On Voting, Violence, and Health:
Essays on Political Economics and Development

by

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Abstract

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The three essays conforming this thesis are representative pieces of my approach to analyzing the causes and consequences of economic underdevelopment. The overaching topic that ties together these essays is role that institutions and culture play in affecting specific behaviors that undermine development. The approach to the questions addressed in each essay is empirical, using data from Perú and Sierra Leone, and relies on economic theory to provide a general framework and deepen our understanding of the observed behaviors. Below, I provide a more detailed summary of the main findings of each chapter in this thesis:

In Chapter 1, "Turnout, Political Preferences, and Information: Evidence from Perú", I explore the role of electoral institutions that encourage citizens to vote on voter behavior. These institutions are widely used around the world, and yet little is known about the effects of such institutions on voter participation and the composition of the electorate. In this paper, I combine a field experiment with a change in Peruvian voting laws to identify the effect of fines for abstention on voting. Using the random variation in the fine for abstention and an objective measure of turnout at the individual level, I estimate the elasticity of voting with respect to cost to be -0.21. Consistent with the theoretical model presented in this essay, the reduction in turnout is driven by voters who (i) are in the center of the political spectrum, (ii) are less interested in politics, and (iii) hold less political information. However, voters who respond to changes in the cost of abstention do not have different preferences for policies than those who vote regardless of the cost. Further, involvement in politics, as measured by the decision to acquire political information, seems to be independent of the level of the fine. Additional results indicate that the reduction in the fine reduces the incidence of vote buying and increases the price paid for a vote.

Chapter 2, "Civil Conflict and Human Capital Accumulation: The Long Term Consequences of Political Violence in Perú"', analyzes the consequences of a long lasting civil conflict on human capital accumulation. In this chapter, I provide empirical evidence of

the long- and short-term effects of exposure to political violence on human capital accumulation. Using a novel data set that registers all the violent acts and fatalities during the Peruvian civil conflict, I exploit the variation in conflict location and birth cohorts to identify the effect of the civil war on educational attainment. Conditional on being exposed to violence, the average person accumulates 0.31 less years of education as an adult. In the short-term, the effects are stronger than in the long run; these results hold when comparing children within the same household. Further, children are able to catch up if they experience violence once they have already started their schooling cycle, while if they are affected earlier in life the effect persists in the long run. I explore the potential causal mechanisms, finding that supply shocks delay entrance to school but don't cause lower educational achievement in the long-run. On the demand side, suggestive evidence shows that the effect on mother's health status and the subsequent effect on child health is what drives the long-run results.

In the third and final chapter of this dissertation, "Transportation Choices, Fatalism, and the Value of Statistical Life in Africa", joint work with Edward Miguel, we take a look at the role culture plays in determining the willigness to pay to avoid life thretening situations. Specifically, we exploit a unique transportation setting to estimate the value of a statistical life (VSL) in Africa. We observe choices made by travelers to and from the airport in Freetown, Sierra Leone (which is separated from the city by a body of water) among transport options – namely, ferry, helicopter, speed boat, and hovercraft – each with differential historical mortality risk and monetary and time costs, and estimate the trade-offs individuals are willing to make using a discrete choice model. These revealed preference VSL estimates also exploit exogenous variation in travel risk generated by daily weather shocks, e.g. rainfall. We find that African travelers have very low willingness to pay for marginal reductions in mortality risk, with an estimated average VSL close to zero. Our sample of African airport travelers report high incomes (close to average U.S. levels), and likely have relatively long remaining life expectancy, ruling out the two most obvious explanations for the low value of life. Alternative explanations, such as those based on cultural factors, including the well-documented fatalism found in many West African societies, appear more promising.

To my parents, Ela and Juan Carlos, for always believing in me.
To Valerie, for her unconditional support, love, and patience.

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Preface

The causes and consequences of economic underdevelopment have been given a lot of thought within economics. The essays conforming this thesis are representative pieces of my approach to the topic. In these essays, I use empirical tools and economic theory to deepen our understanding of the way in which institutions and culture affect specific behaviors that undermine development. The inspiration to work on the specific questions addressed comes from life experiences and observations through several trips in Perú and Sierra Leone.

I was born two days before democracy was instated back in my country. Since then, Perú has formally been a democracy, however local and national governments do not seem to cater their policies towards the majority of (poor) citizens that voted them in office. This is a common fact across the developing world, where the poor represent a high share of the voting population, and yet governments consistently fail to deliver to these constituencies. The political science and economic literature has explored in depth one of the causes of this lack of representation, namely, poor accountability. However, one other potential explanation could lay in the incentives that the electoral institutions provide to voters. Chapter 1 analyzes the way in which a particular type of electoral institution and its enforcement mechanisms, i.e. fines for abstention in the context of mandatory voting, differentially affect groups of the population, generating changes in the composition of the electorate, and potentially affecting the outcome of the elections.

The findings of this essay contribute to answering the long standing question of why voters go to the polls when the probability of being pivot is extremely low, a question that can be traced back in the political science and economics literature at least to Downs' seminal work in 1957. My paper contributes to this literature not only by providing experimental evidence from the field showing the magnitude of voters' responsiveness to monetary incentives to vote, but also showing what type of voters are at the margin of voting, an important dimension in this literature, given that who votes ultimately determine what candidates (and policy platforms) get elected. Additionally, I show that voters who abstain do not have different policy preferences, which provides suggestive evidence indicating that if we were to eliminate mandatory voting, the outcome of the election could remain unchanged.

Lack of representation leads to institutional failure. In Chapter 2, I analyze the short- and long-term consequences of the extreme case of institutional failure and rupture of the social contract, i.e. civil conflict. My motivation to work on this topic comes from the fact that while growing up in the 1980's I was a first hand witness of the Peruvian civil conflict. When seeing how the conflict caused thousands of deaths, families separated, economic collapse, among other horrors, I couldn't help to wonder the extent to which the damage will last, especially among children, and even after the war is over. Specifically, in this chapter I analyze the how does being exposed to the civil conflict at different stages of life can have long lasting consequences in educational achievement. The essay emphasizes that experiencing shocks at early stages of life can have irreversible consequences on individual development.

More broadly, the findings of this chapter are related to two issues that are currently very salient in developing countries : (i) the presence of civil conflict, and (ii) the vulnerability to different shock, e.g. weather related or otherwise. While these shocks are as likely to happen anywhere in the world, the capacity of poor countries to cope with the consequences of such shocks is rather limited. This generates the need to provide a deeper understanding of the potential consequences of such shocks, and the extent to which they persist in certain groups of the population, which will allow us to focus relief efforts on them.

While shocks, due to civil conflict or otherwise, can severely limit the acquisition of human capital through effects on the supply side or exogenous shifts in demand, preferences can also play an important role in limiting human capital acquisition. The economics and medical literature has extensively documented the low investment in life saving technologies in developing countries. This low demand has been hypothesized to be related to poor information, high prices or low income, low life expectancy, among others. One factor that has been neglected in the literature is the role that culture plays in constraning the demand for health. In the essay presented in Chapter 3 (joint work with Edward Miguel), we use an unusual transportation situation in one of the poorest countries in the world, Sierra Leone, to illustrate this point. Using data collected on site about individual choices between transportation options that differ in the monetary cost they imply, the time it takes to complete the trip, and the risk of crashing, we are able to infer the willingness to pay for marginal reductions in mortality risk.

The results of the study show that Africans in our sample are willing give up very small amounts of money to reduce the risk of dying. After ruling out several of the leading explanations in the literature, the essay concludes that one potential candidate to explain this low willingness to invest in life saving technologies can be attributed to the important role that fatalism plays in African cultures.

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My beautiful and lovely wife, Valerie, deserves a special place. Always an example of how to pursue your dreams without hesitation. Her passion and hard work are something I will always look up to. I have to thank her especially for being so patient and loving, always willing to listen me rant about my frustrations, give me a big hug afterwards, make me feel better, and giving me the drive to keep going. Seeing you at home at the end of the day is always the best part of it.

Thanks to my family, who has always tried to interpret my few words and understand what I do, and why I do it far from home. They are where I come from, and it is easier to move around with these roots. Thanks to my friends back home, in Lima, who have instead given up trying to understand what I do. They always welcome me back at the bar or at the beach, over a beer and a good cebiche, with cheerful laughs and never ending deep conversations, they make me feel as if I had never left.

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Chapter 1

Turnout, Political Preferences and Information: Evidence from Perú[1]

[1]I am very grateful to my adviser, Elisabeth Sadoulet, for her patient guidance and advise. Edward Miguel provided unvaluable comments and feedback during the process of this project. Alberto Chong, Michael Conlin, Alain de Janvry, Ernesto Dal Bó, Fred Finan, Marco Gonzales-Navarro, Mitch Hoffman, Larry Karp, Valerie Koechlin, and Jeremy Magruder provided very insightful comments, suggestions, support, and encouragment. Special thanks to Alex Solís, my officemate, who has patiently heard the contents of this paper at least a thousand times, and always provided smart feedback. Seminar participants at at the University of San Francisco, Santa Clara University, ITAM, Michigan State University, University of Toronto, Bocconi University, Universitat Pompeu-Fabra, Bristol University, University of New South Wales, GRADE, NEUDC 2011 (Yale), UC Berkeley Development Lunch, ARE Development Workshop, ARE Department Seminar, and at the 5th Experimental Political Science Conference (NYU) provided useful suggestions. The data collection and experimental design were undertaken by César Ciudad and Leonardo González, at COSISE Red. I am extremely thankful to them for granting me access to the data and experimental protocol. Roberto Rodríguez provided superb research assistance. Alina Xu and David Arnold assisted with the data cleaning. Financial support from the Institute of Business and Economic Research (IBER) and Center for Evaluation and Global Action (CEGA) is greatly appreciated. The standard disclaimer applies.

1.1 Introduction

Thirty-three countries around the world encourage participation in elections through compulsory voting. Such laws are often believed to help ensure that all voters' preferences are adequately represented. However, it is not clear the extent to which voting incentives affect turnout. Further, voting institutions may change the composition of the electorate and therefore the outcome of elections. For example, mandating voting could distort electoral outcomes by inducing less informed or uninterested voters into the polls. On the other hand, mandatory voting could ensure representation of particular groups of voters', for example the poor, who might not vote otherwise. If the voting mandate were removed, this group's preferences will not be reflected in the policies enacted. Since, both voting and enforcement institutions are costly, there could be significant welfare losses if the objectives of higher participation and more involvement are not achieved.

To understand how voting institutions affect the outcome of an election, it is important to first explain voters' decision to participate, an open question for most of the economics and political science literature. Moreover, we need to know what type of voter is more likely to respond to incentives, the magnitude of voters' responsiveness, and the implications for public choice. In this paper I use data that combines a field experiment with a change in Peruvian voting laws, which allows me to identify the effect of fines for abstention on voting. I find that a reduction in the cost of abstention decreases turnout, and that this decrease is more than proportional among (i) centrist voters, (ii) those who have a lower subjective value of voting, and (iii) voters who hold less political information. These results are consistent with the predictions of the rational choice model of voter behavior with imperfect information presented in the paper.

More specifically, the experiment used in this project exploits the fact that knowledge about the reduction in the fine for abstention was not widespread. I use this data to study the 2010 municipal elections, where experimental variation in the perceived cost of abstention was generated by informing voters in the treatment group about the *new* levels of the fine for not voting. Voters assigned to the control group were reminded about the fine, without any mention of the exact amount. Using the random variation in the fine for abstention and an objective measure of turnout at the individual level, I estimate the elasticity of voting with respect to cost to be -0.21. Extrapolating the results, this means that if voluntary voting were implemented (i.e. the fine was reduced to zero), turnout would decrease from 94.2 percent to about 74 percent, roughly what we observe in countries where voting is voluntary.

Consistent with the predictions of the model, the reduction in turnout is driven by voters with specific characteristics: centrist voters, those less interested in politics, and the uninformed. However, this change in the composition of the electorate does not necessarily imply that the outcome of the election will be affected. Poor people are not more likely to respond to changes in the fine. Interestingly, voters whose turnout decisions are more sensitive to a change in the fine do not have distinct policy preferences. Furthermore, voters who respond to the reduction in the fine by abstaining do not acquire less political information. I further explore distortions in other markets induced by mandating voting. Specifically, I analyze how does a reduction in the penalties for not voting affect the market for votes, i.e. vote buying. My findings suggest that the exogenous change in the fine for abstention introduced by the treatment reduces the incidence of vote buying by 20 percent, and increases the price politicians pay for the marginal vote by 76 percent, which is consistent with an exogenous shift in the supply of votes.

Voting behavior has been studied by both economists and political scientists for a long time, yet there is no canonical model for understanding turnout decisions. While theoretical research modeling the determinants of voter turnout has increased in the last decade, few empirical studies have been conducted in the field to study voter behavior, let alone to test the predictions of these models. This is especially the case in developing countries. In this paper, I provide evidence supporting the predictions of one of the models derived from the classic *"calculus of voting"* literature (Downs, 1957; Rikker and Ordeshook 1968).[2]

The empirical results from this paper are closely related to several strands of the literature on voter behavior and electoral institutions. First, I contribute to the growing literature on the determinants of voter turnout.[3] The data used in this paper combines an institutional change with experimental evidence from the field to understand how a change in the incentives to vote affects turnout.[4] Unlike the previous literature, I am able to quantify the changes in the cost of (not) voting at the individual level. These changes

[2]Merlo (2006) and Martinelli (2007) provide excellent reviews of the theoretical models of turnout. The models available in the literature can be classified as those that emphasize the probability of being pivotal as the main motivation to vote (Borgers, 2004; Ledyard, 1984; Palfrey and Rosenthal, 1985); those that argue that citizens are driven to the polls to fulfill their civic duty and do the right thing (Harsanyi, 1980; Feddersen and Sandroni, 2006; Feddersen, Gailmard, and Sandroni, 2009, Coate and Conlin, 2004); and uncertainty voter models, which endogenize a component of the cost of voting (Deagan, 2006; Deagan and Merlo, 2009, Feddersen and Pesendorfer, 1996, 1999; Matsusaka, 1995).

[3]Several of these papers use large scale field experiments to identify the positive effects of different types of voter mobilization campaigns on turnout in the United States (Gerber and Green, 2000, 2001 and Gerber et al., 2003). This literature has also shown that social pressure is an important extrinsic motivation for voting (Gerber et al. 2008) and that voting is habit forming: voting in one election significantly increases the probability of going to the polls in the next election (Gerber et al., 2003). Another strand of the literature emphasizes that more informed voters are more likely to vote. Areas where the TV or radio coverage expanded earlier were more likely to show higher turnout (Gentzkow, 2006, Lasen, 2005). This fact has been shown to hold with specific information campaigns at the individual level (Banerjee et al., 2011). A few empirical studies more closely related to my paper use natural experiments to test whether changes in the cost of voting affect the likelihood of going to the polls in the election day. Brady and McNulty (2011) show that an increase in the cost of voting induced by an unexpected reduction in the number of polling stations in California's 2003 gubernatorial elections generated 3.03 percentage point reduction in polling place turnout, while absentee vote increases by 1.18 percentage points. Another commonly used source of exogenous variation is the presence of inclement weather conditions in the election day. These studies find that, on average, an additional millimeter of rain tends to reduce turnout by 1 percentage point (Knack, 1994, Gomez et al., 2007, Hansford and Gomez, 2010, Fraga and Hersh, 2010). In terms of partisan effects, the results are mixed.

[4]Laboratory experiments along these lines have been conducted by Gerardi et al. (2011).

in the perceived fine are induced by a randomly assigned treatment, which allows me to causally interpret the effect on turnout, and to provide the first estimates in the literature of the cost elasticity of voting, a parameter necessary for evaluating policy interventions affecting the cost of voting.[5]

To a large extent, the lack of credible evidence on the effects of electoral rules on turnout decisions is due to the fact that there are not many changes in electoral rules around the world. When there are, it is nearly impossible to collect individual level information, and more importantly, objective measures of turnout. Further, these institutions apply to every voter, which limits our ability to causally interpret changes in behavior. This paper contributes to the growing literature that uses field experiments to understand voter behavior in developing countries.[6] Experimenting with the salience and information about an institutional change is a promising research tool to get causal estimates from specific institutional features. New laws are passed frequently, and for different reasons, they are not always publicized or citizens are not aware of them because of selective and limited attention. Even though it is nearly impossible to randomize an institution, we can experiment with its salience and information about it.

A third strand of literature closely related to this paper analyzes how policy making responds to changes in the electorate. The standard median voter model predicts that any change in the composition of the electorate affects who gets elected through a change in the characteristics of the median voter (Persson and Tabellini, 2000; Husted and Kenny, 1997). Miller (2008) and Fujiwara (2011) analyze specific events in which groups of the population with identifiable policy preferences were enfranchised. As a consequence, they observe that policies respond to the new composition of the electorate. Unlike these studies, in the case analyzed here, there is no reason to expect that the groups that stop going to the polls due to a reduction in the fine have particular policy preferences. As such, though the reduction in the cost of abstention changes the composition of the electorate, I find that citizens who stop voting do not have significantly different policy preferences, which suggests that we should not expect changes in the policies enacted.

Finally, the results of the paper speak to the growing literature analyzing vote buying in developing countries (Finan and Schechter, 2011; Vicente, 2008; Vicente and Wantchekon, 2009). Government regulation can generate externalities in associated markets. A potential unexpected result of mandating voting could be to affect the market for votes. My results are consistent with a shift in the supply of votes caused by a reduction in the cost of abstention, thus reducing the incidence of vote buying, and increasing the price of each vote, making it more costly to politicians to influence the outcome of the elections.

In the next section, I present a theoretical model to characterize voter behavior and motivate the empirical analysis. Section 1.3 gives institutional background on the Peruvian electoral system and the change in the law that reduced the fine for abstention.

[5]Examples of such policies are the increase in polling stations, transportation to the polling stations, electronic voting, availability of ID cards, etc.

[6]Pande (2011) provides a comprehensive survey of this literature.

Section 1.4 explains the experimental design and the data that used in the empirical analysis, which is presented and discussed in Sections 1.5 and 1.6. Finally, Section 1.7 summarizes and discusses my findings.

1.2 The Model

In this section I present a slight variation of the basic model from Degan (2006), Merlo (2006), and Degan and Merlo (2011), in which I introduce an additional term of interest to motivate the empirical analysis. The objective of the model is to identify the voters who are at the margin between going to the polls or abstaining, which allows me to characterize the change in the electorate induced by a reduction in the fine for abstention.

The theory builds on a rational choice model where the voting decision is based on a threshold strategy: if the cost of voting is lower than the benefits, citizens go to the polls, otherwise, they abstain. I consider an election where voters share a common prior about the distribution of ideological positions of the candidates, but are uncertain about their actual positions. The net cost of voting has three components: (i) an exogenous benefit of voting, i.e. the utility derived from fulfilling one's civic duty, (ii) a fine for abstention, and (iii) an endogenous cost of voting, which is the utility loss due to the possibility of making a *voting mistake*, i.e. voting for a candidate whose ideological position is far from the voter's. This endogenous component drives the predictions of the model, which imply that a reduction in the cost of abstention will reduce turnout. Voters at the margin are the ones driving the reduction in turnout, and they (i) are in the political center, (ii) have a lower subjective value of voting, and (iii) are uninformed.

Assume that there are two candidates running in the election, which I denote by $j \in J = \{L, R\}$. Each candidate has a position y_j in a uni-dimensional policy (or ideological) space $Y = [-1, 1]$. We can interpret the ideological or policy space as left/right, where 0 represents the center. I denote by L the candidate who has the lower y_j, thus $y_L < y_R$.

Citizens know their own ideological position $y_i \in [-1, 1]$, but are uncertain about the candidate's position. From the voter's perspective, the candidate's ideological positions are random variables (y_L, y_R) distributed according to a joint probability distribution $F(y_L, y_R | y_L < y_R)$. Without loss of generality, I assume that $F(\cdot)$ is uniformly distributed on the support $[-1, 1]$. The main source of heterogeneity between voters is the amount of information each voter i holds about the candidates, which I denote by $\Omega_i \in \Omega$, a refinement of $F(\cdot)$. If a voter is completely uninformed about the ideological position of the candidates, she observes $F(\cdot)$, while if she has perfect information, $\Omega_i = (y_L, y_R)$, thus knowing exactly where the candidates are located. Information is assumed to be an exogenous, individual level characteristic.

Voters are also heterogeneous in the subjective benefit they derive from voting, or from fulfilling their civic duty. This utility is represented by d_i, which follows a uniform distribution on the support $[0, 1]$. There is a cost of not going to the polls, a fine for not voting, M_i. Voters observe a noisy signal about the level of the fine for not voting, and hence each voter has a different perceived fine ($M_i = M + \varepsilon_i$). For analytical purposes, I normalize M_i to range between zero (no fine) to one (maximum perceived fine).

The voter's problem can be conceptualized as a two stage maximization. First, she evaluates the costs and benefits of voting. If she decides to vote, she chooses between the two candidates based on which has a higher probability of being closer to her own ideological position, given her information set. The optimization over the turnout decision and candidate choice is thus given by:

$$\underset{t\in\{0,1\},v\in\{L,R\}}{Max} \; t\left[d_i - C(v_i\,;\,y_i,\Omega_i)\right] - (1-t)M_i \tag{1.1}$$

where, $t_i \in \{0,1\}$ denotes the turnout decision, $v_i \in \{L,R\}$ is the candidate choice, and $C(v_i\,;\,y_i,\Omega_i)$ is the utility loss associated with making a *"voting mistake"* by choosing candidate v_i, given the voter's position (y_i) and information set (Ω_i).

There is a continuum of voters of measure 1, hence no voter can be pivotal. This means that all the costs and benefits of voting are realized at the time of the election. Each citizen evaluates candidate y_j based on a utility function of the form:[7]

$$u(y_i,y_j) = -(y_i - y_j)^2 \tag{1.2}$$

The uncertainty in the candidate's ideological position generates the possibility of making a mistake by voting for the *"wrong"* candidate, which carries a utility loss. Given the information held by citizen i (Ω_i) and her ideological position (y_i), the voter's expected utility loss of voting for candidate L is given by:[8]

$$C(L\,;\,y_i,\Omega_i) = E\left[\mathbf{1}\left\{u(y_{i,}y_L) < u(y_{i,}y_R)\right\}\cdot\left(u(y_{i,}y_R) - u(y_{i,}y_L)\right)\,|\,\Omega_i\right] \tag{1.3}$$

Note that Equation (1.3) is greater than zero only when a *voting mistake* occurs, i.e. when a vote for candidate L is cast while she should have voted for R (i.e. when $u(y_{i,}y_L) < u(y_{i,}y_R)$). This utility loss is realized when casting the vote, and can be thought of as a sense of regret for choosing the *wrong* candidate. If a voter is perfectly informed, she always votes for the *correct* candidate and does not face any utility loss, thus $C(L\,;\,y_i,\Omega_i) = C(R\,;\,y_i,\Omega_i) = 0$. Voters who hold less information have a higher probability of making a *voting mistake*, and hence are more likely to face a utility loss.

Working backwards through the voter's problem from Equation (1.1), I characterize the candidate choice:

$$v^*(y_i,\Omega_i) = \begin{cases} L & if\; C(L\,;\,y_i,\Omega_i) < C(R\,;\,y_i,\Omega_i) \\ R & if\; C(R\,;\,y_i,\Omega_i) < C(L\,;\,y_i,\Omega_i) \end{cases} \tag{1.4}$$

[7] Alvarez (1998) provides a justification for the use of a quadratic functional form in the context of an electoral environment with uncertainty about the candidates' policy positions. All of the results in this section also hold for more general single-peaked pay-off functions of the form: $u_i = -|y_i - y|^\beta$, $\beta \geq 1$

[8] The expression for the utility loss of voting for candidate R is symmetric.

if $C(R\,;\, y_i, \Omega_i) = C(L\,;\, y_i, \Omega_i)$, the citizen randomizes between the two options. Simplifying the expression above, citizen i votes for candidate L *iff*:[9]

$$C(L\,;\, y_i, \Omega_i) - C(R\,;\, y_i, \Omega_i) < 0$$

$$E\left[u(y_i, y_L) - u(y_i, y_R)\,|\,\Omega_i\right] > 0 \tag{1.5}$$

Substituting the utility function (1.2) in Equation (1.5) and making the condition bind, one can derive τ_i, the ideological position y_i that will make the voter indifferent between choosing either candidates, i.e. makes Equation (1.5) equal to zero:

$$\tau_i = \frac{E\left[y_R^2 - y_L^2\,|\,\Omega_i\right]}{2E\left[y_R - y_L\,|\,\Omega_i\right]} \tag{1.6}$$

The optimal voting rule for voter i, $v^*(y_i, \Omega_i)$ is completely specified by the voter's ideological position (y_i), and her ideological cut-off (τ_i). Voter i chooses candidate L *iff* $y_i < \tau_i$, and candidate R *iff* $y_i > \tau_i$. If the information set held by citizen i is $\Omega_i = (y_L, y_R)$, the cut-off will be exactly the midpoint between the two ideological position of the candidates: $\tau_i = \frac{y_L + y_R}{2}$, and when $\Omega_i = F(\cdot)$, the cut-off is zero. Given the assumption on the distribution of $F(\cdot)$, τ_i is symmetrically distributed with mean zero. Note that the previous formulation always leads to sincere voting. Unlike other theoretical settings (Feddersen and Peserdorfer, 1996), there is no strategic voting in this model.

Using this result, we can characterize the turnout decision, given that the utility loss of voting is $C(y_i, \Omega_i) \equiv C(v_i^*(y_i, \Omega_i))$:

$$t(y_i, \Omega_i) = \begin{cases} 1 & if\ C(y_i, \Omega_i) - d_i \leq M_i \\ 0 & if\ C(y_i, \Omega_i) - d_i > M_i \end{cases} \tag{1.7}$$

The model predicts that an exogenous change in the cost of abstention (M_i) will cause lower turnout. Further, voters at the margin between going to the polls and abstaining can be characterized in terms of the three dimensions of heterogeneity. Hence, upon a reduction in M_i, we will observe that citizens who abstain will more likely be those who:

1. *Have an ideology closer to τ_i:*

   Note that the utility loss of voting $C(y_i, \Omega_i)$ peaks at the ideological cutoff τ_i. Intuitively, the closer a citizen is to her ideological cut-off, the more likely she is to make a *"voting mistake"* for any pair (y_L, y_R). Hence, the payoff loss associated with voting is higher for voters closer to τ_i.[10]

   Given that τ_i is symmetric with mean zero, voters with centrist ideology will face a higher expected loss from voting, and thus (in expectation) will be at the margin.

[9] The expression is symmetric for the vote for candidate R.

[10] Take for example any two generic citizens, j and k with ideological positions $y_j < y_k < \tau$. For any candidate positions (y_L, y_R) for which both citizens make a voting mistake by voting for L, the associated payoff loss is higher for citizen k as long as $u_i(\cdot)$ is strictly concave.

2. *Have a lower subjective benefit of voting* (d_i)*:*

   The parameter d_i follows a uniform distribution, which is independent of Ω_i (and thus of the utility loss of voting). From Equation (1.7), it is clear that a lower d_i implies a higher net cost of voting, and thus, for any ideology or information set the probability of voting is lower.

3. *Have less information:*

   $C(y_i, \Omega_i)$ is decreasing on Ω_i, implying that less informed people are more likely to make a *"voting mistake,"* and hence have a higher expected utility loss of voting for any given y_i.

The predictions of the model will be tested in Section 1.5.

1.3 Institutional Background

Since 1933, voting in Perú, as in most Latin American countries, is mandatory for all citizens between 18 and 70 years old. Abstention is penalized with civil disenfranchisement. Citizens who are unable to show proof of voting (an official stamp on the ID card) are denied public or private services for which official identification is required.[11] In order to get back full citizenship, a fine has to be paid in the National Bank, and once the payment is done, the bank official places a stamp on the ID card. De facto, enforcement of the sanctions is mixed: it is usually stronger at banks, the judiciary, public notary, passport or driver license offices, or the public registry. Softer enforcement is usually observed at lower levels of government or basic service delivery, such as police stations, municipalities, birth or death registry, social programs, among others.[12]

The high level of the fine for abstention has historically led to high turnout. For example, in the June 2006 presidential election, 87.7 percent of the eligible population (18 years old or older) voted, while in the local elections held in 2002, turnout was 83.1 percent.[13] Until 2006 the fine was S/.144 (144 Nuevos Soles, $\backsim US\$50$), which represented

[11] Civil disenfranchisement implies an effective ban on getting official certificates from the national registrar, taking part in any judiciary or administrative process, signing a contract, taking a government job, getting a passport, being part of the social security system, getting a driver's license, or in general identifying themselves officially (which includes doing any transaction in a bank, such as cashing a check). Not having voted in an election does not restrict the right to vote in any other election.

[12] In Perú, the official ID card is used for voting, thus most of the population older than 18 years old is registered to vote. Votes can only be cast in person on the election day, and citizens can only vote in the district where they are registered. In case someone lives in a district different from the one where she is registered, she is subject to the fine level of the latter. Voting by mail or other mechanism for remote or delayed voting is non-existent.

[13] The mild enforcement is reflected in the percentage of the population that actually pays the fines. For example, in the November 2006 local elections, out of the 12.4 percent of abstainers, about 14.1 percent of them had paid their fines as of July 2010. In urban districts, this proportion is higher. For example, in the region of Lima, the abstention rate was 11.87 percent, and out of the abstainers, 17.9 percent paid

about 26 percent of the minimum official monthly wage. That year, Congress started discussing whether or not to change voting to a voluntary regime, with strong proponents on both sides. A final agreement was reached in August 2006, when it passed a law according to which voting was still mandatory, but the fine was reduced for everyone, with larger reduction for citizens registered in the poorest districts.

The poverty level of the district was determined based on a ranking generated by the national statistical institute (INEI). Overall, districts were classified into one of three poverty (and fine) categories: abstainers registered in non-poor districts (184 municipalities) are subject to a fine of S/.72 ($\backsim US\$25$); those in poor districts (793 municipalities) saw the fine reduced to S/.36 ($\backsim US\$12.50$), while in extremely poor municipalities (852 municipalities), the fine was reduced to S/.18 ($\backsim US\$6$).

Importantly, no major news outlet reported the changes in the fine, and no campaigns were conducted to spread the information about the new fine structure.[14] In fact, most of the population is still uninformed about the *new* fine, as will be shown in Section 1.4. The fact that electoral laws changed, and that very few people were informed about it, presented a unique opportunity to explore the effects of (dis-)incentives to vote on voter behavior, and to test the predictions of the model.

1.4 Experimental Design and the Data

The goal of the empirical analysis is to identify the effects of changes in the cost of abstention on turnout by comparing voters exposed to different levels of the fine. One way to address the question would be to compare voting behavior of citizens in districts with different level of the fine for abstention, however this strategy would face two major challenges. On the one hand, the fact that voters are not informed about the new levels of the fines imply that the researcher would not observe any variation in the independent variable of interest (the perceived fine). Even if this variation were observable, it would probably be correlated with other relevant variables, such as information, or interest in politics, which leads to a bias in the estimated effects. Additionally, it would be impossible to disentangle the effect of district specific characteristics, such as the electoral context (candidates running for office, availability of polling stations, etc.) or poverty level, from the effect of the different fine levels. For example, given the well documented association

the fine as of July 2010.

[14] *El Comercio*, the major newspaper in the country only published two very short articles about this on July 6^{th} (when the law was still under debate) and on November 20^{th}, 2006 (the day after local elections were held). Additionally, the government offices in charge of publicizing electoral rules and providing electoral information, the ONPE (National Office of Electoral Processes) and the JNE (Electoral Jury), get a share of their annual revenues from the collection of these fines and use turnout as a performance indicator, hence they did not have incentives to publicize the new law. In 2004, the share of the budget of the ONPE coming from collection of fines was 24.5 percent, while for the JNE, this share was 30.5 percent. Informal conversations with government officials at the time indicated that the heads of both offices were committed to keeping high turnout in elections, so no efforts were made to publicize the law.

between wealth and turnout (Matsusaka, 1995, Perea, 2002, Frey, 1971), if we compared turnout in the average poor district with that in the average non-poor district, we would not be able to know whether the differences are due to wealth or the fine.

