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

Mariella Gonzales Gianmarco León-Ciliotta Luis R. Martínez*

First version: December 2018
This version: October 2019

Abstract

We study voters' response to marginal changes to the fine for electoral abstention in Peru. A smaller fine lowers voter turnout, but the effect of an exemption from compulsory voting is five times larger than that of a full fine reduction, suggesting that non-monetary incentives are the most relevant aspect of compulsory voting. We show that informational frictions limit adaptation to large-scale regulatory changes, causing our elasticity estimates to be substantially smaller than previous experimental estimates in the same setting. We find a negligible impact on representation, as 86% of the extra votes caused by a larger fine are blank or invalid.

Keywords: voter turnout, compulsory voting, voter registration, political representation, informational frictions, scale-up

JEL codes: D72, D78, D83, K16

*Gonzales and Martínez: Harris School of Public Policy, University of Chicago. León-Ciliotta: Department of Economics, Universitat Pompeu Fabra; Barcelona GSE; IPEG; CEPR. Emails: mariegonzalesn@uchicago.edu; gianmarco.leon@upf.edu; luismartinez@uchicago.edu. We would like to thank Pablo Beramendi, Peter Buisseret, Anthony Fowler, Thomas Fujiwara, Patricia Funk, Horacio Larreguy, John List, Vincent Pons, Alessandro Tarozzi and seminar/workshop participants at the University of Chicago, University of British Columbia, Universitat Pompeu Fabra, Pontificia Universidad Católica de Chile, Universidad del Pacífico, University of California - San Diego, PACDEV, NEUDC, PECA, MPSA, NOVAFRICA african development conference, Chicago-area development economics workshop, LACEA-RIDGE political economy workshop and IEB political economy workshop for valuable comments and suggestions. All remaining errors are ours. León-Ciliotta acknowledges financial support from the Spanish Ministry of Economy and Competitiveness, through the Severo Ochoa Programme for Centres of Excellence in R&D (SEV2015- 00563), grant ECO2011-25272, and RYC-2017-23172 (AEI/FSE).

1 Introduction

The effectiveness of monetary incentives has attracted the attention of economists studying a wide array of topics (Gneezy et al., 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 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 and the introduction of a mandate to vote has been endorsed by political theorists (e.g., Lijphart, 1997; Chapman, 2019) and prominent public figures, including former US president Barack Obama (The Washington Post, 2015). In this regard, improving our knowledge on the relevance of the monetary incentive provided by the abstention fine for the functioning of compulsory voting is particularly important, given the potential trade-off between the increased effectiveness of a larger fine and its greater burden on those who are sanctioned.

In this paper, we exploit a nationwide natural experiment providing plausibly exogenous variation in the value of the abstention fine in Peru, a country with more than 20 million voters that has had compulsory voting since 1931. Using granular administrative data covering four national election cycles over 16 years, we document a sophisticated and multidimensional voter response along several margins, including turnout, registration, information acquisition and electoral outcomes. We exploit a second natural experiment provided by the age threshold for the senior-citizen exemption from compulsory voting to obtain an estimate of the aggregate effect of the mandate to vote. This enables us to do a back-of-the-envelope calculation of the relative importance of monetary and non-monetary incentives for the functioning of compulsory voting. Finally, we contrast our findings with previous

[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 US.

experimental work and illustrate some important but understudied limitations of small-scale field experiments in political economy as blueprints for large-scale public policies.

Until 2006, the value of the abstention fine was homogeneous throughout Peru. Following that year's national elections, a reform classified districts into three categories (high, medium and low fine) and differentially reduced the value of the abstention fine. Using a difference-in-difference strategy, we show that a larger fine has a robust positive effect on voter turnout. On average, a 10 Peruvian Sol [S/] fine increase (approximately US$3) leads to a 0.5 percentage point (pp) increase in turnout, with a corresponding elasticity of 0.03. Since these estimates are based on district-level variation, they incorporate potential spillover effects from real or presumed changes in the behavior of peers (e.g., Nickerson, 2008), which would not be possible with variation at the individual level. Although imperfect enforcement could lead the effective fine to be smaller than its nominal value, we find that accounting for limited enforcement has only a moderate effect on the magnitude of these estimates.

This average effect masks a highly heterogeneous response along several dimensions. The effect of a same-sized fine change is more than three times larger in the second election after the reform in 2016 than in the first one in 2011, consistent with increased adaptation over time. This larger response leads to a sizable 5 pp turnout gap between high- and low-fine districts in 2016. The marginal effect of the fine is also almost 50% larger in the presidential run-off than in the general election taking place only two months earlier, suggesting differences in the marginal voters across election types. We also find that the response in turnout is more pronounced for 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 marginal effect of the fine on turnout into the separate channels of voter registration (selection) and the propensity to vote (behavioral response). 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 provided by the reform creates an incentive for low-turnout voters to strategically misreport their address to 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 under reasonable assumptions concerning the turnout rates for these voters, the changes in registration explain 37-47% of the overall effect of the fine on turnout. The remaining 53-63% corresponds to

the behavioral response in the likelihood of voting, conditional on registration.

León (2017) reports results from a field experiment providing information on the modified value of the abstention fine to voters in several districts in the Lima region (Peru) in 2010. A comparison of our turnout elasticity of 0.03 to that of 0.22 reported by León reveals substantial 'voltage drop' in the effect of the large-scale policy (Al-Ubaydli et al., 2017). We hypothesize that informational frictions concerning the modified policy incentives are an important determinant of the reduced effect, based on León's finding that voters remained highly unaware of the modified value of the abstention fine several years after the initial reform. To examine this hypothesis, we construct a monthly panel of queries originating in Peru for 44 different search terms in the Google search engine between 2005 and 2016. A subset of these queries are related to the abstention fine (e.g., "fine for not voting"). Using a complementary difference-in-difference research design, we find that the relative frequency with which people search the web for information about the fine steadily increases after the reform and is particularly large in later years. This result indicates that people are imperfectly informed about the fine and endogenously 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.

Theory predicts that in a setting with few barriers to electoral participation abstention is likely driven by the uninformed or uninterested (Feddersen and Pesendorfer, 1996). Hence, changes to the abstention fine should draw voters who are arguably more likely to cast a blank or invalid vote. Our data shows that a S/ 10 fine increase leads on average to a 0.27 pp increase in the share of blank votes and to a 0.1 pp increase in the share of invalid votes in the presidential first round. We measure these shares relative to the number of registered voters, making them directly comparable to the effect on turnout for this type of election (0.43 pp). These results imply that for every ten extra votes caused by a marginally larger fine, there is an almost nine-vote increase in the number of blank or invalid votes. While we cannot rule out that these votes partly correspond to mistakes by the less educated (Fujiwara, 2015) or to a form of political protest (Ujhelyi et al., 2019), we can conclude that the induced change in electoral participation has a negligible impact on representation.

The pecuniary incentive provided by the abstention fine is only one of several incentives to vote provided by compulsory voting. These include the restriction on government services faced by non-voters and the expressive function of the law as a signalling 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 composition of the electorate in 2016, exploiting the exemption from the mandate to vote for citizens with ages of 70 or more. To solve the 'ecological inference' problem (Cho and Manski, 2009), we compare turnout rates in voting booths with a higher share of voters with ages slightly above 69 to those with a higher share of 69 year-olds, while flexibly controlling for the age structure of all other voters. This comparison takes place within the same district or polling station. We find that the senior-citizen 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, suggesting increased adaptation to the exemption over time. To ensure that we are not picking up a worsening of health and mobility among the elderly, we use individual-level turnout data from Chile (a neighboring country without compulsory voting) to show that the natural decline in participation among the elderly leads to only a 5 pp drop in turnout between ages 69-75.

If the abstention fine was the only reason why compulsory voting affected turnout, we should observe that the effect of a full fine reduction roughly coincides with the effect of the exemption from compulsory voting. However, a back-of-the-envelope calculation using our elasticity estimates for 2016 yields that a 100% fine reduction leads to a drop in turnout only 18% as large as the one caused by the exemption from compulsory voting between ages 69-72. This indicates that the non-monetary incentives are the main drivers behind the effectiveness of compulsory voting. The conclusion does not fundamentally change if we take into account the probability of enforcement, plausible growth of the elasticity in the future, or the specific age-group for which we estimate the aggregate effect of compulsory voting. The ensuing policy implication is that compulsory voting with moderate fines can substantially reduce the burden on non-voters without incurring large losses in effectiveness.

This paper contributes to the vast literature studying voter turnout.[2] One strand of this literature has studied the effects of extrinsic incentives such as bad weather (Hansford and Gomez, 2010), distance to the polling station (Brady and McNulty, 2011; Cantoni, 2019), registration costs (Holbein and Hillygus, 2016; Braconnier et al., 2017) and convenience voting (Hodler et al., 2015; Kaplan and Yuan, 2018). Only Panagopoulos (2012) and León (2017) have studied through field experiments the effects of monetary incentives of different sizes.[3] Our first contribution is to document a positive turnout effect of marginal monetary incentives 'in the wild'.[4] More broadly, we connect the literature on extrinsic incentives with

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

[3]Loewen et al. (2008) and Shineman (2018) provide monetary incentives of a fixed size as part of experiments studying political participation and knowledge. Concurrent work by Carpio et al. (2018) exploits our same reform to study the effect of voter turnout on the party affiliation of candidates in municipal elections.

[4]Our findings on the irregular changes in voter registration constitute new evidence on the unintended consequences of targeted policies (Camacho and Conover, 2011; Cassan, 2015). Unlike vote- or voter-buying

a separate strand of the turnout literature studying intrinsic incentives such as voters' sense of civic duty (Gerber et al., 2008), habit formation (Coppock and Green, 2016; Fujiwara et al., 2016) and social image concerns (Funk, 2010; Dellavigna et al., 2017). Our findings indicate that rich psychological considerations, such as the unwillingness to contravene the law, significantly outweigh material costs in the calculus of voting.

Another large strand of the literature on electoral participation 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 connects 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; List et al., 2019; Vivalt, 2019). Our findings of heterogeneous effects by election type, time horizon or income level illustrate the potential for context dependence in small experiments. Additionally, while previous work has mostly worried about changes in the implementation of development programs at a larger scale (Davis et al., 2017; Bold et al., 2018), we shed light on informational frictions as a major hindrance to adaptation and a source of voltage drop in the response to large-scale regulatory changes.[5]

Our paper also complements the empirical literature on compulsory voting (Funk, 2007; Fowler, 2013; Jaitman, 2013; Cepaluni and Hidalgo, 2016; Hoffman et al., 2017; Bechtel et al., 2018). Previous research has largely focused on the effects of the introduction or elimination of compulsory voting laws on turnout and downstream outcomes. 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 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. Hence, non-monetary incentives are the main drivers of the effectiveness of mandatory voting.

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. In this regard, our results lend empirical support to models of rational

(Nichter, 2008), they correspond to a previously unknown form of voter misbehavior.

[5]Our results on information acquisition also relate to a small literature studying information spillovers of voter mobilization efforts (Chong et al., 2019; Fafchamps et al., 2018; Giné and Mansuri, 2018). While previous studies directly provide information through salient interventions, we document the endogenous acquisition and slow diffusion of information about modified policy incentives in the wild.

abstention (Feddersen and Pesendorfer, 1996).

2 Institutional Background

General elections in Peru, encompassing the first round of the presidential election and multi-district 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.[6] In the presidential election, a candidate must obtain at least 50% of the votes nationwide to win in the first round, which never happens during our sample period. As a result, a run-off election between the two leading candidates takes place approximately two months after the general election.[7] Voter turnout has been traditionally high and remained above 80% throughout the sample period (see 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, DNI (Documento Nacional de Identidad), when they turn 18 years old. The DNI includes the person's home address and it must be renewed every eight years (up to the age of seventy) to ensure that the information remains up to date. 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).[8] 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.

Voting is compulsory for citizens between the ages of 18 and 69 (both inclusive) since 1933. Voting is done in person at pre-determined polling stations and voters are provided with a sticker on their DNI as proof of participation.[9] 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 a valid excuse.[10] This is similar to other

[6]The regions are the highest-level subnational division and include 23 departments and two special provinces that share the same status. Regions are further divided in 198 provinces and 1854 districts.

[7]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.

[8]There is a separate underage version of the DNI for citizens under the age of 18. According to the 2017 ENAPRES national household survey, 99.3% of the population has a DNI (INEI, 2018).

[9]Voting usually occurs undisturbed and waiting times are short. In 2016, the average (median) number of polling stations per district was 17 (10) and the average (median) number of voters per polling station was 4,662 (3,535). Within polling stations, the average (median) number of voters per voting booth was 297 (296). Voters in Lima and Callao were allowed to choose their polling station for the first time in 2016.

[10]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

countries with compulsory voting (Cepaluni and Hidalgo, 2016). Fines accumulate, but failure to settle an outstanding fine does not prevent someone from voting in the future. 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 after paying a processing fee of about S/21 (US$6.4).[11] All restrictions are lifted once the fine has been settled (i.e., paid or excused).

Until 2006, the fine for not voting was the same for all voters in all districts, set at 4% of an official reference unit known as UIT.[12] At the start of 2006, the UIT was set at S/3,400 (approximately US$1,040) and the corresponding value of the fine was S/136 (roughly US$42).[13] Shortly after the national elections of 2006, Congress approved a law that reformed the abstention fine. The law classified districts into three categories based on their level of poverty: high, medium and low fine.[14] All voters experienced a reduction to the value of the fine, but those registered in districts in the latter categories enjoyed larger reductions. For voters in high-fine districts, the fine was cut in half to 2% of the UIT, while for those in districts classified as medium- and low-fine, the monetary sanction was set at 1% and 0.5% of the UIT. These amounts roughly corresponded to US$25, US$12.5, and US$6.

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% 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) remains unclear.[15] However, the only elections to be held under this classification were the subna-

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.

[11]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.

[12]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.

[13]The average value of the official exchange rate in 2006 was S/3.27 per US$1. We use this value for all calculations in the paper. The average exchange rate for the period 2001-2016 was quite similar, at S/3.12.

[14]Districts classified as 'non-poor' were assigned a high fine, while those classified as 'poor' received a medium fine and the 'extreme poor' got a low fine. For the remainder of the paper, we refer to districts classified as non-poor, poor and extreme poor as high-fine, medium-fine and low-fine, respectively.

[15]See Resolución 4222-2006-JNE from October 27, 2006. Despite having all relevant social and economic

tional elections of November 2006. Shortly before the 2010 subnational elections, the JNE released a new district classification, which still remains in place.[16] 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 assigned a high fine (non-poor), 18% were assigned a medium fine (poor) and 30% a low fine (extreme poor).[17] To the best of our knowledge, this classification has not been used for any other public policy. We verify below that the results are not driven by idiosyncratic changes in voter turnout in poorer or richer areas.

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 had reduced and segmented the fine and the districts had been re-classified. Figure 1 also shows that the value of the fine in low-fine districts was almost identical to the minimum daily wage in 2011 and was slightly below it in 2016. The value of the fine in each category has remained constant as a percentage of UIT since the 2006 reform and the observed variation between 2011-2016 is driven by the yearly adjustment of the reference unit.

Enforcement of the fine was traditionally moderate. Fines normally expire after four years and the national government often provides amnesties, thereby dissuading debtors from settling outstanding fines. Roughly 20% of the 4.7 million fines issued in 2011 were settled (see Appendix figure A2). However, enforcement improved substantially in 2012, when a collection unit was created within the JNE.[18] As a result, almost 50% 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.

indicators available at the time, we have not been able to replicate this classification. We have communicated with officials at several government agencies and they have not been able to elucidate this issue either.

[16]See Resolución 2530-2010-JNE from October 1, 2010.

[17]Appendix Table A1 shows that 10.4% of districts were assigned a high fine, 43.4% a medium fine and 46.2% a low fine in 2006. All districts initially classified as high-fine in 2006 remained in this category in 2010. On the other hand, only 15% and 56% of districts initially classified as medium- and low-fine remain in the same category in 2010.

[18]See Resolución 0738-2011-JNE from October 20, 2011. 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 do not expire after four years if a collection process is under way. JNE (2015) reports that 42% of fine payments between 2012 and 2015 resulted from coercive collection. In 2015, 45,840 collection processes were opened, leading to 5,155 instances of bank accounts being frozen.

