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# ESSAYS ON POLITICAL ECONOMY IN DEVELOPING COUNTRIES

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To Victoria, Andrzej and my mom

# TABLE OF CONTENTS

LIST OF FIGURES . . . . . vii

LIST OF TABLES . . . . . viii

ACKNOWLEDGMENTS . . . . . x

ABSTRACT . . . . . xi

1 POLITICS NEVER END: PATRONAGE EFFECTS OF INCREASED TRANSPARENCY . . . . . 1

ABSTRACT . . . . . 2
1.1 Introduction . . . . . 2
1.2 Institutional Background . . . . . 9
1.2.1 Municipal Bureaucracy . . . . . 9
1.2.2 Public Education . . . . . 11
1.2.3 Randomized Anti-corruption Audits . . . . . 12
1.3 Empirical Strategy . . . . . 13
1.3.1 Data . . . . . 13
1.3.2 Research Design . . . . . 16
1.4 Results: Municipal Employment . . . . . 18
1.4.1 Municipal Bureaucracy . . . . . 18
1.4.2 Municipal Public School Employees . . . . . 21
1.4.3 Heterogeneity by Detected Levels of Corruption in Municipalities . . 23
1.5 Mechanisms: Patronage versus Hiring to Correct Irregularities . . . . . 27
1.5.1 Patronage Engagement . . . . . 27
1.5.2 Hiring to Correct Irregularities . . . . . 31
1.5.3 Alternative Channels . . . . . 34
1.6 Public Goods Production . . . . . 36
1.6.1 Educational Performance of Students . . . . . 37
1.6.2 Public Finances and Capital Investment . . . . . 38
1.7 Concluding Remarks . . . . . 40
1.8 Appendix . . . . . 42
1.8.1 Additional Background Information . . . . . 42
1.8.2 Are audits random? . . . . . 47
1.8.3 Municipal Public Employees: Additional Results . . . . . 48
1.8.4 Public Finances . . . . . 53
1.8.5 Public Education: Additional Results . . . . . 54
1.8.6 Reelection: Additional Results . . . . . 62
1.8.7 Empirical Appendix . . . . . 64

2 HOW EFFECTIVE ARE MONETARY INCENTIVES TO VOTE? EVIDENCE FROM A NATIONWIDE POLICY . . . . . 66

ABSTRACT . . . 67
2.1 Introduction . . . 68
2.2 Institutional Background . . . 73
2.3 Empirical Strategy . . . 77
2.3.1 Data . . . 77
2.3.2 Research Design . . . 78
2.4 Results: Voter Turnout . . . 80
2.4.1 Baseline Results . . . 81
2.4.2 Heterogeneous Effects . . . 83
2.4.3 Enforcement of the Fine . . . 86
2.4.4 Changes in Registration and the Propensity to Vote . . . 87
2.4.5 Previous Experimental Estimates, Adaptation and Voltage Drop . . . 93
2.5 Results: Electoral Outcomes and Political Attitudes . . . 95
2.5.1 Political Attitudes and Other Behaviors (Survey evidence) . . . 99
2.6 The Aggregate Effect of Compulsory Voting . . . 100
2.6.1 Assessing the Importance of the Abstention Fine . . . 106
2.7 Concluding Remarks . . . 107
2.8 Appendix . . . 110
2.8.1 Additional Background Information . . . 110
2.8.2 Voter Turnout: Additional Results . . . 112
2.8.3 Enforcement: Additional Results . . . 119
2.8.4 Voter Registration: Additional Results . . . 122
2.8.5 Web searches: Additional Results . . . 127
2.8.6 Political Outcomes: Additional Results . . . 131
2.8.7 Senior Exemption: Additional Results . . . 134

3 POLITICAL RESPONSES TO NATURAL RESOURCE SHOCKS . . . 137

ABSTRACT . . . 138
3.1 Introduction . . . 138
3.2 Institutional Background . . . 142
3.2.1 Elections . . . 142
3.2.2 Mining sector . . . 143
3.3 Empirical Strategy . . . 144
3.3.1 Data . . . 144
3.3.2 Research Design . . . 147
3.4 Results: Electoral Outcomes . . . 148
3.4.1 Baseline Results: Anti- and Pro-mining vote shares . . . 149
3.4.2 Other Electoral Outcomes and Political Attitudes . . . 152
3.5 Mechanisms: Environmental damage versus Socioeconomic improvement . . . 155
3.5.1 Environmental damage . . . 155
3.5.2 Socioeconomic outcomes . . . 157
3.5.3 Selective Migration . . . 159
3.6 Concluding remarks . . . 160
3.7 Appendix . . . 162

3.8 Construction of electoral dataset and main variables . . . . . 162

REFERENCES . . . . . 165

# LIST OF FIGURES


1.1 The Effect of the Audit on Hires of Municipal Public Employees . . . . . 19
1.2 The Effect of the Audit on Hires of Municipal School Employees . . . . . 22
1.3 The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees 24
1.4 The Heterogeneous Effect of Hires in Municipal Schools by Detected Irregularities 33
1.5 The Effect of the Audit on Additional Schooling Outcomes (1st-8th grade) . . . 37
1.6 Number of Municipalities Audited Per Year . . . . . 42
1.7 Share of Municipalities that were Audited . . . . . 43
1.8 Distribution of Times a Municipality has been Audited . . . . . 43
1.9 Distribution of Irregularities Associated with Corruption . . . . . 44
1.10 Timing between the Release of Audit Results and 2004 Mayoral Election . . . . 44
1.11 Years between the Release of Audit Results and Mayoral Elections, 2004-2016 . 45
1.12 The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees by Type of Contract . . . . . 48
1.13 Effects of the Audit on Federal Transfers and Municipal Expenditures . . . . . 53
1.14 The Heterogeneous Effect of Separations in Municipal Schools by Detected Irregularities . . . . . 54
1.15 The Effect of the Audit on Student Test Scores in Municipal Public Schools . . 55
1.16 The Effect of the Audit on Student Test Scores in Municipal Public Schools . . 56

2.1 The Abstention Fine by Election and Fine Category . . . . . 76
2.2 The Reform to the Abstention Fine and Voter Turnout . . . . . 81
2.3 The Reform to the Abstention Fine and Voter Registration . . . . . 88
2.4 The Reform to the Abstention Fine and Information Acquisition . . . . . 95
2.5 The Reform to the Abstention Fine and Blank or Invalid Votes . . . . . 98
2.6 Senior Exemption from Compulsory Voting and Voter Turnout . . . . . 104
2.7 Voter Turnout in National Elections . . . . . 110
2.8 Share of Fines Settled for the 2011 and 2016 Elections . . . . . 111
2.9 The Reform to the Abstention Fine and Turnout by 2006 Poverty Category . . . 116
2.10 The Reform to the Abstention Fine and Turnout: Heterogeneous Effects by Share of Poor . . . . . 117
2.11 Election-specific Estimates of the Marginal Effect of the Abstention Fine on Turnout and the Elasticity . . . . . 118
2.12 Disentangling the Behavioral Elasticity of Turnout from the Registration Effect 124
2.13 The Reform to the Abstention Fine and Information Acquisition (monthly level) 129
2.14 Share of Booths without Voters from each Age . . . . . 134
2.15 Distribution of Registered voters by age in Peru and Chile . . . . . 135
2.16 Voter Turnout by Age in Peru and Chile: Full Distribution . . . . . 136

3.1 Aggregate Anti- and Pro-mining vote shares in national elections . . . . . 145
3.2 Vote share for Anti- and Pro-mining parties in General Election 2021 . . . . . 146

# LIST OF TABLES


1.1 The Effect of the Audit on Hires of Municipal Public Employees . . . . . 20
1.2 The Heterogeneous Effect of the Audit on Hires of Municipal School Employees 26
1.3 The Effect of the Audit on Hires for First-term versus Second-term Mayors . . . 28
1.4 The Heterogeneous Effect of the Audit on Mayoral Reelection . . . . . 30
1.5 The effects of the Audit on Payroll and Capital Expenditures . . . . . 39
1.6 Summary Statistics . . . . . 46
1.7 Are Audits Random? . . . . . 47
1.8 Main Results: Robustness . . . . . 49
1.9 Main Results: Goodman-Bacon Decomposition . . . . . 49
1.10 The Effect of the Audit on Dismissals and Municipal Public Employees at end of year . . . . . 50
1.11 The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees: Robustness . . . . . 51
1.12 Alternative Channels for the Effect of Audit on Hires . . . . . 52
1.13 Effects of the Audit on New Municipal Public School Employees: Robustness . . 57
1.14 Effects of the Audit on Municipal Public School Employees . . . . . 58
1.15 The Effect of the Audit on Municipal Public School Principals . . . . . 59
1.16 Effects of the Audit on Separated Municipal Public School Employees . . . . . 60
1.17 Effects of the Audit on School Assets Quality . . . . . 61
1.18 The Effect of Multiple Audits on Mayoral Reelection . . . . . 62
1.19 The Effect of the Audit on Mayoral Reelection . . . . . 63

2.1 The Marginal Effect of the Abstention Fine on Voter Turnout . . . . . 83
2.2 Heterogeneous Effects of the Abstention Fine on Voter Turnout . . . . . 84
2.3 The Marginal Effect of the Abstention Fine on Voter Registration by Age . . . 90
2.4 The Marginal Effect of the Abstention Fine on Voter Registration . . . . . 92
2.5 The Marginal Effect of the Abstention Fine on Invalid and Blank Votes . . . . 97
2.6 The Marginal Effect of the Abstention Fine on Political Attitudes and Behaviors 100
2.7 Senior Exemption from Compulsory Voting and Voter Turnout . . . . . 105
2.8 Assignment of Districts to Poverty Categories in 2006 and 2010 . . . . . 110
2.9 Main Results Clustering by Region (Wild Cluster Bootstrap) . . . . . 112
2.10 Event-study estimates of the effect of the reform on turnout and registration . . 113
2.11 The Marginal Effect of the Abstention Fine on Voter Turnout in Run-Off elections 114
2.12 Heterogeneous effects by Age of the Electorate . . . . . 115
2.13 Effects of Expected Fine on Turnout . . . . . 119
2.14 The Marginal Effect of the Abstention Fine on Settlement of Outstanding Fines 120
2.15 Improved Enforcement and the Long-run Effect of the Fine on Turnout . . . . . 121
2.16 The Value of the Abstention Fine and Voter Registration: Heterogeneous Effects by Distance to Districts with Lower Fine . . . . . 122
2.17 The Value of the Abstention Fine and Age-specific Voter Registration, controlling for predicted voters . . . . . 123
2.18 The Value of the Abstention Fine, Nighttime lights and Migration . . . . . 124

2.19 The Value of the Abstention Fine and Age-specific Voter Registration, controlling for access to DNI . . . . . 125
2.20 The Value of the Fine and Voter Turnout controlling for Registration . . . . . 126
2.21 Search Terms included in Google Trends Analysis . . . . . 130
2.22 Vote Share for Winner and Runner-up in First Round . . . . . 133
2.23 The Marginal Effect of the Abstention Fine on Invalid and Blank Votes: Full sample . . . . . 133

3.1 The Effect of Mining Production on Anti- and Pro-mining vote shares . . . . . 150
3.2 The Heterogeneous Effect of Gold Production on Anti- and Pro-mining vote shares 151
3.3 The Effect of Mining Production on Turnout, Spoiled Votes and Political Attitudes 154
3.4 The Effect of Mining Production on Vegetation and Polution . . . . . 156
3.5 The Effect of Mining Production and Value on Socioeconomic Outcomes . . . . 158
3.6 Effect of Mining Production on Selective Migration . . . . . 160
3.7 The Effect of Mining Production on Voters and Votes . . . . . 162
3.8 Policies considered to classify political parties . . . . . 163
3.9 Details on the construction of variables . . . . . 164

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# ABSTRACT

This dissertation studies politicians' and voters' behavior. The first chapter studies whether politicians strategically respond when confronted with potential electoral backlash from increased transparency. I answer this question using exogenous variation generated by randomized audits to local governments in Brazil. I show that the execution of an audit leads to an increase in the number of public employees hired by the mayor. This effect is greater in municipalities where auditors uncovered higher levels of corruption. I find evidence consistent with mayors hiring more employees as a form of patronage to compensate for the loss of electoral support resulting from the audit. I closely examine the education sector using additional detailed data and find that hiring more school employees does not improve student outcomes, revealing limited direct consequences of audits on public goods production. Moreover, I show that an audit increases the share of payroll expenditures but decreases capital investment and that this substitution translated into a deterioration in the quality of school assets. These results suggest that patronage enables politicians to offset the potential electoral penalty of an audit by hiring employees who do not contribute much to public goods production.

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

In the third chapter, I examine changes in the Peru's two largest export products: copper and gold. Using difference-in-differences estimators, I show that higher levels of gold production increases (decreases) the vote share for anti-mining (pro-mining) parties. Conversely, an increase in copper production leads to a close to zero—and not statistically signif-

icant—change in the vote shares for anti- and pro-mining parties. Moreover, I find evidence consistent with voters caring about the environmental damage caused by gold production when casting their vote and prioritizing this effect over improvements in socioeconomic indicators. Overall, these findings highlight that voters heavily weigh social considerations compared to self-interest when deciding for whom to vote.

# CHAPTER 1

# POLITICS NEVER END: PATRONAGE EFFECTS OF INCREASED TRANSPARENCY

# ABSTRACT

Transparency that provides voters with negative information about politicians leads to electoral penalties. Do politicians strategically respond when confronted with this type of potential electoral backlash? I answer this question using exogenous variation generated by randomized audits to local governments in Brazil. I show that the execution of an audit leads to an increase in the number of public employees hired by the mayor. This effect is greater in municipalities where auditors uncovered higher levels of corruption. I find evidence consistent with mayors hiring more employees as a form of patronage to compensate for the loss of electoral support resulting from the audit. I closely examine the education sector using additional detailed data and find that hiring more school employees does not improve student outcomes, revealing limited direct consequences of audits on public goods production. Moreover, I show that an audit increases the share of payroll expenditures but decreases capital investment and that this substitution translated into a deterioration in the quality of school assets. These results suggest that patronage enables politicians to offset the potential electoral penalty of an audit by hiring employees who do not contribute much to public goods production.[1]


## 1.1 Introduction

Evidence suggests that transparency that reveals political malfeasance to the public negatively affects the incumbents' electoral performance [Ferraz and Finan, 2008, Bobonis et al., 2016]. However, transparency may be of little value if it does not improve politicians' behavior. On the one hand, these reforms might realign dysfunctional politicians' incentives with voters' interest, pushing politicians to take corrective actions (e.g., dismissing "bad


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apple" employees to signal that they are not corrupt), thereby increasing welfare [Shleifer and Vishny, 1993]. On the other hand, politicians might unanticipatedly use policy for their political benefit as a response to increased transparency [Lucas, 1976], as they have various means to achieve their goals. For instance, to compensate for the loss of electoral support, politicians might increase patronage—that is, distribute public jobs in exchange for votes [Weingrod, 1968].

In this paper, I ask: do politicians strategically respond to potential electoral backlash from political transparency? Studying politicians' responses to transparency has important policy implications, as creating better institutional incentives in one area might lead to a deterioration in another [Holmstrom and Milgrom, 1991]. For example, if politicians increase patronage without considering the quality of the new hires, there may end up being fewer qualified people in the bureaucracy. Perhaps one of the most influential papers on how transparency leads to better outcomes is the work by Ferraz and Finan [2008]. Using data from the first few waves of a new government audit program in Brazil, the authors found that disclosing corruption information to the public very close to the election enhanced electoral accountability by reducing the likelihood of reelection of corrupt mayors. In the current paper, I go beyond this by looking at public employment outcomes to examine how the politicians respond to the audit. In particular, I examine the same reform as Ferraz and Finan, but over a longer span and four full mayoral terms. Later waves occur at different points throughout the political cycle, during which time the incumbent can respond strategically before the next election. With this design, I study whether politicians change their political strategy by increasing the number of hires to achieve their reelection goal when faced with increased transparency. So far, there is little empirical evidence on whether politicians use policy for their political benefit as a strategic response to increased transparency.

I provide two main contributions. First, I show that, faced with an audit, which generates electoral backlash [Ferraz and Finan, 2008], the incumbent increases his patronage

engagement to compensate for the loss of electoral support. This increase in patronage can be interpreted as "forced action" pushed by the audit. This first contribution is consistent with theoretical predictions [Acemoglu et al., 2008] as I show that politicians invest in different patronage avenues when facing new policy constraints. Second, I provide evidence that even though the audit increases the number of public employees, they do not improve public goods production. My paper is the first study that examines the downstream consequences of anti-corruption audits on public education. The evidence I present is in line with arguments made by Stigler [1971], which predict that a policy reform that induces the policymaker to substitute means to achieve the same goal (i.e., reelection) would reduce efficiency.[2] In my paper, I provide evidence on such efficiency reduction by showing that after an audit, politicians hire more employees who do not contribute much to public goods production.

In 2003, the Brazilian federal government launched an anti-corruption program designed to detect and punish irregularities in municipal expenditures. The program selects municipalities at random and audits their expenditures of federally transferred funds from previous years. My primary analysis employs a staggered difference-in-differences design focusing on how an audit changes public employment under the jurisdiction of the mayor. I compare employment outcomes in a municipality that was randomly drawn to be audited to those in eligible municipalities that were either never audited or were randomly chosen to be audited in later years. I confirm this design's validity, showing that a range of local economic, demographic, and public labor characteristics cannot predict which municipalities get audited.

Based on these analyses, I provide three main findings. First, examining the contracts of employees working for the municipal government across all sectors, I show that an audit leads to a larger number of hires of public employees. The effect on hires is about 15%, and it is greater for employees with temporary work contracts, which is where the mayor has considerable discretion. Importantly, I verify that the number of hires outstrips dismissals,

2. Coate and Morris [1995] formalized a model in which a policy reform will induce the policymaker to choose a less efficient transfer mechanism.

resulting in a net rise in employees. The effects on hires are robust to recent advances in differences-in-differences method. Focusing on education and using identified data on the universe of school employees (consisting of teachers and teacher aides), I show that an audit increases the number of new school employees, mostly inexperienced ones. Municipal governments play a crucial role in this sector, given that they manage most primary schools and spend one-third of their budget on them. In addition, one-fourth of municipal employees work in this sector. Moreover, I find that the effect on hires in the municipal bureaucracy and on new school employees is greater among municipalities in which an audit uncovered higher corruption levels.

Second, I investigate two possible mechanisms that could explain why an audit leads to additional hires. One mechanism is that the corruption revealed by an audit decreases mayors' reelection chances, leading mayors to increase their patronage engagement to compensate for the loss of electoral support. Another alternative is that the information revealed by an audit improves the ability of mayors to take corrective actions (i.e., dismissing perceived "bad apples" working in the municipality), thereby allowing the mayor to hire new employees to replace the dismissed ones. I find evidence consistent with the first mechanism: Mayors increase the number of hires not as a form of corrective action, but rather as political leverage to shore up their reelection chances. Consistent with this interpretation, I show that the effect on hires is larger in the presence of electoral incentives, such as prospects for reelection, where there are greater gains from improving popularity. To do this, I compare the treatment effect from municipalities where the mayor was audited in its first term with those audited in its second term (who face a term limit). In addition, I examine whether the release of audit results differentially changes the reelection rates for a mayor who hired more or fewer employees during his term. I provide evidence showing that hiring additional employees offsets the negative effect of an audit on mayoral reelection rates.

To investigate the second mechanism, I exploit the audit data's richness from almost 800 audit reports and codify whether the schools located in an audited municipality were linked

to irregularities or not. This data allow me to test directly whether additional hires occur in the specific municipal public schools where auditors uncovered irregularities. I observe greater and statistically significant positive effects on hires among schools where auditors did not uncover irregularities located in corrupt municipalities. This evidence rules out the idea that mayors are increasing hires to take corrective actions.

I go on to show that alternative explanations unrelated to patronage are also unlikely to account for these findings. I start by considering explanations that would directly affect hires (e.g., political turnover, resignations after an audit). Then, I consider explanations related to changes in the public finances of the municipality (e.g., larger inflows of transfers following an audit). In total, I rule out six alternative explanations.

Third, I examine the effects of an audit on public goods outcomes, in particular, the quality of education. To do this, I first use data on students in municipal public schools in Brazil. I do not observe improvements in students' educational performance, as measured by grade repetition, dropout rates, and standardized test scores, suggesting that hiring additional school employees does not improve educational outcomes. Moreover, I show that an audit increases the share of payroll expenditures but decreases capital investment and that this substitution translated into a deterioration in the quality of school assets. This implies that the direct consequences of audits on public goods production are limited, thereby reinforcing the idea that patronage is the mechanism explaining the effect on hires. In particular, the lack of improvement in public goods rules out the possibility that better public goods, rather than the patronage itself, explain the gains in reelection rates for hiring more employees.

This paper relates and contributes to four main areas of literature. First, it adds to the vast body of work studying transparency and political accountability.[3] Previous research suggests that voters punish politicians in the polls after adverse electoral shocks, such as

3. For a recent overview on the literature of political accountability, see Ashworth [2012]. For a literature review on political scandals, see Invernizzi [2016].

political scandals [Hirano and Snyder Jr., 2012] and corruption disclosure [Ferraz and Finan, 2008, Bobonis et al., 2016, Buntaine et al., 2018, Dunning et al., 2019]. Using the same reform examined in this paper, Ferraz and Finan [2008] show that audits conducted close to elections improve accountability by reducing the election of corrupt politicians. However, my work shows that the picture is more complicated because some politicians can respond strategically and offset the potential electoral penalty, specifically by hiring employees who do not contribute much to public goods production.

The research on how politicians respond to negative electoral shocks is largely theoretical and focuses on two areas. On the one hand, it addresses politicians experimenting with risky policies as a gamble for resurrecting their electoral chances [Majumdar and Mukand, 2004, Izzo, 2020]. On the other hand, it addresses the choice of the politician communicating or not with the public when allegations initially surface [Basinger and Rottinghaus, 2012]. Empirically, Poblete-Cazenave [2021] examines an alternative margin of politician's responses, campaign spending.[4] Instead, I focus on a different margin of response: politicians distributing public jobs in exchange for votes to compensate for the loss of electoral support. In this regard, my results lend empirical support to models showing that, under certain conditions, politicians will turn to authoritarian practices or coercion when faced with threats to their political survival [Fergusson et al., 2018, Bueno de Mesquita and Smith, 2010].

Second, this study contributes to the literature on the personnel economics of the state. Previous research has focused mainly on financial incentives on the applicant pool, recruitment, and screening, and on incentives to improve performance [Finan et al., 2017]. This paper links this literature with those related to corruption and electoral accountability. My

4. In a recent paper, Lauletta et al. [2020] examines Brazilian audits from 2003–2004 to study the link between audits and quality of government. In contrast to my findings, they found a decrease in public employment. This difference arise from differences in the dataset on labor outcomes, in time frame examined (only two out of 13 years of audits), and in research design used (only state fixed effects). Poblete-Cazenave [2021] studies whether Brazilian audits that occurred before the last year of the mayoral term had any effect on public employment. In contrast to my findings, they found a not significant effect (large standard errors). This different results arise from differences in the dataset on labor outcomes, in time frame examined (2001-2008), and in research design used (compare outcomes only in audited municipalities before versus after the elections).

work highlights the role of public employee hires as the politician's strategic response when confronted with electoral backlash. It also suggests that patronage is a politically successful tool, as it effectively offsets the electoral penalty of disclosing corrupt acts from an audit on reelection rates. My results are consistent with other research showing that governments use jobs for patronage and redistribution purposes [Brollo et al., 2017, Colonnelli et al., 2020, Alesina et al., 2000, Robinson and Verdier, 2013].

Third, my findings fill a gap in the literature on the determinants of quality in public services. While past work implies that audits enhance welfare Avis et al. [2018], the evidence on the causal effect of anti-corruption audits on downstream consequences, and in particular quality of public goods, is limited and concentrated on public health [Zamboni and Litschig, 2018, Dizon-Ross et al., 2017] and roads [Olken, 2007].[5] I contribute to this literature by showing that audits do not improve student outcomes. My results also speak to the literature connecting public services and turnover. In particular, my findings contrast with those of Akhtari et al. [2020], who found that municipalities with a new party in office experience higher turnover in schools and lower student test scores. One reason for this difference is that the present study shows that an audit leads to an inflation of the number of school employees and not simply a "reshuffling" of employees across schools. Thus, even if there is a negative effect on test scores due to turnover, it might be compensated for by the additional employees.

Lastly, my paper adds to the literature on the design of policies to deter corruption. Some of these studies show that audits are effective in reducing future corruption [Avis et al., 2018, Olken, 2007], but are uninformative about what happens to the bureaucrats linked to corrupt acts. I add to this body of work by showing that mayors do not dismiss "bad apple" employees after the audit. Instead, they hire more employees in exchange for votes,

5. Ferraz et al. [2012] find a negative association between corruption detected in audits and educational outcomes. [Colonnelli and Prem, 2021] focus on private sector outcomes and find that anti-corruption audits increase the number of firms concentrated in sectors that heavily rely on public procurement, such as manufacturing and transportation. According to their classification, private education is one of the least government-dependent sectors.

undermining the audits' effectiveness. Related emerging literature shows that audits can backfire [e.g., Gerardino et al., 2020].[6] Chong et al. [2015] show that providing voters with corruption information disengages them from the political process. In my setting, I show that audits backfire as they lead to a larger number of employees with no corresponding improvements in public goods. I also find that anti-corruption audits backfire regarding electoral accountability: Hiring additional employees offsets the negative effect of disclosing corruption information from an audit on mayoral reelection rates. Overall, my results provide empirical evidence of a seesaw effect [Acemoglu et al., 2008], demonstrating how an effective reform in one dimension may lead to a deterioration in others.

## 1.2 Institutional Background

This section provides relevant details about municipal governments and municipal elections in Brazil. It describes the different types of public sector workers under municipality jurisdiction and discusses differences in their selection and exit processes. It also delineates the structure of public education. Finally, it describes the randomized anti-corruption audits of municipal expenditures.

### *1.2.1 Municipal Bureaucracy*

Public employees represented 16% of the workforce in Brazil in 2015 (more than 70 million employees), and more than 50% of public employees worked under municipality jurisdiction. A mayor (Prefeito) and a council of local legislators (Camara de Vereadores) govern municipalities. The mayor and legislators are simultaneously elected every four years. Mayors face a two-term limit.[7]

6. For more literature related to audits, see Wong [2021], Gonzalez-Lira and Mobarak [2019], Duflo et al. [2013], Lichand and Fernandes [2019].

7. Mayors can hold office for only two consecutive terms. However, they can return after a one-term hiatus or run for higher offices. In practice, it is very uncommon [Ferraz and Finan, 2011].

There are two main types of municipal public employees: tenure-track and temporary.[8] Among municipal public employees in 2015, 72% were employees with tenure-track, while 28% were temporary employees.[9] Selection and exit processes, as well as the type of work contract, vary widely among these types of workers. Selection of either type of municipal public employee depends exclusively on the corresponding municipality authority (e.g., director), with no state or federal government influence).

After three years of service, tenure-track employees get tenure, which provides job security. These employees are selected based on objective criteria (i.e., academic and professional credentials and a job-specific entrance exam). The selection process (*Concurso Publico*) starts with a public announcement of the required job, along with a job description, salary, and academic requirements. It ends with a public statement listing all candidates, selected candidates, and examination scores. Depending on the number of listed jobs, the process can last from a few months to a year.[10] Among the hires municipal public employees, only 34% were tenure-track employees in 2015. Dismissals can occur only after a judicial ruling and only for a limited number of reasons (e.g., misconduct).

Second, the municipal government has the discretion to select and hire temporary employees directly and without examination. Moreover, the exit process is fast and does not require judicial ruling nor oversight. Temporary jobs are usually exclusive to positions of managers, directors (e.g., school principals), or advisors. However, they can also be filled by health and education staff and other occupations when the municipality determines the job "meets a temporary need of exceptional public interest" (Article 37 of the Brazilian Con-

8. This does not include elected positions (e.g., mayor). It is also possible to hire employees through a private company (e.g., janitors). These employees are not in the current sample because they work for private companies and are neither employed nor dismissed by the municipal authorities.

9. The share of temporary employees among the final sample of municipalities is lower, about 19%.

10. For example, in October 2011, the municipality of *Camalaú* started a Concurso Publico to fill a position for a doctor (Edital PMC 001/2011). The process ended in December 2011. A much larger process occurred in the municipality of *Damião* in May 2019, when a Concurso Publico was put in place to fill 35 positions across 22 occupations (Edital 001/2019). By January 2020, most jobs were filled, although some of them required extra time).

stitution). Even though the need for the position should be temporary in theory, in reality, there are no limits on employment length. Among the hires of municipal public employees, 66% are temporary. Similarly, among municipal public employees who left their positions, 67% were temporary.

### *1.2.2 Public Education*

Municipalities are responsible for providing public services such as primary education, health care, and sanitation. In 2015 32% of municipalities' expenditures was allocated to education and 24% to health. The rest was distributed among other sectors.

Municipalities manage most primary schools (*ensino fundamental*, first to eighth grade), while state governments manage most secondary schools. By 2015, municipal governments managed 66% of primary schools, while state governments managed 68% of secondary schools.[11] Municipalities are responsible for the infrastructure and operations of the school: distributing school lunches, providing school transportation, hiring, paying salaries, and training of teachers. However, most funding for public education comes from the federal government [Akhtari et al., 2020] in the form of block grants based on student enrollment (fund called FUNDEF/FUNDEB).[12]

In 2015, 24% of municipal employees worked in the education sector. Schools' payrolls include a principal, teachers, and support staff (e.g., teacher aides). Most principals are temporary employees, directly appointed by mayors. Among municipal public schools in 2015, 82% of employees were teachers, while 18% were teacher aides. 74% of teachers were employees with tenure-track, while 26% were temporary employees. Support staff is temporary.

11. Private entities managed about 16% of primary schools.

12. At least 60% of FUNDEF revenues must be spent on teachers' salaries [Ferraz et al., 2012].

### *1.2.3 Randomized Anti-corruption Audits*

In 2003, the federal government created an autonomous anti-corruption office, the Office of Comptroller-General (CGU). In the same year it was formed, the CGU launched an anti-corruption program (*Programa de Fiscalização por Sorteios Públicos (FPSP)*) designed to detect and punish any irregularities in municipal expenditures. The program selects municipalities at random and audits their expenses from federally transferred funds during previous years.[13] The program consisted of 40 rounds of randomized lotteries over the 2003–2015 period, each of which was independent, meaning that a municipality could be drawn more than once (see Appendix Figure 1.8). In each round, about 60 municipalities were randomly selected. The number of rounds per year varied significantly over time. Appendix Figure 1.6 shows the number of municipalities audited at least once over time and illustrates the annual variation of the program.[14] During this period, 40% of municipalities were audited at least once (see Appendix Figure 1.7).

The CGU gathers information on all federal funds transferred to every selected municipality during the previous three to four years and issues a random selection of inspection orders (e.g., school construction). Each order stipulates an audit task for a specific project within a specific sector. These orders focus on financial management and documentation, the procurement process, wages, and quality of service funded by the transfer. After that process, about ten auditors are sent to the municipality for one or two weeks to examine and verify the information in those inspection orders. Auditors perform interviews, review documentation, visit hospitals or schools, among other activities.[15] The time between the

13. All municipalities with a population of up to 500,000 inhabitants are eligible for selection. The program randomly selects a fixed number of municipalities per state in each lottery. For smaller states, only one or two municipalities are selected in a single lottery, while for larger states, several municipalities are selected (up to seven in the recent lotteries).

14. The program has changed in other regards over time. From August 2005 (17th lottery), the CGU targeted a limited number of randomly selected sectors in municipalities with larger populations, while smaller municipalities were still subject to audits in all sectors.

15. Auditors are hired based on a competitive public examination and earn highly competitive salaries [Avis et al., 2018].

lottery announcement and the audit execution can be anywhere from one month to a year. On average, auditors visit the municipality one month after the announcement. After the completion of inspections, auditors submit a full report to the central CGU office in Brasília. A summary of each audited municipality's main findings is released to the media and published on the Internet. The federal police, prosecutors (MPF), and the federal courts of accounts (TCU) also receive this summary, resulting in legal consequences as warranted. On average, the summary is published seven months after the audit announcement.

## 1.3 Empirical Strategy

In this section, I present the data sources and research design. I leave the description of the complementary strategy for the study of the mechanism of hiring to correct irregularities for the following section 1.5.

### *1.3.1 Data*

This paper uses municipal employees, anti-corruption audits, education, and election data from different sources. I present summary statistics for all eligible municipalities and audited municipalities in Appendix Table 1.6.

**Municipal Bureaucracy.** The main source of data for employees' movements across the entire municipal bureaucracy is *Relação Anual de Informações Sociais* (RAIS), which is managed by the Ministry of Economy (*Ministério da Economia*). It is an annual survey that reports administrative information on the universe of labor contracts in both the public and formal private sectors. This survey is used for various social insurance programs in Brazil; there are consequences if misreporting occurs[Akhtari et al., 2020]. It includes the worker's demographic characteristics, such as education level, gender, age, and whether they left employment voluntarily or were dismissed. It also includes information on the type of contract (i.e., temporary or tenure-track), hiring date, and end date of employment. Finally,

in RAIS, each worker is assigned an occupational category specific to their current job (according to the Brazilian classification of occupations 2002 CBO). I use annual data from 2000 to 2015 to build a panel of employment outcomes by municipal government (employer). I use annual data from 2000 to 2015 to build a panel of employment outcomes by municipal government (employer). One limitation of this dataset is that it is anonymized, which impedes tracking the same employee overtime.

**Education.** The data on education come from several sources. First, I use the School Census (*Censo Escolar*) dataset from the National Institute for Research on Education *(*Instituto Nacional de Estudos e Pesquisas Educacionais Anísio Teixeira, INEP). It is an annual survey of the universe of public schools in Brazil. The census is conducted in May and is available from 2007 (the academic year begins in March and ends in December). This census includes characteristics of the school, enrollment, student characteristics, and a panel of school employees with individual and school identifiers. I use this panel to compute the movement of individual school employees. Specifically, I compute the number of new employees to a school by taking the pool of employees in a given school year and checking to see if those same employees were present at the same school the year prior. I compute the number of employees that left a school by taking the pool of employees in a given school year and checking to see if those same employees were present in the same school the next year. The data do not provide information regarding whether employees who left a school did so voluntarily or were dismissed or transferred. I build a panel of school-level movement of employees from 2008 to 2015. Second, I use *Prova Brasil*, a nationwide, standardized exam administered biannually in November since 2007 to all 4th and 8th graders in public schools. [16] I use *Prova Brasil* data from 2007 to 2015 to measure student achievement. To facilitate interpretation, I standardize math and Portuguese language test scores according to the individual level distribution of test scores for students in eligible but never-audited

16. *Prova Brasil* is administered only in schools with at least 20 students enrolled in that particular grade level.

municipalities.[17] Finally, I use school-level data on dropout rates from the National Institute for Educational Research and Study and Executive Secretary (INEP).

**Audits.** The data on irregularities and audits come from the CGU. The data from lotteries 2–40[18] include the audited municipalities and the number of inspection orders for each municipality and lottery. Although all audit reports are public, the CGU only began to code the information from the reports starting with the 20th lottery in March 2006. For the most recent 20 lotteries, I obtain a description of detected irregularities by inspection order; the total number of detected irregularities is 122,397.

Following the CGU split between minor and more severe irregularities and the language of Avis et al. [2018], some examples of detected corruption include the following: fraud in the selection of beneficiaries of welfare programs (above the limit on household income), over-invoicing of goods and services, abandonment of public works by the contractor (without application of contractual sanctions), and fraud in the procurement of goods and services (e.g., not advertising the request for the work).[19] Appendix Figure 1.9 shows the share of audited inspection orders with at least one detected irregularity categorized as corruption over time.

**Municipal Elections.** The electoral data come from the Tribunal Superior Eleitoral (TSE). This dataset provides public records on election results in municipal elections in 2000, 2004, 2008, 2012, and 2016. Elections are held every four years in October, with the mayor taking office in January of the following year.

On this point, while Ferraz and Finan [2008] examine the electoral outcomes of the 2004 mayoral election, I use data from audits that occur during four full mayoral terms to study whether the mayor responds strategically when provided with more time before the next

17. CGU can perform audits outside the scope of FPSP that are not considered in this work.

18. Lottery 1 was a pilot.

19. Some examples of detected mismanagement are inconsistencies on the registered information of beneficiaries of welfare programs (e.g., not located at registered addresses), lack of documentation (e.g., absence of records of incoming stock on hospitals' warehouses), and keeping expired inventory (e.g., expired medicine in hospitals).

election.

