How Effective Are Monetary Incentives For Turnout? Voters' Multidimensional Response to a Nationwide Policy

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Abstract

We study voters' response to the provision of different-sized monetary incentives for turnout by the government of Peru, leveraging exogenous variation in the value of the abstention fine from a nationwide reform that affected districts differentially. Using multiple empirical strategies and administrative electoral data, we document a sophisticated voter response along several margins, including turnout and registration, settlement of outstanding fines and information acquisition. We find a positive marginal effect of monetary incentives on turnout, but also unintended effects on voter registration resulting from geographic targeting. We further show that the abstention fine explains a large share of the aggregate effect of compulsory voting on turnout. We document heterogeneous effects across elections and adaptation over time, illustrating the limitations of localized field experiments in political economy. Our estimate of the impact of marginal pecuniary incentives on turnout provides a useful benchmark to assess the cost-effectiveness of voter mobilization campaigns.

Keywords: elections, voter turnout, voter registration, compulsory voting, Peru
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1 Introduction

Voting is the main tool at the disposal of citizens to ensure that government policies represent their interests, as well as to hold public officials to account (Ashworth, 2012). Voter turnout has been dropping worldwide for the last 30 years and has reached alarmingly low levels in some countries (World Bank, 2017). Recent presidential elections in the Americas, for example, had roughly half of the electorate failing to cast a vote.[1] Low turnout raises concerns about the representativeness of elected officials, the legitimacy of the electoral process and the upholding of democratic values (Lijphart, 1997; Besley and Persson, 2018).[2] The current state of affairs arguably calls for increased efforts in the design and evaluation of scalable policies that help bring voters to the polls.

A large literature studying voter mobilization campaigns has produced a substantial amount of practical knowledge (Green and Gerber, 2015), but most of these interventions have only been tested through field experiments with a limited geographic scope and a short duration.[3] Unfortunately, large-scale policies can be affected by multiple issues that are hard to detect in field experiments of this nature, which may lead to widely different effects after policy scale-up (e.g., Grossman et al., 2018). Various other policy alternatives have been found to help raise voter turnout, such as increasing the number of polling stations (Brady and McNulty, 2011; Cantoni, 2019) or introducing convenience voting (Hodler et al., 2015; Kaplan and Yuan, 2018). However, the cost-effectiveness of most available options remains unclear and is difficult to evaluate in the absence of a benchmark.[4]

Publicly-provided monetary incentives are a scarcely studied but potentially powerful tool to increase voter turnout. Their consequences are not obvious. A simple behavioral model in the spirit of Downs (1957) or Riker and Ordeshook (1968) predicts that pecuniary incentives mechanically increase turnout by raising the opportunity cost of electoral abstention. However, a richer model in the spirit of Bénabou and Tirole (2003, 2006) suggests that extrinsic incentives may crowd out the intrinsic motivation to vote provided by social image concerns or a sense of civic duty (Dellavigna et al., 2017), leaving both the sign and magnitude of the net effect an open empirical question (e.g., Gneezy and Rustichini, 2000; Funk,

[1]Voter turnout was 63% in Mexico, 66% in Costa Rica, 54% in Colombia and 46% in Venezuela in 2018; 48% in Chile and 59% in Honduras in 2017; 55% in the United States in 2016.

[2]Notwithstanding that electoral abstention may be a rational response to informational limitations under certain conditions (Feddersen and Pesendorfer, 1996).

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

[4]Previous research shows that those interventions that are most effective tend to be highly personalized and require higher amounts of labor, making them costlier (Green et al., 2013; Gerber and Green, 2017).

2010). Furthermore, widespread public provision of such incentives may have hard-to-predict general equilibrium effects due to social contagion and other spillovers, slow adaptation due to informational constraints, and the availability of other response margins.

In this paper, we study a large-scale public policy providing different-sized monetary incentives for voter turnout. The setting for our study is Peru, which is one of 27 countries in the world to have compulsory voting (IDEA, 2018).[5] The sanctions for electoral abstention include the payment of a fine, which was homogeneous for all citizens in all districts until 2006. A reform that year classified districts into three poverty categories labeled non-poor, poor and extreme poor. The value of the fine was reduced in all districts, but the magnitude of the decrease was larger in the latter categories. This reform provides plausibly exogenous variation in the monetary incentive to vote (across districts and over time), which we combine with granular administrative data for national elections over a 16-year period to study voters' response along several margins.

Using a difference-in-difference strategy, we show that a larger fine leads to higher turnout, with an average marginal increase of 0.5 percentage points (pp) for a 10 Peruvian Sol [S/] hike (approximately US$3).[6] This average masks a highly heterogeneous response. For example, the marginal effect of the fine is more than three times as large in 2016 than in 2011, the first election after the reform. The gain in turnout from a S/10 increase jumps from 0.2 pp to 0.7 pp between these years, with a corresponding change in the elasticity from 0.01 to 0.05. These are large long-run effects and correspond to a gap in turnout of more than 5 pp between non-poor and extreme-poor districts in 2016. We also observe that the marginal effect of the fine is not uniform across different types of elections. It is almost 50% larger for the presidential run-off than for the general election, which combines the legislative election with the first round of the presidential race and takes place two months earlier. This result constitutes *prima facie* evidence that the same intervention can have very different effects depending on the nature of the election in which it is implemented, arguably because marginal voters who respond to the change in incentives differ across election types.

The impact of a marginal increase to the abstention fine on turnout can be decomposed into separate effects on the number of votes cast (the numerator) and the number of registered voters (the denominator). These distinct effects are often overlooked by short-lived experiments because registration is unlikely to change in the short run. Voter registration

[5]These include most of South and Central America, as well as Australia, Belgium, DRC, Egypt, Mexico, Thailand, Turkey and Singapore. Only a dozen of these, including Peru, actually enforce compulsory voting. The voting-age population in the latter group is approximately 220 million, which is only slightly smaller than the 235 million in the United States (U.S. Census Bureau, 2011).

[6]The legal minimum daily wage in 2006 was S/16 (US$5 approx.). Figure 1 shows that turnout in the 2006 presidential run-off, the last election before the reform, was 87.7%.

is automatic in Peru and people are legally required to vote in their district of residence, which they can change by modifying the address on their national identification card (DNI). We show that the number of registered voters decreases disproportionately in districts with a higher fine after the reform, with a S/10 increase in the value of the fine leading to a 1% decrease in voter registration. As with turnout, the marginal effect of the fine on registration is significantly larger in the longer run. However, variation in the number of registered voters can explain at the most 20% of the documented effect of the fine on turnout, indicating that adjustment in behavior, rather than selection, is the principal driving mechanism.

We verify that the results on voter registration are not confounded by differential migration between districts in different poverty categories or by government campaigns to increase access to identification documents. We argue instead that voters are strategically changing their registered address in order to avoid paying a larger fine. In support of this explanation, we show that the registration effect is only present for young adults under 30 and is mostly driven by ages 18 to 20. This latter age group corresponds predominantly to first-time voters, who must apply for a DNI and can therefore report a fraudulent address at a very low cost. The Peruvian context makes it unlikely that this result is driven by young adults moving away from home (e.g., college students) and we provide evidence against such alternative explanations. This type of voter misbehavior could have negative long-run consequences for political participation and representation, as young people with a low baseline propensity to vote are self-selecting into districts providing weak incentives for them to do so in the future.

Naturally, the interpretation of the marginal effects of the fine must take into account the probability of enforcement.[7] The percentage of settled fines (i.e., either paid or excused) increases over the sample period, moving from 22% in 2011 to almost 50% in 2016. Although this 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. The observed improvement in the share of settled fines is related to measures taken by the government after 2011, including the creation of a coercive collections unit. These measures targeted a set of mostly non-poor districts, but this was fortuitous as the renewed effort at enforcement did not deliberately incorporate the poverty categories determining the value of the abstention fine. Leveraging cross-district variation in increased enforcement, we show that these improvements can account for no more than 20% of the observed long-run marginal effect of the fine on voter turnout.

We also document a sophisticated and heterogeneous response to marginal changes to the abstention fine by non-voters who actually face the penalty. Outstanding fines can be settled

[7]The interpretation of the elasticity estimates is not subject to this concern, since their computation factors away the probability of enforcement, as long as this probability is homogeneous across districts.

either by paying the corresponding amount or by submitting a claim for an excuse based on health reasons, death of a relative, or other extenuating circumstances. Submitting an excuse requires payment of a processing fee equal to the value of the fine for the extreme-poor category, but the transaction cost of an excuse is arguably higher. We show that debtors in different poverty categories settle their debts in the way that is less costly to them. As the value of the fine increases, debtors in the extreme-poor category respond by paying less often, while those in the other categories submit relatively more claims for excuses. We find that the marginal effect of the value of the fine on the settlement rate is a precise zero before the strengthening of enforcement and that the large increase in the share of fines settled in later years is entirely driven by a rise in excuses in non-poor districts.

Overall, our results provide evidence of a dynamic and multi-dimensional response by voters to the incentives provided by the reform to the abstention fine. The larger observed impact in the longer run is consistent with increased adaptation over time as a result of a weakening of voting habits (Coppock and Green, 2016; Fujiwara et al., 2016) or social contagion (Nickerson, 2008). Another mechanism is the slow diffusion of information about the reform. We hypothesize that this mechanism is particularly relevant in our setting, given that the highly sophisticated response we observe would seem to require detailed knowledge of the incentives provided by the policy. 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 show 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.

Our analysis indicates that voters are highly responsive to the pecuniary incentives provided by the abstention fine in Peru. But this is only one of the incentives to vote provided by compulsory voting (Funk, 2007). The contribution of the deterrent effect of the fine relative to the expressive effect of the law as a signal of socially desirable behavior remains unclear. 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. Our empirical strategy exploits the fact that citizens above the age of 70 are exempt from the mandate to vote and involves comparing turnout rates in voting booths with a higher share of 70 year-old voters to those with a higher share of 69 year-olds. This comparison takes place within the same district or polling station, flexibly controlling for the entire age structure of the electorate registered at each booth. We find that the senior citizen exemption from compulsory voting leads to a decrease in voter turnout of approximately 10 pp. A back-of-the-envelope calculation using our elasticity estimates for the same year yields that the monetary incentive explains roughly 50% of the aggregate effect of compulsory voting.

This paper contributes to the vast literature studying voter turnout and provides new evidence on the general-equilibrium effects of pecuniary incentives.[8] To the best of our knowledge, only two previous studies, both involving field experiments, have estimated the marginal effect of monetary incentives on turnout.[9] 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 is particularly relevant for us, as it studies the same reform we do. These papers consistently find a positive short-run marginal effect of monetary incentives, but do not study responses along other margins, adaptation over time or endogenous information acquisition. Our study investigates the response of millions of voters, along several margins and over several years, to an actual nationwide government policy providing monetary incentives to vote.[10]

We also contribute to a growing literature analyzing the external validity of experimental studies for policy scale-up (e.g., Deaton, 2010; Vivalt, 2017; Rosenzweig and Udry, 2018). The natural experiment we study allows us to uncover various important aspects of large-scale policies that previous experimental research has not been able to examine. In this regard, the availability of an experimental study in our same setting provides a rare opportunity to compare an experimental evaluation to one performed at scale. Our findings on the heterogeneous effects of monetary incentives according to the type of election illustrate the potential for context dependence in small experiments. Our findings of adjustment in registration by young voters exemplify the general equilibrium effects that small experiments struggle to anticipate or capture (Banerjee et al., 2017; Muralidharan and Niehaus, 2017). Our estimates also incorporate spillover effects resulting from voters' adaptation to changes in turnout by neighbors and peers. Finally, our results on information acquisition and long-run adaptation shed new light on the extent to which policy scale-up may be affected by informational constraints and imperfect compliance (Al-Ubaydli et al., 2017). The literature so far has mostly worried about the validity of experimental estimates when scale-up involves changes in implementation (Davis et al., 2017; Bold et al., 2018). We underscore a limitation specific to interventions in political economy, namely the limited reach of information about

[8]Classic treatments include Downs (1957); Riker and Ordeshook (1968); Palfrey and Rosenthal (1985). See also Blais (2000) and Feddersen (2004).

[9]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.

[10]Contemporaneous work by Carpio et al. (2018) answers a complementary question related to the effects of the reform we study on the party affiliations of candidates. The two papers differ in topic, outcomes, data and methodology. Reassuringly, their RDD estimates for the effect of the reform on turnout in non-poor districts (relative to poor) for the local elections of 2010 and 2014 are very similar to our DD estimates for the national elections of 2011 and 2016.

changes to the rules regulating the interaction of civilians with the state.

Our paper also complements a large empirical literature on the effects of compulsory voting laws on electoral participation (Funk, 2007; Fowler, 2013; Cepaluni and Hidalgo, 2016; Hoffman et al., 2017; Bechtel et al., 2018; Singh, 2019). Previous research has largely focused on the extensive margin, comparing outcomes before and after the introduction or elimination of compulsory voting. For the most part, these studies are not able to disentangle the relative contribution of the monetary incentive provided by the fine that is usually imposed on non-voters from the expressive effect of the law as a signal of socially desirable behavior. To the best of our knowledge, this is the first paper to simultaneously estimate the marginal effect of the abstention fine and the aggregate effect of the bundle of incentives provided by compulsory voting and compare them.

Our findings on the irregular change of voters' registered address are also related to the literature on voter misbehavior (Nichter, 2008; Finan and Schechter, 2012; Hidalgo and Nichter, 2016; Larreguy et al., 2016), as well as to that on the unintended consequences of targeted policies (Camacho and Conover, 2011; Cassan, 2015). We add to this literature by documenting a previously overlooked form of voter misbehavior that is not aimed at affecting electoral outcomes, but may have large effects on representation. Additionally, our results on information acquisition speak to the literature studying information spillovers of voter mobilization efforts (Chong et al., 2018; Fafchamps et al., 2018; Giné and Mansuri, 2018). While the previous literature has directly provided information through salient interventions, we provide evidence on the endogenous acquisition and slow diffusion of information about policy incentives in the wild.

Our results are also immediately relevant for public policy. Monetary incentives to vote are a rarely used but highly effective policy alternative available to governments across the globe. Even though these incentives are usually present in the form of fines in compulsory voting systems, they 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 also allow for a better-informed debate about the desirability of compulsory voting, the potential distributional consequences of the use of fines and the difficulties that arise from efforts to ameliorate them.[11] Additionally, our estimate of the marginal effect of pecuniary incentives on turnout provides a useful benchmark for the assessment of the cost-effectiveness of public policies aimed at mobilizing voters.

