

How Revenue and Information Shape Citizen Political Behavior

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ABSTRACT

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Many developing countries exhibit deficits in governance, including corruption, rent-seeking, the suboptimal provision of public goods, and weak accountability. This dissertation uncovers the micro-foundations of political failure by evaluating how government revenue windfalls and information asymmetries affect the will or ability of citizens to curb rent-seeking and hold politicians accountable. The first chapter provides one of the first causal, micro-level tests of the prominent claim that windfalls lower *demand* for good governance in comparison to taxation. It also sheds light on the relationship between revenue and information by examining whether windfalls and taxes produce differences in how citizens become politically informed. The second chapter turns attention to the role of information and examines how new information on government spending affects citizen political participation and incumbent support. The final chapter analyzes whether windfalls induce citizen groups to engage in rent-seeking behavior to appropriate wealth in more divided societies. To identify causal effects at the individual level, I employ experimental and quasi-experimental research designs and original survey and behavioral data from two separate, large-scale field projects conducted in Indonesia. Overall, the dissertation deepens understanding of the causes of political failure by examining not only *whether* windfalls and information asymmetries have adverse effects on citizen political behavior but also *when* and *why*.

Table of Contents

I	Dissertation Chapters	1
1	Introduction	2
1.1	Motivation	3
1.2	The Literature	4
1.3	Dissertation Overview and Empirical Approach	6
1.4	Chapter Summaries	7
1.5	Organization of the Dissertation	11
2	Keeping the Public Purse	12
2.1	Introduction	13
2.2	Theory and Hypotheses	18
2.2.1	Windfalls, taxes, and political action	18
2.2.2	Why do taxes motivate political action?	19
2.2.3	Revenue and the relationship between information and political action	21
2.3	The Indonesia Context	23
2.4	Research Design	26
2.4.1	The experiments	27
2.4.2	Sampling and randomization	32
2.4.3	Data	33
2.4.4	Randomization check	34
2.5	Main Results	36

2.5.1	Estimation and hypothesis testing	36
2.5.2	Monitoring	38
2.5.3	Participation	40
2.5.4	Incumbent sanctioning	43
2.5.5	Summary of main results	45
2.6	Does Revenue Condition the Impact of Information on Political Action?	47
2.7	<i>Why</i> Does Taxation Motivate Action?	51
2.8	Conclusion	55
2.9	Appendix to Paper: Experimental Design	58
2.10	Online Appendix	61
2.10.1	Does the effect of taxation depend on satisfaction?	61
2.10.2	Does the effect of taxation depend on mailbox distance?	64
2.10.3	Were participants sensitive to the price of public goods?	65
2.10.4	Does the effect of taxation depend on previous tax experience or priors?	66
3	How Does Information Affect Political Behavior?	72
3.1	Introduction	73
3.2	Literature and Hypotheses	76
3.3	The Indonesia Context	79
3.4	Experimental Design	81
3.4.1	The campaign and information experiment	81
3.4.2	Sampling and randomization	85
3.4.3	Data	86
3.5	Main Results: How Information Affects Political Decisions	89
3.5.1	Estimation	89
3.5.2	First stage: Updated priors	89
3.5.3	Incumbent support	91
3.5.4	Participation	95

3.5.5	Summary of main results	97
3.6	How Information Effects Vary	97
3.6.1	Prior beliefs	97
3.6.2	Other contextual factors	105
3.7	Conclusion	107
4	Is Bigger Always Better?	110
4.1	Introduction	111
4.2	The Context	114
4.3	Literature and Hypotheses	116
4.4	Identification Strategy	118
4.4.1	Data	119
4.4.2	The regression discontinuity	122
4.4.3	Validity tests	127
4.5	Main Results	131
4.5.1	Did bigger windfalls cause appropriation?	131
4.5.2	Did bigger windfalls yield welfare dividends?	139
4.5.3	Did bigger windfalls facilitate cohesion or exacerbate social conflict?	145
4.5.4	Discussion and robustness checks	148
4.6	Conclusion	150
4.7	Online Appendix	153
4.7.1	The conflict intensity score	153
4.7.2	Density tests	154
4.7.3	Robustness checks	159
5	Concluding Remarks	165

II	Appendix: Campaign Script	169
III	Bibliography	186
	Bibliography	187

List of Figures

2.1	Hypotheses	23
2.2	Campaign Setup	28
2.3	Experiment ‘Gameboards’	30
2.4	Costs of Participation	42
2.5	Main Results	46
2.6	Spending Priors	48
2.7	Why Taxation Motivates Political Action	55
2.8	Taxation and Satisfaction	63
2.9	The Distribution of Prior Beliefs on Taxation	68
2.10	Effects of Taxation, Conditional on Prior Beliefs	71
3.1	Campaign Setup	82
3.2	Spending ‘Gameboard’	84
3.3	Spending Priors	98
3.4	Spending Priors, By Category	100
3.5	Priors on Actual Spending	104
3.6	Demand for Information	106
4.1	The Regression Discontinuity Design	124
4.2	McCrary Density Tests	129
4.3	Effects of Bigger Windfalls on Perceptions of Appropriation	133
4.4	Effect of Bigger Windfalls on Allocation I	136

4.5	Effects of Bigger Windfalls on Allocation (Civilians) II	137
4.6	Effect of Bigger Windfalls on Welfare (Civilians) I	142
4.7	Effect of Bigger Windfalls on Welfare (Civilians) II	143
4.8	Effects of Bigger Windfalls on Social Divisions	147
4.9	Densities Around Population Thresholds	155
4.10	McCrary Tests of Population Thresholds	156
4.11	Densities Around Conflict Thresholds	157
4.12	McCrary Tests of Conflict Thresholds	158

List of Tables

2.1	Summary Statistics	35
2.2	Randomization Check and Baseline Data	36
2.3	Willingness to Monitor	39
2.4	Political Participation	41
2.5	Incumbent Support	44
2.6	The Effect of Information on Attitudes, Conditional on Revenue	49
2.7	Why Taxation Motivates Political Action	54
2.8	Detailed Summary of the Treatments	60
2.9	Does the Effect of Taxation Depend on Satisfaction?	62
2.10	Does the Effect of Taxation on Participation Depend on Mailbox Distance?	64
2.11	The Price of Public Goods	66
2.12	Does the Effect of the Tax Treatment Depend on Actual Tax Experience?	69
2.13	Does the Effect of the Tax Treatment Depend on Prior Beliefs about the Share of Taxes in Total Revenue?	70
3.1	Summary Statistics	87
3.2	Randomization Check	88
3.3	Average Treatment Effect of Information	92
3.4	The Effect of Information Conditional on Party ID	94
3.5	The Effect of Information Conditional on Prior Spending Beliefs	102
3.6	Effect of Information on Political Behavior Conditioned on Beliefs	103

3.7	The Effect of Information Conditional on other Contextual Factors	109
4.1	Descriptive Statistics for Outcome Variables	120
4.2	GAM Strength and Cooperation	121
4.3	Village-level Aid Windfalls	125
4.4	Estimates of Jumps in Village-Level Pre-Treatment Covariates	130
4.5	First Stage: Effect of Bigger Windfalls on Perceptions of Appropriation	132
4.6	First Stage: Effect of Bigger Windfalls on Allocation	135
4.7	Effect of Bigger Windfalls on Welfare—Civilians	140
4.8	Effect of Bigger Windfalls on Welfare—Former Combatants	141
4.9	Effect of Bigger Windfalls on Social Divisions	146
4.10	Additional Analysis	152
4.11	Data and Sources for Conflict Intensity Index	153
4.12	Robustness Check for Effect of Bigger Windfalls on Perceptions of Appropriation . .	160
4.13	Robustness Check for the Effect of Bigger Windfalls on Allocation	161
4.14	Robustness Check for Effect of Bigger Windfalls on Welfare—Civilians	162
4.15	Robustness Check for Effect of Bigger Windfalls on Welfare—Former Combatants .	163
4.16	Robustness Check for Effect of Bigger Windfalls on Social Divisions	164

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Part I

Dissertation Chapters

Chapter 1

Introduction

1.1 Motivation

Many developing countries exhibit deficits in governance, including corruption, rent-seeking, the suboptimal provision of public goods, and weak accountability. Bad governance implies a failure or inability on the part of citizens to constrain politicians from opportunistic behavior and secure efficient public goods provision. If good governance in democracies depends on citizens selecting honest and hard-working politicians, or removing dishonest and lazy ones from power, the question arises: Why do citizens often fail to curb corruption and rent-seeking?

This dissertation focuses on two of the leading political economy explanations for what causes political failure. One prominent explanation centers on where government gets its revenue from. Natural resource rents and other types of windfall revenue are widely believed to be bad for good governance, especially in comparison to alternate sources of revenue like broad-based taxation (Robinson, Torvik and Verdier, 2006; Leite and Weidemann, 1999; Caselli and Michaels, 2009; Moore, 1998). The notion is that windfalls undermine the incentives of both politicians and citizens to ensure that resources are used optimally for the public good. An alternate explanation is that political failure arises not from a lack of citizen *will* to curb rent-seeking but rather from a lack of *ability* to do so. This inability is attributed to the existence of information asymmetries between politicians and the public. Without information on politician preferences and actions in office, citizens are ill-positioned to take political action to elicit better performance (Barro, 1973; Ferejohn, 1999; Besley, 2006).

The negative impacts of windfalls and information asymmetries on governance and development have already been studied extensively. Yet, important questions remain as to how they cause political failure through their impact on citizens. The three chapters in this dissertation shed light on the micro-foundations of political failure by evaluating how revenue and information affect the will or ability of citizens to curb political rent-seeking. They employ experiments and quasi-experiments as well as original survey and behavioral data to identify the causal impact of windfalls, taxes, and information on citizen political attitudes and actions at the individual level.

1.2 The Literature

Where a government gets its revenue from affects whether it works in the best interest of its citizens. This notion is at the heart of a substantial political economy literature on the so-called ‘resource curse’, the paradox that windfalls of revenue from oil, gas or other natural resources often undermine development rather than enable it. Scholars have linked natural resource windfalls not only to corruption, rent-seeking, and the sub-optimal provision of public goods (Leite and Weidemann, 1999; Tornell and Lane, 1999; Robinson, Torvik and Verdier, 2006) but also to a host of related detrimental outcomes, including poor economic growth (Sachs and Warner, 2001); the weakening of accountability in democracies (Moore and Rakner, 2002; Gibson and Hoffman, 2007); the perpetuation of autocratic regimes (Ross, 2001, 2004); and civil war onset and duration (Humphreys, 2005).

Scholars have also argued that windfalls from other types of revenue are similar to those from natural resources and result in many of the same outcomes. These other types of revenue include foreign aid (Moore, 1998; Dalgaard and Olsson, 2006) and, for local governments, central government transfers (Treisman, 2000; Brollo et al., 2010). What natural resource, foreign aid, and central transfer windfalls have in common is that they are large, typically discretionary, inflows of revenue that go directly to government and do not arise from the broad-based production of goods and services in the economy (Dalgaard and Olsson, 2006).

At the heart of this literature is the idea that windfalls undermine the incentives of both politicians and citizens to ensure that resources are used optimally for the public good. Perhaps the dominant explanation in political science is that windfalls reduce government dependence on domestic taxation, weakening citizen motivation to constrain politicians from opportunistic behavior (Beblawi and Luciani, 1987; Huntington, 1991; Ross, 2001, 2004). Others theorize that windfalls actually increase the incentives for citizens to appropriate wealth for personal gain. Drawing on rent-seeking contest models developed by Tullock (1980), this literature claims that windfalls induce citizens, as members of interest groups, to engage in rent-seeking competition that dissipates any potential benefits (Tullock, 1980; Lane and Tornell, 1996; Tornell and Lane, 1998, 1999; Svensson,

2000).

Still others focus on how windfalls alter politician incentives. Robinson et al (2006) theorize that long-run resource booms increase the value of holding office, which induces politicians to use patronage to secure re-election. Brollo et al (2010) predict that revenue booms exacerbate information asymmetries and make it easier for politicians to steal, which in turn attracts more dishonest types to run for office and makes it easier for dishonest incumbents to get re-elected. Political failure in the principal-agent tradition arises in part because citizens are unable to use elections to constrain politicians from rent-seeking behavior. Information also plays an important role in this framework. For Robinson et al (2006) whether politicians can engage in rent-seeking depends on whether citizens have access to information to monitor rent-seeking behavior. Others go so far as to claim that windfalls actually exacerbate information asymmetries between politicians and citizens (Gadenne, 2011; Devarajan et al., 2011; Brollo et al., 2010). This emphasis on information highlights the need to investigate whether windfalls undermine development through incentive or information channels. Overall, a central challenge for the broader resource curse literature—from which this dissertation departs—now lies in identifying which explanations find empirical support, in what contexts, and how different mechanisms relate.

This emphasis on information in the windfall literature draws on an already expansive literature in the political agency tradition that has long noted the dangers of information asymmetries to effective accountability (Barro, 1973; Ferejohn, 1999; Besley, 2006). A growing empirical literature has demonstrated that improving access to information indeed affects whether and how citizens vote (Chong et al., 2011; Banerjee et al., 2011), improves public goods provision (Reinikka and Svensson, 2003), and reduces the re-election probabilities of corrupt incumbents (Ferraz and Finan, 2008). Yet, important questions remain about how citizens use information to make political decisions and mitigate opportunistic behavior by politicians. For instance, how do prior beliefs condition the impact of information on citizen political behavior? And, how do different and potentially conflicting dimensions of information affect citizen political participation and incumbent support? These questions highlight the need for additional research on how information shapes citizen political behavior directly as well as through its relationship to revenue.

1.3 Dissertation Overview and Empirical Approach

The dissertation consists of three chapters—each written as stand-alone papers—that shed light on the micro-foundations of how windfalls and information shape citizen political behavior. Chapter 2 aims to clarify mechanisms behind the resource curse by providing one of the first causal, micro-level tests of the prominent claim that windfalls, compared to taxes, produce less citizen *demand* for good governance. It also sheds light on the relationship between revenue and information by examining whether windfalls and taxes produce differences in how citizens become politically informed. Chapter 3 turns attention to the role of information and examines how new information on government spending affects citizen political participation and incumbent support. Chapter 4 returns the focus to windfalls, this time analyzing whether windfalls induce citizen groups to engage in rent-seeking behavior to appropriate wealth in more divided societies. In doing so, it highlights the importance of context to considering why windfalls are beneficial in some instances and harmful in others.

The three chapters are based on two distinct, large-scale field projects implemented in Indonesia during nearly two years of fieldwork conducted between 2007 and 2010. Chapters 2 and 3 are based on a set of experiments embedded into a public awareness campaign conducted in Blora—a resource rich and transfer-dependent district in Central Java. The experiments employed behavioral exercises to create distinct tax and windfall environments and to vary citizens information on government spending. The main data source is an original individual-level survey undertaken during the same visit. The campaign and survey were conducted with 1,863 randomly sampled and assigned citizens from 93 villages in Blora. The setting for Chapter 4 is a community-driven reconstruction project implemented in post-conflict Aceh, Indonesia. This chapter employs a regression discontinuity and original survey data from civilians, former combatants, and village heads to identify whether windfalls induce groups to engage in rent-seeking competition.

The emphasis on experimental (or quasi-experimental) research designs and original micro-level data accomplishes two things. First, experiments not only ensure that outcomes are driven by the causes of interest (revenue or information) but also make possible the isolation and testing of specific

mechanisms. This is the case in Chapter 2, where I designed the revenue experiment specifically to test the validity of the claim that windfalls produce weaker *demand* for good governance than taxes. Doing so required designing the revenue experiment to minimize the alternate possibility that windfalls undermined citizen political action by exacerbating information asymmetries.

Second using original micro-level data makes it possible to dig deeper into how revenue and information affect citizen political behavior at the individual level. In addition to providing key outcome measures, the survey data also provides a more in-depth picture into underlying mechanisms. This is particularly important for Chapter 2, which examines different explanations for *why* taxation creates greater demand for good governance. Moreover, the surveys provide pre-treatment covariates to examine how treatment effects vary. This is central to Chapter 3, which explores how prior beliefs on government spending condition the effect of information, and for Chapter 4, which analyzes how pre-existing divisions between civilians and former combatants conditioned the effect of aid windfalls.

As a newly decentralized democracy grappling with many development problems—corruption, natural resource management, a history of separatist conflict—Indonesia presents a relevant and interesting context in which to explore how revenue and information affect citizen political behavior. There is a well-known tradeoff here though. Conducting the field research at the local level made it possible to undertake detailed tests of hypotheses drawn from leading theories of political failure, but the approach inevitably raises questions about external validity. In seeking to test the validity of precise mechanisms, this dissertation aimed first and foremost to identify the effects of windfalls and information with a high degree of internal validity.

1.4 Chapter Summaries

Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government

Chapter 2 presents the first micro-level, causal test of what has become a general claim in political science: That windfalls undermine—and taxes strengthen—citizen incentives to hold government

accountable. I test this claim by embedding a novel revenue experiment into a public awareness campaign conducted in Bora—resource rich and central transfer dependent district in Indonesia. The experiment created distinct tax and windfall environments using an exercise in which the tax group paid a simulated tax to the district government and was primed on the share of taxes in total district revenue. The windfall group did not pay a simulated tax and the share of budget government that came from windfalls was primed. To isolate and test the claim that windfalls produce weaker demand for good governance, I designed the experiment to minimize any possible information asymmetries that arise from windfalls. Moreover, the experiments vary different aspects of taxation to shed light on *why* taxes create stronger incentives than windfalls to take political action.

The chapter's second goal is to investigate how windfalls and taxes might also affect the ways in which citizens become politically informed. Specifically, I investigate whether taxes (compared to windfalls) motivate citizens to *acquire* information on government or affect how individuals *process* information on government performance. To explicitly test this latter possibility, I embedded an over-lapping experiment into the public awareness campaign in which the treatment group obtained new information on actual government spending while the control group received placebo information only.

The campaign was conducted with 1,863 randomly sampled and randomly assigned citizens in Bora district in collaboration with the Indonesian NGOs PATTIRO and LPAW. The project was implemented in the months leading up to district head elections in June 2010. The main outcomes of interest—scrutiny, political participation, and reduced support for the incumbent—are measured through both a survey and participation in a postcard campaign. The postcard campaign offers a revealed preference measure of actual willingness to take political action at a small cost and, as such, overcomes the concerns associated with social desirability bias in costless survey responses.

This paper makes three contributions. It provides among the first evidence that taxes, compared to windfalls, can motivate political action. The results show that the tax group was more willing to monitor government and more likely to withdraw support from the incumbent, although there is little evidence that members were more willing to participate in politics. Second, the paper

uncovers the micro-foundations for why windfalls and taxes produce different incentives, showing that participants were moved by an increase in the perceived share of taxes in total revenue, which resulted in greater feelings of ownership over the budget. Finally, it reveals that different sources of government revenue also affect how citizens become politically informed: Windfalls decreased the motivation to seek out information on government but did *not* make citizens more tolerant of wasted revenue. These last findings have important implications for understanding the endogenous sources of pressure for transparency as well as the efficacy of using exogenous (e.g. donor funded) information interventions to promote accountability in windfall contexts.

How Does Information Affect Political Behavior?

Chapter 3 turns its focus to examining how new information affects citizen political behavior, namely political participation, incumbent support, and willingness to remain informed in the future. To evaluate how citizens use information to make political decisions, this chapter digs deeper into the information experiment conducted with 1,863 randomly sampled citizens in Blora (detailed in the previous section). The information experiment provided the treatment group with new information on government spending while the control group received placebo information only. Importantly, the information treatment contained different dimensions of information: On the budget share that politicians spent on goods and services, on routine administration, and on themselves. I use rich pre-treatment survey data on participants' prior beliefs on government spending to analyze how the impact of new information varies at the individual level.

The results—measured in the same survey and postcard campaign described above—show that information caused a substantial increase in perceptions that politicians were performing worse than previously expected. Yet, information only caused higher levels of participation and reduced support for the incumbent when participants received bad news on at least one information dimension. These results suggest the importance of considering how multiple and potentially competing information dimensions could affect political behavior at the individual level. The findings also show that initially providing better information increased willingness to remain informed in the future, and this effect was bigger for those who originally did not want the information. For those

interested in whether one-off information interventions have sustainable effects, this finding indeed indicates that an initial reduction in information costs could lead to information-seeking in the future.

Is Bigger Always Better? How Windfall Size Affects Welfare and Conflict

Chapter 4 examines whether the impact of windfalls on development depends on the underlying degree of power competition in society. Drawing on the rent-seeking contest literature (Tullock, 1980; Tornell and Lane, 1998; Svensson, 2000), it investigates whether bigger windfalls deliver more benefits in homogeneous and cooperative societies and are dissipated when there exist multiple powerful and competing groups. The chapter does this by looking at how social fragmentation between former combatants and civilian victims conditioned the effectiveness of aid windfalls at the village-level in post-conflict Aceh, Indonesia. While windfalls were primarily intended for civilian victims, it is possible that bigger inflows incentivized former combatants to appropriate the wealth in villages where relations with citizens were already contentious as opposed to those in which they were cooperative.

To determine whether bigger windfalls induced appropriation, undermined economic benefits, and exacerbated social divisions, I exploit the fact that villages were assigned to varying-sized windfalls on the basis of arbitrary cutoffs in two continuous measures of eligibility. I use a regression discontinuity and original survey data from civilians, former combatants, and village heads in 212 villages to identify the causal effect of windfall size.

The main results show that, in villages where the combatant group was strong and relations with civilians contentious, former combatants were more likely to appropriate the windfall than in low competition villages. Yet, contrary to the predictions, bigger windfalls did not result in fewer economic benefits or more social tensions for civilians in high competition villages, despite appropriation by former combatants. This raises questions about why bigger windfalls did not deliver even *greater* benefits to civilians in low competition villages compared to high competition ones, as predicted. Overall, in showing that the effect of windfalls varied across villages that were more and less predisposed to group competition, this chapter emphasizes the importance of context

to understanding the relationship between windfalls and development. It also highlights the need for future research on how social divisions mediate the relationship between aid windfalls and development when the revenue is targeted to benefit certain groups.

1.5 Organization of the Dissertation

In sum, this dissertation explores the micro-foundations of how revenue and information shape the political behavior of citizens. In doing so, it seeks to shed light not only on the role of citizens in contributing to political failure but also on the possible avenues by which corruption, rent-seeking and ineffective accountability can be mitigated. The chapters in this dissertation proceed in the order just outlined. Chapters 2 and 4 are accompanied by online appendices that contain additional analysis and exposition. The Appendix (Part II) contains the complete script and illustrations for the public awareness campaign that is the focus of the first two chapters. Chapter 5 concludes by summarizing the main findings and highlighting what broader lessons can be learned by considering the chapters together about how windfalls and information impact citizen political behavior.

Chapter 2

Keeping the Public Purse: An Experiment in Windfalls, Taxes, and the Incentives to Restrain Government

2.1 Introduction

Where a government gets its revenue from is widely believed to affect whether it works in the best interest of its citizens. Natural resource wealth, foreign aid, and other types of windfall revenue hold out the promise of development and better welfare for citizens.¹ Yet, all too often, windfalls are associated with a host of detrimental outcomes, including poor economic performance, the under-provision of public goods, corruption, and weak transparency and accountability (Sachs and Warner, 2001; Tornell and Lane, 1999; Robinson, Torvik and Verdier, 2006; Leite and Weidemann, 1999; Caselli and Michaels, 2009; Brollo et al., 2010; Ross, 2001).

An enduring explanation for this phenomenon is that windfalls free politicians from the need to finance government through taxation, resulting in a more politically quiescent population and weaker restraints on opportunistic politicians (Moore, 1998; Ross, 2004).² When a government in need of tax revenue confronts a population capable of protest or evasion, politicians have an incentive to elicit compliance by making policy or governance concessions (Levi, 1988; North and Weingast, 1989; Bates and Lien, 1985; Timmons, 2005). By now it is almost a truism in political science that taxation mobilizes citizens to demand such concessions, as Huntington (1991, 65) asserts: “The lower the level of taxation, the less reason for the public to demand representation.” Policymakers have similarly embraced this notion: “When governments can survive on natural resource rents, they do not need to tax their citizens. In turn, citizens do not expect or demand public services, clean government, or even basic accountability.”³

Yet, there is good reason to treat this claim as an hypothesis rather than a conclusion. For one, while there is ample support that windfalls and taxes produce variation in political mobilization, there is little micro-level, causal evidence that this can be attributed to differences in *demand* for good governance. Such evidence is particularly important given that some have advised caution

¹While the literature on the resource curse has highlighted the negative link between oil and mineral wealth and development, both foreign aid and central transfers have also been characterized as windfall revenue. Windfall revenue generally accrues directly to governments as large discretionary inflows of income that do not arise from the standard production of goods and services in the economy (Dalgaard and Olsson, 2006).

²Both economic and political economy explanations for how windfalls, particularly natural resources, undermine development abound. For review articles on the resource curse, see Ross (1999) and Rosser (2006).

³The Center for Global Development, <http://www.cgdev.org>, last accessed November 2010.

in embracing this claim (Herb, 2003, 2005). On one hand, taxation (or the lack thereof) has been used to explain the rise of parliamentary institutions in early modern Europe (North and Weingast, 1989; Tilly, 1992; Root, 1994; Downing, 1992); persistent authoritarianism in the rentier states of the Middle East (Mahdavi, 1970; Beblawi, 1987); and pressure for democratization and responsive policy-making in contemporary developing countries such as China (Bernstein and Lu, 2003) and Poland, as well as cross-nationally (Ross, 2004). Recently, however, Haber and Menaldo (2011) use detailed cross-national time series analysis to argue that natural resources do *not* cause authoritarianism. Others have pointed out that revenue booms often bolster citizens' expectations and create upward pressure for government spending (Alesina, Tabellini and Campante, 2008; Kakonge, 2011). One way to adjudicate conflicting national and cross-national evidence is to turn to the micro-foundations for empirical support, but the impact of windfalls and taxes on individual psychology has not yet been demonstrated. Without such evidence, the fundamentally micro-level claim that taxation produces stronger incentives than windfalls to hold politicians accountable remains unsubstantiated.

A second reason to question this claim is that there exists a compelling alternative mechanism by which taxation facilitates political action: Better information. According to this view, windfalls undermine accountability by exacerbating information asymmetries between government and citizens. This argument draws on a substantial political agency literature that has long noted the dangers of an uninformed public to accountability (Barro, 1973; Ferejohn, 1999; Besley, 2006). Windfalls potentially deepen information asymmetries because they give politicians 'more room to steal' while still pleasing uninformed voters (Brollo et al., 2010) or because politicians are freer to spend resource rents on patronage when transparent institutions are weak (Robinson, Torvik and Verdier, 2006). By this view, taxation comes with information benefits that windfalls lack; for instance it reduces uncertainty about the size of the total budget or facilitates comprehension of public finances in per capita terms (Gadenne, 2011; Sandbu, 2006; Devarajan et al., 2011). This literature implies that the most direct way to mitigate the detrimental effects of windfalls is to improve citizens' access to information.

The main goal of this paper is to provide the first individual-level, causal test of what has

become a general claim in political science: That taxes strengthen—and windfalls undermine—citizens’ motivation to hold government accountable. I do this by embedding a novel revenue experiment into a public awareness campaign conducted in a resource-rich district in Indonesia. The experiment created distinct tax and windfall environments using an exercise in which the tax group both paid a simulated tax to the district government and was primed on the share of taxes in total government revenue. Those in the windfall group did not pay a simulated tax and the share of the government budget that came from windfalls was primed. Importantly, the experimental design minimizes the possible information benefits of taxation in order to isolate and test the claim that taxation produces a stronger incentives to take political action. Moreover, by varying different aspects of taxation in known ways, the experiment not only sheds light on *whether* taxation motivates political action but also *why*.

The second goal of the paper is to redress the fact that stronger motivation and better information have so far been presented as alternate and distinct mechanisms for explaining why taxation facilitates political action. Importantly, the motivational effect of taxation could have important implications for how citizens become politically informed, suggesting that these mechanisms might in fact relate. One possibility investigated in the paper is that taxation affects whether people *acquire* information by motivating citizens to become politically informed or to demand better transparency from government. Another possibility is that, once people are in possession of information on government performance, revenue conditions how they *process* that information. Notably, revelations of misused funds could provoke more political action when citizens pay taxes (Sandhu, 2006; Ross, 2004). I test this latter possibility by embedding an over-lapping information experiment into the public awareness campaign in which the treatment group obtained new information on actual government spending while the control group received placebo information only.

The campaign was conducted with 1,863 randomly sampled and randomly assigned adults from 93 villages around Blora district. Since Indonesia’s ‘big bang’ decentralization in 2001, districts like Blora have acquired both significant resources and authority to manage local development. Virtually every district relies on windfall revenue in the form of central transfers, with some districts—like

oil- and gas-rich Blora—beneficiaries of substantial additional revenue from natural resources.⁴ The campaign itself was conducted in partnership with Indonesian NGOs and implemented by trained canvassers in the three months preceding district head elections in 2010.⁵ The main outcomes of interest—measured through both a survey and participation in a postcard campaign—are the effects of taxation on monitoring, political participation, and withdrawing support from the incumbent (sanctioning).

The main results show that taxation, compared to windfalls, can indeed affect political action. The tax treatment caused a five percentage point increase in the share of participants willing to monitor the budget. It also produced a six percentage point rise in using the postcard to signal dissatisfaction to the incumbent. Interestingly, the tax treatment did not lead to more political participation as measured both by turnout in the postcard campaign and in the survey. This suggests that, at least in the context of this experiment, the perceived benefits of taking action induced by taxation did not exceed the costs of more overt and costly types of political participation. The fact that taxation did cause more monitoring and sanctioning can be attributed to the shift in the perceived share of taxes and windfalls in the budget induced by the treatment, which affected feelings of ownership over the budget and the relevance of the budget to daily life. These findings provide not only the first micro-level evidence that taxation can produce stronger incentives than windfalls to constrain government but also insights into *why* this is the case.

The findings also shed light on how taxation might affect the link between information and accountability. By showing that taxation increased the willingness to monitor government, these results take a step towards explaining where a more politically informed public comes from in the first place. In so doing, they imply that any information benefits of taxation might not be intrinsic but rather attributable to motivation after all. Yet, once in possession of better information on government spending, the windfall group was not more tolerant of misuse. The information

⁴Focusing on the sub-national level in Indonesia is consistent with recent research that has suggested that the resource curse is not only a national but also local phenomenon and that central transfers to local governments are revenue windfalls (Brollo et al., 2010).

⁵The threat of a local resource curse was a central concern for the two Indonesian partner organizations, PATTIRO and LPAW. These organizations have been working with district leaders in Blora since 2008 to promote transparency in oil and gas revenues and the use of resource windfalls for social and economic development.

treatment caused a notable 44 percentage point increase in the share of participants who felt that government performed worse than initially believed, but the magnitude of the effect was about the same in both the tax and windfall groups. This indicates that the information did not provoke a more negative evaluation of government performance in the tax environment. Moreover, revelations of misused funds did not induce more monitoring, participation, or incumbent sanctioning in the tax than the windfall group; if anything, the effect of information on political action was as great, if not greater, in the windfall group.

This last finding has important implications for a growing empirical literature studying the effects of information on political behavior and accountability. Recent studies have demonstrated that exogenously improving access to information—through the efforts of government, the media or civil society—affects whether and how citizens vote (Humphreys and Weinstein, 2010; Chong et al., 2011; Banerjee et al., 2011), reduces corruption in public goods provision (Reinikka and Svensson, 2003), and hurts the re-election probabilities of corrupt incumbents (Ferraz and Finan, 2008). Yet, it is also true that transparency “is a necessary, but not sufficient, condition to reduce corruption. In addition to access to information, [people] need an *ability to process* the information and the *ability and incentives to act* on the processed information” (Kolstad and Wiig, 2008, 524). Critically, public goods in developing countries are rarely or only minimally financed by taxpayers (Bird and Oldman, 1990; Bird and Zolt, 2005), but this literature has not yet considered whether the impact of information on citizens’ political action will be less robust in windfall contexts because of weaker incentives to use information to hold government accountable. In testing and finding no support for this possibility, the results dispel this concern and point instead to the potentially substantial dividends to using expenditure transparency to combat the detrimental effects of revenue windfalls.

This paper proceeds as follows. Section 2.2 presents hypotheses for why taxes and windfalls create different incentives for citizens to hold government accountable, and why different sources of government revenue might affect how citizens become politically informed. Section 2.3 provides background information on Indonesia as the setting for the experiment. The research design, including the experiments and data collection are described in Section 2.4. Section 2.5 presents the main results for the effect of windfalls and taxes on political action while Section 2.6 evaluates the

evidence that revenue conditions the effect of information on political behavior. In Section 2.7 I use features of the experimental design and survey data to dig deeper into why taxation motivates political action. Section 2.8 concludes by revisiting the implications of this study for both academic literature and policy.

2.2 Theory and Hypotheses

2.2.1 Windfalls, taxes, and political action

Corruption and rent-seeking imply a failure or inability on the part of citizens to restrain opportunistic politicians by credibly threatening to remove them from power. A substantial political economy literature has focused on how taxation catalyzes citizens to discipline government—discipline presumed to be weak or absent where windfalls have diminished fiscal reliance on tax revenue. Brennan and Buchanan (1980) show formally that a revenue-maximizing Leviathan can be induced to provide public goods when there is strong complementarity between the tax base and the good itself. Bates and Lien (1985) develop a model in which government has an incentive to accede to the policy preferences of the tax base, especially the owners of mobile assets, for the purpose of raising tax revenues. The notion of taxation as a ‘revolutionary constraint,’ in which taxes determine in part whether citizens overthrow government to obtain more favorable redistribution, is central to theories of democratization in Acemoglu and Robinson (2006), Boix (2003), and Bueno de Mesquita et al (2003). While much of the literature has developed with non-democratic government as the starting point, the logic has readily extended to explain the quality of governance in democracies as well (Moore and Rakner, 2002; Gibson and Hoffman, 2007).

Of central interest in this paper are three aspects of political action that citizens can use to discipline government. First, taxation could motivate individuals to monitor and scrutinize government (Devarajan et al., 2011).⁶ Monitoring politicians’ behavior is essential to political

⁶Testimony to how widely this is asserted comes from Collier and Hoeffler (2005a) who write “Public scrutiny is generally weaker in countries with high natural resource rents because of lower taxation, as such patronage politics are more likely” and make monitoring endogenous in a formal model by introducing “a standard political science relationship in which citizens are provoked into scrutiny by taxation.”

oversight and to the existence of an informed public (Miller, 2005; Banks, 1989; Olken, 2007; Davis and Hayes, 1993; Bjorkman and Svensson, 2010; Grossman and Hanlon, 2011). Second, taxation could make citizens more willing to take overt political action, from low-cost action like signing a petition or voting in elections to more high-cost action like joining a protest. Yet, citizens can use such participation either to signal support for or dissatisfaction with the incumbent. The third aspect of restraint stems from the observation that higher taxes (holding spending constant) are widely thought to hurt support for the incumbent and to induce political action *against* the government (Niemi, Stanley and Vogel, 1995; Morrison, forthcoming).⁷

Following the literature, the main predictions are that taxes, compared to windfalls (and holding spending constant) motivate citizens to:

H1 *Monitor government.*

H2 *Participate in politics.*

H3 *Withdraw support for the incumbent (sanctioning).*

2.2.2 Why do taxes motivate political action?

While the predominant view in political science is that taxation gives citizens stronger incentives to monitor, participate, and sanction, we still know little about *why* this would be the case. The literature suggests three distinct aspects of taxation that might drive incentives: The tax burden, the share of taxes in total revenue, and the relationship between taxing and spending. Each in turn are thought to motivate political action by altering individuals' attitudes towards government.

One explanation for why taxes produce greater demand for good governance than windfalls centers on the tax burden, or the share of income paid in taxes. The literature has long acknowledged that taxes tend to mobilize when they 'bite directly' (Brautigam, 2002, 11). Thus, income, property or other direct taxes that create clear, and especially excessive, burdens are most closely linked

⁷An alternate hypothesis that has received less attention in political science is that the effect of taxation on incumbent sanctioning could be conditional on satisfaction with government performance; if a person is satisfied with the government, then making a contribution could elicit a 'warm glow', as suggested by recent neural studies (Harbaugh, Mayr and Burghart, 2007). I test this alternate prediction in the online appendix and find instead that taxation actually caused a sharper increase in incumbent sanctioning among those who were initially satisfied with the government.

to political action (Scott, 1976; Moore, 1998).⁸ Research on the endowment effect in behavioral economics and psychology provides insight into why a higher tax burden mobilizes action. According to prospect theory, people are more averse to out-of-pocket losses than to foregone gains of an equivalent value (Kahneman and Tversky, 1984; Kahneman, Knetsch and Thaler, 1991; Morewedge et al., 2009). Sandbu (2006) summarizes why the endowment effect would make citizens care more about \$1 of misspent tax revenue than \$1 of misspent windfall revenue:

[The] endowment effect is likely to occur with earned income. Tax payments are generally perceived as a cost that people have to pay out of their earnings, and so people have an incentive to hold the government accountable for how it spends their money. Natural resource wealth that is wasted or stolen, in contrast, is more likely to be perceived as a foregone gain, since it has never passed through the hands of the population and therefore has never been ‘earned’ or ‘possessed.’ The endowment effect implies that the motivation to hold the government accountable is less strong in the case of natural resource revenue than in the case of taxes.

Another possibility is that citizens are motivated to take political action by the share of total government revenue that comes from taxes. The ratio of taxes to total revenue has traditionally been regarded as “the degree to which governments are dependent upon taxpayers for their revenues. The higher that dependency, the more likely it is that governments will have to listen to their citizens” (Waterbury, 1997, 157). Government’s fiscal dependence on taxation is associated with the probability that it will offer policy or governance concessions. Thus, the higher the proportion of taxes in total revenue, the more citizens might feel they have leverage over politicians and that pressure on government will likely succeed in eliciting concessions.

Finally, citizens could be motivated to take action not by higher taxes alone but rather by the level of taxes relative to spending. Ross (2004) defines this as the ‘price’ of public goods: “High taxes would not produce greater demand for representative government, if the taxes were offset by

⁸As Mahon (2005) articulates: “The main advantage of a direct tax was the clarity of its incidence...it was...thought that knowing for certain the scale of his contribution would make the citizen into a more responsible guardian of the public purse.”

greater benefits. Nor would a small tax bill necessarily lead to political quiescence. Both the size of the tax burden, and the quality and quantity of government spending matter; citizens ultimately care about the ‘price’ they pay for the government services they receive.” In a cross national test, Ross (2004) indeed finds that it is the ratio of taxes to spending that brings about democratization. The price of public goods captures the importance of expectations of fairness and reciprocity in the ‘fiscal contract’ between state and society over the terms of taxation. As the price of public goods rises—and particularly when taxes exceed the benefits provided in return—reciprocity in the fiscal contract is breached, and deviations from norms of fairness and reciprocity are known to be important psychological motivators (Bolton, Katok and Zwick, 1998; Camerer, 2003). Since windfalls enable the provision of goods and services at little or no cost to the public, citizens are more willing to acquiesce to poor performance.

2.2.3 Revenue and the relationship between information and political action

Predictions on why taxation motivates political action also have important implications for understanding the relationship between revenue, information, and accountability. As discussed in the Section 2.1, both weak taxation and information asymmetries have been proposed as leading—but so far distinct—mechanisms linking windfalls to bad governance. Perhaps because a more informed public is the result of taxation, the taxation literature often presumes that citizens have perfect information on the goods and services provided by government (Bates and Lien, 1985; Brennan and Buchanan, 1980). Similarly, theories of how windfalls exacerbate information asymmetries often overlook the effect on taxation. A central challenge for research going forward is identifying empirically which mechanisms have explanatory power, under what conditions, and how different mechanisms relate to one another. This paper, therefore, not only tests the motivational effect of taxation but also considers the implications of this mechanism for the information story.

One possibility, already captured in H1, is that taxation motivates citizens to become more informed or to demand transparency concessions from government. For many—especially in developing countries where education levels and access to information are low—the costs of information exceed the perceived benefits and lead to political ignorance (Downs, 1957; Bimber, 2001; Jerit,

Barabas and Bolsen, 2006). If paying taxes increases the perceived benefits to seeking out information, then a government's source of revenue could help explain how a good information environment emerges endogenously. This is important because, while access to information is essential to accountability, we still know little about the causes of fiscal transparency.

Another possibility is that taxation conditions how individuals evaluate the information on government performance that they hold. The micro-foundations just discussed reveal why this might be the case. If taxation induces an endowment effect and makes citizens care more waste, then learning about misspent revenue should provoke a stronger political reaction in a tax environment (Sandbu, 2006). Or, if citizens are sensitive to the 'price' of public goods, then information that reveals that citizens are getting less for their taxes should motivate political action (Ross, 2004). In sum, information that reveals wasted or misused funds and shifts beliefs about government spending downward should be more likely to provoke political action in a tax than a windfall environment. As mentioned in Section 2.1, this prediction has important implications for understanding not only how information is processed but also whether improving access to information can mitigate the detrimental effects of revenue windfalls. I test this prediction by adding the final hypothesis:

H4 *Beliefs that government funds are wasted or misused will provoke higher levels of monitoring, participation, and sanctioning for citizens in tax than in windfall contexts.*

The main hypotheses are summarized in Figure 2.1 following the experimental design and the format in which results will be presented. The 2x2 table shows overlapping windfall and tax, and low and high information, environments, which create four experimental conditions also depicted in the figure below using. The first three hypotheses—that taxation leads to higher levels of monitoring, participation, and sanctioning—are captured by a shift upwards of the tax line vis-a-vis the windfall line (also denoted by the + in the far column of the table). The fourth hypothesis is an interaction hypothesis in that the effect of information on all three types of political action is predicted to be greater in a tax than a windfall environment. This prediction is captured by the fact that the slope of the tax line increases more sharply than the slope of the windfall line (also

indicated by the positive interaction term in the bottom right cell of the table).⁹

		Windfall Context (C1)	Tax Context (T1)	Difference (T1-C1)
Low info	(C2)	Group 1	Group 2	+
High info	(T2)	Group 3	Group 4	+
Difference	(T2-C2)	+/-	+/-	+

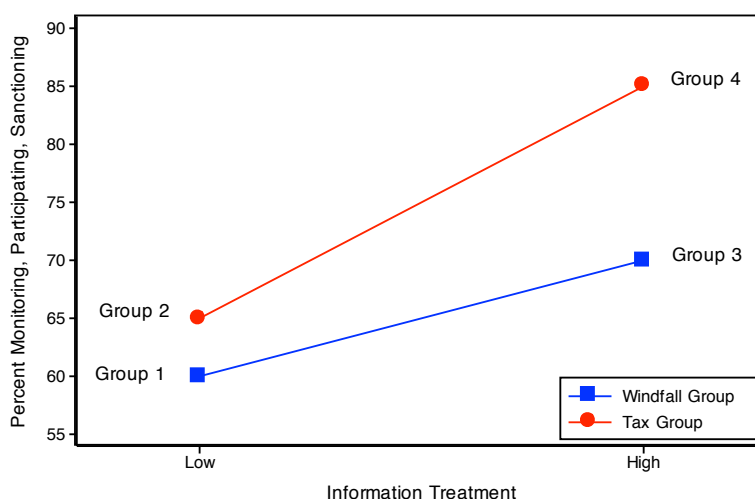


Figure 2.1: Hypotheses. *The table and figure depict the four main hypotheses. Taxes are predicted to lead to more monitoring, participating, and sanctioning than windfalls, as denoted by the far column of the table and the shift upwards of the tax line vis-a-vis the windfall line in the figure. Evidence that the effect of information on political action will be greater in a tax than a windfall environment is captured by the positive interaction effect in the bottom-right cell of the table and by the fact that the slope of the tax line rises more steeply than the slope of the windfall line.*

2.3 The Indonesia Context

As one of the world’s most populous and newly decentralized democracies, Indonesia presents a highly relevant context for testing these hypotheses. Indonesia’s ‘big bang’ decentralization in 2001

⁹I focus in the paper on testing the effect of *negative* information because this fits the empirical setting—very few participants found the information positive—and simplifies the predictions. It is still difficult to predict *ex ante* the impact of negative information on all of the outcomes of interest. While learning that government is doing a bad job should produce more incumbent sanctioning, it could lead to higher or lower levels of monitoring and participation (Banerjee et al., 2011; Chong et al., 2011). Hence the +/- in the bottom row.

devolved the authority to manage local development in key areas—including health, education, water sanitation and infrastructure—to the district-level. This made districts the second most important level of government in Indonesia after the national level. District governments now receive more than 40 percent of total public funds to manage development locally. The promise of decentralization lies in its ability to improve service delivery and strengthen accountability by bringing state and society into closer proximity (Tiebout, 1956). A recent World Bank (2007) report notes, however, that “local governments now have significant authority over planning and budgets, but they do not yet have clear incentives to use these funds to maximize economic development and service delivery outputs for local citizens.”

Democracy and accountability, while vibrant in post-Suharto Indonesia, are imperfect. Indonesians now directly elect both the executive branch and legislative branch at both national and local levels. Electoral participation is still relatively high; voter turnout in the 2009 parliamentary elections was estimated at 71 percent and average turnout in local elections was estimated at 69 percent (Meitzner, 2009). There also appears to be evidence of an anti-incumbency bias and turnover is frequent. Yet, Indonesians often seem to express quiescence in the face of corruption and poor governance. One possible explanation for this is (on Java at least) might be a deep-seated culture of *nrimo*, a concept which loosely translates into “acceptance of everything without protest” and which is closely associated with accepting one’s destiny (Irawaty, 2008). Javanese often attribute bad outcomes, such as floods due to a dam breaking, to fate rather than to government mismanagement. Such cultural contexts raise important questions about what mobilizes citizens to hold their politicians accountable for poor performance.

One potentially important obstacle to political engagement in Indonesia stems from the fact that these elected district governments are still fiscally dependent on revenue windfalls. The main source of funding for districts are central government transfers through general allocation funds (*Dana Alokasi Umum*, DAU), which on average finances more than 80 percent of district government expenditures (World Bank, 2007, 23). Additionally, about ten percent of regional governments receive income from natural resources, with producing districts receiving six percent of the oil

revenues and 12 percent of the gas revenues that accrue to the center (World Bank, 2007).¹⁰ For some well-endowed districts, this has resulted in substantial windfalls and, anecdotally, high levels of corruption and mismanagement.¹¹

Moreover, Indonesia's tax system is both highly centralized and relatively weak overall. The central government is responsible for setting tax rates and collecting revenue. District governments receive shares of their natural resource, income and property taxes but have limited authority to raise additional revenue through formal taxation (Brodjonegoro and Martinez-Vazquez, 2002; Fane, 2003; Lewis, 2003).¹² The most recent legislation in August 2009 rationalized the fiscal system, limiting local governments to a prescribed set of taxes and fees to minimize the growing proliferation of illicit fees and taxes. Overall, tax effort (especially income tax effort) has been weak in Indonesia, which could be due in part to the substitution of domestic taxes with taxes on natural resources (Bahl and Tumennasan, 2004). While recently the government has endeavored to strengthen its tax capacity, the vast majority of citizens have low or nonexistent formal tax burdens.

On the expenditure performance side, many districts in Indonesia are plagued by corruption and poor budgeting. Common problems include weak capacity, rent-seeking, large allocations for routine expenditures, poor reflection of policy priorities, and weak legislative oversight. There are a number of obstacles to improving the budgetary process in Indonesia. While the system contains a vehicle for participatory budgeting called the *musrenbang*, many are skeptical about the effectiveness of this mechanism.¹³ Several NGOs in Indonesia, including PATTIRO, conduct independent budget monitoring to increase public awareness of poor budgeting practices (Par, 2008; KOP, 2008).

These features of fiscal and political life in Indonesia are all evident in Blora district. While Blora is a relatively poor agricultural district, it anticipates substantial windfalls from the Cepu oil

¹⁰ Aceh and Papua New Guinea have different deals negotiated as part of their special autonomy.

¹¹ One commonly cited example is Kutai Kartanegara (East Kalimantan), where natural resource windfalls are thought to have led to a profusion of corruption and patronage (Evaquarta, 2010).

¹² Local governments do have authority to raise additional 'local-own' revenue (*Pendapatan Asli Daerah*, or PAD), but PAD remains a small share of revenue at only 8.5 percent of total revenue (World Bank, 2007, 151).

¹³ *Musrenbang* stands for *musyawarah rencana pembangunan* and refers to a series of consultative meetings on budget priorities that start at the village level and go up to the district level.

and gas block in addition to the transfers it already receives from the government (which comprise the vast majority of its revenue). Concerns about the conditions for a local resource curse are compounded by a history of budget management and inefficiency. Allegations of corruption were particularly rife during the tenure from 1999-2009 of the former legislative chairman, Pak Warsit, who actually ran for district head in the 2010 elections.¹⁴ For organizations like PATTIRO and LPAW seeking to promote responsible government management of revenue windfalls, districts like Bloro present important opportunities for—and potentially significant obstacles to—strengthening citizen demand for good government performance. It was in this context that the public awareness campaign took place.

2.4 Research Design

To test the hypotheses described in Section 2.2 experiments were embedded in a public awareness campaign conducted one-on-one by canvassers in participants' homes. The experiments employed novel exercises to create distinct tax and windfall environments and to vary citizens' information on government spending. The experiments were over-lapped to create four groups: (1) a less informed windfall group, (2) a less informed tax group, (3) a better informed windfall group, and (4) a better informed tax group. A total of 1,863 participants from 93 villages around Bloro were randomly sampled from the adult population and assigned to one of the four experimental conditions. I use an original survey and postcard campaign to measure differences in government monitoring, political participation, and incumbent support across the experimental groups.

¹⁴Warsit was arrested and tried in 2008 for corruption amounting to 5.6 miliar rupiah (USD \$721,000 in 2011 dollars) in the 2004 district budget. He was originally convicted (along with three deputy parliamentarians) by a provincial court but Warsit's conviction was reversed by the Supreme Court just prior to the official candidate registration date for district head elections. The ruling was reversed for Warsit and not for his three deputies. The decision, and its timing, was widely regarded as suspect by the media and civil society in Bloro. According to Indonesian law (Law 32/2004 on Regional Governance, sub-article 58f), anyone found guilty and sentenced to five or more years in prison cannot run for office.

2.4.1 The experiments

The public awareness campaign was designed to familiarize citizens in Blora with the concept of the district budget and the role of elected officials in managing it. The campaign began with a three illustration introduction (see Figure 2.2). The opening illustrations reminded participants that they vote for their representatives; that their representatives make decisions about public services; and that they should think about the future of the district when making political decisions.

All participants then took part in a household budgeting exercise (Figure 2.2). When they initially consented to participate, individuals earned a small income in exchange for their time, equivalent to about a half a day's wage for an agricultural laborer. Specifically, the windfall group earned 10,000 rupiah (about \$1) and the tax group earned 14,000 rupiah (about \$1.40). For the budget exercise, the canvasser asked the participant to use this income to illustrate how they allocate in their income in their own household budget. Participants were asked to select six cards representing their household expenditures (e.g. clothes, education) and to divide their income across these expenditures in accordance with how they planned to spend it. This exercise helped to familiarize participants with the concept of a budget. It also aimed to deepen participants' perceived ownership over their earned income, thus setting the stage for the revenue experiment.