**Other Data.** Socioeconomic and demographic data at the municipality level correspond to the 2000 census by the Brazilian Institute of Geography and Statistics (IBGE). I also uses municipal-level public finance data related to sectorial expenses and federal transfers drawn from Institute for Applied Economic Research (IPEA).

### *1.3.2 Research Design*

I aim to estimate the causal effect of the random audits on municipal public employment. For this purpose, I examine exogenous variation resulting from anti-corruption audits that were conducted randomly across municipalities over time. The nature of this randomization lends itself naturally to a staggered difference-in-difference analysis with municipality and year fixed effects.

Even though previous work has strongly established the validity of audits' randomization [Ferraz and Finan, 2008, 2011, Zamboni and Litschig, 2018, Colonnelli and Prem, 2021], I also test this in the data. Appendix Table 1.7 shows the results of cross-sectional regressions where the dependent variable, an indicator of whether the municipality was audited between 2003 and 2015, regressed on state fixed effects and a set of local economic and demographic characteristics.[20] Based on observable characteristics, audited municipalities are similar to never-audited municipalities. The test for joint orthogonality shows that the coefficients are not statistically significant.

I estimate the following flexible event-study model:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_{y=-4}^{-2} \eta_y D_{i,t}^y + \sum_{y=0}^{5} \beta_y D_{i,t}^y + \epsilon_{i,t} \tag{1.1}$$

Here, $Y_{i,t}$ is the outcome variable in municipality $i$ at year $t$, $\alpha_i$ is a municipality fixed

20. The implied audit probability in any given round is constant within a state

effect, and $\lambda_t$ is a year fixed effect. $D^y_{i,t}$ is an indicator variable taking the value of 1 if it is year $y$ relative to the audit announcement year in municipality $i$. These indicator variables are always 0 for eligible but never-audited municipalities. I normalize $\eta_{-1} = 0$, so all other coefficients $\eta_y$ and $\beta_y$ represent differences in outcomes relative to the year prior to the audit announcement. Observations more than three years before or more than four years after the audit announcement are represented by dummy variables $D^{-4}_{i,t} = \mathbb{1}(tT^*_i \leq -4)$ and $D^5_{i,t} = \mathbb{1}(tT^*_i \geq 5)$, respectively. The set of coefficients $\beta_y$ represents the average difference in the outcomes between audited municipalities and non-audited municipalities relative to what that difference was before the audit announcement, conditional on the set of fixed effects.

I cluster the standard error term $\epsilon_{i,t}$ at the municipality level to allow for arbitrary correlation within municipalities.

I remove the few municipalities that were ineligible for the program. Then, I remove those municipalities without employment data for each year within the studied period. Finally, I exclude municipalities that were audited but for which I do not observe a full window of event-years within -3 to 4. That results in a set of treated (at different points in time) and never-treated municipalities. The final sample using municipality-level data includes a balanced panel of 4,057 eligible municipalities over the 2000–2015 period, consisting of 73% of all the eligible municipalities.

Appendix 1.8.7 provides more details on the research design when studying heterogeneous effects and summarizing results.

**School Employees.** In addition to the primary municipality-level analysis, I also conduct a school-level analysis by adapting the same empirical design to the more disaggregated dataset from the School Census. I use the following specification:

$$Y_{s,t} = \alpha_s + \lambda_t + \eta D^{-2}_{i,t} + \sum_{y=0}^{3} \beta_y D^y_{i,t} + \epsilon_{s,i,t} \tag{1.2}$$

$Y_{s,t}$ is the outcome variable (new and separated school employees). $\alpha_s$, and $\lambda_t$ are school and year fixed effects, respectively. The rest of the variables are analogous to those in equation (1.1). Observations more than two years before or more than three years after the audit are represented by dummy variables $D_{i,t}^{-2} = \mathbb{1}(tT_i^* \leq -2)$ and $D_{i,t}^{3} = \mathbb{1}(tT_i^* \geq 3)$, respectively. $\epsilon_{s,i,t}$ is an error term clustered at the municipality level.

The endpoints are different from the main specification since I only observe individual school employees' movements from 2008 to 2015. I remove the few municipalities that were ineligible for the program and schools without outcome data for each year within the studied period.[21]

Appendix 1.8.7 provides more details on the research design.

## 1.4 Results: Municipal Employment

In this section, I present evidence on how audits affect hires of the entire municipal bureaucracy. Next, using the more disaggregated data at the school level, I provide an estimated effect of audits on municipal public school teachers and principals. I then evaluate the heterogeneity of hires by detected levels of corruption in the municipality.

### *1.4.1 Municipal Bureaucracy*

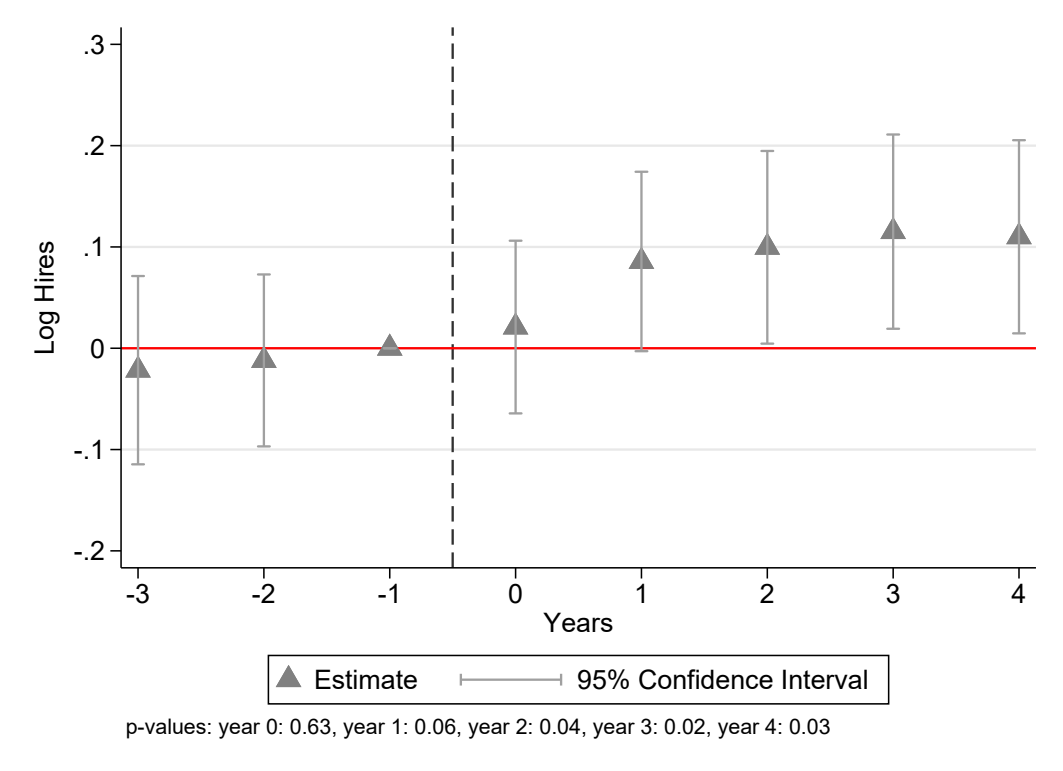
Figure 1.1 shows the causal effect of the audits on hires. The triangle markers show point estimates of log hires relative to the year before an audit along with 95% confidence intervals of $\beta_c$ in equation (1.1). The estimates for prior to the audit are small and statistically insignificant (p-value for a joint hypothesis test of 0.8992). The estimates for after the audit show that audits lead to a persistent increase in hires, starting from one year after the

21. The DD's specification is the following:

$$Y_{s,t} = \beta \text{PostAudited}_{i,t} + \alpha_s + \lambda_t + \epsilon_{s,i,t} \tag{1.3}$$

audit announcement. As previously mentioned, on average, auditors visit municipalities one month after the announcement and a summary of findings is published about six months after. Therefore, it is not surprising that the effect on hires manifests later. The audits led to an increase in hires of 9% for years 1–2 and of 11% for years 3–4. The estimates are precise and statistically significant.

**Figure 1.1:** The Effect of the Audit on Hires of Municipal Public Employees




Notes: Graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the audit. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. The unit of observation in this figure is a municipality-year. Regression includes municipality and year fixed effects. Regression includes 64,912 observations from 4,057 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 1.3). Standard errors clustered by municipality.

Table 1.1 presents estimates of equation (1.6) using log hires as the dependent variable. The results show that audits lead to more hires for all types of contracts and positions, with estimates (column 1) showing an increase in hires of 15.5%. As discussed earlier, temporary employees face shorter and more discretionary selection processes than do tenure-track employees. Therefore, it is reasonable to assume that the estimates for hires would be

greater for temporary employees. To test this assumption, I change the dependent variable to hires of workers with temporary and tenure-track contracts (columns 2 and 3, respectively). The effect of an audit on temporary hires (15.8%) is twice as large as that for tenure-track (8.8%). To compare the effects across managerial positions (e.g., directors) and non-managerial positions (e.g., teachers), I change the dependent variable to hires by occupation (columns 4 and 5, respectively). The effect on hires for non-managerial positions (15.5%) was higher than that for managerial ones (10%). All coefficients are statistically significant at the 5% level.

**Table 1.1:** The Effect of the Audit on Hires of Municipal Public Employees

| Dependent Variable: | Log Hires | | | | |
|---|---|---|---|---|---|
| | Baseline | Type of work status | | Type of Position | |
| | | Temporary | Tenure-track | Managers | Non-managers |
| | (1) | (2) | (3) | (4) | (5) |
| PostAudited | 0.155*** | 0.158*** | 0.088** | 0.100*** | 0.155*** |
| | [0.037] | [0.053] | [0.044] | [0.035] | [0.040] |
| Observations | 64,912 | 64,912 | 64,912 | 64,912 | 64,912 |
| Municipalities | 4057 | 4057 | 4057 | 4057 | 4057 |
| Municipality FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| Mean of dep. var | 4.632 | 3.176 | 3.351 | 1.904 | 4.439 |

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-5 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. Post second-time Audited takes the value of 1 after the municipality was audited for a second time. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Appendix Table 1.8 displays the results of several robustness tests on the effect of audits on hires. The point estimates are similar and still significant when I use Callaway and Sant'Anna [2020a] estimator (column 1), observations with a positive number of hires (column 2), and first-time hires (column 3). Estimates are also large and significant when I

change the dependent variable to the natural logarithm of hires (column 4) or the number of hires in levels (column 5).[22]

Finally, the results in Appendix Table 1.10 show that the number of hires outstrips the number of dismissals, resulting in a net rise in employees. Column 1 shows that audits also led to an increase in dismissals. It is important to note that dismissals correspond to employees who the municipality government fires. The point estimate for dismissals (9.4% increase) is smaller than that for hires (15.5%). Column 2 shows a point estimate of 3.7% for the number of employees at the end of the year. These data show that the audits inflated the bureaucracy's overall size.

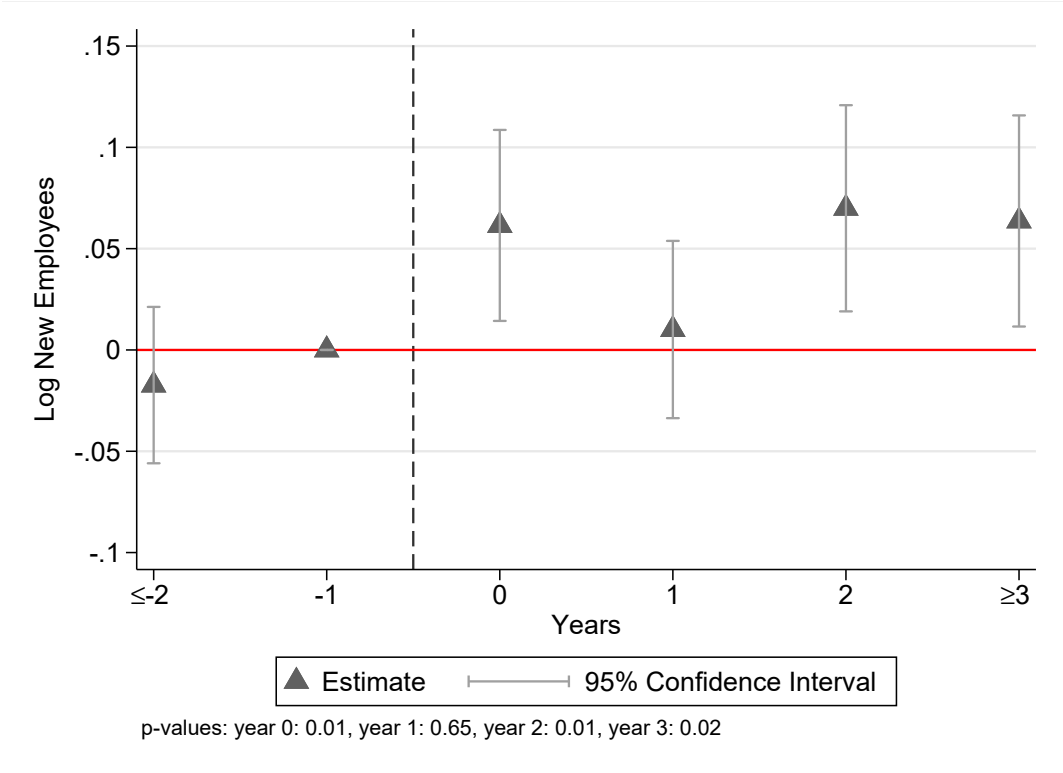
### *1.4.2 Municipal Public School Employees*

This subsection presents results showing how audits affect hires of school employees. For these analyses, I use granular data on the universe of school employees in Brazil. This data provides two advantages in comparison with the previous one. First, the data include individual and school identifiers, which allows me to conduct a more precise calculation on the number of new employees and a richer analysis at the school-level. Second, this data can be connected with student outcomes, such as test scores. Education is a substantial sector for municipal government, receiving 32% of expenditures and housing 24% of municipal employees in 2015. It also represents a challenge in the fight against corruption, with 29% of detected corruption being linked to education.

**School Employees.** Figure 1.2 shows the effect of audits on new school employees, consisting of teachers and teacher aides. The markers show the new school employees relative to the year before the audit (omitted year). The data show a significant and persistent increase (annual average of 4.7%) in the number of new employees following the audit.

22. Appendix Table 1.9 shows the DD estimates [Goodman-Bacon, 2021]. It is worth noting that less than 10% of the identifying variation is a result of the treatment timing. Rather, the majority of variation comes from comparisons to municipalities with no audits during the sample period. The comparison of later-treated municipalities in hires is negative, on average, and accounts for a bias in the overall DD estimate (however, the weight is minimal).

**Figure 1.2:** The Effect of the Audit on Hires of Municipal School Employees




Notes: Graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of new employees. The unit of observation in this figure is a school-year. Regression includes school and year fixed effects. Regression includes 467,849 observations from 3,531 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

Appendix Table 1.13 presents estimates of equation (1.3) and several robustness tests on the effect of audits on new school employees. The estimate in column 1 shows that an audit leads to a 6.5% increase in the number new school employees. The point estimates are similar and still significant when I use the Callaway and Sant'Anna [2020a] estimator (column 2), excluded observations with zero new employees (column 3), and excluded schools without student test scores results (column 4). Estimates are also significant and positive when I change the dependent variable to the natural logarithm of the number of new employees (column 5) or the number of new employees in levels (column 6). As a placebo test, column 7 shows that the audits have an effect close to zero on new employees in state and federal schools that are not audited by CGU. Changes in employee payroll seem more plausible in

public schools managed by municipalities directly affected by the audits.

The results in Appendix Table 1.14 show that the number of new employees outstrips the number of separated employees, resulting in a net rise in school employees. Column 1 shows a point estimate for dismissals of 2.2% increase, which is smaller than that for hires (6.5%). Column 2 shows a point estimate of 3.1% for the number of employees per school. Lastly, using *RAIS* data from 2000-2015, I show that the effect of the audits on the aggregated number of new school employees at the municipal level is 9% (column 3). This result shows that the observed effect of a net rise in school employees cannot be explained by transfers or "reshuffling" of employees across schools but instead corresponds to inflation of the education bureaucracy's overall size.

**School Principals.** Appendix Table 1.15 shows estimates of the effect of audits on school principals. Audits have a small effect on the share of new principals (column 1), but a negative and significant effect of -4.1 percentage points (pp) in the share of tenure-track principals (column 2). This result suggests greater direct contracting for principals after the audit.

### *1.4.3 Heterogeneity by Detected Levels of Corruption in Municipalities*

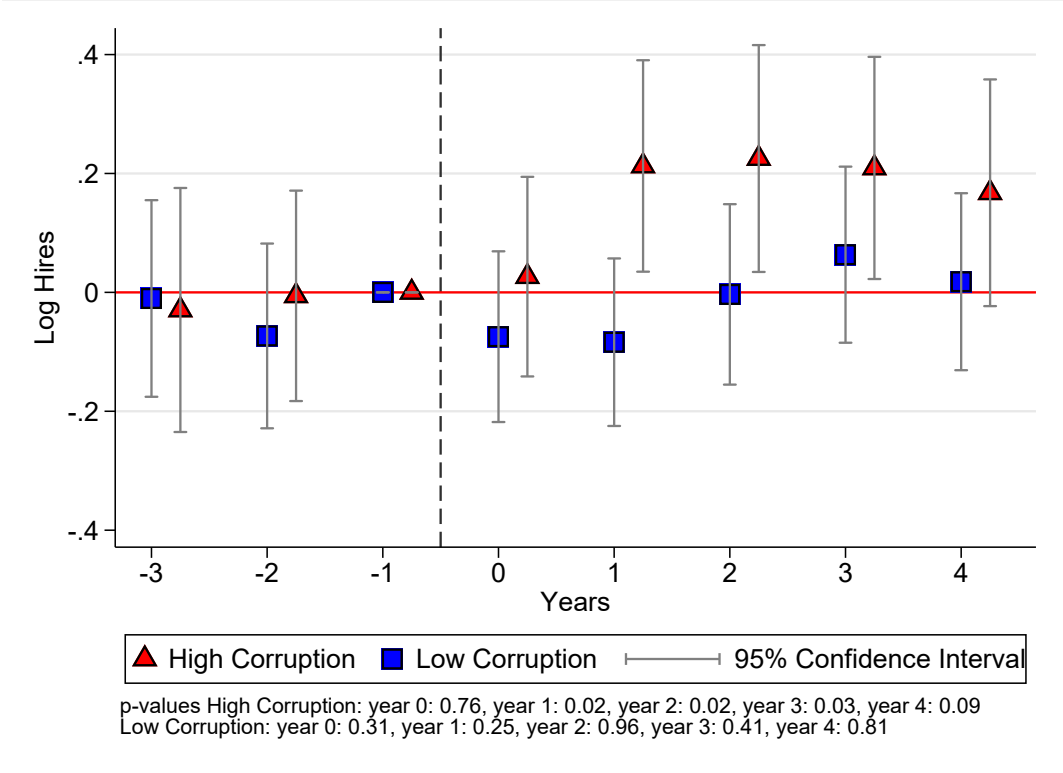
In this subsection, I present results of the evaluation of the heterogeneity of the effect on hires by detected levels of corruption in the municipality. I estimate equation (1.5). The group segmentation is by the level of detected corruption. High corruption municipalities correspond to those whose share of audited inspection orders with irregularities labeled as corruption is higher than the median. [23]

Figure 1.3 shows point estimates for log hires relative to the year before an audit along with the 95% confidence intervals of $\tilde{\beta}_y^k$ in equation (1.5). The estimates for before the audit were small and statistically insignificant for both groups, but there was a persistent

23. The sample in this exercise excludes municipalities that were audited for the first time during lotteries 1–19, a period for which the data on corruption were unavailable.

increase in hires for the high corruption group after the audit: 22% for years 1–2 and 19% for years 3–4. These estimates are precise and statistically significant. Estimates are close to zero for low corruption municipalities for all time periods, indicating a null response to the audit. Importantly, estimates for the high and low corruption municipalities are statistically different from each other. Overall, these results indicate that municipalities with higher detected corruption levels are more responsive to the audit.

**Figure 1.3:** The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees




Notes: Graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit interacted with respective dummies for high and low corruption municipalities. The omitted time category is the year before the audit for each group of municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Transformation of dependent variable is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on new labor contracts added. The unit of observation in this figure is a municipality-year. Regression includes municipality and year fixed effects. Regression includes 56,192 observations from 3,512 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 1.3). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Standard errors clustered by municipality.

Appendix Table 1.11 shows the results of a series of robustness analyses on the previous results. Column 1 shows that the audits led to a significantly larger effect on hires in high

corruption municipalities than low corruption ones. The effects are similar in magnitude when I split the sample to compare only high corruption and never-audited municipalities (column 2) and only low corruption and never-audited municipalities (column 3). Estimates are also large and significant when I sequentially change the definition of high corruption as municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the percentiles 60, 70, and 80 (columns 4-6).

Appendix Figure 1.12 shows heterogeneous estimates for hires for temporary and tenure-track employees. The markers in Panel A show log hires of temporary employees relative to the year before the audit. There was an increase in hires in high corruption municipalities after the audit, while there was almost no change in low corruption municipalities. Panel B shows log hires of tenure-track employees relative to the year before an audit. The estimates are small and statistically insignificant for both groups. Similar to the results presented earlier, these findings show that audits lead to a larger effect in hires for temporary employees, for whom the mayor has more discretion to appoint. This effect is driven by the most corrupt municipalities.

Finally, I examine the analogous heterogeneity for new school employees. Table 1.2 shows the heterogeneous effect of the audits on new employees in municipal schools. The estimate in column 1 indicates that an audit leads to a significant increase (10%) in new school employees in high corruption municipalities; the effect is close to zero in low corruption municipalities. To test whether this effect holds for just those individuals who are novices in the sector, I changed the dependent variable in column 2 to new employees who are novices in the sector (i.e., those who had never previously worked in a school). This resulted in a significant increase (15.5%) in high corruption municipalities, while no significant effect in low corruption ones. Instead, the dependent variable in column 3 corresponds to the new employees with previous experience working in a school. This resulted in a small and not significant change in neither low nor high corruption municipalities. Finally, I changed the dependent variable in column 4 to new employees holding multiple jobs in public schools.

The effect is a significant increase (3.5%) in high corruption municipalities, suggesting that the audit also increases the number of new employees who handle a heavy workload.

**Table 1.2:** The Heterogeneous Effect of the Audit on Hires of Municipal School Employees

| Dependent Variable: | New employees | | | |
|---|---|---|---|---|
| | All | Novice in education | Experience in education | Multiple school jobs |
| | (1) | (2) | (3) | (4) |
| PostAudited × Low Corruption | -0.006 | -0.007 | -0.016 | -0.014 |
| | [0.021] | [0.026] | [0.017] | [0.015] |
| PostAudited × High Corruption | 0.102*** | 0.158*** | 0.016 | 0.035** |
| | [0.025] | [0.024] | [0.018] | [0.014] |
| Observations | 447,264 | 447,264 | 447,264 | 447,264 |
| Municipalities | 3415 | 3415 | 3415 | 3415 |
| School FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| p-value $H_0$: a-b=0 | 0.001 | 0.000 | 0.198 | 0.017 |
| Mean of dep. var | 1.463 | 0.796 | 1.057 | 0.444 |

Notes: Dependent variable in the header. Transformation of dependent variable in columns 1-4 was done by estimating the inverse hyperbolic sine transformation ($log(y_i + (y_i^2 + 1)^{0.5})$). PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Regressions in columns 1-4 use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a window of [-2,3] around the audit year. The unit of observation in this table is a school-year. Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Overall, these results indicate that municipalities with higher detected corruption levels drive the overall effect on new school employees. Moreover, the audit increases the number of inexperienced and with heavy workload new employees.

## 1.5 Mechanisms: Patronage versus Hiring to Correct Irregularities

This section investigates mechanisms by which audits lead to additional hires of public employees. I consider two main competing mechanisms that could explain the previously reported results. One mechanism is that the corruption revealed by audits decreases mayors' chances of reelection; consequently, mayors increase their patronage engagement to compensate for the loss of electoral support. Another possibility is that the information revealed by audits improves the ability of mayors to take corrective actions and dismiss of perceived "bad apples,", thereby allowing them to hire new employees to replace the dismissed ones. I provide evidence supporting the first mechanism and ruling out the second. Then, I provide other evidence that further rules out other alternative explanations.

### *1.5.1 Patronage Engagement*

A first potential mechanism that could explain why audits lead to additional hires is that mayors increase their patronage engagement to compensate for the loss of electoral support due to what audits reveal. On this point, Ferraz and Finan [2008] examine the same reform and find that disclosing corruption information to the public decreased reelection rates. The authors use data from the first thirteen waves of lotteries that occurred in 2003 and 2004. They compare the electoral outcomes of municipalities for which audit results were published just a few months before versus after the 2004 mayoral election (see Appendix Figure 1.10). I study the audits during a longer-time scale and four full mayoral terms. Later waves occur throughout the entire political cycle, which gives the incumbent more time, even years during his first term, to respond strategically before the next election (see Appendix Figure 1.11). In particular, I examine the 40 waves of lotteries conducted between 2003 and 2015 and evaluate electoral outcomes over four mayoral terms (2004-2016).

This mechanism requires the presence of electoral incentives, such as prospects for reelec-

tion, which are outlined by political agency models and rent-seeking [Persson and Tabellini, 2000]. Considering this, I test if the effect of audit on hires is heterogeneous by whether a mayor may be up for reelection and thus have electoral incentives. Testing this empirically is complicated, as political outcomes may be affected by the audits. To overcome this, I define the variables "first term" and "second term" as a fixed characteristic in a municipality, where "first term" takes the value of one in municipalities where the mayor was audited in its first term. Analogously, "second term" takes the value of one in municipalities where the mayor was audited in its second term.

**Table 1.3:** The Effect of the Audit on Hires for First-term versus Second-term Mayors

| Dependent Variable: | Log Hires | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| [$a$] PostAudited × First term | 0.165*** | 0.217** | 0.213** | 0.199** |
| | [0.051] | [0.089] | [0.089] | [0.090] |
| [$b$] PostAudited × Second term | 0.126* | 0.034 | 0.012 | 0.005 |
| | [0.071] | [0.072] | [0.073] | [0.076] |
| Observations | 55,215 | 29,454 | 28,846 | 26,688 |
| Municipalities | 3681 | 3311 | 3311 | 2872 |
| Municipality FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| Sample | All | | | |
| Ony Two term mayors | | Yes | Yes | Yes |
| Excludes mayor following audited one | | | Yes | Yes |
| Excludes mayors who run for other office | | | | Yes |
| p-value a-b=0 | 0.651 | 0.103 | 0.074 | 0.092 |
| Mean of dep. var | 4.721 | 4.589 | 4.577 | 4.613 |

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. First term takes the value of 1 for municipalities in which the audit occurs during the mayor's first term. Second term takes the value of 1 for municipalities in which the audit occurs during the mayor's second term. Column 2 restricts the sample to electoral periods in which the mayor is observed during his first and second term. Column 3 additionally excludes mayors following the audited mayor. Column 4 additionally excludes mayors who run for mayor in other municipalities or for mayor after a one-term hiatus. All regressions use data from 2001 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Sample uses data from 2001 to 2015 and and include municipalities with complete data on elections. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Table 1.3 shows that the effect of audit on hires is heterogeneous by whether a mayor was audited in its first or second term. A first-term mayor may be up for reelection and thus have electoral incentives, while a second-term mayor faces a term limit. Column 1 shows that the effect of the audit on hires in municipalities where the mayor was audited in its first term is 30% larger than that where he was audited in its second term. Then, to assess the robustness of this result, I sequentially restrict the sample (columns 2-4). In column 2, I restrict the sample to include only mayors who remain in office during two terms, as first-term mayors who survived a second term are more comparable than first-term mayors who do not. The effect on hires for first-term mayors remains positive and significant, while the effect for second-term mayors becomes small and not statistically significant. In column 3, I restrict further the sample and exclude those mayors following the audited one. In column 4, I exclude mayors who run for office after a one-term hiatus as their electoral incentives in the second term differ from those who do not run again. Overall, results are very similar, showing that mayors who are up for reelection and who have more to gain (i.e., reelection) from shoring up their popularity drive the effect of audit on hires.

**Table 1.4:** The Heterogeneous Effect of the Audit on Mayoral Reelection

| Dependent Variable: | D(reelected mayor) | | | |
|---|---|---|---|---|
| | Mean: 0.278 | | | |
| | High Corruption municipalities | | Low Corruption municipalities | |
| | (1) | (2) | (3) | (4) |
| Audited in Current Term | -0.339** | -0.218* | -0.075 | 0.048 |
| | [0.140] | [0.117] | [0.138] | [0.100] |
| Audited in Current Term × Log Hires | 0.054** | 0.036** | 0.007 | -0.007 |
| | [0.022] | [0.018] | [0.021] | [0.016] |
| Log Hires | 0.043*** | 0.012** | 0.046*** | 0.011** |
| | [0.005] | [0.005] | [0.005] | [0.005] |
| Eligible for reelection | | 0.520*** | | 0.521*** |
| | | [0.006] | | [0.005] |
| Observations | 13,898 | 13,898 | 14,636 | 14,636 |
| Municipalities | 3491 | 3491 | 3659 | 3659 |
| Municipality FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. Log Hires is the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the total new labor contracts added during years 1-4 in the term. All regressions use election data from 2004, 2008 and 2012 and 2016. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19), municipalities that have been audited more than once, and municipalities without full employment data. The sample excludes municipalities that are not included in the balanced panel used to generate Figure 1.1. Sample in columns 1-2 includes never-audited and High Corruption audited municipalities. Sample in columns 3-4 includes never-audited and Low Corruption audited municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Variables High Corruption and Low Corruption always take the value of 0 for municipalities that have never been audited. The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

Then, I examine whether the release of audit results differentially changes the reelection rates for a mayor who hired more or fewer employees during his term. I estimate (1.7), presenting the results in Table 1.4.[24] The dependent variable is the likelihood of a mayor getting reelected. I first examine the heterogeneous effect of the audit release across the most corrupt municipalities (columns 1 and 2). In column 1, the baseline estimate shows a significant drop of 34 percentage points in the likelihood of being reelected. In contrast, reelection rates decrease less in municipalities that were audited and hired more employees. In column 2, I also control for whether a mayor was eligible for reelection. Results are similar: the estimate of the interaction between the audits and hires is positive and significant. I then examine the heterogeneous effect for municipalities where auditors uncovered less corruption (columns 3 and 4), finding that the audits do not have a significant effect on reelection rates. Moreover, there was no differential effect by whether a mayor hired more people. The interaction estimate is statistically indistinguishable from zero. The coefficients in columns 1–4 show that hiring more employees is positively related to reelection rates. Overall, these results indicate that hiring additional employees offsets the negative effect of audits on mayoral reelection rates. This effect is significant for the most corrupt mayors, for whom the audit represents a more significant political burden.[25]

### *1.5.2 Hiring to Correct Irregularities*

Bureaucracies are typically unable to monitor their employees perfectly. The information revealed by audits may improve the ability of mayors to take corrective actions against perceived "bad apples" who work in their municipality (i.e., dismissing or transferring them)

24. Appendix Table 1.18 shows that the release of the audits has a significant effect on the likelihood of mayoral reelection in municipalities that have been audited more than once. To avoid dealing with this margin of endogeneity, I restrict the sample of audited municipalities to those audited only once.

25. Appendix Table 1.19 shows that, in net, and considering the mayor's strategic response, being audited in current term does not lead to a decrease in reelection rates.

[Finan et al., 2017].[26] The mayor would then need to hire new employees to replace the dismissed ones, which would explain the observed increase in hires.

**Detected Irregularities in Schools.** To investigate this mechanism, I processed nearly 800 audit reports to examine the audit data's richness, which allowed me to test directly whether additional hires occur in municipal public schools where auditors uncovered irregularities. After the audit, a mayor who hires to correct irregularities would be expected to make employment changes in schools where auditors uncovered irregularities. To test whether the observed effects on new employees come from those schools, I estimate the following equation:

$$\begin{aligned} Y_{s,t} = \alpha_s + \lambda_t + \eta_{-2} D_{i,t}^{-2} \times \text{Detected Irregularities}_{s,t} + \\ \sum_{y=0}^{3} \beta_y D_{i,t}^{y} \times \text{Detected Irregularities}_{s,t} + \eta_{-2} D_{i,t}^{-2} \times \text{Not Detected Irregularities}_{s,t} + \\ \sum_{y=0}^{3} \beta_y D_{i,t}^{y} \times \text{Not Detected Irregularities}_{s,t} + \epsilon_{s,t} \end{aligned} \tag{1.4}$$

$\alpha_s$, and $\lambda_t$ are school and year fixed effects, respectively. $D_{i,t}^{y}$ is an indicator variable taking the value of 1 if it is year $y$ relative to the audit year in municipality $i$. Detected Irregularities$_{s,t}$ is a dummy variable that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities$_{s,t}$ is a dummy variable indicating whether auditors did not uncover irregularities: either because auditors did not find any or because they did not investigate this school.

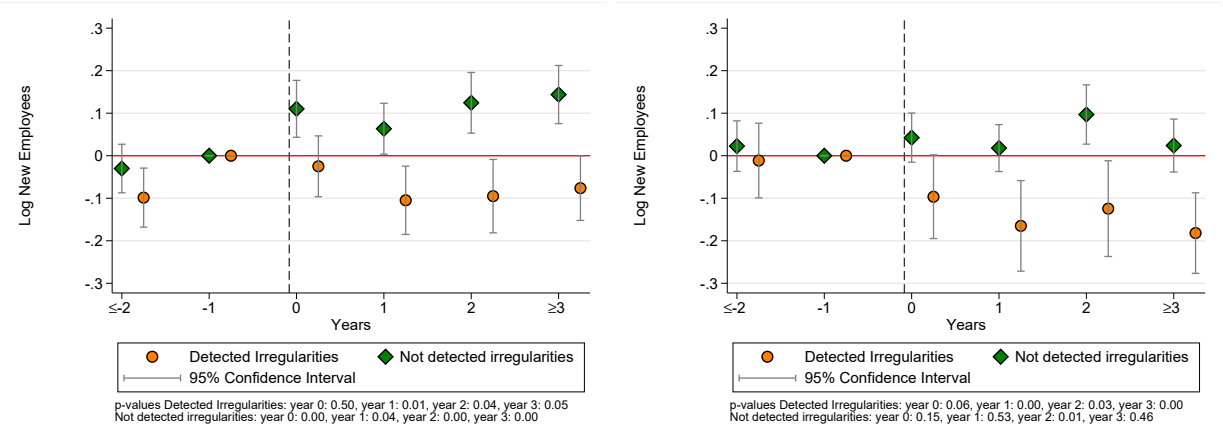
Figure 1.4 shows the differential effects of audits on log new employees in schools where auditors uncovered irregularities compared to the other schools conditional on a set of school and year fixed effects. Given the previous results showing high heterogeneity of the effects of audits based in levels of corruption in municipalities, I estimate equation 1.4 separately

26. This mechanism would be consistent with research showing that audits generate incentives to reduce corruption through their disciplining effect [Avis et al., 2018].

for high and low corruption municipalities. Panel A shows estimates in high corruption municipalities. Estimates in schools where auditors detected irregularities were minor (close to zero) and imprecise (the estimate for two or more years before the audit is statistically significant). On the other hand, estimates after the audit in schools where auditors did not uncover irregularities are positive and significant.

Panel B shows estimates in low corruption municipalities. Estimates after the audit in schools where auditors detected irregularities are negative and precise. These results suggest that hires remained frozen after audits. One plausible explanation is that mayors might want to investigate detected irregularities before hiring new employees. On the other hand, after the audit, estimates in schools where auditors did not uncover irregularities are small and insignificant.

**Figure 1.4:** The Heterogeneous Effect of Hires in Municipal Schools by Detected Irregularities




(a) High Corruption Municipalities

(b) Low Corruption Municipalities

Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation$(log(y_i + (y_i^2 + 1)^{0.5}))$ on the number of new employees. Detected Irregularities is a dummy that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities is a dummy indicating whether auditors did not uncover irregularities either because auditors did not find any or because they did not investigate this school. The unit of observation in this figure is a school-year. Regressions include school and municipality-year fixed effects. Regressions include 165,107 observations from 2,547 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

Overall, the evidence does not support the idea that mayors are increasing employment as a means of taking corrective actions. If it were driven by corrective actions, the effect on hires would be larger in schools where auditors uncovered irregularities, which is not what was observed.

### *1.5.3 Alternative Channels*

In this subsection I consider several alternative channels could explain the effects of audits on hires. Then, I rule them out.

First, I consider alternative channels that could explain the effects on hires by directly leading to changes in employment in general. Appendix Table 1.12 shows the results of the examination of these channels.