[11]The desirability of compulsory voting remains debatable (Birch, 2009; Brennan and Hill, 2014), but the idea has received support from academics and high-profile figures like former US president Barack Obama in recent years (The Guardian, 2016; Chapman, 2019).

2 Institutional Background

2.1 Electoral System and Voter Registration

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.[12] In the presidential election, a candidate must obtain at least 50% of the votes in order 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.[13]

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.[14] 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.

2.2 Compulsory Voting and the Reform to the Abstention Fine

Similarly to other 26 countries around the world, voting is compulsory in Peru (IDEA, 2018). Citizens between the ages of 18 and 69 (both inclusive) must pay a fine if they abstain from voting. As a result, voter turnout has been traditionally high. Figure 1 shows that aggregate turnout in national elections remained above 80% throughout the sample period, but has been declining since 2006. Noticeably, this period coincides with the reform to the fine for abstention that is the focus of our analysis.

The mandate to vote was introduced in the 1933 Constitution. Until 2006, the fine for not voting was completely homogeneous, set at 4% of an official reference unit known as UIT.[15] At the start of 2006, the UIT was set at S/3,400 (approximately US$1,040) and

[12]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.

[13]Voters 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.

[14]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).

[15]The 'Unidad Impositiva Tributaria' (UIT) is a reference value that is used to determine various thresh-

the corresponding value of the fine was S/136 (roughly US$42).[16] In August 2006, 10 days after the inauguration of president Alan García, Congress approved a law that reformed the abstention fine. The law stipulated that all districts should be classified into one of three categories, depending 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 non-poor districts, the fine was cut in half to 2% of the UIT, while for those in poor and extreme poor districts, 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.

This reform followed preliminary discussions in which the elimination of compulsory voting was considered. It was a compromise between the desire to preserve the high levels of electoral participation induced by compulsory voting and the concern about the regresiveness of the homogeneous fine in place at the time. The reform was presented by a conservative party (Unidad Nacional), but was approved with 95% of roll-call votes, indicating it had widespread support. Importantly, the passing of the law was barely covered by the popular press. Surveying residents of poor and non-poor districts around Lima in 2010, León (2017) shows that the average perception of the value of the abstention fine was S/124 (standard deviation S/54), not much different from the pre-reform level.

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 issued by the national electoral jury (Jurado Nacional de Elecciones, JNE) in 2006 remains unclear.[17] However, the first and only elections to be held under the initial poverty 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.[18] In the new classification, districts were assigned to the poverty category with the largest share of their population, according to a poverty map produced by INEI and which is based on the 2007 population census. We can perfectly replicate the 2010 assignment using the poverty map data.

The maps in Figure 2 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

olds and limits in the tax code, the price of many public services, the value of fines, and other sanctions. It is updated every year to adjust for inflation.

[16] 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.

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

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

and that a large number were reclassified. In 2006, 10.4% of districts were classified as non-poor, 43.4 % as poor and 46.2% as extreme poor. All districts initially classified as non-poor in 2006 remained in this category in 2010. On the other hand, only 15% and 56% of districts initially classified as poor and extreme poor remain in the same category in 2010. According to the adjusted classification, 52.2% of districts are non-poor, 17.9% are poor and 29.9% are extreme poor. Table A1 in the online appendix provides the number of districts in each of the categories in the original and modified assignments.

Figure 3 shows the value of the abstention fine in place for each poverty category in each national election during the sample period. The 2006 elections 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 after the poverty classification was adjusted in 2010. The figure also shows that the value of the fine for the extreme poor category in 2011 was almost identical to the minimum daily wage, while those for poor and non-poor districts were twice and four times as large, respectively. The value of the fine for each poverty category has remained constant as a percentage of the UIT since the 2006 reform. The observed variation over time is entirely driven by the yearly UIT adjustment.

2.3 Enforcement of the Abstention Fine

Voting in Peru is done in person at pre-determined polling stations and voters are provided with a sticker on their DNI as proof of participation.[19] Those who abstain from voting and do not meet the age requirement for exemption are hence unable to use this document for multiple purposes until they pay the fine or provide a valid excuse. Restricted services include registering a birth or marriage, doing any transaction at public or private banks, benefiting from the social security system or getting official documents from the registrar. The restrictions also extend to 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. However, not having voted in the past or not having paid the fine does not prevent someone from voting in the future.

People with an outstanding fine can pay it at any of the around 600 branches of the national bank (Banco de la Nación, BN) throughout the country. Upon paying, they receive

[19]Voting usually occurs undisturbed and waiting times are short. In the 2016 elections, the average number of voters per polling station was 5,902. Within polling stations, the average number of voters per 'voting table' was 298. Also in 2016, voters in Lima and Callao were allowed to choose their polling station for the first time.

the sticker for the DNI and the fine is immediately cleared from the system. Alternatively, they can submit an excuse to the JNE after paying a processing fee of about S/21 (US$6.4). Acceptable excuses must be properly documented and valid reasons 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. During the sample period, excuses had to be submitted in person at any of the 17 JNE offices in the country (less than one per region), making this a costly process.Hence, even though the processing fee for an excuse is roughly equivalent to the value of the fine in extreme poor districts after the reform (see Figure 3), the transaction cost of paying the fine is significantly lower.

In practice, only a fraction of the people with outstanding fines actually settle them. For instance, Figure 4 shows that only 22% of the 4.7 million fines issued after the 2011 national elections were settled. The periodic amnesties provided by the Peruvian government, together with the fact that the fine normally expires after four years, dissuade many voters from paying or providing an excuse. However, enforcement improved substantially after this election, when a collection unit was created within the JNE.[20] 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 [21] Figure 4 shows that the increase in settlements was mostly driven by non-poor districts. This is consistent with the information contained in the JNE annual reports from 2012-2015, which list a set of districts in Lima and Callao provinces on which the activities of the collections unit were focused. All of these districts were classified as non-poor. In section 6 we return to this issue and examine the extent to which the change in enforcement helps to explain our findings.

3 Empirical Strategy

In this section, we present the various data sources used in the paper and introduce the empirical strategy for the analysis of the marginal effect of the abstention fine on voter turnout and registration. We leave the exposition of the complementary strategies for the analysis of online information acquisition and the exemption from compulsory voting after age 70 for sections 7 and 8, respectively.

[20] See Resolución 0738-2011-JNE from October 20, 2011.

[21] JNE (2015) reports that 42% of fine payments between 2012 and 2015 resulted from coercive collection. In 2015, 45,840 collection processes were opened, leading to 5,155 instances of bank accounts being frozen.

3.1 Data

The national office for electoral processes (Oficina Nacional de Procesos Electorales, ONPE) provided us with administrative data for the national elections in 2001, 2006, 2011 and 2016. This data covers both the general election, combining the legislative election with the first round of the presidential race, and the presidential run-off that takes place two months later. The data includes the number of registered voters and the number of votes cast in each election at the district level. From ONPE and JNE, we also got disaggregate district-level information on voter registration for predetermined age groups for all election cycles, except 2006. The value of the abstention fine and the assignment of districts to poverty categories for each election (the latter since 2006) is publicly available in resolutions issued by JNE. This agency also provided the number of fines issued per election and the amount of money collected from fine payments and excuse processing fees. Unfortunately, this last piece of data is only reliably available from the 2006 subnational elections onward.[22] Combining the information from these sources, we have a balanced panel of 1,769 districts, out of the 1,854 in the country. Most of the districts with incomplete data are new ones that were created during the sample period. Our final sample includes 1,755 districts, corresponding to 94% of the total number of districts and to more than 96% of the country's electoral population.[23]

From ONPE, we also obtained highly disaggregate data on the number of registered voters by age at the 'voting table' level. This is the most granular level at which electoral data is recorded, and corresponds to the specific voting booth within a polling station. Reliable information on voter characteristics is only available at this level of disaggregation for the national elections in 2001 and 2016.[24] Regrettably, information on voter turnout at the same level is not available for 2001, so we must restrict this analysis to the 2016 elections only.

To study information acquisition, we construct a dataset on the frequency with which people in Peru conduct queries in Google related to 44 different search terms. The dataset is based on publicly-available country-level data from the Google trends application. 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").

[22] The data is available since 2001, but the values up to the 2006 national elections are quite small and inconsistent with the reported turnout and the corresponding number of fines that should have been issued for those years.

[23] We exclude another 4 districts that were not assigned to a poverty category in 2006, but existed at the time, as well as 10 others that changed category in 2014 and reversed to the previous assignment in 2016.

[24] The resulting totals at the district level differ substantially from the reported figures for the other years.

Table A6 in the online appendix provides the full list of search terms used in the analysis. For each search term, we have monthly information on the 'Google Trends' index, which takes larger values for the more popular ones, from January 2005 to December 2016. 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.

3.2 Research Design

We aim to estimate the causal effect of changes to the value of the abstention fine on voter turnout and registration. For this purpose, we exploit plausibly exogenous variation in the value of the fine stemming from the differential reduction experienced by non-poor, poor and extreme poor districts between the national elections of 2006 and 2011. This variation lends itself naturally to a difference-in-difference analysis with district and time fixed effects.

As mentioned above, the initial reform took place shortly after the 2006 presidential run-off, but the relevant poverty categories at the time of the 2011 elections were the ones defined in 2010. Given that the criteria employed to classify districts into the three poverty categories in 2006 remains unclear and that there were no national elections before districts were re-classified in 2010, we could ignore the initial assignment. However, this assignment may have been based on informative district characteristics that are unobserved to the econometrician. Even if the 2006 assignment is uninformative, it was implemented and valid for around four years and may have affected voters' perception of the value of the fine and their related behavior. To account for these possibilities, we allow the outcomes to vary flexibly over time in districts that are located in the same province and were assigned to the same poverty category in 2006. Our baseline specification hence includes district fixed effects and quite stringent 'election x province x 2006-poverty-category' fixed effects. The latter also control for common shocks, allowing them to vary between provinces or 2006 poverty categories.

As a result of these modeling choices, our empirical strategy involves comparing within-district variation in the outcomes of interest, between districts located in the same province and that were initially assigned to the same poverty category.[25] The identifying assumption is that in the absence of the 2010 assignment there should be no systematic change in the outcomes between districts belonging to different poverty categories, but located in the same province and assigned to the same category in 2006. To provide evidence of parallel trends, we make use of information from the two electoral cycles preceding the reform and estimate

[25] 63 districts lacking at least one other district from the same province with the same assignment in 2006 are effectively dropped from the regression to avoid having singleton groups (Correia, 2015). As a result, the estimations below report 13,536 observations from 1,692 districts.

a difference-in-difference model of the form:

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

where $y_{d,p,e}$ is an outcome of interest in district d from province p in election e. α_d is a district fixed effect, while $\delta_{p,e,c_{06}}$ is the 'election x province x 2006-poverty-category' fixed effect. The other terms correspond to a full set of interactions between dummy variables $\mathbb{1}(\cdot)$ for each election t in the sample period (e.g. 2011 presidential run-off) and respective dummy variables for each poverty category k, as defined in the 2010 assignment (c_{10}). The omitted election is the 2006 presidential run-off (last national election before the reform) and the omitted 2010 poverty category is 'poor'. $\epsilon_{d,p,e}$ 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 (non-poor or extreme poor) and the omitted group (poor) 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 the pre-trends described above 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. Even though the pool of candidates varies across regions in legislative elections, our empirical strategy only involves comparisons within the same province (and 2006 poverty category), which is smaller than the region. By excluding subnational elections we also shut down the potential effects of the reform on voter buying (Hidalgo and Nichter, 2016). The irregular movement of voters across districts is a common clientelistic practice in Peru and these voters are known as "swallows" (votantes golondrinos).[26] The incentive to engage in this practice is very low in national elections. Presidential elections are single-district, and while candidates in legislative elections could potentially benefit from moving voters across regions, such actions are likely to have a high cost (given the longer distance) and a low payoff (due to the large number of voters per region). Reassuringly, the turnout estimates reported in Carpio et al. (2018) for the subnational elections of 2010 and 2014 are in line with our estimates for national elections below.

To estimate the effect of a marginal change to the abstention fine on our relevant out-

[26] Resolución 1400-2006-JNE mentions the discovery of abnormal increases in the number of registered voters for the 2006 subnational elections across the country. For the 2018 subnational elections, it was estimated that there were at least 100,000 swallow voters in more than 150 districts (La República, 2017).

comes, we employ the following specification:

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

where $\text{Fine}_{d,e}$ is the value of the abstention fine in Peruvian soles (S/) and α_d and $\delta_{p,e}^{\text{cat06}}$ are fixed effects analogous to those in equation (1). Hence, we are exploiting the same source of variation as in the difference-in-difference specification. The coefficient of interest is ν, which represents the average change in the outcome (e.g. percentage points of turnout) for a S/1 increase in the value of the fine. We also estimate the elasticity of turnout with respect to the fine by using logarithms for the outcome and the value of the fine. $u_{d,p,e}$ is an error term that we cluster at the province level.

We weight specifications (1) and (2) by the number of registered voters for the 2001 elections, as our object of interest is the decision made by each individual voter and we want to assign equal importance to all voters, irrespective of district size. Results are qualitatively similar if we do not weight by the number of voters, as shown in the online appendix.

4 The Value of the Abstention Fine and Voter Turnout

In this section, we analyze the effect of the reform to 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. Secondly, we show estimates of the marginal effect of the fine on turnout and the corresponding elasticity. We complement this analysis by examining possibly heterogeneous impacts by type of election and time horizon.

Figure 5 shows estimates of the coefficients of interest in equation (1), with their associated 95 percent confidence intervals. The arrowhead markers correspond to point estimates of the average difference in turnout between districts in the relevant category (non-poor or extreme poor) and the omitted one (poor), relative to the difference in the 2006 presidential run-off (omitted election). We use upward-pointing arrowheads for districts classified as non-poor, indicating that these districts have a relatively higher fine for abstention after 2010 (twice as large as in poor districts). Similarly, we use a downward-pointing arrowhead for districts classified as extreme poor to indicate that these districts have a relatively lower fine (half the size of that in poor districts).[27] The dashed line shows the timing of the adjusted assignment to the poverty categories in October 2010.

[27] Table A2 in the online appendix shows the corresponding point estimates. Figure A1 also in the appendix shows results from a more disaggregate specification that estimates separate coefficients for each combination of assignments in 2006 and 2010.

