

Essays on Institutions, Aid, and Conflict

By

Matthew Daniel DiLorenzo

Dissertation

Submitted to the Faculty of the  
Graduate School of Vanderbilt University  
in partial fulfillment of the requirements

for the degree of

DOCTOR OF PHILOSOPHY

in

Political Science

August, 2016

Nashville, Tennessee

Approved:

Brett Benson, Ph.D.

Joshua Clinton, Ph.D.

Daniel Nielson, Ph.D.

James Lee Ray, Ph.D.

Hye Young You, Ph.D.

For Pepe

## ACKNOWLEDGMENTS

First, I thank my committee: Brett Benson, Jim Ray, Josh Clinton, Hye Young You, and Dan Nielson. My time at Vanderbilt coincided with a period of transition in our department's International Relations subfield and Brett served as an anchor throughout everything. He encouraged me to pursue ideas that interested me even when they were outside his area of expertise and always pushed me to subject my arguments and intuitions to rigorous scrutiny. I am very grateful for his guidance in helping me become an independent scholar. Whether he is asking a question in a talk or commenting on a piece of written work, no point is too major or too minor for Jim to raise. In writing these essays, imagining Jim's likely reaction helped me avoid sloppiness in my language and logic. I expect this thought experiment to serve me well throughout my career. Of course, on occasions where that thought experiment failed, Jim was happy to identify errors and points of confusion himself. I am very thankful for Jim's guidance and the time he spent reading and commenting on my work, especially since he expended most of that effort after he was technically retired. I was a frequent visitor to Josh's office over the past five years. I am grateful for his patience for methodological questions and his enthusiasm for research, both of which have made me a better political scientist. Hye Young provided extensive comments on multiple drafts of each of the substantive chapters. Her suggestions were extremely helpful, both in terms of research design and framing. Dan generously agreed to serve as my outside member. I expected his expertise to be particularly helpful on the two aid chapters, but, in fact, his advice greatly improved all three chapters. All taught me a great deal about research in political science and were always happy to advise me on matters both professional and personal. I am very grateful for their feedback, guidance, and support.

Chapter 3 started as a term paper in Michaela Mattes' International Cooperation

seminar. I am thankful for her thoughtful and detailed feedback on that chapter and on my other work throughout my first two years at Vanderbilt. I developed Chapter 4 and some of Chapter 2 in Cindy Kam's Prospectus Writing seminar. Her comments were indispensable in the development of my projects. Giacomo Chiozza also provided helpful feedback on Chapter 4. Brenton Kenkel provided extremely useful and thorough feedback on Chapters 2 and 3. Tobias Heinrich, Matthew Wilson, and Tyson Chatagnier provided excellent feedback on Chapter 2. I am also thankful for the guidance of faculty members at Vanderbilt who otherwise had little involvement in my projects, including Dave Lewis, Alan Wiseman, Carol Atkinson, Jon Hiskey, Brooke Ackerly, Emily Nacol, Zeynep Somer-Topcu, and Suzanne Globetti.

The other graduate students in my cohort – Bryan Rooney, Mark Richardson, Michal Eskenazi, Carrie Roush, Allison Archer, Gui Russo, and Whitney Lopez – provided extensive feedback on my work in the course of our year-long prospectus writing sequence. (Double secret thanks to Bryan Rooney for serving as the officiant at my wedding.) Thanks are due also to Eelco van der Maat, Anna Carella, Becca McBride, Steve Utych, Bryce Williams-Tuggle, Claire Evans, Julia Harris, Meri Long, Drew Engelhardt, Marc Trussler, Laura Sellers, Scott Limbocker, Oscar Castorena, and Maggie Deichert for general camaraderie. My professors and advisers from Salem State University and the University of New Hampshire – Stacy VanDeveer, Alynna Lyons, Mary Malone, Dante Scala, Tama Andrews, Kanishkan Sathasivam, and Richard Levy – set an excellent example of what it means to be a teacher and scholar and inspired me to become a political scientist. Tina Bembry, Darlene Davidson, and Natasha Duncan were always generous with their time in helping me navigate administrative matters in our department, and for that I thank them.

I also want to thank my friends and family who have supported me throughout graduate school. Dan Natoli went so far as to move to Nashville with me in 2011. Others who deserve thanks for “general support” include my parents Denise and

Dan, Meme, Emma, Giuls, Brett, Sawyer, Ernie, Tricia, Elizabeth, Chris, Tyler, Dave, Bobbee, Brad, Sam, Dan, Ruby, and Olive.

My wife, Patty, has made many personal and professional sacrifices in supporting my career. She has been there for me through all the ups and downs of graduate school, and I do not know that I could have done it without her. Given her steadfast love and support over the last seven and a half years, I want to take this opportunity to acknowledge that she is my third-best friend. Of the theoretical and empirical advancements made in this dissertation, my son, Graham, opposed few of them and did little to sabotage them.<sup>1</sup> But, seriously, Patty and Graham have brought a nearly-overwhelming amount of joy to my life, and I am incredibly lucky and proud to have them as my family.

Finally, I want to thank my grandfather, Pepe. Leaving my family and friends in New England to attend Vanderbilt was difficult, primarily because it entailed being mostly absent for the final two years of Pepe's life. I owe much of my sense of humor and work ethic to his influence, and he was incredibly supportive and proud of my decision to attend Vanderbilt. This dissertation is dedicated to him.

Any remaining mistakes are, of course, the fault of Eelco van der Maat.

---

<sup>1</sup>The previous two sentences contain excerpts of the essays "My Third-Best Friend" and "How I Want to Be Remembered" from Handey (2008).

## TABLE OF CONTENTS

	Page
DEDICATION . . . . .	ii
ACKNOWLEDGMENTS . . . . .	iii
LIST OF TABLES . . . . .	ix
LIST OF FIGURES . . . . .	xi
1 Introduction . . . . .	1
1.1 Summary of Chapters . . . . .	2
2 Bypass Aid and Unrest in Autocracies . . . . .	4
2.1 Introduction . . . . .	4
2.2 Aid and Democratization . . . . .	6
2.3 Bypass Aid and Autocratic Survival . . . . .	9
2.3.1 The Model . . . . .	10
2.4 Research Design . . . . .	13
2.5 Results . . . . .	16
2.5.1 Exogenous Variation in Bypass Aid . . . . .	22
2.6 Conclusion . . . . .	32
2.7 Appendix for “Bypass Aid and Unrest in Autocracies” . . . . .	35
2.7.1 Extensions to Model . . . . .	35
2.7.1.1 Probability of Revolution . . . . .	35
2.7.1.2 What If Bypass Aid Makes Revolution Easier? . . . . .	35
2.7.2 Countries Included in Analysis . . . . .	37
2.7.3 Correlations Between Independent Variables . . . . .	39
2.7.4 Measure of Unrest Excluding Riots . . . . .	40
2.7.5 Logit Models for Dichotomous Unrest Variable . . . . .	42

2.7.6	Zero-inflated Negative Binomial Regression . . . . .	43
2.7.7	Aid Scandal Cases . . . . .	44
2.7.8	Evaluating Exclusion Restriction . . . . .	45
2.7.9	Bypass Aid and Investment in Repressive Capacity . . . . .	50
2.7.10	Validity Check for <i>AidData</i> Measure . . . . .	52
2.7.11	Alternative Operationalizations of Bypass Aid with OECD Data . . . . .	53
2.7.12	Controlling for Continent, Region, and Recipient Country . . . . .	54
3	The Design of International Institutions for Humanitarian Aid . . . . .	56
3.1	The Crisis Relief Regime . . . . .	59
3.1.1	Is the Regime Working? . . . . .	61
3.2	Strategic Interaction in the Provision of Humanitarian Aid . . . . .	64
3.2.1	Model I: Bilateral Humanitarian Relief . . . . .	65
3.2.2	Model II: Adding an Expert Organization . . . . .	66
3.2.2.1	Equilibrium for Model II . . . . .	68
3.2.3	Model III: Allowing Limited Earmarking . . . . .	69
3.2.3.1	Equilibrium Analysis for Model III . . . . .	70
3.3	Comparisons Across Models: A Simulation . . . . .	72
3.3.1	Can Earmarking Lead to Better Outcomes? . . . . .	72
3.3.2	Simulation Results . . . . .	75
3.4	Conclusion . . . . .	76
3.5	Appendix for “The Design of International Institutions for Humanitarian Aid” . . . . .	78
3.5.1	An Empirical Test of CERF’s Impartiality . . . . .	78
3.5.2	Equilibrium Analysis for Model II . . . . .	81
3.5.3	Equilibrium Analysis for Model III . . . . .	85
3.5.4	Comparison of Models II and III with Model I . . . . .	88

4 Leader Survival, Natural Disasters, and International Conflict . . . . .	89
4.1 Disasters and Conflict . . . . .	91
4.2 Theory . . . . .	93
4.3 Research Design . . . . .	100
4.4 Results . . . . .	104
4.5 Discussion . . . . .	113
4.6 Conclusion . . . . .	114
4.7 Appendix for “Natural Disasters, Leader Survival and International Con- flict” . . . . .	116
4.7.1 Notes on Disaster Data . . . . .	116
4.7.2 Summary Statistics . . . . .	118
4.7.3 Polity <i>xrreg</i> variable . . . . .	119
4.7.4 Including Total Aid and Civil Conflict . . . . .	120
4.7.5 Year-Fixed Effects . . . . .	122
4.7.6 Country-Fixed Effects . . . . .	123
4.7.7 Zero-inflated Negative Binomial Regression Models . . . . .	124
4.7.8 Controlling for Disaster Propensity . . . . .	126
4.7.9 Correlations Between Key Variables . . . . .	127
4.7.10 Empirical Distribution of Regime and Removal Type . . . . .	128
4.7.11 Alternative Dependent Variable: ICB Crisis Initiations . . . . .	129
4.7.12 Cross Tabulation of Disaster Deaths and MID Initiations . . . . .	131
BIBLIOGRAPHY . . . . .	134



## LIST OF TABLES

Table	Page
2.1 Summary Statistics for Key Variables . . . . .	16
2.2 OECD Aid Channels and Domestic Unrest, 2005-2010 . . . . .	19
2.3 <i>AidData</i> Measure of Bypass Aid . . . . .	23
2.4 Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010 . . .	30
2.5 Stage II - Bypass Aid and Unrest, 2005-2010 . . . . .	31
2.6 Bootstrapped Second Stage Coefficients . . . . .	32
2.7 Temporal and Spatial Domain . . . . .	38
2.8 Correlations Between Independent Variables . . . . .	39
2.9 OECD Aid Channels and Domestic Unrest (Excluding Riots), 2005-2010	41
2.10 OECD Aid Channels and Domestic Unrest (Dichotomous Dependent Variable), 2005-2010 . . . . .	42
2.11 Zero-inflated Negative Binomial Regression Models . . . . .	43
2.12 Cases from Aid Scandal Data, 2003-2008 . . . . .	45
2.13 Evaluating Exclusion Restriction . . . . .	47
2.14 Bypass Aid and Change in Military Expenditures (% Gov. Spending)	51
2.15 Alternate Measures of Bypass Aid and Unrest, 2005-2010 . . . . .	53
2.16 Continent-, Region-, and Country-fixed Effects Models, 2005-2010 . .	55
3.1 Comparison of Top Donors to CERF versus Overall Aid Allocation .	64
3.2 Recipient Payoffs Across Models I, II, and III . . . . .	73
3.3 UNSC Membership and Likelihood of Receiving CERF Aid, 2006-2013 (Logit Models) . . . . .	79
3.4 Parameter Combination Likelihood . . . . .	86

4.1	Disasters, Coalition Size, Removal Type, and International Conflict (Negative Binomial Regression) . . . . .	106
4.2	Earthquake Events, Coalition Size, Removal Type, and International Conflict (Negative Binomial Regression) . . . . .	108
4.3	Disasters, Removal Type, and Conflict in Small-Coalition Regimes (Negative Binomial Regression) . . . . .	112
4.4	Summary Statistics . . . . .	118
4.5	Controlling for Aid and Civil Conflict . . . . .	121
4.6	Base Models with Year-Fixed Effects (Negative Binomial Regression)	122
4.7	Base Models with Country-Fixed Effects (Negative Binomial Regression)	123
4.8	Zero-inflated Negative Binomial Regression Models . . . . .	125
4.9	Controlling for Disaster Propensity (Negative Binomial Regression) .	126
4.10	Correlations Between Independent Variables . . . . .	127
4.11	Empirical Distribution of Regime and Removal Type, $W \geq 0.5$ Threshold	128
4.12	Empirical Distribution of Regime and Removal Type, $W \geq 0.75$ Threshold	128
4.13	Alternative Dependent Variable: ICB Crisis Initiations (Negative Bi- nomial Regression) . . . . .	130
4.14	Distribution of MID Cases for Large-Coalition Leaders . . . . .	131
4.15	Cases of MID Initiations for Large-Coalition, Irregular Removal Lead- ers with Greater than Mean Disaster Deaths in Previous Period . . .	132
4.16	Cases of MID Initiations for Large-Coalition, Regular Removal Leaders with Greater than Mean Disaster Deaths in Previous Period . . . . .	133

## LIST OF FIGURES

Figure	Page
2.1 Scatterplot of Bypass Aid and Domestic Unrest, 2005-2010 . . . . .	17
2.2 Expected Count of Unrest Events Across Possible Bypass Aid Shares .	20
2.3 Comparing Aid Measures . . . . .	21
2.4 Union of Confidence Intervals Approach to Relaxing Exclusion Restriction.	48
3.1 CERF Aid as Proportion of Total Humanitarian Aid . . . . .	63
3.2 Sequence of Game in Model II . . . . .	68
3.3 Sequence of Game in Model III . . . . .	71
3.4 Comparing Model III to Model II . . . . .	74
3.5 Comparing Model II to Model III . . . . .	75
4.1 Expected Count of MIDs Analysis. . . . .	109
4.2 Expected Count of Hostile MIDs Analysis. . . . .	110

## Chapter 1

### Introduction

I have collected the following three essays under the title of *Essays on Institutions, Aid, and Conflict*. Each is a self-contained essay, but there are two themes motivating and tying them together. First, there is a substantive focus on understanding how climate-induced crises and emergencies interact with political institutions to affect violence, political outcomes, and human welfare. One strand of research asks how disasters and environmental change might influence political upheaval and violence within and between states (e.g., Stalley 2003; Nelson 2010; Drury and Olson 1998; Nel and Righarts 2008; Omelicheva 2011; Bergholt and Lujala 2012; Slettebak 2012; Nardulli, Peyton and Bajjalieh 2013). Another considers how states might cooperate to effectively mitigate and prevent climate-related crises (e.g., Haas, Keohane and Levy 1993; Biermann and Boas 2010; Keohane and Victor 2011; Tir and Stinnett 2012). Here I address both traditions, but also go further to consider the political implications of states' aid strategies for domestic politics in recipient countries.

Second, there is a theoretical emphasis on using existing social science theories to understand current, real-world problems. I apply leading theories – economic theories of democratization, principal-agent theory, and theories of political survival and the risk of consequences of losing office – to questions of the aid-democratization relationship, the design of institutions for emergency relief, and climate-conflict connections. In doing so, I highlight a number of shortcomings of those theories and offer refinements and extensions. The rest of this brief introduction outlines the main arguments, methods, and findings of the three essays.

## 1.1 Summary of Chapters

### Chapter 2: “Bypass Aid and Unrest in Autocracies”

Scholars and policymakers have argued that bypassing recipient governments by channeling aid through intergovernmental and nongovernmental organizations can help donors pursue development goals without bolstering autocratic regimes. In Chapter 2, I argue that bypass aid can subsidize government transfers to citizens and thus help autocrats deter challenges to their rule. I show that autocracies experience less domestic unrest as the proportion of aid delivered through bypass channels increases. To address potential endogeneity, I conduct an instrumental variables analysis. I argue that scandals where governments are suspected of or caught misusing aid should exogenously affect the distribution of aid across channels by causing donors to deliver more aid through bypass channels overall. I use an original data set of aid scandals to instrument for bypass aid in two-stage models to confirm the baseline results.

### Chapter 3: “The Design of International Institutions for Humanitarian Aid”

Strategic and political interests tend to dictate states’ allocations of bilateral emergency aid. In 2006, the United Nations created the Central Emergency Response Fund (CERF) to counteract this bias and help provide emergency humanitarian relief efficiently and impartially. A key feature of the organization is the banning of earmarking. What are the implications of this institutional design choice for the supply of humanitarian relief and welfare in recipient countries? In Chapter 3, I formalize a donor’s decision to provide aid bilaterally *ex post* versus (or in combination with) delegating *ex ante* to a more efficient institution that may allocate aid in ways contrary to donor interests. I consider how allowing for limited earmarking affects the supply of international humanitarian aid. Using the models and a simulation, I

show that a policy of allowing partial earmarking increases contributions and may improve average outcomes for crisis victims.

#### Chapter 4: “Leader Survival, Natural Disasters, and International Conflict”

Existing work finds that natural disasters affect leader survival, and that the risk and consequences of losing office influence leaders’ international conflict behavior. In Chapter 4, I consider how disasters may indirectly affect the risk of international conflict by affecting leader survival. I argue that disasters should only matter for the conflict behavior of large-coalition leaders. As disaster deaths increase, large-coalition leaders fearing irregular removal from office should be more prone to conflict, while those anticipating regular removal should engage in conflict less frequently. I find evidence that deaths from disasters are associated with modest increases in conflict initiation among large-coalition leaders facing irregular removal throughout the period of 1950 to 2007. Against the expectations of the theory, however, large-coalition leaders facing *regular* removal also appear to become more likely to initiate conflicts in the wake of disasters. Consistent with the theory, disaster deaths are unrelated to conflict behavior for small-coalition leaders. However, small-coalition leaders facing irregular removal appear to be less likely to initiate conflicts as more disaster events occur, which is the opposite of what existing theory would expect. In arguing that different types of threats to leader survival survival matter differently for international conflict, Chapter 4 offers an important qualification to theories of leader survival and international conflict.

## Chapter 2

### Bypass Aid and Unrest in Autocracies

#### 2.1 Introduction

Does foreign aid undermine demand for democratic accountability in autocracies? Existing work on this question focuses on government-to-government aid, yet donors frequently bypass recipient governments by giving aid through intergovernmental and nongovernmental organizations (IGOs and NGOs). For example, more than 60% of the \$700 million Haiti received in development assistance in 2008 went to NGOs, multilateral organizations, or public-private partnerships (Dietrich 2013, 698). That figure was nearly 80% for \$2.5 billion in international aid to Sudan in 2009 (OECD 2014). By preventing recipient governments from misusing aid, bypassing is thought to be one way that donors can better achieve development goals (Dietrich 2013, 699; Ear 2013; Acht, Mahmoud and Thiele 2015). However, political scientists have given relatively little attention to the domestic *political* implications of bypass aid for recipient countries. Many who argue that aid helps stabilize autocratic regimes limit the scope of their arguments to government-to-government aid (e.g., Bueno de Mesquita and Smith 2009, 315; Kono, Montinola and Verbon 2013; Ahmed and Werker 2015), yet this is problematic given that donors tend to bypass poorly-governed, autocratic regimes (Dietrich 2013, 705).

In this paper I consider how bypass aid affects autocratic survival. Optimists point out that aid is often less fungible than critics assume, partly because donors can bypass corrupt recipients (e.g., Altincekic and Bearce 2014, 21; Bermeo 2016, 7). But it is not clear that bypass aid should be politically neutral given that other types of non-government foreign income – e.g., remittances – can help autocrats (Ahmed 2012, 148). Using a simple model of revolution I show how bypass aid can reduce

unrest in autocracies. The intuition is straightforward: insofar as bypass aid improves the wellbeing of a recipient population, it lowers the concessions that an incumbent regime would have to make to retain power. Thus, while bypass aid can further humanitarian goals, it simultaneously acts as a political subsidy to autocrats. This is consistent with research that shows that aid of any kind – government or bypass – undermines pressures for democracy (e.g., de Waal 1997; Terry 2002; Polman 2010).

Using data from the Organization for Economic Cooperation and Development (OECD 2014), I show that bypass aid is associated with less frequent domestic unrest in autocracies. This finding is robust to a number of different model specifications that account for confounding factors and various operationalizations of bypass aid, including an original alternative measure of bypass aid I construct from the *AidData* organization’s project-level data set (Tierney et al. 2011). However, there is potentially an endogeneity problem: governments that have a greater latent repressive capacity and can deter unrest should also be more likely to receive aid through bypass channels. To address this difficulty, I collected an original data set of international aid scandals – instances where media sources reveal a recipient government’s misuse of aid – to identify exogenous variation in bypass aid. When donors observe aid scandals, they should be more inclined to deliver aid through bypass channels. Thus, international aid scandals should act as an exogenous shock to overall aid channel distribution. The instrumental variables analysis provides more evidence bypass aid can reduce unrest in autocratic countries.

This paper makes a number of contributions. First, the paper highlights the potential consequences of an aid strategy that many see as politically innocuous. Indeed, others have pointed out that donors appear to resolve the dilemma of not wanting to aid poorly-governed recipient countries where aid is most needed by bypassing recipient governments (e.g., Acht, Mahmoud and Thiele 2015, 20). In fact, Bermeo (2016) builds into her formal model the assumption that fungible aid reduces the likelihood



of democratization while non-fungible aid – which includes “bypass” aid – does not. While bypassing could plausibly advance development goals, this paper shows that it can also undermine pressure for political reform. Second, it answers a neglected question in the literature on aid and political survival: why do autocratic governments allow bypass aid? If anything, existing work seems to suggest that autocrats should be wary of bypass aid for its potential to sustain rebel movements (Nunn and Qian 2014, 1632-1633; Polman 2010, 111). The answer I provide is that bypass aid, like government aid, can reduce domestic unrest and enhance an autocrat’s ability to retain power. Third, the paper introduces a new data set of international aid scandals that is well suited for making causal inferences about the effect of aid channel distribution on political outcomes in recipient countries. Fourth, I develop a new measure of bypass aid using data from the *AidData* project (Tierney et al. 2011). This measure provides a reasonable alternative (or complement) to OECD data when scholars are concerned about underreporting during years where donors were plausibly adapting to new reporting requirements. Finally, by studying the political effects of bypass aid, the paper contributes to our understanding of economic development given that political institutions are important determinants of economic (under)development (e.g., Acemoglu and Robinson 2001, 2006; Bueno de Mesquita et al. 2003).

## 2.2 Aid and Democratization

A key question in the foreign aid literature is whether or not aid is bad for democracy. Many argue that aid makes democratization less likely (e.g., van de Walle 2001; Bräutigam and Knack 2004; Djankov, Montalvo and Reynal-Querol 2008; Kono and Montinola 2009; Ahmed 2012; Ear 2013; Kono, Montinola and Verbon 2013). Following arguments that link the historical development of democracy to elites’ needs to tax citizens (e.g., Tilly 1990), aid skeptics contend that, “non-tax revenue enables leaders to forgo taxing the citizenry, resulting in a decreased demand for represen-

tative democracy and good governance” (Dietrich and Wright 2013, 60; Djankov, Montalvo and Reynal-Querol 2008; Morrison 2009). Thus, when aid substitutes for tax revenue, governmental demands on citizens are less onerous, depressing popular demand for democratic accountability. Others argue that aid allows autocrats to increase repressive capacity and deter revolution (Kono, Montinola and Verbon 2013). Yet many are less pessimistic (e.g., Alesina and Dollar 2000; Wright 2009; Kersting and Kilby 2014). Alesina and Dollar (2000) argue that donors can incentivize democratization with the promise of future aid, though Wright (2009) suggests this is only true for autocratic leaders who are likely to retain office following a democratic transition. There are also studies that find no effect of aid on democratization (e.g., Knack 2004; Altincekic and Bearce 2014). A primary source of disagreement concerns the different empirical strategies used to test these theories (Wright 2009).

While recent literature on the determinants of states’ aid allocation decisions considers different types and channels of aid (e.g., Milner 2006; Dietrich 2013; Acht, Mahmoud and Thiele 2015), few have explored the political consequences of bypassing recipient governments.<sup>1</sup> Indeed, aid increasingly flows through NGOs and UN agencies that operate relatively autonomously within recipient countries (e.g., Riddell 2007; Bütthe, Major and Souza 2012; Dietrich 2013). Scholars usually ignore this type of aid when studying the relationship between aid and democratization, focusing instead on government-to-government aid. This matters for two reasons. First, corruption in the recipient country is a strong predictor of bypass (Dietrich 2013; Acht, Mahmoud and Thiele 2015; Ear 2013, 10). Second, the theoretical mechanisms that link foreign aid to autocratic stability depend on government control of aid (Bueno

---

<sup>1</sup>A vast literature on the determinants of aid giving considers many different conceptual dimensions of aid, including the choice of bilateral versus multilateral channels and the purpose for which aid is given (Alesina and Dollar 2000; Neumayer 2003; Drury, Olson and Van Belle 2005; Dollar and Levin 2006; Milner 2006; Bueno de Mesquita and Smith 2009; Dreher, Sturm and Vreeland 2009; Fink and Redaelli 2011; Milner and Tingley 2012; Raschky and Schwindt 2012; Dreher, Fuchs and Nunnenkamp 2013; Heinrich 2013; Fuchs, Dreher and Nunnenkamp 2014; Kevlihan, DeRouen and Biglaiser 2014).

de Mesquita and Smith 2007, 2009).<sup>2</sup> As Altincekic and Bearce (2014) point out, aid is often less fungible than these theories suggest, and it is not obvious that existing theories apply to non-fungible aid.

Though Altincekic and Bearce (2014) are right to encourage engaging theories of democratization,<sup>3</sup> they assume that non-fungible aid presents no strategic advantage to a recipient government. Bermeo (2016) takes this assumption as given in arguing that while non-fungible aid may provide utility to a recipient government, it does not harm the prospects for democratization. Whether this conclusion follows from the theoretical framework that Altincekic and Bearce (2014) adopt – Acemoglu and Robinson’s (2001) model of democratization – is unclear. For instance, Ahmed (2012) argues that remittances, a source of income that governments have little control over, can help autocrats. However, as Altincekic and Bearce (2014) point out, Ahmed’s (2012) empirical tests combine government-to-government aid with remittances into one measure of “unearned foreign income.” Theoretically, Altincekic and Bearce (2014) focus only on how aid influences the incentives and behavior of the recipient government, yet the incentives of opposition actors are equally important determinants of equilibrium outcomes in Acemoglu and Robinson’s (2001) model. In other contexts, work that emphasizes the importance of opportunity costs in the decision to revolt also suggests that focusing on state capacity provides only a partial view of the relevant incentives (e.g., Collier and Hoeffler 2004).

In sum, we know little about the political effects of bypass aid in autocratic regimes. While some speculate that bypass aid could stymie political reform (Dietrich 2013, 708; Bueno de Mesquita and Smith 2009), and case studies suggest that bypass aid sometimes undermines democracy (e.g., Ear 2013; de Waal 1997), others see bypassing recipient governments as a way to increase aid effectiveness, or at least as

---

<sup>2</sup>An exception is literature on democracy aid. This literature finds some evidence that democracy aid promotes democratic governance in recipient countries (Goldsmith 2001; Dunning 2004; Finkel, Pérez-Liñán and Seligson 2007; Scott and Steele 2011; Dietrich and Wright 2013).

<sup>3</sup>See also Wright (2009).

a reason to doubt the existence of a political aid curse (Altincekic and Bearce 2014; Bermeo 2016). In the next section, I develop a formal model of dictatorship and revolution to bring some clarity to this question.

### 2.3 Bypass Aid and Autocratic Survival

Many theories seek to explain transitions from autocracy.<sup>4</sup> Following Altincekic and Bearce (2014), I consider the role of bypass aid within the context of economic theories of democratization (e.g., Acemoglu and Robinson 2001). The willingness of elites to cede power to regime opponents is an important part of democratization, and popular resistance, both violent and non-violent, can sometimes exert pressures on elites to democratize or can constrain the direction of institutional change (Bermeo 1997; Stephan and Chenoweth 2008; Teorell 2010). In emphasizing the role of crises in bringing about institutional or leadership change, some have noted that “[m]ass protests, riots, strikes, uprisings, and assorted terrorist actions are often prominent features of crises, helping to shape their severity and paths” (Dogan and Higley 1998, 7). However, exerting political pressure is costly and requires overcoming collective action problems (Olson 1965). Actors’ willingness to try to overcome these problems depends on the benefits they receive in the status quo and expected future benefits. Foreign aid, insofar as it provides a source of relief under the status quo, can affect these considerations, and thus affect the likelihood of domestic unrest. Where goods go unprovided, people are more willing to incur costs if it means a potential change from the status quo. For instance, “[i]n the second half of the nineteenth century, by its reluctance to take on responsibility for famine relief, the British Government discredited itself and so nurtured the nationalist movement [in India], which in turn sought to use famine as an issue for mass mobilization” (de Waal 1997, 12). Channeling aid directly to people through local or international NGOs thus potentially

---

<sup>4</sup>For an extensive review of theories of democratization, see Coppedge (2012).

removes a source of grievance, decreasing the likelihood that an unpopular government faces resistance or collapses. This is the intuition of how bypass aid can affect the incentive to revolt in the model below.

Though this is a straightforward hypothesis, I formalize the argument for a few reasons. First, Altincekic and Bearce (2014) draw on Acemoglu and Robinson’s (2001) formal model in arguing that non-fungible aid should not benefit autocratic regimes. Since I am using the same theoretical framework to predict the opposite outcome, the model helps show that I am not introducing any new or unstated assumptions to the argument. Second, using the formal model allows me to derive a clear comparative static on the variable of interest: the proportion of aid delivered through bypass channels. This is important since including measures of total aid along with a proportion that is calculated using total aid may raise concerns about bias in the empirical tests. Lastly, incorporating aid into this framework provides an important baseline for future extensions. In particular, the baseline model should prove useful in endogenizing donor choices over aid channels with an explicit model of domestic politics in the recipient country.

### 2.3.1 The Model

Consider a dynamic game played between a group of elites and a population.<sup>5</sup> At the start of the game, the elites control the government. In any round, the elites can choose to allot a portion  $x$  of the available resources,  $R$ , to the population. I assume that prior to receiving any international aid, the stock of resources is 1. In every round the country receives an amount of aid denoted by  $a > 0$ . For simplicity I assume that  $a$  is exogenous and constant across rounds. Let  $\beta \in [0, 1]$  (also exogenous) be the proportion of aid that goes directly to the population. In every round, the

---

<sup>5</sup>The game closely follows a simplified version of Acemoglu and Robinson’s (2001) model presented in Gehlbach (2013). The solution concept is Markov perfect equilibrium. Similar to others (e.g., Meiorowitz and Tucker 2013), I abstract away from problems of collective action in treating the elites and the population as unified actors, acknowledging that these problems are not trivial.

population derives utility from any foreign aid it receives directly ( $\beta a$ ), plus whatever the government allocates to it ( $x$ ). (The total amount of bypass aid is therefore equal to  $\beta a$ .) The government derives utility from the portion of aid that it receives  $((1 - \beta)a)$  as well any remaining state resources  $(1 - x)$ .

The state of the world in time period  $t$  is defined by the pair  $(G^t, c^t)$  where  $G^t \in \{A, R\}$  and  $c^t \in \{c_L, c_H\}$ . If  $G^t = A$ , the country is in a state of autocracy, while if  $G^t = R$  the country is in the state of a revolutionary regime. The parameter  $c$  indicates the cost to the population of removing the incumbent regime from power in a given state. Thus, the government can be either in one of two autocratic states –  $(A, c_L)$  or  $(A, c_H)$  – or one of two states of a revolutionary (i.e., post-autocratic) regime –  $(R, c_L)$  or  $(R, c_H)$ . I assume that  $c_L \sim U[0, 1]$  is drawn at the beginning of the game by Nature and applies in all future rounds. As a simplifying assumption, I follow Gehlbach (2013) in assuming that  $c_H$  is high enough such that the population never chooses to initiate a revolution in the state  $(A, c_H)$  even when the regime transfers no resources to the population.<sup>6</sup> In the state  $(A, c_L)$ , however, the population may want to revolt. In either state of autocracy, the population can decide whether or not to oust the elites from office and take over the government. If the population initiates a revolution, it takes over the government and inherits the government's resources, minus the costs of revolution.

In any given round, the probability that the next state is  $(A, c_L)$  (if there is no revolution) is  $q \in (0, 1)$  and the probability that the state is  $(A, c_H)$  is  $1 - q$ . Let  $\delta \in (0, 1)$  be a common discount factor. Denote the value to the population of state  $(A, c_H)$  as  $V(A, c_H) = \beta a + \delta[qV(A, c_L) + (1 - q)V(A, c_H)]$ . Here  $x$  is zero because an autocrat has no incentive to make concessions when revolution is not credible. The above expression can be rewritten in terms of  $V(A, c_H)$  as

---

<sup>6</sup>This amounts to assuming that  $c_H \geq 1 + (1 - \beta)a$ . Alternatively, one could assume that the opposition does not get to move if  $c = c_H$  and derive the same result.

$$V(A, c_H) = \frac{a\beta + \delta qV(A, c_L)}{\delta(q-1) + 1}. \quad (2.1)$$

When costs are low enough so that revolution is politically possible, the elites may want to transfer some resources to the population to prevent a revolution. I assume that the elites receive nothing under a revolutionary government. The value to the population of living in state  $(A, c_L)$  is now a function of the resources transferred to them by the elites, the amount of foreign aid they receive, and their expectations about the likelihood of being in the other of the autocratic states in the future, which can be expressed as

$$V(A, c_L) = x + \beta a + \delta[qV(A, c_L) + (1-q)V(A, c_H)]. \quad (2.2)$$

Substituting (2.1) into (2.2) and simplifying yields  $V(A, c_L) = \frac{a\beta + \delta(q-1)x + x}{1-\delta}$ . The population will be content under autocracy if the amount of resources that the elites transfer to the population makes them at least as well off as they would be under a revolutionary regime.<sup>7</sup> Since a revolution succeeds with certainty, the value of a revolutionary regime is  $V(R, c_L) = \frac{1+(1-\beta)a-c_L}{1-\delta}$ . To avoid revolution, the elites' offer must satisfy  $V(A, c_L) \geq V(R, c_L)$ , or

$$\frac{a\beta + \delta(q-1)x + x}{1-\delta} \geq \frac{1 + (1-\beta)a - c_L}{1-\delta}. \quad (2.3)$$

The government's utility is decreasing in  $x$ , so it sets  $x$  such that Equation 2.3 holds with equality. Solving for this optimal offer and taking its first derivative with respect to  $\beta$  shows that the government's equilibrium transfer is decreasing in the proportion of bypass aid ( $\frac{\partial x^*}{\partial \beta} < 0$ ). As the proportion of aid delivered through bypass channels increases, smaller offers from the government deter unrest. So long

---

<sup>7</sup>Under the assumption that the value of a revolutionary regime also includes the full amount of future bypass and government aid ( $V(R, c_L) = \frac{1-c_L+a}{1-\delta}$ ), the offer that buys off the population is still decreasing in  $\beta$  ( $\frac{\partial x^*}{\partial \beta} = -\frac{a}{\delta(q-1)+1} \leq 0$ ).

as aid provides some direct benefit to the population, governments can more easily stave off revolution.<sup>8</sup> This is independent of any effect that government aid would have on allowing the government to make bigger offers. The intuition behind the result for the proportion itself is that as more of the total “pie” of aid goes directly to the population now, there is less to be gained from taking over the government (and, by extension, the aid it receives). In sum, in periods that are unfavorable for revolution, bypass aid acts as a humanitarian boon – without it the population would get nothing. In periods where revolution is possible, bypass aid acts a political subsidy to the government by making it easier to buy off the opposition. In the next section I develop a research design to test this hypothesis.<sup>9</sup>

## 2.4 Research Design

Testing the theory requires data on autocratic regimes, domestic unrest, and aid. Following recent work on the effects of aid on government survival and democratization (e.g., Wright 2009), I use country-year-level data on OECD-aid-receiving authoritarian regimes from Geddes, Wright and Frantz (2014). The data set includes information on 61 autocratic countries from 2005 to 2010.<sup>10</sup>

Since others have shown that domestic unrest threatens authoritarian regimes (Bueno de Mesquita and Smith 2010), I use Kono, Montinola and Verbon’s (2013) indicator of domestic unrest as my dependent variable. This variable is a count of

---

<sup>8</sup>For proof that the probability of revolution decreases with bypass aid, see Appendix A. In the Supplementary Files I consider an extension of the model where bypass aid not only increases the population’s utility but also increases the likelihood of a successful revolution. Even if bypass aid increases the likelihood of a successful revolution, incumbents may nevertheless allow it because it can improve their survival prospects under some conditions.

