) exploits our same reform to study the effect of voter turnout on the party affiliation of candidates in municipal elections.

[4] Previous work on scale-up has mostly worried about challenges in service delivery (e.g., development programs) (Davis et al. 2017, Bold et al. 2018). Research in behavioral public finance also highlights informational frictions (e.g., Duflo et al. 2006) but has not examined their relevance for policy scale-up.

about what would happen were the fine to change. We add to this literature by showing that even though the value of the fine matters, this monetary incentive can only explain a small share of the aggregate effect of compulsory voting.

Lastly, our findings also speak to the literature on electoral participation and representation. There is ample empirical evidence showing that changes to the composition of the electorate affect electoral outcomes and downstream policies (Miller 2008, Cascio and Washington 2013, Fujiwara 2015). However, most previous studies focus on the removal of substantial barriers to effective participation. We add to this literature by showing that marginal increases in participation have a negligible impact on representation in an environment lacking such barriers and with high turnout at baseline. In this regard, our results lend empirical support to models of rational abstention (Feddersen and Pesendorfer 1996).

I. Institutional Background

General elections in Peru, encompassing the first round of the presidential election and multidistrict legislative elections, are held concurrently every five years. In the legislative election, voters in each of the 25 regions of the country elect their representatives to the unicameral congress using a system of proportional representation.[5] In the presidential election, a candidate must obtain at least 50 percent of the votes nationwide to win in the first round (which never happens during our sample period.) As a result, a runoff election between the two leading candidates takes place approximately two months later.[6] Voter turnout has been traditionally high and remained above 80 percent throughout the sample period (see online Appendix Figure A1). However, turnout has been declining since 2006, which coincides with the reform reducing the monetary incentive to vote that we study.

All citizens must obtain a national identification card, Documento Nacional de Identidad (DNI), when they turn 18. The DNI includes the person's home address and must be renewed every 8 years (up to the age of 70). The DNI also acts as the electoral document, and the address on file is used to determine the district where the person is required to vote (registration is automatic).[7]

Voting is compulsory for citizens between the ages of 18 and 69 (both inclusive) since 1933. Voting is done in person at predetermined polling stations, and voters are provided with a sticker on their DNI as proof of participation. This sticker is the main mechanism for enforcement, backed by an electronic database. Those who abstain from voting and do not meet the age requirement for exemption face restricted access to government and financial services until they pay a fine or provide

[5] The regions are the highest-level subnational division and include 23 departments and 2 special provinces that share the same status. Regions are further divided in 198 provinces and 1,854 districts.

[6] Voters also elect representatives at the levels of the district, province, and region every four years. If happening on the same year, subnational elections are not held on the same day as the national elections.

[7] In 2017, 99.3 percent of the population had a DNI (INEI 2018). Proof of address (e.g., a utility bill) is formally required when the DNI is first issued and when it is renewed, but enforcement of this requirement varies. For example, young adults often live with their parents or other relatives and may not have any valid documents to their name. Poorer people may also struggle to meet this requirement.

a valid excuse.[8] Other countries with compulsory voting impose similar restrictions (Jaitman 2013, Cepaluni and Hidalgo 2016). Fines accumulate, but failure to settle an outstanding fine does not prevent someone from voting again or accessing social protection programs. People can pay the fine at any of the around 600 branches of the National Bank (Banco de la Nación, BN) throughout the country. Alternatively, they can submit an excuse and supporting documents to the JNE (Jurado Nacional de Elecciones) after paying a processing fee of about S/21 (US$6.4).[9] All restrictions are lifted once the fine has been settled (i.e., paid or excused).

Until 2006, the abstention fine was the same in all districts, set at 4 percent of an official reference unit known as UIT.[10] At the start of 2006, the value of the fine was S/136 (roughly US$90 PPP).[11] Shortly after the national elections held on that year, Congress approved a law that reformed the abstention fine. The law classified districts into three categories based on their level of poverty: nonpoor, poor, extreme poor. All districts saw a reduction to the value of the fine, but those in the latter categories enjoyed larger falls. Henceforth, we refer to these districts as high-, medium-, and low-fine. In high-fine districts, the fine was cut in half to 2 percent of the UIT, while in districts classified as medium- and low-fine, it was set at 1 percent and 0.5 percent, respectively. These amounts roughly corresponded to US$45, US$23, and US$11.

This reform followed preliminary discussions in which the elimination of compulsory voting was considered. The resulting regulatory change was a compromise between the desire to preserve the high levels of electoral participation induced by compulsory voting and the concern about the regressive nature of the homogeneous fine in place at the time. The reform was presented by a conservative party (Unidad Nacional) but gained 95 percent of roll call votes, indicating widespread support. Approval of the law was barely covered in the press, and voters remained mostly uninformed about it for several years (León 2017).

The district classification was delegated to the National Statistical Office (Instituto Nacional de Estadística e Informática, INEI), but the criteria used for the initial assignment released in 2006 by the National Electoral Jury (Jurado Nacional de Elecciones, JNE) remain unclear (Resolución 4222-2006-JNE). However, the only elections to be held under this classification were the subnational elections of November 2006. Shortly before the 2010 subnational elections, the JNE released a new district classification, which remains in place until the present day (Resolución 2530-2010-JNE). In the new classification, districts were assigned to the fine category corresponding to the largest share of their population, according to a poverty map based on the 2007 population census. Fifty-two percent of districts were

[8] Restricted services include registering a birth or marriage, doing any transaction at public or private banks, getting official documents from the registrar, accepting a job in the public sector, taking part in any judicial or administrative process, signing a contract, or obtaining a passport or a driver's license, among others. Enforcement of these restrictions varies by service. Customers rarely face restrictions for small transactions in private banks, but this is not the case for large transactions or access to government services.

[9] Valid reasons for an excuse include being abroad for educational or medical reasons, natural disasters, disabilities, death of a family member, or having had the DNI recently stolen, among others.

[10] The "Unidad Impositiva Tributaria" (UIT) is a reference value that is adjusted yearly for inflation. It is used to determine thresholds in the tax code, the price of public services, and the value of fines and sanctions.

[11] The UIT was set at S/3,400 in 2006. The PPP-adjusted exchange rate in 2006 was S/1.51 per US$1. We use this value for all calculations in the paper. The nominal exchange rate was S/3.27.

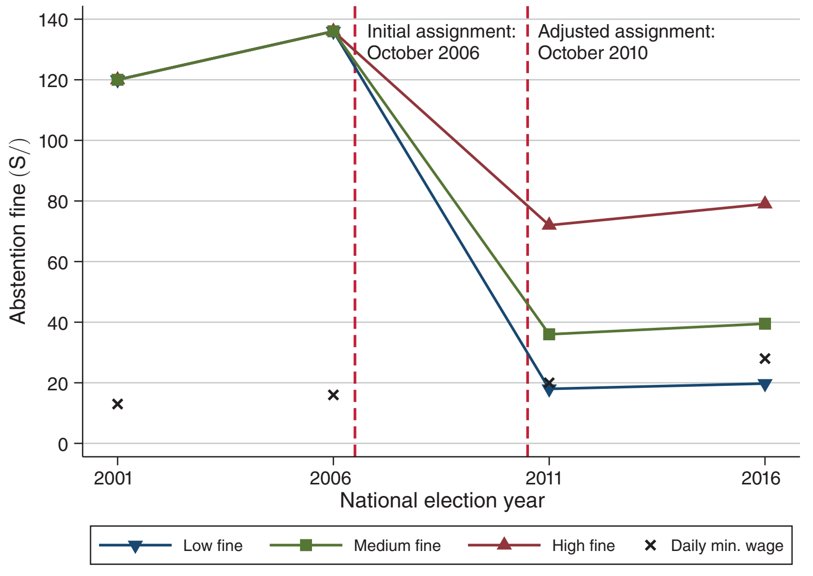

FIGURE 1. THE ABSTENTION FINE BY ELECTION AND FINE CATEGORY

Notes: The graph shows the value of the abstention fine in each category for the national elections of 2001, 2006, 2011, and 2016. Values are displayed in current Peruvian soles (S/) but are defined in constant units for tax purposes (UIT), which are updated yearly to adjust for inflation. The graph also shows the nominal value of the legal minimum daily wage for each election year. The PPP-adjusted exchange rate in 2006 was S/1.51 per US$1. The average yearly inflation rate for the period 2001–2016 was 2.75 percent. The vertical dashed lines indicate the date in which the initial reform to the abstention fine took place (October 27, 2006) and the date in which districts were reclassified (October 1, 2010).

assigned a high fine (nonpoor), 18 percent were assigned a medium fine (poor), and 30 percent a low fine (extreme poor).[12] We verify below that our results are not driven by idiosyncratic changes in voter turnout in poorer or richer areas. To the best of our knowledge, this classification has not been used for any other public policy.

Figure 1 shows the value of the abstention fine in each category for each national election during the sample period. The elections in 2006 were the last ones held under the previous regime with a uniform fine. The following ones in 2011 and 2016 took place after the reform and also after the districts had been reclassified. Figure 1 also shows that the value of the fine in low-fine districts is similar to the minimum daily wage. The value of the fine in each category has remained constant as a percentage of the UIT since the 2006 reform. Slight growth between 2011 and 2016 is driven by the yearly adjustment of the UIT.

Neither the enforcement of the fine nor the restrictions in access to government and financial services faced by nonvoters changed in the 2006 reform. Fines normally expire after four years, and the national government often provides amnesties, thereby dissuading debtors from settling outstanding fines. Roughly 20 percent of the 4.7 million fines issued in 2011 were settled (see online Appendix Figure A2).

[12] Online Appendix Table A1 shows that all districts classified as high-fine in 2006 remained in this category in 2010, while only 15 percent and 56 percent of districts classified as medium- and low-fine remained in the same category.

However, enforcement improved substantially in 2012, when a collection unit was created within the JNE (Resolución 0738-2011-JNE).[13] As a result, almost 50 percent of the fines issued in 2016 had been settled by mid-2018. This increase was mainly driven by high-fine districts. We verify below that our estimates of the marginal effect of the abstention fine on turnout are not confounded by differential changes to enforcement across fine categories.