**Political turnover.** Using data from the first few waves, Ferraz and Finan [2008] show that Brazilian audits reduce the probability of reelection of the incumbent mayor. In the same context, Akhtari et al. [2020] show that a new party in the mayor's office leads to upheaval in employee turnover, and the effect is concentrated mainly in the first year after the election. Therefore, the results on hires may be driven by a new mayor who comes to power due to the audits and who hires new employees.

Considering this, I first re-estimate the baseline results, excluding from the sample those municipalities that were audited in the third or fourth year of the mayoral term. By doing this, I compare only those municipalities audited early in their mayoral terms with non-audited municipalities. By excluding the municipalities audited late in the mayoral term, I avoid the possibility that my estimates are driven by a short period during a term, right before a new election happens. Instead, in this exercise, my estimate on hires incorporates the effects of three to four years in the audited mayor's term before a new election occurs. Column 1 of Appendix Table 1.12 shows that the estimate for hires using this sub-sample drops relative to the baseline estimates, but it is still a significant increase of 10.4%.

Then, I re-estimate the baseline results, excluding the years corresponding to the mayor's

first year in office, in which most of the new hires due to political turnover occur. Reassuringly, column 2 shows that the estimate for hires is very similar to the baseline result in column 1 of Table 1.1. Overall, while political turnover is a relevant factor, the results in this section show that changes in mayoral turnover do not account for a large share of the effect on hires.

**Resignations.** Malfeasance revealed by the audits might lead bureaucrats to resign. Plausibly, the effects would be larger in those municipalities with higher levels of detected corruption. A larger number of resignations would require subsequent employee turnover, which could at least partially explain the main results. Column 3 of Appendix Table 1.12 provides evidence that audits do not lead to changes in the number of resignations of employees, which rules out the possibility that the findings are driven by decisions taken by the employee.

**Inadequate employees' educational qualifications.** An alternative explanation is that audits reveal that employees' education qualifications are inadequate, resulting in the mayor dismissing them to replace them with more qualified individuals. Columns 4 and 5 of Appendix Table 1.12 show the effect of audits on the share of hires and dismissals of individuals with a college degree. The effects are not statistically significant and are close to zero, suggesting that audits do not change the education level of new or dismissed employees, which rules out employee qualification as a contributing factor on hires.

**Inability to dismiss employees.** Another explanation is that, because mayors might be unable to fire employees due to institutional constraints, they might take other actions (i.e., hiring new employees) to reduce corruption. As mentioned previously, mayors have considerable discretion to fire temporary employees. To test this possibility, I looked at schools, where 55% of employees are temporary. Column 1 of Appendix Table 1.16 shows that audits lead to an increase in separations of about 4% in the most corrupt municipalities. Moreover, as column 2 shows, most of the effect of audits on separated employees is concentrated in schools where auditors did not detect irregularities. Together, these findings

indicate that mayors are able to fire presumably corrupt employees (column 1), but do not seem to base their dismissals on corruption (column 4), which rules out the possibility that they hire employees to reduce corruption.

Finally, increases in hires could be related to the public finances of the municipality. I examined two such factors.

**Changes in funds from the Central Government.** An alternative explanation for an increased number of hires after an audit is that the federal government reallocates funds to audited municipalities; after all, in Brazil, municipalities receive large transfers from the federal government, receiving 51.46 billion dollars from the federal government in 2015[27], which represented 47% of their total expenditures. In line with Colonnelli and Prem [2021], I find no evidence that municipalities experienced changes in the inflow of federal transfers after the audits (Panel A in Appendix Figure 1.13), which rules out the possibility that the increase in hires owes to an increase in federal funds.

**Changes in the allocation of expenses across sectors.** A final alternative explanation is that audits generate a change in the budget allocation across different sectors. A mayor might allocate more or fewer funds to particular sectors in response to the irregularities detected through the audits. Panels B-D in Appendix Figure 1.13 show the effects of the audits on municipal expenditures in education, health, and social welfare. The effects after the audit are close to zero and not statistically significant, ruling out the possibility that hires are driven by changes on the allocation of expenses across sectors.

## 1.6 Public Goods Production

This section examines the effect of audits on public goods—that is, the quality of various goods and services offered by municipal governments and employees—and culminates in an answer to the question of where the budget for hiring additional employees is coming from.

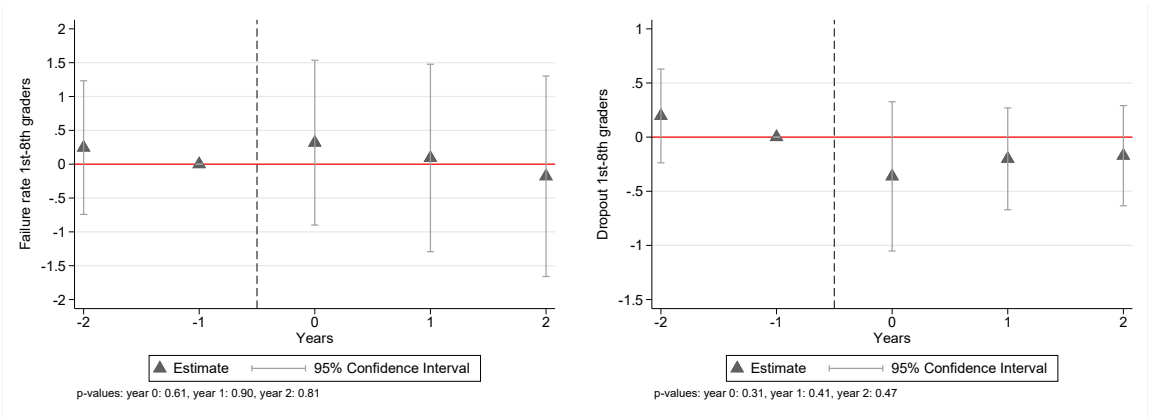
27. The nominal exchange rate in 2015 was LCU 3.33.

### *1.6.1 Educational Performance of Students*

Audits may produce positive consequences for public goods production due to their effects on hires. To examine this, I use school level data to examine the effects on dropout and failure rates. Then, I use data on students' universe in municipal public schools in Brazil to show how audits affect students' math and language scores.

Figure 1.5 shows event study estimates of the effect of an audit on student performance of 1st–8th graders in municipal schools. Panel A shows event study estimates for grade-level failure rates in municipal schools. Estimates are small and imprecise. The average failure rate in the sample is 10.7%. Panel B of Figure shows event study estimates on dropout rates of 1st–8th graders. The average dropout rate in my sample is 3.1%. Estimates are small (close to zero) and estimates are not significant at conventional levels.

**Figure 1.5:** The Effect of the Audit on Additional Schooling Outcomes (1st-8th grade)




(a) Failure Rates (b) Dropouts

Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Regressions include school and year fixed effects. Regressions use data from 2007 to 2015 and includes 203,523 observations from 3,712 municipalities. Sample covers a balanced panel of municipalities with a the window of [-3,3] around the audit year. Standard errors clustered by municipality.

Finally, Appendix Figure 1.15 shows event study estimates of the effect of an audit on student test scores in math and Portuguese language in municipal public schools. I present scores separately for 4th and 8th graders because, while all municipalities offer elementary schools, not all offer middle schools. Estimates of the effect of audits on test scores are close

to zero and statistically insignificant.[28]

Overall, these results show that, while audits lead to additional hires, they do not lead to better public services. That is, audits did not improve student performance. As I showed before, patronage enables politicians to offset the potential electoral penalty of audits by hiring employees who do not contribute much to public goods production.

These findings serve to strengthen the argument that patronage is the main factor driving increases in hires. This also suggests that the gains in reelection rates for hiring more employees (Table 1.4) are not explained by better public goods, but by the patronage itself.

### *1.6.2 Public Finances and Capital Investment*

As shown in Panel A of Figure 1.13, municipalities did not receive larger inflows of transfers following an audit. This raises the question of where the budget for hiring additional employees is coming from. Column 1 of Table 1.5 shows that audits increase payroll expenditures (as the share of total expenditures) by 2 pp for the most corrupt municipalities and by 1 pp for the less corrupt ones. In contrast, audits decrease the share of capital investment expenditure by 1 pp for the most corrupt municipalities (column 2). The effects are significant at conventional levels.

Consistently, this substitution from capital to payroll expenditures translated into a deterioration in the quality of school assets. Appendix Table 1.17 shows that the quality of computers and Internet for teacher and student use decreased following an audit. The estimate in column 1 implies that an audit led to a decrease of 4 pp in the share of principals who state that the quality of the computers available for students is high or good in the most corrupt municipalities. The estimate in column 2 shows that an audit led to a decrease of 5.5 pp in the share of principals who state that the quality of Internet for student use in the most corrupt municipalities. Similarly, columns 3 and 4 show that principals who state

28. Appendix Figure 1.16 shows the event study estimates for an alternative set of treated municipalities (audited in odd years), showing similar results.

**Table 1.5:** The effects of the Audit on Payroll and Capital Expenditures

| Dependent Variable: | Share of Expenditures | |
|---|---|---|
| | Payroll | Capital |
| | (1) | (2) |
| PostAudited × Low Corruption | 0.010** | -0.004 |
| | [0.005] | [0.004] |
| PostAudited × High Corruption | 0.020*** | -0.010** |
| | [0.005] | [0.004] |
| Observations | 41,675 | 41,675 |
| Municipalities | 2780 | 2780 |
| Municipality FE | Yes | Yes |
| Year FE | Yes | Yes |
| p-value a-b=0 | 0.098 | 0.262 |
| Mean of dep. var | 0.611 | 0.166 |

Notes: Dependent variable in the header is expressed as the share of total municipal expenditures. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. All regressions use data from 2000 to 2015, except for 2004, for which I do not have reliable data on expenditures on payroll or capital. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Sample excludes municipalities with missing data. The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

that the quality of computers and Internet for teacher use are lower in the most corrupt municipalities.

Overall, these results are consistent with patronage being a politically successful tool. Exchanging votes for jobs directly benefits voters and their families, while the political benefits of providing public goods may be more diffused and materialize in a longer-term horizon.

## 1.7 Concluding Remarks

This paper studies the strategic response of mayors to the electoral backlash of government audits. It provides three main findings. First, audits increase the number of public employee hires and that effect is greater among municipalities in which audits uncovered higher corruption levels. Second, when considering the mechanisms that drive these results, the evidence is consistent with mayors hiring additional employees as a form of patronage to compensate for the electoral support lost due to audits. Finally, even though audits lead to additional hires, they do not positively affect public goods and services provision.

The effect of disclosing corruption on electoral accountability has been analyzed in field experiments and studies in which the information was revealed only a few days [Buntaine et al., 2018] or a few months [Ferraz and Finan, 2008] prior to the election. However, the timing of the information being released to voters is relevant, as an extended gap between the audit and the election provides the incumbent with sufficient time to respond strategically. Gratton et al. [2018] show that politicians' strategic behavior drives the timing in which political scandals are released to the public. I consider a different margin of how time matters. In particular, I exploit the gap between the time when damaging information about politicians is released and the next election. Depending on this gap, the incumbent has more or less time to react.

My findings show, given sufficient time, that politicians are able to respond strategically to electoral backlash from government audits. Through patronage, politicians offset the potential electoral penalty of the audit by hiring employees who do not contribute to public goods production. As a result, policymakers must be cautious about the unintended consequences of audits. The effectiveness of audits depends on the reactions (which are sometimes unanticipated) of those politicians to the new policy, which is, in principle, the "Lucas critique" [Lucas, 1976].

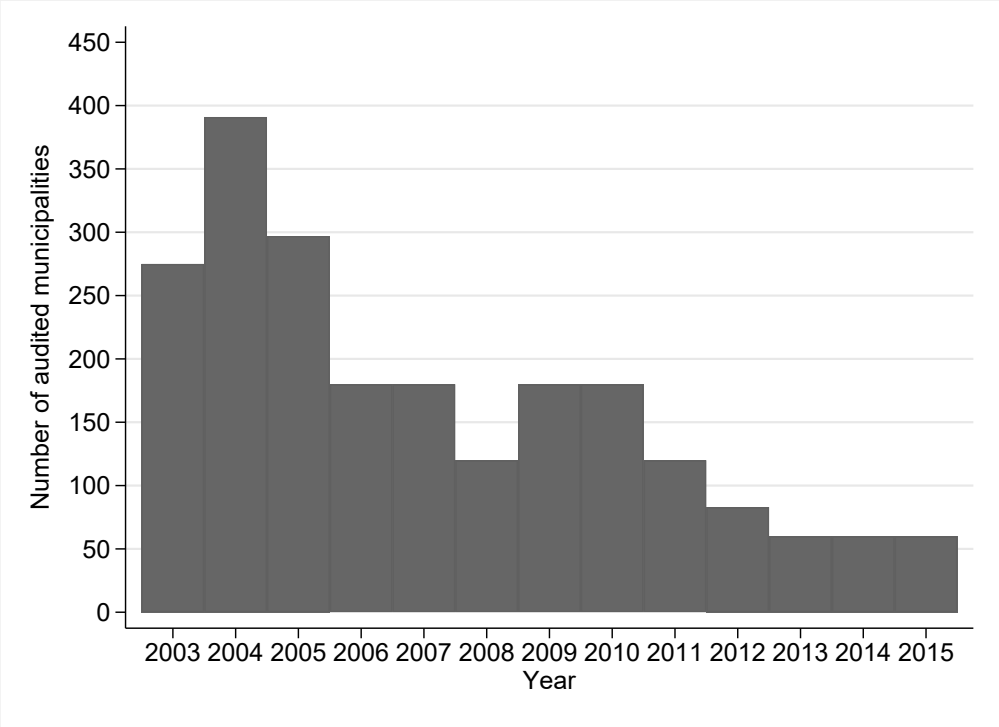
Moreover, the findings show that anti-corruption audits do not have real effects on student outcomes, which is surprising, given that Avis et al. [2018] find that audits are effective

in reducing corruption. More work is needed to understand why reducing corruption levels does not translate into a real improvement in public services. The results on capital expenditures show that the audits lead to a substitution from investment to payroll spending. Future research should focus on studying whether there are long-term effects due to reducing investment spending.

## 1.8 Appendix

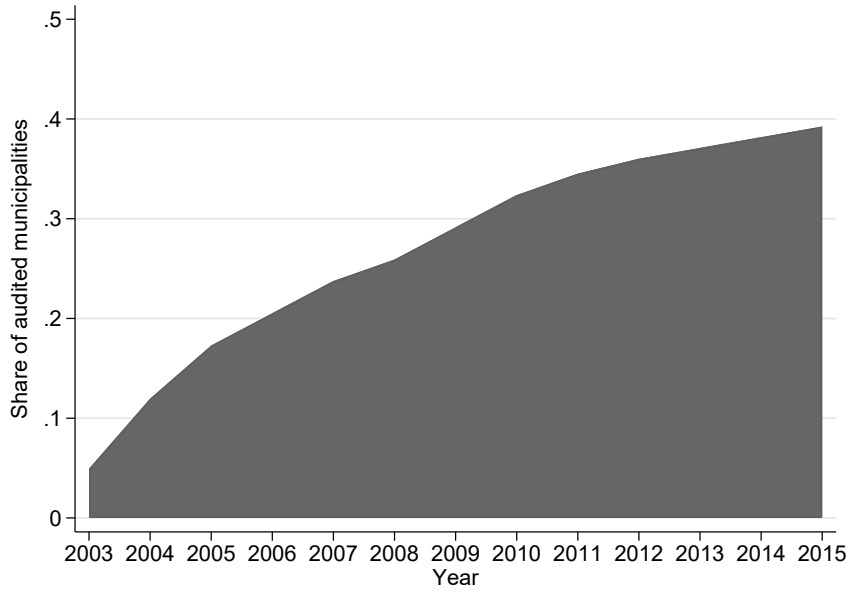
### *1.8.1 Additional Background Information*

**Figure 1.6:** Number of Municipalities Audited Per Year



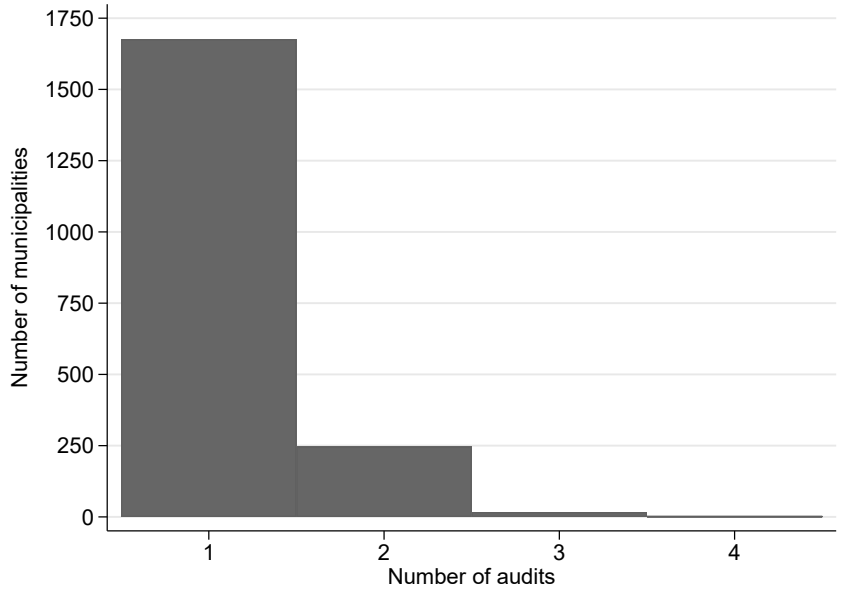

Notes: This figure plots the number of municipalities that were audited for the full duration of the program (from 2003 to 2015).

**Figure 1.7:** Share of Municipalities that were Audited



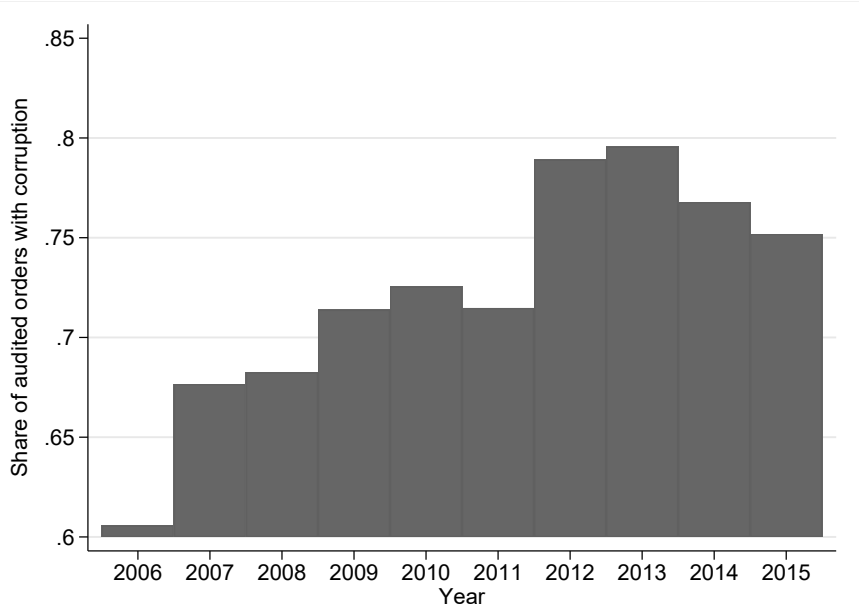

Notes: This figure plots the number of municipalities that were audited at least once for the full duration of the program (from 2003 to 2015), as the share of the total number of eligible municipalities.

**Figure 1.8:** Distribution of Times a Municipality has been Audited



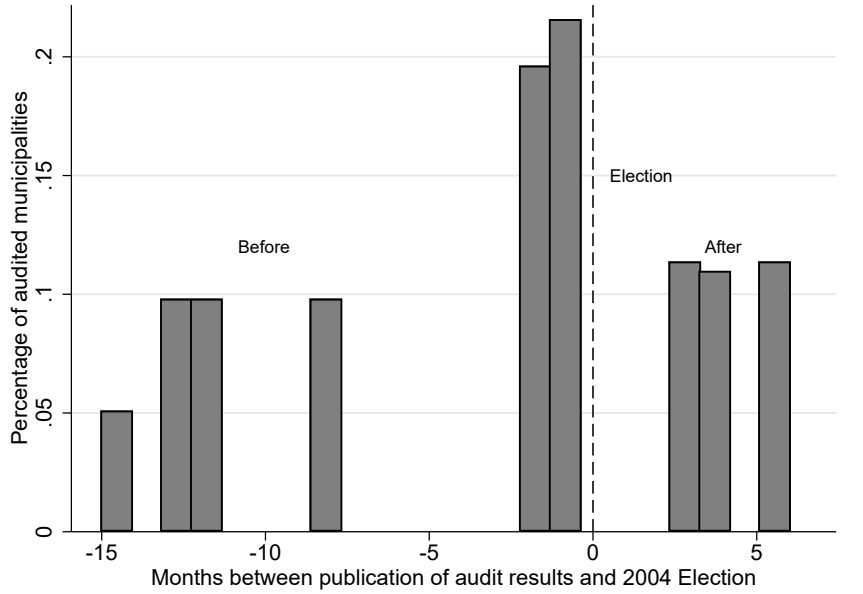

Notes: This figure plots the distribution of the number of times a municipality has been audited for the full duration of the program (from 2003 to 2015).

**Figure 1.9:** Distribution of Irregularities Associated with Corruption



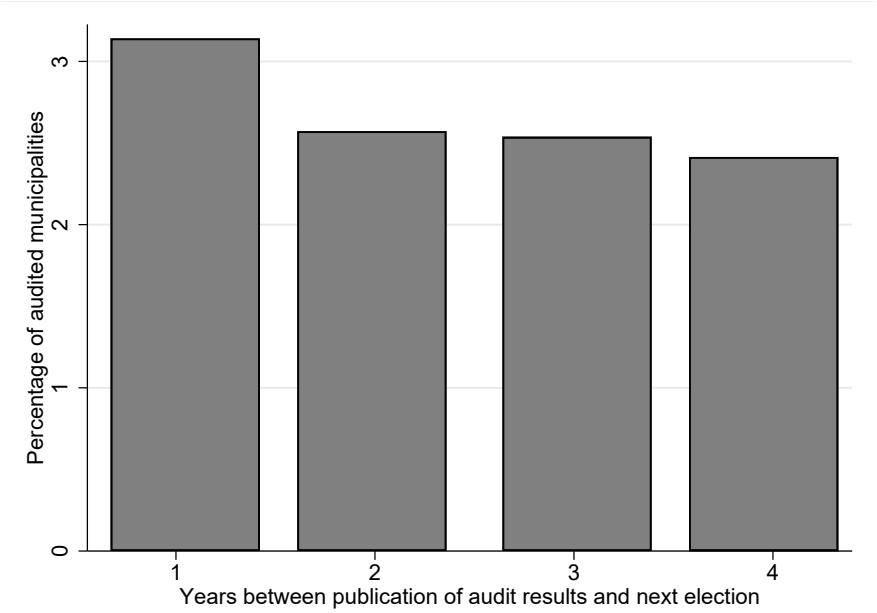

Notes: This figure displays the distribution of irregularities per service order associated with corruption.

**Figure 1.10:** Timing between the Release of Audit Results and 2004 Mayoral Election




Notes: Figure shows the share of audited municipalities by the number of months between the release of the audit reports in the first thirteen lotteries and the 2004 Mayoral Election. These lotteries correspond to the ones used by Ferraz and Finan [2008].

**Figure 1.11:** Years between the Release of Audit Results and Mayoral Elections, 2004-2016




Notes: Figure shows the share of audited municipalities by the number of years between the release of the audit reports and the following Mayoral Election.

**Table 1.6:** Summary Statistics

| | N | Mean | Std. Dev. | Min. | Max. |
|---|---|---|---|---|---|
| Panel A. Eligible municipalities | | | | | |
| | | A1. Public employees in municipalities | | | |
| Employees at end of year | 64,912 | 671.0 | 990.6 | 1.0 | 15,500.0 |
| Hires | 64,912 | 164.0 | 327.5 | 0.0 | 9,743.0 |
| Dismissals | 64,912 | 31.9 | 120.1 | 0.0 | 6,073.0 |
| Temporary employees at end of year | 64,912 | 123.0 | 333.4 | 0.0 | 10,686.0 |
| | | A2. Other Characteristics | | | |
| Illiteracy rate 2000 | 4,056 | 0.207 | 0.119 | 0 | 1 |
| Gini 2000 | 4,056 | 0.553 | 0.067 | 0 | 1 |
| Income per capita 2000 | 4,056 | 5.670 | 0.570 | 4 | 8 |
| Population 2000 | 4,056 | 9.358 | 1.063 | 7 | 13 |
| Urban rate 2000 | 4,056 | 0.604 | 0.226 | 0 | 1 |
| | | A3. Reelection | | | |
| Reelected mayor | 60,447 | 0.320 | 0.467 | 0 | 1 |
| Panel B. Audited municipalities | | | | | |
| | | B1. Public employees in municipalities | | | |
| Employees at end of year | 17,616 | 722.6 | 1,035.8 | 1.0 | 14,703.0 |
| Hires | 17,616 | 177.2 | 367.6 | 0.0 | 9,743.0 |
| Dismissals | 17,616 | 31.2 | 103.3 | 0.0 | 2,728.0 |
| Temporary employees at end of year | 17,616 | 132.9 | 336.8 | 0.0 | 10,686.0 |
| | | B2. Other Characteristics | | | |
| Illiteracy rate 2000 | 1,101 | 0.218 | 0.120 | 0 | 1 |
| Gini 2000 | 1,101 | 0.561 | 0.066 | 0 | 1 |
| Income per capita 2000 | 1,101 | 5.622 | 0.576 | 4 | 7 |
| Population 2000 | 1,101 | 9.462 | 1.057 | 7 | 13 |
| Urban rate 2000 | 1,101 | 0.601 | 0.225 | 0 | 1 |
| | | B3. Reelection | | | |
| Reelected mayor | 16,364 | 0.321 | 0.467 | 0 | 1 |

Notes: This table reports summary statistics at the municipality level, using RAIS, electoral and Census data for the period 2000–2015. The sample in Panel A includes all eligible municipalities, including those that are audited. The sample in Panel B includes only municipalities audited as part of the CGU anti-corruption program. Variables summarizing information on public employees in municipality correspond to number of labor contracts. Income per capita in log local currency per capita. Population in natural logarithm.

### *1.8.2 Are audits random?*

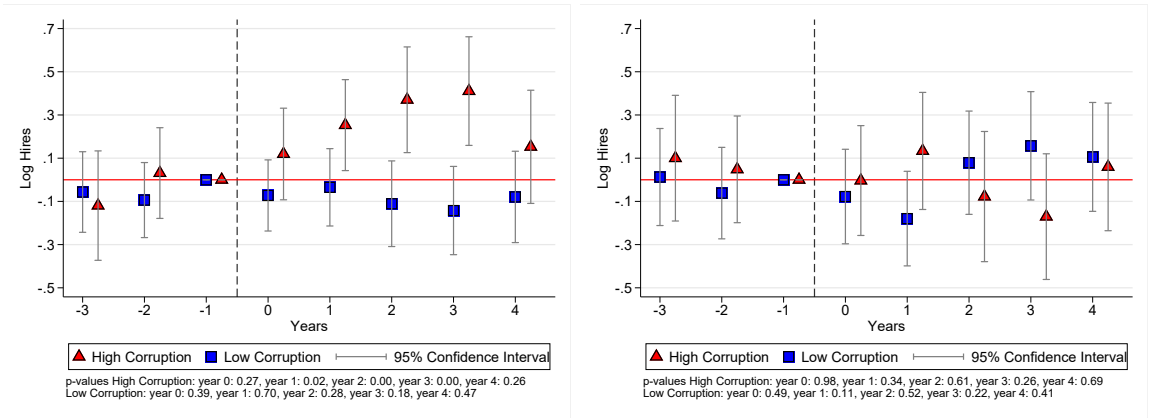
**Table 1.7:** Are Audits Random?

| Dependent Variable: | Audited |
|---|---|
| | (1) |
| Hires 2002 | 0.000 |
| | [0.000] |
| Dismissals 2002 | -0.000 |
| | [0.000] |
| Employees at end of year 2002 | -0.000 |
| | [0.000] |
| Temporary Employees at end of year 2002 | 0.000 |
| | [0.000] |
| Share of Illiterate population 2000 | 0.000 |
| | [0.001] |
| Income Gini 2000 | 0.119 |
| | [0.112] |
| Income per capita 2000 | -0.009 |
| | [0.030] |
| Population 2000 | 0.017 |
| | [0.011] |
| Share Urban 2000 | 0.064 |
| | [0.041] |
| GDP 2002 | -0.008 |
| | [0.017] |
| Observations | 5,271 |
| State FE | Yes |
| p-value: Joint orthogonality | 0.316 |
| Mean of dep. var | 0.344 |
| R-squared | 0.034 |

Notes: This table illustrates the randomness in the selection of municipalities to audit. Column 1 presents coefficients from a cross-sectional regression $\text{Audited}_{i,s} = \alpha_s + \gamma \times X_i + \epsilon_{i,s}$. The outcome variable, $\text{Audited}_{i,s}$, is an indicator for whether the municipality is audited between 2003-2015. All specifications include state fixed effects. The sample includes all eligible municipalities. RAIS variables and natural logarithm of estimated GDP are measured in 2002. Demographic variables from the 2000 Census. Robust standard errors are presented in parenthesis. *** p¡0.01, ** p¡0.05, * p¡0.1.

### *1.8.3 Municipal Public Employees: Additional Results*

**Figure 1.12:** The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees by Type of Contract




(a) Log Hires - Temporary

(b) Log Hires - Tenure-track

Notes: Each graph shows point estimates and 95% confidence intervals of a municipality level regression on a full set of dummy variables for each year relative to the audit interacted with respective dummies for high and low corruption municipalities. The omitted time category is the year before the audit for each group of municipalities. High corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the median. Low corruption corresponds to municipalities with a share of audited inspection orders with irregularities labeled as corruption lower than the median and never audited municipalities. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on new labor contracts added by type of contract (i.e. tenure-track or temporary). The unit of observation in this table is a municipality-year. Regressions include municipality and year fixed effects. Regressions include 56,192 observations from 3,512 municipalities and use data from 2000 to 2015. The sample covers a balanced panel of municipalities (only event-years within -3 to 4 around the audit year as explained in Section 1.3). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Standard errors clustered by municipality.

**Table 1.8:** Main Results: Robustness

| Dependent Variable: | Log Hires | | | Ln Hires | Hires |
|---|---|---|---|---|---|
| | Callaway et. al. (2020) | Hires>0 | First-time Hires | | |
| | (1) | (2) | (3) | (4) | (5) |
| PostAudited | 0.100** | 0.134*** | 0.135*** | 0.183*** | 14.966** |
| | [0.043] | [0.030] | [0.051] | [0.054] | [7.610] |
| Observations | 64,912 | 61,833 | 64,912 | 64,912 | 64,912 |
| Municipalities | 4057 | 4057 | 4057 | 4057 | 4057 |
| Municipality FE | | Yes | Yes | Yes | Yes |
| Year FE | | Yes | Yes | Yes | Yes |
| Mean of dep. var | 4.632 | 4.863 | 3.159 | 3.749 | 164 |

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i+(y_i^2+1)^{0.5})$) on the new labor contracts added. The dependent variable in column 4 is the natural logarithm of new labor contracts plus 0.01. The dependent variable in column 5 is new labor contracts. PostAudited takes the value of 1 after the municipality was audited. Estimator in column 1 corresponds to the aggregate ATT estimated using Callaway and Sant'Anna [2020b]. Sample excludes observations with zero hires in column 2. Dependent variable in column 3 corresponds to first-time hires only, and excludes transfers or re-employment. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.9:** Main Results: Goodman-Bacon Decomposition

| Dependent Variable: Log Hires | | |
|---|---|---|
| DD Comparison | Weight | Average DD Estimator |
| Earlier T vs. Later C | 0.033 | -0.036 |
| Later T vs. Earlier C | 0.052 | 0.023 |
| T vs. Never treated | 0.915 | 0.169 |
| DD Estimate | | 0.155 |

Notes: Dependent variable corresponds to the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. DD estimators were estimated using Goodman-Bacon et al. [2019]. Regressions uses data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Regressions includes municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.10:** The Effect of the Audit on Dismissals and Municipal Public Employees at end of year

| Dependent Variable: | Dismissals | Employees at end of year |
|---|---|---|
| | (1) | (2) |
| PostAudited | 0.094** | 0.037*** |
| | [0.043] | [0.014] |
| Observations | 64,912 | 64,912 |
| Municipalities | 4057 | 4057 |
| Municipality FE | Yes | Yes |
| Year FE | Yes | Yes |
| Mean of dep. var | 2.030 | 6.667 |

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of contracts where the employee was dismissed, and total contracts at the end of the year. PostAudited takes the value of 1 after the municipality was audited. All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.11:** The Heterogeneous Effect of the Audit on Hires of Municipal Public Employees: Robustness

| Dependent Variable: | Log Hires | | | | | |
|---|---|---|---|---|---|---|
| | Baseline | Split Sample | | Changing cutoffs: High Corruption | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| PostAudited | | 0.335*** | 0.103* | | | |
| | | [0.080] | [0.057] | | | |
| PostAudited × Low Corruption | 0.098* | | | 0.075 | 0.077 | 0.104** |
| | [0.057] | | | [0.054] | [0.051] | [0.049] |
| PostAudited × High Corruption | 0.334*** | | | 0.430*** | 0.573*** | 0.714*** |
| | [0.080] | | | [0.090] | [0.110] | [0.147] |
| Observations | 56,192 | 51,088 | 52,400 | 56,192 | 56,192 | 56,192 |
| Municipalities | 3512 | 3193 | 3275 | 3512 | 3512 | 3512 |
| Municipality FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes |
| p-value $H_0$: a-b=0 | 0.014 | | | 0.001 | 0.000 | 0.000 |
| Mean of dep. var | 4.606 | 4.624 | 4.602 | 4.606 | 4.606 | 4.606 |
| Percentile | 50 | 50 | 50 | 60 | 70 | 80 |
| Sample | | High Corruption | Low Corruption | | | |

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Sample in column 2 includes never audited municipalities and High Corruption municipalities. Sample in Column 3 includes never audited municipalities and Low Corruption municipalities. In columns 4-6 I change the definition of high corruption as municipalities with a share of audited inspection orders with irregularities labeled as corruption higher than the percentiles 60 (75%), 70 (80%), and 80 (84%). All regressions use data from 2000 to 2015. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

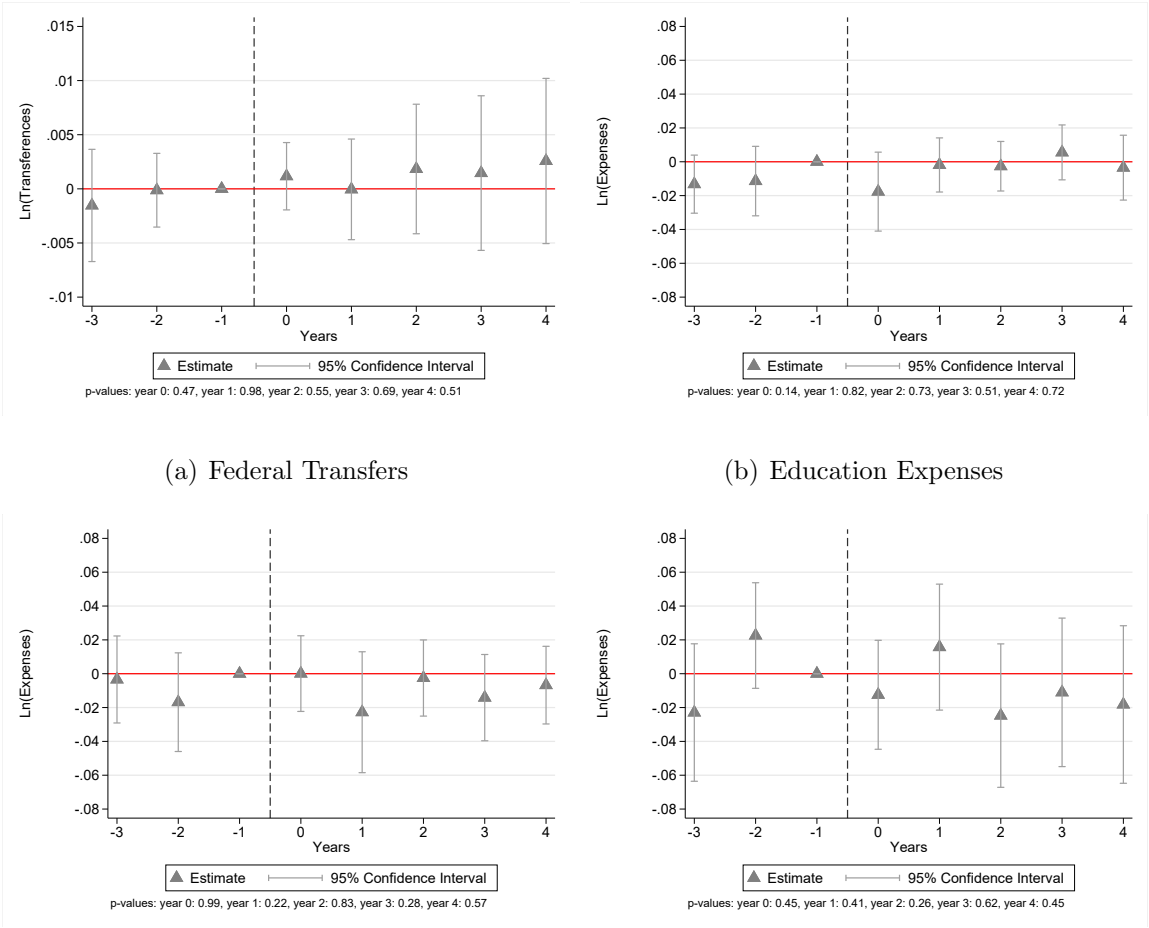
**Table 1.12:** Alternative Channels for the Effect of Audit on Hires

| Dependent Variable: | Hires | | Resignations | Share Hires w/ college | Share Dismissals w/college |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| PostAudited | 0.104** | 0.148*** | | -0.003 | 0.001 |
| | [0.049] | [0.039] | | [0.004] | [0.007] |
| PostAudited × Low Corruption | | | 0.044 | | |
| | | | [0.057] | | |
| PostAudited × High Corruption | | | -0.040 | | |
| | | | [0.069] | | |
| Observations | 56,416 | 48,684 | 56,192 | 61,833 | 41,855 |
| Municipalities | 3526 | 4057 | 3512 | 4057 | 3857 |
| Municipality FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |
| p-value $H_0$: a-b=0 | | | 0.334 | | |
| Mean of dep. var | 4.612 | 4.520 | 2.201 | 0.318 | 0.332 |