3 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 strategies for the analysis of online information acquisition and the exemption from compulsory voting for the respective sections below.

3.1 Data

We use administrative data for the national elections in 2001, 2006, 2011 and 2016 from the national office for electoral processes (Oficina Nacional de Procesos Electorales, ONPE). The data covers the general election, combining the legislative election with the first round of the presidential race, and the presidential run-off taking place two months later. The data includes the number of registered voters, the number of votes cast and the number of invalid and blank votes in each election by district. The value of the abstention fine and the assignment of districts to the different fine categories is publicly available in resolutions issued by JNE before each election. Our main sample includes 1,755 districts, corresponding to 94% of the total number of districts and covering more than 96% of the almost 23 million registered voters in 2016.[19]

JNE provided 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. We also obtained from ONPE and JNE district-level information on registered voters for six age groups (18-20, 21-29, 30-35, 36-50, 51-75, 75+) for all election cycles, 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).

3.2 Research Design

We aim to estimate the causal effect of the value of the abstention fine on voters' behavior along several margins. For this purpose, we exploit plausibly exogenous variation in the value of the fine stemming from the differential reduction across districts after 2006. Variation at

[19] After excluding districts with missing data, we are left with 1,769 districts out of the 1,854 in the country. The districts with incomplete data are predominantly new ones that were created during the sample period. We exclude another four districts that were not assigned to a fine category in 2006, but existed at the time, as well as ten others that changed category in 2014 and reversed to the previous assignment in 2016.

this higher level provides a unique opportunity to capture direct and indirect effects caused by changes in the behavior of peers, which would not be possible with individual-level variation.

The natural experiment we exploit lends itself naturally to a difference-in-difference analysis with district and time fixed effects.[20] 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 remains unclear). Additionally, even if the 2006 assignment is uninformative, it was in place for four years and may have 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 the different elections 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 x province x 2006 category' fixed effects.[21] 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 in different categories, but that are located in the same province and were initially assigned to the same category in 2006. To provide evidence of parallel trends in the pre-reform period, we first estimate the following event-study model:

$$\mathrm{y}_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \sum_k \sum_t \beta_{k,t} \left[\mathbb{1}(e = t) \times \mathbb{1}(\mathrm{c}_{10} = k)\right] + \epsilon_{d,p,e} \tag{1}$$

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 'election x province x 2006 category' fixed effect. The other terms correspond to a full set of interactions between dummy variables $\mathbb{1}(\cdot)$ for each election date t in the sample period (e.g. 2011 presidential run-off) and respective dummies for each fine category k from the 2010 assignment (c_{10}). The omitted election is the 2006 presidential run-off (last national election before the reform) and the omitted category corresponds to medium-fine districts. $\epsilon_{d,p,e}$ is an error term that we cluster at the province level (192 clusters) to allow for arbitrary correlation within provinces, including spatial autocorrelation.

[20] Carpio et al. (2018) 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 event-study estimates.

[21] Our estimation effectively drops 63 districts lacking at least one other district from the same province with the same assignment in 2006 in order to avoid having singleton groups (Correia, 2015). 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 from 1,755 districts. We verify below that the results are robust to using the less conservative election x province fixed effects.

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 run-off (omitted election), conditional on the set of fixed effects. Those coefficients corresponding to elections before the reform in 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 in 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 in order to keep the selection and behavior of local candidates and incumbents constant and focus on voter behavior. Hence, our empirical strategy always involves comparing voters with different incentives to turn out, but faced with the same set of candidates.[22] By excluding subnational elections we also shut down the potential effects of the reform on voter buying (Hidalgo and Nichter, 2016).[23] There is no incentive to engage in this practice in single-district presidential elections.[24]

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

$$y_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \nu \text{ Fine value}_{d,e} + u_{d,p,e} \tag{2}$$

where $\text{Fine}_{d,e}$ 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 in the difference-in-difference specification. 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 increase 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. $u_{d,p,e}$ is an error term clustered again at the province level, which flexibly accommodates serial and spatial correlation of the error term across districts within provinces. We verify below that the results are robust to clustering at the region level, which is even more generous in allowing for spatial correlation, but forces us to use a wild cluster bootstrap procedure to account for the small number of clusters (Cameron et al., 2008). We weight all our regressions by the number of registered voters in 2001 to capture average effects at the voter level.

[22] Even though the pool of candidates varies across regions in legislative elections, our empirical strategy only involves comparisons within the same province (and 2006 category), which is smaller than the region.

[23] The irregular movement of voters across districts is a common clientelistic practice in Peru and these voters are known as 'swallows' ("votantes golondrinos"). Resolución 1400-2006-JNE mentions abnormal increases in the number of registered voters for the 2006 subnational elections in various districts. News reports claimed there were at least 100,000 'swallow voters' for the elections in 2018 (La República, 2017).

[24] Although candidates in legislative elections may benefit from moving voters across regions, they face a high cost and a low payoff due to transport costs and the large number of voters per region.

4 The Value of the Abstention Fine and 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 provide evidence of heterogeneous impacts. Next, we examine the sensitivity of the results to variation (across districts and over time) 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.

4.1 Main Results

Figure 2 shows point estimates and 95% confidence intervals of $\beta_{k,t}$ in equation (1).[25] 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 run-off (omitted election). The abstention fine in high-fine districts is twice as large as in medium-fine districts and four times as large as in low-fine districts after 2006. 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, lending support to the hypothesis that voter turnout followed parallel trends across all categories between 2001 and 2006. These results increase our confidence in attributing any subsequent relative change in turnout to the reform to the abstention fine. In the period after the reform, we observe a systematic divergence in turnout among the three groups. As expected, high-fine districts show a steady relative increase in turnout, while low-fine districts show a steady relative decrease. Turnout increases 1.3 percentage points (pp) in the high-fine category in 2011 relative to the omitted category, but the magnitude of the effect is much larger in both directions in 2016, indicating a growing impact of the reform over time. In this year, voter turnout in high-fine districts was 2.4 and 3.0 pp higher than in medium-fine ones in the general and run-off 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.[26] There is a 5.4 pp gap in turnout between the high-fine and low-fine districts in the 2016 presidential

[25] Appendix Table A2 shows the corresponding estimates. Appendix Figure A3 shows results from a more disaggregate specification that estimates separate coefficients for each combination of 2006/2010 assignments.

[26] These results are consistent with the RDD estimates of 2.1/2.7 pp reported by Carpio et al. (2018) for the difference in turnout between high- and medium-fine districts in the subnational elections of 2010/14.

run-off. This is a sizable effect, comparable to that of some of the most effective voter mobilization initiatives that have been studied (Green et al., 2013), and which could lead to inequality in representation across districts. Figure 2 also provides preliminary evidence of a heterogeneous effect across election types. We formally test this hypothesis below.

Panel A in Table 1 presents estimates of equation (2), where we evaluate the effect of marginal changes to the abstention fine on voter turnout. The estimate of ν in column 1 implies that a S/10 increase in the value of the fine (roughly US$3) leads to an increase in turnout of about 0.5 percentage points. This corresponds to a 0.58% increase over the sample mean of 0.85. Column 1 in Panel B shows the corresponding estimate of the elasticity. We estimate an average elasticity of turnout with respect to the value of the fine of 0.03. Both coefficients are very precisely estimated and are statistically significant at the 1% level.[27] As mentioned above, these estimates incorporate potential spillovers from changes in the behavior of peers and general equilibrium effects related to voters internalizing the fact that other voters in their district also face a modified incentive.

The remaining columns show that the results are robust to changes to the specification or to the introduction of additional controls. The estimates are hardly changed when we use the less conservative province-election fixed effects in column 2. Columns 3-5 examine the possibility that the results are confounded by time-varying differences across districts. In column 3, we allow turnout to vary flexibly in each election by the fixed share of people classified as poor or extreme poor. This way we ensure that the results are not confounded by idiosyncratic changes in turnout across levels of income or by changes in other targeted social policies. After adding these controls, we observe a 30% reduction to the marginal effect on turnout in panel A and a 20% reduction to the elasticity, but the coefficients remain positive, precise and of the same order of magnitude. We cannot reject that they are equal to the baseline estimates in column 1 at conventional levels. Column 4 includes as controls the time-varying shares of voters with primary, secondary and higher education per district to examine whether the estimates are biased by differential changes in the composition of the electorate over time.[28] Controlling for these educational attainment shares leads to somewhat larger estimates. Finally, column 6 includes the log number of polling stations as an additional control, allowing us to test for confounding changes in other determinants of the cost of voting (Brady and McNulty, 2011; Cantoni, 2019). The results are hardly affected.[29]

[27] Appendix Table A5 shows that the results are unaffected if we cluster the error term at the region level in order to more generously account for spatial correlation.

[28] This information 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).

[29] The results are robust to other measurements (i.e., level or ratio relative to registered voters or area).

4.2 Heterogeneous Effects

Table 2 shows results from extensions of equation (2) that include interactions with other variables to study potential heterogeneity in the marginal effect of the fine. Columns 1-3 consider heterogeneity in the level effect, while columns 4-6 look at the elasticity.

In column 1, we introduce the interaction with a dummy for the elections in 2016 (general and run-off). The omitted category corresponds to the elections in 2011, as the value of the fine was homogeneous across all districts before then. Consistent with the evidence in Figure 2, we observe a substantially larger effect in the longer term. A same-sized increment to the fine leads to an increase in turnout that is more than thrice as large in 2016 than in 2011. The implied elasticity of turnout with respect to the fine is 0.011 in 2011 and 0.048 in 2016 (column 4). This increasing response over time is consistent with gradual learning about the modified policy incentives. We explore this mechanism in greater detail in section 4.5. 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 non-voters in subsequent elections (Coppock and Green, 2016; Fujiwara et al., 2016).

Column 2 explores whether the marginal effect of the fine on turnout varies depending on the type of election. For this purpose, we include an interaction between the value of the fine and an indicator for the presidential run-offs that took place in June 2011 and 2016. The omitted category corresponds to the general elections from April in those same years. We find that the marginal effect of the fine is almost 50% larger in the run-off than in the general election, jumping from 0.39 pp to 0.58 pp for a S/10 increase. Similarly, column 5 shows an elasticity of 0.023 for the general election and 0.037 for the run-off. The fact that we observe a non-negligible effect in the general election is important because it ensures that we are not just picking up differential abstention in response to changes in the pool of presidential candidates over time. The heterogeneous effect is unlikely to be driven by increased learning about the reform, given that the two elections are held less than two months apart.[30] 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 run-off, which is consistent with

[30] Appendix Figure A4 provides separate estimates of the marginal effect of the fine for each individual election in 2011 and 2016. We obtain these coefficients by including interactions of the fine with a full set of election-specific dummies in equation (2). The results show a steady increase in the marginal effect of the fine over time. In the 2016 presidential run-off, a S/10 fine increase leads to a 0.8 pp increase in voter turnout (elasticity of 0.06). The differences in the marginal effect of the fine (or the elasticity) across election types are statistically significant even within the same year.

the larger aggregate turnout we always observe in the former (Appendix Figure A1).[31] 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 than the presidential election or (iii) the larger number of presidential candidates.[32] Voters may also face stronger extrinsic incentives to vote in the general election in the form of pressure from local brokers working for congressional candidates.

Columns 3 and 6 examine potential heterogeneity in the marginal effect of the fine and the elasticity depending on the share of district residents below the poverty line. The omitted category in this case is the share of non-poor. The baseline estimate is statistically indistinguishable from zero, indicating a null response by this group. The interactions with the shares of poor population are positive and large in both columns. For the average poor person, a S/10 fine increase leads to a 0.5 pp increase in turnout (elasticity of 0.04). In both cases the net effect is statistically different from zero at the 1% level. These results indicate that it is turnout by the poor, for whom a marginal fine increase represents a greater economic burden, that is most responsive to a larger fine.

Overall, these heterogeneous effects 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; Muralidharan and Niehaus, 2017).

4.3 Enforcement of the Fine

The correct interpretation of the marginal effect of the fine must take into account the probability of enforcement. While imperfect, we can approximate the strength of enforcement using the share of fines that are settled (i.e., paid or excused). By July 2018, 38% of fines issued for all elections between the subnational elections of 2006 and the presidential run-off of 2016 had been settled.[33] Focusing on national elections, we find that the percentage of settled fines grew from 22% in 2011 to almost 50% in 2016 (Appendix Figure A2).

Although these figures are far from negligible, sophisticated voters may take into account

[31] This idea can be easily formalized. Assume that voters derive an expressive benefit from voting (e.g., Dellavigna et al., 2017) that is larger in the general election than in the presidential run-off. Voters also face a cost of voting that includes a deterministic component (decreasing in the abstention fine) and a stochastic component (e.g., weather shocks). In this environment, a threshold rule for the random shock will determine electoral abstention and the threshold will be higher for the general election (i.e., different marginal voters). If the probability of more extreme realizations of the shock is decreasing, a same-sized increase to the abstention fine (hence, a same-sized increase to the threshold) will have a smaller effect on turnout in the general election due to the smaller number of voters it affects at the margin.

[32] Appendix Table A4 tests for the latter possibility and finds no evidence of a heterogeneous effect in the run-off depending on the first-round vote share of the candidates progressing to the final stage.

[33] Unfortunately, reliable information on fine settlement is unavailable for earlier elections.

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.[34] In Appendix Table A7, we incorporate the probability of enforcement into the analysis by adjusting the value of the fine by the share of fines settled in the current or previous election.[35] As expected, the estimated effect of the fine on turnout increases by as much as 45% (i.e., 0.71 pp increase for a S/ 10 higher fine). Theoretically, the estimate for the elasticity should not be affected as long as enforcement is constant across districts. In practice, the elasticity estimates if anything decrease once we account for enforcement.

The overall increase in settled fines seems caused by the 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 is there evidence suggesting that the fine categories were actively used for this purpose. It is further reassuring that our previous estimates show a positive effect of the fine on turnout in 2011 (i.e., column 2 in Table 2), before the changes to enforcement took place, as this indicates that we are not just picking up their potentially confounding effect. Still, the aggregate data shows that the increase in settled fines was mostly concentrated in high-fine districts (Appendix Figure A2), which suggests that our finding of a substantially larger effect in 2016 could be compromised. Appendix Table A8 shows results from a series of robustness tests based on information about the districts targeted by the collections unit and the variation in the share of fines settled.[36] All of our predictors of improved enforcement after 2012 are positively correlated with voter turnout in 2016. However, inclusion of additional controls or exclusion of targeted districts leads to a reduction of no more than 20% in the magnitude of our estimated long-run effect of the value of the fine.

[34]If peoples' voting decision is driven by the effective fine rather than its statutory value (i.e. multiply the value of the fine in equation (2) by the probability of enforcement ρ), then $E[\hat{\nu}]$ equals $\rho\nu < \nu$.

[35]Naturally, the share of fines that are paid is itself affected by the value of the fine. Appendix Table A6 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.

[36]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 construct separate indicators for targeted districts in Lima and Callao and for provincial capitals. We also consider a more agnostic, catch-all 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.

4.4 Changes in Registration and the Propensity to Vote

In this section we disaggregate the marginal effect of the abstention fine on voter turnout into separate effects on the number of votes (numerator) and the number of registered voters (denominator). This analysis allows us to establish whether the observed effect of the fine on turnout is partially driven by increased registration in low-fine districts by voters with a low propensity to vote (i.e., a selection effect) rather than by an actual behavioral change in the propensity to vote within a given location. It also allows us to examine potential unintended consequences of the geographically-targeted abstention fines put in place by the reform.

As mentioned in section 2, 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.[37] We begin by examining whether the differentiated fine across districts 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 run-off. 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 identification strategy. After the reform, we observe a disproportionate increase in the number of voters in low-fine districts. Specifically, voter registration grows around 4.4% more in these districts than in medium-fine ones in 2011 and 6.1% more in 2016. The difference with high-fine districts is even starker, at 5% in 2011 and 8.2% in 2016.[38] These differences are all statistically significant at the 1% level. High-fine districts show a decline in the number of registered voters relative to medium-fine ones, but it is small and insignificant (-0.7% in 2011, -2.1% in 2016).