None of the estimates for the elections before the reform are statistically significant at conventional levels, implying that voter turnout followed parallel trends in all three poverty categories across the four national elections of 2001 and 2006. These results lend increased credibility to the research design and make us confident we can attribute any subsequent change to the changes in abstention fines. In the period after the reform, we observe a systematic divergence in turnout among the three groups. As expected, non-poor districts show a steady increase in turnout, while in extreme-poor districts we find the opposite effect. The impact of the reform on turnout is statistically significant for the non-poor group since the 2011 general election, but the magnitude of the effects is much larger and very precise for both groups in 2016. In the 2011 elections, people in non-poor districts were 1.3 percentage points (pp) more likely to vote than in poor districts, but there was no statistically significant difference in turnout between poor and extreme poor. In 2016, voter turnout in non-poor districts was 2.3 and 2.9 pp higher than in poor ones for the general election and the run-off elections respectively, while in extreme-poor districts it was 1.4 and 2.5 pp lower than in poor ones for these same elections. If we compare non-poor to extreme-poor districts, where the difference in the value of the fine is largest, we observe respective turnout gaps of 3.7 and 5.4 pp for the general and run-off elections of 2016. These are large effects and constitute evidence of a substantial response by voters to the monetary incentives provided by the reform. Figure 5 provides visual evidence of a growing impact of the reform over time, as well as of possible heterogeneity between the general and run-off elections. We formally examine these sources of heterogeneity below.

Panel A in Table 1 presents estimates of equation (2). The estimate of ν in column 1 indicates that a S/10 increase in the value of the fine (roughly \$3) leads on average to an increase in turnout of about half a percentage point (0.0049). 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, which we obtain by replacing the levels of the abstention fine and voter turnout with their logs. We estimate an average fine-elasticity of turnout of 0.03. Both coefficients are very precisely estimated and are statistically significant at the 1% level.

The remaining columns in the table show results from extensions of equation (2), in which we study potential heterogeneity in the marginal impact of the monetary incentive by including interactions of the fine with other variables. In column 2, we introduce the interaction with a dummy for the 2016 elections (general and run-off). The omitted category corresponds to the elections in 2011, as the value of the fine was homogeneous across districts for all previous elections. Consistent with the evidence in Figure 5, 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 three times larger in 2016 than in 2011. The implied elasticity of turnout with respect to the fine is 0.011 for 2011 and 0.048 for 2016 (column 2, Panel B). This increasing response over time is consistent with the idea that agents gradually learn about the modified incentives provided by the reform and adjust their behavior accordingly. We explore this mechanism in greater detail in section 7.

Column 3 explores whether the marginal effect of the fine on turnout varies depending on the type of election. For this purpose, we include in our baseline specification 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 2011 and 2016. 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, panel B 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. One plausible explanation is that voters are more intrinsically motivated to participate in the general election than in the presidential run-off.[28] If this is the case, voter turnout in the general election should be systematically higher than in the run-off, as Figure 1 shows. A stronger intrinsic motivation to vote in the general election may result from voters generally caring more about the outcome of the legislative than the presidential election, or from a weaker sense of civic duty for the run-off (having voted a few weeks before in the general election). Supporters of presidential candidates that do not progress to the run-off may also feel disillusioned and, thus, less compelled to turn out to vote. Unfortunately, our data does not allow us to dig deeper into the mechanisms driving the heterogeneity between types of election. However, this finding highlights the importance of taking such heterogeneity into account when evaluating voter mobilization initiatives, since the effect of the exact same policy can vary depending on the type of election taking place and the marginal voters being affected.

It is worth noting that the effect of the abstention fine in the general election in column 3 remains positive and statistically significant. Its magnitude is not far from the average effect reported above. This fact is important, as voters in the legislative election are voting for different candidates across regions and they are choosing from a large pool of candidates in the first round of the presidential race. Hence, the results for the general election provide

[28] Assume that voters derive an expressive benefit from voting (e.g., Dellavigna et al., 2017) that is larger in the general than in the run-off election. Voters also face a cost of voting that includes a deterministic component (including the abstention fine) and a random one (e.g. the weather). 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.

evidence that we are not simply capturing differential abstention across income groups over time in response to changing characteristics of the top contenders in the presidential election.

Finally, column 4 examines potential heterogeneity in the marginal effect of the fine depending on the poverty category to which the district was assigned. Such differential effects could arise because of the magnitude of the change or differential exposure to enforcement. In this column, we include the interaction of the abstention fine with a dummy for districts classified as extreme poor in 2010. Reassuringly, the estimate for the interaction term is very small and statistically insignificant. Likewise, we do not find evidence of a different elasticity of voting with respect to the fine for extreme-poor districts in panel B.[29]

In sum, we observe substantial heterogeneity in the marginal effect of the abstention fine on voter turnout depending on the election year and the type of election, but not between poverty categories. Figure 6 provides further evidence on the heterogeneity documented in Table 1. Panel (a) shows 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 hike to the fine leads to a substantial 0.8 pp increase in voter turnout. This specification also allows us to compare the effect of the fine for the two types of election in the same year, and vice versa. All differences are statistically significant at the 5% level. Panel (b) shows analogous estimates for the elasticity.

5 The Value of the Abstention Fine and Voter Registration

We can decompose the estimated effect of the abstention fine on voter turnout into potentially separate effects on the decision to vote (the numerator) and the decision to register to vote (the denominator). Studies analyzing voter turnout, whether employing experimental methods or not, often overlook these distinct dimensions. In some cases, there is no reason to expect the specific intervention or factor under study to affect voter registration, while in other cases the short duration of the study (especially in the case of field experiments) does not allow sufficient time for registration effects to materialize.[30] However, if citizens are awarded some leeway with regards to voter registration, this preliminary decision can have profound effects on representation and downstream policy outcomes (Bertocchi et al., 2017).

As mentioned in section 2, voter registration in Peru is automatic for all citizens of voting

[29]Tables A3 and A4 in the online appendix show that the results are very similar if we use less-conservative province x election fixed effects or if we run the regressions without weighting by registered voters in 2001.

[30]Studies on the effects on turnout of voter registration campaigns or policies include Ansolabehere and Konisky (2006); Nickerson (2015); Holbein and Hillygus (2016); Braconnier et al. (2017); Chong et al. (2018)

age (18 or older), as recorded in their national identification document (DNI). The DNI also contains the person's home address, which determines the district in which the person is registered to vote. Citizens must obtain an adult DNI (different from the under-age version) when they turn 18 years old and must renew the DNI every eight years or if their personal information changes, including a change of address. Hence, it is possible to change the district of registration by reporting a different address. Although proof of address should be provided for such a change, in practice this is not always the case. It is, of course, illegal to report a fraudulent address.[31]

In this section, we study voter registration as an outcome potentially affected by the reform to the abstention fine. In particular, we test the hypothesis that the segmentation of the fine across districts led to irregular 'migration' of voters towards areas with a lower fine. For this purpose, we first estimate equation (1) using the log of the number of registered voters in a district as the dependent variable. For this regression, we only have one observation per election cycle for each district because voter registration does not change between the general election and the presidential run-off in the same year. Hence, we set 2006 as the omitted election cycle, while the omitted category still corresponds to districts classified as poor in 2010. Figure 7 shows the results, while Table A2 in the online appendix shows the corresponding point estimates.

As before, we can validate the difference-in-difference research design by looking at the period before the reform. The difference in voter registration across categories remains remarkably stable between 2001 and 2006, lending support to our identification strategy. After the reform, however, voter registration increases substantially in extreme-poor districts relative to the other two categories. Specifically, voter registration grows approximately 4.4% more in extreme-poor districts than in poor ones in 2011 and 6.1% more in 2016 (p=0.066 for both coefficients being equal). The difference with non-poor districts is even starker, at 5% in 2011 and 8.2% in 2016. All of these differences are statistically significant at the 1% level. On the other hand, in non-poor districts there is a relative decline in the number of registered voters, but of a much smaller magnitude (0.7% in 2011 and 2.1% in 2016) and not statistically different from zero. In other words, we observe systematic migration of voter registration from non-poor and poor districts to extreme-poor ones, which face the lowest fine for abstention. A potential explanation for the non-significant difference between non-poor and poor districts is that, conditional on changing address to avoid a higher fine, changing it to an extreme-poor district dominates changing it to a poor one, all else equal.

[31] Since 2015, address misreporting is punished with a fine equivalent to 0.3% of the UIT (El Comercio, 2015). Although misreporting is rarely investigated per se, the authorities made a substantial effort during the sample period to detect instances related to voter-buying (see footnote 26).

In Table 2 we provide estimates of the marginal effect of the fine on voter registration and we examine the extent to which changes in registration help explain the documented impact on voter turnout. Column 1 shows elasticity estimates from equation (2), using log voters as the dependent variable and log fine as the explanatory variable of interest. Consistent with the findings in Figure 7, a larger fine leads to a smaller number of registered voters in the district. We estimate a registration elasticity of -0.046, significant at the 1% level. Column 2 shows that the magnitude of the negative effect of the fine on voter registration grows over time, with the elasticity jumping from -0.035 in 2011 to -0.057 in 2016. As with turnout, the heterogeneous effect across election cycles provides evidence of gradual adaptation to the policy incentives over time.

In Column 3, we turn to investigate the marginal effect of the abstention fine on the numerator of the turnout rate, namely, the (log) number of votes cast in the election, which we use as a dependent variable. As expected, given the positive turnout elasticity reported above, we observe a smaller and statistically insignificant effect: the number of votes cast falls less than the number of registered voters for a same-sized increment to the value of the fine.[32] This indicates that the mechanical effect leading from fewer voters to fewer votes is being offset by the relatively high propensity to vote by those voters that remain in the district and are exposed to the higher fine. Column 4 shows that the reduction in the number of votes cast caused by an increase to the value of the fine is larger in 2011 than in 2016. This is consistent with the greater turnout effect in the latter year, documented in Table 1.

Taken together, the evidence points to an abstention fine increase having a strong negative effect on voter registration, together with a strong positive effect on voter turnout. This combination of responses is likely driven by a combination of a behavioral response with voter selection (i.e. only high-propensity voters are left in high-fine districts). To gauge the relative importance of selection in explaining the positive effect of the fine on turnout, we re-estimate equation (2), including log registered voters as an additional control. Admittedly, this is a somewhat crude approach and the additional control fits the description of a 'bad control' (Angrist and Pischke, 2009), but the sensitivity of the turnout elasticity to the additional control is informative about the mediating effect of voter registration. Columns 5-8 in Table 2 show the results. The comparison of column 5 with column 1 in Table 1 (panel B) reveals that the average turnout elasticity decreases only 10% when we control for voter registration. Similarly, the comparison of column 6 with column 2 in Table 1 (panel B again) shows that the turnout elasticity is 20% smaller in 2011 and 7% smaller in 2016

[32]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 2 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 $\frac{\partial \ln T}{\partial F} = \frac{\partial \ln V}{\partial F} - \frac{\partial \ln R}{\partial F}$

after controlling for voter registration. For completeness, columns 7 and 8 show comparable estimates with the level of turnout, rather than its natural log, as the dependent variable. The changes are essentially the same. We conclude that even though voters are indeed responding to the reform by strategically changing their address of registration to low-fine districts, the change in the composition of the electorate cannot explain more than 20% of the observed effect on turnout.

Naturally, our findings on voter registration depend on people being willing to incur the cost of DNI renewal in order to potentially avoid paying a larger fine in the hypothetical case that they fail to turn out to vote.[33] This assumption seems unwarranted for most of the population, as the renewal involves a payment of S/22, as well as multiple visits to the office of the national registry (RENIEC). We only find this assumption plausible in the case of young adults that have only recently reached 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, in Table 3 we estimate equation 2 separately for the log number of registered voters in different age groups. We find that the magnitude of the effect of the abstention fine on voter registration monotonically decreases for older age-groups (with the exception of 75+ in column 6) and is only statistically significant for the first two groups, corresponding to ages 18 to 20 and 21 to 29. In particular, column 1 shows a registration elasticity of -0.276 for the 18-20 age-group, which is six times larger than the average effect reported in Table 2.

The results in this section provide evidence that voters respond strategically to the differential abstention fine across districts by changing their address of registration to those with the lowest fine (extreme poor). This behavior, however, is limited to young adults (mostly first-time voters) facing a very small cost of address misreporting. The resulting change in the composition of the electorate explains only a small fraction of the observed change in voter turnout, which seems to be mostly driven by a change in behavior by voters who are not changing address. Still, this type of voter misbehavior, which differs from the politically-driven 'voter buying' that has been documented in the previous literature (Hidalgo and Nichter, 2016), could have important consequences for political participation, representation, and electoral outcomes. At the local level, this behavior is producing a mismatch between the public officials that voters elect and those that choose the policies that

[33]Table A5 in the online appendix provides evidence against actual migration as the mechanism driving the observed changes in voter registration. We show that economic activity, as proxied by nighttime lights (Henderson et al., 2012), is actually increasing in districts with a higher fine and that the value of the fine does not affect whether a respondent in the ENAHO national survey reports living in her birth district.

affect them. More generally, this behavior is also attracting voters with an arguably low propensity to vote to districts where they face even weaker incentives to do so, which will likely reduce their turnout increasingly as time goes by (Fujiwara et al., 2016).

6 The Value of the Fine, Enforcement and Debtors' Response

6.1 The Value of the Abstention Fine and the Settlement Rate

In this section, we examine the effect of the reform to the value of the abstention fine on the settlement of outstanding fines. Settlement is required to gain access to a host of government and financial services. Fines can be settled either by paying the charge at any of the more than 600 branches of the National Bank or by submitting a claim for an excuse in-person or by post to one of 17 JNE offices in the country. Processing of an excuse requires payment of a fee that is approximately equal to the value of the fine for the extreme poor category.

We analyze the aggregate settlement of outstanding fines per election and district and also study the disaggregate response along the margins of payment and excuses. Unfortunately, this data is not consistently available before the 2006 subnational elections. We are thus forced to include this election in the sample, in order to have at least one election before the 2010 assignment.[34] Note that the lack of data for previous elections also prevents us from testing for pre-trends in this setting.

Column 1 in Table 4 shows estimates of equation (2) using the share of fines settled (panel A), paid (panel B) and excused (panel C) as dependent variables. Panel A shows that a S/10 increase to the fine is associated with a 2.3 pp increase in the share of settled fines. This is a large effect and corresponds to a 6.2% increase relative to the sample mean of 0.36. A comparison of panels B and C provides the first evidence of heterogeneous responses in fine settlement. For the same S/10 fine hike, we find a 0.2 pp decrease in fine repayment (1% change over sample mean) and a quite substantial 2.5 pp increase in the share of fines that are excused, corresponding to a 15% increase over the sample mean. Mechanically, the coefficients in panels B and C add up to the one in panel A. They are all statistically significant at the 1% level.

