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How effective are monetary incentives to vote? Evidences from a nationwide policy

Mariella Gonzales, Gianmarco León-Ciliotta, Luis R. Martínez

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Mariella Gonzales Gianmarco León-Ciliotta Luis R. Martínez*

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Abstract

We combine two natural experiments, multiple empirical strategies and administrative data to study voters' response to marginal changes to the fine for electoral abstention in Peru. A smaller fine leads to a robust decrease in voter turnout. However, the drop in turnout caused by a full fine reduction is less than 20% the size of that caused by an exemption from compulsory voting, indicating the predominance of the non-monetary incentives provided by the mandate to vote. Additionally, almost 90% of the votes generated by a marginally larger fine are blank or invalid, lending support to the hypothesis of rational abstention. Higher demand for information and larger long-run effects following an adjustment to the value of the fine point to the existence of informational frictions that limit adaptation to institutional changes.

Keywords: voter turnout, voter registration, compulsory voting, informational frictions, external validity, Peru

JEL codes: D72, D78, D83, K42

*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, 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, PACDEV, PECA, NOVAFRICA african development conference, Chicago-area development economics workshop, MPSA, 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, very little is known about voters' response to marginal changes to the size of these incentives.[1] Even though a larger fine mechanically increases the cost of electoral abstention, 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 leaves both the sign and the magnitude of the net turnout effect as open empirical questions. Whether any induced change to electoral participation affects election outcomes is also not clear, given that abstention may be an optimal strategy for those who are uninformed or uninterested in the electoral process (Feddersen and Pesendorfer, 1996).

The debate on compulsory voting is far from settled and the introduction of a mandate to vote has recently been endorsed by political theorists and prominent public figures.[2] A better understanding of the relationship between marginal changes to the abstention fine, political participation and electoral outcomes has important policy implications, given the potential trade-off between the increased effectiveness of a larger fine and its greater burden on those who are sanctioned. Existing research shows that compulsory voting leads to higher turnout even with very low fines or enforcement (Funk, 2007; Cepaluni and Hidalgo, 2016; Hoffman et al., 2017), but is uninformative about what would happen were the fine to change. Only two previous studies have analyzed monetary incentives to vote (Panagopoulos, 2012; León, 2017). However, both involve small field experiments of potentially limited external validity for large-scale policies, insofar as they fail to capture social multipliers, limited adaptation due to informational frictions, or potentially heterogeneous and multi-dimensional responses.

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 for many decades. We use granular administrative data covering four national election cycles over 16 years to document a sophisticated and

[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.

[2] During a speech in 2015, then US president Barack Obama claimed "it would be transformative if everybody voted, that would counteract money more than anything." (The Washington Post, 2015). Chapman (2019) argues that mandatory voting is consistent with the prominent role of elections in modern democracies and reinforces the political equality of all citizens.

multi-dimensional voter response along several margins, including turnout, registration, information acquisition and invalid and blank votes. 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 bundle of incentives provided by compulsory voting. This enables us to do a back-of-the-envelope calculation comparing the intensive and extensive margins of a compulsory voting regime. The contrast of our findings with the previous experimental literature allows us to illustrate some of the limitations to the external validity of small-scale field experiments in political economy. In particular, we show that informational frictions lead to limited adaptation to large-scale regulatory changes and to 'voltage drop' relative to experimental estimates.

Until 2006, the value of the abstention fine was homogeneous throughout Peru. A reform that year classified districts into three categories 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/] increase (approximately US$3) leads to 0.5 percentage points (pp) higher turnout, with a corresponding elasticity of 0.03.[3] The fact that these estimates are based on district-level variation means that they incorporate both direct and indirect effects caused by changes in the behavior of peers (Nickerson, 2008), which would not be possible with variation at the individual level.

This average effect masks a highly heterogeneous response along several dimensions. The effect of a same-sized fine increase is more than three times as large in the second election after the reform in 2016 than in the first one in 2011, indicating increased adaptation to the policy incentives over time. In the long run, we observe a substantial 5 pp turnout gap between high- and low-fine districts. The marginal effect of the fine is also almost 50% larger for the presidential run-off than for the general election taking place only two months earlier, suggesting differences in the marginal voters across election types. These heterogeneous effects highlight the potential for context dependence in highly-localized or short-lived field experiments studying voter mobilization initiatives, as has been common.

To dig further into the different margins of response, we decompose the turnout effect into separate effects on the number of votes cast (the numerator) and the number of registered voters (the denominator). Voters are automatically registered in their district of residence, which they can change by modifying the address on their national identification card (DNI).

[3]The percentage of settled fines (i.e., either paid or excused) is 22% in 2011 and 49% in 2016, indicating that the expected fine is smaller than the nominal one. Hence, we are likely underestimating the marginal effect of a monetary incentive provided with certainty. The increase in the share of settled fines is partly driven by measures taken by the government in 2012, including the creation of a coercive collections unit, which fortuitously targeted mostly high-fine districts. We verify that our results are robust to controlling for differential changes in enforcement. We also show that marginal increases to the value of the fine lead non-voters to substitute away from payment and to increasingly submit excuses instead.

We find that the number of registered voters decreases disproportionately in high-fine districts after the reform and estimate a registration elasticity of -0.05. This registration effect is only present for young adults and is mostly driven by first-time voters with ages 18-20, who can strategically manipulate their reported address at very low cost when they initially apply for a DNI. As with turnout, the marginal effect of the fine on registration is significantly larger in the longer run. However, the variation in the number of registered voters can explain no more than 20% of the marginal effect of the fine on turnout. Hence, the observed turnout response is mostly driven by people that remain in the same district and adjust their voting behavior rather than by those with a low propensity to vote self-selecting into low-fine districts.

Peruvian politics were highly volatile over the past decades, meaning that no party fielded a presidential candidate in all of the elections during the sample period. We are thus unable to study the impact of marginal changes to the abstention fine on party vote shares. An alternative but also relevant measure of the electoral impact of a stronger monetary incentive to vote is provided by the share of blank and invalid votes, which plausibly correspond to uninformed or uninterested voters. Focusing on the first round of the presidential election, we find that a S/ 10 increase to the value of the fine leads on average to a 0.37 pp increase in the number of blank (0.27 pp) or invalid votes (0.1 pp) as a share of registered voters, which is equivalent to 86% of the observed effect of the fine on turnout for this type of election. This effect is also larger in the longer run, providing further evidence of gradual adaptation. These findings indicate that forced electoral participation has little impact on election outcomes and stand in contrast to previous studies showing that the removal of voting barriers strongly affect representation (Miller, 2008; Cascio and Washington, 2013; Fujiwara, 2015). In this regard, our results lend empirical support to the hypothesis of rational abstention (Feddersen and Pesendorfer, 1996) and undermine changes to representation as a motivation for policies aiming to increase voter turnout (Lijphart, 1997).

The growing adaptation to the regulatory change that we document along several dimensions could be the result of social multipliers (Nickerson, 2008) or the progressive weakening of habits (Coppock and Green, 2016; Fujiwara et al., 2016). Also plausible is the slow diffusion of information about the modified policy incentives. We hypothesize that this mechanism is particularly relevant in our setting, given existing evidence that voters in Peru remained highly unaware of the modified value of the abstention fine as late as 2010, four years after the reform (León, 2017). To examine this hypothesis, we construct a monthly panel of queries for 44 different search terms in the Google search engine for the period 2005-2016. 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 in Peru search the internet for information about the fine increases disproportionately after the reform and is particularly large in the later years. This result suggests that people are not perfectly informed about the regulatory change and must endogenously increase their demand for information in response to the reform. It underscores a limitation specific to interventions in political economy, namely the informational frictions about changes to the rules regulating the interaction of civilians with the state.

The pecuniary incentive provided by the abstention fine is only one of the incentives to vote provided by compulsory voting. These include the non-monetary sanction imposed on non-voters (e.g., restrictions on government services) 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 what is the aggregate effect of the bundle of incentives provided by compulsory voting. To answer this question, we use rich data at the 'voting-booth' level and leverage idiosyncratic variation in the age composition of the electorate in the 2016 elections. A second natural experiment is provided by the exemption from the mandate to vote for citizens above the age of 69. We compare turnout rates, within the same district or polling station, 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 the other voters. 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 data on participation in the 2017 presidential election in neighboring Chile, a comparable country without compulsory voting, to estimate a benchmark for the natural decline in electoral participation among the old. In Chile, we find only a 5 pp drop in turnout between ages 69-75. A back-of-the-envelope calculation using our elasticity estimates for 2016 yields that a 100% fine reduction would lead to a drop in turnout only 18% as large as the one caused by the exemption from compulsory voting between ages 69-72 (10% of the drop by age 75). These results show that the extensive margin of compulsory voting introduction is far more effective at increasing voter turnout than the intensive margin of changes to the abstention fine. They suggest that a compulsory voting regime with low 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 and provides several pieces of evidence indicative of a highly strategic behavior on the part of voters (Blais, 2000; Feddersen, 2004).[4] As mentioned above, two previous studies have used field experiments

[4]Classic treatments include Downs (1957); Riker and Ordeshook (1968); Palfrey and Rosenthal (1985).

to estimate the marginal effect of monetary incentives on turnout.[5] Panagopoulos (2012) provides varying payments to voters in two local elections in California, while León (2017) provides information on the modified value of the abstention fine to voters in Peru. The latter studies the same reform we do, making it particularly relevant for comparison.[6] Both papers find a positive short-run effect of monetary incentives on turnout. In the case of León (2017), this effect is more than three times as large as the average effect we report. We show that the turnout gain from the large-scale provision of pecuniary incentives differs substantially from the experimental estimates and we provide new evidence on informational frictions to adaptation as a contributing mechanism to the observed voltage drop. We further show that the vast majority of additional votes caused by marginal fine increases are blank/invalid.

Our paper also complements the empirical literature on the effects of compulsory voting laws (Funk, 2007; Fowler, 2013; Cepaluni and Hidalgo, 2016; Hoffman et al., 2017; Bechtel et al., 2018). Previous research has largely focused on the extensive margin effects of the introduction or dismantling of compulsory voting regimes on turnout and electoral outcomes. Our contribution to this literature is to simultaneously estimate the marginal effect of the abstention fine and the aggregate effect of exemptions from compulsory voting in order to establish the relative importance of the monetary incentive provided by the fine.

Another large literature has studied a wide array of voter mobilization initiatives (Green et al., 2013; Green and Gerber, 2015; Gerber and Green, 2017), but most of these interventions have only been tested through small-scale field experiments.[7] Our paper connects this literature with a growing literature analyzing the external validity of experimental studies for policy scale-up (Deaton, 2010; Al-Ubaydli et al., 2017; Banerjee et al., 2017; Muralidharan and Niehaus, 2017; Vivalt, 2017; Rosenzweig and Udry, 2018). Our findings illustrate the issues of context dependence and voltage drop that have been highlighted by the latter. However, while existing studies have 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 a set of relatively understudied challenges faced in the scale-up of experimental interventions in political economy. In particular, we show that informational frictions lead to attenuated effects and slow adaptation to regulatory changes.