These results are consistent with intentional manipulation of voters' reported address in order 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 at least two visits to the office of the national registry (RENIEC). We only find this mechanism plausible in the case of young adults that reach voting age (18) and must apply for a new DNI, thereby having to pay the application cost anyways. Additionally, young adults are

[37] Address misreporting is punished with a fine equivalent to 0.3% of the UIT, which is slightly less than the value of the abstention fine in low-fine districts (El Comercio, 2015). Although misreporting is rarely investigated per se, the authorities made increased efforts to detect instances related to voter-buying during the sample period (see footnote 23).

[38] See Appendix Table A2 for the corresponding estimates.

likely to be living with their parents or relatives, making it easier to avoid providing a proof of address to their name. To test this hypothesis, columns 2-7 in Table 3 provide separate estimates of equation 2 for the log number of registered voters in six different age groups. We first report in column 1 an average elasticity of -0.045 for this sample, which excludes the year 2006 due to lack of data. We find 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-group, which is six times larger than the average effect. These results are robust to multiple further tests.[39]

The evidence thus shows that young voters are strategically responding to the spatially-differentiated fine for abstention by changing their reported address to low-fine districts. This type of voter misbehavior lacks the political motivation driving the better-known phenomena of vote- or voter-buying (Hidalgo and Nichter, 2016), but could affect youth representation (Bertocchi et al., 2017). It could also have further detrimental effects on electoral participation through habit formation (Coppock and Green, 2016; Fujiwara et al., 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). Importantly, small field experiments will usually struggle to capture such a response, either because incentives are provided at the individual level or because the short duration of the study does not allow sufficient time for registration effects to materialize.

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 disaggregate the net turnout effect into separate effects on the number of votes and the number of voters:

$$\text{Turnout (T)} \equiv \frac{\text{Votes (V)}}{\text{Registered voters (R)}} \Rightarrow \ln T = \ln V - \ln R$$
$$\Rightarrow \frac{d \ln T}{d \ln F} = \frac{d \ln V}{d \ln F} - \frac{d \ln R}{d \ln F} \quad (3)$$

Table 4 provides estimates of the two marginal effects in the right-hand-side of equation (3).

[39] Appendix Table A9 shows that the results are unaffected if we control for log predicted voters by age group based on the 2007 census. Hence, the results are not confounded by predictable changes in the number of voters dating back several years (e.g., differential birth rates in the 1990s). Appendix Table A10 further shows that the value of the fine is uncorrelated with nighttime luminosity and with the share of respondents in the ENAHO survey that report living in their district of birth, helping us rule out changes in economic conditions or actual migration as the underlying mechanism. Finally, Appendix Table A11 shows that the results are robust to controlling for district-specific changes in the share of ENAHO survey respondents that report having a DNI. Hence, the results are not driven by differential changes in DNI demand or supply. One final possibility is that the reform provides a stronger incentive to young adults (i.e., students) in low-fine districts to update the address in their DNI (Braconnier et al., 2017). This seems highly unlikely given that (i) only 2% of 18-20 year-olds live outside their parent's household, (ii) an even smaller percentage migrates for educational purposes, (iii) universities tend to be located in larger and richer cities.

Column 1 shows that the elasticity of registration in the full sample (-0.046) is very similar to the previous estimate excluding 2006 (-0.045), while column 2 shows that the elasticity increases from -0.035 in 2011 to -0.057 in 2016. As with turnout, the larger long-run elasticity indicates 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, indicating that the mechanical effect leading from fewer voters to fewer votes is being offset by the increasing propensity to vote by those voters that do not change district in response to a larger fine. Column 4 shows that 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). Given that the effect on registration is also larger in 2016, this result indicates that the behavioral response in the propensity to vote is growing as well.

Using these estimates, we can better understand the contribution of changes in voter registration and the propensity to vote to the net effect on turnout if we further decompose the elasticity of votes. For this purpose, we assume that log votes is a function of the log value of the fine and of log voters, which is itself also a function of the fine:

$$\begin{aligned} &\ln V = \ln V(\ln R(\ln F), \ln F) \\ \Rightarrow &\frac{d \ln V}{d \ln F} = \underbrace{\frac{\partial \ln V}{\partial \ln R}}_{\substack{\text{Turnout}| \\ \Delta\text{registration}}} \frac{d \ln R}{d \ln F} + \underbrace{\frac{\partial \ln V}{\partial \ln F}}_{\substack{\text{Behavioral} \\ \text{elasticity}}} \end{aligned} \tag{4}$$

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% increase in the number of registered voters, all else equal). This is equivalent to voter turnout among those that change their address of registration, which 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% increase in the value of the fine, conditional on registration). 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 voter turnout with respect to the value of the fine. 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).[40] If movers vote at the average rate observed

[40]Intuitively, if movers never vote, their change of address will not affect the number of votes (i.e., the full

in 2011/16 of 83%, Figure 4 shows that the resulting behavioral elasticity of turnout is 0.022, corresponding to 73% of that of turnout. However, it seems likely that movers vote at lower-than-average rates. If their turnout is one standard deviation below the average (77%), the behavioral elasticity becomes 0.019, while if their turnout is two standard deviations below the average (71%), the behavioral elasticity takes a value of 0.016. These seem reasonable bounds, as the assumed turnout rates correspond to the 13th and the 6th percentiles of the turnout distribution across districts in 2011/16. The resulting behavioral elasticities correspond to 63 and 53% of the turnout elasticity, meaning that 37-47% of the effect of the fine on turnout can be attributed to the change in registration by low-turnout voters.[41]

4.5 Previous Experimental Estimates and Voltage Drop

In this section, we compare our estimates of the marginal effect of monetary incentives on voter turnout to the ones provided by previous experimental studies and argue that informational frictions to adaptation lead to a smaller impact of the large-scale policy (voltage drop). We use data from web searches to provide evidence on the gradual and endogenous acquisition of information about the abstention fine in Peru following the reform.

Only two previous studies, both involving field experiments, have estimated the marginal effect of monetary incentives on voter turnout. Panagopoulos (2012) exploited a quirk in California state law allowing him to directly provide a monetary incentive to vote to randomly-chosen voters in two local elections in 2007 and 2010. He estimates that a $1 incentive leads to a 0.15 pp increase in turnout, which corresponds to 0.46 pp for a S/10 incentive.[42] León (2017) used a field experiment to analyze the same reform we study, providing a unique opportunity to compare experimental and non-experimental results in the same setting. After showing that there was a large misperception among voters with respect to the value of the abstention fine, León provided information in-person about its modified value to a random subset in ten districts in the Lima region. Examining turnout in the 2010 subnational elections, he estimates that a S/10 increase in the perceived value of the fine leads to a 1.7 pp increase in turnout, with an implied elasticity of 0.22.

Our estimate of an average turnout increase of 0.49 pp for a S/10 larger abstention fine,

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.

[41] As an alternative exercise, in Appendix Table A12 we re-estimate equation (2) including log registered voters as an additional control. Even though log voters fits the description of a 'bad control' in this regression (Angrist and Pischke, 2009), the sensitivity of the turnout elasticity to the additional control can prove informative about the mediating effect of voter registration. The results show that the turnout elasticity drops as much as 20% relative to the baseline estimates when we control for voter registration.

[42] Shineman (2018) finds a 38 pp increase in voter turnout for a fixed $25 incentive in a local election in San Francisco, corresponding to a 1.5 pp gain in turnout per dollar (4.5 pp for a S/10 incentive).

which incorporates both the direct and indirect effects of the provision of a stronger monetary incentive (e.g., social contagion), is almost identical to the one found by Panagopoulos (2012), but is less than a third of the size of the more directly comparable estimate provided by León (2017). Such voltage drop is not uncommon when the effects of large-scale policies are compared to those of field experiments at a lower scale (Al-Ubaydli et al., 2017). One plausible explanation for voltage drop in our setting is that the millions of voters we study are imperfectly informed about the modified value of the fine, while the treatment in León (2017) provided salient and individualized information about these changes to voters. In this regard, the average perception of the value of the fine at baseline in León's sample was S/124 (standard deviation S/54), very close to its pre-reform level of S/136. Lack of knowledge about the reform or low salience of the modified value of the fine plausibly lead to imperfect compliance and to a dampened marginal effect of the fine on turnout.

To better understand the acquisition of information about the abstention fine, we use data on nationwide internet searches.[43] Using publicly-available data for Peru from the Google trends application, we construct a dataset on the popularity of 44 different search terms in the Google search engine. The search terms in the sample include three terms related to the abstention fine, which roughly translate to "election fine", "ONPE fine" and "fine for not voting." We also include several search terms related to elections (e.g., "candidates"), others associated with government and politics (e.g., "president"), the names or nicknames of former presidents and important political figures (e.g., "Fujimori"), as well as generally popular search terms (e.g., "soccer"). Appendix table A13 provides the full list of search terms used in the analysis. For each search term, we have monthly-level data between January 2005 and December 2016 from a Google Trends index, which is increasing in search frequency. We normalize the index at 100 for the search term "vicepresident" in April 2016. Full details on the construction of the dataset are available in the online appendix.

Using this data, we implement a difference-in-difference design to examine whether the frequency of internet searches related to the abstention fine grew disproportionately to other search terms after the reform.[44] We estimate the following specification:

$$\ln \text{Google trends index}_{i,m,y} = \theta_i + \omega_m + \sum_{\tau \geq 2006} \lambda_\tau \left[\mathbb{1}(\text{fine-related})_i \times \mathbb{1}(\text{year} = \tau)_y \right] + \zeta_{i,m} \quad (5)$$

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

[43] Roughly one third of Peruvians used the internet in 2007, almost one half in 2016, making internet searches a meaningful measure of information acquisition for a sizable share of the population (INEI, 2018).

[44] We used the Internet Archive to verify that the ONPE website provided information about the value of the fine and outstanding fines at the start of our sample period on web searches in early 2005.

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 relative popularity of the search terms related to the fine changes relative to 2005, the omitted year.[45] $\zeta_{i,t}$ is an error term that we cluster two-way by search-term (44 clusters) and month (144 clusters).

Figure 5 plots the results. Relative to 2005, the popularity of fine-related search terms grows almost two log points in 2006. This is to be expected, as this was a congested electoral year that had both national and subnational elections. Over the following three years, which had no elections, the relative popularity of fine-related searches decreased back to its baseline level. In 2009, the year before the adjusted district assignment to the fine categories, Google searches related to the abstention fine were just as common, relative to other search terms, as they were four years before. In 2010, when the district assignment was adjusted and subnational elections took place, we observe again a rise in fine-related web searches. This increase has roughly the same magnitude as the one from 2006, suggesting indeed the presence of seasonality related to the timing of elections. However, in the following years we do not observe a decrease in the popularity of fine-related searches. On the contrary, the relative frequency with which people search the web for information about the abstention fine rises further and remains high until the end of the sample period in 2016, ending almost four log points above the 2005 level. The estimates also become increasingly precise.

These results are consistent with gradual and partial learning about the regulatory changes to the fine and can help us explain our previous findings of larger long-run responses to the reform along several dimensions. The growing alleviation of informational frictions should arguably lead to further adaptation and greater convergence of the large-scale turnout elasticity to the experimental estimate in León (2017). Social contagion (Nickerson, 2008) and habit formation (Coppock and Green, 2016; Fujiwara et al., 2016) may also contribute in this regard. 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).[46]

More generally, the results in this section 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 policies that modify the institutional context that regulates their behavior. This limitation differs from the variation in administration quality that the previous literature on policy scale-up has mostly focused on (Davis et al., 2017; Bold et al., 2018; List et al., 2019).

[45] Appendix Figure A5 shows equivalent results from the disaggregate specification at the monthly level.

[46] 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.

It implies, for instance, that the results from many field experiments involving direct engagement with potential voters (e.g., in-home visits, direct mail) may be largely uninformative for large-scale policy implementation, insofar as the latter mostly leaves it up to individuals to acquire costly information and learn about any modified incentives.[47] This is a problem akin to the endogenous take-up of new technologies, insofar as those who demand information likely differ from the average voter for whom a treatment effect is estimated.

5 The Value of the Abstention Fine and Electoral Outcomes

In this section we examine whether the increase in voter turnout induced by marginal changes to the abstention fine affects electoral outcomes. We focus our attention on the share of blank and invalid votes.[48] Theory predicts that abstention is likely driven by the uninformed or uninterested in the absence of significant barriers to electoral participation (Feddersen and Pesendorfer, 1996). Hence, the voters drawn to the polls by a marginally larger fine are arguably more likely to cast a blank or invalid vote. This hypothesis finds some support in the existing literature. León (2017) finds that the effect on turnout of a perceived change to the size of the abstention fine is stronger for people that self declare as uninterested or uninformed about politics. Hoffman et al. (2017) additionally show that for every 10 extra votes generated by compulsory voting in Austria, there is an increase of 1.5-3 votes in the number declared as invalid.

In Peru, a vote is considered blank if the ballot is deposited completely unmarked. There is no "none of the above" option (Ujhelyi et al., 2019). A vote is considered invalid if it has any mark other than one cross (+) or ×-symbol on the logo of one party or the picture of the respective candidate.[49] Blank and invalid votes are subtracted from the total number

[47]Experimental estimates often correspond to the combined Local Average Treatment Effect (LATE) of exposure to an incentive and knowledge about it, while large-scale policy estimates capture an Intention to Treat (ITT) effect that does not condition on information. In settings with informational frictions, the ITT is likely to be the more relevant parameter for cost-benefit analysis and implementation decisions.

[48]The volatility of Peruvian politics, combined with the prohibition of consecutive presidential reelection, prevent us from systematically observing a stable set of parties before and after the reform. President Alberto Fujimori (1990-2000) fled the country without finishing his third term in office amid a major corruption scandal. This prompted the 2001 election won by Alejandro Toledo from Peru Posible, who defeated former president and APRA candidate Alan García (1985-1990) in the run-off. Toledo was succeeded in 2006 by García, who defeated outsider candidate Ollanta Humala in the run-off. Humala ran under a new party called PNP and would go on to win in 2011 against Fujimori's daughter, Keiko, who ran under another new party called Fuerza Popular. Fujimori would be defeated again in the 2016 run-off, this time by Pedro Pablo Kuczynski, who represented yet another new party called PPK. Kuczynski was removed from office amid corruption allegations in 2018 and was replaced by vicepresident Martin Vizcarra. Both Alan García and Alejandro Toledo ran again in 2016 under APRA and Peru Posible, respectively. However, we cannot rule out that any correlation between the value of the fine and their respective vote shares (both under 6%) is somehow related to differential policies during their previous time in office.

[49]Reasons for a vote being considered invalid include marking more than one candidate/party, using

of votes before calculating candidate vote shares. In the presidential run-off, this provides a strong incentive for party representatives to meticulously scrutinize every vote going to the other party in an attempt to have it discarded. To minimize the impact of the ex-post inflation of invalid votes, we focus our attention on the first round of the presidential election.

Column 1 in Table 5 re-estimates equation (2) for turnout, excluding the presidential run-off. The estimated coefficient of 0.043 is only slightly smaller than the average effect reported in Table 1 (0.049). Column 2 replicates the heterogeneity analysis across election cycles for this sub-sample, finding once more a substantially stronger effect in 2016. Column 3 shows results from equation (2) using the share of blank votes as the dependent variable. This share is defined relative to the number of registered voters, making it directly comparable to estimate for turnout in column 1. We find that a S/10 fine increase causes a 0.27 percentage point (pp) increase in the share of blank votes. Column 4 shows that the marginal effect of the fine on the share of blank votes is also increasing over time, jumping from 0.18 pp to 0.34 pp in 2016 for a S/10 fine hike. Columns 5 and 6 replicate the analysis for the share of invalid votes, defined also with respect to the number of registered voters. We find that a S/10 fine increase leads on average to a 0.1 pp increase in the share of invalid votes (column 5). Column 6 shows that this effect is also larger in 2016 (0.015).