<sup>9</sup>It might also be the case that bypass aid could stabilize a regime if the regime intercepts aid or coopts bypass actors to invest in repressive capacity. In this case, the prediction would essentially be the same as models that assume government control of aid, but through a different mechanism. While the results here are similar to those in Smith (2008), the model here shows that government control of aid is not a necessary condition for aid to deter unrest. In Section 2.7.9 of the Supplementary Files I examine the relationship between bypass aid and military expenditures in the sample and find that it is not associated with changes in military expenditures.

<sup>10</sup>The names of the countries appear in Table 2.7 in Section 2.7.2 of the Supplementary Files.



peaceful demonstrations, general strikes, and riots from Banks (2011). These events are particularly relevant for testing the theory, since the model emphasizes that the costs of organizing and demonstrating may deter revolution. While the theory also makes predictions about the incidence of regime failure, the temporal bounds on the aid and autocratic regime data sets leave only six years – 2005 through 2010 – for testing the theory (after lagging the independent variables by one year). Given that the temporal domain of this study is relatively short, using a measure of unrest rather than a measure of regime failure allows me to capture more variation in an alternative, relevant dependent variable.

To measure bypass aid, I follow Dietrich (2013) in using the OECD’s Creditor Reporting System (CRS) data base. Beginning in 2004, the OECD CRS database reports amount of aid that OECD donors delivered through public channels, NGOs, multilateral organizations, public-private partnerships, and “other” channels (OECD 2014). I use gross disbursements of Official Development Assistance for all types of aid to all sectors from all donors (not just DAC donors) in constant 2012 US dollars<sup>11</sup> to create a proportional measure of *bypass aid* that divides aid received through NGOs and civil society, multilateral organizations, and public-private partnerships by all aid categories.<sup>12</sup> While some of this aid may be given to governments, the OECD database does not provide enough information to determine where aid actually ended up in a given case. For instance, some aid channeled through the International Monetary Fund or World Bank would be labeled as multilateral bypass aid even if the loans ultimately end up going to the government. This same critique might apply to some NGOs that work closely with recipient governments. However, in many other cases it is reasonable to expect that multilateral aid does bypass the recipient government. For example, much of the work carried out by the United Nations Refugee Agency

---

<sup>11</sup>I accessed the OECD CRS database on April 7, 2015. The data base allows users to select either “Commitments” or “Gross Disbursements,” but not “net” disbursements.

<sup>12</sup>As discussed below, the results are robust to various operationalizations of this measure, including one that excludes public-private partnerships.

is conducted independently by that organization. Absent detailed information on the eventual destination of aid given through these channels, this is the best existing measure and one that follows previously published research (e.g., Dietrich 2013).

I control for a few factors that may both affect unrest and the distribution of aid across channels in a recipient country.<sup>13</sup> Since I am interested in evaluating the relationship between bypass aid and domestic unrest rather than developing an explanatory model of unrest, the primary model specification focuses on potential confounders. Because more corrupt countries may be more likely to receive bypass aid and less likely to face mass unrest, I follow Dietrich (2013) in using the World Governance Indicators data set to create a measure of governance quality ranging between 0 and 5, with greater values indicating better governance. Involvement in civil conflict may lead donors to choose bypass to avoid directly aiding openly belligerent governments, and conflict may affect unrest. Alternatively, aid through government channels may increase in times of civil conflict if third parties attempt to bolster a friendly regime. As such, I control for a count of civil conflicts in the previous year using the UCDP-PRIO Armed Conflict Dataset v.4-2014 (Gleditsch et al. 2002). Finally, because the occurrence of disasters may lead to more aid being delivered through specialized NGOs (or to governments weakened by a disaster) and disasters may affect unrest in autocratic systems (Quiroz Flores and Smith 2013), I control for a count of natural disasters from the International Disaster Database (EM-DAT 2009). I account for unobserved heterogeneity within years using year-fixed effects in most models. To account for the possibility that unrest events in one year make unrest events in the following year more likely, I include a one-year-lagged measure of unrest events. In another model, I account for additional determinants of unrest by including a logged measures of real GDP from Gleditsch (2002), total population from the Penn World Tables (Heston, Summers and Aten 2012), a logged

---

<sup>13</sup>I lag all independent variables by one year to alleviate concerns about reverse causality.

measure of total aid, and a count of the number of terrorist attacks in the previous year from the Global Terrorism Database.<sup>14</sup> Table 2.1 reports summary statistics for the main variables in the analysis.

Table 2.1: Summary Statistics for Key Variables

Statistic	N	Mean	St. Dev.	Min	Max
Unrest Events	344	0.663	1.868	0	18
Bypass Share	344	15.239	17.119	0.000	79.587
Governance Index	350	2.003	0.704	0.780	4.496
Civil Conflicts	350	0.223	0.568	0	4
Natural Disasters	350	2.323	4.144	0	37
Year	350	N/A	N/A	2005	2010
Total Government Aid (log)	344	4.053	2.028	0.000	7.758
Total Bypass Aid (log)	344	3.370	1.901	0.000	8.290
Total Aid (log)	344	5.529	1.969	0.000	8.881
Terror Attacks	350	0.080	0.421	0	5
Real GDP (log)	344	10.663	1.617	7.459	16.048
Population (log)	338	9.452	1.323	7.121	14.096

## 2.5 Results

Before moving on to the statistical models, it is instructive to take a first look at the relationship between bypass aid and domestic unrest. Figure 2.1 plots the proportion of aid delivered through bypass channels in a given country against the count of unrest events in the subsequent year. A casual look at this figure suggests that there may be a negative relationship between bypass aid and unrest.

To be a bit more rigorous, Table 2.2 reports the results of five regression models.<sup>15</sup> The key independent variable in each model is the proportion of aid delivered through bypass channels in year  $t - 1$ . The models in Table 2.2 consistently show a negative

<sup>14</sup>Following Dietrich (2013), I include only attacks that were directed against US targets/symbols.

<sup>15</sup>Since the outcome variable is over dispersed, I follow Kono, Montinola and Verbon (2013) in using negative binomial regression models rather than a Poisson model throughout the paper. Results for an alternative specification where the outcome is a dichotomous indicator of whether or not at least one unrest event occurred and the results of zero-inflated negative binomial regression models are in Sections 2.7.5 and 2.7.6 of the Supplementary Files.

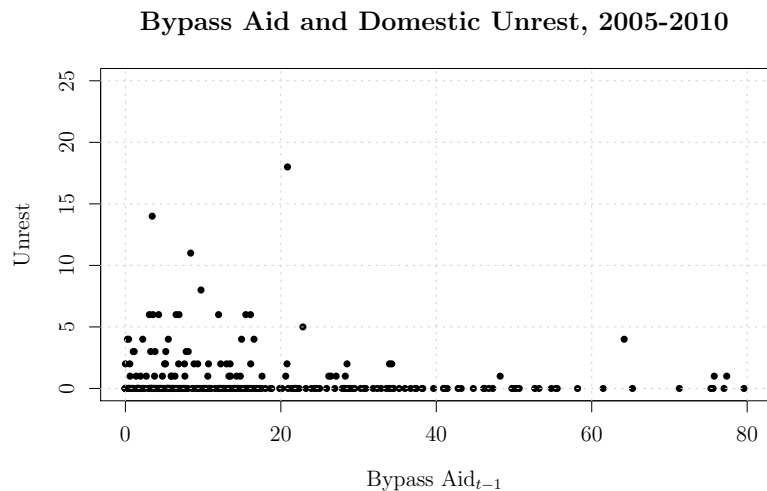


Figure 2.1: Scatterplot of Bypass Aid and Domestic Unrest, 2005-2010

association between the percentage of bypass aid and the occurrence of domestic unrest. Model 1 controls for potential recipient-level confounders, and Model 2 adds year dummies to Model 1. In both models, the relationship between the share of aid received through bypass channels and domestic unrest is negative and statistically significant. As expected, better governance is associated with less unrest. The measure of previous civil conflicts is positive (except for in Model 3) but is never statistically significant. The count of natural disasters is a statistically significant and positive in all models except Model 3. As expected, unrest in year  $t - 1$  is positively associated with unrest in year  $t$  and is statistically significant.

Model 3 accounts for additional determinants of unrest which entails losing about five percent of the observations due to missing data. Here the coefficient on the bypass share variable is still negative, though it does drop in statistical significance to the 90% confidence level. The coefficient on real GDP output is negative though not statistically significant, as is the measure of total aid. The estimated coefficient for the count of terrorist attacks is positive and not statistically significant. Finally, the population variable is positive and statistically significant. While these variables may

affect unrest, it is not clear they would affect the distribution of aid across channels, and as such should not confound the relationship between bypass aid and unrest. Because of this, I omit these variables from the remainder of the analysis.

The formal model produces a clear prediction about the relationship between bypass aid and the likelihood of domestic unrest: regardless of the total size of aid flows, as more aid is delivered through bypass agents, domestic unrest should decrease. An alternative way to articulate the main hypothesis is that, holding government (i.e., non-bypass aid) aid fixed, an increase in total bypass aid should reduce unrest. Model 4 reports the results of replacing the bypass share variable with measures of total bypass and government aid. Consistent with the other models, the relationship between bypass aid and domestic unrest is negative and statistically significant even when controlling for the total amount of aid delivered through government channels. Interestingly, the effect of total government aid on unrest is positive and statistically significant, highlighting possible heterogeneous effects of aid on unrest.

Finally, to ensure the main results are not sensitive to the choice of estimating the parameters using a negative binomial regression model, Model 5 reports the results of OLS estimates using the same variables included in Model 2. Again, the share of bypass aid is negatively associated with unrest.

To provide a substantive interpretation of this result, I calculated expected counts of unrest events using Model 1 in Table 2.2. To implement this procedure, I bootstrap random samples ( $B=1000$ , with replacement) from the data and estimate coefficients for Model 1 in Table 2.2 on each data set. For each set of coefficients, I calculate the expected count of unrest events across a range of values for the bypass aid variable from 0% to 50% (in increments of .5) holding the control variables (governance, civil conflicts, and natural disasters) at their unconditional mean values. I estimate  $E[\text{Unrest Events}_{ij} | \hat{\beta}_i, X_j] = e^{\hat{\beta}_i X_j}$  where  $\hat{\beta}_i$  is a vector of calculated coefficients from bootstrapped sample  $i$  with  $i = 1, \dots, 1000$  and  $X_j$  is a vector of 100 values for bypass

Table 2.2: OECD Aid Channels and Domestic Unrest, 2005-2010

	<i>Dependent variable: Count of Unrest Events</i>				
	<i>Negative Binomial Regression</i>				<i>OLS</i>
	<i>Model 1</i>	<i>Model 2</i>	<i>Model 3</i>	<i>Model 4</i>	<i>Model 5</i>
Bypass Share	-0.029*** (0.010)	-0.037*** (0.014)	-0.024* (0.014)		-0.019*** (0.006)
Total Bypass Aid (log)				-0.286* (0.155)	
Total Government Aid (log)				0.316** (0.144)	
Governance Index	-1.216*** (0.226)	-1.473*** (0.245)	-1.398*** (0.331)	-1.308*** (0.255)	-0.323*** (0.102)
Civil Conflicts	0.157 (0.278)	0.129 (0.281)	-0.343 (0.350)	0.041 (0.291)	0.233 (0.227)
Natural Disasters	0.111*** (0.029)	0.122*** (0.026)	-0.005 (0.034)	0.114*** (0.035)	0.118** (0.060)
Lagged Unrest	0.149** (0.059)	0.153*** (0.057)	0.105* (0.063)	0.170*** (0.060)	0.203** (0.090)
Real GDP Output (log)			-0.167 (0.238)		
Total Aid (log)			-0.158 (0.160)		
Terrorist Attacks			0.229 (0.250)		
Population (log)			0.868** (0.389)		
Constant	1.518*** (0.478)	2.225*** (0.542)	-3.352 (2.088)	1.690** (0.730)	1.110*** (0.314)
Year Dummies	No	Yes	Yes	Yes	Yes
Observations	338	338	320	338	338
R <sup>2</sup>					0.213
Adjusted R <sup>2</sup>					0.189
Log Likelihood	-281.889	-279.160	-258.757	-281.174	
$\theta$	0.215*** (0.042)	0.222*** (0.043)	0.258*** (0.054)	0.213*** (0.041)	
Akaike Inf. Crit.	575.779	580.320	547.514	586.349	
Residual Std. Error					1.666
F Statistic					8.854***

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

White's (1980) heteroskedasticity-consistent standard errors (HC1 variant) in parentheses.

Models 1-4 are negative binomial regression models. Model 5 estimated via OLS.

aid (which varies across iterations of  $X_j$ ) and the control variables held at their means.

Figure 2.2 reports the mean predicted count of unrest for each level of bypass aid along with the 2.5% to 97.5% quantiles of the distribution. The  $x$ -axis plots various values of the bypass aid share variable and the  $y$ -axis plots the expected count of unrest events. Increasing the share of aid delivered through bypass channels from 0% to the mean amount (about 15%) leads to a decrease in the expected count of unrest events of about 36% (from 0.59 to 0.38). Changing from 0% to one standard deviation above the mean leads to a decrease in the expected count of unrest events of about 61% (from 0.59 to 0.23).

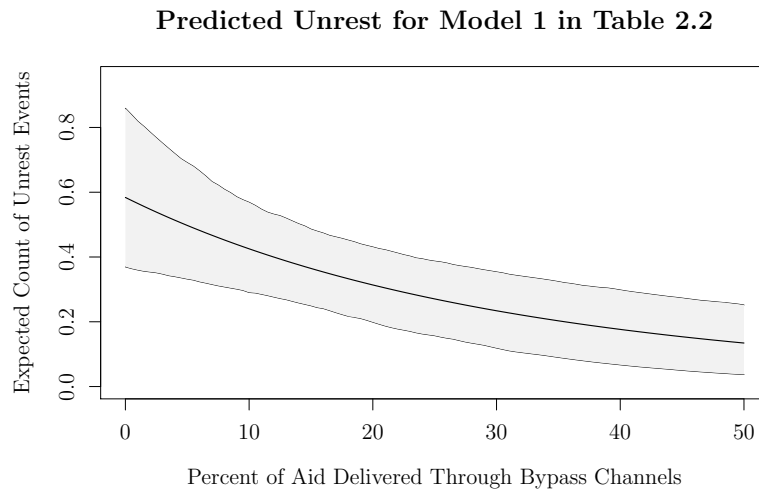


Figure 2.2: Expected Count of Unrest Events Across Possible Bypass Aid Shares

One potential concern in using the OECD data on aid channels is that the data could be subject to measurement bias due to underreporting as donors were adjusting to new reporting requirements that the OECD implemented in 2004.<sup>16</sup> Unfortunately, no other source provides comprehensive data on aid channels. However, the *AidData* project (Tierney et al. 2011), which records aid data at the project level, does contain information about the stated purpose for which donors commit aid. To create an alternative indicator of bypass aid, I search for patterns in the “pur-

<sup>16</sup>Dietrich (2013, 702) also acknowledges this issue in footnote 13 in her article.

OECD versus *AidData* Bypass Measures, 2004-2009

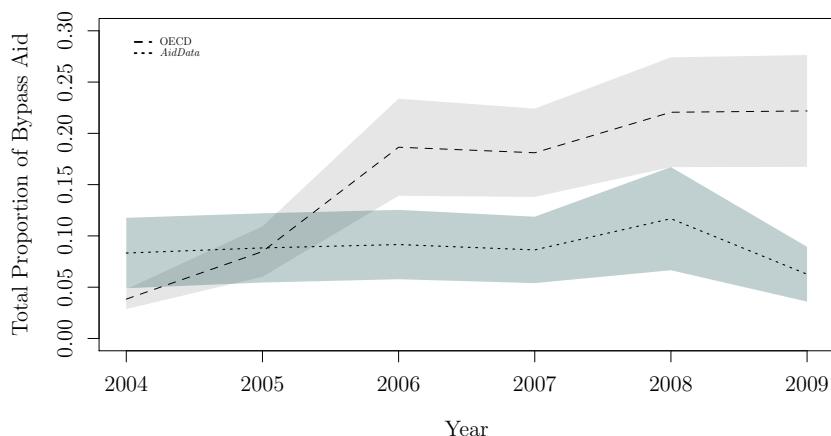


Figure 2.3: Comparing Aid Measures

The dashed line is the mean level of OECD bypass aid for all countries in the sample, and the dotted line is the mean level of bypass aid using my measure constructed from *AidData*. The shaded areas mark 95% confidence intervals.

pose” variable that might indicate that aid was given through bypass channels. The string of patterns I search for in *R* (R Core Team 2014) is “ngos|NGOs|NGO’s|civil society|food|Food|Material relief.” I create a measure of the proportion of aid that was committed to projects that included one or more of these terms for each recipient year.<sup>17</sup> This measure is moderately correlated with the measure constructed from OECD data ( $\rho = 0.54$ ).

Figure 2.3 plots the means of each measure  $\pm 1.96$  standard deviations for every year from 2004 to 2009. The mean level of bypass aid in the OECD measure is closely clustered around about four percent in 2004, while the *AidData* measure is more varied, centered around eight percent. The *AidData* measure is relatively consistent across time and may be undercounting bypass aid. Whether this is the case is unclear, though the measure is arguably less susceptible to concerns about underreporting during the early years of OECD reporting requirements. Table 2.3 reports

<sup>17</sup>Though disbursements would be more appropriate, this data is not available from *AidData*. See Section 2.7.10 in the Supplementary Files for a description of a validity check for this coding procedure.



the results of the basic model specification of Model 2 from Table 2.2 substituting in the alternative measure of bypass aid. Again, there is clear support for the theory in that the measure of bypass aid is negatively associated with unrest events. Model 6 uses the same sample from Table 2.2, while Model 7 extends the data set back to 1996. Because the results are similar and in the post-2004 sample and there is no clear benchmark to compare *AidData* measure in the pre-2004 period (and Figure 2.3 suggests that undercounting may not be actually much of a concern), I use the OECD measure in the remainder of the analysis. Additionally, the *AidData* measure captures only commitments, not disbursements. Because donors often withhold or delay the disbursement of aid commitments, using the OECD data on actual disbursements likely does a better job capturing the political effects of bypass aid in recipient countries. Still, the congruity of the results from using this alternative measure with the OECD measure is encouraging.

### 2.5.1 Exogenous Variation in Bypass Aid

So far the analysis provides support for the theory: bypass aid is associated with less domestic unrest in autocratic countries. Substantively, an increase from zero to the mean level of bypass aid is associated with a modest decrease in the expected count of unrest events. This finding is robust to using alternate operationalized versions of the bypass aid variable (See Section 2.7.11 of Appendix) and controlling for a number of potential confounders and determinants of unrest. Most importantly, the analysis above attempts to account for observable and unobservable potentially confounding variables.<sup>18</sup> The main threat to inference is that a government's latent capacity for repression may affect both the propensity of donors to give through bypass channels and the likelihood of unrest, which I attempt to account for with Dietrich's (2013) measure of governance quality. Though not reported, the main

---

<sup>18</sup>The main result is also robust to including continent-, region-, or country-fixed effects. See Section 2.7.12 of the Supplementary Files.

Table 2.3: *AidData* Measure of Bypass Aid

	<i>Dependent variable:</i>	
	<i>Model 6</i>	<i>Model 7</i>
Bypass Share ( <i>AidData</i> )	-0.031** (0.016)	-0.022** (0.010)
Governance Index	-1.233*** (0.269)	-0.884*** (0.250)
Civil Conflicts	0.231 (0.243)	0.216 (0.181)
Natural Disasters	0.114*** (0.028)	0.122*** (0.015)
Lagged Unrest	0.195*** (0.055)	0.241*** (0.027)
Constant	1.776*** (0.646)	1.166* (0.675)
Year-fixed Effects?	Y	Y
Observations	338	655
Log Likelihood	-299.927	-544.248
$\theta$	0.234*** (0.044)	0.212*** (0.030)
Akaike Inf. Crit.	621.853	1,120.496

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

White's (1980) HC1 standard errors in parentheses.

Negative binomial regression models.

results in Table 2.2 are robust to including a measure of military expenditures as a percentage of government spending from the World Bank, but including that measure reduces the number of observations in the data set by more than half (from 338 to 140 observations) due to missing data. There would be a problem of reverse causality if domestic unrest affects the distribution of aid across channels in a country. This would matter if outside countries channeled more resources to imperiled governments in times of unrest, since the data would then capture a relative decrease in bypass aid during times of heightened unrest. Yet this is unlikely to be driving the results in the main analysis since I lag the bypass share variable by one year and include a lagged dependent variable.

Nonetheless, concerns about endogeneity may remain. My strategy is to identify an exogenous source of variation in bypass aid using an instrumental variables approach. The goal is to construct a measure that is (1) plausibly causally related to and strongly correlated with the distribution of aid in country  $i$  at time  $t$ , (2) not caused by unrest or factors that affect bypass aid or unrest in country  $i$  at time  $t$ , and (3) does not affect unrest through alternate channels.

I attempted to do this by constructing a new data set of international aid scandals, distinguishing between “major” and “minor” scandals. I define a major aid scandal as an instance where an aid donor expressed concern or alleged that a recipient government had used aid funds for purposes other than those intended by the donor(s) and at least one major media outlet reported on those allegations. I define a minor scandal as an instance where an international news source published an article that discusses actual or potential aid misuse by a specific recipient government. Aid scandals should be a good instrument since when these scandals and concerns about aid use are prominent, international donors should be more inclined to bypass recipient governments. Anecdotal evidence suggests that donors respond to concerns about corruption and mismanagement by channeling aid to NGOs and civil society actors

(French 1996; Akam 2012). It might be that leaders of donor countries intrinsically value improving the human condition in foreign countries and want recipient governments to use aid effectively in pursuit of that goal. Alternatively, a leader may see aid as an important diplomatic tool that helps her attain foreign policy goals, and when domestic audiences observe misuse by recipient governments they may withdraw support for aid. As Lancaster (2007, 7) argues, “publics in aid-giving countries will turn against aid for development and other purposes if they regard it as having been wasted or used corruptly.” Since scandals may attract media attention and help turn aid policy into a salient political issue, they should be associated with changes in aid policy. Reorienting one’s aid portfolio so that aid more often bypasses recipient governments is one way to redeem the aid enterprise and retain an important foreign policy tool.

I focus specifically on allegations of corruption that are linked to aid use, not general corruption, and only include cases related to development and emergency aid rather than military aid. I count only scandals reported in news stories about specific recipient countries, not general articles about foreign aid and corruption (many of which are opinion pieces). Articles must be about corruption in using aid at that time or new revelations of recent corruption in aid practices. For example, I do not count articles that reference previously well-documented aid misuse by a regime that is no longer in power at the time of writing. The data set also excludes cases where agents of donor countries were complicit in corruption (for example, a 1993 scandal in the use of Italian aid in Bangladesh).<sup>19</sup> With these guidelines in place, I collected data on international aid scandals from 1990 to 2010.<sup>20</sup> The primary analysis below includes a total of 27 aid scandals in the years 2003 through 2008 – 15 major scandals and 12 minor scandals.<sup>21</sup> The main measure I use counts all major aid scandals in

---

<sup>19</sup>See Section 2.7.7 of the Supplementary Files for a description of the data collection process.

<sup>20</sup>I limited the initial collection effort to the post-Cold War period to avoid problems associated with political aid giving during the Cold War, but also for feasibility.

<sup>21</sup>The complete data set extends up to 2014, though since the Geddes, Wright and Frantz (2014)

year  $t - 2$  (for the purpose of predicting the bypass share in year  $t - 1$ ) outside of country  $i$ .<sup>22</sup>

A potential threat to the validity of the instrument is that scandals may affect unrest in autocracies through other channels. One possibility is that aid scandals in country A could lead to unrest in country A, which could spillover to country B. Another possibility is the opposite relationship: autocrats may preemptively employ repressive measures to deter unrest when other neighboring autocracies are facing threats to survival. Alternatively, if citizens in country B use country A as a benchmark and see that the regime in charge of country A is misusing international aid supplies, they may favorably update their beliefs about their own leaders in country B, which might lead to less unrest. While these mechanisms are plausible in principle, it is not clear they would apply in autocracies where outside information is controlled and restricted.<sup>23</sup>

Still, to reduce the chances that aid scandals would affect unrest through these channels, I create a second measure of “extra-continental aid scandals” that counts major scandals that occurred outside country  $i$ ’s continent.<sup>24</sup> If aid scandals do lead to unrest in a country, it should be less likely that that unrest spills over to a country outside the region of the country experiencing the scandal and unrest. Further, scandals that occur outside a country’s continent should be less salient to the population in country  $i$ , and it should be less likely that citizens in country  $i$  use a country outside their region as a benchmark against which to evaluate their own leaders. As such, aid scandals outside country  $i$ ’s continent should influence the

---

data ends in 2010, I only use the aid scandal data up to 2008 given the two-year lag. Section 2.7.7 in the Supplementary Files reports a slightly simplified version of the data that also lists cases used in a robustness check below.

<sup>22</sup>The results below are unaffected by including a control variable that counts any scandals that occurred in country  $i$ .

<sup>23</sup>It might be the case that aid scandals lead donors and/or human rights to apply pressure for reform more broadly, which could affect domestic unrest in outside countries. However, in this case it would seem that the effect on domestic unrest would be positive, which is the opposite of what I predict.

<sup>24</sup>The results are also robust to instead using UN regions rather than continents.

distribution of aid in that country only by affecting donors' overall aid strategies. While in principle *all* scandals are relevant for the analysis, the idea is that excluding these scandals from the analysis helps satisfy the exclusion restriction.

A challenge of the data is that while the scandals vary across years, they vary little within years. Thus, including year-fixed effects makes it difficult or impossible to estimate any independent effect of aid scandals. Because the theory linking aid scandals to the distribution of aid relies on an effect that is common to multiple countries, the use of year-fixed effects eliminates the ability to detect this type of effect. I address this in two ways. First, I control for year-level factors that might confound the relationship between aid scandals, bypass aid, and unrest. Economic instability might lead more recipient governments to misuse aid, leading to an uptick in scandals, and may set in motion political conflict that leads to unrest. I take the average GDP growth and average inflation for consumer prices for all countries in the year  $t - 2$  (the year in which any aid scandals occurred) using data from the World Bank World Development Indicators data set (World Bank 2013). Similarly, the occurrence of natural disasters might put unexpected pressure on governments, increasing the temptation to redirect development aid toward patronage uses and affecting global unrest. I include a count of the total number of natural disasters in year  $t - 2$  as recorded in the International Disaster Database. Controlling for these factors in both stages of the model should reduce the chances that some year-level factor confounds the key relationship of interest.<sup>25</sup>

Second, to permit the use of year-fixed effects, I calculate a third measure that weights scandals by the foreign policy similarity of recipient country  $i$  to countries where an aid scandal occurred. First, I create a measure of the average foreign policy

---

<sup>25</sup>I omit recipient country-fixed effects from the main analysis below because of problems with model convergence caused by the loss of a large number of degrees of freedom in a small data set in the bootstrapping procedure to adjust standard errors. However, the second-stage coefficient on the predicted bypass share remains statistically significant at the 90% confidence level when including recipient country-fixed effects in both stages of the unadjusted instrumental variables models implemented below.

similarity for each country dyad throughout the ten years up to and including the scandal using a measure of voting similarity in the United Nations General Assembly (Strezhnev and Voeten 2013) then rescale this measure to vary between 0 and 1. I record any countries outside country  $i$ 's continent that experienced a major aid scandal or a minor scandal in *The New York Times*,<sup>26</sup> and sum the rescaled affinity scores of country  $i$  with those countries. The value of this measure in country  $i$  during year  $t$  is therefore  $x_{it} = \sum_{j=1}^J \tau_{ij}$ , where  $\tau_{ij}$  is the average foreign policy similarity of country  $i$  and country  $j$  throughout the ten years up to and including the year of the scandal and  $J$  is the total number of countries outside country  $i$ 's continent that experienced a major or minor aid scandal in year  $t$ . The idea is that when scandals occur, donors might disproportionately reconsider aid policies to countries with international policy preferences that align with countries that misuse aid. This measure allows for variation across countries within years (that is not just due to differences in continents) and at least makes the use of time-fixed effects sensible.

I use these measures to predict bypass share in year  $t - 1$ . I follow Dietrich (2013) (who follows Aitchison (1986)) in first transforming the bypass aid proportion variable by taking the natural log of the ratio of bypass aid to non-bypass aid to make the measure continuous.<sup>27</sup> Table 2.4 reports the first stage regressions and shows that the coefficient on all three measures of aid scandals are statistically significant, positive, and have F-statistics that exceed the rule-of-thumb threshold of at least 10 recommended by Staiger and Stock (1997) in all three models.<sup>28</sup>

---

<sup>26</sup>Because not all years have major aid scandals, the inclusion of minor scandals from the *New York Times* prevents the loss of a large number of observations (due to the inclusion of year-fixed effects) while still retaining a relevant factor that should influence donors' policies.

<sup>27</sup>To avoid taking the natural log of zero, I add .0001 to the numerator of each case. Estimating the first stage models on the raw bypass percentage variable also indicates that the three instruments exceed the  $F > 10$  threshold, but generating predictions from those models can lead to expected values outside the  $[0,100]$  interval. The transformation process keeps the variable values within a sensible range and follows Dietrich (2013).

<sup>28</sup>In Section 2.7.8 of the Supplementary Files I carry out the "plausibly exogenous" test for relaxing the exclusion restriction outlined by Conley, Hansen and Rossi (2012). The instrumental variables results weaken in that test, but are qualitatively in the same direction as the results in the main text. Use of that method requires using two-stage least squares and does not allow for transforming

The first and second stage equations for the instrumental variables regression are

$$\text{Count}[\text{Unrest Events}_{i,t}] = \alpha B_{i,t-1} + \beta \mathbf{X}_{i,t-1} + \eta_{it} \quad (\text{Negative binomial, Stage II}) \quad (2.4)$$

$$E[\text{Logged Bypass Ratio}_{i,t-1}] = \gamma S_{i,t-2} + \beta \mathbf{X}_{i,t-1} + \epsilon_{it} \quad (\text{OLS, Stage I}), \quad (2.5)$$

where  $B_{i,t-1}$  is the proportion of bypass aid in country  $i$  in year  $t - 1$ ,  $\mathbf{X}_{i,t-1}$  is a vector of country-specific control variables in year  $t - 1$ , and  $S_{i,t-2}$  is the measure of aid scandals outside country  $i$  in year  $t - 2$ . The dependent variable in the first stage is a (log-transformed) proportion and the dependent variable in the second stage is a count of unrest events. To implement the two-stage model, I generate predicted values of bypass aid and include them in the models predicting unrest. I then transform these predicted values back into the interval  $[0, 100]$  and include them in a model predicting a count of domestic unrest events, excluding the instrument (aid scandals) used in the first stage.

Tables 2.4 and 2.5 report the results from the first and second stage models, respectively. In all of the second-stage models in Table 2.5, the predicted bypass share is negatively associated with domestic unrest, though the result weakens in Model 9 (the one-tailed p-value is 0.06). Because the measure of bypass aid included in the second stage is an estimate with associated uncertainty that is not accounted for, this procedure underestimates standard errors. I adjust for this by bootstrapping random samples (with replacement) from the data. For each sample, I estimate the models depicted in Tables 2.4 and 2.5. I repeat this process 1000 times, extracting the second stage model coefficients to obtain a distribution. Table 2.6 reports the mean of the coefficient estimates on the predicted bypass share from the two second

---

the bypass ratio variable, so I am unable to implement the procedure using the exact same models as the main models here. As such, a direct comparison with the results here is unfortunately not possible.



Table 2.4: Stage I - Aid Scandals and Aid Channel Distribution, 2005-2010

	<i>Dependent variable: Logged Bypass Ratio</i>		
	(Model 8, Stage I)	(Model 9, Stage I)	(Model 10, Stage I)
Major Aid Scandals	0.24*** (0.03)		
Extra-Continental Scandals		0.31*** (0.04)	
Extra-Continental Scandals (Affinity Weighted)			0.23*** (0.07)
Governance Index	-1.82*** (0.12)	-1.79*** (0.12)	-1.79*** (0.12)
Civil Conflict	0.08 (0.15)	0.08 (0.15)	0.08 (0.15)
Natural Disasters	0.03 (0.02)	0.03 (0.02)	0.03 (0.02)
Lagged Unrest	-0.06 (0.05)	-0.04 (0.05)	-0.05 (0.05)
Average Global GDP Growth <sub>t-2</sub>	0.33*** (0.11)	0.29*** (0.11)	
Average Global Inflation <sub>t-2</sub>	0.003* (0.002)	0.003* (0.002)	
Total Global Disasters <sub>t-2</sub>	0.01*** (0.003)	0.01*** (0.003)	
Constant	-6.81*** (1.31)	-6.05*** (1.25)	-0.82** (0.33)
Year Dummies	N	N	Y
F-Statistic on Instrument	55.78	58.41	10.86
Observations	338	338	338
R <sup>2</sup>	0.51	0.51	0.52
Adjusted R <sup>2</sup>	0.50	0.50	0.51
Residual Std. Error	1.48 (df = 329)	1.47 (df = 329)	1.46 (df = 327)
F Statistic	42.51*** (df = 8; 329)	43.08*** (df = 8; 329)	36.06*** (df = 10; 327)

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).  
 Estimated standard errors in parentheses.  
 OLS estimates.

stage models along with 90% confidence intervals. The 90% confidence interval on the predicted bypass share does not include zero in Model 8, though it does in Models 9 and 10. Still, the result is clearly close to reaching statistical significance, and for both Models 9 and 10, the 80% confidence interval does not include zero. Given the relatively small sample size, the possibly conservative nature of bootstrapping procedure, and the fact that the estimated coefficient is always leaning negative, it seems reasonable to interpret these results as supporting the conclusions of the “naïve” statistical models.