Our findings on the irregular change of voters' registered address speak to the literature on voter misbehavior (Nichter, 2008; Finan and Schechter, 2012), as well as to that on the unintended consequences of targeted policies (Camacho and Conover, 2011; Cassan, 2015).

[5] Loewen et al. (2008) and Shineman (2018) provide fixed monetary incentives for turnout to voters in local elections in Canada and the US as part of experiments studying political participation and knowledge.

[6] Concurrent work by Carpio et al. (2018) exploits our same reform to study the effect of voter turnout on candidates' party affiliation. The papers differ in their research question, scope, outcomes, data and method.

[7] Exceptions include Arceneaux et al. (2006); Enos and Fowler (2016); Marx et al. (2017); Pons (2018).

We document a previously overlooked form of voter misbehavior caused by a geographically-targeted policy in a setting with limited state capacity. Unlike vote- or voter-buying, this misbehavior is not aimed at affecting electoral outcomes, but could affect representation. Additionally, our results on information acquisition 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 have directly provided information through salient interventions, we document the endogenous acquisition and slow diffusion of information about modified policy incentives in the wild.

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.[8] 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.[9] 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 automatically registered to vote.[10] 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 may live with their parents or other relatives and may not have any valid documents to their name. Poorer people may also experience difficulties meeting 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

[8]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.

[9]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.

[10]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).

with a sticker on their DNI as proof of participation.[11] 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.[12] This is similar to other 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).[13] 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.[14] 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).[15] In August 2006, 10 days after the inauguration of president Alan García, Congress approved a law that reformed the abstention fine. The law classified districts into three categories based on their level of poverty: non-poor, poor, or extreme poor. All citizens experienced a reduction to the value of the fine, but those registered to vote in districts in the latter categories enjoyed larger reductions. For voters in districts classified as non-poor, the fine was cut in half to 2% of the UIT, while for those in dsitricts classified as poor and extreme poor, the new fine was set at 1% and 0.5% of the UIT, respectively. These amounts roughly corresponded to US$25, US$12.5, and US$6. For the remainder of the paper, we refer to non-poor districts as 'high-fine' and to poor and extreme poor districts as 'medium-fine' and 'low-fine', respectively.

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

[11] 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.

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

[13] 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.

[14] 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.

[15] 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.

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. Importantly, the passing of the law was barely covered by the popular press and voters remained mostly uninformed about the reform for several years (León, 2017).

The classification of districts into the three categories was delegated to the national statistical office (Instituto Nacional de Estadística e Informática, INEI), but the criteria used for the initial classification released in 2006 by the national electoral jury (Jurado Nacional de Elecciones, JNE) remains unclear.[16] However, the first and only elections to be held under this initial classification were the subnational elections of November 2006. Shortly before the 2010 subnational elections, the JNE released a new district classification, which still remains in place.[17] In the new classification, which we can perfectly replicate, districts were assigned to the 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).[18] As a result of the knife-edge criterion employed, districts with similar levels of poverty were assigned to different categories (see Appendix Figure A3). We exploit this below for identification and the analysis of heterogeneous effects.

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

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

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

[18]The maps in Appendix Figure A2 provide the location of the districts in each category in 2006 and 2010. We observe that districts in the three categories are spread throughout the country and that a large number were reclassified. Appendix Table A1 shows that in 2006, 10.4% of districts were assigned a high fine, 43.4% a medium fine and 46.2% a low fine. 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.

settled (see Appendix figure A4). However, enforcement improved substantially in 2012, when a collection unit was created within the JNE.[19] 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.

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 the exemption from compulsory voting after age 70 and online information acquisition for sections 8 and 7 respectively.

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.[20]

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

[19] 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.

[20] 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.

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 changes to 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 this higher level provides a unique opportunity to capture both direct and indirect effects caused by changes in the behavior of peers, something that 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. As mentioned above, districts were initially classified into the three fine categories shortly after the 2006 elections, but were reclassified before the next election in 2011. It is the variation provided by this latter classification that we exploit in our estimations. Despite the criteria employed in 2006 remaining unclear, this assignment may have been based on informative district characteristics that we do not observe. 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 that were assigned to different categories, 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:

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

where $y_{d,p,e}$ is an outcome of interest in district d from province p in election e. α_d is a

[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.

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 within-province correlation.

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:

$$y_{d,p,e} = \alpha_d + \delta_{p,e}^{\text{cat06}} + \nu \text{ Fine}_{d,e} + u_{d,p,e} \tag{2}$$

where $\text{Fine}_{d,e}$ is the value of the abstention fine in 100s of current Peruvian soles (S/) and α_d and $\delta_{p,e}^{\text{cat06}}$ 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. $u_{d,p,e}$

[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 the discovery of abnormal increases in the number of registered voters for the 2006 subnational elections across the country. There were at least 100,000 swallow voters in more than 150 districts 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.

is an error term clustered again at the province level. We weight all our regressions by the number of registered voters in 2001 to capture average effects at the voter level.

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 results from the difference-in-difference specification, which lend support to the identifying assumption of parallel trends and provide evidence of an increasing response over time. We then show estimates of the marginal effect of the fine on turnout and the corresponding elasticity. Next, we consider heterogeneous impacts by type of election, time horizon, and poverty status. Finally, we examine the robustness of the results to changes in enforcement during the sample period.

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, which is the omitted election. The monetary penalty for electoral abstention 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 in October 2010.

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, for the same modified fine values, 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-

[25] Appendix Table A2 shows the corresponding estimates. Appendix Figure A6 shows results from a more disaggregate specification that estimates separate coefficients for each combination of 2006/2010 assignments.

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 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. As mentioned above, these estimates of the marginal effect of the fine on turnout incorporate the general equilibrium effect arising from voters taking into account the fact that other voters in their district also face a modified incentive.

The remaining columns in Table 1 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 shares of poor and extreme poor. This is a very stringent specification, given that treatment assignment is based on these shares. It exploits the fact that these shares do not perfectly predict a district's category as a result of the knife-edge assignment criterion employed. 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. Furthermore, we cannot reject that they are equal to the baseline estimates in column 1 at conventional levels. Column 5 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.[27] Controlling for these educational attainment shares leads to somewhat larger estimates. Finally, column 6 includes the log number of polling stations

[26] Reassuringly, these results are very similar to those reported by Carpio et al. (2018) using a Regression Discontinuity Design for the subnational elections of 2010 and 2014.

[27] This information is not available for 2006. The point estimate (standard error) of the marginal effect of the fine in the baseline specification for this reduced sample is 0.040 (0.010). The corresponding elasticity estimate (standard error) is 0.023 (0.006).

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.[28]

4.2 Heterogeneous Effects

Table 2 show 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 7. It is also consistent with dynamic peer effects if the marginal voters affected by the changed monetary incentives increasingly drive others to also not vote as time goes by (Nickerson, 2008; Giné and Mansuri, 2018; Chong et al., 2019). A third possibility is that habit formation acts as a countervailing force and that those that are induced to not vote by the lower fine in 2011 become intrinsically less likely to vote in 2016, on top of new marginal voters that are affected subsequently (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-off elections 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. This heterogeneity is unlikely to be driven by increased learning about the reform over time, given that the two elections are held less than two months apart.[29] It suggests instead that the

[28] Results are also robust to the number of stations or the ratio with respect to registered voters or area.

[29] Appendix Figure A5 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 and the elasticity across election

marginal voters affected by adjustments to the value of the fine differ across election types. One plausible explanation is that voters are more intrinsically or extrinsically motivated to participate in the general election than in the presidential run-off.[30] This is consistent with the systematically larger turnout that we observe in the general election (Appendix Figure A1). A stronger intrinsic motivation to vote in the general election may result from a stronger sense of civic duty in the first election of the cycle, from voters caring more about the outcome of the legislative than the presidential election or from them deriving a greater benefit from expressing their preferences over a larger number of choices. They may also face stronger pressure to participate in the general election from local political brokers.[31]

Columns 3 and 6 examine potential heterogeneity in the marginal effect of the fine and the elasticity depending on the shares of non-extreme poor and extreme poor in the district. 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 and extreme poor are both positive and large, with the former being much larger and statistically different from the latter. For the average person in non-extreme poverty, a S/10 fine increase leads to a 0.81 pp increase in turnout (elasticity of 0.068), while for the average person in extreme poverty, a same-sized increase to the fine leads to 0.32 pp higher turnout (elasticity of 0.019). In both cases the net effect is statistically different from zero. These results indicate that it is the non-extreme poor who are mostly affected by marginal changes to the abstention fine. For the non-poor, who are plausibly more intrinsically-motivated and also disproportionately affected by the restrictions placed on non-voters (Cepaluni and Hidalgo, 2016), the incentive to vote is high enough to make

types are statistically significant even within the same year.

[30] 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 (negatively affected by the abstention fine) and a random one (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 than in the run-off, due to the smaller number of voters it affects at the margin.

[31] Another possibility is that supporters of presidential candidates that do not progress to the run-off feel disillusioned and, thus, less compelled to turn out to vote. Appendix Table A4 tests for this possibility by examining whether the effect of the fine on turnout in the run-off differs depending on the first-round vote share of the candidates progressing to the final stage. We do not find evidence of a heterogeneous effect. A final possibility is that the heterogeneity is driven by characteristics of the three candidates progressing to the run-off in 2011 and 2016: Keiko Fujimori both times, facing Ollanta Humala in 2011 and Pedro Pablo Kuczynski in 2016. In this regard, it is reassuring that the effect of the abstention fine in the general election in columns 2 and 7 is only slightly lower than the average effect, as voters in the legislative election choose among different candidates across regions and they are choosing from a large pool of candidates in the first round of the presidential race. These results ensure that we are not simply capturing differential abstention in response to changing characteristics of the top contenders in the presidential election.

them non-responsive to marginal changes to the value of the fine. The burden of small fine changes is also likely to be negligible for them.

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 (Muralidharan and Niehaus, 2017). As a result, the external validity of the previous studies by Panagopoulos (2012) and León (2017) on the effectiveness of monetary incentives to vote could be compromised. In section 7 below we compare our estimates to those of these previous studies and we delve into informational frictions and gradual adaptation as one mechanism contributing to 'voltage drop' in the effectiveness of the large-scale policy.

4.3 Enforcement of the Fine

The interpretation of the marginal effects of the fine must take into account the probability of enforcement. The percentage of settled fines (i.e., paid or excused) grows from 22% in 2011 to almost 50% in 2016 (Appendix Figure A4). Although the latter figure is far from negligible, the expected fine remains much smaller than the nominal one, indicating that we are likely underestimating the marginal effect of a monetary incentive provided with certainty. Naturally, the share of fines that are paid is itself affected by the value of the fine. Appendix Table A5 shows that a higher value of the fine leads to a lower share being paid. 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 indicate that whether people settle an outstanding fine or not depends on factors other than the value of the fine (e.g., the non-monetary burden).