2.4.1.1 Revenue experiment

In this experiment, the tax treatment required participants to make a simulated tax payment to the district government and primed the share of taxes in total revenue; the windfall treatment required no personal contribution and primed the salience of central transfers and natural resource revenue in the district budget. Following the household budget exercise, the canvasser introduced the district government budget, explaining that just as households get income that they allocate across expenditures, so does the government. In the windfall treatment, participants were asked to return their 10,000 rupiah in income to their pockets and the canvasser then took out a separate 10,000 rupiah representing district government income per capita. Using the windfall board (Figure 2.3, top), the canvasser divided the amount into 8,000 rupiah representing the share of revenue from central government transfers, natural resources and other windfall revenues, and 2,000 rupiah

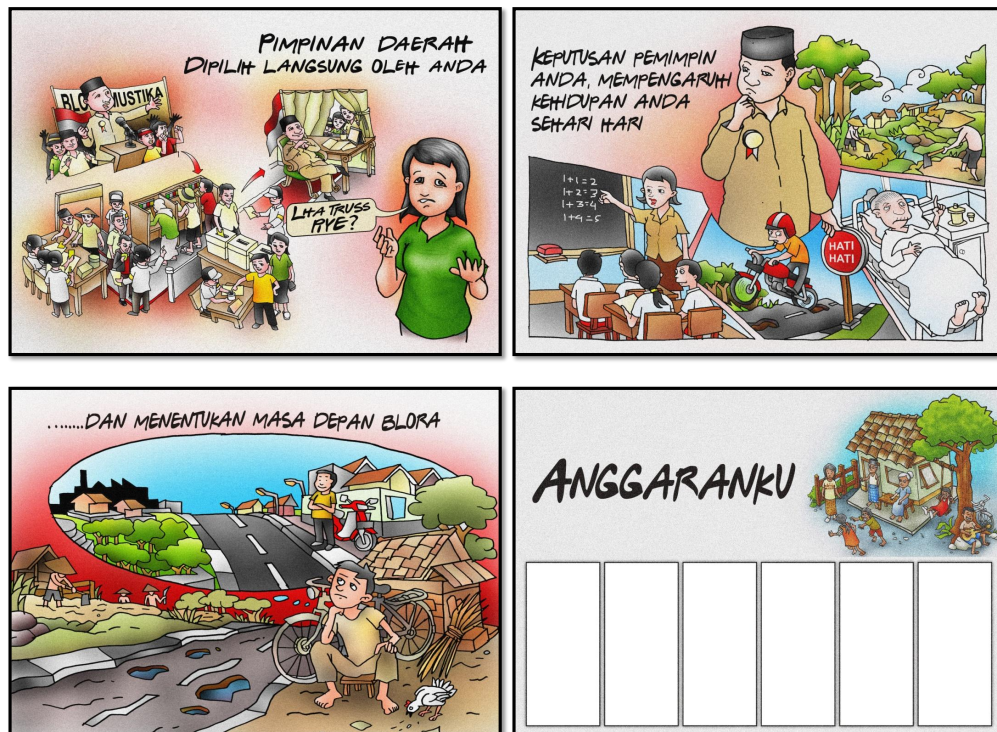


Figure 2.2: Campaign Setup. You elect your leaders, and then what do they do (top left)? They make decisions about public services, like education, health, infrastructure, and farming (top right). Think about what you want Blora to look like in the future (bottom left). The ‘My Budget’ household budget exercise game board (bottom right).

representing revenue from other, unspecified sources.

In the tax treatment, things proceeded slightly differently after individuals allocated their income in the household budget exercise. Rather than having participants return their income to their pockets, the canvasser reminded participants that they (or citizens like them) often have to pay taxes to the district government. To represent these tax obligations, the canvasser transferred 4,000 of the participants' 14,000 rupiah income from the household budget board to the tax board (Figure 2.3, middle). The canvasser then added 6,000 rupiah to the tax board from a separate pot, representing income from other, non-tax sources. Both windfall and tax groups ended with the equivalent of 10,000 rupiah on the board representing government income per capita, where the only variation between the groups was the tax payment itself and the emphasized share of total revenue from taxes or windfalls in the budget.¹⁵ Canvassers used this 10,000 rupiah to illustrate real government spending in the subsequent information experiment.

2.4.1.2 Information experiment

The main goal of this experiment was to use new information to alter participants's beliefs about government spending performance. This experiment utilized the fact that many citizens in Blora did not have accurate information on government spending beforehand. The treatment group obtained information on the share of the budget spent on programs and services for citizens from an analysis of the 2008 realized budget performed by the author and partner organizations.¹⁶ The control group received placebo information in the form of a list of facts and figures taken from the 2008 Blora statistical yearbook (*Blora dalam Angka*).

Specifically, the information treatment used the 10,000 rupiah composed during the revenue experiment, representing government income per capita, and the district budget game-board (Figure 2.3, bottom). The district budget game-board depicts how district elected leaders allocate revenue

¹⁵Individuals in both the windfall and tax treatments ultimately kept 10,000 rupiah in income to control for income effects.

¹⁶The 2008 budget was selected because it was the most recent year for which realized spending data was available and for which the incumbent district head and former legislative chairman (both candidates in the 2010 district head elections) were responsible. Details on how the budget analysis were conducted are in the field manual, available on the author's website.

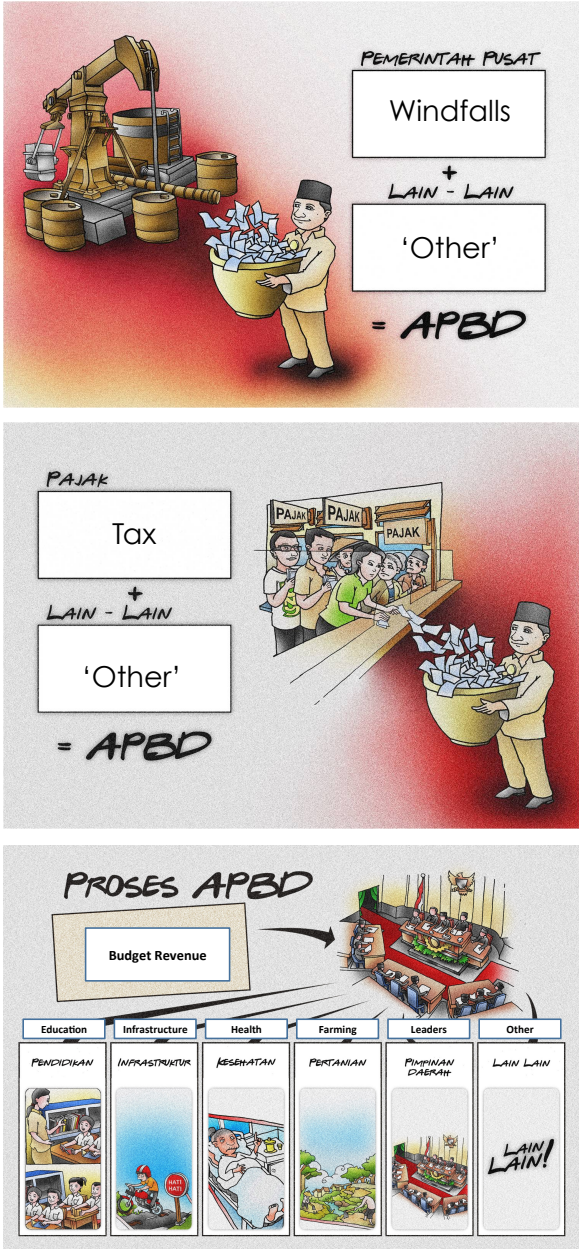


Figure 2.3: Experiment 'game-boards.' The windfall game board (top); the tax game board (center); and the information experiment spending game board (bottom).

across five categories (plus an ‘other’ category): education, infrastructure, health, farming, and politicians. The information was presented in four parts. First, the canvasser divided the 10,000 rupiah to illustrate the share of the total budget spent on each of the six sectors. Second, the canvasser showed, *within* sectors, the share of the total budget spent on programs for citizens versus routine administration. Third, to illustrate the fact that corruption further erodes spending on programs for citizens, the campaign cited two recent cases of alleged corruption and removed two cents from the board.¹⁷ Finally, the canvasser disaggregated spending in the ‘politicians’ category, breaking out the share that went to running the executive and legislative offices from the share that Blora’s 46 elected representatives allocated to themselves for salaries, healthcare, travel, and other private benefits. Overall, participants learned that about 30 percent of the total budget was spent on direct programs and services for citizens. Since participants initially believed that government was spending substantially more on citizens; this treatment identifies whether poor spending performance (negative information) elicits a stronger reaction from the tax than the windfall group.

2.4.1.3 A note on design

The design merits some explanation as one of the first attempts to use an experiment to simulate windfall and tax contexts.¹⁸ One main advantage of the design is that it isolates the hypothesized incentive effect of taxation from the information channel, which it does by giving both the windfall and tax groups the kind of information that taxation is thought to be intrinsically better at providing. One possible benefit of taxation is that it presents public finances in per capita terms (Sandbu, 2006)—here information for both windfall and tax groups was presented as such. Similarly, both groups learned about revenue and spending in terms of proportions of the total budget,

¹⁷Participants were told that the actual scale of corruption in the budget was not known and that the two cents removed was meant to represent the fact that corruption exists.

¹⁸This is (to my knowledge) the first experimental test of the causal impact of taxes (versus windfalls) on political preferences and behavior. Other studies have experimentally manipulated a tax to identify impacts on economic behavior (Chetty, Looney and Kroft, N.d.; Sausgruber and Tyran, 2008; Finkelstein, forthcoming; Blumkin, Ruffle and Ganun, N.d.). There are also been several experimental studies of the determinants of tax compliance (Alm, Jackson and McKee, 1992; Alm and McKee, 2003; Cummings et al., 2006; Torgler, 2007).

not in terms of magnitudes, which are difficult to comprehend. The experiment also minimized fiscal illusion for both groups by establishing clear linkages between revenue, spending, and the district government.¹⁹ These design features should increase confidence that any effect of taxation is due to stronger incentives, not better information.

A second advantage is that the design helps to clarify *why* taxation creates stronger incentives to take political action. The tax treatment exogenously varies three distinct and theoretically-motivated aspects of taxation: The tax burden, the perceived share of taxes in total revenue, and the perceived ratio of taxes to spending. How the experiment does this, and the advantages of this approach, are elaborated in the Appendix. While it was not possible to vary each aspect of taxation independently, I present survey evidence in Section 2.7 to uncover which aspects of taxation drive the main results.

2.4.2 Sampling and randomization

Participants in the campaign were randomly sampled using multi-stage cluster sampling from the adult population in Blora. The target population was all individuals between the ages of 17 (the voter eligibility age) and 65 who had resided in Blora for at least six months. First, 93 of 295 villages were randomly sampled within strata formed by subdistrict and urban-rural status. Within each village, one sub-village unit (*dusun*) was randomly sampled, followed by the random selection of 20 households from an updated list of all households in the *dusun*. Canvassers then sampled one participant in each household on arrival using simple random sampling from a full list of eligible household members made in consultation with a household member. The random assignment of participants to treatment was done in advance by the author. Random assignment was blocked at the village-level so that, of the 20 participants per village, five were randomly assigned to each of the four experimental conditions.

A number of steps were also taken to minimize design and timing effects. To reduce effects

¹⁹Fiscal illusion is often a raised as concern with respect to the link between taxation and accountability; complex systems of fiscal federalism weaken the mapping from taxes to spending and thus making it harder for citizens to know where to place blame (McCulloch, 1975 (original edition 1845; Buchanan, 1967; Buchanan and Wagner, 1977; Dollery and Worthington, 1996; Herb, 2003).

associated with variation in canvasser abilities, assignment to treatment in each village was blocked on individual canvassers so that each canvasser conducted two treatment and two control campaign versions per village. Quotas were also set so that gender balance across experimental conditions and canvassers was achieved. Finally, to minimize timing effects, villages were divided geographically into three groups and the order in which implementation teams visited villages was randomized. Random assignment was implemented by providing canvassers with a schedule for each village that matched a randomly selected household to an experimental condition and informed canvassers of whether they should sample a male or female participant in that household.²⁰

2.4.3 Data

Data comes from two sources: Participation in a postcard campaign and an original survey. Survey data was collected by canvassers during the same visit. Upon consent, canvassers implemented a pre-treatment module that inquired into individual and household characteristics, such as demographics, public goods usage, political participation, and experience with taxation. Additionally, post-treatment modules were implemented immediately following both the revenue and information experiments to provide measures of the main outcomes as well as of underlying attitudinal change. The survey was identical for all participants, regardless of their treatment assignment.

The well-known concern with surveys is that they do not provide reliable measures of real political behavior because responses are costless and subject to social desirability bias. I therefore use participation in a postcard campaign as a revealed preference measure of political behavior. At the end of the campaign, canvassers gave all participants the opportunity to return postcards. The postcard asked participants to indicate whether they were “satisfied with the district government in Bora and don’t want to change anything about how it works” (a reward for good performance) or whether they “want the district government in Bora to do a better job” (a sanction for bad performance).²¹ To crystallize the perceived benefits of taking action, participants were informed

²⁰A more detailed description of the sampling and randomization procedure is available in the design memo, available on the author’s website. The final sample consists of 1,863 individuals, including the original goal 1,860 plus three additional participants randomly sampled in a small number of villages with non-compliance.

²¹If they selected the latter, they were also asked to indicate their level of support for five different governance reforms

that the results of the postcard campaign would be shared with candidates in the lead up to the district head elections to encourage them to respond to public opinion.²²

Returning the postcard intentionally entailed a small cost. Participants had up to 24 hours to deposit their postcards in specially designated mailboxes located in their community. On average, participants lived about a 10 minute walk from the mailbox location. This small cost in terms of time and energy is akin to various types of low cost political action, like signing a petition or contacting an official. The postcard campaign also resembles the voting decision in that participants had to decide both whether to turnout and whether to use their postcards to reward or punish the incumbent government. Table 2.1 presents descriptive statistics on the main outcome measures from both the survey and postcard campaign.

2.4.4 Randomization check

Evidence from a one-way anova of baseline survey data shows that, as expected, randomization produced a balance in pre-treatment covariates across the four experimental conditions (Table 2.2, Panel A). The data in Panel B also provides a picture of participants' previous experience with taxation in the district. Overall, 93 percent of all participants reported that their household had paid at least one tax in the previous 12 months.²³ Of those, 62 percent reported that they had personal experience with paying a tax. Participants were also familiar with the idea of taxes going to the local government; about 67 percent said they thought tax payments primarily go to the district. The fact that citizens in Blora have some experience with taxation plausibly facilitated the priming effect of the tax treatment.²⁴ As can be seen in Panel C, participants also generally had a low level of political knowledge; only nine percent had heard something about the work of the

promoted by PATTIRO and LPAW, and to indicate their priority reform. I do not analyze the outcomes for each reform here since those were primarily intended for the partner organizations.

²²This was accomplished in a subsequent voter education experiment, results forthcoming.

²³Over 90 percent of all households report paying streetlight tax (Rp. 46,000 annually) and property tax (*pajak bumi dan bangunan*, or PBB, Rp. 34,000 annually). The biggest single tax, paid by 57 percent of the population, is the vehicle tax at almost Rp. 260,000 annually. Only three percent of households pay income tax, with an average annual payment of Rp. 1.2 million.

²⁴Tellingly, when asked during the pilot how they felt about paying the tax in the campaign, many participants responded 'same as always' (*biasa saja*).

	Range		<i>mean</i>	<i>SD</i>	Confidence Interval		<i>n</i>
	<i>min</i>	<i>max</i>			<i>Lower</i>	<i>Upper</i>	
Panel A: Monitoring							
Willing to learn more about budget ^a (%)	0	1	.79	.01	.77	.80	1,863
Willing to learn more about govt. ^b (%)	0	1	.78	.01	.76	.80	1,862
Scrutinize budget ^c (%)			.91	.01	.89	.92	1,858
Panel B: Participation							
Political action ^d (mean of 5)	0	5	1.32	.02	1.27	1.36	1,863
Participation (postcard campaign) ^e (%)	0	1	.78	.01	.77	.80	1,863
Panel C: Incumbent Sanctioning							
Plan to vote for incumbent ^f (%)	0	1	.53	.02	.48	.57	458
Plan to vote for former leg. Chairman ^g (%)	0	1	.09	.01	.07	.12	458
Net sanctioning (postcard campaign) ^h (%)	-1	1	.75	.01	.73	.77	1,857
Panel D: Perceptions of Government							
Performance worse than expected ⁱ (%)	0	1	.49	.01	.46	.52	1,851
Panel E: Aspects of Taxation							
Gamble to win more ^j (%)	0	1	.49	.01	.48	.51	1,853
Gamble to avoid loss ^k (%)	0	1	.18	.01	.17	.19	1,848
Posteriors on taxes/total revenue ^l (%)	0	1	.24	.00	.23	.25	1,836
Posteriors on windfalls/total revenue ^l (%)	0	1	.63	.01	.62	.64	1,842
Changed opinion about district gov't. ^m (%)	-1	1	.67	.01	.65	.69	1,850
Dissatisfaction with government ⁿ (%)	0	1	.67	.01	.65	.69	1,845
Distrust district head ^o (%)	0	1	.55	.01	.53	.57	1,853
Citizen ownership over budget ^p (%)	0	1	.78	.01	.77	.80	1,830
Relevance of budget to daily life ^q (%)	0	1	.86	.01	.85	.88	1,850
Citizens have power ^r (%)	0	1	.77	.01	.75	.79	1,853
Appropriate to criticize ^s (%)	0	1	.87	.01	.85	.88	1,858

Note: Table reports the summary statistics for the main dependent variables from both the survey and the postcard campaign.

^aHow interested are you in learning more about how the district government spends money in the budget? (interested=1)

^bHow interested are you in learning more about what the government of Blora is doing? (interested=1)

^cYou should pay more attention to what the district government does. (agree=1)

^dRegarding a problem or an issue that was affecting your daily life of your community, would you in the future [contact a village/subdistrict official, contact the district head, contact a local legislator, contact the media or an NGO, take part in a demonstration]? (average response of 5)

^eReturned postcard=1

^fSupport for the [incumbent district head=1/former legislative chairman=1].

^gTurned out and sanction=1, turnout and reward=-1, abstain=0

^hElected leaders in the district government are doing a [worse job (=1)/better job (=1)/the same job (=0)] than you thought they were?

ⁱImagine there are two envelopes—inside one envelope there is 0 rupiah and inside the other envelope there is 4,000 rupiah...Would you rather receive [...] for certain or would you rather take the risk between obtaining 0 or 4,000 rupiah? (recorded amount for certain as a percentage of Rp. 4,000).

^jLet's say there's a chance that you will have to spend 4,000 rupiah from your income...You could, however, pay some amount for sure to avoid the possibility it will break. Would you rather pay [...] for sure or take the risk? (recorded amount they would rather take the risk as a percentage of Rp. 4,000).

^kFor every 10,000 rupiah the district government gets in income, how much do you think comes from taxpayers in Blora?

^l...comes from the central government and oil and gas?

^mWould you say the information you just heard changed your opinions about district government? (yes=1)

ⁿHow satisfied are you with how the district government manages the budget? (dissatisfied=1)

^oHow much do you trust the district head to do the right thing for people in Blora? (distrust=1)

^pArg A: The money in the budget belongs to citizens in Blora vs Arg B: The money in the budget belongs to the district government (agree with A=1).

^qThe budget is relevant to your daily life (agree=1).

^rPeople have power to get the district government to do what they want (agree=1).

^sIt is appropriate for you to criticize government (agree=1).

Table 2.1: Summary Statistics

	Range		Sample Mean	Treatment Groups				Fk-1, n-1	P-value oneway ANOVA	n
	min	max		1 <i>Windfall, no info</i>	2 <i>Tax, no info</i>	3 <i>Windfall, info</i>	4 <i>Tax, info</i>			
Panel A: Demographics										
Age	17	65	41.7	41.7	41.7	41.8	41.5	0.06	.980	1,818
Female (%)	0	1	50	51	50	50	50	0.04	.989	1,863
Muslim (%)	0	1	99	99	99	100	99	0.86	.460	1,863
Javanese (%)	0	1	100	100	100	100	100	0.33	.802	1,862
Married (%)	0	1	90	89	90	92	91	0.86	.461	1,862
Completed at least primary school	0	1	72	71	74	73	70	0.78	.506	1,862
Can read a newspaper (%)	0	1	81	80	84	82	79	1.18	.315	1,861
Numeracy quiz score ^a	0	5	1.9	1.9	1.9	1.9	1.9	0.03	.993	1,863
Employed Full-time (%)	0	1	64	66	62	64	63	0.62	.604	1,863
Working in agriculture (%)	0	1	70	71	68	69	71	0.45	.716	1,477
Panel B: Tax Experience										
Paid a tax in previous 12 months (%)	0	1	93	93	92	95	94	1.13	.335	1,863
Personal experience paying a tax (%)	0	1	62	64	61	63	60	0.76	.514	1,824
Believe taxes go to district government (%)	0	1	67	65	70	66	67	0.86	.459	1,608
Panel C: Political Knowledge										
Political awareness quiz score ^b	0	5	1.9	1.9	1.9	1.9	1.9	0.11	.956	1,863
Know about district head ^c (%)	0	1	9	9	9	10	9	0.17	.918	1,847

Notes: Table reports results from a randomization check using a oneway ANOVA across the four experimental conditions.

^aMean number of answers correct (out of 5) from a math quiz in the survey.

^bMean number of answers correct (out of 5) from a political awareness quiz in the survey.

^cHow much would you say you've heard about the work of the district head over the last 12 months? (not much/nothing=1, some/a lot=0).

Table 2.2: Randomization Check and Baseline Data

district head in the previous 12 months. This supports the notion that the information provided in the second experiment would be new to many participants.

2.5 Main Results

2.5.1 Estimation and hypothesis testing

Treatment effects and hypotheses tests presented here employ design-based inference. Let Z^{rt} denote treatment assignment, where the superscript $r \in \{0, 1\}$ indicates assignment to the tax treatment when $r = 1$ or the windfall treatment when $r = 0$. Similarly, $t \in \{0, 1\}$ indicates assignment to the information treatment when $t = 1$ or to the control when $t = 0$. Since the experiments were cross-cutting, there were four possible treatment assignments: $Z^{rt} \in \{Z^{00}, Z^{10}, Z^{01}, Z^{11}\}$. We are interested in estimating treatment effects across several pairings of these groups so to generalize

the superscripts can be dropped. Let $Z \in \{0, 1\}$ where $Z = 1$ refers to a group designated to be the treatment group in a particular pairing and $Z = 0$ the control. There are N individuals and $0 < M < N$ are randomly assigned to treatment. Using potential outcomes notation, Y_i^1 then denotes the outcome for individual i assigned to treatment and Y_i^0 the outcome an individual in the control. In an experiment an unbiased estimate of the average treatment effect (τ) can be obtained simply by taking the difference in means outcomes for the treatment and control groups (Neyman, 1923; Rubin, 1974):

$$\tau = E[Y^1 - Y^0] = \frac{1}{M} \sum_{i=1}^M Y_i^1 - \frac{1}{N - M} \sum_{i=M+1}^N Y_i^0$$

Note, the estimate of the average treatment effect used here is based on assignment to treatment (not actual treatment status). There were only seven participants who did not receive the treatment version to which they were initially assigned (which is often called ‘non-compliance’), in all cases due to canvasser error. Using treatment assignment is conservative in the presence of such non-compliance since it makes it more difficult to identify treatment effects by mixing the treatment and control groups.²⁵

For the variance of the estimate of this average treatment effect, I use standard errors first developed by Neyman (1923). This approach is conservative (produces bigger standard errors) because it assumes that the covariance between potential outcomes at the individual level (which cannot be measured directly) is zero. I also provide ‘randomization inference’ p-values from a Fisher exact test (Fisher, 1935; Rosenbaum, 2002). I implement the test by comparing the probability of observing a treatment effect as big as the true effect (produced by the actual treatment assignment) to the randomization distribution of treatment effects under the null, generated by reproducing the random assignment in 10,000 simulations.²⁶

²⁵Average treatment effects for only those who complied with their initial treatment assignment, obtained by using the treatment assignment indicator as an instrument for actual treatment status, are available from the author upon request.

²⁶In a regression approach, when random assignment is blocked it is common to cluster standard errors at the level at which blocking occurred. Clustering at the village-level in this case produced standard errors that are *smaller*, which is an indication of negative intra-cluster correlation in the treatment groups. An advantage of randomization inference p-values is that they are non-parametric and do not depend on the clustered nature of the data. No

2.5.2 Monitoring

I first investigate the effects of revenue on participants' willingness to monitor government and become politically informed (H1), proxied by three survey questions reported in Table 2.3. The table layout mirrors the predictions found in Table 2.1. Each panel reports the means of the windfall and tax groups overall (row one), in the low information environment (row two), and the high information environment (row three). In this section, I focus on identifying whether taxes produce more political action than windfalls, captured by the predicted positive difference between the tax and windfall groups reported in the final column of each panel. The subsequent section revisits these tables to evaluate support for the fourth hypothesis by comparing the differences in the impact of information on political action in the windfall and tax groups as revealed in the final row of each panel.

The first two questions in Table 2.3 inquire directly into how willing participants are to learn more about how the district government manages the budget (Panel A) as well as what the district government is doing in general (Panel B). Taxes have a positive effect on willingness to monitor both the budget and government more broadly, and the effect pertaining to budget scrutiny is clearly significant. Overall, the tax group was five percentage points more willing to monitor the budget than the windfall group. The four percentage point difference is almost significant at the 95 percent confidence level for the low information group (p -value=.065) and is a significant six percentage points higher in the high information group (p =.017) (see also Panel A of Figure 2.5).²⁷ Substantively, the magnitude of the treatment effect is about half the size of the effect of having at least some primary school education; those who had completed at least primary school were 11 percentage points more willing to monitor the budget. While the response to a more abstract question asking participants whether they should “pay more attention” to the district government provides no evidence of treatment effects (Panel C), the first two measures support the prediction

modelling assumptions are needed to calculate the randomization distribution of the test statistic; the validity of the test depends on the randomization alone (Imbens and Rubin, 2009).

²⁷I also confirm that this result is driven by a shift *upwards* in the tax group vis-a-vis the windfall group rather than a shift *downwards* of the windfall group. In the pre-treatment survey, 71 percent of all participants wanted to learn more about the budget; the treatment increased willingness to monitor in both the windfall and tax groups but more so in the latter.

	Windfall (C1)	Tax (T1)	Diff (T1-C1)
Panel A: Willingness to monitor the budget^a			
All (Tax)	.76	.81	.05***
<i>(n)/(s.e.)</i>	(932)	(931)	(.02)
<i>RI p-value</i>			.003
Low Info (C2)	.73	.77	.04*
<i>(n)/(s.e.)</i>	(466)	(465)	(.03)
<i>RI p-value</i>			.065
High Info (T2)	.79	.85	.06**
<i>(n)/(s.e.)</i>	(466)	(466)	(.02)
<i>RI p-value</i>			.017
Diff (T2-C2)	.07**	.08***	.01
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.006	.001	.710
Panel B: Willingness to monitor government^b			
All (Tax)	.76	.79	.03*
<i>(n)/(s.e.)</i>	(931)	(931)	(.02)
<i>RI p-value</i>			.086
Low Info (C2)	.73	.75	.02
<i>(n)/(s.e.)</i>	(466)	(465)	(.03)
<i>RI p-value</i>			.363
High Info (T2)	.80	.84	.04
<i>(n)/(s.e.)</i>	(465)	(466)	(.03)
<i>RI p-value</i>			.136
Diff (T2-C2)	.07***	.09***	.01
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.003	.001	.713
Panel C: Pay more attention to government^c			
All (Tax)	.91	.90	-.01
<i>(n)/(s.e.)</i>	(928)	(930)	(.01)
<i>RI p-value</i>			.707
Low Info (C2)	.91	.91	.00
<i>(n)/(s.e.)</i>	(465)	(464)	(.02)
<i>RI p-value</i>			.970
High Info (T2)	.91	.90	-.01
<i>(n)/(s.e.)</i>	(463)	(466)	(.02)
<i>RI p-value</i>			.581
Diff (T2-C2)	.01	.00	-.01
<i>(s.e.)</i>	(.02)	(.02)	(.03)
<i>RI p-value</i>	.670	.938	.711

Table reports sample means and treatment effects over sample size or standard errors in parentheses. Randomization inference p-values from a Fisher exact test reported below and * $p < .10$, ** $p < .05$, and *** $p < .01$ based on the RI p-values. The intersection of the 'Diff' columns is the difference-in-difference (interaction) effect of the treatments.

^aHow interested are you in learning more about how the district government spends money in the budget? (*interested=1*)

^bHow interested are you in learning more about what the government of Blora is doing? (*interested=1*)

^cYou should pay more attention to what the district government does. (*agree=1*)

Table 2.3: Willingness to Monitor

that taxation creates a greater demand for information on government.

2.5.3 Participation

I next investigate the impact of windfalls and taxes on participants' willingness to take overt political action (H2) using data from both the survey and postcard campaign, with results presented in Table 2.4. To assess whether the treatments made participants more willing to take action to address issues affecting their lives or their communities, the survey asked about five types of non-electoral political behavior. These included contacting a village or subdistrict official, the district head, a local legislator or the media, or taking part in a demonstration. The results reported in Panel A of Table 2.4 show no evidence that taxation had an impact on plans to take more overt political action, either directly or in response to updated beliefs about government spending. This pattern also holds when analyzing the types of action individually. The survey also asked about plans to vote in the upcoming district head elections but the measure is problematic in that an unrealistically high 99 percent in each group said they planned to vote in the elections (not shown). This reflects the difficulty of relying on costless self-reported measures of political behavior when social desirability bias is likely high.

Participation in the postcard campaign, as a revealed preference, provides a better measure of willingness to take political action. As can be seen in Panel B of Table 2.4, however, taxation did not cause more participants to exert effort to return their postcards; while 78 percent of all participants returned their postcards, there is no detectable variation in return rates across experimental conditions (see also Panel B of Figure 2.5. As an actual behavioral measure, returning the postcard entailed a small cost. Such costs have been identified as an obstacle to participation (Riker and Ordeshook, 1968). While these costs are balanced across groups pre-treatment (and thus could not explain differences in outcomes across groups) it is possible to analyze whether there was variation across groups in willingness to accept these costs.

In both the overall data and in the low information environment, the effect of the tax treatment on willingness to return the postcard did not depend on participants' distance from the mailbox, as shown in Panels A-B of Figure 2.4. Yet, as can be seen in Panel C, in the high information

	Windfall (C1)	Tax (T1)	Diff (T1-C1)
Panel A: Willingness to take political action^a			
All (Tax)	1.29	1.34	.05
<i>(n)/(s.e.)</i>	(932)	(931)	(.05)
<i>RI p-value</i>			.164
Low Info (C2)	1.26	1.33	.07
<i>(n)/(s.e.)</i>	(466)	(465)	(.06)
<i>RI p-value</i>			.190
High Info (T2)	1.32	1.35	.03
<i>(n)/(s.e.)</i>	(466)	(466)	(.07)
<i>RI p-value</i>			.529
Diff (T2-C2)	.06	.03	-.03
<i>(s.e.)</i>	(.06)	(.07)	(.09)
<i>RI p-value</i>	.249	.624	.631
Panel B: Participated in postcard campaign^b			
All (Tax)	.79	.78	.00
<i>(n)/(s.e.)</i>	(932)	(931)	(.02)
<i>RI p-value</i>			.883
Low Info (C2)	.77	.78	.01
<i>(n)/(s.e.)</i>	(466)	(465)	(.03)
<i>RI p-value</i>			.633
High Info (T2)	.80	.79	-.02
<i>(n)/(s.e.)</i>	(466)	(466)	(.03)
<i>RI p-value</i>			.528
Diff (T2-C2)	.03	.00	-.03
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.192	.830	.414

Table reports sample means and treatment effects over sample size or standard errors in parentheses. Randomization inference p-values from a Fisher exact test reported below and * $p < .10$, ** $p < .05$, and *** $p < .01$ based on the RI p-values. The intersection of the 'Diff' columns is the difference-in-difference (interaction) effect of the treatments.

^aRegarding a problem or an issue that was affecting your daily life of your community, would you in the future [contact a village/subdistrict official, contact the district head, contact a local legislator, contact the media or an NGO, take part in a demonstration] (average response of 5)

^bReturned postcard=1

Table 2.4: Political Participation

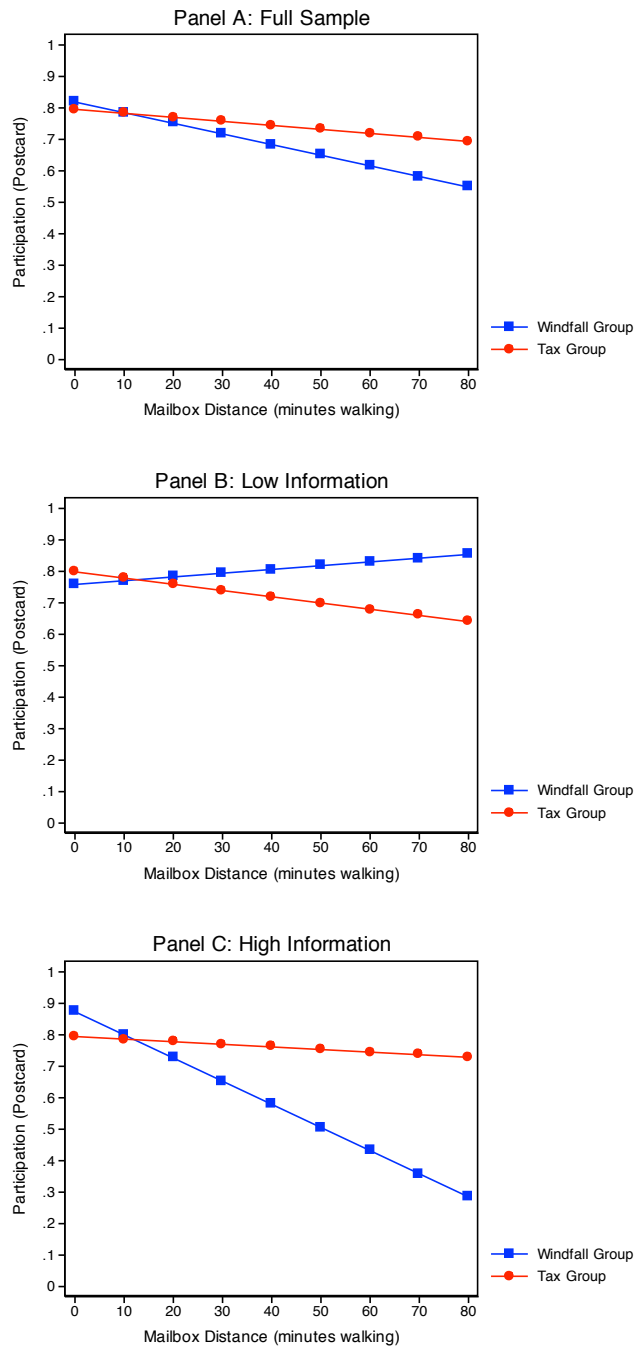


Figure 2.4: Costs of Participation. *The figures show the impact of the tax treatment on participating in the postcard campaign for the full sample (top), the low information group (middle), and the high information group (bottom). The interaction is positive and significant at $p=.052$ in the latter.*

environment—where participants were more dissatisfied with government performance and the benefits to participation were plausibly higher—there was a positive and significant interaction between the tax treatment and mailbox distance ($p=.052$). In other words, when participation costs were high, the tax group was more likely than the windfall group to return the postcard, which suggests some support for H2.

2.5.4 Incumbent sanctioning

I turn finally to examining the effect of windfalls and taxes on support for the incumbent government (H3). I use survey data on self-reported vote choice as well as the overall sanctioning effect of the postcard campaign, presented in Table 2.5. The public awareness campaign took place in the three months leading up to district head elections, which provided an opportunity to investigate the impact of the treatments on anticipated vote choice. The three candidates for district head included the incumbent, the former chairman of the district legislature, and a third challenger with ties to the bureaucracy and a popular muslim organization. Notably, the former legislative chairman was widely suspected of corruption at the time of the election, as described in Section 2.3. Participants were asked in the survey which candidate they most supported at that time, with the prediction that taxation makes citizens more likely to withdraw support from the incumbent (sanctioning).

While low statistical power makes it difficult to detect significant differences across groups, the signs on the treatment effects in Table 2.5 provide suggestive evidence that the tax treatment produced *higher* support for the incumbent district head (Panel A) and *lower* support for the former legislative chairman (Panel B).²⁸ One plausible explanation for this outcome is that participants attributed bad spending performance to both the incumbent district head and the former legislative chairman. In that case, the election would be a choice between two poorly performing incumbents and, given the suspicions of corruption surrounding the former legislative chair, participants opted

²⁸Low power is due to two reasons. First, candidates officially registered during the fourth week of implementation, so the questions on vote preferences were added to the survey only after the campaign had already been conducted with 400 participants (the highest possible n is 1485). (The order in which villages were visited was randomized, however, so the remaining sample still reflects a random sample of the population.) Second, the non-response rate on this question was high because it is a politically sensitive question.

	Windfall (C1)	Tax (C1)	Diff (T1-C1)
Panel A: Support for the Incumbent			
All (Tax)	.51	.55	.04
<i>(n)/(s.e.)</i>	(231)	(227)	(.05)
<i>RI p-value</i>			.466
Low Info (C2)	.48	.51	.04
<i>(n)/(s.e.)</i>	(115)	(109)	(.07)
<i>RI p-value</i>			.590
High Info (T2)	.54	.58	.03
<i>(n)/(s.e.)</i>	(116)	(118)	(.07)
<i>RI p-value</i>			.667
Diff (T2-C2)	.06	.06	.00
<i>(s.e.)</i>	(.07)	(.07)	(.09)
<i>RI p-value</i>	.339	.394	.980
Panel B: Support for Challenger			
All (Tax)	.11	.08	-.03
<i>(n)/(s.e.)</i>	(231)	(227)	(.03)
<i>RI p-value</i>			.342
Low Info (C2)	.11	.10	-.01
<i>(n)/(s.e.)</i>	(115)	(109)	(.04)
<i>RI p-value</i>			.758
High Info (T2)	.10	.06	-.04
<i>(n)/(s.e.)</i>	(116)	(118)	(.04)
<i>RI p-value</i>			.323
Diff (T2-C2)	-.01	-.04	-.03
<i>(s.e.)</i>	(.04)	(.04)	(.05)
<i>RI p-value</i>	.812	.326	.576
Panel C: Net sanctioning–Postcard Campaign			
All (Tax)	.74	.76	.01
<i>(n)/(s.e.)</i>	(928)	(929)	(.02)
<i>RI p-value</i>			.529
Low Info (C2)	.71	.77	.06**
<i>(n)/(s.e.)</i>	(465)	(464)	(.03)
<i>RI p-value</i>			.041
High Info (T2)	.78	.75	-.03
<i>(n)/(s.e.)</i>	(463)	(465)	(.03)
<i>RI p-value</i>			.253
Diff (T2-C2)	.07**	-.02	-.09**
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.014	.480	.023

Table reports sample means and treatment effects over sample size or standard errors in parentheses. Randomization inference p-values from a Fisher exact test reported below and * $p < .10$, ** $p < .05$, and *** $p < .01$ based on the RI p-values. The intersection of the ‘Diff’ columns is the difference-in-difference (interaction) effect of the treatments.

^aSupport for the incumbent district head (support=1)

^bSupport for the challenger (the former legislative chairman) (support=1)

^cTurned out and sanction=1, turnout and reward=-1, abstain=0

Table 2.5: Incumbent Support

for the “lesser of two evils.” There is no evidence that the treatments affected support for the other challenger (not shown), which could be because less was known about him at the time as the public awareness campaign was implemented before the start of the official political campaign period.

I corroborate these results by analyzing the postcard campaign as the main revealed preference measure of willingness to take action to demand better performance from government. To provide a composite ‘net sanctioning effect’ measure, I code all those who returned their postcard and signaled that they want government to do better as 1, those who returned their postcard and said they were satisfied with the status quo as -1, and those who abstained from participating as 0. As can be seen in Panel C of Table 2.5 and Figure 2.5, there is little evidence that taxation on average produced a greater impetus to sanction the incumbent than windfalls. Yet, the average effect obscures interesting variation across low and high information groups. While the tax treatment had no effect in the high information environment, taxpayers were six percentage points more likely than the windfall group to use their postcards to sanction the incumbent in the low information environment (p-value=.041). These results indicate support for H3, at least in the low information environment. They also point to an unexpected relationship between revenue and information, which I discuss more in the next section.

2.5.5 Summary of main results

The main results support H1, that windfalls and taxes create different incentives to become politically informed about government in general and the budget in particular. Evidence for H3 is also provided in showing (suggestively) that taxes and windfalls affected support for candidates in the district head elections and that taxes motivated participants in the low information environment to use their postcards to sanction the incumbent. There is less support for the hypothesis that different sources of government revenue create a differential willingness to participate in politics, as predicted by H2. One possible explanation for this pattern of results is that taxation increased the perceived benefits of restraining government for participants. Yet, the benefits of taking political action still have to exceed the costs and behavior like monitoring is less costly than more overt forms of political participation. The findings support this interpretation when the costs of partic-

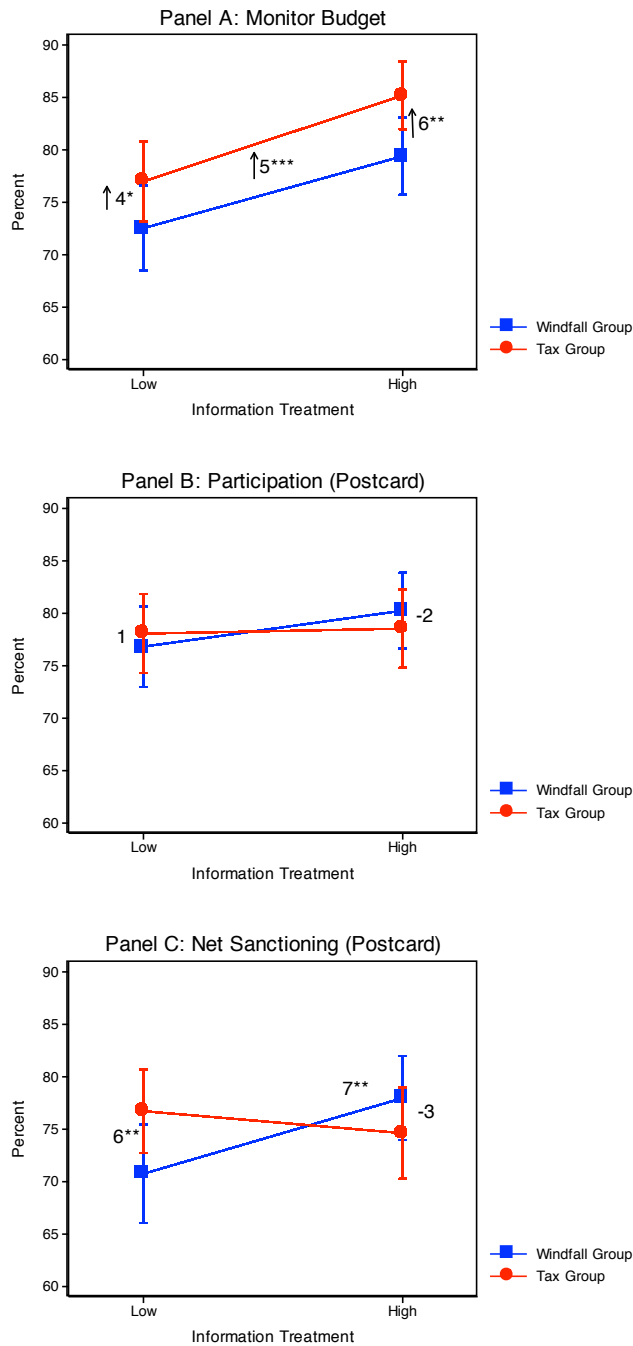


Figure 2.5: Main Results. *Figures show results for the effect of the tax and information treatments on willingness to monitor the budget (Panel A), participation in the postcard campaign (Panel B), and the net sanctioning effect of the postcard campaign (Panel C).*

ipation in the postcard campaign are taken into account: Participation in the tax group dropped off significantly less steeply than participation in the windfall group as participants' distance from the mailbox increased but only in an environment where participants were more disgruntled about government spending.

The findings also shed light on the relationship between taxation and information. In showing that taxation made participants willing to learn more about government, it supports the notion that taxation enhances demand for information. In the next section I investigate whether windfalls and taxes also condition how individuals process information on government spending.

2.6 Does Revenue Condition the Impact of Information on Political Action?

The fourth hypothesis predicts that the impact of information on political action depends on whether taxpayers finance public goods. To evaluate this hypothesis, we revisit the results presented above, but this time with a focus on the interaction between revenue and information. Specifically, I regard as support for this hypothesis evidence that information causes more monitoring, participation, and incumbent sanctioning in a tax than a windfall environment. This is denoted by a positive and significant interaction in the bottom right cells of each panel or a steeper increase in the slope of the tax line, as shown in Figure 2.5 for the main measures.

With respect to monitoring, Panels A and B of Table 2.3 show that, contrary to the prediction, the effect of information was approximately equivalent in both the windfall and tax groups. For instance, the bottom row in Panel A shows that better information caused a seven percentage point increase in willingness to monitor government in the windfall group and an eight percentage point increase in the tax group. The fact that these magnitudes are about the same is captured by the lack of a positive and significant interaction term. These results suggest that the relationship between information and taxation might be additive rather than interactive.

In contrast, the information had no effect on participation in the postcard campaign in either the windfall or tax groups, as reported in Panel B of Table 2.4. The fact that, in the high information

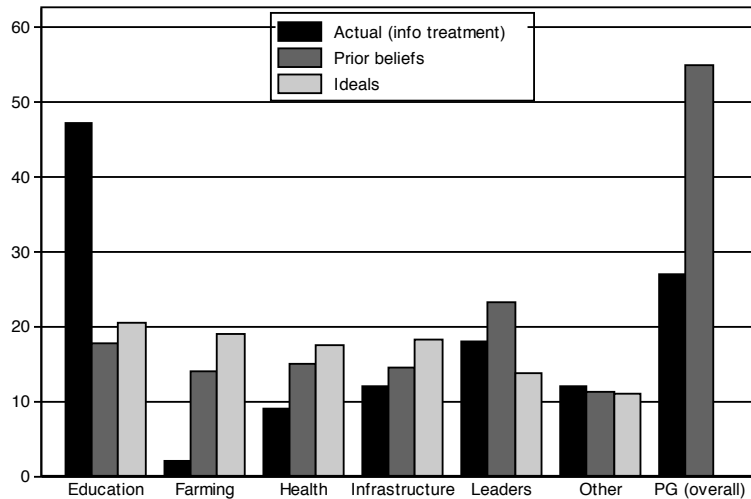


Figure 2.6: Spending Priors. *Figure compares actual spending (as revealed by the treatment) to prior ideals and beliefs about government spending across the six categories highlighted in the information campaign.*

environment, the tax group was more likely to return postcards than the windfall group at high costs of participation weakly suggests some support for H4. The results for the net sanctioning effect of the postcard campaign presented in Panel C of Table 2.5 show yet another pattern, however: A significant negative interaction between revenue and information. This occurs because, while information increased the net sanctioning effect of the postcard campaign by a significant seven percentage points in the windfall group, it had no detectable effect on the tax group. This indicates either a ceiling or a substitution effect in that participants were provoked to punish the incumbent either by taxation or by the information but there was little added value to having both. All in all, while the data shows three different patterns in how revenue and information relate, there is little evidence to support the main pattern of interest: That information provokes more political action when people pay taxes.²⁹

Before dismissing H4, however, it is important to check that the information treatment ‘worked’

²⁹It is difficult to shed additional light on which pattern is ‘true’ in the context of this experiment. On one hand, it is possible to theorize why taxation and information might be substitutes; this would be the case, for instance, if taxation primarily motivated people to become politically informed. In the experiment, participants were not given the opportunity to acquire their own information, however, making the results all the more puzzling. Future studies interested in shedding more light on the relationship between revenue and information should allow for downstream information acquisition.

	Windfall (C1)	Tax (C1)	Diff (T1-C1)
Panel A: District leaders doing a worse job than expected			
All (Tax)	.56	.57	.01
<i>(n)/(s.e.)</i>	(927)	(924)	(.02)
<i>RI p-value</i>			.508
Low Info (C2)	.34	.34	.01
<i>(n)/(s.e.)</i>	(463)	(458)	(.03)
<i>RI p-value</i>			.849
High Info (T2)	.77	.79	.02
<i>(n)/(s.e.)</i>	(464)	(466)	(.03)
<i>RI p-value</i>			.530
Diff (T2-C2)	.43***	.45***	.01
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.000	.000	.746
Panel B: Dissatisfaction with budget management			
All (Tax)	.68	.70	.01
<i>(n)/(s.e.)</i>	(915)	(917)	(.02)
<i>RI p-value</i>			.585
Low Info (C2)	.51	.53	.02
<i>(n)/(s.e.)</i>	(452)	(453)	(.03)
<i>RI p-value</i>			.529
High Info (T2)	.86	.86	.00
<i>(n)/(s.e.)</i>	(463)	(464)	(.02)
<i>RI p-value</i>			.873
Diff (T2-C2)	.35***	.33***	-.01
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.000	.000	.736
Panel C: Distrust the district head			
All (Tax)	.56	.55	-.01
<i>(n)/(s.e.)</i>	(926)	(927)	(.02)
<i>RI p-value</i>			.716
Low Info (C2)	.42	.39	-.03
<i>(n)/(s.e.)</i>	(464)	(464)	(.03)
<i>RI p-value</i>			.338
High Info (T2)	.69	.71	.01
<i>(n)/(s.e.)</i>	(462)	(463)	(.03)
<i>RI p-value</i>			.653
Diff (T2-C2)	.27***	.32***	.04
<i>(s.e.)</i>	(.03)	(.03)	(.04)
<i>RI p-value</i>	.000	.000	.319

Table reports sample means and treatment effects over sample size or standard errors in parentheses. Randomization inference p-values from a Fisher exact test reported below and * $p < .10$, ** $p < .05$, and *** $p < .01$ based on the RI p-values. The intersection of the 'Diff' columns is the difference-in-difference (interaction) effect of the treatments.

^aElected leaders in the district government are doing a worse job (=1)/better job or about the same job (=0) than you thought they were?