Table 2.5: Stage II - Bypass Aid and Unrest, 2005-2010

	<i>Dependent variable: Count of Unrest Events</i>		
	(Model 8, Stage II)	(Model 9, Stage II)	(Model 10, Stage II)
Predicted Bypass Share	-0.05** (0.02)	-0.03 (0.02)	-0.05* (0.03)
Governance Index	-1.64*** (0.43)	-1.37*** (0.39)	-1.71*** (0.48)
Civil Conflicts	0.11 (0.27)	0.07 (0.27)	0.11 (0.27)
Natural Disasters	0.14*** (0.03)	0.14*** (0.04)	0.13*** (0.04)
Lagged Unrest	0.15* (0.08)	0.17** (0.08)	0.15** (0.08)
Average Global GDP Growth <sub>t-2</sub>	-0.26 (0.20)	-0.28 (0.20)	
Average Global Inflation <sub>t-2</sub>	0.01* (0.004)	0.01 (0.003)	
Total Global Disasters <sub>t-2</sub>	-0.0000 (0.01)	-0.001 (0.01)	
Constant	3.53 (2.20)	3.51 (2.20)	2.62*** (0.99)
Year Dummies	N	N	Y
Observations	338	338	338
Log Likelihood	-281.95	-283.09	-281.87
$\theta$	0.21*** (0.04)	0.20*** (0.04)	0.21*** (0.04)
Akaike Inf. Crit.	581.89	584.18	585.74

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

Estimated standard errors in parentheses.

Negative binomial regression models.

Table 2.6: Bootstrapped Second Stage Coefficients

	Variable	$\bar{\beta}$	$\hat{se}(\beta)$	90% lower	90% upper
<i>Model 8</i>	Predicted Bypass Share	-0.054	0.027	-0.098	-0.010
<i>Model 9</i>	Predicted Bypass Share	-0.035	0.024	-0.074	0.004
<i>Model 10</i>	Predicted Bypass Share	-0.050	0.031	-0.102	0.002

Results from 1000 random samples (with replacement) from data.

N in each sample is 338.

First-stage outcome variable is logged bypass ratio.

Second-stage outcome variable is count of unrest events.

In the Supplementary Files I conduct a number of robustness checks. The results are robust to a variety of research design choices including dropping riots from the count of unrest events in the dependent variable, using a logit model with a dichotomous indicator for unrest events, using a zero-inflated negative binomial regression model, including continent-, region-, or country-fixed effects, and using alternative measures for bypass aid. I also present some tests to evaluate the plausibility of the exclusion restriction in the instrumental variable models in Section 2.7.8 of the Supplementary Files.

On balance the results provide fairly consistent support for the theory. In the baseline models I consistently find a negative association between the proportion of aid received through bypass channels and the occurrence of domestic unrest in autocracies. This finding is mostly confirmed in an analysis that uses aid scandals as an instrument for bypass aid, and the analysis suggests that these scandals are likely a valid instrument for bypass aid.

## 2.6 Conclusion

This paper shows both theoretically and empirically that bypassing recipient governments has political implications. Whereas some are ambivalent about the role of bypass aid in the story of political survival (Bueno de Mesquita and Smith 2009),

or assume that bypass aid is politically neutral (Altincekic and Bearce 2014; Bermeo 2016), the results here suggest that bypassing can still benefit autocratic regimes in terms of reducing domestic unrest. While bypass aid may improve human welfare in the short term, it may also reduce the incentive for people to challenge autocratic governments. Unlike existing theories, the theory does not rely on a government intercepting, misusing, or taxing aid. At the very least, the paper shows that much of what has been treated as “non-fungible” aid is arguably fungible for recipient governments and suggests that the assumption that bypass aid does nothing to harm the prospects for political reform is unfounded. This is not to say that bypass aid fails to achieve development goals or uniformly deters incentives for democratization. There may be important variation within countries that mutes any effects of bypass aid on unrest, and it is possible that some types of aid that get labeled as “bypass aid” – such as aid that is intended to promote the development of democracy – can help the process of political reform. But the results here show that, on average, bypass aid is associated with a lower frequency of unrest in autocracies. Insofar as acts of unrest place can pressure autocratic governments to take steps toward democracy, this study is relevant for understanding potential obstacles to democratic reform.<sup>29</sup>

From a policy perspective, donors concerned with promoting democracy should be wary (or at least cognizant) of the potential for bypass aid to undermine political resistance to autocracies. The results suggest that bypass aid should not be seen as an alternative to conditional aid, as it is sometimes implicitly treated (Altincekic and Bearce 2014). Further, the findings also have implications for studying the effectiveness of sanctions by highlighting indirect mechanisms through which strategies designed to avoid or punish corruption may fail to achieve foreign policy objectives.

---

<sup>29</sup>Of course, this assumes that popular resistance is an important stage of democratization, which may not always be true. Future research might consider the conditions under which autocratic regimes may be most vulnerable to domestic unrest to test for a conditional effect of bypass aid on regime failure. However, the limited availability of data on bypass aid makes it difficult to get leverage on this question given the rarity of regime failure.

The theory and empirical results suggest a few interesting extensions. First, although the model does not account for bargaining over aid-for-policy deals (e.g., Bueno de Mesquita and Smith 2009), it suggests that bypass aid might facilitate such deals through a mechanism different from what Bermeo (2016) suggests if bypass aid helps autocrats survive. Perhaps constituents in democratic donor countries do care about helping the poor (e.g., Milner 2006), but donors understand the strategic implications of bypass aid in deciding to delegate to multilateral or nongovernmental organizations. Alternatively, endogenizing the choice of aid channels by donors would be an interesting extension to the framework here if we assume donors are concerned with promoting democracy. Indeed, democracy-minded donors might withhold bypass aid in periods that are favorable to revolution. In fact, the findings here suggest that the decision to bypass should be endogenous to the political effects of bypassing. Second, treating bypass aid as a strategic resource for authoritarian governments points to a potential explanation for attacks on aid groups by rebel movements. If aid depresses the incentive to revolt, then groups fighting for political change may seek to expel aid groups to boost citizens' motivations to resist an incumbent regime. Future research should address these questions. For now, the model and results here provide reasons to think that bypass aid is not politically neutral.

## 2.7 Appendix for “Bypass Aid and Unrest in Autocracies”

### 2.7.1 Extensions to Model

#### 2.7.1.1 Probability of Revolution

The probability of revolution in the model is the probability that the optimal transfer  $x^*$  is less than the resources available to the government, which can be written as

$$\Pr(x^* < 1 + (1 - \beta)a), \quad (2.6)$$

or

$$\Pr(c_L < \delta(q - 1)(a(\beta - 1) - 1) - a\beta). \quad (2.7)$$

Since  $c_L \sim U[0, 1]$ , this is equivalent to  $\delta(q - 1)(a(\beta - 1) - 1) - a\beta$ , which is decreasing in  $\beta$ .

#### 2.7.1.2 What If Bypass Aid Makes Revolution Easier?

An important stylized fact to account for is that bypass aid may actually help sustain rebel groups. That is, while it may remove a source of grievance, aid may simultaneously empower or keep afloat groups with the capability to challenge the government. Sometimes this may even be a strategic objective of donors. For example, following the Russian Civil War, the Hoover administration in the United States tried to use humanitarian aid to undermine the Bolsheviks:

“[B]y creating a food and medicine distribution system outside the control of the Soviet government, Hoover hoped to establish an alternative power center through which the loyalty of the recipient populations, weakened as they were at the end of the civil war by massive starvation and disease, might be turned against the Bolsheviks” (Belgrad 1997, 4)

The baseline model assumes that an attempted revolution succeeds with certainty. I now relax this assumption to account for the possibility of a failed revolution, which also allows me to account for the fact foreign aid may affect the probability of a revolution succeeding.<sup>30</sup> Let  $\rho(\beta) \in (0, 1)$  denote the probability that a revolution is successful, and let  $1 - \rho(\beta)$  denote the probability of failure. Let  $\rho(\beta)$  be an increasing, concave function of the proportion of bypass aid.<sup>31</sup> For now I assume that in the case of success the resources of the state are available to be evenly divided across the population forever, minus the costs of revolution. If revolution fails, the population gets nothing forever. I also assume that the government can decide how much aid to allow into the country up to some exogenously determined threshold,  $\hat{a}$ . Think of this as the maximum amount of aid that the international community would be willing to contribute.

Now the population chooses not to revolt if

$$\frac{a\beta + \delta(q-1)x + x}{1 - \delta} = \frac{\rho(\beta)(a(1 - \beta) - c_L + 1)}{1 - \delta}. \quad (2.8)$$

The value of  $x$  that will buy the population off is now

$$\hat{x} = \frac{\rho(\beta)(a(-\beta) + a - c_L + 1) - a\beta}{\delta(q-1) + 1}. \quad (2.9)$$

The derivative of this optimal  $\hat{x}$  with respect to  $\beta$  is

$$\frac{\partial \hat{x}}{\partial \beta} = -\frac{a\rho(\beta) + \rho'(\beta)(a(\beta - 1) + c_L - 1) + a}{\delta(q-1) + 1}, \quad (2.10)$$

where  $\rho'(\beta)$  denotes the first derivative of the probability that the revolution

---

<sup>30</sup>Additionally, governments may host humanitarian aid operations that pose threats to their neighbors. Polman (2010, 111) details a number of international aid efforts that inadvertently financed rebel groups (“refugee warriors”) challenging one or more neighboring governments.

<sup>31</sup>Note that the model implicitly accounts for the possibility that government aid increases the odds that the government wins in a revolutionary contest, since as the proportion of bypass aid decreases, the probability that the government wins ( $1 - \rho(\beta)$ ) increases.

succeeds with respect to  $\beta$ . Now the optimal  $\hat{x}$  is decreasing in  $\beta$  only if

$$\rho'(\beta) \leq \rho'(\beta)^* \equiv -\frac{a(\rho(\beta) + 1)}{a(\beta - 1) + c_L - 1}, \quad (2.11)$$

which is always positive. This threshold is less than 1 (i.e., it is possible that the relationship between bypass aid and the optimal offer may be negative or positive) if the condition  $a(\beta + \rho(\beta)) + c_L - 1 > 0$ . This highlights that governments may allow aid to bypass them even when it potentially increases the likelihood of a successful revolution because of the effects that it has on undermining the incentive for revolution.

## 2.7.2 Countries Included in Analysis

Table 2.7 reports the country names (sometimes abbreviated) used in the analysis as they appear in the Geddes, Wright and Frantz (2014) data.



Table 2.7: Temporal and Spatial Domain

	Country	Year(s) in Data		Country	Year(s) in Data
1	Afghanistan	2010	32	Madagascar	2010
2	Algeria	2005 - 2010	33	Malaysia	2005 - 2010
3	Angola	2005 - 2010	34	Mauritania	2005 - 2010
4	Armenia	2005 - 2010	35	Morocco	2005 - 2010
5	Azerbaijan	2005 - 2010	36	Mozambique	2005 - 2010
6	Bangladesh	2008 - 2008	37	Myanmar	2005 - 2010
7	Belarus	2005 - 2010	38	Namibia	2005 - 2010
8	Botswana	2005 - 2010	39	Nepal	2005 - 2006
9	Burkina Faso	2005 - 2010	40	Oman	2005 - 2010
10	Cambodia	2005 - 2010	41	Pakistan	2005 - 2008
11	Cameroon	2005 - 2010	42	Rwanda	2005 - 2010
12	Cen African Rep	2005 - 2010	43	Saudi Arabia	2005 - 2010
13	Chad	2005 - 2010	44	Singapore	2005 - 2010
14	China	2005 - 2010	45	Sudan	2005 - 2010
15	Congo-Brz	2005 - 2010	46	Swaziland	2005 - 2010
16	Congo/Zaire	2005 - 2010	47	Syria	2005 - 2010
17	Cuba	2005 - 2010	48	Tajikistan	2005 - 2010
18	Egypt	2005 - 2010	49	Tanzania	2005 - 2010
19	Eritrea	2005 - 2010	50	Thailand	2007
20	Ethiopia	2005 - 2010	51	Togo	2005 - 2010
21	Gabon	2005 - 2010	52	Tunisia	2005 - 2010
22	Gambia	2005 - 2010	53	Turkmenistan	2005 - 2010
23	Guinea	2005 - 2010	54	Uganda	2005 - 2010
24	Iran	2005 - 2010	55	United Arab Emirates	2005 - 2010
25	Ivory Coast	2005 - 2010	56	Uzbekistan	2005 - 2010
26	Jordan	2005 - 2010	57	Venezuela	2006 - 2010
27	Kazakhstan	2005 - 2010	58	Vietnam	2005 - 2010
28	Kuwait	2005 - 2010	59	Yemen	2005 - 2010
29	Kyrgyzstan	2005 - 2010	60	Zambia	2005 - 2010
30	Laos	2005 - 2010	61	Zimbabwe	2005 - 2010
31	Libya	2005 - 2010			

### 2.7.3 Correlations Between Independent Variables

Table 2.8: Correlations Between Independent Variables

	Aid Scandals	Civil Conflict	Governance	Natural Disasters	Bypass Share (%)
Aid Scandals	1	-0.011	0.003	-0.036	0.231
Civil Conflict	-0.011	1	-0.313	0.154	0.339
Governance	0.003	-0.313	1	-0.056	-0.462
Natural Disasters	-0.036	0.154	-0.056	1	0.042
Bypass Share (%)	0.231	0.339	-0.462	0.042	1

#### 2.7.4 Measure of Unrest Excluding Riots

Following Kono, Montinola and Verbon (2013), the dependent variable used in the main text is a count of strikes involving more than 1,000 people directed at the government or national policies, riots involving more than 100 people, and anti-government demonstrations involving more than 100 people from Banks (2011). Since the theory emphasizes actions that would challenge a government specifically and the count of riots does not specify that the riots need to have anything to do with government policies, it is important to ensure that the main result does not depend on the inclusion of riots in the dependent variable. (Additionally, from a normative perspective, it might be desirable for bypass aid to reduce unrest if the unrest events it affects are riots.) Table 2.9 reports the results of rerunning the first two models in Table 2.2 while excluding riots from the count variable. In both models the results are consistent with those reported in the main text.

Table 2.9: OECD Aid Channels and Domestic Unrest (Excluding Riots), 2005-2010

	<i>Dependent variable:</i>	
	(1)	(2)
Bypass Share	-0.027** (0.012)	-0.034** (0.016)
Governance Index	-1.107*** (0.248)	-1.282*** (0.273)
Civil Conflict	0.336 (0.290)	0.283 (0.299)
Natural Disasters	0.115*** (0.032)	0.108*** (0.030)
Lagged Unrest	0.109 (0.067)	0.121* (0.063)
Constant	0.767 (0.545)	1.258** (0.598)
Observations	338	338
Year-fixed Effects?	No	Yes
Log Likelihood	-218.452	-216.496
$\theta$	0.229*** (0.054)	0.241*** (0.057)
Akaike Inf. Crit.	448.905	454.992

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

Negative binomial regression models.

Heteroskedasticity-consistent standard errors in parentheses.

## 2.7.5 Logit Models for Dichotomous Unrest Variable

To ensure that the results of the count models are not driven by outlying cases with many unrest events, Table 2.10 reports the results of the key models from Table 2.2 in the main text where the dependent variable is a dummy variable that takes on a value of “1” when there is at least one unrest event in the data. In both models, the relationship between bypass aid and unrest is negative and statistically significant.

Table 2.10: OECD Aid Channels and Domestic Unrest (Dichotomous Dependent Variable), 2005-2010

	<i>Dependent variable: Unrest Dummy</i>	
	(1)	(2)
Bypass Share	-0.023** (0.011)	-0.028** (0.013)
Governance Index	-0.951*** (0.308)	-1.044*** (0.330)
Civil Conflict	-0.149 (0.287)	-0.152 (0.301)
Natural Disasters	0.150*** (0.056)	0.162*** (0.059)
Lagged Unrest	0.388*** (0.112)	0.422*** (0.117)
Constant	0.141 (0.666)	0.506 (0.710)
Year-fixed Effects?	No	Yes
Observations	338	338
Log Likelihood	-139.922	-137.142
Akaike Inf. Crit.	291.843	296.284

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

Logit regression models. Estimated standard errors in parentheses.

## 2.7.6 Zero-inflated Negative Binomial Regression

To account for the possibility of excessive zeroes in the dependent variable, Table 4.8 reports the results of two negative zero-inflated binomial regression models where the inflation variable is a factored version of the country code variable. (I omit the coefficients on the model of the probability of observing a zero count.)

Table 2.11: Zero-inflated Negative Binomial Regression Models

	<i>Dependent variable: Count of Unrest Events</i>	
	(1)	(2)
Bypass Share	-0.020* (0.010)	-0.034*** (0.012)
Governance Index	-0.313 (0.362)	-0.778* (0.406)
Civil Conflict	0.401* (0.235)	0.460* (0.238)
Natural Disasters	0.043** (0.019)	0.050** (0.020)
Lagged Unrest	-0.002 (0.039)	-0.024 (0.040)
Constant	1.295* (0.706)	2.138*** (0.818)
Year Dummies	N	Y
Observations	338	338
Log Likelihood	-222.704	-219.802

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

Standard errors in parentheses.

Zero-inflated negative binomial regression models.

### 2.7.7 Aid Scandal Cases

Table 2.12 reports the basic case information for the aid scandals used in the construction of the instrumental variable. A complete version of the data with full citation information will be available on the author’s website. I conducted the data collection in three rounds as follows: On the first round, I used *Lexis Nexis Academic* to search for “corruption AND foreign aid” in the category of newspapers from 1990 to 2014, sorted the results by relevance, and then manually sifted through the results to identify and record possible scandals. In the second round, I repeated this process but with the search phrase “aid AND misus\* AND government.” In the third round of data collection, I again searched for “aid AND misus\* AND government,” but did a separate search for each individual year from 1990 to 2014. Note that the observations are scandals (or minor scandals), not news stories about scandals, even while they are based on news stories.

Table 2.12: Cases from Aid Scandal Data, 2003-2008

Number	Year	Month	Day	Country	Major?
1	2003	10	18	Philippines	0
2	2003	10	19	Zimbabwe	0
3	2004	2	24	Iraq	1
4	2004	7	14	Haiti	0
5	2005	1	13	Indonesia	0
6	2005	2	13	Sri Lanka	1
7	2005	7	6	Nigeria	0
8	2005	12	7	Malawi	0
9	2006	1	22	Cambodia	0
10	2006	5	7	Vietnam	0
11	2006	6	1	Uganda	1
12	2006	10	4	Macedonia	1
13	2007	2	20	Colombia	1
14	2007	7	31	Iraq	0
15	2007	8	20	Zimbabwe	1
16	2007	12	27	Pakistan	0
17	2008	2	6	Afghanistan	1
18	2008	2	11	Indonesia	1
19	2008	3	26	Afghanistan	0
20	2008	5	8	Myanmar	1
21	2008	6	30	Pakistan	1
22	2008	7	4	Pakistan	1
23	2008	7	19	Bulgaria	1
24	2008	7	27	Costa Rica	1
25	2008	9	23	Iraq	1
26	2008	10	15	Tajikistan	0
27	2008	11	3	Zimbabwe	1

### 2.7.8 Evaluating Exclusion Restriction

While the requirement that the instrumental variable be strongly correlated with the endogenous variable of interest is straightforward to evaluate, it is more difficult to ensure that the aid scandals do not affect unrest through alternate channels. As discussed above, there are a few potential mechanisms through which outside aid scandals could potentially affect domestic unrest independently of their effects on bypass aid. In the main text I argued that these threats should not be serious theoretically



given that one of the key instrumental variables is a count of aid scandals that occurred outside country  $i$ 's continent in year  $t - 2$ . Here I perform some statistical tests to evaluate those potential violations of the exclusion restriction.

Table 2.13 presents the results of a series of bivariate models. One concern is that aid scandals might affect overall aid amounts, which could affect a government's repressive capacity. However, there are equally plausible arguments for both a positive and negative effect here. On one hand, aid scandals might lead donors to reduce overall aid flows in light of evidence of misuse. On the other hand, donors may only reduce aid to the country experiencing an aid scandal, reallocating fixed aid budgets to other countries and regions. In any case, while aid scandals are strongly correlated with the channel distribution of aid in a given country, Models 1 and 2 in Table 2.13 shows that they do not appear to affect total aid amounts.

Models 3 and 4 shows that there appears to be no direct effect of aid scandals on unrest, and Model 5 shows (perhaps surprisingly) that there is no effect of aid scandals in country  $i$  on unrest in country  $i$ . Model 6 shows that outside unrest is weakly correlated with less unrest in country  $i$ , and Model 7 suggests that unrest outside a given country's continent is not associated with domestic unrest. A possible explanation for the result in Model 6 is that autocrats increase repression when outside countries experience unrest to deter unrest, but since there is no evidence that scandals in country  $i$  affect unrest in that country (Model 5), this concern should not apply to aid scandals.

#### *Implementing Conley, Hansen and Rossi's (2012) "Plausibly Exogenous" Test*

The tests in the previous section seek to identify potential theoretical mechanisms through which aid scandals might affect unrest outside of their effects on the distribution of bypass aid. On balance, it seems reasonable that aid scandals would serve as an exogenous source of variation in aid channel distribution.

Others have outlined procedures for formally relaxing the exclusion restriction

Table 2.13: Evaluating Exclusion Restriction

	<i>Dependent variable:</i>						
	DV: Total Aid		DV: Unrest Events				
	<i>OLS</i>		<i>Negative binomial</i>				
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Major Aid Scandals	16.195 (19.961)		-0.024 (0.054)				
Major Aid Scandals Outside Continent		31.847 (26.259)		-0.008 (0.070)			
Scandals in Country					-1.864 (1.295)		
Outside Unrest						-0.031* (0.018)	
Unrest Outside Continent							-0.005 (0.019)
Constant	734.114*** (77.673)	721.415*** (73.893)	-0.353* (0.206)	-0.398** (0.197)	-0.386** (0.159)	0.678 (0.660)	-0.310 (0.443)
Observations	344	344	344	344	344	344	344
R <sup>2</sup>	0.002	0.004					
Adjusted R <sup>2</sup>	-0.001	0.001					
Log Likelihood			-323.800	-323.892	-322.810	-322.211	-323.865
$\theta$			0.140*** (0.024)	0.140*** (0.024)	0.143*** (0.024)	0.146*** (0.025)	0.140*** (0.024)
Akaike Inf. Crit.			651.599	651.784	649.620	648.421	651.729
Residual Std. Error (df = 342)	1,104.885	1,103.578					
F Statistic (df = 1; 342)	0.658	1.471					

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

(Conley, Hansen and Rossi 2012) in the context of two-stage least squares estimation (2SLS). Implementing those procedures here is messy for a number of reasons. First, the models in the main paper use different estimators in each stage of the analysis to accommodate the count structure of the outcome variable (unrest events). I use OLS to estimate the relationship between the various measures of aid scandals and the logged bypass ratio, and then negative binomial regression to estimate the relationship between bypass aid and a count of unrest events. To adjust for the incorrect standard errors in the second stage of this procedure, I rely on bootstrapped coefficients from the second stage to draw inferences. Second, the analysis in the main paper transforms the predictions for the first stage dependent variable (logged bypass ratio) before including it as a covariate in the second stage. The transformation of the dependent variable is intended to make the dependent variable continuous following Dietrich (2013). When transforming the predictions from these models back into the [0,100]

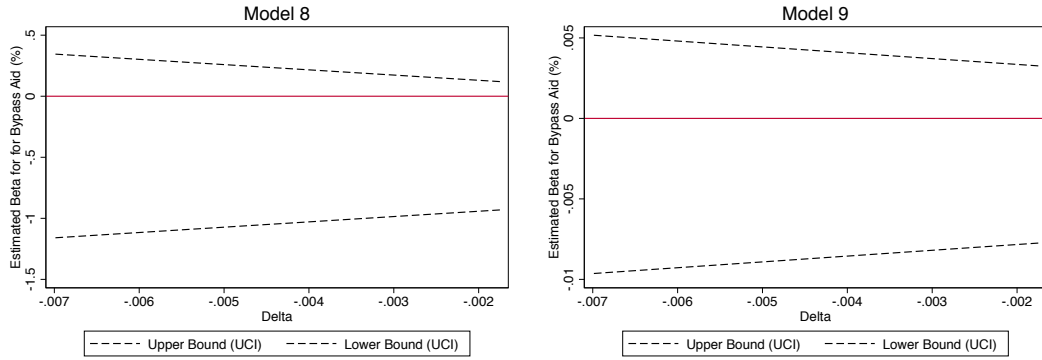


Figure 2.4: Union of Confidence Intervals Approach to Relaxing Exclusion Restriction.

interval, the models generate predictions that lie in that interval. This is not the case when using the raw percentage variable, which in the prediction stage can result in negative values or values above 100. For these reasons, the main analysis does not implement 2SLS.

Nonetheless, I implemented the procedures in Conley, Hansen and Rossi (2012) using modified versions of two of the models in the main text (Models 8 and 9). I use the percentage of bypass aid as the endogenous regressor in the first stage in all three models. The second stage outcome variable is a dummy variable indicating whether or not at least one unrest event occurred. I estimated robust standard errors using Stata’s `vce(robust)` command. The procedures outlined in Conley, Hansen and Rossi (2012) allow for researchers to relax the exclusion restriction in an instrumental variables model by introducing a parameter  $\gamma$  that represents any direct effect that the excluded instrument may have on the second-stage outcome variable (in this case, any direct effect that aid scandals could have on the likelihood of unrest.) The user specifies the minimum and maximum bounds for  $\gamma$ , call these  $\gamma_L$  and  $\gamma_H$ . I set  $\gamma_L=0$  for all three models, and set  $\gamma_H$  equal to the estimated coefficient from a regression of the dichotomous unrest variable on each of the aid scandals measures. This value is -.007 for both Model 8 and Model 9.

Figure 2.4 reports the results of this procedure including the instruments used in Models 8 and 9 from the main text along with the control variables included in those models. In all three cases, the mean of the estimated coefficient on the logged bypass ratio is negative, though the 80% union of confidence intervals includes zero in all three cases. As the assumption of no direct effect on the outcome is relaxed, the confidence intervals widen slightly. Again, a direct comparison with the results in the paper is not possible given the different methods employed in each case. That is, the models used in the “plausibly exogenous” test are not the same as those reported in the main paper, and are less appropriate for evaluating the key hypothesis given the nature of the data. Although the results are weaker when using this alternative framework, it is at least encouraging that these different design choices produce results that point substantively to the conclusions in the main analysis.

Ultimately the exogeneity of the excluded instrument needs to be justified on theoretical grounds, and the previous section identifies some potential threats to inference and shows that they are likely unwarranted. Further, the results in the main text show that the association between bypass aid and unrest is robust to a variety of alternate research design choices in ways that are consistent with the theory. Indeed, the main analysis explicitly seeks to control for confounders, and, as noted in the main text, the main results are also robust to controlling for military expenditures.

### 2.7.9 Bypass Aid and Investment in Repressive Capacity

The theory suggests that government control of aid is not a necessary condition for aid to depress the incentive to revolt. Still, it is possible that bypass aid would influence unrest not through benefitting citizens, but rather because recipient regimes intercept or tax bypass aid and use these resources to invest in greater repressive capacity. In Table 2.14 I regress (logged) changes in military expenditures as a percent of government spending from the World Bank from year  $t$  to  $t - 1$  on bypass aid. Model 1 uses the proportion of bypass aid as the key independent variable. Model 2 uses the logged total of bypass aid in millions of US dollars, controlling for logged government aid. Since interstate crises and civil conflicts might affect both bypass aid and military expenditures, I control for a count of militarized interstate disputes and civil conflicts in the previous year. The count of the number of militarized interstate disputes (MIDs) that a country was involved in during the previous year is based on data from the Correlates of War project (Palmer et al. 2015). (The results do not depend on the inclusion of these variables.) To account for unobserved heterogeneity, I include year-fixed effects. In both models, the relationship between bypass aid and changes in military expenditures is negative, though not statistically significant. This is consistent with the theory. If bypass aid provides a service that benefits people, then autocratic regimes need not invest in greater repressive capacity.

Table 2.14: Bypass Aid and Change in Military Expenditures (% Gov. Spending)

<i>Dependent variable: Change in Military Expenditures</i>		
	(1)	(2)
Bypass Share	-0.020 (0.021)	
Total Bypass Aid (log)		-0.145 (0.169)
Total Government Aid (log)		0.190 (0.176)
International Disputes	0.213 (0.250)	0.200 (0.240)
Civil Conflict	-0.533 (0.642)	-0.657 (0.623)
Constant	-0.341 (0.363)	-0.513 (0.512)
Year Dummies	Y	Y
Observations	140	140
R <sup>2</sup>	0.055	0.054
Adjusted R <sup>2</sup>	0.005	-0.003
Residual Std. Error	2.425 (df = 132)	2.436 (df = 131)
F Statistic	1.107 (df = 7; 132)	0.940 (df = 8; 131)

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).  
 White's (1980) HC1 standard errors in parentheses.  
 OLS estimates.

#### 2.7.10 Validity Check for *AidData* Measure

A preliminary validity check suggests that the coding procedure used to construct the alternate measure of bypass aid from the *AidData* project succeeds in identifying cases where bypass is likely. In a random sample of 50 cases where the bypass indicator takes a value of “1,” all of the coded projects could plausibly bypass the government. The unique purpose codes in the sample are: “Democratic participation and civil society, Material relief assistance and services, Strengthening civil society, Support to international NGOs, Emergency food aid, Food crop production, Support to national ngos, Food aid/Food security programmes, Food security programmes/-food aid, Support to local and regional ngos.” Of course, there is still potential for measurement error. For example, it could be the case that a donor gives aid for the purpose of “Support to national ngos,” but does so through the recipient government, and the aid never makes it to NGOs (or never makes it to citizens). As discussed in the main text, this is the same problem scholars face in using the OECD data. Still, relative to aid labeled as “budget support,” we should be confident that a great deal of what this measure picks up is in fact bypassing the government.

### 2.7.11 Alternative Operationalizations of Bypass Aid with OECD Data

Table 2.15 reports the results of models that use alternative operationalizations of bypass aid. Model 1 calculates the bypass share variable excluding aid delivered through public-private partnerships. Model 2 uses a measure of bypass aid as a percentage of real GDP output. As in the main text, bypass aid is statistically significant and negatively associated with unrest events.

Table 2.15: Alternate Measures of Bypass Aid and Unrest, 2005-2010

	<i>Dependent variable: Unrest events</i>	
	(1)	(2)
Bypass Share (Excluding Public-Private Partnerships)	-0.038*** (0.014)	
Bypass Aid as % GDP		-0.839* (0.479)
Governance Index	-1.482*** (0.245)	-1.183*** (0.280)
Civil Conflicts	0.130 (0.281)	0.338 (0.412)
Natural Disasters	0.122*** (0.026)	0.121*** (0.025)
Lagged Unrest	0.153*** (0.057)	0.096* (0.055)
Constant	2.243*** (0.542)	1.703*** (0.630)
Year Dummies	Y	Y
Observations	338	295
Log Likelihood	-279.085	-262.536
$\theta$	0.222*** (0.043)	0.268*** (0.055)
Akaike Inf. Crit.	580.171	547.071

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

White's (1980) HC1 standard errors in parentheses.

Negative binomial regression models.



### 2.7.12 Controlling for Continent, Region, and Recipient Country

To account for unobserved heterogeneity that might affect the results in ways not captured by the included covariates, Table 2.16 reports the results of the main model when controlling for continent-, region, and country-fixed effects. Across all three models, the relationship between bypass aid and unrest is negative and statistically significant.

Table 2.16: Continent-, Region-, and Country-fixed Effects Models, 2005-2010

	<i>Dependent variable: Count of Unrest Events</i>		
	(1)	(2)	(3)
Bypass Share	-0.040*** (0.014)	-0.040*** (0.013)	-0.030** (0.013)
Governance Index	-1.473*** (0.249)	-1.348*** (0.339)	-3.018** (1.306)
Civil Conflicts	0.178 (0.288)	0.105 (0.334)	0.694** (0.353)
Natural Disasters	0.130*** (0.026)	0.130*** (0.031)	0.026 (0.059)
Lagged Unrest	0.158*** (0.060)	0.051 (0.057)	-0.102** (0.043)
Constant	2.223*** (0.584)	2.173* (1.217)	5.406* (2.808)
Year Dummies	Y	Y	Y
Continent Dummies	Y	N	N
Region Dummies	N	Y	N
Country Dummies	N	N	Y
Observations	338	338	338
Log Likelihood	-278.550	-266.173	-214.403
$\theta$	0.224*** (0.043)	0.319*** (0.069)	0.958*** (0.255)
Akaike Inf. Crit.	585.100	578.346	570.806

\*p<0.1; \*\*p<0.05; \*\*\*p<0.01 (Two-tailed tests).

White's (1980) HC1 standard errors in parentheses.

Negative binomial regression models.

## Chapter 3

### The Design of International Institutions for Humanitarian Aid

Need often comes second to politics in explaining patterns of both development and emergency aid (Alesina and Dollar 2000; Neumayer 2003; Drury, Olson and Van Belle 2005; Dollar and Levin 2006; Strömberg 2007; Fink and Redaelli 2011; Dreher, Fuchs and Nunnenkamp 2013). Many studies find that “colonial links, alliances, [and] strategic interests” drive donors’ foreign aid policies (Alesina and Dollar 2000, 34). A recent review of the foreign aid literature remarks that, “the finding that strategic concerns of the donor countries are important driving forces persists across studies of different contexts, data, and methods such that there is a reasonable consensus that foreign aid is often unrelated to the needs of the recipient country” (Qian 2015, 280). This is no less true of humanitarian aid and disaster relief (e.g., Drury, Olson and Van Belle 2005; Strömberg 2007; Fink and Redaelli 2011).<sup>1</sup>

Policy makers and aid agencies are well aware of this problem. In an attempt to distribute aid more equitably, the United Nations’ Office for the Coordination of Humanitarian Affairs formed the Central Emergency Response Fund (CERF) in 2006 as a “pooled relief fund.” The CERF is unique as an aid organization in that it collects contributions before humanitarian crises occur, so donors cannot give or withhold aid based on whether or not they want to help a particular country. Instead, states contribute to the CERF, and the organization allocates aid where it is needed.<sup>2</sup> Unlike many of its development-oriented counterparts (e.g., multilateral development

---

<sup>1</sup>Of course, aid may be genuinely humanitarian and strategic in cases where donors perceive a strategic interest in improving welfare. For instance, former US President George W. Bush intensified the fight against AIDS in Africa partly because of his belief that hopelessness and underdevelopment is a root cause of terrorism (Bush 2010, 336).

<sup>2</sup>The CERF website highlights a number of independent reports that suggest that this is, in fact, how the organization operates. I provide a novel test in the Appendix that suggests further support for this claim.

banks), states are not allowed to earmark their contributions to the CERF. In 2013, about 60 states contributed a total of \$466 million to the organization. Unfortunately, CERF aid as a proportion of global humanitarian aid has failed to exceed five percent since the organization's founding.