II. Empirical Strategy

In this section, we present the data sources and research design for the analysis of the marginal effects of the abstention fine. We leave the exposition of the complementary strategy for the study of the exemption from compulsory voting for Section V below.

A. *Data*

We use administrative data for the national elections in 2001, 2006, 2011, and 2016 from the National Election Office (Oficina Nacional de Procesos Electorales, ONPE). The data cover the general election, combining the legislative election with the first round of the presidential race, and the presidential runoff. We observe the number of registered voters, the number of votes cast, and the number of invalid and blank votes per election at the district level. JNE publishes the value of the abstention fine and the assignment of districts to the different fine categories before each election. Our main sample includes 1,755 districts, corresponding to 94 percent of the total and covering 96 percent of the almost 23 million registered voters in 2016.[14]

JNE provided us with the number of fines issued per election and the amount of money collected from fine payments and processing fees for excuses since the subnational elections in 2006. Unfortunately, reliable information on fine settlement is unavailable for earlier elections. We also obtained from ONPE and JNE district-level information on registered voters for 6 age groups (18–20, 21–29, 30–35, 36–50, 51–75, 75+) for all years except 2006. ONPE also provided fine-grained data on the number of registered voters for each one-year age group at the voting booth level (i.e., within polling station) for 2016. We also use publicly available individual-level data from the 2017 presidential election in neighboring Chile made available by the National Electoral Service (Servicio Electoral de Chile).

B. *Research Design*

We aim to estimate the causal effect of the value of the abstention fine on voter turnout and electoral outcomes. We exploit the differential reduction of the

[13] This unit has the power to freeze any debtor's bank accounts and credit cards after sending two notifications to the person's home. Fines no longer expire after four years if a collection process is under way. JNE (2015) reports that 42 percent of fine payments between 2012 and 2015 resulted from coercive collection.

[14] After excluding districts with missing data, we are left with 1,769 districts out of the 1,854 in the country. We exclude a further 14 districts for inconsistencies in the fine assignment.

fine across districts after 2006. This natural experiment lends itself naturally to a difference-in-difference analysis.[15]

As mentioned above, districts were initially classified into the three fine categories in 2006 but were reclassified before the next election in 2011. It is the variation provided by this latter classification that we exploit in our estimations. However, the 2006 assignment may have been based on informative district characteristics that we do not observe (as the criteria employed remain unclear). Even if temporary, this assignment may have also affected voters' perception of the value of the fine and their behavior. To account for these possibilities, we allow the outcomes to vary flexibly over time in districts that are located in the same province and were assigned to the same category in 2006. Our baseline specification thus includes district fixed effects and the quite stringent "election by province by 2006 category" fixed effects.[16] The latter also control for common shocks, allowing them to differ across provinces and/or 2006 fine categories. The identifying assumption is that in the absence of the 2010 assignment, there should be no systematic change in the outcomes between districts located in the same province and that belonged to the same category in 2006. To provide evidence in support of the parallel trends assumption, we first estimate the following model:

$$(1) \qquad y_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \sum_k \sum_t \beta_{k,t}\big[\mathbf{1}(e = t) \times \mathbf{1}(c_{10} = k)\big] + \epsilon_{d,p,e},$$

where $y_{d,p,e}$ is an outcome in district d from province p in election e; α_d is a district fixed effect, while $\delta_{p,e,c_{06}}$ is the "province $\times$ election $\times$ 2006-category" fixed effect. The other terms correspond to a full set of interactions between dummy variables $\mathbf{1}(\cdot)$ for each election date t in the sample period (e.g., 2011 presidential runoff) and respective dummies for each fine category k from the 2010 assignment (c_{10}). The omitted election is the 2006 presidential runoff (last national election before the reform), and the omitted category corresponds to medium-fine districts. The variable $\epsilon_{d,p,e}$ is an error term that we cluster at the province level (192 clusters) to allow for arbitrary correlation within provinces, including spatial autocorrelation.

The set of coefficients $\beta_{k,t}$ capture the average difference in the outcome between districts in category k (high or low fine) and the omitted group (medium fine) relative to what that difference was for the 2006 presidential runoff (omitted election), conditional on the set of fixed effects. Those coefficients corresponding to elections before the reform (2001 and 2006) allow us to test for pre-trends and help validate the research design. The coefficients corresponding to the elections after the reform (2011 and 2016) allow us to measure its aggregate effects and to characterize the time profile of the impact.

We exclude subnational elections from the analysis to keep the selection and behavior of local candidates and incumbents constant and focus on voter behavior.

[15] Carpio et al. (2019) use a complementary Regression Discontinuity Design (RDD) with the share of population below the poverty line as running variable. Reassuringly, their RDD estimates of the difference in turnout across fine categories are very similar to our difference-in-difference estimates.

[16] Our estimation effectively drops 63 singleton districts lacking at least 1 other district from the same province with the same assignment in 2006. As a result, the regressions below report the effective sample of 13,536 observations from 1,692 districts rather than the full sample of 14,040 observations in 1,755 districts.

Our empirical strategy always involves comparing voters with different incentives to participate in elections but faced with the same set of candidates. By excluding subnational elections, we also shut down the potential effects of the reform on voter buying (Hidalgo and Nichter 2016).

We then modify the specification to estimate the causal effect of a marginal change to the abstention fine on our relevant outcomes:

$$y_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \nu Fine_{d,e} + u_{d,p,e}, \tag{2}$$

where $Fine_{d,e}$ is the value of the abstention fine in 100s of current Peruvian soles (S/) and α_d and $\delta_{p,e,c_{06}}$ are fixed effects analogous to those in equation (1). Thus, we exploit the same source of variation as above. The coefficient of interest is ν, which captures the average causal effect on the outcome (e.g., percentage points of turnout) for a S/100 change in the value of the fine. We also estimate the corresponding elasticities by replacing the outcome and the value of the fine with their logarithms. Importantly, while this specification makes no difference between fine increases and decreases, our variation originates exclusively in fine decreases and does not allow us to capture asymmetric responses. The variable $u_{d,p,e}$ is an error term clustered again at the province level, which flexibly accommodates serial and spatial correlation of the error term across districts within the same province.[17] We weight all our regressions by the number of registered voters in 2001 to capture individual-level effects.

III. Results: Voter Turnout

In this section, we present estimates of the causal effect of the abstention fine on voter turnout. We first present evidence from the event-study model that lends support to the identifying assumption of parallel trends and indicates an increasing response over time. We then show estimates of the marginal effect of the fine and the corresponding elasticity and document heterogeneous impacts. Next, we examine the sensitivity of the results to variation in the probability of enforcement. We also decompose the marginal effect of the fine on turnout into separate effects on voter registration (selection) and the propensity to vote (behavior). Finally, we compare our estimates to previous experimental results and provide evidence on informational frictions as a major contributor to voltage drop.

A. *Baseline Results*

Figure 2 shows point estimates and 95 percent confidence intervals of $\beta_{k,t}$ in equation (1) for voter turnout.[18] The arrowhead markers show the average difference in turnout between districts in the high- or low-fine categories and those in the omitted medium-fine category, relative to the difference in the 2006 presidential runoff

[17] Online Appendix Table B1 shows that the main results are robust to clustering at the region level, using a wild cluster bootstrap procedure to account for the small number of clusters (Cameron, Gelbach, and Miller 2008).

[18] Online Appendix Table B2 shows the corresponding estimates. Online Appendix Figure B1 shows results from a more disaggregated specification with separate coefficients for each combination of 2006/2010 assignments.

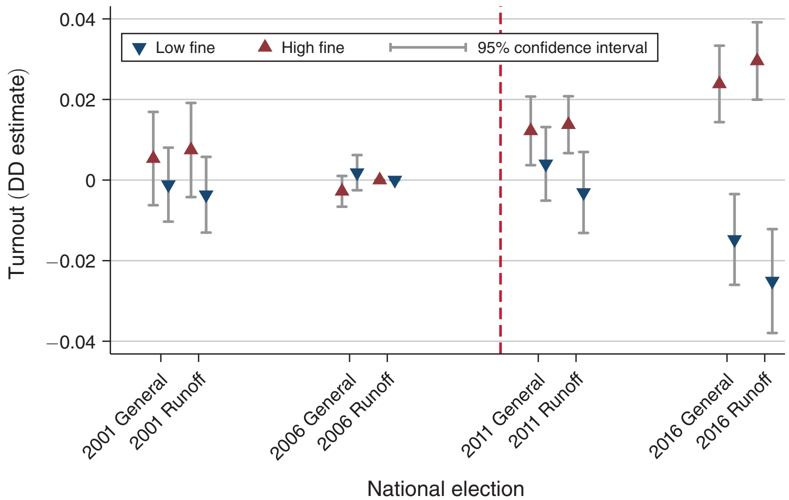

Figure 2. The Reform to the Abstention Fine and Voter Turnout

Notes: The graph shows point estimates and 95 percent confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with respective dummies for districts assigned a high fine (nonpoor) and a low fine (extreme poor) in 2010. The omitted category includes districts assigned a medium fine (poor) in 2010. The omitted election is the 2006 presidential runoff. Regression includes district and province $\times$ election $\times$ 2006-category fixed effects. Regression includes 13,536 observations from 1,692 districts. Districts are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 clusters). The dashed line indicates the date of adjusted district assignment (October 2010).

(omitted election). The dashed line shows the timing of the adjusted assignment to the fine categories.

The estimates for the elections before the reform are small and statistically insignificant at conventional levels, indicating that voter turnout followed parallel trends across all categories between 2001 and 2006. These results increase our confidence in attributing any subsequent change in turnout to the reform. After the reform, we observe indeed a systematic divergence in turnout among the three groups. High-fine districts show a steady relative increase in turnout, while low-fine districts show a steady relative decrease. Turnout is already 1.3 percentage points (pp) higher in the high-fine category in 2011. In 2016, voter turnout in high-fine districts was 2.4 and 3.0 pp higher than in medium-fine ones in the general and runoff elections, respectively, while in low-fine districts it was 1.5 and 2.5 pp lower than in medium-fine ones in those same elections. There is a 5.4 pp gap in turnout between the high-fine and low-fine districts in the 2016 presidential runoff. This is a sizable effect, comparable to that of some of the most effective voter mobilization initiatives that have been studied (Green, McGrath, and Aronow 2013), and could lead to inequality in representation across districts. Figure 2 also provides preliminary evidence of a heterogeneous effect across election types.