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-3 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the new labor contracts added (columns 1-2) and on the number of work contracts of employees who resigned (column 3). PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Sample in column 1 excludes municipalities that were audited during years 3 or 4 of the mayoral term. Sample in column 2 excludes observations that correspond to the first year of a mayoral term. Sample in column 3 excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). All regressions use data from 2000 to 2015, except column 1. The sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year as explained in Section 1.3. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

### *1.8.4 Public Finances*

**Figure 1.13:** Effects of the Audit on Federal Transfers and Municipal Expenditures




(a) Federal Transfers

(b) Education Expenses

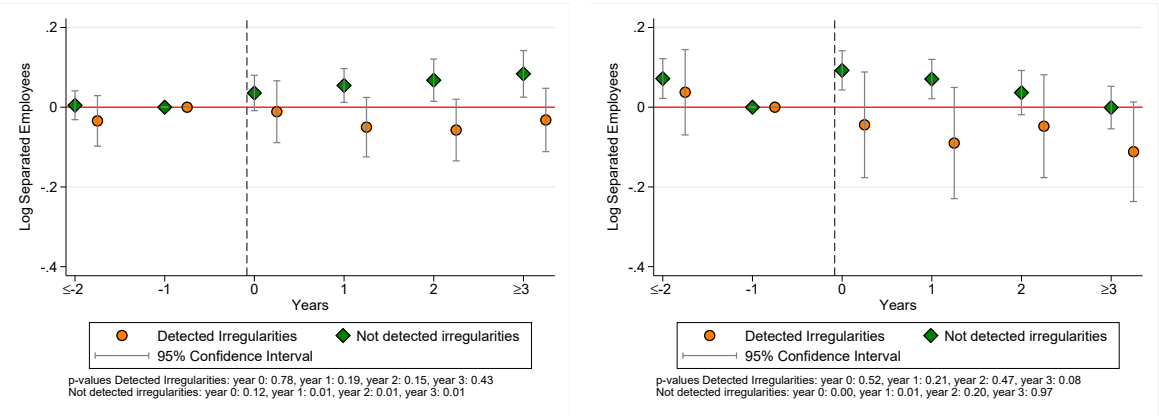
(c) Health Expenses

(d) Social Welfare Expenses

Notes: Each graph shows point estimates and 95% confidence intervals of regression of municipality level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Dependent variable is expressed in log points of constant local currency units. All regressions use data from 2000 to 2015 and include municipalities with complete data on transfers for the full sample. Regressions include municipality and year fixed effects. Regressions in Panels A and B include 87,360 observations from 5,460 municipalities. Regressions in Panels C-D include 45,472 observations from 2,842 municipalities. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

### *1.8.5 Public Education: Additional Results*

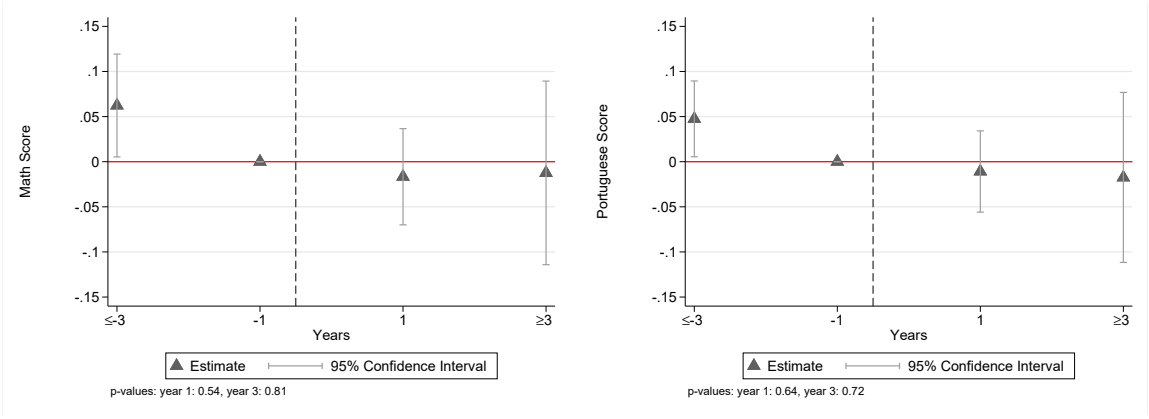
**Figure 1.14:** The Heterogeneous Effect of Separations in Municipal Schools by Detected Irregularities




(a) High Corruption Municipalities (b) Low Corruption Municipalities

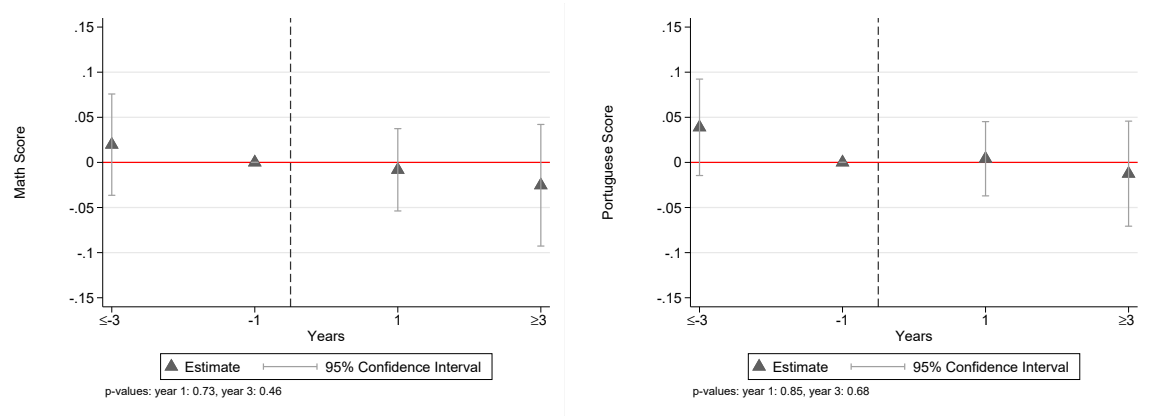
Notes: Each graph shows point estimates and 95% confidence intervals of regression of school level on a full set of dummy variables for each year relative to the audit. The omitted category is the year before the first audit. Transformation of dependent variables is calculated by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of separated employees. Regressions include school and municipality-year fixed effects. Regressions include 165,107 observations from 2,547 municipalities and use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Standard errors clustered by municipality.

**Figure 1.15:** The Effect of the Audit on Student Test Scores in Municipal Public Schools




(a) 4th grade - Math

(b) 4th grade - Portuguese Language

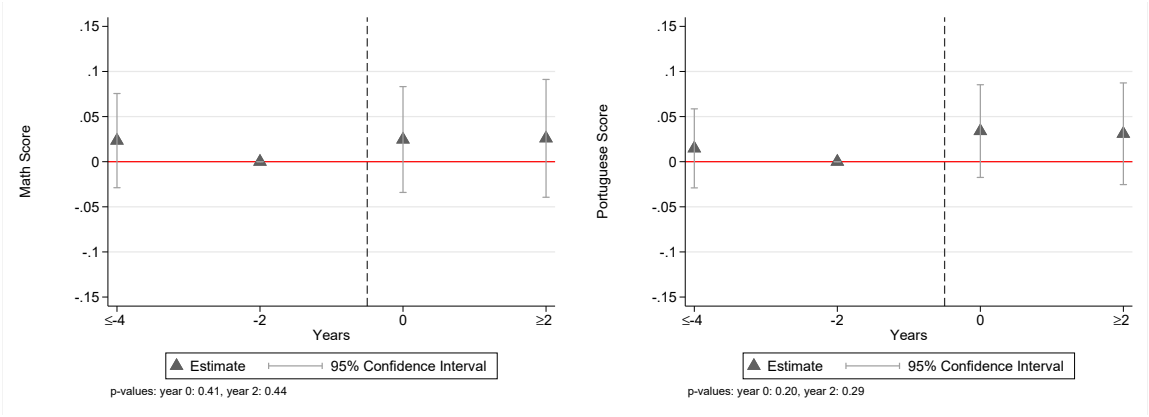



(c) 8th grade - Math

(d) 8th grade - Portuguese Language

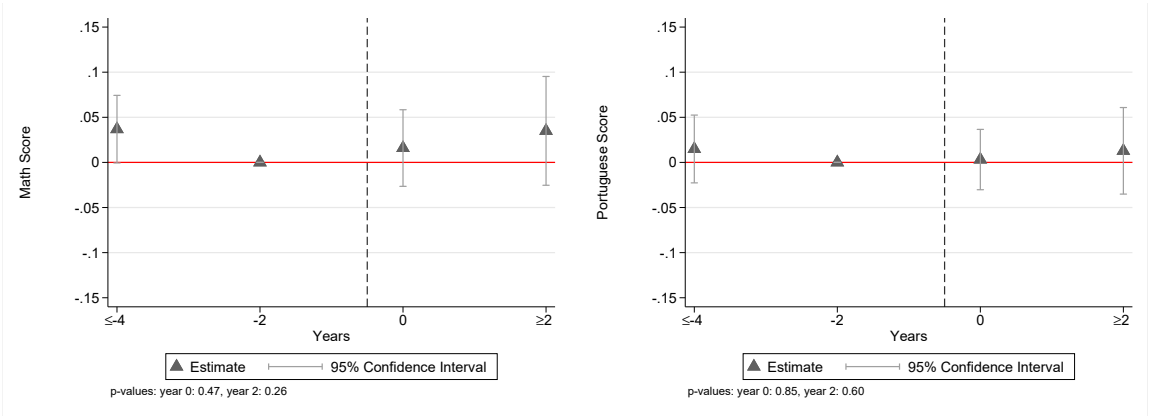
Notes: Each graph shows point estimates and 95% confidence intervals of a regression of individual level test scores on a full set of dummy variables for years -3, -1, 1 and 3 relative to the audit. The omitted category is the year before the first audit. Individual Test scores correspond to the Prova Brasil exams (2007, 2009, 2011, 2013 and 2015) and are standardized based on the distribution of individual level test scores from students in non-audited municipalities up to 2015. The unit of observation in this figure is a student-year. Regressions include school and year fixed effects. They also include student level controls (an indicator variable for gender, age, whether the student is white, and whether the student's mother reads). Regressions on 4th grade tests scores include 2,887,564 observations from 3,203 municipalities. Regressions on 8th grade tests scores include 1,206,038 observations from 2,074 municipalities. Sample covers a balanced panel of municipalities. Sample of audited municipalities include those drawn in lotteries 31-33, and 36-37. Standard errors clustered by municipality.

**Figure 1.16:** The Effect of the Audit on Student Test Scores in Municipal Public Schools




(a) 4th grade - Math

(b) 4th grade - Portuguese Language




(c) 8th grade - Math

(d) 8th grade - Portuguese Language

Notes: Each graph shows point estimates and 95% confidence intervals of a regression of individual level test scores on a full set of dummy variables for years -2, 0, 2 and 4 relative to the audit. The omitted category is the year of the audit. Individual Test scores correspond to the Prova Brasil exams (2007, 2009, 2011, 2013 and 2015) and are standardized based on the distribution of individual level test scores from students in non-audited municipalities up to 2015. Regressions include school and year fixed effects. They also include student level controls (an indicator variable for gender, age, whether the student is white, and whether the student's mother reads). Regressions on 4th grade tests scores include 2,887,564 observations from 3,203 municipalities. Regressions on 8th grade tests scores include 1,206,038 observations from 2,074 municipalities. Sample covers a balanced panel of municipalities. Sample of audited municipalities include those drawn in lotteries 34-35, and 28-30. Standard errors clustered by municipality.

**Table 1.13:** Effects of the Audit on New Municipal Public School Employees: Robustness

| Schools: | Municipal | | | | | | State&Federal |
|---|---|---|---|---|---|---|---|
| Dependent Variable: | Log New Employees | | | | Ln New Employees | New Employees | Log New Employees |
| | | Callaway et. al. (2020) | New Employees>0 | *Prova Brasil* | | | |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| PostAudited | 0.065*** | 0.072* | 0.046*** | 0.087*** | 0.151*** | 0.259*** | -0.010 |
| | [0.019] | [0.037] | [0.013] | [0.026] | [0.047] | [0.069] | [0.024] |
| Observations | 447,264 | 447,264 | 336,551 | 170,568 | 447,264 | 447,264 | 105,896 |
| Municipalities | 3415 | 3415 | 3414 | 3291 | 3415 | 3415 | 3383 |
| School FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Mean of dep. var | 1.463 | 1.463 | 1.939 | 2.159 | -0.226 | 3.726 | 2.505 |

Notes: Dependent variable in the header. Transformation of dependent variables in columns 1-4 and 7 was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of new employees in municipal schools. The dependent variable in column 5 is the natural logarithm of new employees plus 0.01. The dependent variable in column 6 is new employees. PostAudited takes the value of 1 after the municipality was audited. Estimator in column 1 corresponds to equation 1.3. Estimator in column 2 corresponds to the aggregate ATT estimated using Callaway and Sant'Anna [2020b]. Sample excludes observations with zero new employees in column 3. Sample excludes observations from schools that did not participate in *Prova Brasil* in column 4. *Prova Brasil* is administered only in schools with at least 20 students enrolled in that particular grade level. Sample in columns 1-6 includes all municipal schools, sample in column 7 includes only state and federal schools. All regressions use data from 2008 to 2015. Sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.14:** Effects of the Audit on Municipal Public School Employees

| Dependent Variable: | Separated employees | Employees at time of Census | Hires in education |
|---|---|---|---|
| | (1) | (2) | (3) |
| PostAudited | 0.022 | 0.031*** | 0.090** |
| | [0.015] | [0.009] | [0.044] |
| Observations | 447,264 | 447,264 | 64,912 |
| Municipalities | 3415 | 3415 | 4057 |
| Years | 2008-2015 | 2008-2015 | 2000-2015 |
| Data source | School Census | School Census | *RAIS* |
| School FE | Yes | Yes | No |
| Year FE | Yes | Yes | Yes |
| Municipality FE | No | No | Yes |
| Mean of dep. var | 1.401 | 2.588 | 2.871 |

Notes: Dependent variable in the header. Transformation of dependent variables was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$). Dependent variables in columns 1-2 correspond to the number of separated employees, employees at time of Census in municipal schools, respectively. Dependent variable in column 3 corresponds to the new labor contracts added in the sector of education and where the municipal government was the employer. PostAudited takes the value of 1 after the municipality was audited. Regressions in columns 1-2 use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Regressions in columns 1-2 include school and year fixed effects. Regression in column 3 uses data from 2000 to 2015 and the sample covers a balanced panel of municipalities with a window of [-3,4] around the audit year. Regression in column 3 includes municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.15:** The Effect of the Audit on Municipal Public School Principals

| Dependent Variable: | Share of Principals | |
|---|---|---|
| | New | Tenure-track |
| | (1) | (2) |
| PostAudited | -0.003 | -0.041*** |
| | [0.015] | [0.012] |
| Observations | 53,705 | 53,705 |
| Municipalities | 3154 | 3154 |
| School FE | Yes | Yes |
| Year FE | Yes | Yes |
| Mean of dep. var | 0.410 | 0.107 |

Notes: Dependent variable in the header. PostAudited takes the value of 1 after the municipality was audited. Regressions use data from individual survey responses from the Prova Brasil school principal questionnaire (2007, 2009, 2011, 2013 and 2015). New principals correspond to principals that stated that started working in the school less than 2 years ago. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.16:** Effects of the Audit on Separated Municipal Public School Employees

| Dependent Variable: | Separated employees | |
|---|---|---|
| | All | High Corruption |
| | (1) | (2) |
| PostAudited × Low Corruption | -0.005 | |
| | [0.016] | |
| PostAudited × High Corruption | 0.038*** | |
| | [0.013] | |
| PostAudited × Not detected irregularities | | 0.041*** |
| | | [0.015] |
| PostAudited × Detected irregularities | | 0.013 |
| | | [0.022] |
| Observations | 540,384 | 482,512 |
| Municipalities | 4010 | 3569 |
| School FE | Yes | Yes |
| Year FE | Yes | Yes |
| p-value $H_0$: a-b=0 | 0.032 | 0.208 |
| Mean of dep. var | 1.387 | 1.387 |

Notes: Dependent variable in the header. Transformation of dependent variable was done by estimating the inverse hyperbolic sine transformation($log(y_i + (y_i^2 + 1)^{0.5})$) on the number of separated employees in municipal schools. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Detected Irregularities is a dummy that indicates whether auditors uncovered irregularities in a particular school in a given lottery. Not Detected Irregularities is a dummy indicating whether auditors did not uncover irregularities either because auditors did not find any or because they did not investigate this school. Regressions use data from 2008 to 2015 and the sample covers a balanced panel of municipalities with a the window of [-2,3] around the audit year. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.17:** Effects of the Audit on School Assets Quality

| | Good quality | | | |
|---|---|---|---|---|
| | Students use | | Teachers use | |
| Dependent Variable: | Computer | Internet | Computer | Internet |
| | (1) | (2) | (3) | (4) |
| PostAudited × Low Corruption | 0.008 | 0.024 | 0.063*** | 0.035 |
| | [0.023] | [0.024] | [0.022] | [0.025] |
| PostAudited × High Corruption | -0.040** | -0.055*** | -0.039** | -0.040** |
| | [0.019] | [0.016] | [0.020] | [0.018] |
| Observations | 50,614 | 50,538 | 50,462 | 50,470 |
| Municipalities | 3154 | 3154 | 3154 | 3154 |
| School FE | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes |
| p-value a-b=0 | 0.097 | 0.004 | 0.000 | 0.013 |
| Mean of dep. var | 0.464 | 0.350 | 0.494 | 0.461 |

Notes: Dependent variable in the header. Dependent variable takes the value of 1 if the principal states that the quality is good or very good, and 0 otherwise. PostAudited takes the value of 1 after the municipality was audited. High Corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is higher than the median. Low corruption takes the value of 1 if the share of audited inspection orders with irregularities labeled as corruption is lower than the median or if the municipality was never audited. Regressions use data from individual survey responses from the Prova Brasil school principal questionnaire (2007, 2009, 2011, 2013 and 2015). Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). Regressions include school and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

### *1.8.6 Reelection: Additional Results*

**Table 1.18:** The Effect of Multiple Audits on Mayoral Reelection

| Dependent Variable: | D(reelected mayor) | |
|---|---|---|
| | Mean: 0.278 | |
| | (1) | (2) |
| [$a$] Audited in Current Term | -0.010 | -0.001 |
| | [0.024] | [0.019] |
| Audited in Previous Term | -0.013 | -0.023 |
| | [0.020] | [0.022] |
| [$b$] Audited in Current Term × Audited in Previous Term | 0.073 | 0.105* |
| | [0.080] | [0.062] |
| Eligible for reelection | | 0.524*** |
| | | [0.005] |
| Observations | 16,484 | 16,484 |
| Municipalities | 4121 | 4121 |
| Municipality FE | Yes | Yes |
| Year FE | Yes | Yes |
| p-value $H_0$: a+b=0 | 0.393 | 0.077 |

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. All regressions use election data from 2004, 2008 and 2012 and 2016. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19). The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

**Table 1.19:** The Effect of the Audit on Mayoral Reelection

| Dependent Variable: | D(reelected mayor) Mean: 0.279 | |
|---|---|---|
| | (1) | (2) |
| Audited in Current Term | -0.018 | 0.002 |
| | [0.027] | [0.021] |
| Eligible for reelection | | 0.523*** |
| | | [0.005] |
| Observations | 15,171 | 15,171 |
| Municipalities | 3810 | 3810 |
| Municipality FE | Yes | Yes |
| Year FE | Yes | Yes |

Notes: Dependent variable in the header. Audited in Current Term is an indicator variable for whether the mayor was audited and audit results were released before the elections. Sample excludes those municipalities that have been audited, but for which I do not have data on levels of corruption (lotteries 1–19), and municipalities that have been audited more than once. The sample excludes municipalities that are not included in the balanced panel used to generate Figure 1.1. All regressions use election data from 2004, 2008 and 2012 and 2016. The unit of observation in this table is a municipality-election cycle. Regressions include municipality and year fixed effects. Standard errors clustered by municipality. *** p<0.01, ** p<0.05, * p<0.1

### *1.8.7 Empirical Appendix*

This section provides detailed information on the the research design.

**Heterogeneous Effects**. When studying heterogeneous effects, I replace the event-year dummies in equation 1.1 with $\sum_k(\sum_{y=-4}^{-2}\tilde{\eta_y^k}D_{i,t}^y D_i^k + \sum_{y=0}^{5}\tilde{\beta_y^k}D_{i,t}^y D_i^k)$. The estimation equation is as follows:

$$Y_{i,t} = \alpha_i + \lambda_t + \sum_k(\sum_{y=-4}^{-2}\tilde{\eta_y^k}D_{i,t}^y D_i^k + \sum_{y=0}^{5}\tilde{\beta_y^k}D_{i,t}^y D_i^k) + \epsilon_{i,t} \tag{1.5}$$

$D_i^k$ is equal to 1 if a municipality belongs to group $k$.

**Summarizing results**. In the tables, I summarize the magnitudes and joint statistical significance of the event study estimates in a DD's specification according to the following specification:

$$Y_{i,t} = \beta \text{PostAudited}_{i,t} + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{1.6}$$

where $\text{PostAudited}_{i,t}$ is an indicator variable taking the value of 1 for all years after the audit in the audited municipality, and 0 otherwise. $\text{PostAudited}_{i,t}$ is always 0 for never-audited municipalities. The fixed effects are analogous to those in equation (1.1). The coefficient of interest is $\beta$, which captures the average causal change in the outcome variable (e.g., percentage change of hired municipal public employees) of audited municipalities compared to the eligible but never-audited municipalities, conditional on the set of municipality and year fixed effects.[29]

**Prova Brasil.** In addition to the primary municipality-level analysis, I also conduct a student-level and principal-level analysis by adapting the same empirical design to the more

29. When studying heterogeneous effects based on differential preexisting characteristics, I estimate the following interaction specification:

$$Y_{i,t} = \beta_1 \text{PostAudited}_{i,t} + \beta_2 \text{PostAudited}_{i,t} \times \text{C}_i + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{1.7}$$

where $\text{C}_i$ is a characteristic of the municipality measured pre-audit, unless otherwise specified.

disaggregated dataset from Prova Brasil. I use the following specification:[30]

$$Y_j = \alpha_{s(j)} + \lambda_{t(j)} + \eta D_{i,t}^{-3} + \beta_1 D_{i,t}^{1} + \beta_3 D_{i,t}^{3} + \gamma X_j + \epsilon_j \tag{1.9}$$

$Y_j$ is the outcome variable for student or school principal $j$. $\alpha_{s(j)}$ and $\lambda_{t(j)}$ are school and year fixed effects, respectively. $X_j$ are control variables (e.g., student's age, student's gender). The rest of the variables are analogous to those in equation (1.1). Observations more than three years before or more than three years after the audit are captured by dummies $D_{i,t}^{-3} = \mathbb{1}(tT_i^* \leq -3)$ and $D_{i,t}^{3} = \mathbb{1}(tT_i^* \geq 3)$, respectively. $\epsilon_i$ is an error term clustered at the municipality level.

The endpoints are different from the main specification since I only observed data points in 2007, 2009, 2011, 2013, and 2015. Given that the Prova Brasil data are biannual, to have a balanced panel of audited municipalities, I only include event-year dummies for -3, 1, and 3; a full set of dummies (-1 is omitted).

30. Alternatively, I could include event-year dummies for -2, 2, and 4, a full set of dummies (0 is omitted).

$$Y_i = \alpha_{s(i)} + \lambda_{t(i)} + \eta D_{i,t}^{-4} + \beta_0 D_{i,t}^{0} + \beta_2 D_{i,t}^{2} + \gamma X_i + \epsilon_i \tag{1.8}$$

# CHAPTER 2

# HOW EFFECTIVE ARE MONETARY INCENTIVES TO VOTE? EVIDENCE FROM A NATIONWIDE POLICY

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# ABSTRACT

We study voters' response to marginal changes to the fine for electoral abstention in Peru, leveraging variation from a nationwide reform. A smaller fine has a robust, negative effect on voter turnout, partly through irregular changes in voter registration. However, representation is largely unaffected, as most of the lost votes are blank or invalid. We also show that the effect of an exemption from compulsory voting is substantially larger than that of a full fine reduction, suggesting that non-monetary incentives are the main drivers behind the effectiveness of compulsory voting.[1]

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## 2.1 Introduction

The effectiveness of monetary incentives has attracted the attention of economists studying a wide array of topics [Gneezy et al., 2011]. But despite the fact that multiple countries around the world have mandatory voting enforced through monetary sanctions, little is known about voters' response to marginal changes to the size of these incentives, especially when applied at a large scale.[2] Even though a larger abstention fine mechanically increases the cost of not voting, a stronger extrinsic incentive may crowd out the intrinsic motivation to vote provided by social image concerns or a sense of civic duty [e.g., Bénabou and Tirole, 2003, 2006]. This makes the magnitude of the net turnout effect an open empirical question. Whether any resulting change in electoral participation affects election outcomes is also not clear, given that those induced to vote by a marginally larger fine are likely to be particularly uninformed or uninterested in the electoral process [Feddersen and Pesendorfer, 1996].

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

In this paper, we study voters' response to marginal changes in monetary incentives to vote. We exploit a nationwide reform to the value of the abstention fine in Peru, a country with more than 20 million voters and compulsory voting since 1933. We provide three main findings. First, a lower fine has a robust negative effect on voter turnout. This effect is partly

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

driven by irregular changes in registration, as spatial variation in the value of the fine causes young voters to increasingly register in districts with lower fines. Second, the reduction in turnout spurred by a lower fine appears to have little impact on representation, since it is largely matched by reductions in the number of blank or invalid votes. Third, evidence from a second natural experiment that exploits an age-based exemption from compulsory voting suggests that monetary incentives are not the main driver behind the effectiveness of compulsory voting.

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

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

We can decompose the turnout effect into the separate channels of voter registration and the propensity to vote. In Peru, citizens are automatically registered to vote in their district of residence, as recorded in their national identification card (DNI). However, the spatial

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

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

In a setting with few barriers to electoral participation, abstention is likely driven by uninformed or uninterested voters [Feddersen and Pesendorfer, 1996]. Hence, a higher abstention fine should draw voters who are more likely to spoil their vote. We find that a S/ 10 lower fine leads to a 0.37 pp decrease in the share of spoiled (i.e. blank or invalid) votes in the presidential first round, equivalent to 86% of the estimated effect on turnout for this type of election (0.43 pp). While these votes may also correspond to mistakes by the less educated [Fujiwara, 2015] or to a form of political protest [Ujhelyi et al., 2019], what seems

certain is that the induced change in electoral participation has a very small impact on representation. Additional evidence on political attitudes and behaviors from a large household survey further shows that a larger fine leads to reduced interest in politics.

The pecuniary incentive provided by the abstention fine is one of several incentives to vote provided by compulsory voting. These include the restriction on government services faced by non-voters (i.e. non-pecuniary cost) and the expressive function of the law as a signalling device for socially desirable behavior [Funk, 2007]. It is difficult to gauge the magnitude of the effect of marginal fine changes on voter turnout without knowing the aggregate effect of compulsory voting. To answer this question, we use detailed data at the 'voting-booth' level and leverage idiosyncratic variation in the age of voters across voting booths in 2016. We exploit the exemption from the mandate to vote for citizens with ages of 70 or more and compare turnout rates in voting booths with varying shares of voters around the threshold, while flexibly controlling for the age structure of all other voters. This comparison takes place within the same district or even the same polling station. We find that the exemption from compulsory voting leads to a decrease in voter turnout of almost 10 pp at age 70, 20 pp by age 72, and 40 pp by age 75. Using individual-level data on turnout in Chile in 2017 (a neighboring country without compulsory voting), we show that the natural decline in electoral participation at these ages is substantially smaller.

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

This paper contributes to the vast literature studying voter turnout.[3] Several papers have studied the effects of monetary incentives, but only through field experiments [Loewen et al., 2008, Panagopoulos, 2012, León, 2017, Shineman, 2018]. Our first contribution is to estimate the effectiveness of marginal monetary incentives to vote 'in the wild'.[4] More broadly, we connect the literature on extrinsic incentives to vote with a separate strand studying intrinsic incentives, such as voters' sense of civic duty [Gerber et al., 2008], habit formation [Coppock and Green, 2016, Fujiwara et al., 2016] and social image concerns [Funk, 2010, Dellavigna et al., 2017]. Our findings suggest that non-monetary incentives significantly outweigh pecuniary costs (in the form of fine reductions) in the calculus of voting.

Another large body of work has studied a wide array of voter mobilization initiatives [Gerber and Green, 2017], but most of these interventions have only been tested through small-scale field experiments. Our paper links this literature with a separate body of research analyzing the usefulness of experimental studies for policy scale-up [Deaton, 2010, Al-Ubaydli et al., 2017, Banerjee et al., 2017, Muralidharan and Niehaus, 2017, List et al., 2019, Vivalt, 2019]. In particular, the heterogeneous effects we uncover illustrate the potential for context dependence in small experiments. We also shed light on informational frictions as a source of 'voltage drop' in the response to large-scale institutional changes.[5]

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

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

4. Panagopoulos [2008] shows that countries with stricter penalties or enforcement have higher turnout. Concurrent work by Carpio et al. [2018] exploits our same reform to study the effect of voter turnout on the party affiliation of candidates in municipal elections.

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

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

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

## 2.2 Institutional Background

General elections in Peru, encompassing the first round of the presidential election and multi-district legislative elections, are held concurrently every five years. In the legislative election, voters in each of the 25 regions of the country elect their representatives to the unicameral congress using a system of proportional representation.[6] In the presidential election, a candidate must obtain at least 50% of the votes nationwide to win in the first round (which never happens during our sample period.) As a result, a run-off election between the two leading candidates takes place approximately two months later.[7] Voter turnout has been traditionally high and remained above 80% throughout the sample period (see Appendix Figure 2.7). However, turnout has been declining since 2006, which coincides with the reform reducing the monetary incentive to vote that we study.

All citizens must obtain a national identification card, DNI (Documento Nacional de

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

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

Identidad), when they turn eighteen. The DNI includes the person's home address and must be renewed every eight years (up to the age of seventy). The DNI also acts as the electoral document and the address on file is used to determine the district where the person is required to vote (registration is automatic).[8]

Voting is compulsory for citizens between the ages of 18 and 69 (both inclusive) since 1933. Voting is done in person at pre-determined polling stations and voters are provided with a sticker on their DNI as proof of participation. This sticker is the main mechanism for enforcement, backed by an electronic database. Those who abstain from voting and do not meet the age requirement for exemption face restricted access to government and financial services until they pay a fine or provide a valid excuse.[9] Other countries with compulsory voting impose similar restrictions [Jaitman, 2013, Cepaluni and Hidalgo, 2016]. Fines accumulate, but failure to settle an outstanding fine does not prevent someone from voting again or accessing social protection programs. People can pay the fine at any of the around 600 branches of the National Bank (Banco de la Nación, BN) throughout the country. Alternatively, they can submit an excuse and supporting documents to the JNE after paying a processing fee of about S/21 (US$6.4).[10] All restrictions are lifted once the fine has been settled (i.e., paid or excused).

Until 2006, the abstention fine was the same in all districts, set at 4% of an official reference unit known as UIT.[11] At the start of 2006, the value of the fine was S/136 (roughly

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

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

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

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

US$90 PPP).[12] Shortly after the national elections held on that year, Congress approved a law that reformed the abstention fine. The law classified districts into three categories based on their level of poverty: non-poor, poor , extreme poor. All districts saw a reduction to the value of the fine, but those in the latter categories enjoyed larger falls. Henceforth, we refer to these districts as high, medium, and low fine. In high-fine districts, the fine was cut in half to 2% of the UIT, while in districts classified as medium- and low-fine, it was set at 1% and 0.5% respectively. These amounts roughly corresponded to US$45, US$23, and US$11.

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

The district classification was delegated to the National Statistical Office (Instituto Nacional de Estadística e Informática, INEI), but the criteria used for the initial assignment released in 2006 by the National Electoral Jury (Jurado Nacional de Elecciones, JNE) remains unclear (Resolución 4222-2006-JNE). However, the only elections to be held under this classification were the subnational elections of November 2006. Shortly before the 2010 subnational elections, the JNE released a new district classification, which remains in place until the present day (Resolución 2530-2010-JNE). In the new classification, districts were assigned to the fine category corresponding to the largest share of their population, according to a poverty map based on the 2007 population census. Fifty-two percent of districts were assigned a high fine (non-poor), 18% were assigned a medium fine (poor) and 30% a low fine (extreme poor).[13] We verify below that our results are not driven by idiosyncratic changes

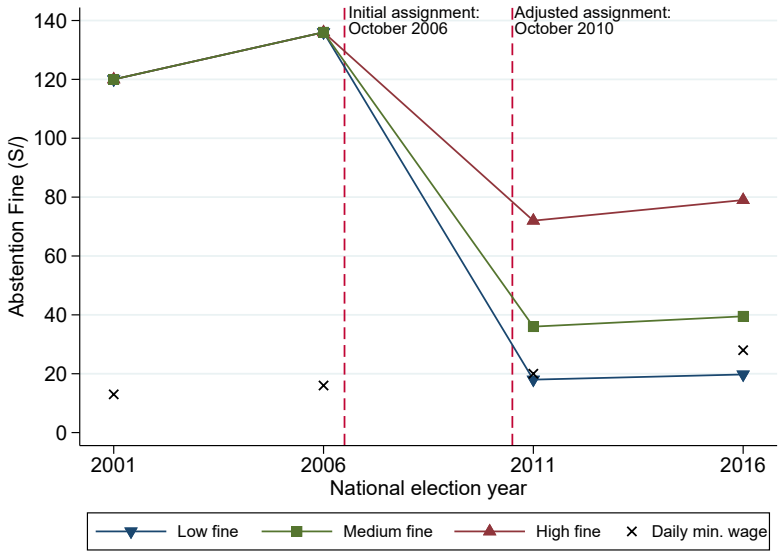
12. The UIT was set at S/3,400 in 2006. The PPP-adjusted exchange rate in 2006 was S/1.51 per US$1. We use this value for all calculations in the paper. The nominal exchange rate was S/3.27.

13. Appendix Table 2.8 shows that all districts classified as high-fine in 2006 remained in this category in

in voter turnout in poorer or richer areas. To the best of our knowledge, this classification has not been used for any other public policy.

Figure 2.1 shows the value of the abstention fine in each category for each national election during the sample period. The elections in 2006 were the last ones held under the previous regime with a uniform fine. The following ones in 2011 and 2016 took place after the reform and also after the districts had been re-classified. Figure 2.1 also shows that the value of the fine in low-fine districts is similar to the minimum daily wage. The value of the fine in each category has remained constant as a percentage of the UIT since the 2006 reform. Slight growth between 2011 and 2016 is driven by the yearly adjustment of the UIT.