^bHow satisfied are you with the way the district government manages the budget? (dissatisfied=1)

^cHow much do you trust the district head to do the right thing for people in Blora? (distrust=1)

Table 2.6: The Effect of Information on Attitudes, Conditional on Revenue

in that it indeed shifted participants beliefs about government spending. I use data from a district budget exercise conducted with participants just prior to the information experiment to measure participants' initial ideals and beliefs about public spending. All participants were asked to use the 10,000 rupiah from the revenue experiment (and the district budget game-board) to illustrate how they would spend the funds if they were the decision-maker (ideals) and how they think government actually spends the funds (beliefs). Participants were also asked to estimate the share they thought government actually spent both on: (a) all programs for citizens, and (b) on the programs of greatest priority to the respondent. As can be seen in Figure 2.7, while actual spending on education was far greater than what participants wanted or expected, spending was lower than desired or expected for health, infrastructure and farming. In general, participants estimated that government spent about 55 percent of its revenue on public services and direct programs for them; the campaign revealed the true amount in 2008 to be 30 percent.

Table 2.6 confirms that what participants learned caused them to view the district government's performance in a substantially worse light. The information caused a 43 percentage point increase in the share of participants who felt government was doing worse than expected and a 45 percentage point increase in the tax group. Better information increased dissatisfaction with management of the budget by 34 percentage points (Panel B) and distrust in the district head by 30 percentage points (Panel C). Results of a similar pattern and magnitude apply for questions on dissatisfaction with the district government overall and distrust in local legislators.

All in all, the results in this section show that the new information indeed caused learning; namely, participants learned that government was performing worse than initially believed. Contrary to H4, however, both the windfall and tax groups were equally displeased. This strongly suggests that the incentive effect of taxation was not conditional on beliefs about government spending. If anything, the results point to the effects of information being as great, if not greater, in windfall contexts.³⁰ One important implication of these findings is that, while taxation motivates citizens to restrain government, there are also substantial dividends to improving access to information in

³⁰A caveat to this statement is that we might expect the effect of providing information in a tax environment to be different (higher or lower) if people also have the opportunity to become better informed on their own, which I did not allow for with this design.

windfall environments directly.

2.7 Why Does Taxation Motivate Action?

While the findings provide little support for H2 and H4, taxes increased both the willingness to monitor government (H1) and to use the postcard to sanction the incumbent government (H3). This section provides deeper insight into these results by using the survey data and features of the experimental design to shed light on why taxes motivated monitoring and sanctioning. As discussed in Section 2.4.1.3 and the appendix, the revenue and information experiments exogenously varied three theoretically-motivated aspects of taxation: The tax burden, the share of taxes in total revenue, and the ratio of taxes to spending (the ‘price’ of public goods). Of note, in finding no support for H4, I can also rule out the possibility that participants were driven to action by increases in the price of public goods induced by the experiment.³¹ I therefore focus instead on investigating how the tax treatment affected the first two aspects of taxation.

Panel A in Table 2.7 present measures designed to capture whether participants felt tax burden, or the ‘loss’ of their earned income associated with the tax payment itself. Out-of-pocket loss is difficult to measure directly so the survey employed two questions adapted from the procedure developed by Becker, DeGroot, and Marshcak to elicit risk tolerance for monetary gambles (Becker, DeGroot and Marschak, 1964). The main measure is willingness to gamble for a gain, where the tax group is expected to be more risk-seeking than the windfall group. This comes from the notion that a member of the tax group—who suffered the pain of an out-of-pocket loss of 4,000 rupiah from their endowment—should be more eager to take a risk to win that 4,000 rupiah back than a member of the windfall group would be to win an ‘additional’ 4,000 rupiah (Kahneman and Tversky, 1979).³² The results presented in the first row of Table 2.7 show, however, that the windfall and tax groups

³¹This is because, by experimental design, sensitivity to the price of public goods would also have manifested itself as a positive interaction between the revenue and information experiments.

³²The measures used here derive from the S-shaped value function in prospect theory, which kinked at the reference point (the origin) and more steeply sloped in the domain of losses than of gains. The assumption is that the tax shifted those in the tax group down the slope into the domain of losses while the windfall group remained at its reference point. This is consistent with the observation by Kahneman and Tversky (1979, 286) that: “An unexpected tax withdrawal from a monthly pay check is experienced as a loss, not as a reduced gain.”

display no difference in risk tolerance for a gamble to win additional funds.

I also employ a measure of willingness to gamble to avoid a loss. Prospect theory suggests a greater sensitivity to an initial loss of an endowment than to an additional deduction, which implies that the windfall group would be more risk-seeking than the tax group in a gamble to avoid a loss of 4,000 rupiah. The results in the second row are in the opposite direction than predicted and also substantively small.³³ Overall, while it is difficult to rule out that participants registered the tax burden induced by the experiment, the data provides little confirmatory support.

There is strong evidence, however, that the revenue experiment shifted the perceived share of tax revenue to total revenue in the district budget (Panel B). Row three shows that participants in the baseline on average estimated that 30 percent of total revenue comes from taxes, and that—as expected—there was no difference across groups. Following the tax treatment, the tax group updated its beliefs to estimate that about 38 percent came from taxes while the windfall group decreased its estimate to 10 percent, a significant 26 percentage point difference (row six). Similarly, the windfall group increased its perceived share of windfall revenue to about 80 percent, compared to nearly 46 percent in the tax group, a significant 34 percentage point difference (row seven).³⁴

This evidence of the tax treatment’s impact is reflected in turn in a clear change in attitudes towards the budget, as proxied by three survey questions presented in Panel C of Table 2.7. In general, the tax treatment caused a six percentage point increase in the share of respondents who claimed that taxation changed their opinion about the district government (row six). To measure specifically feelings of ownership over the district budget, one question asked participants whether they thought the money in the budget “belongs to citizens in Blora” or “belongs to the district government” (row seven). The tax group was four percentage points more likely to claim ownership

³³The fact that the sign is positive is consistent with a discussion in the literature that people have trouble integrating losses and that losses tend to accumulate; if so, the result could provide some support that participants actually registered the tax payment (Coombs and Avrunin, 1977; Thaler and Johnson, 1990).

³⁴While the campaign script highlighted that the ratio in the tax treatment was hypothetical to avoid deception, these results indicate that participants interpreted them as fact. The second step taken during the design stage to avoid deception was to use the real ratio for the windfall group and to fix the ratio for the tax group based on our best estimate on individuals’ prior beliefs. We estimated this to be 40 percent from focus group discussions, although, as can be seen here, the real priors were 30 percent on average. We were close but not exact and consequently there was some unintentional deception.

for citizens. Similarly, to measure affinity to the district budget, another question asked participants whether they agreed with the statement that “the budget is relevant to your daily life” (row eight). The difference between the tax and windfall groups is a positive and significant five percentage points for this measure.

There is no evidence, however, for other types of attitudinal change suggested in Section 2.2. One possibility is that people simply dislike paying taxes and taxation elicits anger and frustration towards the government. Rows nine and ten present measures of dissatisfaction and distrust in the district government, but there is no evidence of a significant difference between the windfall and tax groups. Given the strong indication that participants shifted the perceived share of taxes in total revenue, I also investigate whether the tax treatment made citizens feel more empowered vis-a-vis local government, associated with perceptions of the government’s fiscal dependence on society.³⁵ To assess this possibility, the survey asked participants whether they think citizens “have power to get the district government to do what they want” and whether they agree that “it is appropriate for citizens to criticize government.” Rows 11 and 12 of Panel B show no evidence of any difference in feelings of empowerment between the windfall and tax groups.

In sum, the data suggests that the incentive effect of taxation observed in the main results is most likely due not to the tax payment itself but rather to a shift in beliefs about the share of government revenue from taxation. This in turn resulted in a greater sense of perceived ownership over district government funds and an appreciation of the relevance of the budget to daily life. There is no support for the possibility that taxation evokes more negative feelings towards the government or that citizens felt more empowered vis-a-vis government as a result of the perceived increase in its fiscal dependence on society.

³⁵While the tax treatment did not bring about an objective change in citizens’ leverage over government, it nonetheless might have primed a stronger sense of efficacy or higher expectations of government responsiveness, associated with government’s fiscal dependence on citizens. The notion that a change in perceptions could matter comes from the role of opportunity structure in the literature on collective action and contentious politics. According to this view, collective action emerges as a response to changes in political constraints confronting groups (Tilly, 1978; McAdam and Zald, 1996; Tarrow, 1998). Kurzman (1996) has argued that what mattered to successful mobilization in the Iranian revolution was not an objective change in the opportunity structure but rather a perceived change.

		Sample	Windfall	Tax	Diff (T1-C1)			
		Mean	(C1)	(T1)	Diff	(se)	p-value	n
Panel A: Tax Burden								
1	Gamble for gain ^a (%)	.49	.49	.49	.00	(.02)	.939	1,853
2	Gamble to avoid loss ^b (%)	.18	.17	.19	.02*	(.01)	.061	1,848
Panel B: Taxes/Total Revenue								
3	Priors on taxes/total revenue ^c	.30	.31	.30	-.01	(.10)	.501	1,179
4	Posteriors on taxes/total revenue ^c	.24	.10	.38	.29***	(.57)	.000	1,836
5	Posteriors on windfalls/total revenue ^d	.63	.80	.46	-.34***	(.80)	.000	1,842
Panel C: Attitude change								
6	Changed opinion about district gov't. ^e	.71	.68	.75	.06***	(.02)	.000	1,857
7	Citizen ownership over budget ^f	.78	.77	.80	.04**	(.02)	.029	1,830
8	Relevance of budget to daily life ^g	.86	.84	.89	.05***	(.02)	.000	1,850
9	Dissatisfaction with government ^h	.69	.68	.70	.01	(.02)	.585	1,832
10	Distrust district head ⁱ	.55	.56	.55	-.01	(.02)	.716	1,853
11	Citizens have power ^j	.77	.78	.76	-.02	(.02)	.248	1,853
12	Appropriate to criticize ^k	.87	.86	.87	.02	(.02)	.304	1,858

The first three columns report the means for the full sample, the windfall group, and the tax group respectively. Columns 4-6 report the difference in means, standard errors, and randomization inference p-values, where * $p < .10$, ** $p < .05$, and *** $p < .01$. All data comes from survey measures taken immediately following the revenue experiment, except for rows 7 and 8, which were taken following the information experiment.

^aImagine there are two envelopes—inside one envelope there is 0 rupiah and inside the other envelope there is 4,000 rupiah...Would you rather receive [...] for certain or would you rather take the risk between obtaining 0 or 4,000 rupiah? (recorded amount for certain as a percentage of Rp. 4,000).

^bLet's say there's a chance that you will have to spend 4,000 rupiah from your income...You could, however, pay some amount for sure to avoid the possibility it will break. Would you rather pay [...] for sure or take the risk? (recorded amount they would rather take the risk as a percentage of Rp. 4,000).

^cFor every 10,000 rupiah the district government gets in income, how much do you think comes from taxpayers in Blora?

^d...comes from the central government and oil and gas?

^eWould you say the information you just heard changed your opinions about district government? (yes=1)

^fArg A: The money in the budget belongs to citizens in Blora vs Arg B: The money in the budget belongs to the district government (agree with A=1).

^gThe budget is relevant to your daily life (agree=1).

^hHow satisfied are you with how the district government manages the budget? (dissatisfied=1)

ⁱHow much do you trust the district head to do the right thing for people in Blora? (distrust=1)

^jPeople have power to get the district government to do what they want (agree=1).

^kIt is appropriate for you to criticize government (agree=1).

Table 2.7: Why Taxation Motivates Political Action

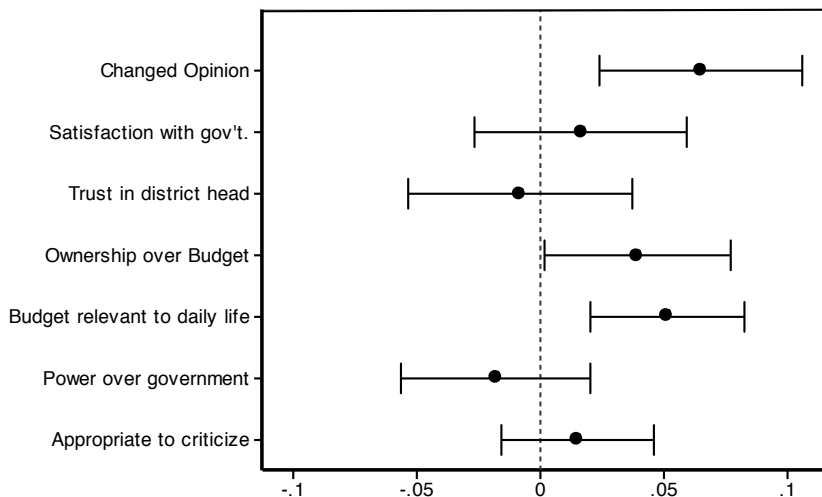


Figure 2.7: Why taxes motivate political action. *The figure presents results from Panel C of Table 2.7 graphically. The bars indicate the differences between the tax and windfall groups for survey measures of attitudes towards government (first two rows), attitudes towards the budget (second two rows), and perceived leverage over government (bottom two rows).*

2.8 Conclusion

Understanding when and why windfall revenue undermines development remains a central focus of political economy research. While a number of compelling explanations for the resource curse exist, knowledge of which mechanisms have explanatory power, under what conditions, and how they relate to one another remains elusive. This paper sought to address this challenge by conducting a test of the validity of a specific but prominent mechanism: That windfalls and taxes have a differential impact on citizens' motivation to take political action and hold politicians accountable. This paper provides possibly the first micro-level, causal evidence that taxes indeed motivate citizens to monitor government and sanction the incumbent more than windfalls. In finding less support for the prediction that taxation mobilizes political participation, the findings highlight the need to consider how taxation affects the perceived benefits of taking action vis-a-vis the costs.

The paper also provides insights into *why* taxes motivate political action. The literature has variously suggested—but offered little evidence to substantiate—whether citizens are motivated to

take action by their tax burdens, by the share of taxes in total revenue, or by the ratio of taxes to spending. The results indicate that higher levels of monitoring and sanctioning can be attributed to the fact that the revenue experiment caused participants to perceive a greater share of the total budget came from taxes. This was associated with a greater sense of ownership over district public funds and an appreciation of the relevance of the budget to their daily lives.

A further contribution of this paper is to shed light on the role played by revenue in the relationship between information and accountability. The evidence indicates that taxation increased demand for information on government, implying that taxation could help to explain how a more informed society comes about. There is no support for the prediction the hypothesis that revenue conditions how people *evaluate* information on government spending, however; revelations of misused funds did not elicit a greater response from taxpayers than from individuals in a windfall environment.

The findings presented here have important implications for understanding not only the causes of the resource curse but also policy options to mitigate it. For one, the paper lends support to policy approaches that favor requiring citizens to pay taxes or otherwise contribute directly to the provision of public goods. The heavy reliance on different types of windfall revenue in developing countries raises concerns about a resource curse emerging in a wide variety of contexts, from transfer-dependent local governments in newly decentralized democracies to villages receiving windfall aid through community-driven development initiatives (Mansuri and Rao, 2004). The findings here provide some support for policies that seek to strengthen citizen contributions to funding public goods to improve both monitoring and accountability.

The results also suggest that there are direct gains to improving expenditure transparency and helping citizens become better informed about how politicians are using public funds, especially in windfall environments. This paper was motivated in part by the concern that citizens are more permissive of poor spending performance when government is financed by windfalls instead of taxes. Yet, this does not appear to be the case: The impact of providing participants with negative information on government spending was as great, if not greater, in windfall than in tax contexts. While much of the focus on transparency in natural resource rich places has centered on

improving revenue transparency through initiatives such as the Extractive Industries Transparency Initiative (EITI), the results here help to demonstrate the importance of elevating the emphasis on expenditure transparency (Kolstad and Wiig, 2008).

The findings also point to avenues for future research. Additional research is needed to investigate more closely the effects on political behavior of changes to the tax burden, the share of taxes in total revenue, and the price of public goods. While it was beyond the scope of this experiment to exogenously change each of these quantities individually and with sufficient variation, the approach taken here points to the benefits of clarifying further the micro-foundations underpinning the incentive effect of taxation. Further, while this paper simulated a lump-sum personal income tax, additional research should focus on identifying causally the impact of other types of direct and indirect taxes—including value-added taxation, a leading source of tax revenue in developing countries—on political behavior.

Finally, this paper highlights the need for additional research identifying the causal effect of taxes and windfalls on the political behavior of politicians and the quality of government. While this paper shows that taxation gives citizens stronger incentives to restrain government, evidence that it likewise gives politicians the incentives to make government more transparent, responsive and efficient is still needed. Only through such future research will the full effects of windfalls and taxes on motivating citizens to become more vocal and active keepers of the public purse be understood.

2.9 Appendix to Paper: Experimental Design

To shed light on *why* taxation produces incentives to take political action, the tax treatment exogenously varied three distinct and theoretically-motivated aspects of taxation. The first aspect was the tax payment itself: Members of the tax group faced a 29 percent tax rate on their earned income from the campaign, which implies that they also had a higher tax burden than members of the windfall group (defined as the ratio of taxes paid to income, see Panel A).

The experimental design strove for realism in simulating this tax payment. In particular, the tax treatment imitated the core components of an actual payment, defined as a mandated *transfer* from an individual's *income* to the *government* at an exogenous rate. To elicit the sense of an income tax payment, participants had to feel that they were paying income they had *earned*.³⁶ Canvassers emphasized at the outset that the payment was in exchange for the two hours of the participants' time that the campaign would take; the household budget exercise was intended to deepen further participants' sense of ownership over the payment. In this context, the role of the canvassers in transferring the income was akin to employer income tax withholding. An additional advantage was that tax compliance was held constant at 100 percent. A final benefit was that, by simulating a tax payment rather than simply priming previous tax experience, the treatment was designed to resonate with participants regardless of whether they had previous real-world experience with taxation. This was important because it was believed at the outset that few citizens in Blora had substantial exposure to taxation.

The second difference was that the tax and windfall treatments altered the relative perceived share of total revenue from taxes in the budget (Panel B). In particular, the tax group learned that 40 percent of total revenue came from taxes. In contrast, the windfall group was informed that 80 percent of the budget comes from windfalls, which implies that up to 20 percent could come from taxes, depending on participants' beliefs about other sources of revenue. To avoid deception, the share of taxes to total revenue in the windfall treatment approximated reality; the share in the tax treatment was calibrated to reflect participant priors based on estimates obtained during piloting

³⁶According to Kahneman and Tversky (1979, 286): "An unexpected tax withdrawal from a monthly pay check is experienced as a loss, not as a reduced gain."

and focus groups. Participants in both groups were also informed that the revenue composition of the budget was hypothetical and illustrative only.

Finally, the experiments jointly affected the ‘price’ of public goods, defined here as the ratio of tax revenue to spending on direct programs and services for citizens in the budget. As Ross (2004, 247) notes, “A rise in the price of government services can either take the form of a rise in taxes for a constant set of government services, or a constant level of taxes with a cut in government services.” While the tax experiment altered the numerator of that ratio as just described, the information experiment altered the denominator. Specifically, the spending experiment informed participants that 30 percent of the budget was spent on citizens, whereas beliefs in the low information group are participants’ priors (Panel C). Panel D shows how the ‘price’ of public goods changes across the experimental conditions. When true spending is *less* than prior beliefs on average, it is straightforward to see how the price of public goods rose across experimental conditions. Moreover, the ratio of taxing to spending only exceeded parity for informed taxpayers.³⁷ If citizens are indeed mobilized to hold government accountable not just as prices rise but only when taxes *exceed* spending, informed taxpayers should exhibit the highest responses. The experimental design is therefore consistent with the original hypotheses.

³⁷Given that average priors on government spending were 30 percent, the mean price of public goods was \$.43 for the windfall group and \$.83 for the tax group in the low information environment. By experimental design, the price of public goods in the high information environment was set to be \$.67 for the windfall group and \$1.33 for the tax group.

(A) Tax Payment/Burden (Rp.)			(C) Spending/Total Revenue (%)		
	Windfall	Tax		Windfall	Tax
Low info	$\frac{0}{10,000}$	$\frac{4,000}{14,000}$	Low info	$\frac{\text{priors}}{100}$	$\frac{\text{priors}}{100}$
High info	$\frac{0}{10,000}$	$\frac{4,000}{14,000}$	High info	$\frac{30}{100}$	$\frac{30}{100}$

(B) Taxes/Total Revenue (%)			(D) Price of Public Goods (%)		
	Windfall	Taxes		Windfall	Taxes
Low info	$\frac{\leq 20}{100}$	$\frac{40}{100}$	Low info	$\frac{\leq 20}{\text{priors}}$	$\frac{40}{\text{priors}}$
High info	$\frac{\leq 20}{100}$	$\frac{40}{100}$	High info	$\frac{\leq 20}{30}$	$\frac{40}{30}$

Notes: The panels present details of the three aspects of taxation varied exogenously by the experiments. Panel A shows the tax burden (the ratio of taxes paid to income earned in the experiment). Panel B depicts the shift in the perceived share of taxes in total revenue. Panel C shows the effect of the information treatment, where the low information group kept its initial beliefs about government spending while the high information group learned that government spent 30 percent of total revenue on citizens. Panel D is the price of public goods, obtained by putting the numerator in Panel B over the numerator in Panel C.

Table 2.8: Detailed Summary of the Treatments

2.10 Online Appendix

This online appendix contains supplementary results, discussion, and robustness checks for the main paper. The appendix is organized roughly in the order in which additional analysis is referred to in the paper.

2.10.1 Does the effect of taxation depend on satisfaction?

Section 2.2 in the main paper discusses the alternate prediction that the effect of taxes on incumbent support is conditional on satisfaction with government performance. In other words, higher taxes (holding spending constant) will not necessarily lead to less incumbent support but rather to more rewarding of incumbents for those who are satisfied with government performance.

To investigate support for this alternate prediction, I examine how the effect of the tax treatment on four of the main outcomes of interest varies depending on a pre-treatment measure of satisfaction with government performance. Satisfaction was measured in the survey by the question: “How satisfied are you with the way the district government in Blora is doing its job overall?” where dissatisfied was coded 1 and satisfied coded 0.

Table 2.9 shows that the net sanctioning effect of the postcard campaign is the only outcome that varies based on satisfaction. Yet, the significant negative interaction in the full sample and in the low information environment indicate that the tax treatment caused more sanctioning among those who were initially satisfied with district government performance. This result is also shown in Figure 2.8, which shows that on average, and particularly in the low information group, the tax treatment caused a steeper increase in net sanctioning among those who were originally satisfied. In sum, there is little support for the alternate prediction that paying taxes makes people reward incumbents, even when pleased with their performance.

	Performance Worse ^a			Monitor Budget ^b			Participation (Postcard) ^c		Sanctioning (Postcard) ^d			
Panel A: Tax Treatment (Overall)												
Tax	.04 (.03)			.05* (.03)			.03 (.03)		.07** (.03)			
Dissatisfaction	.29*** (.03)			.03 (.03)			.02 (.03)		.08** (.03)			
Tax*dissatisfaction	-.05 (.05)			-.01 (.04)			-.07* (.04)		-.11** (.05)			
Panel B: Tax Treatment (Low Info)												
Tax	.05 (.04)			.03 (.04)			.05 (.04)		.15*** (.05)			
Dissatisfaction	.41*** (.04)			-.01 (.04)			.04 (.04)		.14*** (.05)			
Tax*dissatisfaction	-.08 (.06)			.03 (.06)			-.08 (.06)		-.17** (.07)			
Panel C: Tax Treatment (High Info)												
Tax	.02 (.04)			.08** (.04)			.01 (.04)		-.01 (.04)			
Dissatisfaction	.19*** (.04)			.07* (.04)			.01 (.04)		.02 (.04)			
Tax*dissatisfaction	.00 (.05)			-.03 (.05)			-.05 (.05)		-.05 (.06)			
Constant	.41	.13	.67	.75	.74	.76	.77	.74	.79	.70	.63	.77
n	1,758	874	884	1,768	883	885	1,768	883	885	1,762	881	881

Notes: Table reports results from separate regressions of the form $Y_{ij} = \alpha + \beta_1 T_{ij} + \beta_2 X_{ij} + \beta_3 (T_{ij} * X_{ij}) + \epsilon_{ij}$ where T_{ij} is the treatment indicator (tax group=1) and X_{ij} is the pre-treatment covariate of interest. Neyman standard errors are reported in parentheses, where * $p < .10$, ** $p < .05$, and *** $p < .01$. Dissatisfaction was measured pre-treatment in the survey with the question: “How satisfied are you with the way the district government in Blora is doing its job overall?” where dissatisfied was coded 1 and satisfied coded 0.

^a Elected leaders in the district government are doing a worse job (=1)/better job (=0) than you thought they were?

^b How interested are you in learning more about how the district government spends money in the budget? (interested=1)

^c Returned postcard=1

^d Turned out and sanction=1, turnout and reward=-1, abstain=0

Table 2.9: Does the Effect of Taxation Depend on Satisfaction?

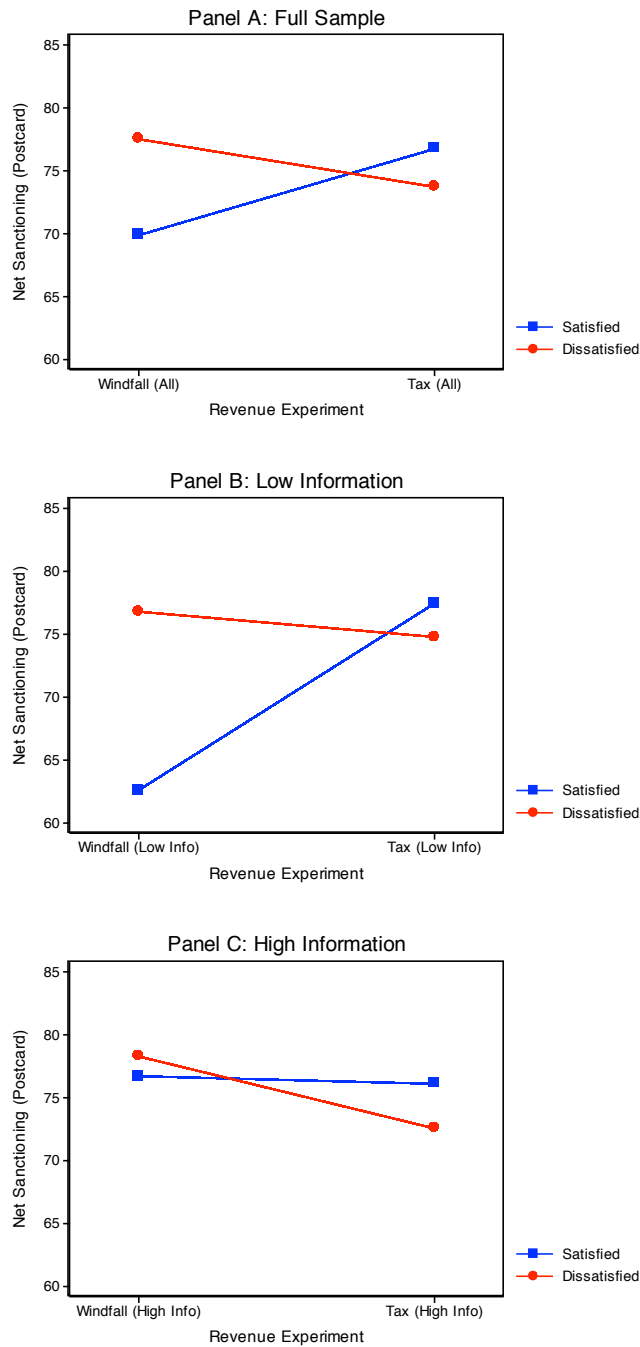


Figure 2.8: Does the effect of taxation on net sanctioning in the postcard campaign depend on satisfaction? *The figures show the tax treatment effect on net sanctioning in the postcard campaign, conditional on satisfaction, for the full sample (top), the low information group (middle), and the high information group (bottom).*

2.10.2 Does the effect of taxation depend on mailbox distance?

Section 2.5.3 in the main paper shows that the tax group is more likely to return postcards than the windfall group as the cost of participation increases. Table 2.10 presents the regression results behind Figure 2.5 in the main paper. The results show that there is a positive and significant interaction between the tax treatment and mailbox distance in the high information environment ($p=.052$). The triple interaction between the tax treatment, information treatment, and mailbox distance is also positive and significant at the 90 percent confidence level.

	Participation (Postcard Campaign) ^a			
	Full sample	Low Info	High info	Full sample
Tax treatment	-.02 (.03)	.04 (.05)	-.08* (.04)	.04 (.05)
Mailbox distance (min)	-.00* (.00)	.00 (.00)	-.01*** (.00)	.00 (.00)
Tax*mailbox distance	.00 (.00)	.00 (.00)	.01* (.00)	.00 (.00)
Info treatment				.12** (.05)
Tax*info treatment				-.12* (.06)
Info*mailbox distance				-.01** (.00)
Tax*info*mailbox distance				.01* (.01)
Constant	.82	.76	.87	.76
N	1,849	924	925	1,849

Notes: The first three columns reports results from separate regressions of the form $Y_{ij} = \alpha + \beta_1 T_{ij} + \beta_2 X_{ij} + \beta_3 (T_{ij} * X_{ij}) + \epsilon_{ij}$ where T_{ij} is the treatment indicator (tax group=1) and X_{ij} is the pre-treatment covariate of interest. The fourth column reports all constituent elements of a triple interaction between the tax treatment, information treatment, and mailbox distance. Neyman standard errors are reported in parentheses, where * $p < .10$, ** $p < .05$, and *** $p < .01$. Mailbox distance is a continuous measure (in minutes) obtained from the survey question: “During the rainy season, how many hours or minutes does it take to walk to the [postcard mailbox location]?”

^aReturned postcard and sanctioned=1, returned postcard and rewarded=-1, abstained=0.

Table 2.10: Does the Effect of Taxation on Participation Depend on Mailbox Distance?

2.10.3 Were participants sensitive to the price of public goods?

As discussed in Section 2.4.1.3, Section 2.7 and the appendix, the experiment was designed to shift the perceived price of public goods, defined as the ratio of the share of taxes to the share of spending on citizens in the budget. By experimental design, a positive interaction between the tax and information experiments would be regarded as support for the notion that participants reacted to the rising price of public goods induced by the experiment. The results presented in the paper show little evidence that this is the case, however. I confirm this lack of support with an additional survey measure presented in Table 2.11. The survey asked participants whether they felt their taxes were too high, about equal to, or relatively low compared to “the value to you of the public services [they] receive from the district government.”

There is some indication that the tax treatment caused participants to feel their taxes were too high relative to services in the low information environment. Yet, the information treatment caused on average a 22 percentage point increase in the share of participants who felt the price of public goods was too high and this effect was about the same size in both the windfall and tax groups. In other words, while participants indeed registered an increase in the price of public goods, this was primarily due to a downward shift in the denominator of the ratio (beliefs about government spending) rather than an upward shift in the numerator (beliefs about the share of taxes in total revenue).

	All (Info)	Windfall (C1)	Tax (C1)	Diff (T1-C1)
All (Tax)		.42 (921)	.46 (924)	.04 (.03)
Low info (C2)	.33 (922)	.30 (461)	.36 (461)	.106 (.04)
High info (T2)	.55 (923)	.54 (460)	.56 (463)	.095 (.02)
Diff (T2-C2)	.22*** (.03)	.24*** (.04)	.20*** (.04)	.546 (.05)
	.000	.000	.000	.445

Table reports sample means and treatment effects over sample size or standard errors in parentheses. Randomization inference p-values from a Fisher exact test reported below and * $p < .10$, ** $p < .05$, and *** $p < .01$ based on the RI p-values. The intersection of the ‘Diff’ columns is the difference-in-difference (interaction) effect of the treatments. The question wording was: “Your taxes are too high (=1), about equal to (=0), too low compared to (-1) the value to you of the public services you receive from district government.”

Table 2.11: The Price of Public Goods

2.10.4 Does the effect of taxation depend on previous tax experience or priors?

Section 2.7 in the main paper uses features of the experimental design (discussed in the appendix to the paper) and the survey data to explore why taxation motivates more political action. Specifically, it focuses on whether participants reacted to the simulated tax payment or to the shift in the perceived share of taxes and windfalls in the district budget. The results provide little conclusive support that participants reacted to the tax burden induced by the treatment and suggest instead that the results were driven by the perceived changes in the budget share coming from taxes. In this section I use pre-treatment data on participants previous experience with taxation and their prior beliefs about the share of revenue from taxes to look for additional support for these findings.

I begin by looking at whether the effect of the tax treatment on three main outcomes of interest varied depending on previous experience with taxation. One possibility is that the simulated tax payment primed actual experience with paying taxes. If this were the case, we would expect to see that the effect of the tax treatment on political action outcomes would be bigger for those with more previous experience with taxation or bigger real-world burdens. Another possibility is that the tax treatment effectively simulated a tax payment *regardless* of previous tax experience. This would suggest that the effect of the tax treatment would not depend on prior exposure to taxation.

Table 2.12 presents the effect of the tax treatment on three of the main outcomes of interest, conditional on four different measures of real world tax experience. The measures include whether: (1) the household had paid at least one tax in the previous 12 months; (2) the participant had personally ever paid a tax; (3) annual household taxes in the previous 12 months were high (greater than the mean); and (4) the household was poor (implying the 4,000 rupiah tax from the 14,000 rupiah income would matter more the participant). There is almost no evidence that any of these factors conditioned the effect of the tax treatment on monitoring, participation, or sanctioning. This suggests that participants either did not react to the simulated tax payment in the experiment or—since this is difficult to rule out for reasons mentioned in the paper—that the tax payment did not ‘work’ by priming real world tax experience.

I next look at whether the effect of the tax treatment on political action varied depending on prior beliefs about the share of taxes in total revenue. The main results suggest that the tax treatment had a significant impact on shifting the perceived share of windfalls and taxes in the budget. The tax treatment primed a (hypothetical) scenario in which 40 percent of the district government budget came from taxes. I predict that the tax treatment would provoke political action among those with low priors (priors less than 40 percent) because these participants would have learned that a greater share of the budget comes from taxes than initially believed. Conversely, the tax treatment could have reduced political action among those with high priors (priors greater than or equal to 40 percent), would have learned that government revenue was less dependent on taxes.

Table 2.13 presents results for the effect of the tax treatment on three of the main outcomes of interest, conditional on prior beliefs about the share of taxes in total revenue, coded as 1 if priors were less than 40 percent (meaning more political action predicted) and zero if they were greater than or equal to 40 percent (the results are robust to coding observations at 40 percent as 1 instead of 0). The results show that, in the low information environment, the tax treatment provoked a worse outlook on government performance among those who learned there were more taxes in the environment. The tax treatment caused significantly more monitoring among those who learned that there were more taxes in the budget (see also Panel A of Figure 2.10). While

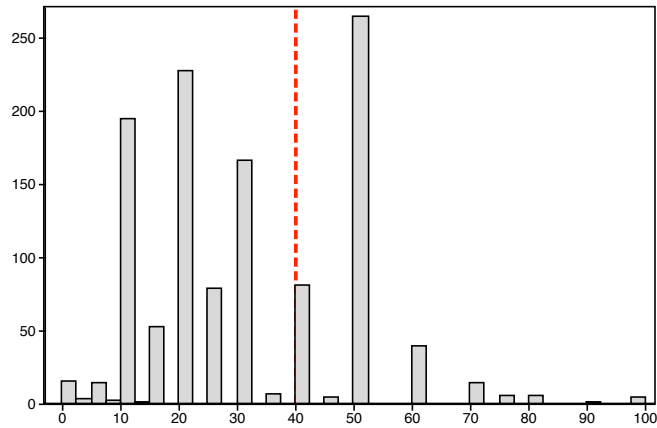


Figure 2.9: Distribution of prior beliefs on taxation. *The figure shows the pre-treatment distribution of prior beliefs about the share of taxes in total revenue.*

these results are consistent with predictions, the effect was surprisingly opposite for participation and net sanctioning in the postcard campaign: The tax treatment induced more participation and sanctioning in the postcard campaign among those who learned that taxes were a *smaller* share of the budget.

One possible explanation is that the results reflect not the effect of a decrease in the perceived share of *taxes* in total revenue but rather an increase in the perceived share of *windfalls* in total revenue. Yet, as can be seen in the final two columns, there is no indication that perceptions of the share of windfalls in total revenue varied depending on tax priors. Another possibility is that this is an artefact of the fact that 37 percent of the sample is missing data on prior tax beliefs; restricting analysis to those with tax priors might not capture the effect of the shift in the perceived share of windfalls and taxes induced by the treatment on political action in the full sample.

	Learn more about Budget [†]			Participation (Postcard) [‡]			Net Sanctioning (Postcard) [§]		
	Overall	Low Info	High Info	Overall	Low Info	High Info	Overall	Low Info	High Info
Panel A: Paid taxes^a									
Tax treatment	-.05 (.08)	-.13 (.09)	.06 (.13)	-.03 (.08)	-.06 (.11)	.01 (.10)	-.09 (.09)	-.11 (.12)	-.06 (.12)
Paid taxes	-.04 (.05)	-.17*** (.06)	.12 (.10)	.01 (.06)	.04 (.08)	-.04 (.08)	-.04 (.06)	-.03 (.08)	-.06 (.08)
Treatment*paid taxes	.10 (.08)	.18* (.10)	.00 (.13)	.03 (.08)	.08 (.11)	-.03 (.11)	.11 (.09)	.19 (.12)	.03 (.13)
Panel B: Paid taxes personally^b									
Tax treatment	.02 (.03)	.00 (.05)	.04 (.04)	-.02 (.03)	-.02 (.04)	-.02 (.04)	.00 (.03)	.02 (.05)	-.02 (.05)
Participant paid	-.03 (.03)	-.04 (.04)	-.03 (.04)	-.01 (.03)	-.06 (.04)	.03 (.04)	-.03 (.03)	-.10** (.05)	.04 (.04)
Treatment*participant paid	.05 (.04)	.07 (.06)	.02 (.05)	.03 (.04)	.05 (.06)	.01 (.06)	.02 (.04)	.07 (.06)	-.02 (.06)
Panel C: Annual taxes high^c									
Tax treatment	.04 (.03)	.03 (.04)	.05 (.03)	.00 (.02)	.02 (.03)	-.01 (.03)	.02 (.03)	.07* (.04)	-.04 (.04)
Annual tax high	.03 (.03)	.07 (.04)	-.01 (.04)	-.04 (.03)	-.01 (.04)	-.07* (.04)	-.04 (.03)	.00 (.05)	-.07* (.04)
Treatment*annual tax high	.03 (.04)	.04 (.06)	.03 (.05)	-.02 (.04)	-.02 (.06)	-.02 (.06)	-.01 (.04)	-.02 (.06)	.01 (.06)
Panel D: Poor^d									
Tax treatment	.08*** (.02)	.06 (.04)	.09*** (.03)	-.01 (.03)	.02 (.04)	-.03 (.04)	.00 (.03)	.06 (.04)	-.06 (.04)
Poor	-.02 (.03)	-.09** (.04)	.05 (.04)	.06** (.03)	.05 (.04)	.08** (.04)	.05 (.03)	.02 (.05)	.08** (.04)
Treatment*Poor	-.05 (.04)	-.03 (.06)	-.08 (.05)	.01 (.04)	-.01 (.05)	.04 (.05)	.04 (.04)	.01 (.06)	.07 (.06)

Table reports the average effect of the tax treatment over standard errors in parentheses, where * $p < .10$, ** $p < .05$, and *** $p < .01$.

[†]How interested are you in learning more about how the district government spends money in the budget? (interested=1)

[‡]Returned postcard=1

[§]Turned out and sanction=1, turnout and reward=-1, abstain=0

^aHousehold paid at least one tax in the previous 12 months.

^b"You just mentioned that you pay taxes. Do you personally pay taxes or is another member of your household usually the one to pay? (personally=1)

^cAnnual household taxes are greater than the mean=1.

^d"I would like you to think of your village in terms of three levels of poverty/wealth. Imagine that each level has about the same number of households in it. In your opinion, relative to other households in your village, which level is your household on? (lowest level=1)

Table 2.12: Does the Effect of the Tax Treatment Depend on Actual Tax Experience?

	Performance Worse			Learn more about Budget			Participation (Postcard)			Net Sanctioning (Postcard)			First Stage	
	<i>Overall</i>	<i>Low Info</i>	<i>High Info</i>	<i>Overall</i>	<i>Low Info</i>	<i>High Info</i>	<i>Overall</i>	<i>Low Info</i>	<i>High Info</i>	<i>Overall</i>	<i>Low Info</i>	<i>High Info</i>	% Taxes	% Windfalls
Tax treatment	-.04 (.05)	-.16** (.07)	.07 (.05)	-.03 (.04)	.01 (.06)	-.06 (.05)	.09** (.04)	.17*** (.06)	.02 (.06)	.12*** (.05)	.25*** (.06)	.00 (.06)	.25*** (.02)	-.30*** (.02)
Low priors	-.12*** (.04)	-.23*** (.06)	-.02 (.05)	-.07* (.04)	-.05 (.05)	-.08* (.05)	.05 (.04)	.07 (.05)	.02 (.05)	.09** (.04)	.14** (.06)	.03 (.05)	-.05*** (.02)	.04** (.02)
Treat*low priors	.07 (.06)	.24*** (.08)	-.06 (.06)	.11** (.05)	.04 (.07)	.19*** (.06)	-.14*** (.05)	-.21*** (.07)	-.06 (.07)	-.16*** (.05)	-.29*** (.08)	-.04 (.08)	.04** (.02)	-.03 (.02)
Constant	.67	.52	.82	.81	.77	.85	.75	.71	.78	.70	.63	.76	.14	.76
N	1,176	576	600	1,179	579	600	1,179	579	600	1,177	579	598	1,178	1,178

Table reports the average effect of the tax versus windfall treatments, conditional on the pre-treatment covariate, over standard errors in parentheses, where * $p < .10$, ** $p < .05$, and *** $p < .01$. The variable 'low priors' was measured in the survey by: "For every 10,000 rupiah the district government gets in income, how much do you think comes from taxpayers in Blora?" Responses are coded 1 if priors were less than 4,000 (or 40 percent), implying participants initially perceived that taxes were a smaller share of the budget than what they learned in the campaign, which is consistent with the prediction that the treatment would produce higher levels of monitoring, participation, and sanctioning.

Table 2.13: Does the Effect of the Tax Treatment Depend on Prior Beliefs about the Share of Taxes in Total Revenue?

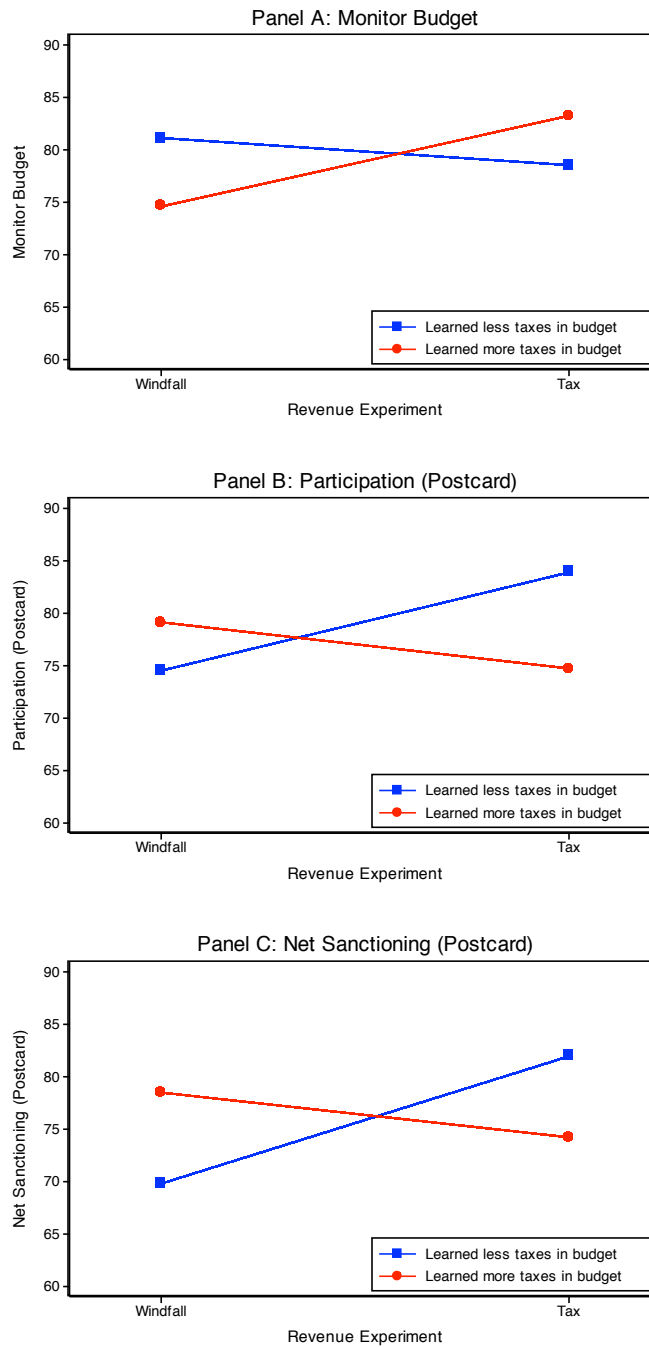


Figure 2.10: The effects of the tax treatment on political action, conditional on prior beliefs about the share of taxes in total revenue. *The panels show the effect on the tax treatment on willingness to learn more about the budget (top), participation in the postcard campaign (middle), and net sanctioning in the postcard campaign (bottom) for the full sample.*

Chapter 3

How Does Information Affect Political Behavior? Evidence from an Experiment in Indonesia

3.1 Introduction

Identifying the determinants of effective accountability is a central concern in political economy research. In many developing democracies, it has become apparent that elections and democratic institutions do not necessarily ensure that politicians act in the best interest of citizens. Of those countries that transitioned to democracy in the previous two decades, many have fallen into a “gray zone,” characterized by disconnected political elites, feeble policy-making capacity, endemic political corruption, and low levels of political engagement (Carothers, 2002).

One enduring explanation for why accountability fails centers on the existence of information asymmetries between politicians and citizens. In the political agency tradition, democracy is conceptualized as an accountability relationship where citizens (the principals) elect politicians (the agents) to work on their behalf (Barro, 1973; Ferejohn, 1986; Besley, 2006). Yet, politicians often are self-interested and have an information advantage over citizens. The challenge for voters is thus either to select good (honest, hard-working) politicians or discipline bad ones using re-election as an incentive. Doing so requires that citizens have adequate information on incumbent performance or the quality of candidates contesting the elections. While some have pointed out that better information could have perverse effects for policy and welfare (Pratt, 2005; Humphreys and Weinstein, 2010; Malesky, Schuler and Tran, 2011), a growing empirical literature provides evidence that information strengthens accountability and improves government performance (Ferraz and Finan, 2008; Reinikka and Svensson, 2003; Besley and Burgess, 2002).

This paper explores how citizens use information to make political decisions. There is substantial evidence that information affects whether and how citizens vote, both in the United States (Bartels, 1996; Alvarez, 1999; Stromberg, 2004) and in developing countries from Uganda (Humphreys and Weinstein, 2010), to Mexico (Chong et al., 2011), to India (Banerjee et al., 2011). An important recent empirical development is the use of experiments—which exogenously improve access to government, media or civil society-provided information—to identify the causal effect of information on citizen political behavior (Pande, 2011). Experiments bypass the concerns that whether an individual is politically informed is likely correlated both with the outcomes of interest (such as turnout)

and a host of other possible explanatory variables. Yet, important questions remain: When does new information increase or decrease the perceived benefits of taking political action? How does the effect of information on political behavior vary depending on individuals' initial expectations about government performance? And, does the relationship between information and political behavior depend on characteristics that affect whether an individual comprehends or values new information?

To address these questions, I embedded an information experiment into a public awareness campaign conducted at the district-level in Indonesia. The treatment group received information on how the government spent the budget across and within key categories for development, including education, infrastructure, health, and agricultural sectors. Overall, the information was designed to provide the treatment group with a clearer picture on the budget share that local politicians allocated to programs and services for citizens, to routine administration, and to themselves in the form of private benefits. The control group received placebo information only.

The campaign was conducted with 1,863 randomly sampled and randomly assigned citizens from 93 villages around Blora, a predominantly rural district in Central Java. Since Indonesia's 'big bang' decentralization in 2001, districts like Blora have acquired both significant resources and authority to manage local development. Yet, corruption is rife and the information environment at the local level remains poor. The campaign was conducted in partnership with Indonesian NGOs and implemented by trained canvassers in the three months preceding district head elections in 2010.¹ The main outcomes of interest—measured through both a survey and participation in a postcard campaign—are the effects of information on incumbent support, political participation, and the willingness to remain politically informed.

This paper makes three contributions to the emerging experimental literature. First, the results provide new empirical evidence of how information affects both incumbent support and participation. The treatment had a powerful effect on participants' beliefs about government performance. Overall, the information caused a 52 percentage point increase in the share of people who thought

¹Blora's selection as a project site was also determined in large part by the NGO partners' implementation capacity. The partners had been operating in the district since 2008 advocating for better budget management.

government was performing worse than previously expected. Yet, there is only weak evidence from the postcard campaign that this information decreased incumbent support either in the absolute or vis-a-vis challengers in the local elections. The data suggests the information had a positive effect on participation in the postcard campaign. Members of the treatment group were four percentage points more likely to return postcards than the control group but only when the costs of participation were low.

Second, the paper investigates how prior beliefs and other individual characteristics condition the effect of information on political behavior. While the macro context in this study is held constant (since the project took place in only one district), rich pre-treatment survey data makes it possible to study how information's impact varies depending on underlying factors. The results show that prior beliefs on two dimensions—the share of revenue that politicians spend on goods and services for citizens and take for themselves—indeed conditioned the effect of information on perceptions of government as performing worse than previously expected. With respect to political behavior, information increased participation but only when participants obtained bad news on at least one dimension. This result highlights the complexity of needing to take into account how multiple dimensions of information might work in complementary or competing directions to understand more fully how information affects political decisions at the individual-level.

The third contribution arises from addressing a question that has received little attention to date: Does one-off information provision also affect whether citizens choose to remain politically informed in the future? Much of the experimental literature has studied the effects of one-off information provision on voting behavior but has not yet considered its impact on demand for more information. Yet, public scrutiny and demand for information can play an important role in accountability outside of elections (Bauhr, Grimes and Herring, 2010). When citizens exert effort to monitor government, for instance by simply reading a newspaper, mis-managing or diverting budgetary funds becomes more costly for agents. Monitoring of agents has been associated with more efficient and effective outcomes in a wide range of principal-agent relations (Miller, 2005; Banks, 1989; Olken, 2007; Davis and Hayes, 1993; Bjorkman and Svensson, 2010; Grossman and Hanlon, 2011).

The results show that the information treatment caused an eight percentage point increase in the share of participants willing to remain politically informed on the government and the budget. Moreover, those gains primarily came from those who were disinterested in such information at the outset. An important implication of these findings is that one-off exogenous improvements to access to information—through the efforts of government, the media or civil society—could create a demand for information that sustains a more politically informed society over time.