One way to isolate the effect of district specific characteristics from different levels of the fine would be to compare districts that are just on the threshold between being classified as poor and non-poor, or between being extremely poor and poor. In expectation, districts that are just on both sides of each of the thresholds should be comparable in all relevant characteristics. Further, if we believe that the monetary cost of abstention matters in the decision to vote, had voters been informed about the reduction in the fine, we would observe a decrease in turnout in the elections that took place after the reduction in fines, i.e. the November 2006 and October 2010 local elections. On the other hand, this change in turnout would not be present in the elections that took place before the law came into effect, for example in the 2002 local elections.

Figure 1.1 shows the results of a regression discontinuity analysis for the last three local elections (2002, 2006, 2010).[15] For each of these elections, districts are ranked from richest to poorest, plotting their turnout, and fitting a cubic polynomial for municipalities in each of the three poverty levels.[16] The vertical lines indicate the thresholds at which a district is categorized as non-poor, poor, or extremely poor. There is no statistically significant difference in turnout between districts located at each side of the thresholds in any of the elections analyzed, as one would expect if the population were informed about the new levels of the fine.

The results presented in Figure 1.1 can be interpreted as evidence that changes in the monetary cost of not voting do not influence the decision to go to the polls. Alternatively, it could mean that the cost matters for turnout decisions, but that voters were not informed about the change in the fine. Voters decide whether or not to go to the polls based on their perceived cost of abstention, and if these beliefs are still aligned with the old level of the fine (which did not vary across poverty categories), we shouldn't expect to see a difference at each threshold.

1.4.1 Experimental Design and Sample

Following the latter interpretation of the results from Figure 1.1, an experiment was designed to generate within district, individual level variation in the cost of abstention.[17]

[15] For the 2010 elections, I exclude the 10 districts where the experimental data was collected from the sample to allow a cleaner comparison. The plots for 2002 and 2006 include these districts, but the basic results remain the same if I exclude them. The regression versions of the Figure are available upon request.

[16] In municipal elections, voters elect the mayor for the district, the mayor for the province, and the regional president. These are the three sub-national levels of government. In this paper, I use district and municipality interchangeably.

[17] The data collection and experimental design were entirely undertaken by César Ciudad and Leonardo González, at COSISE Red. I am extremely thankful to them for granting me access to the data and experimental protocol.

This was done by randomly providing information on the actual levels of the fine to voters in 10 districts in the Region of Lima just before the municipal elections of October, 2010. After the election, all the subjects in the treatment and control groups were re-interviewed and, among other information, an objective measure of turnout was collected by asking respondents to show official proof of voting. The advantage of this strategy is that it allows to compare an objective measure of the voting behavior of people who likely believe that the fines were still at the previous level (control group) with those whose information set had been updated by the treatment.

Within each district, villages (in rural areas) or neighborhoods (in urban areas) were randomly sampled, and within each village interviews were conducted with individuals eligible to vote (between 18 and 70 years old) from a random sample of households.[18] By clustering the randomization at the village level, comparisons within villages can be made, thus isolating the effect of any district (and village) specific characteristic. The unit of observation is the individual, but the treatment status is determined at the household level, hence in the empirical analysis I allow for arbitrary correlation of the errors within the household by clustering them at that level. Figure 1.2 shows the location of the districts in a map, indicating their poverty category.

The baseline interview took place between one and four weeks before the municipal elections of October 3^{rd}, 2010. Questions regarding household characteristics, composition and expenditures were included. Also the survey recorded information about basic demographics, political preferences, policy priorities for the district, knowledge about the current electoral process, past voting, and usage of public services. Importantly, everyone was asked whether they knew if there were consequences for not voting. If the respondent answered that there was a fine, the survey inquired about the amount of the fine. At the end of the interview, the enumerator provided the treatment.

If the household was chosen to be part of the treatment group, the enumerator read a script informing the respondent about the level of the fine in effect in the district where

[18] In the national census, the villages are called “centro poblado.”

she was registered to vote.[19] In order to reinforce the message, the enumerator showed a copy of the official newspaper where the law was published, also she gave the respondent a flier with the exact text of the script. To avoid differential salience between the treatment and control group, the latter received a reminder that voting is mandatory and that there is a fine for not voting (without mentioning anything about the amount of the fine).[20] Respondents in the control group also received a flier repeating the script.

The follow-up survey was gathered between one and three weeks after the election. The main variable collected in the survey was whether or not each respondent voted in the election. Voting is measured through a self reported variable, but also an objective measure of voting was collected by asking each respondent to show their ID card, where the enumerator confirmed if it had the official stamp or not.[21] Among the 2,276 respondents in the follow-up survey, only 5 of them refused to tell the enumerator whether they voted or not. 67 percent of the respondents agreed to show their ID cards. There does not seem to be a tendency to lie about voting. Out of those for whom I have the self reported and objective measures of voting, only 6 respondents reported that they did not vote, and their ID cards had the official stamp, while the opposite happened in 7 cases. 11.6 percent of voters who refused to show their ID cards (or claimed not to have them at the moment of the interview) reported having abstained.

Given the low lying rate, in order to maximize the sample size, in the analysis I define

[19] Along the questionnaire, we asked every respondent the district where she is registered to vote. Every enumerator had a list of the 1,834 districts in the country, with their corresponding poverty level, so they were able to tell each respondent the exact level of the fine applicable the district where she was registered. The script for the treatment group was as follows (see Figure A.1):

> *Dear Sir/Madam,.*
>
> *On August 2006, Congress passed a law in which the fines for not voting were reduced (Ley No. 28859). According to this law, those who do not vote are no longer subject to a fine of S/.144, but the fines are now lower for everyone, and they vary according to the poverty level of the district where you vote.*
>
> *According to the information that you just provided me, if you do not vote in the upcoming elections you will be subject to a fine of S/.(AMOUNT IN THE DISTRICT WHERE SHE'S REGISTERED).*

[20] The exact script for the control group was as follows (see Figure A.1):

> *Dear Sir/Madam,*
>
> *In Perú, voting is mandatory by law, and not voting is subject to a sanction that implies a fine.*

[21] The option to pay the fine and get the official stamp in the ID card is only available once the full voting record is centralized, which usually happens more than a month after the elections. Hence the only way in which the respondents could have the stamp at the moment of the interview was by having voted.

the turnout variable based on the objective measure of voting for those who showed their ID, while I take the self reported values for those who did not. In the empirical analysis in the next section I show that the results are robust to using only the self reported or objective measure of voting. The survey also included questions about political preferences, information about the political process, the candidates and parties running, and a battery of questions about vote buying.

1.4.2 Descriptive Statistics

Overall, the baseline and follow-up surveys contain information about 2,276 individuals from 1,668 households. I provide the descriptive statistics for the balanced sample of respondents in Table 1.1. Voters registered in extremely poor districts represent 23 percent of the sample, while 38.8 percent vote in a poor district and the remaining 37 percent in a non-poor district. On average, 42 percent of the sample is male, they are about 40 years old, with 9.6 years of education, and spend S/.255.1 ($\backsim US\$94$) per capita per month.

The ideological position of the population is highly concentrated in the center, with 8.3 percent locating themselves in the left and 25.1 percent in the right. This outcome comes from self reports in a scale ranging from extreme left (1) to extreme right (5). I take the categories in the middle (2, 3 and 4) to represent the political center. Ideology is not unidimentional, and thus I use a second measure based on policy preferences to capture a broader range of ideological distributions. In the survey, voters were asked to name (in order) the first five policies that they would implement if elected mayor of the district. This was an open question, and the enumerators placed the answers in one of twenty eight policy categories. For each of these categories, the policy preferences are ordered from not mentioned (zero) to most preferred (five). I aggregate these questions by taking the first principal component, and dividing the sample into quintiles. The center is defined by those in the quintiles 2, 3, and 4, while the first and fifth quintiles define the ideological extremes.[22] The Policy Extreme 1 is related to preference for public goods, such as health and education infrastructure, roads, accessibility, etc. On the other hand, the Policy Extreme 2 is associated with public goods which are more easily appropriated by an agent (club goods), such as youth labor training, security, promotion of private investment, etc. The questions that define the ideological position of each voter were asked in the baseline survey, before the treatment was administered, so they can be taken as predetermined.

The subjective value of voting is a difficult concept to quantify, and as such is approximated by using different variables that measure the interest voters have on politics, the current electoral race and the campaign. Very few people (8.2 percent) declare themselves to be very interested in politics, while 46.8 percent are somewhat interested, and 45.1 percent are not interested at all. The small interest in politics is also reflected in a small

[22]The coefficients for each policy item loading into the principal component analysis are listed in Table A.1.

proportion of people who declare themselves to be very interested in the results or the campaign of the current election (39.9 percent and 10.5 percent, respectively). Respondents who are somewhat interested in the results of the election represent 44.3 percent of the sample, while 55.6 percent are somewhat interested in the campaign. Finally, 15.3 percent and 33.9 percent are not interested in the results or the campaign, respectively. It is important to note that none of these questions were placed one after another, but rather as separate as possible. Most of them were asked in different modules of the questionnaire in order to avoid confirmatory bias in the responses.

Political knowledge and information are measured in several ways. Open ended questions asked respondents to name all the candidates and parties running in the election for the municipality where they are registered to vote. In order to get a uniform measure of knowledge, I express the knowledge indices as the ratio of the number of candidates (and/or parties) that the respondent is able to name, divided by the total number of candidates (and/or parties) running in the district's election. On average, respondents are able to name 38.8 percent of the candidates and 29 percent of the parties running. Additionally, the survey includes questions about the political process in general. 17 questions about knowledge of the political structure of the country, and electoral rules were asked.[23] On average, respondents were able to get 9.3 questions right (54.7 percent).

Table 1.1 provides descriptive statistics for the treatment and control group, showing that there are no statistically significant differences by treatment status in the relevant variables.[24] Even though there was not a lot of time between the baseline and follow-up surveys (30 days, on average), the survey team was unable to track down about 13 percent of the households from the baseline survey, which represents 19.8 percent of the respondents interviewed in the baseline. Table A.2 shows the balance of variables between attrited individuals and those who were tracked. Overall, the sample of attriters seems to be not statistically different from those who were tracked, and thus we should not expect the attrition to imply any biases to the estimated results.

The main variable of interest is the perceived fine for abstention.[25] Given that the treatment was randomly allocated, we should observe that the perceived fine is balanced

[23]The questions include information about the length of the term, reelection possibilities for two consecutive periods, length of term, and existence of run-off elections for president, congressmen and mayor, the official minimum and maximum age for which voting is mandatory, and which are the government institutions in charge of the elections, ID cards and political claims.

[24]Differences between treatment and control are not significant within each of the poverty levels. The only variable that seem to be systematically unbalanced is the proportion of voters who are on the left. The control group seems to have a higher proportion of leftists than the treatment group. Details are available upon request to the author.

[25]The question was structured in the following way: First, respondents were asked if they knew what were the consequences of not voting. If among the answers, the respondent mentioned a fine, she was asked if she knew how much was it. For people who did not mentioned a fine among the sanctions for not voting, I assume that she thinks that there is no fine (i.e. it is S/.0). Also, if the respondent mentioned a fine among the consequences of abstention, but did not remember the exact amount, she was asked to place the fine in a range, where each of the ranges provided include the new levels of the fine. For voters who chose one of the ranges, I use the median of each range as their perceived fine.

between the treatment and control groups within each poverty category. Figure 1.3 shows the distribution of this variable in the baseline and follow-up surveys for the control and treatment group by poverty level of the district where each respondent is registered to vote. In each graph, the vertical line represents that actual level of the fine.[26] Importantly, in the baseline survey the average respondent reports that the fine for not voting is S/.122.29 (see Panel A of Table 1.2), which is very close to its level before August, 2006. This confirms that the majority of the population was not informed about the change in the voting laws. There is significant dispersion in the data, ranging from people who think that voting is voluntary (i.e. reports that the fine is zero), to those who think that the fine is around S/.300. The distributions of these perceptions do not differ by treatment status within each poverty level. Panel A of Table 1.2 shows the mean perceived fine in each of the groups, as well as the t-tests for differences in means.[27]

Not only those in the treatment group learned that the fines for not voting had decreased. For example, the average respondent registered in a non-poor district who received the treatment reports in the follow-up survey that the fine for not voting is S/.66.77, while the non-poor in the control group the average perceived fine is S/.90, which is significantly lower than the S/.126 reported in the baseline survey. The difference between treatment and control groups among voters from non-poor districts is statistically significant. For people voting in poor districts, I find a similar pattern. The distribution of perceived fines clearly moves to the left for both the treatment and control groups but the former is centered at S/.42, which is close to the actual S/.36 stipulated for this group, while the control group reports on average that the fine is S/.71. Voters from extremely poor districts are more likely to learn about the new levels of the fine. While the treatment group reports a perceived fine of S/.19, the mean for control group is S/.36. This is also apparent from Figure 1.3, where we see that the distribution of perceived fines shifts to the left, for both the treatment and control groups. Overall, the treatment had the desired effect of informing the population about the new level of the fine, however the control group also learned about the new fines. This is especially true for people voting in extremely poor districts.

As Panel B in Table 1.2 shows, 94.2 percent of the respondents voted in the October 2010 elections.[28] The effective reduction in the cost of not voting led to lower turnout. On average, respondents in the treatment group were 3.1 percentage points less likely to show

[26]In the left panel, for the baseline survey, the vertical line represents the old level of the fine (S/.144), while in the graphs in the right, the lines are set at the new levels of the fine: S/.72 for voters in non-poor districts, S/.36 for those in poor districts, and S/.18 for voters in extremely poor districts.

[27]These results represent the direct effect of the treatment on the perceived fines, i.e. the first stage of the regressions without controls.

[28]There are two reasons why turnout in the sample is higher than the official statistics. First, the sampling framework only included voters between 18 and 70 years old, whereas the official turnout rate is computed among all registered voters, thus including voters who are older than 70 (who are no longer mandated to vote). Second, conversations with government officials in Perú suggested that the electoral roster is not perfectly updated, thus there is a substantial number of dead voters who's names are still in the official roster.

up to vote the day of the elections. This result can be interpreted as a reduced form effect, or the direct effect of the treatment on turnout. The magnitude of this effect is related to the magnitude of the reduction of the perceived fine. In non-poor districts the reduction in the fine led to a difference of 2.1 percentage points in turnout between the treatment and control groups. Likewise, in poor districts, treated voters are 5.1 percentage points less likely to vote, while voters in extremely poor districts turnout decreased in 1 percentage point (not significant).

The low and non-significant effect for the extremely poor is not surprising, since the treatment did not differentially affected voters in the treatment and control groups.[29] Overall, the perceived fine for the extremely poor were on average lower for everyone. As a consequence, in these districts, the average turnout is at least 2 percentage points lower than in the control group in poor and non-poor districts (93.5 percent versus 96.7 percent and 95.9 percent, respectively). Given that the experiment did not affect the perceived fines for the extremely poor, I drop them for the subsequent analysis.[30]

Summarizing, the descriptive data shown above supports the basic hypothesis that a reduction in the fines for not voting leads to lower turnout. The next section outlines a more formal framework to test the predictions of the model presented in Section 1.2.

1.5 Empirical Strategy and Results

1.5.1 Basic Facts

The empirical strategy implemented to test the predictions of the theoretical model outlined in Section 1.2 exploits the exogenous variation in the change in the perceived fine provided by the treatment status in order to identify its effect on turnout. The local average treatment effect identified from the instrumental variables regressions will thus estimate the effect of a reduction in the fine for abstention on turnout for voters whose beliefs about the fine were updated.

The first part of the empirical analysis looks at the direct effect of the treatment on turnout. The reduced form equation is given by:

$$Vote_{ij} = \beta_1 NP_{ij} \cdot T_{ij} + \beta_2 P_{ij} \cdot T_{ij} + \beta_3 P_{ij} + \beta_4 NP_{ij} + \gamma X_{ij} + \delta_k + \eta_{ij} \tag{1.8}$$

$Vote_{ij}$ is an indicator of whether voter i, registered to vote in district j, voted in the election of October 3^{rd}, 2010. The treatment status is given by the indicator variable T_{ij}. Given that there are two distinct treatment groups depending on the poverty level of the

[29]Learning in the control group in extremely poor districts is associated with the time between the baseline and follow-up surveys (30 days, on average). The amount of time between the surveys is not statistically different between voters in districts with different poverty levels, but I observe that learning happens more often among the extreme poor, and the effect is independent of the size of the village.

[30]I have run all the tables below including the extreme poor, and they are available upon request. All of the patterns and main results remain unchanged. The main results including this group are shown in the Table A.3.

district where voter i is registered, in all the regressions I separate the effect of the different treatment levels by interacting the treatment dummy with the poverty level of the district (NP_{ij} for voters from non-poor districts, and P_{ij} for those from poor districts, while the extreme poor are the excluded category). The inclusion of the dummies indicating the level of poverty of the district where voting allows restricting the comparison to treatment and control units within the same level of the fine. I also include some relevant controls that are likely to affect voting decisions, such as age, the log of per capita expenditures, education and gender. These variables are included in the matrix X_{ij}. Finally, δ_k denotes a fixed effect at the level of the village where interview took place (where the respondent lives), and η_{ij} is a random error term.

It is not straight forward that we should expect a reduction in the fine for not voting to cause lower turnout. Gerber et al. (2003) show that voting is habit forming, and voting in one election makes voters significantly more likely to vote in the next election. In the Peruvian context, where mandatory voting has been in place for more than 80 years, and turnout is consistently high, it could be that the habit effect is stronger that the monetary effect. Table 1.3 presents the reduced form estimates of the effects of the treatment on turnout. Overall, the monetary effect seems to dominate the habit effect. Treated voters in non-poor municipalities are 2.7 percentage points less likely to vote than the controls in this poverty category (Column 1). Likewise, voters in poor districts showed up at the polling station 5.2 percentage points less often than the ones in the control group in the same poverty category (Column 2). Pooling voters does not affect the magnitude of significance of the results (Column 3). All the regressions shown include controls and village fixed effects, and the standard errors are clustered at the household level.[31] These results are remarkably similar to the descriptive statistics shown in Panel A of Table 1.2.

The decrease in turnout is roughly proportional to the official decrease in the fine. In non-poor districts, where the fine was reduced by 50 percent, the effect of the treatment on turnout is 2.1 percentage points, while in poor districts, where the fine was reduced to one fourth of its original level, it is roughly double that size (5.1 percentage points). Voters update their beliefs differentially, and in order to say something about the magnitude of their response to different changes in the fine for not voting, we need to scale the reduced form findings by the change in the perceived fine caused by the treatment. The first stage regression in the instrumental variable approach measures this, and is given by:

$$\triangle Fine_{ij} = \beta_1 NP_{ij} \cdot T_{ij} + \beta_2 P_{ij} \cdot T_{ij} + \beta_3 P_{ij} + \beta_4 NP_{ij} + \gamma X_{ij} + \delta_k + \nu_{ij} \tag{1.9}$$

$\triangle Fine_{ij} = (Fine_2 - Fine_1)_{ij}$ represents the change in the perceived fine between the follow-up and baseline surveys. In this case β_1 and β_2 tell us the difference in the average change in the perceived fine between the treatment and the control group for voters from non-poor and poor municipalities, respectively. This comparison is made within the same poverty level of the district registered and between people who were interviewed in the

[31] The results are very similar when I do not include controls, or village fixed effects, as shown in column (1) in Table A.4.

same village.

The results from the first stage regression are displayed in Table 1.4. Column (1) present the results for voters registered in non-poor municipalities: the difference in the perceived fine for the treatment and control groups is S/.18.8. Similarly, the treatment effect for voters in poor districts is a reduction in the perceived fine of S/.30.5. Column (3) pools the results. Overall, Table 1.4 provide a strong first stage for my IV strategy, with an F-statistic for the excluded instruments of 28.7 in the pooled specification.

In the second stage, I look at the effect of the changes in the perceived fine, instrumented by the treatment status in each poverty level, on turnout. The regression equation is displayed in Equation (1.10):

$$Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij} \tag{1.10}$$

β_1 is the estimated local average treatment effect (LATE) of a change of S/.1 in the fine for not voting on the likelihood of voting for those whose information was updated due to the treatment. The main identifying assumption is that the treatment only affects turnout through the change in the perceived fine, and hence the treatment is uncorrelated with ϵ_{ij}. The fact that the treatment was randomized, and that the main variables in the analysis are not statistically different from each other between the treatment and control groups supports this assumption.

The instrumental variables results are presented in Table 1.5. An exogenous decrease in the perceived fines for not voting cause fewer people to attend to the polls. A reduction of S/.1 in the fine for abstention causes a significant decrease in the likelihood of voting of 0.14 percentage points for non-poor voters, as shown in Column (1). Similarly, for voters in poor municipalities, the effect is of 0.17 percentage points (Column (2)). Pooling the results, the average voter in my sample is 0.16 percentage points less likely to go to the polls (Column (3)). The average voter, who perceives that the fines were reduced by S/.56.65 (45.7 percent from her initial perception of S/.124), has a 9.59 percentage points (10.15 percent) lower probability of voting. This implies a reduction in turnout from 94.5 percent to 85.4 percent, and a price elasticity of voting of -0.21.[32]

Extrapolating these results to the whole population, driving the fines to zero could lead turnout to 74.7 percent, a level comparable to the one observed in some countries where voluntary voting is in place. To put these results in context with the previous evidence, Gerber et al. (2008) find that reminders to vote emphasizing social pressure messages cause an increase in turnout between 4.8 and 8.1 percentage points. In my experiment, a reduction of S/.56.7 ($\backsim US\$20$) leads to a reduction in turnout of 9.6 percentage points.[33]

[32]The reduced form, first stage and two stage least squares including the extremely poor are shown in Table A.3. Similarly, Table A.4. shows the main results without controls. In both Tables, the main results remain unchanged.

[33]Gerber et al. (2008) found that sending mailings informing recipients that it is public information whether or not they voted and listing the recent voting record of each registered voter in the household had an effect of 4.8 percent on turnout. Listing not only the household's voting records but also the voting records of those living nearby led a 8.1 percent higher turnout.

Table A.5, shows the heterogeneity of the effects of the reduction in the fine on voting by several demographic characteristics. Overall, I find the effect is constant between people of different ages, educational levels and expenditure levels. However, women seem to be significantly more sensitive to changes in the perceived fines. Contrary to what is commonly believed, poor voters are not more likely to respond to changes in the fine for not voting, which is consistent with the constant elasticity found.

1.5.2 Robustness and Validity Checks

One potential concern with the interpretation of my result is that the elasticity of voting with respect to the cost might not be constant. Computing the elasticity using the results from the separate estimations, I find that the for non-poor it is -0.18, as compared to -0.21 in poor districts. These elasticities are not statistically different from each other. This evidence supports the idea of a constant price elasticity. In itself, this is and important result for the Peruvian representation system, since the largest reduction in the fine took place in the poorest districts, and hence turnout would be reduced more than proportionally in these groups.

It is important to note that when I split the sample I am only using one instrument in each regression, rather than two. Still, the first stage regressions have very strong predictive power, with F-statistics ranging between 14.7 and 41.03, which reinforces the idea that the previous results are not driven by one of the two instruments in the first stage.

An important robustness check regards measurement of the dependent variable. As mentioned above, the dependent variable is constructed based on both self-reported and objective measures of voting. I run the main specification with both variables separately and with different sample sizes in Table 1.6. The results are very similar across the different samples and voting measures. In the sample for which both self-reported and objective voting measures are available, turnout is higher since people who reported not voting were less likely to show their ID cards. In this sample, the results using the self reported measure of voting is attenuated but still large and significant.

Table 1.7 presents a validity test for the effect of the treatment on turnout. If the treatment did affect the perceptions about the magnitude of the fines, it should have affected turnout in 2010, but it would have had no way of affecting past behavior. Table 1.7 shows the results of running the same specifications as in Table 1.5, but using turnout in 2006 as a dependent variable. The change in the perceived fines do not have a statistically significant effect on the self reported measure of voting in 2006. Also, it is reassuring to see that the coefficients across the different samples are very close to zero.

1.5.3 Ideological Position

The model predicts that voters with a centrist ideology are more likely to abstain upon a reduction in the fine, since they are more likely to make a *voting mistake*. The random variation in the cost of not voting provided by the treatment allows me to causally interpret the effect of changes in the cost of abstention induced by the treatment on turnout within each ideological position category. That is, the interactive term between the change in the perceived fines and the ideological position, instrumented by the treatment dummies and their interactions, provide causal evidence of whether people with centrist ideologies are the more likely to react to a change in the cost of abstention, as the model predicts. More precisely, given the three ideological positions, left, center and right, denoted by P_{ij}^{l} ($l = 1, 2, 3$), the effect of the reduction in fines on turnout for each ideological position is identified by equation (1.11).

$$Vote_{ij} = \sum_{n=1}^{3} \beta_n \triangle F_{ij} \cdot P_{ij}^{n} + \sum_{n=1}^{3} \beta_{n1} P_{ij} \cdot P_{ij}^{n} + \sum_{n=1}^{3} \beta_{n2} NP_{ij} \cdot P_{ij}^{n} + \beta_{10} P_{ij} + \beta_{11} NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij} \quad (1.11)$$

In order to compare people within the same fine level, the model in Equation (1.11) includes interactions between all the relevant coefficients and the poverty level dummies. The only effects that I constrain to be constant across poverty categories are the control variables (X_{ij}). The coefficients of interest in this case are β_n, and if the predictions of the model hold, we should observe that the coefficients associated with the interaction between the indicators of political extremes with the change in the perceived fines will be zero (β_1 and β_3). On the other hand, the coefficient testing for the effects of changes in fine on voting among centrists voters (β_2) should be positive, meaning that a larger decrease (increase) in the perceived fine causes lower (higher) turnout.

Table 1.8 shows the results from Equation (1.11). In Column (1) I use the self reported measure of political ideology, and find that the bulk of the effect of the change in the fine on turnout observed in Table 1.5 comes from voters who place themselves in the political center. Voters on both political extremes seem to be insensitive to changes in the cost of not voting. The results in Column (2), using the second measure of ideological position based on policy preferences, are even more stark. Voters in the the second through fourth quintiles of the policy preference scale are responsible for the whole effect of changes in the fine for not voting, while voters in the political extremes show effects close to zero and statistically insignificant. Overall, the results from Table 1.8 are consistent with the first prediction of the model, and show that people in the political extremes are less likely to respond to a change in incentives (not) vote.

This result has important implications in terms of how to structure the incentives to vote and its potential effects on political competition and social conflict. If the electorate in the political center was reduced, we might observe parties bunching in the extremes, which could lead to higher polarization and social conflict.

1.5.4 Interest in Politics / Subjective Value of Voting

Voters with a higher subjective value of voting (d_i) need lower incentives to attend to the polls, compared with those who derive lower utility gains from voting. The subjective benefit of voting is an unobserved individual characteristic, so I use a battery of questions on interest in politics, in the results of the current election, and in the campaign.

As shown in Table 1.9, voters who are more interested in politics go to the polls regardless of the change in the perceived fine. People who report being somewhat interested in politics are less likely to vote when the fine for abstention is reduced. Consistent with the predictions of the model, the effect is smaller in magnitude than the one we observe for voters who are not interested in politics. Similarly, voters who are very interested in the political campaign or in the results of the election are unlikely to respond to a reduction in the fine, while people who are somewhat interested have a significant effect, but again, lower in magnitude than those with a low interest in the campaign or in the results of the election. This result is consistent with the second prediction of the model.

Arguably, inducing uninterested voters to go to the polls could introduce noise in the election, and can change the results in contested elections. By allowing them to select out of the pool of voters, we can to avoid this potential risk.

1.5.5 Political Information

The model also predicts that $C(y_i, \Omega_i)$ is decreasing in Ω_i, which implies that less informed voters are more likely to make a *"voting mistake"*, and hence have a higher expected cost of voting for any given y_i. Empirically, I test this prediction by interacting different measures of political information with the change in the perceived fine, always relying on the treatment status to identify the LATE. More precisely, I run the test for this prediction through the following equation:

$$Vote_{ij} = \beta_1 \triangle F_{ij} + \beta_2 \triangle F_{ij} \cdot Info_{ij} + \beta_3 P_{ij} \cdot Info_{ij} + \beta_4 NP_{ij} \cdot Info_{ij} + \beta_5 P_{ij} + \beta_6 NP_{ij} + \gamma X_{ij} + \delta_j + \epsilon_{ij} \tag{1.12}$$

As before, in Equation (1.12) I am only comparing people within poverty categories. Following the model, we expect to observe that the effect of reductions in the cost of not voting is steeper for people who have less precise information about the politicians' ideological stance. The model also imply that having perfect information about the politicians means that the voter cannot make a *"voting mistake"*, and thus she should vote regardless of the cost of abstention. Following this prediction, we should expect β_2 to be negative, while for people with perfect information ($Info_{ij} = 1$), $\beta_1 + \beta_2$ should be equal to zero.

Table 1.10 tests this hypothesis using four different measures of political information. I use four normalized indices to proxy for political knowledge. The first three of them measure the percentage of candidates and/or parties running for office that the voter is able to name. I also use a normalized political information score, which uses information from seventeen questions about the electoral process and political institutions, knowledge about the electoral offices, official voting age, reelection rules, etc.

In all four columns of Table 1.10, the interaction between the information indices and the change in the perceived fine (instrumented by the treatment and the relevant interaction) is negative and significant, meaning that people who have higher levels of information are less likely to change their turnout decision when they learn that the fine has been reduced. Moreover, the magnitude of these coefficients line up remarkably well with the predictions of the model. People who are fully informed about the candidates and/or parties running in the local election are unaffected by the changes in the fine since the coefficient of the interaction offsets the direct effect.[34]

Previous evidence shows that more informed voters are more likely to hold the elected officials accountable and less likely to elect corrupt politicians.[35] It is possible that by reducing the cost of not voting, and allowing less informed voters to select out of the voters' pool, we could increase the quality of elected officials.

1.6 Policy Preferences, Information Acquisition and Vote Buying

The results from Tables 1.8, 1.9, and 1.10 are consistent with the predictions of the theoretical model, and have important implications for the design of voters' incentives. A lower fine for not voting draw a lower share of the population the polls. This effect is particularly important for centrist voters, those who have lower subjective value of voting (or who are less interested in politics), and the uninformed . The natural question following these results regards its implications for the aggregation of citizen preferences in electing a government.

1.6.1 Policy Preferences

Electoral institutions in democratic societies are designed to maximize voter representation and to ensure that policies are catered towards the interests of the majority. Mandating citizens to participate in elections imposes a cost on society, and it could be justified if the incentives to vote achieve a better representation of voter preferences. Theoretical arguments are mixed. Depending on the assumptions on the type of information available to voters, different authors have argued that compulsory voting can be welfare increasing or decreasing. For example, Krishna and Morgan (2011) present a theoretical model showing that under voluntary voting, information aggregation holds, and mandating people to vote imposes a net cost to society. Along the same lines, Borgers (2004) reaches a similar conclusion based on a model with simple private value majoritarian elections. On the other hand, Ghosal and Lockwood (2009) use a model with common values to show that compulsory voting Pareto dominates voluntary participation.

[34]One potential concern with the information variables use here is that a voter might not need to know all of the candidates to make an informed choice. A strategic voter (not contemplated in the model presented here) would need to know only those who have chances of winning the election. In alternative specifications, I defined my information variables as the percentage of candidates/parties mentioned out of the 5 candidates who ended up with the higher amount of votes in each district. The results hold under these measures of political information and the results are available upon request.

[35]See for example, Ferraz and Finan, 2008; Banerjee et al., 2011, Chong et al., 2011, Pande, 2011

Even though I am not able to rule out any of these models, I can provide suggestive evidence that can help us think about the extent to which different incentive schemes to participate in elections can affect policy outcomes.