**Figure 2.1:** The Abstention Fine by Election and Fine Category




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

Neither the enforcement of the fine nor the restrictions in access to government and financial services faced by non-voters changed in the 2006 reform. Fines normally expire after four years and the national government often provides amnesties, thereby dissuading

2010, while only 15% and 56% of districts classified as medium- and low-fine remained in the same category.

debtors from settling outstanding fines. Roughly 20% of the 4.7 million fines issued in 2011 were settled (see Appendix Figure 2.8). However, enforcement improved substantially in 2012, when a collection unit was created within the JNE (Resolución 0738-2011-JNE).[14] As a result, almost 50% of the fines issued in 2016 had been settled by mid-2018. This increase was mainly driven by high-fine districts. We verify below that our estimates of the marginal effect of the abstention fine on turnout are not confounded by differential changes to enforcement across fine categories.

## 2.3 Empirical Strategy

In this section we present the data sources and research design for the analysis of the marginal effects of the abstention fine. We leave the exposition of the complementary strategy for the study of the exemption from compulsory voting for section 2.6 below.

### *2.3.1 Data*

We use administrative data for the national elections in 2001, 2006, 2011 and 2016 from the National Election Office (Oficina Nacional de Procesos Electorales, ONPE). The data covers the general election, combining the legislative election with the first round of the presidential race, and the presidential run-off. We observe the number of registered voters, the number of votes cast and the number of invalid and blank votes per election at the district level. JNE publishes the value of the abstention fine and the assignment of districts to the different fine categories before each election. Our main sample includes 1,755 districts, corresponding to 94% of the total and covering 96% of the almost 23 million registered voters in 2016.[15]

JNE provided us with the number of fines issued per election and the amount of money

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

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

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

### *2.3.2 Research Design*

We aim to estimate the causal effect of the value of the abstention fine on voter turnout and electoral outcomes. We exploit the differential reduction of the fine across districts after 2006. This natural experiment lends itself naturally to a difference-in-difference analysis.[16]

As mentioned above, districts were initially classified into the three fine categories in 2006, but were reclassified before the next election in 2011. It is the variation provided by this latter classification that we exploit in our estimations. However, the 2006 assignment may have been based on informative district characteristics that we do not observe (as the criteria employed remains unclear). Even if temporary, this assignment may have also affected voters' perception of the value of the fine and their behavior. To account for these possibilities, we allow the outcomes to vary flexibly over time in districts that are located in the same province and were assigned to the same category in 2006. Our baseline specification thus includes district fixed effects and the quite stringent 'election by province by 2006 category' fixed effects.[17] The latter also control for common shocks, allowing them to differ across

16. Carpio et al. [2018] use a complementary Regression Discontinuity Design (RDD) with the share of population below the poverty line as running variable. Reassuringly, their RDD estimates of the difference in turnout across fine categories are very similar to our difference-in-difference estimates.

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

provinces and/or 2006 fine categories. The identifying assumption is that in the absence of the 2010 assignment there should be no systematic change in the outcomes between districts located in the same province and that belonged to the same category in 2006. To provide evidence in support of the parallel trends assumption, we first estimate the following model:

$$\mathrm{y}_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \sum_k \sum_t \beta_{k,t} \left[\mathbb{1}(e = t) \times \mathbb{1}(\mathrm{c}_{10} = k)\right] + \epsilon_{d,p,e} \tag{2.1}$$

where $y_{d,p,e}$ is an outcome in district $d$ from province $p$ in election $e$. $\alpha_d$ is a district fixed effect, while $\delta_{p,e,c_{06}}$ is the 'election by province by 2006 category' fixed effect. The other terms correspond to a full set of interactions between dummy variables $\mathbb{1}(\cdot)$ for each election date $t$ in the sample period (e.g. 2011 presidential run-off) and respective dummies for each fine category $k$ from the 2010 assignment ($c_{10}$). The omitted election is the 2006 presidential run-off (last national election before the reform) and the omitted category corresponds to medium-fine districts. $\epsilon_{d,p,e}$ is an error term that we cluster at the province level (192 clusters) to allow for arbitrary correlation within provinces, including spatial autocorrelation.

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

We exclude subnational elections from the analysis to keep the selection and behavior of local candidates and incumbents constant and focus on voter behavior. Our empirical strategy always involves comparing voters with different incentives to participate in elections, but faced with the same set of candidates. By excluding subnational elections we also shut down the potential effects of the reform on voter buying [Hidalgo and Nichter, 2016].

We then modify the specification to estimate the causal effect of a marginal change to

the abstention fine on our relevant outcomes:

$$y_{d,p,e} = \alpha_d + \delta_{p,e,c_{06}} + \nu \text{ Fine value}_{d,e} + u_{d,p,e} \tag{2.2}$$

where $\text{Fine}_{d,e}$ is the value of the abstention fine in 100s of current Peruvian soles (S/) and $\alpha_d$ and $\delta_{p,e,c_{06}}$ are fixed effects analogous to those in equation (2.1). Thus, we exploit the same source of variation as above. The coefficient of interest is $\nu$, which captures the average causal effect on the outcome (e.g. percentage points of turnout) for a S/100 change in the value of the fine. We also estimate the corresponding elasticities by replacing the outcome and the value of the fine with their logarithms. Importantly, while this specification makes no difference between fine increases and decreases, our variation originates exclusively in fine decreases and does not allow us to capture asymmetric responses. $u_{d,p,e}$ is an error term clustered again at the province level, which flexibly accommodates serial and spatial correlation of the error term across districts within the same province.[18] We weight all our regressions by the number of registered voters in 2001 to capture individual-level effects.

## 2.4 Results: Voter Turnout

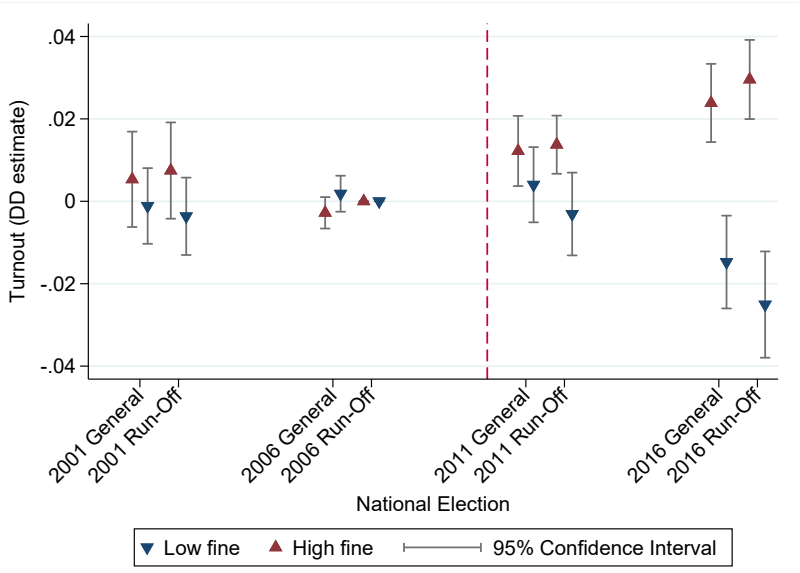
In this section, we present estimates of the causal effect of the abstention fine on voter turnout. We first present evidence from the event-study model that lends support to the identifying assumption of parallel trends and indicates an increasing response over time. We then show estimates of the marginal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

18. Appendix Table 2.9 shows that the main results are robust to clustering at the region level, using a wild cluster bootstrap procedure to account for the small number of clusters [Cameron et al., 2008].

### *2.4.1 Baseline Results*

Figure 2.2 shows point estimates and 95% confidence intervals of $\beta_{k,t}$ in equation (2.1) for voter turnout.[19] The arrowhead markers show the average difference in turnout between districts in the high- or low-fine categories and those in the omitted medium-fine category, relative to the difference in the 2006 presidential run-off (omitted election). The dashed line shows the timing of the adjusted assignment to the fine categories.

**Figure 2.2:** The Reform to the Abstention Fine and Voter Turnout




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

The estimates for the elections before the reform are small and statistically insignificant at conventional levels, indicating that voter turnout followed parallel trends across all categories between 2001 and 2006. These results increase our confidence in attributing any subsequent

19. Appendix Table 2.10 shows the corresponding estimates. Appendix Figure 2.9 shows results from a more disaggregated specification with separate coefficients for each combination of 2006/2010 assignments.

change in turnout to the reform. After the reform, we observe indeed a systematic divergence in turnout among the three groups. High-fine districts show a steady relative increase in turnout, while low-fine districts show a steady relative decrease. Turnout is already 1.3 percentage points (pp) higher in the high-fine category in 2011. In 2016, voter turnout in high-fine districts was 2.4 and 3.0 pp higher than in medium-fine ones in the general and run-off elections respectively, while in low-fine districts it was 1.5 and 2.5 pp lower than in medium-fine ones in those same elections.There is a 5.4 pp gap in turnout between the high-fine and low-fine districts in the 2016 presidential run-off. This is a sizable effect, comparable to that of some of the most effective voter mobilization initiatives that have been studied [Green et al., 2013], and could lead to inequality in representation across districts. Figure 2.2 also provides preliminary evidence of a heterogeneous effect across election types.

Panel A in Table 2.1 presents estimates of equation (2.2) using voter turnout as dependent variable. The estimate of $\nu$ in column 1 implies that a S/10 decrease in the value of the fine (roughly US$7) leads to a fall in turnout of about 0.5 percentage points. This corresponds to a 0.58% decrease over the sample mean of 0.85. Column 1 in Panel B shows that the corresponding estimate of the elasticity is 0.03. Both coefficients are statistically significant at the 1% level. As mentioned above, these estimates incorporate potential spillovers from changes in the behavior of peers and other general equilibrium effects.

The remaining columns correspond to several robustness tests. Our point estimates are very similar when we include election (column 2) or election by province fixed effects (column 3) instead of election by province by 2006 category fixed effects, or when we run our regressions unweighted (column 4). Columns 5-7 show that time-varying differences across districts do not affect our baseline results. In these regressions, we control flexibly for the share of people classified as poor or extreme poor (column 5), the share of voters with primary, secondary and higher education (column 6), and the number of polling stations in the district (column 7).[20] Our estimates remain economically and statistically significant.

20. Information on education is not available for 2006. The point estimates (standard error) for the

**Table 2.1:** The Marginal Effect of the Abstention Fine on Voter Turnout

| | Baseline | Election FE | Election x Province FE | No weights | Additional controls | | |
|---|---|---|---|---|---|---|---|
| | | | | | Poverty shares | Education shares | Polling stations |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| | Panel A - Dependent variable: Voter Turnout$_{i,t}$ | | | | | | |
| Fine value$_{i,t}$ (S/ x 100) | 0.049*** | 0.073*** | 0.046*** | 0.062*** | 0.035*** | 0.061*** | 0.046*** |
| | [0.008] | [0.012] | [0.009] | [0.010] | [0.011] | [0.011] | [0.009] |
| R-squared | 0.0180 | 0.0693 | 0.0257 | 0.0245 | 0.0637 | 0.0770 | 0.0511 |
| Mean of dependent variable | 0.845 | 0.844 | 0.844 | 0.793 | 0.845 | 0.831 | 0.845 |
| | Panel B - Dependent variable: ln Voter Turnout$_{i,t}$ | | | | | | |
| ln Fine value$_{i,t}$ | 0.030*** | 0.040*** | 0.028*** | 0.037*** | 0.023*** | 0.037*** | 0.029*** |
| | [0.005] | [0.006] | [0.006] | [0.006] | [0.007] | [0.007] | [0.006] |
| R-squared | 0.0180 | 0.0564 | 0.0240 | 0.0220 | 0.0614 | 0.0699 | 0.0395 |
| Mean of dependent variable | -0.171 | -0.172 | -0.172 | -0.240 | -0.171 | -0.188 | -0.171 |
| Observations | 13,536 | 14,040 | 14,040 | 13,536 | 13,536 | 10,152 | 13,536 |
| Districts | 1692 | 1755 | 1755 | 1692 | 1692 | 1692 | 1692 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | No | No | Yes | Yes | Yes | Yes |
| Election FE | No | Yes | No | No | No | No | No |
| Election x Province FE | No | No | Yes | No | No | No | No |
| Weighted | Yes | Yes | Yes | No | Yes | Yes | Yes |

Notes: All columns use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016, except column 6 (data on education level of voters is unavailable for 2006). Column 5 includes the time-invariant shares of poor and extreme poor inhabitants interacted with election fixed effects. Column 6 includes the time-varying shares of registered voters with primary, secondary and tertiary education. Column 7 includes log polling stations. All columns are weighted by the number of registered voters in 2001, except column 4. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

### *2.4.2 Heterogeneous Effects*

We study potential heterogeneity in the turnout response to a changing value of the abstention fine by estimating extensions of equation (2.2) that include interactions of the fine with dummies for various subsamples. Table 2.2 shows the results.

marginal effect of the fine and the corresponding elasticity in this reduced sample are 0.040 (0.010) and 0.023 (0.006).

**Table 2.2:** Heterogeneous Effects of the Abstention Fine on Voter Turnout

| Dependent variable: | Turnout$_{i,t}$ (Mean=0.85) | | | ln Turnout$_{i,t}$ (Mean=-0.17) | | |
|---|---|---|---|---|---|---|
| | Long-run | Run-off | Poverty | Long-run | Run-off | Poverty |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| (ln) Fine value$_{i,t}$ (S/ x 100) [a] | 0.020** | 0.039*** | -0.022 | 0.011** | 0.023*** | -0.021 |
| | [0.008] | [0.009] | [0.022] | [0.005] | [0.006] | [0.022] |
| (ln) Fine value$_{i,t}$ × $\mathbb{1}$(2016)$_t$ [b] | 0.051*** | | | 0.038*** | | |
| | [0.005] | | | [0.003] | | |
| (ln) Fine value$_{i,t}$ ×$\mathbb{1}$(Run-Off)$_t$ [b] | | 0.019*** | | | 0.014*** | |
| | | [0.004] | | | [0.003] | |
| (ln) Fine value$_{i,t}$ × Share Poor$_i$ [b] | | | 0.072*** | | | 0.058** |
| | | | [0.021] | | | [0.025] |
| Observations | 13,536 | 13,536 | 13,536 | 13,536 | 13,536 | 13,536 |
| Districts | 1692 | 1692 | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.0277 | 0.0194 | 0.0299 | 0.0325 | 0.0201 | 0.0235 |
| p-value $H_0$: a+b=0 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 | 0.000 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: All columns use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. The value of the fine in columns 1-3 is measured in 100s of current Peruvian Soles (S/). In columns 4-6, we use the natural log of the value of the fine. Columns 1 and 4 include the interaction of the fine with a dummy for the 2016 elections. Columns 2 and 5 include the interaction of the fine with a dummy for presidential run-off elections. Columns 3 and 6 include the interaction of the fine with the share of poor population (non-extreme and extreme) in the district. All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

We first examine heterogeneous effects across the two post-reform elections. Consistent with the evidence in Figure 2.2, we observe a much larger effect in the longer term. A same-sized decrease to the value of the fine leads to a drop in turnout that is more than

three times as large in 2016 as in 2011. The estimated elasticity is 0.011 in 2011 and 0.049 in 2016. This increasing response over time is consistent with gradual learning about the modified policy incentives. We further explore this mechanism in section 2.4.5. It is also consistent with dynamic peer effects if the marginal voters induced not to vote by the lower fine increasingly drive others to also not vote as time goes by [Nickerson, 2008, Chong et al., 2019]. A third possibility is that habit formation acts as a countervailing force and that those induced not to vote by the lower fine in 2011 are joined by a new wave of marginal non-voters in subsequent elections [Coppock and Green, 2016, Fujiwara et al., 2016].

We next explore whether the marginal effect of the fine on turnout varies depending on the type of election. The marginal effect of the fine is around 50% larger in the run-off than in the general election, with the elasticity jumping from 0.023 to 0.037. This heterogeneous effect is unlikely to be driven by increased learning, with the elections less than two months apart.[21] It suggests instead that the marginal voters affected by changes to the value of the fine differ across election types. One plausible explanation is that voters have a stronger incentive to participate in the general election than in the presidential run-off, which is consistent with the larger aggregate turnout in the former (see Appendix Figure 2.7).[22]

Finally, we study heterogeneity depending on the share of district residents below the poverty line. The baseline estimate is statistically indistinguishable from zero, indicating a null response in a municipality with a poverty rate of zero. On the other hand, the estimated elasticity in a municipality with all voters below the poverty line is 0.037. These results indicate that poorer people, for whom a marginal fine increase represents a greater economic burden, are more responsive to a larger fine.[23]

21. Appendix Figure 2.11 provides separate estimates of the marginal effect of the fine for each individual election in 2011 and 2016. We find that the differences in the marginal effect of the fine (or the elasticity) across election types are statistically significant even within the same year.

22. A stronger intrinsic motivation to vote in the general election may result from (i) a stronger sense of civic duty in the first election of the cycle, (ii) greater interest in the outcome of the legislative election or (iii) the larger number of presidential candidates in the first round. Regarding the latter, Appendix Table 2.11 finds no evidence of a heterogeneous effect in the run-off depending on the first-round vote for the top two candidates. Voters may also face stronger pressure from local brokers in the general (legislative) election.

23. Appendix Figure 2.10 replicates the event-study analysis from Figure 2.2, disaggregating each fine

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

### *2.4.3 Enforcement of the Fine*

The correct interpretation of the marginal effect of the fine must take into account the probability of enforcement. We approximate the strength of enforcement using the share of fines that are settled (i.e., paid or excused). By July 2018, 38% of all fines issued between the subnational elections of 2006 and the 2016 run-off had been settled. Appendix Figure 2.8 shows that settled fines grew from 22% in 2011 to almost 50% in 2016. Although these figures are not negligible, sophisticated voters may realize that the expected fine is smaller than its nominal value. We are thus likely underestimating the marginal effect of a monetary incentive provided with certainty. In Appendix Table 2.13, we incorporate the probability of enforcement by adjusting the value of the fine by the share of fines settled in the current or previous election.[24] The effect of the fine on turnout increases 10-30% (i.e., 0.64 pp drop for a S/ 10 fall), while the estimated elasticity decreases by a similar amount.

The overall increase in settled fines seems to be related to improvements to enforcement that took place after 2012, including the creation of a collections unit within the JNE. There is no evidence linking the reform to the abstention fine with the toughening of enforcement nor implying that the fine categories were actively used for the latter. The positive effect of the fine on turnout in 2011, before the changes to enforcement took place, ensures that we

category into two same-sized groups based on the share of poor population in the district. The plot shows that the fall in turnout in low-fine districts is much larger in those with high poverty. The opposite pattern is observed in high-fine districts, though the difference is not as large. Moreover, turnover in medium-fine districts with low poverty is almost identical to that in medium-fine districts with high poverty, which is reassuring given that these two groups face the same monetary incentive to vote.

24. Naturally, the share of fines that are paid is itself affected by the value of the fine. Appendix Table 2.14 shows that a higher value of the fine leads to a lower share being paid, keeping enforcement constant (i.e., 2011). However, this decrease in fine repayment is offset by an equivalent increase in the share of fines excused, leading to a net zero effect on the share of fines settled. These findings constitute further evidence of voters' sophisticated response to changes in the monetary incentive to vote.

are not just picking up the effect of these changes. Still, the aggregate data shows that the increase in settled fines was mostly concentrated in high-fine districts (Appendix Figure 2.8), which suggests that our finding of a substantially larger effect in 2016 could be compromised. Appendix Table 2.15 shows results from further robustness tests based on information about the districts targeted by the collections unit and the variation in the share of fines settled.[25] All of our predictors of improved enforcement after 2012 are positively correlated with voter turnout in 2016. However, neither their inclusion as controls nor the exclusion of targeted districts causes the long-run effect of the value of the fine to fall by more than 20%.

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

### *2.4.4 Changes in Registration and the Propensity to Vote*

In this section we disaggregate the effect of the fine on turnout into separate effects on the number of votes (numerator) and the number of registered voters (denominator). As mentioned in section 2.2, all eligible voters (18 or older) are automatically registered to vote in the district corresponding to the home address reported in their DNI. Hence, even though registration is not a choice variable per se, voters can adjust their voting district by reporting a different address. Proof of address should be provided for such a change, but in practice this requirement is often waived and there is abundant anecdotal evidence of irregularities in the voter registry, though most of it is related to fraudulent registration for electoral gain.[26]

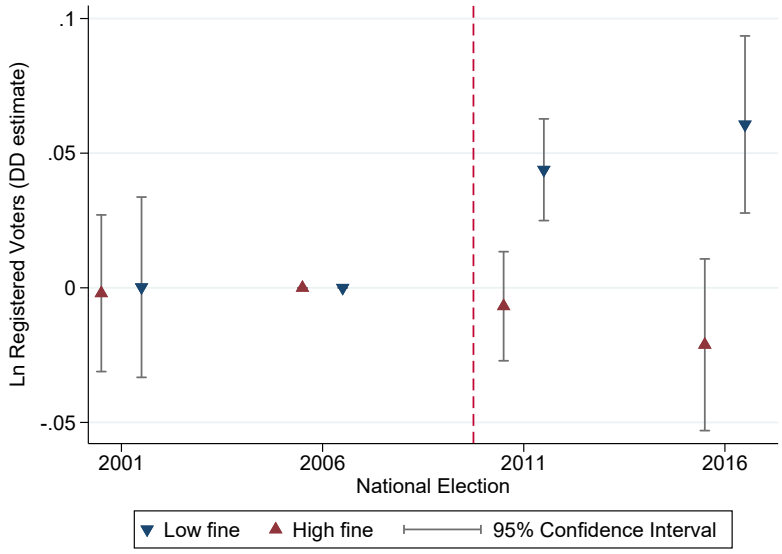
We begin by examining whether the reform led to higher registration in districts with

25. The vast majority of targeted districts are located in the provinces of Lima and Callao [JNE, 2015]. The collections unit also focused its attention on large cities and provincial capitals. We also consider a more agnostic, catch-all approach, in which we calculate for each district the change in the share of fines settled between 2006 and 2014. We then re-estimate the flexible version of equation (2.2) allowing for a time-varying effect and include the interaction of these variables with a dummy for 2016 as controls.

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

lower fines. We first estimate equation (2.1) using log registered voters as the dependent variable. In this case we only have one observation per district-cycle, since the voter registry remains unchanged between the general election and the presidential run-off. We set 2006 as the omitted election cycle and keep the medium-fine districts as the omitted category. Figure 2.3 shows the results. The difference in voter registration across categories remains remarkably stable between 2001 and 2006, lending support to our identification strategy. After the reform, we observe a disproportionate increase in the number of voters in low-fine districts. Specifically, voter registration grows 4.4% more in these districts than in medium-fine ones in 2011 and 6.1% more in 2016. The difference with high-fine districts is even starker, at 5% in 2011 and 8.2% in 2016 (see Appendix Table 2.10). High-fine districts show a decline in registered voters relative to medium-fine ones, but it is small and insignificant. A back-of-the-envelope calculation suggests that excess registration in low-fine districts amounts to 130,000 voters in 2016, equivalent to 0.5% of total registered voters.

**Figure 2.3:** The Reform to the Abstention Fine and Voter Registration




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

These results suggest that voters intentionally change their reported address to avoid paying a larger abstention fine. However, this interpretation seems unreasonable for most of the population, as DNI renewal requires a payment of S/22 and in-person visits to the office of the National Registry (Registro Nacional de Identificación y Estado Civil, RENIEC). This seems more plausible in the case of young adults that must apply for a new DNI and pay the respective fee when they turn eighteen. Young adults are also likely to be living with parents or relatives, making it easier to avoid providing a proof of address. Columns 2-7 in Table 2.3 estimate registration effects for six different age groups. Column 1 first shows an average elasticity of -0.045 for this sample (which excludes the year 2006 due to lack of data). The remaining columns show that the elasticity monotonically decreases with age and is only statistically significant for the 18-20 and 21-29 age groups. In particular, column 2 shows a registration elasticity of -0.28 for the 18-20 age-group, which is six times larger than the average. Appendix Table 2.16 further shows that the registration elasticity grows with proximity to districts with a lower fine.[27]

Our results show that young voters are strategically responding to the spatial variation in the fine for abstention by changing their reported address to low-fine districts. This type of voter misbehavior lacks the political motivation behind vote- or voter-buying [Nichter, 2008, Hidalgo and Nichter, 2016], but could affect youth representation [Bertocchi et al., 2017]. It could also increasingly hurt electoral participation through habit formation [Coppock and Green, 2016, Fujiwara et al., 2016]. These findings also illustrate the potential for unintended consequences when large-scale, targeted policies are implemented in settings with limited state capacity [Camacho and Conover, 2011, Cassan, 2015].

27. The previous results are robust to several tests. We use census data to show that the results are not confounded by predictable changes in the number of voters (e.g., differential birth rates) in previous decades (Appendix Table 2.17). We also use data on nighttime luminosity and a large household survey (ENAHO) to rule out changes in economic conditions or actual migration as the underlying mechanism (Appendix Table 2.18). We further verify that the results are not driven by differential changes in the share of population having a DNI (Appendix Table 2.19). Finally, it seems unlikely that the effect is driven by college students in low-fine districts updating their address [e.g., Braconnier et al., 2017], as (i) only 2% of 18-20 year-olds do not live with their parents, (ii) even fewer migrate for educational purposes, (iii) universities tend to be located in larger and richer cities.

**Table 2.3:** The Marginal Effect of the Abstention Fine on Voter Registration by Age

| | Dependent variable: ln Voters$_{i,t}$ | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | 18-20 | 21-29 | 30-35 | 36-50 | 51-75 | 75+ |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| ln Fine value$_{i,t}$ | -0.045** | -0.276*** | -0.055*** | -0.031 | -0.021 | -0.017 | -0.057 |
| | [0.019] | [0.043] | [0.020] | [0.022] | [0.020] | [0.024] | [0.051] |
| Observations | 5,076 | 5,076 | 5,076 | 5,076 | 5,076 | 5,076 | 5,076 |
| Districts | 1692 | 1692 | 1692 | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.001 | 0.03 | 0.001 | 0.0003 | 0.0002 | 0.0001 | 0.001 |
| Mean of dep. var | 10.71 | 7.996 | 9.279 | 8.770 | 9.431 | 9.191 | 7.318 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x '06 Category FE | Yes | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: ln Voters is the natural log of the number of registered voters for the election cycle. Sample includes the election years 2001, 2011 and 2016. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

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

$$\text{Turnout (T)} \equiv \frac{\text{Votes (V)}}{\text{Registered voters (R)}} \Rightarrow \ln T = \ln V - \ln R$$
$$\Rightarrow \frac{d \ln T}{d \ln F} = \frac{d \ln V}{d \ln F} - \frac{d \ln R}{d \ln F} \tag{2.3}$$

Table 2.4 provides estimates of the two marginal effects in the right-hand-side of equation (2.3). Column 1 shows that the elasticity of registration in the full sample (-0.046) is very similar to the estimate in Table 2.3 (-0.045), while column 2 shows that the elasticity increases from -0.035 in 2011 to -0.057 in 2016, indicating gradual adaptation to the policy. Columns 3 and 4 replicate the analysis using log votes as the dependent variable. The elasticity of votes (-0.016) is substantially smaller than that of voters, which means that the mechanical effect leading from fewer voters to fewer votes is being offset by the increasing propensity to

vote by those voters that do not change district in response to a larger fine. Column 4 shows that the behavioral response in the propensity to vote is growing as well: the reduction in the number of votes caused by an increase to the value of the fine is larger in 2011 (-0.024) than in 2016 (-0.009).

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

$$\begin{aligned} &\ln V = \ln V(\ln R(\ln F), \ln F) \\ \Rightarrow &\frac{d \ln V}{d \ln F} = \underbrace{\frac{\partial \ln V}{\partial \ln R}}_{\substack{\text{Turnout}| \\ \Delta\text{registration}}} \frac{d \ln R}{d \ln F} + \underbrace{\frac{\partial \ln V}{\partial \ln F}}_{\substack{\text{Behavioral} \\ \text{elasticity}}} \end{aligned} \tag{2.4}$$

The term on the left-hand side of equation (2.4) corresponds to the elasticity of votes with respect to the fine, which we have estimated in column 3 of Table 2.4. On the right-hand side, we have two unknowns (terms in braces) and the elasticity of registered voters with respect to the fine, for which we also have an estimate in column 1 of Table 2.4. The first unknown is the partial derivative (elasticity) of votes with respect to registration (i.e. the relative effect on votes of a 1% increase in the number of registered voters, all else equal). This is equivalent to voter turnout among those that change their address of registration, which we refer to as 'movers'. The other unknown is the partial derivative (elasticity) of votes with respect to the fine (i.e. the change in votes caused by a 1% increase in the value of the fine, all else equal). This is the purely behavioral response in the propensity to vote.

Based on different assumptions about voter turnout among movers, we can provide bounds on the behavioral elasticity of turnout. Equations (2.3) and (2.4) show that at the extremes this elasticity is bounded by the respective elasticities of votes (-0.016) and

**Table 2.4:** The Marginal Effect of the Abstention Fine on Voter Registration

| Dependent variable: | ln Voters$_{i,t}$ | | ln Votes$_{i,t}$ | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| ln Fine value$_{i,t}$ [a] | -0.046*** | -0.035*** | -0.016 | -0.024* |
| | [0.015] | [0.012] | [0.016] | [0.014] |
| ln Fine value$_{i,t}$ × $\mathbb{1}(2016)_t$ [b] | | -0.022*** | | 0.015* |
| | | [0.009] | | [0.009] |
| Observations | 6,768 | 6,768 | 13,536 | 13,536 |
| Districts | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.002 | 0.002 | 0.0002 | 0.0003 |
| Mean of dependent variable | 10.68 | 10.68 | 10.50 | 10.50 |
| p-value $H_0$: a+b=0 | | 0.002 | | 0.646 |
| District FE | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes |

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

turnout (0.03).[28] If movers vote at the average rate observed in 2011/16 of 83%, the resulting behavioral elasticity is 0.022, corresponding to 73% of the total elasticity of turnout. However, it seems likely that movers vote at lower-than-average rates. If their turnout is one, two or three standard deviation below the average (77%, 71% and 64% respectively), the behavioral elasticity in turn becomes 0.019, 0.016 and 0.013. Turnout of 64% is below the 5th percentile of the district-level distribution in 2011/16. Such turnout is consistent with the 20 pp gap for 18-20 year-old voters shown in section 2.6 below (i.e. those driving the registration effect). The resulting behavioral elasticity corresponds to 43% of the aggregate turnout elasticity, meaning that as much as 57% of the effect of the fine on turnout can

28. Intuitively, if movers never vote, their change of address will not affect the number of votes (i.e., the full and partial elasticities of votes in equation (2.4) are equal). If movers always vote, we can use the elasticities of votes and registration to back out the partial votes elasticity, which is then equal to the turnout elasticity.

plausibly be attributed to the change in registration by low-turnout voters.[29]

### *2.4.5 Previous Experimental Estimates, Adaptation and Voltage Drop*

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

To better understand the demand for information about the abstention fine, we use data on nationwide internet searches in the Google search engine. We construct a dataset on the popularity of 44 different search terms at the term-month level. Most of these terms are related to politics and three of them pertain directly to the abstention fine. Appendix 2.8.5 provides detailed information on the construction of the dataset. Using this data, we

29. Appendix Figure 2.12 shows the implied behavioral elasticity of turnout corresponding to different turnout rates for movers. Appendix Table 2.20 shows results from equation (2.2) controlling for log registered voters, a 'bad control' as defined by Angrist and Pischke [2009]. The results show that the turnout elasticity drops 20% relative to the baseline estimates when we control for voter registration.

implement the following difference-in-difference model:

$$\ln \text{Google trends index}_{i,m,y} = \theta_i + \omega_m + \sum_{\tau \geq 2006} \lambda_\tau \left[ \mathbb{1}(\text{fine-related})_i \times \mathbb{1}(\text{year} = \tau)_y \right] + \zeta_{i,m} \tag{2.5}$$

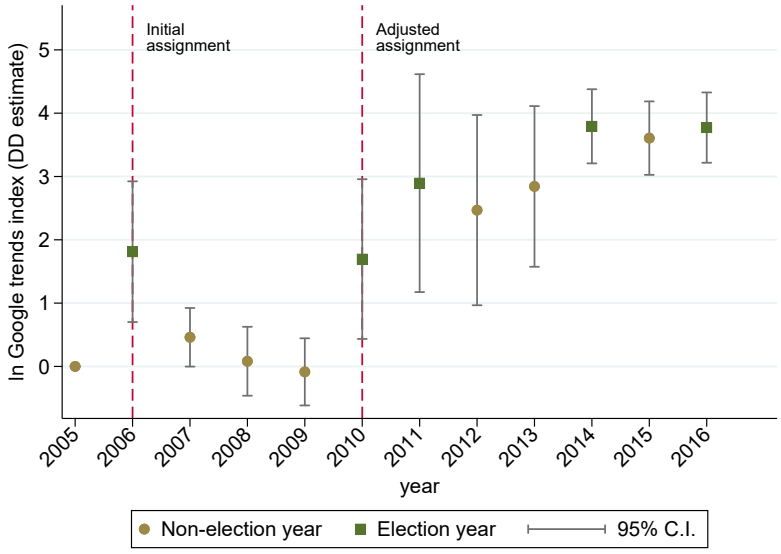
where the dependent variable is the natural log of one plus the Google Trends index for search term $i$ in month $m$ in year $y$. $\theta_i$ is a search-term fixed effect and $\omega_m$ is a month fixed effect. These fixed effects absorb persistent differences in popularity across search terms and common shocks to Google searches affecting all terms equally (e.g., improved internet access). The coefficients of interest, $\lambda_\tau$, tell us how the popularity of the search terms related to the fine vis-à-vis other terms changes relative to 2005, the omitted year.[30] $\zeta_{i,t}$ is an error term that we cluster two-way by search-term (44 clusters) and month (144 clusters).

Figure 2.4 plots the results. With the exception of 2006, a year with national and subnational elections, the popularity of fine-related search terms remains stable until 2009. In 2010, when districts were reassigned across fine categories and subnational elections took place, we observe again a rise in fine-related web searches. However, this increase in popularity continues to grow in the following years. These results suggest gradual learning about the reform and help explain our previous findings of larger long-run responses in turnout and registration. As informational frictions are further alleviated, we can expect the large-scale turnout elasticity to converge towards the experimental estimate in León [2017]. However, full convergence seems unlikely as it would require the elasticity to grow at the same yearly rate observed in 2016 for 22 more years (i.e., until 2038).[31] More generally, these results suggest that informational frictions contribute to imperfect compliance to large-scale policy incentives. Our findings highlight a limitation specific to interventions in political economy, namely that citizens may be plainly unaware about changes to the institutional environment.

30. Appendix Figure 2.13 shows equivalent results from the disaggregate specification at the monthly level.

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

**Figure 2.4:** The Reform to the Abstention Fine and Information Acquisition




Notes: The graph shows point estimates and 95% confidence intervals of a regression of the natural log of a search-term popularity index from Google trends on year dummies interacted with an indicator for search terms related to the fine for abstention. Regression includes search-term and 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.

## 2.5 Results: Electoral Outcomes and Political Attitudes

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

We first focus our attention on spoiled (i.e. blank or invalid) votes. This decision is partly driven by the volatile and personalistic nature of Peruvian politics, which prevents us from observing a comparable set of political parties both before and after the reform.[32] An additional theoretical justification is that in the absence of significant barriers to electoral participation, abstention is likely driven by the uninformed or uninterested, who are arguably more likely to spoil their vote [Feddersen and Pesendorfer, 1996, Hoffman et al., 2017, León,

32. Appendix 2.8.6 provides a brief overview of Peruvian politics during the sample period. We observe that individual politicians, rather than parties, are the main agents. Unfortunately, not all candidates participate in all elections and their platforms fluctuate substantially over time. Appendix Table 2.22 finds no effect of the fine on the vote share for the two leading candidates in the first round.

2017, Freire and Turgeon, 2020].

In Peru, a vote is considered blank if the ballot is deposited unmarked, while it is considered invalid if it has any mark other than a cross on the logo of one party or the picture of the respective candidate.[33] Blank and invalid votes are subtracted from the total number of votes before calculating candidate vote shares. We focus our attention on the first round of the presidential election for this part of the analysis, but Appendix Table 2.23 shows that the results are qualitatively similar if we include the run-off. Importantly, ballots lack a "none of the above" option, so voters that do not support any of the candidates will often intentionally spoil the ballot to avoid manipulation during counting. Hence, we treat blank and invalid votes as equivalent and aggregate them for the analysis.