This paper argues that the key institutional design feature of the CERF – the complete banning of earmarking – may limit the effectiveness of the organization by reducing its capacity. I analyze a game of emergency aid provision between a potential aid donor and an expert, altruistic aid organization.<sup>3</sup> The aid organization is altruistic in that it always wants to minimize the negative impacts of some exogenous crisis affecting a recipient country. I compare a donor's aid policy and recipient welfare outcomes across three states of the world: a world without the specialized organization, a world where the organization exists but bans earmarking, and a world where the organization allows "limited" earmarking. I define a limited earmark as a policy granting the organization complete control over a donor's aid if some pre-specified observable condition applies in a particular case. The policy differs from a traditional earmark in that if the condition is not met, the organization can still use part of the donor's contribution. I show that relative to a complete ban on earmarking, giving donors the option of a limited earmark not only elicits larger contributions to the pooled fund but can improve average outcomes for aid recipients. Intuitively, donors contribute more to the pooled fund when they are able to place restrictions on their aid. When the institution operates with a limited budget, crisis victims who are unlikely to receive bilateral assistance often fare better when the organization allows partial earmarking. As such, the limited earmarking option improves outcomes in precisely those situations that the organization is most interested in addressing: emergencies that are, or are likely to be, underfunded.

The models also illustrate some interesting dynamics regarding the role of shared

---

<sup>3</sup>I refer to the aid organization as an "organization," "institution," and "pooled fund" interchangeably throughout the paper.

preferences in principal-agent relationships. Specifically, the analysis highlights that an altruistic pooled fund should anticipate that donors who care most about providing emergency relief will be willing to act unilaterally so long as doing so is not too costly. The pooled fund not only channels resources to underfunded emergencies, but also gives fewer resources to crisis situations that it anticipates will receive bilateral funding. It is this incentive that creates a conundrum for the organization and for efficient donors. With the exception of donors that are very inefficient at providing aid, donors that generally share the organization's preference for reducing humanitarian crises and would like to reap efficiency gains by cooperating find that the organization provides less aid the more likely it is that a donor would act bilaterally in the organization's absence.

This paper makes a number of contributions. First, the paper provides an answer to a puzzle in the literature on strategic explanations for foreign aid. If states typically give aid for strategic reasons, why do they sometimes delegate *ex ante* to pooled funds? The analysis suggests that the potential gains in efficiency from working through an institution can be enough for self-interested, rational states to contribute to a pooled fund even when it is possible that the aid will be used to help states that they would otherwise avoid helping. Second, it contributes to the literature on delegation to international organizations (e.g., Hawkins et al. 2006; Milner 2006; Milner and Tingley 2012). Most work that considers the choice of bilateral versus multilateral aid still presupposes that states can give aid on a case-by-case basis (e.g., Milner 2006; Raschky and Schwindt 2012).<sup>4</sup> The theory here accounts for cases where states may choose delegation *ex ante*. Third, the paper contributes to

---

<sup>4</sup>Though the choice of bilateral versus multilateral delivery of aid may depend on a number of factors, the choice to give aid in the first place is still often influenced by at least some political or strategic motive. Accordingly, it is not always the case that multilateral organizations provide aid based solely on need. See Neumayer (2003) and Dollar and Levin (2006). There are, of course, good reasons for states to work through multilateral organizations – e.g., to avoid incurring the transaction costs of setting up a new aid project for every crisis – but it is not clear why states would want to commit to this solution in advance when it seems they could just as easily deliver aid multilaterally on a case-by-case basis. See, for example, Abbott and Snidal (1998).

the formal literature on international institutional design and the “perverse” effects of international organizations (Gilligan and Johns 2012, 240). The analysis suggests that completely banning earmarking, though motivated by a desire to improve welfare outcomes, may limit the effectiveness of the crisis relief regime. Finally, by focusing on an institution that is designed to provide relief from natural disasters, it speaks to the literature on designing institutions to adapt to the effects of climate change (Haas, Keohane and Levy 1993; Biermann and Boas 2010; Keohane and Victor 2011; Tir and Stinnett 2012). Considering how to design institutional mechanisms to prevent or alleviate suffering in countries that are unable to manage the effects of natural disasters by themselves is increasingly important given that climate change is expected to increase the frequency and severity of natural disasters (Helmer and Hilhorst 2006; Schneider et al. 2007; Van Aalst 2006; Oppenheimer et al. 2014).

The paper proceeds as follows. In the next section, I briefly review the history of international efforts to provide effective emergency aid. I argue that while the CERF appears to succeed in providing relief impartially, the aid it provides amounts to a marginal proportion of humanitarian aid. I then develop and present the formal models and discuss some of their key results, as well as the intuition behind their equilibria. Finally, I use a simulation to illustrate the potential gains to recipient welfare from allowing limited earmarking.

### 3.1 The Crisis Relief Regime

Efforts to construct an international regime for humanitarian disaster and crisis relief date back half a century.<sup>5</sup> Throughout the 1960s, the United Nations (UN) called for coordinated international action to respond to disasters.<sup>6</sup> In 1971, the

---

<sup>5</sup>The classic definition of an international regime is provided by Stephen Krasner: “International regimes are defined as principles, norms, rules, and decision-making procedures around which actor expectations converge in a given issue-area” (Krasner 1982, 185).

<sup>6</sup>Unless otherwise noted, the history of the disaster regime is based on the account provided on the UNISDR’s website, available online at <http://www.unisdr.org/who-we-are/history>.

UN created the Disaster Relief Office (UNDRO), which initially served as a forum for disseminating technical standards for reducing disaster risk and helping states coordinate domestic policies. The UN continued appealing for technical cooperation and coordination of natural disaster early warning systems throughout the 1970s and 1980s, and it declared the 1990s the International Decade for Natural Disaster Reduction. In 1994, the UN General Assembly convened the World Conference on Natural Disaster Reduction. In doing so, the UN sought to raise global awareness of the disproportionate impact of natural disasters in the developing world in the hopes of spurring North-South cooperation. The UN's International Strategy for Disaster Reduction (UNISDR) became a part of the UN secretariat in 1999 as the institutional body responsible for continuing the agenda of the 1990s and coordinating international efforts to increase disaster preparedness. Following the Indian Ocean Tsunami of 2005, the second World Disaster Reduction Conference produced the Hyogo Framework for Action (HFA), which outlined how public and private sectors could coordinate at the domestic level to reduce disaster vulnerability.<sup>7</sup> The HFA called for the building of institutional capacity at the domestic level to implement policies consistent with disaster risk reduction and to enhance the technical ability of states to monitor and warn vulnerable populations of impending crises.

While much of this agenda focuses on promoting crisis preparedness at the domestic level, it also involves an international component. In 2006, the UN established the Central Emergency Response Fund (CERF) in Resolution 60/124 as the successor organization to the Central Emergency *Revolving* Fund. Riddell notes that,

“[t]he establishment of the CERF aims to remove two of the main problems of the prevailing system – the discretionary funding of emergencies on a case-by-base basis and the growing practice of donors earmarking their funds to specific agencies and purposes to create a cumbersome, and at

---

<sup>7</sup>The text of the Hyogo Framework is available online at <http://www.unisdr.org/we/coordinate/hfa>.

times almost unworkable, bureaucratic system” (Riddell 2007, 323-324).

The innovation and appeal of the CERF is that it designed to respond to crises as they develop. The institution aims to complement other forms of humanitarian assistance, operating mainly by funding specialized UN development and aid agencies that have the capacity and expertise required to respond to disasters quickly. For example, when Hurricane Agatha hit Guatemala in 2010, the Guatemalan government requested assistance from CERF, and CERF responded by distributing funds to a number of specialized organizations, including the World Health Organization, the World Food Programme, and UNICEF (Berry-Koch 2012). These organizations coordinated and provided humanitarian relief in their respective areas of specialty in concert with international and national nongovernmental organizations.

### 3.1.1 Is the Regime Working?

This regime seeks to distinguish crisis relief from the broader development agenda and prioritize it in a world where humanitarian crises are expected to increase in frequency and severity. It aims to prevent donors’ political interests from determining which countries do or do not receive emergency aid by constructing an impartial institution with the capacity to respond to crises quickly and efficiently. As such, an assessment of the regime in terms of its needs to consider two questions. First, do regime agents provide aid in an impartial, need-based manner? Second, does the capacity of the regime allow it to have an important impact on outcomes? Regarding the first question, the overarching goal and purpose of CERF is “to make funding for emergencies more equitable and less contingent on donor predilections” (Taylor and Stoddard 2012, 7). Evaluating whether the CERF works as advertised is difficult. One way to measure its efficacy is to consider changes in the variance of funding coverage before and after the CERF was created. One report finds that before the CERF’s creation, “variance in funding coverage across emergencies was significantly higher



than in the post-CERF years,” and that variance in funding across sectors decreased in the post-CERF period (Taylor and Stoddard 2012, 7). The authors argue that “it is reasonable to conclude that CERF allocations... directly contributed to the increased equitability observed in overall humanitarian funding since 2006” (Taylor and Stoddard 2012, 9). This report does not rule out that existing explanations for aid allocation do not similarly motivate CERF aid, but evaluating existing theories of foreign aid allocation in the context of the CERF is difficult because it is a pooled fund. As such, it is not possible to test for whether variables like dyadic colonial ties and alliances between donors and recipients affect aid allocation since we only observe transfers of aid from the organization to recipients.<sup>8</sup> On balance, however, it seems reasonable to take at face value the organization’s claim of impartiality in delivering humanitarian assistance.<sup>9</sup>

On the second question of whether the efforts of the CERF have had a substantial impact on improving relief outcomes, the evidence is less encouraging. CERF aid represents a very small proportion of overall humanitarian aid. Figure 3.1 plots the proportion of total humanitarian provided by the CERF from its inception in 2006 to 2011.<sup>10</sup> Throughout this period CERF aid never exceeded five percent of total global humanitarian aid. This fact in itself is not necessarily discouraging, as it is theoretically possible that the absolute size of the gap between what donors provide and what is needed is covered by proportion of aid provided by the CERF. Yet “global amounts of humanitarian aid continue to fall well short of what even conservative estimates suggest is needed” (Riddell 2007, 324). Indeed, the initial funding target of CERF was \$500 million, of which donors pledged only \$263 million in 2006. The

---

<sup>8</sup>It may be possible for states to condition their donations to CERF on whether or not a disaster has afflicted a country that they want to help. In other words, a country may wait for a disaster to happen, then deliver money through CERF with the expectation that most of its contribution will flow to its preferred recipient. However, the top recipients of CERF aid tend to change over time, while the top donors has remained relatively constant.

<sup>9</sup>See Section 3.5.1 of the Appendix for a novel empirical test that provides more support for this claim.

<sup>10</sup>2011 is the last year for which Global Humanitarian Assistance (2011) provides data.

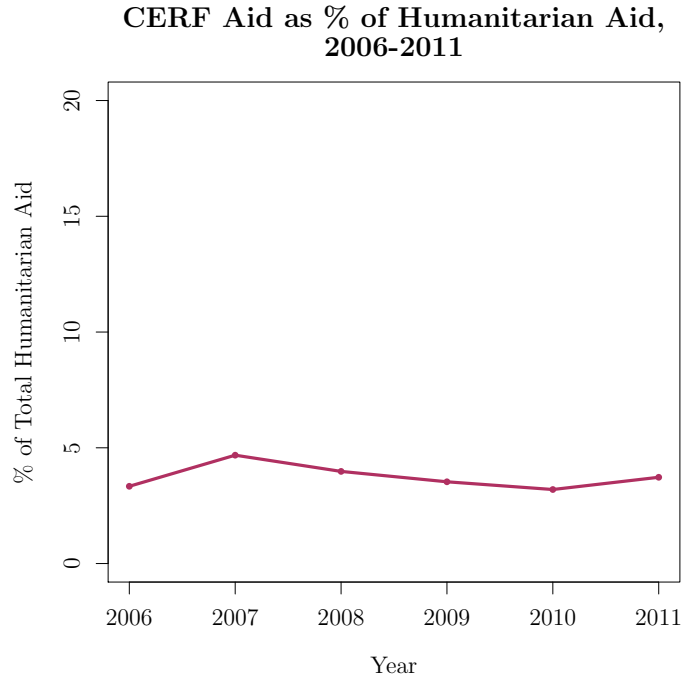


Figure 3.1: CERF Aid as Proportion of Total Humanitarian Aid

The solid line shows the proportion of aid contributed in each year by the CERF (drawn from the OCHA CERF website) out of the total amount of global humanitarian aid as recorded by Global Humanitarian Assistance (2011).

target, if obtained, would still only represent “less than 10 per cent of all humanitarian aid provided in 2005” (Riddell 2007, 324).

Table 3.1 lists the top ten donor governments for the CERF according to the CERF’s website alongside the top overall aid donors in aid allocation according to the *AidData* project (Tierney et al. 2011). While there is a great deal of overlap, it is notable that the top two overall donors (the United States and Japan) do not appear on the list of top donors to the CERF. The same goes for France and Italy. Additionally, four of the top donors to the CERF are not major overall aid donors (Norway, Spain, Ireland, and Denmark). This rough comparison suggests that at least a few major countries that provide a great deal of aid in other domains may be reluctant to provide aid to the CERF.

So, while the regime appears to succeed in providing need-based relief, it provides a

Table 3.1: Comparison of Top Donors to CERF versus Overall Aid Allocation

Ranking	CERF	Overall Aid Allocation
1	United Kingdom	United States
2	Norway	Japan
3	Sweden	Germany
4	Netherlands	France
5	Canada	United Kingdom
6	Spain	Netherlands
7	Ireland	Canada
8	Germany	Sweden
9	Denmark	Australia
10	Australia	Italy

small portion of overall humanitarian aid. This begs the question of how to restructure donors' incentives so as to increase the capacity and effectiveness of the organization. In the next section I develop a formal principal-agent model to study the strategic incentives facing donors and the organization.<sup>11</sup>

### 3.2 Strategic Interaction in the Provision of Humanitarian Aid

In this section I develop three related formal models. The first model considers a baseline scenario of a donor's optimal aid decision when there is no option of delegating to an expert institution. The second model seeks to incorporate the essential features of the CERF and the strategic problem faced by states that donate to the CERF. First, the organization should be at least as efficient as any individual donor since it is intimately tied to a network of expert organizations and agencies. Second, the organization should distribute aid where it is needed. In other words, its preference is for the extent of a crisis to be minimized no matter where it occurs. Third, donors can both contribute to the organization and provide relief bilaterally. In the third model, I consider the effect of allowing the donor to earmark part of its contribution. For Models II and III I provide brief sketches of the intuition of the

<sup>11</sup>Scholars frequently invoke principal-agent models in studying donors' decisions to provide aid. See, for example, Milner (2006).

equilibria, leaving the details of the analysis to the Appendix.

### 3.2.1 Model I: Bilateral Humanitarian Relief

Consider a donor,  $D$ , and a victim of a humanitarian crisis,  $V$ .<sup>12</sup> The victim is passive and non-strategic (and, accordingly, is not a player), but it is affected by the donor's behavior. At the beginning of the game, Nature draws a shock  $\omega \in [\omega_L, \omega_H]$  where  $0 \leq \omega_L \leq \omega_H$  for the victim and a measure of preference intensity  $\alpha \in \{0, 1\}$  for the donor. The shock,  $\omega$ , can be thought of as a humanitarian crisis or emergency. The donor knows the distribution of  $\alpha$ , which takes a value of 1 with probability  $p \in (0, 1)$  and 0 with probability  $1 - p$ . If  $\alpha = 0$ , the donor is unaffected by outcomes in the victim country. If  $\alpha = 1$ , the donor's utility function is sensitive to outcomes in the victim country. The  $\alpha$  parameter is intended to capture benefits that a donor may receive from the wellbeing of a recipient country. For example, it could be that constituents in the donor country intrinsically care about outcomes in the recipient country. Alternatively, the recipient country could be an important ally, and disasters may affect the ability of the recipient to carry out the provisions of security agreements. Additionally, these concerns may be specific to a particular regime in power in the recipient country, and disasters may weaken that regime's hold on power.<sup>13</sup> The model is agnostic about which of these particular incentives operates in any given case and simply assumes that the donor country does or does not care about how a disaster affects outcomes in a recipient country. The donor observes  $\alpha$  and  $\omega$  and can choose an amount of bilateral relief,  $r$ , to deliver to the victim.

The victim's utility function is  $u_V(r|\omega, \beta) = \beta r - \omega$ , where  $\beta \in (0, 1)$  is a measure of the donor's efficiency. That is, for 1 unit of aid provided by the donor, a proportion of that aid equal to  $\beta$  benefits the recipient. The donor's utility function is defined

---

<sup>12</sup>The donor could refer to a country or a private charitable organization, though throughout I refer to it as a country.

<sup>13</sup>For empirical evidence of a relationship between disasters and leader survival, see Quiroz Flores and Smith (2013).

according to the quadratic loss function  $u_D(r|\omega, \beta, \alpha, k) = -\alpha(\beta r - \omega)^2 - kr$ , where  $k > 0$  is the marginal transaction cost of providing bilateral relief. This cost can be thought of as a “start-up” cost for implementing an aid program. For example, the United States’ efforts to fight Ebola in Liberia entail it also incurring the costs of building medical facilities. Aid recipients benefit from this investment, but the donor pays the costs. The greater the scale of the aid project, the higher the costs will be for the donor.

Whether the donor provides any aid to the recipient depends on the costs of providing aid, how efficient the donor is, and whether or not it shares the preferences of the recipient (that is, whether or not  $\alpha = 1$ ). Clearly, if  $\alpha = 0$  then the donor does not provide any aid (since its utility would be defined solely by  $-kr$ ). When  $\alpha = 1$ , then the donor maximizes its utility with respect to  $r$ , the solution to which is

$$r_1^* \equiv \max\left\{0, \frac{2\beta\omega - k}{2\beta^2}\right\}. \quad (3.1)$$

This is positive as long as  $\beta > \frac{k}{2\omega}$ . The recipient’s payoff is given by  $u_V(r_1^*) = \beta r_1^* - \omega$ .

### 3.2.2 Model II: Adding an Expert Organization

Now add to this model an institution ( $A$ ) that represents a specialized pooled fund for humanitarian relief. In this version of the game, the donor chooses an amount of capacity to endow the institution with denoted by  $c \geq 0$  prior to Nature’s draw of  $\omega$  and  $\alpha$ .<sup>14</sup> This sequence of moves captures that the donor may not know whether or not it will care about the effects of a humanitarian crisis at the time that it makes its contribution to the institution. I model this contribution as a lump-sum cash

---

<sup>14</sup>Assuming a budget constraint for the donor would be more realistic but makes the analysis unwieldy. That the donor pays a marginal cost for providing aid works as an indirect constraint.

transfer that cannot be earmarked.<sup>15</sup> The donor only pays a cost for contributing to the institution if  $\alpha = 0$ . This cost function,  $f(c)$ , is an increasing convex function of  $c$ . I give the cost function a specific functional form by treating the loss as proportional to the squared amount that the donor contributed to the institution with parameter  $h \in [0, 1]$ . Therefore the cost function is  $f(c) = hc^2$  and the loss incurred for the donor is  $-hc^2$ .<sup>16</sup> Once the donor has chosen  $c$  and Nature has drawn  $\omega$  and  $\alpha$ , Nature reveals  $\omega$  to both donor and institution, but it reveals  $\alpha$  only to the donor. The institution knows the distribution of  $\alpha$  (i.e., the institution believes that  $\alpha = 1$  with probability  $p$  and  $\alpha = 0$  with probability  $1 - p$ ). This assumption is motivated by the fact that aid agencies often complain that bilateral contributions are unpredictable and unreliable in any given crisis. Next, the institution can use the resources provided by the donor ( $c$ ) to allocate aid ( $a$ ) to the recipient such that  $0 \leq a \leq b + c$ , where  $b \geq 0$  is the institution's preexisting budget. The institution also pays a marginal transaction cost  $\gamma > 0$  for delivering aid. Polman argues that, “[s]tart-up costs in distant crisis-hit countries are sky-high. Aid organizations have to recruit and hire staff, rent and furnish housing and office space, and bring in materials and equipment, such as Land Cruisers, aid supplies, satellite dishes, computers, air conditioners, office equipment, and generators” (Polman 2010, 40).

Upon observing the institution's allocation, the donor can distribute additional resources,  $r$ , to the recipient in the form of bilateral aid. The institution's efficiency is normalized to 1, meaning that the parameter  $\beta$  now measures how much the recipient benefits from a unit of aid from the donor relative to a unit of aid from the institution.<sup>17</sup> The donor's efficiency ( $\beta$ ), the marginal costs of providing aid for the

---

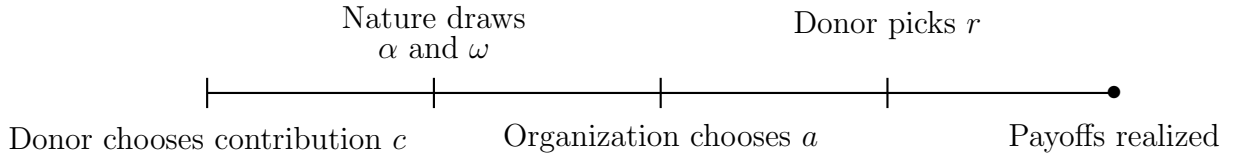
<sup>15</sup>That is, the donor cannot specify limits or place conditions on when the institution can and cannot use the aid.

<sup>16</sup>I square the loss function for providing aid to the institution for the convenience of having a convex cost function. The  $h$  parameter is included to provide flexibility for the scale of this loss.

<sup>17</sup>Coordination in itself should lead to greater efficiency in aid delivery. Indeed, “[i]n times of crisis, when needs are great and officials are overburdened, trickles of uncoordinated donations can be a distraction, some aid workers say, requiring a lot of attention without solving the biggest problems” (Nossiter 2014). Additionally, governments often view specialized organizations as more

donor and the organization ( $k$  and  $\gamma$ , respectively), and the scale of the loss the donor incurs from giving aid when it otherwise would not ( $h$ ) are all common knowledge at the beginning of the game. The game sequence is summarized in Figure 3.2.

Figure 3.2: Sequence of Game in Model II



The organization’s goal is to minimize the negative effects of the shock on the victim, and thus its problem is to maximize  $u_A(a|\omega, \beta, r, \gamma) = -(a + \beta r - \omega)^2 - \gamma a$ , subject to the constraint that  $a \leq b + c$ . This utility function ensures that the organization does not want to provide too much aid to the victim such that its utility is increased beyond the pre-crisis level, since its goal is providing disaster recovery, not development assistance. Finally, the donor’s utility function is now also a function of the aid provided by the institution and the potential costs of contributing to the institution:  $u_D(r, c|a, \omega, \beta, \alpha) = -\alpha(a + \beta r - \omega)^2 - kr - (1 - \alpha)hc^2$ .

In sum, the organization would prefer that the shock is as small as possible, while the donor only wants to minimize the shock if its preferences align with the victim. Both donor and organization observe the shock, but the donor is less efficient at providing aid than the institution and the institution is unsure of how the donor values outcomes for the victim.

### 3.2.2.1 Equilibrium for Model II

I solve the game for the pure strategy Nash equilibria. The details of the analysis can be found in Section 3.5.2 of the Appendix. The intuition is as follows. Just as effective. For example, a British development minister was quoted in the New York Times as saying, “[n]ongovernmental organizations are not only watchdogs, but also some of the best deliverers of assistance... . Oxfam can do excellent water engineering. The British Red Cross delivers health care. They are close to the people on the ground” (Crossette 1992).

in the world of bilateral aid, the donor country never gives aid in the final stage of the game if it does not have an interest relieving the crisis (i.e., if  $\alpha = 0$ ). There is also a threshold of efficiency ( $\beta^*$ ) below which the donor gives no aid even if it does have an interest in helping since it is too inefficient. The organization conditions its contribution on the efficiency of the donor. When the donor is too inefficient to provide aid, the organization provides either as much aid as is needed to relieve the crisis or the entirety of its available resources. When the donor could potentially contribute to the crisis, the organization contributes less the more likely it is that the donor will come to the aid of the victim. This affects the behavior of the donor at the beginning of the game when it decides how much aid to channel through the organization. Less efficient donors give more aid to the organization as it becomes more likely that they share the preferences of the victim, but there is also an incentive to free ride and provide less aid to the organization the greater its existing stock of resources. More efficient donors behave similarly, but because of the organization's incentives to optimally under-provide aid when it expects that the donor will take up responsibility for helping the victim, the efficient donor gives less the more efficient it is. The model is thus consistent with some of the stylized facts motivating the paper. For example, highly efficient aid providers like the United States tend not to contribute much to the organization.

### 3.2.3 Model III: Allowing Limited Earmarking

Now consider a modified version of Model II where the donor can place restrictions on the conditions under which the institution is allowed to use its aid. I model this by having Nature draw a new variable,  $\theta$ .<sup>18</sup> Nature's move now consists of a draw of

---

<sup>18</sup>I introduce this new variable rather than assuming that the organization observes  $\alpha$  for two reasons. First, I want  $\alpha$  to represent the donor's interest in helping in any given case, not its predetermined interests. Using  $\theta$  as the earmarking variable keeps the donor's immediate interests separate from its stated goals in providing aid. Second, there are problems with assuming that the organization knows  $\alpha$ . The assumption that the organization is uncertain about the donor's



$\omega, \alpha$ , and  $\theta$ . I assume that if  $\theta = 0$ , the organization can only use a portion of the donor's aid denoted by  $q \in [0, 1]$ , whereas if  $\theta = 1$ , all of the donor's contribution may be used by the organization. I assume the distribution of  $\theta$  is such that  $\theta = 1$  with probability  $m$  and  $\theta = 0$  with probability  $1 - m$ . I treat  $m$  and  $q$  as exogenous parameters drawn by Nature. If  $m$  is very low, it is unlikely that the donor's aid will be fully available to the organization. (Model II is a special case of Model III where  $m = 1$ .) Nature reveals  $\theta$  to both the donor and organization after the donor makes its first move.<sup>19</sup>

The  $q$  parameter is assumed to be common knowledge, binding, and enforceable at the time that the donor makes its contribution to the organization (at the time the donor chooses  $a$ ). Whereas a traditional earmark might assume that no aid can be used if the situation does not meet the requirement specified *ex ante* by the donor (i.e.,  $q = 0$ ), I allow that proportion to vary. Since the  $q$  parameter limits the amount of resources available to the organization if the earmark conditions do not apply, it also limits the amount of costs the donor may incur from having the organization use the aid in a way that harms its interests. That is, instead of a loss of  $-hc^2$ , if  $\theta = 0$  and  $\alpha = 0$ , the donor only incurs a loss of  $-h(qc)^2$ . Figure 3.3 summarizes the modified sequence of the game.

### 3.2.3.1 Equilibrium Analysis for Model III

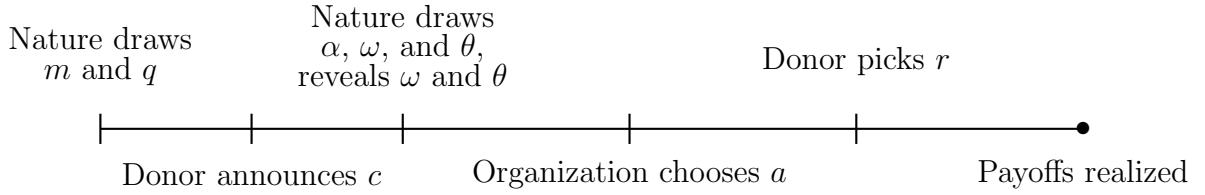
Similar to the treatment of Model II, I present the intuition of the key results from the equilibrium analysis of Model III here and leave the details of the analysis

---

interests is substantively motivated. In the model, assuming that the organization knows  $\alpha$  leads it to contribute resources only when it knows that  $\alpha = 0$  or when the donor's budget constraint is binding.

<sup>19</sup>I assume that  $\theta$  is objectively apparent to both the donor and the organization. In other words, I assume away the potential for the organization or the donor to argue over whether the conditions specified *ex ante* actually apply in any given case, acknowledging that this is not a trivial problem in the principal-agent literature. See, for example, Hawkins and Jacoby (2006, 206).

Figure 3.3: Sequence of Game in Model III



to Section 3.5.3 of the Appendix. When earmarking is allowed, the donor makes the same optimal allocation at the end of the game. However, the actual contribution and the efficiency threshold may differ since both are functions of what the organization provides, which depends on whether or not the conditions of the earmark apply (i.e., whether  $\theta = 0$  or  $\theta = 1$ ). Similarly, the organization conditions its allocation on the efficiency of the donor and the likelihood that the donor will help in the last stage. The key result of Model III relates to the donor's decision in the first move of the game, which I summarize in Proposition 1.

**Proposition 1.** *The donor gives at least as much to the organization when the organization allows partial earmarking as it does when the organization bans earmarking.*

The proof of Proposition 1 follows from the discussion in Section 3.5.3 of the Appendix. The result is intuitive. When donors are at least partially insulated from the costs of providing aid, they are willing to give more up front. The analysis also shows that the efficient donor's contribution to the organization decreases with the likelihood that it makes an unconditional transfer (alternatively, as the conditions that allow for the use of aid become broader) and in the proportion of its aid that will remain available to the organization if  $\theta = 0$ . For less efficient donors, there are conditions under which the optimal contribution the organization is increasing in  $q$ . Because inefficient donors will be unable to help in the last stage of the game,

they would prefer that the organization is not so constrained that even it cannot do anything to help.

### 3.3 Comparisons Across Models: A Simulation

Limited earmarking leads to greater contributions to the organization, which seems intuitive and potentially encouraging. What is unclear is whether this can improve outcomes for potential victims of humanitarian crises. It is straightforward to show that the victim is always at least as well off when the organization is a player (Models II and III) as when it depends wholly on bilateral aid (Model I). I present the details of this analysis in Section 3.5.4 of the Appendix. Unfortunately, given the difficulty of comparing two different allocations by the institution that may or may not be binding, it is less straightforward to solve analytically for the victim's expected utility to determine whether Model III generally outperforms Model II (or vice versa) when the organization must act on its own (when  $\alpha = 0$ ). This is the focus of the next section.

#### 3.3.1 Can Earmarking Lead to Better Outcomes?

Given that the earmark restricts the amount of funds the organization can use, under what conditions, if any, do higher contributions lead to better outcomes in recipient countries? I investigate this question using a simulation.<sup>20</sup> The simulation generates the model parameters from a uniform distribution and calculates the players' equilibrium contributions given those parameters along with the recipient's utility.<sup>21</sup> Most of these variables have a minimum of zero and a maximum of one, though I set  $k$  and  $\gamma$  (the proportional costs of aid to the donor and organization, respectively) have an upper bound of .1, and  $m$  and  $q$  are set relatively high to vary

---

<sup>20</sup>The simulation is executed in *R* (R Core Team 2014).

<sup>21</sup>A single run of the simulation generates  $p, q, m, b, \omega_L, \omega_H, \omega, h, k, \beta,$  and  $\gamma$ .

between .75 and 1.<sup>22</sup> These parameters are then used to calculate thresholds  $\beta'$  and  $\beta''$ . Finally, I calculate the utility of the recipient for each of the three games as show in Table 3.2.

Table 3.2: Recipient Payoffs Across Models I, II, and III

Model	Recipient Payoff
I	$\beta r_1^* - \omega$
II	$a_2^* + \beta r_2^* - \omega$
III	$a_3^* + \beta r_3^* - \omega$

---

<sup>22</sup>I set these parameters high to illustrate the effect of allowing just a minimal amount of earmarking.

### Frequency of Victim Utility in Model III > Model II

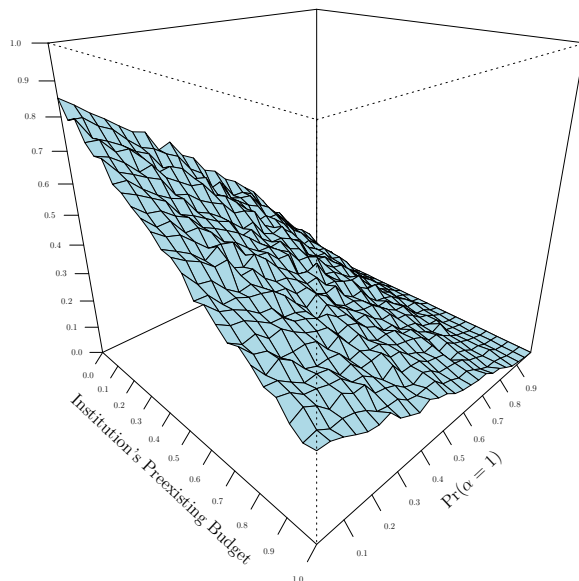


Figure 3.4: Comparing Model III to Model II

For each iteration of the game, I code whether the victim's utility is higher in Model III than in Model II, and vice versa.<sup>23</sup> This process is repeated 1000 times, after which point I take the proportion of games where the victim did better in one model versus the other, fixing the organization's preexisting budget ( $b$ ) and probability that the donor cares about outcomes in the recipient country ( $p$ ). I allow  $b$  to vary between 0 and 1, increasing in increments of .04 (such that I run the model for 25 values of  $b$ ), and I allow  $p$  to vary between .01 and .99 (also running for 25 values of  $p$ ).<sup>24</sup>

---

<sup>23</sup>Since utility is an ordinal concept, measuring the difference in utilities has no meaning. However, it is still possible to say whether the recipient has a higher utility in one institutional setting or not.

<sup>24</sup>For each combination of  $b$  and  $p$ , I compute equilibrium allocations and victim utilities given the rest of the model parameters. For example, I set  $b = 0$  and  $p = .01$ , generate the other parameters, equilibrium contributions, and utilities of the model 1000 times, then repeat the process with  $p \cong .05$ , and so on for all values of  $b$  and  $p$ .

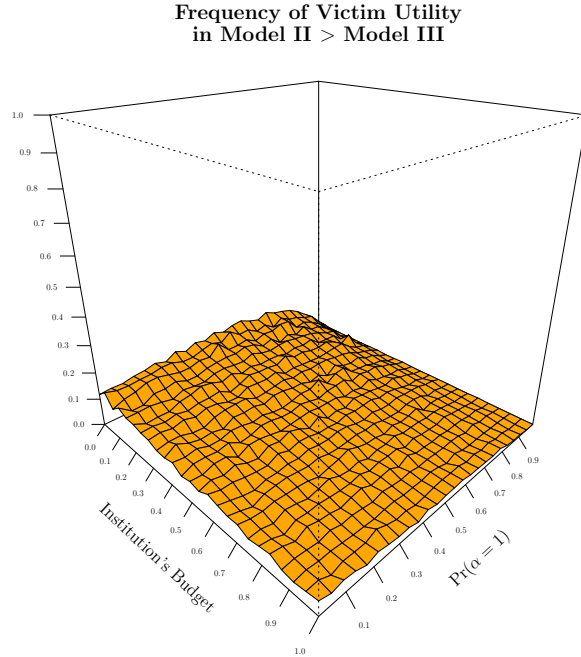


Figure 3.5: Comparing Model II to Model III

### 3.3.2 Simulation Results

Figure 3.4 shows that there is a region – when the organization has very few resources (when  $b$  is low) and when a donor is unlikely to want to provide bilateral aid to a victim (when  $p$  is low) – where more than fifty percent of the time the victim of a crisis does better when the organization allows limited earmarking. This advantage diminishes as  $b$  and  $p$  increase. This is unsurprising, since when the organization already has a large standing pool of resources (when  $b$  is large), then outcomes in the recipient country are less sensitive to the possible constraint of the earmark. In other words, regardless of how much the donor contributes and what the conditions of the contribution are, it is likely that the organization will have enough resources to address a crisis. In the case of  $p$ , the advantage diminishes because the institution contributes less as  $p$  increases, and thus outcomes are less affected by what the donor contributes at the beginning of the game.