These initial results should be interpreted with caution, though. As mentioned above, enforcement of the abstention fine improved substantially after 2012, when the national government created a collections unit within the JNE and awarded it with the power to freeze the bank accounts of debtors. This led to a sharp increase in the aggregate share of fines settled (see Figure 4). To better understand the role of the changes to enforcement,

[34] We also include the 2010 and 2014 subnational elections. The results are hardly affected if we exclude them.

column 2 in Table 4 shows results when we include the interaction of the fine with a dummy for the elections in 2014 and 2016. We observe that before these changes took place, the marginal effect of the fine on the settlement rate is a precisely-estimated zero (base term in panel A). However, this zero effect masks small offsetting effects in payment and excuses. In particular, for the elections in 2010 and 2011, a S/10 fine increase leads to a 0.26 pp drop in the share of fines paid (panel B) and to a 0.24 pp increase in the share of fines excused (panel C). These findings are consistent with the idea that whether people settle an outstanding fine or not depends on the burden resulting from the restrictions faced and not on the value of the fine. Furthermore, conditional on finding it desirable to settle the fine, a larger fine makes it relatively more attractive to pay for an excuse, all else equal. We also observe that the large marginal effects in column 1 are entirely driven by the elections under improved enforcement. The net marginal effect on settlement and excuses for the post-2014 elections, captured by the second-row coefficients in panels A and C, is an order of magnitude larger than that for the earlier elections, captured by the base term, indicating that improved enforcement had a substantial impact on debtors' behavior.

Finally, column 3 examines potential heterogeneity across poverty categories by including an interaction of the fine with a dummy for districts classified as extreme poor. Panel A shows that the average marginal effect of the fine on the settlement rate is actually negative and small for extreme-poor districts, with a S/10 hike leading to a 0.21 pp decrease in fine settlement. This negative effect results from roughly same-sized decreases of around 0.1 pp in payments (panel B) and excuses (panel C). The results show that the large positive effect of the value of the fine on the rates of fines excused and settled is driven by richer districts, in which the payment rate responds very little. This makes sense, as people in these districts are more likely to be affected by the coercive collection of outstanding fines (e.g. greater use of financial services), increasing their willingness to settle them. At the same time, the processing fee for an excuse in these districts is substantially lower than the value of the fine, making it more attractive to settle the fine by submitting a claim for an excuse. In extreme-poor districts, on the other hand, a larger fine makes people either less willing to pay it (if marginally indifferent due to low reliance on restricted services) or less able to do so (i.e. liquidity constraints). Conditional on wanting to settle the fine, paying it is likely to be a dominant strategy in these districts, as the cost is roughly equivalent to the processing fee for an excuse, but the transaction cost is much lower. Overall, these heterogeneous effects point once again to a highly sophisticated response to the incentives provided by the reform to the abstention fine.

6.2 Robustness of Results on Turnout to Changes in Enforcement

The previous findings raise the concern that our estimates of the marginal effect of the abstention fine on voter turnout and registration may be confounded by the changes to enforcement taking place during the sample period. Even though these changes only began in 2012, leaving our results for the 2011 election unaffected, our finding of a substantially larger marginal effect of the fine on turnout in 2016 could still be compromised. In this regard, it is reassuring that we have not found any evidence indicating that the reform to the abstention fine prompted the toughening of enforcement or that the poverty categories determining the value of the fine were intentionally used to target the renewed efforts at fine collection. Still, the spurious correlation between the share of non-poor people in a district in 2010 and the share of people affected by coercive collection after 2012 could give rise to omitted variable bias for the 2016 election. The results in Table 4 suggest this is worth exploring in detail.

To tackle the problem of the potentially confounding effect of improved enforcement, we rely on information on the districts targeted by the collections unit at JNE, as recorded in the agency's annual summaries of activities for the period 2012-2015 (e.g., JNE, 2015, p. 177-182). The vast majority of these districts are located in the provinces of Lima and Callao. The JNE documents also make it clear that additional efforts were concentrated in large cities and provincial capitals. Using these pieces of information, 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 version of equation (2) that allows for a time-varying effect, including these variables as controls.

A comparison of the results in column 1 of Table 5 with the original ones in column 2 of Table 1 (panel A), reveals that the estimates are entirely unaffected when we allow turnout to vary differentially in the targeted districts of Lima and Callao in 2016. Column 2 further shows that the results are equally unaffected if we exclude from the sample all the districts in these provinces, whether they were targeted or not. In column 3, we see that the marginal effect of the fine on turnout in 2016 decreases somewhat once we allow turnout to vary flexibly in provincial capitals in that year. The aggregate turnout effect for a S/10 increase in the 2016 election drops from 0.7 pp to 0.65 pp, a roughly 8% decrease in magnitude. Column 4 shows that excluding all provincial capitals has no effect on the results. In column 5 we use the measure of actual gains in fine settlement between 2006 and 2014. This is another example of a 'bad control' (Angrist and Pischke, 2009), but we are again just interested in examining the sensitivity of our coefficients of interest to the inclusion of what is likely to be a quite powerful control variable. We observe only a 15%

decrease in the magnitude of the 2016 marginal effect of the fine on turnout. Finally, column 6 includes all three controls simultaneously. In this horse race specification, all the additional variables have a positive and significant coefficient, indicating that we are indeed capturing factors associated with higher turnout. However, the aggregate marginal effect of the fine on turnout in 2016 remains at 80% of its baseline value. We conclude that only a small fraction of the estimated effect of the fine can be explained by improved enforcement.

7 The Demand for Information about the Abstention Fine

Our previous findings provide evidence of a sophisticated and multi-dimensional response to the economic incentives provided by the reform to the fine for abstention. Adjustment along all of the margins we have analyzed requires somewhat detailed knowledge about the fine and about the institutional framework that surrounds it. However, León (2017) shows that information about the reform was not readily available and that people's perception of the value of the fine remained close to its pre-reform value in 2010. In this section, we look for evidence that voters sought to acquire the necessary information about the reform to the abstention fine that allowed them to respond accordingly.

People can acquire information about the abstention fine from the media, by word-of-mouth or by visiting a government office, among other channels. Unfortunately, data on most of these sources of information, including local newspapers and radio stations, is not readily available. One source that does leave a data trail is online searches. People can search the web to see where they are registered to vote or whether they have an outstanding fine. They can also search for information about the value of the fine in their district or about the process that must be followed to obtain an excuse, all of which is available on the JNE and ONPE websites. Data from a national household survey shows that 31% of the population used the internet in 2007, while 46% used it in 2016 (INEI, 2018).[35] This means that almost a third of Peruvians were using the internet before the reform and almost one half were using it by the end of the sample period. These figures are high enough to make an exercise based on internet searches meaningful with regards to learning about the acquisition of information by a sizable share of the population.

As mentioned in section 3, we use publicly available data from Google trends to construct a monthly panel of search intensity for 44 different search terms between 2005 and 2016. Three of these search terms relate to the abstention fine. Since internet use appears to have grown almost 50% in the period that we study, a simple before-after comparison of

[35] The number for 2016 is consistent with the number of Facebook accounts registered in the country at the time, corresponding to 55% of the population (Gestión, 2016).

the frequency of internet searches related to the abstention fine will likely be confounded by this dramatic increase. A more nuanced approach involves asking whether the frequency of internet searches related to the abstention fine in Peru grew disproportionately to other search terms after the reform. We use a difference-in-difference design to answer this question and estimate the following empirical model:

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

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.[36] $\nu_{i,t}$ is an error term that we cluster two-way by search-term (44 clusters) and month (144 clusters).

Figure 8 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 have no elections, the relative popularity of fine-related searches decreases back to its baseline level. In 2009, the year before the adjusted district assignment to the poverty categories, Google searches related to the abstention fine were just as common as they were four years before.

In 2010, when the adjusted assignment preceded the subnational elections by a few days, 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 are somewhat larger and more precise from 2014 onward, which could be related to the toughening of enforcement studied in the previous section. Taken together, these results provide *prima facie* evidence of increased demand for information about the abstention fine after the reform, and lend increased credibility to our findings from the previous sections.

[36]Figure A2 in the appendix shows results from a more disaggregate specification at the monthly level.

8 Pecuniary and Non-pecuniary Incentives of Compulsory Voting

The results in section 4 provide evidence that the size of the abstention fine has a large and robust positive effect on voter turnout, conditional on having compulsory voting. In this section, we examine the extent to which the monetary incentive provided by the fine explains the aggregate effect of compulsory voting. Previous research has argued that compulsory voting provides a bundle of monetary and non-monetary incentives, including the expressive value of the law as a source of information on actions or behaviors that society deems desirable. An emphasis on these non-monetary incentives helps explain the fact that compulsory voting has large effects on turnout even in settings with a negligible fine and very weak enforcement (Funk, 2007). Our objective in this section is to benchmark the estimated fine-elasticity of turnout against an estimate of the aggregate effect of compulsory voting, so as to 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-table level. The voting table corresponds to the specific booth in which voters cast their vote, within polling stations. Voters are assigned to a specific voting table according to a 'voting group' number that appears on their DNI and can only vote at that specific table. Each voting table is meant to have around 300 voters. Once this number is reached, new registered voters assigned to the polling station are allocated to a new table, generating random variation in the age composition of each table. In our sample, 75% of tables have a number of registered voters between 281 and 334.

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 with ages between 18 and 69 (both inclusive). Our identifying assumption is that 70 year-old voters 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 table for the 2016 elections, we calculate the share of the electorate in each table with each possible age, ranging from 16 to 122.[37] Our empirical strategy compares voter turnout in tables with varying shares of 'almost-exempt' 69-year-old voters and 'barely-exempt' 70-year old ones. Naturally, differences in the shares of these two age groups could be a reflection of broader differences in the age composition of the electorate. Hence, our regression flexibly controls for the share of registered voters belonging to every other age group. We also include

[37]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.

district or polling station fixed effects to ensure that we are not picking up differences across locations, including the value of the abstention fine. The richness of the data allows us to compare voting tables 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 69- and 70-year-olds. Pooling data from the 2016 general and run-off elections, we estimate the following specification:

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

where the dependent variable is the turnout rate in table p, located in district d for election e (general or run-off). α_d and γ_e are district and election fixed effects. We replace the former for the more stringent polling station fixed effects in some specifications. $\text{share}(\text{age} = \tau)_{p,d}$ measures the share of registered voters in voting table p 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. Hence, λ_{70} tells us the effect on turnout from having a voting table including exclusively 70-year-old voters (all exempt from compulsory voting), relative to one with only 69-year-olds (all required to vote). $\epsilon_{p,d,e}$ is an error term that we cluster at the district level (1,854 clusters). Regressions are weighted by the number of registered voters in 2016 per voting table.

Figure 9 plots the estimates from this regression for ages around the threshold for exemption from compulsory voting. The coefficients for all ages between 60 and 68 are small and statistically insignificant, indicating that voters slightly younger than 69 turn out to vote at almost identical rates to 69-year-olds. These results constitute evidence of the comparability of voters with very similar ages and help validate our identification strategy. The coefficients for ages 70 and up, on the other hand, are all negative and statistically different from zero. Furthermore, they drop dramatically, indicating a sharp decline in voter turnout from age 70 onward. This could, of course, be related to the deterioration of health and other impediments preventing elderly voters from going to the polls. But even the age-70 coefficient, telling us the effect of a one-unit increase in 70-year-old voters at the expense of 69-year-olds, shows a drop in turnout of slightly less than 10 pp.

The magnitude of the reduction in turnout at age 70 is more clearly seen in column 1 of Table 6, which shows the corresponding point estimate of -8.5 pp. The remaining columns in the table provide evidence of the robustness of this result. In column 2 we restrict the sample to voting tables with between 280 and 300 registered voters and find a roughly similar effect, indicating that the results are not driven by very small or very large voting tables. Columns

3 and 4 provide separate estimates for the general election and the presidential run-off. Both columns show similar results, as opposed to the differential effect of marginal changes to the value of the fine documented above, indicating that the extensive and intensive margins of compulsory voting affect different sets of marginal voters.[38] Finally, column 5 shows that the results are robust to the substitution of district fixed effects with the more stringent polling station fixed effects. The coefficient is again very similar, at -10.3 pp. These estimates of the effect of compulsory voting on turnout are broadly in line with the findings for other settings of between 5 and 15 pp (e.g., Funk, 2007; Lopez de Leon and Rizzi, 2014; Cepaluni and Hidalgo, 2016; Hoffman et al., 2017).[39]

We can now use our estimate of the aggregate effect of compulsory voting 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 2 of Table 1 (panel B) indicates that a complete elimination of the fine (100% reduction) would lead to a 4.8% reduction in turnout, equivalent to a 4 pp drop from the observed 2016 turnout rate of 0.82. A reduction in turnout of this size is equivalent to 47% of the drop resulting from the exemption from compulsory voting at age 70, reported in column 1 of Table 6 (39% if we use the estimate in column 5 instead). Separate back-of-the-envelope calculations for the general election and the presidential run-off, combining the elasticities in panel (b) of Figure 6 with the estimates in columns 3 and 4 of Table 6, reveal that the abstention fine explains 34% and 62% of the respective aggregate effect of compulsory voting in these elections.

These back-of-the-envelope calculations show that monetary incentives can explain about one half of the aggregate effect of compulsory voting. The fact that the value of the abstention fine is not irrelevant for the effectiveness of compulsory voting has important policy implications. In particular, it indicates that the introduction of compulsory voting with a low or weakly enforced fine, while politically palatable, may not yield the expected increase in voter turnout. At the same time, our back-of-the-envelope calculations also shows that the non-monetary incentives provided by compulsory voting are just as important as the monetary incentives when trying to explain the aggregate effect on voter turnout. Overall, these results indicate that political participation is driven both by a rational response to economic incentives and by social image concerns.

[38] These results should be interpreted with caution, as they are based on only one election of each kind. In particular, the 2016 run-off between Keiko Fujimori and Pedro Pablo Kuczynski was highly contested. The election was won by Kuczysnki by a quarter of a percentage point.

[39] Fowler (2013) and Bechtel et al. (2018) find much larger effects of compulsory voting on turnout of around 30 percentage points in Australia and Switzerland. The much larger impact in these cases may be driven by the type of election, baseline turnout rates or enforcement levels, among other factors.