Column 1 in Table 2.5 re-estimates equation (2.2) for turnout, excluding the presidential run-off. The estimated coefficient of 0.043 is only slightly smaller than the average effect reported in Table 2.1. Column 2 replicates the heterogeneity analysis across election cycles for this sub-sample, finding once more a substantially stronger effect in 2016. Column 3 shows results using the share of blank or invalid votes as the dependent variable. This share is defined relative to the number of registered voters, making the estimated effect directly comparable to the one for turnout reported in column 1. We find that a S/10 fine decrease causes a 0.37 pp decrease in the share of blank or invalid votes. Column 4 shows that the marginal effect of the fine is also increasing over time, jumping from -0.22 pp in 2011 to -0.48 pp in 2016 for a S/10 fine drop. Figure 2.5 shows the corresponding event study based on equation (2.1). We observe a systematic divergence in the share of blank or invalid votes across categories after the reform, mostly driven by high-fine districts.

These results suggest that the majority of voters that are brought to the polls by a marginally larger fine are not voting for any of the available candidates.[34] The 0.37 pp

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

34. This finding stands in contrast to the null result reported by Carpio et al. [2018]. The difference plausibly arises because of the different types of election under study (regional vs national), which may

**Table 2.5:** The Marginal Effect of the Abstention Fine on Invalid and Blank Votes

| Dependent variable: | Voter Turnout$_{i,t}$ | | Spoiled Votes $_{i,t}$ | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Fine value$_{i,t}$ (S/ x 100) [a] | 0.043*** | 0.017* | 0.037*** | 0.022*** |
| | [0.008] | [0.009] | [0.007] | [0.008] |
| Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ [b] | | 0.045*** | | 0.026*** |
| | | [0.005] | | [0.005] |
| Observations | 6,768 | 6,768 | 6,768 | 6,768 |
| Districts | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.0152 | 0.0236 | 0.0117 | 0.0146 |
| Mean of dependent variable | 0.851 | 0.851 | 0.122 | 0.122 |
| p-value $H_0$: a+b=0 | | 0.000 | | 0.000 |
| District FE | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes |

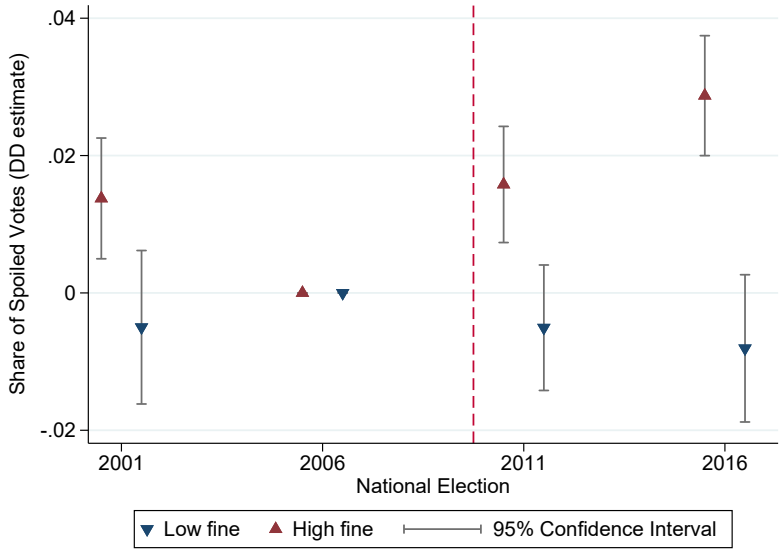
Notes: Dependent variable in columns 3-4 is the sum of blank and invalid votes, as a share of the number of registered voters. Sample includes first round of the presidential elections in 2001, 2006, 2011 and 2016. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

decrease in blank or invalid votes is equivalent to 86% of the 0.43 pp drop in turnout caused by a S/ 10 smaller fine. In other words, for every 10 extra votes caused by a higher fine, we observe 8.6 more votes that are blank or invalid, which is consistent with theoretical models of rational abstention [Feddersen and Pesendorfer, 1996]. These results could also indicate that marginal fine changes mostly affect poorer, less-educated voters, who are more prone to make a mistake [Fujiwara, 2015], but this possibility finds little support in the data.[35] It is also possible that a lower fine causes a drop in turnout by anti-establishment voters that do not support any of the candidates, but are neither uninformed nor uninterested [Ambrus et al., 2019, Ujhelyi et al., 2019]. Unfortunately, protest voters will often spoil the ballot, preventing us from using the distinction between blank and invalid votes to adjudicate among

affect clientelistic practices involving local brokers, or due to the local nature of the RDD estimates.

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

**Figure 2.5:** The Reform to the Abstention Fine and Blank or Invalid Votes




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

these interpretations. Anecdotal evidence shows growing discontent with the political system in recent years, lending support to the latter interpretation [The Washington Post, 2020].

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

### *2.5.1 Political Attitudes and Other Behaviors (Survey evidence)*

In this section, we study reported political attitudes and behaviors in Peru's National Household Survey (Encuesta Nacional de Hogares, ENAHO) for the period 2004-2016. Survey questions have changed substantially over time and not all are available before and after the reform to the abstention fine. We focus on the only four questions with sufficient coverage across survey waves. These questions concern (i) trust in political parties, (ii) party affiliation, (iii) information acquisition and (iv) interest in politics. For this part of the analysis, we run regressions at the individual-level, assigning the value of the fine to each respondent based on district of residence and year.[36] We cluster standard errors at the province level.

Table 2.6 shows the results. The dependent variable in column 1 is a dummy equal to one if the respondent reports sufficient or considerable trust in political parties. A S/10 fine decrease leads to a 0.2 pp increase in trust. However, the estimate is imprecise. In column 2, the dependent variable is a dummy equal to one if the respondent reports being a member of a political party or organization. In this case, the point estimate is close to zero and also insignificant. The dependent variable in column 3 is a dummy equal to one if the respondent reports being interested in politics. A S/10 fine decrease causes a significant 1.4 pp increase in this probability, equivalent to 2.7% of the sample mean. This result is consistent with the idea that a stronger extrinsic incentive to vote may crowd out the intrinsic motivation [Bénabou and Tirole, 2003, 2006]. Finally, the dependent variable in column 4 is a dummy equal to one if the respondent reports being frequently informed about political affairs. The point estimate indicates that a S/10 fine decrease leads to a 0.7 pp increase in this probability, but this estimate is also imprecise.

Taken together, the survey evidence suggests that a larger fine decreases political engagement: reduced interest, lower trust in parties and reduced acquisition of information (though the latter two are imprecisely estimated). These findings suggest that the previous results

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

**Table 2.6:** The Marginal Effect of the Abstention Fine on Political Attitudes and Behaviors

| Dependent variable: | Trust in parties | Party membership | Interest in politics | Information acquisition |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Fine value$_{i,t}$ (S/ x 100) | -0.023 | 0.003 | -0.141** | -0.065 |
| | [0.020] | [0.005] | [0.060] | [0.056] |
| Observations | 281,841 | 281,841 | 136,872 | 136,872 |
| Districts | 1459 | 1459 | 1221 | 1221 |
| R-squared | 0.001 | 0.002 | 0.044 | 0.083 |
| Mean of dependent variable | 0.059 | 0.006 | 0.510 | 0.384 |
| Survey years: | 2004-2016 | 2004-2016 | 2004-2011 | 2004-2011 |
| District FE | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes |
| Individual controls | Yes | Yes | Yes | Yes |

Notes: Dependent variable in the header. Source is ENAHO survey. Not all questions are asked every year. All outcomes are binary. In column 1, "Trusts parties" takes the value of 1 if the respondent reports "sufficient" or "considerable" trust in political parties; In column 2, "Party affiliate" takes the value of 1 if the respondent reports belonging to a political group or party; In column 3, "Interested in politics" takes the value of 1 if the respondent reports being interested in politics, rather than being indifferent or repelled; ; In column 4, "Frequently informed" takes the value of 1 if the respondent reports frequently being informed about political affairs, rather than occasionally, depending on the topic, or never. Individual controls include dummies for gender, primary and secondary education, and age. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns are weighted by the survey expansion factor. Standard errors clustered by province *** p<0.01, ** p<0.05, * p<0.1

on spoiled votes may be partly driven by endogenous changes in political engagement and not purely by selection into voting by the ex-ante uninformed or uninterested.

## 2.6 The Aggregate Effect of Compulsory Voting

We now turn to study the contribution of the monetary incentive provided by the abstention fine relative to the aggregate effect of compulsory voting on turnout. Underlying this exercise is the idea that compulsory voting provides both monetary and non-monetary incentives. The latter include the expressive value of the law as a signalling device for socially-desirable behaviors and the non-pecuniary penalty (i.e. restrictions on government services) imposed

on non-voters [Funk, 2007, Cepaluni and Hidalgo, 2016]. Previous work provides several estimates of the effect of compulsory voting on turnout, but comparing these with our estimated elasticity would require somewhat strong assumptions about the external validity of findings from other settings. Hence, we would like to benchmark the elasticity against an estimate of the aggregate effect of compulsory voting within our setting.

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

Our estimate of the aggregate effect of compulsory voting exploits variation between individuals of different ages in the exposure to the mandate to vote. Voting is mandatory for citizens between the ages of 18 and 69 (both inclusive). Our identifying assumption is that voters with ages slightly above 69 are essentially identical to 69 year-old voters, except for the fact that the latter are subject to compulsory voting while the former are not. Using information on the age of every single registered voter at each voting booth for the 2016 elections, we calculate the booth-specific shares of registered voters with each possible age, ranging from 16 to 122.[37] Our empirical strategy involves comparing voter turnout in booths with varying shares of 'almost-exempt' 69-year-old voters and 'barely-exempt' 70-plus voters, exploiting idiosyncratic variation across the threshold. To ensure that we are not capturing other differences correlated with the age composition of the electorate, our regression flexibly controls for the share of registered voters belonging to every other age group.[38] We also include district or polling station fixed effects. Ultimately, the richness

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

38. Appendix Figure 2.14 shows the share of booths without any voters for each one-year age group between 18 and 80. Less than 10% of booths fail to have a voter for each age up to 62. Slightly more than 20% of

of the data allows us to compare voting booths in the same location, and that look almost identical in terms of the age composition of the registered voters, except for the fact that they have different shares of voters with ages 69 or slightly above. Pooling data from the 2016 general and run-off elections, we estimate the following specification:

$$\text{turnout}_{b,d,e} = \alpha_d + \gamma_e + \sum_{\tau \in \{16,\ldots,122\} \setminus \{69\}} \lambda_\tau \ \text{share}(\text{age} = \tau)_b + \epsilon_{b,d,e} \tag{2.6}$$

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

The most conservative estimate of the aggregate effect of compulsory voting, very close in spirit to a regression discontinuity design [e.g., Jaitman, 2013, Cepaluni and Hidalgo, 2016], relies exclusively on a comparison of 70-year-old voters to 69-year-olds (i.e., $\lambda_{70}$). However, the observed difference in turnout between ages 69 and 70 may fail to fully capture the aggregate effect of compulsory voting if people adapt slowly to the exemption as a result of limited information or the force of habit. Hence, it seems desirable to compare turnout among the 69 year-olds to other nearby age groups not far from the threshold (i.e., up to ages 72 or 75). However, a decrease in voter turnout several years after age 69 could also be a

booths fail to have a voter aged 70. This number steadily rises to 30% by age 75 and to 40% by age 80.

reflection of a worsening of health or limited mobility. To have a benchmark for the 'natural' rate of decline in electoral participation with age, we use individual-level data on voter turnout in the 2017 presidential election in Chile, a neighboring country without compulsory voting since 2012. We estimate the individual-level equivalent of equation (2.6) for Chile, with district and run-off fixed effects, with a dummy for voting as dependent variable.

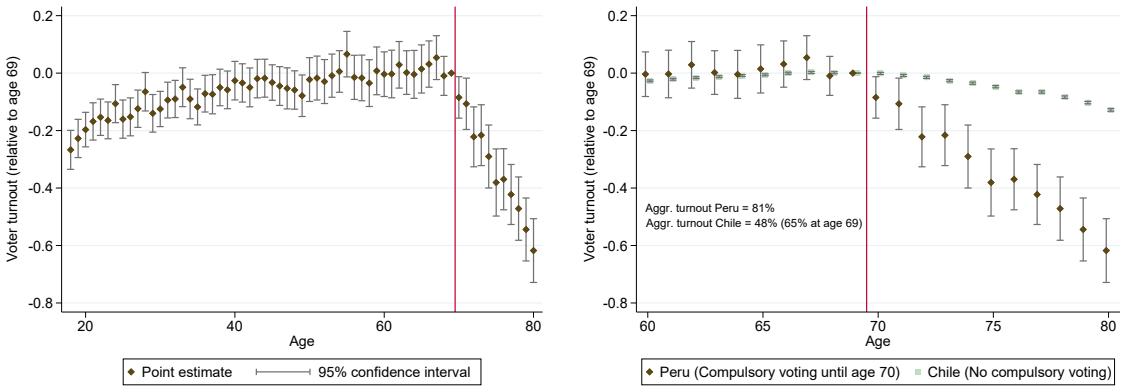
Panel (a) in Figure 2.6 shows the estimates of equation (2.6) for ages 20-80. Three patterns emerge. First, voter turnout is roughly constant between the ages of 40 and 69. This suggests that compulsory voting is effectively offsetting any differential propensity to vote across this large set of ages. Second, turnout steadily drops for those under 40 and is 20 pp lower at age 20 than at age 69. This indicates that electoral abstention is coming predominantly from younger voters, consistently with our finding regarding these same voters as the ones who appear to be manipulating their registered address to avoid paying a larger fine. Third, there is a dramatic decline in electoral participation in the years immediately after age 69, unlike any other fluctuation we observe throughout the age distribution. Turnout falls by 8 pp at age 70, 22 pp at age 72 and 38 pp at age 75. Effects of this magnitude are comparable to previous findings by Fowler [2013], Jaitman [2013] and Bechtel et al. [2018] on the impact of compulsory voting in Australia, Argentina and Switzerland.

Given our previous findings on slow adaptation to marginal fine changes, it seems plausible that the drop in turnout between ages 69 and 75 reflects a similarly gradual response to the exemption from compulsory voting. To address concerns about the confounding effect of other factors affecting the elderly, panel (b) plots the results for ages 60 through 80 for Peru and Chile.[39] We fail to observe any systematic difference in electoral participation between the ages of 60 and 69 in either country. But while voter turnout plummets in Peru after age 69, there is only a smooth decline in Chile, amounting to no more than a five percentage-point drop by age 75. Hence, it appears that we can attribute most of the sharp decline in

39. Appendix Figure 2.15 shows a smooth density of the age distribution around the cut-off for both countries.

turnout observed in Peru between ages 69 and 75 to the exemption from compulsory voting.

**Figure 2.6:** Senior Exemption from Compulsory Voting and Voter Turnout




(a) Peru: Ages 18-80

(b) Peru vs Chile: Ages 60-80

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

Columns 1-5 in Table 2.7 examine the robustness of these results. To facilitate interpretation, we re-estimate equation (2.6) aggregating the shares of registered voters with ages between 70 and 75. Column 1 corresponds to our baseline specification and shows that turnout drops 21 percentage points (pp) on average between these ages, relative to age 69. In columns 2 and 3 we see that this coefficient is hardly affected if we restrict the sample to the more homogeneous set of voting booths with close to 300 voters or if we substitute the district fixed effects with the more stringent polling-station fixed effects. Column 4 shows results from a more conservative specification aggregating the shares of voters with ages 70-72 instead. Consistent with Figure 2.6, the turnout effect of the exemption from compulsory voting decreases, but remains substantial at 13.4 pp. Column 5 shows results from another modified specification in which we weight the share of voters with each age between 70 and

75 by the number of elections without compulsory voting to which they have been exposed.[40] A one-unit increase in this new variable is equivalent to having all registered voters in that booth exposed to one additional election without compulsory voting. The results indicate that each additional election with the exemption leads to a 12.9 pp drop in the probability of voting, which is again consistent with increased adaptation.

**Table 2.7:** Senior Exemption from Compulsory Voting and Voter Turnout

| | Dependent variable: Turnout$_i$ (Mean: 0.82) | | | | | |
|---|---|---|---|---|---|---|
| | Baseline | 280-300 voters | Polling station FE | Share 70-72 | Elections w/o CV | Election type |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Share ages 70-75$_i$ [a] | -0.209*** | -0.182*** | -0.215*** | | | -0.174*** |
| | [0.041] | [0.034] | [0.028] | | | [0.043] |
| Share ages 70-72$_i$ | | | | -0.134*** | | |
| | | | | [0.037] | | |
| $\sum_{j=70}^{75}$ Share age j $\times$ elections exempt$_j$ | | | | | -0.129*** | |
| | | | | | [0.020] | |
| Share ages 70-75$_i$ $\times \mathbb{1}$(Run-Off)$_t$ [b] | | | | | | -0.069*** |
| | | | | | | [0.017] |
| Observations | 148,448 | 109,462 | 148,448 | 148,448 | 148,448 | 148,448 |
| Districts | 1854 | 1109 | 1854 | 1854 | 1854 | 1854 |
| Polling stations | 4723 | 2725 | 4723 | 4723 | 4723 | 4723 |
| R-squared | 0.19 | 0.16 | 0.08 | 0.19 | 0.19 | 0.19 |
| p-value a+b=0 | | | | | | 0.000 |
| District FE | Yes | Yes | No | Yes | Yes | Yes |
| Polling station FE | No | No | Yes | No | No | No |
| Election type FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Share by age | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: Data at the voting-booth level for the national elections of 2016 (General and Presidential Run-Off). The regressor of interest is the share of registered voters with ages slightly above the cut-off for exemption from compulsory voting (CV): 70-75 in all columns except column 4, where it is 70-72. Regressions include age-specific shares of registered voters for all other ages between 16 and 122. The omitted category is age 69. All columns include district fixed effects, except column 3 which includes polling-station fixed effects. All regressions are weighted by the number of registered voters per booth. Column 2 shows results using only booths with 280-300 voters. The regressor of interest in column 5 is the sum of registered voters for each age between 70 and 75 multiplied by the number of elections without compulsory voting that the cohort has been exposed to. Column 6 shows heterogeneous effects for the presidential run-off. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

In column 6 we study heterogeneous effects by type of election by including an additional interaction with a dummy for the presidential run-off. The results indicate that voter turnout drops 17.4 pp in the general election (the omitted category) and a further 6.9 pp in the run-off.

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

Taken together with the also heterogeneous effect of marginal fine changes reported above, this finding indicates that voters are more responsive to both the intensive and extensive margins of compulsory voting in the presidential run-off.

### *2.6.1 Assessing the Importance of the Abstention Fine*

We now use our estimate of the aggregate effect of compulsory voting to do a back-of-the-envelope calculation and benchmark the estimated effect of the abstention fine. To ensure comparability, we employ in this calculation our elasticity estimates for 2016. The point estimate in column 4 of Table 2.2 shows that a complete elimination of the fine (100% reduction) would lead to a 4.8% fall in turnout, equivalent to a 3.9 pp drop from the observed 2016 turnout rate of 0.82. A reduction in turnout of this size is equivalent to 19% of the average fall in turnout between ages 69 and 75 shown in column 1 of Table 2.7.[41] The effect of a full fine reduction on voter turnout pales in comparison to that of an exemption from compulsory voting. Policy-wise, this result suggests that countries may be able to extract most of the gains from compulsory voting at a low administrative and distributional cost by only setting a moderate monetary sanction for non-compliance.

This comparison warrants several comments. First, it is likely that we are underestimating the effect of the exemption from compulsory voting. One reason is that voters slightly above the age of 70 are plausibly still affected by their exposure to mandatory voting (i.e. expressive function of the law, voting habits), which dampens the effect of the exemption. Moreover, voters around the age of 70 tend to vote at the highest rates in all kinds of settings, suggesting that they are more intrinsically-motivated to vote than younger voters. Voter turnout peaks around age 70 in our data for Peru and Chile (Appendix Figure 2.16). A similar pattern has also been documented for the US [Franklin, 2018].[42]

41. Also equivalent to 46% of the drop at age 70, 18% of the drop at age 72 and 10% of the drop at age 75.

42. Based on the average age of voters per district, Appendix Table 2.12 suggests that older voters are in fact less responsive to a larger fine (i.e. we overestimate the elasticity for the 70+ age-group).

Second, our comparison assumes a constant elasticity, while the turnout response to a change in the fine may vary at very low levels. For instance, any non-zero fine forces people to also incur the non-pecuniary costs (i.e. restrictions in government services). Even though the reform we study generates large differences in the value of the fine across categories, it might be better to think of our comparison as pertaining to very large (i.e. 75% reduction between high- and low-fine districts), but perhaps not complete fine reductions. Such a comparison is still highly policy relevant. Having said this, our elasticity may in fact be overestimating the effect of a full fine reduction if the elimination of the monetary incentive crowds in the intrinsic motivation to vote [Bénabou and Tirole, 2006]. Also, the elasticity we use is based on the nominal value of the fine and is less conservative than the one that accounts for enforcement (Appendix Table 2.13).

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

## 2.7 Concluding Remarks

In this paper we study voters' response to marginal changes to the value of the fine for electoral abstention in Peru, exploiting a nationwide policy reform that affected districts differentially. We provide three main findings. First, a lower fine has a robust negative effect on voter turnout. This effect is partly driven by irregular changes in registration, as spatial variation in the value of the fine causes young voters to increasingly register in districts with lower fines. Second, the reduction in turnout spurred by a lower fine appears to have little impact on representation, since it is largely matched by reductions in the number of blank

or invalid votes. Third, results from a second natural experiment provided by an age-based exemption from compulsory voting suggest that monetary incentives are not the main driver behind the effectiveness of compulsory voting.

Our findings have important policy implications. Monetary incentives to vote are a rarely used alternative available to governments across the globe. Our results show that marginal changes to these incentives have a robust and non-negligible effect on voter turnout, though additional research is needed on the potentially unequal responses to rewards and penalties. Our findings also show that voters respond in a rich, multi-dimensional way. Policy-makers must be cautious about the unintended consequences of targeted policies. Furthermore, our results illustrate various limitations to the external validity of the findings from small-scale field experiments testing voter mobilization initiatives for large-scale policy implementation.

Our results also show that the omnibus bundle of incentives provided by compulsory voting is much more effective at increasing voter turnout than even large changes to the value of the abstention fine. Thus, making voting mandatory is a policy option worth considering if the aim is to maximize turnout. Importantly, the fine used to enforce compulsory voting can be set at moderate values without largely undermining the effectiveness of the system, while reducing the burden that this sanction imposes on non-voters, especially the poor.[43] At the same time, our findings also speak to the broader motivations for increasing voter turnout. One such objective is to ensure appropriate representation of all citizens [Lijphart, 1997]. In this regard, our finding of an almost one-to-one increase in blank and invalid votes with the additional votes generated by a marginal fine increase indicates that gains in turnout achieved through extrinsic incentives may fail to affect representation or downstream policy outcomes. A larger extrinsic incentive also seems to partially crowd out the intrinsic incentive to engage with political affairs. However, one has to be cautious about extending this conclusion to settings in which large shares of the population face significant barriers to

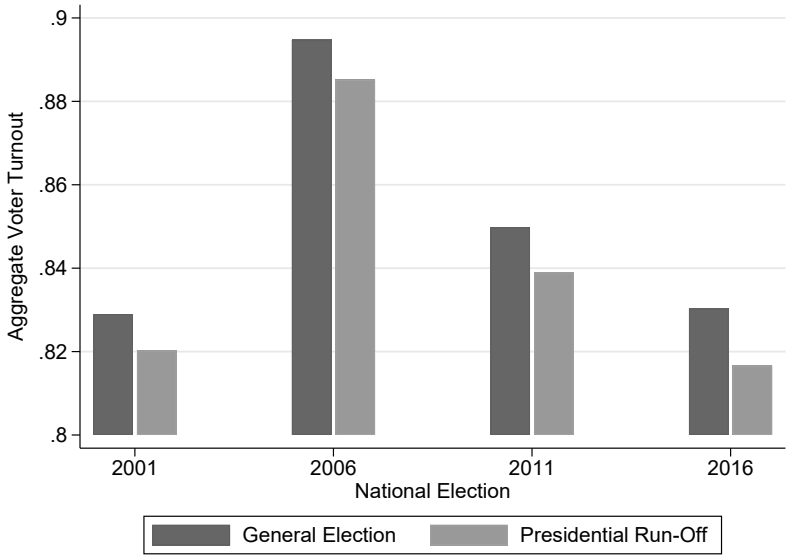
43. Lower fines mechanically reduce revenue for the agency collecting them and can affect its operative capacity. In 2017, 55% of JNE's revenue (2.7 million USD) came from the collection of electoral fines. However, lower fines are unlikely to have large effects on public finance or government spending.

electoral participation.

## 2.8 Appendix

### *2.8.1 Additional Background Information*

**Figure 2.7:** Voter Turnout in National Elections



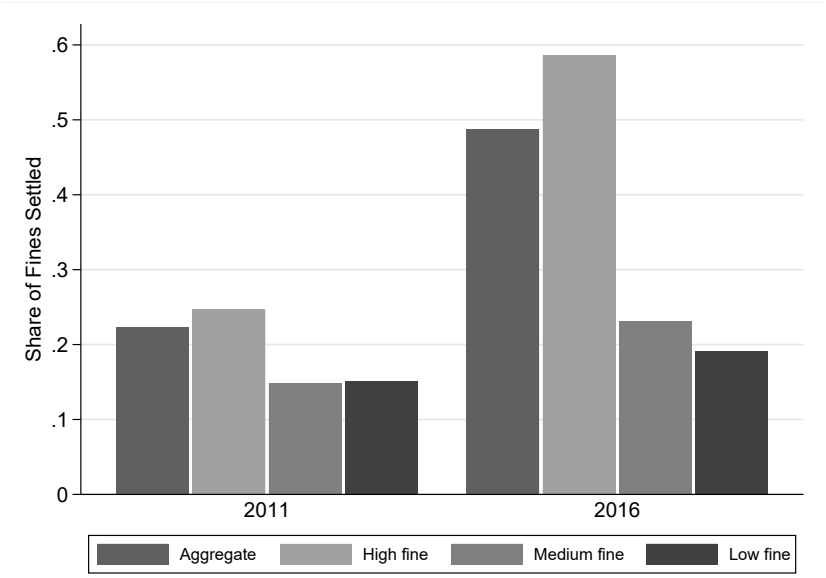

Note: Figure shows aggregate voter turnout for each national election in Peru between 2001 and 2016. The general election includes the first round of the presidential election and the legislative election.

**Table 2.8:** Assignment of Districts to Poverty Categories in 2006 and 2010

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

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

**Figure 2.8:** Share of Fines Settled for the 2011 and 2016 Elections




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

### *2.8.2 Voter Turnout: Additional Results*

**Table 2.9:** Main Results Clustering by Region (Wild Cluster Bootstrap)

| Dependent variable: | Voter Turnout$_{i,t}$ | | | |
|---|---|---|---|---|
| | level | log | ln Voters$_{i,t}$ | Spoiled Votes$_{i,t}$ |
| | (1) | (2) | (3) | (4) |
| Fine value$_{i,t}$ (S/ x 100) | 0.049*** | | | 0.037*** |
| | [0.007] | | | [0.007] |
| | (0.000) | | | (0.001) |
| ln Fine value$_{i,t}$ | | 0.030*** | -0.046*** | |
| | | [0.005] | [0.015] | |
| | | (0.000) | (0.015) | |
| Observations | 13,536 | 13,536 | 6,768 | 6,768 |
| Districts | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.0180 | 0.0180 | 0.00161 | 0.0117 |
| Mean of dependent variable | 0.845 | -0.171 | 10.68 | 0.122 |
| District FE | Yes | Yes | Yes | Yes |
| Election x Province x '06 Category FE | Yes | Yes | Yes | Yes |

Notes: ln Voters is the natural log of the number of registered voters for the election cycle (same for general and run-off elections). Blank votes and invalid votes in columns 4-5 are measured as shares of the number of registered voters. Columns 1-2 use data from national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. Columns 3-4 use data from the first round of the presidential elections in 2001, 2006, 2011 and 2016. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns are weighted by the number of registered voters in 2001. Standard errors clustered by region in brackets (25 units). Cluster-robust wild-bootstrap p-value in parentheses. *** p<0.01, ** p<0.05, * p<0.1

**Table 2.10:** Event-study estimates of the effect of the reform on turnout and registration

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

Notes: Column 1 corresponds to Figure 2.2 in the text, while column 2 corresponds to Figure 2.3. In column 1, the dependent variable is turnout and the omitted election is the 2006 presidential run-off. In column 2, the dependent variable is the natural log of the number of registered voters and the omitted election cycle is 2006. Voter registration is constant within an election cycle (i.e. general election and run-off). Regressions include district and province-election-category fixed effects (using 2006 classification). Observations are weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 clusters). *** p<0.01, ** p<0.05, * p<0.1

**Table 2.11:** The Marginal Effect of the Abstention Fine on Voter Turnout in Run-Off elections

| | Dependent variable: Turnout$_{i,t}$ (Mean: 0.84) | | | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Fine value$_{i,t}$ (S/ x 100) | 0.055*** | | 0.055*** | 0.054*** |
| | [0.009] | | [0.009] | [0.014] |
| Vote share of run-off candidates$_{i,t-1}$ | | 0.003 | 0.003 | 0.001 |
| | | [0.014] | [0.013] | [0.036] |
| Vote share of run-off candidates$_{i,t-1}$ × Fine value$_{i,t}$ | | | | 0.002 |
| | | | | [0.026] |
| Observations | 6,768 | 6,768 | 6,768 | 6,768 |
| Districts | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.0233 | 0.000138 | 0.0235 | 0.0235 |
| District FE | Yes | Yes | Yes | Yes |
| Election-Province-Category '06 FE | Yes | Yes | Yes | Yes |

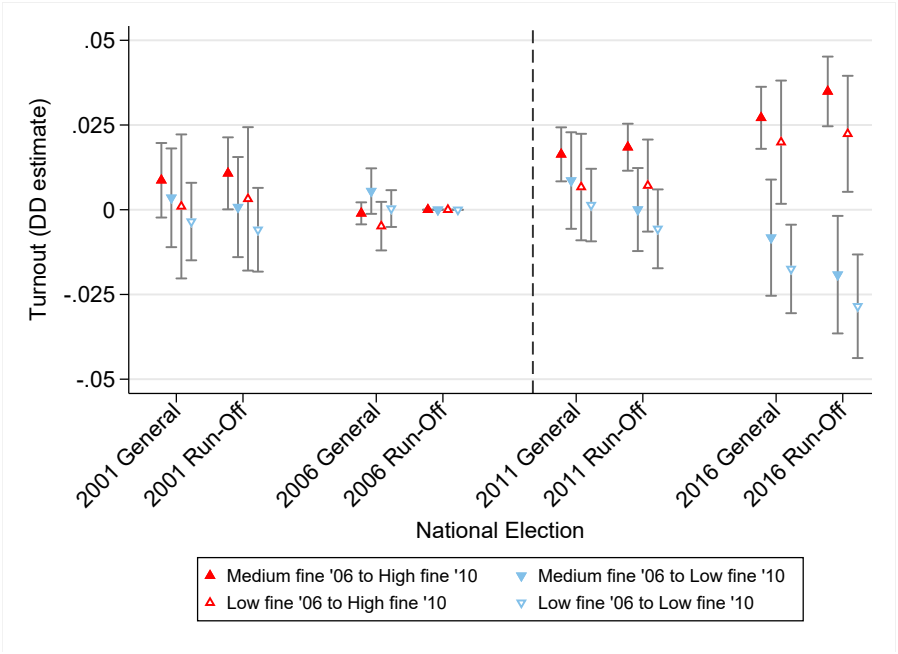
Notes: Dependent variable is voter turnout (0-1). Vote share of run-off candidates$_{i,t-1}$ is the sum of the vote shares in the first round of the presidential election for the two candidates that progressed to the run-off (top two candidates in the aggregate). All regressions only use data from presidential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

**Table 2.12:** Heterogeneous effects by Age of the Electorate

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

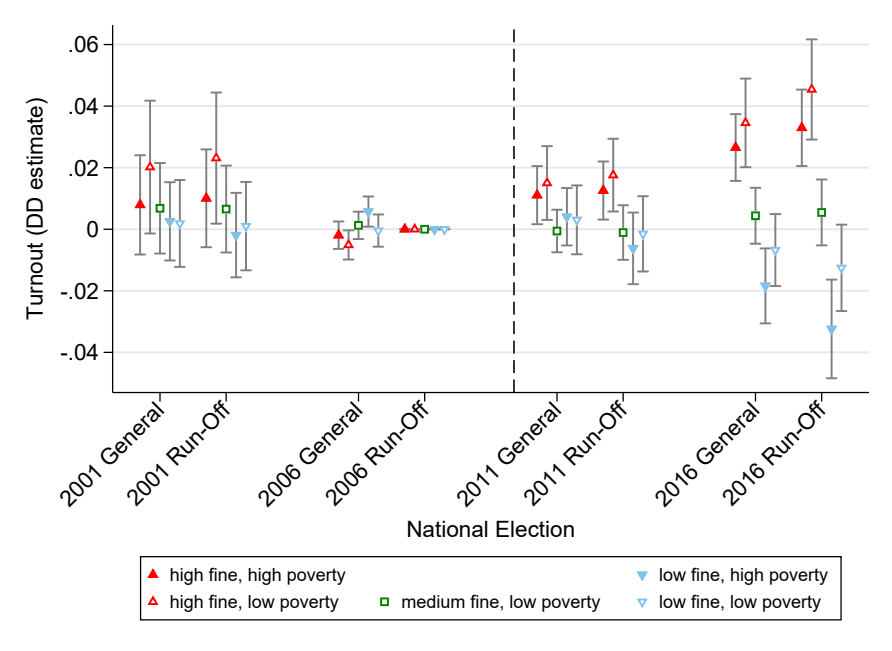
Notes: All regressions use data from national elections in 2001, 2006, 2011 and 2016. Columns 1 and 2 include the interaction of the fine with the average age of registered voters in 2016. Columns 3 and 4 include similar interactions for the top two terciles of the age distribution. Columns 2 and 4 include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

**Figure 2.9:** The Reform to the Abstention Fine and Turnout by 2006 Poverty Category



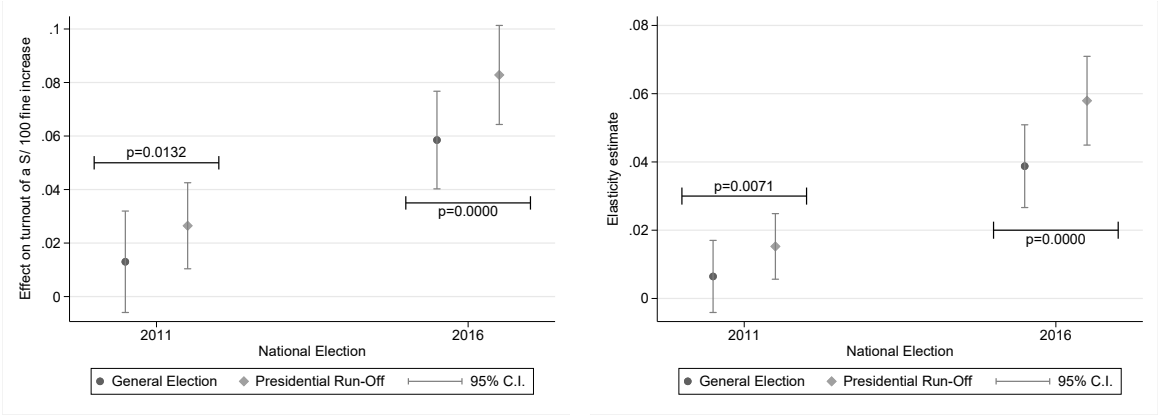

Notes: The graph shows point estimates and 95% confidence intervals of a regression of district-level turnout on a full set of election dummies interacted with dummies for each combination of poverty categories in 2006 and 2010. All districts classified as high fine in 2006, remained in that category in 2010 and are absorbed by the time fixed effects. There is one omitted combination for each of the remaining 2006 poverty categories (medium fine and low fine), which corresponds in both cases to districts classified as medium fine in 2010. The omitted election is the 2006 presidential run-off. Regression includes district and province-election-category fixed effects (using 2006 classification). Regression includes 13,536 observations from 1,692 districts. Districts are weighted by the number of registered voters for the 2001 elections. Standard errors are clustered by province (192 clusters). The dotted line corresponds to October 2010, when districts were re-classified with regards to the abstention fine.