While the region where earmarking leads to better outcomes on average is relatively small, it tends to do better when victims are unlikely to receive adequate bilateral support (where  $p$  is low) and when the organization has few resources (where  $b$  is low). These are close to the cases that the CERF aims to address and similar to real budget constraints. The intuition is that because the victim's utility depends heavily on the organization and the organization depends heavily on the immediate contributions made by donors, the organization is able to make larger contributions when conditions allow complete access to the donor's contribution (when  $\theta = 1$ ). When the conditions of the earmark do not apply (when  $\theta = 0$ ), the organization still has  $b + qc$  resources at its disposal, where the donor's initial contribution to the organization is larger than it would have been under Model II.

However, Figure 3.4 only tells us when allowing partial earmarking tends to give the recipient a higher payoff than banning earmarking. Where this frequency is less than fifty percent, there are two general possibilities: (1) banning earmarking gives the victim a higher payoff; or (2) Models II and III give equal payoffs. To investigate this possibility, I plot the frequency with which the victim's payoff under Model II is greater than Model III in Figure 3.4 (using the same scale as Figure 3.5). The results suggest that the victim is rarely better off in Model II. Figures 3.4 and 3.5 together imply that most of the time the victim is just as well off in Model III as it is in Model II. For much of the parameter space, the victim often does at least as well as Model II *or better* under Model III.

### 3.4 Conclusion

It is well documented that donors have strategic reasons for providing aid. This paper suggests that accounting for that fact in the design of institutions for international humanitarian relief could be beneficial. Allowing donors to insulate themselves at least partially from the negative effects of perceived misallocations not only leads

to greater contributions to a pooled fund for emergency relief, but can also lead to better outcomes for countries facing crises.

More generally, this paper contributes to the literature on delegation to international organizations as a principal-agent problem in a number of ways (Hawkins et al. 2006). First, the analysis highlights that the potential gains in efficiency from expert organizations are often enough to get states to contribute even when funds are pooled and not subject to earmarking. This is in contrast to much of the literature which assumes principals must exercise some discretion over the agents they hire. Second, the paper illustrates how extra-contractual features of principal-agent relationships interact with design features to affect outcomes. Specifically, there is a tendency to assume that closely aligned preferences between principal and agent make for more effective delegation relationships. Here, the assumption that the principal (the donor) can act on its own leads the agent (the organization) to contribute less to a shared interest the more likely it is that the preferences of the actors align (though this is only the case for relatively efficient donors). Finally, the paper emphasizes the importance of agent preferences and strategies in affecting the delegation decisions of principals, which others have argued is a neglected area of study in the principal-agent literature (Hawkins and Jacoby 2006). Specifically, the paper develops the implications of assuming altruism and strategic thinking on the part of an agent for delegation decisions and policy outcomes.

The model is also agnostic about what happens once aid reaches its destination, though this obviously also has implications for recipient welfare. Indeed, I abstract away from domestic politics in the victim country entirely, though this could be an interesting avenue for future research, especially since others have argued that actors who are less averse to suffering (i.e., actors who do not have altruistic preferences as I model them here) may be more effective in alleviating poverty (e.g., Svensson 2000, 61). It is unclear to what extent this argument would apply to emergency assistance



that largely bypasses government channels. For now, this paper helps identify how existing institutions might be reformed to increase the supply of crisis relief.

### 3.5 Appendix for “The Design of International Institutions for Humanitarian Aid”

#### 3.5.1 An Empirical Test of CERF’s Impartiality

One observable factor linked to receiving development aid (i.e., development aid) is temporary membership on the United Nations Security Council (UNSC) (e.g., Kuziemko and Werker 2006; Dreher, Sturm and Vreeland 2009). Since the CERF is designed to provide impartial rapid response to humanitarian disasters, political factors like membership on the UNSC should not influence the likelihood of a state receiving aid from the CERF. Alternatively, humanitarian emergencies should be strong predictors of receiving CERF aid.

To test whether Security Council membership increases a state’s likelihood of receiving CERF aid, I constructed a data set comprised of all countries that the OECD lists as being eligible for Official Development Assistance. For each country-year, I code whether or not a country received assistance from the CERF during that year using data from the OCHA CERF website. The key independent variables are (1) the total number of deaths that occurred from natural disasters during a given country-year, and (2) whether or not that country held a rotating seat on the UNSC during that year. The former should be a strong predictor of receiving CERF aid, while the latter should not affect whether or not a country receives CERF aid.<sup>25</sup>

Table 3.3 presents the results of the statistical tests. Model 2 adds recipient

---

<sup>25</sup>I measure disaster deaths using the International Disaster Database. See EM-DAT (2009). The measure is natural log of the total number of deaths from disasters that occurred in a given year (I added one to each case to account for zero values). For every year from 2006 to 2013, I manually coded a dummy variable to indicate whether or not a country was one of the ten temporary members of the UNSC using the official website of the UNSC. See UNSC (2014). I also included measures of GDP per capita from World Bank (2013) and regime type from Marshall, Jaggers and Gurr (2010) as additional measures of need. More developed countries should have less need for disaster assistance, as should more democratic countries. See, for example, Kahn (2005).

Table 3.3: UNSC Membership and Likelihood of Receiving CERF Aid, 2006-2013  
(Logit Models)

	<i>Dependent variable:</i>	
	CERF Recipient? (Dummy)	
	(Model 1)	(Model 2)
ln(Disaster Deaths)	0.262*** (0.038)	0.454*** (0.091)
UN Security Council Member	0.101 (0.428)	-0.074 (0.570)
GDP per capita	-0.0004*** (0.00004)	0.0002 (0.0002)
Polity Score	-0.030* (0.016)	-0.047 (0.101)
Constant	-0.108 (0.160)	-19.866 (4,031.799)
Recipient-fixed Effects?	No	Yes
Observations	741	741
Log Likelihood	-392.495	-215.867
Akaike Inf. Crit.	794.989	659.734

*Note:* \*p<0.1; \*\*p<0.05; \*\*\*p<0.01

Standard errors in parentheses.

Recipient dummies included in Model 2.

country-fixed effects to Model 1 to account for unobserved heterogeneity. Consistent with the claims of the CERF and independent reports evaluating the CERF's allocation of disaster aid, the results in Table 3.3 support the claim the CERF allocates aid independently of political pressures. The coefficient on the disaster deaths variable is positive and statistically significant in both models, indicating that receiving aid from the CERF becomes more likely as the extent of a humanitarian emergency increases. The coefficients on the measures of GDP per capita and domestic institutions are also consistent with the expectation that more developed, democratic countries should have less need for CERF aid. Alternatively, the coefficient on the UN Security Council Member dummy variable is not statistically significant. Clearly this test does not rule out the possibility that CERF aid is motivated by strategic factors, but it at least provides some further support for the claim that CERF allocates relief aid independently of political factors that have been shown to affect aid allocation in other domains.

### 3.5.2 Equilibrium Analysis for Model II

#### *The Donor's Last Move*

Consider the donor's final move at the last stage of the game. At this point, the donor has observed the agent's allocation and knows  $\alpha$  and  $\omega$ . If  $\alpha = 0$ , the donor will not provide any aid. In the case where  $\alpha = 1$ , then the donor may provide a positive amount of aid. Defining  $a_2^*$  as the institution's optimal allocation, the allocation that maximizes the donor's utility in the last stage with respect to  $r$  is

$$r_2^*(c, a, \alpha) = \begin{cases} 0 & \text{if } \alpha = 0, \\ 0 & \text{if } \alpha = 1 \text{ and } \beta \leq \beta^* \equiv \frac{k}{2(\omega - a_2^*)}, \\ \frac{2\beta(\omega - a_2^*) - k}{2\beta^2} & \text{if } \alpha = 1 \text{ and } \beta > \beta^* \equiv \frac{k}{2(\omega - a_2^*)}. \end{cases} \quad (3.2)$$

#### *The Institution's Choice*

The institution's allocation depends on its beliefs about whether the donor will give bilateral aid as well as the value of  $\beta$ . In the case where  $\beta$  is sufficiently low, then the institution knows that the donor will not provide aid whether or not it wants to help the recipient in principle (i.e., whether or not  $\alpha = 1$  or  $\alpha = 0$ ). The threshold that factors into the institution's decision (and that ultimately factors into the donor's decision in the first stage) is whether or not  $\beta > \beta' \equiv \frac{k}{2\omega}$ .<sup>26</sup> If  $\beta \leq \beta'$ , then the institution chooses  $a$  to maximize its utility subject to the constraint that  $a \leq b + c$ , the solution to which is

$$a_{L2}^* = \max\left\{0, \min\left\{\omega - \frac{\gamma}{2}, \frac{b + c}{1 + \gamma}\right\}\right\}.^{27} \quad (3.5)$$

---

<sup>26</sup>This threshold of  $\beta$  is slightly different than the  $\beta^*$  threshold identified in Equation (3.2). If  $\beta \leq \frac{k}{2\omega}$ , then the donor never provides bilateral aid at the end of the game regardless of  $\alpha$ , and the institution's problem is straightforward. If  $\beta > \frac{k}{2\omega}$ , then the institution makes its allocation under uncertainty about what the donor will do.

<sup>27</sup>This problem is solved using the following Kuhn-Tucker Lagrangian:

$$\mathcal{L}_{A0} = -(a - \omega)^2 - \gamma a - \lambda(a(1 + \gamma) - b - c) \quad (3.4)$$

Now suppose that  $\beta > \beta'$ . While the type of donor that wants to minimize disaster damage in the recipient will always contribute (which occurs with probability  $p$ ), with probability  $(1 - p)$  the donor will not want to help. The institution now seeks to maximize its expected utility where uncertainty about the donor's type matters. The donor's allocation when  $\alpha = 1$  is simply  $r^* = \max\{0, \frac{2\beta(\omega-a)-k}{2\beta^2}\}$ .<sup>28</sup>

The solution to this maximization problem is

$$a_{H2}^* = \max\{0, \min\{\frac{\gamma}{2(p-1)} + \omega, \frac{b+c}{1+\gamma}\}\} \quad (3.6)$$

which, again, depends on whether or not the budget constraint is binding. This expression is decreasing in  $p$  and is non-negative as long as  $p \leq 1 - \frac{\gamma}{2\omega}$ . Intuitively, as the institution increasingly believes that the donor will contribute aid at the end of the game, it contributes less itself. Summarizing the institution's allocation we have

$$a_2^*(c) = \begin{cases} a_{L2}^* \equiv \max\{0, \min\{\omega - \frac{\gamma}{2}, \frac{b+c}{1+\gamma}\}\} & \text{if } \beta \leq \beta', \\ a_{H2}^* \equiv \max\{0, \min\{\omega + \frac{\gamma}{2(p-1)}, \frac{b+c}{1+\gamma}\}\} & \text{if } \beta > \beta'. \end{cases} \quad (3.7)$$

### *The Donor's First Move*

At the beginning of the game, the donor chooses the amount of cash,  $c$ , it will provide to the institution. The donor has not yet observed  $\omega$  but has prior beliefs that  $\omega \sim U[\omega_L, \omega_H]$ , and believes that it will want to help the recipient with probability  $p$ . The donor knows that if  $\beta \leq \beta'$ , then it is credible that it will not contribute at the end of the game either way. In the first stage the donor can only make a best guess about  $\omega$ , thus its decision depends on the expected value of  $\omega$ . The critical factor from the donor's perspective is whether  $\beta > \beta' \equiv \frac{k}{2\omega}$ . Since  $\omega \sim U[\omega_L, \omega_H]$ ,  $E[\omega] = \frac{\omega_L + \omega_H}{2}$ .

where  $\lambda$  is the Lagrange multiplier. The first order conditions for this maximization problem are

$$\frac{\partial \mathcal{L}_{A0}}{\partial a} \leq 0, \quad \frac{\partial \mathcal{L}_{A0}}{\partial \lambda} \geq 0, \quad a \cdot \frac{\partial \mathcal{L}_{A0}}{\partial a} = 0, \quad \lambda \cdot \frac{\partial \mathcal{L}_{A0}}{\partial \lambda} = 0 \quad (3.5)$$

<sup>28</sup>The Kuhn-Tucker Lagrangian and FOC for this problem are analogous to those used in solving for  $a_L^*$ .

Therefore, the relevant threshold in the first stage is  $\beta'' = \frac{k}{2E[\omega]} = \frac{k}{\omega_L + \omega_H}$ . Consider this case first.

When  $\beta \leq \beta''$ , the amount that the institution contributes is independent of  $p$ , since it knows that the donor either does not want to contribute or will find contributing too costly. Whether or not the institution's budget constraint binds depends on the true value of  $\omega$ . If  $\omega \in [\omega_L, \frac{b+c}{\gamma+1} + \frac{\gamma}{2}]$  then the institution's budget constraint is not binding and it provides  $\omega - \frac{\gamma}{2}$ . If  $\omega \in (\frac{b+c}{\gamma+1} + \frac{\gamma}{2}, \omega_H]$ , then the institution's budget constraint binds and it provides  $\frac{b+c}{1+\gamma}$ . Thus, the donor's expected utility in this case is:

$$p \left( \int_{\omega_L}^{\frac{b+c}{\gamma+1} + \frac{\gamma}{2}} -(\omega - \frac{\gamma}{2} - \omega)^2 f(\omega) d\omega + \int_{\frac{b+c}{\gamma+1} + \frac{\gamma}{2}}^{\omega_H} -(\frac{b+c}{1+\gamma} - \omega)^2 f(\omega) d\omega \right) + (1-p)(-hc^2). \quad (3.8)$$

The value of  $c$  that maximizes Equation (3.8) is

$$c_{L2}^* = \max\left\{0, -\frac{p(-4b + \gamma^2 + \gamma + 4(\gamma + 1)\omega_H)}{4(3(\gamma + 1)^2 h(p - 1) - p)}\right\}. \quad (3.9)$$

When positive,  $c_{L2}^*$  is increasing in  $p$  ( $\frac{\partial c_{L2}^*}{\partial p} > 0$ ), and is decreasing in the institution's existing resources,  $b$  ( $\frac{\partial c_{L2}^*}{\partial b} < 0$ ). The optimal allocation is not a function of the donor's efficiency or its costs of delivering aid, since it does not deliver any bilateral aid in the final stage.

Now consider the case where  $\beta > \beta''$ . In this case, the donor knows that if  $\alpha = 1$ , it will not be too costly for it to provide some bilateral aid at the end of the game. The optimal bilateral aid package it provides ( $r_2^*$ ) depends on the value of  $a_2^*$ . If  $\omega \in [\omega_L, \frac{b+c}{\gamma+1} - \frac{\gamma}{2(p-1)}]$  then the institution's budget constraint is not binding and it will provide  $\omega + \frac{\gamma}{2(p-1)}$ . If  $\omega \in (\frac{b+c}{\gamma+1} - \frac{\gamma}{2(p-1)}, \omega_H]$ , then the institution's budget constraint binds and it provides  $\frac{b+c}{1+\gamma}$ . For notational convenience, I write the donor's optimal offer given that the institution's budget constraint is not binding as  $r_N$ , and

the optimal offer given that the institution's constraint *is* binding as  $r_B$ . The donor maximizes its expected utility:

$$\begin{aligned}
EU_D = & \\
& p \left( \int_{\omega_L}^{\frac{b+c}{\gamma+1} - \frac{\gamma}{2(p-1)}} - \left( \frac{\gamma}{2(p-1)} + \omega - \omega + \beta r_N \right)^2 - k r_N f(\omega) d\omega \right. \\
& \left. + \int_{\frac{b+c}{\gamma+1} - \frac{\gamma}{2(p-1)}}^{\omega_H} - \left( \frac{b+c}{1+\gamma} - \omega + \beta r_B \right)^2 - k r_B f(\omega) d\omega \right) \\
& + (1-p)(-hc^2).
\end{aligned}$$

Solving for the optimum of this expression with respect to  $c$  yields

$$c_{H2}^* = \max\left\{0, \frac{kp}{4\beta h(1+\gamma)(1-p)}\right\}. \quad (3.10)$$

This allocation is increasing in the likelihood that the donor shares the preferences of the recipient ( $\frac{\partial c_{H2}^*}{\partial p} > 0$ ), decreasing in the efficiency of the donor relative to the institution ( $\frac{\partial c_{H2}^*}{\partial \beta} < 0$ ), and increasing in the costs of delivering aid bilaterally ( $\frac{\partial c_{H2}^*}{\partial k} > 0$ ). Summarizing the donor's contribution we have

$$c_2^* = \begin{cases} c_{L2}^* \equiv \max\left\{0, -\frac{p(-4b+\gamma^2+\gamma+4(\gamma+1)\omega_H)}{4(3(\gamma+1)^2h(p-1)-p)}\right\} & \text{if } \beta \leq \beta'' \equiv \frac{k}{\omega_L+\omega_H}, \\ c_{H2}^* \equiv \max\left\{0, \frac{kp}{4\beta h(1+\gamma)(1-p)}\right\} & \text{if } \beta > \beta'' \equiv \frac{k}{\omega_L+\omega_H}. \end{cases} \quad (3.11)$$

### 3.5.3 Equilibrium Analysis for Model III

#### *The Donor's Last Move*

The donor's last move in Model III is the same as in the previous model, but it must also account for the possibility that  $\theta = 0$ , in which case the institution's allocation may be different if the budget constraint is binding. Still, defined in terms of the institution's optimal allocation in Model III,  $a_3^*$ , the donor's allocation is the same.

$$r_3^* = \begin{cases} 0 & \text{if } \alpha = 0, \\ 0 & \text{if } \alpha = 1 \text{ and } \beta \leq \beta^* \equiv \frac{k}{2(\omega - a_3^*)}, \\ \frac{2\beta(\omega - a_3^*) - k}{2\beta^2} & \text{if } \alpha = 1 \text{ and } \beta > \beta^* \equiv \frac{k}{2(\omega - a_3^*)}. \end{cases} \quad (3.12)$$

#### *The Institution's Choice in Model III*

In this model the institution's allocation depends both on  $\beta$  and  $\theta$ . Writing the donor's optimal contribution to the pooled fund as  $c_3^*$ , the institution's optimal allocation is given by

$$a_3^* = \begin{cases} a_{L3,0}^* \equiv \max\{0, \min\{\omega - \frac{\gamma}{2}, \frac{b+qc_3^*}{1+\gamma}\}\} & \text{if } \beta \leq \beta' \text{ and } \theta = 0, \\ a_{L3,1}^* \equiv \max\{0, \min\{\omega - \frac{\gamma}{2}, \frac{b+c_3^*}{1+\gamma}\}\} & \text{if } \beta \leq \beta' \text{ and } \theta = 1, \\ a_{H3,0}^* \equiv \max\{0, \min\{\omega + \frac{\gamma}{2(p-1)}, \frac{b+qc_3^*}{1+\gamma}\}\} & \text{if } \beta > \beta' \text{ and } \theta = 0, \\ a_{H3,1}^* \equiv \max\{0, \min\{\omega + \frac{\gamma}{2(p-1)}, \frac{b+c_3^*}{1+\gamma}\}\} & \text{if } \beta > \beta' \text{ and } \theta = 1. \end{cases} \quad (3.13)$$

#### *The Donor's First Move in Model III*

Solving for the donor's optimal allocation in this model is similar to Model II, only now there are four possible states of the world for each type of state (efficient or inefficient). These states and their probabilities are summarized in Table 3.4. The donor forms expectations about each outcome based on its prior beliefs about  $\omega$ , how it expects the institution to respond, and the likelihood that it wants to help the



recipient minimize the shock. For instance, with probability  $pm$  the donor both cares about the recipient ( $\alpha = 1$ ) and the institution is allowed to use the full amount of aid ( $\theta = 1$ ). In this case, the institution makes its optimal non-binding contribution up to a threshold, after which it contributes everything it can. The donor considers what this threshold is and how it would respond in the last stage. The process for each of the other states of the world is similar.

Table 3.4: Parameter Combination Likelihood

$\theta$	$\alpha$	$\Pr(\text{State}_i)$
$\theta = 0$	$\alpha = 0$	$(1 - m)(1 - p)$
$\theta = 0$	$\alpha = 1$	$(1 - m)p$
$\theta = 1$	$\alpha = 0$	$m(1 - p)$
$\theta = 1$	$\alpha = 1$	$mp$

The donor's optimal contribution when it is less efficient is now

$$c_{L3}^* \equiv -\frac{p(m(q-1)-q)(-4b+\gamma^2+\gamma+4(\gamma+1)\omega_H)}{4((m-1)q^2-m)(3(\gamma+1)^2h(p-1)-p)}. \quad (3.14)$$

The condition that determines whether the less efficient donor provides a positive contribution is the same as in Model II, and the size of the contribution becomes larger for less efficient donors. This can be seen by considering the ratio of  $-\frac{(m(q-1)-q)}{((m-1)q^2-m)}$ , which is what distinguishes  $c_{L2}^*$  from  $c_{L3}^*$  and is always greater than a factor of 1 by the fact that  $0 \leq m, q \leq 1$ . When positive this allocation is decreasing in the likelihood that the contribution will be unconditional ( $\frac{\partial c_{L3}^*}{\partial m} < 0$ ). The relationship between  $q$  and the optimal allocation of the inefficient donor depends on  $q$  and  $m$ . Assuming that the condition for a positive allocation holds,  $\frac{\partial c_{L3}^*}{\partial q} > 0$  if and only if  $m > \frac{q^2}{(q-1)^2}$ . For  $q > \frac{1}{2}$  this can never hold, in which case increases in  $q$  lead to smaller allocations to the institution. However, for  $q \in [0, \frac{1}{2})$ , the effect of  $q$  on  $c_{L3}^*$  depends on  $m$ .

For more efficient donors, the optimal contribution to the institution is now given by

$$c_{H3}^* \equiv \frac{kp(m+q-mq)}{4\beta(\gamma+1)h(p-1)((m-1)q^2-m)}. \quad (3.15)$$

Like less efficient donors, this allocation is always positive and is always greater than the donor's contribution when it cannot earmark. To see that the allocation is always positive, notice that  $0 \leq m, q \leq 1$  implies that the numerator is strictly nonnegative and that the denominator is positive. That  $c_{H3}^*$  is always greater than  $c_{H2}^*$  can be seen by considering the ratio of  $\frac{(m+q-mq)}{((m-1)q^2-m)}$ , which is what distinguishes  $c_{H2}^*$  from  $c_{H3}^*$  and is always greater than a factor of 1 by the fact that  $0 \leq m, q \leq 1$ .

Note also that the allocation is decreasing in  $m$  and  $q$ . That is, the more likely it is that the donor will be making an unconditional transfer, the less it contributes ( $\frac{\partial c_{H3}^*}{\partial m} < 0$ ), and the same goes for the amount of contribution that remains available for the institution to use ( $\frac{\partial c_{H3}^*}{\partial q} < 0$ ).

### 3.5.4 Comparison of Models II and III with Model I

To see that the victim is always at least as well off when the institution is a player as it is without the institution, suppose that the utility of the victim is greater under Model I than under Model II. That is, suppose  $\beta r_1^* - \omega > a_2^* + \beta r_2^* - \omega$ . This implies that  $a_2^* < \beta(r_1^* - r_2^*)$ . If  $\alpha = 0$ , then  $r_1^*, r_2^* = 0$ , implying that  $a < 0$ , which cannot hold since  $a$  is constrained to be nonnegative. If  $\alpha = 1$ , then plugging in the optimal values of  $r_1^* = \frac{2\beta\omega - k}{2\beta^2}$  and  $r_2^* = \frac{2\beta(\omega - a_2^*) - k}{2\beta^2}$  leads to a contradiction ( $0 < 0$ ). The same argument shows that the victim is at least as well off in Model III as in Model I.

A similar logic shows that when the donor has both the interest in alleviating a crisis (when  $\alpha = 1$ ) and the capacity to help ( $\beta > \beta^*$ ), Model II and Model III give the victim an identical payoff. To see this, notice that the assumption that  $a_2^* + \beta r_2^* - \omega < a_3^* + \beta r_3^* - \omega$  where  $r_1^*, r_2^* > 0$  implies that  $a_2^* - a_3^* < \beta\left(\frac{2\beta(\omega - a_3^*) - k}{2\beta^2} - \frac{2\beta(\omega - a_2^*) - k}{2\beta^2}\right)$ , which, after simplifying, implies  $0 < 0$ . Reversing the inequality in the initial assumption obviously leads to the same contradiction. Therefore, when  $\alpha = 1$  and  $\beta > \beta^*$ , it must be the case that  $a_2^* + \beta r_2^* - \omega = a_3^* + \beta r_3^* - \omega$ .

## Chapter 4

### Leader Survival, Natural Disasters, and International Conflict

A growing literature studies how natural disasters affect the likelihood of conflict within and between states. While there is some agreement on the conditions under which disasters matter for civil conflict, there is no consensus on the question of whether disasters matter for interstate conflict (e.g., Nelson 2010; Kelman 2012; Akcinaroglu, DiCicco and Radziszewski 2011). This paper considers the implications of recent research on leader survival for the disasters-conflict nexus. Specifically, I argue that disasters may indirectly affect the risk of international conflict through their effects on leader survival. Leaders of relatively inclusive “large winning coalition” systems are sensitive to disaster deaths, while autocratic leaders who rely on a small coalition of supporters are vulnerable to revolutionary threats catalyzed by disasters (Quiroz Flores and Smith 2013, 821-822). When large-coalition leaders fear irregular removal and post-tenure punishment (e.g., exile or imprisonment), international conflict may become an attractive policy tool. Alternatively, insecure large-coalition leaders anticipating regular removal from office should become less belligerent (Chiozza and Goemans 2011, 32-35). This paper departs from existing theory in arguing that not all threats to survival should matter for international conflict.<sup>1</sup> I argue that international conflict is not necessarily a useful tool for leaders concerned with threats from domestic extra-institutional sources. Since disasters are supposed to affect the survival of small-coalition leaders through domestic extra-institutional channels (e.g., revolutionary movements), they should not affect the international conflict behavior of small-coalition leaders in ways existing theory expects.

---

<sup>1</sup>To be clear, this variable is not novel to the more general literature on leader survival (e.g., Smith 2008).

I test this theory using data from the International Disaster Database (Guha-Sapir, Below and Hoyois 2015). I find a positive association between disaster deaths and international conflict initiation in large-coalition systems where leaders face irregular removal from office. However, in most models I find the same result for large-coalition leaders facing regular removal. Overall, there is little statistical evidence that the relationship between disasters and conflict differs across the two groups: both appear slightly more likely to initiate international conflicts as deaths from disasters increase. Small-coalition leaders facing regular removal are no more or less likely to initiate international conflicts as disaster impacts (both deaths and disaster events) increase, consistent with the theory. Yet, small-coalition leaders facing irregular removal appear to be *less* likely to initiate international conflicts when disaster events occur. Given evidence that disaster events increase the risk of removal for small-coalition leaders (Quiroz Flores and Smith 2013), we would expect that leaders among that group facing the possibility of irregular removal should be *more* likely to fight. As such, this finding is inconsistent with recent research on leader survival and international conflict and suggests that future research on the topic should consider conditions under which different types of threats matter.

The paper makes a number of contributions. First, it contributes to research on climate change and political violence. Prevailing wisdom holds that the effects of climate-related disasters will matter mostly for domestic politics and human security. While true, this overlooks links between domestic turmoil and international conflict. Critics of the climate-conflict literature have repeatedly called for theory-driven contributions to this research agenda (e.g., Nordas and Gleditsch 2007, 633; Gleditsch 2012, 6; Salehyan 2008, 320; Streich and Mislan 2013). This paper answers that call. Second, the paper advances theories of political survival by qualifying the conditions under which international conflict might benefit leaders. Threats to leader survival are varied (e.g., Smith 2008, 780), yet theories of leader survival and international conflict

assume that any factor that affects the risk of removal is relevant for international conflict (Chiozza and Goemans 2011). Though the occurrence of disasters increases the risk that small-coalition leaders lose office (Quiroz Flores and Smith 2013, 838), I show that disasters do not appear to affect the conflict behavior of small-coalition leaders as existing theory would expect. Finally, the paper contributes to a growing literature that goes beyond the traditional dichotomy of democracy versus autocracy in studying the effects of domestic institutions on foreign policy (e.g., Weeks 2012).

The paper proceeds as follows. The second section briefly reviews the existing literature on natural disasters and conflict. The third section develops a theory of natural disasters, leader survival, and international conflict, deriving testable hypotheses. The fourth section presents a research design to evaluate those hypotheses. The fifth section presents the results of the empirical tests, discusses the implications of the results, and describes a number of robustness checks. The sixth section concludes.

#### 4.1 Disasters and Conflict

The United Nations Office for Disaster Risk Reduction defines a disaster as “[a] serious disruption of the functioning of a community or a society involving widespread human, material, economic or environmental losses and impacts, which exceeds the ability of the affected community or society to cope using its own resources.”<sup>2</sup> Floods, droughts, earthquakes, hurricanes, and tsunamis are well-known examples of natural events that might constitute disasters. Indeed, whether an event is a “disaster” depends on the extent of the human and economic damage it causes. A hurricane in a developed country might cause minimal damage, while the same hurricane could cause

---

<sup>2</sup>See <http://www.unisdr.org/we/inform/terminology>. For a review of various definitions of “disaster,” see Perry (2007) and Quarantelli (2005). The International Disaster Database (Guha-Sapir, Below and Hoyois 2015) records an event as a disaster if it causes at least ten deaths, affects at least one hundred people, causes authorities to declare a state of emergency, or leads to a call for international assistance.

widespread death and suffering in an underdeveloped state. As such, the hurricane would only count as a “disaster” in the latter case.<sup>3</sup>

Why would natural disasters influence the probability of conflict? Existing literature on disasters and conflict focuses primarily on domestic conflict (e.g., Drury and Olson 1998; Brancati 2007; Nel and Righarts 2008; Omelicheva 2011; Bergholt and Lujala 2012; Slettebak 2012; Nardulli, Peyton and Bajjalieh 2013). For instance, natural disasters may increase societal grievances while simultaneously reducing state capacity, creating a window of opportunity for civil conflict (Nel and Righarts 2008, 166). Additionally, disasters can aggravate scarcity problems, helping opposition movements attract recruits (Brancati 2007, 722). These effects vary across contexts. Indeed, Omelicheva (2011, 463) argues that preexisting institutions and domestic conditions explain most of the negative impacts attributed to disasters. Besley and Persson (2011, 5) find that disasters – which they argue serve as “negative shocks to wages and positive shocks to aid flows” – interact with weak institutions to affect domestic political violence. In any case, disasters can sometimes have dramatic political consequences.

While most work focuses on disasters and *civil* conflict, some scholars emphasize the international implications of disasters (e.g., Yim et al. 2009; Nelson 2010; Kelman 2012; Akcinaroglu, DiCicco and Radziszewski 2011). For example, disasters could either provide opportunities for rapprochement or exacerbate conflicts between strategic rivals (e.g., Kelman 2012, 14). These studies have not produced conclusive results. Though major disasters tend to increase the probability of conflict initiation, there are no clear cases where a major disaster (at least 10,000 deaths) was directly related to the initiation of a serious conflict (Nelson 2010, 174). Case studies suggest that disasters may interact with domestic factors (e.g., civil strife) to indirectly affect

---

<sup>3</sup>There is evidence that disasters are less frequent and less severe in developed, democratic countries (e.g., Kahn 2005; Toya and Skidmore 2007; Plümper and Neumayer 2009; Keefer, Neumayer and Plümper 2011; Quiroz Flores and Smith 2013).

international conflict, but no systematic test confirms this hypothesis (Nelson 2010). Streich and Mislán (2013, 16) conclude from this that “[d]isasters generally do not lead to the initiation of conflict,” which is a fair assessment.

Yet, there are reasons to expect that disasters should be associated with international conflict in ways that existing research does not anticipate. First, disasters may affect the risk of leaders losing office (Quiroz Flores and Smith 2013). Second, leaders’ incentives to use international conflict may change when they become less secure in office (Chiozza and Goemans 2011; Goemans 2000). My theory builds on these two sets of findings to suggest a plausible link between disasters and international conflict.

## 4.2 Theory

To develop a theory of the relationship between natural disasters and international conflict, I draw on selectorate theory (Bueno de Mesquita et al. 2003) and Chiozza and Goemans’s (2011) theory of how the risk and consequences of losing office influence conflict behavior. Selectorate theory says that the proportion of constituents whose support is necessary for a leader to retain power (the size of the winning coalition) shapes domestic and foreign policy. In large-coalition systems, leaders tend to provide public goods to maintain power, their supporters are less loyal, and leaders have few slack resources to use as compensation for perceived failure. In small-coalition systems, leaders depend mostly on distributing private goods, their supporters are more loyal, and leaders retain more state resources for themselves. I consider the implications of disasters for leader survival and international conflict in large-coalition and small-coalition systems separately.

Insofar as disasters cause deaths and damage in large-coalition systems, leaders should face an increased risk of losing office.<sup>4</sup> Quiroz Flores and Smith (2013, 828)

---

<sup>4</sup>Drury and Olson (1998, 154-155) detail many cases (winning coalition size,  $W \in \{0, 0.25, 0.5, 0.75, 1\}$ , from Bueno de Mesquita and Smith (2010) appears after year) – Haiti (1954,  $W = 0.5$ ), Nicaragua (1954,  $W = 0.5$ ), Mexico (1985,  $W = 0.5$ ), Guatemala (1976,  $W = 0.5$ ),



argue that this is so because these leaders have incentives to provide public goods like security, and their constituents will evaluate them on this policy dimension. Although this incentive should generally cause leaders to invest in disaster preparedness, when disasters *do* occur, supporters may doubt a leader’s competence. Because the extent of a disaster depends partially on the preparations and response of a government (e.g., Kahn 2005, 283), it may reveal information about a leader’s competence to govern.<sup>5</sup> Coalition members in large-coalition systems are intolerant of incompetence since they can expect to do similarly well under a challenger with identical incentives to provide public goods (Bueno de Mesquita et al. 2003, 279). When large-coalition leaders fail to prevent deaths from disasters, coalition members will perceive them as being incompetent, and their security in office should be threatened. Empirically, these leaders are more likely to lose office as deaths from disasters increase (Quiroz Flores and Smith 2013, 840).

Large-coalition leaders might fear regular or irregular removal. Consider the risk of irregular removal first. An example is Chile, where public support for coups increased following the 2010 earthquake (Carlin, Love and Zechmeister 2014, 11). Chiozza and Goemans (2011, 18-32) argue that insecure leaders anticipating irregular removal might gain from international conflict through two mechanisms. First, a leader may “fight for survival” by sending suspected coup plotters within his regime to the front lines to eliminate an internal threat *à la* Saddam Hussein’s 1990 invasion of Kuwait (Chiozza and Goemans 2011, 27). Second, a leader may “gamble for survival” by behaving belligerently in the hopes of obtaining new resources to distribute to supporters. These could be public goods (e.g, national prestige, enhanced security through

---

Bangladesh (1970, *W* missing) – where unrest wrought by disasters heightened the risk of or led to irregular removal for a number of leaders and regimes. For an analysis of the Guatemala case, see Gawronski and Olson (2013).