This paper proceeds as follows. Section 3.2 reviews the literature on information and political behavior and describes the hypotheses to be tested in this paper. Section 3.3 describes the situation in Indonesia and Blora district, where this study takes place. In Section 3.4 I review the experimental design, data and randomization. The main results for the average treatment effect of information are presented in Section 3.5. In Section 3.6 I investigate how the impact of information varies depending on individuals' prior beliefs about government spending as well as on other individual-level characteristics.

3.2 Literature and Hypotheses

How information affects citizens' political decisions—and, by extension, accountability and government performance—is the subject of an already substantial literature. While it is widely accepted that political knowledge is essential to accountability, it is similarly accepted that the public often is not well informed. For many, the total costs exceed the perceived benefits of being politically informed, which rationally leads to ignorance (Downs, 1957; Bimber, 2001; Jerit, Barabas and Bolsen, 2006). A large uninformed population is likely particularly prevalent in developing countries, where low education levels and a lack of access to political information make the costs of becoming informed prohibitively high.

In a political agency framework, better information on either incumbent policies or candidate quality enables citizens to make more reasoned decisions about whether to reward or punish incumbents at the polls (Besley, 2006). This notion is predicated on the twin assumptions that individuals vote retrospectively and update their beliefs on government performance when con-

fronted with new information. A central prediction in the literature is that new information that reflects poorly on an incumbent will produce a decline in support while positive information will yield rewards. Ferraz and Finan (2008) find support for this claim in showing that random audits of municipal finances that reveal (at least two) instances of corruption reduce incumbent re-election probabilities. In a survey experiment in Uganda in which the treatment group was exposed to a scorecard on MP performance, Humphreys and Weinstein (2010) demonstrate that information indeed affects incumbent support and the direction of the impact depends on whether the news is positive or negative.

New information is also predicted to affect individuals' decision of *whether* to participate in politics at all. Much of the literature has focused on voter turnout, which is important because high turnout ensures that election outcomes reflect the will and interests of the true majority (Lijphart, 1997). According to models of strategic voting, better information typically should lead to higher turnout. Feddersen and Pesendorfer (1996) show that it can be optimal for less informed voters to abstain from making a decision and leave the decision to the better informed, even when voting is costless. Matsusaka (1984) highlights that turnout is not only a matter of being objectively well-informed but also of feeling confident in one's political knowledge.

Others have argued that voting is more of an expressive than a strategic act. Individuals will turnout as long as the perceived benefits of taking political action exceed the actual costs of going to the polls. Perceived benefits could include the social or psychological rewards of fulfilling a duty or sending government a signal. In a model by Banerjee et al (2011), the polity is divided between partisan and 'ethical' voters, where the latter can be swayed by new information on incumbent performance, incumbent quality, or challenger quality. Better information distinguishes candidates from one another and the authors predict that bad news will increase turnout and decrease incumbent vote share.

The empirical evidence predominantly supports the notion that information increases turnout. In their experimental test conducted in urban slums in India, Banerjee et al (2011) find that better information on incumbent performance increased turnout by 3.5 percentage points in the treatment group. These results are consistent with an existing empirical literature that has largely found a

positive relationship between information and turnout in other contexts (Lassen, 2005; Stromberg, 2004). Yet, in a public awareness campaign experiment conducted at the municipal level in Mexico, Chong et al (2011) find that negative information reduced turnout and increased apathy among voters. This is consistent with the logic of expressive voting insofar as the negative information reduced the perceived benefit of taking action, for instance by making people more cynical about politics.

While much of the theoretical and empirical focus has been on voter turnout, it readily extends to the propensity to join in a range of other forms of political action, from the more mundane (like signing a petition) to the more costly (like joining a protest). Another type of political action that has largely been over-looked empirically is government monitoring, with important implications for restraining government misbehavior. Initially providing individuals with better information could increase their willingness to remain informed in the long run, either by enhancing the perceived benefits of being well-informed or by breaking-down the barriers that arise from complex and intimidating subject matter. On the other hand, there could be decreasing marginal returns to additional information, or individuals who find the information too confusing could be further disempowered. All in all, information could have an important impact on non-electoral political participation and, while it is difficult to predict *ex ante* whether the effect will be positive or negative, the inquiry is nonetheless important.

Drawing on this literature, this paper tests the predictions that information on government spending performance will affect incumbent support, political participation, and the willingness to remain politically informed. Additionally, I investigate how information's impact varies depending on individuals' prior beliefs about government spending. There are at least three reasons to investigate how prior beliefs condition information processing. First, theories of information are based on the assumption that individuals are Bayesian updaters and studying priors both tests this assumption and identifies what information content resonates. Second, the distance between prior beliefs and the truth determines the magnitude of the scope for learning, with obvious implications for the change in the perceived benefits of taking action. Third, as the expressive voting literature suggests, whether news is good or bad determines higher or lower levels of political participation

depending on the context, and taking into account prior beliefs clarifies what is driving the change.

Of course, prior beliefs are not the only factor that could affect the impact of better information on political behavior; people's ability to comprehend the information as well as the relevance of the information to their lives most likely matters as well. Education is widely considered a leading predictor of how well individuals comprehend new information (Lassen, 2005; Pande, 2011). The literature also suggests that both women and the poor might find information on government spending performance more relevant to their daily lives. Knowing the true cost of public goods could be more pertinent to the poor because they rely more on government-provided services than the well-off. Based on findings in a political campaign experiment in Benin, Wantchekon (2003) suggests that women might care more about information on government performance in providing public goods than men because they are more attuned to household welfare. I will explore empirically how the effect of information on incumbent support and political participation varies depending on an individual's level of education, poverty, and sex.

3.3 The Indonesia Context

Indonesia is an important context to investigate the impact of information on political behavior. After the collapse in 1998 of more than 30 years of autocratic rule under President Suharto, Indonesia undertook a sweeping program of democratization and decentralization. Indonesia's 'big bang' decentralization in 2001 devolved substantial resources and decision-making authority to the district-level, making district governments the second most important level of government for development after the central level. Additionally, direct elections for local legislators were introduced in 1999 followed by direct elections for executive district heads (*bupatis/walikotas*) in 2005 (on a rolling basis) (Skoufias et al., 2011).

The promise of decentralization lies in its ability to bring government closer to the people, enabling citizens to hold government accountable and improving public service delivery (Tiebout, 1956). Yet, effective accountability remains weak at the local level in Indonesia. Corruption is rampant and it is widely believed that decentralization has spurred the devolution of what

Indonesians call KKN—a common acronym standing for corruption (*korupsi*), collusion (*kolusi*) and nepotism (*nepotisme*)—to the local level. Budget-making is a key arena for local corruption and common problems range from outright embezzlement and manipulation for personal gain to weak capacity, large allocations for routine expenditures, and poor reflection of policy priorities (Bank, 2003). Rinaldi et al (2007) attribute the proliferation of corruption at the local level to a general climate of enrichment and the strengthening of local legislatures vis-a-vis the executive so that district heads must buy support to get elected. The authors also cite imperfect legal enforcement, low levels of political scrutiny, and a weak independent media as contributing factors. While civil society and independent media outlets have increasingly investigated budget mismanagement and aimed to increase public awareness of poor budgeting practices at the local level, it is also difficult for these actors to obtain information (Par, 2008; KOP, 2008).

Such problems with corruption and budget mismanagement are evident in Blora district, a predominantly agricultural district in Central Java, Indonesia.² Allegations of corruption were rife during the tenure of the former legislative chairman, Pak Warsit, from 1999-2009. Warsit was arrested and tried in 2008 for corruption amounting to 5.6 miliar rupiah (USD \$721,000 in 2011 dollars) in the 2004 district budget. He was originally convicted (along with three deputy parliamentarians) by a provincial court but Warsit's conviction was reversed by the Supreme Court just prior to the official candidate registration date for district head elections.³ Relevant to the results presented in Section 3.5.3 on vote choice, Warsit ran as a challenger to the incumbent district head in the 2010 local elections in Blora.

Many citizens in Blora lack exposure to information on local politics and budgeting, as reflected in the survey data collected for this project. While 92 percent of all respondents were able to correctly identify the president, only 56 percent were able to identify the district head. Moreover, when asked if they knew what the local budget was (the *Anggaran Pendapatan Belanja Daerah*, or APBD) only 24 percent were able to correctly define it and only six percent said they had heard

²One reason for the selection of this district is because it is rich in oil and gas, which is the focus of Paler (2011).

³The ruling was reversed for Warsit and not for his three deputies. The decision, and its timing, was widely regarded as suspect by the media and civil society in Blora. According to Indonesian law (Law 32/2004 on Regional Governance, sub-article 58f), anyone found guilty and sentenced to five or more years in prison cannot run for office.

some or a lot about it in the previous 12 months. Respondents also reported having heard little to nothing about the work of the local government in the previous 12 months—only nine percent had heard about the district head and only eight percent had heard news about local legislators. After asking respondents whether they had ever sought out information on government spending, 91 percent said no. Tellingly, the number one reason they gave for why was that they did not know how to get the information (43 percent), which surpassed the share who claimed that the information was not relevant to their daily life (36 percent). It was in this context that the “Your Voice, Your Opportunity” public awareness campaign and information experiment was conducted.

3.4 Experimental Design

To test the impact of information on citizens’ support for the incumbent government, political participation, and willingness to remain politically informed, an information experiment was embedded in a public awareness campaign. A total of 1,863 participants from 93 villages around Biora were randomly sampled from the adult population and assigned to treatment and control groups. The treatment group received information on how the government spent the budget across a number of different categories—including education, health, infrastructure, agriculture—as well as the share politicians allocated to themselves. An original survey and postcard campaign are used to measure the effect of the information on attitude change and the main outcomes of interest.

3.4.1 The campaign and information experiment

3.4.1.1 The Setup

The public awareness campaign into which the information experiment was embedded was designed to familiarize all participants with what the district budget is and the role of elected officials in managing it. The campaign was conducted by canvassers one-on-one in participants’ homes and the canvassers used colorful visuals, real money, and interactive exercises to convey content. The public awareness campaign began with introductory illustrations and a household budget exercise (see Figure 3.1). The first illustration reminded participants that they vote for their elected leaders

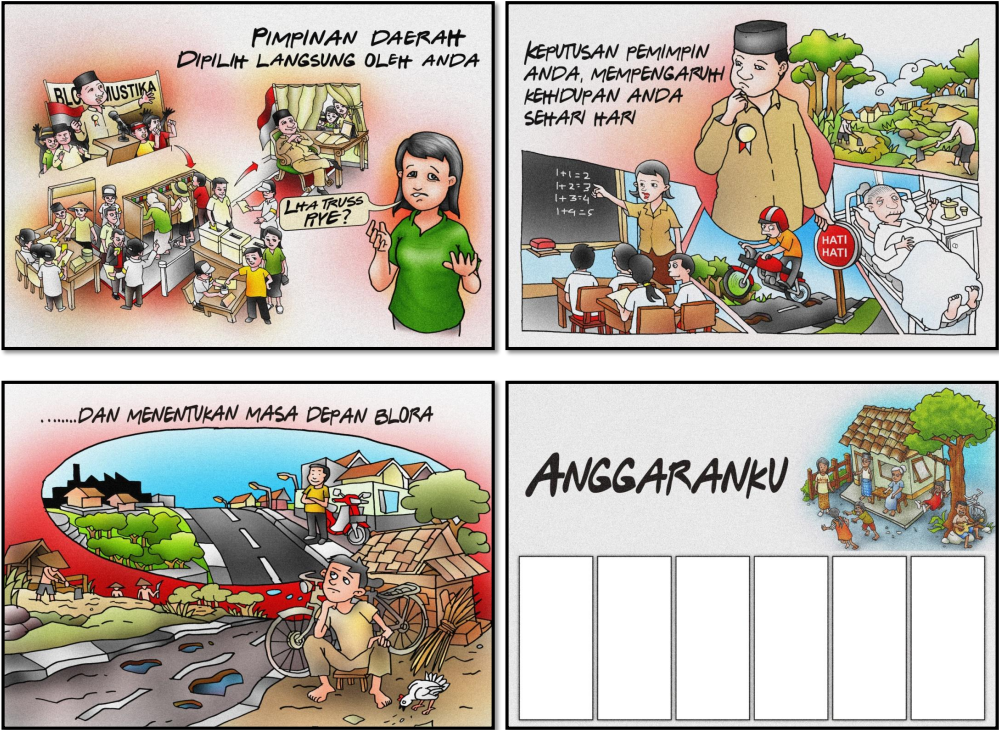


Figure 3.1: Campaign Setup. You elect your leaders, and then what do they do (top left)? They make decisions about public services, like education, health, infrastructure, and farming (top right). Think about what you want Blora to look like in the future (bottom left). The ‘My Budget’ household budget exercise game board (bottom right).

but it is often hard to know what those leaders do once in office. The second illustration informed participants that the bupati and DPRD are responsible for providing public services like education, health and infrastructure. To encourage participants not just to think in terms of their needs today but also their needs in the future, the third illustration urged them to consider what they want Blora to be like five to ten years from now. Participants then participated in an exercise involving the allocation of funds across different household expenses to present the concept of a budget in familiar terms. This exercise was followed by a revenue experiment designed to vary participants' beliefs on where district government revenue comes from.⁴

3.4.1.2 The Treatment

The information experiment presented the treatment group with real information on government spending in 2008.⁵ The information presented was based on an analysis of realized spending in 2008 performed by the author and partner organizations. To illustrate spending, the canvasser used 10,000 rupiah (representing district government income *per capita*, scaled) in small denominations and a spending 'game-board' (shown in Figure 3.2). The game-board contained boxes for six categories of spending: education, infrastructure, health, farming, 'politicians', and other. The 'politicians' category represented how much politicians allocated to themselves for salaries, health benefits, travel and other private expenses as well as how much they spent to run the executive and legislative offices.

Overall, the treatment emphasized the share of the budget that local politicians allocated to programs and services for citizens (g), routine administration (b), and to themselves in the form of private benefits (r), where $g + b + r = 1$. The canvasser first used the 10,000 rupiah to illustrate the share of the total budget spent in each of the six categories.⁶ Second, within each category, the canvasser disaggregated the share spent on programs and services for citizens versus routine

⁴The revenue experiment is the focus of a separate paper (2011).

⁵The 2008 budget was selected because it was the most recent year for which realized spending data was available and for which the incumbent district head and former legislative chairman (both candidates in the 2010 district head elections) were responsible.

⁶Overall 47 percent of the budget was spent on education; 12 percent on infrastructure; 9 percent on health; 2 percent on agriculture, 18 percent on politicians; and 12 percent on other expenses.

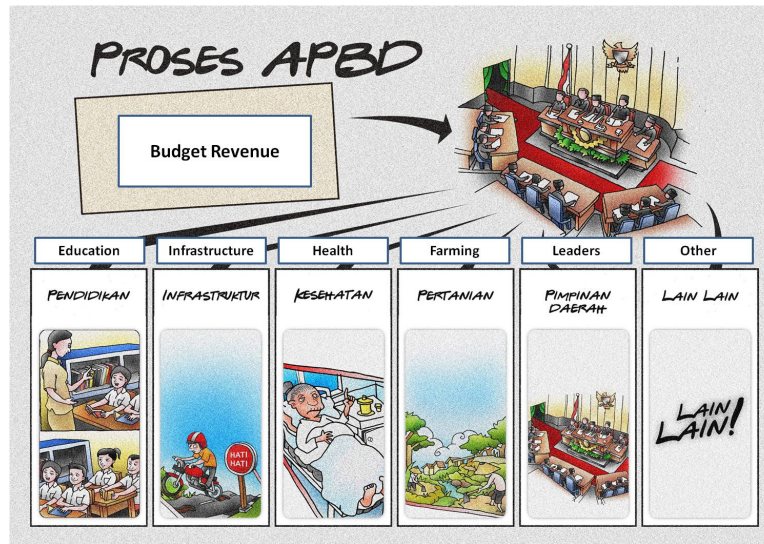


Figure 3.2: Spending ‘Gameboard’. This board and 10,000 rupiah in small denominations was used to illustrate spending across and within six key sectors for development.

administration. The only exception was the ‘politicians’ category, where the canvasser disaggregated spending into the share that went to routine administration versus the share that politicians allocated to their private benefits. In sum, the treatment group learned that about 30 percent of the budget goes to goods and services for citizens, 52 percent goes to the administration of those goods and services, and 18 percent goes to politicians, of which 2 percent of the entire budget goes to private benefits for 46 individuals.⁷

The control group received placebo information in the form of a list of facts and figures taken from the 2008 Blora statistical yearbook (*Blora dalam Angka*). This information had no relevance to the budget and included random facts, such as the annual rainfall in Blora and the composition of trash collected in the district. It should be noted that this experimental design helps to overcome the concern that it is difficult to disentangle the impact of new information from the motivational effects of the campaigns and materials through which it is provided (Pande, 2011). Since both the treatment and control groups received the public awareness campaign in this case, the motivational

⁷The campaign also primed participants on corruption in citing two recent cases of alleged corruption and removing 200 cents (*ratus*) from the board to make the point that not all funds designated for citizens reaches them. Participants were told that the actual scale of corruption in the budget was not known and that the 200 cents removed was meant to represent the fact that corruption exists.

effect should be held constant and any effect can plausibly be attributed to the impact of treatment content alone.

3.4.2 Sampling and randomization

Participants in the campaign were randomly sampled, using multi-stage cluster sampling, from the adult population in Bora. The target population was all individuals between the ages of 17 (the age of voter eligibility) and 65 who had resided in Bora for at least the previous six months. First, 93 of 295 villages were randomly sampled within strata formed by subdistrict and urban-rural status. Within each village, one sub-village unit (*dusun*) was randomly sampled, followed by the random selection of 20 households from an updated list of all households in the *dusun*. Canvassers then sampled one participant in each household on arrival using simple random sampling from a full list of eligible household members made in consultation with the head of household or first contact. The random assignment of participants to treatment was done in advance by the author. Random assignment was blocked at the village-level so that, within each village with 20 participants, 10 were randomly assigned to each of the experimental conditions.

A number of steps were also taken to minimize design and timing effects. To reduce effects associated with variation in canvasser abilities, assigned to treatment in each village was also blocked on individual canvassers so that each canvasser conducted two treatment and two control campaign versions per village. Quotas were also set so that gender balance across experimental conditions and canvassers was achieved. Finally, to minimize timing effects, villages were divided geographically into three groups and the order in which implementation teams visited villages was randomized within groups. Random assignment was implemented by providing canvassers with a schedule for each village that matched a randomly selected household to an experimental condition and informed canvassers of whether they should sample a male or female participant in that household.⁸

⁸A more detailed description of the sampling and randomization procedure is available in the design memo, available on the author's website. The final sample consists of 1,863 individuals, including the original goal 1,860 plus three additional participants randomly sampled in a small number of villages with non-compliance.

3.4.3 Data

Data comes from two sources: participation in a postcard campaign and an original survey. Survey data was collected by canvassers during the same visit. A pre-treatment module containing questions on demographics, political knowledge and participation, and public goods usage was administered upon obtaining participant consent and prior to beginning the campaign. Canvassers also conducted a post-treatment module immediately following the information experiment (and prior to introducing the postcard). The concern arises, however, that surveys do not provide reliable measures of real political behavior because responses are costless and subject to social desirability bias. I therefore use participation in a postcard campaign as a revealed measure of political behavior.

At the end of the campaign, canvassers gave all participants the opportunity to return postcards. The postcard asked participants to indicate whether they were “satisfied with the district government in Biora and don’t want to change anything about how it works” (a reward for good performance) or whether they “want the district government in Biora to do a better job” (a sanction for bad performance).⁹ To crystallize the perceived benefits of taking action, participants were informed that the results of the postcard campaign would be shared with candidates in the lead up to the district head elections to encourage them to respond to public opinion.¹⁰ Returning the postcard intentionally entailed a small cost. Participants had up to 24 hours to deposit their postcards in specially designated mailboxes located in their community. On average, participants lived about a 10 minute walk from the mailbox location. This small cost in terms of time and energy is akin to various types of low cost political action, like signing a petition or contacting an official. The postcard campaign also resembles the voting decision in that participants had to decide both whether to participate and whether to use their postcards to reward or punish the incumbent government.

Table 3.1 provides descriptive data on the main outcome measures from both the survey and

⁹If they selected the latter, they were also asked to indicate their level of support for five different governance reforms promoted by PATTIRO and LPAW, and to indicate their priority reform. I do not analyze the outcomes for each reform here since those were primarily intended for the partner organizations.

¹⁰This was accomplished in a subsequent voter education experiment, results forthcoming.

postcard campaign. Table 3.2 presents evidence using the pre-treatment survey data that randomization achieved balance in covariates across the experimental conditions (Panel A).

		Range		<i>mean</i>	<i>SD</i>	Confidence Interval		<i>n</i>
		<i>min</i>	<i>max</i>			<i>Lower</i>	<i>Upper</i>	
Panel A: Learning								
1	Information new ^a	-1	1	.68	.01	.66	.71	1,863
2	Budget knowledge ^b	0	1	.29	.01	.27	.31	1,855
3	Changed opinion ^c	-1	1	.67	.01	.65	.69	1,850
4	Performance worse ^d	0	1	.49	.01	.46	.52	1,851
5	Politicians are self-interested ^e	0	1	.44	.01	.42	.47	1,850
6	Distrust district head ^f	0	1	.55	.01	.53	.57	1,853
7	Distrust local legislators ^f	0	1	.61	.01	.59	.64	1,857
Panel B: Incumbent Support								
8	Net sanctioning (postcard) ^g	-1	1	.75	.01	.73	.77	1,857
9	Vote choice ^h							
	(a) Incumbent	0	1	.53	.02	.48	.57	458
	(b) Former Leg. Chairman (Challenger)	0	1	.09	.01	.07	.12	458
	(c) Challenger 2	0	1	.38	.02	.33	.42	458
10	Won't follow endorsement ⁱ	0	1	.73	.01	.71	.75	1,857
Panel C: Turnout and Participation								
11	Political action ^j	0	5	1.32	.02	1.27	1.36	1,863
12	Turnout (postcard campaign) ^k	0	1	.78	.01	.77	.80	1,863
Panel D: Remaining Politically Informed								
13	Learn more about budget ^l	0	1	.79	.01	.77	.80	1,863
14	Learn more about govt. ^m	0	1	.78	.01	.76	.80	1,862

Note: Table reports the summary statistics for the main dependent variables from both the survey and the postcard campaign.

^a Would you describe the information you just received as [totally new (=1)/somewhat new (=0)/not new at all (-1)]

^b You have a pretty good understanding of issues related to the budget in Blora. (agree=1)

^c Would you say the information you just heard [changed/did not change] your opinion about district government? (changed=1)

^d Elected leaders in the district government are doing a [worse job (=1)/better job (=−1)/the same job (=0)] than you thought they were?

^e District leaders in Blora care more about their personal interests than about the needs of the people. (agree=1)

^f How much do you trust the [district head/local legislators] to do the right thing for people in Blora? (distrust=1)

^g Turned out and sanction=1, turnout and reward=−1, abstain=0

^h Support for the [incumbent district head=1/former legislative chairman=1/challenger 2=1].

ⁱ Let's say that in the upcoming district head elections, you decide you like Candidate A, but you learn that a village leader whom you respect supports Candidate B. How likely would you be to switch your preference from Candidate A to Candidate B? (unlikely=1).

^j Regarding a problem or an issue that was affecting your daily life of your community, would you in the future [contact a village/subdistrict official, contact the district head, contact a local legislator, contact the media or an NGO, take part in a demonstration]? (average response of 5)

^k Returned postcard=1

^l How interested are you in learning more about how the district government spends money in the budget? (interested=1)

^m How interested are you in learning more about what the government of Blora is doing? (interested=1)

Table 3.1: Summary Statistics

	Range		Sample Mean	Control Mean	Treated Mean	Diff (T-C) RI p-value	n
	<i>min</i>	<i>max</i>					
Panel A: Demographics							
Female (%)	0	1	.50	.50	.50	.999	1,863
Age	17	65	41.7	41.7	41.7	.983	1,818
Completed primary school (%)	0	1	.72	.72	.72	.679	1,862
Muslim (%)	0	1	.99	.99	.99	.593	1,863
Javanese (%)	0	1	1.0	1.0	1.0	.617	1,862
Married (%)	0	1	.90	.90	.91	.168	1,862
Can read a newspaper (%)	0	1	.81	.82	.80	.395	1,861
Numeracy (%)	0	5	1.9	1.9	1.9	.884	1,863
Want information (%)	0	1	.71	.71	.72	.873	1,860
Poor (subjective) (%)	0	1	.45	.46	.44	.762	1,863
Employed full-time (%)	0	1	.64	.64	.63	.833	1,863
Work in agriculture (%)	0	1	.70	.70	.70	.969	1,477
Mailbox distance (%)	0	80	9.85	9.64	10.06	.874	1,849
Panel B: Spending priors							
Goods and services (all) (%)	0	100	54.8	54.9	54.8	.767	1,717
Education (%)	0	100	17.8	17.9	17.7	.628	1,521
Infrastructure (%)	0	100	14.5	14.7	14.4	.463	1,521
Health (%)	0	100	14.9	14.9	15.0	.731	1,521
Agriculture (%)	0	100	13.9	14.2	13.6	.034	1,521
Politicians (%)	0	100	23.2	22.8	23.7	.181	1,521
Panel C: Party ID							
Support incumbent's party (P1)	0	1	.22	.21	.23	.892	1,192
Support challenger 1's party (P2)	0	1	.30	.32	.28	.668	1,192
Support challenger 2's party (P3)	0	1	.42	.41	.44	.927	1,192

Notes: Table reports results from a randomization check, where column 6 reports randomization inference p-values for the test of the null hypothesis that the difference in means between between the treatment and control groups on pre-treatment covariates is zero.

Table 3.2: Randomization Check

3.5 Main Results: How Information Affects Political Decisions

3.5.1 Estimation

This section presents results for the effect of information on learning, participation, incumbent support, and willingness to stay politically informed. Estimates of the average effect of information are obtained by running a regression of the form:

$$Y_{ij} = \alpha_j + \beta T_{ij} + \epsilon_{ij} \quad (3.1)$$

where Y_{ij} is the outcome for individual i in village j , β the coefficient of interest on the treatment assignment indicator T_{ij} , and ϵ_{ij} the individual random error. Since random assignment was blocked at the village-level, all estimates include village fixed effects, captured in the term α_j .¹¹ Standard errors are clustered at the village-level except in cases where clustering produces smaller standard errors due to negative intraclass correlation. In such cases, the more conservative uncorrected standard errors are presented (Arceneaux and Nickerson, 2009).¹² I also present randomization inference p-values for the test of the null hypothesis of no treatment effect.¹³

3.5.2 First stage: Updated priors

The results, presented in Panel A of Table 3.3, show that the information was new and caused participants to change their beliefs about government performance, consistent with Bayesian updating. While the information on government performance was overwhelmingly negative, the news on how much politicians took for themselves was positive, which suggests the effects could be somewhat

¹¹All analysis is based on treatment assignment rather than treatment take-up. There were only six cases of treatment non-compliance, all due to canvasser error.

¹²The uncorrected standard errors do not account for heteroskedasticity as it has been shown that standard errors that are unclustered but robust to heteroskedasticity are inconsistent with the fixed effects estimator (Stock and Watson, 2008).

¹³Randomization inference compares the observed treatment effect to a ‘reference’ distribution of treatment effects that would occur under the null hypothesis. To obtain the reference distribution, I reproduce the random treatment assignment process 10,000 times and obtain an estimate of the treatment effect each time. The randomization inference p-value is the probability of seeing a treatment effect as big or bigger than the one produced by the actual assignment, given the reference distribution (Imbens and Rubin, 2009).

off-setting.

The first three rows in Panel A provide strong evidence that the information was new and affected beliefs about government. While 51 percent of the control group said that the campaign information was new—reflecting the impact of the campaign itself—this number was 34 percentage points higher for the treatment group. The treatment group was also 11 percentage points more likely to agree that they “had a pretty good understanding of important issues related to the budget,” a measure that plausibly reflects heightened confidence in political knowledge. The treatment group was also 46 percentage points more likely to acknowledge that the campaign had “changed [their] opinions about district government.”

Moreover, the information caused participants (on average) to view government performance in a substantially worse light. Row four of Panel A presents a survey measure of whether participants thought government was doing worse than previously believed. The results indicate that 23 percent of the control group felt that government was doing worse than expected, compared to 75 percent in the treatment group—a significant 52 percentage point difference. Yet, the treatment also had a positive effect on views of the budget share that politicians allocated to themselves. When asked to choose whether they agreed with the statement that “district leaders in Blora care more about their personal interests than about the needs of the people,” the treatment group was 17 percentage points *less* likely to agree (Table 3.3, row seven). As discussed more in Section 3.6.1, this is consistent with the fact that participants initially believed that politicians were taking more for themselves than they actually were.

Nevertheless, it seems plausible that the effect of the information overall is negative. The magnitude of the negative effects along the government performance dimension is greater than the magnitude of the positive effect on politician self-interest. Moreover, the information harmed impressions of both the district head and local legislators by a variety of additional metrics. For instance, rows six and seven show that the treatment group was 30 percentage points more likely to distrust the bupati (row five) and 28 percentage points more likely to distrust local legislators (row six) compared to the control.¹⁴ The results nonetheless highlight the need to investigate further the

¹⁴Results of a similar pattern and magnitude apply for measures of dissatisfaction and beliefs about effort and ability

role of prior beliefs and how individuals weight the different dimensions of information provided, which I return to in subsequent sections.

3.5.3 Incumbent support

The literature predicts that negative information on the incumbent will produce a decline in support while positive information will yield rewards. Whether bad news translates into reduced incumbent vote share depends, however, on how willing voters are to switch support to other candidates. I draw on both the survey and the postcard campaign to analyze the effects of the information on incumbent support in both the absolute and relative to other candidates. The results, presented in Panel B of Table 3.3, indicate that the information had little effect, despite the shift in attitudes revealed in Panel A. If anything, the results are mixed: Participants sanctioned the incumbent in the postcard campaign but rewarded him in comparison to challengers in the election.

Row eight of Table 3.3 presents whether participants used their postcards to reward or sanction the incumbent for his performance. The postcard captures a referendum on the incumbent. I code all those who returned their postcard and signaled that they want government to do better as 1, those who returned their postcard and indicated they were satisfied with the status quo as -1, and those who abstained from participating as zero.¹⁵ In the control group, 74 percent used their postcards to signal dissatisfaction with the incumbent government. While sanctioning was three percentage points higher in the treatment group, the difference is not significant at conventional levels.

I next check whether the information treatment affected anticipated vote choice in the upcoming elections. As previously mentioned, the public awareness campaign was implemented in the three months leading up to district head elections. The three candidates included the incumbent district head, the former legislative chairman, and a third challenger with ties to the bureaucracy and a popular Muslim organization. As discussed in Section 3.3, the former legislative chairman was

(available upon request).

¹⁵It is not possible to look solely at how participants voted on their postcard, conditional on returning it. Ninety-eight percent of those who returned their postcard used it to signal dissatisfaction with the status quo, which raises concerns about ceiling effects given how close the sanctioning rate is to the upper bound.

		α	β	<i>se</i>	<i>RI pvalue</i>
Panel A: Learning					
1	Information new ^a	.51	.34***	(.02)	.000
2	Feel knowledgeable about budget ^b	.23	.11***	(.02)	.000
3	Opinion changed ^c	.45	.46***	(.02)	.000
4	Performance worse ^d	.23	.52***	(.03)	.000
5	Politicians self-interested ^e	.53	-.17***	(.02)	.000
6	Distrust bupati ^f	.40	.30***	(.02)	.000
7	Distrust DPRD ^f	.47	.28***	(.02)	.000
Panel B: Incumbent Support					
8	Net sanctioning (postcard) ⁱ	.74	.03	(.02)	.238
9	Vote choice ^j				
	(a) Incumbent	.20	.03	(.02)	.139
	(b) Former Leg. Chairman (Challenger)	.13	-.03	(.03)	.368
	(c) Challenger 2	.31	-.02	(.04)	.642
10	Won't follow endorsement ^k	.71	.05**	(.02)	.023
Panel C: Turnout and Participation					
11	Political action (mean of 5) ^g	1.29	.04	(.04)	.425
12	Turnout (postcard campaign) ^h	.77	.02	(.02)	.291
	(a) Turnout if mailbox near (≤ 10 min)	.77	.04*	(.02)	.114
	(b) Turnout if mailbox far (> 10 min)	.80	-.04	(.04)	.271
Panel D: Remain Politically Informed					
13	Willing to learn more about budget ^l	.75	.08***	(.02)	.000
14	Willing to learn more about govt. ^m	.74	.08***	(.02)	.000

Table reports results of from separate fixed effect regressions of the form $Y_{ij} = \alpha_j + \beta T_{ij} + \epsilon_{ij}$, where column 1 is the mean for the control from a fixed effects regression, column 2 the treatment effect, and column 4 the randomization inference p-value. Standard errors in the third column are clustered at the village-level except when there is negative intracluster correlation, in which case uncorrected standard errors are reported.

^a Would you describe the information you just received as [totally new (=1)/somewhat new (=0)/not new at all (-1)]

^b You have a pretty good understanding of issues related to the budget in Blora. (agree=1)

^c Would you say the information you just heard [changed/did not change] your opinion about district government? (changed=1)

^d Elected leaders in the district government are doing a [worse job (=1)/better job (=0)/the same job (=0)] than you thought they were?

^e District leaders in Blora came more about their personal interests than about the needs of the people. (agree=1)

^f How much do you trust the [district head/local legislators] to do the right thing for people in Blora? (distrust=1)

^g Turned out and sanction=1, turnout and reward=-1, abstain=0

^h Support for the [incumbent district head=1/former legislative chairman=1/challenger 2=1].

ⁱ Let's say that in the upcoming district head elections, you decide you like Candidate A, but you learn that a village leader whom you respect supports Candidate B. How likely would you be to switch your preference from Candidate A to Candidate B? (unlikely=1).

^j Regarding a problem or an issue that was affecting your daily life of your community, would you in the future [contact a village/subdistrict official, contact the district head, contact a local legislator, contact the media or an NGO, take part in a demonstration]? (average response of 5)

^k Returned postcard=1

^l How interested are you in learning more about how the district government spends money in the budget? (interested=1)

^m How interested are you in learning more about what the government of Blora is doing? (interested=1)

Table 3.3: Average Treatment Effect of Information

widely suspected of corruption. Participants were asked in the survey which candidate they most supported at that time. Statistical power is low for these questions, but the results presented in row 9a-c weakly suggest that the treatment caused *greater* support for the incumbent and weaker support for both challengers.¹⁶

Why might the information treatment have improved incumbent support vis-a-vis challengers, despite the evidence that it was bad news for the incumbent? One possible explanation is that the bad news on government performance was indeed offset by the good news on politician self-interest. Another possibility is that the information reflected negatively on the performance of both the incumbent district head *and* the former legislative chairman (since both were jointly in charge of the 2008 budget), but that the treatment also primed participants' own information that the former legislative chairman was a lower quality candidate due to the corruption allegations. Participants could therefore have decided to support the incumbent as a "lesser-of-two-evils" option.¹⁷

¹⁶There is low statistical power for analyzing these questions for two reasons. First, candidates officially registered during the fourth week of implementation of campaign implementation, so the questions on vote preferences were added to the survey only after the campaign had already been conducted with 400 participants (the highest possible n is 1485). (The order in which villages were visited was randomized, however, so the remaining sample still reflects a random sample of the population.) Second, the non-response rate on this question was high because it is a politically sensitive question. Only 458 participants in total (across the four groups) responded with candidate choices.

¹⁷Little was known about the second challenger at the time of the public awareness campaign and voters might have been reluctant to switch their support to a lesser-known candidate (Bartels 1986, Bartels 1988, Alvarez 1992).

	Perform Worse ⁱ			Incumbent ⁱⁱ		Challenger 1 (Leg. Chair) ⁱⁱ			Challenger 2 ⁱⁱ			Non-Response ⁱⁱ			
Info	.52*** (.04)	.53*** (.04)	.55*** (.04)	-.06* (.03)	.00 (.03)	.08* (.04)	-.04 (.06)	-.04 (.04)	-.12* (.07)	.05 (.08)	.03 (.08)	-.08 (.07)	.05 (.04)	.00 (.04)	.00 (.05)
Incumbent's party (P1) ^a				.24*** (.08)			-.13* (.07)			-.15 (.12)			-.12 (.08)		
Info*P1				.19** (.09)			-.06 (.11)			-.12 (.14)			-.11 (.10)		
Challenger 1 party (P2) ^a		.01 (.06)			-.16*** (.05)		.28** (.10)			-.03 (.07)			.08 (.06)		
Info*P2		.04 (.07)			-.02 (.07)		-.05 (.11)			-.07 (.12)			.07 (.08)		
Challenger 2 party (P3) ^a			.07 (.05)			.02 (.06)			-.15* (.08)		.03 (.08)				.02 (.06)
Info*P3			-.04 (.07)			-.21*** (.08)			.15 (.09)		.22* (.13)				.04 (.07)
Constant	.24	.20	.17	.24	.34	.29	.23	.11	.26	.42	.39	.37	.47	.42	.44
N	1187	1187	1187	656	656	656	656	656	656	656	656	656	656	656	656

Note: Table reports the results of separate fixed effects regressions of the form $Y_{ij} = \alpha_j + \beta_1 T_{ij} + \beta_2 X_{ij} + \beta_3 (T_{ij} * X_{ij}) + \epsilon_{ij}$ for different X_{ij} reported in the rows. All X_{ij} are measured in the survey pre-treatment. Standard errors are clustered at the village-level except when there is negative intracluster correlation, in which case uncorrected standard errors are reported.

^aParty identification is measured in the survey by first asking participants how strongly they support the four leading political parties (Partai Demokrat, PDI-P, Golkar, and PKB) and then asking them to select the party they identify with most closely (coded 1, otherwise 0). The incumbent district head belonged to Golkar, the former legislative chairman PDI-P, and the second challenger they represented a PD and PKB coalition.

ⁱElected leaders in the district government are doing a [worse job (=1)/better job (=−1)/the same job (=0)] than you thought they were?

ⁱⁱSupport for the [incumbent district head=1/former legislative chairman=1/challenger 2=1/non-response=1]

Table 3.4: The Effect of Information Conditional on Party ID

Another possibility is that individuals with strong party ties continued to support the incumbent or preferred to abstain rather than switch support to a candidate from a different party (Chong et al., 2011). Table 3.4 investigates whether the effect of the information on vote choice varies depending on the participants' party identification. We see that the impact of the information on perceptions that the government was performing worse than previously expected did not depend on the participants' party ID. Yet, the information *increased* support for the incumbent among party supporters while undermining support from the challengers' parties. There is also no evidence that party ID conditioned non-response (either not knowing or refusing to answer), which is akin to abstaining in the context of this survey question. Thus, despite receiving bad news on the incumbent, the treatment caused members of the incumbent's party to rally behind him.¹⁸

Interestingly, while the treatment did not have a significant impact on either incumbent support or anticipated vote share, there is evidence that it affected how firmly participants planned to stand by their expected vote choice. The susceptibility of voters to influence by opinion leaders is a concern as it is associated with the propensity to take information shortcuts, like voting along party or ethnic lines, without particular consideration for policy stances. To get a sense of how malleable participants were to having their political opinions changed, the survey employed a hypothetical question inquiring about a scenario in which the participant supported Candidate A and they learned that a village leader whom they respected supported Candidate B. When asked how likely they would be to switch their votes, the treatment group was five percentage points less likely to say they would switch (row ten of Table 3.3). This suggests that the information might not have made voters feel more confident about their political decision.

3.5.4 Participation

The expressive voting literature predicts that information will lead to higher levels of turnout and political participation if it increases the perceived benefits of taking action vis-a-vis the costs. The results presented in Panel C of Table 3.3 suggest that the information had a positive effect on political participation, and this is particularly apparent when the costs of taking action are low.

¹⁸Party ID did not condition any of the other main outcomes of interest (results available from author).

Importantly, information had a strong positive and significant effect on a less overt kind of political action, however: willingness to monitor government in the future.

Row 11 of Table 3.3 reports results from survey questions asking participants how many different types of non-electoral political action they would take regarding an issue that affected their lives or their community. Specifically, participants were asked about their willingness to contact a village or subdistrict official, contact the district head, contact a local legislator, contact the media or an NGO, or take part in a demonstration. On average, participants were willing to take just over one type of political action and there is no evidence that informed participants planned to take more action than less informed ones. This pattern also holds when analyzing the types of action individually. The survey also inquired into plans to vote in the upcoming district head elections. An unrealistically high 99 percent in each group said they planned to vote in the elections, which reflects the difficulty of relying on costless self-reported measures of political behavior when social desirability bias is high.

Participation in the postcard campaign, as a revealed preference, provides a better measure of actual willingness to take political action. As can be seen in row 12, 77 percent of participants in the control group returned their postcards and the return rate was two percentage points higher in the treatment group, although this difference is not significant. As an actual behavioral measure, returning the postcard entailed a small cost. Such costs have been identified as an obstacle to participation (Riker and Ordeshook, 1968). I check whether the distance to the mailbox location conditioned the propensity to return the postcard; if the information increased the perceived benefit of returning the postcard vis-a-vis the costs, we would expect to see higher participation in the treatment group than in the control among those who lived furthest from the mailbox location. On average, participants lived about a 10 minute walk (in the rainy season) from the mailbox location. Rows 12a-b of Table 3.3 indicate that the information increased participation by four percentage points among those who lived at or below the mean mailbox distance. This suggests that the information had a positive effect on turnout but only when the costs of participation were low. These results are consistent with empirical studies showing that negative information has a mobilizing effect on voters (Banerjee et al., 2011).

The final outcome of interest is whether the information treatment made participants more willing to take the less overt political action of remaining informed in the future. I measure participants' interest in remaining politically informed using survey questions that inquire into their willingness to learn more in the future about how the district government spends public funds and how the district government operates in general. As can be seen in Panel D of Table 3.3, the desire to learn this information was a significant eight percentage points higher in the treatment than in the control group.

3.5.5 Summary of main results

The information treatment gave participants new information that substantially affected their attitudes towards the district government. While participants received good news on politician self-interest, the content was overwhelmingly negative with respect to outlook on government performance. Yet, despite the information being negative overall on average, the effects on incumbent support were mixed: The information decreased support for the incumbent in the absolute but increased support relative to challengers. The information had a weakly positive effect on participation and other overt types of political participation but only when the costs of action were low. Importantly, the information had a notable impact on participants' willingness to remain politically informed in the future. This suggests that initially providing people with information on government spending can potentially have downstream effects that contribute to a more politically informed society over time.

3.6 How Information Effects Vary

3.6.1 Prior beliefs

In this section I look at how prior beliefs on government spending condition the effect of information on both incumbent support and political participation. This is particularly important given the results presented so far, which show the information had a powerful effect on attitudes but resulted in little change in incumbent support or political participation on average. The focus on average

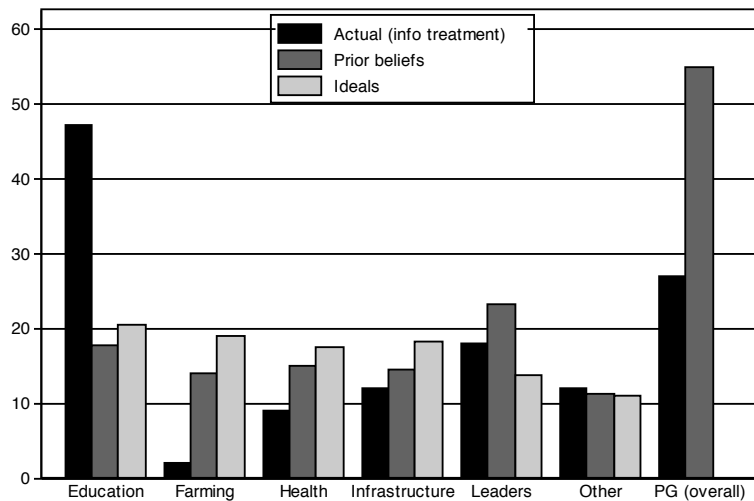


Figure 3.3: Spending priors. *Figure compares actual spending (as revealed by the treatment) to prior ideals and beliefs about government spending across the six categories highlighted in the information campaign.*

treatment effects could be obscuring important underlying changes at the individual level. To assess the role of prior beliefs, I estimate the following regression, where P_{ij} is the measure of prior beliefs on budget share for individual i in village j .

$$Y_{ij} = \alpha_j + \beta_1 T_{ij} + \beta_2 P_{ij} + \beta_3 (T_{ij} * P_{ij}) + \epsilon_{ij} \quad (3.2)$$

I measured participants' initial ideals and beliefs about public spending using a district budget exercise incorporated into the campaign just prior to the information experiment. All participants were asked to use 10,000 rupiah and the spending 'game-board' to illustrate: (a) how *they* would spend they would allocate funds across the categories if they were the district head (ideals); and (b) how they think the district head actually allocates the funds (beliefs). Participants were also asked to estimate the share they thought government actually spent both on all programs for citizens and on programs in the categories of greatest priority to the participant.

Figure 3.3 compares actual spending (as revealed in the treatment) to participants' initial beliefs and ideals across the six spending categories. While actual spending on education was far greater than expected, it was substantially lower than desired or anticipated for health, infrastructure and

farming.¹⁹ Overall, participants estimated that government spent about 55 percent of its revenue on public services and direct programs for them while the campaign revealed the true amount to be about 30 percent in 2008, which is consistent with finding that the information had a substantially negative effect on evaluations of government performance. The fact that the treatment reduced perceptions of politicians as self-interested can plausibly be explained by the fact that individuals believed that politicians were initially taking more for themselves.

I compose several versions of P_{ij} using these measures, where $P_{ij} \in [0, 10]$ and 0 indicates the extreme where none of the budget is spent on that category while 10 is the extreme where all of the budget is spent on that category.²⁰ This implies that a full point increase in the prior is equivalent to a 10 percentage point increase in the perceived share of the budget spent in a category.²¹

As described in Section 3.4.1.2, the treatment emphasized the share of the budget that local politicians allocated to programs and services for citizens (g), routine administration (b), and to themselves in the form of private benefits (r), where $g + b + r = 1$. I focus on how priors on how the budget share spent on programs and services for citizens and on politicians condition the effect of information. I also assess whether the effect of information is conditioned by priors in each of the four spending categories for development: education, infrastructure, health and agriculture. Figure 3.4 presents the distribution of each of the priors measures of interest (the red line denotes the true budget share as revealed to the treatment group).

As can be seen in Table 3.5, while the key priors conditioned the effect of information on perceptions of government performance, there is little evidence that they affected the main incumbent support and political participation outcomes. I look first at whether the effect of information on perceptions that government was performing worse varied depending on prior beliefs on the budget share spent on goods and services for citizens, reported in Panel A of Table 3.5. The positive and significant interaction term indicates that the information caused those with initially high priors

¹⁹The high share of spending on education is actually due to a central government earmark under the *Bantuan Operasional Sekolah* program.

²⁰The analysis in this section focuses on beliefs, although the results are similar for ideals. Available from the author upon request.

²¹Using a dichotomous version of priors equal to 1 if the prior was above the true budget share and 0 otherwise produced nonsensical results, which implies that participants did not register the exact cut-off when updating.

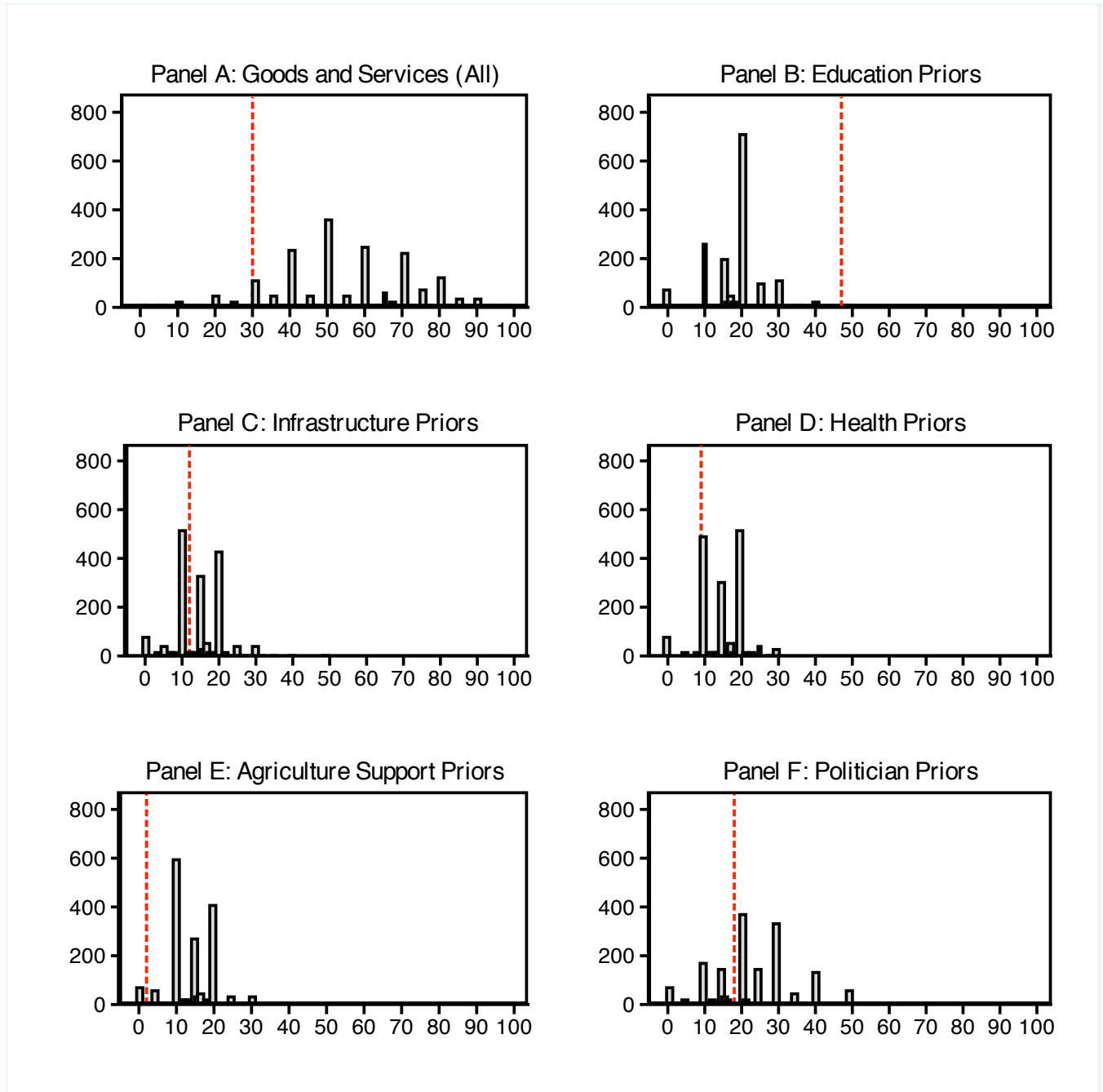


Figure 3.4: Spending Priors, by Category. *This figure presents the pre-treatment distribution of prior beliefs on the different categories of spending highlighted in the information treatment.*

to downgrade their perceptions of government performance substantially more than those with low priors. This is as expected since the information is most negative for those with initially high and positive expectations of government performance. Interestingly, however, the information did not improve evaluations of government performance for those who were poised for positive learning. Even among those who believed that government spent nothing on overall goods ($P_{ij} = 0$)—and therefore learning it actually spent 30 percent could have had the biggest positive effect—the information caused a 21 percentage point increase in the share who viewed government as performing worse than expected. This suggests that other negative information in the campaign, such as the corruption prime, must have overwhelmed this particular dimension.