One way to address this issue is to analyze whether people who prefer certain policies are more likely to respond to the incentives to (not) vote. If that is the case, a reduction of the fine for abstention will lead to under-representation of people who have these preferences, and thus the policies preferred by this group will not be enacted (assuming perfect commitment by politicians). To implement this test, I use the policy preference questions, aggregating them into 10 categories that represent broad policy issues, and then analyzing whether voters who prefer each policy are more or less likely to respond to changes in the fine.

The results from this analysis are presented in Table 1.11. The interaction terms between policy preferences and changes in the fine for not voting are not statistically significant and very close to zero, suggesting that voters with particular policy preferences are not over-represented among those who stop voting. The only interaction coefficient that comes through statistically and economically significant is the one for voters who have preferences for policies that promote agricultural activities (i.e. water projects, investment in improved seeds, etc.). The negative interaction coefficient, which is about of the same magnitude as the average effect for the population, implies that the effect of the changes in the fine is completely offset for this group, and they are not likely to stop voting when the fines are reduced.

Overall, these results suggest that voters who abstain when facing lower fines for not voting do not have significantly different policy preferences than those who still vote. Assuming perfect commitment by politicians, this implies that the change in the electorate due to lower incentives to vote will not cause a change in the policies implemented by elected officials.

1.6.2 Information Acquisition

Proponents of mandatory voting argue that mandating people who vote not only increases participation, but also involves people in the political process, for example by acquiring political information. The underlying model is one similar to the one proposed here, but it endogenizes information acquisition (Martinelli, 2005, Deagan, 2011, Oliveros, 2011). The intuition behind these models is that for sufficiently high penalties for not voting, abstention will drop and people might demand more political information to avoid making a *voting mistake*.

The follow-up questionnaire assessed the level of political information held by each respondent, so I can test whether people who perceive a lower penalty for not voting are less likely to acquire political information. In Table 1.12 I regress the change in the different measures of political information on the change in the perceived fines, instrumented by the treatment status. The effect of a change in the perceived fine on information acquisition is very close to zero and not statistically significant. Voters who face lower costs for abstention do not acquire information differentially than their peers who face a higher fine.[36]

1.6.3 Vote Buying

Electoral processes in developing countries are often prone to vote buying.[37] Vote buying represents a net loss for society since it tends to distort voters preferences, affecting the results of an election. It could be argued that in electoral systems with mandatory voting, voters who go to the polls because of the mandate are more likely to accept money for their votes. If this were the case, the mandate to vote will generate a negative externality. Using the exogenous variation in the cost of not voting, I am able to test whether a reduction in the cost of not voting affects the amount of vote buying and the price paid for each vote. I do this by using information collected in the final section of the follow-up survey, where respondents were asked if they were offered (and if they accepted) any in-kind gift or cash by someone associated with any candidate or political party before the election took place.[38] Also they were asked if the money or in-kind gift was given directly to the person, or indirectly as for example in a mass giveaway.

Table 1.13 shows the effects of the change in perceived fines (instrumented by the treatment) on whether the voter accepted money for her vote, and the amount of money accepted. As a result of a reduction of the fine, we observe a lower share of the population attending to the polls, and thus the pool of potential votes to be bought is reduced. Further, those voters still attend to the polls despite the lower sanctions of abstention are more likely to be well informed, have a strong political position and are interested in politics. Arguably, these voters are less willing to sell their vote, and when they do, a higher amount of money is required.

Effectively, the reduction in turnout due to the treatment generates an exogenous shift in the supply of votes. The results in Column (1) show that a decrease in the fine for abstention of S/.1 leads to a 0.1 percentage points lower likelihood of accepting money for the vote. The standard errors are large, but the magnitude of the effect is non negligible. On average, this implies a 19 percentage point reduction in the incidence vote buying due to the reduction in the fine for not voting.

Column (2) shows the effect on the amount of money received directly from a candidate or her representatives before the election. A decrease in the fine of S/.1 leads to an increase in the price of the vote of S/.0.03. This implies that for the average voter, who perceived that the fines were reduced by S/.56, her vote became 76 percent more expensive than

[36]These results must be taken with a grain of salt for two reasons. First, even though around the elections is the time when voters are more likely to get informed about the candidates and the political process overall, we must keep in mind that the average time between surveys was short (29 days). Second, in the medium or long run people who stop voting might also change their behavior in terms of information acquisition.

[37]See: Vicente, 2008; Vicente and Wantchekon, 2009; Finan and Schechter, 2011.

[38]For in-kind gifts, the survey asked respondents to put a monetary value to the good.

the average S/.2.2 for what she settled before. As a robustness check for this result, in Column (3) I use as a dependent variable the amount of money indirectly received by the voter. If there is a negotiation between the voter and the political operator about the price of the vote, I do not expect this negotiation to affect the amount received in a massive giveaway of money or souvenirs. Indeed, I find a statistically and economically insignificant effect. Overall, the a reduction in the fine for abstention leads to a lower incidence of vote buying, and when it happens, each vote becomes more expensive, making it more expensive to politicians to have influence on the outcome of the election through vote buying.

1.7 Summary and Discussion

Electoral institutions that encourage or mandate citizens to vote are widespread around the world. Such institutions are often introduced in the spirit of democratization, hoping to achieve better representation, and to involve the citizenship in the political process. However, since both voting and enforcement institutions are costly, there could be significant welfare losses if the objectives of higher participation and more involvement are not achieved.

In this paper use a dataset that allows me to combine a natural experiment provided by a change in Peruvian voting laws with a field experiment to identify the effect of fines for abstention on voting. I find that a reduction in the cost of abstention decreases turnout, and that this reduction is more than proportional among (i) centrist voters, (ii) those who have a lower subjective value of voting, and (iii) voters who hold less political information. These results are consistent with the predictions of the rational choice model of voter behavior with imperfect information presented in the paper.

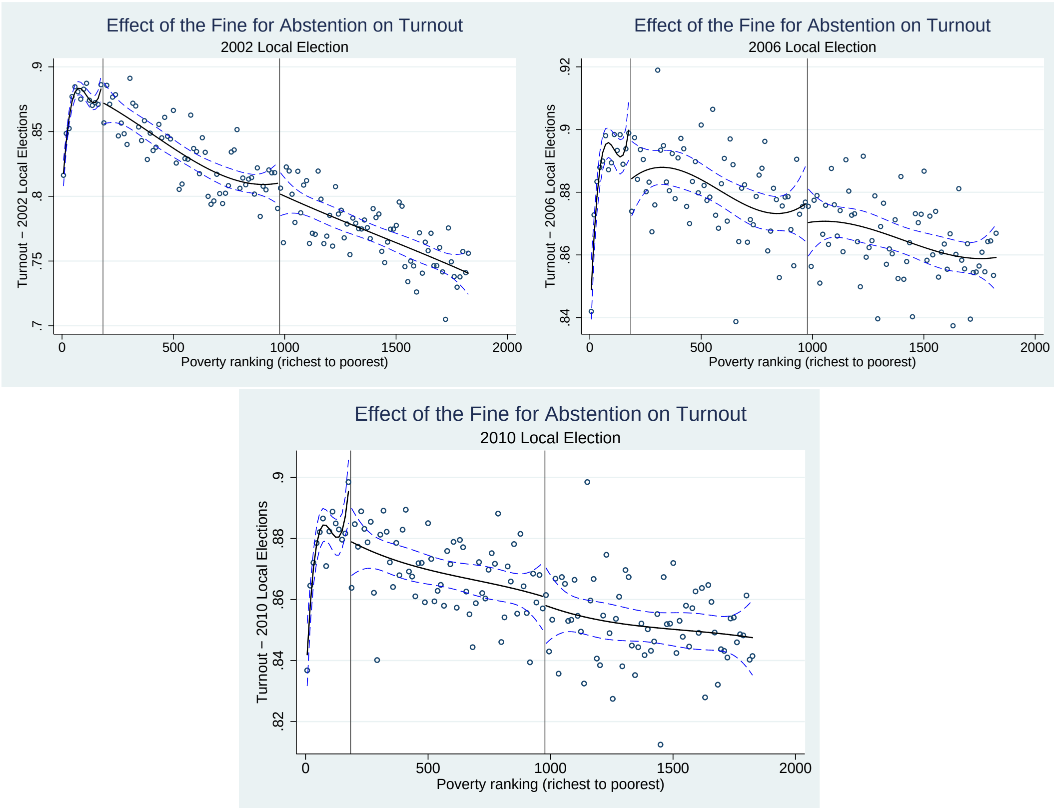
The estimates imply that cutting the fines for not voting by half leads to a 10 percentage point reduction in turnout. Further, the experimental design allows me to compute the elasticity of voting with respect to the cost, which I find to be -0.21. To my knowledge, this is the first paper to be able to estimate this parameter, which is key to evaluate policy interventions that attempt to affect the cost of voting, such as increasing in the number of polling stations, implementing electronic voting, etc.

Even though we observe a change in the electorate due to the reduction in the fine for not voting, this does not necessarily imply that the outcome of the election will be affected. On average, voters who stop going to the polls due to the reduction in the fine do not seem to have different policy preferences than their peers who do not respond to the change in the cost of abstention. This result implies that a reduction in the incentives to attend to the polls will likely not lead to a change in the policies enacted. Further, the fact that some people do not vote as a response to the treatment does not lead them to acquire less political information.

Additionally, I find that a decrease in the fine for not voting decreases the externalities on related markets. Particularly, I find that the the reduction in the fine for abstention reduces the pool of voters who are willing to sell their vote, thus reducing the incidence of vote buying and increasing the price paid by politicians to buy votes. Hence, lowering the incentives to vote reduces the chances politicians have to influence the election by making it more expensive.

The results presented have strong implications for the design of electoral institutions. First, voters respond to monetary incentives to go to the polls, and the extent in which they respond is non-negligible. Second, the experimental evidence suggests that the objectives of mandatory voting, namely ensuring representation and involvement in politics, do not seem to be affected by the reduction in the incentives. If these results holds when the incentives are completely eliminated, mandatory voting would lead to a welfare loss to society, however, if the polarization of society has a negative weight in the policymaker's objective function, mandating voting might dominate.

Figure 1.1: Discontinuity Analysis: Effect of Non-Voting Fine Law on Turnout



Notes: This figures plot the official turnout rates at the district level in the 2002, 2006, and 2010 municipal elections. Districts are ranked from richest to poorest, and the vertical lines indicate the thresholds at which a district is categorized as non-poor, poor, or extremely poor.

Figure 1.2: Geographic location of the districts in the survey

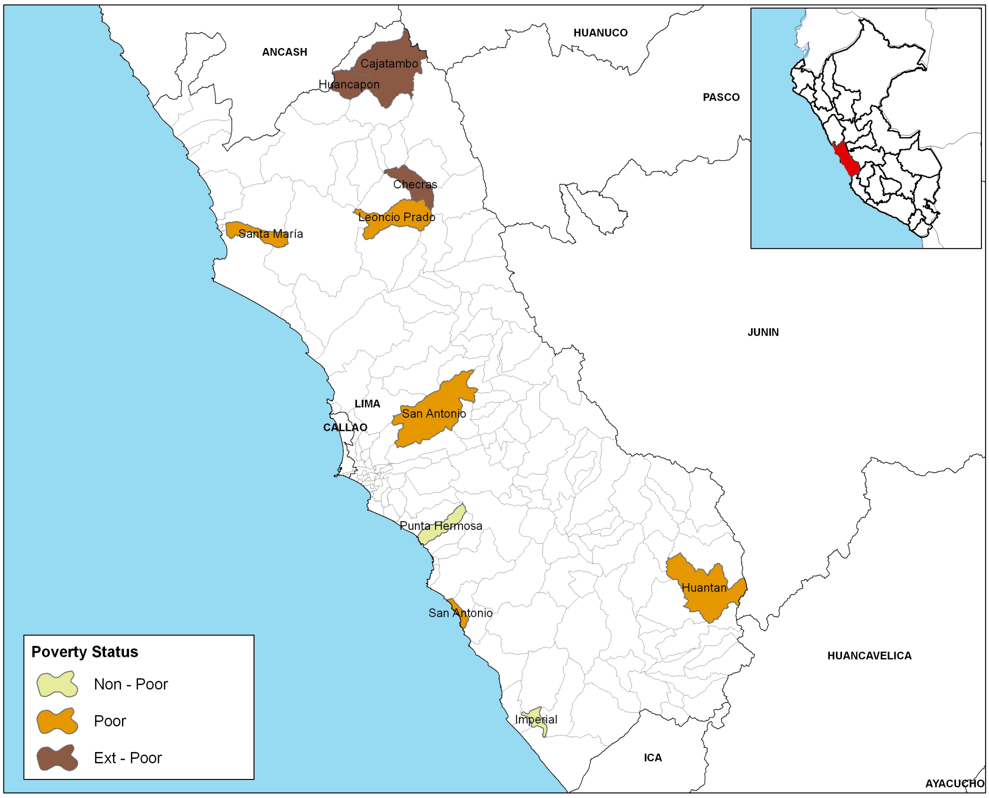


Figure 1.3: Perceived fines, by treatment and poverty status

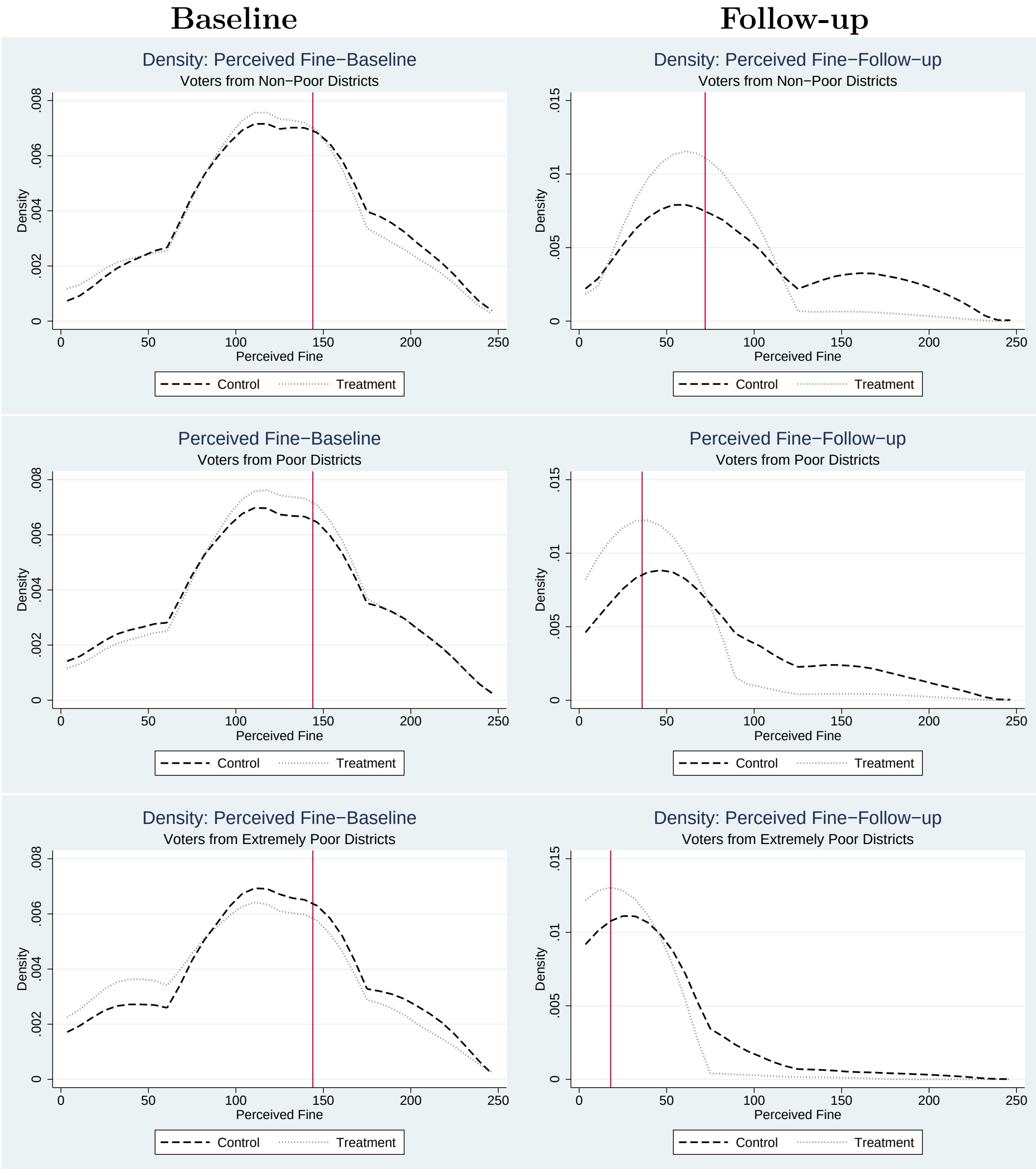


Table 1.1: Balance Between Treatment and Control Groups

Variable	**Obs.**	**Full Sample**		**Treatment**		**Control**		**T - C**
		Mean	**Std. Dev.**	**Mean**	**Std. Dev.**	**Mean**	**Std. Dev.**	
Perceived Fine (Baseline)	2275	122.292	57.325	124.233	57.806	120.362	56.822	-3.871
Gender	2275	0.424	0.494	0.419	0.494	0.428	0.495	0.009
Age	2275	39.885	13.358	39.916	13.256	39.874	13.456	-0.041
Yrs. of education	2275	9.586	4.063	9.593	4.090	9.576	4.038	-0.017
Log(PC Expenditures)	2275	5.190	0.871	5.176	0.883	5.203	0.859	0.027
Center	2201	0.667	0.472	0.671	0.470	0.662	0.473	-0.009
Left	2201	0.083	0.275	0.076	0.264	0.090	0.286	0.014
Right	2201	0.251	0.434	0.253	0.435	0.248	0.432	-0.005
Policy Extreme 1 (Pub. goods)	2275	0.207	0.406	0.213	0.409	0.202	0.402	-0.010
Policy Center	2275	0.598	0.491	0.614	0.487	0.580	0.494	-0.034
Policy Extreme 2 (Club goods)	2275	0.195	0.396	0.173	0.379	0.216	0.412	0.043
Very Interested in politics	2243	0.082	0.274	0.080	0.272	0.083	0.276	0.003
Interested in politics	2243	0.468	0.499	0.459	0.498	0.477	0.500	0.018
Not Interested in politics	2243	0.451	0.498	0.461	0.499	0.440	0.497	-0.021
V. Interested in the results	2275	0.399	0.490	0.390	0.488	0.407	0.492	0.017
Interested in the results	2257	0.443	0.497	0.433	0.496	0.454	0.498	0.020
Not Interested in the results	2275	0.153	0.360	0.170	0.376	0.136	0.343	-0.034
V. Interested in the campaign	2253	0.105	0.307	0.106	0.307	0.105	0.306	-0.001
Interested in the campaign	2253	0.556	0.497	0.549	0.498	0.563	0.496	0.014
Not Interested in the campaign	2253	0.339	0.473	0.345	0.476	0.332	0.471	-0.013
Name recall- Candidates running	2275	0.388	0.350	0.390	0.354	0.387	0.346	-0.002
Name recall- Parties running	2275	0.290	0.317	0.297	0.322	0.283	0.311	-0.014
Name recall- Cand.+Parties running	2275	0.339	0.315	0.343	0.322	0.335	0.308	-0.008
Political information score	2275	0.547	0.179	0.545	0.180	0.550	0.179	0.005

Table 1.2: Turnout and Perceived Fine, by Treatment and Poverty Status

	Total	**Treatment**	**Control**	**T - C**	**P-value**
	PANEL A: Turnout				
Non-Poor	0.948	0.938	0.959	-0.021	(0.175)
Poor	0.940	0.913	0.967	-0.054	(0.001)***
Extreme Poor	0.935	0.930	0.940	-0.010	(0.641)
Total	0.942	0.927	0.958	-0.031	(0.002)***
	PANEL B: Perceived Fines				
Baseline					
Non-Poor	126.5	123.8	129.4	-5.605	(0.144)
Poor	122.1	122.3	122.0	0.230	(0.951)
Extreme Poor	115.9	111.9	120.0	-8.066	(0.132)
Total	122.3	120.4	124.2	-3.871	(0.107)
Follow-up					
Non-Poor	78.5	66.8	91.0	-24.197	(0.000)***
Poor	57.3	42.1	71.2	-29.047	(0.000)***
Extreme Poor	27.9	19.4	36.6	-17.199	(0.000)***
Total	58.2	46.1	70.2	-24.111	(0.000)***
Change					
Non-Poor	-48.0	-57.0	-38.5	-18.593	(0.000)***
Poor	-64.8	-80.1	-50.9	-29.277	(0.000)***
Extreme Poor	-88.0	-92.5	-83.4	-9.133	(0.121)
Total	-64.1	-74.2	-54.0	-20.239	(0.000)***

Notes: The actual changes that occurred were: for people voting in Non-poor districts, S/.72 (from S/.144 to S/.72); for those voting in Poor districts, S/.108 (from S/.144 to S/.36); and for people registered to vote in Extremely Poor districts, S/.126 (from S/.144 to S/.18).

Table 1.3: Reduced Form - Effect of Treatment on Voting

	Dep. Var: Voted in the 2010 Election		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-.027		-.026
	(0.015)*		(0.015)*
Treatment: Fine S/.36		-.052	-.053
		(0.016)***	(0.016)***
Gender	-.0009	0.018	0.013
	(0.016)	(0.016)	(0.011)
Age	0.001	0.001	0.001
	(0.0007)	(0.0006)**	(0.0005)***
Yrs. of education	0.002	0.004	0.004
	(0.002)	(0.003)	(0.002)**
Log(PC Expenditures)	0.004	0.011	0.007
	(0.008)	(0.013)	(0.008)
Votes in Non-Poor district	0.876		0.818
	(0.058)***		(0.05)***
Votes in Poor district		0.76	0.818
		(0.121)***	(0.054)***
Village FE	Y	Y	Y
Mean dep. var.	0.9482	0.9410	0.9446
Obs.	850	882	1732
R^2	0.953	0.947	0.947

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$Vote_{ij} = \beta_1 NP_{ij} \cdot T_{ij} + \beta_2 P_{ij} \cdot T_{ij} + \beta_3 P_{ij} + \beta_4 NP_{ij} + \gamma X_{ij} + \delta_k + \eta_{ij}$

Table 1.4: First Stage - Effect of Treatment on Changes in Perceived Fine

	Dep. Var: $\triangle$ Perceived Fine		
	Non-Poor	Poor	All
Treatment: Fine S/.72	-18.807		-19.317
	(4.905)***		(4.854)***
Treatment: Fine S/.36		-30.465	-30.340
		(4.756)***	(4.692)***
Gender	-2.962	-2.135	-2.839
	(4.946)	(4.741)	(3.393)
Age	0.333	0.409	0.363
	(0.201)*	(0.182)**	(0.133)***
Yrs. of education	0.266	-.753	-.243
	(0.74)	(0.703)	(0.499)
Log(PC Expenditures)	-4.101	-1.684	-2.369
	(3.524)	(3.532)	(2.520)
Votes in Non-Poor district	-35.548		-41.581
	(22.271)		(16.028)***
Votes in Poor district		-54.903	-41.491
		(32.882)*	(16.904)**
Village FE	Y	Y	Y
Mean dep. var.	-48.00	-64.99	-56.65
Obs.	851	882	1733
F-statistic	14.68	41.03	28.66
R^2	0.399	0.528	0.463

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$\triangle Fine_{ij} = \beta_1 NP_{ij} \cdot T_{ij} + \beta_2 P_{ij} \cdot T_{ij} + \beta_3 P_{ij} + \beta_4 NP_{ij} + \gamma X_{ij} + \delta_k + \nu_{ij}$

Table 1.5: IV - Effect of Change in Perceived Fines on Turnout

	Dep. Var: Voted in the 2010 Election		
	Non-Poor	Poor	All
$\triangle$ Perceived Fine	0.0014	0.0017	0.0016
	(0.0009)*	(0.0006)***	(0.0005)***
Gender	0.0034	0.022	0.018
	(0.0175)	(0.017)	(0.0124)
Age	0.0005	0.0008	0.0007
	(0.0008)	(0.0007)	(0.0005)
Yrs. of education	0.0013	0.0056	0.0042
	(0.0024)	(0.0031)*	(0.002)**
Log(PC Expenditures)	0.0101	0.0142	0.0109
	(0.0108)	(0.0145)	(0.0087)
Votes in Non-Poor district	0.9275		0.8878
	(0.0684)***		(0.0573)***
Votes in Poor district		0.8539	0.8836
		(0.1334)***	(0.0614)***
Village FE	Y	Y	Y
Mean dep. var.	0.9482	0.9410	0.9446
Obs.	850	882	1732
F-statistic	14.68	41.03	28.66

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$

Table 1.6: Robustness: Effect of Changes in Perceived Fine on Turnout - Different Measures of Turnout

		Dep. Var: Voted in the 2010 Election			
		Available Sample		Comparable Sample	
	Benckmark	Self Reported	Sticker	Self Reported	Sticker
△ Perceived Fine	0.0016	0.0013	0.0015	0.001	0.0015
	(0.0005)***	(0.0005)***	(0.0005)***	(0.0005)**	(0.0005)***
Gender	0.018	0.0142	0.0104	0.0018	0.0109
	(0.0124)	(0.0122)	(0.0126)	(0.0116)	(0.0128)
Age	0.0007	0.001	0.0002	0.0005	0.0002
	(0.0005)	(0.0005)*	(0.0005)	(0.0004)	(0.0005)
Yrs. of education	0.0042	0.0049	0.0014	0.0025	0.0014
	(0.002)**	(0.002)**	(0.0021)	(0.002)	(0.0021)
Log(PC Expenditures)	0.0109	0.0081	0.0115	0.0069	0.0118
	(0.0087)	(0.0082)	(0.0075)	(0.0064)	(0.0075)
Votes in Non-Poor district	0.8878	0.8808	0.9749	0.9681	0.9738
	(0.0573)***	(0.0551)***	(0.0453)***	(0.0419)***	(0.0454)***
Votes in Poor district	0.8836	0.8779	0.9851	0.9799	0.9842
	(0.0614)***	(0.059)***	(0.0538)***	(0.0497)***	(0.054)***
Village FE	Y	Y	Y	Y	Y
Obs.	1732	1729	1130	1127	1127
F-statistic	28.6595	28.2653	17.2611	16.8161	16.8161

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses.

Columns (4) and (5) use only the sample of observations for which both outcomes are available. Regression equation:

$Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$

Table 1.7: Robustness: Effect of Changes in Perceived Fine on Past Turnout

	Dep. Var: Voted in the 2006 Election		
	Non-Poor	Poor	All
△ Perceived Fine	-.0016	0.0007	0.00006
	(0.001)	(0.0006)	(0.0005)
Gender	-.0090	0.0212	0.0117
	(0.0175)	(0.0157)	(0.0109)
Age	0.0049	0.0023	0.0035
	(0.0011)***	(0.0009)**	(0.0007)***
Yrs. of education	0.0112	0.0078	0.0085
	(0.0029)***	(0.0023)***	(0.0017)***
Log(PC Expenditures)	-.0104	0.0175	0.004
	(0.0117)	(0.0153)	(0.0083)
Votes in Non-Poor district	0.6142		0.6965
	(0.1039)***		(0.0764)***
Votes in Poor district		0.6745	0.7007
		(0.1794)***	(0.0814)***
Village FE	Y	Y	Y
Mean dep. var.	0.9459	0.9444	0.9451
Obs.	758	791	1549
F-statistic	11.92	32.33	23.44
R^2	0.9419	0.1499	0.7375

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$Vote_{ij}^{t-1} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$. The dependent variable is self reported, and it refers to turnout in the November, 2006 municipal election.

Table 1.8: Effect of Changes in Perceived Fine on Turnout, by Political Preferences

	Dep. Var: Voted in the 2010 Election	
	(1)	(2)
△ Fine*Left	-.0009 (0.0026)	
△ Fine*Center	0.0015 (0.0006)***	
△ Fine*Right	0.0009 (0.0008)	
△ Fine*Policy Extreme 1 (Pub. Goods)		0.001 (0.0013)
△ Fine*Policy Center		0.002 (0.0007)***
△ Fine*Policy Extreme 2 (Club Goods)		0.0006 (0.0009)
Controls	Y	Y
Village FE	Y	Y
Obs.	1665	1732

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation: $Vote_{ij} = \sum_{n=1}^{3} \beta_n \triangle Fine_{ij} \cdot P_{ij}^n + \sum_{n=1}^{3} \beta_{n1} P_{ij}^n \cdot P_{ij} + \sum_{n=1}^{3} \beta_{n1} P_{ij}^n \cdot NP_{ij} + \beta_{10} P_{ij} + \beta_{11} NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$, P_{ij}^n is a dummy variable representing political preferences $n = 1, 2, 3$ for individual i interviewed in village k. In Column (1), "Left", "Center", and "Right" are self reported variables indicating positions in the ideological scale, which ranges from 1 to 5. People choosing 1 and 5 are categorized as "Left" or "Right", respectively, while 2, 3 and 4 are considered in the "Center". The second measure of ideological positions (used in Column(2)) is an aggregation of several measures of policy preferences. I use responses from a question where I asked respondents to name (in order) the first five policies that she would implement if she were elected mayor of the district. For each of these categories, the policy preferences are ordered from not mentioned (zero) to most preferred (five). I aggregate these questions by taking the first principal component, and dividing the sample into quintiles. The center is defined by those in the quintiles 2, 3, and 4, while the first and fifth quintiles define the ideological extremes: Policy Extreme 1 (Pub. Goods), Policy Extreme 2 (Club Goods), respectively. The results from the principal component analysis is shown in Table A.1.

Table 1.9: Effect of Changes in Perceived Fine on Turnout, by Interest in Politics

	Dep. Var: Voted in the 2010 Election		
	(1)	(2)	(3)
$\triangle$ Fine*Very interested in politics	0.0001 (0.0018)		
$\triangle$ Fine*Interested in politics	0.0012 (0.0007)*		
$\triangle$ Fine*Not interested in politics	0.0018 (0.0007)***		
$\triangle$ Fine*Very interested in results		0.0007 (0.0006)	
$\triangle$ Fine*Interested in results		0.0018 (0.0007)***	
$\triangle$ Fine*Not interested in results		0.0039 (0.002)**	
$\triangle$ Fine*Very interested in pol. campaign			0.0023 (0.002)
$\triangle$ Fine*Interested in pol. campaign			0.0009 (0.0005)*
$\triangle$ Fine*Not interested in pol. campaign			0.0023 (0.001)**
Controls	Y	Y	Y
Villafe FE	Y	Y	Y
Obs.	1713	1717	1714

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$Vote_{ij} = \sum_{n=1}^{3} \beta_n \triangle Fine_{ij} \cdot I_{ij}^n + \sum_{n=1}^{3} \beta_{n1} I_{ij}^n \cdot P_{ij} + \sum_{n=1}^{3} \beta_{n1} I_{ij}^n \cdot NP_{ij} + \beta_{10} P_{ij} + \beta_{11} NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$, I_{ij}^k is a dummy variable representing interest in politics $n = 1, 2, 3$ for individual i interviewed in village k.

Table 1.10: Effect of Changes in Perceived Fine on Turnout, by Political Information

	Dep. Var: Voted in the 2010 Election			
	(1)	(2)	(3)	(4)
△ Perceived Fine	0.0024	0.0022	0.0024	0.0079
	(0.0008)***	(0.0007)***	(0.0008)***	(0.0031)**
△ Fine*Candidate recall	-.0023			
	(0.0012)**			
△ Fine*Party recall		-.0022		
		(0.0011)*		
△ Fine*Candidate and Party recall			-.0027	
			(0.0012)**	
△ Fine*Pol. Info. Score				-.0113
				(0.0053)**
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Obs.	1732	1732	1732	1732

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation: $Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 \triangle Fine_{ij} \cdot Info_{ij} + \beta_3 Info_{ij} \cdot P_{ij} + \beta_4 Info_{ij} \cdot NP_{ij} + \beta_5 P_{ij} + \beta_6 NP_{ij} + \gamma X_{ij} + \delta_j + \epsilon_{ij}$. The information variables are indices ranging from zero to one. The candidate and/or party recall represent the proportion of candidates/parties running in the election in the municipality where the voter is registered. Additionally, the survey included a battery of 17 questions related to the features of the political system, mandatory ages for voting, term limits at different levels of the government, etc. The political information score represents the proportion of questions that the respondent was able to answer correctly.