The explanation for the observed increase in the share of fines settled lies instead in the improvements to enforcement carried out by the Peruvian government over this period. As mentioned in section 2, the national government created a collections unit within the JNE in 2012. Even though our estimates of the marginal effect of the fine on turnout for the 2011 elections are unaffected, our finding of a substantially larger effect in 2016 could be compromised as a result. In this regard, it is reassuring that there is no evidence that the reform to the abstention fine prompted the toughening of enforcement or that the categories determining the value of the fine were intentionally used to target the renewed efforts at fine collection. Still, the aggregate data shows that the increase in settled fines was predominantly concentrated in districts in the high-fine category (Appendix Figure A4). To tackle the potentially confounding effect of improved enforcement, Appendix Table A6 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.[32] We find that our predictors of improved enforcement are all positively correlated with voter turnout in 2016. However, inclusion of additional controls leads to a reduction of no more than 20% in the magnitude of our estimated long-run effect. The results are also robust to the exclusion of the districts we identified as targeted for coercive collection.

5 The Value of the Abstention Fine and Voter Registration

In this section we disaggregate the marginal effect of the abstention fine on voter turnout into separate effects on the number of votes (the numerator) and the number of voters (the denominator). This analysis allows us to establish whether the documented effect of the fine on turnout is driven to some extent by relocation to districts with lower fines by voters with a low propensity to vote rather than by an actual 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 relocate electorally across districts by reporting a different address. Proof of address should be provided for such a change, but in practice this requirement is often waived.[33] We begin by examining whether the differentiated fine across districts led to disproportionately higher voter 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. Hence, 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, however, we find evidence consistent with systematic voter relocation

[32]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.

[33]Since 2015, 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 directly investigated, the authorities made increased efforts to detect instances related to voter-buying during the sample period (see footnote 23).

from high- and medium-fine districts to low-fine ones. Specifically, voter registration grows approximately 4.4% more in low-fine 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.[34] All of these differences are statistically significant at the 1% level. Relative to medium-fine districts, those with a high fine experience a decline in the number of registered voters, but it is small and insignificant (-0.7% in 2011, -2.1% in 2016). Column 1 in Table 3 provides the respective elasticity estimate from equation (2). A larger fine leads to a smaller number of registered voters in the district, with an estimated registration elasticity of -0.046.

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 and have to acquire their adult DNI for the first time. On the one hand, young adults are required to obtain and pay for a new DNI when they turn 18 years old in any case. On the other hand, young adults are 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 3-8 in Table 3 provide separate estimates of equation 2 for the log number of registered voters in six different age groups. This information is not available for 2006, but column 2 shows that the aggregate effect is very similar in this smaller sample. We find that the elasticity monotonically decreases with age and is only statistically significant for the first two groups, corresponding to ages 18-20 and 21-29. In particular, column 3 shows a registration elasticity of -0.28 for the 18-20 age-group, which is six times larger than the average effect. In the online appendix we verify that these results are robust to several further tests.[35] We conclude 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, which differs from the politically-driven 'voter buying' documented in previous studies (Hidalgo and Nichter,

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

[35] Appendix Table A7 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 A8 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 A9 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 'misregistered' young adults (i.e., students) in low-fine districts to update the address in their DNI. 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.

2016), is producing a potentially harmful mismatch in representation. This behavior is also likely to have further detrimental consequences on electoral participation, insofar as voters with an arguably low propensity to vote are self-selecting to districts where they face even weaker incentives to do so in the future.

We next examine the extent to which the response in registration is driving the turnout effect documented above. Column 1 in Table 4 provides again the estimate of the average registration elasticity for comparability. 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 cast as the dependent variable. The elasticity of votes is substantially smaller than that of voters, consistent with the positive turnout elasticity documented above.[36] This indicates that the mechanical effect leading from fewer voters to fewer votes is being offset by the increasing propensity to vote by those voters that are exposed to the higher fine and do not change district. 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 than in 2016, when it becomes small (-0.009) and statistically insignificant. In the latter year, the positive effect of the fine on the propensity to vote is large enough to fully offset its negative effect on the number of voters.

Finally, to gauge the relative importance of registration in explaining the positive effect of the fine on turnout, we re-estimate equation (2) including log registered voters as an additional control. Even though log voters fits the description of a 'bad control' à la Angrist and Pischke (2009) in this regression, the sensitivity of the turnout elasticity to the additional control can prove informative about the mediating effect of voter registration. Columns 5-8 in Table 4 show these results. Log voters is negatively correlated with voter turnout in all of these columns, providing suggestive evidence of adverse selection by people with a low propensity to vote into low-fine districts. However, column 5 shows that the average turnout elasticity drops only 10% when we control for voter registration, relative to the baseline estimate in Table 1. Similarly, column 6 shows that the turnout elasticity is 20% smaller in 2011 and 7% smaller in 2016 after controlling for voter registration, relative to the estimates in Table 2. Columns 7 and 8 show similar results in levels rather than logs.

We conclude that despite the fact that some young voters are responding to the reform by strategically changing their registered address to low-fine districts, the resulting change in the composition of the electorate explains no more than 20% of the observed marginal effect of the abstention fine on voter turnout. However, the documented response in voter

[36]Note that the turnout elasticity estimated in panel B of Table 1 is equivalent, by construction, to the elasticity of votes cast in column 3 of Table 4 minus that of voter registration in column 1. To see this, define T as turnout, V as the number of votes cast, R as the number of registered voters and F as the fine. $T \equiv \frac{V}{R}$. Hence, $\ln T = \ln V - \ln R$ and $\frac{\partial \ln T}{\partial F} = \frac{\partial \ln V}{\partial F} - \frac{\partial \ln R}{\partial F}$.

registration provides an example of the unintended consequences that large-scale, targeted policy incentives can have in a setting with limited state capacity (Camacho and Conover, 2011; Cassan, 2015). 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.

6 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. Unfortunately, Peruvian politics have been highly volatile in the past decades and no party systematically fielded candidates in the presidential elections during the sample period.[37] We are thus unable to study party vote shares. However, we do observe the number of invalid and blank votes in each election and have reason to think that these may be affected by the value of the fine. Theoretical models of rational abstention show that uninformed voters may find it in their best interest to refrain from participating (Feddersen and Pesendorfer, 1996). Hence, we can plausibly expect them to cast an invalid or blank vote if forced to participate. Empirically, 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 are between 1.5 and 3 additional invalid votes.

In Peru, a vote is considered blank if the ballot is deposited completely unmarked. A vote is considered invalid if it has any mark other than a cross (+) or an '×' symbol on the logo of one party or the picture of the respective candidate.[38] Blank votes provide a strong indication of intentional political behavior, but invalid votes cannot be thought of exclusively as an indication of mistakes in voting because people who would otherwise vote

[37]President Alberto Fujimori (1990-2000) fled the country without finishing his term 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.

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

blank will often intentionally cast an invalid vote in order to ensure that the ballot cannot be manipulated ex-post. Both blank and invalid votes are subtracted from the total number of votes before calculating each candidate's 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, in the analysis that follows we focus 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 column 1 of Table 1 (panel A). 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 turnout in column 1. We find that a S/10 fine 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 exclusively driven by the 2016 election.

These results indicate 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. Out of the 0.43 pp of additional turnout that a S/ 10 fine increase generates in the first round of the presidential election, 0.37 pp correspond to blank and invalid votes, equivalent to 86% of all the extra voters. 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 information (Downs, 1957; Lopez de Leon and Rizzi, 2014). Empirically, the magnitude of our estimated effect of the fine on invalid/blank votes is substantially larger than the previous findings on the extensive margin of compulsory voting (e.g., Hoffman et al., 2017), indicating that the voters affected by marginal monetary incentives may be particularly uninformed or uninterested.

A frequently espoused motivation for the implementation of policies aimed at increasing turnout is the possibility that increased participation may affect electoral outcomes (Li-

jphart, 1997). The results above indicate that this is not always the case and stand in contrast to previous findings on the removal of voting restrictions (Miller, 2008; Cascio and Washington, 2013; Fujiwara, 2015). Voters that are forcibly brought to the polls by marginal monetary incentives cast almost exclusively invalid and blank votes, fundamentally undermining changes to representation as a motivation to implement policies aimed at increasing voter turnout. Consistent with the previous literature, this result may not extend to settings in which there are substantial barriers to political participation that differ depending on demographic characteristics or party affiliation (Fraga, 2018).

7 Informational Frictions to Adaptation: Evidence from Web Searches

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 'voltage drop' in large-scale treatment effect. 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. 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, he 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 increase in turnout of 0.49 pp for a S/10 increase in the value of the abstention fine 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 voters we study are imperfectly informed about the modified value of the fine, while the treatment in León (2017) involved providing salient

and individualized information about these changes to voters. Lack of knowledge about the reform or low salience of the modified value plausibly lead to imperfect compliance and to a dampened marginal effect of the fine on turnout. In this regard, León (2017) shows that the average perception of the value of the abstention fine at baseline in his sample was S/124 (standard deviation S/54), very close to its pre-reform level of S/136.

To better understand the underlying acquisition of information about the abstention fine, we use data on nationwide internet searches.[39] Using publicly-available country-level data 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 A10 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 on the '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 in Peru grew disproportionately to other search terms after the reform.[40] We estimate the following specification:

$$\ln \text{Google trends index}_{i,m,y} = \theta_i + \omega_m + \sum_{\tau} \lambda_\tau \left[\mathbb{1}(\text{fine-related})_i \times \mathbb{1}(\text{year} = \tau)_y\right] + \zeta_{i,m} \quad (3)$$

where the dependent variable is the natural log of one plus the Google Trends index for search term i in month m in year y. θ_i is a search-term fixed effect and ω_m is a month fixed effect. These fixed effects absorb persistent differences in popularity across search terms and common shocks to Google searches affecting all terms equally (e.g., improved internet access). The coefficients of interest, λ_τ, tell us how the relative popularity of the search terms related to the fine changes with respect to the omitted year, which is 2005.[41] $\zeta_{i,t}$ is an

[39] Approximately one third of Peruvians used the internet in 2007 and almost one half used it in 2016, making an exercise based on internet searches meaningful with regards to learning about the acquisition of information by a sizable share of the population (INEI, 2018).

[40] We used the Internet Archive to verify that the ONPE website provided information about the value of the fine and whether people had any outstanding fines as far back as February 2005, which corresponds to the start of our sample period on web searches.

[41] Appendix Figure A7 shows equivalent results from the disaggregate specification at the monthly level.

error term that we cluster two-way by search-term (44 clusters) and month (144 clusters).