Panel B shows that the effect of information on beliefs about government performance also depended on prior beliefs about the budget share that politicians allocate to themselves. Here the interaction is significant and negative because the information had a bigger effect for those with low priors (who initially thought government took little for itself). Moreover, in this case, negative learning harmed perceptions of government while positive learning brought rewards: For those who initially believed politicians were taking nothing ($P_{ij} = 0$), the marginal effect of information on viewing government as performing worse was 76 percentage points; for those who initially thought politicians took the most (here $P_{ij} = 8$) the information caused a four percentage point *decrease* in perceptions of government as bad. As can be seen in Panel C, the impact of information also depended on prior beliefs about infrastructure and agriculture but not education or health.

	Performance Worse ⁱ					Punish Incumbent (Postcard) ⁱⁱ					Turnout (Postcard) ⁱⁱⁱ					Learn more about Budget ^{iv}																		
Information	.21**	.76***	.44***	.37***	.41***	.31***	.05	.09*	.00	.05	-.05	-.09	.00	.09**	-.04	.06	-.04	-.09*	.07	.09*	.10*	.05	.08	.06										
	(.09)	(.06)	-(.08)	(.07)	(.08)	(.07)	(.07)	(.05)	-(.06)	(.06)	(.06)	(.06)	(.06)	(.05)	-(.06)	(.05)	(.06)	(.05)	(.06)	(.05)	-(.06)	(.05)	(.06)	(.05)										
Panel A: Goods (Overall)																																		
Priors on goods (g)	-.07***						.00						-.01																					
	(.01)						(.01)						(.01)																					
Info*goods	.06***						-.01						.00																					
	(.02)						(.01)						(.01)																					
Panel B: Politicians																																		
Politician priors (0-10)	.17***						.02						.01																					
	(.02)						(.02)						(.01)																					
Info*politicians	-.11***						-.03						-.03*																					
	(.02)						(.02)						(.02)																					
Panel C: Development categories																																		
Education priors (0-10)		-.09***											-.04*																					
		(.03)											(.02)																					
Info*education		.04											.01																					
		(.04)											(.03)																					
Infrastructure priors (0-10)			-.09***										.06**																					
			(.03)										(.03)																					
Info*infrastructure			.09*										-.03																					
			(.05)										(.04)																					
Health priors (0-10)				-.10***									-.03																					
				(.04)									(.03)																					
Info*health				.06									.04																					
				(.05)									(.04)																					
Agriculture priors (0-10)					-.13***								-.07***																					
					(.03)								(.03)																					
Info*agriculture					.13***								.07*																					
					(.05)								(.04)																					
Constant	.64	-.12	.43	.41	.42	.46	.75	.70	.83	.67	.80	.86	.82	.75	.84	.68	.79	.85	.79	.66	.79	.78	.80	.78										
Sample size	1,711	1,516	1,516	1,516	1,516	1,516	1,515	1,515	1,515	1,515	1,515	1,515	1,515	1,717	1,521	1,521	1,521	1,521	1,521	1,717	1,521	1,521	1,521	1,521	1,521	1,521	1,521	1,521	1,521	1,521	1,521	1,521		

Note: Table reports the results of separate fixed effects regressions of the form $Y_{ij} = \alpha_j + \beta_1 T_{ij} + \beta_2 P_{ij} + \beta_3 (T_{ij} * P_{ij}) + \epsilon_{ij}$ for different P_{ij} reported in the rows. All P_{ij} are measured in the survey pre-treatment as described in the text. Standard errors are clustered at the village-level except when there is negative intracluster correlation, in which case uncorrected standard errors are reported.

ⁱ Elected leaders in the district government are doing a [worse job (=1)/better job (=−1)/the same job (=0)] than you thought they were?

ⁱⁱ Returned postcard and sanctioned=1, returned postcard and rewarded=−1, abstained=0.

ⁱⁱⁱ Returned postcard=1, abstained=0.

^{iv} How interested are you in learning more about how the district government spends money in the budget? (interested=1)

Table 3.5: The Effect of Information Conditional on Prior Spending Beliefs

Yet, there is little evidence that individual priors conditioned the effect of information on incumbent support, participation, and willingness to learn more about the budget. One possible explanation for this is that the priors are working in competing directions. To assess this, I test a triple interaction that captures the effect of information on political behavior for participants that experienced negative learning on both overall goods and politicians, positive learning on both dimensions, and mixed learning. The results in Figure 3.5 and Table 3.6 are telling: For those who only learned positive things along these dimensions, the treatment resulted in both better evaluations of government performance (Panel A) and lower turnout (Panel B). In contrast, for those who learned at least some negative information, the information caused both a worsening view on government performance and higher turnout. This was not the case for willingness to learn more about the budget (Panel C), however, which suggests that the desire to remain informed in the future was not driven by any specific information that participants learned.

	Performance worse ⁱ	Participation (Postcard) ⁱⁱ	Learn about budget ⁱⁱⁱ
	(1)	(2)	(3)
Treatment	.75*** (.21)	.40** (.16)	.03 (.14)
Priors on overall goods (g)	-.04 (.03)	.04* (.02)	-.01 (.02)
Treat*g	-.01 (.04)	-.06*** (.03)	.01 (.03)
Priors on politicians (p)	.19*** (.06)	.11** (.04)	.00 (.04)
Treat*p	-.24*** (.08)	-.18*** (.06)	.01 (.06)
g*p	-.01 (.01)	-.02*** (.01)	.01 (.01)
Treat*g*p	.03* (.01)	.03*** (.01)	.00 (.01)
Constant	.17	.58	.73
Sample size	1488	1493	1493

Note: Table reports results from a triple interaction of the form $Y_{ij} = \alpha_j + \beta_1 T_{ij} + \beta_2 P_{ij}^1 + \beta_3 (T_{ij} * P_{ij}^1) + \beta_4 P_{ij}^2 + \beta_5 (T_{ij} * P_{ij}^2) + \beta_6 (P_{ij}^1 * P_{ij}^2) + \beta_7 (T_{ij} * P_{ij}^1 * P_{ij}^2) + \epsilon_{ij}$ for priors on government spending on citizens and politicians. All P_{ij} are measured in the survey pre-treatment as described in the text. Standard errors are clustered at the village-level except when there is negative intracluster correlation, in which case uncorrected standard errors are reported.

ⁱ Elected leaders in the district government are doing a [worse job (=1)/better job (=−1)/the same job (=0)] than you thought they were?

ⁱⁱ Returned postcard=1, abstained=0.

ⁱⁱⁱ How interested are you in learning more about how the district government spends money in the budget? (interested=1)

Table 3.6: Effect of Information on Political Behavior Conditioned on Beliefs

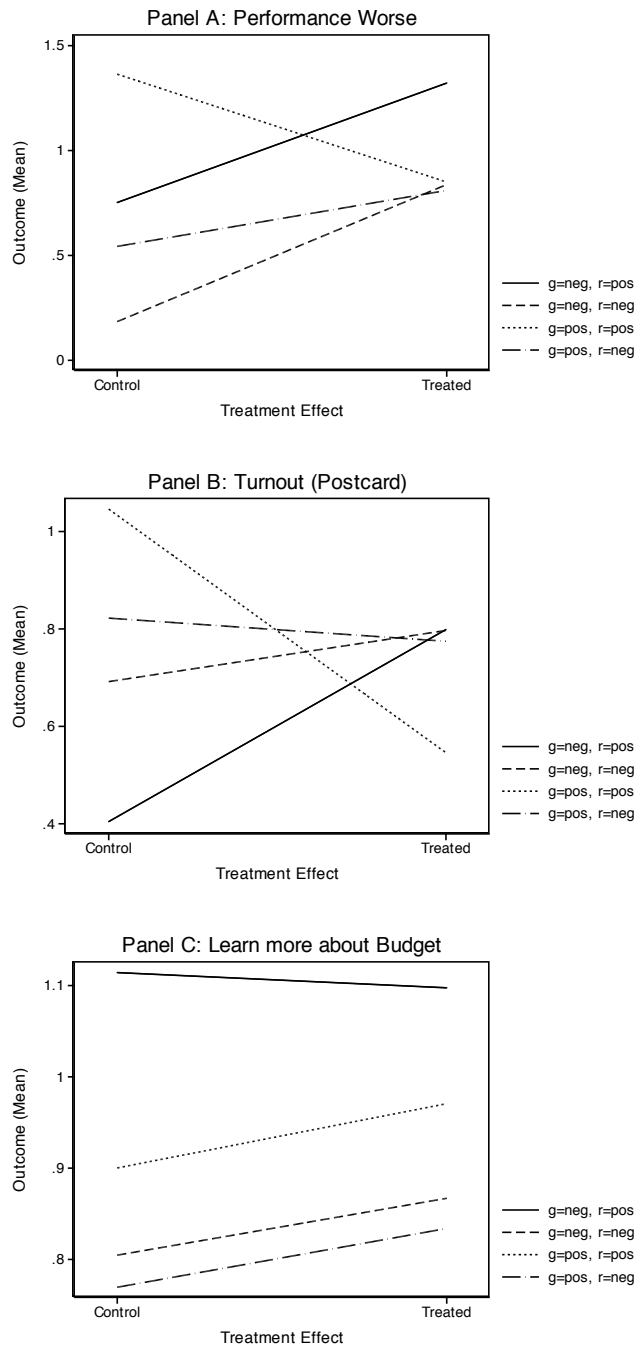


Figure 3.5: Priors on Actual Spending. *This figure shows the triple interaction between the information treatment, prior beliefs on government spending (g), and prior beliefs on how much politicians allocated to themselves (r). Substantively, this can be interpreted as assessing whether the interaction between the information and priors on g also depends on the priors on r. Results are reported for three main outcomes of interest as defined in Table 3.*

Overall, the evidence suggests that prior beliefs on two dimensions—the share of revenue that politicians spend on goods and services for citizens and take for themselves—indeed conditioned the effect of information on perceptions of government as performing worse than previously expected. There is little indication that individual priors conditioned incumbent support or political participation, however.

I explore the possibility that this is because participants received information on multiple dimensions and these dimensions can work in complementary or competing ways. I find that information decreased participation for those who learned positive information on multiple dimensions but increased participation among those who obtained at least some bad news. These findings suggest the need to account for how multiple dimensions of information affect political decisions at the individual-level. Finally, participants' willingness to stay informed in the future did not depend on prior beliefs. This suggests that the treatment inspired future monitoring through some channel other than the content, possibly heightened confidence or capacity.

3.6.2 Other contextual factors

As a final step, I explore how other individual characteristics affect the impact of information on political behavior. I begin with level of education, which is a strong predictor of how well a person can understand and process new information. Panel A of Table 3.7 investigates whether the impact of information on the four main outcomes of interest varied depending on the level of education, where all those with no formal education were coded as 1 and those who had at least completed primary school were coded as zero. While those with some formal education were more likely to acknowledge the information treatment was new, there is little evidence that effects on incumbent support or political participation depend on education. The use of colorful visuals and interactive exercises in the campaign was designed intentionally to facilitate comprehension among those with low levels of education; these results suggest that these efforts succeeded.

In contrast, initial demand for better information on government spending did condition the effect of information on both incumbent support and political participation. Demand for information is captured by the pre-treatment survey question: “How interested are you in learning more about

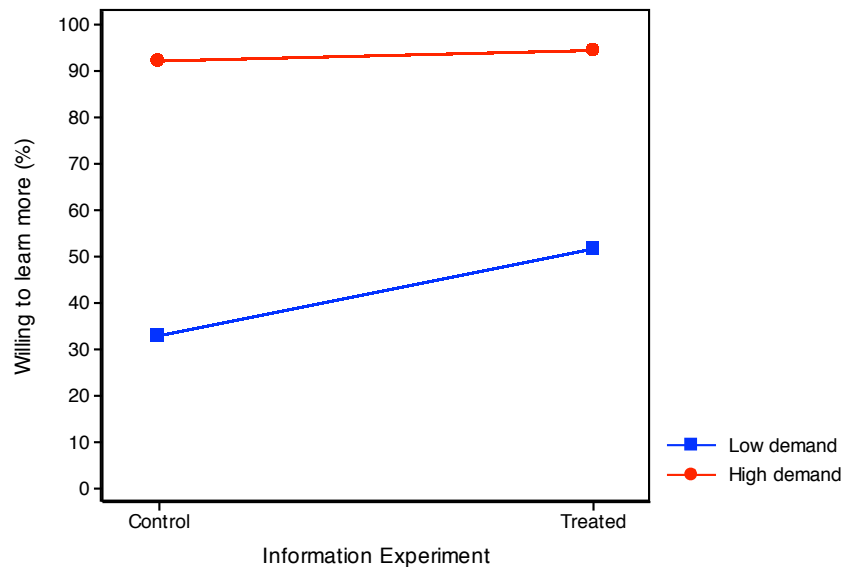


Figure 3.6: Demand for information. *The figure shows how the effect of information on willingness to remain informed in the future varies depending on initial demand for that information. Demand for information is measured both pre- and post-treatment with the survey question: “How interested are you in learning more about how district government spends money in the budget” (interested=1).*

how the district government spends money in the budget.” As can be seen in Panel B, those who initially wanted the information were also significantly more likely both to return the postcard and sanction the incumbent. Interestingly, whether participants wanted to remain politically informed also varied by initial demand; those who were originally *un*-interested in the information expressed the greatest willingness to learn more about government in the future (see also Figure 3.6). This reinforces the finding that one-off information provision can potentially have beneficial downstream effects on the overall information environment by elevating demand for information among those who initially seem unengaged.

Finally, I check whether the effect of information depended on poverty and sex, both factors that are closely associated with demand for public goods and, consequently, the potential relevance of information on government spending performance. Panel C presents shows that the effect of the information did not have a bigger impact on women than men for any of the main outcomes of interest. For poverty, I employ a subjective measure where an individual is coded as 1 if they

consider their household among the poorest in the village and 0 otherwise.²² The poor were substantially more likely to return their postcard and use it to punish the incumbent than the better-off. They also expressed a greater interest in remaining better informed in the future. This indicates that the kind of information intervention employed in this paper can succeed at mobilizing a target, but traditionally politically marginalized, audience.

3.7 Conclusion

This paper examines the impact of better information on government spending on incumbent support and political participation at the individual level. In using an experiment, in which the treatment group received better information on actual government spending performance, I identify the causal effect of better information on political behavior. Theories of information are predicated on the notion that people update prior beliefs when confronted with new information. This is one of the first studies to use rich pre-treatment survey data on individuals' expectations about government spending to examine how the effect of information varies depending on prior beliefs.

The results suggest that the information had a powerful effect on causing individuals to view government as performing substantially worse than previously expected. Despite this strong evidence of attitudinal change, there is little indication that the information decreased incumbent support or affected levels of participation on average. Only when exploring how costs of political participation and prior beliefs matter does it become clear that negative information increased participation. These results are consistent with the predictions of expressive voting models that information increases turnout when it strengthens the perceived benefits of taking political action (Banerjee et al., 2011). They also highlight the importance of thinking about the different dimensions that information operates under and how individuals might weight those dimensions in understanding how political decisions are made at the individual-level.

Another significant finding is that one-off information provision had a positive effect on individuals' desire to remain informed in the future. Moreover, this gain comes primarily from those who

²²Income data is notoriously unreliable.

were initially uninterested in becoming better informed. This finding sheds light on how information might buttress non-electoral forms of accountability, like public scrutiny. It also has important policy implications in suggesting that one-off information campaigns—initiated by the media or civil society, for instance—could stimulate demand for information and an overall improvement to the information environment over time.

	Info New ⁱ				Performance Worse ⁱⁱ				Punish Incumbent ⁱⁱⁱ				Turnout (Postcard) ^{iv}				Learn more about Budget ^v				
Information	.36***	.29***	.34***	.36***	.49***	.49***	.49***	.50***	.01	-.05	-.02	.05	.02	-.04	-.01	.05*	.07***	.18***	.03	.10***	
	(.03)	(.04)	(.03)	(.03)	(.03)	(.05)	(.04)	(.04)	(.03)	(.04)	(.03)	(.03)	(.02)	(.03)	(.03)	(.03)	(.02)	(.04)	(.02)	(.02)	
Panel A: Education																					
Education (low) ^a	.19***				-.25***				-.02				.03						-.14***		
	(.04)				(.05)				(.04)				(.03)						(.04)		
Info*education	-.10**				.10*				.05				-.01						.04		
	(.05)				(.06)				(.04)				(.04)						(.05)		
Panel B: Demand for Information																					
Want info ^b	-.03				.06				-.06				-.09***						.59***		
	(.05)				(.04)				(.04)				(.03)						(.03)		
Info*Want info	.07				.03				.10**				.09**						-.16***		
	(.05)				(.06)				(.05)				(.04)						(.04)		
Panel C: Relevance of Information																					
Female ^c				.09***				-.02				.06**				.05**					.02
				(.03)				(.04)				(.03)				(.02)					(.02)
Info*Female				-.05				.03				-.05				-.05					-.05
				(.03)				(.05)				(.04)				(.03)					(.03)
Poor ^d			.02					-.04				.00				.02					-.10***
			(.04)					(.05)				(.04)				(.03)					(.03)
Info*Poor			.00					.06				.11**				.07*					.11***
			(.04)					(.06)				(.05)				(.04)					(.03)
Constant	.46	.54	.51	.47	.30	.19	.25	.24	.74	.78	.74	.71	.77	.84	.76	.75	.79	.33	.79	.74	
N	1862	1860	1863	1863	1850	1848	1851	1851	1856	1854	1857	1857	1862	1860	1863	1863	1862	1860	1863	1863	1863

Note: Table reports the results of separate fixed effects regressions of the form $Y_{ij} = \alpha_j + \beta_1 T_{ij} + \beta_2 X_{ij} + \beta_3 (T_{ij} * X_{ij}) + \epsilon_{ij}$ for different X_{ij} reported in the rows. All X_{ij} are measured in the survey pre-treatment. Standard errors are clustered at the village-level except when there is negative intracluster correlation, in which case uncorrected standard errors are reported.

^aNo formal education=1, completed at least primary school=0.

^bHow interested are you in learning more about how district government spends money in the budget? (interested=1)

^cI would like you to think of your village in terms of three levels of poverty/wealth. Imagine that each level has about the same number of households in it. In your opinion, is your household on the lowest level (=1), the middle/top level (=0).

^dFemale=1, male=0

ⁱWould you describe the information you just received as [totally new (=1)/somewhat new (=0)/not new at all (-1)]

ⁱⁱElected leaders in the district government are doing a [worse job (=1)/better job (=0)/the same job (=0)] than you thought they were?

ⁱⁱⁱReturned postcard and sanctioned=1, returned postcard and rewarded=-1, abstained=0.

^{iv}How interested are you in learning more about how the district government spends money in the budget? (interested=1)

Table 3.7: The Effect of Information Conditional on other Contextual Factors

Chapter 4

Is Bigger Always Better? The Effect of Windfall Size on Welfare and Conflict

4.1 Introduction

For the many developing countries that rely on natural resources or foreign aid for revenue, such windfalls have the potential to foster growth and development. Yet, as large discretionary inflows of income, revenue windfalls can be more harmful for development than beneficial. Natural resources are associated with poor economic performance and rent-seeking (Sachs and Warner, 2001; Leite and Weidemann, 1999). Others have argued that foreign aid often fails to facilitate development (Boone, 1996; Burnside and Dollar, 2000; Djankov, Montalvo and Reynal-Querol, 2008). Increasingly, researchers are asking the question: Why are revenue windfalls beneficial in some contexts and harmful in others?

This paper investigates whether the impact of windfalls on development depends on the underlying degree of fragmentation and power competition in society. I do this by looking at how relations between former combatants and civilians conditioned the effectiveness of aid windfalls at the village-level in post-conflict Aceh. The BRA-KDP aid program was implemented in Aceh in 2007 to facilitate reconstruction following nearly 30 years of separatist conflict between the Free Aceh Movement (*Gerakan Aceh Merdeka*, or GAM) and the Indonesian government. The central component of the program was the disbursement to villages of varying-sized aid windfalls for local development. BRA-KDP also promoted a transparent and inclusive process for village decision-making on how to spend the windfall. Yet, as is typical in post-conflict contexts, recipient villages varied greatly on the ex ante degree of amicability between civilians and resettled former combatants. Moreover, BRA-KDP potentially, if unintentionally, exacerbated divisions between these groups: The program sought primarily to deliver benefits to civilian conflict victims and prohibited former combatants from directly benefitting. This paper investigates whether bigger windfalls incentivized former combatants to appropriate the wealth in villages where relations with civilians were already contentious as opposed to those in which they were more cooperative. It explores, in turn, whether such appropriation undermined the economic benefits of bigger windfalls for civilians and exacerbated social divisions.

In exploring how social fragmentation and competition affect the allocation of resources, this

paper draws on a rich political economy literature. It is widely appreciated that more ethnically fragmented societies are less successful at public goods provision and strong economic performance (Easterly and Levine, 1997; Alesina, Baqir and Easterly, 1999; Alesina and Ferrara, 2005; Habyarimana et al., 2007). It has also long been suggested that windfalls contribute to conflict onset by exacerbating grievances or giving rebels greater incentives to capture the bounty (Collier and Hoeffler, 2005b; Humphreys, 2005; Ross, 2004). Particularly relevant to this paper is an influential literature on group rent-seeking contests, which focuses on how both windfall *size* and the pre-existing degree of social fragmentation determine whether windfalls are beneficial or harmful for development (Tullock, 1980; Tornell and Lane, 1999; Lane and Tornell, 1996; Tornell and Lane, 1998; Svensson, 2000). The key insight from this literature is that windfalls are good for development when they arrive in settings where groups are homogeneous or cooperative; they yield few gains when received in settings where multiple powerful and competitive groups exist.

To identify how social fragmentation conditions the impact of aid windfalls on local development in Aceh—ruling out possible reverse causality or other potential explanatory variables—I exploit a feature of how windfall amounts were assigned to villages. Specifically, the size of the windfall that each village received was determined on the basis of arbitrary cutoffs in two continuous measures of eligibility. I use a regression discontinuity around a subset of the 12 thresholds created by the BRA-KDP assignment process to estimate the causal effect of bigger windfalls.¹ The data in this paper comes from original surveys conducted with a random sample of civilians, former combatants, and village heads in 212 villages that participated in the BRA-KDP program. The surveys provide measures not only for the main outcomes of interest—appropriation, economic welfare, and social divisions—but also for how outcomes vary across ‘high’ and ‘low’ competition villages, or villages where ex-GAM were likely more and less predisposed to compete for the wealth based on both group strength and interest alignment with civilians.

The empirical approach taken here improves on previous studies on aid windfalls and development in several ways. Most existing studies use cross-national aggregate data and observational

¹It should be emphasized that this paper identifies the effects of windfall *size*. It does not compare villages that received aid windfalls to a ‘control’ group that did not, which is the focus of Barron et al (2009).

or instrumental variables approaches to study the relationship between aid and development.² Such approaches raise concerns about cross-country comparisons and whether the assumptions for causal identification hold. Using a regression discontinuity approach from data collected within a single geographic region has the potential to identify cleanly the causal effect of windfall size on development at the local level.³ Moreover, using micro-level data makes it possible to explore new contexts for group competition. A central empirical challenge for the rent-seeking contest literature has been deriving measures of the existence of multiple powerful and competing groups. Previous studies have tended to use measures of ethnic fractionalization in cross-national tests (Svensson, 2000; Hodler, 2006). Yet, relying on ethnic fractionalization as the proxy for group competition overlooks a number of other relevant settings in which such dynamics might occur. This paper extends such analyses by focusing on power dynamics in a post-conflict setting at the local level.

The main results show that, in high competition villages, former combatants were more likely to appropriate the windfall and receive benefits from BRA-KDP than in low competition villages. This is consistent with the hypothesis that bigger windfalls incentivize appropriation in more divided societies. Yet, contrary to predictions, there is little indication that bigger windfalls resulted in fewer economic benefits for civilians in high competition villages, despite appropriation by former combatants. The findings also do not indicate that bigger windfalls exacerbated social tensions in high vis-a-vis low competition villages, as originally hypothesized. One possible explanation for these results is that bigger windfalls caused more high competition villages to distribute funds equally across all households, although this contradicted the program's intention to deliver greater benefits to the most conflict-affected civilian households. Yet, if this was the case, the results raise questions about why bigger windfalls did not deliver even *greater* economic benefits to civilian households in low competition villages compared to high competition ones. Thus, while this paper is motivated by a concern for the effect of bigger windfalls in socially fragmented places, the results also suggest the need to consider decision-making dynamics in more cohesive communities as well.

²For a review of this literature, see Wright and Winters (2010).

³This paper is most closely related to recent studies that use regression discontinuities to identify the effects of bigger windfalls (in the form of central transfers) on welfare and governance outcomes at the municipal level in Brazil (Brollo et al., 2010; Litschig, 2010; Litschig and Morrison, 2010).

This paper contributes not only to the literature on windfalls, aid, and development but also to a growing body of research on whether community-driven development (CDD) or community-driven reconstruction (CDR) programs deliver welfare benefits, strengthen local democratic institutions, and foster social cohesion (Mansuri and Rao, 2004; Bank, 2007; Fearon, Humphreys and Weinstein, 2009; Casey, Glennerster and Miguel, 2011; Gugerty and Kremer, 2008). By investigating whether aid windfalls actually exacerbate community tensions in certain contexts, this paper emphasizes the need to think about the different contexts in which program impacts might vary. Additionally, in focusing on windfall size, it highlights a so far under-discussed question: What is the optimal windfall size for facilitating development in different contexts?

This paper proceeds as follows. Section 4.2 describes the BRA-KDP program and motivates the inquiry into why the nature of power relations between civilians and former combatants could condition its effectiveness. Section 4.3 draws on theory to formulate hypotheses for testing. The data and strategy for causal identification are described in Section 4.4. Section 4.5 presents evidence of how bigger windfalls affect appropriation, economic welfare and social divisions, and whether that effect varies in villages with more and less contentious relations between civilians and former combatants. Section 4.6 concludes.

4.2 The Context

For nearly 30 years, beginning in the mid-1970s, GAM waged a separatist struggle in Aceh against the Indonesian government. While the conflict evolved in several stages, civilians frequently suffered the brunt of hostilities as the Indonesian military sought to undercut popular support for GAM by terrorizing suspected civilian supporters. The conflict resulted in approximately 30,000 deaths as well as widespread instances of murder, torture, rape, internal displacement and economic destruction.

The peace agreement reached in August 2005 contained provisions to reintegrate GAM combatants and to provide assistance to civilian conflict victims. The Aceh Peace Reintegration Agency (*Badan Reintegrasi-Damai Aceh*, or BRA) was established to manage this process. In an ef-

fort to reach conflict-affected communities broadly, BRA opted for a partnership with the World Bank-supported Kecamatan (Subdistrict) Development Program (KDP). The resultant BRA-KDP project was designed to deliver assistance to local conflict-affected communities. Specifically, it provided aid windfalls ranging in size from 60 to 170 million rupiah (about USD \$6,000-\$17,000) to more than 1,700 villages in 67 subdistricts.

On one hand, BRA-KDP emphasized both welfare gains and social reconciliation among conflict-affected groups. The main goal of the program was to improve the welfare of the most conflict-affected civilian households. It aimed to do this by having villages allocate funds to livelihood support through a transparent, democratic, and inclusive decision-making process. BRA-KDP was implemented by following the same steps in all villages. First, villages held information meetings about the program. Community members subsequently identified the conflict-affected households and developed project proposals that addressed their needs, although communities had discretion over whether to use the funds for local public goods like infrastructure, private goods like livelihood assistance, or some combination thereof.⁴ Villagers then voted on proposals in an open meeting and aid windfalls were deposited in the village's bank account upon final approval by the subdistrict government.

Yet, BRA-KDP potentially, if unintentionally, exacerbated social divisions between civilians and former combatants. Former combatants were expressly prohibited from benefitting directly from BRA-KDP. While this was because ex-combatants were supposed to receive assistance through other channels, at the time BRA-KDP was implemented, many were growing frustrated that their reintegration funds had not been distributed fairly or transparently (Morel, Watanabe and Wrobel, 2009). Anecdotal evidence suggests that former combatants attempted to extort funds or otherwise influence how funds were allocated within communities (Morel, Watanabe and Wrobel, 2009). Reportedly, in some villages, former combatants demanded that funds be spent on public goods that would benefit all community members. In other villages, former combatants pressured community

⁴Communities were encouraged to use their own subjective criteria based on local knowledge but were also provided with guidance from BRA. BRA defined conflict victims as those who had experienced the death or disappearance of family members due to conflict, house or property destruction, displacement, physical disability, psychological trauma, or loss of economic livelihood.

members to divide the windfall equally among all households in the village, called *bagi rata*, despite the objections of program designers. Such equal distribution was strongly discouraged by program designers because it divided the windfall into such small amounts that it could not be used meaningfully for investment and sustainable welfare improvements. There are also reports that former combatants blatantly attempted to extort funds in some villages.

Of particular relevance to this paper, a World Bank report conjectures that GAM tried different strategies in different contexts (Morel, Watanabe and Wrobel, 2009). Namely, GAM extorted funds in villages where it dominated local politics; demanded *bagi rata* in villages where relations with civilians were fractious but it had less power; and supported civilians in villages where GAM was popular and not too powerful. A quasi-experimental impact evaluation of the BRA-KDP program also finds evidence that the program had a negative effect on civilian acceptance of former combatants (Barron et al., 2009). All in all, BRA-KDP presents an important setting for testing political economy theories of the conditions under which bigger windfalls induce competition and appropriation and undermine development.

4.3 Literature and Hypotheses

Political economy theory provides insights into why bigger windfalls improve development in some contexts but not in others. Several recent studies have investigated how windfalls alter politicians' incentives to secure political control. For instance, Robinson et al (2006) theorize that resource booms increase the value to politicians of holding office and induce them to allocate resources to patronage. Similarly, Brollo et al (2010) show that bigger windfalls cause lower quality politicians to seek office. While these papers present compelling theories for why bigger windfalls affect politicians' incentives, the focus on politicians is likely less relevant in the BRA-KDP context. To minimize the risk of elite capture, BRA-KDP was implemented by facilitators employed directly by the program itself and not affiliated with the village government. While the reliance on facilitators does not eliminate the possibility that local elites influenced the program—as discussed more below—it does suggest the need to focus attention on group dynamics in the allocation process.

Another prominent literature, influenced by the seminal work of Tullock (1980), reveals how competition among multiple powerful groups over the allocation of funds determines whether windfalls are beneficial or harmful to development. Tornell and Lane (1999; 1996; 1998) use a model of economic growth to explain why windfalls cause groups to make excessive demands for redistribution that undermine economic growth, a phenomenon they dub the *voracity effect*. They show that positive terms of trade shocks—for instance from foreign aid inflows or natural resource booms—spur growth when there is only one group in society, when there are multiple groups that cooperate, or when multiple competing groups are constrained by institutions in their demands for transfers. In societies with multiple competing groups and few institutional constraints—a situation common in many developing countries—windfalls trigger excessive demands for transfers that harm growth.

More immediately relevant to BRA-KDP, Svensson (2000) investigates how group competition affects the impact of aid windfalls on rent-seeking and corruption. In his model, groups can either decide to spend the windfall on public goods or compete to appropriate the wealth for private gain. When groups are capable of cooperation, all resources are spent on public goods and optimal welfare gains are realized. When groups compete, the windfall is appropriated and welfare is lower because rent-seeking is a costly activity. Svensson focuses on how cooperation is sustained among groups that engage in repeated interactions over resource allocation and highlights the role that punishment plays in preventing groups from deviating from cooperation. As windfalls get bigger and surpass a critical threshold, however, the incentives for groups to capture the wealth for private gain exceeds the drawbacks of punishment. All in all, this model clarifies the micro-foundations for why bigger windfalls might have beneficial effects in homogeneous or cooperative societies but not in competitive ones.

In addition to harming development and welfare, windfalls in such contexts most likely result in heightened social divisions and antagonism between groups, with important implications for social stability. While this notion follows implicitly from Tornell and Lane (1999) and Svensson (2000) and has also been captured in models in which group competition and conflict are synonymous (Hodler, 2006).

I draw on theories of group rent-seeking competition in formulating three hypotheses for testing

in Aceh as to why *bigger* windfalls undermine development:

H1 *Bigger windfalls induced former combatants to appropriate the wealth in villages where relations with civilians was contentious, compared to villages with more cooperative relations.*

H2 *Bigger windfalls improved economic welfare for civilians in villages where relations with former combatants was cooperative compared to more divided villages.*

H3 *Bigger windfalls exacerbated social divisions between former combatants and civilians in villages with already contentious relations compared to villages with more cooperative ones.*

The first hypothesis predicts that bigger windfalls gave former combatants stronger incentives to appropriate wealth in villages with a history of more contentious relations with civilians. In this context, appropriation could take a variety of forms, from blatant extortion to more subtle efforts to pressure communities to spend on public goods or to distribute the wealth equally across all households in the village. The second two hypotheses follow from the first in predicting that appropriation would result in a reduction in the economic benefits of bigger windfalls for civilian households and a deepening of the social divisions between former combatants and civilians in high competition villages compared to low competition ones.

4.4 Identification Strategy

The way in which the BRA-KDP program was implemented makes it possible to identify whether a bigger windfall caused appropriation, undermined economic welfare, and exacerbated social divisions. Because windfall size was determined on the basis of arbitrary cutoffs in continuous measures of two assignment variables—subdistrict conflict intensity and village population—it is possible to use the cutoffs and a regression discontinuity approach to estimate the causal effect of a bigger

windfall. This section first describes the survey data and then details the regression discontinuity approach.

4.4.1 Data

This paper employs original survey data from civilians, former combatants, and village heads. The surveys ask about individual, household, and village-level characteristics a year after the BRA-KDP program, providing a variety of measures for the main outcomes of interest. Additionally, the surveys included questions about the conflict period, which provide relevant ‘pre-treatment’ covariates at the heart of the analysis of group competition.⁵ Of the 1,724 villages in 67 subdistricts that received BRA-KDP grants, approximately $\frac{1}{8}$ were randomly selected, producing a sample of 212 villages.⁶

The main source of data comes from the household surveys conducted with civilians. Within each of the 212 villages, five households were randomly sampled from an updated list of all households in the village. A main respondent in each household was then selected from all household members aged 18 to 65 using simple random sampling. While most measures for the main outcomes of interest were only included on the civilian survey, where possible I also draw on survey data from former combatants to corroborate the evidence. In each village, former combatants were sampled with a $\frac{6}{10}$ probability from an exhaustive list compiled in collaboration with village leaders and KPA (the political incarnation of GAM) representatives.⁷ A survey was also conducted with the head of each sampled village and this data primarily provides village-level context measures. Table 4.4 provides descriptive statistics for the main outcomes of interest.

⁵The surveys were commissioned by the World Bank in Indonesia and designed in collaboration with Patrick Barron (formerly at the World Bank), Macartan Humphreys (Columbia University), Yuhki Tajima (UC-Riverside), and Jeremy Weinstein (Stanford University). Implementation was managed by the survey firm A.C. Nielsen and conducted from July-September 2008, approximately 12 months after BRA-KDP had concluded.

⁶For more on the sampling methodology see the research design memo for the Aceh Reintegration and Livelihood Studies available on the author’s website. While 219 of the villages surveyed were in BRA-KDP subdistricts, seven were dropped from the analysis because they were newly formed and did not exist at the time of initial BRA-KDP treatment assignment, and the parent village is not known.

⁷A former combatant was defined as anyone who had fought in GAM’s fighting arm or was in the military command structure for at least one month since 1998. In 21 of the 212 villages—predominantly former GAM strongholds—we were not permitted to conduct surveys with former combatants.

	Range		Mean	SD	C.I.		n
	<i>min</i>	<i>max</i>			<i>Lower</i>	<i>Upper</i>	
Panel A: Appropriation (%)							
Did not benefit enough people	0	1	.06	.02	.02	.10	282
Did not benefit victims enough	0	1	.07	.02	.03	.11	282
Benefitted ex-combatants too much	0	1	.15	.03	.10	.20	281
Corruption	0	1	.06	.02	.03	.09	268
Extortion	0	1	.04	.01	.01	.06	269
Panel B: Project Selection							
Unimportant activities selected	0	1	.04	.01	.02	.07	282
Village approved equal division	0	1	.48	.03	.41	.55	377
Village approved public goods	0	1	.10	.02	.06	.14	377
Civilians: Received goods from BRA-KDP [†] (%)	0	1	.70	.04	.61	.79	377
Civilians: Received cash	0	1	.91	.04	.83	.98	264
Civilians: Amount of cash received [†] ('000,000 rupiah)	0	6	.64	.06	.52	.75	377
GAM: Received goods from BRA-KDP [†] (%)	0	1	.59	.07	.44	.73	126
GAM: Received cash	0	1	.96	.03	.90	1.02	74
GAM: Amount of cash received ('000,000 rupiah)	0	6	.65	.11	.44	.86	126
Panel C: Welfare (Civilians)[†]							
Assets ('000,000 rupiah)	0	100	16.26	.87	14.50	18.01	377
Protected drinking water (%)	0	1	.41	.05	.30	.51	377
Concrete walls (%)	0	1	.18	.03	.11	.24	377
Land farmed ('000 m2)	0	70	3.47	.37	2.72	4.22	377
Share of school age children in school	0	1	.47	.03	.41	.52	377
Share of HH members sick	0	1	.06	.01	.04	.08	377
Perceive household as poor (%)	0	1	.69	.03	.63	.74	377
Living conditions better now (%)	0	1	.35	.04	.28	.42	377
Panel D: Welfare (Ex-Combatants)							
Assets ('000,000 rupiah)	0	100	17.45	1.00	15.42	19.48	126
Protected drinking water (%)	0	1	.45	.08	.28	.62	126
Concrete walls (%)	0	1	.09	.03	.03	.15	126
Land farmed ('000 m2)	0	70	4.16	.72	2.69	5.62	126
Share of school age children in school	0	1	.38	.05	.29	.48	126
Share of HH members sick	0	1	.05	.01	.03	.06	126
Perceive household as poor (%)	0	1	.74	.04	.66	.82	126
Living conditions better now (%)	0	1	.29	.05	.18	.40	126
Panel E: Divisions (%)							
Divisions: Ex-com and villagers	0	1	.02	.01	.00	.04	376
Benefit more: Former Combatants	-1	1	.08	.05	-.01	.18	371
Benefit more: KPA members	-1	1	-.01	.04	-.10	.07	370
Willing to accept former combatants	0	5	4.92	.02	4.88	4.96	377

Notes: Table reports descriptive statistics for main outcome variables. Panels A, B, and D are for civilian respondents use individual sampling weights, except for Panel B which uses household weights. Panel C contains the same measures as Panel B but for the ex-combatant sample. Individual question wordings can be found in the respective results tables.

[†]Responses in this section weighted at the household (not individual) level.

Table 4.1: Descriptive Statistics for Outcome Variables

		GAM strong	
		No	Yes
Relations Cooperative	Yes (Majority supported GAM)	Unlikely $i=95$ $j=14$	Maybe $i=212$ $j=27$
	No (Majority supported Indonesian government or neither)	Maybe $i=222$ $j=37$	Likely $i=179$ $j=26$

The table shows the over-lapping measures of GAM strength and cooperation taken at the village-level (in the village head survey) and used as the main conditional measure in the analysis. GAM strength is determined by whether the village was a GAM stronghold during the final phase of the conflict. Relations are considered cooperative if the majority of village members supported GAM during the conflict (as opposed to the Indonesian military or neither side). The first row in each cell shows the predicted likelihood of appropriation, where appropriation is most likely where GAM was strong but relations were not cooperative (bottom right cell). The analysis reduces this 2x2 table to a binary measure, where all observations are coded 1 in the likely appropriation context and 0 otherwise. The table also shows the sample sizes for individuals i in villages j obtained by pooling thresholds 1 and 2.

Table 4.2: GAM Strength and Cooperation

4.4.1.1 Measuring Group Competition

The main goal of this paper is to examine whether the effect of aid windfalls on economic welfare and social reconciliation varied across villages depending on the likelihood that groups competed to appropriate the funds. As discussed in Section 4.3, the likelihood of group competition is a function of the relative power of the different groups and the ability for groups to cooperate. The main type of competition of interest in this paper is between civilian conflict victims, who were entitled to receive the lions share of benefits from BRA-KDP, and former GAM combatants.

To form a single measure of a village's predisposition to experience competition, I overlap two measures from the village head survey. As a measure of GAM strength, I code all villages that were considered GAM strongholds during the final years of the conflict ('basis GAM') as 1 (and 0 otherwise). To measure whether relations with civilians were likely cooperative, I code all villages in which "the majority (at least half) of the members of the village supported GAM" as 1. Villages that primarily supported the Indonesian government or that supported neither group were coded zero. Table 4.2 shows how I overlap these measures to form my main indicator of group competition. Specifically, I anticipate that group competition is most likely in villages where

GAM was strong but where it did not enjoy majority support, implying contentious rather than cooperative relations with civilians (bottom right cell). In the analysis, I compare the impact of bigger windfalls on outcomes in these *high competition* villages to outcomes in what I refer to as *low competition* villages, namely all those where the possibility of competition was unlikely (because GAM was weak and relations were supportive) or only somewhat likely (because GAM was strong (weak) but relations were cooperative (competitive)).

4.4.2 The regression discontinuity

4.4.2.1 Windfall Allocations in BRA-KDP

The World Bank initially selected 67 subdistricts to participate in BRA-KDP, with all villages in these subdistricts guaranteed a windfall of some amount.⁸ The amount that each village received was determined by its score on two criteria: conflict intensity (measured at the subdistrict level) and village population. The subdistrict conflict intensity score was produced by the World Bank from a factor analysis of several measures of conflict exposure.⁹ The subdistricts were then divided into three groups by imposing two arbitrary cutoffs in the continuous measure. Specifically, 2.5 was used as the cutoff between subdistricts with ‘high’ and ‘medium’ conflict exposure and 1.9 as the threshold dividing ‘medium’ and ‘low’. Villages were also assigned to one of three population categories by establishing a threshold at 300 persons between ‘small’ and ‘medium’ villages and at 700 persons between ‘medium’ and ‘large’ villages.

All in all, by over-lapping the two assignment tables—each with two cut-points—the World Bank produced a 3x3 table with nine cells. Table 4.3 shows the assignment table and the windfall size allocated to each cell of the table. Windfalls ranged in size from 60 million rupiah (approximately

⁸In selecting subdistricts, the World Bank followed a complex but well-defined rule, described in detail in Barron et al (2009). In brief, first a target number of subdistricts was selected in each of Aceh’s 17 rural districts. Subdistricts were then ranked by the conflict intensity score described in this section and were selected up to the target, conditional on having disbursed at least 60 percent of their KDP funds in the previous year. Including this fiscal capacity criterion has an implication for external validity in that only subdistricts with a proven ability to manage windfalls were eligible for BRA-KDP. While subdistrict capacity does not necessarily reflect on village-capacity, it is plausible that those villages least capable of managing windfall wealth are excluded.

⁹The nine measures included the number of conflict victims in the three years preceding 2005, the number of reported clashes between GAM and the military, and perceptions of conflict intensity from survey data. See the complete list in the online appendix.

USD \$6,000) to 170 million rupiah (approximately USD \$17,000). To estimate whether a *bigger* windfall causes appropriation, undermines welfare, and exacerbates conflict, I exploit the arbitrary cutoffs used to determine windfall size using a regression discontinuity (RD) approach. An RD is appropriate when the probability of receiving treatment is determined by an arbitrary cutoff in one (or more) continuous assignment (also known as running) variables (Imbens and Lemieux, 2007; Hahn, Todd and Van der Klaauw, 2001). With an exogenous cutoff, the area in the immediate vicinity on either side resembles a randomized experiment. An unbiased estimate of the treatment effect is obtained by comparing the outcomes for units on either side of the cutpoint, conditional on correctly modeling the relationship between the assignment and running variables.

A particular challenge here is that the 3x3 matrix produced a total of 12 thresholds. The top panel of Figure 4.1 labels the thresholds and the bottom panel shows the distribution of the 212 sampled villages across the nine treatment groups. While it would be ideal to estimate effects at each of the 12 thresholds individually, this is not possible for two reasons.¹⁰ First, there are not enough observations near the cutoffs at some of the thresholds to make analysis possible (see, for instance, thresholds five and six). Second, some cutoffs exhibit sorting, where the density of observations on either side of the cutpoint is discontinuous (see thresholds seven and eight). Such sorting could indicate that assignment scores were manipulated to affect treatment status and introduces selection concerns.

Thus, the main focus of this paper is on estimating the treatment effect at the first threshold. As discussed more in Section 4.4.3 and in the online appendix, this is the only threshold that both has sufficient observations at the cutpoint and that passes the density test. Specifically, the first threshold represents a jump from 120 to 150 million rupiah (\$12,000-\$15,000)—an increase of 30 million rupiah (\$3,000) at the cutoff. This is equivalent to an increase in 100,000 rupiah (\$10) per capita, or 560,000 rupiah (\$56) per household.¹¹ Additionally, I analyze the data by pooling

¹⁰Estimating effects at each threshold would be ideal because it would reveal how both windfall magnitudes and increases in windfall size affect the outcomes of interest. For instance, a jump of 30 million rupiah from 60 to 90 could have a different effect than a jump from 120 to 150 million rupiah. Similarly the impact of a jump of 10 million rupiah from 60 to 70 could vary significantly from a 50 million rupiah jump from 100 to 150.

¹¹This estimate of household size of 5.6 people per household comes from the survey data.

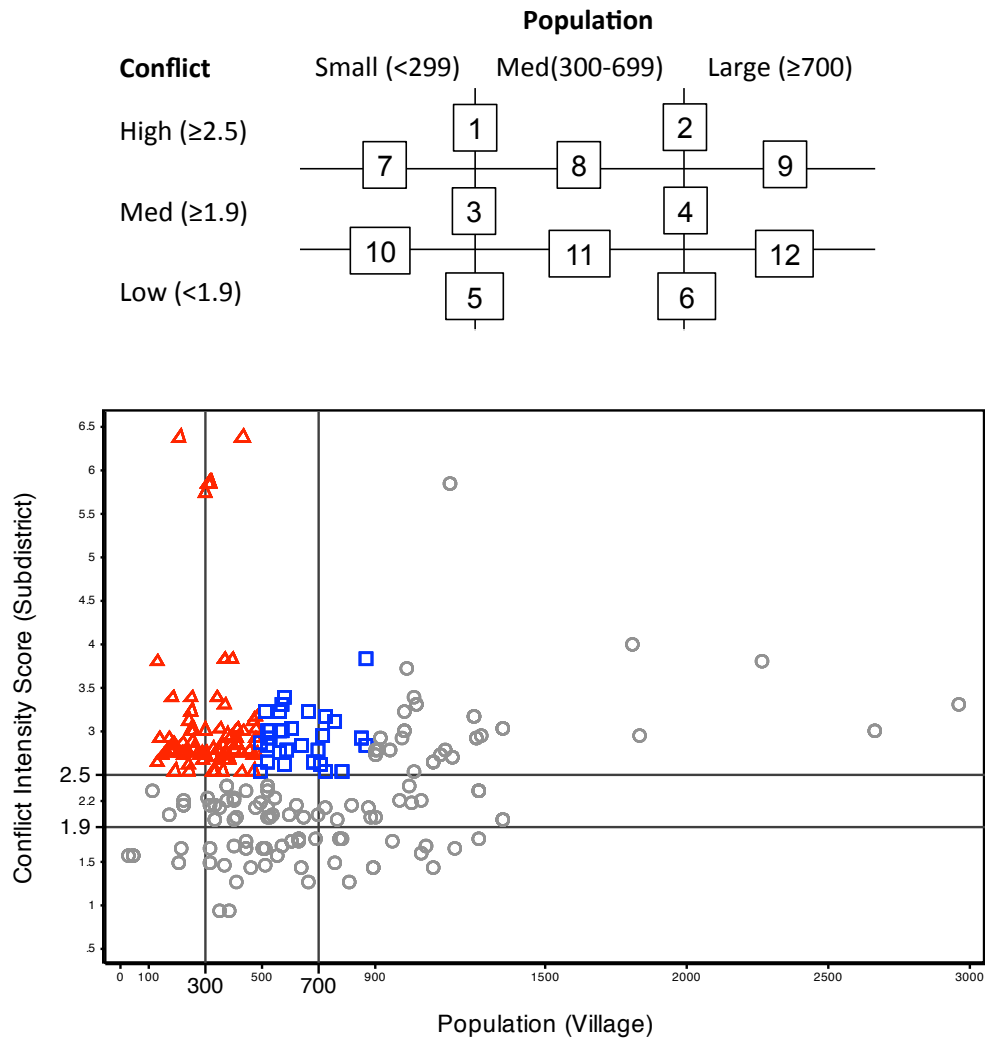


Figure 4.1: The regression discontinuity design. *The top panel shows the 12 thresholds created by BRA-KDP assignment process. The bottom panel shows the distribution across thresholds of actual observations in the data. Analysis in the paper focuses on the two population thresholds in the high conflict areas. Threshold 1 observations are red triangles and are based on a symmetric bandwidth of ± 200 around the population cutoff $P = 300$. Threshold 2 observations are blue squares and are based on a symmetric bandwidth of ± 200 around the population cutoff $P = 700$.*

		Village Population			Diff (P2-P1)	Diff (P3-P2)
		Small (P1) (<299)	Medium (P2) (300-699)	Large (P3) (≥700)		
Conflict Score	High (C1)	120	150	170	30	20
	Medium (C2)	80	100	120	20	20
	Low (C3)	60	70	80	10	10
	Diff (C1-C2)	40	50	50		
	Diff (C2-C3)	20	30	40		

Windfall size expressed in terms of rupiah '000,000. Table shows absolute windfall sizes for the nine different village-level treatments (upper left cells) and the jumps in windfall size across thresholds. The paper focuses on the jumps in windfall size in the high conflict strata (first row), where threshold 1 is a jump of 30 million rupiah from 120 to 150 and threshold 2 is a jump of 20 million rupiah from 150 to 170 million rupiah.