<sup>5</sup>In some cases, disasters appear to be beneficial for leaders (e.g., Healy and Malhotra 2009; Bechtel and Hainmueller 2011; Gasper and Reeves 2011; Lazarev et al. 2014). However, the findings in Quiroz Flores and Smith (2013, 841) suggest that, when disasters do help leaders, the effects are small relative to the negative effects they may have.

victory in war) or private goods (e.g., natural resources) (See also Goemans 2000; Downs and Rocke 1994, 364). Scholars frequently invoke the 1904 Russo-Japanese War and the 1982 Falklands War as instances where insecure leaders sought to use international conflict to ease domestic turmoil (Levy and Thompson 2010, 101). From the perspective of a leader facing post-tenure punishment, international provocation might be worthwhile if it improves survival chances. Empirically, even crises short of war can help these leaders retain office (Chiozza and Goemans 2011, 89).<sup>6</sup> As such, both high-intensity (e.g., war) and low-intensity (e.g., threats) conflict might benefit leaders.

This implies that as disasters worsen in large-coalition systems where leaders might fear irregular removal, leaders should become more prone to engaging in international conflicts. International conflict can introduce an additional policy dimension on which constituents may evaluate leaders, and success on this dimension may compensate for perceived failure in other contexts. Thus, these leaders may want to “gamble for survival.” Some suggestive examples help illustrate the logic. In the 1998 war between Ethiopia and Eritrea, Abbink (2003, 221) argues that Ethiopian president Meles Zenawi faced pressure to respond forcefully to Eritrea’s border violations because his government was “in the midst of serious political problems and opposition from large sections of the public.” One major problem was managing the effects of unusually damaging El Niño-induced flooding that killed hundreds of people in 1997 (Glantz 2000; Guha-Sapir, Below and Hoyois 2015). Given that Ethiopia had only recently taken steps toward democracy and was in the midst of a hostile rivalry with Eritrea, it would be reasonable to expect that Zenawi feared irregular removal through a coup or popular uprising. Tures (2009, 74) argues that Iran’s belligerent foreign policy rhetoric under President Ahmadinejad was driven by efforts to “[distract] the Iranian people from the deleterious shortcomings of its increasingly ineffective pres-

---

<sup>6</sup>The extent to which conflict outcomes affect leader tenure varies depending on domestic conditions and the conditions that led to conflict (e.g., Croco 2011).

ident.” While many factors contributed to popular dissatisfaction with the Iranian government during Ahmadinejad’s tenure, one source of public discontent was the government’s response to earthquakes (Valinejad 2012; Torbati 2012). In 2013, *The New York Times* called public anger over the Philippine government’s response to a typhoon the “biggest challenge” of President Aquino’s presidency (Jacobs 2013). The Philippines is mostly democratic but has experienced a number of attempted coups in the past few decades. Shortly after, Aquino took an increasingly hard line (rhetorically) against China in disputes over islands in the South China Sea, comparing his situation to that of “Czechoslovakia’s leaders in the late 1930s” (Bradsher 2014). The theory would explain this as a case where a leader sought to boost his government’s popularity, which was partially a function of a natural disaster, by framing a standoff with a foreign country as dangerous and taking a strong stance (a potentially risky choice). Finally, in their case studies of international conflict in Central America from 1840-1918, Chiozza and Goemans (2011, 120-121) note that a cholera epidemic contributed to “undermining [leader of the Federal Republic of Central America] Morazán’s legitimacy,” which was an important factor in Morazán’s decision to make war against Guatemala to preserve the union.

Additionally, a leader may fear a military coup if generals believe that she has done a poor job governing. For example, Pakistan has occasionally reverted to military rule when democratically-elected leaders have proven unable to cope with crises, and there were “[r]umours of an army coup” following flooding in Pakistan in 2010 (The Economist 2010). A recent report by the United States’ Congressional Research Service expressed concern that popular dissatisfaction with the political system brought on by environmental crises might lead Pakistan to behave more belligerently toward India (Vaughn et al. 2010). Here and in other similar cases, it is plausible that the “fighting for survival” mechanism may succeed in distracting the military (at least temporarily) during a period of domestic upheaval, while simultaneously winning

public favor through the “gambling for survival” mechanism.

***H1:** Leaders of large-coalition systems who face irregular removal initiate more international conflicts as the human impacts of natural disasters increase.*

Leaders of large-coalition systems who are concerned foremost with not being voted out of office (e.g., stable, consolidated democracies) have little reason to fear forcible removal. As such, holding office is not valuable enough to make international conflict worthwhile, especially if heightened insecurity makes a leader less able to insulate herself from any negative repercussions of conflict. While conflict might help a leader retain power, it is also risky. The costs and risks of engaging in conflict while in a relatively less secure position deter conflict for these leaders. This is the “peace through insecurity” mechanism (Chiozza and Goemans 2011, 32-35). As the effects of disasters worsen in these systems, leaders should feel less secure in office and should find international conflict a less desirable option. For example, some commentators attributed US President Bush’s more dovish foreign policy during the first part of his second term to the public backlash against his response to Hurricane Katrina in 2005 (Daalder and Lindsay 2005). Periods of relative dovishness in India’s foreign policy toward Pakistan have also been attributed to disasters (Kelman 2012, 37).

***H2:** Leaders of large-coalition systems who face regular removal from office initiate fewer international conflicts as the human impacts of natural disasters increase.*

Now consider the role of disasters in small-coalition systems. Here, a coalition member’s fate depends critically on whether the incumbent is in power since the likelihood of being included in an alternative winning coalition is low compared to a large-coalition system. This induces loyalty to an incumbent leader. Additionally,

small-coalition leaders have more slack resources to use as compensation for shocks than do large-coalition leaders (Bueno de Mesquita et al. 2003, 285). Since coalition members are more loyal and the leader is better able to compensate any losses they do experience, human impacts of disasters *per se* should not be destabilizing for small-coalition leaders. Consistent with this theory, Quiroz Flores and Smith (2013, 837) find that leaders in small-coalition systems are unaffected by disaster deaths. It follows that deaths from disasters should be unrelated to international conflict behavior in small-coalition systems.

**H3:** *Leaders of small-coalition systems are no more or less likely to initiate international conflicts as the human impacts of disasters increase.*

Still, disasters might catalyze threats to small-coalition leaders through extra-institutional channels. A disaster can provide a focal point for anti-regime sentiments while simultaneously corralling potential revolutionaries into geographically compact areas, facilitating the recruitment efforts of opposition groups (Quiroz Flores and Smith 2013, 821-822). In principle, this effect operates in both large- and small-coalition systems, though this incentive is weak in large-coalition systems (Smith 2008, 791). So, while dictators may worry little about the human impacts of disasters, the disaster-induced threat of revolution looms large when citizens can overcome coordination problems. Quiroz Flores and Smith (2013, 828) argue that disaster *events* – independent of the human toll – should be associated with an increased likelihood of losing power in small-coalition systems, and they find empirical support for this hypothesis. Nel and Righarts (2008, 159) recount how an earthquake in 465/464 B.C. helped “Messenian helots who were enslaved when Sparta” initiate a revolutionary challenge to Spartan rulers (which ultimately failed). The 1972 earthquake in Nicaragua and the 1976 earthquake in Guatemala have also been linked to increased

leader insecurity through similar mechanisms (Valenta and Valenta 1987; Jonas 1991; Kelman 2012).

Given that disasters may affect the survival of small-coalition leaders, Chiozza and Goemans's (2011) theory would predict that these leaders should become more or less likely to use international conflict depending on how they expect to lose office. The question is whether the fighting or gambling for survival mechanisms would diminish a threat that comes from outside the institutional system governing leader turnover. In general, leaders might be threatened through three sources. First, a threat might come domestically through existing institutional channels (e.g., members of winning coalition). Second, a threat might come from domestic extra-institutional channels (e.g., a rebel group). Finally, threats may come from outside a given state (e.g., a foreign rival state). I will focus on the first two.

From the discussion above and the argument in Quiroz Flores and Smith (2013), the way that disasters affect large-coalition leaders is by harming groups that are necessary for them to retain power. Conversely, the way that disasters threaten small-coalition leaders is by affecting groups that exist outside of the electorate. Thus, while it is a disaster that creates a threat in both cases, the "source" of the threat differs across the two regime types. While others have pointed out that threats to leaders come from a variety of sources (e.g., Smith 2008), theories of leaders and international conflict ignore the source of a threat. Although Chiozza and Goemans (2011, 5) do consider revolutionary threats as a potential source of interstate conflict, their argument focuses on cases where (1) the revolutionary threat comes from groups that enjoy safe havens across international borders or (2) the threat of revolution is internal to the existing regime (e.g., a rival military faction). In the first case, a conflict becomes international because of the need to eliminate a threat that happens to cross national borders. In the second case, fighting or gambling for survival might reduce internal opposition. However, small-coalition leaders should be well-equipped to

offset any negative impacts of disasters that affect their winning coalitions. Whether or not revolutionaries typically act from across borders is an empirical question that is outside the scope of this paper. It is not clear that natural disasters, if they create a revolutionary threat, should systematically increase the likelihood that that threat comes from across a border. It is unclear how the fighting or gambling for survival mechanisms would make international conflict a useful response to an extra-institutional domestic threat like a revolutionary movement. In terms of selectorate theory (Bueno de Mesquita et al. 2003), groups outside the “selectorate” receive little benefit from any goods the government provides or would obtain from international conflict. Similarly, a leader cannot send these opponents off to war since she does not control the revolutionaries. It would seem odd for the leader of a small-coalition regime to send armed forces abroad exactly when there is a need to defend against a potential revolution. If this argument is correct, then disasters should not affect the international conflict incentives of small-coalition leaders even while they may increase the risk of losing office for small-coalition leaders.

***H4:** Regardless of the anticipated consequences of losing office, leaders of small-coalition systems are no more or less likely to initiate international conflicts as the frequency of disaster events increases.*

### 4.3 Research Design

Testing the theory requires data on natural disasters, domestic institutions, and international conflict. I start with a data set of leader-years from the *Archigos* data set (version 4.0) (Goemans, Gleditsch and Chiozza 2009).<sup>7</sup> To measure conflict propensity, I count the number of militarized interstate disputes (MIDs) a country initiated

---

<sup>7</sup>Earlier versions of this paper used *Archigos* version 2.9. I have updated all the analyses with version 4.0 of the data, which allows the extension of the temporal domain from 2004 to 2007 (the full data set extends to 2014, though the domain here is now limited by the availability of data from the Correlates of War and the *W* score discussed below).

during a given year (or during a given portion of a year if a leader exited office mid-year) using the Correlates of War (COW) Militarized Interstate Disputes data set (Jones, Bremer and Singer 1996). MIDs are “united historical cases in which the threat, display or use of military force short of war by one member state is explicitly directed towards the government, official representatives, official forces, property, or territory of another state” (Jones, Bremer and Singer 1996, 168). This is the primary dependent variable in the analysis below.<sup>8</sup> I include all MID initiations since even lower-level acts of conflict may affect leader survival and thus are relevant for the theory (Chiozza and Goemans 2011, 89), though I also report the results using a count of “high-hostility” MIDs that are coded as having hostility levels 4 and 5 (the use of force and war, respectively).

The key independent variables are (1) human impacts of disasters, (2) disaster events, (3) domestic institutions of leader survival, and (4) the anticipated means and consequences of losing office. Using the International Disaster Database (Guha-Sapir, Below and Hoyois 2015),<sup>9</sup> I count both the total number of disaster events and the total number of deaths from natural disasters that occurred in a given country during the previous year, or during the previous portion of a year for years in which a leader took power mid-year. I lag the disaster variables by one year to ensure that any disasters occurred (or started) prior to the onset of conflict.<sup>10</sup> While others distinguish between rapid- and slow-onset disasters, I am mainly interested in the human toll of disasters in large-coalition systems, and as such I include both. It may be the case that coalition members are more or less forgiving for certain types of disasters,

---

<sup>8</sup>This variable counts all instances where a state is coded “1” for both the “sidea” and “orig” variables in the MID participant-level (v. 4.01) data set. The “sidea” variable indicates that a state is on the initiating side of a conflict, and the “orig” variable indicates that state was an initiator on the first day that the conflict started.

<sup>9</sup>For a critical review of existing data sources on disasters, see Tschogl, Below and Guha-Sapir (2006), and for the EM-DAT data base specifically, see Guha-Sapir, Hargitt and Hoyois (2004). Despite its limitations, EM-DAT provides the most comprehensive data on natural disasters spatially and temporally (Kelman 2012, 74).

<sup>10</sup>See the Appendix for a description of the data and procedures for dealing with missing countries.



but this is difficult to know *ex ante*. Alternatively, some types of disasters might pose greater threats to small-coalition regimes. For example, earthquakes may more effectively help rebels overcome collective action problems than a drought (or maybe not). Because I do not have strong theoretical expectations about these relationships, I follow Quiroz Flores and Smith (2013) in including all types of natural disasters. I take the natural log of the disaster variables, adding one to each observation to account for cases with zero deaths or events.

To operationalize winning-coalition size, I use the  $W$  score from Bueno de Mesquita and Smith (2010). The variable is a function of component measures in the Polity IV data set (*xrcomp*, *xropen*, and *parcomp*) and the Cross-National Time Series Data Archive’s measure of regime type (Banks 2011). The variable ranges between 0 and 1 in increments of .25, with values closer to 1 indicating larger winning-coalitions. I use this measure to create two categories of winning-coalition size. I code countries that score 0.5 or higher as large-coalition systems, and those that score below 0.5 as small-coalition systems.<sup>11</sup>

I use the Polity IV Regulation of Chief Executive Recruitment (*xrreg*) variable (Marshall, Jaggers and Gurr 2010) to measure how a leader expects to be removed from office. This is similar to the approach taken by Goemans (2000, 56-57), who uses “mixed” regimes (regimes that are neither fully autocratic nor fully democratic) to proxy for leaders who might anticipate irregular removal. Alternatively, Chiozza and Goemans (2011) indirectly model the risk of losing office, since they theorize that the risk of international conflict and the risk of losing office are endogenous. However, their estimates of removal risks include components of domestic regimes that factor into the  $W$  score in Bueno de Mesquita and Smith (2010), which I use to code large-coalition and small-coalition systems. Instead, I use the *xrreg* variable, a

---

<sup>11</sup>Bueno de Mesquita and Smith (2008, 193) refer to predicted coalition sizes of .3 and .46 as “substantial,” so 0.5 would seem a reasonable and possibly even conservative cut point for separating out large- from small-winning-coalition systems. Below I also report results for an alternative threshold of  $W \geq 0.75$ .

three-category ordinal variable measuring the procedures of leader removal in a given country. (Note that this is *not* one of the components of the  $W$  score.) This indicator may not dynamically capture the threat of irregular removal, but where it does code irregular removal we should be more confident that leaders do plausibly fear an irregular exit.<sup>12</sup> A score of three indicates regular removal, lesser scores indicate irregular removal.<sup>13</sup> Using these two sources of data, I code a leader's membership in one of four categories of institutions: large-coalition/regular-removal, large-coalition/irregular-removal, small-coalition/regular-removal, and small-coalition/irregular-removal.<sup>14</sup>

### *Control Variables*

I control for potential confounders that might be correlated with disaster impacts and conflict (Ray 2003; Achen 2005). First, since developed countries are better able to prevent impacts from disasters (Kahn 2005) and may also be less likely to engage in international conflict (Gartzke 2007), I include a measure of real GDP (logged and lagged by one year) from Gleditsch (2002). Second, larger countries might experience more disasters simply by virtue of their size and they may also share more borders which could lead to a higher likelihood of fighting over territory (Bremer 2000, 30-31; Gibler 2007). To account for this, I include a (logged) measure of land area in square kilometers (World Bank 2013).<sup>15</sup> I also control for military capabilities using the Correlates of War Composite Indicator of National Capabilities (CINC) score (Singer, Bremer and John Stuckey 1972), since well-equipped militaries might be

---

<sup>12</sup>In Table 4.11 in the Appendix, I report the empirical distribution of removal type by my coding of regime type.

<sup>13</sup>The first and third categories of *irreg* clearly correspond to irregular and regular removal, respectively. The second category is less clear, but more closely corresponds to irregular removal. The exact wording is reproduced in the Appendix.

<sup>14</sup>A random sample of large-coalition irregular removal country-years in the data include Guatemala (1962), Czechoslovakia (1963), Albania (1982), Turkey (1983), and Colombia (1968). Large-coalition regular removal country years include Bolivia (1989), Venezuela (1976), South Africa (1953), France (1972), and Canada (1952). Small-coalition country-years include Iraq (1963), Kuwait (1998), Nepal (2003), Equatorial Guinea (1972), and Burkina Faso (1991).

<sup>15</sup>Where this measure is missing, I manually coded land area in squared kilometers using Wikipedia.

able to successfully manage the effects of a disaster and prevent deaths, and stronger countries might be more conflict-prone (or may have more opportunities to engage in conflict) (Bremer 2000, 25). I lag this variable by one year. In some of the models below, I also account for additional determinants of conflict with a count of years since the last MID, a dummy for the post-Cold War period, and a count of years that a leader has been in office. I also present the results controlling for a categorical decade variable to account for conflict opportunity across time.<sup>16</sup> The statistical tests use data for the years 1950-2007. The spatial and temporal domains of the data are limited by *Archigos* and the data from Bueno de Mesquita and Smith (2010). Table 4.4 in Section 4.7.2 of the Appendix reports summary statistics for the key variables.

#### 4.4 Results

I estimate negative binomial regression models with leader-clustered standard errors where the outcome variable is a count of conflicts initiated by a leader during a given year.<sup>17</sup> I interact the disaster deaths variable with the large-coalition/irregular-removal and large-coalition/regular-removal dummy variables, leaving small-coalition systems out as the baseline category. To evaluate *H1* and *H2*, Table 4.1 reports the estimated marginal effects of disaster deaths on conflict initiation in small-coalition systems (the base coefficient) and large-coalition regular and irregular removal systems.<sup>18</sup> Consistent with *H1*, the positive coefficient on the interaction term between large-coalition systems with irregular removal and disaster deaths in Models 1 and 2 suggests that large-coalition leaders who face irregular removal from office initiate more international conflicts as deaths from natural disasters increase, though the re-

---

<sup>16</sup>The results are also robust to using year-fixed effects instead. See Section 4.7.5 in the Appendix.

<sup>17</sup>I use a negative binomial model rather than a Poisson regression model because of overdispersion in the outcome variables. Section 4.7.12 of the Appendix reports a list of observations where leaders of large-coalition systems initiated at least one MID following a year with an above-average number of disaster deaths.

<sup>18</sup>I carried out the significance tests for the interaction terms according to the procedures outlined in Hilbe (2007, 523-526).

relationship is only significant at the  $p < .10$  level in Model 1. Model 2, which uses an alternate threshold of 0.75 to separate large- from small-winning coalition systems, provides stronger support for  $H1$ . Models 3 and 4 add a number of additional covariates including decade-fixed effects to Models 1 and 2. The pattern here is similar: the result for large-coalition irregular removal leaders is statistically significant in both cases but stronger in the model that uses the alternate  $W$  threshold. Finally, Models 5 and 6 include only high-hostility MIDs in the outcome variable. While the estimated coefficient on disasters is not statistically significant for either subset of large-coalition leaders in Model 5, it is still positive for both. In Model 6, the effect of disasters is statistically significant and positive only among large-coalition leaders facing irregular removal.

Notably, and against the expectations of  $H2$ , disasters also seem to be positively associated with conflict in large-coalition systems where leaders likely face *regular* removal. Indeed, in four of the six models (Models 1-4), the marginal effect of disasters for large-coalition regular removal leaders is positive and significant. Further, in a test of the linear restriction that the effects are the same across the regular and irregular regimes, only in Models 2 and 6 is there even weak evidence that the coefficients are statistically distinguishable from one another (the  $p$ -values in the  $F$ -tests are 0.07 and 0.2, respectively).

Overall, Table 4.1 provides support for  $H1$ , but it provides less evidence that the positive association between disasters and conflict is exclusive to large-coalition, irregular removal regimes.<sup>19</sup> The models so far attempt to account for differences within regime types by controlling for a number of potentially confounding factors that might affect disasters and international conflict propensity. To address possible concerns about selection bias or that disaster deaths and conflict are endogenous to regime

---

<sup>19</sup>When considering a subset of large-coalition, regular removal leaders with lower-than-mean GDP and CINC scores, the coefficient on the disaster deaths variable is negative, consistent with  $H2$ , though not statistically significant. These results are available upon request.

Table 4.1: Disasters, Coalition Size, Removal Type, and International Conflict (Negative Binomial Regression)

	<i>Dependent variable: MIDs Initiated</i>					
	Base	$W \geq 0.75$	Extra controls	$W \geq 0.75,$ Extra controls	Hostile MIDs	$W \geq 0.75,$ Hostile MIDs
	(1)	(2)	(3)	(4)	(5)	(6)
Disaster Deaths <sub>t-1</sub> (log)	-0.034 (0.041)	0.007 (0.030)	-0.029 (0.038)	0.005 (0.028)	-0.014 (0.044)	0.008 (0.035)
Large-Coalition, Regular Removal (LC-R)	-0.819* (0.236)	-0.763* (0.193)	-0.853* (0.238)	-0.836* (0.199)	-0.917* (0.305)	-0.893* (0.256)
Large-Coalition, Irregular Removal (LC-I)	-0.279 (0.212)	-0.835* (0.228)	-0.257 (0.206)	-0.852* (0.222)	-0.302 (0.242)	-1.142* (0.337)
Disaster Deaths <sub>t-1</sub> (log) × LC-R	0.082* (0.048)	0.039 (0.040)	0.082* (0.046)	0.044 (0.038)	0.055 (0.053)	0.030 (0.048)
Disaster Deaths <sub>t-1</sub> (log) × LC-I	0.080 (0.052)	0.138* (0.059)	0.072 (0.049)	0.142* (0.057)	0.048 (0.062)	0.127 (0.081)
$\hat{\beta}_{Disaster\ Deaths}$ for Large-coalition, Irregular Removal Leaders	0.046 <sup>†</sup> (0.0350)	0.146* (0.050)	0.043 <sup>†</sup> (0.033)	0.147* (0.050)	0.034 (0.047)	0.136* (0.074)
$\hat{\beta}_{Disaster\ Deaths}$ for Large-coalition, Regular Removal Leaders	0.048* (0.028)	0.046* (0.028)	0.053* (0.026)	0.048* (0.027)	0.041 (0.033)	0.038 (0.033)
CINC <sub>t-1</sub>	5.515* (1.754)	5.564* (1.618)	5.727* (1.673)	5.943* (1.551)	5.917* (2.437)	5.763* (2.222)
Land area (km <sup>2</sup> , log)	0.138* (0.038)	0.129* (0.038)	0.107* (0.039)	0.101* (0.039)	0.124* (0.047)	0.119* (0.047)
Real GDP <sub>t-1</sub> (log)	0.205* (0.039)	0.212* (0.040)	0.180* (0.039)	0.183* (0.039)	0.123* (0.049)	0.125* (0.048)
Peace Years			-0.016* (0.004)	-0.016* (0.004)	-0.020* (0.007)	-0.020* (0.007)
Post-Cold War Dummy			-0.471* (0.275)	-0.438* (0.264)	-0.704* (0.331)	-0.653* (0.309)
Leader Tenure			-0.016* (0.008)	-0.021* (0.009)	-0.014 (0.011)	-0.021* (0.011)
Constant	-5.332* (0.571)	-5.380* (0.562)	-4.356* (0.686)	-4.394* (0.665)	-4.235* (0.855)	-4.281* (0.833)
Decade-fixed Effects	N	N	Y	Y	Y	Y
Observations	6,843	6,843	6,843	6,843	6,843	6,843
Log Likelihood	-3,373.894	-3,364.967	-3,304.790	-3,295.242	-2,501.214	-2,490.623
$\theta$	0.630* (0.062)	0.637* (0.063)	0.734* (0.075)	0.740* (0.076)	0.480* (0.054)	0.492* (0.056)
Akaike Inf. Crit.	6,765.789	6,747.934	6,645.579	6,626.483	5,038.428	5,017.245

Note: <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

All models include CINC score, land area, and real GDP.

Models 3-6 include decade-fixed effects, peace years, post-Cold War dummy, and leader tenure.

type in ways that are not captured in the analysis, I estimated an alternative model replacing the disaster deaths variable with a count of earthquake events. Deaths from earthquakes may still be endogenous to regimes, but the timing of earthquake events is random.<sup>20</sup> Table 4.2 shows that the main result from Table 4.1 – the positive association between earthquakes and conflict initiation in both types of large-coalition systems but not small-coalition systems – also carries over to this specification.

---

<sup>20</sup>The results are also robust to controlling for the composite *polity2* score from Marshall, Jaggers and Gurr (2010), though doing this is problematic since components of this measure are used to construct both the measure of  $W$  and my measure of removal type. These results are available upon request.

Table 4.2: Earthquake Events, Coalition Size, Removal Type, and International Conflict (Negative Binomial Regression)

	<i>Dependent variable: MID Initiations</i>	
	Base (7)	Extra controls (8)
Earthquake Events <sub>t-1</sub>	-0.023 (0.099)	0.007 (0.093)
Large-Coalition, Regular Removal (LC-R)	-0.716* (0.205)	-0.737* (0.210)
Large-Coalition, Irregular Removal (LC-I)	-0.235 (0.184)	-0.211 (0.178)
Earthquake Events <sub>t-1</sub> × LC-R	0.323* (0.130)	0.295* (0.127)
Earthquake Events <sub>t-1</sub> × LC-I	0.324* (0.180)	0.258 (0.160)
$\hat{\beta}_{EarthquakeEvents}$ for Large-Coalition, Irregular Removal Leaders	0.301* (0.147)	0.265* (0.131)
$\hat{\beta}_{EarthquakeEvents}$ for Large-Coalition, Regular Removal Leaders	0.300* (0.087)	0.302* (0.089)
CINC	5.839* (1.908)	6.001* (1.805)
Land area (km <sup>2</sup> , log)	0.138* (0.038)	0.107* (0.039)
Real GDP <sub>t-1</sub> (log)	0.193* (0.038)	0.170* (0.038)
Peace Years		-0.016* (0.004)
Post-Cold War Dummy		-0.434* (0.249)
Leader Tenure		-0.015* (0.008)
Constant	-5.254* (0.546)	-4.298* (0.665)
Observations	6,843	6,843
Log Likelihood	-3,364.102	-3,295.959
$\theta$	0.633*** (0.061)	0.738*** (0.074)
Akaike Inf. Crit.	6,746.204	6,627.918

*Note:* †p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

Both models include CINC score, land area, and real GDP.

Model 8 includes decade-fixed effects, peace years, post-Cold War dummy, and leader tenure.

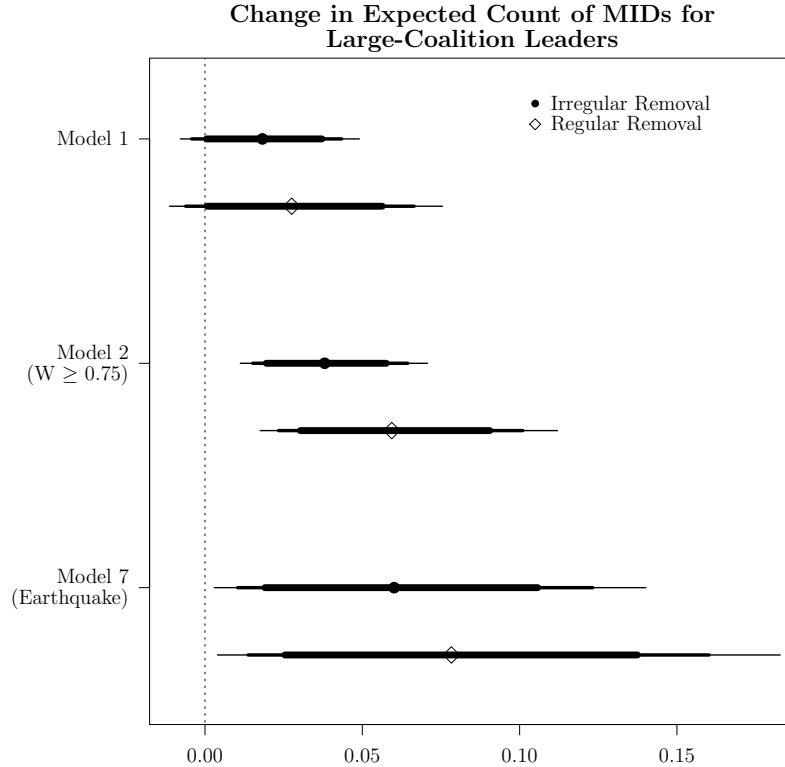


Figure 4.1: Expected Count of MIDs Analysis.

The thickest to thinnest lines represent the 80%, 90%, and 95% confidence intervals respectively. The control variables are held at their means within each regime type group, and the disaster deaths variable is increased from the mean to the mean plus one standard deviation within each group. In the results for Model 7, the earthquakes variable is changed from 0 to 1.

To see how disaster deaths affect a “typical” leader’s propensity for conflict, I analyze expected counts of MID initiations for the average leader in each regime category (Long 1997, 237, 224). I characterize the uncertainty associated with these estimates using 10,000 simulated sets of coefficients from the estimated sampling distributions of the estimated coefficients from Models 1 and 2 in Table 4.1 and Model 7 in Table 4.2. The coefficients are drawn from a multivariate normal distribution with mean  $\beta_j$  and variance  $\sigma_j$ , where  $\beta_j$  is a vector of estimated coefficients from model  $j$  and  $\sigma_j$  is the leader-cluster-adjusted variance-covariance matrix for model  $j$ . For each set of coefficients, I calculated the change in the expected count of MIDs when the



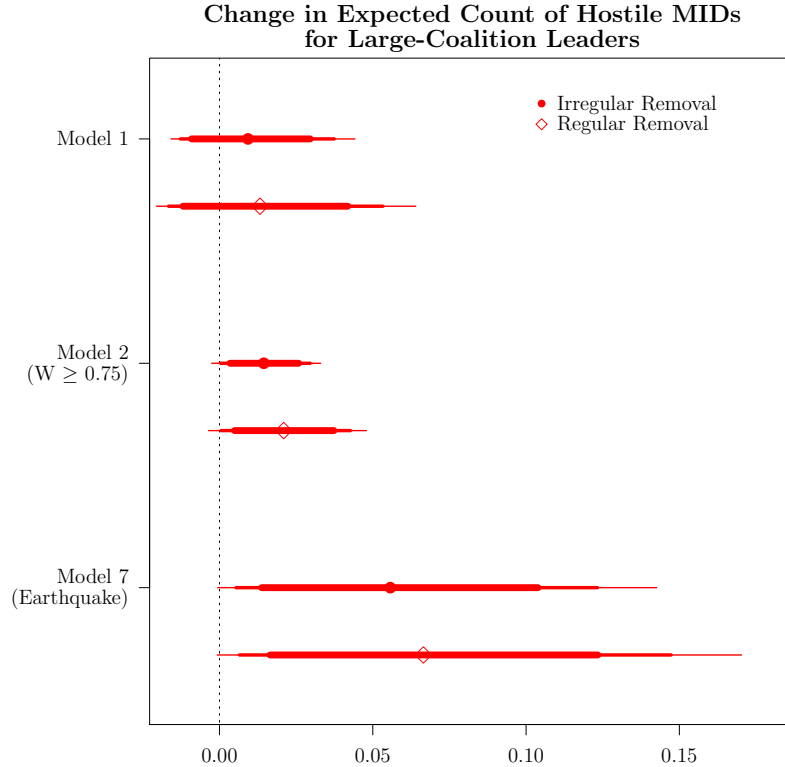


Figure 4.2: Expected Count of Hostile MIDs Analysis.

The thickest to thinnest lines represent the 80%, 90%, and 95% confidence intervals respectively. The control variables are held at their means within each regime type group, and the disaster deaths variable is increased from the mean to the mean plus one standard deviation within each group. In the results for Model 7, the earthquakes variable is changed from 0 to 1.

control variables are set to their means within regime categories when the disaster deaths variable is increased from its mean to one standard deviation above its mean. I take the mean of the distribution of differences and the quantiles for the 80%, 90% and 95% confidence intervals and plot them in Figure 4.1. In Model 1, the change in expected count of MIDs for both types of leaders is small and statistically distinguishable from zero only when considering the 80% confidence interval (The mean percentage change is about 10% and 11% for each type of leader, respectively.) In Model 2, which uses the alternative threshold of  $W \geq 0.75$ , those changes increase to about 38% and 41%, respectively, and the 90% and 95% confidence intervals do

not include zero. Increasing the earthquake count from 0 to 1 in Model 7 leads to a statistically significant 35% change in the expected count of MID initiations for both groups of leaders. These changes are more modest when reestimating those models with only high-hostility MIDs included in the outcome variable (Figure 4.2). When including the covariates from Model 1, the change in expected count is only 7% for both groups, though the confidence intervals include zero. In Model 2, that change increases to about 30% and 32% for leaders facing irregular and regular removal, respectively. (The 90% confidence interval does barely exclude zero.) Finally, reestimating Model 7 using only high-hostility MIDs leads to an approximate increase in the expected count of MIDs of 46% for both groups at the 90% confidence level. Disasters are associated with modest increases in conflict propensity, but the relationship appears weaker and less precise when considering only high-hostility MIDs.

Table 4.3: Disasters, Removal Type, and Conflict in Small-Coalition Regimes (Negative Binomial Regression)

	<i>Dependent variable: MID initiations</i>			
	Regular Removal (9)	Irregular Removal (10)	Regular Removal (11)	Irregular Removal (12)
Disaster Deaths <sub>t-1</sub> (log)	0.063 (0.117)	-0.016 (0.062)	0.005 (0.085)	-0.029 (0.064)
Disaster Events <sub>t-1</sub> (log)	0.089 (0.656)	-0.465 <sup>†</sup> (0.314)	0.242 (0.473)	-0.451 <sup>†</sup> (0.339)
CINC	52.280 (91.433)	44.883 (70.849)	101.996 (107.695)	35.004 (72.814)
Land area (km <sup>2</sup> , log)	0.222* (0.124)	0.012 (0.087)	0.110 (0.144)	-0.004 (0.090)
Real GDP <sub>t-1</sub> (log)	0.080 (0.218)	0.346* (0.108)	-0.022 (0.185)	0.369* (0.100)
Peace Years			-0.044 (0.062)	-0.021 <sup>†</sup> (0.015)
Post-Cold War Dummy			0.374 (0.881)	-0.419 (0.357)
Leader Tenure			0.008 (0.014)	-0.025* (0.010)
Constant	-6.046* (2.326)	-4.900* (1.536)	-4.217* (2.491)	-3.775* (1.698)
Decade-fixed Effects	N	N	Y	Y
Observations	551	1,221	551	1,221
Log Likelihood	-172.330	-719.517	-157.783	-698.156
$\theta$	3.082 (4.577)	0.769* (0.155)	7.173 (18.544)	1.081* (0.251)
Akaike Inf. Crit.	356.660	1,451.034	345.566	1,426.311

*Note:* <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

All models include CINC score, land area, and real GDP.