Panel A in Table 1 presents estimates of equation (2) using voter turnout as dependent variable. The estimate of ν in column 1 implies that a S/10 decrease in the value of the fine (roughly US\$7) leads to a fall in turnout of about 0.5 percentage points. This corresponds to a 0.58 percent decrease over the sample mean of 0.85.

Table 1—The Marginal Effect of the Abstention Fine on Voter Turnout

	Baseline (1)	Election FE (2)	Election × Province FE (3)	No weights (4)	Additional controls: Poverty shares (5)	Additional controls: Education shares (6)	Additional controls: Polling stations (7)
Panel A. Dependent variable: Voter Turnout$_{i,t}$							
Fine value$_{i,t}$ (S/ × 100)	0.049	0.073	0.046	0.062	0.035	0.061	0.046
	[0.008]	[0.012]	[0.009]	[0.010]	[0.011]	[0.011]	[0.009]
R^2	0.0180	0.0693	0.0257	0.0245	0.0637	0.0770	0.0511
Mean of dependent variable	0.845	0.844	0.844	0.793	0.845	0.831	0.845
Panel B. Dependent variable: ln Voter Turnout$_{i,t}$							
ln Fine value$_{i,t}$	0.030	0.040	0.028	0.037	0.023	0.037	0.029
	[0.005]	[0.006]	[0.006]	[0.006]	[0.007]	[0.007]	[0.006]
R^2	0.0180	0.0564	0.0240	0.0220	0.0614	0.0699	0.0395
Mean of dependent variable	−0.171	−0.172	−0.172	−0.240	−0.171	−0.188	−0.171
Observations	13,536	14,040	14,040	13,536	13,536	10,152	13,536
Districts	1,692	1,755	1,755	1,692	1,692	1,692	1,692
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election × Province × Category '06 FE	Yes	No	No	Yes	Yes	Yes	Yes
Election FE	No	Yes	No	No	No	No	No
Election × Province FE	No	No	Yes	No	No	No	No
Weighted	Yes	Yes	Yes	No	Yes	Yes	Yes

Notes: All columns use data from national elections (general and presidential runoff) in 2001, 2006, 2011, and 2016, except column 6 (data on education level of voters is unavailable for 2006). Column 5 includes the time-invariant shares of poor and extreme poor inhabitants interacted with election fixed effects. Column 6 includes the time-varying shares of registered voters with primary, secondary, and tertiary education. Column 7 includes log polling stations. All columns are weighted by the number of registered voters in 2001, except column 4. Standard errors are clustered by province (192 units).

Column 1 in panel B shows that the corresponding estimate of the elasticity is 0.03. Both coefficients are statistically significant at the 1 percent level. As mentioned above, these estimates incorporate potential spillovers from changes in the behavior of peers and other general equilibrium effects.

The remaining columns correspond to several robustness tests. Our point estimates are very similar when we include election (column 2) or election by province fixed effects (column 3) instead of "province × election × 2006-category," or when we run our regressions unweighted (column 4). Columns 5–7 show that time-varying differences across districts do not affect our baseline results. In these regressions, we control flexibly for the share of people classified as poor or extreme poor (column 5); the share of voters with primary, secondary, and higher education (column 6); and the number of polling stations in the district (column 7).[19] Our estimates remain economically and statistically significant.

[19] Information on education is not available for 2006. The point estimates (standard error) for the marginal effect of the fine and the corresponding elasticity in this reduced sample are 0.040 (0.010) and 0.023 (0.006).

Table 2—Heterogeneous Effects of the Abstention Fine on Voter Turnout

Dependent variable:	Turnout$_{i,t}$ (Mean = 0.85)			ln Turnout$_{i,t}$ (Mean = −0.17)		
	Long-run (1)	Runoff (2)	Poverty (3)	Long-run (4)	Runoff (5)	Poverty (6)
(ln) Fine value$_{i,t}$ (S/ × 100) [a]	0.020	0.039	−0.022	0.011	0.023	−0.021
	[0.008]	[0.009]	[0.022]	[0.005]	[0.006]	[0.022]
(ln) Fine value$_{i,t}$ × $\mathbf{1}$(2016)$_t$ [b]	0.051			0.038		
	[0.005]			[0.003]		
(ln) Fine value$_{i,t}$ × $\mathbf{1}$(Runoff)$_t$ [b]		0.019			0.014	
		[0.004]			[0.003]	
(ln) Fine value$_{i,t}$ × Share Poor$_i$ [b]			0.072			0.058
			[0.021]			[0.025]
Observations	13,536	13,536	13,536	13,536	13,536	13,536
Districts	1,692	1,692	1,692	1,692	1,692	1,692
R^2	0.0277	0.0194	0.0299	0.0325	0.0201	0.0235
p-value H$_0$: a + b = 0	0.000	0.000	0.000	0.000	0.000	0.000
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election × Province × Category ’06 FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns use data from national elections (general and presidential runoff) in 2001, 2006, 2011, and 2016. The value of the fine in columns 1–3 is measured in 100s of current Peruvian soles (S/). In columns 4–6, we use the natural log of the value of the fine. Columns 1 and 4 include the interaction of the fine with a dummy for the 2016 elections. Columns 2 and 5 include the interaction of the fine with a dummy for presidential runoff elections. Columns 3 and 6 include the interaction of the fine with the share of poor population (non-extreme and extreme) in the district. All columns are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 units).

B. *Heterogeneous Effects*

We study potential heterogeneity in the turnout response to a changing value of the abstention fine by estimating extensions of equation (2) that include interactions of the fine with dummies for various subsamples. Table 2 shows the results.

We first examine heterogeneous effects across the two post-reform elections. Consistent with the evidence in Figure 2, we observe a much larger effect in the longer term. A same-sized decrease to the value of the fine leads to a drop in turnout that is more than three times as large in 2016 as in 2011. The estimated elasticity is 0.011 in 2011 and 0.049 in 2016. This increasing response over time is consistent with gradual learning about the modified policy incentives. We further explore this mechanism in Section IIIE. It is also consistent with dynamic peer effects if the marginal voters induced not to vote by the lower fine increasingly drive others to also not vote as time goes by (Nickerson 2008, Chong et al. 2019). A third possibility is that habit formation acts as a countervailing force and that those induced not to vote by the lower fine in 2011 are joined by a new wave of marginal nonvoters in subsequent elections (Coppock and Green 2016; Fujiwara, Meng, and Vogl 2016).

We next explore whether the marginal effect of the fine on turnout varies depending on the type of election. The marginal effect of the fine is around 50 percent larger in the runoff than in the general election, with the elasticity jumping from 0.023 to 0.037. This heterogeneous effect is unlikely to be driven by increased learning, with the elections less than two months apart.[20] It suggests instead that the marginal voters affected by changes to the value of the fine differ across election types. One plausible explanation is that voters have a stronger incentive to participate in the general election than in the presidential runoff, which is consistent with the larger aggregate turnout in the former (see online Appendix Figure A1).[21]

Finally, we study heterogeneity depending on the share of district residents below the poverty line. The baseline estimate is statistically indistinguishable from zero, indicating a null response in a municipality with a poverty rate of zero. On the other hand, the estimated elasticity in a municipality with all voters below the poverty line is 0.037. These results indicate that poorer people, for whom a marginal fine increase represents a greater economic burden, are more responsive to a larger fine.[22]

Overall, these results highlight the potential for context dependence in small field experiments studying voter mobilization initiatives in a very localized setting, over a short time horizon or in only one type of election, as has been the norm (Gerber and Green 2017).

C. *Enforcement of the Fine*

The correct interpretation of the marginal effect of the fine must take into account the probability of enforcement. We approximate the strength of enforcement using the share of fines that are settled (i.e., paid or excused). By July 2018, 38 percent of all fines issued between the subnational elections of 2006 and the 2016 runoff had been settled. Online Appendix Figure A2 shows that settled fines grew from 22 percent in 2011 to almost 50 percent in 2016. Although these figures are not negligible, sophisticated voters may realize that the expected fine is smaller than its nominal value. We are thus likely underestimating the marginal effect of a monetary incentive provided with certainty. In online Appendix Table C1, we incorporate the probability of enforcement by adjusting the value of the fine by the share of fines settled in the current or previous election.[23] The effect of the fine on turnout increases 10–30 percent (i.e., 0.64 pp drop for a S/10 fall), while the estimated elasticity decreases by a similar amount.

[20] Online Appendix Figure B3 provides separate estimates of the marginal effect of the fine for each individual election in 2011 and 2016. We find that the differences in the marginal effect of the fine (or the elasticity) across election types are statistically significant even within the same year.

[21] A stronger intrinsic motivation to vote in the general election may result from (i) a stronger sense of civic duty in the first election of the cycle, (ii) greater interest in the outcome of the legislative election, or (iii) the larger number of presidential candidates in the first round. Regarding the latter, online Appendix Table B3 finds no evidence of a heterogeneous effect in the runoff depending on the first-round vote for the top two candidates. Voters may also face stronger pressure from local brokers in the general (legislative) election.

[22] Online Appendix Figure B2 replicates the event-study analysis from Figure 2, disaggregating each fine category into two same-sized groups based on the share of poor population in the district. The plot shows that the fall in turnout in low-fine districts is much larger in those with high poverty. The opposite pattern is observed in high-fine districts, though the difference is not as large. Moreover, turnout in medium-fine districts with low poverty is almost identical to that in medium-fine districts with high poverty, which is reassuring given that these two groups face the same monetary incentive to vote.

[23] Naturally, the share of fines that are paid is itself affected by the value of the fine. Online Appendix Table C2 shows that a higher value of the fine leads to a lower share being paid, keeping enforcement constant (i.e., 2011). However, this decrease in fine repayment is offset by an equivalent increase in the share of fines excused, leading to a net zero effect on the share of fines settled. These findings constitute further evidence of voters' sophisticated response to changes in the monetary incentive to vote.