9 Discussion: Lessons for Policy Scale-up and Targeting

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 provide an explanation for the observed discrepancy. We also use our findings regarding voters' multi-dimensional response to monetary incentives for turnout to illustrate the challenges faced in the scale-up and targeting of voter mobilization initiatives.

Only two previous studies, both involving field experiments, have estimated the marginal effect of monetary incentives on voter turnout. Costas 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. Gianmarco León (2017) provided information about the modified value of the abstention fine in Peru to a random set of voters in ten districts near Lima. 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 less than a third of the size of the 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). In our setting, the smaller effect size is likely driven by the fact that the voters we study are imperfectly informed about the reform and the modified value of the fine, while the treatment in León (2017) involved providing salient information about these changes to each individual voter. Lack of knowledge about the reform (or low salience) leads to imperfect compliance and to a dampened marginal effect of the fine on turnout. In this regard, the positive trend we observe in the size of the effect over time, combined with our findings on information acquisition, suggest that voters are becoming increasingly aware of the reform, which may lead to convergence of the policy effects with the experimental results in the long term.

Even though the existing literature on policy scale-up has identified imperfect compliance as a source of voltage drop (Al-Ubaydli et al., 2017), it has mostly attributed it to differences in implementation when public employees replace members of a research team or NGO (Davis et al., 2017). This is understandable, as the vast majority of the literature has been concerned with interventions related to the delivery of goods and services. In our setting, however, imperfect compliance results from 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 relationship to the state. This is a problem for scale-up because most field experiments on voter mobilization involve some engagement with potential voters (e.g., in-home visits, direct mail), while policy changes to the rules and incentives often leave it up to individuals to acquire information and learn about them.

Another recurrent concern regarding policy scale-up is low external validity or context dependence of the findings from localized field experiments (Banerjee et al., 2017; Muralidharan and Niehaus, 2017). Such a concern is especially relevant for the topic at hand, as all of our current knowledge is based on field experiments taking place in two towns in California (Panagopoulos, 2012) and close to a dozen districts around Lima (León, 2017). Our setting allows us to study the response of millions of voters in thousands of districts across an entire country. However, an interesting trade-off arises because our difference-in-difference estimator captures the average treatment effect on the treated (ATT), while the experiments measure the average treatment effect (ATE) for the study sample, which is the relevant parameter for policy (Blundell and Dias, 2009).

The limitations to the external validity of the field experiments come also from the fact that, as is common in the literature, these experiments are very short-lived and only examine the effect of monetary incentives on turnout in the next election. Our finding of a heterogeneous effect of the abstention fine on turnout by election type provides a clear illustration of the limitations to the external validity of the experimental studies on voter mobilization. Our results on voter registration point in the same direction, as they highlight a response margin that is not usually studied by field experiments and that takes time to materialize.

Furthermore, the fact that there are so few studies on this topic, despite its immediate policy relevance, is a reflection of the fact that credible sources of variation in monetary incentives to vote are not readily available. In this regard, the fact that (Panagopoulos, 2012) was able to exploit a quirk in California state law allowing for the private provision of monetary incentives to vote likely reflects broader differences in the legal and political institutions of that particular state and constitutes a good example of randomization or site-selection bias (Allcott, 2015; Banerjee et al., 2017).

Yet another often-cited concern regarding scale-up is that localized trials may fail to capture general-equilibrium or spillover effects. Our finding on the disproportionate increase in voter registration in districts with lower fines by young adults provides a good example of such an effect. Our estimates on the marginal effect of the fine on turnout also incorporate a general-equilibrium effect of a different kind, which results from voters facing a modified abstention fine brought about by a policy reform incorporating into their behavior the fact that other voters in their district face a similar modified incentive.

As mentioned in section 2, the segmentation of the fine aimed at reducing the impact of the fine on poorer households. However, our findings on strategic registration by young voters in districts with lower fines provide evidence on the unintended consequences of geographic targeting. Additionally, our results on the heterogeneous response in settlement of outstanding fines across poverty categories suggest that the segmented fine may not be successful at reducing the negative distributional consequences of the homogeneous fine.

10 Concluding Remarks

How much turnout does a $1 incentive yield? Can a policy reform aimed at increasing turnout have unintended consequences on voters' behavior? These are important questions to answer when increasing turnout is a desirable policy objective and the cost-effectiveness of many potential policy interventions remains difficult to evaluate.

We show that direct monetary incentives are a powerful driver of electoral participation. Using administrative data from Peru and exploiting plausibly exogenous variation from a policy reform, we find that a a S/10 incentive (roughly US$3) leads to an increase in turnout of 0.5 pp, implying an elasticity of 0.03. Using an alternative identification strategy, we estimate that the removal of compulsory voting at age 70 leads to a drop in voter turnout of 9-10 pp. A back-of-the-envelope calculation indicates that the monetary incentive provided by the abstention fine explains roughly 50% of the aggregate effect on voter turnout brought about by the bundle of incentives provided by compulsory voting. Political participation appears driven in equal parts by a rational response to economic incentives and by social image concerns triggered by the expressive value of the law.

The average effect of a marginal increase to the abstention fine masks important dimensions of heterogeneity. The effect is larger in the longer term, which is consistent with voters not being immediately aware of the change, but gradually learning about it. We provide evidence from web searches related to the fine that confirms that the demand for information increases after the reform. The marginal effect of the fine is also larger for the presidential run-off than for the general election, which highlights the importance of testing voter mobilization initiatives across different types of election.

Voters respond along other margins as well. We show that the number of registered voters increases disproportionately in districts with a lower fine. This effect is driven by young adults registering to vote for the first time, which we interpret as evidence of fraudulent reporting of home addresses by those that face a specially low cost of misreporting. The resulting changes to the composition of the electorate across districts can have implications for representation and downstream policies. These results also illustrate an unintended consequence of targeted policies that may be difficult to anticipate in localized field experiments.

We also observe that voters in richer districts increasingly settle outstanding fines by submitting excuses as the value of the fine increases, while those in poorer districts respond by paying the fine at a lower rate. This heterogeneous response undermines the reform's stated aim of making the fine less burdensome for the poor.

Overall, our results provide evidence of a gradual, sophisticated and heterogeneous response by voters to large-scale, policy-induced monetary incentives for turnout. The body of evidence we present provides strong proof of voter rationality with regards to electoral participation. It also illustrates some of the challenges faced in the scale-up of voter mobilization initiatives tested through localized field experiments, as well as the potential pitfalls from targeted policies when state capacity is limited. Looking forward, our estimates of the marginal effect of a monetary incentive on voter turnout provide a useful benchmark against which to evaluate the cost-effectiveness of voter mobilization policies and campaigns.

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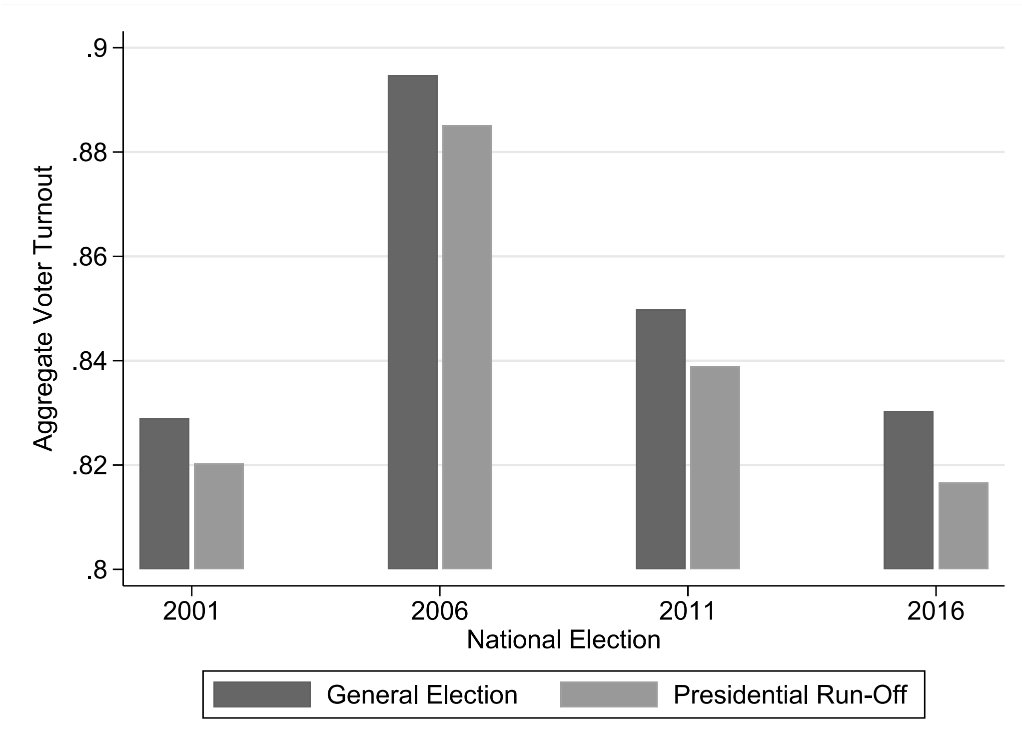
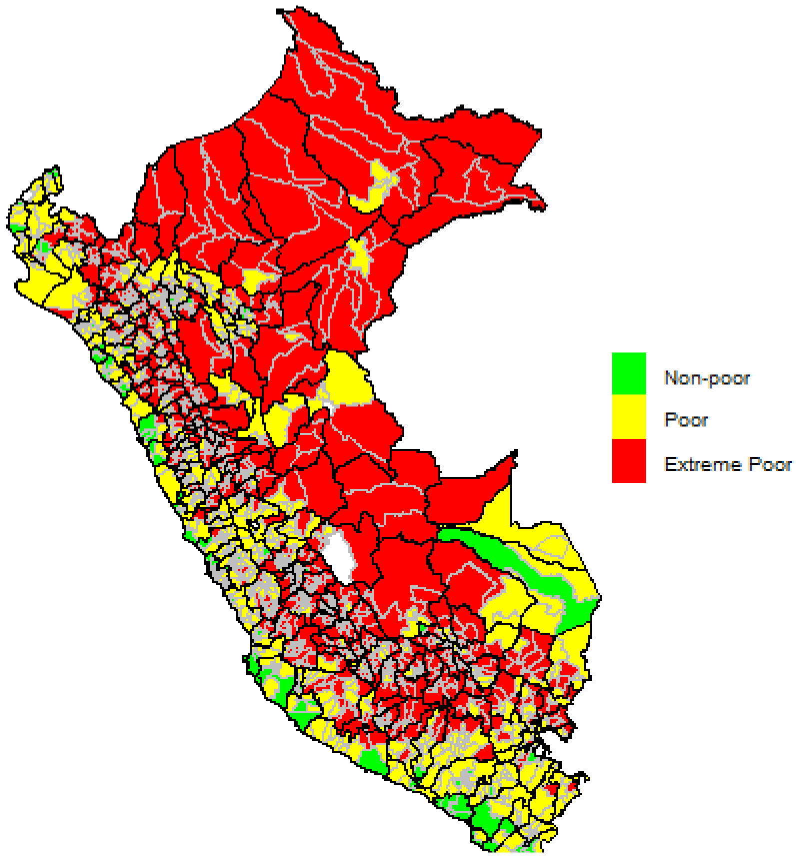
Figure 1: Aggregate Voter Turnout in National Elections

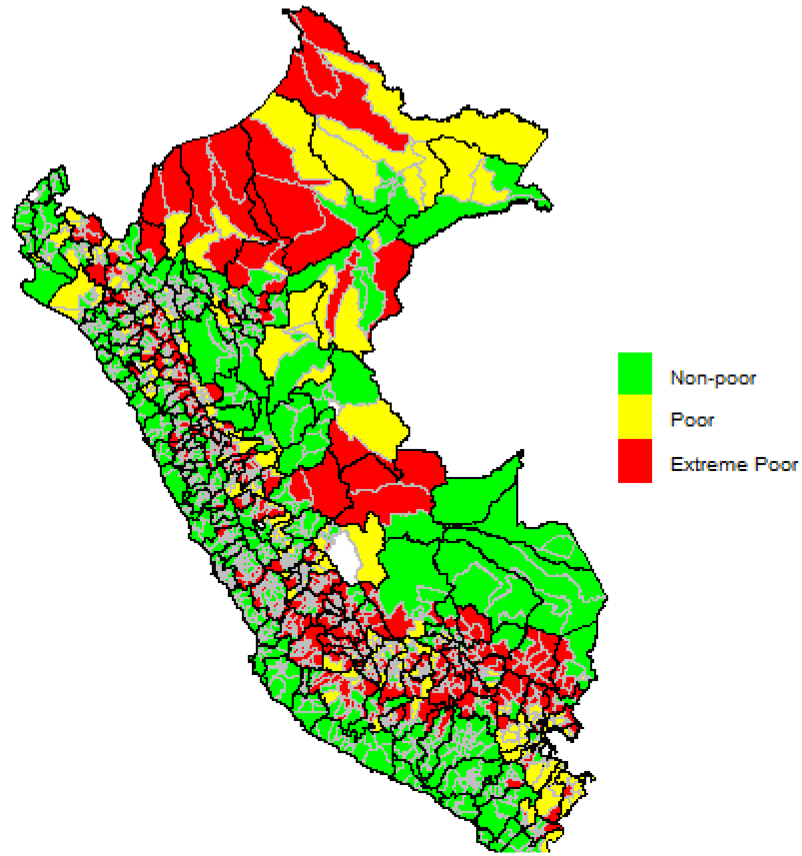


Note: The graph shows aggregate voter turnout for each national election in Peru between 2001 and 2016. The general election includes the first round of the presidential election and the legislative election, which take place on the same day. The presidential run-off takes place two months later if no candidate obtains more than 50% of the votes.