Table 1.11: Effects by policy preferences

	Dep. Var.: Voted in the 2010 Election	
	Coeff. on	Coeff. on
	$\triangle$ Perceived Fine	$\triangle$ Perceived Fine*Policy
Policy		
Health	0.0019	-.0005
	(0.0008)**	(0.0009)
Education	0.0009	0.0012
	(0.0005)*	(0.001)
Infrastructure	0.001	0.0007
	(0.0011)	(0.0012)
Order and Security	0.0022	-.0012
	(0.0007)***	(0.001)
Promote micro-enterprises/training	0.0016	0.0002
	(0.0005)***	(0.0012)
Agriculture	0.0022	-.0020
	(0.0007)***	(0.0008)**
Youth/Women	0.0013	0.0013
	(0.0006)**	(0.0011)
Cleaning/Environment	0.0013	0.0007
	(0.0005)**	(0.001)
Institutions	0.0018	-.0010
	(0.0006)***	(0.001)
Social/work programs	0.0017	-.0004
	(0.0006)***	(0.001)

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation: $Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 \triangle Fine_{ij} \cdot Policy_{ij} + \beta_3 Policy_{ij} \cdot P_{ij} + \beta_4 Policy_{ij} \cdot NP_{ij} + \beta_5 P_{ij} + \beta_6 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$. The coefficients shown in each row come from separate regressions. Policy preferences include: (1) Health: Infrastructure, health professionals, and training for health workers; (2) Education: Infrastructure, teachers, and training for teachers; (3) Infrastructure: Roads and access to them, sewage, water, electricity and telecommunications infrastructure, build markets, churches, community building, main square; (4) Order and Security: Traffic, more policemen in the streets, fight drugs and gangs; (5) Promote micro-enterprises/training: promote micro/small firms, train local entrepreneurs, promote private investment, promote tourism; (6) Agriculture: Build dams and irrigation infrastructure, technical assistance to agriculture, seed banks, support livestock farmers; (7) Youth/Women: Women empowerment and equality, youth policies, sporting events; (8) Cleaning/Environment: street cleaning, increase green areas, promote recycling; (9) Institutions: Transparency in managing the municipality, fight corruption, modernize the bureaucracy, participatory decision-making, land titling; (10) Social/work programs: Job training programs, help those in poverty, food aid, child care, generate jobs. For each of these categories, the dependent variable is a dummy indicating whether the respondent named at least one of the policies in this category as one of her five priorities for the district.

Table 1.12: Effects of Fines on Information Acquisition

	Dep. Var.:			
	(1)	(2)	(3)	(4)
	△ Candidate Recall	△ Party Recall	△ Cand.+Party Recall	△ Pol. Info Score
△ Perceived Fine	-.0002	-.0005	-.0004	3.00e-06
	(0.0005)	(0.0005)	(0.0005)	(0.0003)
Gender	-.0236	-.0371	-.0304	-.0265
	(0.0125)*	(0.0137)***	(0.0121)**	(0.0092)***
Age	-.0003	0.0006	0.0001	-.0009
	(0.0005)	(0.0006)	(0.0005)	(0.0004)**
Yrs. of education	-.0036	-.0065	-.0051	-.0061
	(0.0017)**	(0.0018)***	(0.0016)***	(0.0013)***
Log(PC Expenditures)	-.0176	-.0106	-.0141	-.0046
	(0.0094)*	(0.0094)	(0.0084)*	(0.0066)
Votes in Non-Poor district	-.0667	0.0069	-.0299	0.2032
	(0.0688)	(0.0704)	(0.0633)	(0.0462)***
Votes in Poor district	-.0878	0.0173	-.0352	0.1978
	(0.0713)	(0.0736)	(0.066)	(0.0492)***
Village FE	Y	Y	Y	Y
Obs.	1733	1733	1733	1733
F-Statistic	28.675	28.675	28.675	28.675
R^2	0.0954	0.02	0.0564	0.0452

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation: $\triangle Info_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$, where $\triangle Info_{ij}$ represents the change in the political information between the baseline and follow-up surveys. The dependent variable is the difference in the information measures between the follow-up and baseline surveys.

Table 1.13: Effects of Fines on Vote buying

	Dep. Var:			
	(1) 1=Accepted Money or a Gift	(2) Amount Accepted Directly	(3) Amount Accepted Indirectly	(4) Amount Accepted Total
$\triangle$ Perceived Fine	-.0010	-.0303	0.0043	-.0276
	(0.0009)	(0.0161)*	(0.0071)	(0.0178)
Gender	-.0365	-.9115	0.0005	-1.1150
	(0.0227)	(0.4431)**	(0.1748)	(0.4829)**
Age	-.0014	-.0379	-.0208	-.0541
	(0.0009)	(0.0221)*	(0.0084)**	(0.0236)**
Yrs. of education	-.0010	-.0277	-.0189	-.0280
	(0.0032)	(0.0637)	(0.0196)	(0.0684)
Log(PC Expenditures)	-.0082	0.1046	0.0956	0.2389
	(0.0143)	(0.3056)	(0.1242)	(0.3389)
Votes in Non-Poor district	0.4222	1.0224	3.3786	4.7187
	(0.0989)***	(2.5372)	(1.0954)***	(2.7913)*
Votes in Poor district	0.498	0.9889	3.5448	4.9071
	(0.1056)***	(2.5821)	(1.1264)***	(2.8405)*
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Mean dep. var.	0.287	2.20	0.818	3.25
Obs.	1733	1733	1733	1733
F-statistic	28.675	28.675	28.675	28.675

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation: $Y_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 P_{ij} + \beta_3 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$. In Column (1), Y_{ij} is an indicator for whether voter i accepted money from a politician or his/her representative for her vote. In Column (2) through (4), it measures the amount of money accepted (directly or indirectly) to buy a vote.

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Chapter 2

Civil Conflict and Human Capital Accumulation: The Long-term Effects of Political Violence in Perú[1]

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2.1 Introduction

Civil conflicts have been widespread throughout the world in the post-WWII period. During the past decade, economists have analyzed the consequences of these conflicts, with particular attention to their welfare effects. The short-run impacts of civil conflicts are clearly catastrophic. However, recent analyses provide mixed evidence on the persistence of the effects of conflict on human capital accumulation.

Using data from the Peruvian civil conflict, this paper provides estimates of the effect of exposure to civil conflict on short- and long-run educational achievement, showing that the impact on human capital is persistent, particularly if exposure to conflict happens early in life. Specifically, the average person exposed to political violence before school age (in-utero, early childhood, and pre-school age ranges) has accumulated 0.31 fewer years of schooling upon reaching adulthood, with stronger short- than long-term effects. In contrast, individuals who experience the shock after starting school fully catch up to peers who were not exposed to violence

Understanding the scale and persistence of civil conflict on economic development is key, especially in developing countries, where most of the conflicts in the second half of the 20^{th} century have occurred. Economic growth theory suggest that, after a shock, the economy returns to its steady state level (as does human capital), but these models offer very little insight on the pace of recovery. Empirical cross-country and cross-regional studies suggest that countries see a steep decline on a variety of welfare indicators as a cosequence of war. They also show that there is significant recovery in most of these dimensions, but that this process varies in its duration.[2] As Blattman and Miguel (2009) suggest, beyond the trends revealed by cross-country or cross-regional evidence, it is hard to draw conclusions on how violence affects individual and household welfare, for which we need detailed individual-level analyses.

Micro-level studies have gone further in unveiling the relationship between civil conflict and individual welfare. Research in this area has focused on the immediate effects of conflict on health and educational outcomes. Several authors have found that there are significant effects of exposure to violence on education and health outcomes.[3] If the

[2] Chen, Loayza and Reynal-Querol (2008) look at 41 countries that suffered civil conflicts between 1960 and 2003, finding that after the war ends, there is significant recovery in terms of economic performance, health, education and political development. Moreover, Cerra and Saxena (2008) find that most of the output losses due to conflict are recovered in a very short period of time. Miguel and Roland (2011) look at the long-term consequences of the massive US bombings in Vietnam, finding that 27 years after the end of the war there was no detectable impact on poverty rates, consumption levels, literacy levels, infrastructure, or population density. Davis and Weinstein (2002), and Brakman, Garretsen and Schramm (2004) arrive at similar conclusions based on evidence from the Allied bombing in Japan and West Germany, respectively. In general, this literature concludes that the effects of severe periods of violence on economic outcomes and human welfare tend to vanish over time.

[3] Akresh and de Walque (2010) use micro data collected four years after the Rwandan genocide to assess its impact on school attainment of children exposed to the conflict. They find that children (directly) exposed to violence accumulate 0.5 fewer years of primary education. Akresh, Verwimp, and Bundervoet (2010) look at the effects of the same conflict on child stunting, comparing the effect of violence with

findings from the cross-country literature hold at the individual level, we should observe that people are able to recover from these shocks after a certain period of time. If this were the case, the studies cited above would only be measuring the short-term consequences of violence, while neglecting the fact that these effects will disappear as time goes by. Further, the pace of recovery might be different across groups of the population and some of them might even face irreversible losses. For example, evidence suggests that other type of shocks (notably related to health) experienced in-utero or during early childhood are persistent and may even determine the income gradient.[4] These potential long-run outcomes have deep implications for policy design for post-conflict societies.

This paper contributes to the literature relating civil conflict to human welfare in several respects. First, I provide the first micro-estimates in the literature about the short- and long-term effects of civil conflict on educational attainment, showing that the effects of violence are persistent over time. Second, I use a high-quality data set, representative at the national level, which contains the universe of human rights violations reported during the Peruvian civil conflict across districts and years. Further, the structure of the data allows me to estimate the short-term effects of violence by comparing siblings exposed to conflict at different stages of their lives. Finally, using alternative data sets, I determine the extent to which supply and demand shocks can account for the persistent effect of violence.

economic shocks, concluding that girls and boys exposed to the conflict have lower height for age z-scores. Using a similar research design, Akresh, Bundervoet and Verwimp (2010), assess the effects of the civil war in rural Burundi on health outcomes shortly after the termination of the conflict, finding that an extra month of exposure to the conflict reduces the children's height for age z-scores by 0.047 sd's. Arcand and Wouabe (2009) analyze the 27-year-long Angolan civil conflict, finding that in the short-run, conflict intensity worsens child health, does not significantly affect household expenditures, increases school enrollment and decreases fertility, as would be predicted by a Neoclassical unitary household model. The long-term impacts found in this study are significantly different from those documented for the short-term. In one of the only studies that is able to identify the impact of a direct exposure to violence (either by being abducted or otherwise directly affected) on education and labor market outcomes, Annan and Blattman (2010) find that educational losses are closely associated with length of time abducted, while those reporting the most psychological distress have been exposed to the most severe war violence and are disproportionately, but not exclusively former combatants. Outside of Africa, Shemyakina (2011) analyzes the effect of the 1992-1998 civil conflict in Tajikistan, finding that children who had experienced violence-related shocks are less likely to be enrolled in school. The effects found are stronger for girls than for boys. Likewise, Swee (2009) finds that living in a municipality exposed to the Serbian-Bosnian conflict decreases the likelihood of completing secondary education. Ichino and Winter-Ebmer (2004) and Akbbulut-Yuksel (2009) look at the long-term effects of WWII on educational outcomes, finding similar effects. In Latin America, Camacho (2009) shows that women's exposure to the Colombian conflict during pregnancy causes children to be born with lower weight.

[4]Barker (1988) gave rise to the "fetal origins hypothesis", which has been used to refer to the critical period programming caused by conditions experienced in the fetal stage. Case and Paxon (2010 and 2011) show that health conditions in the early life determine the income gradients in the long-run. Mancini and Yang (2011) find that weather shocks in the early life have long lasting consequences in health, education and income among Indonesian girls. Almond and Currie (2011) provides a comprehensive review on economist's work on this topic.

Using data from the 2007 and 1993 national census in Perú, my identification strategy exploits the variation in the temporal and geographical incidence of the conflict, relying on a large set of geographic and time fixed effects, along with province-specific time trends. After partialing out district- and year-specific variation, I argue that the incidence of violence is not correlated with any determinant of educational achievement: the geographical and temporal expansion of the conflict followed clear political and strategic guidelines from the rebel group, taking the war from rural areas in the highlands to the rich coastal districts (to attempt at controlling Lima, the capital city), and the coca region in the jungle (to secure sources of financing).

The results show that the average person exposed to political violence before school-age (during in-utero, early childhood, and pre-school age) accumulated 0.31 fewer years of schooling upon reaching adulthood. The short-term effects are larger than in the long-run, particularly if exposure to conflict happened early in life. Shocks in the pre-birth/in-utero period have a similar effect in the short- and long-run. Those who experience the shock in early childhood or preschool age on average only partially recover, while individuals who are exposed to violence once they have started their schooling cycle fully catch up to peers who were not exposed to violence. This suggests that children who are affected during very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. Those who experience the shock in early childhood or pre-school age partially recover, while people exposed to violence once they have started their schooling cycle are able to fully catch up with their peers who did not experience violence in this period.

To put these results in context, Duflo (2001) finds that the effect of the massive school construction program in Indonesia on school attainment in the long-run is of a slightly smaller magnitude, but in the opposite direction: each school constructed per 1,000 children led to an increase of 0.12 to 0.19 years of education. In the context of war exposure, Akresh and de Walque (2010) found that four years after the Rwandan genocide, children (directly) exposed to violence accumulate 0.5 fewer years of primary education, about half of what I find in my short-term estimates.

Seen through the lens of a classic education production function model, the evidence suggests that exposure to violence affects adult human capital accumulation through through both supply and demand side effects. On the supply side, I show that a teacher being killed in the district has a strong impact on educational attainment in that it delays school entrance. However, this effect does not have a long-term impact. On the demand side, suggestive evidence shows that the effect is not explained by short- or long-term shocks on household wealth, but I observe a persistent decrease in mother's health status after a violence shock, which translates into lowered child health.

Overall, the results in this paper show that shocks during the early stages of one's life have long-run irreversible consequences on human welfare. Relief efforts should thus be targeted to pregnant mothers and young children, and then to children in the early stages of their schooling cycle in order to minimize the long-term welfare losses for society.

In the next section, I present a historical perspective of the Peruvian civil conflict and describe the data used. Section 3 provides a simple theoretical model to help us understand the potential causal channels, as well as the empirical strategy. Section 4 presents the main results of the paper, discussing additional suggestive evidence about the causal channels. Finally, Section 5 summarizes and discusses the results.

2.2 Historical Overview and the Data

2.2.1 The Civil Conflict in Perú

Between 1980 and 1993, Perú suffered an intense period of violence caused by constant fighting between the rebel group Partido Comunista del Perú-Sendero Luminoso (PCP-SL) and the national army.[5] The Peruvian Truth and Reconciliation Commission (CVR, for its acronym in Spanish) estimates that this conflict caused the death of about 69,290 people (about 0.31% of the population), making the Peruvian case one of the longest and most brutal conflicts in Latin America.

Toward the end of the 1970's, Perú was transitioning to democracy. On May 17^{th}, 1980, the night before the presidential election, the PCP-SL made its first attack: a group of five men broke into the voter registration office in the district of Chuschi, Ayacucho (in the southern Andes) and burned the ballot boxes and the registry. No injuries were reported, but on that day the PCP-SL formally declared war on the Peruvian state (CVR, 2004).[6]

Between 1970 and 1992, Perú experienced a deep economic collapse. This decline hit peasants in the rural highlands particularly hard, worsening regional inequalities (Weinstein, 2007).[7] At the same time, education was expanding while employment opportunities for educated individuals remained stagnant. This expansion of the educational sector created an illusion of progress in the population, which was not matched by job opportunities for the newly educated workforce. University enrollment more than doubled from 1970 to 1990 (from 19% to 40%), while the unemployment rate for university graduates in the early 1990's was more than double the unemployment rate for those with other levels of education (McClintock, 1998).

The CVR considers this *"status inconsistency"* the main breeding ground upon which the PCP-SL was able to spread its ideas during the late 1970's in Ayacucho. In this area, the rebel group was able to build a critical mass of young and relatively educated supporters, who established the ideological foundations of the war and recruited the initial army. Importantly, this motivation was relevant in the initial stages of the war and in its

[5]Additional armed groups participated in the conflict as well. The main ones were the Movimiento Revolucionario Túpac Amaru (MRTA), paramilitaries, and government-led militias (especially during the 1990's).

[6]It is important to note that before the war was formally declared on that date, there had been no previous violent political activity headed by the PCP-SL.

[7]In the southern highlands – where the PCP-SL emerged – the infant mortality rate was 128/1000 births, while the nationwide rate was about 92/1000. More than 80% of the population in the area lack access to drinking water, and the ratio of people per doctor was astronomically high (17,000 per doctor), while the nationwide ratio was 1,255.

original location. The expansion of the conflict during the 1980's, on the other hand, was motivated by political and strategic reasons.

The armed conflict started in the region of Ayacucho, where most of the PCP-SL's activity was concentrated between 1980 and 1982. The political strategy of the PCP-SL was inspired by the Chinese revolution and consisted of war advancing from rural areas to the cities. Thus, the main strategic target was Lima, the capital city. Additionally, the PCP-SL aimed to control the coca-producing region in the Amazon. This strategic movement of the war is depicted in Figure 2.1.[8]

As Figure 2.2 shows, there were two clear peaks in violent activities. The first started in 1983, when the government launched their anti-terrorist activities. The second period of intense violence was triggered by the decision of the central committee of the PCP-SL in its first congress (1988) to prioritize the war in the cities (Weinstein, 2007 and CVR, 2004).

Among the victims of the PCP-SL attacks were popular leaders and landholders. The civil population was also severely threatened by the rebels: whenever a village declared itself opposed to the revolution, it was brutally punished. Victims of roadside attacks for collection of supplies and food for the army were mostly traders and farmers. Attacks on the civil population are presented in Table 2.1.[9] Public infrastructure was also a frequent target of the attacks; unfortunately, I only have data on human rights violations. For my purposes, it is important to note that school infrastructure was not affected by either of the parties involved in the conflict.

In September 1992, when violence in the country was at its peak and attacks in the cities were frequent, the head of the revolutionary army, together with most of the central committee of the party, were captured and incarcerated. From that point on, violent attacks from the PCP-SL decreased significantly and its power within the country was limited.[10]

Overall, there were fatalities reported in all but two departments (out of 25) of Perú at some point. The CVR estimates that 54% of the deaths can be attributed to the PCP-SL; the Movimiento Revolucionario Túpac Amaru (MRTA) was responsible for 1.5% of the deaths; and the remaining 43.5% were perpetrated by agents of the state (police, army, navy, etc.) or paramilitary groups.

[8]One potential concern is that, if the violence started in places where educational levels were high, there is a correlation between pre-violence levels of education and violence incidence. Following the argument above, this would affect only the initial period and location of the war. To address this, in Column (2) Table B.5, I exclude Ayacucho from the regressions and the results hold. In any case, the correlation between violence and education should be positive, and this would lead to an underestimation of my results.

[9]The data included in Table 2.1 has to be interpreted carefully, since about 20% of the individual cases of human rights violations do not have information on the occupation of the victim.

[10]Even though after the capture of Abimael Guzman, reports of human rights violations were still reported to the CVR, the government of Alberto Fujimori was responsible for the vast majority of the violence. The ex-president was convicted for some of these charges.

2.2.2 The Data

Information about the presence and intensity of violence comes from the data collected by the Truth and Reconciliation Commission (CVR), which has detailed records of every human rights violation reported during the period of violence. Particularly, the information used in this paper corresponds to illegal detentions, kidnapping, murder, extrajudicial executions, torture, or rape. Individual-level records from the 2007 and 1993 censuses allow me to identify the year and district of birth of each individual. I merge the violence data with the census, thus I can identify the number of human rights violations that took place in the district and year of birth, as well as in every year before and after. Additionally, I use data from the 1992 DHS to analyze the potential causal pathways through which the observed effect is acting.

In 2001, during the transition to democracy, the government appointed the CVR, which was in charge of shedding light on the violent period between 1980 and 2000 and establishing responsibility over human rights violations in that period.[11] The CVR was a flagship program of the transition government, and it was declared one of its priorities. It was well resourced, with a total budget of about US$19 million over two years of operation, provided by the government and aid agencies. Apart from designating reputable commissioners, the CVR also recruited top academics and young professionals for the two years it operated.

One of the main tasks of the CVR was to travel around the country holding public hearings during which they gathered testimonies from victims, relatives, witnesses, and survivors to report any act of violence between 1990 and 2000.[12] All the testimonies were individually coded in order to identify the type of act (rape, murder, torture, etc), location, potential responsible group (armed forces, PCP-SL, MRTA, etc.), identity of the victim, location and date when the act took place, and individual characteristics. The data gathered from this process was merged and cross-tabulated by the identity of the victim with the original registry information from six other data sets gathered at different points in time by human rights organizations, the Red Cross, the judiciary, NGOs, and the ombudsman's office. In this process, the CVR identified approximately 45,000 cases. After dropping double-coded cases and those that could not be cross-validated, the sample size drops to 23,149 individual fatalities (only disappeared or dead). Additionally, in a separate data set, the CVR coded the testimonies and previous reports of violent acts,

[11] A total of thirteen commissioners were appointed. The CVR had to be politically impartial, thus the Commissioners picked were representative public figures from civil society, human rights organizations, academics, the military, the church, and represented different political views. Despite claims that the left was over-represented in the CVR, the public consensus is that the commissioners represented an impartial political view.

[12] Public audiences were widely advertised in the locality where the audience was going to be held, as well as in neighboring localities. The main locations where the audiences were held were determined based on previous reports of the incidence of violence from human rights organizations, the ombudsman, or the press. Additionally, communities could ask for an audience to be held in their town. There were no complaints at the time that the CVR emphasized politically active or unstable areas.

which include detention, kidnapping, torture, and rape, among others; in this data set, each of the 12,807 observations represent one violent act recorded. The final dataset that I work with is an aggregate of these. Overall, I have 36,019 unique reports of violent acts.

One of the drawbacks of the CVR information is that it comes from a non-random sample. The characteristics of the data-gathering process make this a self-selected sample, since people voluntarily attended the public hearings to tell their stories. Due to this fact, I use the presence of violence in the district, rather than intensity, where presence of violence is the occurrence of at least one violent act in the district. The intensity of violence is more subject to bias if particular unobserved characteristics lead to higher reporting in some areas. Section 4.2.1 further discussess the potential biases implied by the sample and my variable definition choice.

The intensity of violence is more subject to bias if particular unobserved characteristics lead to higher reporting in some areas. Other effects of the sample composition are further discussed later in the paper.

Importantly, the reported occupation of the victims allows me to identify whether a teacher was a victim of violence in a particular year and district, which is helpful when trying to pin down the causal channels. It is important to note that the data set only includes human rights violations and not attacks on public infrastructure; hence, I am only identifying the effect of being exposed to violence against human beings in the immediate environment (within the district), and not the effect of the destruction of economic infrastructure or public utilities.

The individual-level information used in the analysis comes from a 2% random samples of the 2007 and 1993 national censuses. Importantly, respondents reported their age and the district where their mothers lived at the moment of birth (or the district where the respondent was born). My final data set is at the individual level and includes individual information, as well as variables recording the number of human rights violations in each year of the respondent's life, in his/her district of birth. It is worth mentioning that errors in the reported age or district of birth may lead to an erroneous assignment of violence exposure. The wrong assignment of the year in which the respondent was exposed to violence is of special concern here. Errors are due to missing information on the month of birth, and people making mistakes reporting age, which could lead me assign violence exposure with a margin of error. To minimize this potential problem, violence exposure will be analyzed during certain sensitive periods of life, rather than assigning it to specific years.[13]

The outcome of interest is educational achievement. To measure the long-term effects of violence, I use the number of years of primary and secondary schooling accumulated

[13]The age reports in the Peruvian census presents bunching at ages that are mutiple of 5 (and less so in ages exactly contiguous to 5, 10, 15, etc.). This is a common problem of using self reported age data. The bunching causes me to wrongly assign the violence to different ages, and attenuates my coefficients. On the other hand, as long as this problem is present in all of the cohorts, and across regions, it should not bias the estimated coefficients.

during one's lifetime.[14] This effect can only be measured among people who are old enough to have finished their schooling cycle by the time of the data collection. Hence, I use the information from 2007 and restrict the analysis to people who were at least 18 years old at the time of the interview. Also, in order to have a suitable control group, I include people who were born in a period without violence (after 1975). Figure 2.2 explains the timeline of the conflict intensity and the overlap periods with the individual-level sample.

On the other hand, when analyzing the short-term effects of violence I use the information from the 1993 census, which was taken right at the point when political violence started declining. The advantage of using a sample of people in school age (6 to 17 years of age) is that we can assume that most of them still live in the same household, and therefore we can compare siblings who were exposed to violence at different stages of their life, holding constant all district-specific and household-specific characteristics.

The main independent variable is the number of years of exposure to violence during different stages of early life. The stages of life that I consider are: in-utero/pre-birth (1 to 2 years before birth), early childhood (0 until 3 years old), pre-school (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 year s old). The definition of the periods in life that I use is purposefully broad, and it responds (i) to the potential errors in reported age, and (ii) to the fact that I want to capture the effects of violence exposure during pregnancy.[15]

Table 2.2 presents descriptive statistics, by violence exposure status. On average, people in the 2007 sample have about 9.4 years of primary and secondary education (out of a maximum of 11). People who were ever exposed to violence in the relevant period in their districts have, on average, one more year of education (9.7) compared to those whose birth district was never exposed to violence while they were children (8.7 years). On the other hand, when I compare the educational achievement of children in school age observed in the 1993 census, those born in districts never exposed to violence in their birth districts have attended school for 4.5 years, while those in districts ever exposed have 0.25 fewer years of education. All covariates shown are balanced between people born in violent and non-violent districts. Table 2.3 shows a more formal test of balance to analyze whether violence took place in districts with particular predetermined characteristics. The results show that districts/years in which violence took place have no statistically different pre-determined characteristics than those districts/years that were peaceful.

[14] For this reason, I truncate the education variable at 11 years, which corresponds to the completion of the secondary schooling cycle in Perú. The main results from the paper are unchanged if the dependent variable is not truncated.

[15] Given that age is reported, rather than birth date, part of prenatal period could be part of the first year of life. For example, a person who is a week away from her 20^{th} birthday at the time of the census will report her age as 19, in which case the prenatal period will cover all but one week of the first year of life (I thank an anonymous referee for this graphic example). On top of this, given that I want to capture violence shocks on household welfare or maternal health during pregnancy, I define the pre-birth/in-utero period as going back two years before birth. This is consistent with Camacho (2009). Further evidence on this is presented in the last section of the paper.

2.3 Theoretical Framework and Empirical Strategy

Consider a typical education production function model in the spirit of those discussed in Hanushek (1979), where the stock of education (S_t) for an individual in period t is a function of her endowments in each period ($E_1, ..., E_t$), the history of educational inputs to which she had access ($N_1, ..., N_t$), factors related to the (time-invariant) demographic characteristics X (i.e. gender, language), and community characteristics ($C_1, ..., C_t$).

$$S_t = s(E_1, ..., E_t, N_1, ..., N_t, X, C_1, ..., C_t) \tag{2.1}$$

The endowment at each period of time, $E_1, ..., E_t$, is determined by both demand- and supply-side factors. Among the former, there are genetic factors (G), household's endowments (E_0^h), and environmental experiences and conditions at the start of each period (V_t). The supply side factors to be considered are denoted by C_t, and one can think of them as school supply, or number of teachers available in the community:

$$E_t = g(G, E_0^h, V_t, C_t) \tag{2.2}$$

The date and location of birth jointly determine the exposure of any given child to the violence. Hence, the reduced form of the model allows me to identify the deviation of an individual outcome from individuals born in the same year, those in the birth district, and the long-run trend in the expansion of education in the province. To be able to identify this effect, I exploit the exogenous variation provided by the moment when the civil conflict started, as well as its geographical localization.

The reduced form equation to be estimated directly follows equations (1) and (2):

$$S_{ijt} = \alpha + \sum_{\tau=t-2}^{t+17} \beta_\tau Violence_{j\tau} + \gamma_p(t) + \delta X_i + \eta_j + \nu_t + \epsilon_{ijt} \tag{2.3}$$

where S_{ijt} is the number of years of schooling achieved by individual i born in district j, and in year t. X_i is a vector of individual time-invariant characteristics, such as gender or ethnicity. The district of birth fixed effects (η_j) control for any specific characteristics of all children born in the same locality. Similarly, the year of birth fixed effects (ν_t) absorb any shock common to all children born in the same year. $\gamma_p(t)$ is a flexible province-specific trends, which is included in all the regressions to account for the differential developments of each province of the country through time, as for example, differentiated economic development, or the intensity in the construction of schools in a particular province. Further, this variable isolates the variation in a person's outcome in deviation from the long-run trend in his/her birth province. Finally, ϵ_{ijt} is a random error term.

One must bear in mind the inclusion of this large set of fixed effects when interpreting the results, since they do not represent the impact of violence on schooling at the national level, but rather the average effect with respect to local averages and year averages, and purged of province-specific flexible trends. Further, the estimates should be interpreted as conservative, since the district fixed effects are eliminating some valuable cross-sectional variation in the violence data.

A particular problem arises due to the fact that educational achievement is a stock variable, hence districts with higher educational achievement in a given year will very likely have similar (or higher) educational achievement the following year. Likewise, there might be education spillover effects between districts. To deal with the spatial and time correlation in the error terms, standard errors allow for an arbitrary variance-covariance structure within birth district by clustering them.

$Violence_{j\tau}$ represents the exposure to violence in the birth district j, during year $\tau = t-2,..,,t+17$, where t is the year of birth. The focus of the estimation is thus on β_τ. Given that I am interested in detecting the effects of exposure in different periods of life, I aggregate these indicators into variables that capture the number of years exposed during each relevant period: pre-birth/in-utero(1 to 2 years before birth), early childhood (0 until 3 years old), pre-school age (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old).

Consistent with the model presented above, exposure to violence can affect individual endowments $(E_1, ..., E_t)$ through several channels. For example, violent attacks of the PCP-SL can affect E_0^h by killing or otherwise affecting a member of the household, which represent a direct income shock for the household that could last several years. Hence, if a household suffers from this shock some years before the child is born, it could still affect the nutrition of the child through food availability, for example. Other potential pathways are the nutrition of the mother, or of the child herself once she is born, which may cause irreversible consequences for her future school attainment through long-lasting effects on cognitive abilities. Camacho (2009) presents evidence suggesting that violence-related stress before birth has negative effects on the child's birth weight, which in turn affects cognitive development. Another channel through which violence exposure could affect the child before s/he is born is through traumatic experiences that affect the mothers and thus the child's development. Finally, this effect can also be more direct, psychologically affecting the child himself, which will in turn affect his cognitive abilities (Grimard and Lazlo, 2010).

Violence could also affect community educational resources (C_0). However, the destruction of educational infrastructure during the conflict by any of the parties involved was not an issue in Perú: the PCP-SL had strong beliefs about the role of education in the revolution, which is clear from the great influence they had on the teachers' union. Schools are a highly valued asset within a community; thus, if the army was to gain the support of the community to fight the terrorists, it did not have an incentive to destroy school infrastructure. A consistent series of information about the number of schools or the number of teachers at the district level is not available. On the other hand, knowing the close relationship between the teachers and the rebels, one channel through which violence affected C_t was the capture or even murder of the local teacher by the national army: about 3% of the reported human rights violations were against teachers (see Table 2.1). It was not an easy task to replace a teacher in a violent area.

The main identifying assumption needed to consistently estimate the causal effect of exposure to violence on educational achievement is that, after controlling for a broad set of district and year fixed effects, and a province-specific time trend, the error term is uncorrelated with the incidence of violence. This assumption will be violated if there was a selection problem whereby districts affected by violence were also those with lower growth of educational achievement.