Table 4.3: Village-level Aid Windfalls

observations from thresholds 1 and 2. While there are insufficient observations to obtain a separate effect for the second threshold, this approach both boosts power and allows me to analyze data from both thresholds in the high conflict score strata—the strata of greatest substantive interest in this paper. At threshold 2, windfall size jumps 20 million rupiah from 150 to 170, which is equivalent to an additional \$2.90 per capita or about \$16 per household. By pooling I estimate the effect of an increase in windfall size from about 132 to 154 million rupiah for individuals in 106 villages (53 villages on each side of the cutpoint).

4.4.2.2 Estimation Strategy

Formally, the treatment effect of interest (τ) is the difference in limits as the cutoff is approached from both sides:

$$\tau = E[Y_{ij}^1 - Y_{ij}^0 | P_j = c] = \lim_{P_j \downarrow c} E[Y_{ij} | P_j = c] - \lim_{P_j \uparrow c} E[Y_{ij} | P_j = c] \quad (4.1)$$

where Y_{ij}^1 and Y_{ij}^0 refer to outcomes for individual i in village j in the treatment and control groups, respectively; P_j the assignment variable (population); and c the cutoff (at $P_j = 300$ for threshold 1). This can be estimated in a regression framework using the following equation:

$$Y_{ij} = \alpha + \tau Z_j + f(\tilde{P}_j) + f(\tilde{P}_j)Z_j + X_j + e_{ij} \quad (4.2)$$

where Z_j is a dummy for treatment assignment that equals one for villages to the right of the cutoff (the treatment group) and zero for those to the left (the control). The function $f(\cdot)$ denotes polynomials of \tilde{P}_j , where \tilde{P}_j is the running variable centered at zero (obtained by subtracting the value of the cutoff from the assignment variable). The term X_j denotes pre-treatment covariates included as controls. The error term e_{ij} consists of both individual and village random components; standard errors are clustered at the village level in all analysis. All analysis also takes into account appropriate survey sampling weights to return the population average treatment effect at the threshold.

The baseline model in Equation 2 reflects the fact that obtaining an unbiased estimate of τ requires modeling correctly the relationship between Y_{ij} and \tilde{P}_j in the vicinity of the cutoff (Lee and Lemieux, 2010). Since regression takes into account observations located far from the cutoff, polynomials in \tilde{P}_j and interactions with Z_j reduce the risk that distant data points drive the estimation of treatment effects. The main specification I use is highly flexible: It employs both both linear and quadratic forms of \tilde{P}_j and fully interacts them with Z_j so that separate regressions are estimated on each side of the threshold (a quadratic spline). As a robustness check, I also present results from a logistic regression with a linear spline in the online appendix.¹²

Estimation in RD also depends on the choice of bandwidth, or the size of the neighborhood around the cut-point used in analysis. I restrict the data to a symmetrical population bandwidth of ± 200 around the centered cutoff (an actual population range of 100-500 for $P_j = 300$ and of 500-900 for $P_j = 700$). I pool thresholds 1 and 2 by dividing the observations around the two population thresholds into two groups, with all observations in the population range 100-500 assigned to the first threshold and 501-900 to the control so that there is no overlap. Within each group, I center the population threshold by subtracting the value of the cutoff and pool the groups together such that all observations in the range $[0,200]$ are designated $Z_j = 1$ and from $(0,-200]$ $Z_j = 0$. It should be noted that centering \tilde{P}_j implies that the treatment effect at the cutoff (where $\tilde{P}_j = 0$) is wholly captured in τ .

¹²Because of the singularity of some of the data, it is not possible to get convergence on many logistic models with quadratic splines.

This is a ‘sharp’ RD in that I proceed as if the cutoff completely determined treatment assignment. While the vast majority of villages received windfalls in the amount originally assigned, it is possible that windfall size for some villages differed in actuality (typically called ‘non-compliance’). According to program designers, when there were discrepancies in the number of villages or in village population size, allocations could be readjusted at the village-level, although overall subdistrict allocations could not be changed (Morel, Watanabe and Wrobel, 2009, 45). In the presence of such non-compliance, it is common to use a ‘fuzzy’ RD approach in which the treatment effect is estimated only for those units that complied with their treatment assignment (Angrist, Imbens and Rubin, 1996). In the absence of complete documentation of such non-compliance, as is the case here, the intent-to-treat estimate provided by a sharp RD is appropriate.¹³

Importantly, the main goal of the empirical analysis is not to identify the average treatment effect at the threshold but rather to assess how the impact of a bigger windfall varies across villages that are more or less predisposed to group competition. I accomplish this by introducing into Equation 2 the binary variable for group competition, G_j , discussed in Section 4.4.1.1:

$$Y_{ij} = \alpha + \beta Z_j + \gamma Z_j G_j + f(\tilde{P}_j) + f(\tilde{P}_j) Z_j + G_j + f(\tilde{P}_j) G_j + f(\tilde{P}_j) Z_j G_j + X_j + e_{ij} \quad (4.3)$$

where γ captures the interaction between windfall size and group competition. A positive and significant coefficient would indicate that a bigger windfall caused more appropriation in a divided village than a cohesive one.

4.4.3 Validity tests

Under-pinning the validity of regression discontinuity is the assumption that, when the assignment variable is accounted for, the neighborhood in the immediate vicinity of the cutoff approximates a randomized experiment. One possible violation of this assumption occurs if units manipulate their scores on the assignment variable so as to receive (or avoid) treatment. A widely used check

¹³In one extreme case, all villages in the subdistrict Manyak Payed negotiated a re-allocation of the total subdistrict grant. Another case recorded by the World Bank was in village Gampong Mesjid, which demanded and received an extra five million rupiah from a separate fund. Manyak Payed is not in the high conflict strata and Gampong Mesjid did not enter the sample so these instances of known non-compliance do not affect the analysis.

for such sorting, proposed by McCrary (2008), is a test of the density of the observations in the immediate vicinity of the threshold. A significant difference in the density of observations at the cutoff is regarded as an indication of selection bias. Figure 4.2 presents the results of density tests performed around the first threshold (top panel), the second threshold (middle panel), and both panels pooled (bottom panel). There is no evidence that sorting occurred around these thresholds.¹⁴

Additionally, regression discontinuity designs are based on the assumption that only variables affected by the treatment should exhibit a discontinuity at the cut-off. This implies that there should be continuity in pre-treatment covariates at the cut-point (Lee and Lemieux, 2010). To check this assumption, I conduct placebo tests in which I estimate Equation 2 on several pre-treatment covariates measured at the village level (the level at which treatment was assigned). Panel A reports a test of the pre-treatment covariates used in formulating the group competition measure. Panel B presents general measures of village characteristics, including the terrain of the village and village revenue and expenditures in 2006, that are included as controls in the analysis. Finally, Panel C reports several measures of village conflict-affectedness and proxies for GAM strength, including village conflict-affectedness, the number of GAM living in the village in 2005 (when the conflict ended) and whether GAM was sufficiently supported by the village during the conflict to sleep in the village and plan operations there. While the sample size is small, there are two things to note in this table. The first is that the data suggests a jump in terrain at the threshold, which I account for by including it as a control in all specifications. The second is that there is weak evidence of a jump *downward* in the group competition measure at the threshold. This jump is in the opposite direction of the predictions, however, and should bias me against finding support for the hypotheses.

¹⁴This is implemented in Stata by code provided by McCrary on his website <http://www.econ.berkeley.edu/~jmccrary/DCdensity/>. See the online appendix for additional density tests of the other thresholds, which help to confirm the focus on the first two thresholds.

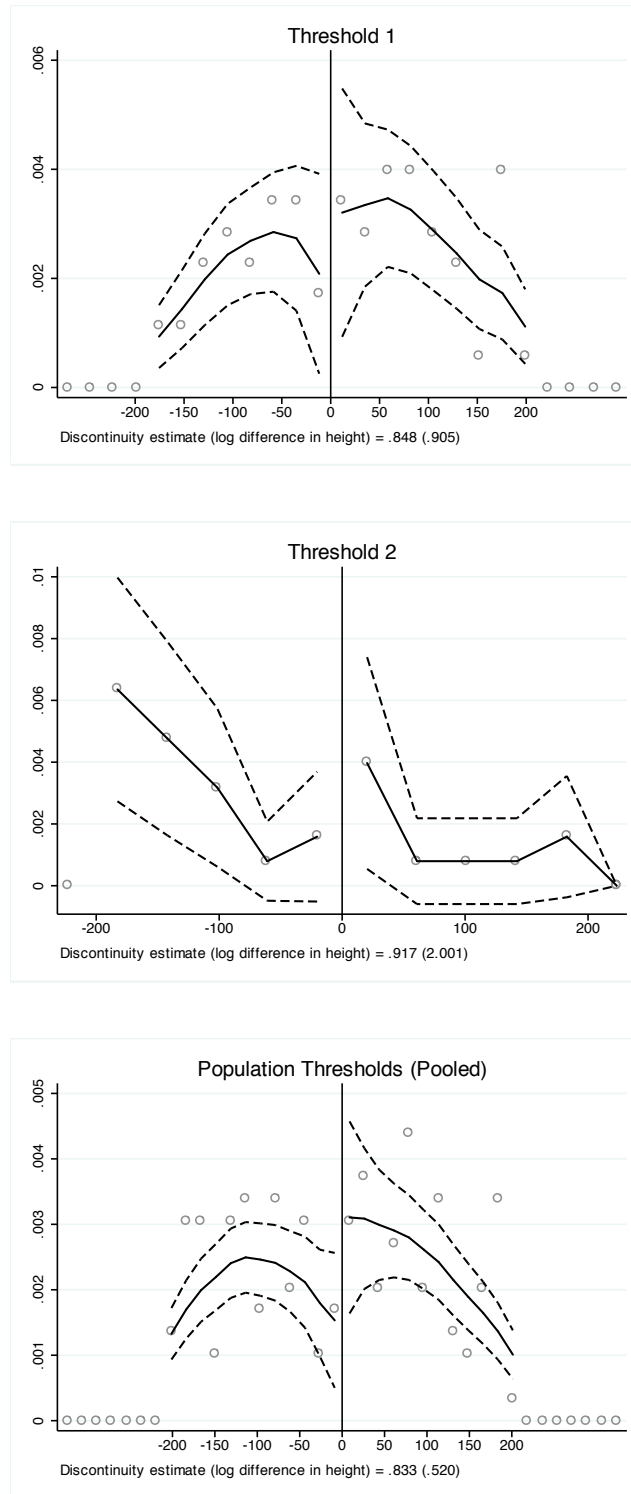


Figure 4.2: McCrary Density Tests. *McCrary tests of discontinuities in the densities of observations around threshold 1, threshold 2, and both thresholds pooled.*

		Threshold 1			Threshold 1+2		
		mean	(s.e.)	obs	mean	(s.e.)	obs
Panel A: Main Measures for Appropriation Context							
1)	Strength: Village was GAM stronghold (2001-2005) ^a	-0.32	(.31)	73	-0.22	(.27)	104
2)	Cooperation: Majority of village supported GAM ^b	0.33	(.39)	71	0.02	(.31)	99
3)	Appropriation context (strength*cooperation) ^c	-.58*	(.29)	70	-0.38	(.25)	98
Panel B: Village Characteristics (controls)							
4)	Hilly terrain ^d	-.58*	(.34)	74	-.53**	(.25)	105
5)	Village revenue in 2006 ^e (million rupiah)	-0.62	(1.03)	71	-0.09	(.86)	101
6)	Village expenditures in 2006 ^f (million rupiah)	-1.08	(1.05)	70	-0.43	(.89)	100
7)	Village was high-conflict affected ^g	-0.17	(.22)	74	0.06	(.21)	105
Panel C: Additional Measures of Village Conflict-Affectedness							
8)	Average number of civilian conflict victims ^h	37.52	(77.36)	75	77.52	(73.98)	106
9)	Number of GAM living in village ⁱ	0.65	(6.28)	51	-2.94	(5.73)	75
10)	GAM spent nights sleeping in village ^j	0.06	(.36)	71	0.17	(.30)	101
11)	GAM planned operations in village ^j	-0.13	(.41)	69	-0.16	(.29)	98

Notes: The table reports estimates of jumps in village (assignment) level pre-treatment covariates at threshold 1 and thresholds 1 and 2 pooled. Results are estimated (as in the main analysis) using a linear regression of the outcome on a model that fully interacts the indicator for being to the right of the cutoff with linear and quadratic forms of the population running variable. All measures come from the village head survey.

^a Village was a 'basis GAM' (GAM stronghold) during the period from 2001-2005.

^b [During the period from 2001-2005], the majority (at least half) of the members of the village supported GAM (=1) or supported the Indonesian military or neither side (=0).

^c Composite of the above two measures that equals 1 if the village was a GAM stronghold but the majority of the village did not support GAM and 0 otherwise.

^d Village located on hilly (=1) or flat (=0) terrain.

^e Log of total village revenue in 2006.

^f Log of total village expenditures in 2006.

^g Armed confrontations, abductions or house burnings occurred every month from 2001-2005 (=1) or sometimes/never (=0).

^h Average number of conflict victims in village, based on individual-level survey responses aggregated to village-level.

ⁱ Number of GAM living in village at the end of the conflict in August 2005.

^j Yes=1, no=0.

Table 4.4: Estimates of Jumps in Village-Level Pre-Treatment Covariates

4.5 Main Results

4.5.1 Did bigger windfalls cause appropriation?

I begin by investigating whether a bigger windfall induced former combatants to appropriate the wealth (H1). The first set of data, presented in Table 4.5 and Figure 4.3, come from five measures of perceptions of appropriation in BRA-KDP implementation in the civilian survey. Panel A in the table reports average treatment effects at the cutpoint for the first threshold as well as the marginal effect of a bigger windfall in villages where group competition was more and less likely. Panel B presents the results obtained by pooling thresholds 1 and 2. Of greatest interest is whether the interaction terms are positive and significant, indicating that more appropriation occurred in villages predisposed to group competition than in those that were not.

The first two columns report perceptions that not enough people in general, and not enough victims in particular, benefitted from BRA-KDP. The positive coefficients on the average effects (significant in Panel B) indicate that a bigger windfall caused an increase in the share who felt that too few people and too few victims benefitted. The positive (but not significant) interaction term in both panels supports the possibility that the bigger windfall caused more people to feel that few benefitted in high competition villages compared to low competition ones. The following three columns present more direct questions of appropriation. The question in column 3 asked respondents whether they agreed or disagreed with the statement that the activities selected in BRA-KDP benefitted former GAM combatants too much. The positive and significant interaction term in both panels strongly suggests that the bigger windfall caused more people to feel that GAM benefitted too much in high competition compared to low competition villages. A similar pattern holds for the subsequent two questions asking whether people thought that money was diverted (column 4) or extorted (column 5) during the implementation process. Overall, these results support H1.

	Perceptions of Appropriation				
	(1) Too few people benefitted ^a	(2) Too few victims benefitted ^b	(3) Excom benefitted ^c	(4) Corruption ^d	(5) Extortion ^e
Panel A: Threshold 1					
$1[Pop \geq Cutoff]$.06 (.09)	-.03 (.07)	-.03 (.12)	-.06 (.11)	-.02 (.11)
<i>N</i>	263	263	262	250	251
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	-.14 (.13)	-.12 (.13)	-.52*** (.10)	-.25*** (.08)	-.23*** (.06)
<i>High possibility</i>	.10 (.09)	.02 (.06)	.52*** (.16)	.12 (.08)	.10 (.09)
Interaction					
$1[Pop \geq Cutoff] * competition$.24 (.19)	.14 (.17)	1.04*** (.20)	.37*** (.10)	.33*** (.11)
<i>N</i>	254	254	253	244	245
Panel B: Threshold 1 + 2 (Pooled)					
$1[Pop \geq Cutoff]$.16** (.07)	.11* (.06)	.04 (.14)	-.12 (.07)	-.09 (.07)
<i>N</i>	350	350	349	335	334
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.08 (.06)	.04 (.08)	-.32** (.13)	-.32*** (.06)	-.24*** (.07)
<i>High possibility</i>	.13 (.09)	.13 (.10)	.52*** (.10)	.12 (.10)	.07 (.11)
Interaction					
$1[Pop \geq Cutoff] * competition$.05 (.09)	.09 (.11)	.84*** (.16)	.44*** (.12)	.31** (.13)
<i>N</i>	331	331	330	319	318

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aDo you agree or disagree that the activities selected did not benefit enough people in the village (agree=1).

^bDo you agree or disagree that the activities selected did not benefit conflict victims (agree=1).

^cDo you agree or disagree that the activities selected benefitted ex-GAM combatants, PETA and/or IDPs too much (agree=1).

^dThere were diversions of money/KKN (corruption, collusion, nepotism) (agree=1).

^eMoney was extorted from the process (agree=1).

Table 4.5: First Stage: Effect of Bigger Windfalls on Perceptions of Appropriation

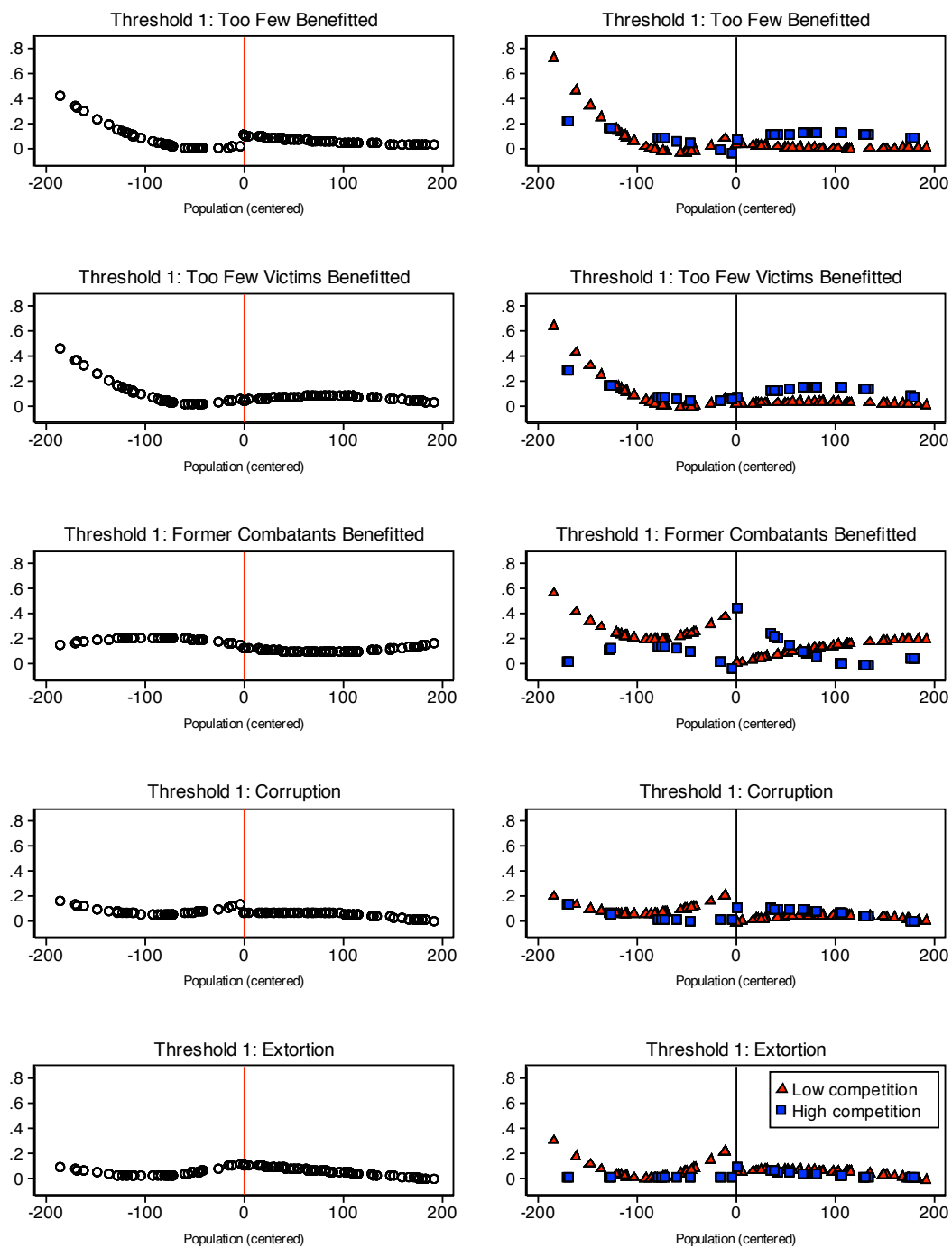


Figure 4.3: Effect of Bigger Windfalls on Perceptions of Appropriation. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

I next evaluate the possibility that a bigger windfall affected which projects were selected for funding. As discussed above, the program aimed to deliver the largest share of benefits to civilian conflict victims but gave communities discretion over which projects to fund. The primary options were funding livelihood assistance or investing in public goods; disbursing the wealth equally across all households was strongly discouraged. Nevertheless, anecdotal evidence suggests that former combatants tried to appropriate the BRA-KDP windfall by pressuring community members to do equal distribution or to fund public goods. Columns 1-3 in Table 4.6 and Figure 4.4 investigate whether windfall size affected how community members allocated BRA-KDP funds in high versus low competition villages.

The first column in Table 4.6 presents results from a broad survey question asking respondents whether they felt “the activities selected [in BRA-KDP] were not the most important ones for the village.” There is little evidence that bigger windfalls caused greater dissatisfaction among civilians in how the funds were spent in high competition villages. The second and third columns report the share of people who recall that their village opted for equal division among all households or for public goods. Notably, consistent with H1, high competition villages were significantly more likely to select equal distribution than low competition villages (Panel A, column two). There is no evidence that windfall size affected the decision to invest in public goods, however.

	Projects Selected			Received Aid (Civilians)			Received Aid (Combatants)		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Bad projects selected ^a	Equal Division ^b	Public Goods ^c	Received assistance ^d	Received money ^e	Amt. money received ^f	Received assistance ^d	Received money ^e	Amt. money received ^f
Panel A: Threshold 1									
<i>1[Pop ≥ Cutoff]</i>	.03	-.35***	-.03	-.27	.35	-.22	-.30*	.19	-.23
	(.03)	(.13)	(.09)	(.18)	(.27)	(.42)	(.16)	(.21)	(.39)
<i>N</i>	263	351	351	351	245	351	121	69	121
Marginal effect conditional on group competition [†]									
<i>Low/some possibility</i>	-.02	-.56***	-.06	-.40*	-.10	-1.20***	-.73***	-.04	-.80**
	(.03)	(.15)	(.11)	(.20)	(.11)	(.36)	(.19)	(.06)	(.33)
<i>High possibility</i>	.00	.29	-.11	.09	.56*	1.00**	.32	.60*	.76
	(.03)	(.23)	(.10)	(.29)	(.31)	(.39)	(.36)	(.29)	(.66)
Interaction									
<i>1[Pop ≥ Cutoff]*competition</i>	.02	.86***	-.05	.49	.66**	2.21***	1.05***	.64**	1.56**
	(.04)	(.22)	(.16)	(.35)	(.33)	(.50)	(.39)	(.28)	(.67)
<i>N</i>	254	332	332	332	236	332	113	64	113
Panel B: Threshold 1 + 2 (Pooled)									
<i>1[Pop ≥ Cutoff]</i>	-.02	.12	.00	.15	.23	.03	-.17	.19	-.30
	(.08)	(.23)	(.07)	(.21)	(.22)	(.27)	(.16)	(.18)	(.36)
<i>N</i>	350	498	498	498	325	498	181	98	181
Marginal effect conditional on group competition [†]									
<i>Low/some possibility</i>	-.12	.17	-.01	.25	-.04	-.26	-.45**	-.01	-.25
	(.09)	(.33)	(.10)	(.27)	(.05)	(.42)	(.21)	(.03)	(.36)
<i>High possibility</i>	-.03	.14	-.20	.01	.51	.87**	.25	.63**	.59
	(.07)	(.26)	(.13)	(.26)	(.35)	(.39)	(.38)	(.28)	(.74)
Interaction									
<i>1[Pop ≥ Cutoff]*competition</i>	.10	-.03	-.19	-.23	.55	1.13**	.70	.64**	.84
	(.12)	(.39)	(.17)	(.37)	(.36)	(.56)	(.50)	(.28)	(.78)
<i>N</i>	331	464	464	464	305	464	168	88	168

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aLooking back at the implementation of BRA-KDP do you agree or disagree that the activities selected were not the most important ones for the village? (agree=1)

^bEqual cash disbursement among all households=1.

^cPublic goods (agriculture/traders/aquaculture/savings and loans) =1

^dReceived assistance from BRA-KDP=1.

^eReceived cash from BRA-KDP=1.

^fAmount of money received (in millions of rupiah).

Table 4.6: First Stage: Effect of Bigger Windfalls on Allocation

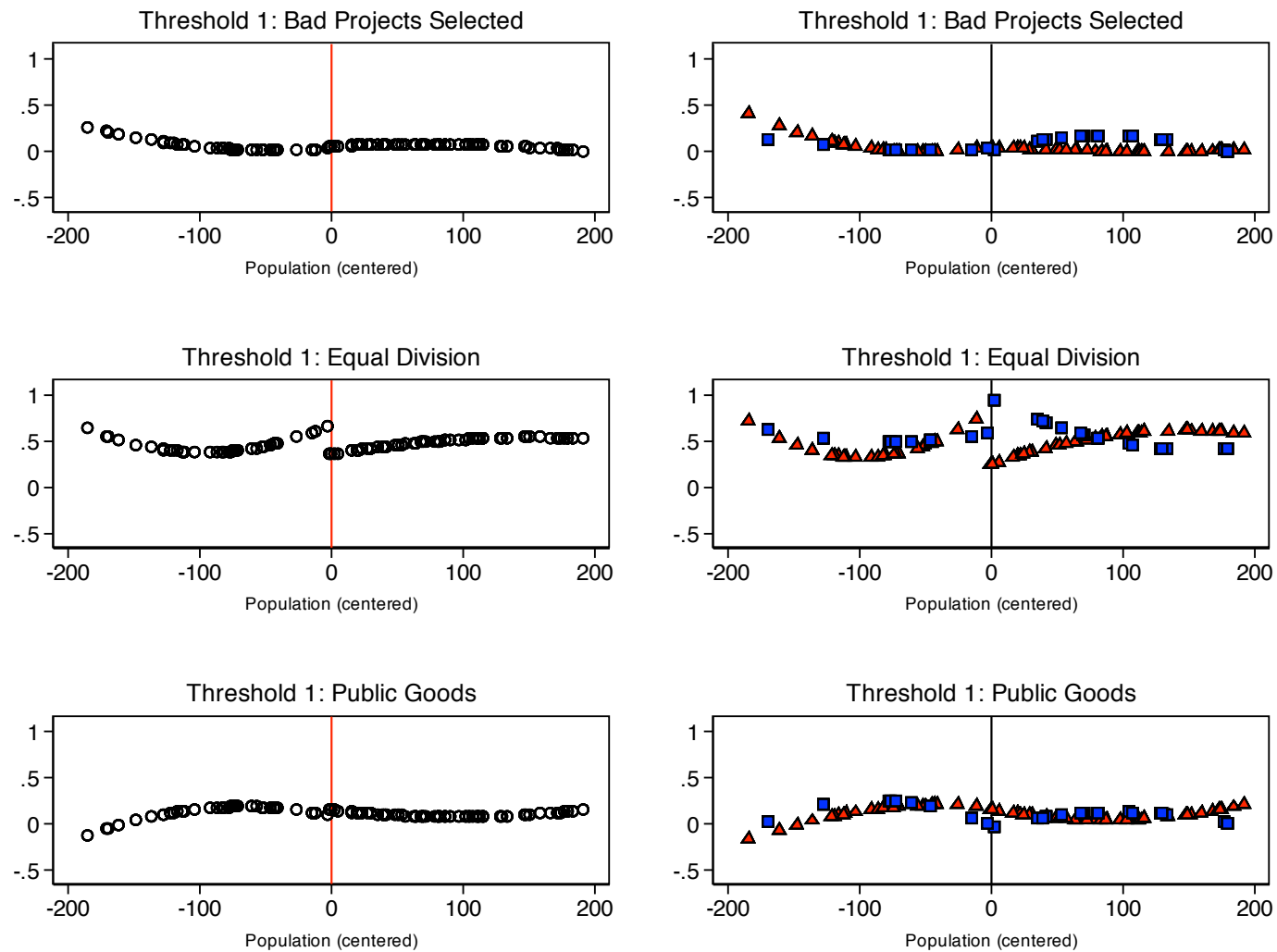


Figure 4.4: Effect of Bigger Windfalls on Allocation I. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

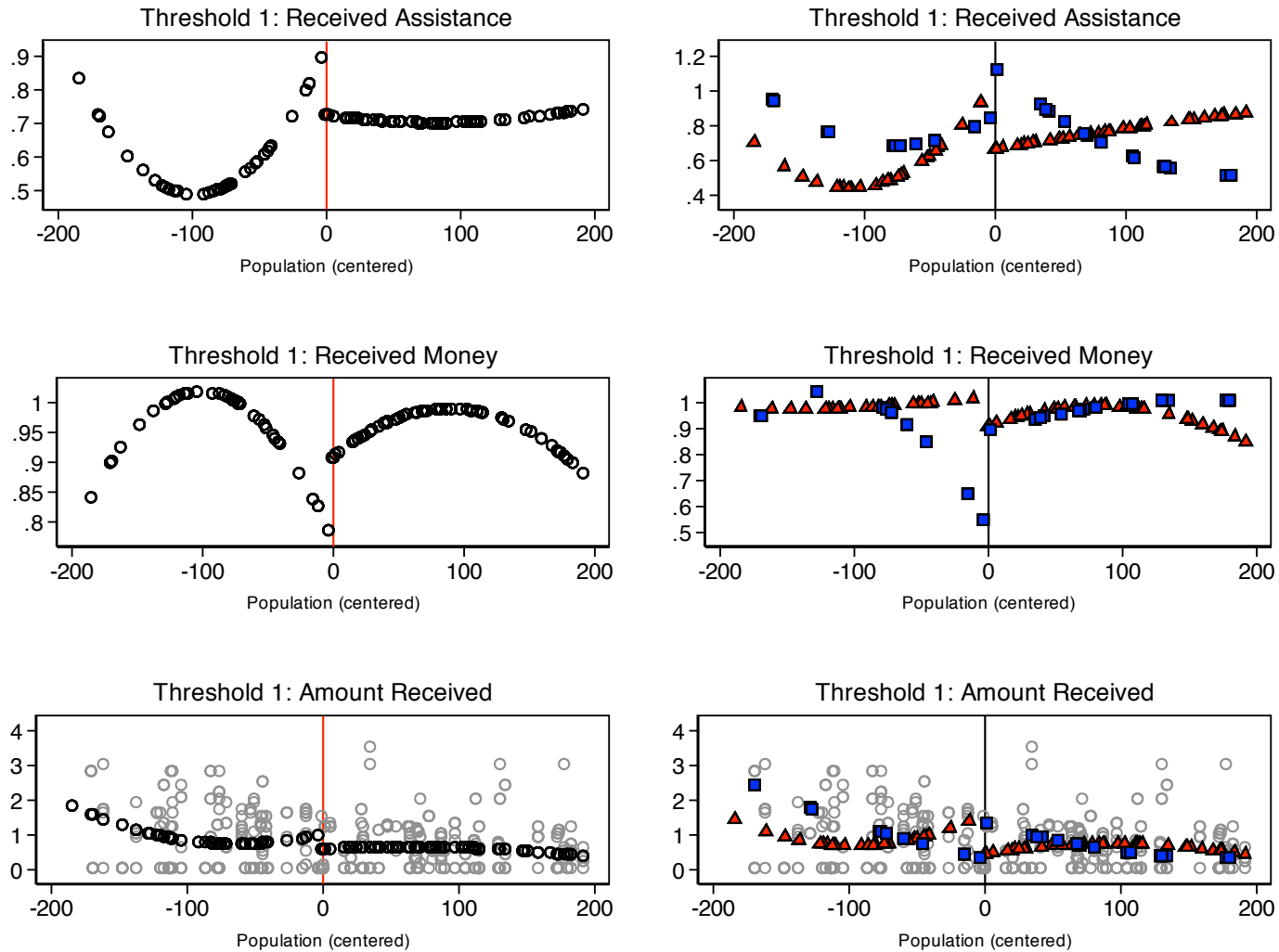


Figure 4.5: Effect of Bigger Windfalls on Allocation (Civilians) II. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

Columns 4-6 in Table 4.6 and Figure 4.5 look more closely at the share of civilian households that received assistance from BRA-KDP. If bigger windfalls induced appropriation by GAM in high competition villages, then we would expect to see a *negative* interaction, indicating that civilians received greater benefits in low than in high competition villages. Among these measures, the most notable finding is that bigger windfalls resulted in civilians receiving *more* money in high competition than low competition villages (Panels A and B, column six). There is also some indication that households in high competition villages were more likely to receive cash (compared to goods for livelihood assistance), which is consistent with the previous finding that these villages were more likely to opt for equal disbursement. To shed more light on whether appropriation occurred, I use data from the former combatant surveys to assess whether bigger windfalls resulted in more direct benefits to ex-GAM combatants, despite the prohibition. The evidence indicates that former combatants were significantly more likely to receive assistance—and to receive a bigger amount—in high competition villages (Panels A and B, columns 7-9), which is consistent with H1.

All in all, on one hand, these results indicate support for the first hypothesis that bigger windfalls induced appropriation in fractious villages. Critically, bigger windfalls increased perceptions of appropriation; the equal distribution of funds across all households; and the share of benefits going to GAM in high competition vis-a-vis low competition villages. On the other hand, the data points to a puzzle in that civilians also did better in high competition villages. Granted, equal distribution implies that civilian households would also be somewhat better off since the bigger windfall translated into an additional 560,000 rupiah (\$56) per household (for threshold 1). Yet, we would still expect that civilians would have received even more had GAM not appropriated a share of the wealth. Instead, the data suggests that bigger windfalls actually resulted in civilians receiving *less* in low competition villages (Table 4.6, columns 4-6). In sum, while the results suggest some support for the hypothesis that windfall size affects appropriation, they also highlight the need to dig deeper into how allocation decisions were made in low competition villages.

4.5.2 Did bigger windfalls yield welfare dividends?

I next investigate whether bigger windfalls delivered more economic benefits to civilians and, in particular, whether the outcomes vary across villages that were more and less prone to group competition to appropriate the wealth. The second hypothesis predicts that bigger windfalls resulted in greater welfare benefits for civilians in low competition than in high competition contexts, where appropriation was more likely to occur. The results from the previous section indicate that bigger windfalls indeed resulted in former combatants in high competition villages receiving more. Yet, civilians in these villages also apparently received more. This section explores the impact of bigger windfalls on economic welfare by looking at how both civilians and former combatants fared under BRA-KDP.

Table 4.7 presents eight different indicators of well-being and the results for the civilian population. It is possible that BRA-KDP assistance enabled households to buy assets, such as cell phones, motorbikes, or livestock. This possibility is captured in the first measure, which is an estimate of total asset value based on a list of 17 assets (and 2008 prices). The questions in the subsequent two columns measure whether assistance from BRA-KDP facilitated improvements in quality of living, including better access to water from a protected source (column two) and more usage of quality materials in housing construction or repairs (column three). Question four inquires into the total land area farmed by the household.

It is also possible that households used BRA-KDP funds to pay school fees or to obtain medical help for sick family members. Columns 5 and 6 accordingly report the share of school-aged children in the household attending school and the share of household members who suffered from any sickness or injury in the past one month that prevented work. The final two measures in Table 4.7 capture perceptions of poverty. The seventh column measures whether the respondent believed her household was among the poorest third of all households in the village while the final column reports whether living conditions had improved over the past 12 months. For all measures (except perceived poverty in column 7, which is reversed), a negative interaction should be regarded as support for H2, indicating that bigger windfalls led to greater improvements in economic welfare for civilians in low competition villages where appropriation was unlikely.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Asset value ^a	Protected water ^b source	Concrete walls ^c	Land farmed ^d	Share of kids in school ^e	Household members sick ^f	Poor ^b	Living conditions improved ^h
Panel A: Threshold 1								
<i>1[Pop ≥ Cutoff]</i>	-2.13	.31	-.05	-1.27	.07	-.05	.19	-.04
	(3.81)	(.27)	(.21)	(2.53)	(.13)	(.04)	(.15)	(.13)
<i>N</i>	351	351	351	351	351	351	351	351
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-7.59*	.33	.02	-6.10**	.13	-.05	.12	-.13
	(3.97)	(.32)	(.17)	(3.06)	(.11)	(.04)	(.26)	(.13)
<i>High possibility</i>	6.27*	-.08	-.14	2.04	.01	-.02	.21	-.14
	(3.55)	(.30)	(.23)	(3.14)	(.19)	(.07)	(.13)	(.18)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	13.85***	-.42	-.16	8.14*	-.12	.03	.09	-.01
	(5.24)	(.44)	(.28)	(4.39)	(.23)	(.08)	(.29)	(.22)
<i>N</i>	332	332	332	332	332	332	332	332
Panel B: Threshold 1 + 2 (Pooled)								
<i>1[Pop ≥ Cutoff]</i>	-2.53	-.08	-.06	1.02	-.03	-.01	-.06	-.07
	(3.01)	(.21)	(.13)	(1.88)	(.12)	(.04)	(.15)	(.15)
<i>N</i>	498	498	498	498	498	498	498	498
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-4.10	-.19	.04	.99	-.09	-.01	-.28*	-.21
	(3.64)	(.24)	(.13)	(2.84)	(.16)	(.04)	(.15)	(.14)
<i>High possibility</i>	8.16**	-.05	-.05	1.35	.00	.03	.07	-.13
	(3.32)	(.28)	(.22)	(3.11)	(.17)	(.08)	(.17)	(.19)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	12.25**	.14	-.09	.36	.09	.04	.34	.08
	(4.85)	(.35)	(.25)	(4.14)	(.24)	(.08)	(.22)	(.23)
<i>N</i>	464	464	464	464	464	464	464	464

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

†Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTotal asset value calculated based on a list 17 assets and 2008 prices.

^bWhat is your main source of drinking water? (unprotected well or spring=0, all other options=1).

^cWhat is the material used most in your house wall (concrete=1, timber/bamboo/other=0).

^dHow much land is being farmed by this household (in thousands of meters-squared)?

^eShare of school-age children in household currently attending school.

^fShare of family members in household who have suffered from any sickness or injury that prevented them from work or going to school in the past one month.

^gIn your opinion, relative to others in your village, how poor is your household? (in the middle/among the richest third=1, among the poorest third=0)

^hLooking back, how do you rate your living conditions now compared to 12 months ago? (better=1, same/worse=0)

Table 4.7: Effect of Bigger Windfalls on Welfare—Civilians

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Asset value ^a	Protected water ^b source	Concrete walls ^c	Land farmed ^d	Share of kids in school ^e	Household members sick ^g	Poor ^b	Living conditions improved ^h
Panel A: Threshold 1								
<i>1[Pop ≥ Cutoff]</i>	-2.65	.24	.15	-.12	-.08	.12**	.31	-.09
	(5.29)	(.32)	(.09)	(3.30)	(.17)	(.05)	(.24)	(.13)
<i>N</i>	121	121	121	121	121	121	121	121
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-10.45	.17	.12	3.82	-.33*	.15**	.28	-.47***
	(7.43)	(.25)	(.07)	(2.52)	(.18)	(.06)	(.20)	(.14)
<i>High possibility</i>	-2.86	-.39	-.17	-9.25**	-.24	.10	.30	.20
	(11.20)	(.45)	(.12)	(3.80)	(.19)	(.07)	(.45)	(.16)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	7.59	-.56	-.29**	-13.07***	.08	-.05	.01	.68***
	(13.09)	(.53)	(.11)	(4.52)	(.23)	(.09)	(.50)	(.16)
<i>N</i>	113	113	113	113	113	113	113	113
Panel B: Threshold 1 + 2 (Pooled)								
<i>1[Pop ≥ Cutoff]</i>	-1.50	.08	.18*	-2.69	-.07	.07*	.16	.01
	(4.62)	(.24)	(.11)	(2.86)	(.14)	(.04)	(.19)	(.12)
<i>N</i>	181	181	181	181	181	181	181	181
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-3.85	-.12	.13	.36	-.22	.08*	.04	-.24
	(7.05)	(.23)	(.12)	(3.02)	(.16)	(.05)	(.18)	(.16)
<i>High possibility</i>	-3.71	-.38	.15	-9.94***	-.09	.13*	.10	.23
	(10.20)	(.45)	(.17)	(2.98)	(.27)	(.08)	(.40)	(.23)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	.14	-.25	.02	-10.30**	.13	.06	.07	.46
	(11.83)	(.52)	(.17)	(4.00)	(.31)	(.09)	(.43)	(.30)
<i>N</i>	168	168	168	168	168	168	168	168

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the former combatant sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTotal asset value calculated based on a list 17 assets and 2008 prices.

^bWhat is your main source of drinking water? (unprotected well or spring=0, all other options=1).

^cWhat is the material used most in your house wall (concrete=1, timber/bamboo/other=0).

^dHow much land is being farmed by this household (in thousands of meters-squared)?

^eShare of school-age children in household currently attending school.

^fShare of family members in household who have suffered from any sickness or injury that prevented them from work or going to school in the past one month.

^gIn your opinion, relative to others in your village, how poor is your household? (in the middle/among the richest third=1, among the poorest third=0)

^hLooking back, how do you rate your living conditions now compared to 12 months ago? (better=1, same/worse=0)

Table 4.8: Effect of Bigger Windfalls on Welfare—Former Combatants

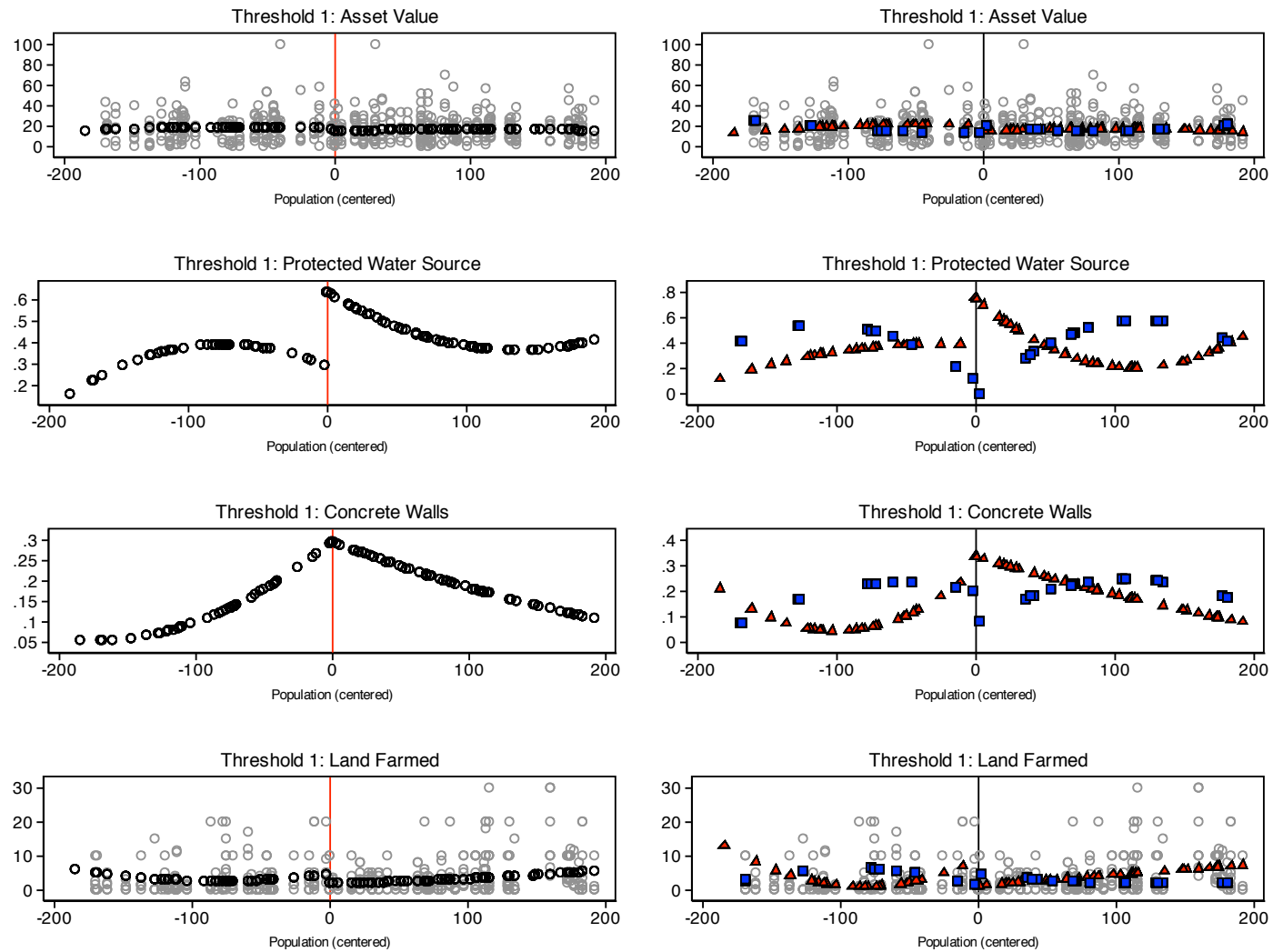


Figure 4.6: Effect of Bigger Windfalls on Welfare (Civilians) I. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

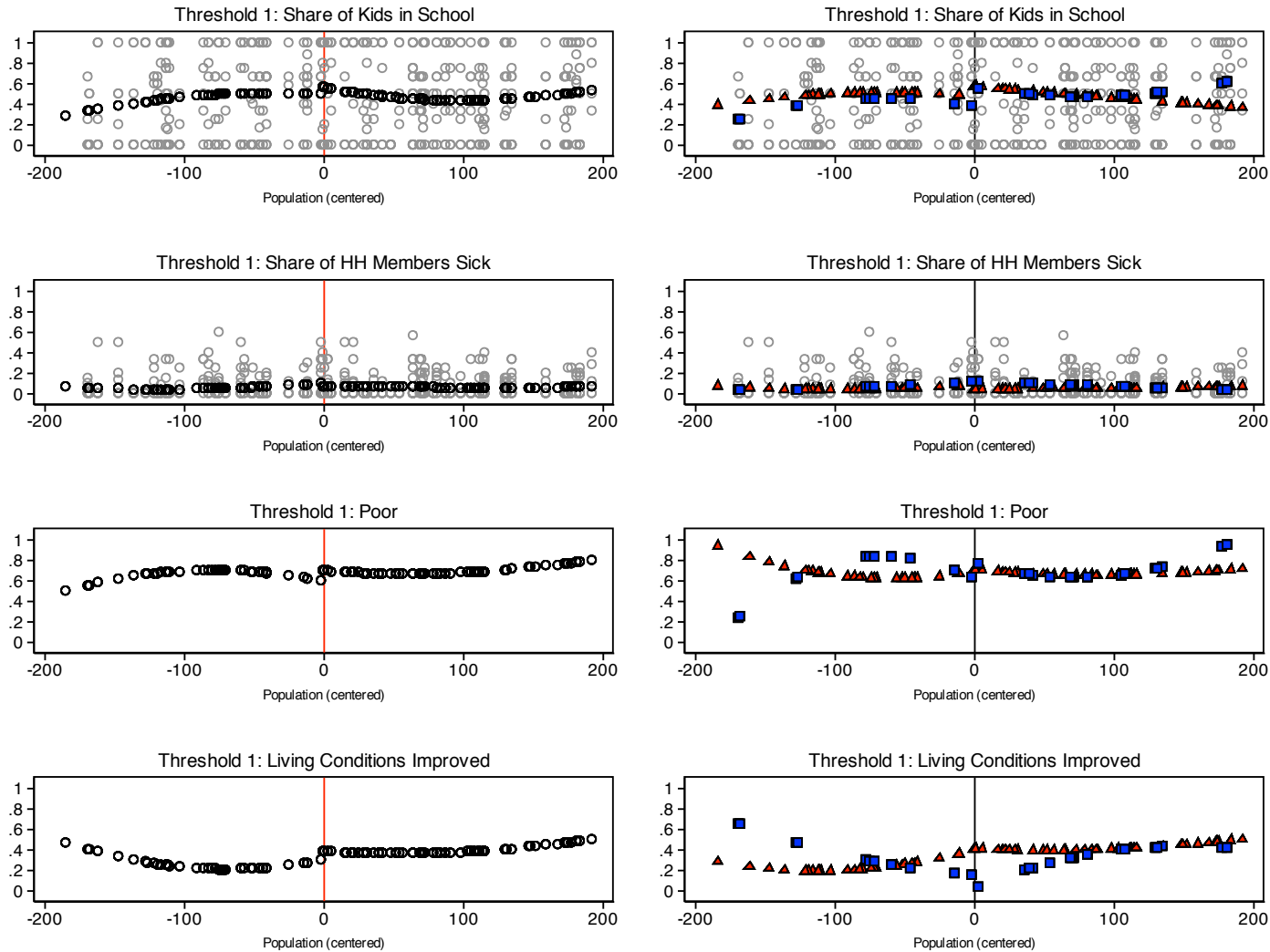


Figure 4.7: Effect of Bigger Windfalls on Welfare (Civilians) II. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

Overall, however, the results provide little support for the second hypothesis. Interestingly, there is little indication from any of the measures that bigger windfalls improved welfare on average. Moreover, in evaluating whether welfare outcomes varied across competition contexts, civilians actually appear to be better off in high competition villages. Specifically, the evidence indicates that bigger windfalls increased the value of asset holdings for civilians in high vis-a-vis low competition villages (Panels A and B, column 1). There is also a weak indication that bigger windfalls enabled civilians in high competition villages to clear more land for farming. While these results provide little support for H2 they are consistent with the findings in Table 4.6 that bigger windfalls delivered more assistance to civilians in high competition villages.

While former combatants were prohibited from benefitting directly from BRA-KDP, I use the same eight indicators from the former combatants survey to corroborate whether ex-GAM nonetheless also enjoyed welfare benefits from bigger windfalls. The results are presented in Table 4.8. Here a positive interaction is taken as evidence of appropriation, implying that former GAM members benefitted more from BRA-KDP in high competition villages. The most notable result is that bigger windfalls caused a *decrease* in land farmed in high competition compared to low competition villages (Panels A and B, column 4). A possible explanation is that this is not actually evidence of a decline in welfare but rather that former combatants used BRA-KDP assistance to move out of agriculture and into another sector, such as trade. Tellingly, bigger windfalls also caused former combatants in high competition villages to feel that their living conditions had improved (Panel A, column 8).

In sum, there is little evidence to support the hypothesis that bigger windfalls realized greater welfare dividends for civilians in low competition vis-a-vis high competition villages. Rather, the data suggests that bigger windfalls led to improved welfare for both civilians and former combatants in more fractious villages compared to less divided ones. While this finding is contrary to H2, it is consistent with the earlier results presented in Table 4.6 that bigger windfalls resulted in more direct assistance for both civilians and former combatants in high competition contexts.