Figure 2: District Poverty Classification



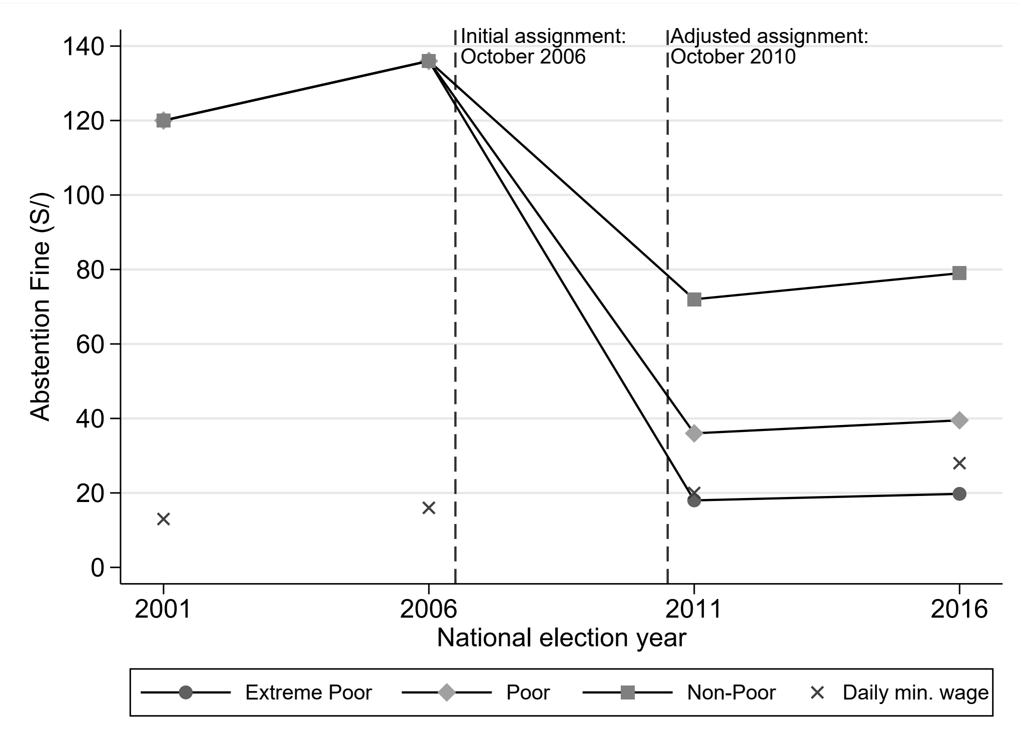
(a) 2006 assignment



(b) 2010 assignment

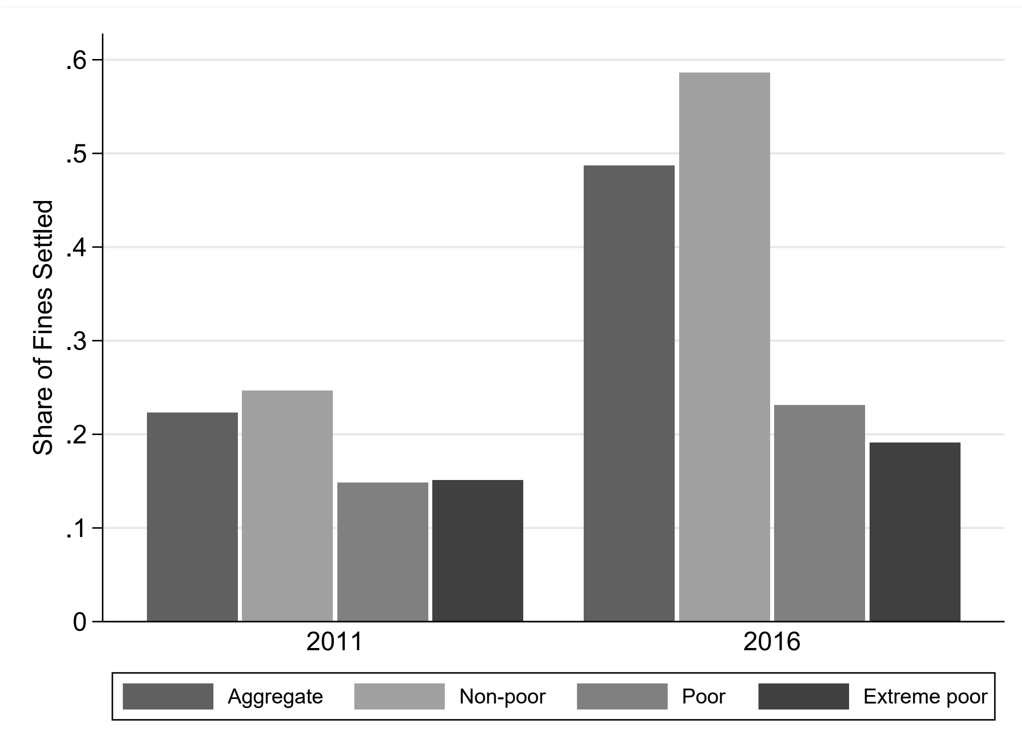
Notes: The map in panel (a) shows the location of districts in each poverty category according to the initial assignment in 2006. The map in panel (b) shows the location of districts in each category following the adjusted assignment of 2010. See text for details on classification criteria. Dark lines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

Figure 3: The Abstention Fine by Election and Poverty Category



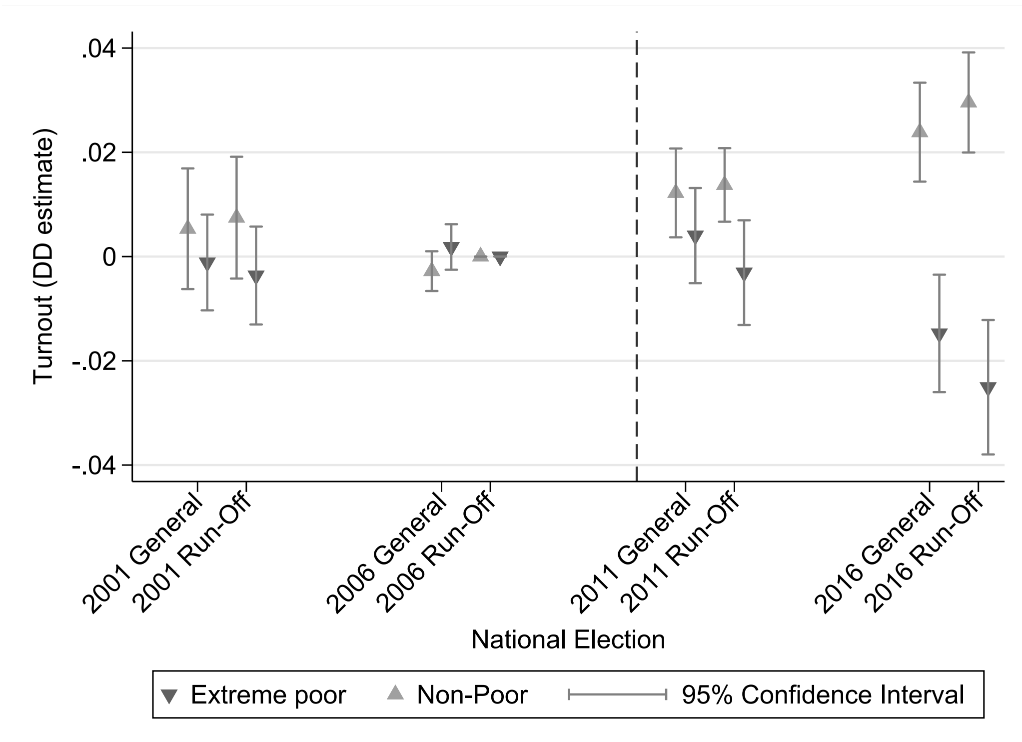
Notes: The graph shows the value of the fine for abstention corresponding to each poverty category for the national elections of 2001, 2006, 2011 and 2016. Values are displayed in nominal soles, but are defined in constant units for tax purposes (UIT), the value of which is updated yearly to adjust for 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 poverty categories took place (October 27, 2006) and the date in which districts were reclassified (October 1, 2010).

Figure 4: Share of Fines Settled by Poverty Category, 2011 and 2016 Elections



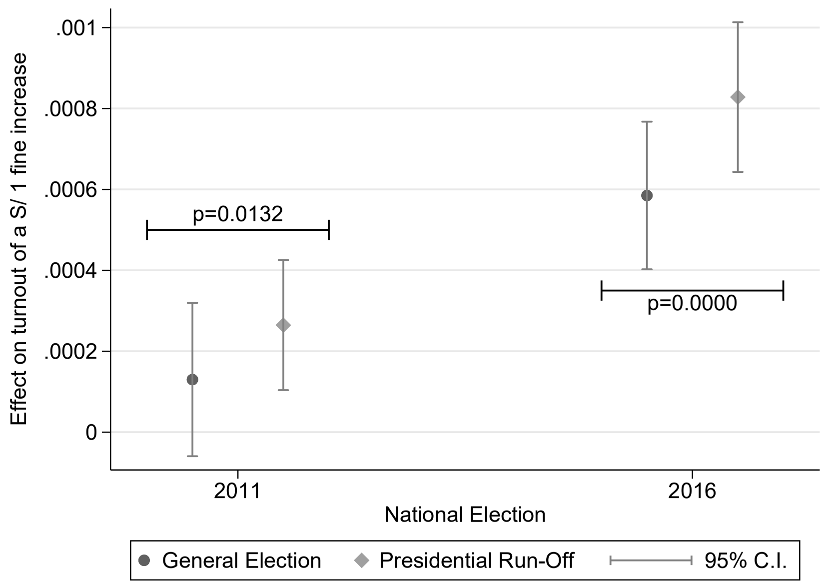
Notes: The graph shows the share of abstention fines settled in each poverty category, as well as the countrywide aggregate, for the national elections of 2011 and 2016. Settled fines include paid fines and valid excuses.

Figure 5: The Reform to the Abstention Fine and Voter Turnout

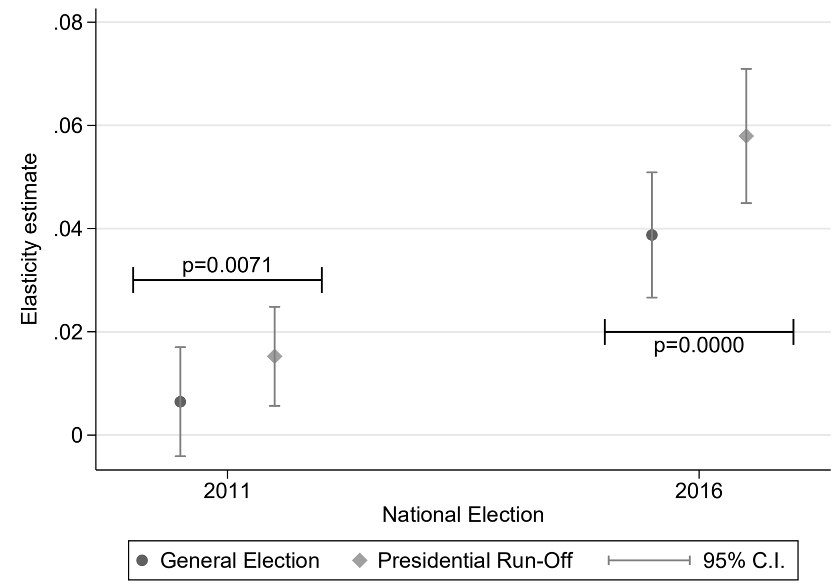


Notes: The graph shows point estimates and 95% confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with respective dummies for districts classified in 2010 as 'non-poor' and 'extreme Poor'. The omitted category includes districts classified as 'poor' in 2010. The omitted election is the 2006 presidential run-off. Regression includes district and province x election x 2006-poverty-category fixed effects. 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 dashed line indicates the date of the adjusted district assignment (October 2010).

Figure 6: Election-specific Estimates of the Marginal Effect of the Abstention Fine on Turnout and the Elasticity



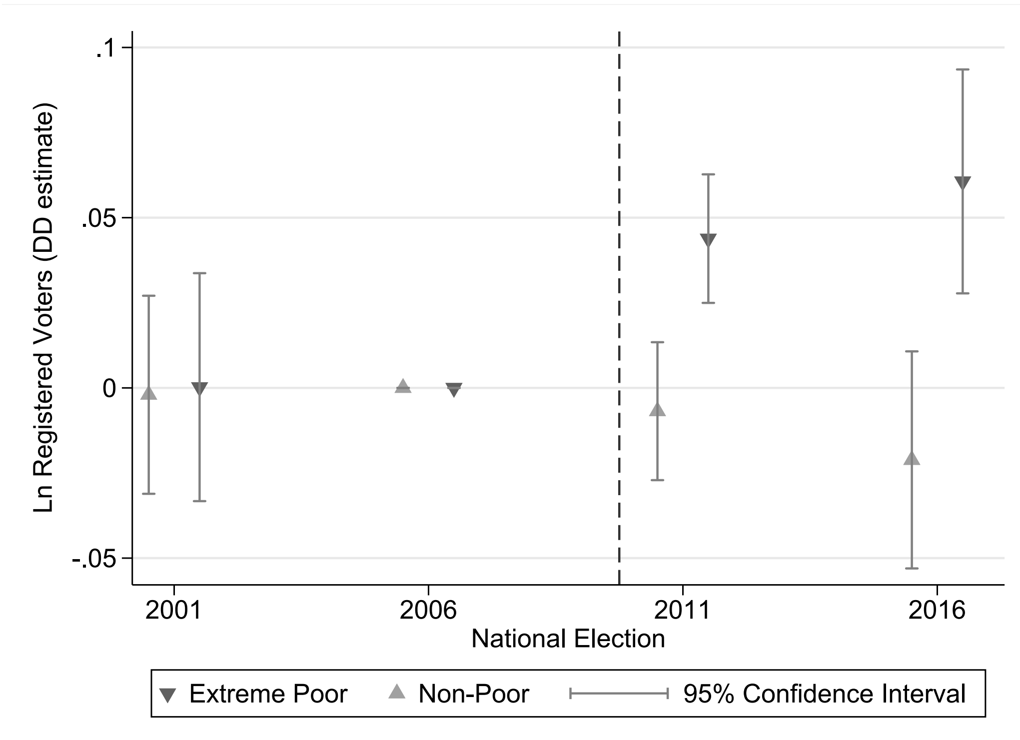
(a) Level effect



(b) Elasticity

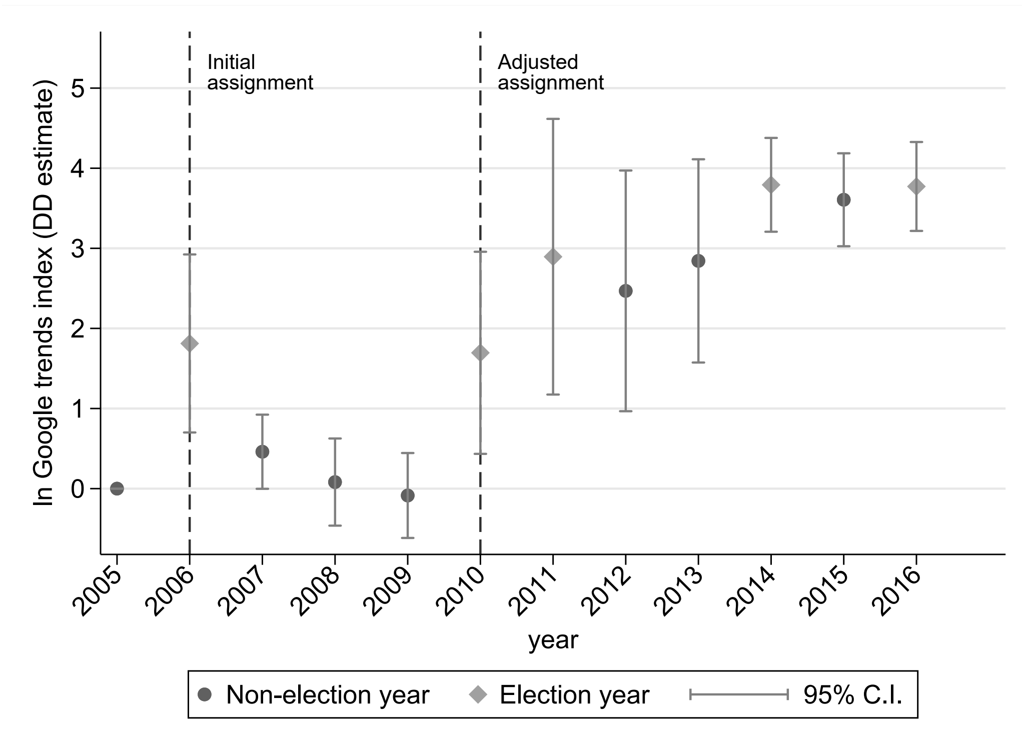
Notes: Panel (a) shows point estimates and 95% confidence intervals of a regression of district-level turnout on the value of the fine for abstention interacted with a full set of election-date dummies. Panel (b) shows point estimates and 95% confidence intervals of the equivalent regression for replacing 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).