The overall increase in settled fines seems to be related to improvements to enforcement that took place after 2012, including the creation of a collections unit within the JNE. There is no evidence linking the reform to the abstention fine with the toughening of enforcement nor implying that the fine categories were actively used for the latter. The positive effect of the fine on turnout in 2011, before the changes to enforcement took place, ensures that we are not just picking up the effect of these changes. Still, the aggregate data show that the increase in settled fines was mostly concentrated in high-fine districts (online Appendix Figure A2), which suggests that our finding of a substantially larger effect in 2016 could be compromised. Online Appendix Table C3 shows results from further robustness tests based on information about the districts targeted by the collections unit and the variation in the share of fines settled.[24] All of our predictors of improved enforcement after 2012 are positively correlated with voter turnout in 2016. However, neither their inclusion as controls nor the exclusion of targeted districts causes the long-run effect of the value of the fine to fall by more than 20 percent.

Overall, while enforcement is a relevant factor in weighting the costs and benefits of participation in elections, the results in this section show that changes in enforcement do not account for a large share of the effect of the abstention fine on voter turnout.

D. *Changes in Registration and the Propensity to Vote*

In this section, we disaggregate the effect of the fine on turnout into separate effects on the number of votes (numerator) and the number of registered voters (denominator). As mentioned in Section I, all eligible voters (18 or older) are automatically registered to vote in the district corresponding to the home address reported in their DNI. Hence, even though registration is not a choice variable per se, voters can adjust their voting district by reporting a different address. Proof of address should be provided for such a change, but in practice, this requirement is often waived and there is abundant anecdotal evidence of irregularities in the voter registry, though most of it is related to fraudulent registration for electoral gain.[25]

We begin by examining whether the reform led to higher registration in districts with lower fines. We first estimate equation (1) using log registered voters as the dependent variable. In this case, we only have one observation per district-cycle since the voter registry remains unchanged between the general election and the presidential runoff. We set 2006 as the omitted election cycle and keep the medium-fine districts as the omitted category. Figure 3 shows the results. The difference in voter registration across categories remains remarkably stable between 2001 and 2006, lending support to our

[24] The vast majority of targeted districts are located in the provinces of Lima and Callao (JNE 2015). The collections unit also focused its attention on large cities and provincial capitals. We also consider a more agnostic, catchall approach, in which we calculate for each district the change in the share of fines settled between 2006 and 2014. We then re-estimate the flexible version of equation (2) allowing for a time-varying effect and include the interaction of these variables with a dummy for 2016 as controls.

[25] Resolución 1400-2006-JNE documents abnormal increases in voter registration for the 2006 subnational elections in multiple districts. News reports estimated that there were at least 100,000 wrongly registered voters for the 2018 subnational elections (Nunez 2017). Address misreporting is punished since 2015 with a fine equal to 0.3 percent of the UIT, slightly less than the abstention fine in low-fine districts.

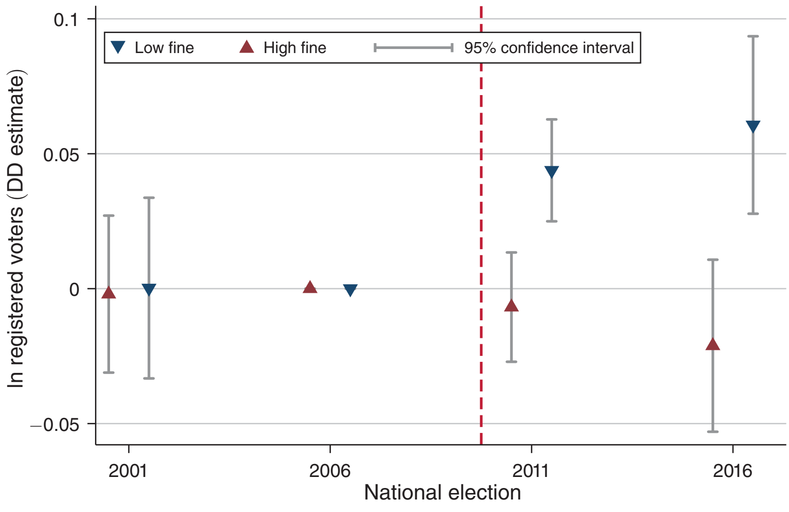

Figure 3. The Reform to the Abstention Fine and Voter Registration

Notes: The graph shows point estimates and 95 percent confidence intervals of a regression of the natural log of district-level registered voters on a full set of election dummies interacted with respective dummies for districts assigned a high fine (nonpoor) and a low fine (extreme poor) in 2010. The omitted category includes districts assigned a medium fine (poor) in 2010. The omitted election year is 2006. Regression includes district and province × election × 2006-category fixed effects. Regression includes 6,768 observations from 1,692 districts. Districts are weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 clusters). The vertical dashed line shows the date of adjusted district assignment (October 2010).

identification strategy. After the reform, we observe a disproportionate increase in the number of voters in low-fine districts. Specifically, voter registration grows 4.4 percent more in these districts than in medium-fine ones in 2011 and 6.1 percent more in 2016. The difference with high-fine districts is even starker, at 5 percent in 2011 and 8.2 percent in 2016 (see online Appendix Table B2). High-fine districts show a decline in registered voters relative to medium-fine ones, but it is small and insignificant. A back-of-the-envelope calculation suggests that excess registration in low-fine districts amounts to 130,000 voters in 2016, equivalent to 0.5 percent of total registered voters.

These results suggest that voters intentionally change their reported address to avoid paying a larger abstention fine. However, this interpretation seems unreasonable for most of the population, as DNI renewal requires a payment of S/22 and in-person visits to the office of the National Registry (Registro Nacional de Identificación y Estado Civil, RENIEC). This seems more plausible in the case of young adults who must apply for a new DNI and pay the respective fee when they turn 18. Young adults are also likely to be living with parents or relatives, making it easier to avoid providing a proof of address. Columns 2–7 in Table 3 estimate registration effects for six different age groups. Column 1 first shows an average elasticity of −0.045 for this sample (which excludes the year 2006 due to lack of data). The remaining columns show that the elasticity monotonically decreases with age and is only statistically significant for the 18–20 and 21–29 age groups. In particular, column 2 shows a registration elasticity of −0.28 for the 18–20 age

Table 3—The Marginal Effect of the Abstention Fine on Voter Registration by Age

	Dependent variable: ln Voters$_{i,t}$						
	All (1)	18–20 (2)	21–29 (3)	30–35 (4)	36–50 (5)	51–75 (6)	75+ (7)
ln Fine value$_{i,t}$	−0.045	−0.276	−0.055	−0.031	−0.021	−0.017	−0.057
	[0.019]	[0.043]	[0.020]	[0.022]	[0.020]	[0.024]	[0.051]
Observations	5,076	5,076	5,076	5,076	5,076	5,076	5,076
Districts	1,692	1,692	1,692	1,692	1,692	1,692	1,692
R^2	0.001	0.03	0.001	0.0003	0.0002	0.0001	0.001
Mean of dep. var	10.71	7.996	9.279	8.770	9.431	9.191	7.318
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election × Province × ’06 Category FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: ln Voters is the natural log of the number of registered voters for the election cycle. Sample includes the election years 2001, 2011, and 2016. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 units).

group, which is 6 times larger than the average. Online Appendix Table D1 further shows that the registration elasticity grows with proximity to districts with a lower fine.[26]

Our results show that young voters are strategically responding to the spatial variation in the fine for abstention by changing their reported address to low-fine districts. This type of voter misbehavior lacks the political motivation behind vote- or voter-buying (Nichter 2008, Hidalgo and Nichter 2016) but could affect youth representation (Bertocchi et al. 2020). It could also increasingly hurt electoral participation through habit formation (Coppock and Green 2016; Fujiwara, Meng, and Vogl 2016). These findings also illustrate the potential for unintended consequences when large-scale, targeted policies are implemented in settings with limited state capacity (Camacho and Conover 2011, Cassan 2015).

We next examine the extent to which the response in registration is driving the effect of the fine on turnout. We focus here on the elasticity, which allows us to easily disaggregate the net turnout effect into separate effects on the number of votes and the number of voters:

$$Turnout\ (T) \equiv \frac{Votes\ (V)}{Registered\ voters\ (R)} \Rightarrow \ln T = \ln V - \ln R$$

$$\Rightarrow \frac{d \ln T}{d \ln F} = \frac{d \ln V}{d \ln F} - \frac{d \ln R}{d \ln F}. \tag{3}$$

[26] The previous results are robust to several tests. We use census data to show that the results are not confounded by predictable changes in the number of voters (e.g., differential birth rates) in previous decades (online Appendix Table D2). We also use data on nighttime luminosity and a large household survey (ENAHO) to rule out changes in economic conditions or actual migration as the underlying mechanism (online Appendix Table D3). We further verify that the results are not driven by differential changes in the share of population having a DNI (online Appendix Table D4). Finally, it seems unlikely that the effect is driven by college students in low-fine districts updating their address (e.g., Braconnier, Dormagen, and Pons 2017), as (i) only 2 percent of 18- to 20-year-olds do not live with their parents, (ii) even fewer migrate for educational purposes, and (iii) universities tend to be located in larger and richer cities.

Table 4—The Marginal Effect of the Abstention Fine on Voter Registration

Dependent variable:	ln $Voters_{i,t}$		ln $Votes_{i,t}$	
	(1)	(2)	(3)	(4)
$\ln Fine_{i,t}$ [a]	−0.046	−0.035	−0.016	−0.024
	[0.015]	[0.012]	[0.016]	[0.014]
$\ln Fine_{i,t} \times \mathbf{1}(2016)_t$ [b]		−0.022		0.015
		[0.009]		[0.009]
Observations	6,768	6,768	13,536	13,536
Districts	1,692	1,692	1,692	1,692
R^2	0.002	0.002	0.0002	0.0003
Mean of dependent variable	10.68	10.68	10.50	10.50
p-value H_0: a + b = 0		0.002		0.646
District FE	Yes	Yes	Yes	Yes
Election × Province × Category '06 FE	Yes	Yes	Yes	Yes

Notes: ln *Voters* is the natural log of the number of registered voters for the election cycle (same for general and runoff elections); ln *Votes* is the natural log of the actual number of votes cast in each election. Sample includes national elections (general and presidential runoff) in 2001, 2006, 2011, and 2016. Regressions are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 units).