These results suggest that the vast majority of voters that are brought to the polls by a marginally larger fine are not voting for any of the available candidates. The 0.37 pp increase in blank and invalid votes that a S/ 10 larger fine generates is equivalent to 86% of the 0.43 pp increase in turnout that the change to the value of the fine causes. If we disaggregate these effects by years, we find that blank and invalid votes account for the entirety of the turnout gain induced by the change to the value of the fine in 2011 and for 78% of the turnout effect in 2016. These results are consistent with theoretical models of rational abstention (Feddersen and Pesendorfer, 1996). They are also supportive of the theory of rational ignorance, according to which the negligible impact of a single vote makes it too costly to acquire political knowledge to inform the vote (Downs, 1957; Lopez de Leon and Rizzi, 2014). Though the increase in the share of invalid/blank votes could also be the result of higher turnout by poorer, less-educated voters, who are more prone to make a mistake (Fujiwara, 2015), this possibility finds little support in the data.[50] It is also possible that a marginally larger fine is causing higher turnout by anti-establishment voters that do not support any of the candidates, which are neither uninformed nor uninterested (Ambrus

symbols other than a cross or an ×, having the center-point of the mark outside of the party logo or candidate picture, any tear or sign of damage, or adding any writing.

[50]In 2016, only 5% of voters were reported as illiterate and a further 5% had less than full primary. In Brazil, Fujiwara (2015) reports that 23% of the population cannot read or write a simple sentence and 42% have no education beyond 4th grade.

et al., 2019; Ujhelyi et al., 2019). We cannot use the distinction between blank and invalid votes to distinguish between these interpretations, since voters that would be inclined to vote blank will often spoil the ballot in order to ensure that it cannot be manipulated afterwards.

While we cannot fully distinguish among these competing interpretations for the increase in the share of blank or null votes, what seems certain is that the variation in electoral participation caused by a marginal change to the monetary incentive to vote has no more than a negligible impact on representation.[51] These results stand in contrast to previous findings in the literature showing large effects of changes in the effective composition of the electorate on electoral outcomes and downstream policies (Miller, 2008; Cascio and Washington, 2013; Fujiwara, 2015). In this regard, an important feature that separates the context we study from those of previous research is the absence of significant barriers to electoral participation, which makes electoral abstention a mostly voluntary action.

6 The Aggregate Effect of Compulsory Voting

In this section, we seek to establish the contribution of the monetary incentive provided by the abstention fine relative to the aggregate effect of compulsory voting on turnout. Underlying this question is the idea that compulsory voting provides both monetary and non-monetary incentives. The latter include the expressive value of the law as a signalling device for socially-desirable behaviors and the non-monetary burden of the sanction imposed on non-voters (Funk, 2007; Cepaluni and Hidalgo, 2016). The previous literature provides several estimates of the effect of compulsory voting on turnout, but comparing these with our estimated elasticity with respect to the value of the fine would require somewhat strong assumptions about the external validity of findings from other settings in Peru. Hence, we would like to benchmark the elasticity against an estimate of the aggregate effect of compulsory voting obtained within the same setting, so as to more credibly establish the relative contribution of monetary and non-monetary incentives to the functioning of compulsory voting.

For this purpose, we use highly granular data on the composition of the electorate for the 2016 national elections at the voting-booth level, within polling stations. Voters in Peru are assigned to a specific voting booth corresponding to a voting-group number that appears on their DNI and they can only vote at that booth. Each voting booth is meant to have no more than 300 voters, but there is some flexibility to this rule. 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. In our sample, 75% of

[51] In order for a marginal fine change to have an aggregate electoral impact larger than the effect on turnout, we would have to make a somewhat heroic assumption about infra-marginal voters changing their vote in response to the change in turnout by the marginal voters that respond to the monetary incentive.

booths have between 281 and 334 registered voters.

Our estimate of the aggregate effect of compulsory voting exploits variation between individuals of different ages in the exposure to the mandate to vote. As mentioned in section 2, 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.[52] Our empirical strategy overcomes the ecological inference problem (Cho and Manski, 2009) caused by the absence of individual-level data on voter turnout by 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 to every other age group. We also include district or polling station fixed effects to ensure that we are not picking up differences across locations, including the value of the abstention fine. Ultimately, the richness of the data allows us to compare voting booths in the same location, and that look exactly 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 more. Pooling data from the 2016 general and run-off elections, we estimate the following specification:

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

where the dependent variable is the turnout rate in booth b, located in district d for election e (general or run-off). α_d and γ_e are district and election fixed effects (i.e. run-off), since we only have data for one election cycle. We replace the former with the more stringent polling-station fixed effects as a robustness check. The variables 'share$(\text{age} = \tau)_b$' 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). $\epsilon_{b,d,e}$ is

[52] The legal voting age in Peru is 18. However, under certain circumstances minors can 'emancipate' from their parents or guardians (e.g. if getting married), in which case they acquire the right to vote.

an error term clustered at the district level (1,854 clusters). 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 senior citizen 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 run-off fixed effects, having as dependent variable a voting dummy.

Panel (a) in Figure 6 shows the estimates of equation (6) for ages 20-80. Three things stand out. First, voter turnout is roughly constant over the thirty-year period between the ages of 40 and 69. This pattern suggests that compulsory voting is effectively offsetting any differential propensity to vote across these age groups. Second, turnout steadily drops below age 40 and is roughly 20 points lower at age 20 than at age 69. These results indicate that violations from compulsory voting are coming predominantly from younger voters, which is consistent with our finding above that it is these same young voters 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, which is unlike any other fluctuation we observe throughout the age distribution. Relative to age 69, turnout drops eight percentage points (pp) at age 70, 22 pp at age 72 and 38 pp at age 75. The 22-point difference in voter turnout between ages 69 and 72 is the same as that in the fifty-year window between ages 19 and 69.

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.[53] To address concerns about the confounding effect of increased morbidity and decreased mobility by the elderly, panel (b) plots the results for ages 60 through 80 for Peru and Chile.[54] We fail to observe any systematic difference in

[53] Hidalgo and Nichter (2016) use a tight bandwidth of 58 days around the 70th birthday and estimate a compulsory-voting effect on turnout of 4.4 percentage-points in Brazil, half as large as ours. The difference is likely driven by the RDD design underestimating the true effect in the presence of a staggered response.

[54] Appendix Figure A6 shows a smooth density of the age distribution around the cut-off for both countries.

electoral participation between the ages of 60 and 69 in either country. But while voter turnout plummets in Peru starting after age 69, in Chile we only observe a smooth decline, amounting to no more than a five 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 and not to other characteristics of the elderly. Importantly, the full results for Chile show that voter turnout is near its maximum around age 70 (Appendix Figure A7), dismissing the idea that voters in the age groups we study face a disproportionately high cost of voting.[55]

In Table 6 we examine the robustness of these results and heterogeneous effects. 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 (pp) 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 70- to 72-year-old voters 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 each cohort has been exposed.[56] 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.

Finally, in columns 6 and 7 we study heterogeneous responses to the exemption from compulsory voting by including additional interactions of the share of voters with ages 70-75 with other variables. Column 6 includes an interaction with a dummy for the presidential run-off. 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 run-off. Taken together with the 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 run-off than in the general election. Column 7 includes an additional interaction with the share of registered voters in the booth with secondary education or higher. We observe that the

[55]The same is true in the US, where voter turnout only declines after age 75 (Franklin, 2018).

[56]For those with ages 70 and 71, the elections in 2016 were the first in which they were not required to vote, while 72 to 75 year-olds had already enjoyed the exemption in the 2014 subnational elections. 75-year-olds were also exempt in the 2011 national elections.

entire drop in turnout resulting from the exemption to compulsory voting is coming from these voters (-29.5 pp). The effect is negligible for the omitted category, which corresponds to voters with no more than primary education. This result stands in contrast to the findings above showing that marginal fine increases affect electoral participation exclusively among the poor. It suggests that the restrictions in access to government services faced by non-voters are much more of a burden for the well-off (Cepaluni and Hidalgo, 2016).

Our finding of an aggregate effect of compulsory voting centered around 20 pp is comparable to previous findings by Fowler (2013), Jaitman (2013) and Bechtel et al. (2018) in Australia, Argentina and Switzerland, respectively. We use this estimate to do a back-of-the-envelope calculation and benchmark the estimated effect of the monetary incentive provided by the abstention fine. For enhanced 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% reduction) would lead to a 4.8% reduction 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% of the average fall in turnout between ages 69 and 75 shown in column 1 of Table 6.[57] In other words, the monetary incentive provided by the fine explains less than 20% of the effect of the exemption from compulsory voting.

This conclusion does not fundamentally change if we subtract the natural decline in electoral participation that we observe in Chile between ages 69 and 75. It is also conservative with regards to enforcement, as we are using the estimate for the elasticity (which, if anything, decreases when we control for enforcement). Furthermore, the fact that the effective fine is smaller than the nominal fine should affect voters' response to the exemption from compulsory voting as much as it affects their response to marginal fine changes. It is also likely that we are underestimating the effect of the exemption from compulsory voting, insofar as voters slightly above the age of 70 are plausibly still affected by the expressive function of the law and by their previous voting habits.[58] Similarly, our elasticity may be overestimating the effect of a 100% fine decrease if a complete elimination of the monetary incentive crowds in the intrinsic motivation to vote (Bénabou and Tirole, 2006).

It is true that 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 (i.e. 0.096), the drop in turnout from a

[57] According to the estimates in Figure 6, the drop in turnout caused by a full fine reduction is equivalent to 46% of the drop resulting from the exemption from compulsory voting at age 70, 18% of the drop at age 72 and 10% of the drop at age 75.

[58] In Appendix Table A3 we examine whether the marginal effect of the fine differs depending on the average age of voters. If anything, we find that older voters are less responsive to a larger fine (suggesting we overestimate the elasticity for the 70+ age-group), but the results are weak after controlling for poverty.

100% fine reduction would still only account for 36% 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% and this would require the elasticity to keep growing at the same yearly rate observed between 2010 and 2016 for at least ten more years.[59]

We conclude that the effect on voter turnout of a full reduction of the abstention fine pales in comparison to that of an exemption from compulsory voting. Looking back on our previous findings, we do not find this conclusion to be entirely surprising. As Figure 2 shows, the abstention fine in low-fine districts is only 25% of its value in high-fine districts, but this difference only drives at the most a roughly five-point gap in turnout between these groups. Policy-wise, the fact that voters are substantially more responsive to the extensive margin of compulsory voting than to marginal changes in the value of the fine 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 non-compliance.

7 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 find that marginal fine changes have a robust positive effect on voter turnout, despite informational frictions leading to voltage drop relative to previous experimental estimates. However, the monetary incentive provided by the fine pales in comparison to the aggregate effect of compulsory voting, which we estimate exploiting a second natural experiment provided by the exemption from compulsory voting after age seventy. We find an aggregate effect of around 20 percentage points, of which our estimate of the fine-elasticity can explain no more than 20%. Thus, the non-monetary incentives provided by compulsory voting, which include the expressive value of the law, social image concerns and the non-monetary burden of the sanction, vastly outweigh the monetary incentive provided by the fine. More broadly, these results suggest that richer psychological considerations trump direct economic costs in the calculus of voting.

Our findings have important policy implications. Monetary incentives to vote are a rarely used alternative available to governments across the globe. These incentives are compatible with voluntary voting in the form of tax deductions, lotteries, discounts on government services or direct transfers for those who participate in elections. Our results show that marginal changes to these incentives, in the form of reductions to the fine for electoral

[59]The observed elasticity of 0.048 in 2016 corresponds to an annual growth of 0.008 since 2010. This number becomes smaller if we take 2006 (year of the initial reform) as the starting year.

abstention, have a robust and non-negligible effect on voter turnout. However, our findings also show that voters respond in a rich, multi-dimensional way, indicating that policy-makers must be cautious about the unintended consequences that targeted policies can give rise to.

Furthermore, our results indicate that the omnibus bundle of incentives provided by compulsory voting is significantly more effective at increasing voter turnout than even large changes to the value of the abstention fine. Thus, if the aim is to maximize turnout, making voting mandatory is a policy option worth considering. Taken together, our results show that the fines used to enforce compulsory voting can be set at moderate values without fundamentally undermining the effectiveness of the system, while reducing the burden that these monetary penalties impose on non-voters, especially the poor.

Our results 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 marginal fine increases indicates that the gain in voter turnout achieved through extrinsic incentives is unlikely to substantially affect representation or downstream policy outcomes. Naturally, one has to be cautious about extending this conclusion to settings in which large shares of the population face substantial barriers to electoral participation.

Overall, our results provide evidence of a gradual, sophisticated and heterogeneous response to large-scale public provision of monetary incentives to vote. They illustrate in various ways how the findings from small-scale field experiments testing voter mobilization initiatives have limited external validity for large-scale policy implementation. In particular, our finding of informational frictions to adaptation in response to regulatory changes plausibly extend to a wide range of interventions in political economy that change the rules governing the interaction of citizens with the state without divulging these changes or making them salient.

References

Al-Ubaydli, O., List, J. A., Lore, D., and Suskind, D. (2017). Scaling for Economists: Lessons from the Non-Adherence Problem in the Medical Literature. *Journal of Economic Perspectives*, 31(4):125–44.

Ambrus, A., Greiner, B., and Zednik, A. (2019). The Effect of a 'None of the Above' Ballot Paper Option on Voting Behavior and Election Outcomes. Working paper.

Angrist, J. D. and Pischke, J.-S. (2009). *Mostly Harmless Econometrics: An Empiricist's Companion*. Princeton University Press, Princeton, NJ.

Banerjee, A., Banerji, R., Berry, J., Duflo, E., Kannan, H., Mukerji, S., Shotland, M., and Walton, M. (2017). From Proof of Concept to Scalable Policies: Challenges and Solutions, with an Application. *Journal of Economic Perspectives*, 31(4):73–102.

Bechtel, M. M., Hangartner, D., and Schmid, L. (2018). Compulsory Voting, Habit Formation, and Political Participation. *Review of Economics and Statistics*, 100(3):467–476.

Bertocchi, G., Dimico, A., Lancia, F., and Russo, A. (2017). Youth Enfranchisement, Political Responsiveness and Education Expenditure: Evidence from the U.S. IZA DP 11082.

Blais, A. (2000). *To Vote or Not to Vote: The Merits and Limits of Rational Choice Theory*. University of Pittsburgh Press.

Bold, T., Kimenyi, M., Mwabu, G., Ng'ang'a, A., and Sandefur, J. (2018). Experimental Evidence on Scaling Up Education Reforms in Kenya. *Journal of Public Economics*, 168:1–20.

Braconnier, C., Dormagen, J.-Y., and Pons, V. (2017). Voter Registration Costs and Disenfranchisement: Experimental Evidence from France. *American Political Science Review*, 111(3):584–604.

Brady, H. and McNulty, J. (2011). Turning Out to Vote: The Costs of Finding and Getting to the Polling Place. *American Political Science Review*, 105(1):115–134.

Bénabou, R. and Tirole, J. (2003). Intrinsic and Extrinsic Motivation. *Review of Economic Studies*, 70(3):489–520.

Bénabou, R. and Tirole, J. (2006). Incentives and Prosocial Behavior. *American Economic Review*, 96(5):1652–1678.

Camacho, A. and Conover, E. (2011). Manipulation of Social Program Eligibility. *American Economic Journal: Economic Policy*, 3(2):41–65.

Cameron, A. C., Gelbach, J. B., and Miller, D. L. (2008). Bootstrap-Based Improvements for Inference with Clustered Errors. *Review of Economics and Statistics*, 90(3):414–427.

Cantoni, E. (2019). A Precinct Too Far: Turnout and Voting Costs. Forthcoming in American Economic Journal: Applied Economics.

Carpio, M. A., Córdova, B., Larreguy, H., and Weaver, J. A. (2018). Understanding the General Equilibrium Effects of Compulsory Voting on Policy: Evidence from Peru. Working Paper.

Cascio, E. U. and Washington, E. (2013). Valuing the Vote: The Redistribution of Voting Rights and State Funds following the Voting Rights Act of 1965. *Quarterly Journal of Economics*, 129(1):379–433.

Cassan, G. (2015). Identity-Based Policies and Identity Manipulation: Evidence from Colonial Punjab. *American Economic Journal: Economic Policy*, 7(4):103–31.

Cepaluni, G. and Hidalgo, F. D. (2016). Compulsory Voting Can Increase Political Inequality: Evidence from Brazil. *Political Analysis*, 24(2):273–280.

Chapman, E. B. (2019). The Distinctive Value of Elections and the Case for Compulsory Voting. *American Journal of Political Science*, 63(1):101–112.