4.5.3 Did bigger windfalls facilitate cohesion or exacerbate social conflict?

Finally, I evaluate the evidence for H3, which predicts that bigger windfalls exacerbated social divisions in high competition villages compared to low competition ones. Table 4.9 and Figure 4.8 present five measures of civilian attitudes towards former combatants. The first measure is the most direct in asking civilians whether they feel that divisions exist in the village between former combatants and other village members. Interestingly, the results in both Panels A and B suggest that bigger windfalls caused divisions between these groups *on average*. In other words, the evidence suggests that bigger windfalls might have enhanced divisions in both low and high competition villages alike. There is little direct support for H3, however, in that bigger windfalls did not exacerbate tensions more in high competition than low competition villages. This is indicated by the lack of a positive and significant interaction term in either panel.

The second and third questions in Table 4.9 inquire into whether people feel that former combatants and members of KPA (GAM's post-conflict political incarnation) benefit more than other groups in the village when decisions about how to allocate resources are made. These measures are considered proxies for social tensions insofar as redistribution that benefits one group over other groups in the village could be a source of discontent. Any indication that GAM or KPA benefitted more in high competition than in low competition villages would be taken as support for H3. Yet, not only are the interaction terms not positive or significant, there is some evidence that bigger windfalls reduced perceptions in high competition villages that KPA benefitted more (Panels A and B, column three). These results again point to a lack of support for H3.

Finally, questions four and five are proxies for broader acceptance of, and trust in, former combatants. Specifically, question four asked respondents about their willingness to embrace former combatants in a number of different types of roles, from membership in village associations to welcomed into the respondent's family through marriage. Contrary to H3, there is little evidence that bigger windfalls produced a greater reluctance to accept former combatants in high competition villages. The final question asked the civilian respondents if they could think of at least one former combatant or KPA member that they would trust to watch their children. Here the result is surprising: Bigger windfalls enhanced trust *more* in high than in low competition villages.

	(1)	(2)	(3)	(4)	(5)
	Divisions: GAM vs. civilians ^a	Benefit more: GAM ^b	Benefit more: KPA ^c	Willing to accept GAM ^d	Trust GAM ^e
Panel A: Threshold 1					
<i>1[Pop ≥ Cutoff]</i>	.12**	-.14	-.07	.05	-.01
	(.05)	(.25)	(.16)	(.15)	(.09)
<i>N</i>	350	346	344	351	350
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.16	.11	.21	-.03	-.25***
	(.10)	(.22)	(.20)	(.10)	(.07)
<i>High possibility</i>	.07	-.29	-.63**	.31	.23**
	(.06)	(.32)	(.24)	(.22)	(.10)
Interaction					
<i>1[Pop ≥ Cutoff]*competition</i>	-.09	-.40	-.84***	.34	.48***
	(.12)	(.40)	(.28)	(.21)	(.12)
<i>N</i>	331	327	325	332	331
Panel B: Threshold 1 + 2 (Pooled)					
<i>1[Pop ≥ Cutoff]</i>	.08**	-.07	-.16	.01	.19**
	(.03)	(.21)	(.13)	(.12)	(.09)
<i>N</i>	497	491	489	498	497
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.09*	.01	-.10	.01	.02
	(.05)	(.22)	(.15)	(.15)	(.09)
<i>High possibility</i>	.09	-.25	-.61***	.31*	.24**
	(.06)	(.33)	(.22)	(.18)	(.12)
Interaction					
<i>1[Pop ≥ Cutoff]*competition</i>	.01	-.26	-.51*	.30	.23
	(.08)	(.36)	(.25)	(.19)	(.15)
<i>N</i>	463	458	456	464	463

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTo what extent do differences between ex-combatants and village members divide people in the village? (Major/minor source of division=1, not a source of division=0).

^bWhen the community has to make a decision about how to allocate resources in the village, sometimes some groups benefit more than others. Generally, do you think GAM/KPA do especially well or badly relative to other people? (Better=1, worse=-1, the same=0). ^cShould GAM be: (1) welcomed in the village, (2) allowed to join community associations, (3) allowed to be village leaders, (4) among close friends, (5) welcomed into your family through marriage? (mean of five possible roles).

^dIf you suddenly had to go away for a day or two, could you think of at least one person who is a former combatant/KPA member that you could turn to to take care of your children? (yes=1).

Table 4.9: Effect of Bigger Windfalls on Social Divisions

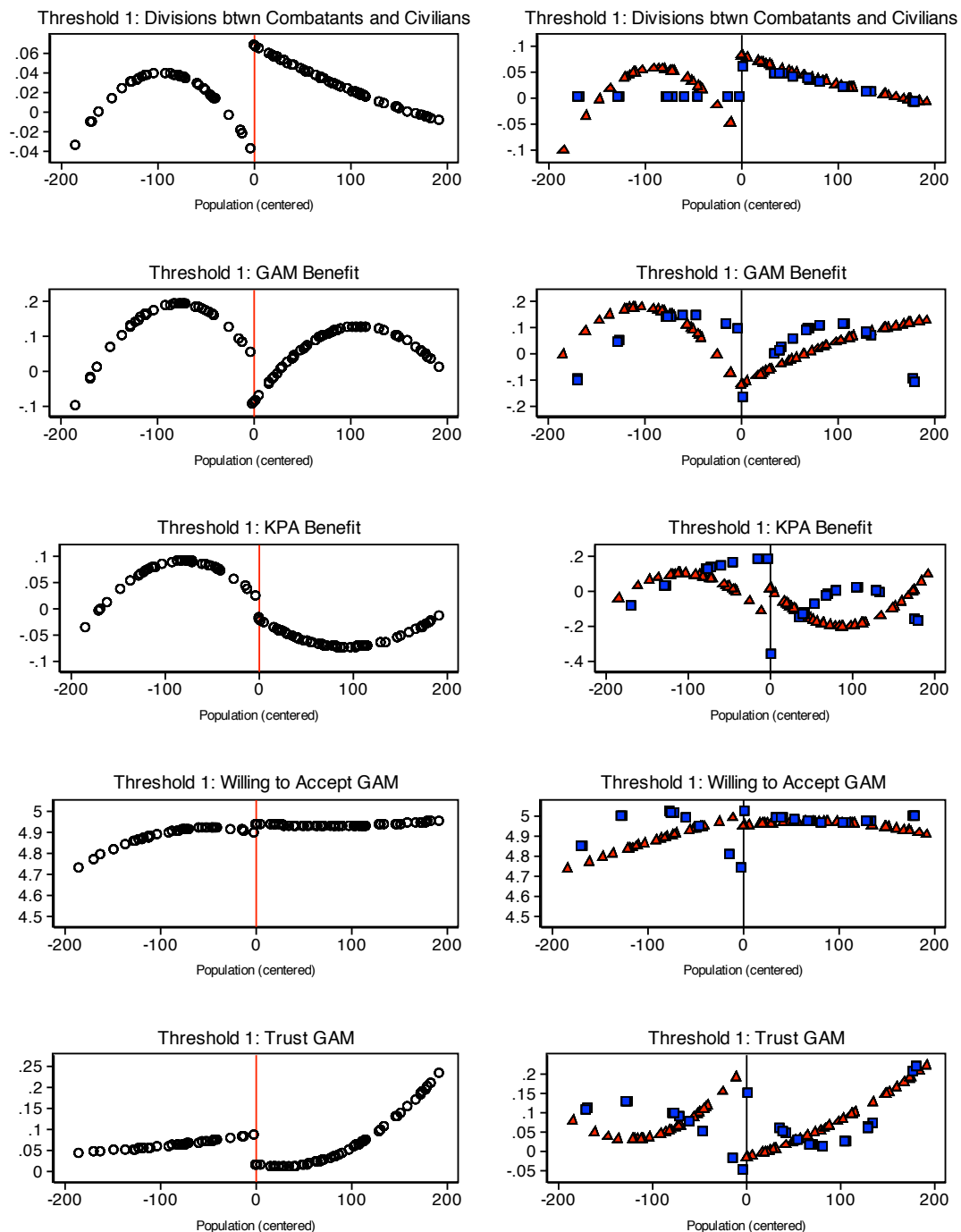


Figure 4.8: Effect of Bigger Windfalls on Social Divisions. *The left panel presents the effect of bigger windfalls for the full threshold 1 sample of civilians. The right panel presents the effect on civilians in low competition (red triangle) and high competition (blue square) villages. The plot is a fitted regression of the outcome on the cutoff indicator and linear and quadratic forms of the assignment variable (without controls), estimated separately on each side of the cutoff (quadratic spline). All regressions include sampling weights and standard errors are clustered at the village level.*

Overall, the results present little evidence that bigger windfalls exacerbated social tensions in villages more prone to group competition *ex ante* than in less divided villages. If anything, the findings suggest that bigger windfalls reduced perceptions that KPA members benefit more, and increased trust in former combatants, in high competition villages. There is a plausible explanation for this pattern that is consistent with the previous findings that bigger windfalls induced equal distribution of wealth across households. If bigger windfalls caused former combatants in high competition villages to lobby for equal distribution across households, then civilian households that were not prioritized as the most conflict-affected would have benefitted as well. It is possible that these households were grateful to former combatants for ensuring that they too received a share of the wealth. This would be consistent with the fact that, by requiring communities to identify civilian households that were ‘more’ and ‘less’ conflict-affected, the targeting process potentially also created divisions among civilians (Morel, Watanabe and Wrobel, 2009).

4.5.4 Discussion and robustness checks

Overall, the results paint a surprising picture of how bigger windfalls affected appropriation, welfare, and social divisions across villages in post-conflict Aceh more and less predisposed to group competition. On one hand, the evidence supports the notion that bigger windfalls induced group competition over resources in villages with a history of more contentious relations between former combatants and civilians. In high competition villages, bigger windfalls led to the more frequent adoption of equal distribution and also increased perceptions of appropriation in BRA-KDP implementation. Bigger windfalls also resulted in former combatants obtaining a greater share of the wealth in high competition villages.

On the other hand, the fact that GAM benefitted more in high competition villages did not reduce benefits to civilians, as anticipated. This paper tested the hypothesis (H2) that bigger windfalls would deliver greater welfare benefits to civilians in low competition villages because former combatants took less of the wealth. Yet, the evidence indicates that bigger windfalls also generated more benefits for civilians in high competition environments. Specifically, bigger windfalls resulted in more direct assistance from BRA-KDP as well as higher levels of economic welfare

on measures of asset value and land farmed for civilians in more divided villages compared to those in more cooperative ones. The data also does not indicate that bigger windfalls exacerbated social tensions between civilians and former combatants in high competition villages. While this is contrary to H3, it is consistent with the evidence that bigger windfalls were more beneficial to civilians in high competition villages than in low competition villages.

What explains why civilians enjoyed more BRA-KDP benefits, better economic welfare, and improved social relations with GAM in high competition villages vis-a-vis low competition ones, despite the evidence that GAM also captured wealth in these villages? I explore a few plausible explanations, with additional data presented in Table 4.10. One possibility is that villagers were better able to coordinate on assistance in the form of livelihood goods (rather than cash) in low competition villages. This would be consistent the findings that bigger windfalls reduced perceived appropriation (Table 4.5, columns 3-6), equal distribution (Table 4.6, column two), and cash allocations (Table 4.6, columns 4-6) in low competition villages. The first column in Table 4.10 investigates whether bigger windfalls resulted in more low competition villages opting for private goods and finds little support for this explanation.

Another possibility is that low competition villages did a better job of channeling bigger windfalls to the most conflict-affected civilian households. If this were the case, bigger windfalls might only produce greater benefits for the subset of civilian conflict-affected households rather than among all civilian households in the village. To assess whether this is the case, I restrict the data to conflict victims and analyze whether bigger windfalls resulted in bigger benefits from BRA-KDP for this group alone.¹⁵ As can be seen in columns 2-3 of Table 4.10, however, there is no evidence that bigger windfalls delivered greater assistance to conflict victims in low competition villages compared to high competition ones. Similarly, there is little indication that bigger windfalls exacerbated social divisions between conflict victims (rather than civilians in general) and former combatants in high competition versus low competition villages (columns 4-5).

¹⁵Conflict victims are defined subjectively, as they were in the BRA-KDP process. Respondents are considered conflict victims if they responded affirmatively when asked in the survey if they consider themselves a conflict victim. To corroborate this response, the survey also collected data on why individuals were victims, with response options ranging from death of a family member to internal displacement to mental illness.

Finally, I explore whether civilians benefitted less in low competition environments because bigger windfalls induced an alternate type of rent-seeking: elite capture. In other words, while former GAM combatants might not have had the will or ability to appropriate BRA-KDP wealth in low competition villages, bigger windfalls could have induced more appropriation by village leaders. To investigate this, column six in Table 4.10 presents a measure of whether people felt that those well-connected to the village leader tend to benefit more from how resources are distributed within the village. The negative and weakly significant interaction term in Panel A suggests that, indeed, bigger windfalls might have resulted in more *elite*—rather than ex-combatant—capture of BRA-KDP funds in low competition villages. All in all, the results suggest that the impact of aid windfalls on development indeed varies by the nature of social fragmentation and power competition and highlight the need for further research into decision-making dynamics in both high and low competition contexts.

4.6 Conclusion

This paper uses a post-conflict reconstruction program in Aceh to investigate whether bigger revenue windfalls induce appropriation, undermine welfare, and exacerbate social tensions in more and less socially fragmented contexts. The main findings are that bigger windfalls induced former combatants to appropriate wealth in villages that were ex ante more prone to group competition, determined by both the strength of the former combatant group and its support among villages. Yet, surprisingly, bigger windfalls also resulted in greater economic benefits for civilians and did not exacerbate social tensions in high compared to low competition villages. The results raise important questions about why bigger windfalls did not deliver even *greater* economic benefits to civilian households in low competition villages.

This paper was motivated by an influential political economy literature on group rent-seeking contests, which has generated important insights into the conditions under which bigger windfalls might help or harm development. The central prediction in this literature is that windfalls are good for development where groups are homogeneous or cooperative and bad for development where there

exists multiple, powerful and competing groups. In finding that rent-seeking competition did not dissipate the benefits of windfalls and, moreover, that bigger windfalls did not lead to greater benefits in more cohesive villages, the results point to the need for more theoretical research on how different group dynamics condition the impact of bigger windfalls on development.

This paper also raises important questions for a growing literature on the effectiveness of community-driven development and community-driven reconstruction programs. In recent years, programs like BRA-KDP have achieved prominence as a means for promoting development at the local level (Mansuri and Rao, 2004; Bank, 2007). Empirical evidence of the effectiveness of such programs is mixed, however. In a large-scale randomized evaluation conducted in Liberia, Fearon et al (2009) find that a CDD program caused higher levels of social cohesion. In contrast, a recent randomized evaluation in Sierra Leone shows that the program improved local public goods but had no democratic or collective action dividends (Casey, Glennerster and Miguel, 2011). Gugerty and Kremer (2008) find that the influx of resources crowded out participation by the socially disadvantaged and attracted involvement by more elite community members. By investigating whether aid windfalls actually exacerbate community tensions in certain contexts, this paper explores whether such programs promote cohesion, as intended, or actually reinforce social divisions in post-conflict settings.

	<u>Projects</u>	<u>Received Aid</u>		<u>Divisions</u>		<u>Who</u>
	<u>Selected</u>	<u>(Conflict Victims)</u>		<u>(Conflict Victims)</u>		<u>Benefits?</u>
	(1)	(2)	(3)	(4)	(5)	(6)
	Private Goods ^a	Received assistance ^b	Amt. money received ^c	Divisions: GAM vs. civilians ^d	Willing to accept GAM ^e	Connected to leaders ^f
Panel A: Threshold 1						
<i>1[Pop ≥ Cutoff]</i>	-.02	-.08	.47	.21*	.07	.02
	(.16)	(.21)	(.38)	(.12)	(.12)	(.13)
<i>N</i>	351	200	200	200	200	344
Marginal effect conditional on group competition [†]						
<i>Low/some possibility</i>	.11	-.20	-.84**	.29*	-.06	.22
	(.11)	(.23)	(.39)	(.16)	(.09)	(.17)
<i>High possibility</i>	.01	.18	1.66***	.06	.19	-.29
	(.30)	(.26)	(.50)	(.10)	(.12)	(.26)
Interaction						
<i>1[Pop ≥ Cutoff]*competition</i>	-.10	.38	2.50***	-.23	.25*	-.51*
	(.32)	(.34)	(.62)	(.21)	(.13)	(.27)
<i>N</i>	332	193	193	193	193	325
Panel B: Threshold 1 + 2 (Pooled)						
<i>1[Pop ≥ Cutoff]</i>	-.02	.00	.22	.15*	.09	-.04
	(.08)	(.24)	(.31)	(.07)	(.10)	(.15)
<i>N</i>	498	255	255	255	255	490
Marginal effect conditional on group competition [†]						
<i>Low/some possibility</i>	.03	.04	-.49	.15	.00	.25
	(.05)	(.29)	(.43)	(.09)	(.10)	(.18)
<i>High possibility</i>	-.02	.05	1.36***	.10	.31***	-.26
	(.29)	(.26)	(.39)	(.10)	(.09)	(.31)
Interaction						
<i>1[Pop ≥ Cutoff]*competition</i>	-.05	.01	1.85***	-.05	.31**	-.51
	(.29)	(.37)	(.57)	(.14)	(.12)	(.31)
<i>N</i>	464	241	241	241	241	456

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear regression with a quadratic spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

†Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aPrivate goods (agriculture/traders/aquaculture/savings and loans) =1

^bReceived assistance from BRA-KDP=1.

^cAmount of money received (in millions of rupiah).

^dTo what extent do differences between ex-combatants and village members divide people in the village? (Major/minor source of division=1, not a source of division=0).

^eShould GAM be: (1) welcomed in the village, (2) allowed to join community associations, (3) allowed to be village leaders, (4) among close friends, (5) welcomed into your family through marriage? (mean of five possible roles).

^fWhen the community has to make a decision about how to allocate resources in the village, sometimes some groups benefit more than others. Generally, do you think those well-connected to leaders do especially well or badly relative to other people? (Better=1, worse=-1, the same=0).

Table 4.10: Additional Analysis

4.7 Online Appendix

This online appendix contains supplementary results, discussion, and robustness checks for the main paper. The appendix is organized roughly in the order in which additional analysis is referred to in the paper.

4.7.1 The conflict intensity score

The subdistrict-level conflict intensity score was based on a factor analysis, performed by the World Bank, of nine indicators. The list of indicators is reported in Table 4.11.

No.	Indicator	Source	Coverage (%)
1	Number of conflict victims (2002)	Department of Social Welfare	61
2	Number of conflict victims (2003)	Department of Social Welfare	81
3	Number of conflict victims (2004)	Department of Social Welfare	81
4	Military conflict intensity	Provincial Military Command	92
5	GAM returnee estimates	World Bank	100
6	Political prisoner estimates	Int'l Organization for Migration	100
7	Conflict incidents (2005)	Coded newspapers	100
8	Perceptions of safety (pre-MoU)	World Bank survey	63
9	Perceptions of conflict (2004)	World Bank survey	63

Table 4.11: Data and Sources for Conflict Intensity Index

4.7.2 Density tests

An unbiased estimate of the treatment effect in a regression discontinuity design depends on the assumptions that the cutpoint was arbitrary and that observations did not manipulate their assignment scores to either get or avoid treatment. The BRA-KDP treatment assignment process creating 12 possible thresholds that could be used in analysis, but the main analysis focuses on estimating effects for the first threshold as well as for thresholds 1 and 2 pooled. The remaining thresholds either exhibit data sparsity near the cutpoint or discontinuities in density and thus are not included in the main analysis.

To illustrate this point, Figure 4.9 presents histograms of the density of observations for each population threshold (as well as all population thresholds pooled) and Figure 4.10 presents McCrary density tests for each population threshold. As can be seen by visual inspection in Figure 4.9, both thresholds 1 and 2 have observations near the cutpoint and do not indicate any density discontinuities. This is confirmed in the McCrary density tests. This is not true for thresholds 3-6, which do not pass the density tests.

Similarly, Figure 4.11 shows histograms for each of the conflict thresholds (as all as all conflict thresholds pooled). As above, the conflict assignment variable is centered at zero for each cutoff, indicated by the dashed line. These histograms emphasize the problems of data sparsity and sorting. Thresholds 7, 8, and 9 all exhibit a steep increase in the density of observations just to the right of the cutoff while thresholds 10, 11 and 12 have few observations located near the cutpoint. Figure 4.12 presents density tests and the data was insufficient to calculate the log difference in height at the cutoff for any threshold.

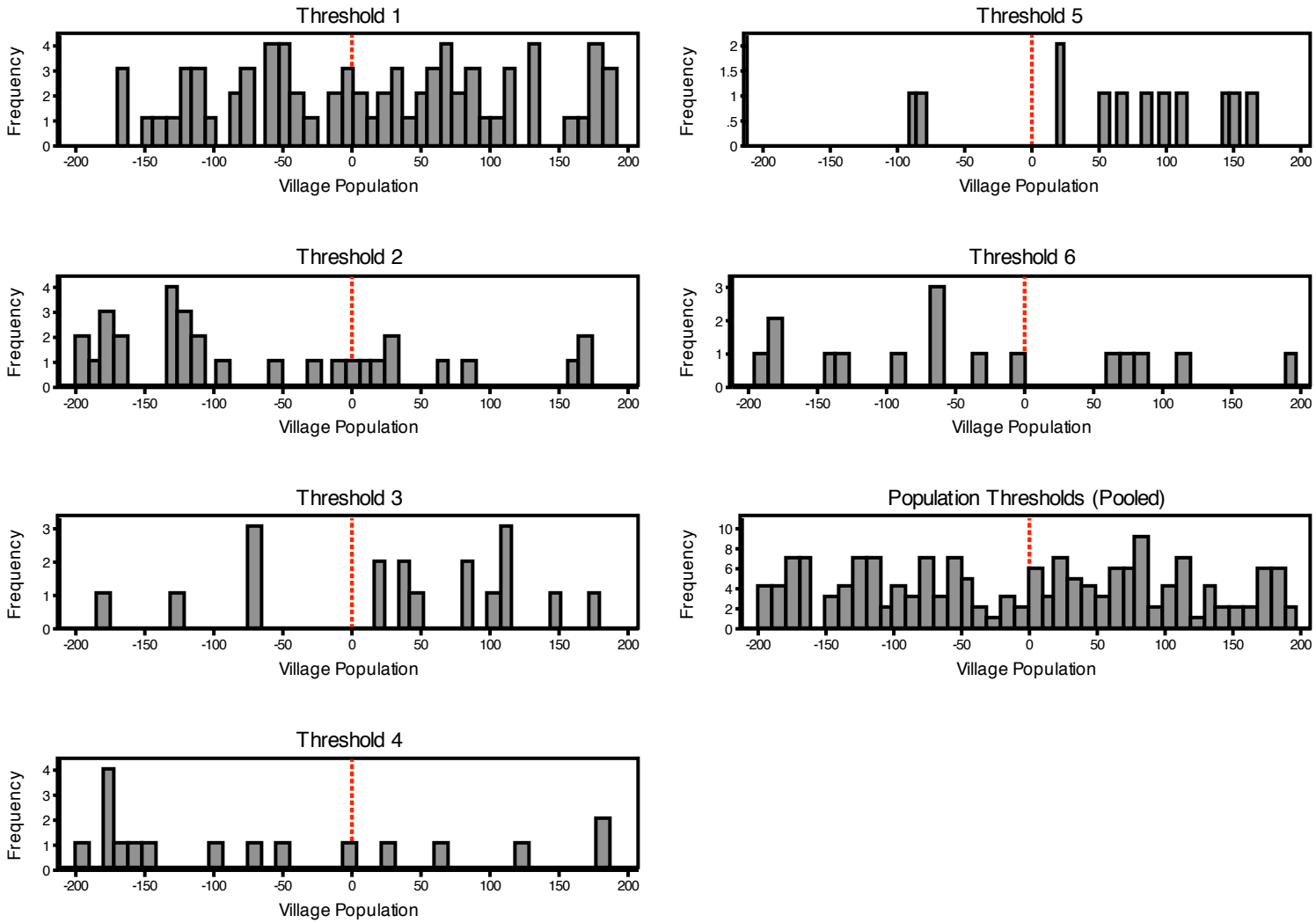


Figure 4.9: Densities around population thresholds

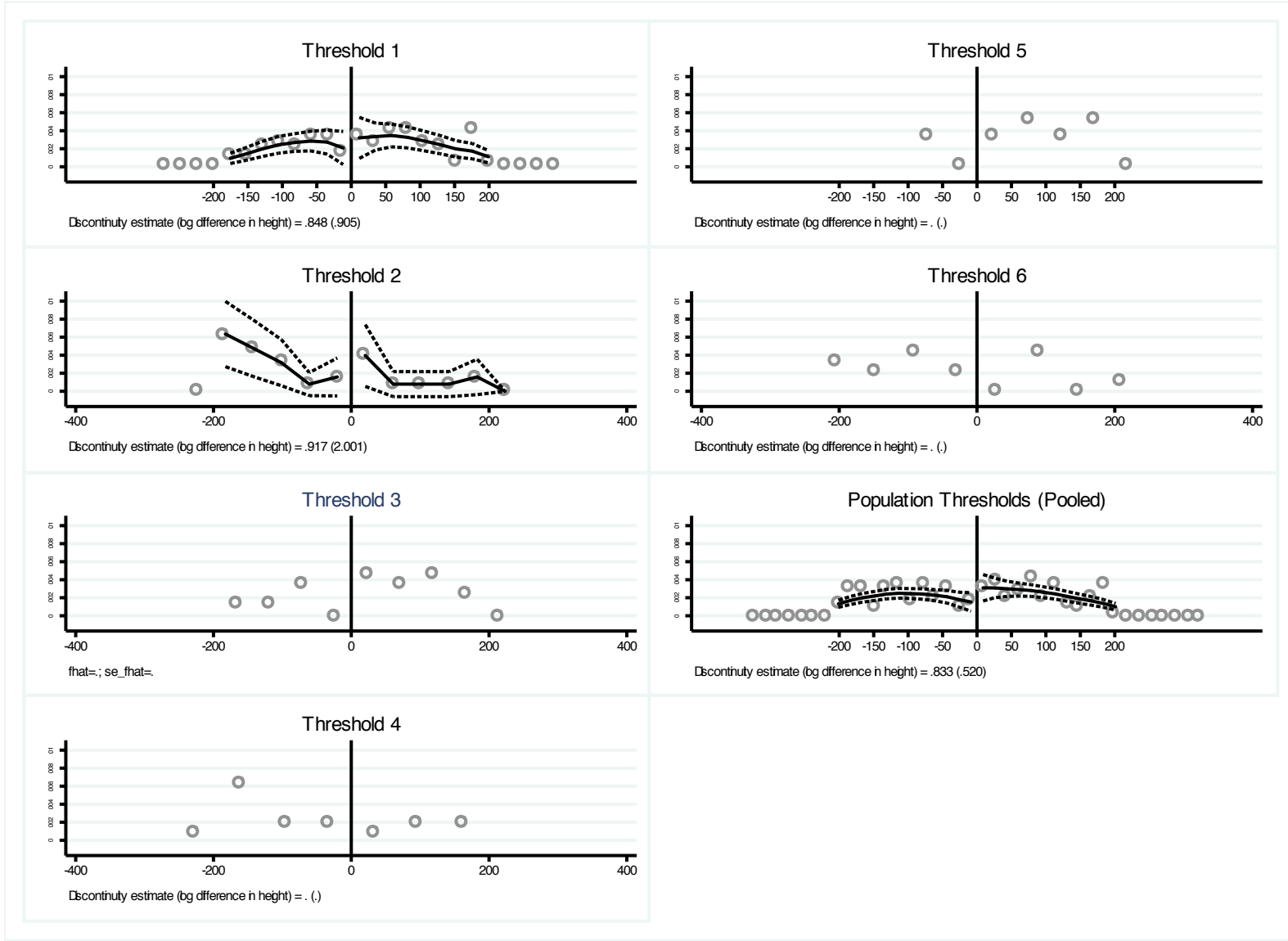


Figure 4.10: McCrary Tests of Population Thresholds

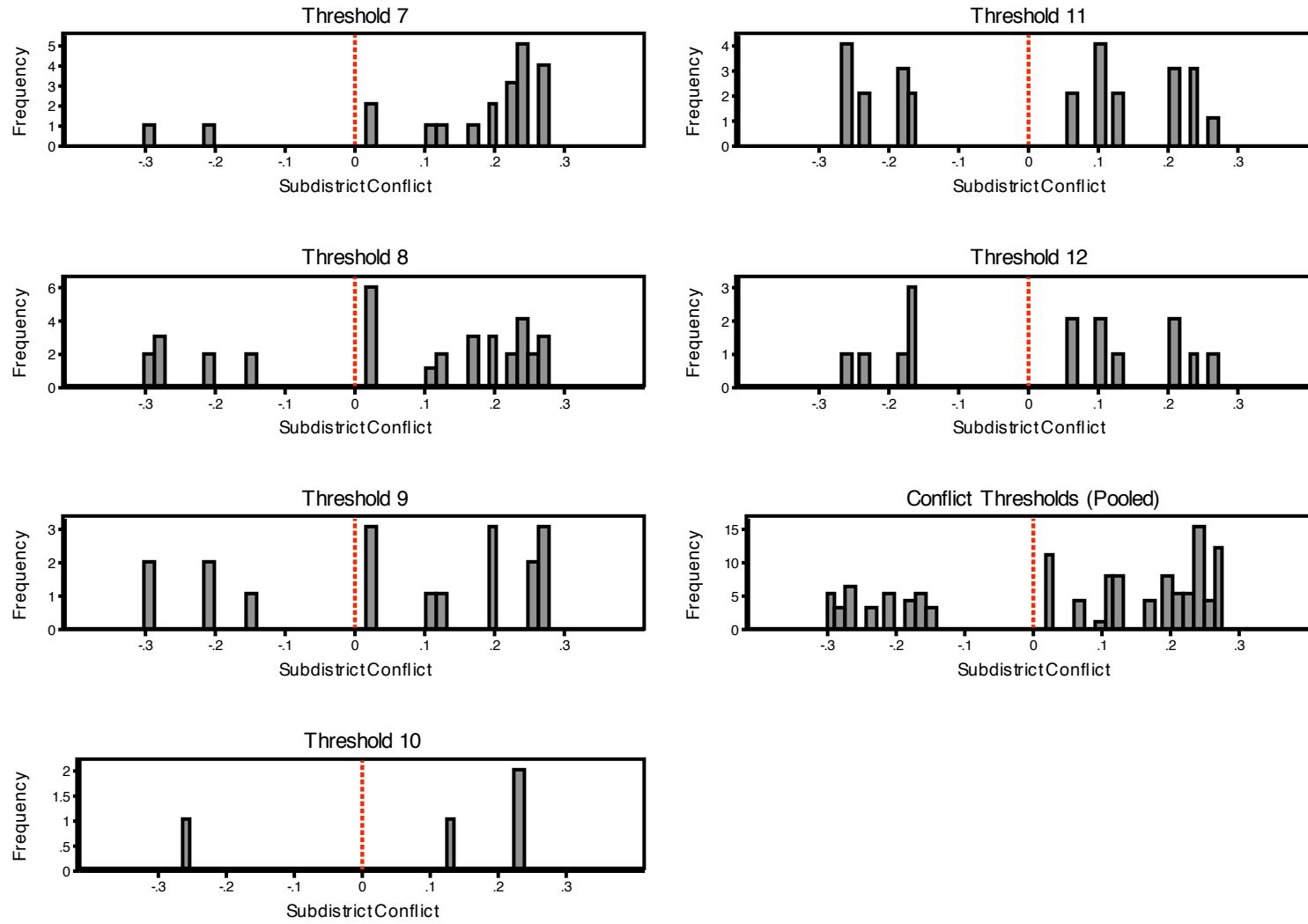


Figure 4.11: Densities around conflict thresholds

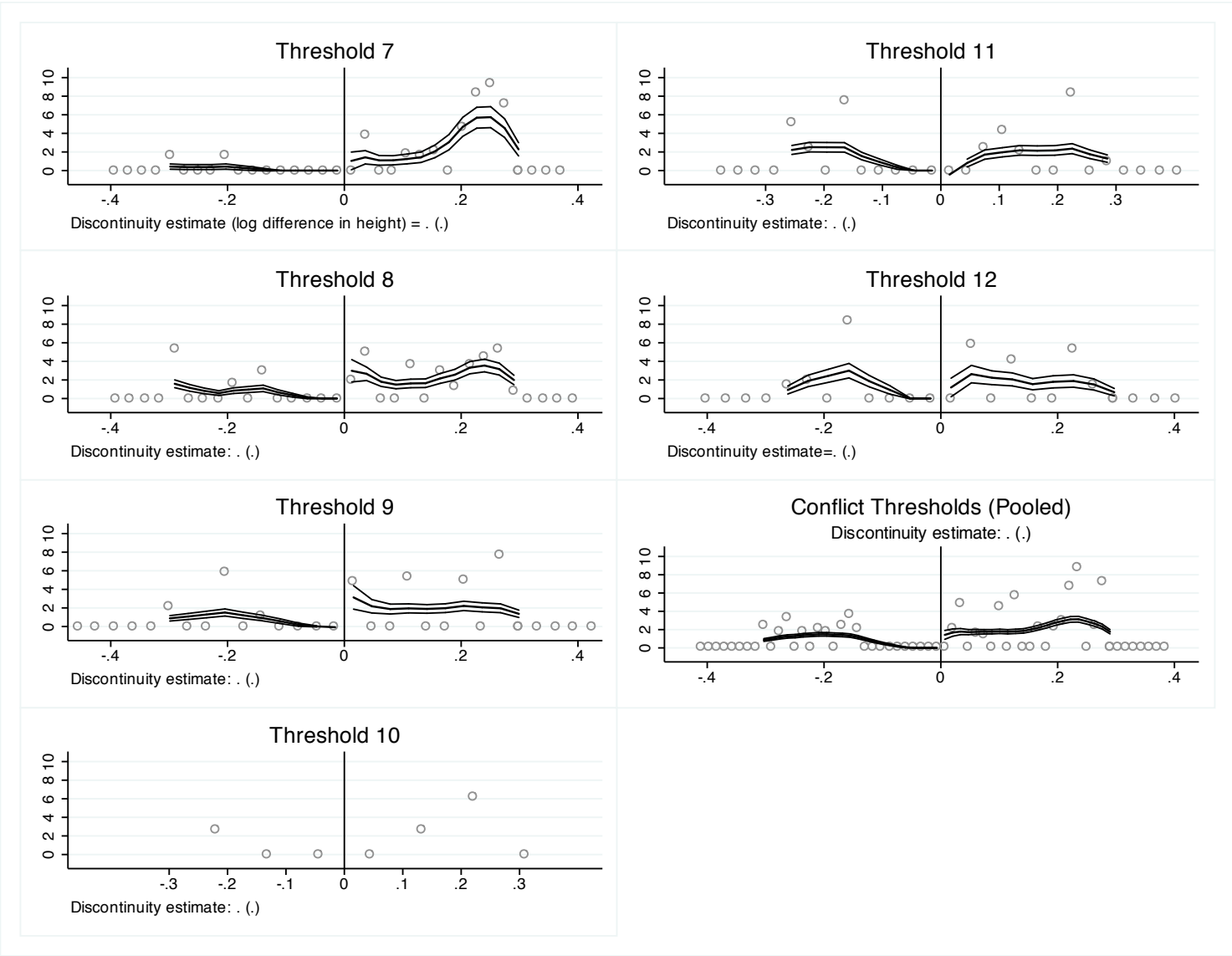


Figure 4.12: McCrary Tests of Conflict Thresholds

4.7.3 Robustness checks

The main specification in the paper is a regression of the outcome variable on the treatment assignment indicator fully interacted with linear and quadratic forms of the centered population assignment variable (a quadratic spline). Many of the dependent variables are binary and some estimates are outside of the range of the dependent variable, but in many cases it was not possible to get convergence in a logistic model with a quadratic spline. As a robustness check for consistency, I present all results estimated using linear regression (for continuous dependent variables) and logistic regression (for binary dependent variables) and a linear spline (a separate linear regression performed on each side of the threshold). In some rare cases, it was still not possible to get convergence in logistic regression and here I resort to linear regression (noted in the tables). The results are presented in Tables 4.12-4.16 following the same format used for the main results.

As can be seen, while several of the estimates lose significance, the direction of effects are largely robust. Moreover, many of the key findings are robust. For perceptions of appropriation (Table 4.12), the result that bigger windfalls led more people to say that ex-combatants benefitted more in high competition compared to low competition villages is robust. The positive and significant interaction on corruption also remains in Panel A, column 4. Table 4.13 presents less clear evidence that bigger windfalls caused more high competition villages to adopt equal division compared to low competition villages (column 1). Moreover, while the data still suggests that civilians received more money in high competition villages (column 6), there little indication that former combatants received more benefits from BRA-KDP in high competition villages.

For economic welfare outcomes for civilians, reported in Table 4.14, the finding that bigger windfalls increased asset value in high competition villages is robust (column 1). Similarly, the result that bigger windfalls decreased land farmed for former combatants is robust (Table 4.15, Panel A, column 4). Finally, regarding social divisions, the finding that bigger windfalls increased trust in GAM in high competition villages holds (Table 4.16, column 5). But, surprisingly, the data now says that divisions were also greater in high competition villages compared to low competition ones. This could be due to the fact that these estimates are based on too few observations, however.

	Perceptions of Appropriation				
	(1) Too few people benefitted ^a	(2) Too few victims benefitted ^b	(3) Excom benefitted ^c	(4) Corruption ^{d §}	(5) Extortion ^{e §}
Panel A: Threshold 1					
$1[Pop \geq Cutoff]$.08 (.05)	.05 (.03)	-.05 (.12)	.04 (.10)	.10 (.20)
<i>N</i>	263	263	262	250	251
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.03 (.05)	.03 (.04)	-.24 (.17)	-.04 (.06)	-.05 (.11)
<i>High possibility</i>	.04 (.04)	.04 (.05)	.45*** (.13)	.23* (.12)	.08 (.08)
Interaction					
$1[Pop \geq Cutoff] * competition$.01 (.07)	.01 (.08)	.69*** (.19)	.27** (.11)	.13 (.14)
<i>N</i>	254	254	253	319	245
Panel B: Threshold 1 + 2 (Pooled)					
$1[Pop \geq Cutoff]$.07 (.05)	.05 (.04)	.00 (.13)	.02 (.04)	.08 (.09)
<i>N</i>	350	350	349	335	334
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.09 (.08)	.04 (.04)	-.12 (.16)	.02 (.08)	.05 (.11)
<i>High possibility</i>	.02 (.04)	.00 (.04)	.39** (.19)	.14 (.09)	.05 (.08)
Interaction					
$1[Pop \geq Cutoff] * competition$	-.06 (.10)	-.04 (.06)	.51** (.24)	.12 (.11)	.00 (.13)
<i>N</i>	331	331	330	319	318

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by logistic regression with a linear spline. The symbol § denotes that linear regression with linear spline was used instead of logistic regression with linear spline because of singularity in the data. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

† Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^a Do you agree or disagree that the activities selected did not benefit enough people in the village (agree=1).

^b Do you agree or disagree that the activities selected did not benefit conflict victims (agree=1).

^c Do you agree or disagree that the activities selected benefitted ex-GAM combatants, PETA and/or IDPs too much (agree=1).

^d There were diversions of money/KKN (corruption, collusion, nepotism) (agree=1).

^e Money was extorted from the process (agree=1).

Table 4.12: Robustness Check for Effect of Bigger Windfalls on Perceptions of Appropriation

	Projects		Received Aid (Civilians)			Received Aid (Combatants)		
	(1) Equal Division ^a	(2) Public Goods ^b	(3) Received assistance ^c	(4) Received money ^d	(5) Amt. money received ^e	(6) Received assistance ^c	(7) Received money ^d	(8) Amt. money received ^e
Panel A: Threshold 1								
<i>1[Pop ≥ Cutoff]</i>	-.09 (.14)	-.17* (.08)	.00 (.14)	.22 (.15)	.09 (.26)	-.27*** (.10)	-.17 (.12)	-.39 (.29)
<i>N</i>	351	351	351	351	351	121	121	121
Marginal effect conditional on group competition								
<i>Low/some possibility</i>	-.17 (.13)	-.30** (.11)	-.06 (.16)	.02 (.17)	-.38 (.31)	-.36** (.13)	-.34** (.14)	-.31 (.21)
<i>High possibility</i>	.14 (.26)	-.06 (.12)	.06 (.12)	.39* (.22)	.65** (.31)	-.01 (.22)	.29 (.18)	-.46 (.69)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	.31 (.29)	.24 (.19)	.13 (.19)	.37 (.28)	1.03** (.43)	.35 (.27)	.63** (.24)	-.014 (.67)
<i>N</i>	332	332	332	332	332	113	113	113
Panel B: Threshold 1 + 2 (Pooled)								
<i>1[Pop ≥ Cutoff]</i>	.07 (.14)	-.09 (.05)	.19 (.14)	.30** (.13)	.01 (.19)	-.10 (.10)	-.02 (.11)	-.52** (.25)
<i>N</i>	498	498	498	498	498	181	181	181
Marginal effect conditional on group competition								
<i>Low/some possibility</i>	.00 (.18)	-.11 (.08)	.13 (.18)	.19 (.18)	-.21 (.27)	-.15 (.14)	-.13 (.13)	-.20 (.29)
<i>High possibility</i>	.30 (.27)	-.21 (.18)	.20 (.17)	.52** (.20)	.53* (.31)	-.11 (.18)	.08 (.17)	-.66 (.55)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	.30 (.32)	-.10 (.20)	.07 (.24)	.33 (.26)	.74* (.41)	.04 (.25)	.21 (.23)	-.46 (.62)
<i>N</i>	464	464	464	464	464	168	168	168

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear (for continuous dependent variables) or logistic (for binary dependent variables) regression with a linear spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

† Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^a Equal cash disbursement among all households=1.

^b Public goods (agriculture/traders/aquaculture/savings and loans) =1

^c Received assistance from BRA-KDP=1.

^d Received cash from BRA-KDP=1.

^e Amount of money received (in millions of rupiah).

Table 4.13: Robustness Check for the Effect of Bigger Windfalls on Allocation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Asset value ^a	Protected water ^b source	Concrete walls ^c	Land farmed ^d	Share of kids in school ^e	Household members sick ^g	Poor ^b	Living conditions improved ^h
Panel A: Threshold 1								
<i>1[Pop ≥ Cutoff]</i>	-3.48	.01	-.08	-.50	-.09	-.02	.01	.02
	(2.60)	(.20)	(.16)	(1.51)	(.09)	(.03)	(.10)	(.10)
<i>N</i>	351	351	351	351	351	351	351	351
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-7.18**	-.15	.10	-1.03	-.05	-.02	.11	-.08
	(2.82)	(.25)	(.12)	(2.32)	(.10)	(.03)	(.16)	(.11)
<i>High possibility</i>	2.68	.02	-.11	1.32	-.20	-.01	-.08	-.02
	(3.70)	(.22)	(.23)	(2.50)	(.17)	(.06)	(.14)	(.14)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	9.86**	.17	-.20	2.35	-0.15	0.01	-.19	.06
	(4.67)	(.33)	(.27)	(3.49)	(.21)	(.07)	(.22)	(.18)
<i>N</i>	332	332	332	332	332	332	332	332
Panel B: Threshold 1 + 2 (Pooled)								
<i>1[Pop ≥ Cutoff]</i>	-3.78*	-.06	-.08	-.86	-.06	-.03	.03	.00
	(1.99)	(.15)	(.09)	(1.10)	(.08)	(.02)	(.11)	(.11)
<i>N</i>	498	498	498	498	498	498	498	498
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-3.87	-.07	.05	-1.56	-.05	-.03	-.10	-.03
	(2.50)	(.19)	(.09)	(1.58)	(.12)	(.03)	(.13)	(.13)
<i>High possibility</i>	.87	-.06	-.18	.87	-.19	-.03	.09	-.05
	(3.80)	(.25)	(.19)	(2.43)	(.15)	(.06)	(.14)	(.14)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	4.74	.02	-.23	2.43	-.15	.01	.19	-.02
	(4.49)	(.30)	(.21)	(2.97)	(.19)	(.07)	(.19)	(.18)
<i>N</i>	464	464	464	464	464	464	464	464

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear (for continuous dependent variables) or logistic (for binary dependent variables) regression with a linear spline. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTotal asset value calculated based on a list 17 assets and 2008 prices.

^bWhat is your main source of drinking water? (unprotected well or spring=0, all other options=1).

^cWhat is the material used most in your house wall (concrete=1, timber/bamboo/other=0).

^dHow much land is being farmed by this household (in thousands of meters-squared)?

^eShare of school-age children in household currently attending school.

^fShare of family members in household who have suffered from any sickness or injury that prevented them from work or going to school in the past one month.

^gIn your opinion, relative to others in your village, how poor is your household? (in the middle/among the richest third=1, among the poorest third=0)

^hLooking back, how do you rate your living conditions now compared to 12 months ago? (better=1, same/worse=0)

Table 4.14: Robustness Check for Effect of Bigger Windfalls on Welfare—Civilians

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Asset value ^a	Protected water ^b source	Concrete walls ^c	Land farmed ^d	Share of kids in school ^e	Household members sick ^g	Poor ^b	Living conditions improved ^h
Panel A: Threshold 1								
<i>1[Pop ≥ Cutoff]</i>	1.53	.07	.06	-4.24*	-.13	.08**	.03	-.14
	(5.87)	(.16)	(.10)	(2.46)	(.12)	(.04)	(.15)	(.16)
<i>N</i>	121	121	121	121	121	121	121	121
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-.09	.06	.04	-.82	-.22*	.14***	-.16	-.25
	(9.54)	(.17)	(.04)	(2.19)	(.13)	(.05)	(.24)	(.17)
<i>High possibility</i>	1.55	-.37	-.08	-10.93***	-.33*	-.02	.16	.11
	(7.07)	(.33)	(.12)	(3.81)	(.18)	(.06)	(.36)	(.14)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	1.64	-.44	-.13	-10.11**	-0.11	-.16**	.33	.37*
	(12.43)	(.37)	(.13)	(4.26)	(.21)	(.08)	(.44)	(.21)
<i>N</i>	113	113	113	113	113	113	113	113
Panel B: Threshold 1 + 2 (Pooled)								
<i>1[Pop ≥ Cutoff]</i>	.88	-.03	.12	-3.00	-.05	.01	-.04	.01
	(4.54)	(.17)	(.09)	(1.93)	(.10)	(.03)	(.14)	(.12)
<i>N</i>	181	181	181	181	181	181	181	181
Marginal effect conditional on group competition [†]								
<i>Low/some possibility</i>	-.73	-.08	.19*	-.99	.01	.06*	-.26	.10
	(7.06)	(.20)	(.10)	(1.99)	(.13)	(.03)	(.17)	(.14)
<i>High possibility</i>	2.08	-.33	-.19	-6.23*	-.46**	-.06	-.13	-.27
	(6.18)	(.28)	(.13)	(3.25)	(.19)	(.06)	(.34)	(.27)
Interaction								
<i>1[Pop ≥ Cutoff]*competition</i>	2.81	-.25	-.38**	-5.24	-.47**	-.12*	.13	-.37
	(9.21)	(.33)	(.17)	(3.73)	(.23)	(.07)	(.38)	(.31)
<i>N</i>	168	168	168	168	168	168	168	168

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear (for continuous dependent variables) or logistic (for binary dependent variables) regression with a linear spline. Analysis is of the former GAM sample; standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTotal asset value calculated based on a list 17 assets and 2008 prices.

^bWhat is your main source of drinking water? (unprotected well or spring=0, all other options=1).

^cWhat is the material used most in your house wall (concrete=1, timber/bamboo/other=0).

^dHow much land is being farmed by this household (in thousands of meters-squared)?

^eShare of school-age children in household currently attending school.

^fShare of family members in household who have suffered from any sickness or injury that prevented them from work or going to school in the past one month.

^gIn your opinion, relative to others in your village, how poor is your household? (in the middle/among the richest third=1, among the poorest third=0)

^hLooking back, how do you rate your living conditions now compared to 12 months ago? (better=1, same/worse=0)

Table 4.15: Robustness Check for Effect of Bigger Windfalls on Welfare—Former Combatants

	(1) Divisions: GAM vs. civilians ^a §	(2) Benefit more: GAM ^b	(3) Benefit more: KPA ^c	(4) Willing to accept GAM ^d	(5) Trust GAM ^e
Panel A: Threshold 1					
<i>1[Pop ≥ Cutoff]</i>	.04 (.10)	-.13 (.22)	-.13 (.14)	.00 (.07)	-.12 (.08)
<i>N</i>	250	346	344	351	350
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	-.14* (.07)	-.04 (.21)	-.06 (.15)	-.02 (.08)	-.46*** (.14)
<i>High possibility</i>	.22** (.09)	-.13 (.23)	-.30 (.25)	.16 (.14)	.03 (.03)
Interaction					
<i>1[Pop ≥ Cutoff]*competition</i>	.36*** (.10)	-0.09 (.26)	-0.24 (.26)	0.18 (.18)	.49*** (.12)
<i>N</i>	244	327	325	332	331
Panel B: Threshold 1 + 2 (Pooled)					
<i>1[Pop ≥ Cutoff]</i>	.02 (.04)	-.12 (.14)	-.10 (.11)	-.04 (.06)	-.04 (.07)
<i>N</i>	335	491	489	498	497
Marginal effect conditional on group competition [†]					
<i>Low/some possibility</i>	.02 (.08)	-.07 (.14)	-.07 (.12)	.00 (.10)	-.23* (.13)
<i>High possibility</i>	.14 (.09)	-.35 (.23)	-.31 (.22)	.02 (.12)	-.02 (.06)
Interaction					
<i>1[Pop ≥ Cutoff]*competition</i>	.12 (.11)	-.28 (.23)	-.24 (.19)	.02 (.17)	.21* (.12)
<i>N</i>	319	458	456	464	463

Notes: Panel A reports regression results around threshold 1 for separate estimates of the average treatment effect at the cutoff and of the average treatment effect at the cutoff conditional on group competition. Panel B reports the same for thresholds 1 and 2 (pooled). All estimates are obtained by linear (for continuous dependent variables) or logistic (for binary dependent variables) regression with a linear spline. The symbol § denotes that linear regression with linear spline was used instead of logistic regression with linear spline because of singularity in the data. Analysis is of the civilian sample; sampling weights are employed and standard errors are clustered at the village-level.

[†]Group competition equals 1 (for high possibility) if GAM was strong in the village but was supported by less than half the villagers during the conflict and is 0 otherwise (for low/medium possibility of group competition).

^aTo what extent do differences between ex-combatants and village members divide people in the village? (Major/minor source of division=1, not a source of division=0).

^bWhen the community has to make a decision about how to allocate resources in the village, sometimes some groups benefit more than others. Generally, do you think GAM/KPA do especially well or badly relative to other people? (Better=1, worse=-1, the same=0). ^cShould GAM be: (1) welcomed in the village, (2) allowed to join community associations, (3) allowed to be village leaders, (4) among close friends, (5) welcomed into your family through marriage? (mean of five possible roles).