Figure 7: The Reform to the Abstention Fine and Voter Registration



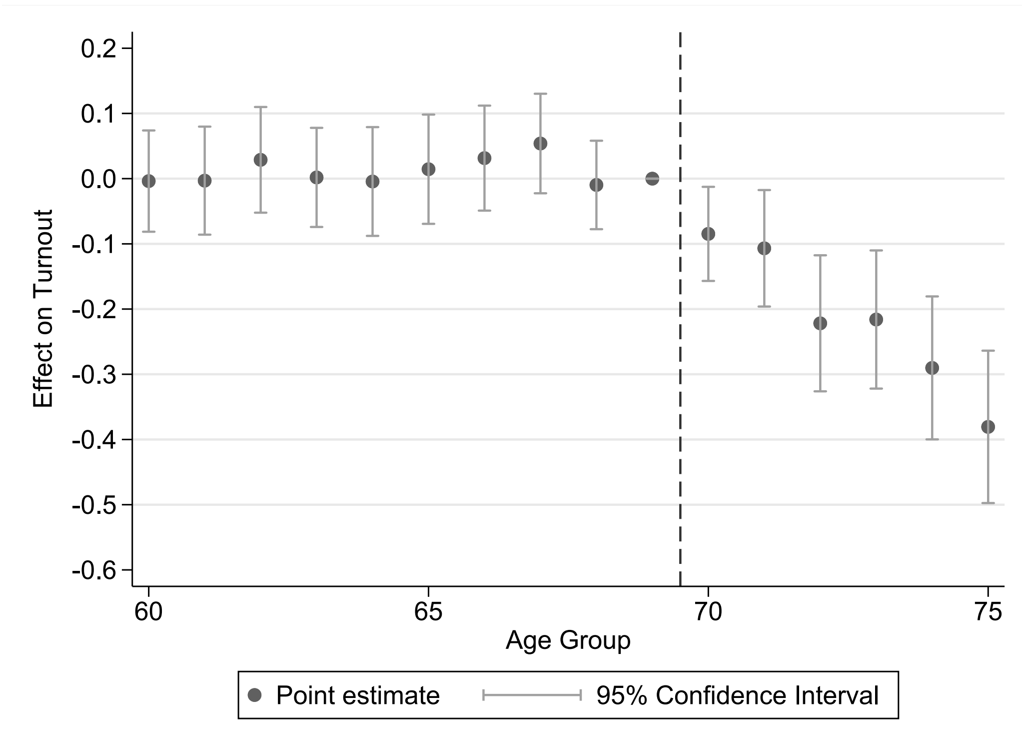
Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of district-level registered voters on a full set of election dummies interacted with respective dummies for districts classified in 2010 as "Non-Poor" and "Extreme Poor". The omitted group is made up of districts classified as "Poor" in 2010. 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 8: The Reform to the Abstention Fine and Information Acquisition



Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of a search-term popularity index from Google trends on year dummies interacted with an indicator for search terms related to the fine for abstention. Regression includes search-term and month fixed effects. The omitted year is 2005. Regression includes 6,336 observations from 44 search terms. See Online Appendix for list of search terms and details on construction of dataset. Standard errors are clustered two-way by search term and by month. 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 9: Senior Exemption from Compulsory Voting and Voter Turnout



Notes: The graph shows point estimates and 95% confidence intervals 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 60 and above 75 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.

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

	Baseline	Heterogeneous effects		
	(1)	(2)	(3)	(4)
(A) Dependent Variable: Turnout$_{\mathbf{i,t}}$				
Fine value$_{i,t}$	0.000487***	0.000197**	0.000390***	0.000497***
	[8.48e-05]	[8.49e-05]	[9.05e-05]	[0.000108]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$		0.000509***		
		[4.87e-05]		
Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{Run-Off})_t$			0.000194***	
			[4.03e-05]	
Fine value$_{i,t}$ $\times$ $\mathbb{1}(c_{10}\text{=Extreme Poor})_i$				-6.56e-06
				[5.02e-05]
R-squared	0.953	0.954	0.954	0.953
(B) Dependent Variable: ln Turnout$_{\mathbf{i,t}}$				
ln Fine value$_{i,t}$	0.0296***	0.0108**	0.0226***	0.0366***
	[0.00531]	[0.00486]	[0.00551]	[0.00859]
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$		0.0375***		
		[0.00328]		
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{Run-Off})_t$			0.0140***	
			[0.00272]	
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(c_{10}\text{=Extreme Poor})_i$				-0.00528
				[0.00535]
R-squared	0.943	0.944	0.943	0.943
Observations	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692
District FE	Yes	Yes	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is voter turnout (0-1) in panel A and the natural log of voter turnout in panel B. All regressions use data from national elections (General: Legislative and Presidential first round; 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 in panel A is measured in current Peruvian Soles (S/). In panel B, we use the natural log of the value of the fine. Column 2 includes the interaction of the fine with a dummy for the 2016 elections. Column 3 includes the interaction of the fine with a dummy for presidential run-Off elections. Column 4 includes the interaction of the fine with a dummy for districts classified as extreme poor in 2010. All regressions include district fixed effects and election by province by 2006 poverty category (non-poor, poor, extreme poor) 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 2: The Marginal Effect of the Abstention Fine on Voter Registration

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$_{i,t}$	-0.0460***	-0.0348***	-0.0164	-0.0240*	0.0268***	0.00872*		
	[0.0149]	[0.0123]	[0.0160]	[0.0136]	[0.00540]	[0.00496]		
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$		-0.0224***		0.0151*		0.0361***		
		[0.00858]		[0.00912]		[0.00331]		
ln Voters$_{i,t}$					-0.0615***	-0.0612***	-0.0495***	-0.0494***
					[0.00516]	[0.00521]	[0.00426]	[0.00430]
Fine value$_{i,t}$							0.000438***	0.000160*
							[8.58e-05]	[8.65e-05]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$								0.000488***
								[4.86e-05]
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	0.996	0.996	0.997	0.997	0.949	0.950	0.960	0.960
District FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header: ln Voters is the natural log of the number of registered voters for the national election cycle; ln Votes is the natural log of the actual number of votes cast in each election. Sample includes national elections (General: Legislative and Presidential first round; 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 current Peruvian Soles (S/). Even-numbered columns include the interaction of ln fine (or the level of the fine in column 8) with a dummy for the 2016 elections. Columns 5-8 include ln Voters as an additional control variable. All regressions include district fixed effects and election x province x 2006-poverty-category (non-poor, poor, extreme poor) 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 3: The Marginal Effect of the Abstention Fine on Age-specific Voter Registration

	Dependent variable: ln(Registered Voters for age group)$_{i,t}$					
	18-20	21-29	30-35	36-50	51-75	75+
	(1)	(2)	(3)	(4)	(5)	(6)
ln Fine value$_{i,t}$	-0.276***	-0.0551***	-0.0307	-0.0206	-0.0169	-0.0574
	[0.0426]	[0.0202]	[0.0219]	[0.0195]	[0.0240]	[0.0508]
Observations	5,076	5,076	5,076	5,076	5,076	5,076
Districts	1692	1692	1692	1692	1692	1692
R-squared	0.994	0.995	0.993	0.993	0.995	0.990
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable in the header: ln Voters is the natural log of registered voters for the relevant age group for the election cycle. Sample includes national elections for the years 2001, 2011 and 2016. The value of the fine is measured in current Peruvian Soles (S/). All regressions 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 Marginal Effect of the Abstention Fine on Settlement of Outstanding Fines

	Baseline	Heterogeneous effects	
	(1)	(2)	(3)
(A) Dependent Variable: Share of fines settled$_{\mathbf{i,t}}$			
Fine value$_{i,t}$ [a]	0.00231***	-2.50e-05	0.00246***
	[0.000235]	[8.31e-05]	[0.000262]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2014+)_t$ [b]		0.00353***	
		[0.000308]	
Fine value$_{i,t}$ $\times$ $\mathbb{1}(c_{10}$ = Extreme Poor$)_i$ [c]			-0.00457***
			[0.000970]
R-squared	0.977	0.979	0.977
p-value [a] + [b] = 0 or [a] + [c] = 0		0.000	0.019
(B) Dependent Variable: Share of fines paid$_{\mathbf{i,t}}$			
Fine value$_{i,t}$ [a]	-0.000202***	-0.000260***	-0.000179**
	[6.94e-05]	[6.97e-05]	[7.54e-05]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2014+)_t$ [b]		8.74e-05	
		[7.48e-05]	
Fine value$_{i,t}$ $\times$ $\mathbb{1}(c_{10}$ = Extreme Poor$)_i$ [c]			-0.000724*
			[0.000433]
R-squared	0.956	0.956	0.956
p-value [a] + [b] = 0 or [a] + [c] = 0		0.037	0.027
(C) Dependent Variable: Share of fines excused$_{\mathbf{i,t}}$			
Fine value$_{i,t}$ [a]	0.00252***	0.000235***	0.00264***
	[0.000212]	[4.98e-05]	[0.000237]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2014+)_t$ [b]		0.00344***	
		[0.000308]	
Fine value$_{i,t}$ $\times$ $\mathbb{1}(c_{10}$ = Extreme Poor$)_i$ [c]			-0.00385***
			[0.000767]
R-squared	0.965	0.969	0.965
p-value [a] + [b] = 0 or [a] + [c] = 0		0.000	0.078
Observations	11,721	11,721	11,721
Districts	1692	1692	1692
District FE	Yes	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes	Yes

Notes: Dependent variable in the header. Sample includes 2011 and 2016 national elections and 2006-2014 subnational elections. Value of the fine in current Peruvian Soles (S/). Regressions include district fixed effects and election by province by 2006 poverty category fixed effects. Column 2 includes the interaction of the value of the fine with a dummy for the 2014 and 2016 elections. Column 3 includes an interaction with a dummy for districts classified as extreme poor in 2010. 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 5: The Complementary Effects of the Value of the Fine and its Enforcement

	Dependent variable: $\text{Turnout}_{i,t}$					
	Targeted districts	Drop Lima & Callao	Province capitals	Drop capitals	Δ Share settled fines (06-14)	All
	(1)	(2)	(3)	(4)	(5)	(6)
$\text{Fine value}_{i,t}$	0.000197**	0.000173**	0.000197**	0.000180**	0.000197**	0.000197**
	[8.49e-05]	[8.72e-05]	[8.49e-05]	[8.98e-05]	[8.49e-05]	[8.50e-05]
$\text{Fine value}_{i,t} \times \mathbb{1}(2016)_t$	0.000509***	0.000507***	0.000456***	0.000509***	0.000409***	0.000370***
	[4.87e-05]	[5.03e-05]	[5.34e-05]	[5.14e-05]	[6.09e-05]	[6.21e-05]
$\mathbb{1}(\text{Targeted District})_i \times \mathbb{1}(2016)_t$	0.00596					0.0107**
	[0.00363]					[0.00446]
$\mathbb{1}(\text{Province capital})_i \times \mathbb{1}(2016)_t$			0.00831***			0.00817***
			[0.00193]			[0.00183]
Δ Share of fines settled $(06\text{-}14)_i \times \mathbb{1}(2016)_t$					0.0243***	0.0204***
					[0.00800]	[0.00704]
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	0.954	0.951	0.954	0.952	0.954	0.954
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Election-Province-Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Dependent variable is voter turnout (0-1). Data includes national elections (General: Legislative and Presidential first round; 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 current Peruvian Soles (S/). All regressions include the interaction of the fine with a dummy for the 2016 elections. 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 regressions include district fixed effects and election-date by province by poverty category (using 2006 classification) 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). *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table 6: Senior Exemption from Compulsory Voting and Voter Turnout

	Dependent variable: Turnout_i				
	Full sample	Registered voters $\approx$ 300	Only general election	Only presid. run-off	Polling station FE
	(1)	(2)	(3)	(4)	(5)
Fraction with age 68_i	-0.00968	-0.0348	-0.0408	0.0215	0.0113
	[0.0346]	[0.0365]	[0.0356]	[0.0391]	[0.0379]
Fraction with age 70_i	-0.0847**	-0.0969***	-0.0924***	-0.0770*	-0.103***
	[0.0368]	[0.0322]	[0.0356]	[0.0432]	[0.0337]
Observations	148,448	109,462	74,205	74,205	148,448
Districts	1854	1109	1835	1835	1854
Polling stations	4723	2725	4705	4705	4723
R-squared	0.799	0.835	0.804	0.822	0.809
District FE	Yes	Yes	Yes	Yes	No
Polling station FE	No	No	No	No	Yes
Mean dependent variable	0.82	0.82	0.83	0.82	0.82

Notes: Dependent variable is voter turnout (0-1). Data at the table level for the national elections (General: Legislative and Presidential first round; Presidential Run-Off) of 2016. The regressors are the fraction of the electorate registered to vote at the table for each age group from 16 to 122. The omitted category is the group with age 69. Estimates for age groups other than 68 and 70 are not shown. Columns 1-4 include district fixed effects, while column 5 includes polling-station fixed effects. Column 2 only includes voting tables that have a number of registered voters between 280 and 300. Column 3 only includes the general election of 2016, while column 4 only includes the presidential run-off. All regressions are weighted by the number of registered voters at the table for the 2016 national elections. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

Appendix (for online publication)

Table of Contents

A Additional Figures and Tables

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

2010 assignment	2006 assignment: Non-Poor	Poor	Extreme Poor	Total
Non-Poor	182	570	165	917
Poor	0	119	195	314
Extreme Poor	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.