Table 4 provides estimates of the two marginal effects in the right-hand side of equation (3). Column 1 shows that the elasticity of registration in the full sample (−0.046) is very similar to the estimate in Table 3 (−0.045), while column 2 shows that the elasticity increases from −0.035 in 2011 to −0.057 in 2016, indicating gradual adaptation to the policy. Columns 3 and 4 replicate the analysis using log votes as the dependent variable. The elasticity of votes (−0.016) is substantially smaller than that of voters, which means that the mechanical effect leading from fewer voters to fewer votes is being offset by the increasing propensity to vote by those voters who do not change district in response to a larger fine. Column 4 shows that the behavioral response in the propensity to vote is growing as well: the reduction in the number of votes caused by an increase to the value of the fine is larger in 2011 (−0.024) than in 2016 (−0.009).

Using these estimates, we can better understand the relative contribution of changes in voter registration and in the propensity to vote to the net effect of the fine on turnout. Assuming that log votes is a function of the log value of the fine and of log voters (itself also a function of the fine), we can further decompose the elasticity of votes:

$$\ln V = \ln V\big(\ln R(\ln F), \ln F\big)$$

$$\Rightarrow \frac{d\ln V}{d\ln F} = \underbrace{\frac{\partial \ln V}{\partial \ln R}}_{\substack{\text{Turnout}\,|\\ \Delta\text{registration}}} \frac{d\ln R}{d\ln F} + \underbrace{\frac{\partial \ln V}{\partial \ln F}}_{\substack{\text{Behavioral}\\ \text{elasticity}}}. \tag{4}$$

The term on the left-hand side of equation (4) corresponds to the elasticity of votes with respect to the fine, which we have estimated in column 3 of Table 4. On the right-hand side, we have two unknowns (terms in braces) and the elasticity of registered voters with respect to the fine, for which we also have an estimate in column 1 of Table 4. The first unknown is the partial derivative (elasticity) of votes with respect to registration (i.e., the relative effect on votes of a 1 percent increase in the number of registered voters, all else equal). This is equivalent to voter turnout among those who change their address of registration, whom we refer to as "movers." The other unknown is the partial derivative (elasticity) of votes with respect to the fine (i.e., the change in votes caused by a 1 percent increase in the value of the fine, all else equal). This is the purely behavioral response in the propensity to vote.

Based on different assumptions about voter turnout among movers, we can provide bounds on the behavioral elasticity of turnout. Equations (3) and (4) show that at the extremes, this elasticity is bounded by the respective elasticities of votes (-0.016) and turnout (0.03).[27] If movers vote at the average rate observed in 2011/2016 of 83 percent, the resulting behavioral elasticity is 0.022, corresponding to 73 percent of the total elasticity of turnout. However, it seems likely that movers vote at lower-than-average rates. If their turnout is 1, 2, or 3 standard deviation below the average (77 percent, 71 percent, and 64 percent, respectively), the behavioral elasticity in turn becomes 0.019, 0.016, and 0.013. Turnout of 64 percent is below the fifth percentile of the district-level distribution in 2011/2016. Such turnout is consistent with the 20 pp gap for 18- to 20-year-old voters shown in Section V below (i.e., those driving the registration effect). The resulting behavioral elasticity corresponds to 43 percent of the aggregate turnout elasticity, meaning that as much as 57 percent of the effect of the fine on turnout can plausibly be attributed to the change in registration by low-turnout voters.[28]

E. *Previous Experimental Estimates*, *Adaptation and Voltage Drop*

Only a few studies involving field experiments have estimated the effect of monetary incentives on voter turnout (Panagopoulos 2012, León 2017, Shineman 2018). Relevant for us, León (2017) studies our same reform, providing a unique opportunity to compare experimental and non-experimental results in the same setting. After showing that voters were largely unaware of the modified fine value, León provided information to a random subset and estimated that a S/10 increase in the perceived value of the fine led to a 1.7 pp increase in turnout in the 2010 subnational elections, with an implied elasticity of 0.22. Our corresponding estimates of 0.49 pp and an elasticity of 0.03 are much smaller. Such voltage drop is not uncommon when the effects of large-scale policies are compared to those of smaller experiments (Al-Ubaydli et al. 2017). In our setting, it seems plausible that voters are imperfectly informed about the reform, which the treatment in León (2017) remedies by providing salient and individualized information. Lack of knowledge about the reform could lead to imperfect compliance and to a dampened marginal effect of the fine on turnout.

[27] Intuitively, if movers never vote, their change of address will not affect the number of votes (i.e., the full and partial elasticities of votes in equation (4) are equal). If movers always vote, we can use the elasticities of votes and registration to back out the partial votes elasticity, which is then equal to the turnout elasticity.

[28] Online Appendix Figure D1 shows the implied behavioral elasticity of turnout corresponding to different turnout rates for movers. Online Appendix Table D5 shows results from equation (2) controlling for log registered voters, a "bad control" as defined by Angrist and Pischke (2009). The results show that the turnout elasticity drops 20 percent relative to the baseline estimates when we control for voter registration.

To better understand the demand for information about the abstention fine, we use data on nationwide Internet searches in the Google search engine. We construct a dataset on the popularity of 44 different search terms at the term-month level. Most of these terms are related to politics, and three of them pertain directly to the abstention fine. Online Appendix E provides detailed information on the construction of the dataset. Using this data, we implement the following difference-in-difference model:

$$(5)\quad \ln Google_{i,m,y} = \theta_i + \omega_m + \sum_{\tau \geq 2006} \lambda_\tau \big[\mathbf{1}(\textit{fine-related})_i \times \mathbf{1}(year = \tau)_y\big] + \zeta_{i,m},$$

where the dependent variable is the natural log of one plus the Google Trends index for search term i in month m in year y; θ_i is a search-term fixed effect, and ω_m is a month fixed effect. These fixed effects absorb persistent differences in popularity across search terms and common shocks to Google searches affecting all terms equally (e.g., improved Internet access). The coefficients of interest, λ_τ, tell us how the popularity of the search terms related to the fine vis-à-vis other terms changes relative to 2005, the omitted year.[29] The variable $\zeta_{i,t}$ is an error term that we cluster two-way by search term (44 clusters) and month (144 clusters).

Figure 4 plots the results. With the exception of 2006, a year with national and subnational elections, the popularity of fine-related search terms remains stable until 2009. In 2010, when districts were reassigned across fine categories and subnational elections took place, we observe again a rise in fine-related web searches. However, this increase in popularity continues to grow in the following years. These results suggest gradual learning about the reform and help explain our previous findings of larger long-run responses in turnout and registration. As informational frictions are further alleviated, we can expect the large-scale turnout elasticity to converge toward the experimental estimate in León (2017). However, full convergence seems unlikely, as it would require the elasticity to grow at the same yearly rate observed in 2016 for 22 more years (i.e., until 2038).[30] More generally, these results suggest that informational frictions contribute to imperfect compliance to large-scale policy incentives. Our findings highlight a limitation specific to interventions in political economy, namely that citizens may be plainly unaware about changes to the institutional environment.

IV. Results: Electoral Outcomes and Political Attitudes

In this section, we examine whether the drop in voter turnout induced by a lower abstention fine affects electoral outcomes and representation. We also study political attitudes and behaviors in the Peruvian National Household Survey (Encuesta Nacional de Hogares, ENAHO).

[29] Online Appendix Figure E1 shows equivalent results from the disaggregate specification at the monthly level.

[30] The elasticity of 0.048 in 2016 corresponds to an annual increase of 0.008 since 2010. This is smaller than the estimate of 0.011 from 2011, indicating a diminishing growth rate of the response over time.

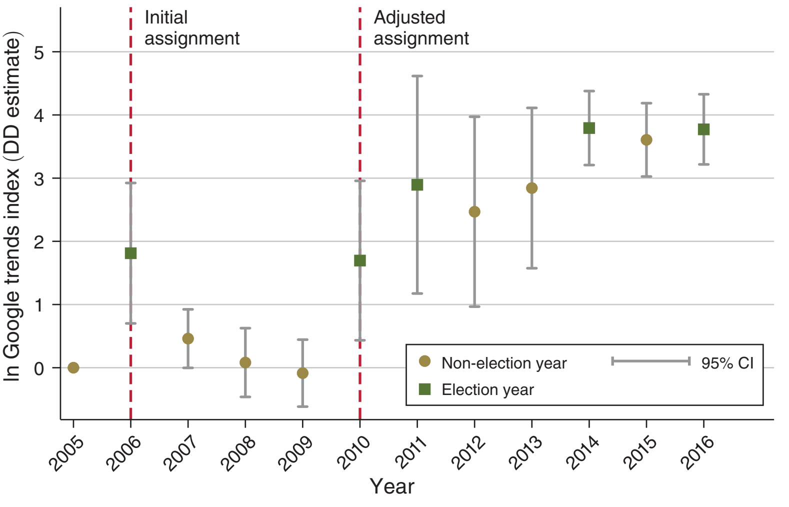

Figure 4. The Reform to the Abstention Fine and Information Acquisition

Notes: The graph shows point estimates and 95 percent confidence intervals of a regression of the natural log of a search-term popularity index from Google trends on year dummies interacted with an indicator for search terms related to the fine for abstention. Regression includes search-term and month fixed effects. The omitted year is 2005. Regression includes 6,336 observations from 44 search terms. See the online Appendix for list of search terms and details on construction of dataset. Standard errors are clustered two-way by search term and by month.

We first focus our attention on spoiled (i.e., blank or invalid) votes. This decision is partly driven by the volatile and personalistic nature of Peruvian politics, which prevents us from observing a comparable set of political parties both before and after the reform.[31] An additional theoretical justification is that in the absence of significant barriers to electoral participation, abstention is likely driven by the uninformed or uninterested, who are arguably more likely to spoil their vote (Feddersen and Pesendorfer 1996; Hoffman, León, and Lombardi 2017; León 2017; Freire and Turgeon 2020).