Figure 4 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, as in the 2007-2009 period. 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 voters only slowly and partially becoming aware of the regulatory changes of the fine and suggest that informational frictions to adaptation contribute to imperfect compliance to large-scale policy incentives and to voltage drop relative to comparable experimental interventions. They also help us explain our previous finding of a systematically larger response to marginal fine changes along several dimensions in the longer run and suggest that our effect of the large-scale policy may eventually converge to the estimate of the salient informational treatment in (León, 2017). Furthermore, the increasing alleviation of informational frictions over time may be complemented by other mechanisms at play, including social contagion of diminished electoral participation (Nickerson, 2008) and the incremental eroding of voting habits (Fujiwara et al., 2016).

Our finding of endogenous information acquisition in response to the large-scale policy reform highlights a limitation specific to interventions in political economy, namely that citizens may be plainly unaware about policies that modify the institutional context that shapes their behavior. This limitation differs from the differences in service delivery between NGO workers and government bureaucrats that the previous literature has mostly focused on (Davis et al., 2017; Bold et al., 2018). It implies, for instance, that the results from many field experiments that involve 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 information and learn about any modified incentives, generating a problem akin to endogenous take-up. Technically, direct

information provision about a given incentive allows the researcher to estimate the combined Local Average Treatment Effect (LATE) of being exposed to the incentive and knowing about it, while large-scale policy analysis provides an estimate of the Intention to Treat Effect (ITT) that does not condition on information acquisition. In settings with costly and imperfect information, the ITT is likely to be the more relevant parameter for cost-benefit analysis and implementation decisions.

8 The Value of the Abstention Fine and 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 voter 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 behaviors that society deems desirable 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 aggregate effect of compulsory voting on turnout, but comparing these with our estimate of the elasticity with respect to the value of the fine would require somewhat strong assumptions about the external validity of findings from other settings to Peru. Our objective is to benchmark our estimated 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 analysis, 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 according to a 'voting group' number that appears on their DNI and they can only vote at that specific 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 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.[42] Our empirical strategy compares 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 in 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:

$$\text{turnout}_{b,d,e} = \alpha_d + \gamma_e + \sum_{\tau \in \{16,\ldots,122\} \setminus \{69\}} \lambda_\tau \ \text{share}(\text{age} = \tau)_b + \epsilon_{b,d,e} \tag{4}$$

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 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., 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

[42]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.

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 (4) for Chile, with district and run-off fixed effects, having as dependent variable a dummy equal to one if the individual voted.

Panel (a) in Figure 5 shows the estimates of equation (4) 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 and are 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 , corresponding to a three-year age window, is the same as that between ages 19 and 69, corresponding to a fifty-year window.

Given our previous findings on slow adaptation to marginal fine changes, it seems plausible that the drop in turnout between ages 69 and 75 is mainly driven by a staggered response to the exemption from compulsory voting.[43] 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 respectively.[44] For both countries we fail to observe any systematic difference in electoral participation between the ages of 60 and 69. As seen in panel (a), voter turnout plummets in Peru starting at age 70. In the case of Chile, which lacks compulsory voting at any age, we only observe a smooth decline in voter turnout after age 69, amounting to no more than a five percentage-point drop by age 75. We conclude that the sharp decline in turnout observed in Peru between ages 69 and 75 can be attributed almost entirely to a staggered response to the modified incentive for participation caused by the exemption from compulsory voting and not to other characteristics of the elderly.

In Table 6 we examine the robustness of these results as well as some potential heteroge-

[43] 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.

[44] Appendix Figure A8 shows a smooth density of the age distribution around the cut-off for both countries.

neous effects. To facilitate interpretation, we re-estimate equation (4) 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 modified and more conservative specification aggregating the shares of 70- to 72-year-old voters instead. Consistent with Figure 5, the turnout effect of the exemption from compulsory voting drops as we restrict the set of age groups affected by the exemption, but remains substantial at 13.4 pp. Column 5 shows results from yet another modified specification in which we multiply each of the shares with ages 70 to 75 by the number of elections without compulsory voting to which each cohort has been exposed before adding them, while still controlling flexibly for the rest of the age distribution as in equation (4).[45] A one-unit increase in this new variable is equivalent to having all registered voters in that booth being exposed to one additional election without the mandate to vote. The results indicate that each additional election without compulsory voting leads to a 12.9 pp drop in the probability of voting, 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 results on marginal fine changes in Table 2 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 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 in Table 2 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 in the

[45]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. The latter were also exempt in the 2011 national elections.

higher end of the results reported by the existing literature, but is comparable to previous findings by Fowler (2013) and Bechtel et al. (2018) in Australia 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 the 2016 elections. 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.[46] In other words, the monetary incentive provided by the fine explains less than 20% of the aggregate effect of compulsory voting. This conclusion does not fundamentally change if we subtract the natural decline in electoral participation between ages 69 and 75 that we observe in Chile.

We conclude that the predicted effect on voter turnout of a complete elimination of the monetary incentive provided by the abstention fine pales in comparison to the effect 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 what it is in high-fine districts, but this difference only drives at the most a roughly five-point gap in turnout between these sets of districts. 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 employed to enforce it has important policy implications. It suggests that countries may be able to extract most of the potential gains from compulsory voting at a low administrative and distributional cost by setting the monetary sanction for non-compliance at a relatively low value.

9 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. 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 senior citizen exemption from compulsory voting after age seventy.

[46] According to the estimates in Figure 5, 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.

We find an aggregate compulsory voting effect in the range of 20-40 percentage points, of which we can explain no more than 20% with our estimate of the fine-elasticity. 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. Additionally, for every point of voter turnout generated by a marginal increase to the abstention fine there is a 0.87 point increase in blank and invalid votes, which is consistent with the hypothesis of rational abstention by the uninformed (Feddersen and Pesendorfer, 1996). Lastly, our estimate of the causal effect of a marginal fine increase on voter turnout is less than a third of the size of the experimental estimate provided by León (2017) for the same setting. We argue that informational frictions to adaptation contribute to 'voltage drop' in the effectiveness of large-scale monetary incentives and use data from web searches to show an endogenous increase in the demand for information following the regulatory change. The body of evidence we present provides strong proof of voter rationality with regards to electoral participation (Feddersen, 2004).

These results 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 findings show that marginal changes to these incentives, in the form of reductions to the fine for electoral 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 relatively low 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. In this regard, our finding of an almost one-to-one increase in blank and invalid votes with the additional votes generated by marginal increases to the abstention fine 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 electorate face large voting restrictions (Fraga, 2018).

Overall, our results provide evidence of a gradual, sophisticated and heterogeneous response to large-scale public provision of monetary incentives to vote. They suggest that 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.

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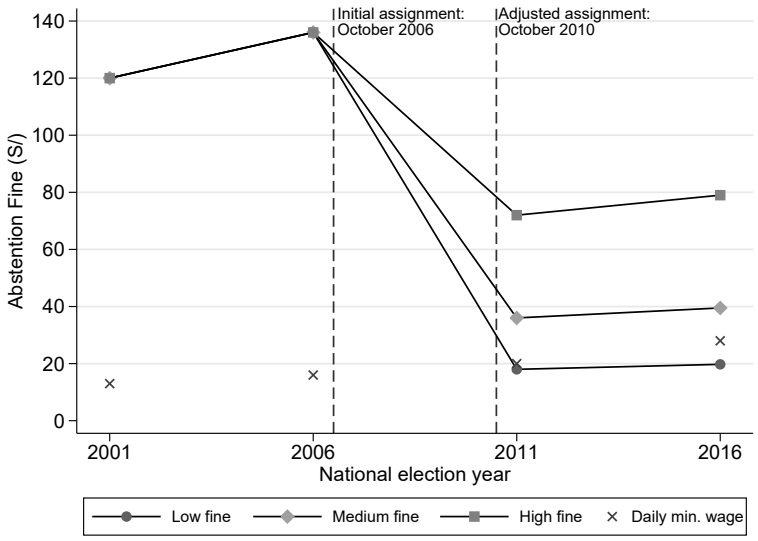
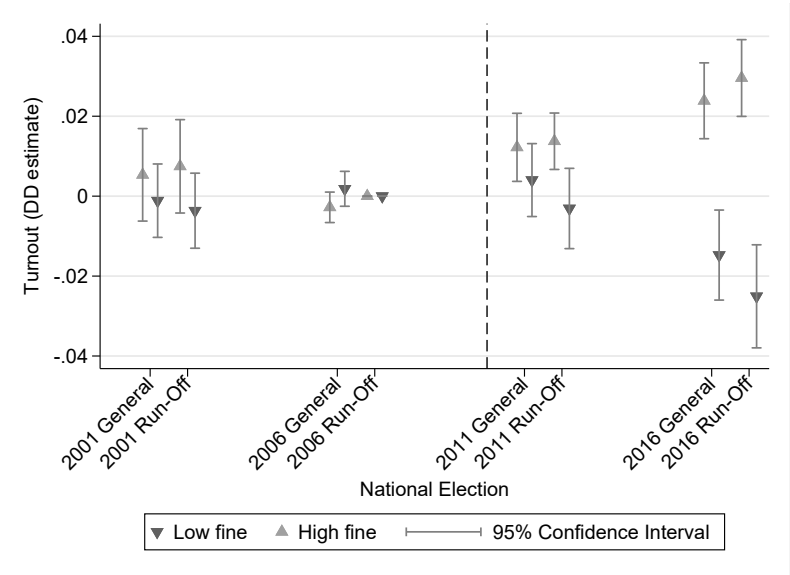
Figure 1: The Abstention Fine by Election and Fine Category



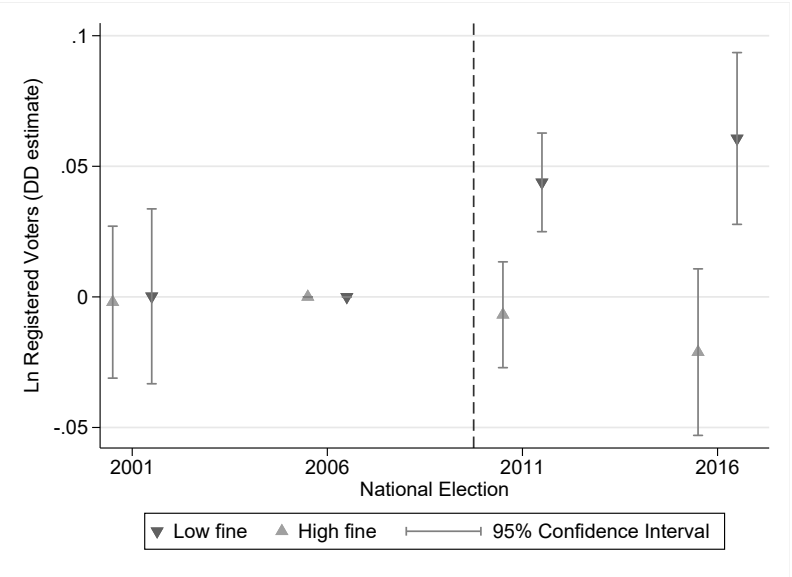
Notes: The graph shows the value of the abstention fine in each category for the national elections of 2001, 2006, 2011 and 2016. Values are displayed in current Peruvian soles (S/), but are defined in constant units for tax purposes (UIT), which are updated yearly to adjust for inflation. The graph 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 assignment of districts to fine categories 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