One way of checking if there is a selection problem is to compare pre-violence levels and trends of education between the districts that were affected by violence and those that are used as controls. In the 1993 census I can compare the educational level of the cohorts that, at the time of the start of the conflict, were old enough to have finished their educational cycle.

Panel A of Table 2.4 shows the average years of education of the cohort of people who were between 17 and 22 in 1980, separating them by the number of years of violence exposure of their birth districts. People born in districts that were never affected by violence had about 7.4 years of education, while those who were born in a district that was exposed to violence for a period of 1 to 3 years have slightly more education (7.5 years). Likewise, those born in districts with higher levels of exposure have about 7.3 years of education. None of the differences between these groups of districts are statistically significant, and the same pattern holds for previous cohorts. Further, since my identification strategy hinges on the fixed effect, I don't only need to see that the pre-violence levels of education balanced, but more importantly, that cohort differences are as well. Panel B in Table 2.4 shows the difference in educational attainment between different cohorts, across districts with different levels of violence exposure. People born in a non-violent district did not attain significantly more education than those born in violent districts.[16]

Another threat to the identification assumption is that the characteristics of the population change as a function of the timing of the violent attacks. This means that the characteristics of the population settled in a particular district are similar across violent and non-violent years. One way to test this assumption is to see whether these pre-determined characteristics in each districtXyear cell are a function of the presence of violence. I run this test on the 2007 and 1993 data in Table 2.3. One important concern is that fertility choices are determined by the presence of violence. If this were the case, the size of cohorts exposed to violence would be smaller than in peaceful years. Results show that cohorts affected by violence were not smaller than the non-affected ones in neither 1993 nor 2007. For the 2007 sample, I see that there are marginal differences in average age and the percentage of native speakers, with violent districts having older people (though the difference is close to zero), and smaller indigenous populations. On the other hand, the gender composition and migration in violent districts do not seem to be different by violence status.

[16]As an additional test for the identification assumption I also run regressions to see whether incidence or presence of violence in the district predict pre-war education levels (or cohort differences). I find an insignificant relationship, and the coefficients are very close to zero. These results are omitted, but are available upon request.

In 1993, there are no significant differences in cohort size, gender composition, migration rates, percentage of indigenous population, or education of the household head. There is a difference in wealth, with violent districts being slightly poorer .[17]

2.4 Results and Discussion

2.4.1 Long-term Consequences of the Conflict

Table 2.5 shows the results of the main specification presented in equation (3). In all the specifications I use a set of variables indicating the number of years that each individual was exposed to violence during each period of the early life: in-utero/pre-birth (1 to 2 years before birth), early childhood (0 to 3 years of age), pre-school (4 to 6), primary school (7 to 12), and secondary school age (13 to 17).[18] Being exposed to violence before entering school -that is, during the pre-birth/in-utero years, early childhood, or pre-school age- has a statistically and economically significant effect on long-run human capital accumulation. As shown in column (1) in Table 2.5, an additional year of exposure to violence before birth implies that the person will accumulate 0.07 fewer years of education; if the shock happens during the early childhood or in pre-school age, it reduces long-term educational achievement by 0.05 years for each year of exposure to violence, respectively.[19] On the other hand, living in a district affected by violence during primary or secondary school age does not have a significant impact on long-run educational achievement. Further, I expect that any violent shock experienced by the household during the years before the mother was pregnant, or about to conceive, will not have any effect on the child's educational outcomes. As a robustness check, Column (2) tests this hypothesis by including indicators for the presence of violence in the district of birth during the years before the in-utero/pre-birth period. As expected, I do not find any statistically significant effect for these variables.[20][21]

[17] I thank an anonymous referee for suggesting this test.

[18] The results are robust of the choice of years grouped together. Figure 2.3 show the results year by year. The inclusion of the years before birth in the "early childhood" period reflects the fact that violence shocks can have persistent effects on the mother's health status and errors in assigning violence due to age reporting.

[19] The point estimates are not significantly different from each other.

[20] In Table B.3 I explore the heterogeneous impacts of violence by gender and ethnicity. The point estimates for the exposure to violence in all periods are larger for girls than those found in my benchmark specification in the first column, and statistically significant for exposure during the early childhood, and in the pre-school period. On the other hand, for men only the exposure to violence during the early childhood seems to be an important determinant of future schooling, and the coefficient is smaller in magnitude. Meanwhile, I find that the effect for native speakers is larger than for Spanish speakers. Though, these results are not statistically significant due to the reduced sample size.

[21] Galdo (2010) estimates the effects of violence exposure on labor market outcomes in Peru using an instrumental variables approach. I run similar regressions using my specification, and using the data from the Peruvian household survey (ENAHO). The results that I obtain in my fixed effects approach are of a similar magnitude. Being exposed to an additional year of violence during in utero or early childhood

One potential concern with the results shown in Column (1) is that the time-series correlation in the exposure of violence might be affecting my estimates. One way to indirectly test this is to include in the same regression the indicator variables for the violence exposure before birth, as well as those indicating the number of years of exposure to violence in each period of the individual's life. I do this in Column (3), finding again no statistically significant results for the exposure to violence during the years before pregnancy.[22] The coefficients associated with violence in the in-utero, early childhood, and pre-school years are still significant at the conventional levels and their magnitude is slightly increased compared to those shown in column (1). [23]

The average child affected by violence in each of the periods analyzed, in-utero, early childhood, or pre-school, had about 1.4, 2.2 and 1.9 years of exposure, respectively. This means that the average child exposed to violence in-utero/pre-birth accumulated about 0.10 fewer years of schooling than his/her peers who lived in peaceful districts or were born in peaceful years. For the average child affected during either early childhood or pre-school age, the effects of an additional year of exposure are 0.11 and 0.10, respectively.[24] Moreover, I can be fully flexible in the functional form assumed to fit equation (3), and include indicators for each year exposed to violence. Figure 2.3 shows these results in a graphical way. Consistent with Table 2.5, the effect of violence exposure before the mother was pregnant (3 to 6 years before) is not statistically different from zero, while this effect is relevant while the child is between -2 and 6 years of age. The coefficients corresponding to older ages are again indistinguishable from zero.[25]

To put these results in context, Duflo (2001) finds that the effect of the massive school construction program in Indonesia on school attainment is of similar magnitude, but in the opposite direction: each school constructed per 1,000 children led to an increase of 0.12 to 0.19 years of education.

2.4.2 Potential Biases and Concerns

2.4.2.1 Sample Composition

The violence data coming from the CVR is mainly self-reported, which can raise a number of problems. First, it is plausible that the under-reporting in the data comes

leads to a decrease in wages of about 2%. These results are displayed on Table B.2.

[22]The F-test of joint significance for exposure to violence three or more years before birth fails to reject at the 10% the null of being jointly zero in column (2) and column (3).

[23]As an additional robustness test, I run the regressions excluding the locations where the conflict was more persistent, and the main insights of the paper hold. I show these results in Table B.5.

[24]An alternative way of thinking about these results is as the treatment on the treated effect for the direct experience of war on educational achievement.

[25]As a robustness check, I run the same specification excluding regions of the country that had a particularly high and continuous presence of violence (Ayacucho and Huancavelica), those that were close to the coca-producing areas (Huanuco and San Martin), and the capital city (Lima), which is significantly more urban and rich than the rest of the country. The results are very similar, and are shown in Table B.5.

from the group that was more affected by violence, i.e. those for whom verbalizing the incidents in front of the Commission was more difficult, such as the victims of sexual violence. Second, testimonies were collected in relatively big towns, which implies that some of the most vulnerable populations (for whom the opportunity cost of reporting the violence was higher) were not able to report human rights violations. Further, it is more likely the the under reporting was more pronounced for violent acts that took place at the initial stages of the conflict, in localities that were least affected, or in places where the population was less proactive about the reporting process. This possible selective under-reporting of the violence data is likely to lead to an under-estimation of my results. Hence, the point estimates found should be interpreted as a lower bound. To partially overcome this issue, I rely on a measure of presence of violence, rather than intensity. For the presence of violence variable to turn on, it is sufficient to observe one act of violence; hence, any under-reporting is minimized by this variable.[26]

Another possible bias in my result may come from the fact that the fatality victims of violence are not on the census. However, these people were those most affected by violence. Hence, the selection problem induced by the fatal victims again introduces a downward bias in the estimation of the effects of violence.

2.4.2.2 Migration

A more serious concern comes from the fact that the questions recorded in the census only ask about the district of birth, where the person lived five years before the interview, and the current location. I do not observe the actual migration history, nor the reasons to leave one's hometown. Not knowing the exact location where each respondent was located each year can cause me to wrongly assign her exposure to violence. The bias implied by this wrong assignment cannot be signed a priori.

There is anecdotal evidence that people who migrated from violent areas were discriminated against in the cities and thus denied access to public services such as education or health care. If that were the case, the point estimates shown before would be overstating the effect of violence on schooling. On the other hand, people who migrate away from conflict areas are likely to go to larger cities, where there are many more employment opportunities and better access to public services, and where they have a social network to support them. Hence, the development outcomes of people who migrated should be better than those of their peers left behind. In this case, including the migrants in the estimation would imply an underestimation of the effect.

Additionally, positive or negative selection into migration could also bias the results: if people who were able to escape from the violent districts were those at the top end of the income distribution, had they stayed, the effect on their human capital accumulation

[26] I have run all the regressions in the paper using an intensity, rather than presence of violence measure, and the magnitude and significance of the main results are consistent with the ones shown. These results are available in Table B.1.

would have been higher. If that were the case, the estimates in Tables 2.5 and 2.6 would be overstating the impact of violence.

There is no clear way to determine the migration bias other than empirically. One indirect way to deal with this issue is to restrict the sample to non-migrants, or those who were living there five years before the 2007 census, and compare the point estimates of the original sample with those of the non-migrants. Table 2.6 presents the estimation of the model splitting the sample between those who report living in their birth-place and those who migrated at different points in their lives. The results show that the effect of violence for the non-migrants is higher than in the full sample, especially for exposure during the in-utero/pre-birth period, for which we observe an effect on those still living in their birth districts of about 0.09, while for migrants it is small and not statistically significant. If exposure happened at other periods of the early life, there are no differences between migrants and non-migrants. The results are very similar when we differentiate between migration at any point in life (Columns (2) and (4)) and when I exclude from this category those who lived in their birth district until 2002 (Columns (3) and (5)). These findings are in line with Escobal and Flores (2009), who document that mothers who migrate out of violent districts have children with higher nutritional statuses when compared to their peers who stayed, but they find no differences in cognitive abilities.

An additional concern might be that people who migrate are different from the ones who stay. For example, they might be richer, more educated, more forward-looking, etc. However, this seems not to be the case in the data I am working with. The evidence shown in Table 2.3 shows that the presence of violence does not influence the composition of the cohorts living in the district, in terms of their size, gender composition, average age, average education, percentage of native speakers, education of the parents, wealth, or even migration. [27]

In sum, the effect observed in Tables 2.5 and 2.6 does not contain a significant migration bias. If anything, the bias exists for people affected by the conflict in the in utero period, and in any case it is an downward bias.

2.4.3 Short-term Results and the Persistence of Violence

The results in the previous section show that living through violent periods during critical periods of life causes lower school achievement in the long-run. This finding contrasts with other studies which document that, after suffering civil wars, countries are able to recover in most areas of development, such as nutrition, education, economic

[27] In an additional robustness check, I regress the probability of migration on the incidence of violence in each year before and after birth, time-invariant individual characteristics, a set of year-specific effects, and a province time trend. These results are shown in Table B.4. Once I include district-specific intercepts, there is no significant association between migration status and exposure to violence. These results support the idea that migration is higher in the districts affected by violence, but this migration responds to a structural, time-invariant characteristic of those districts, and the timing and location of violence does not differentially affect the likelihood of migration.

growth, etc.[28] In this section, I explore the short-term impacts of political violence on schooling, and compare them with the long-terms effects estimated above. I estimate equation (3) on the data from the 1993 census. Given the findings in the previous section, I focus on the children in school age who report living in 1993 in the location where they were born. This also allows for a tighter identification strategy, since I can not only compare children within the same district, but I can also compare children within the same household who are affected by violence at different stages of their lives.

The results are shown in Table 2.7. Being exposed to violence during the in-utero period, early childhood, pre-school age, or primary school age has a statistically and economically significant effect on schooling. Considering that the average child exposed to violence in each of these periods has about 1.4, 2.0, 1.8, and 2.9 years of violence exposure, respectively, the overall effect for the average child affected by violence in each of these periods is 0.98 fewer years of schooling.

Given that the vast majority of children in school age still live in their parents' household, I am able to exploit the variation in the timing of violence exposure between siblings to identify the parameters of interest, keeping constant all time-invariant household characteristics. Results are reported in Columns (2) of Table 2.7. The sibling difference model gives very similar results in terms of magnitude and statistical significance. Taken together, these results shed some light on the potential mechanisms that might be working behind the observed effect. The fact that the point estimates are basically unchanged when I account for time-invariant household characteristics allows me to rule out the hypothesis that the causal pathway through which experiencing violence affect educational achievement is not a persistent shock to household welfare, or other time-invariant household characteristic.

Recall from Table 2.5 that, for people observed in 2007, the average child exposed to violence accumulates 0.31 fewer years of education.[29] Comparing these results, we see that the coefficients associated with shocks in the pre-birth/in-utero period are similar between the long- and short-term estimations (-0.07 and -0.102, respectively), while the coefficients associated with violence exposure in the early childhood or pre-school periods are about three times as large in the short-term than the long-term.

Further, in the estimation of the short-term effects, I see that violence during the primary school age is significant, and the magnitude of the coefficient is non-negligible, while in the long-run, violence in this period does not seem to have an effect on school attainment. This suggests that the effect of violence on human capital accumulation is mitigated as time goes by. More importantly, children who are affected by violence during the very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. On the other hand, those who experience the shock during early childhood or pre-school age partially recover. Finally, people exposed to violence once they have started their schooling

[28] See for example, Miguel and Roland (2011), Davis and Weinstein (2002), Brakman, Garretsen and Schramm (2004), Cerra and Saxena (2008).

[29] In this case, I am unable to test whether shocks during high school affect schooling outcomes, since in my sample I do not observe children who are old enough to be in high school and have suffered violence.

cycle are able to fully catch up with their peers who didn't experience violence in this period. This evidence is consistent with the extensive literature about economic shocks and the critical-period programming (Almond and Currie, 2011; Alderman, Hoddinott, and Kinsey, 2006; Maccini and Yang, 2009, among others).

Comparing the magnitude of these results with the ones available in the literature, Akresh and de Walque (2010) found that four years after the Rwandan genocide, children (directly) exposed to violence accumulate 0.5 fewer years of primary education, about half of what I find in my short-term estimates.

2.4.4 Possible Causal Pathways

As shown in the model above, one potential mechanism behind the observed effect might be a supply side shock: if a teacher was directly affected by violence, it may have made it harder for children to go to school. The CVR recorded the occupation of the victims, thus I can directly test this hypothesis by including in my benchmark regressions a dummy variable for whether a teacher was attacked in the district of birth within each period of the student's life. These results are shown in Table 2.8. In the short-term, conditional on being exposed to violence, an attack on a teacher during the in-utero, early childhood or pre-school periods leads to a significant decrease in schooling of about 0.55, 0.48 and 0.28 years, respectively, as shown in Columns (1) and (2). The fact that the effect is significant for any period before the child was old enough to enter school suggests that the injury or death of a teacher delayed the entrance to school. Further, when I look at the long-term effects of this particular type of violence, I find that having a teacher attacked does not significantly affect the long-term accumulation of human capital. Taken together, these results suggest that violence against teachers leads to a delay in school entrance but does not lower educational achievement in the long-run.

On the other hand, I can also present some suggestive evidence on whether the effect is driven by a demand-side shock, such as effects on health, which affect child's cognitive development (Camacho, 2009). Using data from the 1992 DHS, and a between-siblings difference model, I can test whether violence exposure has an effect on the weight-for-height or height-for-age z-scores. These results are shown in Table 2.9. I indeed find some evidence that the occurrence of a shock between two years before birth and the first year of life has a negative effect on health status. The reduced sample size and the high data demand of the identification based on household fixed effects limits my ability to do statistical inference in this case. Nevertheless, the magnitude of the coefficients for the years -2 through 1 is an order of magnitude larger than the ones associated with violence experience after the first year of life.

One other potential mechanism through which violence exposure might affect future educational outcomes is through household wealth, which in turn has an effect on children's cognitive development. Even though I am not able to directly test this channel, I can use the information contained in the 1992 DHS to provide some suggestive evidence. In Column (1) of Table 2.10, I run an OLS regression of an asset index (Filmer and

Pritchett, 2001) on whether there was violence in the district during the years preceding the survey and some relevant controls. The results suggest that violence did not differentially affect asset tenure at the household level. Further, using a similar strategy, I can determine whether the health status of the mothers in the sample is affected by violence. Column (2) illustrates this point, showing that exposure to violence the year just before the survey is correlated with lower body mass index of the women in reproductive age.[30]

To summarize, I find suggestive evidence of two potential channels through which violence affects educational achievement: (i) supply side shocks, specifically attacks against teachers, increase the educational deficit of children exposed to the shock; and (ii) on the demand side, violent events occurring between one year before birth and one year after birth decreases a person's health status. Finally, this effect does not seem to operate through shocks to household asset tenure, but rather through maternal health.

2.5 Summary and Conclusions

Civil conflict is a widespread phenomenon around the world, with about three-fourths of countries having experienced an internal war within the past four decades (Blattman and Miguel, 2009). The short-term consequences of these conflicts are brutal in terms of lives lost, destruction of economic infrastructure, loss of institutional capacity, deep pain for the families of the people who died in the war, etc. However, the economic literature so far has had little to say about the long-term effects of these conflicts on those who survived, but still were exposed to them. In this paper I address this issue, looking at the long- and short-term consequences of political violence on educational achievement in Perú.

The empirical literature dealing with the effects of civil conflicts, especially at the macro level, shows robust evidence that those countries exposed to severe violence are able to catch up after a certain period of time, recovering their pre-conflict levels in most development indicators. On the micro side, several papers document the very short-term consequences of conflicts on human development, especially on nutrition and education. However, if the trends observed at the macro level are followed at the micro level, one might expect these effects to vanish over time.

In this paper, I analyze the Peruvian case, in which the constant struggles between the army and the rebel group PCP-SL lasted over 13 years, causing the death of about 69,290 people, as well as huge economic losses. Using a novel data set collected by the Peruvian Truth and Reconciliation Commission (CVR), which registers all the violent acts and fatalities during this period, merged with individual level census data from 1993 and 2007, I quantify the long-term effects of violence on human capital accumulation for people exposed to it in the early stages of life. The identification strategy used in

[30]The fact that violence shocks one or two years before birth have an effect on maternal and child health speaks to the results shown in the previous section, where I observe that shocks preceding birth significantly affect educational attainment.

the analysis exploits the exogenous nature of the timing and geographic localization of violence, which allow me to identify the average losses in educational achievement in the long-term, relative to local averages and year averages, and purged from province flexible trends.

The results show that the average person exposed to political violence before school age (in-utero, early childhood, and pre-school age ranges) has accumulated 0.31 fewer years of schooling upon reaching adulthood, with stronger short- than long-term effects. In contrast, individuals who experience the shock after starting school fully catch up to peers who were not exposed to violence. Concerns with the sample composition and migration issues lead us to think that these results ought to be interpreted as lower bounds of the estimated effects.

Short-term effects show that the persistence of the shock depends on the moment in life when the child was exposed to violence. Shocks in the pre-birth/in-utero period have a similar effect in the short and long-run. Those exposed during early childhood or pre-school age experience effects that are three times larger in the short-run, while violence exposure once the schooling cycle has started only has a short-term effect. This suggests that children who are affected by violence during the very early childhood (pre-birth/in-utero) suffer irreversible effects of violence. Those who experience the shock in the early childhood or pre-school age partially recover, while people exposed to violence once they have started their schooling cycle are able to fully catch up with their peers who did not experience violence in this period. This result contrasts with the cross-country findings that the effects of violence vanish over time.

Finally, I look at the potential causal channels though which this effect is working, finding suggestive evidence for two of the hypothesized mechanisms. On the supply side, attacks against teachers decrease the educational achievement of children, mainly by delaying school entrance, but this effect is not persistent. On the demand side, violent events occurring within a year before or after birth decrease the child's health status. This effect does not seem to go through shocks to household asset tenure, but through maternal health.

Overall, the results in this paper contribute to the evidence that shocks during the early stages of one's life have long-term irreversible consequences on human welfare. This suggests that relief efforts should be targeted to pregnant mothers and young children, and then children in the early stages of their schooling cycle, if we want to minimize the long-term welfare losses for the society.

Figure 2.1: Geographical Expansion of the Conflict: # of Fatalities Reported to the CVR, by District

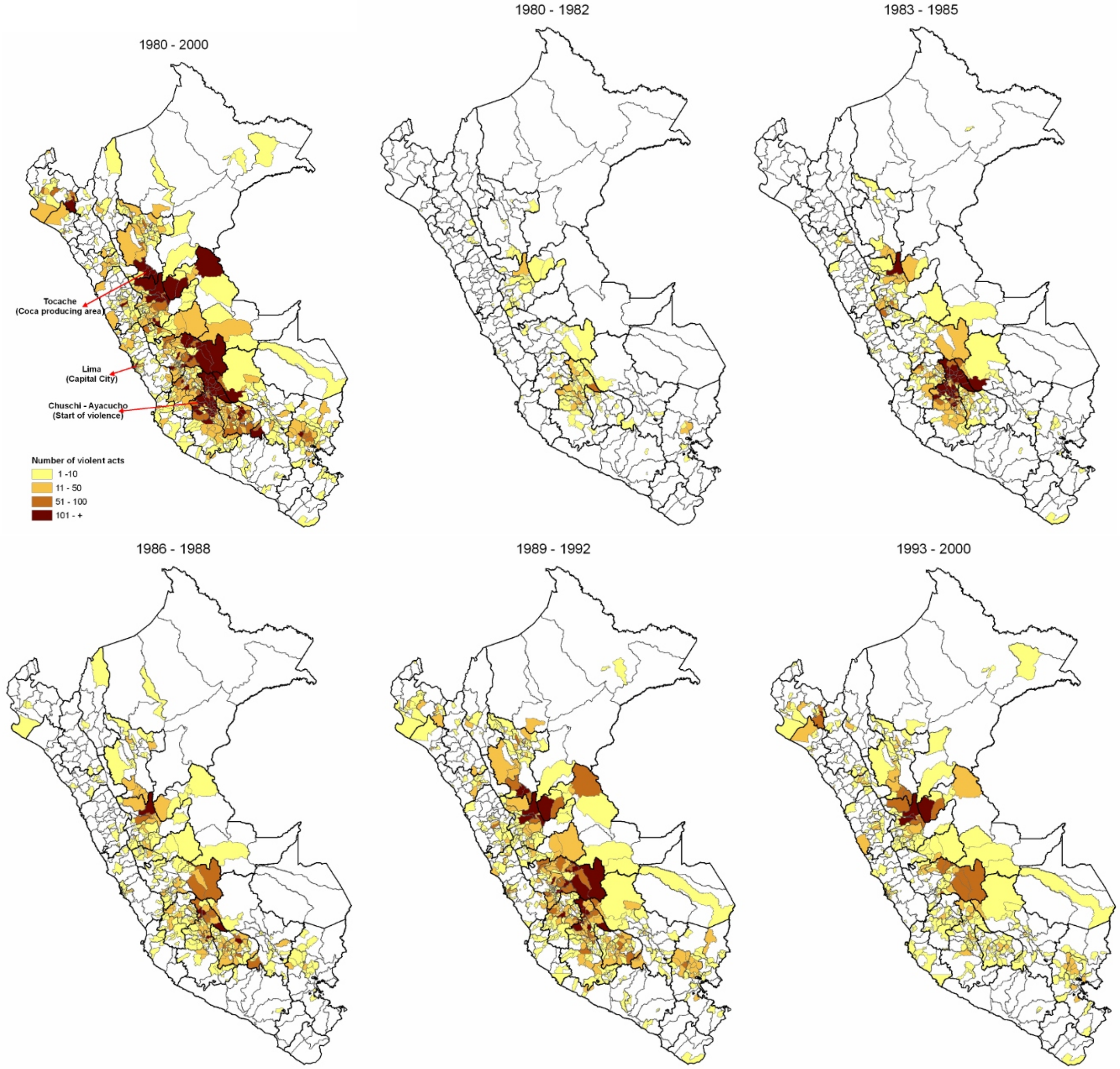
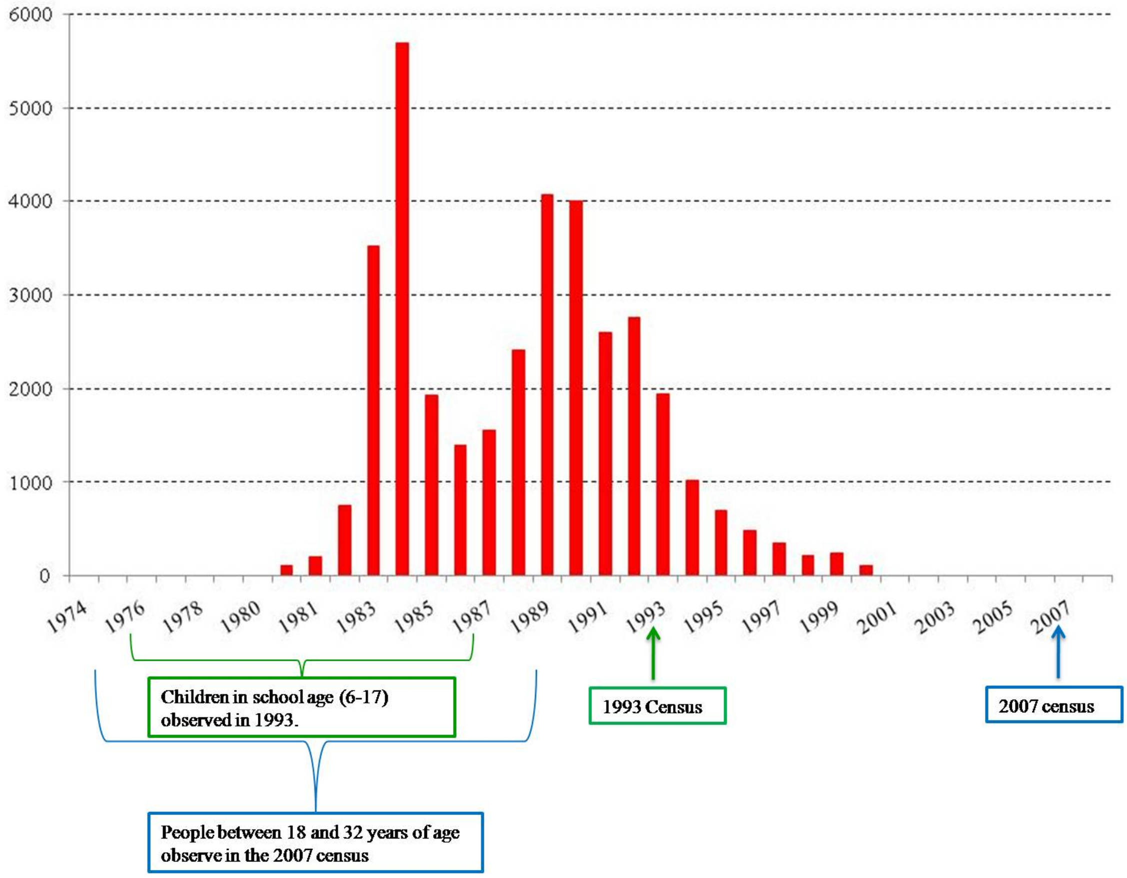


Figure 2.2: The Timing of the Conflict and Structure of the Data: # of Violent Events Reported to the CVR, by Year of Occurrence



Source: CVR, 2004.

Note: The figure shows the number of human rights violations recorded by year, as well as the structure of the data used in the analysis. From the 2007 census, I consider all people between 18 and 32 years old (born between 1975 and 1989). The observations from the 1993 census correspond to all children in school age (born between 1976 and 1987).

Figure 2.3: The effect of Violence Exposure on Human Capital Accumulation, by age

Note: The figure presents the coefficients (and confidence intervals) for exposure to violence between 6 years before birth until 17 years old. The control variables included in the equation are gender, mother's language, district fixed effects, year of birth fixed effects, and a province level cubic trend.

Table 2.1: Demographic Characteristics of the Victims of Human Rights Violations

Occupation of the victim	%
Farmer	47,8
Local authorities	18,4
Sales person, trader	6,9
Housewives	5,5
Independent workers	5,2
Student	3,5
Teacher	3,4
Dependent employees	3,0
Other	2,2
Army	1,8
Manual laborer	1,6
Professionals or intelectual	0,6
Total	100,0

Genderof the victim	%
Male	79,0
Female	21,0
Total	100,0

Educational level of the victim	
No education	16,4
Primary	46,5
Secondary	24,6
Higher	12,5
Total	100,0

Languageof the victim	
Native	70,9
Spanish	29,1
Total	100,0

Source: CVR, 2004.

Table 2.2: Summary Statistics

Variable	2007 Census					1993 Census				
	Obs.	Mean	S.d.	Min.	Max.	Obs.	Mean	S.d.	Min.	Max.
Full Sample										
Yrs of education	139446	9.40	2.82	0	11	75314	4.36	3.23	0	11
Gender (=1 male)	139446	0.49	0.50	0	1	75314	0.51	0.50	0	1
Mothers' language (=1 nat)	139446	0.13	0.34	0	1	75314	0.21	0.41	0	1
Migrant (1=migrated)	139446	0.39	0.49	0	1					
No of yrs exposed (in utero)	139446	0.21	0.53	0	2	75314	0.14	0.44	0	2
No of yrs exposed (early childhood)	139446	0.71	1.19	0	4	75314	0.50	1.03	0	4
No of yrs exposed (pre-school age)	139446	0.82	1.09	0	3	75314	0.64	1.00	0	3
No of yrs exposed (primary school age)	139446	1.70	1.97	0	6	75314	1.47	1.90	0	6
No of yrs exposed (high school age)	139446	0.97	1.51	0	5					
Never exposed to violence										
Yrs of education	40086	8.70	3.22	0	11	31852	4.50	3.29	0	11
Gender (=1 male)	40086	0.49	0.50	0	1	31852	0.51	0.50	0	1
Mothers' language (=1 nat)	40086	0.15	0.36	0	1	31852	0.20	0.40	0	1
Migrant (1=migrated)	40086	0.38	0.48	0	1					
Exposed to violence at least once										
Yrs of education	99360	9.69	2.59	0	11	43462	4.25	3.18	0	11
Gender (=1 male)	99360	0.49	0.50	0	1	43462	0.51	0.50	0	1
Mothers' language (=1 nat)	99360	0.12	0.33	0	1	43462	0.22	0.41	0	1
Migrant (1=migrated)	99360	0.40	0.49	0	1					
No of yrs exposed (in utero)	99360	0.29	0.61	0	2	43462	0.23	0.56	0	2
No of yrs exposed (early childhood)	99360	1.00	1.30	0	4	43462	0.86	1.23	0	4
No of yrs exposed (pre-school age)	99360	1.15	1.13	0	3	43462	1.10	1.10	0	3
No of yrs exposed (primary school age)	99360	2.39	1.95	0	6	43462	2.54	1.87	0	6
No of yrs exposed (high school age)	99360	1.36	1.64	0	5					

Notes: For the 2007 census, I include people between 18 and 32 years old. "Ever exposed to violence" are those exposed in any of the relevant periods of analysis: in utero, early childhood, pre-school, primary school age, or secondary school age. For the 1993 census, the statistics presented are for all children in school age (6-17) who, still lived in their birth district. "Ever exposed to violence" follows the same definition as above.