Cho, W. T. and Manski, C. F. (2009). Cross-level/Ecological Inference. In Box-Steffensmeier, J. M., Brady, H. E., and Collier, D., editors, *Oxford Handbook of Political Methodology*, pages 547 – 569. Oxford University Press.

Chong, A., Leon-Ciliotta, G., Roza, V., Valdivia, M., and Vega, G. (2019). Urbanization Patterns, Information Diffusion and Female Voting in Rural Paraguay. *American Journal of Political Science*, 63(2):23–341.

Coppock, A. and Green, D. P. (2016). Is Voting Habit Forming? New Evidence from Experiments and Regression Discontinuities. *American Journal of Political Science*, 60(4):1044–1062.

Correia, S. (2015). Singletons, Cluster-Robust Standard Errors and Fixed Effects: A Bad Mix. Working Paper.

Davis, J., Guryan, J., Hallberg, K., and Ludwig, J. (2017). The Economics of Scale-Up. NBER working paper 23925.

Deaton, A. (2010). Instruments, Randomization, and Learning about Development. *Journal of Economic Literature*, 48(2):424–55.

Dellavigna, S., List, J. A., Malmendier, U., and Rao, G. (2017). Voting to Tell Others. *Review of Economic Studies*, 84(1):143–181.

Downs, A. (1957). *An Economic Theory of Democracy*. Harper and Row, New York, NY.

El Comercio (2015). Ley para eliminar el voto golondrino fue promulgada. August 27, 2015.

Fafchamps, M., Vaz, A., and Vicente, P. (2018). Voting and Peer Effects: Experimental Evidence from Mozambique. Forthcoming in Economic Development and Cultural Change.

Feddersen, T. J. (2004). Rational Choice Theory and the Paradox of Not Voting. *Journal of Economic Perspectives*, 18(1):99–112.

Feddersen, T. J. and Pesendorfer, W. (1996). The Swing Voter's Curse. *American Economic Review*, 86(3):408–424.

Fowler, A. (2013). Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia. *Quarterly Journal of Political Science*, 8(2):159–182.

Franklin, C. (2018). Age and voter turnout. https://medium.com/@PollsAndVotes/age-and-voter-turnout-52962b0884ef. Accessed: 2019/10/15.

Fujiwara, T. (2015). Voting Technology, Political Responsiveness, and Infant Health: Evidence From Brazil. *Econometrica*, 83(2):423–464.

Fujiwara, T., Meng, K., and Vogl, T. (2016). Habit Formation in Voting: Evidence from Rainy Elections. *American Economic Journal: Applied Economics*, 8(4):160–88.

Funk, P. (2007). Is There An Expressive Function of Law? An Empirical Analysis of Voting Laws with Symbolic Fines. *American Law and Economics Review*, 9(1):135–159.

Funk, P. (2010). Social Incentives and Voter Turnout: Evidence from the Swiss Mail Ballot System. *Journal of the European Economic Association*, 8(5):1077–1103.

Gerber, A. S. and Green, D. P. (2017). Field Experiments on Voter Mobilization: An Overview of a Burgeoning Literature. In Banerjee, A. V. and Duflo, E., editors, *Handbook of Economic Field Experiments*, volume 1, pages 395 – 438. North-Holland.

Gerber, A. S., Green, D. P., and Larimer, C. W. (2008). Social Pressure and Voter Turnout: Evidence from a Large-Scale Field Experiment. *American Political Science Review*, 102(1):33–48.

Giné, X. and Mansuri, G. (2018). Together We Will: Experimental Evidence on Female Voting Behavior in Pakistan. *American Economic Journal: Applied Economics*, 10(1):207–35.

Gneezy, U., Meier, S., and Rey-Biel, P. (2011). When and why incentives (don't) work to modify behavior. *Journal of Economic Perspectives*, 25(4):191–210.

Green, D. P., McGrath, M., and Aronow, P. (2013). Field Experiments and the Study of Voter Turnout. *Journal of Elections, Public Opinion and Parties*, 23(1):27–48.

Hansford, T. G. and Gomez, B. T. (2010). Estimating the Electoral Effects of Voter Turnout. *American Political Science Review*, 104(2):268–288.

Hidalgo, F. D. and Nichter, S. (2016). Voter Buying: Shaping the Electorate through Clientelism. *American Journal of Political Science*, 60(2):436–455.

Hodler, R., Luechinger, S., and Stutzer, A. (2015). The Effects of Voting Costs on the Democratic Process and Public Finances. *American Economic Journal: Economic Policy*, 7(1):141–71.

Hoffman, M., León, G., and Lombardi, M. (2017). Compulsory Voting, Turnout, and Government Spending: Evidence from Austria. *Journal of Public Economics*, 145:103–115.

Holbein, J. B. and Hillygus, D. S. (2016). Making Young Voters: The Impact of Preregistration on Youth Turnout. *American Journal of Political Science*, 60(2):364–382.

IDEA (2018). International Institute for Democracy and Electoral Assistance: Compulsory Voting. https://www.idea.int/data-tools/data/voter-turnout/compulsory-voting. Accessed: 2018/07/27.

INEI (2018). Instituto Nacional de Estadística e Informática: Encuesta Nacional de Programas Presupuestales 2011-2017. `https://www.inei.gob.pe/media/MenuRecursivo/publicaciones_digitales/Est/Lib1520/index.html`. Accessed: 2018/12/11.

Jaitman, L. (2013). The Causal Effect of Compulsory Voting Laws on Turnout: Does Skill Matter? *Journal of Economic Behavior & Organization*, 92:79 – 93.

JNE (2015). Memoria de gestión anual. Jurado Nacional de Elecciones.

Kaplan, E. and Yuan, H. (2018). Early Voting Laws, Voter Turnout and Partisan Vote Composition: Evidence from Ohio. Forthcoming in American Economic Journal: Applied Economics.

La República (2017). Detectan alrededor de 100 mil potenciales votantes golondrinos en 151 distritos. December 02, 2017.

León, G. (2017). Turnout, Political Preferences and Information: Experimental Evidence From Peru. *Journal of Development Economics*, 127:56–71.

Lijphart, A. (1997). Unequal Participation: Democracy's Unresolved Dilemma. *American Political Science Review*, 91(1):1–14.

List, J. A., Suskind, D., and Al-Ubaydli, O. (2019). The Science of Using Science: Towards an Understanding of the Threats to Scaling Experiments. Becker Friedman Institute Working Paper 2019-73.

Loewen, P. J., Milner, H., and Hicks, B. M. (2008). Does Compulsory Voting Lead to More Informed and Engaged Citizens? An Experimental Test. *Canadian Journal of Political Science*, 41(3):655–672.

Lopez de Leon, F. L. and Rizzi, R. (2014). A Test for the Rational Ignorance Hypothesis: Evidence from a Natural Experiment in Brazil. *American Economic Journal: Economic Policy*, 6(4):380–98.

Miller, G. (2008). Women's Suffrage, Political Responsiveness, and Child Survival in American History. *Quarterly Journal of Economics*, 123(3):1287–1327.

Muralidharan, K. and Niehaus, P. (2017). Experimentation at Scale. *Journal of Economic Perspectives*, 31(4):103–24.

Nichter, S. (2008). Vote Buying or Turnout Buying? Machine Politics and the Secret Ballot. *American Political Science Review*, 102(1):19–31.

Nickerson, D. W. (2008). Is Voting Contagious? Evidence from Two Field Experiments. *American Political Science Review*, 102(1):49–57.

Panagopoulos, C. (2012). Extrinsic Rewards, Intrinsic Motivation and Voting. *Journal of Politics*, 75(1):266–280.

Riker, W. H. and Ordeshook, P. C. (1968). A Theory of the Calculus of Voting. *American Political Science Review*, 62(1):25–42.

Roodman, D., MacKinnon, J., Nielsen, M., and Webb, M. (2019). Fast and wild: bootstrap inference in Stata using boottest. *Stata Journal*.

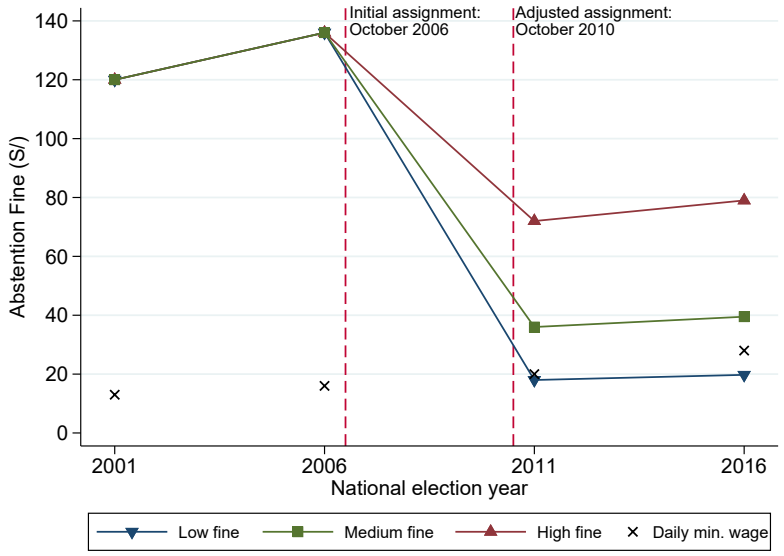
Shineman, V. A. (2018). If You Mobilize Them, They Will Become Informed: Experimental Evidence that Information Acquisition Is Endogenous to Costs and Incentives to Participate. *British Journal of Political Science*, 48(1):189–211.

The Washington Post (2015). President Obama endorses mandatory voting. March 19, 2015.

Ujhelyi, G., Chatterjee, S., and Szabó, A. (2019). None Of The Above. Working Paper.

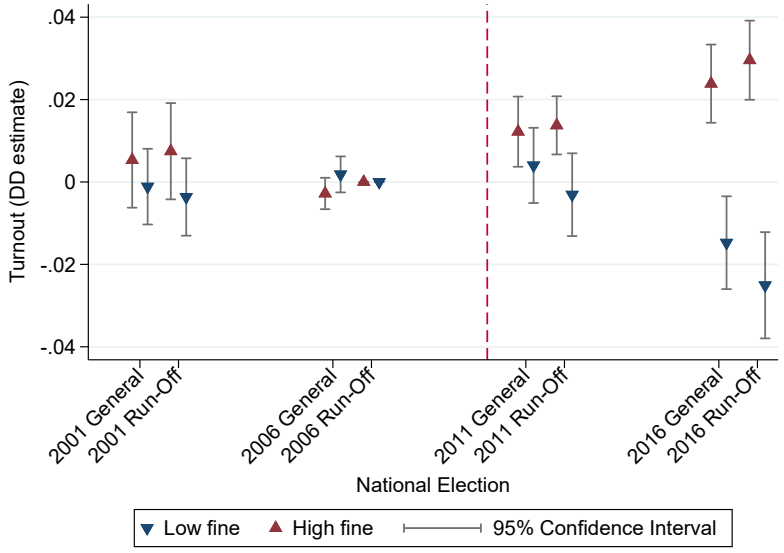
Vivalt, E. (2019). How Much Can We Generalize From Impact Evaluations? Forthcoming in Journal of the European Economics Association.

Figure 1: The Abstention Fine by Election and Fine Category



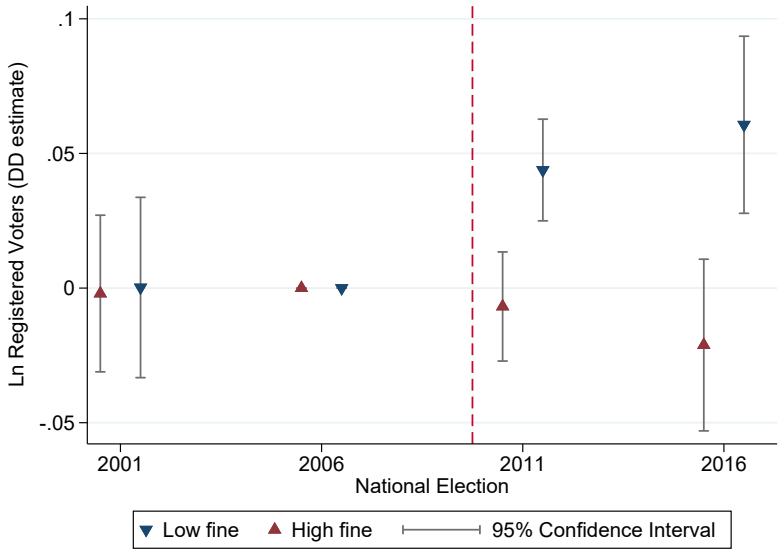
Notes: The graph shows the value of the abstention fine in each category for the national elections of 2001, 2006, 2011 and 2016. Values are displayed in current Peruvian soles (S/), but are defined in constant units for tax purposes (UIT), which are updated yearly to adjust for inflation. The graph also shows the nominal value of the legal minimum daily wage for each election year. The average value of the official exchange rate in 2006 was S/3.27 per US$1. The average yearly inflation rate for the period 2001-2016 was 2.75%. The 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).

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



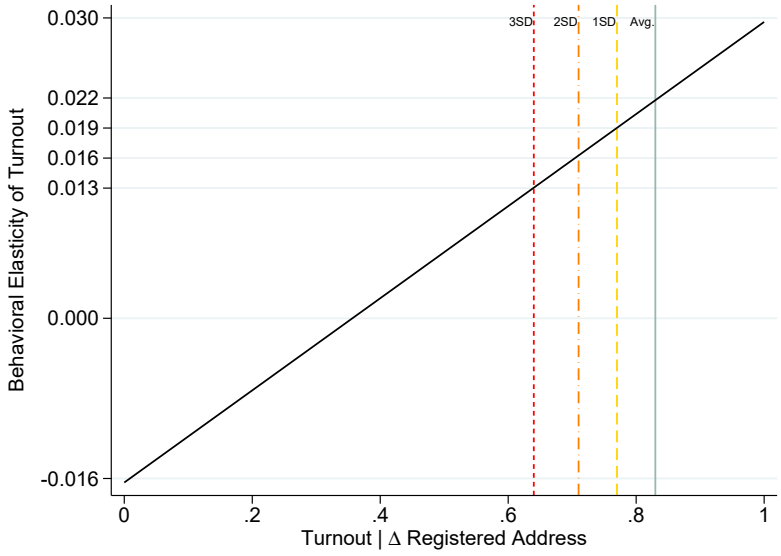
Notes: The graph shows point estimates and 95% confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with respective dummies for districts assigned a high fine (non-poor) and a low fine (extreme poor) in 2010. The omitted category includes districts assigned a medium fine (poor) in 2010. The omitted election is the 2006 presidential run-off. Regression includes district and province x election x 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).

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



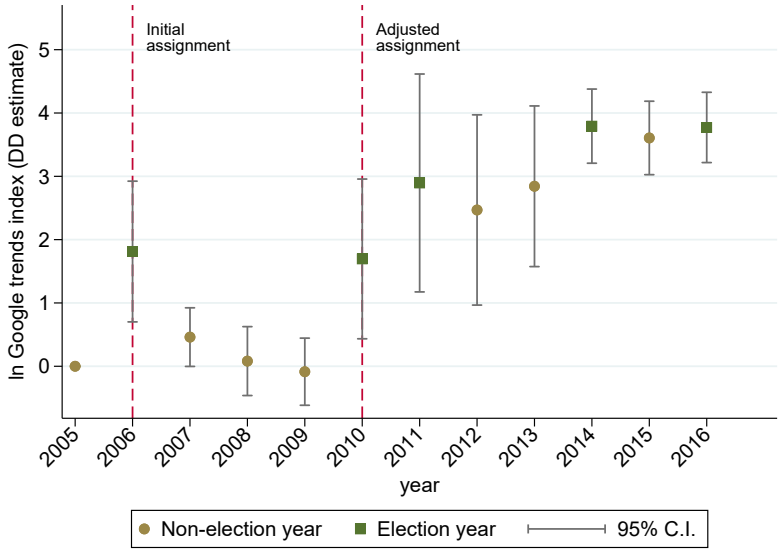
Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of district-level registered voters on a full set of election dummies interacted with respective dummies for districts assigned a high fine (non-poor) and a low fine (extreme poor) in 2010. The omitted category includes districts assigned a medium fine (poor) in 2010. The omitted election year is 2006. Regression includes district and province x election x 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 dashed line shows the date of adjusted district assignment (October 2010).