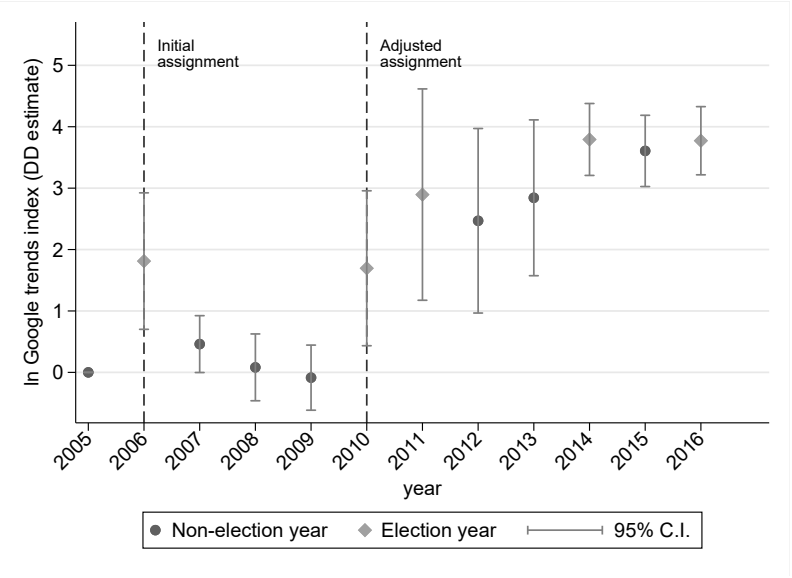
Notes: The graph shows point estimates and 95% confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with respective dummies for districts classified in 2010 as 'non-poor' (high fine) and 'extreme Poor' (low fine). The omitted category includes districts classified as 'poor' in 2010 (medium fine). 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



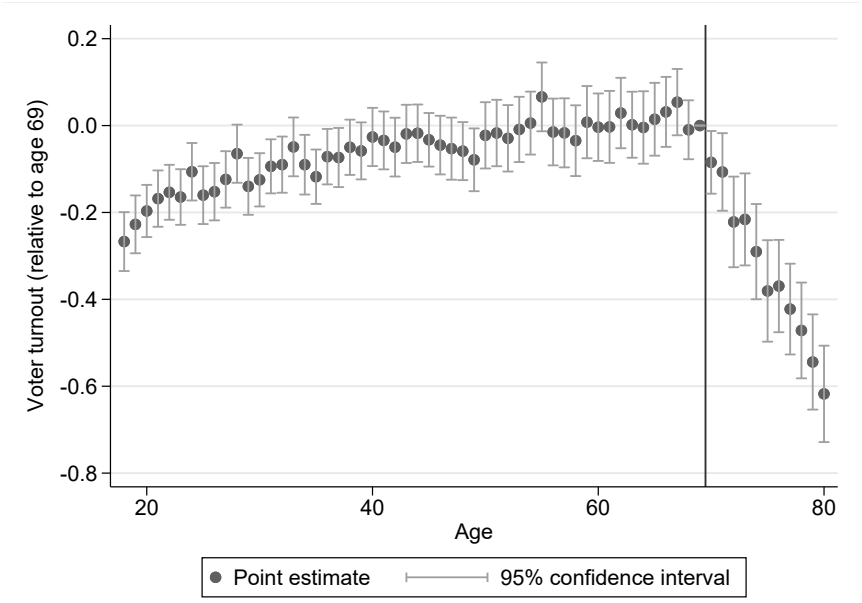
Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of district-level registered voters on a full set of election dummies interacted with respective dummies for districts classified in 2010 as "Non-Poor" (high fine) and "Extreme Poor" (low fine). The omitted group is made up of districts classified as "Poor" in 2010 (medium fine). The omitted election year is 2006. Regression includes district and province-election-category fixed effects (using 2006 poverty classification). 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 indicates the date in which districts were assigned to the poverty categories (October 2010).

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

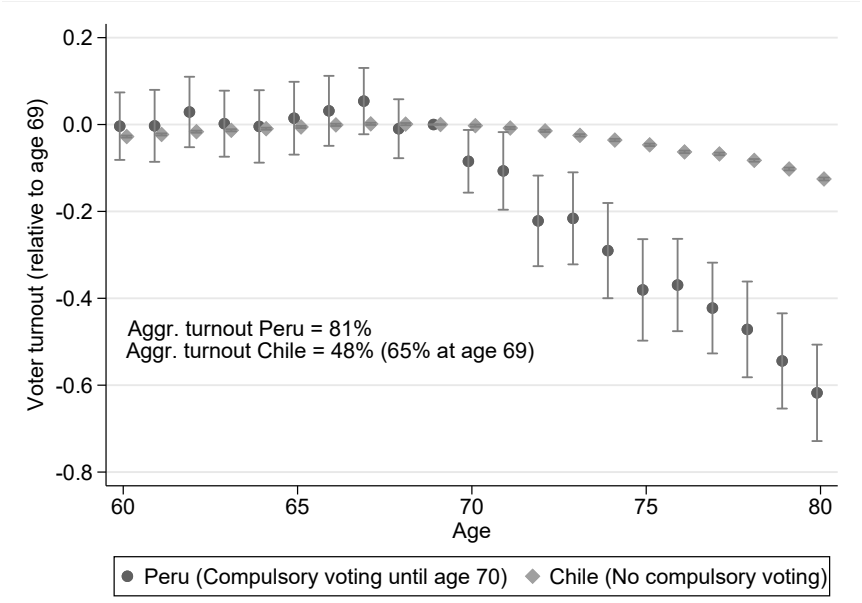


Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of a search-term popularity index from Google trends on year dummies interacted with an indicator for search terms related to the fine for abstention. Regression includes search-term and year fixed 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. The dotted lines indicate the year in which the initial reform to the abstention fine and district classification took place (2006) and the year in which districts were reassigned to the poverty categories (2010).

Figure 5: 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 of a regression of table-level turnout on the fraction of the electorate registered at that table belonging to each age group 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, as well as an election dummy for the presidential run-off. Data includes the general election and presidential run-off from 2016. Sample includes 148,448 observations (voting tables) from 4,723 polling stations in 1,854 districts. Standard errors are clustered at the district level. Tables are weighted by the number of registered voters for the 2016 national elections. Panel (b) shows the same results for ages 60-80 (round markers). Diamond 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). Bars show 95% confidence intervals. 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)	0.0487***	0.0459***	0.0349***	0.0614***	0.0486***
	[0.00848]	[0.00950]	[0.0107]	[0.0109]	[0.00818]
R-squared	0.018	0.026	0.064	0.077	0.085
Mean of dependent variable	0.845	0.844	0.845	0.831	0.845
	Panel B - Dependent variable: ln Voter Turnout$_{i,t}$				
ln Fine value$_{i,t}$	0.0296***	0.0277***	0.0231***	0.0365***	0.0293***
	[0.00531]	[0.00568]	[0.00705]	[0.00652]	[0.00517]
R-squared	0.018	0.024	0.061	0.070	0.070
Mean of dependent variable	-0.171	-0.172	-0.171	-0.188	-0.171
Observations	13,536	13,536	13,536	10,152	13,536
Districts	1692	1755	1692	1692	1692
District FE	Yes	Yes	Yes	Yes	Yes
Election-Province-Category '06 FE	Yes	No	Yes	Yes	Yes
Election-Province FE	No	Yes	No	No	No

Notes: Dependent variable is voter turnout (0-1) in panel A and the natural log of voter turnout in panel B. All columns use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016, except column 4 (data for 2006 unavailable). The value of the fine in panel A is measured in 100s of current Peruvian Soles (S/). In panel B, we use the natural log of the value of the fine. All columns include district fixed effects. All columns include election x province x 2006 poverty category (high fine, medium fine, low fine) fixed effects, except column 2 (election x province FE). Column 3 includes the time-invariant shares of poor and extreme poor inhabitants interacted with election fixed effects as additional controls. Column 4 includes the time-varying shares of registered voters with primary, secondary and tertiary education as additional controls. Column 5 includes log polling stations as additional control. 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=0.845)			ln Turnout$_{i,t}$ (Mean=-0.171)		
	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]	0.0197**	0.0390***	-0.0172	0.0108**	0.0226***	-0.0283
	[0.00849]	[0.00905]	[0.0159]	[0.00486]	[0.00551]	[0.0172]
(ln) Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$ [b]	0.0509***			0.0375***		
	[0.00487]			[0.00328]		
(ln) Fine value$_{i,t}$ ×$\mathbb{1}$(Run-Off)$_t$ [b]		0.0194***			0.0140***	
		[0.00403]			[0.00272]	
(ln) Fine value$_{i,t}$ × Non-extreme Poor$_i$ [b]			0.0983***			0.0963***
			[0.0197]			[0.0293]
(ln) Fine value$_{i,t}$ × Extreme Poor$_i$ [c]			0.0495***			0.0477***
			[0.0153]			[0.0171]
Observations	13,536	13,536	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.028	0.019	0.032	0.033	0.020	0.026
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	0.000	0.000	0.000	0.000
p-value H_0: a+c=0			0.004			0.051
p-value H_0: b=c			0.015			0.049

Notes: Dependent variable is voter turnout (0-1) in columns 1-5 and the natural log of voter turnout in columns 6-10. 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 1-5 is measured in 100s of current Peruvian Soles (S/). In columns 6-10, we use the natural log of the value of the fine. 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. Columns 1/6 includes the interaction of the fine with a dummy for the 2016 elections. Column 2/7 includes the interaction of the fine with a dummy for presidential run-Off elections. Columns 3/8 include the interaction of the fine with the shares of poor and extreme poor population in the district. 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

	Dependent variable: ln Voters$_{i,t}$							
	All	All	18-20	21-29	30-35	36-50	51-75	75+
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ln Fine value$_{i,t}$	-0.0460***	-0.0452**	-0.276***	-0.0551***	-0.0307	-0.0206	-0.0169	-0.0574
	[0.0149]	[0.0191]	[0.0426]	[0.0202]	[0.0219]	[0.0195]	[0.0240]	[0.0508]
Observations	6,768	5,076	5,076	5,076	5,076	5,076	5,076	5,076
Districts	1692	1692	1692	1692	1692	1692	1692	1692
R-squared	0.002	0.001	0.030	0.001	0.0003	0.0002	0.0001	0.0008
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	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 in columns 2-8 includes national elections for the years 2001, 2011 and 2016. 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 4: The Abstention Fine, Voter Registration and Turnout