Table 2.3: Balancing tests: Violence exposure on Predetermined Characteristics

	(1) Log(cell size)	(2) % Male	(3) Avg. Age	(4) % migrants	(5) % Native speakers	(6) Avg. Yrs. of educ. hh head	(7) Avg. Asset index
	Panel A: 2007 Census Sample						
Presence of violence	0.026	0.001	0.000	0.011	-0.009		
	(0.010)**	(0.007)	(0.000)***	(0.007)	(0.005)*		
Constant	1.206	0.488	37.000	0.401	0.295		
	(0.015)***	(0.010)***	(0.000)***	(0.010)***	(0.007)***		
District fixed effects	Yes	Yes	Yes	Yes	Yes		
Year fixed effects	Yes	Yes	Yes	Yes	Yes		
Province cubic trend	Yes	Yes	Yes	Yes	Yes		
Observations	51792	51792	51792	51792	47724		
Number of districts	1825	1825	1825	1825	1825		
R-squared	0.12	0.01	1.00	0.24	0.05		
	Panel B: 1993 Census Sample						
Presence of violence	0.004	0.000	-0.000	0.002	0.002	-0.040	-0.046
	(0.013)	(0.009)	(0.000)*	(0.006)	(0.005)	(0.061)	(0.019)**
Constant	1.132	0.498	27.891	0.518	0.338	-2.766	-0.871
	(0.020)***	(0.014)***	(0.000)***	(0.012)***	(0.008)***	(0.107)***	(0.042)***
District fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province cubic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	32220	32220	32220	32220	32220	32153	31755
Number of districts	1781	1781	1781	1781	1781	1780	1776
R-squared	0.10	0.02	1.00	0.22	0.04	0.06	0.10

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. Each observation in this regression represents a districtXyear cell, and the variable "Presence of violence" is an indicator equals to one whenever there was at least on violent event in the district in that particular year. The sample includes all people born after 1970 interviewed in each sample.

Table 2.4: Pre-Violence Average Years of Education, by Violence Exposure

Panel A: Average years of education, by year cohorts			
No. of years exposed to violence	1958-1963	1953-1957	1948-1952
0	7.4	6.7	6.1
1 - 3	7.5	6.9	6.2
4 - 6	7.3	6.5	5.5
>6	7.0	6.3	5.4
Total	7.4	6.7	6.0

Panel B: Difference in educational attainment between cohorts		
No. of years exposed to violence	[1958-1963] - [1953-1957]	[1953-1957] - [1948-1953]
0	0.70	0.65
1 - 3	0.73	0.66
4 - 6	0.76	0.96
>6	0.75	0.80
Total	0.72	0.70

Source: CVR, 2004 and and National Census 1993.

Notes: Panel A displays the average years of education by the cohort of people born between 1958-62, 1953-57, and 1948-53, who were old enough to have finished high school by the time the violence started. Panel B shows the differences between cohorts. None of the differences by levels of exposure to violence are statistically significant.

Table 2.5: Violence and Human Capital Accumulation: Long-term Effects

	(1)	(2)	(3)
		Years of education	
Exposed to violent events in his/her year -6		-0.055	-0.072
		(0.041)	(0.048)
Exposed to violent events in his/her year -5		-0.019	-0.034
		(0.031)	(0.035)
Exposed to violent events in his/her year -4		-0.021	-0.040
		(0.037)	(0.038)
Exposed to violent events in his/her year -3		-0.051	-0.059
		(0.031)	(0.035)
No of yrs exposed (in utero)	-0.071		-0.079
	(0.021)***		(0.023)***
No of yrs exposed (early childhood)	-0.051		-0.061
	(0.014)***		(0.016)***
No of yrs exposed (pre-school age)	-0.051		-0.063
	(0.015)***		(0.016)***
No of yrs exposed (primary school age)	-0.019		-0.032
	(0.014)		(0.016)**
No of yrs exposed (high school age)	0.000		-0.019
	(0.013)		(0.016)
Gender (male=1)	0.438	0.437	0.437
	(0.030)***	(0.030)***	(0.030)***
Mother's language (native=1)	-1.747	-1.747	-1.747
	(0.064)***	(0.064)***	(0.064)***
Constant	8.613	8.589	8.658
	(0.066)***	(0.054)***	(0.071)***
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dep. var.		9.40	
Observations	139446	139446	139446
R-squared	0.06	0.06	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census. The periods of life considered are defined as follows: early childhood (-2 until 3 years old), pre-school (4 to 6 years old), primary school age (7 to 12 years old), and high school age (13 to 17 years old). The F-test of joint significance for the coefficients before year -2 in Columns (2) and (3) fails to reject the null that they are jointly equal to zero. In Column (2), the F-test is 1.94 (p-value=0.1013), while in Column (3), the F-test is 1.86 (p-value=0.1157).

Table 2.7: Violence and Human Capital Accumulation: Short-term Effects

	(1)	(2)
	Years of Education	
No of yrs exposed (in utero)	-0.102	-0.087
	(0.036)***	(0.042)**
No of yrs exposed (early childhood)	-0.140	-0.146
	(0.028)***	(0.031)***
No of yrs exposed (pre-school age)	-0.133	-0.139
	(0.030)***	(0.032)***
No of yrs exposed (primary school age)	-0.113	-0.111
	(0.027)***	(0.027)***
Gender (male=1)	0.137	0.128
	(0.021)***	(0.022)***
Mother's language (native=1)	-1.000	-0.445
	(0.054)***	(0.118)***
Constant	6.835	134.858
	(229.997)	(1.114)***
Household fixed effects	No	Yes
District of birth fixed effects	Yes	No
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Mean dependent variable	4.36	4.35
Observations	75314	63888
R-squared	0.50	0.54

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people in school age (6-17) who still live in their birth district, interviewed in the 1993 national census.

Table 2.6: Violence and Human Capital Accumulation, by Migration Status

	(1)	(2)	(3)	(4)	(5)
	Years of education				
	Full Sample	Non-Migrants		Migrants	
		Lives in birth district	Lived in birth dist. 5 years ago	Doesn't live in birth district	Didn't live in birth dist. 5 years ago
No of yrs exposed (pre-birth)	-0.071	-0.095	-0.094	-0.023	-0.027
	(0.021)***	(0.028)***	(0.027)***	(0.029)	(0.031)
No of yrs exposed (early ch.)	-0.051	-0.052	-0.055	-0.052	-0.038
	(0.014)***	(0.018)***	(0.018)***	(0.021)**	(0.019)**
No of yrs exposed (pre-school)	-0.051	-0.054	-0.046	-0.059	-0.075
	(0.015)***	(0.019)***	(0.018)**	(0.022)***	(0.024)***
No of yrs exposed (primary school)	-0.019	-0.024	-0.021	-0.018	-0.019
	(0.014)	(0.018)	(0.017)	(0.021)	(0.023)
No of yrs exposed (high school)	0.000	-0.009	-0.001	0.004	0.005
	(0.013)	(0.017)	(0.016)	(0.020)	(0.022)
Gender (male=1)	0.438	0.485	0.476	0.395	0.393
	(0.030)***	(0.040)***	(0.038)***	(0.028)***	(0.029)***
Mother's language (native=1)	-1.747	-1.909	-1.774	-1.151	-1.232
	(0.064)***	(0.086)***	(0.078)***	(0.061)***	(0.067)***
Constant	7.674	7.061	8.244	7.190	9.735
	(0.052)***	(0.066)***	(0.067)***	(0.088)***	(0.110)***
District of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes
Observations	139446	84884	96812	54562	42634
R-squared	0.06	0.07	0.06	0.05	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 17 and 32 years old interviewed in the 2007 national census. Migrants in columns (2) and (4) are defined as those who currently live in a district different from their birth district. In columns (3) and (5), I exclude from the group of migrants those who live in a different district from where they were born, but five years ago lived in the same district where they were born.

Table 2.8: Supply Side Shocks and Human Capital

	(1)	(2)	(3)
	Years of Education		
	Short-term		Long-term
No of yrs exposed (in utero)	-0.063	-0.043	-0.059
	(0.036)*	(0.041)	(0.022)***
No of yrs exposed (early childhood)	-0.107	-0.100	-0.042
	(0.029)***	(0.032)***	(0.016)***
No of yrs exposed (pre-school age)	-0.112	-0.105	-0.043
	(0.032)***	(0.033)***	(0.016)***
No of yrs exposed (primary school age)	-0.101	-0.093	-0.015
	(0.028)***	(0.027)***	(0.015)
No of yrs exposed (high school age)			-0.001
			(0.014)
Teacher was a victim (in utero)	-0.528	-0.554	-0.141
	(0.164)***	(0.155)***	(0.075)*
Teacher was a victim (Early childhood)	-0.331	-0.480	-0.061
	(0.097)***	(0.106)***	(0.047)
Teacher was a victim (pre-school age)	-0.174	-0.281	-0.036
	(0.084)**	(0.085)***	(0.041)
Teacher was a victim (primary school age)	-0.056	-0.125	0.002
	(0.065)	(0.072)*	(0.030)
Teacher was a victim (high school age)			0.051
			(0.036)
Gender (male=1)	0.136	0.129	0.437
	(0.021)***	(0.023)***	(0.030)***
Mother's language (native=1)	-1.000	-0.448	-1.747
	(0.054)***	(0.118)***	(0.064)***
Constant	6.941	134.740	8.604
	(967.574)	(1.120)***	(0.066)***
Household fixed effects	No	Yes	No
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dependent variable	4.35		9.4
Observations	75314	63888	139446
R-squared	0.51	0.54	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample in columns (1) and (2) includes all people in school age (6-17) who still live in their birth district, interviewed in the 1993 national census. For column (3), the sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table 2.9: Demand Side Shocks and Human Capital: Child Health

	(1) Weight for age z-score	(2) Haight for age z-score
Exposed in his/her year -2	-0.064	-0.095
	(0.112)	(0.096)
Exposed in his/her year -1	-0.170	-0.144
	(0.100)*	(0.098)
Exposed in his/her year 0	0.079	0.003
	(0.122)	(0.126)
Exposed in his/her year 1	-0.034	-0.183
	(0.114)	(0.109)*
Exposed in his/her year 2	0.142	-0.089
	(0.096)	(0.101)
Exposed in his/her year 3	-0.024	-0.073
	(0.091)	(0.091)
Exposed in his/her year 4	0.122	0.078
	(0.101)	(0.093)
Gender	-0.026	-0.074
	(0.041)	(0.049)
Constant	-1.571	-7.140
	(1.128)	(1.081)***
Household fixed effects	Yes	Yes
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Observations	7696	7696
R-squared	0.27	0.33

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all children between zero and five years of age interviewed at the DHS 1992.

Table 2.10: Demand Side Shocks and Human Capital: Asset Accumulation and Mother's Health

	(1) Household Asset Index	(2) Mother's Body Mass index
Events 1988	-0.225	-0.070
	(0.266)	(0.202)
Events 1989	-0.215	-0.049
	(0.211)	(0.220)
Events 1990	0.070	0.256
	(0.208)	(0.217)
Events 1991	-0.224	-0.489
	(0.239)	(0.197)**
Events 1992	0.388	-0.236
	(0.267)	(0.201)
Constant	-4.040	21.342
	(0.373)***	(0.600)***
Observations	6221	2972
R-squared	0.42	0.10

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. Source: DHS 1992. Point estimates are from OLS regressions in all cases. Regression in column (1) is at the household level. Controls include age of the household head, dummies for the maximum educational level in the household, number of members of the household, and a dummy for urban areas. In column (2), the unit of observation are mothers between 14 and 49 years of age with children between zero and five years old. Controls include dummies for the educational level, age, an indicator for whether the mother is currently pregnant, number of household members, the asset index, and a dummy for urban areas.

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Chapter 3

Transportation Choices, Fatalism and the Value of Statistical Life in Africa [1]

[1] This essay is joint work with Edward Miguel. We thank Wendy Abt for conversations in Freetown that led to this project. Tom Polley and Katie Wright provided excellent research assistance. Seminar audiences at U.C. Berkeley Energy and Resource Economics Seminar, PACDEV 2010 and WGAPE 2011 provided helpful comments. We appreciate the valuable input from Orley Ashenfelter, Fred Finan, Michael Greenstone, Kurt Lavetti and Enrico Moretti. All errors remain our own.

3.1 Introduction

This paper exploits a unique transportation setting to estimate the value of a statistical life in Africa, and to assess potential mechanisms behind the results. The revealed preference data from transport choices allow us to evaluate the trade-off individuals make between monetary wealth and fatality risk, expressed in dollars per death averted. Our estimation relies on the observed choices that every airplane traveler to or from Sierra Leone has to make to cross the body of water between the national airport and the capital city of Freetown. They can choose between four distinct transport options – namely, ferry, helicopter, speed boat, and hovercraft – which differ in their ticket cost, travel time and mortality risk. These revealed preference value of a statistical life (VSL) estimates also exploit exogenous variation in travel mortality risk generated by daily weather shocks, e.g. rainfall, that differentially affect risk across transport modes.

We find that African airport travelers have very low willingness to pay for marginal reductions in mortality risk, with an estimated average VSL very close to zero (at US$1,736 in PPP terms). Yet our elite African respondents report quite high incomes, close to average U.S. income levels, and likely have relatively long remaining life expectancy, ruling out the two most obvious explanations for the low value they place on life. A plausible, though speculative, explanation for this finding lies in socio-cultural factors, and especially the perceived role of fate in determining life outcomes in West African societies, a point we elaborate on below.

One major challenge in obtaining reliable VSL estimates is the endogeneity of risk that individuals choose to take on (Ashenfelter 2006). The underlying individual factors that affect the decision to enter a risky situation may also be correlated with income, wealth or fatality risk. To credibly estimate the VSL, we need exogenous events that affect the costs or fatality risk individuals face and which we can observe agents' response. An example is Ashenfelter and Greenstone's (2004a) use of legal changes to highway speed limits in the U.S., which leads them to estimate a VSL between US$1.0 and 1.5 million. In our setting, the fact that basically all individuals who wish to travel to or from Sierra Leone by air need to choose among the available travel options to cross from the airport to Freetown helps to partially overcome these concerns.[2] Additionally, variation in daily weather conditions, which differentially affect fatality risk across transport modes, provide exogenous variation in travel risk. We observe respondents making choices under different risk conditions (due to changing weather), which allow us to control for transport mode specific unobserved attributes, in particular, with the inclusion of alternative specific constants in a logit framework.

Our VSL estimates for Africans fall far below previous assessments from rich countries, which typically use hedonic labor market approaches and range from US$1 to 9.2 million (Viscusi and Aldy 2003; Ashenfelter and Greenstone 2004b)[3]. The only compa-

[2] Of course, Sierra Leoneans could simply choose not to leave the country, but they may need to do so for professional or personal reasons.

[3] Ashenfelter and Greenstone (2004a) argue that these estimates are subject to an upward publication

rable estimates available from developing countries, for manufacturing workers in India and Taiwan, reveal VSL's that are an order of magnitude higher than our findings for Africans, between US$0.5 and US$1 million. In the African context, Deaton et al. (2008) use a subjective life evaluation approach, and find that the monetary value attached to the death of a relative is only about 30 to 40% of the average annual income, which is less than one percent of most estimates for wealthy countries. Kremer et al. (2011) use a travel cost approach (namely, willingness to walk longer distances to cleaner drinking water sources) to estimate the willingness to pay for avoiding a child death by diarrhea in rural Kenya. Consistent with our findings, the results in that study suggest that such willingness to pay is low, at just US$769.

This seemingly low demand for health and life in less developed countries has been noted by other scholars, and remains something of a puzzle for both researchers and policymakers. The disease burden in low income countries is much higher than in rich countries, and yet a number of scholars have documented extremely low investments in preventive health and life saving technologies (see Kremer and Miguel 2007; Kremer et al. 2011; Cohen and Dupas 2010). Prevailing explanations (surveyed in Dupas 2011) range from a lack of information about new health technologies (Madajewicz et al 2007), a high income elasticity of demand for health expenditures (Hall and Jones 2007), pervasive liquidity constraints (Tarozzi et al 2011), time inconsistent preferences (DellaVigna and Malmendier 2006), agency problems within the household (Ashraf et al. 2010), and short life expectancy (Oster 2009).

A leading explanation for the low demand for health in poor countries is a high income elasticity of demand for health and life. Hall and Jones (2007) argue that the marginal utility of health expenditures is increasing in income, and consistent with the model they develop, they find that the income elasticity of health expenditures is greater than one (at 1.28)[4]. Yet at least three types of empirical evidence argue against this explanation in our data. First and most importantly, our sample is largely composed of African elites: mainly business people and government or NGO officials who have average incomes comparable to U.S. per capita income, at US$22.26 per hour (in PPP). While the Hall and Jones (2007) view might explain the low estimated demand for health among poor rural households like those studied in Kremer et al. (2011), it cannot readily do so in a population with U.S. income levels. Second, among our Africans respondents, we are unable to reject that the choices made by those with higher incomes are any more sensitive to marginal changes in mortality risk than for poorer travelers. Finally, we estimate much higher VSL's (on the order of those found in the existing literature) among the non-African respondents in our sample, who report comparable incomes to our African respondents yet are much more likely to avoid additional fatal accident risk when choosing among transportation options. Taken together, it does not appear that the low valuation of life we estimate

bias.

[4]Viscusi (2010) discusses recent research on the heterogeneity of VSL estimates. One of the most relevant dimensions of heterogeneity is income; he reviews studies that find an income elasticity of VSL above one (e.g., Evans and Smith 2010).

among African travelers is driven mainly by low income.

A second leading mechanism proposed to explain VSL patterns argues that agents facing high mortality risk in daily life are less willing to pay for additional marginal reductions in risk. This proposition can be formulated as a non-linear relationship between the VSL and mortality risk itself.[5] A related formulation is presented by Oster (2009), who argues that the lack of behavioral change for HIV prevention in Africa could be conceptualized as the rational response of forward-looking agents, given that the marginal utility of investments to avoid infection risk is lower for individuals who already face a shorter expected life span.

While this mechanism may have some relevance in our setting, it also appears unable to explain most of our findings. Life expectancy at birth is indeed relatively low among Africans. However, most of this elevated mortality occurs within the first five years of life. Conditional on survival past this age, there is considerable convergence in life expectancy between Africans and Westerners: Figure 3.1 presents an illustration for the U.S. versus Senegal (where we focus on Senegal because it has the most complete mortality statistics broken down by age in West Africa). Respondents in our sample are 40 years old on average, at which point the gap in remaining life expectancy is only a few years, and is likely to be even smaller when comparing Americans to African elites (like our sample), given they have better access to health care than the average African. Differences like those documented in Figure3.1 appear unable to account for the large gaps in observed transport choices and VSL figures that we estimate between Africans and non-Africans in our data.

Further alternatives proposed in Dupas (2011) and elsewhere – including lack of information about transport risks, liquidity constraints, and within household agency problems – also appear unlikely to account for our findings. In terms of information, transport risks to and from the airport are extremely well-publicized in Sierra Leone. Moreover, our results hold when we exclude the first trip made by each traveler (in which they were arguably less informed than in later trips). In terms of liquidity constraints, our respondents are relatively well-off and thus less likely to be highly constrained. Finally, household agency problems are irrelevant because the travelers in our sample are making decisions for themselves rather than others.

A more promising, although admittedly speculative, explanation for these results lies in differences in cultural attitudes between Africans and non-Africans, and especially the pervasive view in West Africa that fate governs major life outcomes. Accounts highlight fatalism as a widespread cultural attitude in many African societies (Iliffe 1995; Gannon and Pillai 2010), recognizing the key role played by fate or destiny in life outcomes, prominently life or death (Fortes and Horton 1983). In the extreme, fatalistic beliefs can lead to a lack of perceived individual agency and personal responsibility over many

[5] Lavetti (2011) exploits data on commercial deckhands in the Alaskan Bering Sea to estimate the marginal willingness to pay to accept fatal risk. Partially controlling for unobserved individual heterogeneity and endogenous job switching with panel data on wages and mortality rates, he estimates a concave relationship between the marginal willingness to pay for risk reduction and the level of risk.

dimensions of life.[6]

The accounts of African fatalism have gained particular relevance in recent years in explaining the rapid spread of HIV, and more generally, in accounting for the risky sexual behaviors that underlie this spread. To the extent that the timing of one's death is determined by destiny rather than individual decisions, there is little role for preventive behavioral change. Caldwell (2000) and Caldwell et al. (1992) analyze AIDS epidemic in sub-Saharan Africa, attributing a key role to fatalism beliefs in explaining the persistence of risky sexual behaviors even in the midst of the epidemic. For example, they mention that "the belief in destiny, stronger in West Africa than in the East and South,... holds that the date of death is written and changes in lifestyle will not put off the event" (p. 1179). Similarly, Meyer-Weitz and Steyn (1998) studied youth sexual behavior is South Africa and argue that "young people expressed a fatalistic attitude toward HIV prevention and were of the opinion that it was senseless to try and protect themselves from HIV/AIDS."

While there are obviously many other possible differences between Africans and non-Africans along other dimensions that could be contributing the VSL differences we estimate, it is plausible that widespread fatalistic beliefs are playing some role. Making further progress in disentangling the underlying mechanisms is an interesting route for future research.

The paper is organized as follows. In the next section we introduce the setting, section 3 discusses the model and estimation strategy, section 4 describes the data, section 5 presents the results, and the final section concludes.

3.2 Background on Sierra Leone

To reach Sierra Leone's Lungi International airport from the capital of Freetown, travelers must cross an estuary roughly twice the size of San Francisco Bay at its widest point (or about 10 miles). There is no bridge, and it is estimated that the best ground transport option around the estuary would take over six hours on unpaved and often dangerous roads, and thus we have heard of no travelers choosing this option (see map in Figure 3.2). All travelers arriving at Lungi Airport must choose between three (to four) distinct transportation alternatives – helicopter, water taxi, ferry and a hovercraft (when operational) – to cross the estuary.[7] Each of the alternatives varies in terms of

[6] For example, Bascom (1951), when describing social status and individual wealth differences among the Yoruba (in Nigeria), argues that "a person's luck and his success in economic and other affairs is also a matter of destiny (ayanmope, ayanmo) or fate (iwa)", and further "...diviners may be able to recommend sacrifices (ebo) which will influence events in the immediate future, they cannot alter the course of one's life or change his destiny" (p.492). Weprovide further discussion and quotes on this topic below.

[7] The hovercraft has operated intermittently over the past decade. Most recently, after a crash at the Lungi docks, as well as an accident that led it to catch on fire in the estuary in early 2009, the service was discontinued indefinitely. In our data, which included retrospective reports on earlier trips, some

accident risk, trip duration and monetary cost. Importantly for our estimation, fatal accidents on all modes of transport are widely reported in the local and international media and appear well-known to most travelers.[8] Passengers typically make their choice of transport mode on site, on the same day of the trip, taking into account current weather conditions. Further, in our experience there are typically few to no capacity constraints: if a given mode of transport is full at the scheduled time, there are more crafts available, or additional trips can be made by the existing fleet (i.e., the helicopter can simply make another trip between Lungi and Freetown).

Table 3.1 presents summary statistics. While the helicopter is the most expensive option (at US$70 until 2009, rising to US$80 in 2010), it is also the fastest, at only 10 minutes to cross, and has the worst accident record. The sole provider of the service uses poorly maintained Soviet-era helicopters.[9] Since 2005, there have been two helicopter crashes where all of the crew and passengers died, as shown in Table 3.2. Taking into account the frequency of the trips as well as the number of passengers per trip, the historical fatality rate over 2005-2010 for helicopter transport is roughly 22 per 100,000 passenger-trips, which is at least 30 times the fatal accident rate per 100,000 flying-hours in U.S. helicopters.[10]

The cheapest transport option is the ferry, at just US$2 per trip, though it takes approximately 70 minutes to cross the estuary. The ferry landings are also a greater distance from the airport on the Lungi side and from downtown on the Freetown side (relative to the helicopter), adding perhaps another 30 minutes per trip. The ferry has the second worst recent safety record: since 2005, there have been three major ferry accidents in Sierra Leone (including some on other routes), almost certainly due to passenger overcrowding, in which most passengers drowned. Accounting for the frequency of ferry

travelers do choose the hovercraft option and so we do include it as an option in our estimation when appropriate.

[8]There is extensive coverage on the various transport options online. The British High Commission advises (www.fco.gov.uk): "Transport infrastructure is poor. None of the options for transferring between the international airport at Lungi and Freetown are risk-free. You should study the transfer options carefully before travelling". A Sierra Leone tourism site (www.visitsierraleone.org) writes that: "Helicopters and Sierra Leone have a bit of a notorious past, with a couple of crashes widely reported"; and: "The cheapest option of all is to take the ferry to Freetown but it is certainly not the quickest option". The BBC reported the following on one of the helicopter accidents: "A helicopter ferrying passengers to Freetown airport in Sierra Leone has crashed, killing 19 people, including Togo's Sports Minister Richard Attipoe." (BBC News 2007). Similarly, Bloomberg News reported on a ferry accident: "105 people are feared to have drowned in Sierra Leone when a boat capsized." (Bloomberg News 2009). Local newspapers also regularly report on transport safety, including on a water taxi accident (in another part of Sierra Leone's coast): "A passenger speed boat, Sea Master I, plying the Kissy Ferry Terminal/Tagrin route capsized at about 10:00 p.m. on Friday 27th February 2009 after making several distress calls to the pilot office of the Sierra Leone Ports Authority" (New Citizen Press, Freetown 2009).

[9]There are anecdotal reports that quality control on the helicopter service has improved since late 2010, and one of the old helicopters has been replaced with a newer model, but this occurred after the data used in this paper was collected.

[10]U.S. helicopter accident figures come from the 2009 Annual Report on www.helicopterannual.org (accessed October 2011).

trips and the number of passengers per trip, this translates into a fatality risk of 9.5 per 100,000 passenger-trips.

The third major mode of transport is the water taxi, which is a small craft able to accommodate 12 to 18 passengers. Although there have been multiple reports of these boats sailing without proper lights or navigations systems, it appears to be the safest option, with just one recorded accident during 2005-2010 and a mortality risk of 4.5 per 100,000 passenger-trips. The water taxi crosses the estuary in approximately 45 minutes and costs US$40.

Finally, the intermittently available hovercraft had a fatality risk of 4.4 per 100,000 passengers-trips (in four separate accidents with 17 passenger deaths overall), and its cost was US$35, with an estimated travel time of 40 minutes. In the analysis below, we consider the hovercraft as a possible alternative only during periods in which we know it was operating.

3.3 The Model

In a random utility model, individual i obtains the following utility from using transport alternative j at time s to travel between Lungi Airport and Freetown:

$$U_{ijs} = v_i - (c_{js} + w_i t_j) + \varepsilon_{ijs} \tag{3.1}$$

where v_i represents the value to individual i from successfully completing the trip (alive), c_{js} is the monetary cost of the trip in transport mode j, $w_i t_j$ is the opportunity cost of time expressed in terms of the time it takes to complete the trip (t_j), and the value of the individual's time (their wage, w_i), and ε_{ijs} is an i.i.d. type I extreme value error term unobserved by the researcher.

Every transport mode has an associated fatal accident probability at time s, p_{js}. The expected utility derived from choosing transport choice j is the survival probability times the value of survival minus the total travel cost (normalizing the utility in a fatal accident to zero):

$$E(U_{ijs}) = (1 - p_{js})v_i - (c_{js} + w_i t_j) \tag{3.2}$$

An individual's choices between transport modes provide information on the implicit valuation that she assigns for completing the trip alive. In particular, if we have a choice situation where transport options have known fatality risks and we have information on individual time values, we are able to model the trade-off facing the individual to obtain an estimate of her willingness to pay to avoid additional fatality risk. Formally, the individual chooses alternative j at time s if and only if $E(U_{ijs}) \geq E(U_{iks})$, $\forall k$. A revealed preference lower bound on the value of individual i's statistical life, v_i, is:

$$v_i \geq -\frac{w_i(t_j - t_k) + (c_{js} - c_{ks})}{(p_{js} - p_{ks})} \approx -\frac{\Delta Cost}{\Delta Risk} \tag{3.3}$$

where $\Delta Cost$ denotes the difference in total travel costs (monetary and in terms of time) between the alternatives and $\Delta Risk$ is the gap in mortality risk. This constitutes a lower bound since we know individual i is willing to make this trade-off but we do not know how much greater his or her valuation might be.

Figure 3.3 illustrates the intuition with two loci that correspond to equal utility for the main transport modes.[11] The horizontal axis represents the passenger's hourly wage, and the vertical axis plots the value of a statistical life (VSL). The relative risk and cost profiles of each transportation alternative determine the intercepts and slopes. The water taxi is the least risky option but lies between the ferry and helicopter in terms of ticket price and time (Table 3.1). The fastest but riskiest option is the helicopter, which is also the most expensive. As shown graphically, individuals with high wages effectively choose between the helicopter and the water taxi (since the long travel time on the ferry generates high disutility). Those with sufficiently high value of life always choose the water taxi since it is safest, while those with low valuations choose the helicopter (if their wage is high) or ferry (if the opportunity cost of time is low). Panel B of Figure 3.3 presents the same loci using the actual data on accident risk and transport costs in our data as an illustration.

To make more progress on pinning down the actual value of v_i rather than a bound, we employ a discrete choice model that imposes some necessary distributional assumptions. The dependent variable, y_{ijs}, is the observed transport choice, which is determined by risk and cost characteristics. The probability that individual i choose transportation mode j at time s is given by the conditional logit formula (McFadden 1974):

$$P(y_{ijs}) = P(U_{ijs} \geq U_{iks},\, \forall k) = \frac{exp(\alpha_j + \beta_1(1-p_{js}) + \beta_2 c_{ijs})}{\sum_k exp(\alpha_k + \beta_1(1-p_{ks}) + \beta_2 c_{iks})}, \quad k \in \{1,2,3,4\} \tag{3.4}$$

where $1–p_{js}$ is the probability of a safe trip in alternative j at time s. β_1 represents the marginal change in the likelihood of choosing a certain transport mode due to a change in the probability of survival, and intuitively this corresponds to the utility value of completing a trip. The $Cost_{ijs}$ term captures total travel costs including the monetary cost of the ticket itself (c_{js}) and the opportunity cost of time ($w_i t_j$). β_2 thus captures how the likelihood of choosing a mode changes with cost, and corresponds to the monetary value of a unit of utility. The negative of the ratio of these coefficients captures the trade-off between exposure to fatal risk and cost, which can be interpreted as the value of statistical life. Formally:

$$-\frac{\beta_1}{\beta_2} = \frac{\partial P(y_{ijs}) / \partial(1-p_{js})}{\partial P(y_{ijs}) / \partial cost_{ijs}} = -\frac{\partial cost_{ijs}}{\partial(1-p_{js})} \approx -\frac{\Delta Cost}{\Delta Risk} \tag{3.5}$$

The alternative specific constants, α_k, that we also add to the specification capture any characteristics of transport mode k that affect its desirability other than accident risk

[11]For clarity, the loci corresponding to equal utility for the ferry and helicopter is not shown since it lies in a region where both are dominated by the water taxi. We ignore the hovercraft for simplicity since it was not an option for most of our study period.

or cost, e.g., comfort or other amenities. If these attributes are correlated with either the risk or cost of an alternative, the resulting estimates of β_1 and β_2 could be biased, as discussed further below.

The use of alternative specific constants is possible since the probability of completing a trip in each transport mode varies over time depending on daily weather conditions. For example, water-based travel is plausibly riskier in the rainy season, while helicopter accidents are more likely when there is denser cloud cover. We exploit the variation provided by daily weather conditions, obtained from the local weather station in Lungi (available at www.wunderground.com), to predict the risk of a fatal transport accident in each transport alternative. In practice, we regress the occurrence of a fatal accident in mode j on passenger-trip i in day s (F_{ijs}) on a vector of daily weather conditions (Z_s), including precipitation, cloud cover, temperature, visibility and wind speed (among others):

$$Prob(F_{ijs} = 1|Z_s) = \alpha + \beta Z_s' + \varepsilon_{ijs} \tag{3.6}$$

where ε_{ijs} is an idiosyncratic error term clustered by day. We estimate equation (6) separately for each transportation alternative using a probit model to generate daily predicted fatal risk probability for a representative passenger on day s in mode j, or $\hat{p_{js}}$.