Figure 4: Disentangling the Behavioral Elasticity of Turnout from the Registration Effect



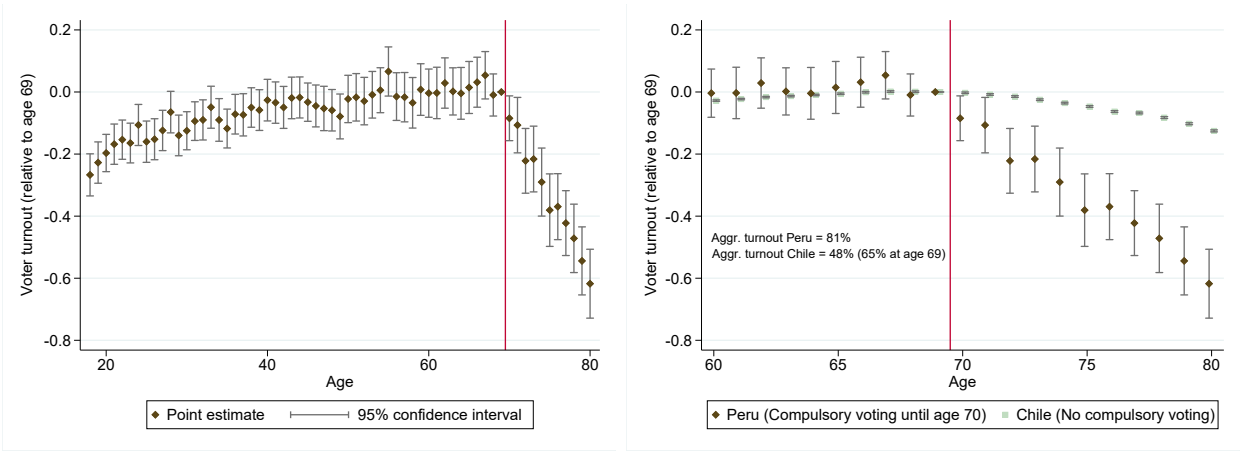
Notes: The graph shows the implied behavioral elasticity of turnout corresponding to different values of the probability of voting (i.e., turnout) for those that changed the address on their DNI to districts with a lower fine for abstention. Calculation based on estimates in Table 4 (columns 1 and 3) and equation (4). Solid vertical line shows average turnout in 2011/16. Dashed, dash-dot and dotted lines display turnout rates that are respectivley one, two and three standard deviations below the average.

Figure 5: The Reform to the Abstention Fine and Information Acquisition



Notes: The graph shows point estimates and 95% 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 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.

Figure 6: Senior Exemption from Compulsory Voting and Voter Turnout



(a) Peru: Ages 18-80 **(b)** Peru vs Chile: Ages 60-80

Notes: Panel(a) shows point estimates and 95% 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 run-off dummy. Data includes the general election and presidential run-off 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 run-off) 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.

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

	Baseline	Election x Province FE	Additional controls		
			Poverty shares	Education shares	Polling stations
	(1)	(2)	(3)	(4)	(5)
	Panel A - Dependent variable: Voter Turnout$_{i,t}$				
Fine value$_{i,t}$ (S/ x 100)	0.049***	0.046***	0.035***	0.061***	0.049***
	[0.008]	[0.010]	[0.011]	[0.011]	[0.008]
R-squared	0.02	0.03	0.06	0.08	0.09
Mean of dependent variable	0.85	0.84	0.85	0.83	0.85
	Panel B - Dependent variable: ln Voter Turnout$_{i,t}$				
ln Fine value$_{i,t}$	0.030***	0.028***	0.023***	0.037***	0.029***
	[0.005]	[0.006]	[0.007]	[0.007]	[0.005]
R-squared	0.02	0.02	0.06	0.07	0.07
Mean of dependent variable	-0.17	-0.17	-0.17	-0.19	-0.17
Observations	13,536	14,040	13,536	10,152	13,536
Districts	1692	1755	1692	1692	1692
District FE	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	No	Yes	Yes	Yes
Election x Province FE	No	Yes	No	No	No

Notes: All columns use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016, except column 4 (data on education level of voters is unavailable for 2006). Column 3 includes the time-invariant shares of poor and extreme poor inhabitants interacted with election fixed effects. Column 4 includes the time-varying shares of registered voters with primary, secondary and tertiary education. Column 5 includes log polling stations. All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

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	Run-off	Poverty	Long-run	Run-off	Poverty
	(1)	(2)	(3)	(4)	(5)	(6)
(ln) Fine value$_{i,t}$ (S/ x 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$_{i,t}$ × $\mathbb{1}(2016)_t$ [b]	0.051***			0.038***		
	[0.005]			[0.003]		
(ln) Fine value$_{i,t}$ ×$\mathbb{1}$(Run-Off)$_t$ [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	1692	1692	1692	1692	1692	1692
R-squared	0.03	0.02	0.03	0.03	0.02	0.03
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: All columns use data from national elections (general and presidential run-off) 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 run-off 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 clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

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

	Dependent variable: ln Voters$_{i,t}$						
	All	18-20	21-29	30-35	36-50	51-75	75+
	(1)	(2)	(3)	(4)	(5)	(6)	(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	1692	1692	1692	1692	1692	1692	1692
R-squared	0.001	0.03	0.001	0.0003	0.0002	0.0001	0.001
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x '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 clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

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 value$_{i,t}$ [a]	-0.046***	-0.035***	-0.016	-0.024*
	[0.015]	[0.012]	[0.016]	[0.014]
ln Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$ [b]		-0.022***		0.015*
		[0.009]		[0.009]
Observations	6,768	6,768	13,536	13,536
Districts	1692	1692	1692	1692
R-squared	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 x Province x 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 run-off elections); ln Votes is the natural log of the actual number of votes cast in each election. Sample includes national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. Regressions are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

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

Dependent variable:	Turnout$_{i,t}$		Blank votes$_{i,t}$		Invalid votes$_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fine value$_{i,t}$ (S/ x 100) [a]	0.043***	0.017*	0.027***	0.018***	0.010**	0.004
	[0.009]	[0.009]	[0.005]	[0.006]	[0.005]	[0.006]
Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$ [b]		0.045***		0.016***		0.011**
		[0.005]		[0.004]		[0.005]
Observations	6,768	6,768	6,768	6,768	6,768	6,768
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.02	0.02	0.01	0.01	0.002	0.003
Mean of dep. var	0.85	0.85	0.09	0.09	0.03	0.03
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes
p-value H_0: a+b=0		0.000		0.000		0.006

Notes: Blank votes and invalid votes in columns 3-6 are measured as shares of the number of registered voters. All columns use data from the 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 clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table 6: Senior Exemption from Compulsory Voting and Voter Turnout

	Dependent variable: Turnout$_i$ (Mean: 0.82)						
	Baseline	280-300 voters	Polling station FE	Share 70-72	Elections w/o CV	Heterogeneity	
						Election	Education
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Share ages 70-75$_i$	-0.209***	-0.182***	-0.215***			-0.174***	-0.000
	[0.041]	[0.034]	[0.028]			[0.043]	[0.079]
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$_i$ $\times \mathbb{1}$(Run-Off)$_t$						-0.069***	
						[0.017]	
Share ages 70-75$_i$ $\times$ Share high school $_i$							-0.295***
							[0.104]
Observations	148,448	109,462	148,448	148,448	148,448	148,448	148,448
Districts	1854	1109	1854	1854	1854	1854	1854
Polling stations	4723	2725	4723	4723	4723	4723	4723
R-squared	0.19	0.16	0.08	0.19	0.19	0.19	0.19
District FE	Yes	Yes	No	Yes	Yes	Yes	Yes
Polling station FE	No	No	Yes	No	No	No	No
Election type FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Share by age	Yes	Yes	Yes	Yes	Yes	Yes	Yes

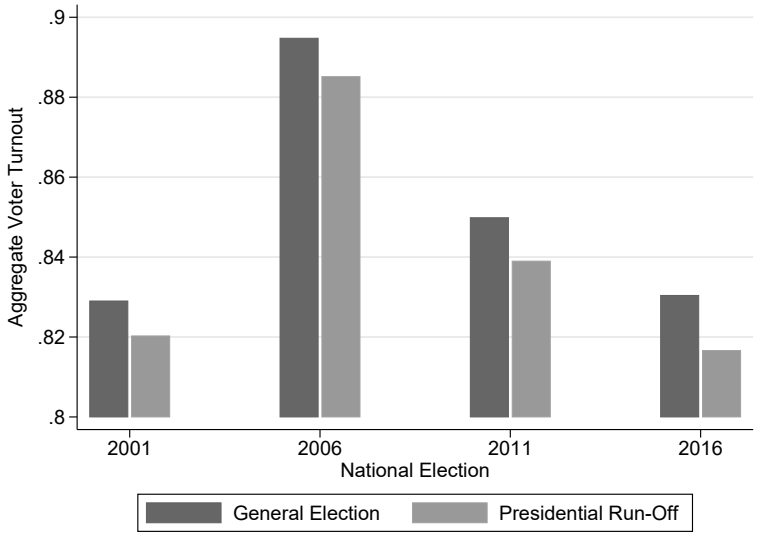
Notes: Data at the voting-booth level for the national elections of 2016 (General and Presidential Run-Off). The regressor of interest is the share of registered voters with ages slightly above the cut-off for exemption from compulsory voting (CV): 70-75 in all columns except column 4, where it is 70-72. 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 run-off, while column 7 shows heterogeneous effects by the share of registered voters with secondary education or higher. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

Appendix (for online publication)

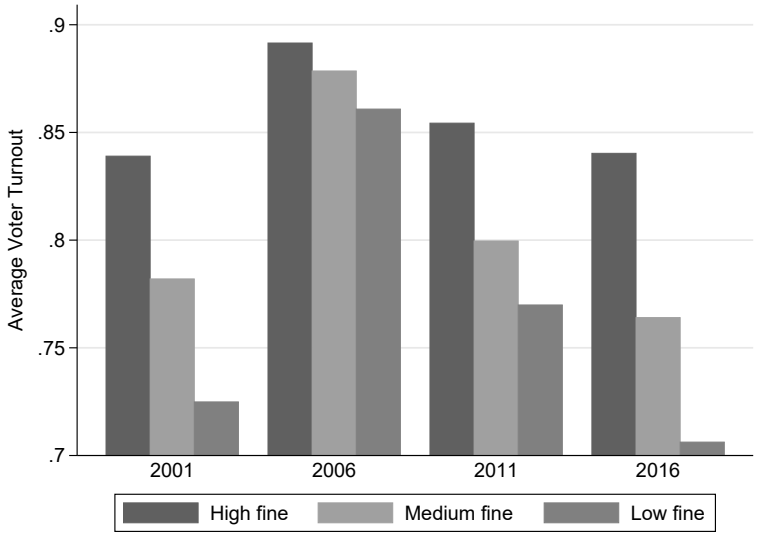
Table of Contents

A Additional Background Information

Figure A1: Voter Turnout in National Elections



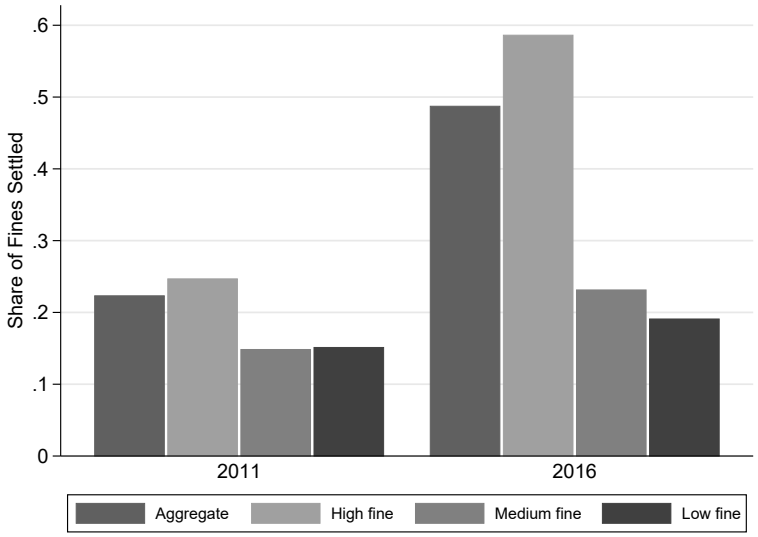
(a) Aggregate (by election type)



(b) Disaggregate by fine category

Note: Panel (a) shows aggregate voter turnout for each national election in Peru between 2001 and 2016. The general election includes the first round of the presidential election and the legislative election. Panel (b) shows voter turnout by fine category, averaged across the two national elections per cycle.

Figure A2: Share of Fines Settled for the 2011 and 2016 Elections



Notes: The graph shows the share of abstention fines settled in each category, as well as the countrywide aggregate, for the national elections of 2011 and 2016 (general and run-off combined). Settled fines include paid fines and valid excuses. Data from June 2018.

Table A1: Assignment of Districts to Poverty Categories in 2006 and 2010

2010 assignment	2006 assignment			
	High fine	Medium fine	Low fine	Total
High fine	182	570	165	917
Medium fine	0	119	195	314
Low fine	0	73	451	524
Total	182	762	811	1,755

Notes: Districts with incomplete election data (including newly created ones) or with inconsistencies in the assignment are dropped. Final sample of 1,755 districts corresponds to 94.7% of the total number of districts in Peru.

B Voter Turnout: Additional Results

Table A2: Event-study estimates of the effect of the reform on turnout and registration

Dependent Variable:	$\text{Turnout}_{i,t}$	$\ln \text{Voters}_{i,t}$
	(1)	(2)
$\mathbb{1}(\text{2001 General})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.005	-0.002
	[0.006]	[0.015]
$\mathbb{1}(\text{2001 Run-Off})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.007	
	[0.006]	
$\mathbb{1}(\text{2006 General})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	-0.003	
	[0.002]	
$\mathbb{1}(\text{2011 General})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.012***	-0.007
	[0.004]	[0.010]
$\mathbb{1}(\text{2011 Run-Off})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.014***	
	[0.004]	
$\mathbb{1}(\text{2016 General})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.024***	-0.021
	[0.005]	[0.016]
$\mathbb{1}(\text{2016 Run-Off})_t \times \mathbb{1}(c_{10}\text{= High fine})_i$	0.030***	
	[0.005]	
$\mathbb{1}(\text{2001 General})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	-0.001	0.000
	[0.005]	[0.017]
$\mathbb{1}(\text{2001 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	-0.004	
	[0.005]	
$\mathbb{1}(\text{2006 General})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	0.002	
	[0.002]	
$\mathbb{1}(\text{2011 General})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	0.004	0.044***
	[0.005]	[0.010]
$\mathbb{1}(\text{2011 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	-0.003	
	[0.005]	
$\mathbb{1}(\text{2016 General})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	-0.015**	0.061***
	[0.006]	[0.017]
$\mathbb{1}(\text{2016 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Low fine})_i$	-0.025***	
	[0.007]	
Observations	13,536	6,768
Districts	1692	1692
District FE	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes

Notes: Column 1 corresponds to Figure 2 in the text, while column 2 corresponds to Figure 3. In column 1, the dependent variable is turnout and the omitted election is the 2006 presidential run-off. In column 2, the dependent variable is the natural log of the number of registered voters and the omitted election cycle is 2006. Voter registration is constant within an election cycle (i.e. general election and run-off). Regressions include district and province-election-category fixed effects (using 2006 classification). Observations are weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 clusters). *** p<0.01, ** p<0.05, * p<0.1