Dependent variable:	ln Voters$_{i,t}$		ln Votes$_{i,t}$		ln Turnout$_{i,t}$		Turnout$_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
(ln) Fine value$_{i,t}$ (S/ x 100) [a]	-0.0460***	-0.0348***	-0.0164	-0.0240*	0.0268***	0.00872*	0.0438***	0.0160*
	[0.0149]	[0.0123]	[0.0160]	[0.0136]	[0.00540]	[0.00496]	[0.00858]	[0.00865]
(ln) Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ [b]		-0.0224***		0.0151*		0.0361***		0.0488***
		[0.00858]		[0.00912]		[0.00331]		[0.00486]
ln Voters$_{i,t}$					-0.0615***	-0.0612***	-0.0495***	-0.0494***
					[0.00516]	[0.00521]	[0.00426]	[0.00430]
Observations	6,768	6,768	13,536	13,536	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692	1692	1692	1692	1692
R-squared	0.002	0.002	0.0002	0.0003	0.121	0.134	0.155	0.164
Mean of dep. var	10.68	10.68	10.50	10.50	-0.171	-0.171	0.845	0.845
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
p-value H_0: a+b=0		0.002		0.646		0.000		0.000

Notes: Dependent variable in the header. ln Voters is the natural log of the number of registered voters for the election cycle; 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. 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/) in columns 7-8, log fine in columns 1-6. Regressions include district and election x province x '06 category fixed effects. 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]	0.0428***	0.0171*	0.0266***	0.0178***	0.0103**	0.00422
	[0.00850]	[0.00908]	[0.00546]	[0.00580]	[0.00473]	[0.00550]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ [b]		0.0451***		0.0155***		0.0107**
		[0.00532]		[0.00386]		[0.00526]
Observations	6,768	6,768	6,768	6,768	6,768	6,768
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.0152	0.0236	0.0112	0.0130	0.00184	0.00281
Mean of dep. var	0.851	0.851	0.0890	0.0890	0.0334	0.0334
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: Dependent variable in the header. 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 include district fixed effects and election x province 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 6: Senior Exemption from Compulsory Voting and Voter Turnout

	Dependent variable: Turnout$_i$						
	Baseline	Registered voters $\approx$ 300	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***	-8.54e-05
	[0.0413]	[0.0336]	[0.0284]			[0.0426]	[0.0794]
Share ages 70-72$_i$				-0.134***			
				[0.0374]			
$\sum_{j=70}^{75}$ share with age j $\times$ # elections w/o CV$_j$					-0.129***		
					[0.0197]		
Share ages 70-75$_i$ $\times \mathbb{1}$(Run-Off)$_t$						-0.0692***	
						[0.0174]	
Share ages 70-75$_i$ $\times$ Share secondary education$_i$							-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	0.188	0.160	0.0834	0.189	0.189	0.189	0.189
Mean of dependent variable	0.822	0.823	0.822	0.822	0.822	0.822	0.822
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: Dependent variable is voter turnout (0-1). Data at the voting booth level for the national elections (General: Legislative and Presidential first round; Presidential Run-Off) of 2016. 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 effect, 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 between 280 and 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)

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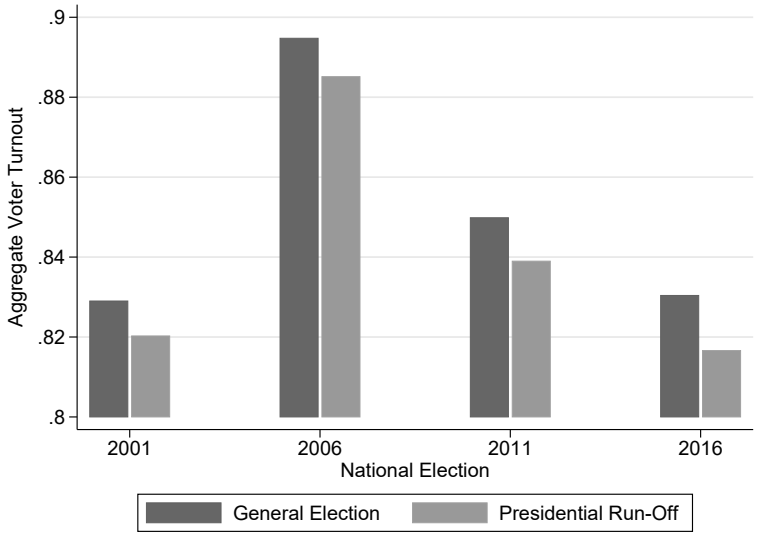
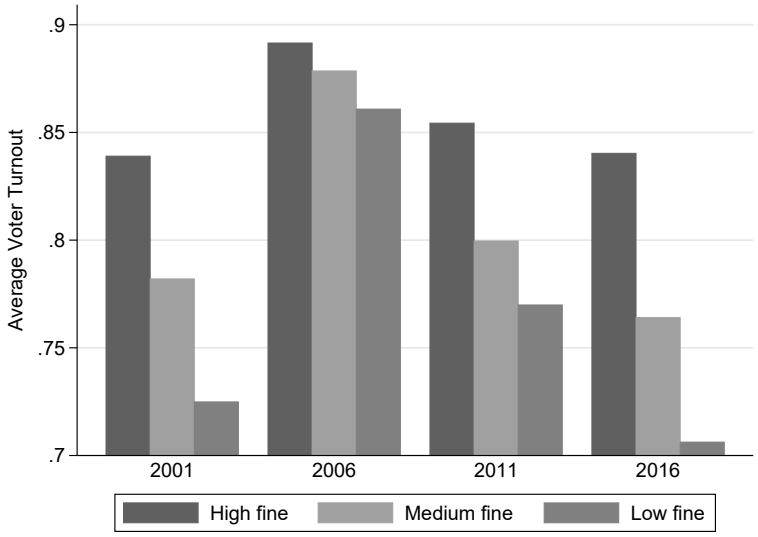
A Additional Figures and Tables

Figure A1: Voter Turnout in National Elections



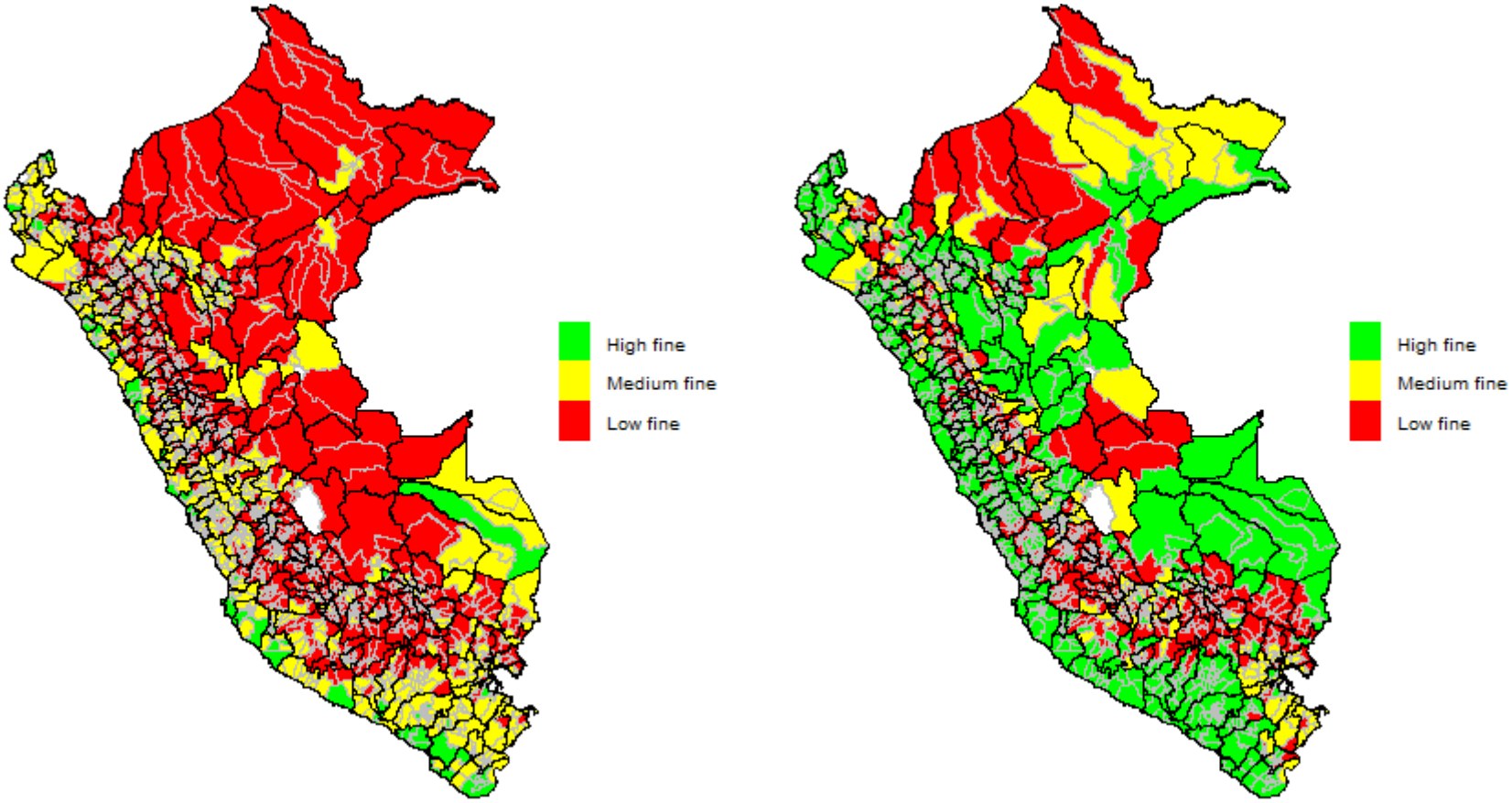
(a) Aggregate (by election type)



(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: Assignment of Districts to Fine Categories