In Peru, a vote is considered blank if the ballot is deposited unmarked, while it is considered invalid if it has any mark other than a cross on the logo of one party or the picture of the respective candidate.[32] Blank and invalid votes are subtracted from the total number of votes before calculating candidate vote shares. We focus our attention on the first round of the presidential election for this part of the analysis, but online Appendix Table F2 shows that the results are qualitatively similar if we include the runoff. Importantly, ballots lack a "none of the above" option, so voters who do not support any of the candidates will often intentionally spoil the ballot

[31] Online Appendix F provides a brief overview of relevant facts of Peruvian politics during the sample period. We observe that individual politicians, rather than parties, are the main agents. Unfortunately, not all candidates participate in all elections, and their platforms fluctuate substantially over time. Online Appendix Table F1 finds no effect of the fine on the vote share for the two leading candidates in the first round.

[32] Reasons for a vote being considered invalid include marking more than one candidate/party, using symbols other than a cross, having the center point of the cross outside of the party logo or candidate picture, any tear or sign of damage, or adding any writing.

Table 5—The Marginal Effect of the Abstention Fine on Invalid and Blank Votes

Dependent variable:	Voter Turnout$_{i,t}$		Spoiled Votes$_{i,t}$	
	(1)	(2)	(3)	(4)
Fine value$_{i,t}$ (S/ $\times$ 100) [a]	0.043	0.017	0.037	0.022
	[0.008]	[0.009]	[0.007]	[0.008]
Fine value$_{i,t}$ $\times$ $\mathbf{1}(2016)_t$ [b]		0.045		0.026
		[0.005]		[0.005]
Observations	6,768	6,768	6,768	6,768
Districts	1,692	1,692	1,692	1,692
R^2	0.0152	0.0236	0.0117	0.0146
Mean of dependent variable	0.851	0.851	0.122	0.122
p-value H$_0$: a + b = 0		0.000		0.000
District FE	Yes	Yes	Yes	Yes
Election $\times$ Province $\times$ Category '06 FE	Yes	Yes	Yes	Yes

Notes: Dependent variable in columns 3–4 is the sum of blank and invalid votes, as a share of the number of registered voters. Sample includes first round of the presidential elections in 2001, 2006, 2011, and 2016. The value of the fine is measured in 100s of current Peruvian soles (S/). All columns are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 units).

to avoid manipulation during counting. Hence, we treat blank and invalid votes as equivalent and aggregate them for the analysis.

Column 1 in Table 5 re-estimates equation (2) for turnout, excluding the presidential runoff. The estimated coefficient of 0.043 is only slightly smaller than the average effect reported in Table 1. Column 2 replicates the heterogeneity analysis across election cycles for this subsample, finding once more a substantially stronger effect in 2016. Column 3 shows results using the share of blank or invalid votes as the dependent variable. This share is defined relative to the number of registered voters, making the estimated effect directly comparable to the one for turnout reported in column 1. We find that a S/10 fine decrease causes a 0.37 pp decrease in the share of blank or invalid votes. Column 4 shows that the marginal effect of the fine is also increasing over time, jumping from −0.22 pp in 2011 to −0.48 pp in 2016 for a S/10 fine drop. Figure 5 shows the corresponding event study based on equation (1). We observe a systematic divergence in the share of blank or invalid votes across categories after the reform, mostly driven by high-fine districts.

These results suggest that the majority of voters who are brought to the polls by a marginally larger fine are not voting for any of the available candidates.[33] The 0.37 pp decrease in blank or invalid votes is equivalent to 86 percent of the 0.43 pp drop in turnout caused by a S/10 smaller fine. In other words, for every 10 extra votes caused by a higher fine, we observe 8.6 more votes that are blank or invalid, which is consistent with theoretical models of rational abstention (Feddersen and Pesendorfer 1996). These results could also indicate that marginal fine changes mostly affect poorer, less-educated voters, who are more prone to make

[33] This finding stands in contrast to the null result reported by Carpio et al. (2019). The difference plausibly arises because of the different types of election under study (regional versus national), which may affect clientelistic practices involving local brokers, or due to the local nature of the RDD estimates.

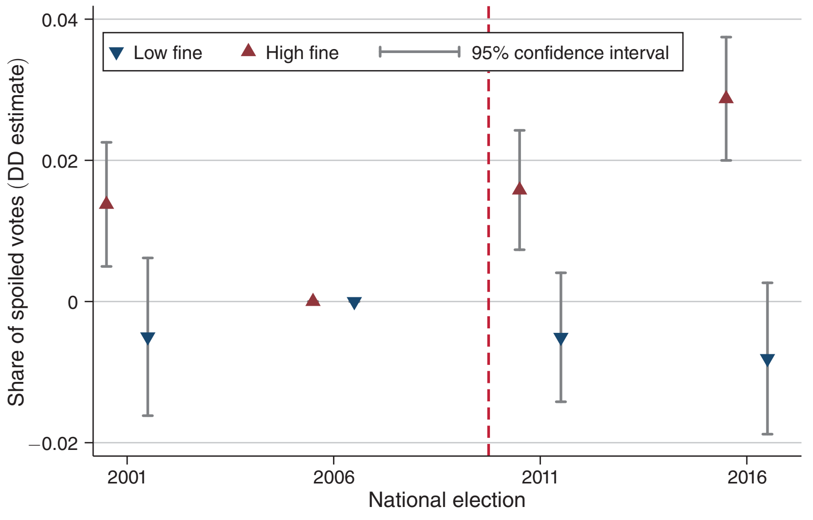

Figure 5. The Reform to the Abstention Fine and Blank or Invalid Votes

Notes: The graph shows point estimates and 95 percent confidence intervals of a regression of district-level blank and invalid votes (as a share of registered voters) in the first round of the presidential election on a full set of election dummies interacted with respective dummies for districts assigned a high fine (nonpoor) and a low fine (extreme poor) in 2010. The omitted category includes districts assigned a medium fine (poor) in 2010. The omitted election is 2006. Regression includes district and province × election × 2006-category fixed effects. Regression includes 6,768 observations from 1,692 districts. Districts are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 clusters). The vertical dashed line indicates the date of adjusted district assignment (October 2010).

a mistake (Fujiwara 2015), but this possibility finds little support in the data.[34] It is also possible that a lower fine causes a drop in turnout by anti-establishment voters who do not support any of the candidates but are neither uninformed nor uninterested (Ambrus, Greiner, and Zednik 2020; Ujhelyi, Chatterjee, and Szabó 2021). Unfortunately, protest voters will often spoil the ballot, preventing us from using the distinction between blank and invalid votes to adjudicate among these interpretations. Anecdotal evidence shows growing discontent with the political system in recent years, lending support to the latter interpretation (Tegel 2020).

These findings imply that marginal changes to the value of the abstention fine have a negligible impact on representation. They stand in contrast to some results in the literature showing large effects of changes in the electorate on election outcomes and downstream policies (Miller 2008, Cascio and Washington 2013, Fujiwara 2015), though they are in line with those in León (2017) and Hoffman, León, and Lombardi (2017). The difference with some of the previous literature is likely explained by the absence of significant barriers to electoral participation in our setting, which is reflected in very high turnout at baseline. Hence, electoral abstention is mostly a voluntary action in Peru, unlike the environments studied in these previous papers.

[34] In 2016, only 5 percent of voters were illiterate and a further 5 percent had less than full primary, in contrast to 42 percent with no education beyond fourth grade in Brazil (Fujiwara 2015).

Table 6—The Marginal Effect of the Abstention Fine on Political Attitudes and Behaviors

Dependent variable:	Trust in parties (1)	Party membership (2)	Interest in politics (3)	Information acquisition (4)
Fine $value_{i,t}$ (S/ × 100)	−0.023	0.003	−0.141	−0.065
	[0.020]	[0.005]	[0.060]	[0.056]
Observations	281,841	281,841	136,872	136,872
Districts	1,459	1,459	1,221	1,221
R^2	0.001	0.002	0.044	0.083
Mean of dependent variable	0.059	0.006	0.510	0.384
Survey years	2004–2016	2004–2016	2004–2011	2004–2011
District FE	Yes	Yes	Yes	Yes
Election × Province × Category ’06 FE	Yes	Yes	Yes	Yes
Individual controls	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header. Not all ENAHO survey questions are asked every year. All outcomes are binary. In column 1, “Trust in parties” takes the value of one if the respondent reports “sufficient” or “considerable” trust in political parties. In column 2, “Party membership” takes the value of one if the respondent reports belonging to a political group or party. In column 3, “Interest in politics” takes the value of one if the respondent reports being interested in politics rather than being indifferent or repelled. In column 4, “Information acquisition” takes the value of one if the respondent reports frequently being informed about political affairs rather than occasionally, depending on the topic, or never. Individual controls include dummies for gender, primary and secondary education, and age. The value of the fine is measured in 100s of current Peruvian soles (S/). All columns are weighted by the survey expansion factor. Standard errors are clustered by province.

Source: ENAHO survey.

Political Attitudes and Other Behaviors (*Survey Evidence*).—In this section, we study reported political attitudes and behaviors in ENAHO survey for the period 2004–2016. Survey questions have changed substantially over time, and not all are available before and after the reform to the abstention fine. We focus on the only four questions with sufficient coverage across survey waves. These questions concern (i) trust in political parties, (ii) party membership, (iii) interest in politics, and (iv) information acquisition. For this part of the analysis, we run regressions at the individual level, assigning the value of the fine to each respondent based on district of residence and year.[35] We cluster standard errors at the province level.