Models 11 and 12 include decade-fixed effects, peace years, post-Cold War dummy, and leader tenure.

The third hypothesis stipulates that disaster deaths should not affect the conflict incentives of small-coalition leaders. Consistent with *H3*, the coefficient on the disaster deaths variable (this coefficient refers to the effect of disaster deaths for the baseline category of small-coalition systems) is close to zero and statistically insignificant across all of the models in Table 4.1. Finally, *H4* predicts that though disaster *events* should increase the hazard of losing office for small-coalition leaders, they

should not affect the conflict behavior of these leaders. Table 4.3 presents the results of an analysis that separates the data into subsets of small-coalition leaders facing regular and irregular removal. For small-coalition leaders facing regular removal, there is no significant effect of disasters on conflict behavior (Models 9 and 11). However, for small-coalition leaders facing *irregular* removal, the direction of the coefficients points to the opposite of what we would expect from Chiozza and Goemans’s (2011) theory: the coefficients on disaster events are negative in both Models 10 and 12. While this is nominally inconsistent with *H4* given that the hypothesis takes an agnostic stance in predicting a null effect, it is notable when interpreted in light of existing theory.

#### 4.5 Discussion

The results above provide mixed support for *H1*, consistent support for *H3*, qualified support for *H4*, and no support for *H2*. The appendix contains a number of robustness checks. Nearly all of the auxiliary analyses produce results similar to those reported here. This is the case when including year-fixed effects, country-fixed effects, controlling for disaster propensity with a count of disasters in a given country up to and including year  $t - 2$ , using zero-inflated negative binomial regression models, controlling for aid, and controlling for civil conflicts.<sup>21</sup>

The findings have a number of important implications. First, even if disasters only affect lower-level conflicts behavior for a subset of leaders, others have shown that current disputes may make future crises and wars more likely (e.g., Colaresi and Thompson 2002). While Nelson (2010) finds no cases of war initiation following major disasters, this paper identifies more subtle conditions under which disasters can affect international tensions. Second, the findings in Chiozza and Goemans (2011, 89) suggest that lower-intensity conflicts and crises can benefit leaders, and as such they

---

<sup>21</sup>Using an un-lagged version of the disaster deaths variable, excluding the control variables, dropping the US, dropping China, dropping Russia, and excluding cases with greater than 10 MID initiations also makes no difference for the key results.

are relevant for theory testing. Third, it may be unreasonable to expect disasters to have a large substantive effect on conflict initiation. Chiozza and Goemans's (2011) models account for dozens of factors thought to influence leader survival, and it is not necessarily surprising that when the analysis is confined to a single type of threat, the substantive impact of that threat is smaller and less precise. Fourth, and related to the last point, the findings here are consistent with most other studies on disasters and conflict that find small effects. Finally, a key finding here is that small-coalition leaders' international conflict behavior does not seem to respond to disasters in ways that existing theory suggests. I have argued that the reason for this is that disasters pose threats to leaders through mechanisms that do not matter for international conflict in small-coalition systems. This is a more general argument that should be tested in other contexts.

#### 4.6 Conclusion

This paper shows that deaths from natural disasters are associated with a modest increased risk of conflict in large-coalition systems. While the paper set out with the expectation that how a leader expects to lose office should condition this relationship within large-coalition regimes, I find only limited statistical evidence that this distinction matters in this context. The paper shows further that small-coalition leaders are no more or less likely to initiate conflicts as a result of disaster deaths. Yet, small-coalition leaders facing irregular removal appear to become less likely to use conflict as more disaster events occur. This is an anomalous result for existing theories and empirical studies of leader survival and international conflict.

From a policy perspective, this paper suggests that international efforts to invest in disaster preparedness are particularly important in countries with relatively inclusive institutions in regions where acts of hostility and belligerence may escalate quickly. Given that aid may be beneficial in these relatively inclusive systems (e.g.,

Bueno de Mesquita and Smith 2007, 280), donors should be able to reduce the likelihood of international conflict by helping these countries prepare for and respond to natural disasters without undermining (steps toward) democracy. The theoretical argument, which emphasizes assessments of leader competency, suggests that efforts should be focused on prevention and infrastructure development, rather than palliative measures.

The findings here should interest scholars studying potential connections between climate change and conflict. Even as past work has repeatedly emphasized the need to draw on theories of international conflict in studying climate-conflict linkages, few scholars have taken this advice seriously. By emphasizing the role that leaders' incentives for political survival play in shaping international conflict decisions, this paper provides a new way to think about how problems of human security might affect international security. While others have highlighted how the domestic consequences of disasters might spill over borders, this paper provides a theoretical framework linking the impacts of disasters to international conflict as a deliberate strategy of some leaders. A similar approach could be applied to acts of international cooperation. Indeed, if leaders become less likely to use conflict under certain conditions, do they substitute other less risky international policies to retain office? Others have proposed mechanisms that would link climate impacts to peace (Gartzke 2012). Such an approach would speak more broadly to theories of leader survival and foreign policy.

While this paper is motivated by a desire to use existing theories to anticipate potential links between disasters and violence, it offers an opportunity to rethink and reevaluate the mechanisms offered in those theories. Perhaps the most important contribution of this paper is the argument that not all threats to leader survival matter for international conflict. I have argued that Chiozza and Goemans's (2011) "fighting" and "gambling" for survival mechanisms would not necessarily reduce threats from extra-institutional, revolutionary threats. The consequences of removal do not seem

to matter when we distinguish between regular and irregular removal within small-coalition regimes and consider the effect of disaster events on conflict behavior. This is an important qualification to existing theories of leaders and international conflict. However, future research should test whether this argument holds in other empirical domains. For now, this paper helps further refine the conditions under which disasters are associated with international conflict.

#### 4.7 Appendix for “Natural Disasters, Leader Survival and International Conflict”

##### 4.7.1 Notes on Disaster Data

###### *Temporal and Spatial Domains*

The following countries/territories are included in EM-DAT but are either not included in *Archigos*, not included in the Correlates of War data set, or do not have coding in the Bueno de Mesquita et al. 2003 *W* score data, and are excluded from the analysis: American Samoa, Anguilla, Azores, Bermuda, Canary Islands, Cayman Islands, Cook Islands, Ethiopia, French Guiana, French Polynesia, Guadeloupe, Guam, Macau, Martinique, Montserrat, Netherlands Antilles, New Caledonia, Niue, Northern Mariana Islands, Palestine (West Bank), Puerto Rico, Reunion, Serbia, St Helena, Tokelau, Turks and Caicos Islands, Virgin Islands (UK), Virgin Islands (US), Wallis, Wallis and Futuna Islands. There are three cases that did not appear in the EM-DAT data but do appear in *Archigos*: Qatar, United Arab Emirates, and Republic of Vietnam. I include Qatar and United Arab Emirates in the analysis because they both appear in the EM-DAT online database even though they have no disasters listed during the temporal domain of this study. Dealing with the Republic of Vietnam (South Vietnam) is less straightforward, because EM-DAT does not distinguish between Vietnam and South Vietnam pre-1975, while *Archigos* has observations for Vietnam and South Vietnam pre-1975. For this reason, I excluded the Republic of

Vietnam from the analysis, as well as all pre-1975 years for Vietnam, since it is not possible to distinguish whether disasters occurred in Vietnam or Republic of Vietnam before 1975.

#### *Coding Disaster Deaths and Events*

Though the database always records the year that a disaster started, many observations list the start day as “00” in the data, and some list the start month as “00”. I treat these cases as if the disaster occurred on the first day of the month (or on the first day of the year if both are missing). The data are aggregated by total deaths per disaster. Accordingly, to contribute to the overall count of disaster deaths for a given leader-year, I require only that the disaster *started* during that leader-year. Disaster end dates are far less precise than start dates in EM-DAT. Often only a month and year is listed, while some cases list only the year. For countries with no disasters listed during a year, I code these values as zero so long as the country appears in the data set in other years.

*Why Not Use a Measure of Economic Damage?* While economic damage might have an effect similar to deaths, empirical measures of economic damage may not reliably convey the extent of a disaster’s economic impact. For example, the International Disaster Database records that Libya experienced \$42.2 million in disaster damages in 1995. Yet, Cohen and Werker (2008, 811) note that insurance covered these damages, so the extent of the loss is not clear from the \$42.2 million figure. Alternative data sets of economic damage may provide more fine-grained estimates of economic losses (e.g., Neumayer, Plümper and Barthel 2014), but for the purposes of this project, they do not necessarily provide any better estimate of disasters’ effects on the confidence of the winning coalition in the leader. Deaths from disasters, which are permanent and more readily visible, should serve as a clearer signal of leader competence. Quiroz



Flores and Smith (2013) also use this measure as an indicator of disasters' impacts on human security.

#### 4.7.2 Summary Statistics

Table 4.4: Summary Statistics

Statistic	N	Mean	St. Dev.	Min	Max
<i>mids_init</i>	6,843	0.199	0.648	0	23
<i>hh_mids_init</i>	6,843	0.131	0.543	0	23
<i>ln_deaths_t1</i>	6,843	1.342	2.262	0.000	14.510
<i>ln_events_t1</i>	6,843	0.462	0.654	0.000	3.638
<i>eq_events_t1</i>	6,843	0.108	0.470	0	10
<i>lwc_i</i>	6,843	0.360	0.480	0	1
<i>lwc_r</i>	6,843	0.381	0.486	0	1
<i>lwc_i_75</i>	6,843	0.092	0.289	0	1
<i>lwc_r_75</i>	6,843	0.377	0.485	0	1
<i>small</i>	6,843	0.260	0.438	0	1
<i>peace_years_mid</i>	6,843	19.581	45.204	1	191
<i>post_cw</i>	6,843	0.380	0.485	0	1
<i>leader_tenure</i>	6,843	8.644	7.381	2	49
<i>ln_realgdp</i>	6,843	10.333	1.914	4.873	16.373
<i>ln_land</i>	6,843	12.274	1.784	6.554	16.612
<i>cinc</i>	6,843	0.007	0.021	0.00001	0.319

#### Key

*mids\_init*: Count of MIDs initiated in year  $t$

*hh\_mids\_init*: Count of MIDs with hostility  $\geq 4$  initiated in year  $t$

*ln\_deaths\_t1*: Natural log of disaster deaths in year  $t - 1$

*ln\_events\_t1*: Natural log of disaster events in year  $t - 1$

*eq\_events\_t1*: Earthquake events in year  $t - 1$

*lwc\_i*: Large-coalition, irregular removal system (threshold  $W \geq 0.5$ )

*lwc\_r*: Large-coalition, regular removal system (threshold  $W \geq 0.5$ )

*lwc\_i\_75*: Large-coalition, irregular removal system (threshold  $W \geq 0.75$ )

*lwc\_r\_75*: Large-coalition, regular removal system (threshold  $W \geq 0.75$ )

*small*: Small-coalition system (threshold  $W < 0.5$ )  
*peace\_years\_mid*: Number of consecutive years without MID prior to year  $t$   
*post\_cw*: Dummy indicator for observation in post-1989 period  
*leader\_tenure*: length of time in years that leader has been in office up to year  $t$   
*ln\_realgdp*: Natural log of real GDP in year  $t - 1$   
*ln\_land*: Natural log of land area in square kilometers  
*cinc*: CINC score for country in year  $t - 1$

#### 4.7.3 Polity *xrreg* variable

The Polity codebook (Marshall, Jaggers and Gurr 2010, 21) describes the second category of *xrreg* as follows:

(2) Designational/Transitional: Chief executives are chosen by designation within the political elite, without formal competition (i.e., one-party systems or “rigged” multiparty elections). Also coded here are transitional arrangements intended to regularize future power transitions after an initial unregulated seizure of power (i.e., after constitutional legitimization of military rule or during periods when the leader of the coup steps down as head of state but retains unrivaled power within the political realm as head of the military). This category also includes polities in transition from designative to elective modes of executive selection (i.e., the period of “guided democracy” often exhibited during the transition from military to civilian rule) or vice versa (i.e., regimes ensuring electoral victory through the intimidation of oppositional leaders or the promulgation of a “state of emergency” before executive elections).

In short, a score of “2” indicates that the institutionalization of leader accession

is fragile (at best), and leaders of these systems are likely more concerned about irregular removal from office than leaders in regimes that score “3.”

#### 4.7.4 Including Total Aid and Civil Conflict

Here I report the results for some alternative models that include measures that may cause problems with post-treatment bias. Aid may reduce disaster impacts and countries may be less likely to engage in provocative international behavior when they are dependent on external assistance. I use the log of the sum of aid commitments (in constant US\$) from the project-level data provided by *AidData* (Tierney et al. 2011). Since Nelson (2010) argues that the association between disasters and international conflict results from the worsening of violent internal conflict which spills over into international conflict, I include a count of civil conflicts in the previous year using the UCDP-PRIO Armed Conflict Dataset v.4-2014 (Gleditsch et al. 2002). However, since these variables potentially intervene between the key independent and dependent variables, I omit these measures from the main analysis. The results are reported in Table 4.5.

Table 4.5: Controlling for Aid and Civil Conflict

	<i>Dependent variable: MID Initiations</i>			
		$W \geq 0.75$		$W \geq 0.75$
	(1)	(2)	(3)	(4)
Disaster Deaths <sub>t-1</sub> (log)	-0.019 (0.043)	0.007 (0.034)	-0.046 (0.038)	-0.009 (0.026)
Large-Coalition, Regular Removal (LC-R)	-0.372 (0.243)	-0.453* (0.202)	-0.738* (0.215)	-0.736* (0.181)
Large-Coalition, Irregular Removal (LC-I)	-0.101 (0.245)	-0.576* (0.245)	-0.158 (0.197)	-0.750* (0.226)
Disaster Deaths <sub>t-1</sub> (log) × LC-R	0.002 (0.055)	-0.028 (0.052)	0.067 (0.045)	0.023 (0.040)
Disaster Deaths <sub>t-1</sub> (log) × LC-I	0.062 (0.049)	0.105* (0.061)	0.077* (0.045)	0.143* (0.055)
$\hat{\beta}_{DisasterDeaths}$ for LC-I Leaders	0.043 <sup>†</sup> (0.033)	0.11* (0.05)	0.031 (0.029)	0.134* (0.05)
$\hat{\beta}_{DisasterDeaths}$ for LC-R Leaders	-0.017 (0.044)	-0.02 (0.046)	0.021 (0.034)	0.013 (0.034)
CINC	4.558* (2.443)	5.208* (2.253)	6.895* (1.590)	7.258* (1.527)
Land area (km <sup>2</sup> , log)	0.147* (0.047)	0.129* (0.047)	0.116* (0.037)	0.106* (0.038)
Real GDP <sub>t-1</sub> (log)	0.244* (0.046)	0.259* (0.046)	0.190* (0.038)	0.199* (0.039)
Aid Commitments (log)	-0.102* (0.041)	-0.100* (0.038)		
Aid Commitments (log, missing = zero)			-0.009 (0.006)	-0.007 (0.006)
Civil Conflicts <sub>t-1</sub>	0.476* (0.083)	0.471* (0.080)	0.446* (0.081)	0.444* (0.080)
Constant	-4.248* (0.640)	-4.195* (0.646)	-4.976* (0.553)	-4.991* (0.538)
Observations	4,419	4,419	6,843	6,843
Log Likelihood	-2,078.738	-2,073.259	-3,322.490	-3,313.483
$\theta$	0.683* (0.078)	0.688* (0.079)	0.663* (0.063)	0.670* (0.063)
Akaike Inf. Crit.	4,179.477	4,168.518	6,666.980	6,648.967

Note: <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

#### 4.7.5 Year-Fixed Effects

In the main text I use decade-fixed effects to account for possible differences across time in conflict opportunity. As an alternative solution, Table 4.6 reports Models 1 and 2 from Table 4.1 in the main text with year-fixed effects. The results are similar to those reported in the main text.

Table 4.6: Base Models with Year-Fixed Effects (Negative Binomial Regression)

	<i>Dependent variable: MIDs Initiated</i>	
	(1)	$W \geq 0.75$ (2)
Disaster Deaths <sub>t-1</sub> (log)	-0.023 (0.039)	0.007 (0.028)
Large-Coalition, Regular Removal (LC-R)	-0.857* (0.230)	-0.786* (0.188)
Large-Coalition, Irregular Removal (LC-I)	-0.300 (0.207)	-0.839* (0.224)
Disaster Deaths <sub>t-1</sub> × LC-R	0.082* (0.046)	0.048 (0.038)
Disaster Deaths <sub>t-1</sub> × LC-I	0.070 (0.049)	0.158* (0.058)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-I Leaders	0.05 <sup>†</sup> (0.033)	0.16* (0.05)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-R Leaders	0.059* (0.026)	0.06* (0.027)
CINC <sub>t-1</sub>	5.029* (1.605)	5.210* (1.485)
Land area (km <sup>2</sup> , log)	0.126* (0.039)	0.119* (0.039)
Real GDP <sub>t-1</sub> (log)	0.223* (0.040)	0.225* (0.039)
Constant	-5.368* (0.649)	-5.409* (0.629)
Year-Fixed Effects	Y	Y
Observations	6,843	6,843
Log Likelihood	-3.320.479	-3.311.933
$\theta$	0.726* (0.075)	0.734* (0.076)
Akaike Inf. Crit.	6,768.958	6,751.866

Note: <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

## 4.7.6 Country-Fixed Effects

In the main text I attempt to account for unobserved heterogeneity across observational units using leader-clustered standard errors. As an alternative solution, Table 4.7 reports Models 1 and 2 from Table 4.1 in the main text with country-fixed effects. The results are similar to those reported in the main text, though the marginal effect of disasters is no longer statistically significant for large-coalition regular removal leaders.

Table 4.7: Base Models with Country-Fixed Effects (Negative Binomial Regression)

	<i>Dependent variable: MIDs Initiated</i>	
	(1)	$W \geq 0.75$ (2)
Disaster Deaths <sub>t-1</sub> (log)	-0.044 (0.027)	0.001 (0.018)
Large-Coalition, Regular Removal (LC-R)	-0.292* (0.150)	-0.210 (0.135)
Large-Coalition, Irregular Removal (LC-I)	-0.262** (0.129)	-0.503** (0.221)
Disaster Deaths <sub>t-1</sub> (log) × LC-R	0.065* (0.036)	0.018 (0.030)
Disaster Deaths <sub>t-1</sub> (log) × LC-I	0.080** (0.033)	0.087 (0.056)
$\hat{\beta}_{DisasterDeaths}$ for LC-I Leaders	0.036* (0.022)	0.088† (0.054)
$\hat{\beta}_{DisasterDeaths}$ for LC-R Leaders	0.021 (0.026)	0.019 (0.026)
CINC <sub>t-1</sub>	-1.984 (2.521)	-1.720 (2.517)
Land area (km <sup>2</sup> , log)	-28.350 (1,017,867.000)	-26.214 (774,654.400)
Real GDP <sub>t-1</sub> (log)	-0.107** (0.046)	-0.093** (0.047)
Observations	6,843	6,843
Log Likelihood	-2,849.161	-2,849.646
$\theta$	2.109*** (0.316)	2.110*** (0.317)
Akaike Inf. Crit.	6,038.321	6,039.291

†p<0.1; \*p<0.05; One-tailed tests. Standard errors in parentheses.

#### 4.7.7 Zero-inflated Negative Binomial Regression Models

A potential concern is that conflict is sufficiently rare to make estimating the relationship between disasters and conflict difficult because of excessive zeros for the conflict variable(s). I address this using zero-inflated negative binomial regression models. The problem with using this model is that it assumes that there are multiple processes that generates zeros for the outcome variable. In the context of this paper, this means that I would have to assume that some leaders are restricted to be at peace for some reason. It is not clear that there are conditions that prevent some leaders from initiating conflicts or making threats, and, as such, it is unclear whether this model is appropriate. However, it is possible that having a weak military would cause a leader to be unable to initiate conflicts. I reestimate the main models using the CINC score as the inflation factor. The zero-inflated negative binomial regression models produces results similar to those reported in Table 4.1.

Table 4.8: Zero-inflated Negative Binomial Regression Models

	<i>Dependent variable: MIDs Initiated</i>	
	$W \geq 0.75$	
	(1)	(2)
Disaster Deaths <sub>t-1</sub> (log)	-0.043 (0.027)	0.007 (0.018)
Large-Coalition, Regular Removal (LC-R)	-0.698* (0.111)	-0.627* (0.098)
Large-Coalition, Irregular Removal (LC-I)	-0.258* (0.099)	-0.659* (0.198)
Disaster Deaths <sub>t-1</sub> (log) × LC-R	0.105* (0.034)	0.052* (0.026)
Disaster Deaths <sub>t-1</sub> (log) × LC-I	0.100* (0.034)	0.149* (0.054)
$\hat{\beta}_{DisasterDeaths}$ for LC-I Leaders	0.057* (0.021)	0.157* (0.052)
$\hat{\beta}_{DisasterDeaths}$ for LC-R Leaders	0.063* (0.021)	0.059* (0.021)
CINC <sub>t-1</sub>	10.398* (1.348)	10.540* (1.353)
Land area (km <sup>2</sup> , log)	0.114* (0.024)	0.108* (0.025)
Real GDP <sub>t-1</sub> (log)	-0.042 (0.033)	-0.026 (0.033)
Constant	-2.140* (0.430)	-2.332* (0.429)
Observations	6,843	6,843
Log Likelihood	-3,299.204	-3,296.751

*Note:* \*p<0.1; \*p<0.05; \*p<0.01. Standard errors in parentheses.  
Zero inflation factor in inflation stage (not shown) is the CINC variable.



#### 4.7.8 Controlling for Disaster Propensity

To account for the possibility that some countries are more prone to experience disasters in ways that would affect the relationship between disasters and leader survival, I control for the disaster propensity of a country. To construct this measure, I take the natural log of disaster events in a given country for all years before and including year  $t - 2$ . The results are reported in Table 4.9, and are similar to the findings in the baseline model in the main text. (Using an unlogged version of the disaster propensity makes no difference for the results.)

Table 4.9: Controlling for Disaster Propensity (Negative Binomial Regression)

	<i>Dependent variable: MID Initiations</i>	
	(1)	$W \geq 0.75$ (2)
Disaster Deaths $_{t-1}$ (log)	-0.035 (0.042)	0.002 (0.026)
Large-Coalition, Regular Removal (LC-R)	-0.820* (0.237)	-0.766* (0.196)
Large-Coalition, Irregular Removal (LC-I)	-0.279 (0.213)	-0.850* (0.230)
Disaster Events Up to Year $t - 2$ (log)	0.008 (0.053)	0.027 (0.054)
Disaster Deaths $_{t-1}$ (log) $\times$ LC-R	0.081* (0.048)	0.037 (0.041)
Disaster Deaths $_{t-1}$ (log) $\times$ LC-I	0.079 (0.052)	0.139* (0.059)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-I Leaders	0.044 <sup>†</sup> (0.029)	0.14* (0.052)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-R Leaders	0.046* (0.029)	0.039 <sup>†</sup> (0.029)
CINC $_{t-1}$	5.523* (1.701)	5.588* (1.568)
Land area (km $^2$ , log)	0.137* (0.037)	0.127* (0.038)
Real GDP $_{t-1}$ (log)	0.203* (0.039)	0.205* (0.038)
Constant	-5.313* (0.527)	-5.311* (0.516)

Note: <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

## 4.7.9 Correlations Between Key Variables

Table 4.10 reports correlations between the key independent variables used in the analysis.

Table 4.10: Correlations Between Independent Variables

	ln_deaths_t1	ln_events_t1	eq_events_t1	lwc_i	lwc_r	lwc_i_75	lwc_r_75	small	peace_years_mid	post_cw	leader_tenure	ln_realgdp	ln_land	cinc
ln_deaths_t1	1	0.785	0.391	-0.062	0.094	0.031	0.091	-0.037	-0.145	0.189	-0.016	0.333	0.289	0.320
ln_events_t1	0.785	1	0.459	-0.076	0.171	0.063	0.168	-0.106	-0.152	0.322	-0.028	0.415	0.309	0.379
eq_events_t1	0.391	0.459	1	0.010	0.024	0.004	0.026	-0.038	-0.074	0.087	-0.026	0.255	0.194	0.292
lwc_i	-0.062	-0.076	0.010	1	-0.588	0.424	-0.583	-0.444	0.002	-0.044	0.154	-0.173	0.049	-0.039
lwc_r	0.094	0.171	0.024	-0.588	1	-0.249	0.993	-0.464	-0.003	0.119	-0.319	0.375	-0.032	0.156
lwc_i_75	0.031	0.063	0.004	0.424	-0.249	1	-0.247	-0.188	0.030	0.199	-0.076	-0.063	-0.051	-0.055
lwc_r_75	0.091	0.168	0.026	-0.583	0.993	-0.247	1	-0.461	0.0003	0.113	-0.317	0.372	-0.034	0.155
small	-0.037	-0.106	-0.038	-0.444	-0.464	-0.188	-0.461	1	0.0003	-0.084	0.184	-0.226	-0.019	-0.131
peace_years_mid	-0.145	-0.152	-0.074	0.002	-0.003	0.030	0.0003	0.0003	1	-0.104	-0.063	-0.285	-0.297	-0.112
post_cw	0.189	0.322	0.087	-0.044	0.119	0.199	0.113	-0.084	-0.104	1	0.066	0.127	-0.054	-0.018
leader_tenure	-0.016	-0.028	-0.026	0.154	-0.319	-0.076	-0.317	0.184	-0.063	0.066	1	-0.099	-0.049	-0.079
ln_realgdp	0.333	0.415	0.255	-0.173	0.375	-0.063	0.372	-0.226	-0.285	0.127	-0.099	1	0.426	0.519
ln_land	0.289	0.309	0.194	0.049	-0.032	-0.051	-0.034	-0.019	-0.297	-0.054	-0.049	0.426	1	0.357
cinc	0.320	0.379	0.292	-0.039	0.156	-0.055	0.155	-0.131	-0.112	-0.018	-0.079	0.519	0.357	1

### Key

*ln\_deaths\_t1*: Natural log of disaster deaths in year  $t - 1$

*ln\_events\_t1*: Natural log of disaster events in year  $t - 1$

*eq\_events\_t1*: Earthquake events in year  $t - 1$

*lwc\_i*: Large-coalition, irregular removal system (threshold  $W \geq 0.5$ )

*lwc\_r*: Large-coalition, regular removal system (threshold  $W \geq 0.5$ )

*lwc\_i\_75*: Large-coalition, irregular removal system (threshold  $W \geq 0.75$ )

*lwc\_r\_75*: Large-coalition, regular removal system (threshold  $W \geq 0.75$ )

*small*: Small-coalition system (threshold  $W < 0.5$ )

*peace\_years\_mid*: Number of consecutive years without MID prior to year  $t$

*post\_cw*: Dummy indicator for observation in post-1989 period

*leader\_tenure*: length of time in years that leader has been in office up to year  $t$

*ln\_realgdp*: Natural log of real GDP in year  $t - 1$

*ln\_land*: Natural log of land area in square kilometers

*cinc*: CINC score for country in year  $t - 1$

#### 4.7.10 Empirical Distribution of Regime and Removal Type

Table 4.11 reports the empirical distribution of removal type for my coding of regime type using the main  $W \geq 0.5$  threshold. To create this table, I reduced the data set to only the last years that leaders were in office, recording the removal type as I have coded it, along with how the leader actually lost office as coded by Goemans, Gleditsch and Chiozza (2009). Though large-coalition leaders face irregular removal less often than small-coalition leaders, my coding of regular versus irregular removal within large-coalition systems still captures this distinction within large-coalition regimes. Only 9% of large-coalition, regular removal leaders lost office through irregular means, while 27% of large-coalition, irregular removal leaders lost office through irregular means. So, while in both groups irregular removal happens less often than it does in small-coalition systems, clearly there is a non-trivial risk of irregular removal for the leaders I have coded as “large-coalition, irregular removal.” The table also shows that the coding scheme also seems to capture a meaningful distinction in removal types within small-coalition regimes. The same is true in Table 4.12 which uses the alternative threshold of  $W \geq 0.75$  to separate large- from small-coalition systems, though the differences are less pronounced.

Table 4.11: Empirical Distribution of Regime and Removal Type,  $W \geq 0.5$  Threshold

	Foreign	Irregular	Natural Death	Regular	Retired Due to Ill Health	Still in Office	Suicide	Unknown
Large, Irregular	0.020	0.270	0.100	0.600	0.010	0	0	0
Large, Regular	0	0.090	0.030	0.850	0.020	0	0	0
Small, Irregular	0.020	0.510	0.050	0.400	0.020	0	0	0
Small, Regular	0.020	0.260	0.280	0.400	0.040	0	0	0

Table 4.12: Empirical Distribution of Regime and Removal Type,  $W \geq 0.75$  Threshold

	Foreign	Irregular	Natural Death	Regular	Retired Due to Ill Health	Still in Office	Suicide	Unknown
Large, Irregular	0.010	0.140	0.020	0.820	0.010	0	0	0
Large, Regular	0	0.090	0.030	0.850	0.020	0	0	0
Small, Irregular	0.030	0.420	0.100	0.430	0.020	0	0	0
Small, Regular	0.030	0.260	0.260	0.420	0.030	0	0	0

#### 4.7.11 Alternative Dependent Variable: ICB Crisis Initiations

To provide an alternative measure of conflict behavior, Table 4.13 reports the results of using a count of ICB crisis initiations (Brecher and Wilkenfeld 1997) rather than MID initiations. The results in the baseline model are no longer statistically significant (and, indeed, this is the only model whether the marginal effect of disasters in large-coalition, irregular removal systems is negatively signed, though the standard errors are considerably larger than the coefficient estimate). However, in the model that uses the alternative  $W$  threshold (Model 2), there is a strong and statistically significant effect of disasters, only in large-coalition, irregular removal systems. In both models, the coefficient for disasters in large-coalition regular removal systems is close to zero and not statistically significant.

Table 4.13: Alternative Dependent Variable: ICB Crisis Initiations (Negative Binomial Regression)

	<i>Dependent variable: ICB Crisis Initiations</i>	
	(1)	$W \geq 0.75$ (2)
Disaster Deaths <sub>t-1</sub> (log)	-0.049 (0.065)	-0.028 (0.042)
Large-Coalition, Regular Removal (LC-R)	-0.399 (0.342)	-0.223 (0.287)
Large-Coalition, Irregular Removal (LC-I)	-0.542* (0.271)	-1.959* (0.568)
Disaster Deaths <sub>t-1</sub> (log) × LC-R	0.053 (0.075)	0.031 (0.057)
Disaster Deaths <sub>t-1</sub> (log) × LC-I	0.034 (0.073)	0.255* (0.114)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-I Leaders	-0.015 (0.045)	0.23* (0.11)
$\hat{\beta}_{Disaster\ Deaths}$ for LC-R Leaders	0.004 (0.042)	0.003 (0.04)
CINC <sub>t-1</sub>	8.334* (1.781)	7.917* (1.757)
Land area (km <sup>2</sup> , log)	0.229* (0.058)	0.221* (0.061)
Real GDP <sub>t-1</sub> (log)	0.035 (0.054)	0.039 (0.053)
Constant	-6.122* (0.707)	-6.225* (0.738)
Observations	6,843	6,843
Log Likelihood	-1,237.361	-1,232.370
$\theta$	0.877* (0.339)	0.892* (0.350)
Akaike Inf. Crit.	2,492.721	2,482.739

Note: <sup>†</sup>p<0.1; \*p<0.05; One-tailed tests. Leader-clustered standard errors in parentheses.

#### 4.7.12 Cross Tabulation of Disaster Deaths and MID Initiations

Table 4.14 reports the raw counts and percentage of observations where leaders in each of the large-coalition categories initiated at least one MID following a year with less than the mean number of disaster deaths and more than the mean number of disaster deaths.

Table 4.14: Distribution of MID Cases for Large-Coalition Leaders

Regime, Removal Type	Zero MIDs	MIDs > 0	% MID Cases
Large-Coalition, Irregular (Deaths $\leq$ Mean)	1609	223	0.069
Large-Coalition, Irregular (Deaths >Mean)	504	132	0.21
Large-Coalition, Regular (Deaths $\leq$ Mean)	1494	154	0.052
Large-Coalition, Regular (Deaths >Mean)	769	200	0.21

Table 4.15 lists the cases in the data where large-coalition leaders initiated at least one MID following a year where disasters killed an above-average number of people in the leader's country. Table 4.16 does the same for large-coalition regular removal leaders.