The results are presented in Table 3.3. The average predicted risk of mortality in each mode is close to the observed mortality rate, and tests of joint significance for all the weather regressors reject that they are equal to zero for all four modes. Precipitation significantly increases mortality risk in ferry and hovercraft trips, while cloud cover affects accident risk in the ferry, helicopter and hovercraft (although with differing signs). Similarly, temperature, visibility and humidity all affect accident risk for certain transport modes. Overall, the helicopter is clearly the riskiest mode of transport, followed by the ferry and the hovercraft, with the water taxi the safest choice.

Figure 3.4 plots the monthly predicted risk of a fatality in each of the three main transportation alternatives from 2005 to 2010, years when the bulk of reported trips in our sample occur. There are clear seasonal patterns in the data. While it is generally safer to travel in the dry months of the year (December through March), the rainy season (June through September) is much riskier due to the rain, low visibility, gusty winds, etc. This variation is also somewhat heterogeneous across transport options: the helicopter is much riskier than other modes during the rainy season. The water taxi is the safest mode of transport year round.

3.4 Data

The transport choice survey data was collected in August 2009 and June 2010 at both Lungi Airport and at the Freetown among travelers arriving to or departing from Sierra Leone. We verified that all respondents had the option of the three main transportation modes on survey days. Enumerators recorded each respondent's transport choice before conducting the interview. The 2010 survey round added self-reported transport choices on earlier trips, namely on their immediately previous trip, and on their first two trips (if applicable). Thus we have data on one trip for each 2009 respondent, and up to four trips for 2010 respondents.[12]

Beyond their actual transportation choices, data was collected on respondents' demographic characteristics (including gender, age, nationality, permanent residence, and educational attainment), and current employment status and earnings.[13]

We complement the survey data with information on all transportation accidents and associated fatalities between January 2005 and July 2010. This information was collected from the U.N.'s Engineering Department in Freetown, and cross-checked with multiple local and international newspapers. The list of all accidents is presented Table 3.2.[14]

Table 3.4 presents the descriptive statistics of the analysis sample focusing on our 721 African respondents for now. Two thirds of African travelers are from Sierra Leone with the remainder mainly from Nigeria (35% of non-Sierra Leoneans), Ghana (10%), Guinea (8%) and Liberia (8%), with smaller numbers from South Africa (5%), Senegal (4%), Gambia (4%), Kenya (3%) and other countries. Overall, 81% used the ferry, 13% the water taxi, and 6% chose the helicopter, with negligible number using the hovercraft. African airport travelers in Sierra Leone are an average of 40.5 years old and 67% male. They are highly educated – 45% hold a university degree and 22% have post-graduate education – and have high incomes by local standards, with average hourly wages of US$22 (in PPP) or $45,000 per year, which is comparable to median U.S. levels. They are a mixture of local and international business people, aid workers and government officials.[15]

Respondents report making their own transportation choices based on what appear to be objective characteristics of each mode, indicating that travelers appear quite well-informed about their respective pros and cons. Most travelers who chose the helicopter mention that they chose it because it is the fastest option (82%, Table C.1). Helicopter travelers are also the most educated (49% have a post-graduate degree), and tend to be better-off (earning US$37/hour). On the other hand, those who chose the ferry claim to do so because of its lower cost (62%) and its safety (87%). Ferry passengers are poorer on average (US$23.5/hour) and less educated on average. Finally, passengers choosing the water taxi mention that their decision was based primarily on safety (53%) and speed (71%), and these passengers fall between the helicopter and ferry clientele in

[12] To provide incentives to complete the survey for passengers who were in a rush to get to the airport or home, each respondent received free cell phone air time worth about US$1 (enough for roughly 10 minutes of calls).

[13] About one third of respondents have missing values for their earnings and wages. We impute missing observations with the average wage of respondents with the same educational attainment category (namely, less than university, some/completed university, post-graduate), continent of origin (African or non-African), and employment sector (international organization/business, local organization/ business, unemployed).

[14] There were additional helicopter accidents during 2001 and 2002 during the tail end of the civil war and its immediate aftermath, but we restrict attention to the period when the war was definitively over.

[15] Table C.2 presents comparable descriptive statistics for non-Africans in the sample.

terms of education and earnings. These patterns are broadly consistent with the intuitions provided by Figure 3.3.

3.5 Main Results

3.5.1 Estimating the Value of a Statistical Life

An advantage of our dataset is that we observe transportation choices made on different dates where the risks associated with the various options differ due to weather fluctuations, as well as observing multiple choices per traveler for some respondents. This allows us to estimate the discrete choice logit model including alternative specific constants, which isolate the effect of specific unobserved attributes of each mode. For example, if travelers perceive that one of the alternatives is more "comfortable" (e.g., the hovercraft) than the others, or if there is a higher risk of being robbed while riding a crowded ferry, say, these differences will be captured by the constant terms.

Table 3.5 shows the main results of the choice model specified in equation 4 for African respondents. In all specifications, we regress the transportation choice indicator on the predicted probability of successfully completing the trip (x1000) and the total travel cost. Each observation is weighted to represent the true proportion of passengers travelling on each of the available modes of transport; that is, we weight each observation by the inverse of its sampling probability.[16] We first display results in column 1 without including alternative specific constants. The results suggest that transport modes with lower accident risk ($\hat{p_{js}}$) are less likely to be chosen by respondents, a counter-intuitive result. More expensive alternatives are less likely to be chosen, as expected. The coefficient estimates imply a negative VSL estimate of US$-48,138, where the negative sign is driven by the unexpected "preference" for riskier transport modes.

The results in column 1 are likely to be biased to the extent that there are unobserved mode specific attributes correlated with either travel safety or costs, which seems likely. For example, the ferry is generally the most crowded mode while also safer that the helicopter. Not accounting for this correlation would lead to a downward biased coefficient on the safety term. Similarly, many passengers (including the authors) dislike the loud rotor noise of the helicopter. Since the helicopter is also the most expensive option, there is an unaccounted correlation between cost and an amenity that would bias estimates on the cost term downward.

Column 2 accounts for alternative specific attributes by including indicator variables for the ferry, helicopter and water taxi (with the hovercraft serving as the excluded category), and the resulting coefficient estimates conform more closely with theory. Transportation options that have a higher likelihood of a safe trip are more likely to be chosen

[16]The sampling probabilities for each transport mode are defined as: (Overall proportion of travelers using transport mode j) / (Proportion of survey respondents using transport mode j).

by respondents (although the positive effect is not statistically significant),[17] while costlier options are less preferred. The alternative specific constants are all statistically significant determinants of respondent choices, justifying their inclusion. Following the choice model presented above, we use these coefficient estimates on the safety and cost terms to generate an estimated average VSL, which is just US$1,736, and not statistically different from zero.

3.5.2 What Explains the Low VSL for Africans?

There are three leading explanations for the low estimated value of life (and relatedly, low observed willingness to pay for health expenditures) in developing countries, and especially in Africa. Some scholars argue that expenditures in life-prolonging technologies are highly sensitive to income (Hall and Jones, 2007), and thus poorer individuals will demonstrate a far lower VSL. Second, it is argued that people with a shorter remaining life span rationally invest less in marginal reductions in mortality risk (Oster 2009). Third, in the African context it has sometimes been argued (mainly by non-economists) that a low observed willingness to pay for health services results from high levels of "acceptance" of morbidity and mortality, which itself is an expression of pervasive fatalism (Fortes and Horton 1983; Caldwell 2000). In what follows, we present evidence that casts doubt on the first two hypotheses, and provide suggestive evidence that fatalism could instead be a partial explanation for the patterns in our data.

If it is indeed the case that the demand for health is highly income elastic (with elasticity greater than one), we should observe that the choices of respondents with higher earnings are more sensitive to marginal changes in life expectancy. We test this hypothesis in column 3 of Table 3.5, where we include interaction terms between the hourly individual wage and our two main regressors (the probability of completing a trip, and total cost). The hypothesis implies a positive coefficient on the interaction between the wage and the probability of completing a trip, but we find that the point estimate is close to zero and not statistically significant. The average VSL remains positive but not statistically significant in column 3.[18] This specification also includes the age and gender-specific remaining life expectancy (for Senegal, the only West African country with reliable data as judged by the Human Mortality Database, as discussed above). If it were the case that marginal reductions in risk were more valuable for those who expected to live longer, we would observe a positive coefficient on the interaction term between remaining life expectancy and the trip safety term. However, the interaction term is statistically insignificant (and

[17] The lack of any meaningful relationship between accident risk and transport preference serves as a justification for our use of a linear indirect utility function that imposes risk neutrality.

[18] In this specification, the wages and remaining life expectancy are de-meaned, such that the main effect for the probability of survival and cost remains unchanged. Additionally, we include interactions between the alternative specific constants and both variables (not shown), which capture their average effect on the probability of choosing a given option. This applies also to the regression in column 3 of Table 3.6.

even negative). The bottom line is that variation in earnings and life expectancy does little to account for the low estimated value of life among African travelers.

In Table 3.6, we estimate the value of a statistical life among 364 non-African travelers to Sierra Leone. Non-Africans have average earnings roughly 50% higher than Africans, at US$33/hour (see Table C.2) and are somewhat more likely to have completed university, but are similar in terms of average age and gender proportions. The estimated average VSL for this group is considerably higher at US$0.53 million (column 2), which, while on the low side, is still of the same order of magnitude as many of the most credible U.S. estimates; for instance, Ashenfelter and Greenstone (2004a) present a range of estimates from US$1.0 to 1.3 million. The results indicate that the differences across Africans and non-Africans are due mainly to different preferences for accident risk. Column 3 tests for the equality of the coefficients on the probability of completing a trip and the cost between Africans and non-Africans, while simultaneously controlling for the full set of interactions with wages and remaining life expectancy, to make sure those effects are not driving any differences. Africans are not any more sensitive to differences in trip costs (even conditional on earnings). More important, African travelers appear indifferent to greater fatal accident risk, while non-Africans strongly prefer safer modes. Taken together, this leads to large differences in estimated VSL's for the two groups.

Further evidence against the income and life expectancy hypotheses is presented in Table 3.7, where we compare the VSL estimates and relevant characteristics for our respondents (African and non-African) with the 1986 U.S. population analyzed in Ashenfelter and Greenstone (2004b). African respondents in our sample have an average hourly wage of US$22.26 (in PPP 2009 dollars), while our non-African sample (of which the largest national group are from the UK at 31%, followed by US citizens at 22%) has earnings of US$33.27, and the average U.S. resident in 1986 had a wage of US$16.05.[19] Even though the African elites included in our sample have similar living standards to both samples of non-Africans, they reveal a much lower VSL at US$1,736, compared to estimates ranging from US$0.5 to 1.5 million for non-Africans.

At age 40 (the average age of our respondents), the average Senegalese can expect to live 33.5 more years, while the U.S. population in Ashenfelter and Greenstone (2004b) could expect to live an additional 38.7 years (again using information in the Human Mortality Database). These moderate observed differences in life expectancy are certainly not large enough to explain the massive gap in VSL under any reasonable level of intertemporal discounting.

An alternative hypothesis that has been proposed to explain the low demand for health in less developed countries is that a pervasive lack of information about health risks leads to too little investment in prevention (Madajewicz et al 2007). Yet this seems unlikely to be a leading explanation in our setting. First, the accidents presented in Table 3.2 were widely reported in Sierra Leonean and West African media. A more formal assessment

[19] The hourly wages are expressed in 2009 US dollars. We use the GDP deflator from the World Development indicators (World Bank, 2011) to express the hourly wage in Ashenfelter and Greenstone (2004a) in 2009 PPP dollars.

of this hypothesis would test whether the estimated VSL is higher for those travelers who are better informed about travel risks. While we cannot compute this directly, it is reasonable to assume that first-time travelers to or from Lungi airport are likely to be less knowledgeable about the relevant risks than more seasoned travelers. When we carry out the estimation excluding all reported trips by first-time Lungi travelers, we find that all of the main patterns described above remain unchanged with an estimated VSL still close to zero (results not shown), suggesting that better information alone is not sufficient to boost the valuation of life.

3.5.3 Can Fatalism Explain the Low VSL among Africans?

There is apparently a difference in Africans and non-Africans' preferences for taking on additional accident risk that cannot be explained away by the obvious candidates of differences in income, baseline mortality risk, or information. A more promising, though admittedly speculative, explanation for these results could lie in different cultural attitudes. While there may be many such differences, and we are unable to definitively pin down "the" channel, a promising candidate explanation is fatalism, the important role fate is held to have in governing major life outcomes in West Africa.

African cultural historians have long pointed to the important role of fatalism in many societies (Iliffe 1995). A belief in fate or destiny is understood as "an innate, though not necessarily impersonal, determinant of an individual's life-history" (Fortes and Horton 1983, p. 7). These beliefs cut across ethnic groups and religions (Gannon and Pillai 2010), but are more prominent in West Africa than other African regions.[20] Among the Tallensi (Northern Ghana) Fortes and Horton (1983) argue that people believe that fate is the key determinant of major life outcomes: "everything that happens has material causes and conditions, but they are effective only by grace of the mystical agencies which are the ultimate arbiters of nature and society. So they say that if a man wishes to prosper he must have skill, industry and thrift. But these are not enough; without the beneficence of Destiny they will be abortive." (p. 24).[21]

Comparable cross-country opinion data in the World Values Survey (WVS) confirms that beliefs about the role of fate differ sharply between African and U.S. populations. The WVS contains a question that captures fatalism in multiple countries in sub-Saharan Africa including Ethiopia, Ghana, Mali, Rwanda, South Africa and Zambia, although unfortunately there is no WVS data for Sierra Leone. Figure 3.5 shows that about 15%

[20]While Fortes and Horton (1983) study the Tallensi, in Northern Ghana, they are emphatic that their conclusions also hold in many other West African cultures. Bascomb (1951), in analyzing the Yoruba (in Nigeria) associates a person's luck with her destiny, arguing that men could work hard and still remain poor if fate mandates so; Bradbury (1957) reports similar findings among the Bini (in Benin), while Herskovits and Herskovits (1938), studying the Dahomeans, also in Benin, document widespread fatalistic attitudes.

[21]In the economic literature, Bernard, et al. (2011) use experimental evidence from Ethiopia to argue that fatalism is closely related to low future life aspirations, and show that individuals with more fatalistic world views show less demand for credit.

of the African respondents believe that literally "everything in life is determined by fate", versus only 2% in the U.S. population. A full 36.5% of Africans report an answer between 1 to 4 on a 10 point scale (where 1 corresponds to "everything being determined by fate" and 10 means that "people shape their fate themselves"), while the comparable proportion of Americans is just 7.4%. The distribution of attitudes among Americans is clearly much more strongly tilted towards individual agency and away from fate, with an average of 7.1, compared to 5.6 among Africans.[22]

The belief that life outcomes are predetermined in Africa has lately been held up as a leading cultural explanation for the rapid spread of the HIV/AIDS epidemic, along with the acceptance of the high mortality and morbidity (Caldwell 2000). Some have tied these beliefs to the sometimes passive resignation observed regarding HIV prevention, since "forces outside their control have more power to cause or block HIV infection than do the individuals themselves" (Hess and McKinney 2007, p. 118).[23] These fatalistic beliefs are present even at a young age, including among the South African youth analyzed by Adebowale (2001). In their sample, those who express more doubts about their ability to control whether or not they contract HIV/AIDS are also more likely to have riskier sexual encounters and have a higher number of casual sexual partners.[24]

3.6 Conclusion

This paper exploits a unique transportation setting to estimate the value of statistical life (VSL) in an Africa setting. Using revealed preferences from choices between options with different mortality risk, and monetary and time costs allows us to estimate the trade-offs individuals are willing in a discrete choice framework. We estimate that the willingness to pay for reduced mortality risk is close to zero among African respondents. The comparable VSL estimate among non-African travelers is half a million U.S. dollars, which is of the same order of magnitude as existing VSL estimates among residents of rich countries.

The finding of a low VSL in Africa is consistent with the well documented low demand for health in less developed countries, and especially in Africa. The leading proposed explanations for this are related to lack of information about new health technologies (Madajewicz et al 2007), a high income elasticity of demand for health expenditures (Hall and Jones 2007), pervasive liquidity constraints (Tarozzi et al 2011), time inconsistent preferences (DellaVigna and Malmendier 2006), agency problems within the household (Ashraf et al. 2010), or short life expectancy (Oster 2009). Our data allow us to assess

[22]Hess and McKinney (2007) use the Powe Fatalism Inventory to compare their sample of Malian married men to Midwestern U.S. men, and find that the former had three times more prevalent fatalism beliefs than the former.

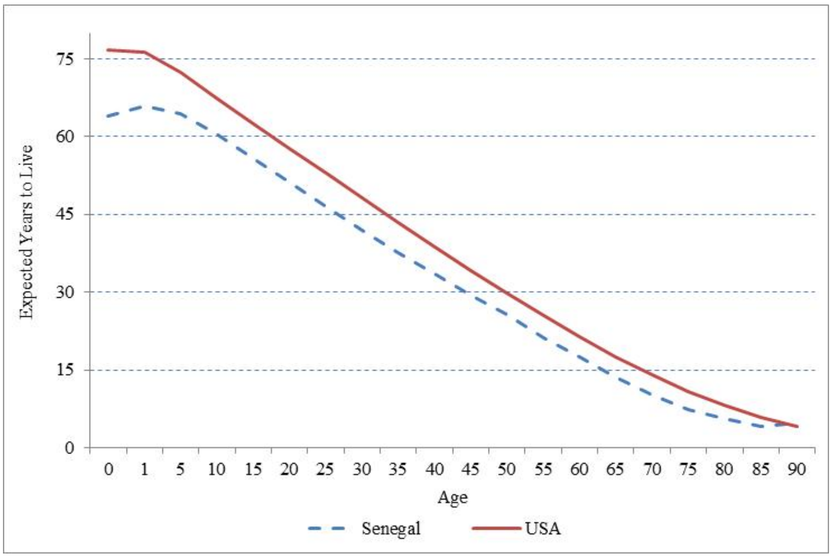
[23]A similar argument is proposed by Latham (1993) and Meyer-Weitz (2005).

[24]Bandura (1997) and Moore and Rosenthal (1991) also document similar testimony along the same lines.

many of these hypotheses, and indicate that the very low estimated VSL in our sample is unlikely to be due to a high income elasticity of health expenditures, lower remaining life expectancy, or poor information about relevant risks.

A more promising explanation for these results lies in differences in socio-cultural attitudes between Africans and non-Africans, and especially the perceived role of fate in governing major life outcomes. Overall, our results highlight the potentially important role of particular cultural perspectives on economic (and other) behaviors in low income countries. Exploring precisely which cultural attitudes are most influential, and why, is an interesting route for future research in the field.

Figure 3.1: Remaining Life Expectancy, By Age



Source: Life Tables Dataset (http://www.lifetable.de) contained in the Human Mortality Dataset (http://www.mortality.org/). For the US, the data corresponds to the year 1999. The only point available in Africa is Senegal, and we use data from 1995-1999.

Figure 3.2: Map of the Study Setting, Lungi Airport and Freetown, Sierra Leone

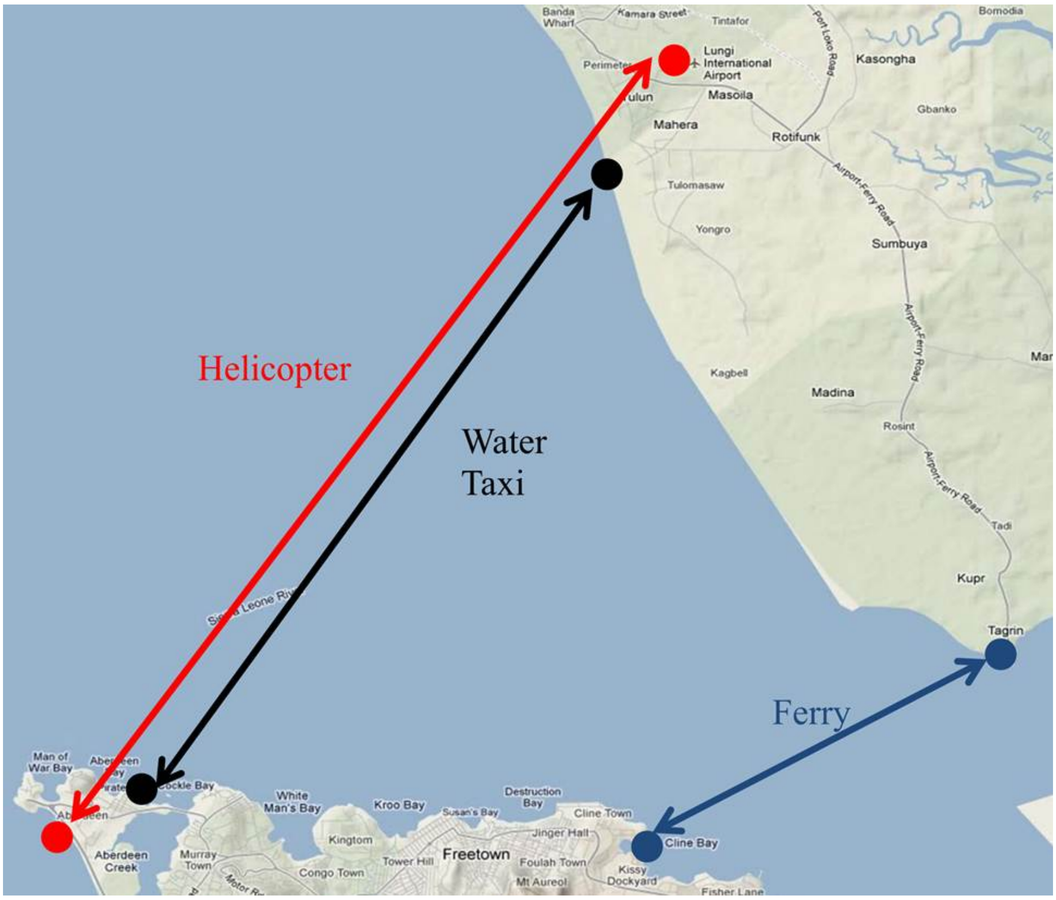
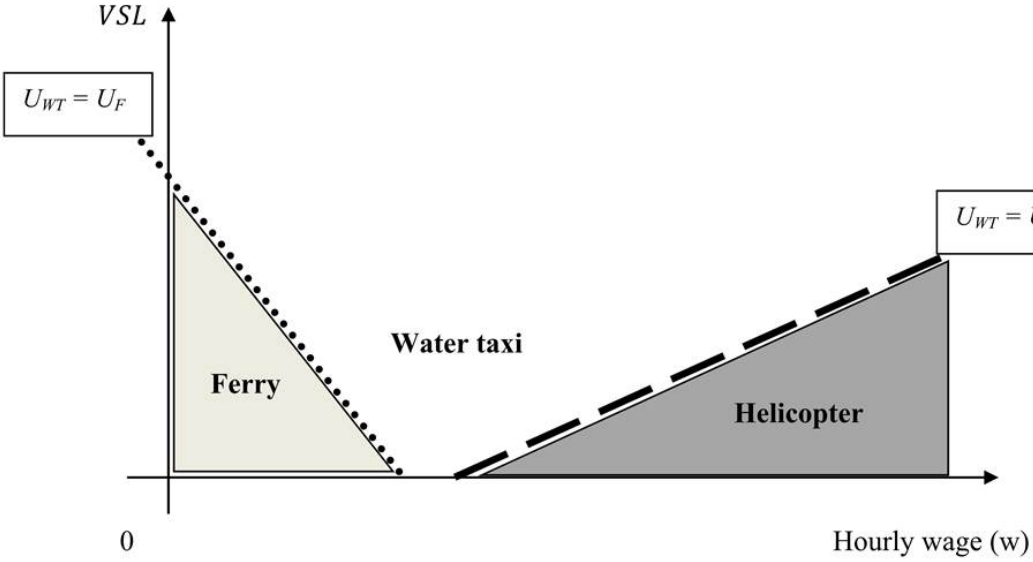
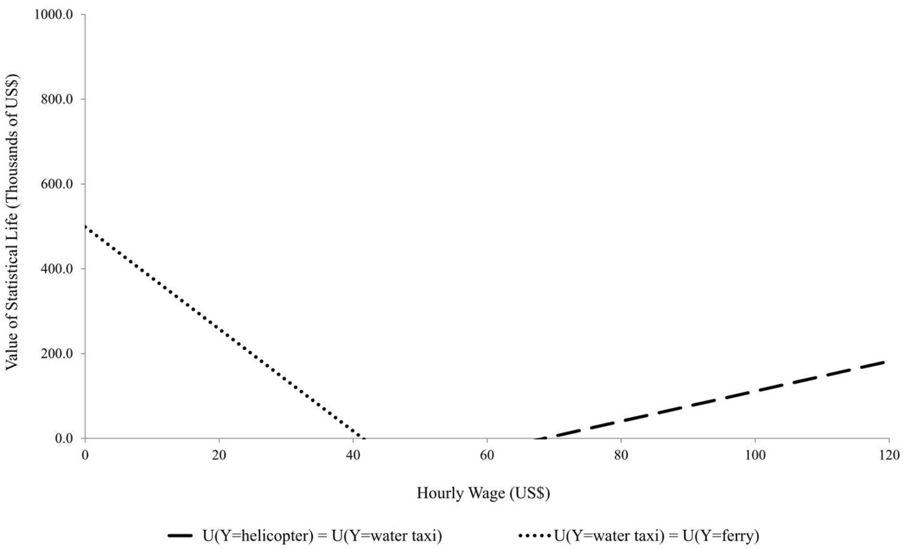


Figure 3.3: Transportation Choices and the Value of a Statistical Life in Sierra Leone
Panel A: Optimal transport choice as a function of wages and value of life

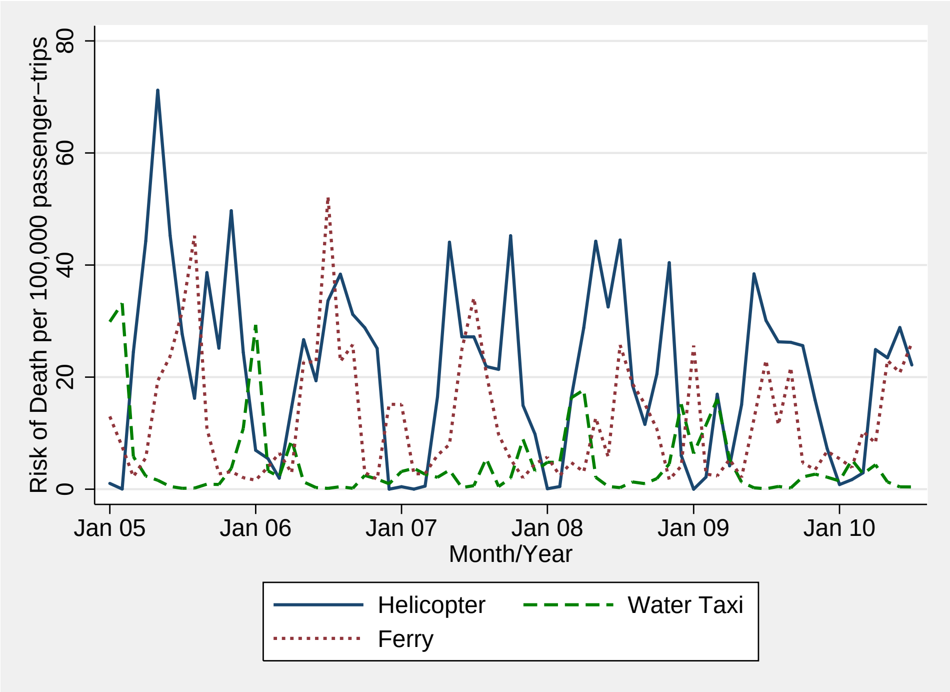


Panel B: Empirical Indifference Curves



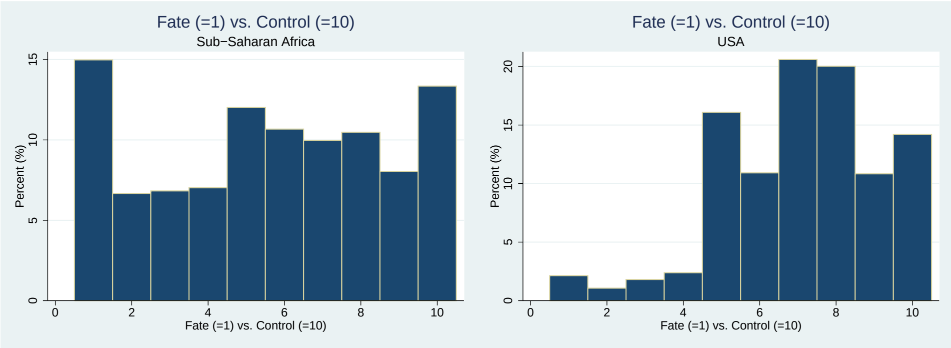
Notes: Each line represents the locus of VSL–Wage for which an individual is indifferent between two transportation options. The figure is computed using the observed mortality risk, transportation cost, and trip duration for each of the modes of transport.

Figure 3.4: Predicted Risk of a Fatality, by Month and Mode of Transport



Notes: The figure shows the monthly predicted risk of dying by transport mode based on the results in Table 3.3.

Figure 3.5: Self-Expressed Fatalism



Source: World Values Survey v.20090901, 2009. World Values Survey Association (www.worldvaluessurvey.org). Aggregate File Producer: ASEP/JDS, Madrid. Notes: Fate vs. Control comes from the latest round of the World Values Survey. The question is worded as follows: "Some people believe that individuals can decide their own destiny, while others think that it is impossible to escape a predetermined fate. Please indicate which comes closest to your view on this scale on which 1 means “everything in life is determined by fate,” and 10 means that “people shape their fate themselves.” The reported average for Africa comes from surveys in Ethiopia, Ghana, Mali, Rwanda, South Africa, and Zambia. The U.S. mean value is 7.1, and the Sub-Saharan Africa sample average is 5.6.

Table 3.1: Transportation Options, Descriptive Statistics and Accidents

	Mode of Transportation				
	Helicopter	Water taxi	Ferry	Hovercraft	Road
Average passenger traffic					
Num. of trips per week	32	42	74	54	
Num. of passengers per week	640	924	3700	1350	
% of travelers choosing this mode	12.20%	17.60%	70.30%	-	
% of sample respondents choosing this mode	18.90%	18.80%	61.40%	0.90%	-
Costs					
Ticket cost in US$ (cjs)	70-80	40	2	35-50	N/A
Transit time in minutes (to/from Freetown dock/helipad)	10	45	70	40	240 +
Waiting time in minutes	0	0	30	0	
Total travel time in minutes (tj)	10	45	100	40	
Accident risk					
Probability of fatal accident per 100,000 passenger-trips (pjs)	22.57	4.58	9.48	4.44	N/A
Probability of accident per 100,000 passenger-trips	22.02	8.39	14.28	26.1	N/A
Predicted probability of accident per 100,000 passenger-trips	21.55	8.32	18.47	55.7	N/A

Notes: Information on fatal accidents was obtained by a comprehensive search of Sierra Leone and international newspapers during the period January 2005 through June 2010, the UN engineering department in Freetown, as well as several news sources. Information on the monetary cost and travel time were obtained during field work in June 2010. Travel time and cost between the dock/helipad and the destination/origin in Sierra Leone is the average among travelers who used each mode of transport. The probability of an accident is computed as the ratio of the total number of accidents observed during the reference period, divided by the number of trips made by transport. Similarly, the probability of a fatal accident is computed as the ratio of the number of fatalities observed during the reference period, divided by the estimated number of passengers that made a trip during the same period. Information on choices was collected in the 2010 Sierra Leone Survey on Transportation Choices. To get information about the time of the trip, the authors we did each trip several times.