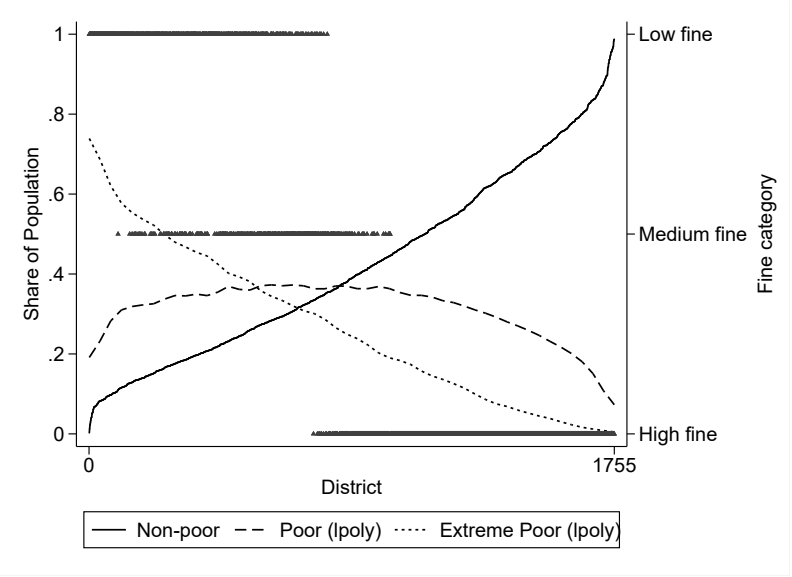


(a) 2006 assignment

(b) 2010 assignment

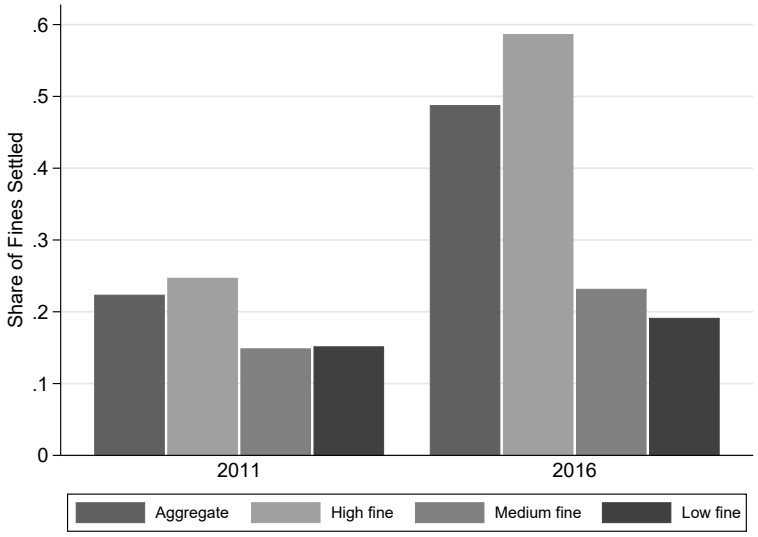
Notes: The map in panel (a) shows the location of districts in each fine category according to the initial assignment in 2006. The map in panel (b) shows the location of districts in each category following the adjusted assignment of 2010. See text for details on classification criteria. Dark lines correspond to provincial boundaries, while the lighter ones show district borders.

Figure A3: Poverty Shares and Assignment of Districts to Fine Categories



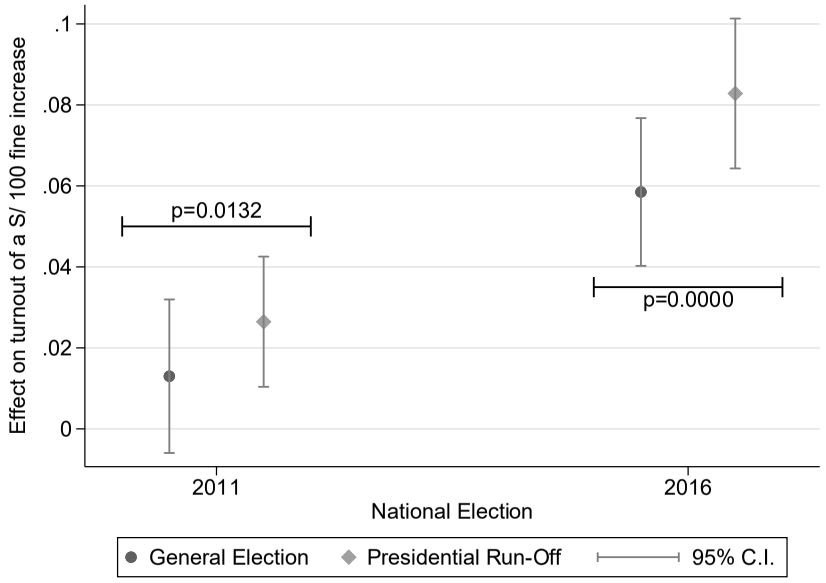
Notes: Solid line shows the share of non-poor population in each district, ranked from lowest to highest (left axis). Dashed (dotted) line shows the respective share of poor (extreme poor) population, using a local polynomial smoother. The triangular markers show the corresponding category for the abstention fine (right axis).

Figure A4: Share of Fines Settled for the 2011 and 2016 Elections

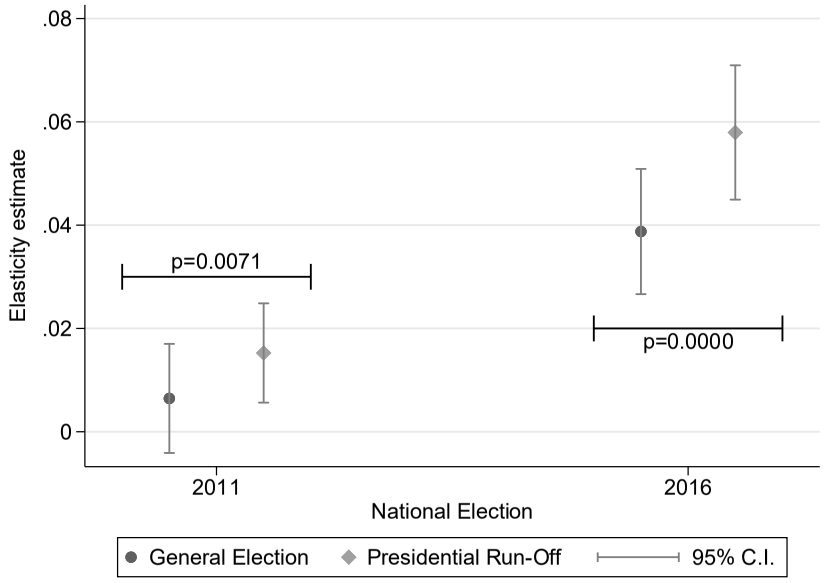


Notes: The graph shows the share of abstention fines settled in each category, as well as the countrywide aggregate, for the national elections of 2011 and 2016 (general and run-off combined). Settled fines include paid fines and valid excuses. Data from June 2018.

Figure A5: 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).

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.

Table A2: Difference-in-difference estimates of the effect of the reform on voter 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).

Table A3: Heterogeneous effects by Unmet Basic Needs

	Dependent Variable: Turnout$_{i,t}$		
	(1)	(2)	(3)
Fine value$_{i,t}$ (S/ x 100) [a]	0.00971	0.00593	0.00716
	[0.0152]	[0.0136]	[0.0136]
Fine value$_{i,t}$ × Share UBN≥ 1$_i$ [b]	0.0503***		
	[0.0157]		
Fine value$_{i,t}$ × Share UBN=1$_i$ [b]		0.0792***	0.0705***
		[0.0167]	[0.0163]
Fine value$_{i,t}$ × Share UBN≥ 2$_i$ [c]		0.0177	
		[0.0180]	
Fine value$_{i,t}$ × Share UBN=2$_i$ [c]			0.0689**
			[0.0297]
Fine value$_{i,t}$ × Share UBN≥ 3$_i$ [d]			-0.0813
			[0.0550]
Observations	13,536	13,536	13,536
Districts	1692	1692	1692
R-squared	0.0301	0.0334	0.0348
District FE	Yes	Yes	Yes
Election-Province-Category '06 FE	Yes	Yes	Yes
p-value H_0: a+b=0	0.000	0.000	0.000
p-value H_0: a+c=0		0.129	0.014
p-value H_0: a+d=0			0.154
p-value H_0: b=c		0.009	0.963

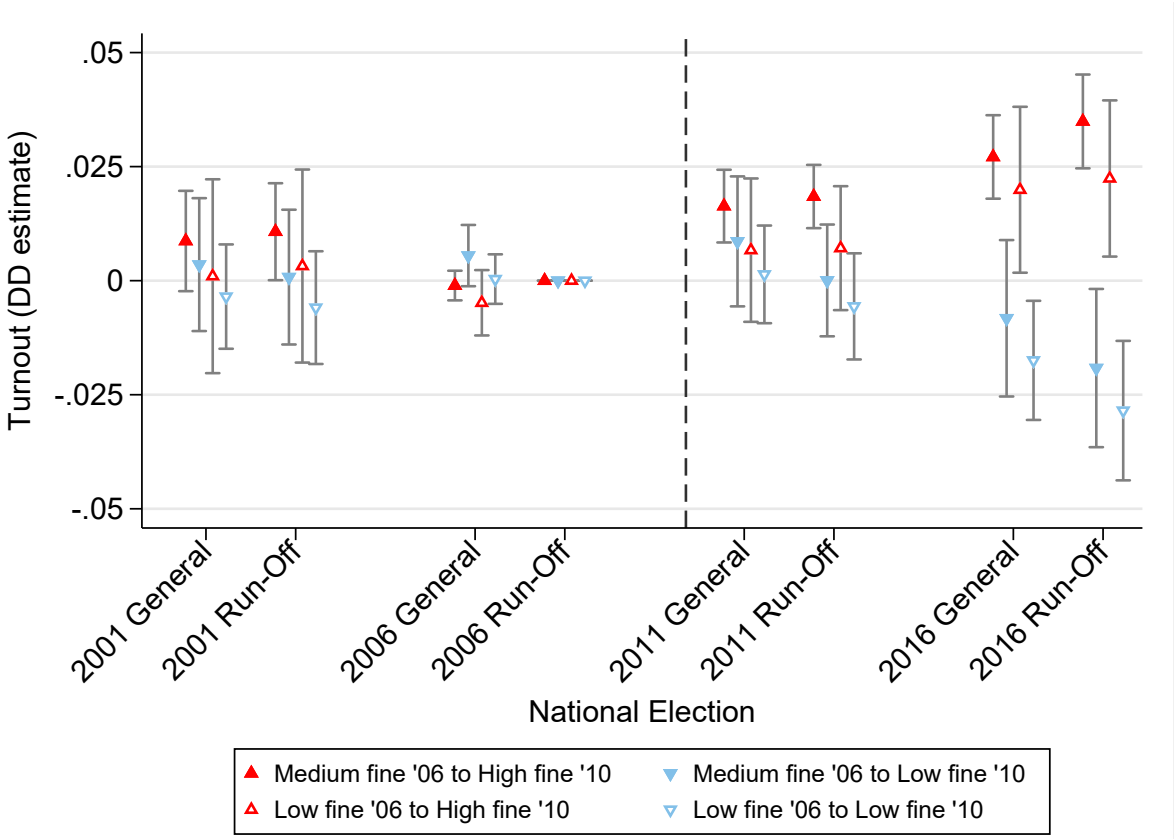
Notes: Dependent variable in the header. All regressions use data from national elections (General: Legislative and Presidential first round; Presidential Run-Off) for the years 2001, 2006, 2011 and 2016. Value of the fine in current Peruvian Soles (S/). Share UBN ≥ x corresponds to the share of households with more than x unmet basic needs according to the 2007 Census. Regressions include district fixed effects and election 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 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$_{i,t}$	0.000546***		0.000546***	0.000539***
	[8.77e-05]		[8.78e-05]	[0.000144]
Vote share of run-off candidates$_{i,t-1}$		0.00298	0.00293	0.00128
		[0.0135]	[0.0135]	[0.0356]
Vote share of run-off candidates$_{i,t-1}$ × Fine value$_{i,t}$				1.87e-05
				[0.000261]
Observations	6,768	6,768	6,768	6,768
Districts	1692	1692	1692	1692
R-squared	0.960	0.959	0.960	0.960
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. The value of the fine is measured in current Peruvian Soles (S/). 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

Figure A6: 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.