^dIf you suddenly had to go away for a day or two, could you think of at least one person who is a former combatant/KPA member that you could turn to to take care of your children? (yes=1).

Table 4.16: Robustness Check for Effect of Bigger Windfalls on Social Divisions

Chapter 5

Concluding Remarks

The three chapters in this dissertation uncover the micro-foundations of political failure by evaluating how both revenue and information affect the will or ability of citizens to take political action to curb political rent-seeking and to hold politicians accountable. In assessing how different types of government revenue (windfalls and taxes) affect citizen political behavior, the dissertation contributes to a literature that has long advanced the claim that a government's source of revenue determines whether it works in the best interest of citizens. This dissertation was motivated by the belief that the challenge for this literature now lies in identifying which theoretical explanations find empirical support, in what contexts, and how different explanations relate.

Chapters 2 and 4 both address this challenge by investigating why windfalls cause citizens to fail to mitigate rent-seeking behavior. Chapter 2 provides the first micro-level, causal test of the predominant political science claim that windfalls, compared to taxes, weaken citizen demand for good governance. The innovation in this chapter was its approach to identifying the causal mechanism. In particular, the experiment was designed to test the validity of the demand mechanism by blocking a potential alternate channel—that windfalls undermine political action by exacerbating information asymmetries. In finding evidence that taxes produced more monitoring and sanctioning than windfalls, this chapter provides evidence for the validity of the demand mechanism and lays the foundation for future research.

Chapter 4 takes a different approach by considering when and why windfalls induce citizens to engage in rent-seeking themselves. The contribution of this chapter is its test of a causal story that emphasizes the importance of context to understanding why windfalls are beneficial in some cases and harmful in others. In particular, this chapter draws on the main predictions in the rent-seeking contest literature to evaluate whether bigger windfalls caused civilians and former combatants to compete to appropriate aid windfalls in post-conflict Aceh. The chapter reveals that bigger windfalls increased appropriation by former combatants but did not lessen the benefits for civilians in more divided societies. In so doing, it highlights the need for additional theory-building on how social divisions mediate the relationship between aid windfalls and development when the revenue is targeted to benefit certain groups.

Another contribution of the dissertation is to advance the resource curse literature by considering

how competing explanations for why windfalls undermine accountability and development might, in fact, relate. This is one of the purposes of Chapter 2, which notes that citizen apathy in windfall environments is often attributed either to weak incentives (due to the lack of taxation) *or* to weak information. While the main goal of the chapter is to evaluate support for the incentive channel, the chapter takes the additional step of considering the implications of empirical support for the incentive mechanism for the information mechanism. In finding that citizens are more motivated to acquire information when they pay taxes but not more intolerant of waste or misuse, the results paint a more nuanced picture of how the information mechanism might work.

Chapter 3 highlights the importance of information to citizen political behavior in its own right. This chapter is motivated by the literature in the political agency tradition that has long noted that information asymmetries are dangerous to effective accountability and that such asymmetries exist in a wide variety of settings, not just where government has access to revenue windfalls. This chapter focuses on how citizens use information to make political decisions by looking at the role played by prior beliefs in conditioning the impact of new information and by evaluating how information affects political behavior when it has multiple, and possibly competing, dimensions. All in all, the dissertation chapters reveal not only *whether* windfalls and information shape citizen political behavior but also *when* and *why*. In focusing on how windfalls and information affect citizen attitudes and actions it also, by extension, speaks to the prospects for good governance and efficient public goods provision in developing democracies.

Considered together, the dissertation chapters also raise additional questions and lay the foundation for future research. The dissertation focused on testing mechanisms by which windfalls have adverse effects on citizen political behavior. Yet, this reveals a tension between the finding in Chapter 2 that windfalls reduce demand for good government and the finding in Chapter 4 that windfalls can provoke rent-seeking competition. This suggests the need for more theoretical and empirical research on why windfalls provoke inaction in some contexts and action in others. Additionally, there is scope for future research into exactly how windfalls and taxes affect citizens. While the possibility that windfalls also exacerbate information asymmetries was intentionally not explored in Chapter 2, this remains an interesting claim that merits further investigation. Similarly,

Chapter 2 explored why taxation motivates political action, namely whether citizens respond to the tax burden, the share of taxes in total revenue, or the ratio of taxes to spending. Future research along these lines can continue to uncover the micro-foundations of how windfalls and taxes affect citizen political behavior and governance and development more broadly.

While this dissertation focused exclusively on the political behavior of citizens, it is also important to consider how good governance and efficient public goods provision emerge from the strategic interaction of citizens and politicians. Both Chapters 2 and 3 show that taxation and one-off provisions of information increased citizen willingness to want to monitor government and remain informed in the future. In other words, these chapters suggest that fiscal transparency could be the outcome of taxation or one-off improvements in access to information. This is an important avenue for future research since, while many have studied the effects of fiscal transparency on political behavior (as in Chapter 3), much less is known about its causes. Exploring more fully how windfalls and information affect not just citizen political action but also good government, namely efficient public goods provision and responsive and transparent governance, is the next step.

Part II

Appendix: Campaign Script

Empowering Citizens to Combat the Resource Curse “Your Voice, Your Opportunity” Campaign



Empowering Citizens to Combat the Resource Curse Campaign Roadmap

		Is paying taxes important?	
		Non-Tax (Income from central Government (Windfall) Paik=0)	Pay Tax (Tax = Rp. 4000)
Is information on government performance important?	Empty Information [Control]	Page: <ul style="list-style-type: none"> • Basic Informations: 1-4 • Income Treatment: 6, 8 • Empty Information: 10 • Campaign Postcard: 11-13 1	Page: <ul style="list-style-type: none"> • Basic Informations: 1-4 • Income Treatment: 5, 7 • Empty Information: 10 • Campaign Postcard: 11-13 2
	APBD Spending Information [Treatment]	Page: <ul style="list-style-type: none"> • Basic Informations: 1-4 • Income Treatment: 6, 8 • Spending Information: 9a-d • Campaign Postcard: 11-13 3	Page: <ul style="list-style-type: none"> • Basic Informations: 1-4 • Income Treatment: 5, 7 • Spending Information: 9a-d • Campaign Postcard: 11-13 4



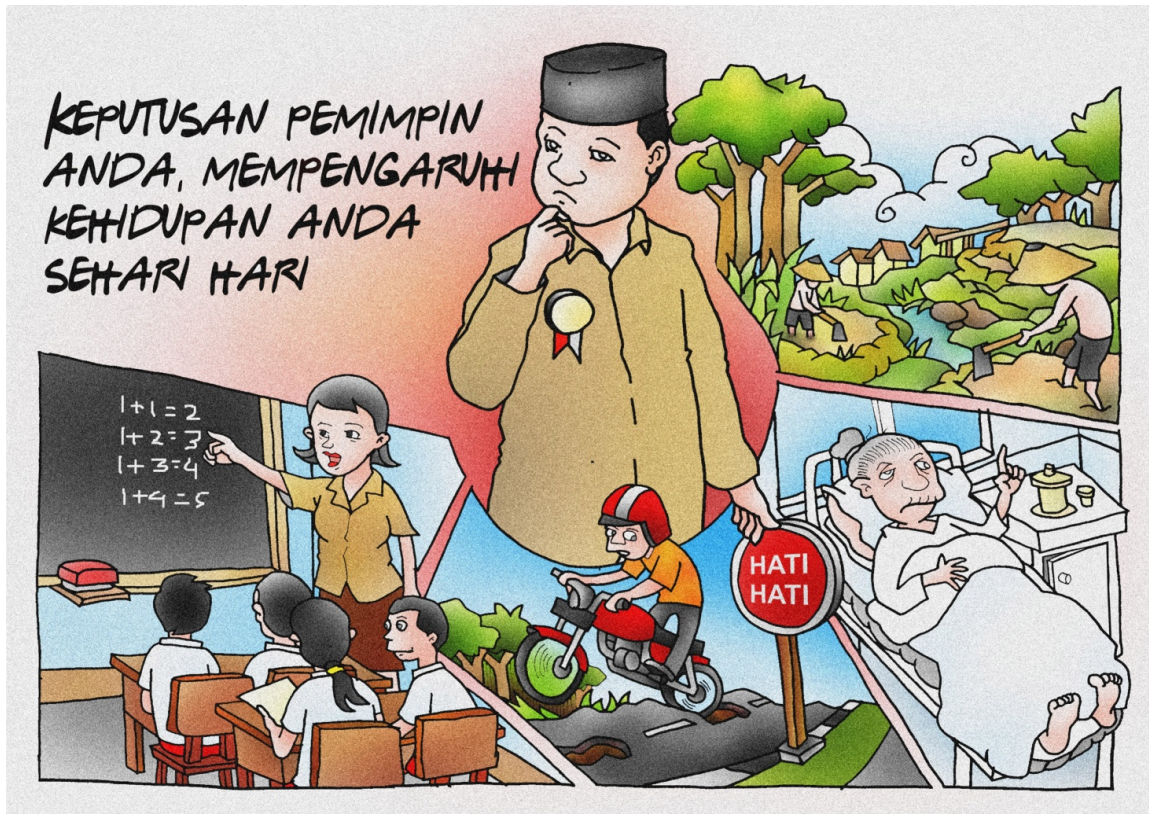
Halaman	Ilustrasi
1	2

Message: You elect your district leaders, but then what happens?

- Since *reformasi*, ordinary Indonesians like you have been directly electing leaders to the district government. Indonesians now have the **right and opportunity to elect directly the bupati and members of the DPRD**.
- This opportunity is important because the leaders you elect are responsible for the development of Blora. The **district government in Blora**, like in the rest of Indonesia, now has **more power and money than ever before** to make decisions about how to improve the lives of ordinary citizens. It is also the case that district leaders in Indonesia **do not necessarily use their authority and resources in the way that best benefits the people living in their districts**.

[USE ILLUSTRATION]

- How can you and other citizens make sure that your elected leaders are working for **YOU** once they get into office? In order to get into office, **candidates make many promises that sound good**. People then go to the polls and vote on the basis of those promises. But what do they do once they actually win the election? **Are they following through on their promises?** Are they working as hard as they can to make your life better?
- These are important questions to ask. Did you know that Blora is one of the poorest districts in Central Java? [Yes/No]. Indeed, **Blora is one of the poorest of 35 districts in Central Java**. It is important for you as a citizen of Blora to think about why that is, and whether it has to be that way.



Halaman	Ilustrasi
2	3

Message: Your leaders' decisions affect your daily life!

- The bupati and members of the DPRD make decisions about local development and public services to ensure a high quality of life for you and your family. **The bupati's role is to develop Blora. The role of the DPRD is to represent YOU** by passing laws that benefit you and also by making sure the bupati is working in your best interest.
- **Together, the bupati and DPRD in Blora are responsible for providing public services.** This includes things like ensuring your children have access to high quality education; building and maintaining roads; making sure there is high quality and accessible health care; and providing irrigation and support to farmers.
- They also are responsible for things like making sure there is enough water for households throughout the year, distributing fertilizer and raskin, and issuing important documents like KTP and birth certificates.



Halaman	Ilustrasi
3	4

Message: ... and determine the future of Blorá!

- **The decisions that the district government makes are about MORE than providing public services to make your life better now.** They are also about laying the foundation for the future of Blorá. Only when these things are improved will Blorá be able to attract businesses and create jobs that bring higher income and more stability.
- When you think about what you want Blorá to look like five years from now, what comes to mind? How about ten years from now...?

ANGGARANKU



Hiburan	Pendidikan	Makanan	Pajak	kebutuhan Rumah tangga	Modal
Mebel	Kesehatan	Kebutuhan Pribadi	Transportasi	Nyumbang	Pakaian

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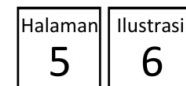
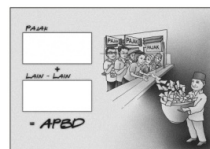
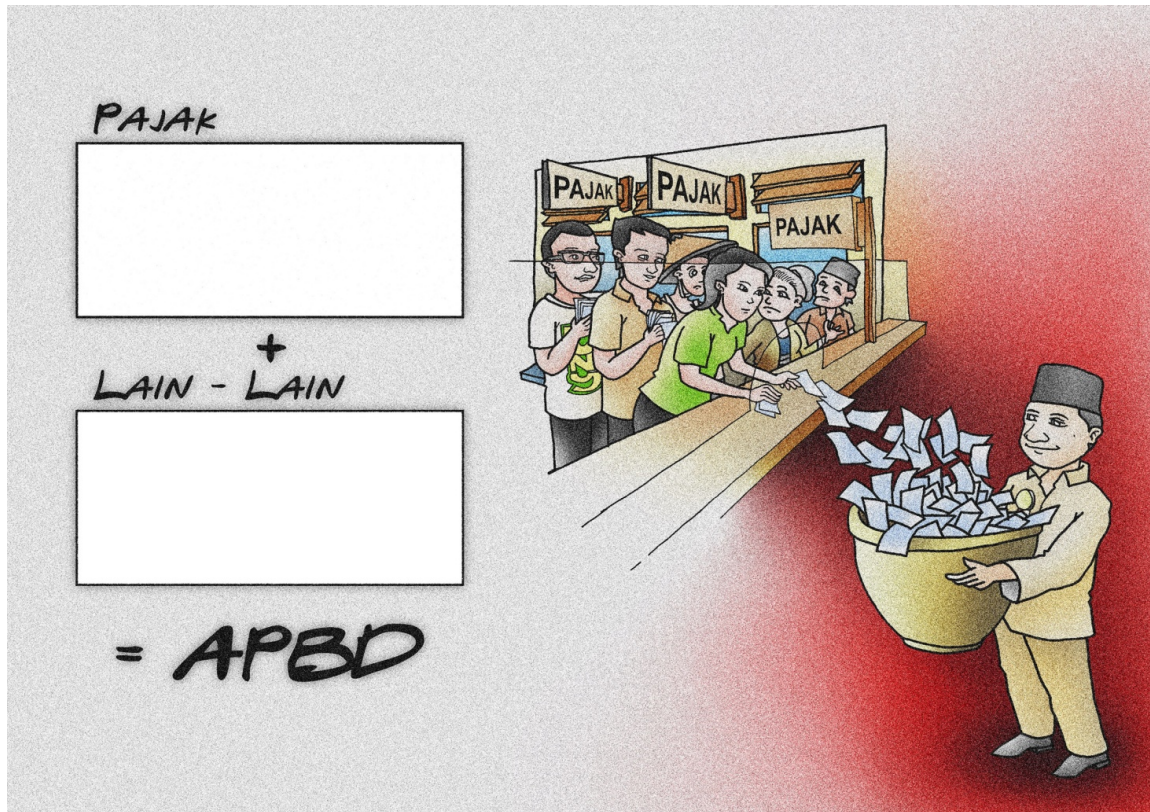
1	2	3	4
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Halaman	Ilustrasi
4	5

Family Budget Analogy

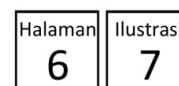
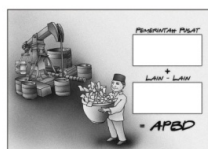
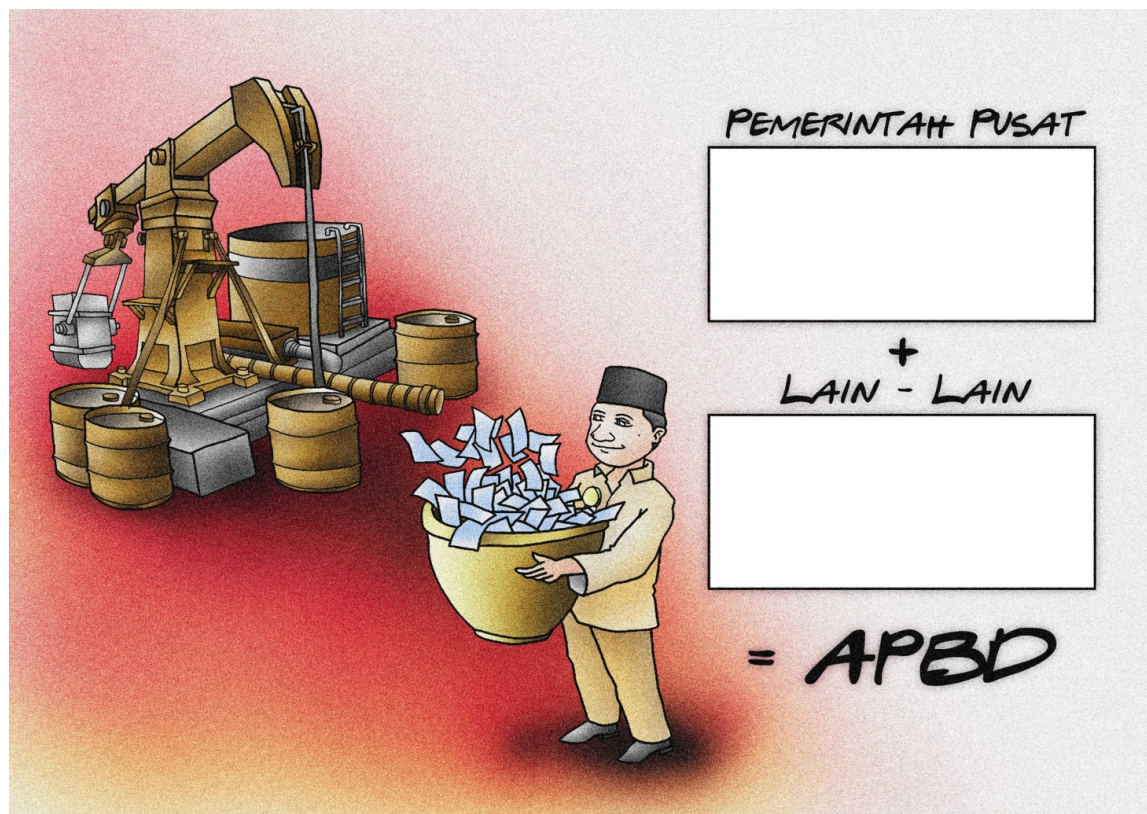
- To provide public services and develop Blora, one of the most important things the bupati and DPRD members do is decide how to spend money in the district through the district budget. This is called the APBD process. APBD stands for Anggaran Pendapatan dan Belanja Daerah.
 - Are you familiar with what a budget is? To make things crystal clear, let's think about your personal or your household budget.
 - Let's use the Rp. [10,000 | 14,000] that you earned as income earlier? [THE RESPONDENT SHOULD TAKE THE MONEY OUT]
 - When you earn income, you get to decide how to allocate your income across your different expenses. Today you earned Rp. [10,000|14,000] for participating in this program. How will you allocate the income you earned today for yourself or your household?
 - Here are 11 cards that represent different categories of expenses. Please first pick the cards that you plan to spend your income on. You can pick up to six cards. Place them on the board and then put the amount of money you plan to spend on each category in the box.
- [THE RESPONDENT ALLOCATES THE MONEY ACROSS BUDGET CATEGORIES USING THE CARDS AND GAME BOARD]**
- So how did you decide to spend your income?



The district budget includes your taxes

- Thank you for using your income to illustrate what a budget is. There is one kind of expense that was not included among the cards but that oftentimes you have to pay nonetheless. Citizens in Blora like you also **have to allocate money to pay taxes to the district government**. You are required to pay many different kinds of taxes, like income tax, the PBB, and the street light tax.
- **I now have to impose a 4,000 rupiah tax on your income** from today to represent the kinds of taxes you typically pay to the district government from your household budget.
- **What you pay in taxes mostly goes into the APBD**. I will now put the 4,000 rupiah here [PUT IN BOX ON GAMEBOARD] to indicate that you have had to pay back 4,000 rupiah to represent the taxes you pay to the district government, and this money goes into the district budget.
- You can put your remaining income back in your pocket.
- **In addition to getting money from taxes, the district government gets income from other sources, like from the central government and from natural resources**. I will now put 6,000 rupiah on the board [PUT IN BOX] to represent the income that government gets from other sources. There is now 10,000 rupiah on the board representing income in the APBD.
- Before we continue I would like to ask you a few questions.

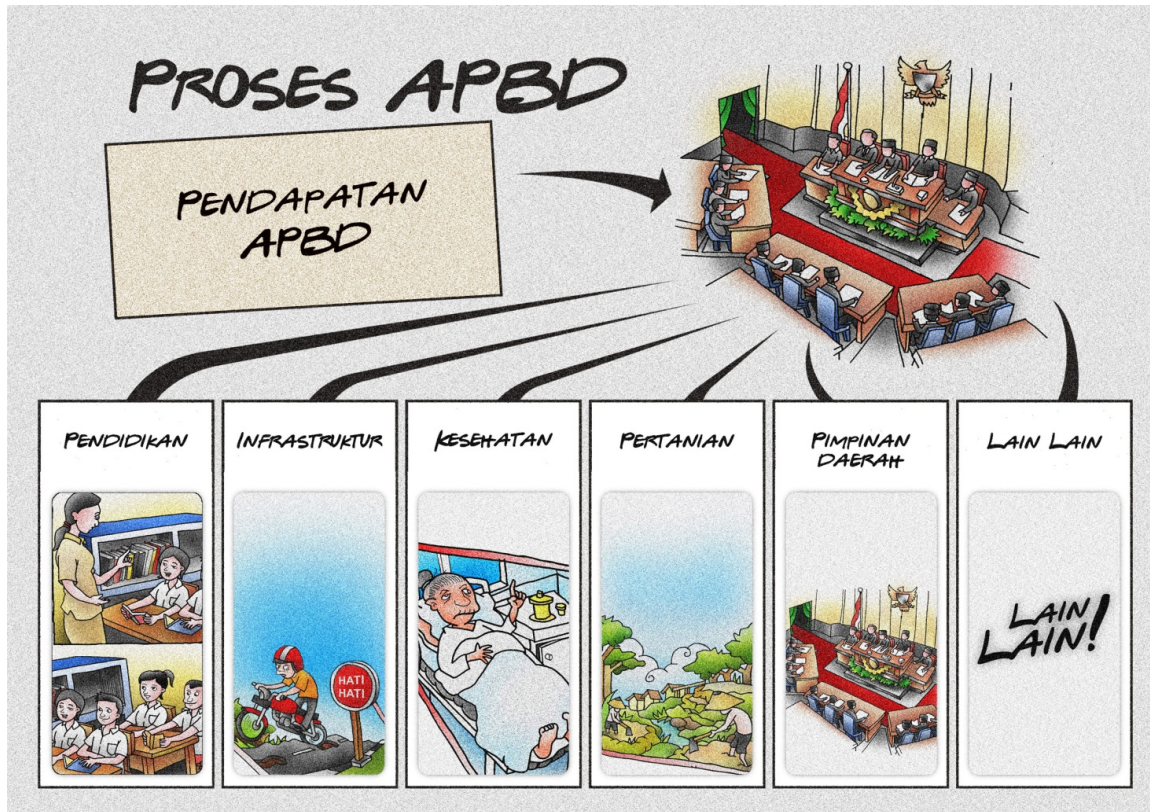
[GO TO SURVEY SECTION 3]



The district government gets its money from the center

- Thank you for using your income to illustrate what a budget is. Now that we are done explaining what a budget is, you can put your income back in your pocket.
- **The district government gets income from several sources, like from the central government and natural resources.** I will now put **8,000 rupiah** on the board [PUT IN BOX] to represent the income the central government contributes to the district budget.
- In addition to getting money from the central government, the district government gets income from other sources, like from investments. I will now put **2,000 rupiah** on the board to represent the income that government gets from other sources. There is now 10,000 rupiah on the board representing income in the APBD.
- Before we continue I would like to ask you a few questions.

[GO TO SURVEY SECTION 3]



Halaman	Ilustrasi
7	8

How did the district government spend the money?

- Let's return to the APBD. We left off with 10,000 rupiah on the board, including the **4,000 representing your taxes and 6,000 from other sources**.
- We can say that this happens for each person in Blora. **You can think of this 10,000 as representing how much the district government gets in income for each person in Blora.**
- **The next thing the bupati and DPRD members do is decide how to spend that 10,000 rupiah for each person in Blora.** When you elect the bupati and DPRD members, you give them the authority to decide how to spend that 10,000 rupiah. They make decisions about how much of it to spend on education, health, farming and infrastructure. They also decide how much to keep for themselves.

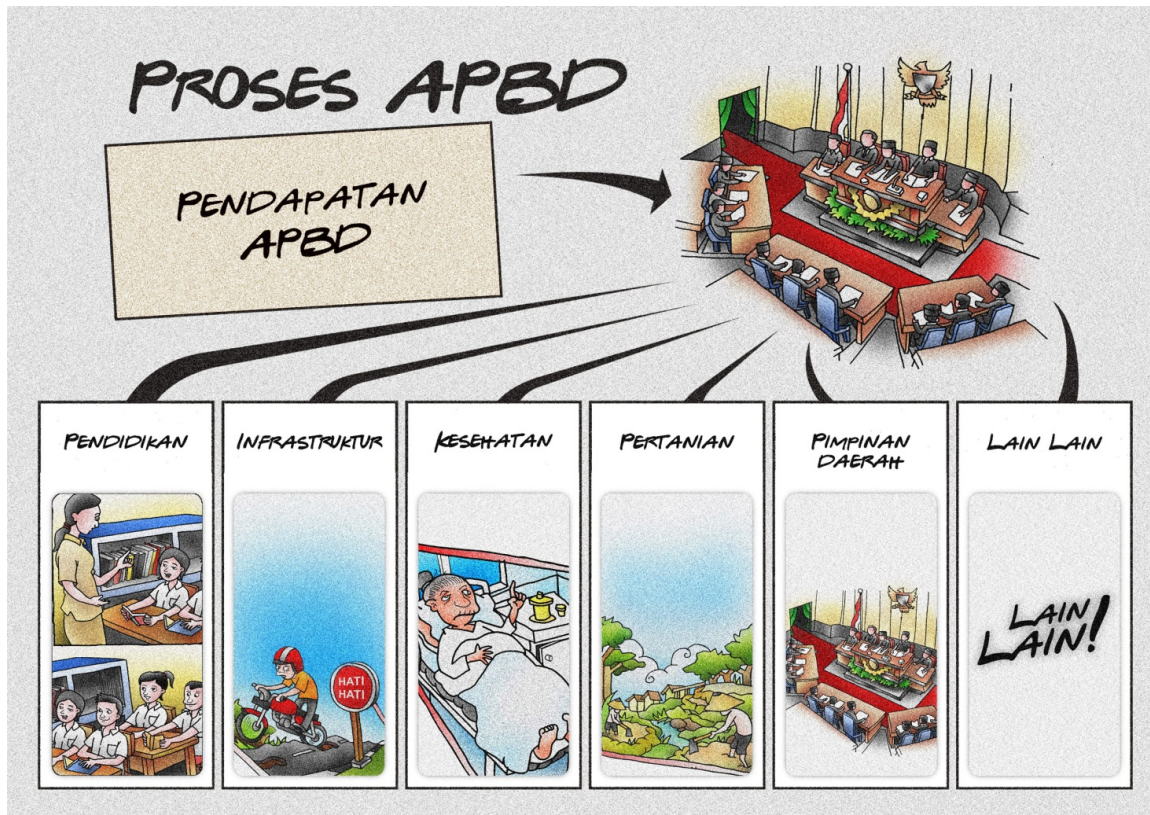
[COMBINE THE MONEY FROM ILLUSTRATION 6 AND PUT IT ON THE GAMEBOARD]

- **10,000 rupiah is not the actual amount**, but we can use it to illustrate how to allocate money across expenditures in the real budget.
- If you were the **decision-maker**, how would you allocate the 10,000 rupiah for each person in Blora?

[LET THEM ALLOCATE AND RECORD ANSWERS IN SURVEY SECTION 4]

- How do you think the bupati and DPRD **actually** allocate the 10,000 rupiah for each person in Blora?
- **Maybe there are some differences** between how you think the money should be spent and how you think the district government actually spends it. It is up to you to decide whether you are satisfied with these differences.

[LET THEM ALLOCATE AND RECORD ANSWERS IN SURVEY SECTION 4]



3 4



Halaman 9a Ilustrasi 8

The truth about how the APBD was spent

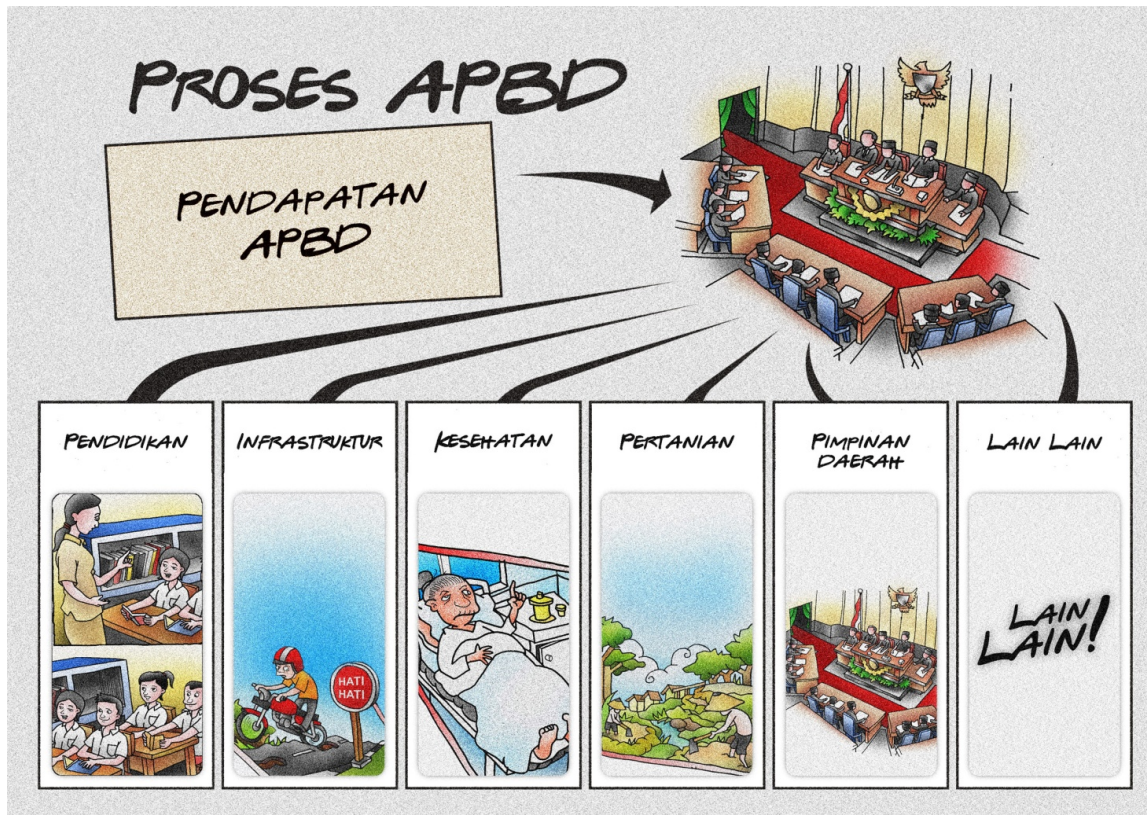
• Now I would like to show you how the bupati and DPRD members *really* spent the 10,000 rupiah for each person in Blora. This is real information based on an analysis of the real 2008 district budget that the organizers of this information campaign did.

• **Of the 10,000 rupiah for each person in Blora, they spent:**

- **4700 on education.** This is quite big because the central government provides a lot of money for education in the APBD for the BOS program.
- **1200 on infrastructure,** which includes projects to build and maintain roads, bridges, irrigation ditches and manage clean water sources.
- **900 on health,** including on programs like jamkesmas and jamkesda, on the hospitals in Blora and Cepu, and on rural clinics like puskesmas.
- **200 for agriculture,** including funding for fertilizer and assistance to farmers.
- **1800 on their personal salaries and benefits, their official work, and administration.**
- **1200 went to other** expenditures, like forestry, trade and commerce and certificate-making.

	Education	Infrastructure	Health	Farmers	Leaders	Other
	4700	1200	900	200	1800	1200
1000s	3	1	0	0	1	0
500s	2	0	1	0	1	2
200s	3	0	1	0	0	0
100s	1	2	2	2	3	2

Continued next page...



3 4



Halaman 9b Ilustrasi 8

The truth about how the APBD was spent (cont'd)

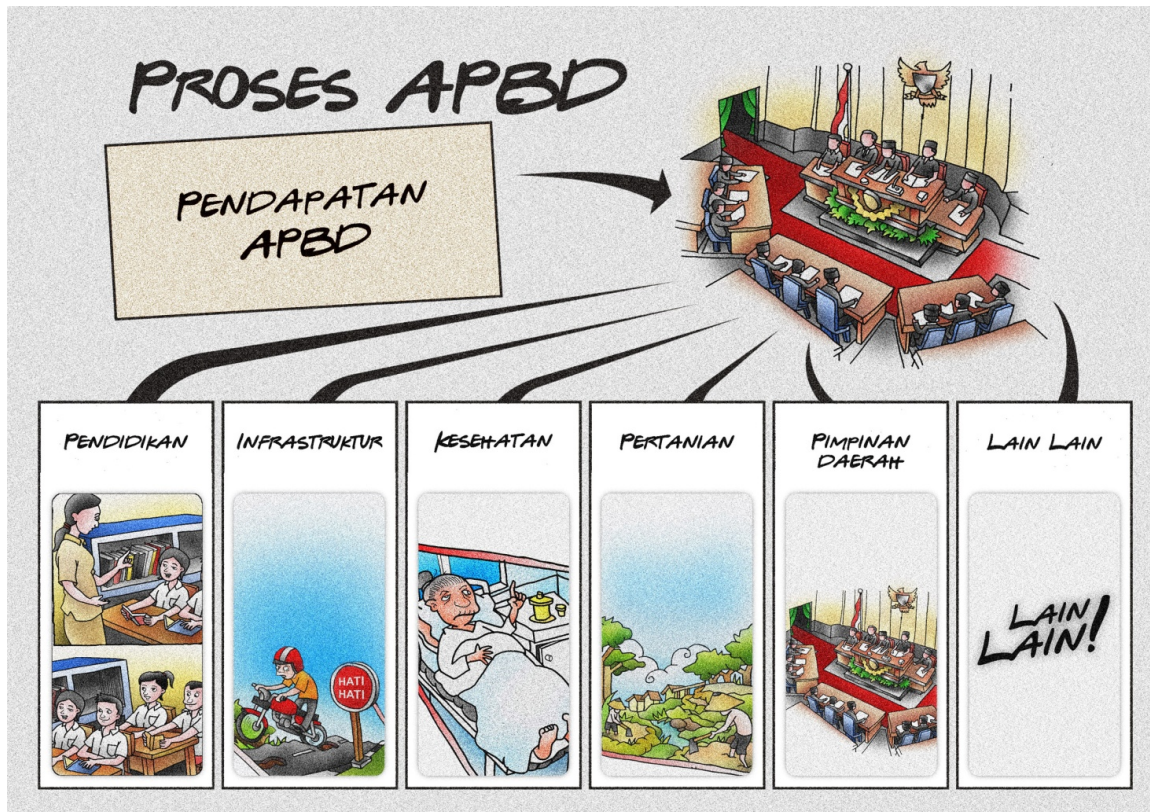
Direct/Indirect

- But just because the bupati and DPRD members have allocated the money to categories like education and health, **it does not mean that the money goes directly to each person**. Some of that money goes to the costs of running the government, including things like salaries for PNS, building maintenance and administration.
- This is how much was spent to run the government compared to how much was spent for you.
 - Of the 4700 they spent on **education**, the district government spent **3800** on itself and **900** on you. To represent this, I will now remove 3800 rupiah from the education box on the board.
 - Of the 1200 spent on **infrastructure**, it spent **100** on itself and **1100** on you.
 - Of the 900 spent on **health**, it spent **600** on itself and **300** on you.
 - Of the 200 spent on **farming**, it spent **100** on itself and **100** on you.
 - Of the 1800 spent on allocated to support the **bupati and DPRD**, **1600** went to the costs of running the government
 - Of the **1200** spent on all **other** expenditures, it spent about **600** on itself and **600** on you.

	Education	Infrastructure	Health	Farmers	Leaders	Other
Start	4700	1200	900	200	1800	1200
Take out	3800	100	600	100	1600	600
Left	900	1100	300	100	200	600

- Of the 10,000 originally allocated for each citizen, **6,800 goes back to the government and 3,000 goes to each citizen for public services and assistance**. Some of the money government spends on itself is necessary, but it is also possible that some of this money is not optimal.
- **It is up to you** to decide whether you think this is a good division of the money that government spends for each citizen.

Cont'd on the next page...



3 4



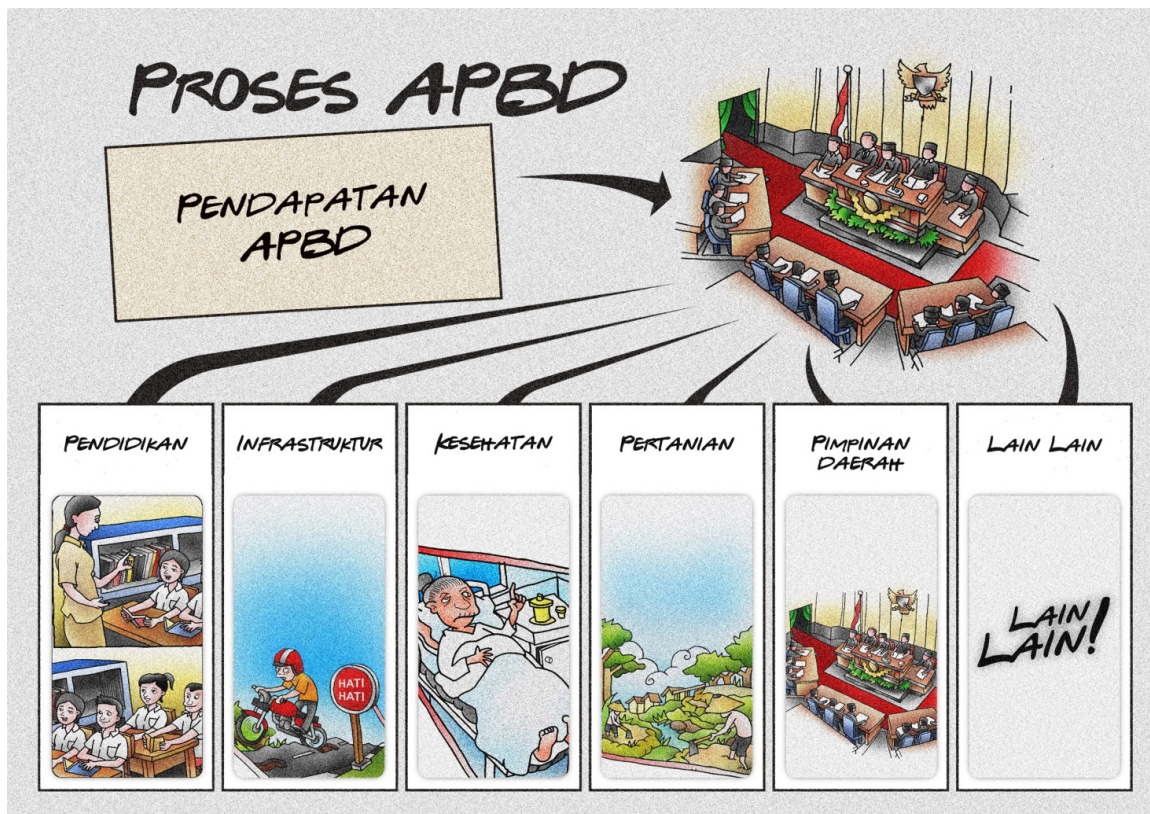
Halaman 9c Ilustrasi 8

The truth about how the APBD was spent (cont'd)

Corruption/Misuse

- **But not even all of that 3000 reaches you.** Sometimes money gets misused or goes missing along the way.
- **I will now take out 100 from education** to symbolize a recent case in which **funds to build and furnish 250 elementary schools went missing.**
- I will also take **100 from infrastructure** to symbolize the fact that last year government did not fulfill its responsibility to monitor infrastructure projects, which **makes it easier for builders to waste and misuse money.**
- We have now **deducted 200 rupiah from the initial 10,000 to represent misuse and corruption.** The real amount lost to corruption might be bigger or smaller, but it's hard to know because when there's no transparency, it's easy for money to exchange hands in the dark.

Continued on the next page...



Halaman	Ilustrasi
9d	8

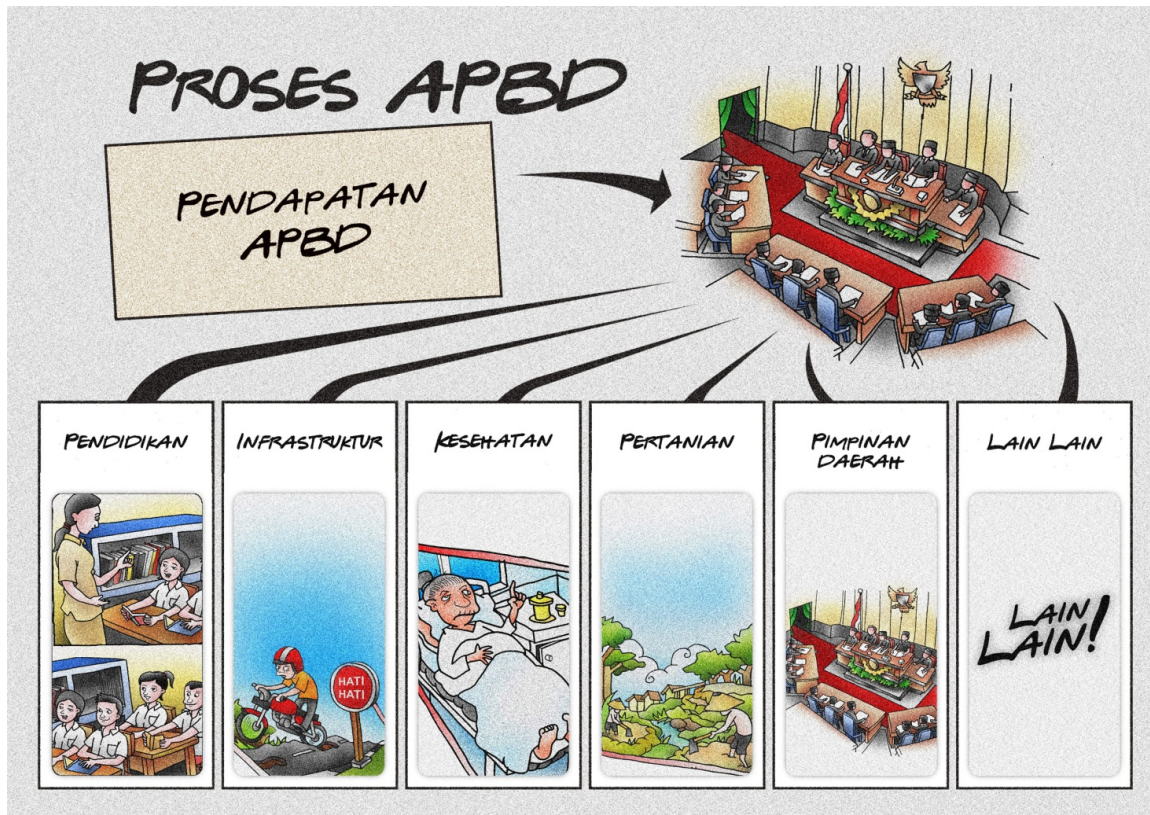
The truth about how the APBD was spent (cont'd)

Spending by Leaders, on Leaders

- And what about the **200 that remains in the box for district leaders**? This is the amount that the bupati and DPRD members have allocated for themselves to do their jobs.
 - ⊗ **100 of that they have allocated for activities directly related to their work.** They budget money for **study missions** and **travel**, for meeting with **constituents**, and for discussing **laws**. **It is up to you** to decide whether you think they used this money effectively to communicate with people around Blora and to pass laws that benefit the district, and you.
 - ⊗ **The remaining 100 the bupati and DPRD members have allocated for their personal use.** They use this money to fund their salaries, health care, uniforms, cars, and homes for the bupati and DPRD leaders. **Each person in Blora contributes this 100 to the bupati and 45 DPRD members, so the real amounts can be big.**
- For instance:
 - ⊗ **375 juta** was spent for the **bupati's salary and healthcare**;
 - ⊗ **1.4 miliar** was spent to **renovate and maintain the bupati's official home** in 2008.
 - ⊗ **147 juta** was spent in **salaries and healthcare for each member of the DPRD** (on average);
 - ⊗ **756 juta** was spent on **homes for the DPRD Chair and Vice Chair**.
- In the end, **it is up to you** to decide whether you think the bupati and DPRD members have earned their income from the APBD by doing a good job and addressing the needs of the people of Blora.

[GO TO SURVEY SECTION 5]

(Campaigner Note: All information from Blora's 2008 realized budget)



1 2



Halaman 10 Ilustrasi 8

Empty Information (Placebo)

Here is some basic information about Blora that district government leaders might consider when deciding how to allocate the APBD:

- Blora is surrounded by the districts of Rembang, Pati, Grobogan, Ngawi, and Bojonegoro.
- The longest distance from west to east is 87 kilometers, and the longest distance from North to South is 58 kilometers.
- There are 16 subdistricts and 295 villages in Blora.
- Of those, 24 are kelurahan and 271 are desas.
- There were 90 days of rain in 2008, with a total of 1336 millimeters of rain. The month with the greatest amount of rainfall is typically February.
- In 2008, 83 percent of the garbage in Blora was organic, and 9 percent was plastic.
- The growth rate of the Blora regional gross domestic product in 2008 was 5.8 percent.
- There is a golf course in Cepu.

(Campaigner note: All information obtained from Blora dalam Angka 2008-2009)

[GO TO SURVEY SECTION 5]



Halaman	Ilustrasi
11	9

Message: Your Voice, Your Opportunity

[PUT THE MONEY AWAY BEFORE CONTINUING!]

- Now is an **important time in Blora**. The **pilkada** are scheduled for 3 June 2010. Both the bupati and DPRD will be starting **new terms in office**. But your job as citizens does not stop with elections. Citizens should tell their elected leaders what they want and **ordinary citizens should put pressure on their leaders** to bring about change after the elections are over.
- It is vital to do this now because Blora will soon start **getting revenue from oil from the Cepu block**. If citizens do not do more to tell the district government what they want, then Blora's natural resource wealth could be **squandered**.
- This is why LPAW is organizing a **postcard campaign** before the bupati elections. The goal of the campaign is to give citizens like you a chance to express how you feel about the quality of government in Blora.

SUARA ANDA, KESEMPATAN ANDA

Saya ingin Pemkab Blora melakukan pekerjaannya dengan lebih baik

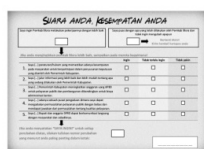
Saya puas dengan apa yang telah dilakukan oleh Pemkab Blora dan tidak ingin mengubah apapun

Berhenti disini!
Kirim kembali kartupos anda

Jika anda menginginkan pemkab Blora lebih baik, sampaikan pada mereka bagaimana!

	Ingin	Tidak terlalu ingin	Tidak yakin
1. Saya [...] peraturan/hukum yang memastikan adanya kesempatan pada masyarakat untuk berpartisipasi dalam penyusunan keputusan yang diambil oleh Pemerintah Kabupaten.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
2. Saya [...] jalur informasi yang lebih baik dan lebih mudah tentang apa yang sedang dilakukan oleh Pemerintah Kabupaten.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
3. Saya [...] Pemerintah Kabupaten meningkatkan anggaran uang APBD untuk pelayanan publik dan pembangunan dibandingkan untuk biaya administrasi kantor.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
4. Saya [...] adanya sebuah pusat pengaduan dimana saya dapat mengadukan permasalahan pelayanan publik dengan bebas dan mendapat jawaban dari permasalahan tentang kualitas pelayanan.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>
5. Saya [...] Bupati dan anggota DPRD dapat berkomunikasi langsung dengan masyarakat dan sebaliknya.	<input type="checkbox"/>	<input type="checkbox"/>	<input type="checkbox"/>

Jika anda menyatakan "SAYA INGIN!" untuk setiap perubahan diatas, silakan tuliskan nomer perubahan yang menurut anda paling penting dalam kotak:



Halaman	Ilustrasi
12	10

Postcard

- Let me describe to you what is on the postcard. If you think might have trouble reading or understanding the postcard, you can **ask a member of your household to help you.**
- First, you can indicate whether you want the district government in Blora to **do a better job**, or whether **you are satisfied** with the district government in Blora and don't want to change anything about how it works. If you pick the second one, then you are ready to return your postcard!
- **If you picked the first one**, then we would like you to tell us how. There is a list of five reforms on the postcard. For each reform, put a check for whether you want/don't want that reform. We are asking how you feel about five reforms:
 - I [want/don't really want] a law that guarantees **opportunities for public participation** in district government decisions.
 - I [want/don't really want] **better and easier access to information** on district government programs and policies.
 - I [want/don't really want] the district government to **spend more of the APBD money on public services** and development and less money on administration.
 - I [want/don't really want] a **central location established where I can freely report problems with public services** and get answers to my questions about service quality.
 - I [want/don't really want] the bupati and DPRD members to have **more direct communication with the public.**
- If there is any reform that you don't know about, then that's ok too. Just tic the box in the column on the far right for 'not sure'
- Then at the very bottom under the 'Want' column, it asks you "if you have said 'I want!' to any of the reforms above, **please write the number of the reform you most want in the box.**" Here you should pick the **one reform** you think is most important from all those you said that you want.
- If you don't want or don't know about any of the reforms, then just **skip** this box.



Halaman	Ilustrasi
13	11

Postcard

- LPAW and the researcher will **collect all the postcards** and tabulate the responses. Then two weeks before the election, **LPAW will hold a public meeting with the bupati candidates** to tell them the results. The results will also be **shared with members of the DPRD, heads of Dinas, the media and other citizens** so that your voice will be heard in many corners of Blora.
- **YOUR POST-CARD IS YOUR VOICE, IT IS A WAY FOR YOU TO COMMUNICATE WITH YOUR POLITICAL LEADERS AND TELL THEM HOW YOU FEEL ABOUT GOVERNMENT IN BLORA.**
- Whether you decide to return the postcard is **completely up to you**. The most important and helpful thing to the campaign is for you to make your decision based on your **personal feelings** and your **reaction to the campaign**. Also, **the post-card is like a secret ballot** – whether you return the postcard and what you vote for on the postcard is your secret.
- If you decide to return the postcard, please deposit it in a special mailbox that has been placed at **[LOCATION] BEFORE MAGRHEB TOMORROW, in other words before tomorrow at 18:00.**
- The postcard should reflect how each individual feels. We would like to **ask your help to not discuss the campaign with your neighbors until after we have picked up the mailbox in your dusun.**
- If your neighbor is interested in the campaign and asks you about it, you can suggest **they go to the dusun head since we left a few extra postcards there.**
- Do you have any questions? If there are any questions or concerns about the campaign, **here's a card with our contact information.**
- **Thank you very much for your time today. Sampai Jumpa!**

Part III

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