Table A3: Heterogeneous effects by Age of the Electorate

	(1)	(2)	(3)	(4)
Panel A - Dependent variable: Voter Turnout$_{i,t}$ (Mean: 0.845)				
Fine value$_{i,t}$ (S/ x 100)	0.145***	0.057	0.056***	0.004
	[0.044]	[0.045]	[0.009]	[0.015]
Fine value$_{i,t}$ × Avg. age of voters$_i$	-0.002**	-0.001		
	[0.001]	[0.001]		
Fine value$_{i,t}$ × Share poor$_i$		0.050***		0.050***
		[0.012]		[0.012]
Fine value$_{i,t}$ × D(Average age: second tercile)$_i$			-0.007**	-0.005*
			[0.003]	[0.003]
Fine value$_{i,t}$ × D(Average age: top tercile)$_i$			-0.014**	-0.009*
			[0.006]	[0.004]
R-squared	0.03	0.03	0.03	0.03
Panel B - Dependent variable: ln Voter Turnout$_{i,t}$ (Mean: -0.17)				
ln Fine value$_{i,t}$ (S/ x 100)	0.090	0.019	0.033***	-0.010
	[0.054]	[0.047]	[0.006]	[0.016]
ln Fine value$_{i,t}$× Avg. age of voters$_i$	-0.001	-0.001		
	[0.001]	[0.001]		
ln Fine value$_{i,t}$ × Share poor$_i$		0.048***		0.047***
		[0.016]		[0.017]
ln Fine value$_{i,t}$ × D(Average age: second tercile)$_i$			-0.004	-0.003
			[0.004]	[0.003]
ln Fine value$_{i,t}$ × D(Average age: top tercile)$_i$			-0.009	-0.005
			[0.007]	[0.005]
R-squared	0.02	0.02	0.02	0.02
Observations	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692
District FE	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes

Notes: All regressions use data from national elections in 2001, 2006, 2011 and 2016. Columns 1 and 2 include the interaction of the fine with the average age of registered voters in 2016. Columns 3 and 4 include similar interactions for the top two terciles of the age distribution. Columns 2 and 4 include an additional 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 clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A4: The Marginal Effect of the Abstention Fine on Voter Turnout in Run-Off elections

	Dependent variable: Turnout$_{i,t}$			
	(1)	(2)	(3)	(4)
Fine value$_{i,t}$ (S/ x 100)	0.055***		0.055***	0.054***
	[0.009]		[0.009]	[0.014]
Vote share of run-off candidates$_{i,t-1}$		0.003	0.003	0.001
		[0.014]	[0.013]	[0.036]
Vote share of run-off candidates$_{i,t-1}$ × Fine value$_{i,t}$				0.002
				[0.026]
Observations	6,768	6,768	6,768	6,768
Districts	1692	1692	1692	1692
R-squared	0.96	0.96	0.96	0.96
District FE	Yes	Yes	Yes	Yes
Election-Province-Category '06 FE	Yes	Yes	Yes	Yes

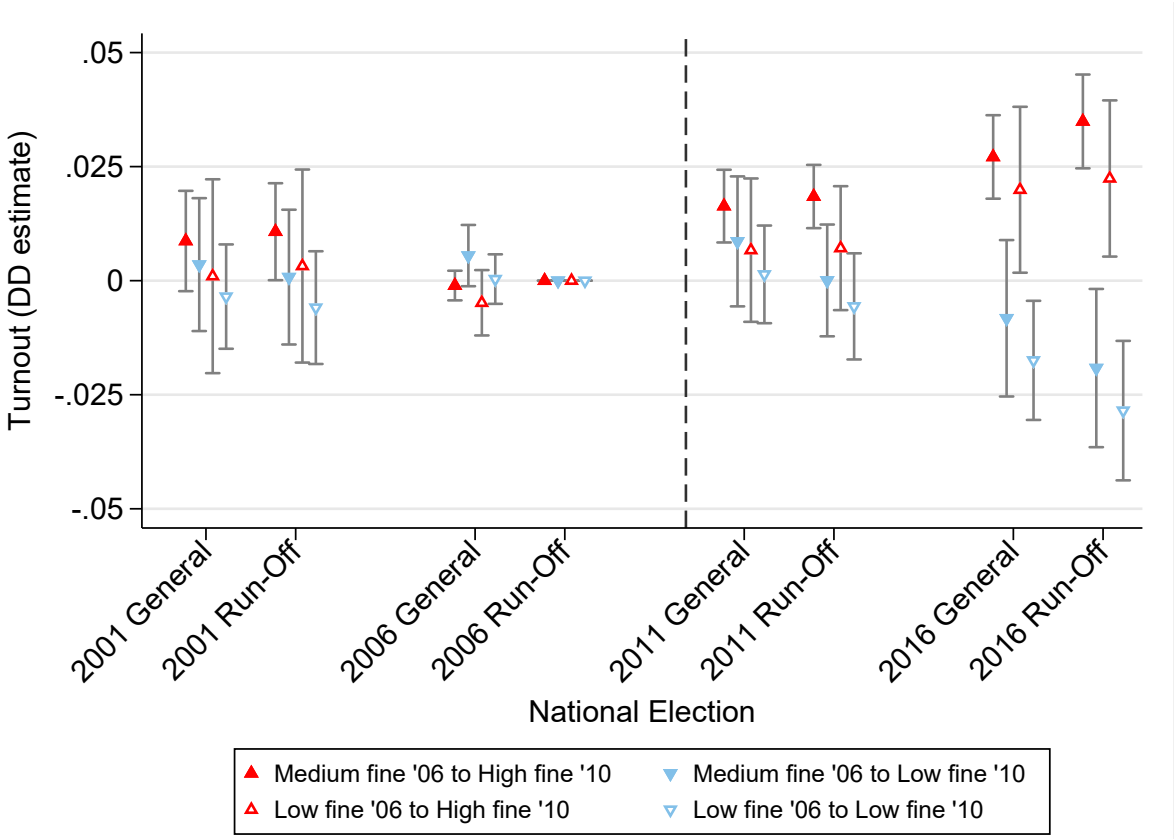
Notes: Dependent variable is voter turnout (0-1). Vote share of run-off candidates$_{i,t-1}$ is the sum of the vote shares in the first round of the presidential election for the two candidates that progressed to the run-off (top two candidates in the aggregate). All regressions only use data from presidential run-off elections for the years 2001, 2006, 2011 and 2016. The abstention fine is the same for all districts until the 2006 elections. All regressions include district fixed effects and election-date by province by 2006 poverty category fixed effects. All regressions are weighted by the number of registered voters for the elections in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A5: Main Results Clustering by Region (Wild Cluster Bootstrap)

Dependent variable:	Voter Turnout$_{i,t}$			Vote share$_{i,t}$	
	level	log	ln Voters$_{i,t}$	Blank	Invalid
	(1)	(2)	(3)	(4)	(5)
Fine value$_{i,t}$ (S/ x 100)	0.049***			0.027***	0.010***
	[0.008]			[0.006]	[0.003]
	(0.000)			(0.000)	(0.015)
ln Fine value$_{i,t}$		0.030***	-0.046***		
		[0.005]	[0.016]		
		(0.000)	(0.000)		
Observations	13,536	13,536	6,768	6,768	6,768
Districts	1692	1692	1692	1692	1692
R-squared	0.0180	0.0180	0.00161	0.0112	0.00184
Mean of dependent variable	0.845	-0.171	10.68	0.0890	0.0334
District FE	Yes	Yes	Yes	Yes	Yes
Election x Province x '06 Category FE	Yes	Yes	Yes	Yes	Yes

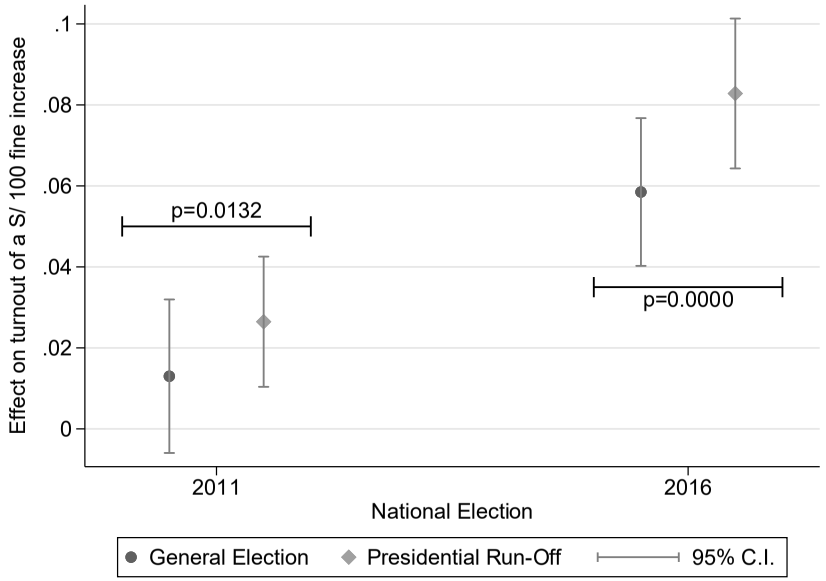
Notes: ln Voters is the natural log of the number of registered voters for the election cycle (same for general and run-off elections). Blank votes and invalid votes in columns 4-5 are measured as shares of the number of registered voters. Columns 1-2 use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. Columns 3-5 use data from the 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 clustered by region in brackets (25 units). Cluster-robust wild-bootstrap p-value calculated using boottest program created by Roodman et al. (2019) in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Figure A3: The Effect of the Reform to the Abstention Fine on Turnout for each 2006 poverty category

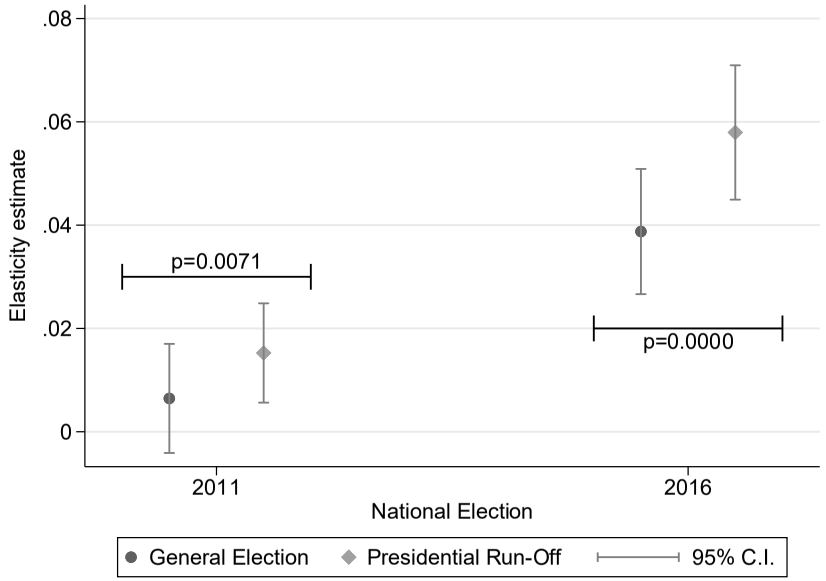


Notes: The graph shows point estimates and 95% confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with dummies for each combination of poverty categories in 2006 and 2010. All districts classified as high fine in 2006, remained in that category in 2010 and are absorbed by the time fixed effects. There is one omitted combination for each of the remaining 2006 poverty categories (medium fine and low fine), which corresponds in both cases to districts classified as medium fine in 2010. The omitted election is the 2006 presidential run-off. Regression includes district and province-election-category fixed effects (using 2006 classification). Regression includes 13,536 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 dotted line corresponds to October 2010, when districts were re-classified with regards to the abstention fine.

Figure A4: Election-specific Estimates of the Marginal Effect of the Abstention Fine on Turnout and the Elasticity



(a) Level effect



(b) Elasticity

Notes: Panel (a) shows point estimates and 95% confidence intervals of a regression of district-level turnout on the value of the fine for abstention interacted with a full set of election-date dummies. Panel (b) shows point estimates and 95% confidence intervals of the equivalent regression replacing turnout and the value of the fine for their natural logs. Regressions use data from national elections (General: Legislative and Presidential first round; Presidential Run-Off) for the years 2001, 2006, 2011 and 2016: 13,536 observations from 1,692 districts. The abstention fine is the same for all districts until the 2006 elections. Regressions includes district and province x election x 2006-poverty-category fixed effects. Districts are weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 clusters).

C Enforcement: Further Results

Table A6: The Marginal Effect of the Abstention Fine on Settlement of Outstanding Fines

Dependent variable: Share of fines	Settled$_{i,t}$		Paid$_{i,t}$		Excused$_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fine value$_{i,t}$ (S/ x 100) [a]	0.231***	-0.003	-0.020***	-0.026***	0.252***	0.024***
	[0.024]	[0.008]	[0.007]	[0.007]	[0.021]	[0.005]
Fine value$_{i,t}$ × $\mathbb{1}(2014/16)_t$ [b]		0.353***		0.009		0.344***
		[0.031]		[0.007]		[0.031]
Observations	11,721	11,721	11,721	11,721	11,721	11,721
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.03	0.13	0.001	0.001	0.03	0.13
Mean of dependent variable	0.37	0.37	0.20	0.20	0.17	0.17
p-value H_0: a+b=0		0.000		0.036		0.000
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x '06 Category FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Sample includes national elections from the years 2011 and 2016 and sub-national elections from 2006, 2010 and 2014. The value of the fine is measured in 100s of current Peruvian Soles (S/). Even-numbered columns include the interaction of the value of the fine with a dummy for the elections of 2014 and 2016. All columns weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A7: Effects of Expected Fine on Turnout

Dependent variable:	Turnout$_{i,t}$ (Mean=0.845)			ln Turnout$_{i,t}$ (Mean=-0.171)		
	(1)	(2)	(3)	(4)	(5)	(6)
(ln) Expected Fine value$_{i,t}$ (S/ x 100)	0.056***	0.064***	0.071***	0.021***	0.024***	0.019***
	[0.012]	[0.016]	[0.010]	[0.004]	[0.003]	[0.003]
Observations	13,536	13,428	13,260	13,516	13,396	12,892
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election-Province-Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes
Enforcement 2001 & 2006	06/10 avg.		06	06/10 avg.		06
Enforcement 2011	11	10	10	11	10	10
Enforcement 2016	16	14	14	16	14	14

Notes: Dependent variable is voter turnout (0-1) in columns 1-3 and the natural log of voter turnout in columns 4-6. All columns use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. The expected fine in columns 1-3 is equal to the value of the fine in 100s of current Peruvian Soles (S/) multiplied by the probability of enforcement. In columns 4-6, we use the natural log of the expected value of the fine. Probability of enforcement is proxied by the share of fines paid. In columns 1,2, 4 and 5, we use the average share of fines paid in the subnational elections of 2006 and 2010 as the probability of enforcement for 2001 and 2006. In columns 3 and 6, we just use the value from the the subnational elections of 2006. In columns 1 and 4, we use the actual shares from 2011 and 2016 for these elections. In columns 2, 3, 5 and 6, we use the subnational elections from 2010 for 2011 and those from 2014 for 2016. All columns include district fixed effects and province x election x 2006 poverty category (high fine, medium fine, low fine) fixed effects. All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A8: Improved Enforcement and the Long-run Effect of the Fine on Turnout

	Dependent variable: $\text{Turnout}_{i,t}$					
	Targeted districts	Drop Lima & Callao	Province capitals	Drop capitals	Δ fines settled	All
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Fine value}_{i,t}$ (S/ x 100)	0.020**	0.017**	0.020**	0.018**	0.020**	0.020**
	[0.008]	[0.009]	[0.008]	[0.009]	[0.008]	[0.009]
$\text{Fine value}_{i,t} \times \mathbb{1}(2016)_t$	0.051***	0.051***	0.046***	0.051***	0.041***	0.038***
	[0.005]	[0.005]	[0.005]	[0.005]	[0.006]	[0.006]
$\mathbb{1}(\text{Targeted District})_i \times \mathbb{1}(2016)_t$	0.006					0.011**
	[0.004]					[0.004]
$\mathbb{1}(\text{Province capital})_i \times \mathbb{1}(2016)_t$			0.008***			0.008***
			[0.002]			[0.002]
Δ fines $\text{settled}_i \times \mathbb{1}(2016)_t$					0.024***	0.020***
					[0.008]	[0.007]
Observations	13,536	12,192	13,536	12,048	13,386	13,386
Districts	1,692	1,524	1,692	1,506	1,692	1,692
R-squared	0.03	0.03	0.04	0.03	0.03	0.04
Mean of dependent variable	0.85	0.83	0.85	0.85	0.85	0.85
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is voter turnout (0-1). Data includes national elections (general and presidential run-off) for the years 2001, 2006, 2011 and 2016. The abstention fine is the same for all districts until the 2006 elections. The value of the fine is measured in 100s of current Peruvian Soles (S/). Column 1 includes the interaction of the 2016 dummy with an indicator for the districts in Lima and Callao that were targeted for coercive collection after 2012. Column 2 excludes the entire department of Lima and the province of Callao. Column 3 includes the interaction of a dummy for provincial capitals with the 2016 indicator. Column 4 excludes all provincial capitals. Column 5 includes the interaction of the 2016 dummy with the change in the share of fines settled between the municipal elections of 2006 and the municipal elections of 2014. Column 6 simultaneously includes all three interactions. All columns include district fixed effects and election x province x 2006 poverty category fixed effects. All regressions are weighted by the number of registered voters for the elections in 2001. Standard errors clustered by province (181 units in column 2, 186 units in column 4, 192 units in all others). *** p<0.01, ** p<0.05, * p<0.1