Table A5: The Marginal Effect of the Abstention Fine on Settlement of Outstanding Fines

	Share of fines settled$_{i,t}$		Share of fines paid$_{i,t}$		Share of fines excused$_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fine value$_{i,t}$ (S/ x 100) [a]	0.231***	-0.00250	-0.0202***	-0.0260***	0.252***	0.0235***
	[0.0235]	[0.00831]	[0.00694]	[0.00697]	[0.0212]	[0.00498]
Fine value$_{i,t}$ × $\mathbb{1}(2014/16)_t$ [b]		0.353***		0.00874		0.344***
		[0.0308]		[0.00748]		[0.0308]
Observations	11,721	11,721	11,721	11,721	11,721	11,721
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.026	0.133	0.0009	0.001	0.031	0.133
Mean of dependent variable	0.365	0.365	0.200	0.200	0.165	0.165
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x '06 Category FE	Yes	Yes	Yes	Yes	Yes	Yes
p-value H_0: a+b=0		0.000		0.036		0.000

Notes: Dependent variable in the header. 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/). All columns include district fixed effects and election x province x 2006 category (high fine, medium fine, low fine) fixed effects. Even-numbered columns include the interaction of the value of the fine with a dummy for the elections of 2014 and 2016. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

Table A6: 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)	0.0197**	0.0173**	0.0197**	0.0180**	0.0197**	0.0197**
	[0.00849]	[0.00872]	[0.00849]	[0.00898]	[0.00849]	[0.00850]
$\text{Fine value}_{i,t} \times \mathbb{1}(2016)_t$	0.0511***	0.0509***	0.0458***	0.0509***	0.0413***	0.0375***
	[0.00493]	[0.00509]	[0.00539]	[0.00514]	[0.00610]	[0.00622]
$\mathbb{1}(\text{Targeted District})_i \times \mathbb{1}(2016)_t$	0.00596					0.0107**
	[0.00363]					[0.00448]
$\mathbb{1}(\text{Province capital})_i \times \mathbb{1}(2016)_t$			0.00840***			0.00821***
			[0.00193]			[0.00182]
Δ fines $\text{settled}_i \times \mathbb{1}(2016)_t$					0.0241***	0.0203***
					[0.00799]	[0.00707]
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	0.029	0.029	0.037	0.026	0.028	0.038
Mean of dependent variable	0.845	0.829	0.845	0.847	0.846	0.846
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

B Voter Registration: Robustness checks

Table A7: 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***	-0.0218	-0.0459**	-0.0547***	-0.0511	-0.0620
	[0.0513]	[0.0268]	[0.0204]	[0.0202]	[0.0310]	[0.0572]
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	0.105	0.113	0.147	0.350	0.414	0.146
Mean of dep. var.	7.996	9.279	8.770	9.431	9.191	7.318
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 A8: The Value of the Abstention Fine, Nighttime lights and Migration

Dependent variable:	ln Night 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)	0.0560	-0.0755**	-0.0989***	-0.0744	-0.152***	-0.160***
	[0.0643]	[0.0310]	[0.0366]	[0.0852]	[0.0467]	[0.0466]
ln Night lights $DN_{i,t}$			0.170*			
			[0.0904]			
Share born in $district_{i,t}$						-0.113**
						[0.0488]
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	0.019	0.001	0.005	0.019
Mean of dependent variable	2.362	10.62	10.62	0.326	11.08	11.08
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Province-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 A9: 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***	-0.0831***	-0.0590*	-0.0439*	-0.0248	-0.0389
	[0.0584]	[0.0245]	[0.0305]	[0.0257]	[0.0359]	[0.0790]
R-squared	0.040	0.002	0.0008	0.0005	0.0002	0.0002
	Panel B - Controlling for change in access to DNI					
ln Fine value$_{i,t}$	-0.348***	-0.0628**	-0.0365	-0.0212	-0.00290	-0.0183
	[0.0545]	[0.0265]	[0.0331]	[0.0285]	[0.0364]	[0.0783]
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	0.055	0.014	0.011	0.011	0.014	0.008
Observations	2,460	2,460	2,460	2,460	2,460	2,460
Districts	820	820	820	820	820	820
Mean of dependent variable	8.350	9.632	9.122	9.778	9.526	7.644
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 national elections for the 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

C Construction of Google Trends dataset

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.

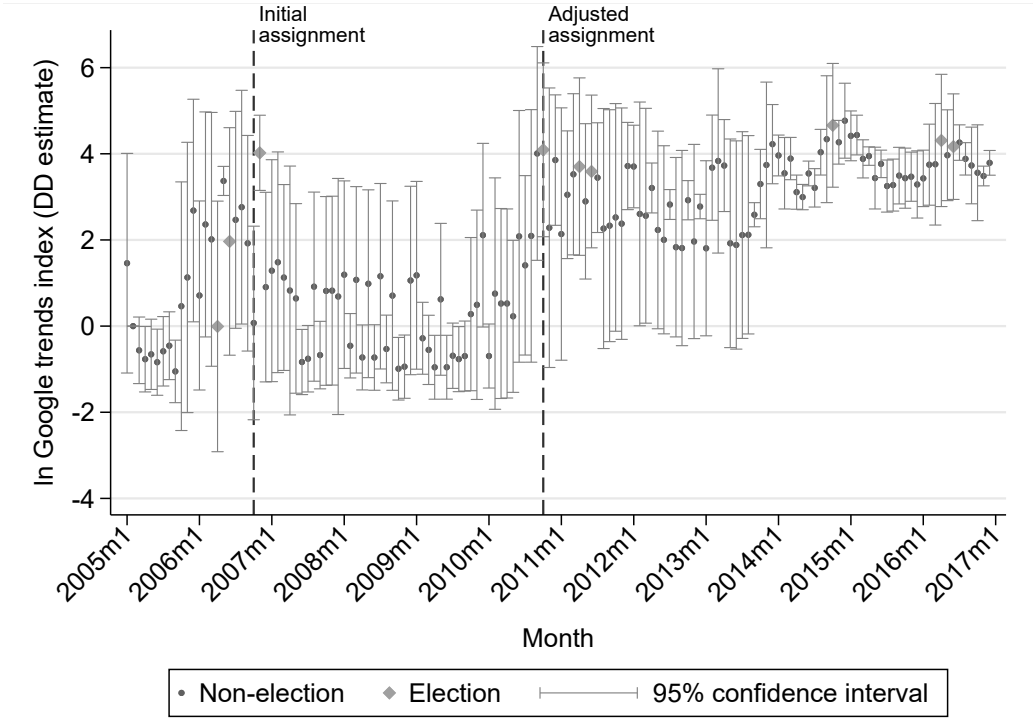
We limited the geographic scope to the country of Peru and collected monthly data from January 2005 to December 2016. We used used double quotation marks (" ") to avoid capturing Google searches for segments of multi-word search terms (e.g. "fine for not voting"). All queries were done in Spanish, in lower case and without any dyacritics. The full list of included search terms is presented in Table A10.

Table A10: 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 (24 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.

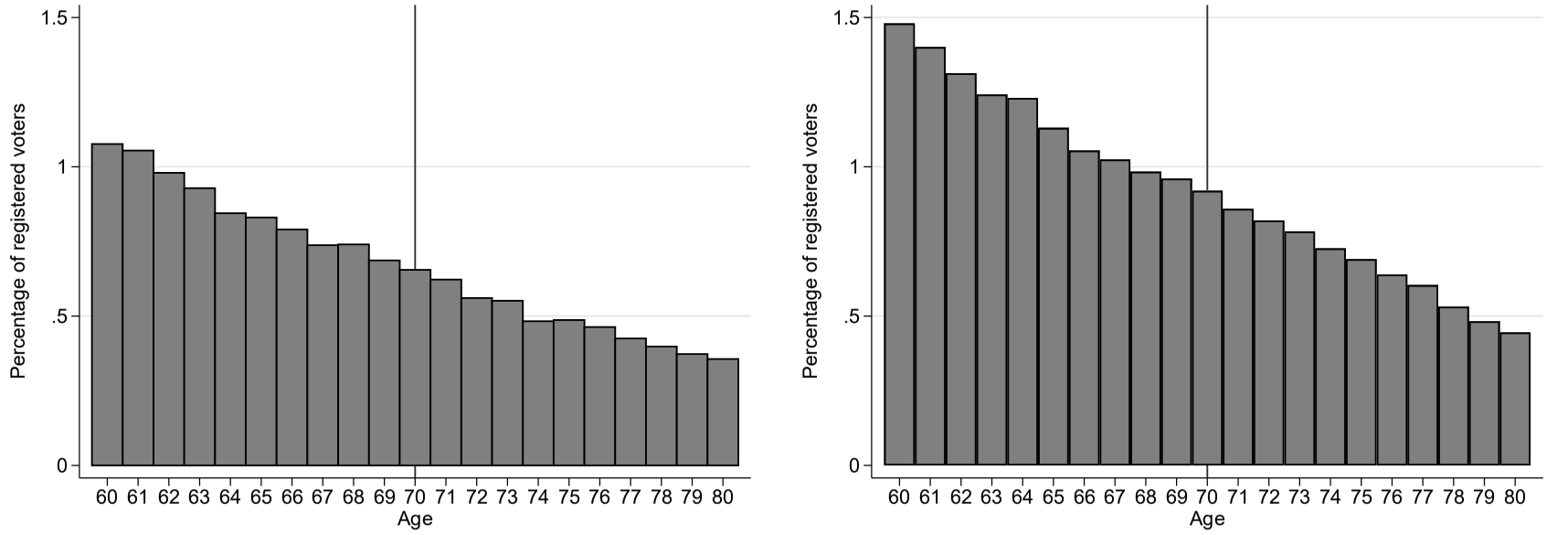
Figure A7: The Reform to the Abstention Fine and Information Acquisition (monthly level)



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).

D Senior Exemption: Robustness Checks

Figure A8: 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.