**Figure 2.10:** The Reform to the Abstention Fine and Turnout: Heterogeneous Effects by Share of Poor




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 fine category and level of poverty. Poverty levels are defined as above or below the median for the respective category. These medians are 40.5%, 70.7% and 82.2% in high-, medium- and low-fine districts, respectively. The omitted category includes districts assigned a medium fine (poor) in 2010 and having high poverty (i.e. above median). The omitted election is the 2006 presidential run-off. Regression includes district and province x election x 2006-category fixed effects. Regression includes 13,536 observations from 1,692 districts. Districts are weighted by the number of registered voters in 2001. Standard errors are clustered by province (192 clusters). The dashed line indicates the date of adjusted district assignment (October 2010).

**Figure 2.11:** Election-specific Estimates of the Marginal Effect of the Abstention Fine on Turnout and the Elasticity




(a) Level effect

(b) Elasticity

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

### *2.8.3 Enforcement: Additional Results*

**Table 2.13:** Effects of Expected Fine on Turnout

| Dependent variable: | Turnout$_{i,t}$ (Mean=0.845) | | | ln Turnout$_{i,t}$ (Mean=-0.171) | | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| (ln) Expected Fine value$_{i,t}$ (S/ x 100) | 0.054*** | 0.058*** | 0.064*** | 0.023*** | 0.028*** | 0.022*** |
| | [0.012] | [0.015] | [0.011] | [0.004] | [0.004] | [0.003] |
| Observations | 12,848 | 12,848 | 12,848 | 12,848 | 12,848 | 12,848 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election-Province-Category '06 FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Enforcement 2001 & 2006 | 06/10 avg. | | 06 | 06/10 avg. | | 06 |
| Enforcement 2011 | 11 | 10 | 10 | 11 | 10 | 10 |
| Enforcement 2016 | 16 | 14 | 14 | 16 | 14 | 14 |

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

**Table 2.14:** The Marginal Effect of the Abstention Fine on Settlement of Outstanding Fines

| Dependent variable: Share of fines | $\text{Settled}_{i,t}$ | | $\text{Paid}_{i,t}$ | | $\text{Excused}_{i,t}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| $\text{Fine value}_{i,t}$ (S/ x 100) [a] | 0.231*** | -0.003 | -0.020*** | -0.026*** | 0.252*** | 0.023*** |
| | [0.024] | [0.008] | [0.007] | [0.007] | [0.021] | [0.005] |
| $\text{Fine value}_{i,t} \times \mathbb{1}(2014/16)_t$ [b] | | 0.353*** | | 0.009 | | 0.344*** |
| | | [0.031] | | [0.007] | | [0.031] |
| Observations | 11,727 | 11,727 | 11,727 | 11,727 | 11,727 | 11,727 |
| Districts | 1692 | 1692 | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.03 | 0.13 | 0.001 | 0.001 | 0.03 | 0.13 |
| Mean of dependent variable | 0.37 | 0.37 | 0.20 | 0.20 | 0.17 | 0.17 |
| p-value $H_0$: a+b=0 | | 0.000 | | 0.036 | | 0.000 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x '06 Category FE | Yes | Yes | Yes | Yes | Yes | Yes |

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

**Table 2.15:** Improved Enforcement and the Long-run Effect of the Fine on Turnout

| | Dependent variable: Turnout$_{i,t}$ | | | | | |
|---|---|---|---|---|---|---|
| | Targeted districts | Drop Lima & Callao | Province capitals | Drop capitals | $\Delta$ fines settled | All |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fine value$_{i,t}$ (S/ x 100) | 0.020** | 0.017** | 0.020** | 0.018** | 0.020** | 0.020** |
| | [0.008] | [0.009] | [0.008] | [0.009] | [0.008] | [0.008] |
| Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ | 0.051*** | 0.051*** | 0.046*** | 0.051*** | 0.041*** | 0.037*** |
| | [0.005] | [0.005] | [0.005] | [0.005] | [0.006] | [0.006] |
| $\mathbb{1}$(Targeted District)$_i$ $\times$ $\mathbb{1}(2016)_t$ | 0.006 | | | | | 0.011** |
| | [0.004] | | | | | [0.005] |
| $\mathbb{1}$(Province capital)$_i$ $\times$ $\mathbb{1}(2016)_t$ | | | 0.008*** | | | 0.008*** |
| | | | [0.002] | | | [0.002] |
| $\Delta$ fines settled$_i$ $\times$ $\mathbb{1}(2016)_t$ | | | | | 0.024*** | 0.021*** |
| | | | | | [0.008] | [0.007] |
| Observations | 13,515 | 12,221 | 13,515 | 12,038 | 13,375 | 13,375 |
| Districts | 1,692 | 1,530 | 1,692 | 1,506 | 1,692 | 1,692 |
| R-squared | 0.03 | 0.03 | 0.04 | 0.03 | 0.03 | 0.04 |
| Mean of dependent variable | 0.85 | 0.83 | 0.85 | 0.85 | 0.85 | 0.85 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes | Yes | Yes |

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

### *2.8.4 Voter Registration: Additional Results*

**Table 2.16:** The Value of the Abstention Fine and Voter Registration: Heterogeneous Effects by Distance to Districts with Lower Fine

| Dependent variable: | ln Voters$_{i,t}$ | |
|---|---|---|
| | (1) | (2) |
| ln Fine value$_{i,t}$ [a] | -0.047*** | |
| | [0.015] | |
| ln Fine value$_{i,t}$ × Distance to district w/ lower fine$_i$ [b] | 0.001* | |
| | [0.000] | |
| ln Fine value$_{i,t}$ × 𝟙(Distance to district w/ lower fine: 0-5km)$_i$ | | -0.040** |
| | | [0.016] |
| ln Fine value$_{i,t}$ × 𝟙(Distance to district w/ lower fine: 5-10km)$_i$ | | -0.005 |
| | | [0.032] |
| ln Fine value$_{i,t}$ × 𝟙(Distance to district w/ lower fine: >10km)$_i$ | | -0.026 |
| | | [0.024] |
| Observations | 6,768 | 6,768 |
| Districts | 1692 | 1692 |
| R-squared | 0.003 | 0.002 |
| District FE | Yes | Yes |
| Election-Province-Category '06 FE | Yes | Yes |
| p-value $H_0$: a+b=0 | 0.002 | |
| Mean of dep. var | 10.68 | 10.68 |

Notes: ln Voters is the natural log of the number of registered voters for the election cycle (same for general and run-off elections). Sample includes national elections (general and presidential run-off) in 2001, 2006, 2011 and 2016. Column 1 includes the interaction of the fine with the distance between the district and the nearest district with a lower fine. Column 2 includes the interaction of the fine with a set of dummies that take the value of 1 if the nearest district with a lower fine is located less than 5 km away, between 5-10 km, and more than 10 km away. Regressions are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

**Table 2.17:** The Value of the Abstention Fine and Age-specific Voter Registration, controlling for predicted voters

| | Dependent variable: ln Voters in age-group$_{i,t}$ | | | | | |
|---|---|---|---|---|---|---|
| | 18-20 | 21-29 | 30-35 | 36-50 | 51-75 | 75+ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ln Fine value$_{i,t}$ | -0.214*** | -0.022 | -0.046** | -0.055*** | -0.051 | -0.062 |
| | [0.051] | [0.027] | [0.020] | [0.020] | [0.031] | [0.057] |
| ln $\widehat{\text{Voters}}_{i,t}$ | 0.584*** | 0.846*** | 1.126*** | 1.640*** | 1.389*** | 1.319*** |
| | [0.187] | [0.296] | [0.205] | [0.100] | [0.102] | [0.196] |
| Observations | 5,076 | 5,076 | 5,076 | 5,076 | 5,076 | 5,076 |
| Districts | 1692 | 1692 | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.11 | 0.11 | 0.15 | 0.35 | 0.41 | 0.15 |
| Mean of dep. var. | 8.00 | 9.28 | 8.77 | 9.43 | 9.20 | 7.32 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x '06 Category FE | Yes | Yes | Yes | Yes | Yes | Yes |

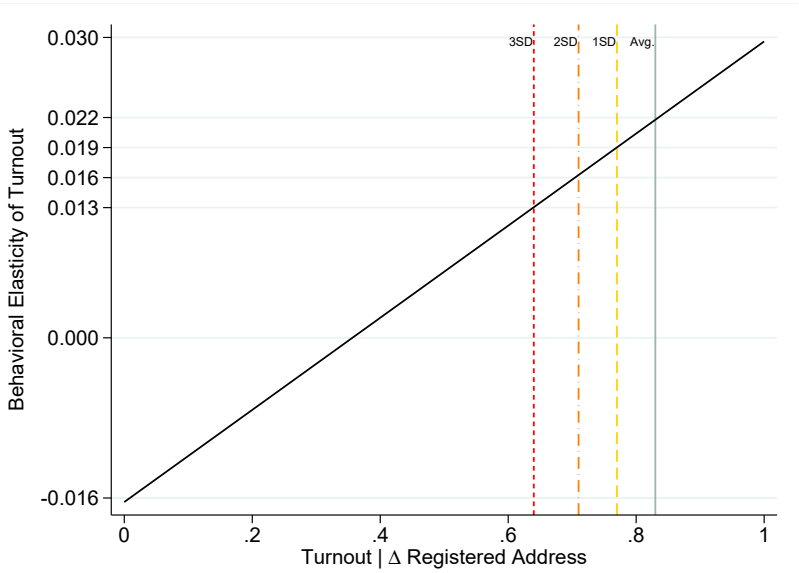
Notes: ln Voters is the natural log of the number of registered voters for the election cycle. Sample includes national elections for the years 2001, 2011 and 2016. ln $\widehat{\text{Voters}}_{i,t}$ is the natural log of the number of predicted voters in that age group, according to the 2007 population census. All columns include district fixed effects and election x province x 2006-poverty-category fixed effects. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

**Table 2.18:** The Value of the Abstention Fine, Nighttime lights and Migration

| Dependent variable: | ln Lights DN$_{i,t}$ | ln Voters$_{i,t}$ | | Share born in district$_{i,t}$ | ln Voters$_{i,t}$ | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Fine value$_{i,t}$ (S/ x 100) | 0.056 | -0.076** | -0.085** | -0.074 | -0.152*** | -0.160*** |
| | [0.064] | [0.031] | [0.035] | [0.085] | [0.047] | [0.047] |
| ln Night lights DN$_{i,t}$ | | | 0.170* | | | |
| | | | [0.090] | | | |
| Share born in district$_{i,t}$ | | | | | | -0.113** |
| | | | | | | [0.049] |
| Observations | 5,076 | 5,076 | 5,076 | 2,319 | 2,319 | 2,319 |
| Districts | 1692 | 1692 | 1692 | 913 | 913 | 913 |
| R-squared | 0.0007 | 0.0008 | 0.02 | 0.001 | 0.01 | 0.02 |
| Mean of dependent variable | 2.36 | 10.62 | 10.62 | 0.33 | 11.08 | 11.08 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Year x Province x Category '06 FE | Yes | Yes | Yes | Yes | Yes | Yes |

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

**Figure 2.12:** Disentangling the Behavioral Elasticity of Turnout from the Registration Effect




Notes: The graph shows the implied behavioral elasticity of turnout corresponding to different values of the probability of voting (i.e., turnout) for those that changed the address on their DNI to districts with a lower fine for abstention. Calculation based on estimates in Table 2.4 (columns 1 and 3) and equation (2.4). Solid vertical line shows average turnout in 2011/16. Dashed, dash-dot and dotted lines display turnout rates that are respectively one, two and three standard deviations below the average.

**Table 2.19:** The Value of the Abstention Fine and Age-specific Voter Registration, controlling for access to DNI

| | Dependent Variable: ln Voters$_{i,t}$ | | | | | |
|---|---|---|---|---|---|---|
| | 18-20 | 21-29 | 30-35 | 36-50 | 51-75 | 75+ |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | Panel A - Baseline in reduced sample | | | | | |
| ln Fine value$_{i,t}$ | -0.372*** | -0.083*** | -0.059* | -0.044* | -0.025 | -0.039 |
| | [0.058] | [0.025] | [0.030] | [0.026] | [0.036] | [0.079] |
| R-squared | 0.04 | 0.002 | 0.001 | 0.001 | 0.0002 | 0.0002 |
| | Panel B - Controlling for change in access to DNI | | | | | |
| ln Fine value$_{i,t}$ | -0.348*** | -0.063** | -0.036 | -0.021 | -0.003 | -0.018 |
| | [0.055] | [0.026] | [0.033] | [0.029] | [0.036] | [0.078] |
| $\Delta$ Share w/ DNI$_i$ $\times$ $\mathbb{1}(2011)_t$ | 1.183** | 1.053*** | 1.078** | 1.123** | 0.999** | 0.684 |
| | [0.498] | [0.362] | [0.439] | [0.500] | [0.502] | [0.576] |
| $\Delta$ Share w/ DNI$_i$ $\times$ $\mathbb{1}(2016)_t$ | 1.688*** | 1.339*** | 1.591*** | 1.563*** | 1.593*** | 1.758*** |
| | [0.604] | [0.420] | [0.526] | [0.598] | [0.612] | [0.632] |
| R-squared | 0.06 | 0.01 | 0.01 | 0.01 | 0.01 | 0.01 |
| Observations | 2,460 | 2,460 | 2,460 | 2,460 | 2,460 | 2,460 |
| Districts | 820 | 820 | 820 | 820 | 820 | 820 |
| Mean of dependent variable | 8.35 | 9.63 | 9.12 | 9.78 | 9.53 | 7.64 |
| District FE | Yes | Yes | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes | Yes | Yes |

Notes: ln Voters is the natural log of the number of registered voters for the election cycle. Sample includes election years 2001, 2011 and 2016. $\Delta$ Share w/ DNI$_i$ is the change in the share of ENAHO respondents that have a national identification document (DNI) between the post-reform years (post-2010) and the pre-reform years. All columns include district fixed effects and election x province x 2006-poverty-category fixed effects. All regressions weighted by the number of registered voters for the 2001 elections. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

**Table 2.20:** The Value of the Fine and Voter Turnout controlling for Registration

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

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

### *2.8.5 Web searches: Additional Results*

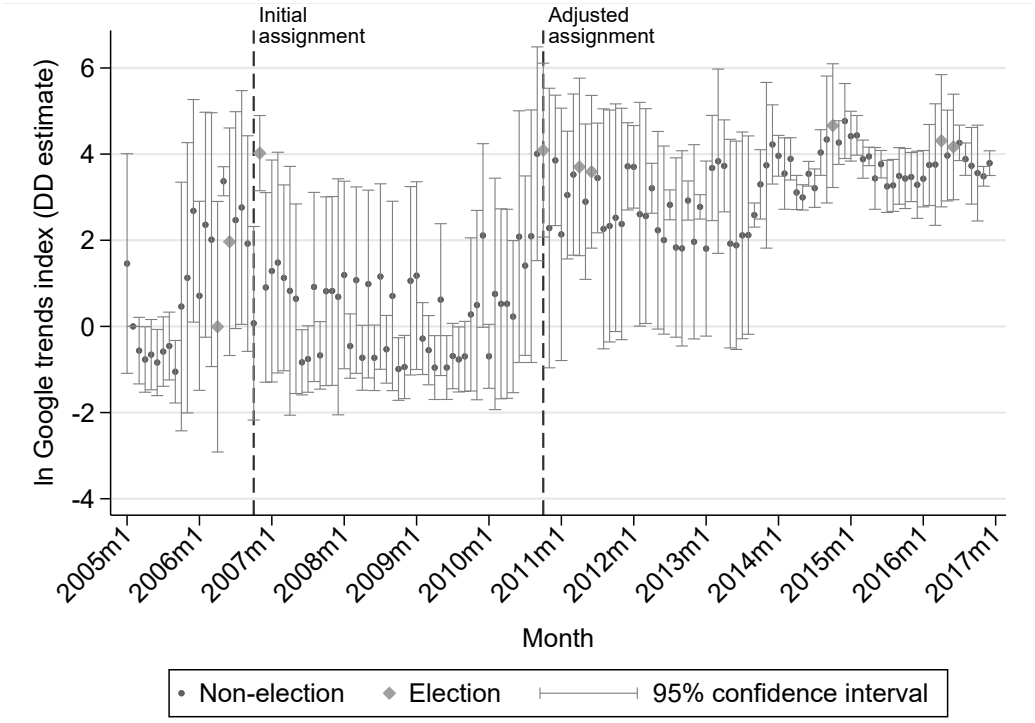
This section provides detailed information on the construction of the dataset on the popularity of various search terms in the Google search engine. For this purpose, we used the Google Trends online application, which we consulted in April 2018 (`https://trends.google.com/trends`). The search terms in the sample include three terms related to the abstention fine, which roughly translate to "election fine", "ONPE fine" and "fine for not voting." We also include several search terms related to elections (e.g., "candidates"), others associated with government and politics (e.g., "president"), the names or nicknames of former presidents and important political figures (e.g., "Fujimori"), as well as generally popular search terms (e.g., "soccer"). Appendix table 2.21 provides the full list of search terms used in the analysis. For each search term, we have monthly-level data between January 2005 and December 2016. We limited the geographic scope to the country of Peru and collected monthly data from January 2005 to December 2016. We 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 diacritics.

The Google Trends application allows queries on as many as five search terms simultaneously. The output is a relative search interest measure available at monthly intervals. This measure ranges from zero to 100, with the latter corresponding to the search term-month with the largest number of searches among those considered. 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. Consecu-

tive 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.

Roughly one third of Peruvians used the internet in 2007, almost one half in 2016, making internet searches a meaningful measure of information acquisition for a sizable share of the population [INEI, 2018]. We used the Internet Archive to verify that the ONPE website provided information about the value of the fine and outstanding fines at the start of our sample period on web searches in early 2005.

**Figure 2.13:** 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).

**Table 2.21:** Search Terms included in Google Trends Analysis

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

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

### *2.8.6 Political Outcomes: Additional Results*

**Overview of Peruvian Politics**

**Background:** Alberto Fujimori was elected for a third term as President of Peru in May of 2000, having won previous elections in 1990 and 1995. His government took an authoritarian turn in 1992, when he shut down Congress and took control of the judiciary through an 'auto-coup'. There were strong allegations of fraud in the presidential election of the year 2000, in which he defeated Alejandro Toledo of "Peru Posible". At the end of that year, Fujimori resigned and fled the country following a series of high-profile corruption scandals involving his inner circle.

**Elections before fine reform:** Fujimori's resignation prompted the 2001 election won by Toledo, who defeated Alan García in the run-off. Toledo had lost to Fujimori in the 2000 elections and was one of his staunchest opponents. He campaigned on a center-left platform focused on inclusive growth, inspired by Tony Blair's New Labour. García was a former president who had governed between 1985 and 1990. He represented APRA, a party founded on radical left-wing principles in the 1920s and rebranded by García in the 1980s as a center-left party in the spirit of the Spanish PSOE, but with a strong populist undertone. In the first round, third place went to Lourdes Flores, who represented a conservative coalition known as "Unidad Nacional" (UN).

A reform in 2001 eliminated immediate presidential re-election, so Toledo did not participate in 2006. This election was won by García, who defeated outsider candidate Ollanta Humala in the run-off. Humala had created a new party called PNP in 2005, but failed to secure enough signatures to participate in the election, running instead as a 'guest candidate' for party "Unión por el Perú" (UPP). This party supports the 'ethnocacerist' movement that aims to return the descendants of the Incas to power. It combines strong left-wing views on the role of the state in the economy with far-right views around race. Humala was a former military and had strong support from Venezuelan President Hugo Chávez. In the first round, third place went again to Lourdes Flores from UN. Fujimori, who was under

arrest and awaiting extradition in Chile, was barred from running. Martha Chavez, one of his strongest supporters in Congress, ran under a new party called "Alianza por el Futuro" and finished fourth, with 7% of the votes. For the run-off, García received endorsements from different segments of the political spectrum that were concerned by the leftist threat posed by Humala.

**Elections after fine reform:** In 2011, Humala ran again and won the run-off against Keiko Fujimori, the former president's daughter. This time, Humala ran under PNP and presented himself as a more moderate candidate, distancing himself from Hugo Chávez. Fujimori ran under a new party called Fuerza Popular. Both candidates had populist platforms, with Fujimori supporting the right-wing policies of his father and Humala still espousing an eclectic combination of elements from the right and the left. In the first round, the centrist vote was split between former President Toledo, who ran under a coalition of "Peru Posible" with two other parties, Pedro Pablo Kuczynski, his former Finance minister who ran under another coalition called "Alianza por el Gran Cambio", and former Mayor of Lima Luis Castañeda, who represented "Solidaridad Nacional". The latter had been part of Lourdes Flores' UN coalition in the previous elections, which ceased to exist in 2008. APRA (the incumbent's party) did not present a candidate for this election.

Keiko Fujimori would be defeated again in the 2016 run-off, this time by Pedro Pablo Kuczynski, who represented yet another new party called PPK. This time, Fujimori moved towards the center and distanced herself from the image of her father. Kuczynski ran under a center-right platform. Both Alan García and Alejandro Toledo ran again in 2016, under APRA and "Peru Posible" respectively, but failed to gain more than 6% of the first round votes each. Lourdes Flores, the conservative presidential candidate in 2001 and 2006, was García's running mate. Coming in third in the first round was Veronika Mendoza, who represented a new coalition of leftist and environmental movements called "Frente Amplio".

**Table 2.22:** Vote Share for Winner and Runner-up in First Round

| Dependent variable: | Winner | Runner-up | Top 2 candidates |
|---|---|---|---|
| | (1) | (2) | (3) |
| Fine value$_{i,t}$ (S/ x 100) [a] | 0.003 | 0.009 | 0.012 |
| | [0.010] | [0.021] | [0.025] |
| Observations | 6,768 | 6,768 | 6,768 |
| Districts | 1692 | 1692 | 1692 |
| R-squared | 7.01e-06 | 0.000126 | 0.000100 |
| District FE | Yes | Yes | Yes |
| Election-Province-Category '06 FE | Yes | Yes | Yes |
| Mean of dep. var | 0.209 | 0.213 | 0.422 |

Notes: Dependent variable correspond to the votes for the winner in the run-off in column 1, the votes for the runner-up in column 2 and the sum of votes for the winner and the runner-up in column 3. Dependent variable in columns 1-3 are expressed as a share of the number of registered voters. Sample includes first round of the presidential elections in 2001, 2006, 2011 and 2016. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

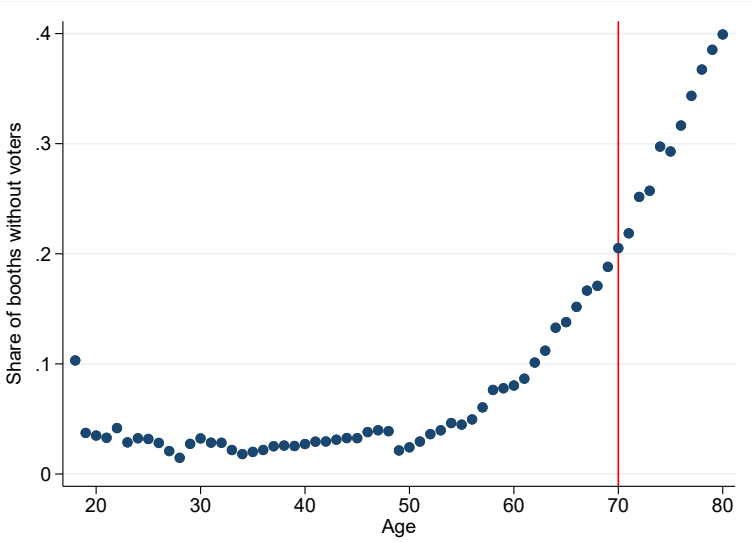
**Table 2.23:** The Marginal Effect of the Abstention Fine on Invalid and Blank Votes: Full sample

| Dependent variable: | Voter Turnout$_{i,t}$ | | Spoiled Votes $_{i,t}$ | |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Fine value$_{i,t}$ (S/ x 100) [a] | 0.049*** | 0.020** | 0.019*** | 0.014** |
| | [0.008] | [0.008] | [0.006] | [0.007] |
| Fine value$_{i,t}$ $\times$ $\mathbb{1}(2016)_t$ [b] | | 0.051*** | | 0.009*** |
| | | [0.005] | | [0.003] |
| Observations | 13,536 | 13,536 | 13,536 | 13,536 |
| Districts | 1692 | 1692 | 1692 | 1692 |
| R-squared | 0.0180 | 0.0277 | 0.0023 | 0.0026 |
| Mean of dependent variable | 0.845 | 0.845 | 0.0975 | 0.0975 |
| p-value $H_0$: a+b=0 | | 0.000 | | 0.000 |
| District FE | Yes | Yes | Yes | Yes |
| Election x Province x Category '06 FE | Yes | Yes | Yes | Yes |

Notes: Dependent variable in columns 3-4 is the sum of blank and invalid votes, as a share of the number of registered voters. Sample includes first and second rounds of the presidential elections in 2001, 2006, 2011 and 2016. The value of the fine is measured in 100s of current Peruvian Soles (S/). All columns are weighted by the number of registered voters in 2001. Standard errors clustered by province (192 units). *** p<0.01, ** p<0.05, * p<0.1

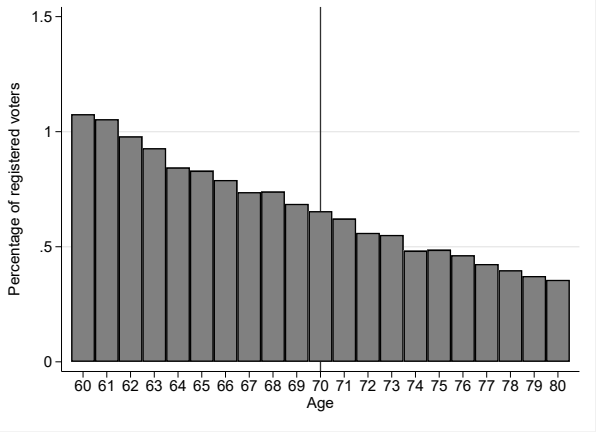
### *2.8.7 Senior Exemption: Additional Results*

**Figure 2.14:** Share of Booths without Voters from each Age



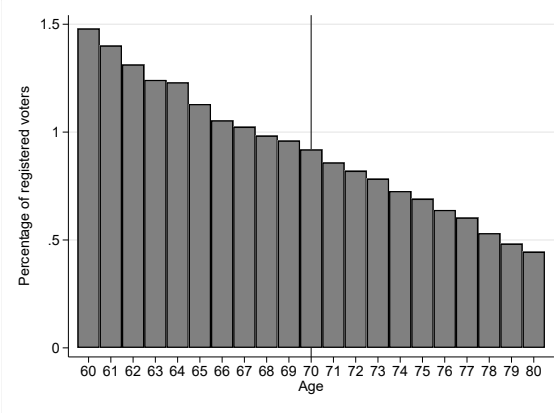

Notes: Figure shows the share of voting booths in the 2016 election that do not have any registered voters for each one-year age group from 18 to 80.

**Figure 2.15:** Distribution of Registered voters by age in Peru and Chile




width=75mm

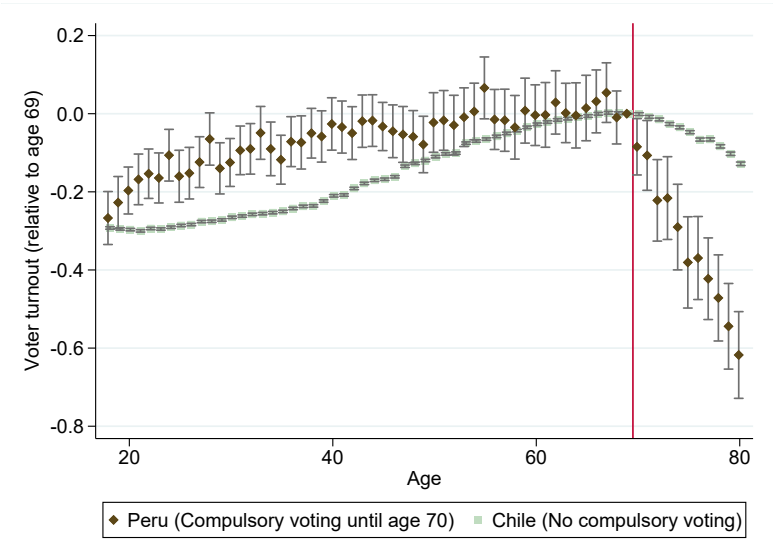
(a) Peru




(b) Chile

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

**Figure 2.16:** Voter Turnout by Age in Peru and Chile: Full Distribution




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

# CHAPTER 3

# POLITICAL RESPONSES TO NATURAL RESOURCE SHOCKS

# ABSTRACT

Do political preferences change in response to a natural resource shock? Does that shift reflect a calculation based on self-interest or does it reflect social considerations based on a changed view of what is desirable for the broader community? I examine changes in the Peru's two largest export products: copper and gold. Using difference-in-differences estimators, I show that higher levels of gold production increases (decreases) the vote share for anti-mining (pro-mining) parties. Conversely, an increase in copper production leads to a close to zero—and not statistically significant—change in the vote shares for anti- and pro-mining parties. Moreover, I find evidence consistent with voters caring about the environmental damage caused by gold production when casting their vote and prioritizing this effect over improvements in socioeconomic indicators. Overall, these findings highlight that voters heavily weigh social considerations compared to self-interest when deciding for whom to vote. [1]


## 3.1 Introduction

Access to discoveries and natural resources depends on the policies implemented by the politicians in power. On the one hand, if a politician is against exploiting natural resources, he might tighten the required environmental regulations and permits, increase revenue taxes or fines, or even cancel operating licenses. On the other hand, a politician who supports exploitation might speed up permits and push for further and faster production. According to the World Bank [2022], natural resource rents represent more than 10% of GDP in more than 30 countries. Despite this importance, we know very little about how natural resources can distort what politicians and parties come to power [Carreri and Dube, 2017], which could


1. Diego Tocre and Gonzalo Torres provided excellent research assistance. I would like to thank Fernando M. Aragon, Oeindrila Dube, Anthony Fowler, Ryan Kellogg, Stephane Lavertu, Luis Martinez and James Robinson, as well as audiences at University of Chicago, H2D2 Research Day 2019, and the 2018 APPAM Fall Research Conference for valuable comments and suggestions. All remaining errors are mine. I acknowledge financial support from the Pearson Institute.

directly affect future production and discoveries.

In this paper, I ask two questions: do political preferences change in response to a natural resource shock? Does that shift reflect a calculation based on self-interest (i.e., an increase in income), or does it reflect sociotropic considerations based on a changed view of what is desirable for the broader community (i.e., less environmental damage)? In the current paper, I first look at vote shares for anti- and pro-mining parties to examine if a mining shock affects who will come to power. I examine these outcomes as those political parties could have a direct effect on future natural resource management. Then, I study potential mechanisms through which that shock could affect vote shares: i) socioeconomic improvement to reflect self-interest and ii) environmental damage to reflect sociotropic considerations. So far, there is little evidence on how voters are weighting self-interest versus sociotropic considerations to decide for whom to vote [Margalit, 2019].

I study Peru since mining is a crucial sector for the country. Besides, the multi-party system allows for a wide variation in electoral outcomes across locations and over time. My primary analysis employs a staggered difference-in-differences with a continuous treatment design focusing on how mining production changes vote shares for anti- and pro-mining political parties. I compare vote share outcomes in mining-producing districts to non-producing districts that either never had positive production or started production in later years. I gather panel data on mining production for each mineral from 2001 to 2021 and four general elections in about 1,800 districts.

Based on these analyses, I provide two main findings. First, I show that higher levels of gold production increases (decreases) the vote share for anti-mining (pro-mining) parties. Instead, an increase in copper production does lead to a small and not statistically significant change in the vote shares for anti- or pro-mining parties. I further show that these effects are not different in regions with or without illegal mining but instead are highly heterogeneous depending on the size of the mine. The effect of gold production on vote share for anti-mining parties in districts with small mines is more than twice the size of that in districts

with large mines. This is consistent with larger mines having a more strict environmental regulation and enforcement than smaller mines [OEFA, 2013]. Moreover, mining production does not affect other electoral outcomes, such as turnout and spoiled (i.e., blank or invalid) votes or political attitudes related to engagement. This suggests that the changes in vote shares are driven by voters already casting a vote for one of the available candidates and not by endogenous political engagement or participation changes.

Second, I investigate two possible mechanisms explaining why mining production affects vote shares for anti- and pro-mining parties. Following von der Goltz and Barnwal [2019], I evaluate the trade-off between socioeconomic benefits and environmental damage. The first mechanism is that mining pollutes the environment [Aragon and Rud, 2016], which has the potential of affecting agriculture productivity and human health [von der Goltz and Barnwal, 2019, Rozo, 2020]. This could harm specific individuals, but it could also be not noticeable for everyone if, for example, someone does not work in agriculture or live further from mines. In this case, voters will have more incentives to vote for anti-mining parties that could endanger mines' current or future operations and fewer incentives to vote for pro-mining parties. An alternative mechanism is that mining production potentially increases income, employment, and government expenditures and improves infrastructure [Aragon et al., 2015, Cust and Poelhekke, 2015]. In that case, voters will have fewer incentives to vote for anti-mining parties. In contrast, voters will have more incentives to vote for pro-mining parties.

The net effect on vote shares depends on what voters perceive as the overall effect of mining. It also depends on whether voters, when casting their vote, prioritize self-interest over social interest (i.e., even if the increasing pollution does not harm them, they care about the negative effect on the community).

I find evidence consistent with both mechanisms. First, I find that gold, but not copper production, harms the environment by reducing vegetation and increasing land pollution. Second, I find that changes in the monetary value of gold and production have a similar and positive effect on the availability of public services, such as the length of connecting roads.

Combined with those from vote shares, these results suggest that voters care about the environmental damage caused by gold production when casting their vote and prioritize this effect compared to the one on socioeconomic indicators. The null effect of copper production on anti- and pro-mining vote shares suggests that voters value little the improvements in outcomes such as connecting roads, as they are relatively small.

I go on to show that alternative explanations unrelated to socioeconomic improvements and environmental damage are also unlikely to account for these findings. I rule out that changes in mining production have induced migration nor a compositional change in the electorate.

This paper contributes to the literature on the within-country and cross-country[2] studies on the economic and political resource curse. Studies have focused on outcomes such as corruption [Caselli and Michaels, 2013, Brollo et al., 2013], incumbency advantage [Ferraz and Monteiro, 2012], local government expenditures and socioeconomic indicators [Zegarra et al., 2007, Aragon and Rud, 2013, Caselli and Michaels, 2013, Loayza and Rigolini, 2015, von der Goltz and Barnwal, 2019, Bartik et al., 2019, Rozo, 2016, Maldonado and Ardanaz, 2017, Martinez, 2022][3], and environmental damage [Aragon and Rud, 2016, Hidayati et al., 2009, Jordanov et al., 2007, von der Goltz and Barnwal, 2019, Rozo, 2020]. Only a handful of qualitative and quantitative studies have examined the effect of natural resources on voting outcomes. Carreri and Dube [2017] find that positive oil-price shocks lead to electoral gains by legislators affiliated with right-wing paramilitary groups in oil-producing municipalities in Colombia. Fedaseyeu et al. [2019] study the shale boom in the United States and find that the Republican candidate gained support disproportionately in counties with oil drilling.

The biggest difference between these papers and my paper is that I directly study vote

2. Cross-country studies on the "natural resource curse" include Sachs and Warner [2001], Goldberg et al. [2008], Robinson et al. [2006]. One limitation of this literature is that the cross-country strategy could suffer from problems such as reverse causality and omitted variable bias [Aragon et al., 2015].

3. Aragon et al. [2015], Cust and Poelhekke [2015] provide a literature review on this topic. Dell [2010] provides evidence of long-lasting negative outcomes from mining in Peru, such as lower consumption levels. However, the mechanism behind these effects is the forced labor system that happened between 1573 and 1812.

shares for anti- and pro-mining parties, which could have a direct impact on future policies related to natural resources (i.e., endanger or promote new mine operations). Another difference is that I study mining resources, which have been understudied in comparison with oil resources [Ross, 2015]. My findings provide two main contributions. First, I show that voters care about the environmental damage caused by gold production when casting their vote for anti- and pro-mining parties and prioritize this effect compared to the improvement in socioeconomic indicators. Second, I show that voters value little the improvements in outcomes such as connecting roads. Overall, this highlights that voters weigh heavily sociotropic considerations compared to self-interest to decide for whom to vote, which is an open question [Margalit, 2019].

## 3.2 Institutional Background

This section provides relevant details about general elections and describes the mining operations in Peru.

### *3.2.1 Elections*

General elections in Peru, including the first round of the presidential and multi-district legislative elections, are held concurrently every five years. [4] In the presidential election, a candidate must obtain at least 50% of the votes nationwide to win in the first round (which never happens during our sample period.) As a result, a run-off election between the two leading candidates occurs approximately two months later.[5]

Peru has a multi-party system. A new political party can be created for a specific general election. To be recognized, the party needs to collect a minimum number of voter signatures

4. 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. 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 into 198 provinces and 1854 districts.