Table 6 shows the results. The dependent variable in column 1 is a dummy equal to one if the respondent reports sufficient or considerable trust in political parties. A S/10 fine decrease leads to a 0.2 pp increase in trust. However, the estimate is imprecise. In column 2, the dependent variable is a dummy equal to one if the respondent reports being a member of a political party or organization. In this case, the point estimate is close to zero and also insignificant. The dependent variable in column 3 is a dummy equal to one if the respondent reports being interested in politics. A S/10 fine decrease causes a significant 1.4 pp increase in this probability, equivalent to 2.7 percent of the sample mean. This result is consistent with the idea that a stronger extrinsic incentive to vote may crowd out the intrinsic motivation (Bénabou and Tirole 2003, 2006). Finally, the dependent variable in column 4 is a dummy equal to one if the respondent reports being frequently informed about

[35] Besides our baseline district and time fixed effects (i.e., year × province × 2006 category), we include the following individual-level controls: gender, educational attainment, and age fixed effects.

political affairs. The point estimate indicates that a S/10 fine decrease leads to a 0.7 pp increase in this probability, but this estimate is also imprecise.

Taken together, the survey evidence suggests that a larger fine decreases political engagement: reduced interest, lower trust in parties, and reduced acquisition of information (though the latter two are imprecisely estimated). These findings suggest that the previous results on spoiled votes may be partly driven by endogenous changes in political engagement and not purely by selection into voting by the ex ante uninformed or uninterested.

V. The Aggregate Effect of Compulsory Voting

We now turn to study the contribution of the monetary incentive provided by the abstention fine relative to the aggregate effect of compulsory voting on turnout. Underlying this exercise is the idea that compulsory voting provides both monetary and nonmonetary incentives. The latter include the expressive value of the law as a signaling device for socially desirable behaviors and the nonpecuniary penalty (i.e., restrictions on government services) imposed on nonvoters (Funk 2007, Cepaluni and Hidalgo 2016). Previous work provides several estimates of the effect of compulsory voting on turnout, but comparing these with our estimated elasticity would require somewhat strong assumptions about the external validity of findings from other settings. Hence, we would like to benchmark the elasticity against an estimate of the aggregate effect of compulsory voting within our setting.

We use highly granular data on the composition of the electorate for the 2016 national elections at the voting booth level (within polling stations). All voters in Peru are assigned to a specific voting booth. Each voting booth is meant to have no more than 300 voters, though there is some flexibility to this rule. In our sample, 75 percent of booths have between 281 and 334 registered voters. Once a booth reaches 300 voters, new registered voters assigned to the polling station are allocated to a new booth, generating idiosyncratic variation in the age structure across booths.

Our estimate of the aggregate effect of compulsory voting exploits variation between individuals of different ages in the exposure to the mandate to vote. Voting is mandatory for citizens between the ages of 18 and 69 (both inclusive). Our identifying assumption is that voters with ages slightly above 69 are essentially identical to 69-year-old voters, except for the fact that the latter are subject to compulsory voting, while the former are not. Using information on the age of every single registered voter at each voting booth for the 2016 elections, we calculate the booth-specific shares of registered voters with each possible age, ranging from 16 to 122.[36] Our empirical strategy involves comparing voter turnout in booths with varying shares of "almost-exempt" 69-year-old voters and "barely-exempt" 70-plus voters, exploiting idiosyncratic variation across the threshold. To ensure that we are not capturing other differences correlated with the age composition of the electorate, our regression flexibly controls for the share of registered voters belonging

[36] The legal voting age in Peru is 18, but minors can vote if they emancipate from their parents.

to every other age group.[37] We also include district or polling station fixed effects. Ultimately, the richness of the data allows us to compare voting booths in the same location and that look almost identical in terms of the age composition of the registered voters, except for the fact that they have different shares of voters with ages 69 or slightly above. Pooling data from the 2016 general and runoff elections, we estimate the following specification:

$$(6) \quad turnout_{b,d,e} = \alpha_d + \gamma_e + \sum_{\tau \in \{16,\ldots,122\} \setminus \{69\}} \lambda_\tau \, share(age = \tau)_b + \epsilon_{b,d,e},$$

where the dependent variable is the turnout rate in booth b, located in district d for election e (general or runoff). The terms α_d and γ_e are district and election fixed effects (i.e., runoff). We replace the former with the more stringent polling station fixed effects as a robustness check. The variables $share(age = \tau)_b$ measure the share of registered voters in voting booth b with age τ. We include one such variable for all possible ages in the data except 69, which is the omitted category. The coefficients of interest, λ_τ, capture the change in turnout resulting from a one-unit increase in the share of voters with age τ at the expense of the omitted category. For instance, λ_{70} tells us the effect on turnout from having a voting booth including exclusively 70-year-old voters (all exempt from compulsory voting), relative to one with only 69-year-olds (all required to vote). The variable $\epsilon_{b,d,e}$ is an error term clustered at the district level. We weight observations (booths) by the number of registered voters.

The most conservative estimate of the aggregate effect of compulsory voting, very close in spirit to a regression discontinuity design (e.g., Jaitman 2013, Cepaluni and Hidalgo 2016), relies exclusively on a comparison of 70-year-old voters to 69-year-olds (i.e., λ_{70}). However, the observed difference in turnout between ages 69 and 70 may fail to fully capture the aggregate effect of compulsory voting if people adapt slowly to the exemption as a result of limited information or the force of habit. Hence, it seems desirable to compare turnout among the 69-year-olds to other nearby age groups not far from the threshold (i.e., up to ages 72 or 75). However, a decrease in voter turnout several years after age 69 could also be a reflection of a worsening of health or limited mobility. To have a benchmark for the "natural" rate of decline in electoral participation with age, we use individual-level data on voter turnout in the 2017 presidential election in Chile, a neighboring country without compulsory voting since 2012. We estimate the individual-level equivalent of equation (6) for Chile, with district and runoff fixed effects, with a dummy for voting as dependent variable.

Panel A in Figure 6 shows the estimates of equation (6) for ages 18–80. Three patterns emerge. First, voter turnout is roughly constant between the ages of 40 and 69. This suggests that compulsory voting is effectively offsetting any differential propensity to vote across this large set of ages. Second, turnout steadily drops for those under 40 and is 20 pp lower at age 20 than at age 69. This indicates that electoral abstention is coming predominantly from younger voters, consistently with our finding regarding

[37] Online Appendix Figure G1 shows the share of booths without any voters for each one-year age group between 18 and 80. Less than 10 percent of booths fail to have a voter for each age up to 62. Slightly more than 20 percent of booths fail to have a voter aged 70. This number steadily rises to 30 percent by age 75 and to 40 percent by age 80.

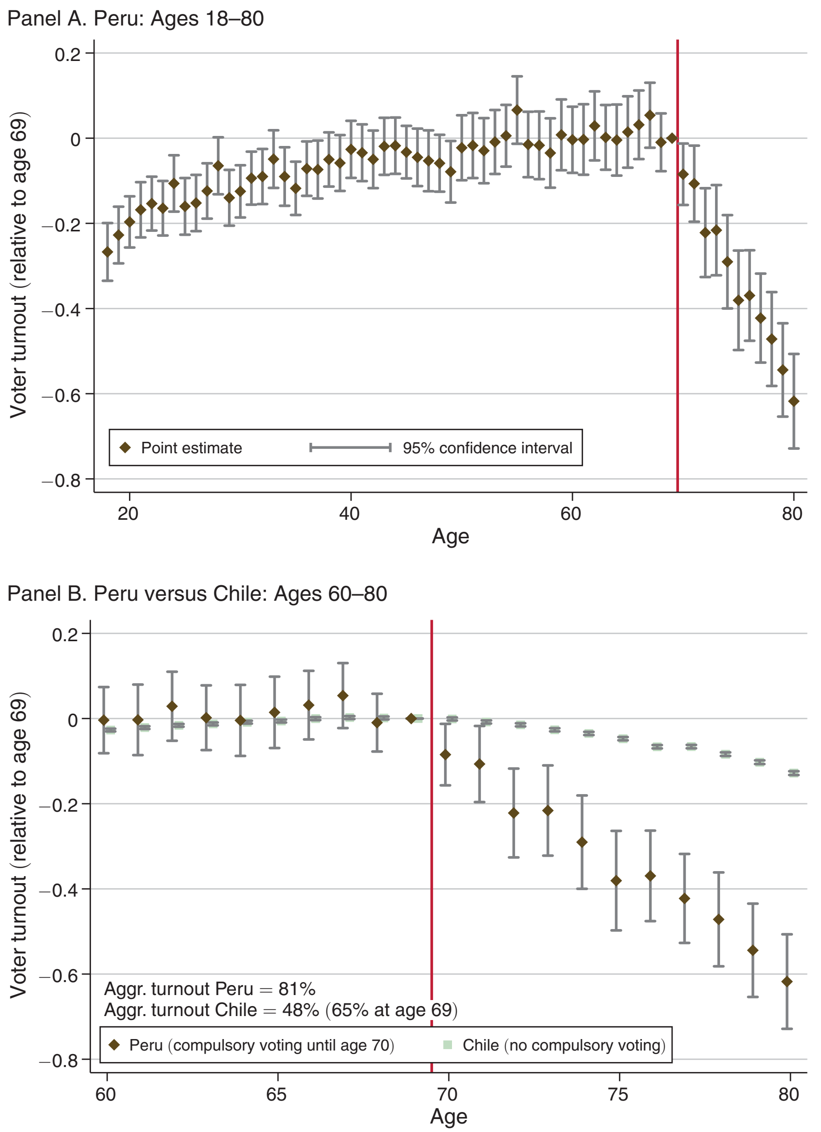

Figure 6. Senior Exemption from Compulsory Voting and Voter Turnout

Notes: Panel A shows point estimates and 95 percent confidence intervals of a regression of turnout at the voting booth level on the fraction of registered voters with each age from 16 to 122 (estimates for ages below 18 and above 80 not shown). The omitted category is the fraction with age 69. Regression includes district fixed effects and a runoff dummy. Data include the general election and presidential runoff from 2016. Sample includes 148,448 observations, 4,723 polling stations, and 1,854 districts. Standard errors are clustered at the district level. Booths are weighted by the number of registered voters. Panel B shows the same results for ages 60–80. Square markers are point estimates of an equivalent regression of individual-level turnout in the 2017 elections in Chile (presidential first round and runoff) on a full set of age dummies (estimates below 60 and above 80 not shown). Sample in Chile includes slightly more than 7 million voters.

these same voters as the ones who appear to be manipulating their registered address to avoid paying a larger fine. Third, there is a dramatic decline in electoral participation in the years immediately after age 69, unlike any other fluctuation we observe throughout the age distribution. Turnout falls by 8 pp at age 70, 22 pp at age 72, and 38 pp at age 75. Effects of this magnitude are comparable to previous findings by Fowler (2013); Jaitman (2013); and Bechtel, Hangartner, and Schmid (2018) on the impact of compulsory voting in Australia, Argentina, and Switzerland.