Table 3.2: Transportation Accidents (January 2005 – June 2010)

Transportation Mode	Type of accident	Location	Date	Weather	Deaths	Source
Helicopter	Crash	Freetown to Lungi	June 3rd, 2007	Rain	19	wikinews.org
	Crash	Lungi Airport	Oct. 18th, 2007	No Rain	22	UN
Ferry	Wreck	Ocean	Mar. 12th, 2006	No rain	120	UN
	Accident	Bailor	Aug. 2nd, 2007	Rain/Thunder	158	Yahoo news
	Wreck	Ocean	Sept. 9th, 2009	Rain	120	Bloomberg
Water Taxi / Speed boat	Accident	Freetown to Lungi	Feb. 27th, 2009	No rain	12	New citizen press
Hovercraft	Accident	Lungi	May 5th, 2006	No rain	6	UN
	Accident	Lungi	Aug., 18th, 2006	Rain	11	UN
	Fire	Lungi to Aberdeen	Nov. 13, 2007	No Rain	0	Awareness times
	Crash	Lungi	May 23rd, 2008	Thunder	0	VSL

Table 3.3: Predicted Risk of a Fatality, probit estimates

	Dep. Var: Fatality=1			
	Ferry	Helicopter	Hovercraft	Water Taxi
Rain or Drizzle	0.839	0.508	0.761	
	(0.343)**	(0.483)	(0.256)***	
Cloud Cover (%)	0.180	-1.305	-3.024	-0.016
	(0.052)***	(0.599)**	(0.588)***	(0.022)
Mean Temp. (F)	0.069	-0.094	0.223	-0.092
	(0.017)***	(0.067)	(0.086)***	(0.022)***
Mean Dew Point (F)	0.009	0.165	0.115	0.037
	(0.052)	(0.170)	(0.118)	(0.020)*
Mean Humidity (%)	-0.024	-0.053	0.121	-0.004
	(0.032)	(0.020)***	(0.060)**	(0.009)
Mean Sea Level Pressure (In.)	5.089	-0.914	-9.709	-10.136
	(2.024)**	(3.596)	(1.931)***	(1.097)***
Max Visibility (Miles)	-0.020	1.582	3.520	-0.109
	(0.060)	(0.408)***	(0.695)***	(0.028)***
Mean Wind Speed (MPH)	-0.025	-0.009	-0.013	-0.006
	(0.040)	(0.070)	(0.031)	(0.009)
Chi-sq joint signif.	162.58	135.45	52.49	94.11
(p-value)	<0.01	<0.01	< 0.01	<0.01
Observations	3,260,800	186,240	179,900	268,884
Log Likelihood	-3490.59	-329.16	-129.08	-111.73
Pred. Risk (mean)	12.25	21.11	11.21	4.63
Pred. Risk (s.d.)	31.90	38.73	76.00	21.94

Notes: Each observtion in the regression represents a passenger in each mode of transport. he period considered starts with the availability of weather data (Jan-2005), and only considers those dates in which each of the transportation modes was available. The weather data corresponds to the Lungi weather station, as recorded on http://www.wunderground.com. Missing values were imputed using the average from the same day of the year on those years with recorded data. Standard errors clustered at the day level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%

Table 3.4: Descriptive statistics, African respondents

Variable	Obs.	Mean	Std. Dev.
Transportation Choices			
Transport taken: Ferry	721	0.81	0.39
Transport taken: Helicopter	721	0.06	0.24
Transport taken: Water Taxi	721	0.13	0.34
Demographic Characteristics			
Gender (male=1)	720	0.67	0.47
Age	713	40.45	13.38
Educational level: less than completed university	714	0.34	0.47
Educational level: complete university	714	0.45	0.5
Educational level: post-graduate	714	0.22	0.41
Nationality: Sierra Leonean	721	0.73	0.44
Nationality: Other African	721	0.27	0.44
Additional information			
Hourly wage (US$ PPP) - measured	522	17.69	46.81
Hourly wage (US$ PPP) - imputed	721	22.26	44.19

Sources: Information on choices was collected in the 2009 and 2010 Sierra Leone Survey on Transportation Choices.

Notes: Descriptive statistics use the data from the observed trips only (one unique individual per observation), and are weighted to represent the actual proportion of travelers. The exchange rate, and the rate of conversion to PPP comes from the World Bank's World Development Indicators, and we assign these conversion rates by the country of permanent residence. We input the missing observations for wages with the average hourly wage of people in the same education category (Less than some university, Some/Completed University, Post-graduate), region of residence (African/Non-African), and job type (International organization or international private business/Local NGO or local business/Unemployed).

Table 3.5: Transportation Choices and the Value of a Statistical Life in Africa

	(1)	(2)	(3)
Prob of complete trip (1-p_{js})	-0.870	0.010	0.056
	(0.255)***	(0.337)	(0.344)
Total transp cost ($Cost_{ijs}$)	-0.018	-0.006	-0.008
	(0.001)***	(0.002)***	(0.002)***
(1-p_{js}) * Wage (/100)			0.874
			(1.143)
$Cost_{ijs}$ * Wage (/100)			0.006
			(0.002)***
(1-p_{js}) * life exp (/10)			-0.3082
			(0.2881)
$Cost_{ijs}$ * life exp (/10)			-0.002933
			(0.00168)*
ASC (Ferry)		3.358	5.586
		(0.916)***	(1.078)***
ASC (Helicopter)		2.053	3.766
		(0.916)**	(0.981)***
ASC (Water taxi)		1.937	3.757
		(0.905)**	(0.906)***
Num. of obs. (respondent-alt. options)	3889	3889	3889
Num. of trips	1237	1237	1237
Num. of respondents	717	717	717
Log-Likelihood	-1081.53	-982.39	-970.25
VSL (in 000 US$ PPP)	-48.132	1.736	7.420
95% CI	[-73.072,-23.191]	[-113.939,117.412]	[-81.961,96.801]

Notes: The probability of a complete trip is defined as the one minus the predicted probability of being in an accident (x1000). Each observation in is a unique traveler-transportation mode pair in the current choice. The dependent variable is an indicator equaling 1 if the traveler chose the transportation mode represented in the traveler-transportation mode pair. In every choice situation, we consider only the transportation modes available (i.e., the hovercraft is often unavailable). All regressions are weighed to represent the actual share of travelers taking each mode of transport. Column (3) includes interactions between the ASC's and the relevant variables (wage and remaining life expectancy). The interactions in Column (3) use deviations from the mean. Standard errors are clustered at the level of the individual decision-maker, significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. The standard errors of the VSL estimates are estimated using the delta method.

Table 3.6: Transportation Choices and the Value of a Statistical Life in Africa and the rest of the World

	(1) Africans	(2) Non-Africans	(3) All
Prob of complete trip (1-p_{js})	0.010	1.598	0.604
	(0.337)	(0.366)***	(0.259)**
Total transp cost ($Cost_{ijs}$)	-0.006	-0.003	-0.005
	(0.002)***	(0.002)	(0.002)***
(1-p_{js}) * African			-1.709
			(0.547)***
$Cost_{ijs}$ * African			-0.003
			(0.003)
(1-p_{js}) * Wage (/100)			-0.342
			(0.769)
$Cost_{ijs}$ * Wage (/100)			0.004
			(0.001)***
(1-p_{js}) * life exp (/10)			-0.288
			(0.210)
Costijs * life exp (/10)			-0.001
			(0.001)
ASC's	Yes	Yes	Yes
Num. of obs (respondent-alt. options)	3889	2051	5808
Num. of trips	1237	657	1852
Num. of respondents	717	364	1056
Log-Likelihood	-982.39	-544.79	-1474.78
VSL (in 000 US$ PPP)	1.736	532.424	129.433
95% CI	[-113.939,117.412]	[-143.137,1207.986]	[-12.227,271.092]

Notes: The probability of completing the trip is defined as the one minus the predicted probability of being in an accident (x1000). Each observation in is a unique traveler-transportation mode pair in the current choice. The dependent variable is an indicator equaling 1 if the traveler chose the transportation mode represented in the traveler-transportation mode pair. In every choice situation, we consider only the transportation modes available (i.e., the hovercraft is often unavailable). All regressions are weighed to represent the actual share of travelers taking each mode of transport. Columns (3) includes interactions between the ASC's and the relevant variables (African, wage and remaining life expectancy). The interactions shown in Column (3) use deviations from the mean. Standard errors are clustered at the level of the individual decision-maker, significantly different than zero at 90% (*), 95% (**), 99% (***) confidence. The standard errors for the VSL estimates are estimated using the delta method.

Table 3.7: Comparing African and non-African populations

	African respondents	Non-African respondents	U.S. (Ashenfelter and Greenstone 2004)
VSL (US$ PPP)[a]	$1,736	$532,424	$1,030,000 to $1,540,000
Wage/hour (2009 US$ PPP)[b]	$22.26	$33.27	$16.05
Remaining life exp. at age 40[c]	33.55		38.74
	Africans		U.S.
Fate vs. Control (1 to 10)[d]	5.6		7.09

Notes:

[a] The reported VSL for the US is the one estimated by Ashenfelter and Greenstone (2004a). Even though they do not provide the confidence interval, the numbers reported correspond to the results from two different estimations. The VSL from Africans and Non-Africans comes from our own estimates reported in Table 3.6.

[b] Wage per hour reported in Ashenfelter and Greenstone (2004a). The mean hourly wage in 1986 is calculated from the 1986 Current Population Survey Outgoing Rotation Group, and it is expressed in 1997 US$ in the original paper ($12.33), to make it comparable, we express it in 2009 US$ using the GDP deflator. For Africa, we use the reported wages in our sample (expressed in US$ PPP).

[c] Life expectancy at age 40 comes from the Life Tables Dataset (http://www.lifetable.de) contained in the Human Mortality Dataset (http://www.mortality.org/). For the US, the data corresponds to the year 1999. The only point available in Africa is Senegal, and we use data from 1995-1999.

[d] Fate vs. Control comes from the most recent round of the World Values Survey (2009). The question is worded as follows: "Some people believe that individuals can decide their own destiny, while others think that it is impossible to escape a predetermined fate. Please indicate which comes closest to your view on this scale on which 1 means “everything in life is determined by fate,” and 10 means that “people shape their fate themselves.” The reported average for Africa comes from all available surveys, including Ethiopia, Ghana, Mali, Rwanda, South Africa, and Zambia.

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Appendix A

Additional Tables and Figures for Chapter 1

Figure A.1: Fliers for the Treatment and Control Groups

Flier for the Treatment group:

Estimado Sr(a):

En Agosto del 2006, el Congreso de la República aprobó una reducción de las multas para los omisos al voto (Ley No. 28859). Según esta ley, aquellos que no voten ya no estarán sujetos a una multa de S/. 132, sino que la multa es menor para todos, y escalonada de acuerdo al nivel de pobreza del distrito.

De acuerdo a la información que Ud. me ha dado, si es que Ud. no vota en las elecciones de octubre de este año, tendrá que pagar una multa de S/.________.

Flier for the Control group:

Estimado Sr(a).:

Recuerde que en el Perú, estar omiso al voto, es decir, no asistir a cualquier elección, está sujeto a una sanción que implica el pago de una multa.

Table A.1: Coefficients for Policy Preference First Principal Component

Policy issues	Coefficients
Health: infrastructure	-0.116
Health: personnel and services	-0.145
Education: infrastructure	-0.114
Education: teachers and services	-0.085
Transport: Ordering transit	0.024
Transport: Infrastructure (roads, access, etc.)	-0.362
Basic services: Water, electricity, sewage, communications	-0.478
Promote tourism	-0.062
Economics: Support micro and small enterprises	-0.027
Economics: Training to local enetrepeneurs	-0.025
Economics: Ag. - tech assistance + training	-0.271
Economics: Ag. - infrastructure projects	-0.113
Economics: promote private investment	-0.020
Youth: Sport activities and infrastructure	-0.026
Youth: Labor training programs	0.024
Women: empowerment and programs	-0.003
Social: More participation/particip. budgets	-0.013
Security: More policemen	0.153
Security: Fight gangs and drugs	0.212
Environment: Cleaning the district/Garbage trucks	0.027
Environment: More green areas	-0.073
Environment: Recycling of solid residues	-0.010
Institutional: Transparency in procedures	-0.020
Institutional: Modernize procedures	-0.029
Infrastructure: Markets, public buildings	-0.052
Social: Children and elderly programs, school lunches, etc.	-0.027
Social: work for the poor	-0.022
Housing: titling,	-0.036

Table A.2: Balance Between Attrited and non-Attrited

Variable	**Obs.**	**Non-Attriters**	**Attriters**	**NA - A**	**P-value**
	2838	125.764	122.292	-3.472	(0.199)
Gender	2838	0.482	0.424	-0.059	(0.012)
Age	2838	39.180	39.885	0.706	(0.265)
Yrs. of education	2838	9.619	9.586	-0.034	(0.860)
Log(PC Expenditures)	2838	5.225	5.190	-0.035	(0.409)
Center	2754	0.670	0.667	-0.004	(0.872)
Left	2754	0.071	0.083	0.012	(0.354)
Right	2754	0.259	0.251	-0.008	(0.685)
Policy Extreme 1	2838	0.171	0.207	0.037	(0.052)
Policy Center	2838	0.609	0.598	-0.011	(0.634)
Policy Extreme 2	2838	0.221	0.195	-0.026	(0.168)
Very Interested in politics	2795	0.065	0.082	0.016	(0.205)
Interested in politics	2795	0.443	0.468	0.025	(0.290)
Not Interested in politics	2795	0.492	0.451	-0.041	(0.081)
Very Interested in the results of this election	2838	0.375	0.399	0.024	(0.307)
Interested in the results of this election	2814	0.455	0.443	-0.012	(0.618)
Not Interested in the results of this election	2838	0.164	0.153	-0.010	(0.544)
Very Interested in the campaign of this election	2809	0.112	0.105	-0.007	(0.653)
Interested in the campaign of this election	2809	0.512	0.556	0.045	(0.058)
Not Interested in the campaign of this election	2809	0.377	0.339	-0.038	(0.091)
Name recall- Candidates running	2837	0.401	0.388	-0.013	(0.436)
Name recall- Parties running	2837	0.308	0.290	-0.019	(0.212)
Name recall- Candidates+Parties running	2837	0.355	0.339	-0.016	(0.289)
Political information score (baseline)	2838	0.561	0.547	-0.014	(0.096)

Table A.3: Robustness: Main Regressions, Including Voters from Extreme Poor Districts

	Reduced Form	First Stage	IV
		Dependent Variable:	
	Voted in 2010	△ in Perceived Fine	Voted in 2010
△ Perceived Fine			0.0015
			(0.0005)***
Treatment: Fine S/.72	-.0208	-19.3585	
	(0.0157)	(4.8621)***	
Treatment: Fine S/.36	-.0508	-30.1273	
	(0.0161)***	(4.6858)***	
Treatment: Fine S/.18	-.0091	-8.5888	
	(0.0201)	(5.9851)	
Gender	0.01	-4.8665	0.0174
	(0.0099)	(2.9300)*	(0.0111)
Age	0.0015	0.3473	0.0009
	(0.0004)***	(0.1188)***	(0.0005)**
Yrs. of education	0.004	-.2727	0.0043
	(0.0015)***	(0.4196)	(0.0016)***
Log(PC Expenditures)	0.0014	-.9997	0.0029
	(0.006)	(2.1271)	(0.0068)
Votes in Non-Poor district	0.8345	-47.9233	0.9112
	(0.0426)***	(13.8454)***	(0.0508)***
Votes in Poor district	0.8686	-50.0673	0.9416
	(0.0478)***	(14.7154)***	(0.0556)***
Votes in Extreme Poor district	0.7051	-66.6642	0.8075
	(0.0668)***	(14.6329)***	(0.0747)***
Mean dep. var.	0.9424	-64.115	0.9424
Obs.	2273	2273	2273
F-statistic			19.57
R^2	0.9455	0.5232	0.5854

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation for these regressions follow the structure detailed in the main text in equations (7),(9), and (10), but including an indicator for voting in an extremely poor district, and the corresponding interactions.

Table A.4: Robustness: Main Regressions, Without Controls

	(1)	(2)	(3)
	Panel A: Reduced Form Dep. Var: Voted in the 2010 Election		
Treatment: Fine S/.72	-.0250 (0.0149)*	-.0217 (0.0152)	-.0258 (0.015)*
Treatment: Fine S/.36	-.0532 (0.0162)***	-.0533 (0.016)***	-.0527 (0.0161)***
R^2	0.0391	0.0181	0.0487
	Panel B: First Stage Dep. Var: △ Perceived Fine		
Treatment: Fine S/.72	-19.5131 (4.8591)***	-18.5018 (5.1395)***	-19.3167 (4.8544)***
Treatment: Fine S/.36	-30.5384 (4.7246)***	-29.1100 (4.7584)***	-30.3400 (4.6921)***
R^2	0.104	0.0506	0.1098
	Panel C: IV Dep. Var: Voted in the 2010 Election		
△ Perceived Fine	0.0016 (0.0005)***	0.0016 (0.0005)***	0.0016 (0.0005)***
Controls	N	Y	Y
Village FE	N	N	Y
Mean Vote 2010	0.9445	0.9445	0.9445
Mean △ Perceived Fine	-56.65	-56.65	-56.65
F-statistic	28.7586	25.2301	28.6595
Obs.	1732	1732	1732

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses.

Table A.5: Effect of Changes in Perceived Fine on Turnout, by Demographic Characteristics

	Dep. Var: Voted in the 2010 Election			
	(1)	(2)	(3)	(4)
△ Perceived Fine	0.0008	0.0025	0.0051	0.004
	(0.0013)	(0.0008)***	(0.0025)**	(0.0023)*
△ Fine*Age	0.00002			
	(0.00004)			
△ Fine*Male		-.0021		
		(0.001)**		
△ Fine*Yrs. Educ.			-.0003	
			(0.0002)	
△ Fine*Log(PC Expenditures)				-.0005
				(0.0004)
Controls	Y	Y	Y	Y
Village FE	Y	Y	Y	Y
Obs.	1732	1732	1732	1732

* significant at 10%; ** significant at 5%; *** significant at 1%. Robust standard errors clustered at the household level in parentheses. Regression equation:
$Vote_{ij} = \beta_1 \triangle Fine_{ij} + \beta_2 \triangle Fine_{ij} \cdot X_{ij} + \beta_3 X_{ij} \cdot P_{ij} + \beta_4 X_{ij} \cdot NP_{ij} + \beta_5 P_{ij} + \beta_6 NP_{ij} + \gamma X_{ij} + \delta_k + \epsilon_{ij}$

Appendix B

Additional Tables for Chapter 2

Table B.1: Violence and Human Capital Accumulation: Long Term Effects

	(1)	(2)	(3)
		Years of education	
Log(No violent event/pop in dist/yr(t-6))		-0.00270	-0.00403
		(0.00219)	(0.00227)
Log(No violent event/pop in dist/yr(t-5))		-0.00073	-0.00132
		(0.00166)	(0.00173)
Log(No violent event/pop in dist/yr(t-4))		-0.00074	-0.00166
		(0.00192)	(0.00194)
Log(No violent event/pop in dist/yr(t-3))		-0.00269	-0.00353
		(0.00150)	(0.00151)*
Log (No violent events) (in utero)	-0.00548		-0.00574
	(0.00134)***		(0.00134)***
Log (No violent events) (early ch.)	-0.00431		-0.00492
	(0.00133)***		(0.00135)***
Log (No violent events) (pre-school)	-0.00370		-0.00427
	(0.00121)***		(0.00122)***
Log (No violent events) (primary school)	-0.00262		-0.00294
	(0.00155)		(0.00155)*
Log (No violent events) (high school)	-0.00193		-0.00246
	(0.00143)		(0.00143)
Gender (male=1)	0.43729	0.43742	0.43721
	(0.03036)***	(0.03037)***	(0.03038)***
Mother's language (native=1)	-1.74779	-1.74734	-1.74706
	(0.06387)***	(0.06385)***	(0.06384)***
Constant	8.22879	8.43130	7.94169
	(0.09499)***	(0.08897)***	(0.13175)***
District of birth fixed effects	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes
Mean dep. var.		9.40	
Observations	139446	139446	139446
R-squared	0.06	0.06	0.06

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table B.2: Violence and Human Capital: long-term Effects on the Labor Market

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Yrs Of educ	Informal sector		Currently working		Log(Monthly wage)	
No yrs exposed	-0.018	0.006	0.006	0.004	0.004	-0.034	-0.033
(in utero)	(0.046)	(0.011)	(0.011)	(0.011)	(0.012)	(0.031)	(0.030)
No yrs exposed	-0.058	-0.008	-0.010	-0.019	-0.019	-0.026	-0.018
(early childhood)	(0.032)*	(0.006)	(0.006)	(0.006)***	(0.006)***	(0.021)	(0.020)
No yrs exposed	-0.005	-0.010	-0.010	-0.008	-0.008	-0.007	-0.010
(pre-school age)	(0.038)	(0.008)	(0.008)	(0.007)	(0.007)	(0.024)	(0.023)
No yrs exposed	-0.007	-0.002	-0.002	-0.007	-0.007	-0.021	-0.022
(primary school age)	(0.036)	(0.007)	(0.007)	(0.006)	(0.006)	(0.021)	(0.021)
No yrs exposed	0.016	-0.008	-0.006	-0.005	-0.005	0.032	0.032
(high school age)	(0.032)	(0.007)	(0.006)	(0.007)	(0.007)	(0.019)*	(0.018)*
Gender (male=1)	0.505	0.112	0.125	0.187	0.188	0.486	0.505
	(0.052)***	(0.008)***	(0.009)***	(0.007)***	(0.007)***	(0.025)***	(0.023)***
Mother's lang (nat=1)	-1.680	0.158	0.088	0.091	0.085	-0.362	-0.183
	(0.110)***	(0.016)***	(0.015)***	(0.015)***	(0.016)***	(0.052)***	(0.049)***
Yrs of educ			-0.029		-0.003		0.079
			(0.001)***		(0.001)**		(0.004)***
Constant	10.612	0.179	0.550	0.644	0.678	7.074	6.036
	(1.937)***	(0.326)	(0.299)*	(0.223)***	(0.223)***	(1.035)***	(0.977)***
District of birth FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth FEs	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Province cubic trend	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean dep Var	9.28	0.55		0.71		6.38	
Obs	22083	22083	22083	22083	22083	12232	12232
R-squared	0.11	0.06	0.09	0.12	0.12	0.15	0.19

Source: Encuesta Nacional de Hogares 2007. * significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes people between 18 and 32 years old interviewed in 2007. The dependent variable in columns (2) and (3) is defined as those workers who are not openly unemployed (work less than 35 hrs. per week), or work in a firm that is not registered, or does not keep accounting books (self reported); in columns (4) and (5), I consider occupied those workers who declare being employed, and working more than 35 hrs. per week; the log of the monthly income is taken over the labor income of those who declare being employed.

Table B.3: Violence and Human Capital, by Gender and Ethnicity

	(1)	(2)	(3)	(4)	(5)
	Years of education				
	Full sample	Women	Men	Native speakers	Spanish speakers
No. of yrs exposed	-0.071	-0.087	-0.063	-0.177	-0.051
(in utero)	(0.021)***	(0.029)***	(0.025)**	(0.083)**	(0.020)***
No. of yrs exposed	-0.051	-0.059	-0.049	-0.034	-0.043
(early childhood)	(0.014)***	(0.020)***	(0.016)***	(0.054)	(0.013)***
No. of yrs exposed	-0.051	-0.079	-0.020	-0.063	-0.044
(pre-school age)	(0.015)***	(0.023)***	(0.017)	(0.058)	(0.015)***
No. of yrs exposed	-0.019	-0.007	-0.029	-0.044	-0.009
(primary school age)	(0.014)	(0.018)	(0.018)*	(0.051)	(0.014)
No. of yrs exposed	0.000	-0.001	0.004	-0.038	0.004
(high school age)	(0.013)	(0.019)	(0.017)	(0.057)	(0.013)
Gender (male=1)	0.438			1.656	0.256
	(0.030)***			(0.054)***	(0.025)***
Mother's language (native=1)	-1.747	-2.238	-1.213		
	(0.064)***	(0.081)***	(0.065)***		
Constant	8.613	7.862	6.939	-1.759	8.190
	(0.066)***	(0.086)***	(0.078)***	(0.280)***	(0.059)***
District of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes
Mean dep. var.	9.40	9.19	9.63	7.74	9.65
Observations	139446	71412	68034	18287	121159
R-squared	0.06	0.07	0.04	0.13	0.02

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table B.4: Migration and Exposure to Violence

	(1)	(2)
	Migration status (=1 migrant)	
Exposed to violent events in his/her yr -6	-0.000	-0.030
	(0.020)	(0.011)***
Exposed to violent events in his/her yr -5	0.008	-0.009
	(0.014)	(0.009)
Exposed to violent events in his/her yr -4	0.021	-0.000
	(0.016)	(0.008)
Exposed to violent events in his/her yr -3	0.007	-0.001
	(0.014)	(0.007)
No. of yrs exposed (in utero)	0.007	-0.011
	(0.008)	(0.004)**
No. of yrs exposed (early childhood)	0.020	0.001
	(0.006)***	(0.003)
No. of yrs exposed (pre-school age)	0.016	0.002
	(0.005)***	(0.003)
No. of yrs exposed (primary school age)	0.005	-0.001
	(0.005)	(0.003)
No. of yrs exposed (high school age)	0.007	-0.003
	(0.006)	(0.003)
Gender (male=1)	-0.027	-0.026
	(0.003)***	(0.003)***
Mother's language (native=1)	-0.183	-0.177
	(0.017)***	(0.017)***
Constant	0.336	-0.407
	(0.010)***	(0.013)***
District of birth fixed effects	No	Yes
Year of birth fixed effects	Yes	Yes
Province specific cubic trend	Yes	Yes
Mean dep. var.	0.39	
Observations	139446	139446
R-squared	0.07	0.02

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census.

Table B.5: Robustness - long-term Effects Excluding Different Regions

	(1) Excluding Lima	(2) Excluding Ayacucho	(3) Excluding Huancavelica	(4) Excluding Huanuco	(5) Excluding San Martin	(6) Excluding Ayacucho and Huancavelics
	Years of education					
No. of yrs exposed	-0.069	-0.069	-0.068	-0.069	-0.127	-0.067
(in utero)	(0.022)***	(0.022)***	(0.021)***	(0.022)***	(0.028)***	(0.022)***
No. of yrs exposed	-0.053	-0.053	-0.048	-0.051	-0.099	-0.055
(early childhood)	(0.014)***	(0.014)***	(0.014)***	(0.014)***	(0.020)***	(0.015)***
No. of yrs exposed	-0.050	-0.054	-0.043	-0.050	-0.084	-0.053
(pre-school age)	(0.015)***	(0.015)***	(0.015)***	(0.015)***	(0.021)***	(0.015)***
No. of yrs exposed	-0.020	-0.019	-0.013	-0.017	-0.044	-0.020
(primary school age)	(0.014)	(0.015)	(0.014)	(0.014)	(0.020)**	(0.015)
No. of yrs exposed	-0.003	-0.002	0.006	-0.000	-0.014	-0.005
(high school age)	(0.013)	(0.013)	(0.013)	(0.013)	(0.019)	(0.013)
Gender (male=1)	0.422	0.412	0.418	0.440	0.574	0.395
	(0.030)***	(0.030)***	(0.030)***	(0.031)***	(0.032)***	(0.030)***
Mother's language (native=1)	-1.745	-1.760	-1.706	-1.751	-1.782	-1.759
	(0.068)***	(0.069)***	(0.067)***	(0.064)***	(0.067)***	(0.075)***
Constant	7.699	7.691	8.592	7.715	7.347	8.655
	(0.052)***	(0.053)***	(0.066)***	(0.052)***	(0.062)***	(0.069)***
District of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Year of birth fixed effects	Yes	Yes	Yes	Yes	Yes	Yes
Province specific cubic trend	Yes	Yes	Yes	Yes	Yes	Yes
Mean dependent variable	9.07	9.42	9.42	9.42	9.42	9.44
Observations	135502	133092	136140	136050	106226	129148
R-squared	0.05	0.05	0.05	0.06	0.06	0.05

* significant at 10%; ** significant at 5%; *** significant at 1%. Standard errors clustered at the district of birth level in parentheses. The sample includes all people between 18 and 32 years old interviewed in the 2007 national census. Lima is the capital city of Perú, and also the most urbanized, and developed. Ayacucho and Huancavelica are regions where the conflict started, and where it has been more persistent in time. Huanuco and San Martin are two regions in which the coca cultivation has been particularly active.

Appendix C

Additional Tables for Chapter 3

Table C.1: Descriptive Statistics, by mode of transportation

	Ferry			Helicopter			Water Taxi		
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Reason for choosing transport mode									
Safer	446	0.87	0.34	304	0.17	0.37	336	0.53	0.50
Faster	446	0.02	0.13	304	0.82	0.38	336	0.71	0.46
Cheaper	446	0.62	0.49	304	0.02	0.14	336	0.32	0.47
Demographic Characteristics									
Gender (male=1)	446	0.69	0.46	304	0.65	0.48	335	0.67	0.47
Age	444	40.48	13.67	302	40.37	12.98	332	39.56	12.65
Educ level $<$ completed university	441	0.33	0.47	304	0.17	0.38	333	0.22	0.41
Educ level complete university	441	0.46	0.50	304	0.33	0.47	333	0.51	0.50
Educ level post-graduate	441	0.21	0.41	304	0.49	0.50	333	0.27	0.44
Nationality Sierra Leonean	446	0.62	0.49	304	0.35	0.48	336	0.30	0.46
Nationality Other African	446	0.19	0.39	304	0.24	0.43	336	0.25	0.43
Nationality Non-African	446	0.19	0.39	304	0.41	0.49	336	0.46	0.50
Additional information									
Hourly wage (US$ PPP) - measured	346	20.49	49.45	184	29.80	51.91	225	22.11	50.41
Hourly wage (US$ PPP) - imputed	446	23.51	45.75	304	37.19	44.98	336	26.50	43.21

Sources: Information on choices was collected in the 2009 and 2010 Sierra Leone Survey on Transportation Choices. Notes: Descriptive statistics use the data from the observed trips only (one unique individual per observation), and are weighted to represent the actual proportion of travelers. The exchange rate, and the rate of conversion to PPP comes from the World Bank's World Development Indicators, and we assign these conversion rates by the country of permanent residence. We input the missing observations for wages with the average hourly wage of people in the same education category (Less than some university, Some/Completed University, Post-graduate), region of residence (African/Non-African), and job type (International organization or international private business/Local NGO or local business/Unemployed).

Table C.2: Descriptive Statistics, by Nationality

	Africa			Non African			All		
Variable	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.	Obs.	Mean	Std. Dev.
Transportation Choices									
Transport taken Ferry	721	0.81	0.39	365	0.56	0.50	1086	0.75	0.44
Transport taken Helicopter	721	0.06	0.24	365	0.12	0.33	1086	0.08	0.27
Transport taken Water Taxi	721	0.13	0.34	365	0.32	0.47	1086	0.18	0.38
Demographic Characteristics									
Gender (male=1)	720	0.67	0.47	365	0.70	0.46	1085	0.68	0.47
Age	713	40.45	13.38	365	39.90	13.62	1078	40.31	13.44
Educ level < completed university	714	0.34	0.47	364	0.18	0.38	1078	0.30	0.46
Educ level complete university	714	0.45	0.50	364	0.50	0.50	1078	0.46	0.50
Educ level post-graduate	714	0.22	0.41	364	0.32	0.47	1078	0.24	0.43
Nationality Sierra Leonean	721	0.73	0.44				1086	0.54	0.50
Nationality Other African	721	0.27	0.44				1086	0.20	0.40
Nationality Non-African							1086	0.26	0.44
Additional information									
Hourly wage (US$ PPP) - measured	522	17.69	46.81	233	33.77	57.14	755	21.33	49.76
Hourly wage (US$ PPP) - imputed	721	22.26	44.19	365	33.27	47.74	1086	25.08	45.36

Sources: Information on choices was collected in the 2009 and 2010 Sierra Leone Survey on Transportation Choices. Notes: Descriptive statistics use the data from the observed trips only (one unique individual per observation), and are weighted to represent the actual proportion of travelers. The exchange rate, and the rate of conversion to PPP comes from the World Bank's World Development Indicators, and we assign these conversion rates by the country of permanent residence. We input the missing observations for wages with the average hourly wage of people in the same education category (Less than some university, Some/Completed University, Post-graduate), region of residence (African/Non-African), and job type (International organization or international private business/Local NGO or local business/Unemployed).