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

based on the turnout in the last national election [Makarin et al., 2020]. Presidential candidates are not allowed to run for immediate re-election since 2001. These two characteristics make Peruvian politics volatile and personalistic in nature [Gonzales et al., 2022]. Not all candidates participate in all elections, and their platforms fluctuate substantially over time, making the comparison of political parties challenging.

Peruvian voters usually can recognize that certain platforms might endanger mines' current or future operation. In this regard, most anti-mining candidates express their support or opposition to mining operations very clearly in their platforms, interviews or debates. For instance, in 2011, former President Ollanta Humala's campaign introduced the slogan "Water, yes; Gold, not."

### *3.2.2 Mining sector*

Mining in Peru has a long history, rewinding to the Spanish colonial period [Dell, 2010]. In the 1990s, Alberto Fujimori's government implemented a series of liberalizing reforms that benefited private investors in multiple sectors, including mining. Some of these policies included the privatization of mining and oil companies and the introduction of contracts to guarantee tax stability. These policies attracted investments from transnational mining companies.

Most mining firms operating in Peru are privately-owned and foreign state-owned companies. Peruvian legislation establishes that any natural resource underground belongs to the State. The central government grants firms the right to explore, extract and process minerals in the form of concessions. The same concession is valid to explore and produce. These are granted on a "first come, first served" basis, with auctions if simultaneous requests are made. The concession is irrevocable (indefinite term) if the firm complies with the imposed obligations and maintains a certain production level. Another way to obtain concessions is through auctions of mines (either operating mines or mining projects with advanced studies) previously owned by the Peruvian government.

The mining sector is essential for the Peruvian economy. It accounts for 14.5% of GDP, 55% of national aggregated exports (Peruvian Central Bank, 2015), and 16% of fiscal revenues [LarrainVial, 2010]. Mining companies pay a royalty rate to the central government, which is proportionate to the value of the mineral production and distributed locally. Besides, 50% of their income tax is transferred to local authorities in mining areas, producing and non-producing districts, as a function of population size and poverty rates (sharing scheme Mining Canon).

Despite the sector's importance, there is increasing tension between the mining companies and surrounding communities. Most focused extractive industries conflict specifically mention environmental or profit-sharing concerns.[6]

The central government, particularly the Agency for Environmental Assessment and Enforcement (OEFA), is responsible for the enforcement of environmental laws applicable to large and mid-size mine units. Regional governments enforce environmental laws to small and artisanal mine activities [Osinergmin, 2017].

## 3.3 Empirical Strategy

In this section, I present the data sources and research design.

### *3.3.1 Data*

I will focus my analysis on general elections data from Peru for 2006-2021. The unit of observation and analysis is the district.[7] The main sample includes 1,614 districts, corresponding to 86% of the total and 92% of the 24 million registered voters in 2021.[8] This paper uses

6. When a conflict escalates, it is usually handled by the central government. These are primarily protests and riots, including road obstructions and are unrelated to armed conflict. The leaders of the two terrorist organizations operating in Peru, the Shining Path and the MRTA, were captured and sentenced in 1992, ending most of the terrorist activities in the country.

7. Administrative boundaries are based on historical and political jurisdictions and revised only rarely [Loayza and Rigolini, 2015].

8. After excluding districts with missing data, there are 1,614 districts left out of the 1,874 in the country.

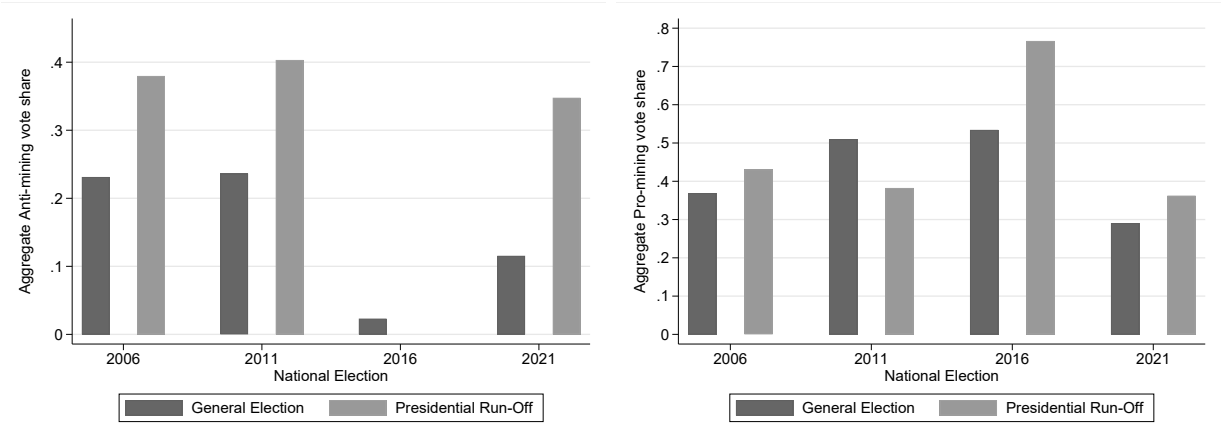
data from multiple sources.

## Electoral Data

I use administrative data for the national elections in 2006, 2011, 2016, and 2021 from the National Election Office (Oficina Nacional de Procesos Electorales, ONPE). The data covers the general election, combining the legislative election with the first round of the presidential race and the presidential run-off. I observe the number of registered voters, the number of votes cast by political party at the district level.

**Anti-mining and pro-mining party classification.** A central part of my empirical exercise involves coding parties as anti-mining or pro-mining (for more details see Table 3.8). The main outcome variables are the vote share for anti-mining and pro-mining parties (Figure 3.1 and Figure 3.2).[9] Using publicly available party's platforms, I classify by hand 59 parties that ran for national elections between 2006 and 2021, based on its preferences towards mining extraction.

**Figure 3.1:** Aggregate Anti- and Pro-mining vote shares in national elections



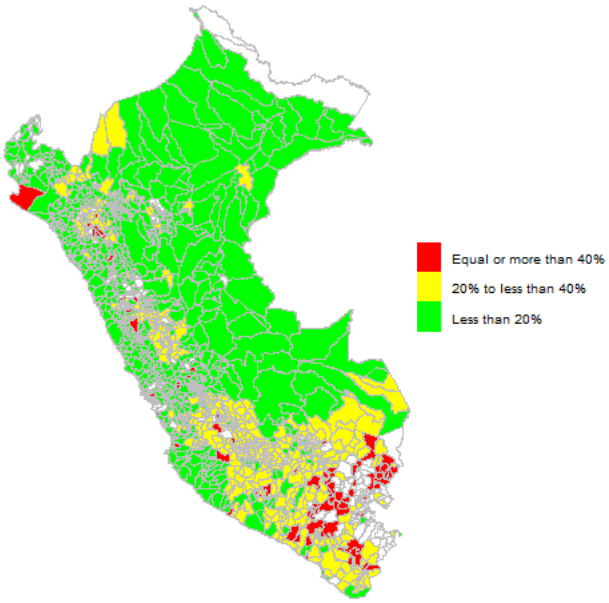

(a) Anti-mining

(b) Pro-mining

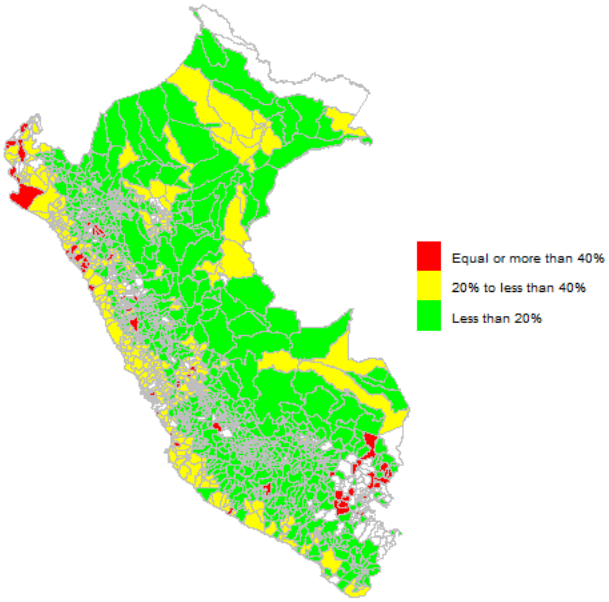
Note: Panel A shows the aggregate anti-mining vote share for each national election between 2006 and 2021. Panel B shows the aggregate pro-mining vote share for each national election between 2006 and 2021.

9. It corresponds to the sum of vote share across all parties classified in the same category. See Appendix 3.9 for more detail.

**Figure 3.2:** Vote share for Anti- and Pro-mining parties in General Election 2021




(a) Anti-mining parties




(b) Pro-mining parties

Notes: Panel (a) shows the average vote share for anti-mining parties by district in the 2021 general elections. Panel (b) shows the average vote share for pro-mining parties by district in the 2021 general elections.

I also use data on political attitudes and behaviors from a large household survey, the Peruvian National Household Survey (ENAHO) from 2004 to 2020.

### Mining Data

The data on mine production is managed by the Ministry of Energy and Mining (MINEM). I use annual data from 2001 to 2020 to build a panel of production by mineral, district, and firm.

To construct an annual measure of the monetary value of mine production, I use data on current minerals' international prices from the Peruvian Central Bank (BCR). The measure of the monetary value of mine production is constructed as:

$$Value_{m,i,t} = Price_{m,t} * Production_{m,i,t} \tag{3.1}$$

where $\text{Value}_{m,i,t}$ is the monetary value of production of a mineral $m$ in district $i$ at year $t$.

Finally, I collect data on all the districts that have ever been explored between 1999 and 2016. To start exploration, a mining firm needs to get the approval of an Environmental Impact Assessment (EIA) or an Environmental Impact Statement.

### Socioeconomic and Environmental datasets

The socioeconomic data comes from three sources. I use annual data from a large household survey (ENAHO), the number of road kilometers from the Ministry of Transport and Communication, and satellite data on night-time lights. For environmental data, I use satellite data on the level of vegetation (NDVI), and land pollution. I construct a district-level panel of these three variables by overlaying average annual satellite images with the shapefile with district administrative boundaries. For more details on the data, see Appendix Table 3.9.

## *3.3.2 Research Design*

I aim to estimate the causal effect of mining production on the vote share of anti-mining and pro-mining parties in general elections. For this purpose, I examine mine production variation across districts over time. The nature of this variation lends itself naturally to the

analysis of a staggered difference-in-difference with continuous treatment with district and election fixed effects.

I estimate the following equation:

$$Y_{i,e} = \beta_m \sum_m Q_{m,i,e} + \alpha_i + \lambda_e + \epsilon_{i,e} \tag{3.2}$$

Here, $Y_{i,e}$ is the outcome variable in district $i$ at election $e$, $\alpha_i$ is a district fixed effect, $\lambda_e$ is a election fixed effect. $Q_{m,i,e}$ is a continuous measure representing the cumulative mine production by mineral and district across the four years before election $e$. The set of coefficients $\beta_m$ represents the average treatment effect of mine production of mineral $m$ on the outcome variable, conditional on the set of fixed effects.

To assess the causal effect of mining production on socio-economic and environmental variables, I estimate the following equation:

$$Y_{i,t} = \beta_m \sum_m Q_{m,i,t} + \alpha_i + \lambda_t + \epsilon_{i,t} \tag{3.3}$$

Here, $Y_{i,t}$ is the outcome variable in district $i$ at year $t$, $\lambda_t$ is a year fixed effect. The rest of fixed effects are analogous to those in equation (3.2). $Q_{m,i,t}$ is a continuous measure representing the mine production by metal, district at year $t$. I cluster the standard error terms at the district level to allow for arbitrary correlation within districts. I weight all our regressions by the number of registered voters in 2006 to capture individual-level effects.

## 3.4 Results: Electoral Outcomes

This section presents evidence on how gold and copper production affect the vote shares for anti- and pro-mining parties and documents heterogeneous impacts. I then evaluate whether

mining production affects other political variables, such as turnout and political attitudes.

### *3.4.1 Baseline Results: Anti- and Pro-mining vote shares*

Panel A in Table 3.1 presents estimates of equation (3.2) using vote shares for anti-mining parties as dependent variable. The estimates of $\beta_m$ in column 1 imply that a 10% increase in gold production increases the vote share for anti-mining parties by about 0.2 percentage points. This corresponds to a 1.02% increase over the sample mean of 0.215. Instead, an increase in copper production does lead to a small and not statistically significant change in the vote share. Estimates are statistically different from each other.

Column 1 in Panel B shows the corresponding estimates using vote shares for pro-mining parties as to the dependent variable. The estimates in column 1 imply that a 10% increase in gold production decreases the vote share for pro-mining parties by about 0.2 percentage points. This corresponds to a 0.40% decrease over the sample mean of 0.46. Instead, an increase in copper production does lead to a small and statistically insignificant change in the vote share. Estimates are statistically different from each other.

The remaining columns correspond to several robustness tests. The point estimates for gold and copper production in Panel A and Panel B are very similar when I exclude districts that have never been explored for minerals (column 2) or when I do not include control variables (column 3). When I run the regressions unweighted (column 4), the estimates remain positive and statistically significant, although they become somewhat smaller.

I study potential heterogeneity in the vote shares response to gold production by estimating extensions of equation (3.2) that include interactions of gold production with dummies for various subsamples. Table 3.2 shows the results.

First, I explore whether the marginal effect of the gold production on vote shares varies depending on whether mining production is located in regions with some illegal mining (column 1). Illegal mining is linked to higher levels of tax evasion [Saavedra and Romero, 2021], violence [Idrobo et al., 2014], and environmental damages [Rozo, 2020]. Therefore, I

**Table 3.1:** The Effect of Mining Production on Anti- and Pro-mining vote shares

| | Baseline | Mining districts | No controls | No weights |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Panel A. Dependent Variable: Vote Share Anti-mining parties | | | | |
| [$a$] log gold production | 0.022*** | 0.023*** | 0.019*** | 0.013* |
| | [0.008] | [0.008] | [0.007] | [0.008] |
| [$b$] log copper production | 0.001 | 0.001 | -0.001 | 0.001 |
| | [0.002] | [0.002] | [0.001] | [0.002] |
| Mean of dep. var | 0.215 | 0.217 | 0.215 | 0.267 |
| p-value $H_0 : a = b$ | 0.003 | 0.003 | 0.006 | 0.114 |
| Panel B. Dependent Variable: Vote Share Pro-mining parties | | | | |
| [$a$] log gold production | -0.019** | -0.021** | -0.015* | -0.008 |
| | [0.008] | [0.008] | [0.008] | [0.007] |
| [$b$] log copper production | -0.003* | -0.003* | -0.001 | -0.001 |
| | [0.002] | [0.002] | [0.001] | [0.001] |
| Mean of dep. var | 0.460 | 0.476 | 0.460 | 0.328 |
| p-value $H_0 : a = b$ | 0.037 | 0.025 | 0.094 | 0.283 |
| Observations | 12,912 | 2,496 | 12,912 | 12,912 |
| Districts | 1614 | 312 | 1614 | 1614 |
| Weighted | Yes | Yes | Yes | No |
| District FE | Yes | Yes | Yes | Yes |
| Election FE | Yes | Yes | Yes | Yes |
| Sample | All | Mining | All | All |
| Controls | Yes | Yes | No | Yes |

All columns use data from national elections (general and presidential runoff) in 2006, 2011, 2016, and 2021. The unit of observation is a district-election cycle. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral during years 1-5 in the Presidential term. Controls include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. Sample includes all districts without missing data, except in column 2 where the sample only includes 361 mining producing or non-producing but explored districts. All columns are weighted by the number of registered voters in 2006, except column 4. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

could expect a differential effect on electoral outcomes. However, the effect of a 10% increase in the gold production is around 0.23 percentage points in vote share for anti-mining parties, and it is very similar in regions with or without illegal mining. The same size effect is around -0.2 percentage points in vote share for pro-mining parties in regions without illegal mining. The effect in regions with illegal mining is positive but smaller and not statistically significant. I further explore this margin of heterogeneity in section 3.5.

**Table 3.2:** The Heterogeneous Effect of Gold Production on Anti- and Pro-mining vote shares

| | Illegal mining | Size Mines | Old Mines | Extraction process |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Panel A. Dependent Variable: Vote Share Anti-mining parties (Mean = 0.267) | | | | |
| [$a$] log gold production | 0.023*** | 0.052*** | 0.022*** | 0.018*** |
| | [0.008] | [0.004] | [0.006] | [0.005] |
| [$b$] $\mathbb{1}$(Illegal areas) $\times$ log gold production | -0.008 | | | |
| | [0.020] | | | |
| [$b$] $\mathbb{1}$(Large mines) $\times$ log gold production | | -0.031*** | | |
| | | [0.009] | | |
| [$b$] $\mathbb{1}$(Old mines) $\times$ log gold production | | | 0.000 | |
| | | | [0.013] | |
| [$b$] $\mathbb{1}$(Flotation) $\times$ log gold production | | | | 0.013 |
| | | | | [0.014] |
| p-value $H_0 : a + b = 0$ | 0.450 | 0.008 | 0.096 | 0.044 |
| Panel B. Dependent Variable: Vote Share Pro-mining parties (Mean = 0.328) | | | | |
| [$a$] log gold production | -0.020** | -0.008** | -0.025*** | -0.021*** |
| | [0.008] | [0.004] | [0.006] | [0.007] |
| [$b$] $\mathbb{1}$(Illegal areas) $\times$ log gold production | 0.010 | | | |
| | [0.016] | | | |
| [$b$] $\mathbb{1}$(Large mines) $\times$ log gold production | | -0.012 | | |
| | | [0.009] | | |
| [$b$] $\mathbb{1}$(Old mines) $\times$ log gold production | | | 0.014 | |
| | | | [0.014] | |
| [$b$] $\mathbb{1}$(Flotation) $\times$ log gold production | | | | 0.005 |
| | | | | [0.017] |
| p-value $H_0 : a + b = 0$ | 0.505 | 0.017 | 0.436 | 0.355 |
| Observations | 12,912 | 12,912 | 12,912 | 12,912 |
| Districts | 1614 | 1614 | 1614 | 1614 |
| District FE | Yes | Yes | Yes | Yes |
| Election FE | Yes | Yes | Yes | Yes |

All columns use data from national elections (general and presidential runoff) in 2006, 2011, 2016, and 2021. The unit of observation is a district-election cycle. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral during years 1-5 in the Presidential term. Controls include the production of all other minerals produced in the country: copper, silver, zinc, lead, iron, molybdenum, and tin. Illegal areas correspond to regions with some illegal mining: Puno, Madre de Dios and Cusco. Large mines correspond to districts with at least one medium or large size mine, according to the legal definition. Old mines correspond to districts with at least one mine that started production before 2006. Flotation correspond to districts with at least one mine that extract gold through a water-intensive process. All columns are weighted by the number of registered voters in 2006. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

Then, I examine whether the effect of gold production varies depending on the size of the mine (column 2). The effect of a 10% increase in the gold production is around 0.5 percentage points in vote share for anti-mining parties in districts with small mines, and it is more than twice the size of that in districts with large mines. Instead, the effect on gold production in vote share for pro-mining parties is larger in districts with large mines. This is consistent with larger mines having a more strict environmental regulation and enforcement than smaller mines [OEFA, 2013]. I explore this mechanism in section 3.5.

In the following columns, I evaluate if the effect of the gold production varies depending on the number of years a mine has been operating (column 3) or by the type of extraction process (column 4). I do not find any significant heterogeneous effects.

### *3.4.2 Other Electoral Outcomes and Political Attitudes*

In this subsection, I examine whether mining production affects other electoral outcomes and political attitudes and behaviors in the Peruvian National Household Survey (ENAHO) in Table 3.3.

I first focus the attention on voter turnout (column 1). It is plausible that the changes in vote shares are driven by voters who did not cast a vote before and are drawn by changes caused by mining production. There is not a significant change in voter turnout, suggesting that the changes in vote shares are not driven by new voters brought to the polls. Reassuringly, Appendix Table 3.7 shows that gold and copper production lead to an increase in the number of registered voters but is substantially lower than the effect on the number of additional votes for anti- and pro-mining parties.

Then, I show results for spoiled (i.e., blank or invalid) votes (see Gonzales et al. [2022] for details). There is no significant change in spoiled votes, suggesting that the changes in vote shares are driven by voters who were already casting a vote for one of the available candidates.

Finally, I study political attitudes and behaviors in the ENAHO survey for the period

2004–2020. I focus on questions concerning (i) information acquisition, (ii) trust in political parties, and (iii) party membership. For this part of the analysis, I run regressions at the individual level, assigning the value of mining production to each respondent based on district of residence and year. The point estimates are close to zero and also insignificant. Overall, the survey evidence suggests that changes in mining production do not change political engagement. These findings suggest that endogenous changes in political engagement do not drive the previous results on anti- and pro-mining vote shares.

**Table 3.3:** The Effect of Mining Production on Turnout, Spoiled Votes and Political Attitudes

| Dependent variable: | Voter Turnout | Spoiled Votes | Information acquisition | Trust in parties | Party membership |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| log gold production | -0.007 | -0.006* | -0.002 | -0.003 | -0.001 |
| | [0.005] | [0.003] | [0.011] | [0.004] | [0.001] |
| log copper production | -0.001 | -0.000 | -0.005 | 0.002* | 0.000 |
| | [0.001] | [0.001] | [0.003] | [0.001] | [0.000] |
| Observations | 12,912 | 12,912 | 162,378 | 439,172 | 439,172 |
| Districts | 1614 | 1614 | 1167 | 1458 | 1458 |
| Mean of dep. var | 0.845 | 0.0898 | 0.284 | 0.0428 | 0.0107 |
| Survey years | | | 2004-2011 | 2004-2020 | 2004-2020 |
| Weighted | Yes | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes |
| Election FE | Yes | Yes | No | No | No |
| Year FE | No | No | Yes | Yes | Yes |
| Individual controls | No | No | Yes | Yes | Yes |

Spoiled votes includes blank votes and invalid votes and is measured as the share of the number of registered voters. ln Voters is the natural log of the number of registered voters for the election cycle (same for general and runoff elections). Outcomes in columns 3-5 are binary and come from ENAHO. Not all ENAHO survey questions are asked every year. In column 3, "Information acquisition" takes the value of one if the respondent reports frequently being informed about political affairs rather than occasionally, depending on the topic, or never. In column 4, "Trust in parties" takes the value of one if the respondent reports "sufficient" or "considerable" trust in political parties. In column 5, "Party membership" takes the value of one if the respondent reports belonging to a political group or party. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral during years 1-5 in the Presidential term in columns 1-2 and annually in columns 3-5. Controls include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. Individual controls include dummies for gender, primary, secondary and college education, and age. Columns 1-2 are weighted by the number of registered voters in 2006. Columns 3-5 are weighted by the survey expansion factor. Sample in columns 1-2 includes national elections (general and presidential runoff) in 2006, 2011, 2016, and 2021 and the unit of observation is a district-election cycle. Standard errors clustered by district. *** $p<0.01$, ** $p<0.05$, * $p<0.1$

## 3.5 Mechanisms: Environmental damage versus Socioeconomic improvement

There is a visible trade-off that population living in mining areas face: real economic benefits versus specific health costs [von der Goltz and Barnwal, 2019]. From the results presented in Section 3.4, it can be inferred that the positive effects of mining are dominating the set of potential negative mechanisms in the case of copper, while the opposite is occurring in the case of gold. In this section I will provide suggestive evidence on the socio-economic and environmental mechanisms for this particular setting.

### *3.5.1 Environmental damage*

In this subsection, I examine whether mining production affects pollution and vegetation. Empirical evidence find that these industries have the potential to pollute the environment by increasing air and water pollutants, such as cyanide, heavy minerals, or acid mine drainage [Aragon and Rud, 2016, Hidayati et al., 2009, Jordanov et al., 2007]. This has the potential of affecting agriculture productivity and human health [von der Goltz and Barnwal, 2019, Aragon and Rud, 2016, Rozo, 2020]. Most of this literature have focused on gold mines. I will assess if that effect is different between gold and copper to assess whether this could explain the differential effect on anti- and pro-mining vote shares.

Column 1 of Table 3.4 shows the estimate using a vegetation index (NDVI) as to the dependent variable. The estimates in column 1 imply that a 10% increase in gold production decreases the NDVI by about 0.13. This corresponds to a 0.42% decrease over the sample mean of 30. Instead, an increase in copper production does lead to a close to zero and statistically insignificant change in NDVI. Column 2 shows the effect on the level of land pollution. The estimate implies a 10% increase in gold production increases pollution by about 0.24. This corresponds to a 0.12% increase over the sample mean. The estimate for copper is negative, small, and not significant.

**Table 3.4:** The Effect of Mining Production on Vegetation and Polution

| Dependent variable: | NDVI | Land pollution |
|---|---|---|
| | (1) | (2) |
| [$a$] log gold production | -1.282*** | 2.425* |
| | [0.461] | [1.404] |
| [$b$] log copper production | 0.009 | -0.136 |
| | [0.059] | [0.243] |
| Observations | 33,240 | 33,240 |
| Districts | 1662 | 1662 |
| Mean of dep. var | 30.69 | 193.2 |
| p-value $H_0 : a = b$ | 0.005 | 0.073 |
| Weighted | Yes | Yes |
| District FE | Yes | Yes |
| Year FE | Yes | Yes |

NDVI measures is a vegetation index that takes the values from 0 to 100 (higher values corresponding rainforest). Land pollution. See Appendix Table 3.9 for more details on the construction of variables can be found in Table. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral. Controls include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. Individual controls include dummies for gender, primary, secondary and college education, and age. All columns are weighted by the area of the district. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

Overall, these results show that gold production harms the environment by reducing vegetation levels and increasing land pollution. The estimates are statistically significant. Instead, copper production does not significantly affect these outcome variables. These results help conciliate the heterogeneous effect between gold and copper of anti-mining and pro-mining vote shares.

### *3.5.2 Socioeconomic outcomes*

In this subsection, I examine whether mining production affects socioeconomic outcomes and if that effect is different between gold and copper to assess whether this could explain the differential effect on anti- and pro-mining vote shares displayed in Section 3.4.

Table 3.5 shows the effect of mining production (Panel A) and mining value (Panel B) on socioeconomic indicators. I display both since the effect of production might differ from the effect of the monetary value of that production if the price of copper is very different from the price of gold.

According to the estimates from Panel A, gold production has a positive effect on the length of the road, nighttime lights, the percentage of households with electricity connections and mobile phones, and expenditures. Estimates in columns 1-3 and 5 are statistically significant. Copper production also has a positive effect on the length of the road, nighttime lights, percentage of households with electricity connections and mobile phones, but much smaller and not statistically significant.[10]

Estimates from Panel B of Table 3.5 show that the effect of the monetary value of gold and copper on socioeconomic outcomes is still positive and much more similar between them. In particular, there is a significant effect on the length of the road, suggesting that mining is positively affecting, mainly infrastructure in the district. [11]

10. Even though mobile phones in Peru are not public services, private operators use the electricity and transport infrastructure to provide the service.

11. This could be explained due to mining transfers to local government, which can only fund public investment projects.

**Table 3.5:** The Effect of Mining Production and Value on Socioeconomic Outcomes

| Dependent variable: | KM of road | Nighttime lights | Electricity | Mobile Phone | Ln Expenditures |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Panel A. Production | | | | | |
| [$a$] log gold production | 26.350** | 0.425** | 0.040 | 0.030** | 0.051* |
| | [10.832] | [0.210] | [0.029] | [0.015] | [0.028] |
| [$b$] log copper production | 4.215** | 0.048 | 0.008 | 0.005 | -0.007 |
| | [1.705] | [0.081] | [0.006] | [0.003] | [0.006] |
| p-value $H_0 : a = b$ | 0.044 | 0.091 | 0.198 | 0.116 | 0.043 |
| Panel B. Monetary value | | | | | |
| [$a$] log gold value | 2.483*** | 0.005 | 0.005** | 0.003*** | 0.002 |
| | [0.855] | [0.019] | [0.002] | [0.001] | [0.002] |
| [$b$] log copper value | 1.930*** | 0.018 | 0.002 | 0.002 | -0.001 |
| | [0.723] | [0.033] | [0.003] | [0.001] | [0.003] |
| p-value $H_0 : a = b$ | 0.609 | 0.687 | 0.253 | 0.543 | 0.249 |
| Observations | 13,218 | 12,406 | 439,175 | 439,175 | 439,175 |
| Districts | 1663 | 936 | 1458 | 1458 | 1458 |
| Mean of dep. var | 123.7 | 17.47 | 0.901 | 0.727 | 8.587 |
| Data years | 2002, 2008, 2010, 2012-2016 | 2001-2018 | 2004-2020 | 2004-2020 | 2004-2020 |
| Weighted | No | No | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |

KM of road is the sum of kilometers of road within the district. Nighttime lights corresponds to the average intensity. Electricity is an indicator variable that takes the value of 1 if the household has access to a connection inside the dwelling. Mobile phone is an indicator variable that takes the value of 1 if the household has a mobile phone. Ln Expenditures correspond to the natural logarithm of the total household expenditures expressed in constant local currency units. Outcomes in columns 3-5 come from ENAHO. Transformation of independent variables in Panel A was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral. Controls in Panel A include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. Transformation of independent variables in Panel B was done by estimating the inverse hyperbolic sine transformation on the monetary value of each mineral, expressed in constant local currency units. Controls in Panel B include the sum of the monetary value of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. Columns 3-5 are weighted by the survey expansion factor. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

### *3.5.3 Selective Migration*

An alternative explanation is that the observed changes in vote shares for anti- and pro-mining parties reflect compositional changes in the labor force. For example, suppose more educated workers migrate to mining-producing districts. In that case, the changes in vote shares and the changes in socioeconomic indicators could be driven by these new workers and not by the demand shock from the mine. Ideally, I would like to identify migrants in the sample and check whether this subpopulation drives the results. However, that information is unavailable since the electoral data is given at the district level.

I address this concern by evaluating whether changes in mining production have led to changes in observable characteristics of the populations in mining-producing areas compared to non-mining areas. In particular, I focus on household head characteristics such as birth district, age, sex, marital status, and education. Table 3.6 shows the effects of mining production on these variables. Estimates for gold and copper production are close to zero and are not significant, suggesting that mining production does not induce migration nor a compositional change in the electorate. Therefore, it is unlikely the results on 3.4 are driven by selective migration.

**Table 3.6:** Effect of Mining Production on Selective Migration

| Dependent variable: | D(born in in district) | Age | D(male) | D(married) | Years of education |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| log gold production | 0.001 | -0.004 | -0.001 | -0.001 | -0.008 |
| | [0.001] | [0.026] | [0.001] | [0.001] | [0.012] |
| log copper production | -0.001 | -0.072** | 0.000 | -0.002 | -0.016 |
| | [0.001] | [0.035] | [0.001] | [0.001] | [0.014] |
| Observations | 339,882 | 439,172 | 439,175 | 439,175 | 407,993 |
| Districts | 1397 | 1458 | 1458 | 1458 | 1458 |
| Mean of dep. var | 0.431 | 51.42 | 0.744 | 0.397 | 8.232 |
| Weighted | Yes | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes | Yes |
| Year FE | Yes | Yes | Yes | Yes | Yes |

D(born in district) is an indicator variable that takes the value of one if the household head reports being born in the district. Age corresponds to the age of the household head. D(male) is an indicator variable that takes the value of one if the household head is male. D(married) is an indicator variable that takes the value of one if the household head is married. Years of education correspond to the number of years of education of the household head. ENAHO Survey years span from 2004 to 2020. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral. Controls include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. All columns are weighted by the survey expansion factor. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

## 3.6 Concluding remarks

I examine changes in the Peru's two largest export products, copper and gold production. Using differences-in-differences estimators, I show that higher levels of gold production increases (decreases) the vote share for anti-mining (pro-mining) parties. Instead, an increase in copper production does lead to a close to zero and not statistically significant change in the vote shares for anti- or pro-mining parties. I find evidence consistent with voters caring about the environmental damage caused by gold production when casting their vote and prioritizing this effect compared to the improvement in socioeconomic indicators. Overall, this highlights that voters weigh heavily sociotropic considerations compared to self-interest

to decide for whom to vote.

## 3.7 Appendix

**Table 3.7:** The Effect of Mining Production on Voters and Votes

| Dependent variable: | ln Voters (1) | ln Votes (2) | ln Anti-Mining Votes (3) | ln Pro-Mining Votes (4) |
|---|---|---|---|---|
| [$a$] log gold production | 0.022* | 0.010 | 0.186*** | -0.069* |
| | [0.013] | [0.016] | [0.063] | [0.036] |
| [$b$] log copper production | 0.007** | 0.007* | 0.018 | -0.001 |
| | [0.004] | [0.004] | [0.014] | [0.008] |
| Mean of dep. var | 10.87 | 10.70 | 8.922 | 9.975 |
| p-value $H_0 : a = b$ | 0.278 | 0.857 | 0.009 | 0.043 |
| Observations | 6,456 | 12,912 | 11,238 | 12,912 |
| Districts | 1614 | 1614 | 1614 | 1614 |
| Weighted | Yes | Yes | Yes | Yes |
| District FE | Yes | Yes | Yes | Yes |
| Election FE | Yes | Yes | Yes | Yes |

ln Voters is the natural log of the number of registered voters for the election cycle (same for general and runoff elections). ln Votes is the natural log of the actual number of votes cast in each election. ln Anti-Mining Votes is the natural log of the number of votes cast for anti-mining parties. ln Pro-Mining Votes is the natural log of the number of votes cast for pro-mining parties. Sample includes national elections (general and presidential runoff) in 2006, 2011, 2016, and 2021. The unit of observation is a district-election cycle. Transformation of independent variables was done by estimating the inverse hyperbolic sine transformation on the total production of each mineral during years 1-5 in the Presidential term. Controls include the production of all other minerals produced in the country: silver, zinc, lead, iron, molybdenum, and tin. All columns are weighted by the number of registered voters in 2006. Standard errors clustered by district. *** p<0.01, ** p<0.05, * p<0.1

## 3.8 Construction of electoral dataset and main variables

This section provides detailed information on the construction of the dataset on classification of political parties regarding their position on mining production: anti-mining and pro-mining. In this regard, I am able to classify all political parties in general elections from the

available platforms.

**Table 3.8:** Policies considered to classify political parties

| Classification | Related policies in on-line platforms |
|---|---|
| Anti-mining | Prohibit export of non-refined minerals, unilateral renegotiation of contracts signed previously between Government and mining companies, government taking control of mining operations, modification or elimination of laws that promoted private investment in mining (such as contracts with assurance of tax stability) |
| Pro-mining | Promote mining investments, set mining taxes below competitor countries, reactivate paralyzed mining projects, simplify administrative permits for mining companies, development of related investments (such as ports) to promote mining, privatize any remaining mining projects belonging to the Government |

Notes: The party should advocate for at least one of the related policies mentioned in the Table to be classified as anti-mining or pro-mining.

**Table 3.9:** Details on the construction of variables

| Variable | Details |
|---|---|
| Anti-mining vote share | Sum of vote share across all parties classified as anti-mining by district-election |
| Pro-mining vote share | Sum of vote share across all parties classified as pro-mining by district-election |
| KM of road$_{i,t}$ | I overlay annual shapefiles of the national, department-level and neighborhood-level road network with the district shapefile. The variable corresponds to the sum of kilometers in those three levels of road network by district-year. I use R-Studio to perform this operation |
| Ln(Night lights$_{i,t}$) | I overlay annual NOAA DMSP-OLS images with the district shapefile. I use the average brightness of any pixel whose centroid lies within its borders. I use Earth Engine to perform this operation |
| Vegetation (NDVI)$_{i,t}$ | Average NDVI of any pixel whose centroid lies within each district's border. The spatial resolution is 1000m and it has a scale of 0 to 1. I use Earth Engine to perform this operation. |
| Land Pollution$_{i,t}$ | Average Land Aerosol Optical Depth (AOT) of any pixel whose centroid lies within each district's border. It is a product from the MODIS Aerosol Products and the spatial resolution is 10 Km. AOT measures the degree to which aerosols prevent the transmission of light by absorption or scattering of light. According to Wang and Christopher [2003], there is a positive and high correlation of 0.7 between the monthly means of AOT and PM 2.5, and above 0.9 between the monthly means of AOT and air quality index. The average AOT in Peru is 0.2. According to Jiay [2017], the mean of AOT for the United States is around 0.15 during this period, and a change of AOT at the magnitude of 0.01 indicates a significant change. I use Earth Engine to perform this operation. |

Notes: I use the 2015 districts-level shapefile for administrative boundaries.

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