Table 4.15: Cases of MID Initiations for Large-Coalition, Irregular Removal Leaders with Greater than Mean Disaster Deaths in Previous Period

leader	cocode	idacr	year						
1	Berisha	339	ALB	1997	67	Khatami	630	IRN	2005
2	Duvalier, Francois	41	HAI	1963	68	Menderes	640	TUR	1952
3	Ruiz Cortines	70	MEX	1956	69	Menderes	640	TUR	1958
4	Laugerud Garcia	90	GUA	1977	70	Evren	640	TUR	1982
5	Azcona Hoyo	91	HON	1989	71	Ozal	640	TUR	1986
6	Callejas	91	HON	1991	72	Ozal	640	TUR	1987
7	Reina	91	HON	1995	73	Ozal	640	TUR	1988
8	Anastasio Somoza Debayle	93	NIC	1977	74	Ozal	640	TUR	1989
9	Alarcon Fabian	130	ECU	1998	75	Sadat	651	EGY	1980
10	Fujimori	135	PER	1995	76	Mubarak	651	EGY	1993
11	Geisel	140	BRA	1975	77	Mubarak	651	EGY	1995
12	Figueiredo	140	BRA	1983	78	Mubarak	651	EGY	1996
13	Kadar	310	HUN	1971	79	Bashar al-Assad	652	SYR	2005
14	Husak	315	CZE	1986	80	Rakhmonov	702	TAJ	1998
15	Georgievski	343	MAC	2002	81	Rakhmonov	702	TAJ	2006
16	Milosevic	345	YUG	1991	82	Akayev	703	KYR	1993
17	Milosevic	345	YUG	2000	83	Karimov	704	UZB	1993
18	Zhivkov	355	BUL	1987	84	Karimov	704	UZB	1999
19	Yeltsin	365	RUS	1993	85	Nazarbayev	705	KZK	1996
20	Yeltsin	365	RUS	1994	86	Mao Tse-Tung	710	CHN	1952
21	Yeltsin	365	RUS	1995	87	Mao Tse-Tung	710	CHN	1955
22	Yeltsin	365	RUS	1996	88	Mao Tse-Tung	710	CHN	1956
23	Yeltsin	365	RUS	1997	89	Mao Tse-Tung	710	CHN	1958
24	Yeltsin	365	RUS	1998	90	Mao Tse-Tung	710	CHN	1960
25	Yeltsin	365	RUS	1999	91	Mao Tse-Tung	710	CHN	1965
26	Putin	365	RUS	2001	92	Mao Tse-Tung	710	CHN	1975
27	Putin	365	RUS	2002	93	Mao Tse-Tung	710	CHN	1976
28	Putin	365	RUS	2003	94	Deng Xiaoping	710	CHN	1985
29	Putin	365	RUS	2005	95	Deng Xiaoping	710	CHN	1986
30	Putin	365	RUS	2006	96	Deng Xiaoping	710	CHN	1987
31	Putin	365	RUS	2007	97	Deng Xiaoping	710	CHN	1988
32	H. Aliyev	373	AZE	1996	98	Deng Xiaoping	710	CHN	1993
33	H. Aliyev	373	AZE	2001	99	Deng Xiaoping	710	CHN	1994
34	Taylor	450	LBR	2000	100	Deng Xiaoping	710	CHN	1995
35	Biya	471	CAO	1987	101	Deng Xiaoping	710	CHN	1996
36	Biya	471	CAO	1994	102	Jiang Zemin	710	CHN	1999
37	Biya	471	CAO	1995	103	Jiang Zemin	710	CHN	2001
38	Biya	471	CAO	1998	104	Jiang Zemin	710	CHN	2002
39	Biya	471	CAO	2005	105	Hu Jintao	710	CHN	2004
40	Patasse	482	CEN	2001	106	Hu Jintao	710	CHN	2005
41	Mobutu	490	DRC	1977	107	Hu Jintao	710	CHN	2007
42	Mobutu	490	DRC	1978	108	Chiang Ching-Kuo	713	TAW	1987
43	Joseph Kabila	490	DRC	2007	109	Lee Teng-Hui	713	TAW	1991
44	Kenyatta	501	KEN	1978	110	Lee Teng-Hui	713	TAW	1995
45	Moi	501	KEN	1995	111	Rhee	732	ROK	1958
46	Mwinyi	510	TAZ	1995	112	Chun Doo Hwan	732	ROK	1983
47	Mkapa	510	TAZ	2000	113	Chun Doo Hwan	732	ROK	1985
48	Mkapa	510	TAZ	2002	114	Chun Doo Hwan	732	ROK	1986
49	Gouled Aptidon	522	DJI	1998	115	Roh Tae Woo	732	ROK	1991
50	Dos Santos	540	ANG	1988	116	Kim Young Sam	732	ROK	1996
51	Dos Santos	540	ANG	2001	117	Prem	800	THI	1987
52	Dos Santos	540	ANG	2002	118	Mahatir Bin Mohammad	820	MAL	1988
53	Dos Santos	540	ANG	2005	119	Mahatir Bin Mohammad	820	MAL	1992
54	Machel	541	MZM	1986	120	Mahatir Bin Mohammad	820	MAL	2003
55	Kaunda	551	ZAM	1983	121	Ahmad Badawi	820	MAL	2004
56	Levy Mwanawasa	551	ZAM	2004	122	Ahmad Badawi	820	MAL	2005
57	Ayatollah Khomeini	630	IRN	1984	123	Ahmad Badawi	820	MAL	2006
58	Ayatollah Khomeini	630	IRN	1987	124	Marcos	840	PHI	1979
59	Ayatollah Khomeini	630	IRN	1988	125	Marcos	840	PHI	1982
60	Rafsanjani	630	IRN	1991	126	Sukarno	850	INS	1957
61	Rafsanjani	630	IRN	1992	127	Sukarno	850	INS	1964
62	Rafsanjani	630	IRN	1993	128	Megawati Sukarnoputri	850	INS	2003
63	Rafsanjani	630	IRN	1994	129	Megawati Sukarnoputri	850	INS	2004
64	Rafsanjani	630	IRN	1995	130	Ahmadinejad	630	IRN	2007
65	Rafsanjani	630	IRN	1996	131	Bakiyev	703	KYR	2005
66	Rafsanjani	630	IRN	1997	132	Bakiyev	703	KYR	2006





## BIBLIOGRAPHY

- Abblink, Jon. 2003. "Badme and the Ethio-Eritrean Border: The Challenge of Demarcation in the Post-War Period." *Africa: Rivista trimestrale di studi e documentazione dell'Istituto italiano per l'Africa e l'Oriente* 58(2):219–231.
- Abbott, Kenneth W and Duncan Snidal. 1998. "Why States Act Through Formal International Organizations." *Journal of Conflict Resolution* 42(1):3–32.
- Acemoglu, Daron and James A Robinson. 2001. "A Theory of Political Transitions." *American Economic Review* 91(4):938–963.
- Acemoglu, Daron and James A Robinson. 2006. *Economic Origins of Dictatorship and Democracy*. New York: Cambridge University Press.
- Achen, Christopher H. 2005. "Let's Put Garbage-Can Regressions and Garbage-Can Probits Where They Belong." *Conflict Management and Peace Science* 22(4):327–339.
- Acht, Martin, Toman Omar Mahmoud and Rainer Thiele. 2015. "Corrupt Governments Do Not Receive More State-to-State Aid: Governance and the Delivery of Foreign Aid Through Non-state Actors." *Journal of Development Economics* 114:20–33.
- Ahmed, Faisal Z. 2012. "The Perils of Unearned Foreign Income: Aid, Remittances, and Government Survival." *American Political Science Review* 106(1):146–165.
- Ahmed, Faisal Z. and Eric D. Werker. 2015. "Aid and the Rise and Fall of Conflict in the Muslim World." *Quarterly Journal of Political Science* 10(2):155–186.
- Aitchison, John. 1986. *The Statistical Analysis of Compositional Data*. New York: Chapman & Hall.
- Akam, Simon. 2012. "A Paradigm Shift for Aid to Africa." *The New York Times* .
- Akcinaroglu, Seden, Jonathan M. DiCicco and Elizabeth Radziszewski. 2011. "Avalanches and Olive Branches: A Multimethod Analysis of Disasters and Peacemaking in Interstate Rivalries." *Political Research Quarterly* 64(2):260–275.
- Alesina, Alberto and David Dollar. 2000. "Who Gives Foreign Aid to Whom and Why?" *Journal of Economic Growth* 63(1):33–63.
- Altincekic, Ceren and David H. Bearce. 2014. "Why there Should be No Political Foreign Aid Curse." *World Development* 64:18–32.
- Banks, Arthur. 2011. "Cross-National Times-Series Data Archive." *Databanks International* .  
**URL:** <http://www.databanksinternational.com>

- Bechtel, Michael M. and Jens Hainmueller. 2011. "How Lasting Is Voter Gratitude? An Analysis of the Short- and Long-Term Electoral Returns to Beneficial Policy." *American Journal of Political Science* 55(4):852–868.
- Belgrad, Eric A. 1997. The Politics of Humanitarian Aid. In *The Politics of International Humanitarian Aid Operations*, ed. Eric A. Belgrad and Nitza Nachmias. Westport, CT: Praeger pp. 3–17.
- Bergholt, Drago and Paivi Lujala. 2012. "Climate-related Natural Disasters, Economic Growth, and Armed Civil Conflict." *Journal of Peace Research* 49(1):147–162.
- Bermeo, Nancy. 1997. "Myths of Moderation: Confrontation and Conflict during Democratic Transitions." *Comparative Politics* 29(3):305.
- Bermeo, Sarah Blodgett. 2016. "Aid Is Not Oil: Donor Utility, Heterogeneous Aid, and the Aid-Democratization Relationship." *International Organization* 70(01):1–32.
- Berry-Koch, Angela M. 2012. 5-Year Evaluation of the Central Emergency Response Fund, Country Study: Guatemala. Technical report Channel Research (on behalf of United Nations Office for the Coordination of Humanitarian Affairs) Ohain, Belgium: .
- Besley, Timothy and Torsten Persson. 2011. "The Logic of Political Violence." *The Quarterly Journal of Economics* 126(3):1–35.
- Biermann, Frank and Ingrid Boas. 2010. "Preparing for a Warmer World: Towards a Global Governance System to Protect Climate Refugees." *Global Environmental Politics* 10(1):60–88.
- Bradsher, Keith. 2014. "Philippine Leader Sounds Alarm on China." *The New York Times* p. A4.  
**URL:** <http://www.nytimes.com/2014/02/05/world/asia/philippine-leader-urges-international-help-in-resisting-chinas-sea-claims.html>
- Brancati, Dawn. 2007. "Political Aftershocks: The Impact of Earthquakes on Intrastate Conflict." *Journal of Conflict Resolution* 51(5):715–743.
- Bräutigam, Deborah A. and Stephen Knack. 2004. "Foreign Aid, Institutions, and Governance in Sub-Saharan Africa." *Economic Development and Cultural Change* 52(2):255–285.
- Brecher, Michael and Jonathan Wilkenfeld. 1997. *A Study of Crisis*. Ann Arbor, MI: University of Michigan Press.
- Bremer, Stuart A. 2000. Who Fights Whom, When, Where, and Why? In *What Do We Know About War?*, ed. John Vasquez. Lanham: Rowman & Littlefield Publishers pp. 3–26.

- Bueno de Mesquita, Bruce and Alastair Smith. 2007. "Foreign Aid and Policy Concessions." *Journal of Conflict Resolution* 51(2):251–284.
- Bueno de Mesquita, Bruce and Alastair Smith. 2008. "Political Survival and Endogenous Institutional Change." *Comparative Political Studies* 42(2):167–197.
- Bueno de Mesquita, Bruce and Alastair Smith. 2009. "A Political Economy of Aid." *International Organization* 63(02):309.
- Bueno de Mesquita, Bruce and Alastair Smith. 2010. "Leader Survival, Revolutions, and the Nature of Government Finance." *American Journal of Political Science* 54(4):936–950.
- Bueno de Mesquita, Bruce, James Morrow, Randolph Siverson and Alastair Smith. 2003. *The Logic of Political Survival*. Cambridge: Cambridge University Press.
- Bush, George W. 2010. *Decision Points*. New York: Broadway Books.
- Bütke, Tim, Solomon Major and André De Mello E Souza. 2012. "The Politics of Private Foreign Aid: Humanitarian Principles, Economic Development Objectives, and Organizational Interests in NGO Private Aid Allocation." *International Organization* 66(04):571–607.
- Carlin, Ryan E., Gregory J. Love and Elizabeth J. Zechmeister. 2014. "Natural Disaster and Democratic Legitimacy: The Public Opinion Consequences of Chile's 2010 Earthquake and Tsunami." *Political Research Quarterly* 67(1):3–15.
- Chiozza, Giacomo and H. E. Goemans. 2011. *Leaders and International Conflict*. Cambridge: Cambridge University Press.
- Cohen, Charles and Eric D. Werker. 2008. "The Political Economy of 'Natural' Disasters." *Journal of Conflict Resolution* 52(6):795–819.
- Colaresi, Michael and William Thompson. 2002. "Hot Spots or Hot Hands? Serial Crisis Behavior, Escalating Risks, and Rivalry." *Journal of Politics* 64(4):1175–1198.
- Collier, Paul and Anke Hoeffler. 2004. "Greed and Grievance in Civil War." *Oxford Economic Papers* 56(4):563–595.
- Conley, Timothy G., Christian B. Hansen and Peter E. Rossi. 2012. "Plausibly Exogenous." *Review of Economics and Statistics* 94(1):260–272.
- Coppedge, Michael. 2012. *Democratization and Research Methods*. New York: Cambridge University Press.
- Croco, Sarah E. 2011. "The Decider's Dilemma: Leader Culpability, War Outcomes, and Domestic Punishment." *American Political Science Review* 105(3):457–477.
- Crossette, Barbara. 1992. "Givers of Foreign Aid Shifting Their Methods."

- Daalder, Ivo H. and James M. Lindsay. 2005. "Bush's Foreign-Policy Strategy: Is the Revolution Over?" *Brookings Institution* .  
**URL:** <http://www.brookings.edu/research/opinions/2005/10/14forceandlegitimacy-daalder>
- de Waal, Alex. 1997. *Famine Crimes: Politics & the Disaster Relief Industry in Africa*. Bloomington, IN: Indiana University Press.
- Dietrich, Simone. 2013. "Bypass or Engage? Explaining Donor Delivery Tactics in Foreign Aid Allocation." *International Studies Quarterly* 57(4):698–712.
- Dietrich, Simone and Joseph Wright. 2013. Foreign Aid and Democratic Development in Africa. In *Democratic Trajectories in Africa: Unravelling the Impact of Foreign Aid*, ed. Danielle Resnick and Nicolas van de Walle. Oxford: Oxford University Press pp. 56–86.
- Djankov, Simeon, Jose G. Montalvo and Marta Reynal-Querol. 2008. "The Curse of Aid." *Journal of Economic Growth* 13(3):169–194.
- Dogan, Mattei and John Higley. 1998. Elites, Crises, and Regimes in Comparative Analysis Origins of Regimes. In *Elites, Crises, and the Origins of Regimes*, ed. Mattei Dogan and John Higley. Lanham: Rowman & Littlefield pp. 3–27.
- Dollar, David and Victoria Levin. 2006. "The Increasing Selectivity of Foreign Aid, 1984–2003." *World Development* 34(12):2034–2046.
- Downs, George W. and David M. Rocke. 1994. "Conflict, agency, and gambling for resurrection: The principal-agent problem goes to war." *American Journal of Political Science* 38(2):362–380.
- Dreher, Axel, Andreas Fuchs and Peter Nunnenkamp. 2013. "New Donors." *International Interactions* 39(3):402–415.
- Dreher, Axel, Jan-Egbert Sturm and James Raymond Vreeland. 2009. "Development Aid and International Politics: Does Membership On the UN Security Council Influence World Bank Decisions?" *Journal of Development Economics* 88(1):1–18.
- Drury, A. Cooper and Richard Stuart Olson. 1998. "Disasters and Political Unrest: An Empirical Investigation." *Journal of Contingencies and Crisis Management* 6(3):153–161.
- Drury, A. Cooper, Richard Stuart Olson and Douglas A. Van Belle. 2005. "The Politics of Humanitarian Aid: U. S. Foreign Disaster Assistance, 1964–1995." *Journal of Politics* 67(2):454–473.
- Dunning, Thad. 2004. "Conditioning the Effects of Aid: Cold War Politics, Donor Credibility, and Democracy in Africa." *International Organization* 58(02):409–423.

- Ear, Sophal. 2013. *Aid Dependence in Cambodia: How Foreign Assistance Undermines Democracy*. New York: Columbia University Press.
- EM-DAT. 2009. "The International Disaster Database."  
**URL:** <http://www.em-dat.be>
- Fink, Günther and Silvia Redaelli. 2011. "Determinants of International Emergency Aid – Humanitarian Need Only?" *World Development* 39(5):741–757.
- Finkel, Steven E., Anibal Pérez-Liñán and Mitchell A. Seligson. 2007. "The Effects of U.S. Foreign Assistance on Democracy Building, 1990-2003." *World Politics* 59(3):404–438.
- French, Howard W. 1996. "Second thoughts on foreign aid: Donors are increasingly concerned that granting financial assistance to under-developed countries through their central governments is a recipe for waste." *The Ottawa Citizen* p. A15.
- Fuchs, Andreas, Axel Dreher and Peter Nunnenkamp. 2014. "Determinants of Donor Generosity: A Survey of the Aid Budget Literature." *World Development* 56:172–199.
- Gartzke, Erik. 2007. "The Capitalist Peace." *American Journal of Political Science* 51(1):166–191.
- Gartzke, Erik. 2012. "Could Climate Change Precipitate Peace?" *Journal of Peace Research* 49(1):177–192.
- Gasper, John T. and Andrew Reeves. 2011. "Make It Rain? Retrospection and the Attentive Electorate in the Context of Natural Disasters." *American Journal of Political Science* 55(2):340–355.
- Gawronski, Vincent T. and Richard Stuart Olson. 2013. "Disasters as Crisis Triggers for Critical Junctures? The 1976 Guatemala Case." *Latin American Politics and Society* 55(2):133–149.
- Geddes, Barbara, Joseph Wright and Erica Frantz. 2014. "Autocratic Breakdown and Regime Transitions: A New Data Set." *Perspectives on Politics* 12(2):313–331.
- Gehlbach, Scott. 2013. *Formal Models of Domestic Politics*. Cambridge: Cambridge University Press.
- Gibler, Douglas M. 2007. "Bordering on Peace: Democracy, Territorial Issues, and Conflict." *International Studies Quarterly* 51(3):509–532.
- Gilligan, Michael J. and Leslie Johns. 2012. "Formal Models of International Institutions." *Annual Review of Political Science* 15(1):221–243.
- Glantz, Michael H. 2000. Lessons Learned from the 1997–98 El Niño: Once Burned, Twice Shy? Technical Report October United Nations Environment Programme.

- Gleditsch, Kristian S. 2002. "Expanded Trade and GDP Data." *Journal of Conflict Resolution* 46(5):712–724.
- Gleditsch, Nils Petter. 2012. "Whither the Weather? Climate Change and Conflict." *Journal of Peace Research* 49(1):3–9.
- Gleditsch, Nils Petter, Peter Wallensteen, Mikael Erikson, Margareta Sollenberg and Haavard Strand. 2002. "Armed Conflict, 1946-2001: A New Dataset." *Journal of Peace Research* 39(5):615–637.
- Global Humanitarian Assistance. 2011. Pooled funding Mechanisms and Large-scale Disasters. Technical report Global Humanitarian Assistance.
- Goemans, H. E., Kristian S. Gleditsch and Giacomo Chiozza. 2009. "Introducing Archigos: A Dataset of Political Leaders." *Journal of Peace Research* 46(2):269–283.
- Goemans, H.E. 2000. *War and Punishment: The Causes of War Termination and the First World War*. Princeton: Princeton University Press.
- Goldsmith, Arthur A. 2001. "Foreign Aid and Statehood in Africa." *International Organization* 55(1):123–148.
- Guha-Sapir, D., R. Below and Ph. Hoyois. 2015. "EM-DAT: International Disaster Database." *Université Catholique de Louvain, Brussels, Belgium* .  
**URL:** [www.emdat.be](http://www.emdat.be)
- Guha-Sapir, Debarati, David Hargitt and Philippe Hoyois. 2004. *Thirty Years of Natural Disasters 1974-2003: The Numbers*. Louvain-la-Neuve, Belgium: Universitaires de Louvain.
- Haas, Peter M., Robert O. Keohane and Marc A. Levy. 1993. *Institutions for the Earth: Sources of Effective International Environmental Protection*. Cambridge: MIT Press.
- Handey, Jack. 2008. *What I'd Say To The Martians and Other Veiled Threats*. New York: Hyperion.
- Hawkins, Darren G., David A. Lake, Daniel L. Nielson and Michael J. Tierney. 2006. Delegation Under Anarchy: States, International Organizations, and Principal-Agent Theory. In *Delegation and Agency in International Organizations*, ed. Darren G. Hawkins, David A. Lake, Daniel L. Nielson and Michael J. Tierney. Cambridge: Cambridge University Press pp. 3–38.
- Hawkins, Darren G. and Wade Jacoby. 2006. How Agents Matter. In *Delegation and Agency in International Organizations*, ed. Darren G. Hawkins, David A. Lake, Daniel L. Nielson and Michael J. Tierney. New York: Cambridge University Press pp. 199–228.

- Healy, Andrew and Neil Malhotra. 2009. "Myopic Voters and Natural Disaster Policy." *American Political Science Review* 103(03):387–406.
- Heinrich, Tobias. 2013. "When is Foreign Aid Selfish, When is it Selfless?" *The Journal of Politics* 75(02):422–435.
- Helmer, Madeleen and Dorothea Hilhorst. 2006. "Natural Disasters and Climate Change." *Disasters* 30(1):1–4.
- Heston, Alan, Robert Summers and Bettina Aten. 2012. *Penn World Table Version 7.1*. Center for International Comparisons of Production, Income and Prices at the University of Pennsylvania.
- Hilbe, Joseph M. 2007. *Negative Binomial Regression*. Cambridge: Cambridge University Press.
- Jacobs, Andrew. 2013. "Philippines' President Faces Growing Anger." *The New York Times* p. A15.  
**URL:** <http://www.nytimes.com/2013/11/14/world/asia/philippines-president-faces-growing-anger.html?pagewanted=all>
- Jonas, Susanne. 1991. *The Battle for Guatemala: Rebels, Death Squads, and U.S. Power*. Boulder: Westview Press.
- Jones, Daniel M., Stuart A. Bremer and J. David Singer. 1996. "Militarized interstate disputes, 1816–1992: Rationale, coding rules, and empirical patterns." *Conflict Management and Peace Science* 15(2):163–212 (DOI: 10.1177/073889429601500203).
- Kahn, Matthew E. 2005. "The Death Toll from Natural Disasters: The Role of Income, Geography, and Institutions." *Review of Economics and Statistics* 87(2):271–284.
- Keefer, Philip, Eric Neumayer and Thomas Plümper. 2011. "Earthquake Propensity and the Politics of Mortality Prevention." *World Development* 39(9):1530–1541.
- Kelman, Ilan. 2012. *Disaster Diplomacy: How Disasters Affect Peace and Conflict*. Abingdon, U.K.: Routledge.
- Keohane, Robert O. and David G. Victor. 2011. "The Regime Complex for Climate Change." *Perspectives on Politics* 9(1):7–23.
- Kersting, Erasmus and Christopher Kilby. 2014. "Aid and Democracy Redux." *European Economic Review* 67:125–143.
- Kevlihan, Rob, Karl DeRouen and Glen Biglaiser. 2014. "Is US Humanitarian Aid Based Primarily on Need or Self-Interest?" *International Studies Quarterly* 58(4):839–854.

- Knack, Stephen. 2004. "Does Foreign Aid Promote Democracy?" *International Studies Quarterly* 48(2):251–266.
- Kono, D. Y., G. R. Montinola and N. Verbon. 2013. "Helping Hand or Heavy Hand? Foreign Aid, Regime Type and Domestic Unrest." *International Political Science Review* Forthcomin:1–16.
- Kono, Daniel Yuichi and Gabriella R. Montinola. 2009. "Does Foreign Aid Support Autocrats, Democrats, or Both?" *The Journal of Politics* 71(02):704.
- Krasner, Stephen D. 1982. "Structural Causes and Regime Consequences: Regimes as Intervening Variables." *International Organization* 36(2):185–205.
- Kuziemko, Ilyana and Eric Werker. 2006. "How Much Is a Seat on the Security Council Worth? Foreign Aid and Bribery at the United Nations." *Journal of Political Economy* 114(5):905–930.
- Lancaster, Carol. 2007. *Foreign Aid: Diplomacy, Development, Domestic Politics*. Chicago: University of Chicago Press.
- Lazarev, Egor, Anton Sobolev, Irina V. Sobolevia and Boris Sokolov. 2014. "Trial by Fire: A Natural Disaster's Impact on Support for the Authorities in Rural Russia." *World Politics* 66(4):641–668.
- Levy, Jack and William Thompson. 2010. *Causes of War*. Malden: Wiley-Blackwell.
- Long, J Scott. 1997. *Regression Models for Categorical and Limited Dependent Variables*. Thousand Oaks, CA: Sage.
- Marshall, Monty G., Keith Jaggers and Ted Robert Gurr. 2010. *Polity IV Project: Dataset Users' Manual*. College Park, MD: University of Maryland.
- Meirowitz, Adam and Joshua A. Tucker. 2013. "People Power or a One-Shot Deal? A Dynamic Model of Protest." *American Journal of Political Science* 57(2):478–490.
- Milner, Helen V. 2006. Why Multilateralism? Foreign Aid and Domestic Principal-Agent Problems. In *Delegation and Agency in International Organizations*, ed. Darren G. Hawkins, David A. Lake, Daniel L. Nielson and Michael J. Tierney. Cambridge: Cambridge University Press pp. 107–139.
- Milner, Helen V. and Dustin Tingley. 2012. "The Choice for Multilateralism: Foreign Aid and American Foreign Policy." *The Review of International Organizations* 8(3):313–341.
- Morrison, Kevin M. 2009. "Oil, Nontax Revenue, and the Redistributive Foundations of Regime Stability." *International Organization* 63(01):107.



- Nardulli, Peter F., Buddy Peyton and Joseph Bajjalieh. 2013. "Climate Change and Civil Unrest: The Impact of Rapid-onset Disasters." *Journal of Conflict Resolution* .  
**URL:** <http://jcr.sagepub.com/cgi/doi/10.1177/0022002713503809>
- Nel, Philip and Marjolein Righarts. 2008. "Natural Disasters and the Risk of Violent Civil Conflict." *International Studies Quarterly* 52(1):159–185.
- Nelson, Travis. 2010. "When Disaster Strikes: On the Relationship between Natural Disaster and Interstate Conflict." *Global Change, Peace & Security* 22(2):155–174.
- Neumayer, Eric. 2003. "What Factors Determine the Allocation of Aid by Arab Countries and Multilateral Agencies?" *Journal of Development Studies* 39(4):134–147.
- Neumayer, Eric, Thomas Plümper and Fabian Barthel. 2014. "The Political Economy of Natural Disaster Damage." *Global Environmental Change* 24(1):8–19.
- Nordas, Ragnhild and Nils Petter Gleditsch. 2007. "Climate Change and Conflict." *Political Geography* 26(6):627–638.
- Nossiter, Adam. 2014. "Ebola Help for Sierra Leone Is Nearby, but Delayed on the Docks." *The New York Times* .
- Nunn, Nathan and Nancy Qian. 2014. "US Food Aid and Civil Conflict." *American Economic Review* 104(6):1630–1666.
- OECD. 2014. "OECD.stat (Database).".  
**URL:** [10.1787/data-00285-en](http://dx.doi.org/10.1787/data-00285-en)
- Olson, Mancur. 1965. *The Logic of Collective Action*. Cambridge: Harvard University Press.
- Omelicheva, Mariya Y. 2011. "Natural Disasters: Triggers of Political Instability?" *International Interactions* 37(4):441–465.
- Oppenheimer, Michael, Maximiliano Campos, Rachel Warren, Joern Birkmann, George Luber, Brian O'Neill and Kiyoshi Takahashi. 2014. Emergent Risks and Key Vulnerabilities. In *Climate Change 2014: Impacts, Adaptation, and Vulnerability. Part A: Global and Sectoral Aspects. Contribution of Working Group II to the Fifth Assessment Report of the Intergovernmental Panel of Climate Change*, ed. C.B. Field, V.R. Varros, D.J. Dokken, K. J. Mach, M.D. Mastrandrea, T.E. Bilir, M. Chatterjee, K.L. Ebi, Y.O. Estrada, R.C. Genova, B. Girma, E.S. Kissel, A.N. Levy, S. MacCracken, P.R. Mastrandrea and L.L. White. Cambridge: Cambridge University Press pp. 1039–1099.
- Palmer, Glenn, Vito D'Orazio, Michael Kenwick and Matthew Lane. 2015. "The MID4 data set: Procedures, coding rules, and description." *Conflict Management and Peace Science* 32(2):222–242 (DOI: 10.1177/0738894214559680).

- Perry, Ronald W. 2007. What is a Disaster. In *Handbook of Disaster Research*, ed. Havidan Rodriguez, Enrico L. Quarantelli and Russell Dynes. New York: Springer pp. 1–15.
- Plümper, Thomas and Eric Neumayer. 2009. “Famine Mortality, Rational Political Inactivity, and International Food Aid.” *World Development* 37(1):50–61.
- Polman, Linda. 2010. *The Crisis Caravan: What’s Wrong with Humanitarian Aid?* New York: Metropolitan Books.
- Qian, Nancy. 2015. “Making Progress on Foreign Aid.” *Annual Review of Economics* 7:277–308.
- Quarantelli, Enrico L. (Ed.). 2005. *What is a Disaster? A Dozen Perspectives on the Question*. London: Routledge.
- Quiroz Flores, Alejandro and Alastair Smith. 2013. “Leader Survival and Natural Disasters.” *British Journal of Political Science* 43(4):821–843.
- R Core Team. 2014. *R: A Language and Environment for Statistical Computing*. Vienna, Austria: R Foundation for Statistical Computing.  
**URL:** <http://www.r-project.org/>
- Raschky, Paul A. and Manijeh Schwindt. 2012. “On the Channel and Type of Aid: The Case of International Disaster Assistance.” *European Journal of Political Economy* 28(1):119–131.
- Ray, James Lee. 2003. “Explaining Interstate Conflict and War: What Should Be Controlled for?” *Conflict Management and Peace Science* 20(2):1–31.
- Riddell, Roger C. 2007. *Does Foreign Aid Really Work?* Oxford: Oxford University Press.
- Salehyan, Idean. 2008. “From Climate Change to Conflict? No Consensus Yet.” *Journal of Peace Research* 45(3):315–326.
- Schneider, Stephen H., Serguei Semenov, Anand Patwardhan, Ian Burton, Chris H.D. Magadza, Michael Oppenheimer, A. Barrie Pittock, Atiq Rahman, Joel B. Smith, Avelino Suarez and Farhana Yamin. 2007. Assessing Key Vulnerabilities and the Risk from Climate Change. In *Climate Change 2007: Impacts, Adaptation and Vulnerability. Contribution of Working Group II to the Fourth Assessment Report of the Intergovernmental Panel on Climate Change*, ed. Martin L. Parry, Osvaldo F. Canziani, Jean P. Palutikof, Paul .J. van der Linden and Clair E. Hanson. New York: Cambridge University Press pp. 779–810.
- Scott, James M. and Carie A. Steele. 2011. “Sponsoring Democracy: The United States and Democracy Aid to the Developing World, 1988-2001.” *International Studies Quarterly* 55(1):47–69.

- Singer, J. David, Stuart Bremer and John Stuckey. 1972. Capability distribution, uncertainty, and major power war, 1820-1965. In *Peace, War, and Numbers*, ed. Bruce Russett. Beverly Hills: Sage Publications pp. 19–48.
- Slettebak, Rune T. 2012. “Don’t Blame the Weather! Climate-related Natural Disasters and Civil Conflict.” *Journal of Peace Research* 49(1):163–176.
- Smith, Alastair. 2008. “The Perils of Unearned Income.” *The Journal of Politics* 70(03):780–793.
- Staiger, Douglas and James H. Stock. 1997. “Instrumental Variables Regression with Weak Instruments.” *Econometrica* 65(3):557–586.
- Stalley, Phillip. 2003. “Environmental Scarcity and International Conflict.” *Conflict Management and Peace Science* 20(2):33–58.
- Stephan, Maria J. and Erica Chenoweth. 2008. “Why Civil Resistance Works: The Strategic Logic of Nonviolent Conflict.” *International Security* 33(1):7–44.
- Streich, Philip A. and David Bell Mislan. 2013. “What Follows the Storm? Research on the Effect of Disasters on Conflict and Cooperation.” *Global Change, Peace and Security* pp. 1–16.
- Strezhnev, Anton and Erik Voeten. 2013. “United Nations General Assembly Voting Data.”  
**URL:** <http://hdl.handle.net/1902.1/12379> UNF:5:s7mORKL1ZZ6/P3AR5Fokkw==
- Strömberg, D. 2007. “Natural Disasters, Economic Development, and Humanitarian Aid.” *The Journal of Economic Perspectives* 21(3):199–222.
- Svensson, Jakob. 2000. “When is foreign aid policy credible? Aid dependence and conditionality.” *Journal of Development Economics* 61(1):61–84.  
**URL:** <http://linkinghub.elsevier.com/retrieve/pii/S0304387899000619>
- Taylor, Glyn and Abby Stoddard. 2012. “Independent Review of the Underfunded Emergencies Window of the Central Emergency Response Fund ( CERF ).” (October).
- Teorell, Jan. 2010. *Determinants of Democratization*. Cambridge: Cambridge University Press.
- Terry, Fiona. 2002. *Condemned to Repeat? The Paradox of Humanitarian Action*. Ithaca: Cornell University Press.
- The Economist. 2010. “Pakistan’s Floods: A Ruined Country.” *The Economist* p. 18.
- Tierney, Michael J., Daniel L. Nielson, Darren G. Hawkins, J. Timmons Roberts, Michael G. Findley, Ryan M. Powers, Bradley Parks, Sven E. Wilson and Robert L. Hicks. 2011. “More Dollars than Sense: Refining Our Knowledge of Development Finance Using AidData.” *World Development* 39(11):1891–1906.

- Tilly, Charles. 1990. *Coercion, Capital, and European States, AD 990-1990*. Malden: Wiley-Blackwell.
- Tir, Jaroslav and Douglas M. Stinnett. 2012. "Weathering Climate Change: Can Institutions Mitigate International Water Conflict?" *Journal of Peace Research* 49(1):211–225.
- Torbati, Yeganeh. 2012. "Iran Government Criticized Over Earthquake Response." *Reuters* .  
**URL:** <http://www.reuters.com/article/2012/08/14/us-iran-quake-idUSBRE87C0OW20120814>
- Toya, Hideki and Mark Skidmore. 2007. "Economic Development and the Impacts of Natural Disasters." *Economics Letters* 94(1):20–25.
- Tschoegl, Liz, Regina Below and Debarati Guha-Sapir. 2006. An Analytical Review of Selected Data Sets on Natural Disasters and Impacts. Technical report Centre for Research on the Epidemiology of Disasters.
- Tures, John A. 2009. "Rattling the Hesam: International Distractions from Internal Problems in Iran." *Asian Politics and Policy* 1(1):50–78.
- UNSC. 2014. "Membership Since 1946." .  
**URL:** <http://www.un.org/en/sc/members/search.asp>
- Valenta, Jiri and Virginia Valenta. 1987. The FSLN in Power. In *Conflict in Nicaragua: A Multidimensional Perspective*, ed. Jiri Valenta and Esperanza Duran. Boston: Allen & Unwin pp. 3–40.
- Valinejad, Afshin. 2012. "Iran's Summer Earthquake Leaves 100,000 Shivering in Tents as Winter Descends." *The Christian Science Monitor* .  
**URL:** <http://www.csmonitor.com/World/Middle-East/2012/1122/Iran-s-summer-earthquake-leaves-100-000-shivering-in-tents-as-winter-descends>
- Van Aalst, Maarten K. 2006. "The Impacts of Climate Change on the Risk of Natural Disasters." *Disasters* 30(1):5–18.
- van de Walle, Nicolas. 2001. *African Economies and the Politics of Permanent Crisis, 1979-1999*. Cambridge: Cambridge University Press.
- Vaughn, Bruce, Nicole T. Carter, Pervaze A. Sheikh and Renee Johnson. 2010. Security and the Environment in Pakistan. Technical report Congressional Research Service.  
**URL:** <http://fpc.state.gov/documents/organization/146411.pdf>
- Weeks, Jessica L. 2012. "Strongmen and Straw Men: Authoritarian Regimes and the Initiation of International Conflict." *American Political Science Review* 106(2):1–22.

- White, Halbert. 1980. "A Heteroskedasticity-Consistent Covariance Matrix Estimator and a Direct Test for Heteroskedasticity." *Econometrica* 48(4):817–838.
- World Bank. 2013. "Databank."  
**URL:** <http://data.worldbank.org/>
- Wright, Joseph. 2009. "How Foreign Aid Can Foster Democratization in Authoritarian Regimes." *American Journal of Political Science* 53(3):552–571.
- Yim, Eugene S., David W. Callaway, Saleh Fares and Gregory R. Ciottone. 2009. "Disaster Diplomacy: Current Controversies and Future Prospects." *Prehospital and Disaster Medicine* 24(4):291–293.