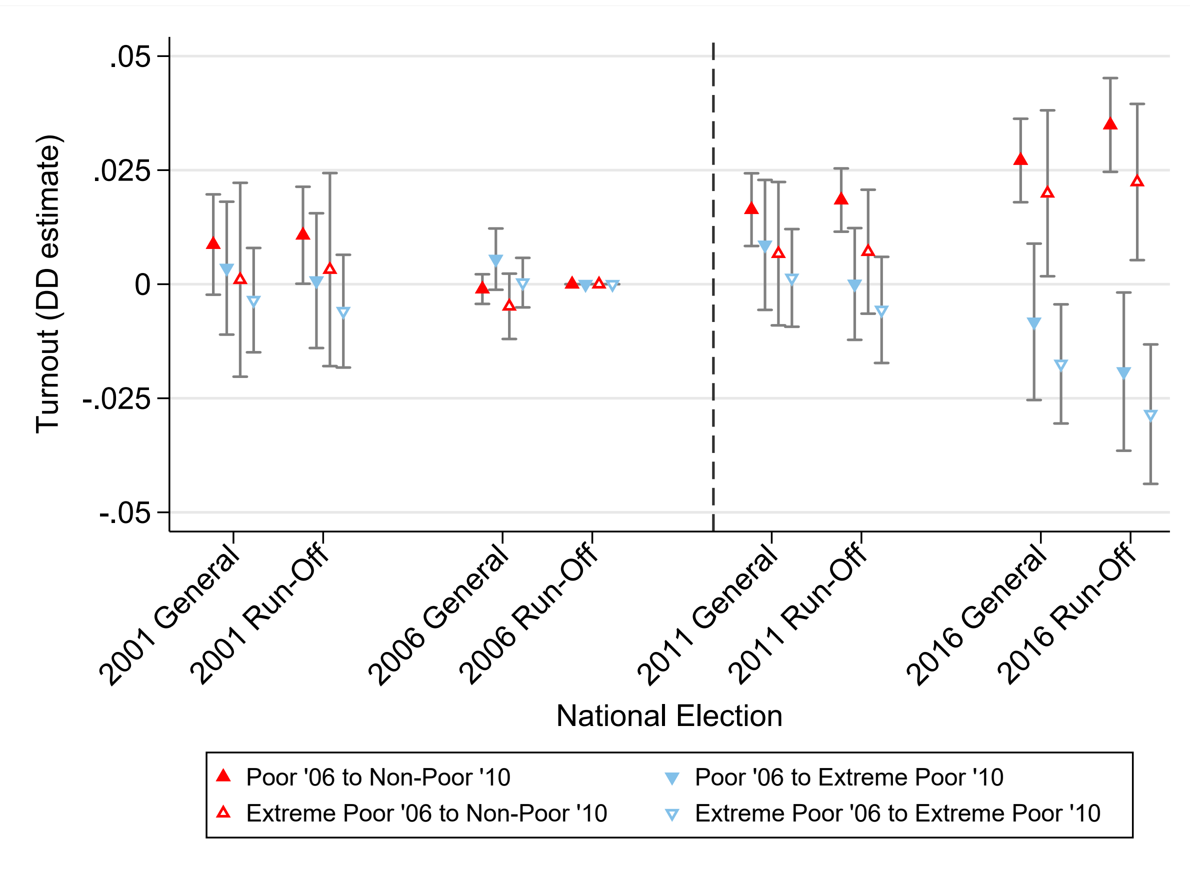
Table A2: Difference-in-difference estimates of the effect of the reform on voter turnout and registration

Dependent Variable:	$\text{Turnout}_{i,t}$	$\ln \text{Voters}_{i,t}$
	(1)	(2)
$\mathbb{1}(\text{2001 General})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.005	-0.002
	[0.006]	[0.015]
$\mathbb{1}(\text{2001 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.007	
	[0.006]	
$\mathbb{1}(\text{2006 General})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	-0.003	
	[0.002]	
$\mathbb{1}(\text{2011 General})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.012***	-0.007
	[0.004]	[0.010]
$\mathbb{1}(\text{2011 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.014***	
	[0.004]	
$\mathbb{1}(\text{2016 General})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.024***	-0.021
	[0.005]	[0.016]
$\mathbb{1}(\text{2016 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Non-Poor})_i$	0.030***	
	[0.005]	
$\mathbb{1}(\text{2001 General})_t \times \mathbb{1}(c_{10}\text{=Extreme Poor})_i$	-0.001	0.000
	[0.005]	[0.017]
$\mathbb{1}(\text{2001 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Extreme Poor})_i$	-0.004	
	[0.005]	
$\mathbb{1}(\text{2006 General})_t \times \mathbb{1}(c_{10}\text{=Extreme Poor})_i$	0.002	
	[0.002]	
$\mathbb{1}(\text{2011 General})_t \times \mathbb{1}(c_{10}\text{=Extreme Poor})_i$	0.004	0.044***
	[0.005]	[0.010]
$\mathbb{1}(\text{2011 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Extreme Poor})_i$	-0.003	
	[0.005]	
$\mathbb{1}(\text{2016 General})_t \times \mathbb{1}(c_{10}\text{=Extreme Poor})_i$	-0.015**	0.061***
	[0.006]	[0.017]
$\mathbb{1}(\text{2016 Run-Off})_t \times \mathbb{1}(c_{10}\text{= Extreme Poor})_i$	-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 5 in the text, while column 2 corresponds to Figure 7. 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).

B Disaggregate Difference-in-Difference Analysis

Figure A1: 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 non-poor 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 (poor and poor extreme), which corresponds in both cases to districts classified as poor 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.

C Voter Turnout: Robustness Checks

Table A3: The Marginal Effect of the Fine on Voter Turnout (Province-election FE)

	Baseline	Heterogeneous effects		
	(1)	(2)	(3)	(4)
(A) Dependent Variable: Turnout$_{i,t}$				
Fine value$_{i,t}$	0.000459***	5.79e-05	0.000279***	0.000414***
	[9.50e-05]	[8.96e-05]	[0.000100]	[0.000147]
Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$		0.000704***		
		[4.87e-05]		
Fine value$_{i,t}$ × $\mathbb{1}(\text{Run-Off})_t$			0.000359***	
			[4.70e-05]	
Fine value$_{i,t}$ × $\mathbb{1}(\text{c}_{10}\text{=Extreme Poor})_i$				-3.17e-05
				[5.84e-05]
R-squared	0.939	0.941	0.940	0.939
(B) Dependent Variable: ln Turnout$_{i,t}$				
ln Fine value$_{i,t}$	0.0277***	0.00307	0.0159***	0.0300***
	[0.00568]	[0.00500]	[0.00588]	[0.0113]
ln Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$		0.0492***		
		[0.00421]		
ln Fine value$_{i,t}$ × $\mathbb{1}(\text{Run-Off})_t$			0.0236***	
			[0.00280]	
ln Fine value$_{i,t}$ × $\mathbb{1}(\text{c}_{10}\text{=Extreme Poor})_i$				-0.00195
				[0.00644]
R-squared	0.925	0.928	0.925	0.925
Observations	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692
District FE	Yes	Yes	Yes	Yes
Election x Province FE	Yes	Yes	Yes	Yes

Notes: Dependent variable is voter turnout (0-1) in panel A and the natural log of voter turnout in panel B. All regressions use data from national elections (General: Legislative and Presidential first round; 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 in panel A is measured in current Peruvian Soles (S/). In panel B, we use the natural log of the value of the fine. Column 2 includes the interaction of the fine with a dummy for the 2016 elections. Column 3 includes the interaction of the fine with a dummy for presidential run-Off elections. Column 4 includes the interaction of the fine with a dummy for districts classified as extreme poor in 2010. All regressions include district fixed effects and election by province 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 (Unweighted)

	Baseline	Heterogeneous effects		
	(1)	(2)	(3)	(4)
(A) Dependent Variable: Turnout$_{\mathbf{i,t}}$				
Fine value$_{i,t}$	0.000621***	0.000338**	0.000496***	0.000602***
	[0.000103]	[0.000107]	[0.000106]	[0.000128]
Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$		0.000498***		
		[6.16e-05]		
Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{Run-Off})_t$			0.000250***	
			[3.77e-05]	
Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{c}_{10}\text{=Extreme Poor})_i$				1.13e-05
				[5.89e-05]
R-squared	0.915	0.916	0.915	0.915
(B) Dependent Variable: ln Turnout$_{\mathbf{i,t}}$				
ln Fine value$_{i,t}$	0.0372***	0.0193**	0.0288***	0.0440***
	[0.00642]	[0.00632]	[0.00650]	[0.0102]
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$		0.0357***		
		[0.00430]		
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{Run-Off})_t$			0.0169***	
			[0.00276]	
ln Fine value$_{i,t}$ $\times$ $\mathbb{1}(\text{c}_{10}\text{=Extreme Poor})_i$				-0.00485
				[0.00654]
R-squared	0.894	0.895	0.895	0.894
Observations	13,536	13,536	13,536	13,536
Districts	1692	1692	1692	1692
District FE	Yes	Yes	Yes	Yes
Election x Province x 2006-Poverty-Category FE	Yes	Yes	Yes	Yes
Unweighted	Yes	Yes	Yes	Yes

Notes: Dependent variable is voter turnout (0-1) in panel A and the natural log of voter turnout in panel B. All regressions use data from national elections (General: Legislative and Presidential first round; 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 in panel A is measured in current Peruvian Soles (S/). In panel B, we use the natural log of the value of the fine. Column 2 includes the interaction of the fine with a dummy for the 2016 elections. Column 3 includes the interaction of the fine with a dummy for presidential run-Off elections. Column 4 includes the interaction of the fine with a dummy for districts classified as extreme poor in 2010. All regressions include district fixed effects and election by province by 2006 poverty category (non-poor, poor, extreme poor) fixed effects. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

D Registration: Economic Activity and Migration

Table A5: The Value of the Abstention Fine, Nighttime lights and Migration

Dependent variable:	Night-lights $\text{DN}_{i,t}$	ln $\text{Voters}_{i,t}$		Share born in $\text{district}_{i,t}$	ln $\text{Voters}_{i,t}$	
	(1)	(2)	(3)	(4)	(5)	(6)
Fine $\text{value}_{i,t}$	0.0105**	-0.000755**	-0.000989***	-0.000744	-0.00152***	-0.00160***
	[0.00525]	[0.000310]	[0.000366]	[0.000852]	[0.000467]	[0.000466]
Night-lights $\text{DN}_{i,t}$			0.0223***			
			[0.00600]			
Share born in $\text{district}_{i,t}$						-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	0.998	0.997	0.997	0.959	0.999	0.999
District FE	Yes	Yes	Yes	Yes	Yes	Yes
Year-Province-Category '06 FE	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent variable	3.179	7.842	7.842	0.613	8.583	8.583

Notes: Dependent variable in the header: 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 abstention fine is the same for all districts up to the 2006 elections. The value of the fine is measured in current Peruvian Soles (S/). All regressions include district fixed effects and year by province by 2006 poverty category (using 2006 classification) 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

E 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.

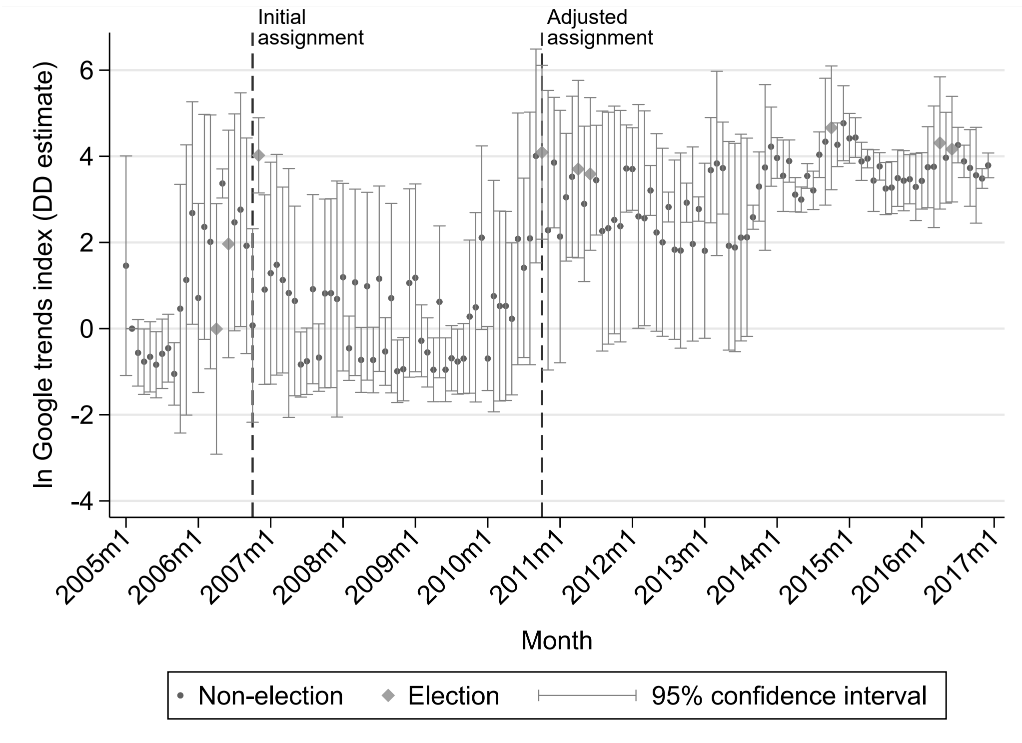
We limited the geographic scope to the country of Peru and collected monthly data from January 2005 to December 2016. We used used double quotation marks (" ") to avoid capturing Google searches for segments of multi-word search terms (e.g. "fine for not voting"). All queries were done in Spanish, in lower case and without any dyacritics. The full list of included search terms is presented in Table A6.

Table A6: 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 A2: 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).