Given our previous findings on slow adaptation to marginal fine changes, it seems plausible that the drop in turnout between ages 69 and 75 reflects a similarly gradual response to the exemption from compulsory voting. To address concerns about the confounding effect of other factors affecting the elderly, panel B plots the results for ages 60 through 80 for Peru and Chile.[38] We fail to observe any systematic difference in electoral participation between the ages of 60 and 69 in either country. But while voter turnout plummets in Peru after age 69, there is only a smooth decline in Chile, amounting to no more than a 5 percentage point drop by age 75. Hence, it appears that we can attribute most of the sharp decline in turnout observed in Peru between ages 69 and 75 to the exemption from compulsory voting.

Columns 1–5 in Table 7 examine the robustness of these results. To facilitate interpretation, we re-estimate equation (6) aggregating the shares of registered voters with ages between 70 and 75. Column 1 corresponds to our baseline specification and shows that turnout drops 21 percentage points on average between these ages, relative to age 69. In columns 2 and 3, we see that this coefficient is hardly affected if we restrict the sample to the more homogeneous set of voting booths with close to 300 voters or if we substitute the district fixed effects with the more stringent polling station fixed effects. Column 4 shows results from a more conservative specification aggregating the shares of voters with ages 70–72 instead. Consistent with Figure 6, the turnout effect of the exemption from compulsory voting decreases but remains substantial at 13.4 pp. Column 5 shows results from another modified specification in which we weight the share of voters with each age between 70 and 75 by the number of elections without compulsory voting to which they have been exposed.[39] A one-unit increase in this new variable is equivalent to having all registered voters in that booth exposed to one additional election without compulsory voting. The results indicate that each additional election with the exemption leads to a 12.9 pp drop in the probability of voting, which is again consistent with increased adaptation.

In column 6, we study heterogeneous effects by type of election by including an additional interaction with a dummy for the presidential runoff. The results indicate that voter turnout drops 17.4 pp in the general election (the omitted category) and a further 6.9 pp in the runoff. Taken together with the also heterogeneous effect of marginal fine changes reported above, this finding indicates that voters are more responsive to both the intensive and extensive margins of compulsory voting in the presidential runoff.

Assessing the Importance of the Abstention Fine.—We now use our estimate of the aggregate effect of compulsory voting to do a back-of-the-envelope calculation

[38] Online Appendix Figure G2 shows a smooth density of the age distribution around the cutoff for both countries.

[39] For those aged 70–71, the 2016 elections were the first in which they were exempt, while 72- to 75 -year-olds had already enjoyed the exemption in 2014 (subnational). Those aged 75 were also exempt in 2011.

Table 7—Senior Exemption from Compulsory Voting and Voter Turnout

	Dependent variable: Turnout$_i$ (Mean: 0.82)					
	Baseline (1)	280–300 voters (2)	Polling station FE (3)	Share 70–72 (4)	Elections w/o CV (5)	Election type (6)
Share ages 70–75$_i$ [a]	−0.209	−0.182	−0.215			−0.174
	[0.041]	[0.034]	[0.028]			[0.043]
Share ages 70–72$_i$				−0.134		
				[0.037]		
$\sum_{j=70}^{75}$ Share age $j \times$ elections exempt$_j$					−0.129	
					[0.020]	
Share ages 70–75$_i \times \mathbf{1}$(Runoff)$_t$ [b]						−0.069
						[0.017]
Observations	148,448	109,462	148,448	148,448	148,448	148,448
Districts	1,854	1,109	1,854	1,854	1,854	1,854
Polling stations	4,723	2,725	4,723	4,723	4,723	4,723
R^2	0.19	0.16	0.08	0.19	0.19	0.19
p-value a + b = 0						0.000
District FE	Yes	Yes	No	Yes	Yes	Yes
Polling station FE	No	No	Yes	No	No	No
Election type FE	Yes	Yes	Yes	Yes	Yes	Yes
Share by age	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Data at the voting booth level for the national elections of 2016 (general and presidential runoff). The regressor of interest is the share of registered voters with ages slightly above the cutoff for exemption from compulsory voting (CV): 70–75 in all columns except column 4, where it is 70–72. Regressions include age-specific shares of registered voters for all other ages between 16 and 122. The omitted category is age 69. All columns include district fixed effects, except column 3, which includes polling station fixed effects. All regressions are weighted by the number of registered voters per booth. Column 2 shows results using only booths with 280–300 voters. The regressor of interest in column 5 is the sum of registered voters for each age between 70 and 75 multiplied by the number of elections without compulsory voting that the cohort has been exposed to. Column 6 shows heterogeneous effects for the presidential runoff. Standard errors are clustered at the district level.

and benchmark the estimated effect of the abstention fine. To ensure comparability, we employ in this calculation our elasticity estimates for 2016. The point estimate in column 4 of Table 2 shows that a complete elimination of the fine (100 percent reduction) would lead to a 4.8 percent fall in turnout, equivalent to a 3.9 pp drop from the observed 2016 turnout rate of 0.82. A reduction in turnout of this size is equivalent to 19 percent of the average fall in turnout between ages 69 and 75 shown in column 1 of Table 7.[40] The effect of a full fine reduction on voter turnout pales in comparison to that of an exemption from compulsory voting. Policy-wise, this result suggests that countries may be able to extract most of the gains from compulsory voting at a low administrative and distributional cost by only setting a moderate monetary sanction for noncompliance.

This comparison warrants several comments. First, it is likely that we are underestimating the effect of the exemption from compulsory voting. One reason is that voters slightly above the age of 70 are plausibly still affected by their exposure to mandatory voting (i.e., expressive function of the law, voting habits), which dampens the effect of the exemption. Moreover, voters around the age of 70 tend to vote

[40] Also equivalent to 46 percent of the drop at age 70, 18 percent of the drop at age 72, and 10 percent of the drop at age 75.

at the highest rates in all kinds of settings, suggesting that they are more intrinsically motivated to vote than younger voters. Voter turnout peaks around age 70 in our data for Peru and Chile (online Appendix Figure G3). A similar pattern has also been documented for the United States (Franklin 2018).[41]

Second, our comparison assumes a constant elasticity, while the turnout response to a change in the fine may vary at very low levels. For instance, any nonzero fine forces people to also incur the nonpecuniary costs (i.e., restrictions in government services). Even though the reform we study generates large differences in the value of the fine across categories, it might be better to think of our comparison as pertaining to very large (i.e., 75 percent reduction between high- and low-fine districts) but perhaps not complete fine reductions. Such a comparison is still highly policy relevant. Having said this, our elasticity may in fact be overestimating the effect of a full fine reduction if the elimination of the monetary incentive crowds in the intrinsic motivation to vote (Bénabou and Tirole 2006). Also, the elasticity we use is based on the nominal value of the fine and is less conservative than the one that accounts for enforcement (online Appendix Table C1).

Third, our back-of-the-envelope calculation could be underestimating the effect of the value of the fine if the response to the reform keeps growing over time. However, even if the elasticity were to double relative to its 2016 value, the drop in turnout from a 100 percent fine reduction would still only account for 37 percent of the estimated effect of the exemption of compulsory voting between ages 69–72. It would take an elasticity of 0.13 for the relative importance of the fine to reach 50 percent, and this would require the elasticity to keep growing at the same yearly rate observed between 2010 and 2016 for at least 10 more years.

VI. Concluding Remarks

In this paper, we study voters' response to marginal changes to the value of the fine for electoral abstention in Peru, exploiting a nationwide policy reform that affected districts differentially. We provide three main findings. First, a lower fine has a robust negative effect on voter turnout. This effect is partly driven by irregular changes in registration, as spatial variation in the value of the fine causes young voters to increasingly register in districts with lower fines. Second, the reduction in turnout spurred by a lower fine appears to have little impact on representation since it is largely matched by reductions in the number of blank or invalid votes. Third, results from a second natural experiment provided by an age-based exemption from compulsory voting suggest that monetary incentives are not the main driver behind the effectiveness of compulsory voting.

Our findings have important policy implications. Monetary incentives to vote are a rarely used alternative available to governments across the globe. Our results show that marginal changes to these incentives have a robust and nonnegligible effect on voter turnout, though additional research is needed on the potentially unequal responses to rewards and penalties. Our findings also show that voters respond in

[41] Based on the average age of voters per district, online Appendix Table B4 suggests that older voters are in fact less responsive to a larger fine (i.e., we overestimate the elasticity for the 70+ age group).

a rich, multidimensional way. Policymakers must be cautious about the unintended consequences of targeted policies. Furthermore, our results illustrate various limitations to the external validity of the findings from small-scale field experiments testing voter mobilization initiatives for large-scale policy implementation.

Our results also show that the omnibus bundle of incentives provided by compulsory voting is much more effective at increasing voter turnout than even large changes to the value of the abstention fine. Thus, making voting mandatory is a policy option worth considering if the aim is to maximize turnout. Importantly, the fine used to enforce compulsory voting can be set at moderate values without largely undermining the effectiveness of the system, while reducing the burden that this sanction imposes on nonvoters, especially the poor.[42] At the same time, our findings also speak to the broader motivations for increasing voter turnout. One such objective is to ensure appropriate representation of all citizens (Lijphart 1997). In this regard, our finding of an almost one-to-one increase in blank and invalid votes with the additional votes generated by a marginal fine increase indicates that gains in turnout achieved through extrinsic incentives may fail to affect representation or downstream policy outcomes. A larger extrinsic incentive also seems to partially crowd out the intrinsic incentive to engage with political affairs. However, one has to be cautious about extending this conclusion to settings in which large shares of the population face significant barriers to electoral participation.

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