D Voter Registration: Robustness Checks

Table A9: The Value of the Abstention Fine and Age-specific Voter Registration, controlling for predicted voters

	Dependent variable: ln Voters in age-group$_{i,t}$					
	18-20	21-29	30-35	36-50	51-75	75+
	(1)	(2)	(3)	(4)	(5)	(6)
ln Fine value$_{i,t}$	-0.214***	-0.022	-0.046**	-0.055***	-0.051	-0.062
	[0.051]	[0.027]	[0.020]	[0.020]	[0.031]	[0.057]
ln $\widehat{\text{Voters}}_{i,t}$	0.584***	0.846***	1.126***	1.640***	1.389***	1.319***
	[0.187]	[0.296]	[0.205]	[0.100]	[0.102]	[0.195]
Observations	5,076	5,076	5,076	5,076	5,076	5,076
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.11	0.11	0.15	0.35	0.41	0.15
Mean of dep. var.	8.00	9.28	8.77	9.43	9.20	7.32
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x '06 Category FE	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 national elections for the years 2001, 2011 and 2016. ln $\widehat{\text{Voters}}_{i,t}$ is the natural log of the number of predicted voters in that age group, according to the 2007 population census. All columns include district fixed effects and election x province x 2006-poverty-category fixed effects. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A10: The Value of the Abstention Fine, Nighttime lights and Migration

Dependent variable:	ln Lights DN$_{i,t}$	ln Voters$_{i,t}$		Share born in district$_{i,t}$	ln Voters$_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fine value$_{i,t}$ (S/ x 100)	0.056	-0.076**	-0.099***	-0.074	-0.152***	-0.160***
	[0.064]	[0.031]	[0.037]	[0.085]	[0.047]	[0.047]
ln Night lights DN$_{i,t}$			0.170*			
			[0.090]			
Share born in district$_{i,t}$						-0.113**
						[0.049]
Observations	5,076	5,076	5,076	2,319	2,319	2,319
Districts	1692	1692	1692	913	913	913
R-squared	0.0007	0.0008	0.02	0.001	0.01	0.02
Mean of dependent variable	2.36	10.62	10.62	0.33	11.08	11.08
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year x Province x Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header. ln Night lights digital number (0-63) in column 1; natural log of the number of registered voters in columns 2,3,5,6; the share of population that reports being born in the district in the ENAHO national survey in column 4. The sample in columns 1-3 includes the national election years 2001, 2006 and 2011. The sample in columns 4-6 includes the national election years 2006, 2011 and 2016. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns include district fixed effects and year by province by 2006 poverty category fixed effects. Regressions are weighted by the number of registered voters for the elections in 2001. Standard errors clustered by province (192 units in columns 1-3, 175 units in columns 4-6). *** p<0.01, ** p<0.05, * p<0.1

Table A11: The Value of the Abstention Fine and Age-specific Voter Registration, controlling for access to DNI

	Dependent Variable: ln Voters$_{i,t}$					
	18-20	21-29	30-35	36-50	51-75	75+
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A - Baseline in reduced sample					
ln Fine value$_{i,t}$	-0.372***	-0.083***	-0.059*	-0.044*	-0.025	-0.039
	[0.058]	[0.025]	[0.031]	[0.026]	[0.036]	[0.079]
R-squared	0.04	0.002	0.001	0.001	0.0002	0.0002
	Panel B - Controlling for change in access to DNI					
ln Fine value$_{i,t}$	-0.348***	-0.063**	-0.037	-0.021	-0.003	-0.018
	[0.055]	[0.027]	[0.033]	[0.029]	[0.036]	[0.078]
Δ Share w/ DNI$_i$ $\times$ $\mathbb{1}(2011)_t$	1.183**	1.053***	1.078**	1.123**	0.999**	0.684
	[0.498]	[0.362]	[0.439]	[0.500]	[0.502]	[0.576]
Δ Share w/ DNI$_i$ $\times$ $\mathbb{1}(2016)_t$	1.688***	1.339***	1.591***	1.563***	1.593***	1.758***
	[0.604]	[0.420]	[0.526]	[0.598]	[0.612]	[0.632]
R-squared	0.06	0.01	0.01	0.01	0.01	0.01
Observations	2,460	2,460	2,460	2,460	2,460	2,460
Districts	820	820	820	820	820	820
Mean of dependent variable	8.35	9.63	9.12	9.78	9.53	7.64
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	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 election years 2001, 2011 and 2016. Δ Share w/ DNI$_i$ is the change in the share of ENAHO respondents that have a national identification document (DNI) between the post-reform years (post-2010) and the pre-reform years. All columns include district fixed effects and election x province x 2006-poverty-category fixed effects. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A12: The Value of the Fine and Voter Turnout controlling for Registration

Dependent variable:	ln Turnout$_{i,t}$		Turnout$_{i,t}$	
	(1)	(2)	(3)	(4)
(ln) Fine value$_{i,t}$ (S/ x 100) [a]	0.027***	0.009*	0.044***	0.016*
	[0.005]	[0.005]	[0.009]	[0.009]
(ln) Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ [b]		0.036***		0.049***
		[0.003]		[0.005]
ln Voters$_{i,t}$	-0.062***	-0.061***	-0.050***	-0.049***
	[0.005]	[0.005]	[0.004]	[0.004]
Observations	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692
R-squared	0.12	0.13	0.16	0.16
Mean of dep. var	-0.17	-0.17	0.85	0.85
p-value H_0: a+b=0		0.000		0.000
District FE	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes

Notes: ln Voters is the natural log of the number of registered voters for the election cycle; Sample includes national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. Regressions are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

E Web Searches: Additional Information

This section provides detailed information on the construction of the dataset on the popularity of various search terms in the Google search engine. For this purpose, we used the Google Trends online application, which we consulted in April 2018 (`https://trends.google.com/trends`). The application allows you to make a query on as many as five search terms simultaneously. The output is a relative search interest measure, available at monthly intervals, that takes positive integer values. This measure is set at 100 for the search term-month with the largest number of searches in the Google search engine.

These characteristics provided several complications. We had to search in batches of no more than five search terms at a time. In this regard, putting together very popular search terms with not-to-popular ones led to the latter being squashed against the lower bound of zero and presenting very little variation. Furthermore, we also needed to have common search terms included in different queries in order for the different relative scales to be made compatible. Once we delimited the set of search terms that we wanted to include in the sample, we tested with various combinations to determine the relative maximum popularity of each search term and created groups based on this criterion, in an attempt to lose as little variation as possible. Consecutive groups always had a common search term that allowed us to chain them and express all values in a common scale. The resulting search interest measure, which we refer to as the Google Trends index, takes a value of 100 for the search term "vicepresident" in April, 2016.

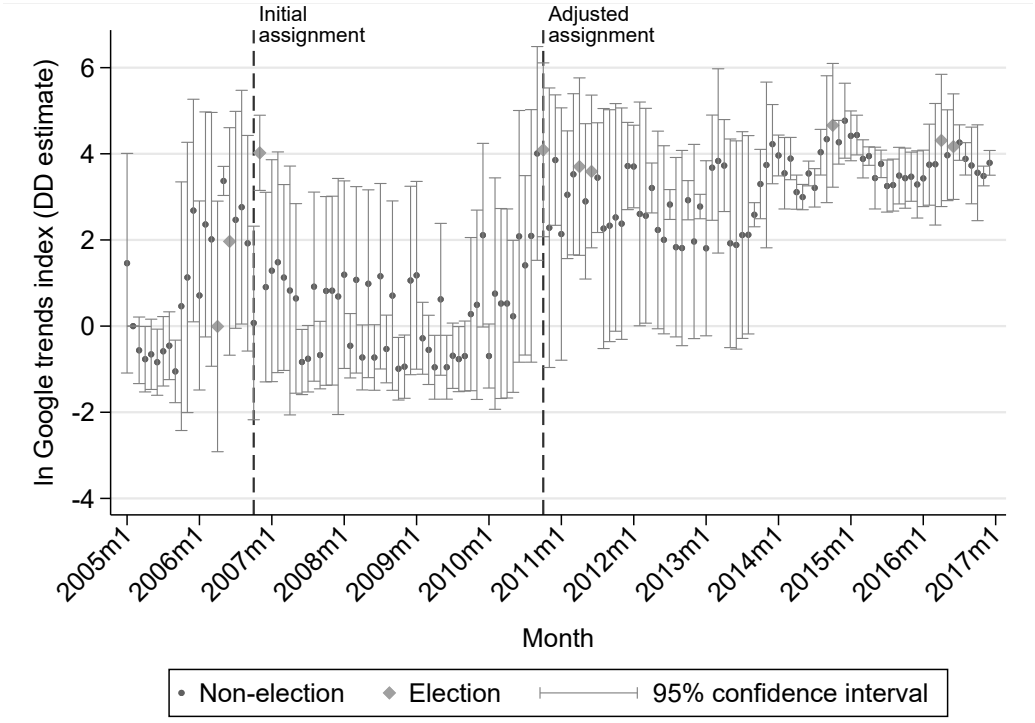
We limited the geographic scope to the country of Peru and collected monthly data from January 2005 to December 2016. We used used double quotation marks (" ") to avoid capturing Google searches for segments of multi-word search terms (e.g. "fine for not voting"). All queries were done in Spanish, in lower case and without any dyacritics. The full list of included search terms is presented in Table A13.

Table A13: Search Terms included in Google Trends Analysis

ID	search term	English translation	Fine-related	Comments
1	alcalde	mayor		
2	candidatos	candidates		
3	canon minero	mining canon		Mining royalty system
4	congreso	congress		
5	constitucion	constitution		
6	corrupcion	corruption		
7	corte suprema	supreme court		
8	departamento	department		Highest level of subnational government (See region).
9	desempleo	unemployment		
10	distrito	district		Lowest level of subnational government
11	dni	DNI		National identification number
12	elecciones	elections		
13	encuesta	opinion poll		
14	fujimori	Fujimori		Surname of former president (Alberto) and former presidential candidate (Keiko)
15	futbol	soccer		
16	gobierno	government		
17	impuesto	tax		
18	inflacion	inflation		
19	infracciones de transito	traffic violation		
20	jne	JNE		Government agency in charge of electoral regulation and oversight
21	keiko	Keiko		Fujimori, presidential candidate in 2011 and 2016
22	local de votacion	polling place		
23	mesa de votacion	voting table/booth		
24	miembro de mesa	election judge		
25	multa electoral	election fine	Yes	
26	multa onpe	ONPE fine	Yes	See ONPE
27	multa por no votar	fine for not voting	Yes	
28	noticias	news		
29	ollanta	Ollanta		First name of former president Ollanta Humala
30	onpe	ONPE		Government agency in charge of electoral organization
31	pbi	GDP		
32	pelicula	movie		
33	poder judicial	judiciary		
34	politica	politics		
35	porno	porn		
36	ppk	PPK		Initials of former president Pedro Pablo Kuczynski
37	presidente	president		
38	provincia	province		Intermediate level of subnational government
39	region	region		Highest level of subnational government (23 departments and 2 special provinces)
40	reniec	RENIEC		Government agency in charge of registry and identification
41	segunda vuelta	second round (run-off)		
42	television	television		
43	vicepresidente	vicepresident		
44	votar	vote (verb)		

Notes: All queries in Google trends used double quotations (“ ”) to avoid capturing Google searches for segments of multi-word search terms. All queries were done in lower case and without dyacritics. Queries were done with geographic scope limited to the country of Peru for the time period between January 2005 and December 2016.

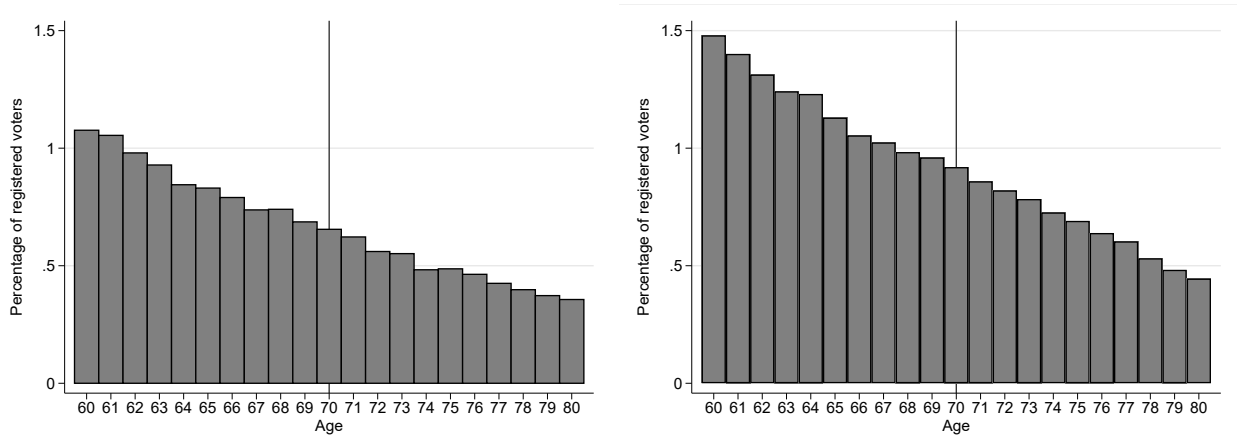
Figure A5: The Reform to the Abstention Fine and Information Acquisition (monthly level)



Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of a search-term popularity index from Google trends on a full set of month dummies interacted with an indicator for search terms related to the fine for abstention. Regression includes search-term and month fixed effects. The omitted month is February 2005. Regression includes 6,336 observations from 44 search terms. See 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. The dotted lines indicate the months in which the initial reform to the abstention fine and district classification took place (August 2006) and in which districts were reassigned to the poverty categories (October 2010).

F Senior Exemption: Additional Results

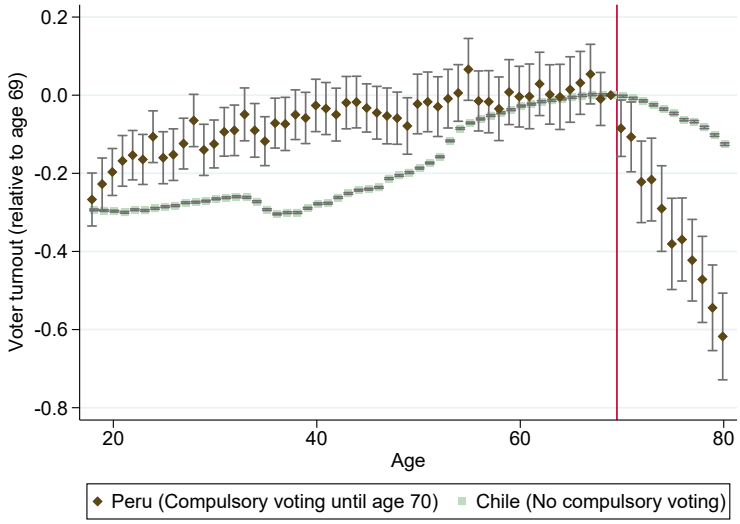
Figure A6: Distribution of Registered voters by age in Peru and Chile



(a) Peru **(b)** Chile

Notes: The graph in panel (a) shows the distribution of registered voters by age in the 2016 national election in Peru. The graph in panel (b) shows the distribution of registered voters by age in the 2017 national election in Chile. The total number of registered voters in Peru in 2016 was 22,901,954. The total number of registered voters in Chile in 2017 was 14,347,288.

Figure A7: Distribution of Registered voters by age in Peru and Chile



Notes: Figure shows estimates for all ages between 20 and 80 from the voting-booth-level regression in Peru and the individual-level regression in Chile. See text for further details.